THURSDAY, AUGUST 3, 1882

FRANCIS MAITLAND BALFOUR

DEATH has been striking heavy blows at Cambridge. Only a little while ago we were mourning the loss of Maxwell, taken from us, as it seemed, in his prime, when we were fondly hoping that for many years yet to come, the bounds of science would continue to be widened by the labours of his powerful mind; and now Balfour has been snatched from us in the very flush of youth, with his work only just begun, for what he had achieved, great as it was, seemed to his friends only an earnest of that which was yet to come.

The editor of NATURE has asked me to write a few words about my lost friend; and I obey, feeling it my duty not to refuse, painful as the task is.

Francis Maitland Balfour was born in 1851 or 1852 (I cannot at this moment find out which), and was therefore, at the time of his death, only about thirty years of age. After spending some years at Harrow, where he gained the reputation of being a bright, clever boy, but raised no adequate expectations of what he was about to become, he entered at Trinity College, Cambridge, in October, 1870. He had the good fortune to become at once the private pupil of Mr. Marlborough Pryor, who had just been elected the first Natural Science Fellow at Trinity, and though Balfour had already turned towards natural science, and indeed had gone pretty fully into the geology of his native county (Haddingtonshire), I cannot but think that the direction of his studies, and so of his future career, was largely determined by Pryor's admirable influence. I myself was called to Trinity College as Prælector at the same time that Balfour entered, and I believe he, in his second term, attended the lectures which I was then giving; but I did not distinctly make his acquaintance till March, 1871, when I took part in an examination at Trinity College, which resulted in Balfour being elected Natural Science Scholar. From that time onward we became more and more intimate, and I took an increasing share in the direction of his studies.

Discovering very early how great his powers of mind were, and learning that his private resources were such as to enable him to disregard the pecuniary advantages of academic success, I did, what seemed to some at the time, a rash thing: I counselled him to neglect the ordinary routine preparation for his degree, and to apply himself at once to original work. His mind from the first was drawn towards morphology rather than towards physiology; and, as I was then attempting to embody in a small volume some of the lectures on embryology which I had given in London and Cambridge, I proposed to him that he should associate himself with me in the work, and undertake at once the investigation of some of the many embryological problems which lay to hand. He did so, and the results of these, his early labours, are partly contained in the papers "On the Layers of the Blastoderm," "On the Primitive Streak," and "On the Development of the Blood-vessels," published in the *Quarterly Journal of Microscopical Science*, July, 1873, and are partly scattered and hidden in the little work "On the Development of the Chick," which bears his name and mine. The larger achievements of his succeeding years have of course overshadowed these works of his 'prentice hand; but while he was engaged on them, that power of acute observation, rapid grasp of the meaning of things, and strict faithfulness of statement, which all have since recognised in him, became evident to myself at least.

In December, 1873, after breaking off his original work for two or three months in order to prepare himself more definitely for the examination, he obtained his B.A. degree in the so-called Natural Sciences Tripos. Happily and wisely just then the University of Cambridge had secured two "tables" at the newly established zoological station at Naples, and in the early winter, Balfour, in company with Mr. A. G. Dew-Smith, started off to work there. His knowledge and insight had already led him to expect that much might be learnt from the investigation of the early history of the Elasmobranch Fishes, and that accordingly was the problem which he set before himself, and on which he worked during that and succeeding years.

The results of those labours, embodied in his Monograph on the Development of the Elasmobranch Fishes, published as a volume in 1878, but as separate papers in the *Journal of Anatomy and Physiology* from 1876 to 1878, and in the *Philosophical Transactions* for 1876, are known to all biologists. This is not the time to point out in detail their value, but this at least may be said that from the very beginning to the very end of the investigation almost every step serves to throw light on important biological problems. Every chapter from the first, which deals with the ovarian ovum, to the last, which treats of the urogenital organs, contains a record of inquiries which have left their stamp on morphological doctrines. When I remember what embryology was, when in 1871 Balfour learnt his first lessons in it from my imperfect lips, and reflect what it is now, the progress of ten years appears little short of marvellous: and how much of that progress is due to the hand and brain which a slip on the treacherous mountain path has snatched from our midst!

In October, 1874, an election to a Natural Science Fellowship, at Trinity College, took place. Happily the governing body of the College had previously determined to make a new departure, and to allow original work, as well as the results of an examination, to weigh in the decision of the electors. I believe I am betraying no secret in saying that had the election been determined by an examination alone, Balfour would never have been Fellow of Trinity, and Cambridge would probably have lost one of its brightest ornaments. Balfour was one of those able men who never "do themselves justice in an examination-room," and his performance in answering the set questions was inferior to that of his competitor. But the winter at Naples had already borne fruit; and Prof. Huxley, who kindly assisted in the examination, gave such testimony as to the value and promise of so much of the work on the Elasmobranchs as had already been done, that no hesitation as to choosing Balfour was possible.

This success encouraged him to even increase his activity. He continued to work either at Naples or in Cambridge, and in 1875, after delivering a short course on embryology for me, he began (upon the invitation of Prof. Alfred Newton and with the support of Mr. J.

W. Clark) to give definite lectures upon animal morphology, at first in conjunction with Mr., now Prof. Milnes Marshall, but after two terms, by himself. From that time up to last Christmas his labours were enormous, and his energy untiring. His class grew rapidly in numbers; he had to separate the students into an elementary and advanced division, each with separate lectures, and courses of practical instruction; and though he soon gained the able assistance of Mr. Adam Sedgwick and others as demonstrators, all his pupils enjoyed the priceless advantages of close personal contact with himself. At the same time he carried on, either by himself, or through his pupils, a large number of independent investigations into various problems of embryology and morphology, and set himself to write that great work on "Comparative Embryology," every page, and indeed every line of which is marked at once by the widest knowledge and the clearest insight, and which will tell men in long years to come how great is our loss to day. And all the while he was most active in university and college matters; every syndicate almost was desirous to secure his services, and in the framing of the new statutes of Trinity College he took among the junior fellows a prominent part.

During all these exertions his friends, and I not less than any of them, watched him with anxious care. But he was wise as well as zealous, and never went too far; and when, the second volume of the big book being off his hands, he started last Christmas for a holiday to Messina, the prospects of his health seemed to me better than ever. On his journey outward, he found one of his pupils who had gone to study at Naples laid up with typhoid fever at Capri, and with characteristic kindness he halted to nurse the patient till friends could arrive from England. On his return home, he himself was struck down by an attack of the same fever, which at first threatened to be severe, but happily proved otherwise, and speedily left him; and soon after there came an event which was to him one of the greatest pleasures of his short life.

His fame was now spreading rapidly wherever science reaches, and honours were coming thick upon him. In 1878 he was elected Fellow of the Royal Society, and in 1881 was not only placed on the Council, but received the high distinction of a Royal Medal. In the same year the University of Glasgow conferred on him the degree of LL.D., the British Association, at the York meeting, chose him as one of the General Secretaries, in December last a brilliant company assembled at Cambridge to greet him as President of the Cambridge Philosophical Society, and while he was on his sick bed, the Committee of the Athenæum elected him, as a distinguished man of science, a Member of that Club. Moreover, other Universities were eager, if possible, to win him for themselves. I believe it is no secret that many efforts were made to induce him to become the successor at Oxford of the lamented Rolleston; and it is certainly no secret that the Government again and again pressed him to take the chair of Natural History at Edinburgh. He, however, remained faithful to his Alma Mater, and though, owing to difficulties arising out of impending changes, his merit, in spite of the esteem and pride with which all men at Cambridge regarded him, remained without adequate recognition in his own University, he chose to remain with us, waiting till the future should bring him his dues.

Happily a special effort disclosed the fact that the difficulties were, after all, not unsurmountable; and at last, this spring, with the approbation, I believe, of the whole University, scientific or otherwise, and certainly to the great joy of his friends, a special chair of Animal Morphology was created for him, and he was placed in it.

With this recognition of his worth, which he, I believe, valued beyond even his weightier honours, with the prospect of the increased facilities which the new statutes would give in the coming session, and with his health becoming rapidly restored (for since his fever he had nursed himself, doing but little work, or what to him was little), all the future seemed brighter than it had ever seemed as yet. And when in early July I parted with him, and heard him promise that on those perilous Alpine tracks, he, remembering his past illness, would try nothing rash or likely to strain his powers, I looked forward to meeting him again, both of us perhaps fuller of hopes and plans than we had ever been before.

Of the details of his death, at the moment of writing, we as yet know very little, save that some fatal slip on the glacier of Fresney, above Courmayeur, hurried him and his guide to an instantaneous death.

And now comes the hardest part of my task. The world of science knew Francis Balfour as an investigator of the brightest promise, who, indeed, as a mere youth, had already solved morphological problems which had heretofore baffled the acutest minds, and of whom it seemed difficult to say how far he might not reach. A smaller circle in this country and in Europe knew him also as a man whose firm will and rapid but clear judgment were all the more effective, because his decisions and resolves were made known to others with a winning courtesy and with a kindly sensitive regard for the feelings of those from whom he might differ in opinion. But only those who had the privilege to be his friends knew his real worth, for they alone were aware how much the light of his personal character outshone even his scientific achievements and his administrative powers. It will need great knowledge and skill on the part of him who attempts to show exactly how much science owes to Francis Balfour as an inquirer, a teacher, and a counsellor; but that will be an easy task compared with the effort to tell to those who did not know him what he really was. Workers in biology all the world over will feel that a light has gone from the world when they hear the sad news that he is dead, Cambridge men who have watched events at Cambridge during the last ten years will know that a wholly irreparable loss has fallen upon their University, but their grief and their loss is a small thing put by the side of the emptiness which is left for them whose daily life was brightened by the light of his countenance. These mourn for Lycidas, and cannot be comforted.

M. FOSTER

THE MOUNT WHITNEY EXPEDITION

EXPERIMENTS at the Alleghany Observatory in 1879 and 1880, upon the selective absorption of the solar rays by the earth's atmosphere, having made it probable that the amount of heat the sun sends us (the solar constant) had been under-estimated by the methods of previous observers, and that conclusions of importance connected with the temperature of the planet remained to be drawn, it became desirable to verify these results, obtained near the sea-level, by direct simultaneous observations at the base and summit of a very high mountain.

The generosity of a citizen of Pittsburg had provided the special apparatus devised for the new methods, but this was too elaborate to be easily moved to a distant and elevated station. Upon the bearing of the contemplated observations on practical problems of meteorology becoming known to Gen. W. B. Hazen, the Head of the U.S. Signal Service, he kindly offered to facilitate the transportation of an expedition from the Alleghany Observatory to the Western Territories; for no wholly suitable site presented itself, save in regions where the aid of the army might be desirable both for transportation and escort.

With the consent of the trustees of this Observatory, the offer was gladly accepted, and the expedition, which, as originally planned, was a private one, proceeded with material aid from the Signal Service; and under the advantage of Gen. Hazen's official direction I am enabled, by his kind permission, to here briefly indicate its main objects and results, in advance of a full publication of them, which will shortly be made.

The site selected, on the suggestion of Mr. Clarence King, and after conference with officers of the Army and Coast Survey familiar with the western wilderness, as suited to the special observations (which made both great altitude and great dryness desirable), was Mount Whitney in the Sierra Nevada of Southern California. It rises to nearly the height of Mont Blanc from one of the most arid regions in the world, and so abruptly, that two stations can be found within easy signalling distance, whose difference of elevation is over 11,000 feet. It is, however, on the other side of the continent, in an imperfectly explored region, and as very little was known of the possibility of carrying such apparatus as ours to the summit, a military escort was provided, on the contingency of our being obliged to occupy some site still more remote from civilisation.

The scientific members of the party, consisting of Capt. O. E. Michaelis, of the Ordnance, of Messrs. J. E. Keeler and W. C. Day, civilian assistants, and the writer, started from Pittsburg on July 7, for a railroad journey of over 3000 miles, which was greatly facilitated by the courtesy of Mr. F. Thomson, of the Pennsylvania Company, which enabled the writer to take all the apparatus with him in a special car. At San Francisco, after some delay, which the kindness of General M'Dowell, commanding the department of the Pacific, enabled us to pass agreeably, the escort was provided, and the party joined by Mr. George Davidson and two Signal Service non-commissioned officers. With these it proceeded to a point about 400 miles further south (Caliente), where we exchanged the swift motion and the comforts of the Pullman car, for the sharp contrast of a route which commenced (with the shade thermometer registering 110° F.) by a slow journey across the Inyo Desert, shadeless and waterless, for one hundred and twenty miles. We reached, at the close of the month, Lone Pine at the foot of the Sierras, where a camp was made, to be occupied as a lower station, and where the instruments were set up. Among these was a massive siderostat, sending a horizontal beam to a specially constructed spectrometer, an instrument larger than the one which usually bears the name, and in which the eye is replaced by a bolometer, so adjusted as to measure the heat separately in any ray of the visible and the invisible spectrum. When these measures are repeated at an altitude so great that the total atmospheric absorption is markedly different, we are enabled to determine the rate of this absorption for each ray, and, inferentially, to place the observer wholly outside our atmosphere, and to reconstruct the whole spectrum with the hitherto unknown distribution of energy which must exist there. This was used in connection with actinometers, pyrheliometers, and the usual instruments for such a research; but the problem of the safe transportation of the larger apparatus to the summit of Mount Whitney, which now rose above us in a seemingly perpendicular wall of granite, here presented itself in its full difficulty. Our final destination at the summit was in the clearest view, for the extraordinary dryness of the air and the absence of all aërial perspective made the mountain seem so near, that it wore the aspect of a quite neighbouring pile of lofty and moss-covered gray rock, patched with white, and of a wildly jagged outline (the "Sierra"). This white (which we knew to be large snow fields), and the use of the telescope, which resolved the "moss" into great forests, dispelled the illusion, and we realised the obstacles we had to surmount. A preliminary exploration showed that the ascent was difficult, and with such apparatus as ours impossible; a long detour was therefore necessary to avoid the precipices on the eastern front, and our mule trains were in fact occupied from seven to eight days in reaching a point below the summit actually but sixteen miles distant. We traversed in the ascent a trackless wilderness, climbing what seemed utterly impracticable stony heights; down which, once or twice a mule lost his footing and rolled with his scientific freight, but over which, though by such a way as siderostats and telescopes probably never travelled before, all finally passed with a degree of safety beyond our hopes.

We had come here to determine (among other objects) what part of the surface temperature of the planet was due to the sun's direct radiant heat, and what part to the effect of the earth's atmosphere in storing this heat.

It was interesting then, if not wholly agreeable, to repeat the experience of former observers in our own persons, and to notice that as we ascended and the air grew colder, the sun grew hotter, till our faces and hands, browned as they already were by weeks of sunshine below, were burned anew, and far more in the cold than in the desert heat. As we still slowly ascended, and the surface-temperature of the soil fell to the freezing-point, the solar radiation became intenser, and many of the party presented an appearance as of severe burns from an actual fire, while near the summit the temperature in a copper vessel, over which was laid two sheets of plain window glass, rose above the boiling-point, and it was certain that we could boil water by the direct solar rays in such a vessel among the snow-fields.

It is possibly worth remark that owing to the dryness of the air, though isolated snow-fields lay above and below us, the ground we travelled over was bare, and it could not be asserted (as it has been of cases at like altitudes in the Alps), that reflection from the snow had anything to do with the state of our very literally "burned" faces.

In view of the lateness of the season it was decided to make the permanent camp at an altitude of rather less than 13,000 feet, rather than wait and make a road to the peak, which rose 2000 feet above us, and was daily climbed for observations with portable instruments.

The sky was perpetually fine, of a deeper violet than I have observed elsewhere, even on Etna, and the dryness extreme, though water from the snow-fields above was abundant. An equatorial telescope of $5\frac{1}{2}$ inches aperture, which was kindly loaned by Prof. E. C. Pickering, it was found too late could not be well used to determine the quantity of the "seeing," owing to a maladjustment of the eye-pieces, which it was necessary to hold in the hand. Under these circumstances a critical estimate of the definition under high powers could not be made, but enough was seen to make it evident that it was generally excellent, and that such a site possessed advantages for an astronomical, as well as for a meteorological station.

It is greatly to be desired that it should be occupied, with the protection of a permanent building, adapted to either object, and it is probable that such provision will be hereafter made.

For us, however, there was no other shelter than our tents, and the high wind, the cold, and the mountain sickness consequent on the rarefied air, made the continuous observations which were kept up synchronously with that at the camp at Lone Pine, a matter of difficulty. These observations were persistently maintained, however, but we were not sorry on September 11 to break up our wintry camp and to descend to summer again. We resumed our journey across the desert, and then across the continent, reaching Pittsburgh on September 28, 1881.

The reductions of the observations are still incomplete, but some conclusions of interest may already be indicated.

It has been said that the determination of the amount of heat the sun sends the earth is the fundamental problem of meteorology, since on this all the phenomena which that science contemplates depend. Accordingly the observations were directed first to this primary object, chiefly through methods which involved the study of the phenomena of selective absorption (so intimately connected with it), and secondarily to these phenomena considered in themselves.

The final result may be affected by some still imperfectly determined corrections, and it will be sufficient to here give an approximative one.

It appears probable that the true solar constant is one-half greater than that determined by Pouillet and by Herschel near the sea level, and even greater than the recent values assigned by M. Violle. The true value, it is believed, will be shown by the data when published to be larger than those hitherto accepted.

Of more general interest, perhaps, is the conclusion as to the limit of that cold which increases under full sunshine as we ascend above our atmosphere. "What," it may be asked, "would the temperature of the soil be on a mountain top rising wholly above the air, or what the temperature of the sunward hemisphere of the earth, if the present absorbing atmosphere were wholly withdrawn?" The personal experiences already alluded to may prepare the general reader for the paradoxical result that if this atmosphere were withdrawn, the temperature would greatly fall, though under a materially greater radiant heat.

The student of the subject is aware that this conclusion follows from the fact that the loss by radiation into space as the atmosphere is withdrawn is much more rapid than the gain by direct solar heat, but even he may not perhaps be prepared for the extent of the fall.

The original observations, which will be given at length, lead, in the writer's opinion, to the conclusion that in the absence of an atmosphere the earth's temperature of insolation would at any rate fall below $-50°$ F., by which it is meant that (for instance) mercury would remain a solid under the vertical rays of a tropical sun were radiation into space wholly unchecked, or even if the atmosphere existing, it let radiations of all wave-lengths pass out as easily as they come in. Remembering, then, that it is not merely by the absorption of our air, but by the selective quality of this absorption, that the actual surface temperature of our planet is maintained, we see that without this comparatively little-known function, it appears doubtful whether, even though the air supported respiration and combustion as now, life could be maintained upon this planet.

These conclusions do not, in the writer's opinion. depend upon the Mount Whitney observations alone, but exist implicitly in the results of previous observers who have, however, not apparently drawn them, with the exception, perhaps, of Mr. Ericsson, who has observed that the surface of the airless moon must remain cold even in sunshine.

We see, if these results be true, that the temperature of a planet may, and not improbably does, depend far less upon its neighbourhood to, or remoteness from, the sun, than upon the constitution of its gaseous envelope, and indeed it is hardly too much to say that we might approximately indicate already the constitution of an atmosphere which would make Mercury a colder planet than the earth, or Neptune as warm and habitable a one.

It must at the same time be admitted that our information as to the special constituents of our own air, which are chiefly here concerned, is still imperfect, though the observations made at Mount Whitney upon the selective action of that undoubtly prominent agent, water-vapour, will, it is hoped, add somewhat to our knowledge.

In the same connection it may be added that the writer's investigations have led him to the conclusion that the "temperature of space," so called, must at any rate be lower than that assigned by Pouillet (if we accept the received values for that of the absolute zero), and in this case the temperature of the earth's surface, in the absence of the quality of selective absorption in our air, would be yet lower than that here given.

In view of the great importance of this quality, interest will attach to the statement that the bolometer observations at the summit and base of Whitney show a different distribution of solar energy (heat, light, or "actinism") at the upper station from that at the lower, and show (among other things) that without our atmosphere, the sun would appear of a strongly bluish tint, thus confirming observations already made at Alleghany by other methods. It may be added in this connection that researches also there made show a like action in the solar atmosphere, so that we are not only to understand that there is a tendency in both atmospheres to absorb the short waves more than the long ones (as the writer has elsewhere stated), but that the solar photosphere (while emitting radiations of all wave-lengths in greater quantity than we receive them), is, through the immense preponderance of the more refrangible rays before absorption, essentially blue, and that white light is *not* "the sum of all radiations," nor even of all visually recognisable ones, but a composition of the small groups of special rays, which, starting from this essentially blue sun, by virtue of their large co-efficients of transmission, and by a kind of survival of the fittest, have struggled through the solar and terrestrial atmospheres, to us, while others of short wave-length have failed on the way. This the Mount Whitney observations, so far as regards the terrestrial atmosphere at any rate, appear to prove.

Doubtless a distinction is to be drawn between the statements of fact and records of direct observation, which will shortly appear in full in the Signal Service publication, and the present inferences from them, for which the writer alone should be held responsible.

In view of the mass of observations on which they rest, and the writer's endeavour to avoid any statement which does not seem to him to express the result of careful and repeated experiments, he hopes, however, that the results to be given in the forthcoming volume will be found to bear out these conclusions, and prove useful contributions from the younger science of solar, to the elder one of terrestrial meteorology. S. P. LANGLEY

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Alleghany Observatory, Alleghany, Pennsylvania, July 13, 1882

ASIA

Asia. By A. H. Keane. Edited by Sir R. Temple. (London: Edw. Stanford, 1882.)

MR. KEANE'S encyclopædic knowledge in matters of philology and ethnology was never put to better use than in the compilation of this account of Asia. Considerable as is the size of the book, the information it contains is compressed to the utmost; every word is pregnant with meaning, and could not be omitted without injury to the reader. The physical geography, the fauna and flora, the commerce and inhabitants of the vast continent of Asia, are all passed under review; tribes and dialects of which most of us have never even heard the names are discoursed upon familiarly, and facts and statistics bristle in every page. The latest authorities have been everywhere consulted; the geographical results of the late Afghan war, for instance, having been laid under contribution, and full use being made of the Palestine Exploration Survey, not only of Palestine itself but of the eastern side of the Jordan as well. It must not be supposed, however, that Mr. Keane's work is dry reading; his literary ability has thrown an interest over the most matter-of-fact statistics and made us realise the characteristics of the countries he describes or the towns and populations he records.

I need not point out the value to the Englishman of a full and trustworthy compendium like this. As Sir Richard Temple shows in his Preface to the book the interests of England in Asia are enormous, and there is much truth in the German assertion that so far as English power and *prestige* are concerned we are no longer Great Britain but Great India. But it is not only in India that English influence is supreme; our dependencies extend as far as Hongkong, and our trade with Japan amounted in 1880 to over five millions of imports alone. To the student of mankind the interest of Asia is greater than that of any of the other continents of the world. Here was the first home of the races who have chiefly influenced the course of human progress; here the early civilisations of Accad, of China, and of Phœnicia grew up and developed; here the great empires of antiquity rose one upon the other; and here was the primæval source of those germs of thought and art that have produced the philosophies, the sciences, and the arts of our own day. It is among the multitudinous tribes and nations of Asia, too, that we can best study that variety of languages, of manners, and of customs which have enabled the modern inquirer to lift a little the veil that covers the first beginnings of civilisation, and there are even some who believe that the great central plateau of Tibet before it was raised to its present elevation was the primæval cradle of mankind, the spot where the anthropoid ape became the still speechless man. It is possible that our young and therefore arrogant western civilisation has yet much to learn from the old culture of the east, and it is a question whether Sir R. Temple is justified in saying that the Chinese are "implacably hostile to real progress" because they hate "modern (European) progress." At all events the example of Japan is not encouraging.

It is a great pity that Mr. Keane's alphabetical list of the races and languages of Asia had to be sacrificed to the exigencies of space. The room now occupied by a number of very useless woodcuts could well have been given up to it. We must be thankful, however, for the Ethnological and Philological Appendix which he has added at the end. In this he sums up in a clear and trenchant manner the chief facts at present known about the ethnology of Asia. He pertinently points out the great distinction that exists between the different types of race and language we find there. "All races are fertile with one another, though perhaps in different degrees," whereas the stock-languages of the continent "are true species which refuse to amalgamate and thus form new species, so that fresh varieties are developed only within each" of them. The conclusion from this fact seems to me to be that while the different races of mankind may be referred to a single primitive pair, the different families of speech have branched off from independent centres. Mr. Keane, however, still clings to the belief in a single primæval language or type of language, and rejects the hypothesis that man was speechless when the leading races were differentiated from one another. But the argument on which he bases his rejection of the theory is founded on a misconception; language and race are not synonymous terms, and those who hold the doctrine of linguistic polygeneity suppose that different forms of speech grew up independently within the same race or even among the members of a fixed race. It was a question of geography not of race.

The assumption of the primitive unity of speech makes Mr. Keane an adherent of another theory which I have done my best elsewhere to combat. This is that languages develop out of an isolating into an agglutinative stage, and then into an inflectional one. I do not deny that language develops; far from it; the whole science of language is based upon such an assumption. But I can see no facts which warrant me in holding that an isolating language, for example, has ever developed except from one stage of isolation into another. In the same way the inflectional languages have developed only from one form of inflection into another; this is certainly true of the Semitic dialects, and as to the Aryan languages, Dr. Delbrück, the latest defender of Mr. Keane's theory, finds himself forced to admit at the end of a long discussion that a confident "yes" cannot be returned to the question whether the agglutination theory is verified in individual cases. It is only in the sense that the jelly-fish may be called simple that the development of language can be said to be from the simple to the complex; from one point of view, it is true, analysis and differentiation may be termed complex, but most of us would consider our modern English grammar a much simpler affair than that of Gothic or Greek. I may note here, by the way, that Mr. Keane has made a slip in saying that the final *r* of *amatur* is the reflexive pronoun *se*. *R* is also the characteristic of the passive in Old Keltic where it cannot come from an earlier *s*; it is further found in some Vedic verbal forms, and apparently in the Greek *δεῦ-ρ-ο*, where it occupies the same position as in the Latin *ama-r-is*.

Different views, however, as to the conclusions to be drawn from our evidence are inevitable in science, more especially in matters where certainty is unattainable. They in no way diminish the value or importance of Mr. Keane's work, which does not depend on the theories held by himself or any one else regarding the facts put forward in it. He has made "Asia" an indispensable book of reference to the geographer, the traveller, and the statistician; I will not add the politician also, as the main business of the latter nowadays seems to be to avoid acquiring accurate information. Here and there, of course, there is a misprint, as when Prof. Sachau's name is spelt *Sachan* (p. 72), or a statement to which exception may be taken. Thus I am not disposed to endorse the assertion that the Turks in Smyrna "reside chiefly in narrow, dirty slums, into which it is dangerous to penetrate alone, and which are cut off from access to the more open and safer quarters." On the contrary, in walking from the Kassaba station to the quay, when alone and at night, I have always taken good care to go through the Turkish quarter and not through the Greek. Elsewhere, however, Mr. Keane does full justice to the Turks of Anatolia, whom he describes in Dr. Scherzer's words as "honourable in all their dealings, frank, kindhearted, and hospitable, while in religious matters they are, contrary to the general impression, the most tolerant of all Oriental races."

A. H. SAYCE

MAGNETO- AND DYNAMO-ELECTRIC MACHINES

Die Magnet und dynamo-elektrischen Maschinen. Von Dr. H. Schellen. Zweite, nach dem gegenwärtigen auf der Pariser electrischen Ausstellung vertretenen Zustande dargestellte und vermehrte Auflage. (Köln, 1882.)

THIS work, which is considerably enlarged from its first appearance, now includes accounts of all the leading forms of dynamo- and magneto-electric machines with the exception of those of Edison, of which no mention is made. The first chapter is devoted to generalities concerning electromagnets, induction, &c. The second describes magneto-electric machines beginning with that of Pixii and ending with that of De Meritens. The third chapter, on dynamo-electric machines, opens with a rather unseemly revival of the dispute as to priority between Werner Siemens and Wheatstone in the discovery of the action-and-reaction principle of the so-called dynamo-machines. It is a matter of history that papers announcing this discovery were read before the Royal Society on the very same day (February 14, 1867) by Wheatstone and by Dr. C. W. Siemens. We cannot help thinking that Dr. Schellen, in his manner of describing the affair, allows himself to take an attitude extremely unjust towards the great English physicist, now no longer amongst the living; and we protest against this very needless attempt to arouse a scandal. Nor is it true that Wheatstone's memoir contained nothing that Werner Siemens had not previously published in Berlin. The proof of this matter is that Wheatstone's principle of exciting the field-magnets by a derived current in a shunt circuit was adopted as a "new method" by Messrs. Siemens Brothers within two years from the present date, and formed, in 1881, the basis of a communication by Dr. C. W. Siemens to the Royal Society, and of another by Mr. Alexander Siemens to the Society of Telegraph Engineers, in which the priority of Wheatstone in this detail is fully and explicitly admitted. In this chapter also the machines invented by Weston and by Brush are described. Chapter IV. treats of those dynamo-electric machines which generate continuous currents, beginning with Pacinotti's machine of 1863, and including the well-known forms of Gramme and Siemens (v. Hefner-Alteneck), the latter of which is described in very great detail. Hefner-Alteneck's new large dynamo with a disk-armature and many peripheral coils, is mentioned, and the general arrangement of its parts shown. The fifth chapter treats of alternate-current machines. Those of Lontin, Gramme, and Siemens are described fully, but the name of Wilde is not even mentioned! A new machine by Siemens and Halske, capable of giving either intermittent-direct or alternate currents, is figured in this chapter. This section of the book is closed by a disquisition on the theory of dynamos and their efficiency, the greater part being a compilation from the researches of Frölich, Hagenbach, and others.

Chapter VII. deals with the voltaic arc, and Chapter VIII. with electric arc lamps. A mass of details concerning the manufacture of carbon pencils and standards of photometry are included in the former. In the latter chapter most of the chief forms of lamp are given, including those of Crompton, Bürgin, Jaspar, and Serrin. Lamps adapted for use in series or derivation, including the so-called differential lamps, are considered in a separate chapter. Amongst the forms described are those of Gramme, Weston, Brush, Hefner-Alteneck (Siemens), Gülcher, and the Pilsen lamp. According to the author, the differential lamp of von Hefner-Alteneck was the first to make practical the introduction into one circuit of a number of lights. Jablochkoff's well-known candle, and its more recent imitations are described briefly, and then the author passes to the semi-incandescent lamps of the Werdermann type. Edison's incandescent lamp is next described, as it was in the year 1879. All Edison's more recent improvements appear to be unknown to the author, who passes by the Edison exhibit at the Paris Exposition with a compliment upon the good quality of its colour! The incandescent lamps of Lane-Fox and of Maxim are both described and figured, whilst that of Swan—antecedent to both of the latter, as well as to Edison's carbonised filament lamp—is described only, and not figured. Details concerning driving-power, distribution, cost, and fire-risks follow. Applications of dynamo-electric machines to metallurgy, electro-chemistry and telegraphy, make a chapter in themselves, as also does the subject of the electric transmission of power. A penultimate section deals with storage batteries, in which we are glad to observe that full justice is done to Planté, the inventor of the accumulator. A rather sketchy chapter on the mathematical theory of electric arc lighting closes the work.

On the whole, though this work contains useful information on many points, it is much to be regretted that it is not so complete as might have been hoped of a book published in 1882. In a science whose applications are developing so fast, this incompleteness detracts greatly from the value of the work.

OUR BOOK SHELF

The Watchmaker's Handbook. By Claudius Saunier. English Edition, Translated, Revised, and considerably augmented by Julian Tripplin and Edward Rigg, M.A.

THERE is no trade, we suppose, in which so many special tools are used as in watchmaking, nor any in which the character of a workman is so readily distinguished by them. The good workman has good tools—a perfect army of them—nearly all self-made, with which he is prepared to execute any piece of work, in a neat, clean, and efficient manner.

This little book describes watchmakers' tools, but deals with many operations inadmissible from a manufacturer's point of view. "Every watchmaker," says the preface, "will at once recognise that receipts are included which are of the nature of makeshifts, and that it would in many cases be better to replace a piece by a new one, rather than to repair it it in the manner indicated." But there is good reason for this:—"The immense number of badly-constructed watches that he (*the workman*) is called upon to put in going order for a trifling remuneration, compels him to replace the older methods of procedure by others, whenever by so doing time can be saved."

If watches were as big as steam-engines there are few people who would not be horrified at the kind of work put into some of them. But they go well? so they may (or may not), thanks to a strong mainspring, until they are pulled to pieces.

All watch repairers, or "jobbers," as they are technically called, and manufacturers too, ought, however, to be interested in this book. It contains a great deal of useful and instructive information, and it must be left to the consciences of such as to the suggestions herein contained, they would, or would not, adopt.

H. DENT GARDNER

Descriptiones Plantarum Novarum et minus Cognitarum. Fasc. viii. Auctore Dr. Regel. Pp. 150. (St. Petersburg, 1881.)

THE Director of the Imperial Botanic Garden describes a number of novelties cultivated under his own eye. One of the most striking is a new Crinum, (*C. Schmidti*) from Port Natal, which scarcely seems separable by description from *C. latifolium*, L. The bulk of the pages, however, is filled with an enumeration of the glumaceous plants at present known from Central Asia, in the study of which Aitchison's Afghan collections have not been overlooked. 195 species of Gramineæ are enumerated, of which 79 are Asiatic, or at any rate are not known from Europe; 75 species are middle European or Mediterranean; and 37 are common to middle Europe, middle Asia, and North America.

Turning over Dr. Regel's pages affords a ready illustration of the wide diffusion of the components of the British flora. Without pretending to absolute accuracy, we noted that of the 109 species of the British gramineous flora, 65 are recorded by Dr. Regel from Central Asia. We looked with some curiosity to see if any light was thrown on the origin of our cereals. But though rye (*Secale cereale*) appears to occur in a wild state in Turkestan, the forms of wheat met with by botanical collectors were all represented by cultivated specimens. Dr. Regel does not seem to have met with, from Central Asia, *Fingerhuthia Africana*, obtained by Aitchison in his second journey; although only known to botanists from South Africa, it was found to be one of the chief fodder-grasses of the Lower Kuram Valley.

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 ensure the appearance even of communications containing interesting and novel facts.*]

The Spectrum of the Light emitted by the Glow-worm

I AM not aware that any account has been published of the nature of the light emitted by the glow-worm, and therefore venture to send the results of some observations I made on the evenings of the 21st and 22nd of the month.

The light, as is well known, proceeds from the lower surface of the penultimate and ante-penultimate segments of the lower abdomen of the insect, and also from two round spots on the last segment—it is of a greenish colour, and when examined with the spectroscope gives a short continuous spectrum extending from about C to *b*, and therefore containing rays of all wavelengths between 656 and 518—the more refrangible portion is far the brightest, and the general appearance is of a broad band of green light reaching from about 587 to 518, with a faint continuous spectrum extending down into the red.

I may add that the observations were made with a small direct-vision spectroscope, with a photographic scale; and also that glow-worms are extremely rare in this district.

Reading, July 29 JOHN CONROY

Oscillations of the Sea-level

It seems to be very generally assumed that the surface of the ocean attains a uniform level, or nearly so, in all lands, and forms a sort of zero-point or datum line, from which the altitude of mountains is calculated, and to which barometrical readings are reduced.

This assumption, however, is called in question by several mathematicians, who allege that the sea-level is by no means that of a regular spheroid, as is generally supposed, but may vary many hundreds of feet in level even along the same parallel of latitude, quite independently of the temporary action of winds or of ocean currents.

According to the law of gravitation, all substances attract one another with a force proportional to their masses. A continent of land will therefore exert an attractive influence upon the sea, and cause it to rise upon its shores to a height which will vary according to the mass of land that causes the attraction, and may amount, it is said, to as much as 1000 metres above the level to which the sea attains in mid-ocean. This extraordinary result is deduced by Ph. Fischer from a discussion of pendulum-observations, and somewhat similar conclusions are arrived at by Listing and Heinrich Bruns.

Founding upon these observations, a German geologist, Dr. Penck, has proposed an explanation of the phenomena presented by the raised beaches, and other tokens of oscillation in the sea-level, which are so conspicuous during the glacial period. If the land has the property of thus drawing the sea towards it in proportion to its mass, it follows that anything which adds to that mass will increase the effect, and thus a great thickness of glacier-ice laid upon a continent, will draw the water towards it, and raise the sea-level in its immediate vicinity; and, according as the ice increases or diminishes, so will the level of the sea rise or fall in proportion. Moreover, the altitude of the sea-beach may vary considerably, it is said, along the border of one and the same continent, by reason of the varying thickness of the ice in different parts. In this way it is conceived an explanation is found for the fact that in Norway the old terraces and sea-beaches do not coincide in level, but vary in altitude at places not very far distant from one another. The action of the ice may in short be so localised that its attractive force will vary considerably along the same line of coast.

These views are certainly somewhat different from those that have hitherto prevailed in regard to the regularity of the sea-level. If there is such a very great difference in the height to which the surface of the sea may attain in different places, the barometer should give more indication of it than it seems to do. Nevertheless, it is to be desired that every means should be taken to ascertain the relative height of the sea in various places so chosen as to test the truth of the views I have mentioned. The apparent connection between glaciation and submergence is now attracting notice in various quarters. Dr. Penck maintains that shifts in the relative level of sea and land go hand in hand with oscillations in the glaciation. In 1865 I called attention to this connection, and suggested what seemed to me to be a possible explanation of it (see *Journ.* of the Geol. Soc. vol. xxi. p. 178).

Penck's views, it will be seen, are somewhat different from those of Adhemar and Croll, to which he points out severl objections. His memoir is entitled "Schwankungen des Meeresspiegels," and appears in the *Jahrbuch der Geograph. Gesellschaft zu München*, Bd. vii. 1882.

T. F. JAMIESON

Ellon, Aberdeenshire, July 31

Voice in Lizards

THE above heading in NATURE, vol. xxvi. p. 29, rather surprises me, as though voices in lizards were a recent discovery. The loud and plaintive "gui—gui—gui" made by the large land lizard of that name, has been well known to me for the last seventeen years, and is of course well known to every Assamese. The call is always heard in twilight, in the depths of the forest, and when once heard is not mistakable for that of any other animal. It is plain, monotonous, loud, and repeated with two second intervals some eight or ten times, when there is a pause of about two minutes, and it is repeated. For those who do not understand the Hunterian system of spelling I would write it gooee—gooee, the oo most prolonged. The gui is about 3 to 3½ feet long—from snout to tip of tail—which latter exceeds the body and head. Colour grey-green, with clear yellow scales here and there—at times grouped—and that gives a mottled appearance. The tail has a double row of sharp scale-spines along its crest, and if suddenly lashed can cut the skin of any bare-legged bystanders.

It lives in holes under, or in, tree stems, often as high up as 30 or 40 feet. The flesh is eaten and prized, the skin used as the membrane in some kinds of guitars. There seem several kinds, one of 3 or 3½ feet, another larger—both land lizards—a still larger kind frequents the rivers, up to 6 feet or more in length. It hisses like the larger snakes, and the peculiar call that gives it the name "gui," can be heard in still forest I should say a mile; one that repeats this monotonous call every evening is loud enough to be an annoyance at times, though it is over 500 yards off.

S. E. PEAL

Sibsagar, Assam

Halo

ABOUT 2 p.m. to-day a remarkable halo was visible here. The sky was partially covered with light cirrus clouds, and some small fleecy drift was rapidly moving from the north-west at a low altitude. I saw a bright bow at about 45° from the sun nearly due north, extending over a clear portion of the sky; this gradually extended till it formed a circle with the sun in the most southern point of its circumference. The width of the bow was rather greater than the diameter of the sun, the whole circle being, as near as I could judge, 45° or 50° in diameter. It was brilliantly white, brighter than the white of any clouds in the neighbourhood; it lasted perhaps fifteen minutes, and gradually broke up and faded. I could see no other interesting halo nor any appearance of parhelia.

W. A. SANFORD

Tynehead, Somerset, July 25

THE ELECTRIC PROPERTIES OF FLAMES

THE electric properties of flames have often invited the investigation of physicists, but the obscurity and contradictory nature of some of the phenomena have been such that in spite of a large number of researches no complete account of these properties has hitherto been given. Most of these researches are enumerated in a paper contributed by Prof. Holtz to Carl's Repertorium last year; but though Holtz has himself added to our knowledge of the electrical property of flames by his researches on the behaviour of flames when employed as electrodes, he left much yet to be investigated in this department.

The latest contribution to our knowledge of the subject appears in the current volume of Wiedemann's *Annalen der Physik und Chemie*, from the joint pens of Herren Dr. Julius Elster and Hans Geitel. As the results of their investigations go far to clear up some of the points which have hitherto been obscure or contradictory, some account of these investigations will probably not be unacceptable to the readers of NATURE.

The chief theories that have been advanced from time to time in explanation of the electrical properties of flames may be reckoned as three in number.

1. Pouillet in 1827 propounded the suggestion that the electricity of flame is due to the process of combustion as such, and therefore presumably analogous to the electrification observed by Volta to result when a burning coal or pastille is placed upon the cap of an electroscope.

2. Matteucci, in 1854, explained the phenomena by supposing that the flame acted upon the two metal electrodes (employed to test its electrification) as an electrolyte; in fact, that it acted as the acid between the two metallic plates of a voltaic cell; a view which practically agrees with that earlier propounded by Hankel.

3. Buff suggested that the explanation was to be sought in a thermo-electric difference between the two electrodes.

Sir William (Mr. Justice) Grove had shown moreover that when a platinum wire is bent so that one end of it stands in the tip of a flame, while the other is immersed in the flame near its base, a current of electricity is set up in the wire. This phenomenon might at first sight be thought to agree with an observation of Hankel, that a flame is "polarised" longitudinally; that is to say, Hankel found there is a difference of potential between the tip and the base of a flame, and this difference of potential is, of course, equivalent to an electromotive force acting along the flame. The want of concord amongst different observers, not only as to the cause of the electrical properties of flame, but even as to what those properties were, is most singular. Probably a great part of it arises from the omission to notice one very important point, namely, the part played in these electrical phenomena by the sheet or mantle of hot air surrounding the flame externally. Almost all these observers have used as their instrument of investigation either a coarse gold-leaf electroscope, or else a galvanometer. The want of sensitiveness and accuracy in the former instrument when applied to small differences of potential, makes the former unsuitable; whilst the high resistance offered by the flame itself to the passage of electricity renders the use of the latter inadvisable.

In beginning their investigation, therefore, Messrs. Elster and Geitel bethought themselves of Sir W. Thomson's quadrant electrometer as admirably suited for delicate and quantitative experiments of the kind in hand,

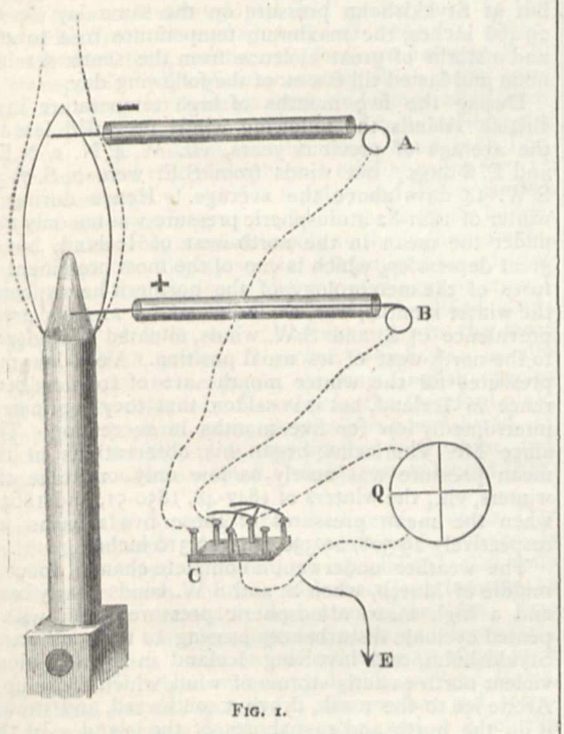

Fig. 1.

and with this instrument they set themselves to investigate the correctness or otherwise of that which previous observers had announced.

Their apparatus was set up as follows:—To keep the needle of the electrometer at a constant charge, it was connected with one pole of a Zamboni's dry pile consisting of 2400 pairs of disks, the other pole being joined to "earth." The electrometer thus arranged was very sensitive, a difference of potential of a single Daniell's cell producing a deflexion of 112 degrees upon the scale of the instrument. One pair of quadrants was as usual put to "earth," and a suitable commutator (C in Fig. 1) was interposed between the electrometer and the flame-apparatus. Experiments were made on the flames of Bunsen burners and of spirit-lamps, both well insulated. A small Bunsen burner specially adapted for this purpose was constructed out of a piece of glass tubing 4 millimetres wide. Fig. 1 shows the manner of exploring the flame. One electrode, A, consisting of a platinum wire inclosed in a glass tube from which the end protruded, was inserted in the tip of the flame. Another electrode, B, of similar kind was fixed so as to pass into the base of the flame. Two points were revealed by the first experiments made:

1. That when set in this fashion the tip was usually electrically negative, as compared with the base of the flame, as Hankel had said, but that sometimes the reverse appeared to be the case.

2. That when the lower electrode was kept fixed, and the upper one was moved to different heights, the potential anywhere within the interior of the flame was the same, being 1·04 times that of one Daniell's cell when the electrodes were 1 millimetre apart, and the same when they were 20 millimetres apart.

If, as Hankel expresses it, a flame were "polarised" longitudinally, cross-sections taken horizontally across the flame should be equipotential surfaces. This is true if the two platinum electrodes A and B are both right within the flame. Whether at the same level or not, when both are completely within the flame they are practically at the same potential—neither of them positive or negative relatively to the other. But if one of the electrodes is displaced to one side, a difference of potential is immediately observed, and this difference is very great if (as in Fig. 1) one electrode passes no further than into the external mantle of hot air.

To examine more particularly the part played by this external mantle was the next point. It will be observed from Fig. 1, that the two electrodes were so chosen that the protruding portion of the platinum wire was equal in length to the width of the flame from side to side. As remarked above, when both were completely immersed side by side in the flame, they showed no electrical difference; but when either of them was moved into the

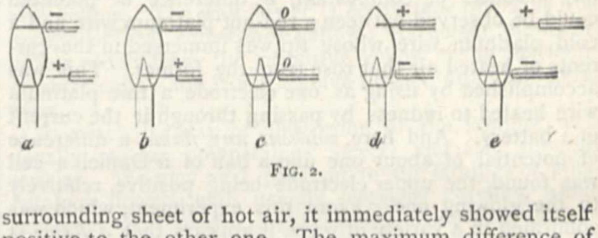

Fig. 2.

surrounding sheet of hot air, it immediately showed itself positive to the other one. The maximum difference of potential was observed when the electrodes occupied the relative positions shown in Fig. 2, *a*, where the electrode B is about half a millimetre outside the flame. When B was pushed in to the position shown in Fig. 2, *b*, the potential fell to less than half what it was before. When pushed completely in, as in Fig. 2, *c*, the two electrodes were so nearly alike, that the difference of potential between them was less than the hundredth part of that of a single Daniell's cell. The upper electrode, A, was now gradually removed. When it reached the position shown in Fig. 2, *d*, it was decidedly positive relatively to B, and when it was placed as in Fig. 2, *e*, its negative potential was almost as great as its positive potential had been in the first position. The potential of A relatively to B is given in the following table for the five positions:—

a.	E.M.F. was	−1·29	of 1 Daniell.
b.	,,	−0·46	,,
c.	,,	−0·009	,,
d.	,,	+0·65	,,
e.	,,	+1·09	,,

To put the matter into words: As long as either of the electrodes is outside the flame and the other inside, the outside one is positive and the inside one negative. The film of hot air outside the flame is always electrically positive, and the flame inside relatively negative.

The same result was obtained by these experimenters with flames of spirit-lamps and with ordinary gas and candle flames. More curious still, when air was made to burn with a flame in an atmosphere of coal-gas the very same phenomena are observed, the hot coal-gas being positive relatively to the air-flame. All these flames showed a potential varying from about $1\frac{1}{3}$ to $1\frac{2}{3}$ times that of a Daniell's cell. The flame of bisulphide of carbon gave a lower result, and so did a magnesium flame. The introduction of any salt of a metal—such as chloride of potassium—into the flame lowered the potential.

Moreover when wires of other metals were employed the differences of potential were not the same as before. Whilst the lower electrode of platinum remained, the upper electrode was replaced by a copper wire when the potential rose to 2 Daniell's cells. With aluminium it was equal to 3, and with magnesium to 3·2 Daniell's. Using a lump of clean sodium as electrode the potential even rose to five times the Daniell cell.

Using as a fluid electrode a drop of water at the end of a capillary tube similar results were obtained, though the differences of potential were smaller.

These experiments corroborate the suggestion that the flame acts like the acids between the poles of a battery-cell, or that the action is an "electrolytic" one.

Messrs. Elster and Geitel succeeded in joining electrically together the flames of twenty-five spirit-lamps, by the device of causing a curved piece of platinum wire to lead from the base of one flame to the tip of the next, and another piece of wire from the base of this to the tip of the succeeding one, and so on. This "flame-battery" of course had a potential twenty-five times as great as that of one flame. But it would not yield much current, owing to the enormous internal resistance of the flames themselves.

Another most important series of researches was then undertaken to investigate whether, without any *flame* or any products of combustion, a difference of potential could be observed between a red-hot platinum wire and a cold platinum wire whose tip was immersed in the currents of heated air that rose from the former. This was accomplished by using as one electrode a thin platinum wire heated to redness by passing through it the current of a battery. And here, *without any flame*, a difference of potential of about one and a half of a Daniell's cell was found, the upper electrode being positive, relatively to the glowing one. From this experiment, which was confirmed in a variety of ways, it appears that *a flame is not in itself a source of electrification at all.* Messrs. Elster and Geitel therefore regard the electrification as a thermo-electric phenomenon; though they use this term in a slightly different sense from that in which it is used in the text-books.

They conclude, therefore, that the production of electrification by flames is (1) independent of the size of the flame; (2) dependent on the nature and state of surface of the electrodes; (3) dependent on the nature of the gases that are burning in the flame; and (4) dependent on the state of ignition of the electrodes.

They therefore regard Pouillet's theory as being wrong, whilst the theories of Matteucci (and Hankel) and of Buff are both, so far as they go, correct. If this so-called thermo-electric origin of the electrification be the true one it is a very important fact indeed; and, as these able experimenters say, will probably explain the back-electromotive force which is observed in the voltaic arc. This is not the least interesting point in this very interesting research.

S. P. T.

THE METEOROLOGY OF ICELAND DURING THE WINTER AND SPRING OF 1881-82

THE observations made last winter by Mr. Thorlacius, observer for the Scottish Meteorological Society at Stykkisholm, Iceland, have been received by the Society, and they are of the greatest interest in connection with the unexampled mild weather which prevailed in this country for the five months ending March. The mean pressures, for these five months, at 32° and sea level, were respectively 29·201, 29·140, 29·295, 29·471, and 29·258 inches, the mean of these months being thus only 29·273 inches. In London the mean of the same months was 30·123 inches, or 0·850 inch higher than that of Stykkisholm. The means for these two places for the twenty-four years ending with 1880 are for London 29·948 inches, and Stykkisholm 29·552 inches, the difference being 0·396 inch, or less than half the difference during the winter of 1881-82. The greatest difference occured in January, the mean pressure for which month in London was 30·365 inches, and at Stykkisholm 29·295 inches. Pressure in the north-west of Iceland was thus 1·070 inch less than in London.

On January 14 the pressure at Greenwich was 30.572 inches, and the maximum temperature 42°·4, but at Stykkisholm on the same day temperature rose to 46°·5, with a storm of wind from the south, and pressure was as low as 28·290 inches, being 2·282 inches lower than at Greenwich. At Greenwich pressure rose at 10 a.m. of the 18th to 30·973 inches, the maximum temperature being 34°·2; but at Stykkisholm pressure on the same day was only 29·466 inches, the maximum temperature rose to 46°·5, and a storm of great violence from the south set in at noon and lasted till 6 a.m. of the following day.

During the five months of high temperature in the British Islands the following winds prevailed less than the average of previous years, viz. W. 4, N. 1, N.E. 6, and E. 8 days; but winds from S.E. were 2, S. 7, and S.W. 14 days above the average. Hence during the winter of 1881-82 atmospheric pressure was not only much under the mean in the north-west of Iceland, but the great depression, which is one of the most prominent features of the meteorology of the northern hemisphere in the winter months, was, as indicated by 21 days' greater prevalence of S. and S.W. winds, situated considerably to the north-west of its usual position. Very low mean pressures for the winter months are of frequent occurrence in Iceland, but it is seldom that they continue uninterruptedly low for five months in succession. Thus, since Mr. Thorlacius began his observations in 1845, mean pressure was nearly as low only on three other winters, viz., the winters of 1847-48, 1850-51, and 1862-63, when the mean pressures of these five months were respectively 29·308, 29·330, and 29·310 inches.

The weather underwent a complete change about the middle of March, when S. and S.W. winds nearly ceased and a high mean atmospheric pressure ruled, with repeated cyclonic disturbances passing to the southward of Stykkisholm, and involving Iceland in a succession of violent north-easterly storms of wind, which broke up the Arctic ice to the north, drove it southward, and stranded it on the north and east shores of the island. In these circumstances the weather became unusually inclement and unseasonable, and Mr. Thorlacius reports that no equally severe and disastrous spring has occurred there within the memory of any one living. About Easter all the food for horses, sheep, and cattle had been used up, and these animals died in great numbers. In his parish alone, 62 horses, 1700 sheep and swine, and 7 cows perished, causing a direct loss of 1220*l.*, and the population has been brought face to face with a serious famine.

Though the Government is taking every measure in its power to mitigate the calamity, the prospect is most gloomy. Besides this, all, or nearly all, of the lambs have died, and owing to the great cold and want of rain, grass has scarcely yet begun to grow, the sea-ice still (July 1) surrounds the entire north and east of the island in immense masses, and no ship can get through it to any of the harbours of these coasts. On the north coast the ice drove ashore about fifty large whales, of which the smallest is said to be forty-five feet in length, which proved an unexpected relief to the poor peasantry, and even to the proprietors of the coast districts.

THE COLOURS OF FLOWERS, AS ILLUSTRATED BY THE BRITISH FLORA[1]

II.—*Further Examples of the General Law*

FLOWERS in which the carpels have arranged themselves in a circle around a common axis, like the *Geraniaceæ* and *Malvaceæ*, thereby show themselves to be more highly modified than flowers in which all the carpels are quite separate and scattered, like the simpler *Rosaceæ* and *Ranunculaceæ*. Still more do families such as the *Caryophyllaceæ*, in which all the five primitive carpels have completely coalesced into a single one-celled ovary. Accordingly, it is not remarkable that the pinks should never be yellow. On the other hand, this family has no very specialised members, like larkspur and monkshood, and therefore, it very rarely produces bluish or purplish flowers. Pinks, in fact, do not display so wide a range in either direction as *Ranunculaceæ*. They begin as high up as white, and hardly get any higher than red or carnation. Of their two sub-families, the *Alsineæ* have the sepals free, the blossoms widely expanded, and no special adaptations for insect fertilisation. They include all the small undeveloped field species, such as the chickweeds (*Stellaria media*, *Arenaria trinervis*, *Cerastium vulgatum*, &c.), stitchworts (*Stellaria holostea*, &c.), and cornspurries (*Spergula arvensis*), which have open flowers of a very primitive character; and almost all of them are white (Fig. 12). These are fertilised by miscellaneous small flies. The *Sileneæ*, on the other hand, including the campions and true pinks, have a tubular calyx, formed by the coalescence of the five sepals; and the expanded petals are raised on long claws, which makes their honey, inclosed in the tube, accessible only to the higher insects. Most of them also display special adaptations for a better class of insect fertilisation in the way of fringes or crowns on the petals. These more profoundly modified kinds are generally pink or red. For example, in the most advanced British genus, *Dianthus*, which has usually vandyked edges to the petals, our four English species are all brightly coloured; *D. armeria*, the Deptford pink, being red with dark spots, *D. prolifer* purplish red, *D. deltoides*, the maiden pink, rosy spotted with white, and *D. cæsius*, the Cheddar pink, bright rose-coloured (Fig. 13).

It is much the same with the allied genus *Lychnis*. Our own beautiful purple English corn-cockle (*L. githago*), is a highly developed campion, so specialised that only butterflies can reach its honey with their long tongues, as the nectaries are situated at the bottom of the tube. Two other species of campion, however, show us interestingly the way in which variations of colour may occur in a retrograde direction even among highly evolved forms. One of them, the day lychnis (*L. diurna*), has red, scentless flowers, opening in the morning, and it is chiefly fertilised by diurnal butterflies. But its descendant, the night lychnis (*L. vespertina*), has taken to fertilisation by means of moths; and as moths can only see white flowers it has become white, and has acquired a faint perfume as an extra attraction (Fig. 14). Still, the change has not yet become fully organised in the species, for one may often find a night lychnis at the present time which is only pale pink, instead of being pure white.

The *Cruciferæ* are a family which display a good deal of variety in colouration. The most primitive and simple forms have yellow flowers, as in the case of the cabbage genus (*Brassica*) including charlock, mustard, and turnip; the rockets (*Barbarea* and *Sisymbrium*); and the gold-of-pleasure (*Camelina sativa*). Most of these are dry-field weeds, and they have open little-developed blossoms. In the genus *Nasturtium* or watercress we have four species, three of which are yellow, while one is white. In treacle-mustard (*Erysimum*), the yellow is very pale, and the petals often become almost white. Just above these earliest forms come the common small white crucifers like *Cardamine hirsuta*, *Cochlearia officinalis*, and *Capsella bursa-pastoris*. Many of these are little if at all superior in organisation to the yellow species, and some of them (as we shall see hereafter) are evidently degenerate weeds of cultivation. But such flowers as *Alyssum maritimum*, with its sweet scent, its abundant honey, its reduced number of seeds, and its conspicuous, spreading milk-white petals, are certainly more developed than small yellow species like *Alyssum calycinum*. Even more remarkably is this the case in the genus *Iberis* or candytuft, which has become slightly irregular, by the two adjoining exterior petals growing larger than the interior ones. Accordingly, they are usually white, like our British species, *I. amara;* while some of the larger exotic species are a pretty pink in hue. The genus *Cardamine* supplies us with like instances. Here the smaller species have white flowers, and so has the large *C. amara*. But in *C. pratensis*, the cuckoo-flower, they are usually tinged with a pinkish purple, which often fades deep mauve; and in some showy exotic species the flowers are a rich pink. So with *Arabis:* our small English kinds are white; *A. petræa*, with larger petals, is often slightly purplish, and some handsome exotics are a vivid purple. In *Hesperis* we get a further degree of modification in that the petals are raised on rather long claws; and the flowers (represented in England by *H. matronalis*, the dame's-violet) are a fine purple, and possess a powerful perfume. Closely allied is the Virginia stock of our gardens (*Malcolmia*), which varies from pale pink to mauve. But the highest of all our crucifers are contained in the genera *Matthiola* and *Cheiranthus*, which have large spreading petals on long erect claws, besides often being sweet scented. The common stock (*M. incana*) is purple, reddish, or even violet; our other British species, *M. sinuata*, is pale lilac; and no member of the genus is ever yellow. The wall-flower (*Cheiranthus cheiri*) is rich orange or red, sometimes yellow: its colour, however, differs widely from the primitive yellow of the charlocks or buttercups; and it will receive further attention hereafter.

So much by way of illustration of the families with usually regular polypetalous flowers and free superior ovaries. We may next pass on to the families of polypetalous flowers with usually irregular corollas, which represent of course a higher stage of development in adaptation to insect visits. Of these, two good illustrative cases are included in the British flora. They are the *Polygalaceæ* and the *Violaceæ*.

Polygala vulgaris, or milkwort, our only British representative of the first named family, is an extremely irregular flower, very minutely and remarkably modified for special insect fertilisation. It is usually a bright blue in colour, but it often reverts to pink, and not infrequently even to white.

The *Violaceæ* or violets are a whole family of bilateral flowers, highly adapted to fertilisation by insects; and as a rule they are a deep blue in colour. This is the case with four of our British species, *Viola odorata*, *V. canina*, *V. hirta*, and *V. palustris*. Here too, however, white varieties easily arise by reversion; while one member of the group, the common pansy, *V. tricolor*, is perhaps the most variable flower in all nature. This case, again, will receive further attention when we come to consider the subject of variegation and of reversion or retrogression.

When we pass on to the *Corollifloræ*, in which the originally separate petals have coalesced into a single united tube, we meet with much more striking results. Here, where the very shape at once betokens high modification, yellow is a comparatively rare colour (especially as a ground-tone, though it often comes out in spots or patches), while purple and blue, so rare elsewhere, become almost the rule.

The family of *Campanulaceæ* forms an excellent ex-

[1] Continued from p. 304.

ample. Its flowers are usually blue or white, and the greater number of them, like the harebell (*Campanula rotundifolia*) and the Canterbury bell (*C. media*), are deep blue (Fig. 15). We have nine British species of the genus, varying from pale sky-blue to ultramarine and purplish cobalt, with an occasional relapse to white. Rampion and sheep's bit, also blue, are clustered heads of similar blossoms. The little blue lobelia of our borders, which is bilateral as well as tubular, belongs to a closely-related tribe. One of our British species, *Lobelia Dortmanna*,

FIG. 12.—Lesser Stitchwort, white: type of simple open Alsineæ.

is sky-blue; the other, *L. urens*, is a dingy purple. Not far from them are the *Dipsaceæ*, including the lilac scabious, the blue devil's bit, and the mauve teasel. Amongst all these very highly-evolved groups blue distinctly forms the prevalent colour.

In the great family of the *Ericaceæ*, or heaths, which is highly adapted to insect fertilisation, more particularly by bees, purple and rose are the prevailing tints, so much so that, as we all have noticed a hundred times over, they often colour whole tracts of hillside together. The bell-shaped blossoms mark at once the position of the heaths

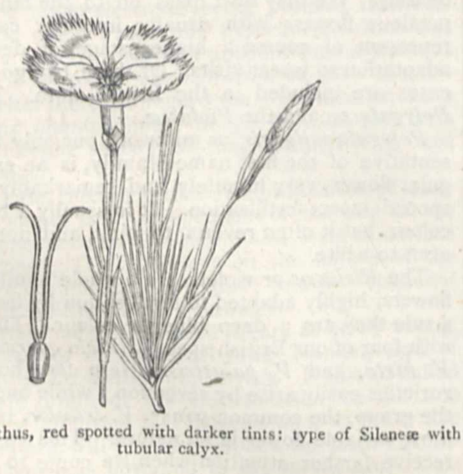

FIG. 13.—Dianthus, red spotted with darker tints: type of Sileneæ with tubular calyx.

with reference to insects; and the order, according to Mr. Bentham, supplies us with more ornamental plants than any other in the whole world. Among our British species, in the less developed forms, like *Vaccinium*, *Arbutus*, and *Andromeda*, the flowers are usually white, flesh-coloured, pinkish, or reddish. The highly developed *Ericæ*, on the other hand, are mostly purple or deep red. *E. vulgaris* has the calyx as well as the corolla coloured with a mauve variety of pink. *Menziesia cærulea* is a deep purplish blue. *Monotropa* alone, a very degraded leafless saprophyte form, has greenish yellow or pale brown free petals.

The *Boraginaceæ* are another very advanced family of *Corolliflora*, and they are blue almost without exception. They have usually highly-modified flowers, with a tube below and spreading lobes above; in addition to which most of the species possess remarkable and strongly-

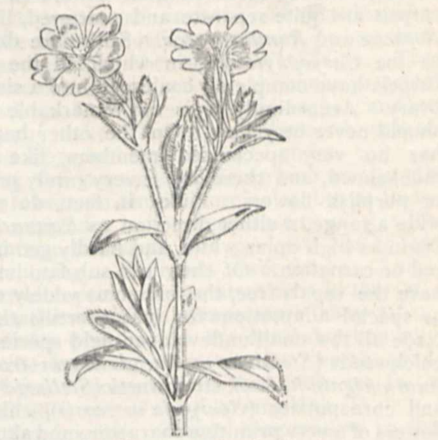

FIG. 14.—Night Lychnis, white: adapted to fertilisation by moths.

developed appendages to the corolla, in the way of teeth, crowns, hairs, scales, parapets or valves. Of the common British species alone, the forget-me-nots (*Myosotis*) are clear sky-blue with a yellow eye; the viper's bugloss (*Echium vulgare*) is at first reddish-purple, and afterwards a deep blue; the lungwort (*Pulmonaria officinalis*) is also dark blue; and so are the two alkanets (*Anchusa*), the

FIG. 15.—Harebell, deep blue: type of Corollifloral blossoms.

true bugloss (*Lycopsis*), the madwort (*Asperugo*), and the familiar borage (*Borago officinalis*); though all of them by reversion occasionally produce purple or white flowers. Hounds-tongue (*Cynoglossum officinale*) is purple-red, and most of the other species vary between purple and blue; indeed, throughout the family most flowers are red at first and blue as they mature. Of these, borage at least is habitually fertilised by bees, and the same is partially true of many of the other species. All of them are adapted to a high class of insect visitors.

Other families of regular *Corolliflorœ* must be glanced at more briefly. Among the *Gentianaceœ*, the less ad-

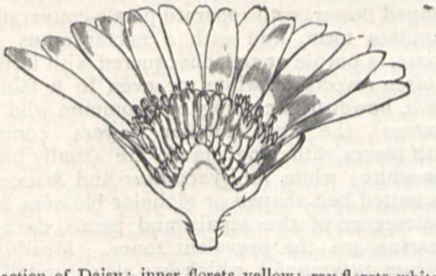

FIG. 16.—Section of Daisy; inner florets yellow; ray florets white, tipped with pink.

vanced types, like the simple *Chlora perfoliata* and *Limnanthemum nymphœoides*, are yellow, perhaps by reversion; but *Menyanthes trifoliata*, a slightly more developed ally of *Limnanthemum*, has white blossoms, tinged outside with red; *Erythrœa centaurium*, with a divided calyx and the cells of the ovary imperfectly

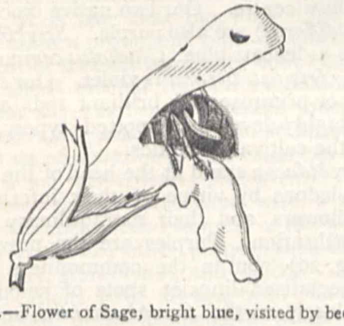

FIG. 17.—Flower of Sage, bright blue, visited by bee.

united, is pink; and the true gentians, *Gentiana verna*, *G. campestris*, *G. nivalis*, &c., with a tubular calyx, long throat, and sometimes fringed hairs to the tube, are bright blue. In *Apocynaceœ*, we have the highly developed periwinkles, *Vinca major* and *V. minor*, normally blue, though pink and white varieties or species are also culti-

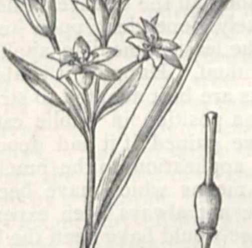

FIG. 18.—*Gagea lutea*, yellow: simplest type of lily.

vated. In *Plumbagineœ* we have the bluish purple sea-lavender (*Statice Limonium*) and the pink thrift (*Armeria vulgaris*). Other families with special peculiarities will receive notice later on.

It is necessary, however, here briefly to refer to the great family of *Compositœ*, some of whose peculiarities can only properly be considered when we come to inquire into the phenomena of relapse and retrogression. Nevertheless, even at the present stage they afford some excellent evidence. In certain ways they may be regarded as the very highest race of flowering plants. Not only are their petals united into a tubular corolla, but their blossoms are compounded into large groups of a very attractive sort. Each flower-head consists of a number of small florets, crowded so as to resemble a single

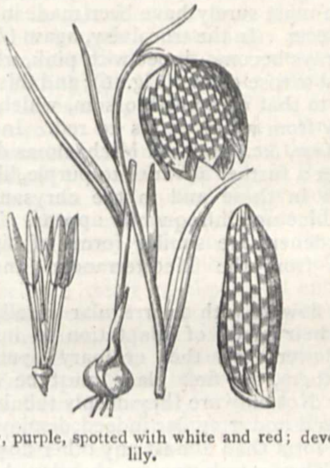

FIG. 19.—Fritillary, purple, spotted with white and red; developed type of lily.

blossom. So far as our present purpose is concerned, they fall naturally into three groups—Jussieu's old-fashioned sub-orders of *Ligulatœ*, *Cynaroideœ*, and *Corymbiferœ*, which are quite sufficient for all ordinary objects of botanical study.

We can only examine the last-named tribe at present, whose central florets, as a rule, are bright golden; a fact which shows pretty certainly that they are descended from a common ancestor who was also yellow. Moreover, these yellow florets are bell shaped. But the outer florets are generally sterile; and instead of being bell-shaped,

FIG. 20.—Spotted Orchid, purple with white patches: type of highly developed bilateral monocotyledons.

they form a long ray; while their corolla is at the same time much larger than that of the central blossoms. In short, they are sterilised members of the compound flower-head, specially set apart for the work of display; and thus they stand to the entire flower-head in the same relation as petals do to the simple original flower. Just as the petal is a specialised and sterilised stamen told off to do duty as an allurer of insects for the benefit of the whole flower, so the ray-floret is a specialised and sterilised blossom told off to do the self-same duty for the benefit of the composite flower-head.

Now, the earliest ray-florets would naturally be bright yellow, like the tubular blossoms of the central disk from which they sprang. And to this day the ray-florets of the simplest corymbiferous types, such as the corn-marigold (*Chrysanthemum segetum*), the sun-flower (*Helianthus annuus*), and the ragwort (*Senecio jacobæa*), are yellow, like the central flowers. In the camomile, however, the ox-eye daisy, and the may-weed (*Anthemis cotula*, *Chrysanthemum leucanthemum*, &c.), the rays have become white; and this, I think, fairly establishes the fact that white is a higher development of colour than yellow; for the change must surely have been made in order to attract special insects. In the true daisy, again (*Bellis perennis*), the white rays become tipped with pink, which sometimes rises almost to rose-colour (Fig. 16); and this stage is exactly analogous to that of apple-blossom, which similarly halts on the way from white petals to red. In our own asters (*A. tripolium*, &c.) and the Michaelmas daisies of America, we get a further advance to purple, lilac, and mauve, while both in these and in the chrysanthemums, true shades of blue not infrequently appear. The *Cinerarias* of our gardeners are similar forms of highly-developed groundsels from the Mediterranean and the Canary Islands.

Tubular flowers with an irregular corolla are obviously higher in their mode of adaptation to insect visits than tubular flowers of the ordinary symmetrical type. Amongst them, the first place must be assigned to the *Labiates*. Not only are they deeply tubular, but they are very bilateral and irregular indeed, displaying more modification of form than almost any other flowers except the orchids. They mostly secrete abundant honey, and often possess highly aromatic perfumes. Almost all of them are purple or blue. Among the best known English species are thyme, mint, marjoram, sage (Fig. 17), and basil, which it need hardly be said are great favourites with bees. Ground-ivy (*Nepeta glechoma*) is bright blue; catmint (*Nepeta cataria*), pale blue; *Prunella*, violet-purple; and common bugle (*Ajuga reptans*), blue or flesh colour. Many of the others are purple or purplish. It must be added that in this family the flowers are very liable to vary within the limit of the same species; and red, white, or purple specimens are not uncommon in many of the normally blue kinds.

The *Scrophularineæ*, and other allied irregular tubular families are mostly spotted, and so belong to a later stage of our inquiry; but even amongst this group, the *Veronica* genus has almost always pure blue flowers; foxglove (*Digitalis purpurea*) is purple; and many of the Broomrapes (*Orobanchaceæ*) are more or less bluish. Blue and lilac also appear abundantly in spots or stripes in many species of *Linaria*, in *Euphrasia*, and in other genera.

We have given so much consideration to the Dicotyledons that the relatively simple and homogeneous Monocotyledons need not detain us long. Their coloration is as a whole both less complicated and less instructive.

The *Alismaceæ* answer very closely to the *Ranunculaceæ*, as being in all probability the earliest surviving type of entomophilous Monocotyledons. Their arrangement is of course trinary, but they have similarly separate carpels, often numerous, surrounded by one, two, three, or many rows of stamens, and then by one row of three petals and one row of three sepals. All our English species, however, are white or rosy, instead of yellow. As they are marsh plants, they seem to have reached or passed the stage of *Ranunculus aquatilis*. One species, *Alisma plantago*, the water-plantain, however, still retains a yellow claw to the petals, though the limb is white or pale pink. So also does *Damasonium stellatum*. These two interesting plants present a remarkable analogy to the water-crowfoot.

Among monocotyledonous families with a united ovary, the *Liliaceæ* are probably the most primitive. Their simplest type in England is *Gagea lutea* (Fig. 18), a yellow lily looking extremely like a bunch of *Ranunculus Ficaria*. In *Lloydia serotina*, a closely allied but more developed form, the petals are white, with a yellow base, and three reddish lines. The wild tulip is likewise yellow. *Allium ursinum*, a somewhat higher type, is pure white. The fritillary (*Fritillaria Meleagris*, Fig. 19), a large, handsome, bell-shaped flower, with separate petals converging into a campanulate form, and with a nectariferous cavity at their base, is purple or red, chequered with lurid marks; but it often reverts to white, or even to a faint yellow. In *Scilla*, however, including our common wild hyacinth (*S. nutans*), the deep tubular flowers, composed of perianth pieces with long claws, are usually blue, rarely pink or white; while in *Hyacinthus* and *Muscari*, which have a united bell-shaped or globular blossom, formed by the coalescence of the sepals and petals, dark-blue and ultramarine are the prevalent tones. Meadow saffron (*Colchicum autumnale*), which has also a united tube and very deep underground ovary, is a fine reddish purple: its stamens secrete honey.

The *Irideæ* and *Amaryllideæ* are more advanced than the lilies, in that they possess inferior ovaries—in other words, their perianth tube has coalesced with the walls of the inclosed carpels. In many cases, especially in the more highly-developed species, their flowers are red, blue, or purple. *Trichonema Bulbocodium* is purplish-blue, with a yellow centre. Our two native crocuses (*C. vernus* and *C. nudiflorus*) are also purple. *Sisyrinchium Bermudianum* is a delicate blue. *Gladiolus communis* is brilliant crimson. *Iris fœtidissima* is violet. Our own Amaryllids are white or primrose, but brilliant reds and purples, as well as highly-developed spotted types, are common amongst the cultivated exotics.

The *Orchidaceæ* stand at the head of the entomophilous Monocotyledons by virtue of their inferior ovary, their irregular flowers, and their extraordinary adaptations to insect fertilisation. Purples are the prevailing groundtones (Fig. 20); but in the commonness of variegation and of specialised lines or spots of colour, the Orchids answer closely to the *Scrophularineæ* among Dicotyledons, and may therefore best be considered in a succeeding section.

GRANT ALLEN

(*To be continued.*)

ASTRONOMICAL OBSERVATORIES [1]

AMONG the contributions of public and private munificence to the advance of knowledge, none are more worthy of praise than those which have been devoted to astronomy. Among all the sciences, this is the one which is most completely dependent upon such contributions, because it has the least immediate application to the welfare of the individual. Happily, it is also the science of which the results are best adapted to strike the mind, and it has thus kept a position in public estimation which it could hardly have gained if it had depended for success solely upon its application to the practical problems of life. That the means which have been devoted to its prosecution have not always been expended in a manner which we now see would have been the best, is to be expected from the very nature of the case. Indeed, a large portion of the labour spent in any kind of scientific research is, in a certain sense, wasted, because the very knowledge which shows us how we might have done better has been gained through a long series of fruitless trials. But it is due both to ourselves and the patrons of astronomy that as soon as any knowledge bearing upon the question of the past application of money to the advance of science is obtained, use should be made of it to point out the mistakes of the past and the lessons for the future. It is now patent to all who have made a wide study of the subject that large amounts have been either wasted or applied in ways not the most effective in the erection and

[1] From the *North American Review*.

outfit of astronomical observatories. Since Tycho Brahé built his great establishment at Uraniburg, astronomical research has been associated in the public mind with lofty observatories and great telescopes. Whenever a monarch has desired to associate his name with science, he has designed an observatory proportional to the magnitude of his ambition, fitted it out with instruments on a corresponding scale, and then rested in serene satisfaction. If we measure greatness by cubic yards, then Peter the Great and "Le Grand Monarque" were the founders of two of the greatest observatories ever built. That of St. Petersburg was completed in 1725, the year of Peter's death, and was an edifice of two hundred and twenty-five feet front, with central towers one hundred and forty feet high. It had three tiers of galleries on the outside for observation, and was supplied with nearly every instrument known to the astronomers of the time, without reference to the practicability of finding observers to use them. It was nearly destroyed by fire in 1747, but was partially rebuilt, and now forms part of the building occupied by the Imperial Academy of Sciences. The Paris Observatory, built half a century earlier, still stands, its massive walls and arched ceilings reminding one rather of a fortress than of an astronomical institution.

Notwithstanding the magnificence of these structures, they have had little essential connection with the progress of astronomy. It is true that the work done at both establishments takes a prominent place in the history of science, but most of it could have been done equally well under wooden sheds erected for the protection of the instruments from the weather. In recent times, the St. Petersburg Observatory has been found so unsuitable for its purpose that no observation of real value can be made, and its existence has been nearly forgotten. The great building at Paris, though associated with a series of astronomical researches second to none in the world, has really served scarcely any other purpose than those of a physical laboratory, store-house, and offices. The more important observations have always been made in the surrounding garden, or in inexpensive wings or other structures erected for the purpose.

With these establishments it will be instructive to compare the Greenwich Observatory. The latter has never won the title of great. It was originally established on the most modest scale, for the special purpose of making such observations as would conduce to the determination of the longitude at sea. Although it has now entered upon its third century, no attempt has ever been made to reconstruct it on a grand scale. Whenever any part of it was found insufficient for its purpose, new rooms were built for the special object in view, and thus it has been growing from the beginning by a process as natural and simple as that of the growth of a tree. Even now, the money value of its structure is less than that of several other public observatories, although it eclipses them all in the results of its work. Haeckel lays it down as a general law of research that the amount of original investigation actually prosecuted by a scientific institution is inversely proportional to its magnitude. Although this may be regarded as a humorous exaggeration, it teaches what the history of science shows to be a valuable lesson.

A glance at the number and work of the astronomical observatories of the present time will show how great a waste of means has been suffered in their erection and management. The last volume of the *American Ephemeris* contains a list of nearly 150 observatories, supposed to be, or to have recently been, in a state of "astronomical activity." The number omitted because they have lain inactive it is impossible to estimate; but it is not unlikely that, in this country at least, they are as numerous as those retained. It is safe to say that nearly everything of considerable value which has been done by all these establishments could have been better done by two or three well-organised observatories in each of the principal civilised countries. Indeed, if we leave out of account local benefits, such as the distribution of time, the instruction of students, and the entertainment of the public, it will be found that nearly all the astronomical researches of really permanent value have been made at a very small number of these institutions. The most useful branch of astronomy has hitherto been that which, treating of the positions and motions of the heavenly bodies, is practically applied to the determination of geographical positions on land and at sea. The Greenwich Observatory has, during the past century, been so far the largest contributor in this direction as to give rise to the remark that, if this branch of astronomy were entirely lost, it could be reconstructed from the Greenwich observations alone. During the past twenty years the four observatories at Greenwich, Pulkowa, Paris, and Washington have been so far the largest contributors to what we may call geometrical astronomy that, in this particular direction, the work of the hundred others, in the northern hemisphere at least, can be regarded only as subsidiary.

This remark, it will be understood, applies only to that special branch of astronomy which treats of the positions and motions of the heavenly bodies. The other great branch of the science treats of the aspect and physical constitution of these bodies. It dates from the invention of the telescope, because, without this instrument and its accessories, no detailed study of the heavenly bodies is possible. The field open to the telescope has, during the last twenty years, been immensely widened by the introduction of the spectroscope, the ultimate results of which it is scarcely possible to appreciate. Photography has recently been introduced as an accessory to both instruments; but this is not so much an independent instrument of research as a means of recording the results of the spectroscope and telescope. To this branch of the science a great number of observatories, public and private, have duly contributed, but, as we shall presently see, the ratio of results to means is far less than it would have been had their work all been done on a well-organised system.

Nearly all great public observatories have hitherto been constructed for the purpose of pursuing the first branch of the science—that which concerns itself, so to speak, with the geometry of the heavens. This was naturally the practice before the spectroscope opened up so new and rich a field. Even now there is one sound reason for adhering to this practice,—namely, that physical investigations, however made, must be the work of individuals rather than of establishments. There is no need of a great and expensive institution for the prosecution of spectroscopic observations. The man of genius with imperfect instruments will outdo the man of routine in the greatest building, with the most perfect appliances that wealth can supply. The combination of qualities which insures success in such endeavours is so rare that it is never safe to count upon securing it. Hence, even now, a great observatory for the prosecution of physical research whould be a somewhat hazardous experiment, unless the work it was to do were well mapped out beforehand.

Considering the great mass of observatories devoted to geometrical astronomy, the first thing to strike the professional student of their work is their want of means for a really useful and long-continued activity; and this notwithstanding that their instrumental equipment may be all that could be required. The reason is that their founders have not sufficiently taken into account the fact that the support of astronomers and the publication of observations is necessary to the usefulness of such an establishment, and requires a much larger endowment than the mere outfit of the building. Let us take, for instance, that omnipresent and most useful instrument, the meridian circle. Four or five of these instruments, of moderate size, located in good climates, properly manned, under skilful superintendence, working in co-operation with each other, would do everything necessary for the department of research to which they are applicable, and a great deal more than is to be expected from all the meridian circles of the world, under the conditions in which they are actually placed. They could, within the first five years, make several independent determinations of the fundamental data of astronomy, including the positions and motions of several hundred of the brighter fixed stars. In five years more they could extend their activity so as to fix the position of every star in the heavens visible to the naked eye; and, during the ten years following, could prepare such a catalogue of telescopic stars as there is no prospect of our seeing during the next half-century.

There are probably not less than twenty meridian circles in this country alone, most of them antiquated, it is true, yet, so far as average size and cost are concerned, amply sufficient for the work in question. How many there may be in other countries it is impossible to estimate, but probably fifty or upward, and the number is everywhere constantly increasing. Should we seek out what they are doing, we should probably find half of them rusting in idleness upon their pivots. With others some industrious professor or student would be found making, unaided, a series of observations to be left among the records of the establishment, or immured in the pages of the *Astronomische Nachrichten*, with small chance in either case of ever being used. We may be sure that the solitary observer will soon find something else to do, and leave the instrument once more in idleness. Others we should find employed in the occasional instruction of students, a costly instrument being used where a rough and cheap one, which the student could take to pieces and investigate at pleasure, would answer a far better purpose. Yet others we should find used in distributing time to the neighbouring cities or states, or regulating chronometers for the shipping of a port. I dare not guess how many we should find engaged in work really requiring an instrument of the finest class, and gaining results which are to contribute to the astronomy of the future, but in our own country there would hardly be more than three.

The general cause of this state of things lies upon the surface. It is as true in astronomy as in any other department of human affairs that the best results can be attained only by a careful adaptation of means to ends. Failures have arisen, not from the intervention of any active opposing agency, but because observatories have been founded without a clear conception of the object to be attained, and therefore without the best adaptation of means to ends. To build an observatory before knowing what it is going to do is much like designing a machine-shop and putting in a large collection of improved tools and machinery before concluding what the shop is to make, and what are the conditions of the market open to its products. Some hints on the considerations which should come into play in the erection of any new observatory may not be out of place, as pointing out the remedy for the evils we have described.

Heretofore the practice has usually been first to decide upon the observatory, and to plan the building; next to provide instruments; and lastly, to select an astronomer, and with his advice, to decide what direction the activities of the establishment should take. This order of proceeding should be reversed. The first thing to be done is to decide what the observatory shall be built to do. The future astronomer would, of course, have a controlling voice in this decision, and should, therefore, be selected in advance. One thing which it is especially important to decide is to which of the two great divisions of astronomical research attention shall principally be directed. If the prosecution of geometrical astronomy is kept in view, the conditions of advance in that department of the science must be kept in mind. The public is too apt to associate astronomy with looking through a telescope. That some of the greatest astronomers of modern times, such as Kepler, Newton, Hansen, Laplace and Leverrier scarcely ever looked through a telescope as astronomers, is not generally understood. For two thousand years astronomy has furnished the great geometers of the world with many of their profoundest problems, and thus has advanced hand in hand with mathematics. It borrows its fundamental data from observation, but the elaboration and development of its results taxes the powers of the mathematical investigator. The work of making the necessary observations is so much easier than that of developing the mathematical theories to which they give rise, that the latter is comparatively neglected alongside the former. It is lamentable to see what a collection of unused observations are found in the pages of scientific periodicals, to say nothing of those which have remained unpublished in the records of observatories. Under these circumstances it is not worth while to found any more observatories for the prosecution of geometrical astronomy, except under special conditions. Among these conditions we may enumerate the following:—

1. The institution should have such an endowment as to secure the continuous services of two or three observers, and to publish at least the results of their observations in a condensed form.

2. The instruments should be of the finest class, but not necessarily of large size. This is not a difficult condition to fulfil, since such instruments are not very costly. One reason for observing it is that it is only within the last few years that the highest perfection has been attained in the construction of instruments of measurement.

If these two conditions can be really fulfilled, it is very desirable to add a few more to the great number of meridian circles now in existence, for the simple reason that it is easy to exceed them in perfection. It is, however, to be remarked that a good climate is a scientific pre-requisite for the success of an observatory of any kind. The value of observations is decidedly lessened by the breaks in their continuity due to the intervention of clouds. It is therefore extremely desirable that, so far as possible, new observatories should hereafter be erected under sunny skies.

If an observatory is to be devoted to physical research, a more modest outfit, both in the way of endowment and of instrumental means, may be sufficient to serve an excellent purpose. Instead of being a great co-operative work, requiring the continuous labour of several persons, physical research may be divided up into sections almost as small as we please, each of which may be worked by an individual astronomer with any instrument suited to the purpose in view. To the success of such an observatory a clear sky is even more necessary than to one engaged in measurement. Whether a great telescope will be necessary, will depend principally upon what is to be done. The consideration which is really of the first importance is the astronomer. The man who is really wanted will do more with the most inexpensive instruments than another one with the most costly ones. As already remarked, physical research is mainly the work of the individual, and what we want is to secure the services of the ablest man and then supply him with such means of research as are necessary to the problems he has in view. New questions are arising so frequently, and the field of physical research is now so wide, that it is impossible to lay down any general rules for a physical observatory, except that means should be furnished for supplying the investigator with any instrument he may want.

A third class of observatories are those intended for instruction in astronomy. The requirements in this direction are so different from those necessary to research that it is impossible to combine the highest efficiency in both directions with the use of the same instruments. The number of observatories especially designed for pure instruction are very few in number. The instruments necessary for the purpose are of the simplest kind; indeed, so far as mere training is concerned, the engineer's level, transit, and theodolite can be made to serve most of the purposes of the astronomical student. What the latter really wants is that training of the eye and the mind which will enable him to understand the theories of instruments, the methods of eliminating the errors to which they are subject, and the mathematical principles involved in their application. In this, as in nearly every department of professional education, we may lay it down as a rule that the wants of a liberal and of a professional education are, so far as the foundation is concerned, identical. We are too prone to lead the student into the minute details of a subject without that previous training in first broad principles which, though it may not immediately tell on his progress as a student, will be felt throughout his life to whatever field of work he may devote himself. Such a transit instrument as Hipparchus might have made—a wooden level mounted on an axis and supplied with slits to serve the purpose of sights—properly mounted in the meridian, could well be made to take the place of the transit instrument for purposes of instruction. Scarcely any higher skill than that of a cabinet-maker would be required in its construction. The object at which the student should then aim would be, with the aid of this instrument, to determine the error of his clock or watch within a few seconds. If he is really acquainted with the principles of the subject, and has his eyes properly trained, he will have no difficulty in soon learning to do this. SIMON NEWCOMB

NOTES

THE following details regarding the sad accident by which Prof. Balfour lost his life have been received since Prof. Foster's article was written. It appears (from a letter from Mr. C. D. Cunningham to the *Times*) that on the 14th ult. Mr. Balfour crossed the Col du Géant, and on descending on the Italian side the idea first occurred to him of attempting the Aiguille Blanche de Peuteret, or, as it is sometimes called, the Aiguille de la Belle Etoile, a peak which is one of the buttresses of Mont Blanc, to the *massif* of which it is joined by an extremely steep snow *arête*. Mr. Cunningham's guide, Emile Rey, had previously attempted the peak, and was able to give Mr. Balfour many details as to the probable line of ascent. Having failed, however, to persuade Mr. Cunningham and the guide Rey to accompany him, Mr. Balfour started from Courmayeur on Tuesday, the 18th, with the guide Johann Petrus, for Aiguille, accompanied by a porter to carry blankets and wood as far as their sleeping-place on the rocks. It was thought, the ascent being new and difficult, he might be absent two nights, and return to Courmayeur on Thursday. As he did not reappear, it was thought he must have crossed to Chamounix, or gone down to the Chalets de Visaille for more provisions. On Friday Mr. Bertolini and Mr. Baker, at the hotel in Courmayeur, became seriously alarmed, and finding the party had not been heard of either at Chamounix or at the Chalets de Visaille, they sent out a search party, which, early on Sunday morning, on reaching the rocks between the Glacier de Brouillard and the Glacier de Fresny, found the bodies of Mr. Balfour and Petrus, both partly covered with snow, at the foot of the steep snow *arête*. As there was little fresh snow about the place, it was probably not an avalanche that caused their death. One may have slipped, and the other not had sufficient strength to hold his companion. The provisions at the sleeping-place having been untouched, the accident must have taken place on Wednesday, the 19th. But it is not certain whether they fell in descent or ascent. Means were taken on the 25th to have the remains brought to the hotel.

THE three missions designated for observation of the Venus transit in Patagonia left on the 20th ult. in the Messageries steamer from Bordeaux, for Buenos Ayres. The arrangement is as follows:—*Rio Negro* (41° S.), M. Perrotin, director of Nice Observatory, accompanied by Lieutenants Tessier and Delacroix, and M. Guénaire, photographer to the Observatory; *Chubut* (43° S.), M. Hatt, hydrographic engineer, assisted by Lieut. Leygue and M. Mion, engineer; *Santa Cruz* (50° S.), Capt. Fleuriais, assisted by Lieutenants Le Pord and de Royer de Saint Julien, and M. Lebrun, naturalist. Arrived at Monte Video, the first two missions will probably embark in the advice boat *La Bourdonnais*, the third in the advice boat *Le Volage*. In the course of observations, detachments from the *Volage* will try to ascend the Rio Santa-Cruz at least to the point reached by Darwin in the *Beagle* expedition. The Chili mission, composed of Lieut. de Bernardière, assisted by Lieut. Barnaud and Ensign Favereau, embarked on the 15th ult. in an English steamer going by the Straits of Magellan.

UNDER the name of a "North German Museum for Natural Science" Dr. G. Haller and Cie have opened at Putbus, on the island of Rügen, a storehouse of natural objects and aids to teaching, whence schools, museums, and private individuals may obtain specimens and collections, representing all the three kingdoms of nature. An institute for investigation of the Baltic forms part of the scheme, and a few students have been enrolled, we learn, for the current summer. Dr. Haller was formerly a privat-docent of zoology in Berne. With the aid of a well-known entomologist, collections of insects of all kinds (exotic included) are furnished; also biological collections of caterpillars, larvæ, pupæ, parasites, &c. It is intended, later on, to supply collections of the insect pests of agriculture. The utensils of entomologists and other apparatus are also provided. Of European mammals, birds, reptiles, amphibia, fishes, &c., many specimens are kept, preserved in the usual way; also preparations for the study of embryology and comparative anatomy, and for varied microscopical work. A variety of live animals for aquaria and terraria are provided. The dry preparations of frogs and other animals obtained by a modification of Semper's method have received special commendation, also the series of embryos and parasites.

FREE libraries do not increase in number so rapidly in England as in America, where they have now reached to 4000. Yet a pamphlet or circular issued by the Bureau of Education must be of considerable interest to any who are engaged in starting or working libraries. It points out the disadvantages of the arrangements of existing library buildings, and gives a general plan by which they may be avoided. The chief American libraries consist of large halls open from floor to roof and surrounded by galleries five or six one over another like a theatre. The author of this paper (Mr Poole of Chicago) objects to this general plan, on account of (1) the waste of this central space, or if this central space is used for reading, for its publicity and noisiness; (2) the difficulty of getting any uniform temperature over the whole of such a building, for while the lower floors are kept at a mild warmth the upper floors become so intensely hot that not even an attendant can work there, and the bindings perish from heat; (3) the wasteful expenditure of the physical strength and time of attendants in going upstairs and round from one part of the library to another; (4) the special convenience for catching fire where all communicate with one centre instead of being divided into fireproof compartments; (5) the difficulty of enlarging such a circular building, as the principal American libraries already require enlarging; and (6) its great expense. In the plan which Mr. Poole suggests ten rooms surround a square space equal to only two of the rooms. Each room should be about 16 feet high, thus easily warmed uniformly. Books should be classified, and in a few cases duplicate copies kept so that a student should find all the books on the subject he wanted in one room, and there should be no journeying to distant galleries; of course where a library is so small that all its books can be stored in one room a great difficulty is avoided. Each room being separate and all being built of fireproof material and only communicating by a light iron gallery, which goes round the central area, they are both quiet and fireproof. In each room a row of reading tables will stand under the windows at one end, and the remaining space will be covered with double shelves, not more than 7½ feet high, with passages 3 feet or 3½ feet wide between. No ladders will thus be required, and the high temperature will be avoided. Yet twenty-five volumes to every square foot of flooring can be stored in this way, and hence a room 40 feet × 40 feet will hold 40,000 volumes; ten such rooms on a floor give 400,000 volumes, and five storeys high will hold 2,000,000.

MR. CLEMENT L. WRAGGE has written to the *Times* earnestly entreating all visitors to Ben Nevis to co-operate with him and the Scottish Meteorological Society to prevent damage to the instruments on the mountain. These are, of course, kept under lock and key, and till lately all has gone well with them. But on the morning of July 23 it was found that wanton mischief had been done to the intermediate station at the Red Burn Crossing, about 2700 feet above the sea. A hole had been made in the thermometer box, the louvre forced off, and the wet bulb thermometer forced from its screws, and broken. The compass points had also been removed. It seems difficult to account for such acts. Mr. Wragge's appeal to the British public will not, we trust, be in vain.

IN connection with the forthcoming electro-technical exhibition in Munich, the Bavarian Kunstgewerbe-Verein has announced a prize competition for light-fittings (lustres, brackets, candelabra, &c.), suitable for the electric light. The Edison illumination, to be maintained by about 80 horse-power, will be no way inferior in extent to that in Paris; the restaurant-hall, with garden, library, and reading room, one or two streets, and the theatre, will be lit with 800 Edison lamps of various strength, from 8 to 100 candles. Mr. Edison's plans for centrally lighting up a whole city quarter with 14,000 lamps of 170,000 total candle-power will be shown; the system is to be tried in New York. Schuckert, of Nürnberg, will, from the roof of the crystal palace, light up the Frauenthürme with a reflector lamp of 10,000 candle-power; also the temporary theatre with an upper light of 4000 candle-power; he will also exhibit several transportable electric lights for war purposes, railways, &c. Special interest will attach to an effort to utilise the water-power of the Hirschau, about three miles from the palace; the current will work a lift or thrashing machine in the palace by day, and illuminate the garden and the Königsplatz by night (11 lamps of 1000 candle-power each). The copper wire will be 3 mm. thick. A provisional plan of the Exhibition is supplied with the *Electrotechnische Zeitschrift* for July.

THE Council of University College, London, have accepted a fund raised in memory of Miss Ellen Watson, a former student. A Memorial Scholarship consisting of the income of the fund is open to students of either sex who display very marked merit in applied mathematics.

THE *Herald* (N.Y.) correspondent with the party in search of the lost crew of the *Jeannette* has been impressed by the beauty of the teeth of natives of Northern Siberia. He saw old men of sixty and seventy with sets of teeth small and pearly white, polished and healthy. Decay and suffering are unknown. A physician of Yakutsk attributed this to the habits and the kind of food eaten by the natives, and to a certain care taken by them from childhood up. First, the natives do not touch sugar in any form, for the simple reason that they cannot afford to buy it. Secondly, they are in the habit of drinking daily large quantities of fermented sour milk summer and winter, which is antiscorbutic, and is very beneficial in preserving the teeth. And lastly, they have the habit of chewing a preparation of the resin of the fir tree, a piece of which, tasting like tar, they masticate after every meal, in order specially to clear the teeth and gums of particles of food that may remain after meals. The gum or resin is prepared and sold by all apothecaries in Siberia, and is much used by Russian ladies.

THE International Committee of the Red Cross Society of Geneva have recently offered a prize of 2000 francs for three studies (to be complementary of each other), on the art of improvising means of help for the wounded and sick; the first to relate to the production of means of treatment, the second to means of transport, the third to the sudden providing of an ambulance or a field-hospital. Papers to be sent in before April 1, 1883.

MM. HACHETTE AND CO. will publish in a few weeks the first volume of a new series—"Les Drames de la Science"—entitled "La Pose du Premier Cable"; the author is M. W. de Fonvielle.

FROM Signor Ricco's report on latitudes of groups of sun-spots in 1881, it appears that 258 groups or formations of spots and cavities were observed (82 presenting only cavities). The groups of the northern hemisphere seemed to have longer duration; more of them reappeared after one or more rotations. They were also richer in spots. The groups of latitudes under 15° were always displaced towards the equator, those of latitudes over 15° towards the poles. The development of groups is more rapid than their disappearance. The distribution was:—In the northern hemisphere, 132 groups in a zone of 22° between +7° and +29° with a maximum at +20°; in the southern, 126 groups in a zone of 30° (therefore broader) between −3° and −33°, maximum at −18°, more pronounced than in the other hemisphere. The centres of the two bands of spots was at the same latitude, 18°. The band without spots or cavities, between the other, was about 10° in breadth, with centre at +2°. In the northern hemisphere the greatest duration belonged to the groups in the lowest latitudes (generally the groups richest in spots and most durable are at the latitudes of maxima).

FOR determination of high temperatures at the Imperial Porcelain Manufactory in Berlin, pyroscopes of noble metal have been long used with the best success; the materials are pure silver and gold, silver alloys with 20, 30, 40, 60, and 80 per cent. of gold, and gold-platina alloys with 5, 10, or 15 per cent. platina. Silver-platina alloys are objectionable, because at high temperatures the silver is very volatile, so that the composition changes. Also alloys of gold with more than 15 per cent. platina are not used, because they do not suddenly melt down; but an alloy richer in gold separates out, while a skeleton richer in platina remains, to melt at a higher temperature. For the measurement with alloys, balls of 1 to 2 gr. weight, between parchment paper, are hammered on the anvil to about the thickness of a penny-piece; the pieces are bent so that they can stand upright, and placed in rows, arranged according to melting-point in small cupels of clay, magnesia, or bone-ash, in such a way that they can be seen from without, through a hole. For a new experiment they have merely to be flattened out again, and put into the same cupel. In this way temperatures from the melting-heat of silver to nearly that of cast-steel can be determined pretty exactly.

WE have received "Fragments of the Coarser Anatomy of Diurnal Lepidoptera," by Mr. Samuel H. Scudder, being an account of dissections of caterpillars and chrysalids of butterflies; it is issued partly with the view of calling attention to the need of work on a subject which is very imperfectly known at present. The "Studies from the Biological Laboratory" of the Johns Hopkins University for June, contains original matter relating to the pulse wave in the coronary artery, the influence of digitaline on the heart, polar action in nerves, temperature and reflex actions, &c. A reprinted memoir by Staff-Commander Tizzard, R.N., and Mr. John Murray, on "Exploration of the Faroe Channel during the Summer of 1880 in Her Majesty's hired Ship *Knight Errant*," with various subsidiary reports, has also reached us, and we hope soon to refer to its contents.

"THE Photographic Studios of Europe," by Mr. H. Baden Pritchard (London: Piper and Carter) gives copious information that the professional photographer will appreciate and find helpful, but has also much to interest the general reader. It is the outcome of a house-to-house visitation of the principal studios in Europe, and a record, in colloquial style, of the practice observed. For convenient reference the information is tabulated in the introductory chapter, under nine headings (the reception-room, the studio, the dark room, &c.), and the names of the photographers follow, in each case, with the page-numbers. Among matter of a special nature we note accounts of photographing prisoners at Millbank and Pentonville, and at the Prefecture of Police in Paris; also a popular account of Dr. Huggins' photographs of the Stars.

SIGNOR MAUDELIN affirms that the violets *V. syrtica*, *V. tricolor*, and *V. arvensis* contain from 0·083 to 0·144 per cent. of salicylic acid. The other species contain none; at least no appreciable quantity. The wild violet has much more than the tricolor. It is the action of salicylic acid that explains the use of the violet in pharmacy.

MR. W. B. COOPER has lately brought before the Franklin Institute a device for increasing the dynamic effect of the vibrations of diaphragms. To one end of a wire or band he attaches a diaphragm or other pulsating body; the wire is passed a half turn or several turns round a drum or pulley, which is rotated towards the diaphragm. To the other end may be attached a lever having a point adapted to indentation of sheet metal passed under it at uniform speed. With such an arrangement (called a "phonodynamograph") Mr. Cooper has embossed brass of the thickness of writing paper by impact of the voice on a diaphragm like that of the phonograph. (The force of the pull is augmented by force derived from friction on the surface of the pulley). The principle is applicable to the telephone, both for increasing the intensity of the electric impulses transmitted, and augmenting their effects at the receiving station, and Mr. Cooper shows how this may be advantageously done.

THE northernmost place in the world where rye and oats mature is at Kengis, in the Swedish province of Norrbotten, 49 miles to north of the Polar Circle, whereas the northernmost spot where corn is grown is at Muoniovara, 98 miles to north of the Circle. The rye yields, it is stated, 98 per cent., and the oats about 90.

THE additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from West Africa, presented by Mrs. Cumberleye; a Ring-necked Parrakeet (*Palæornis torquatus*) from India, presented by Mr. W. K. Stanley; four Egyptian Ouarans (*Psammosaurus scincus*) from Egypt, a Horseshoe Snake (*Zamenis hippocrepis*), eleven —— Snakes (*Zamenis ventrimaculatus*), an Ocellated Sand Skink (*Seps ocellatus*), South European, presented by Messrs. Wylde Beyts and Co.; a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, deposited; a Spotted Bower Bird (*Chlamydodera maculata*) from South Australia, a White-billed Parrakeet (*Tanygnathus albirostris*) from Celebes, a Yellow-billed Sheathbill (*Chionis alba*), captured at sea, off Cape Horn; a Shag (*Phalacrocorax cristatus*), North European, a Cornish Chough (*Fregilus graculus*), British, four Eyed Lizards (*Lacerta ocellata*), South European, purchased; a Humboldt's Lagothrix (*Lagothrix Humboldti*) from Upper Amazon, received in exchange; five Undulated Grass Parrakeets (*Melopsittacus undulatus*), a Geoffroy's Dove (*Peristera geoffroii*), bred in the Gardens. The following insects have emerged during the past week in the Insect House:—Silk Moths: *Telea promethea;* Butterflies: *Vanessa antiopa*, *Vanessa polychlorus*, *Vanessa io*, *Melanagria galathea*, *Gonoepteryx rhamni*, *Thecla betulæ*, *Erebia blandina*, *Hipparchia janira;* Moths: *Deilephila euphorbiæ*, *Bombyx castrensis*, *Liparis monacha*, *Liparis dispar*, *Chelonia caja*.

OUR ASTRONOMICAL COLUMN

CONTINENTAL OBSERVATORIES.—The last number of the *Vierteljahrsschrift der Astronomischen Gesellschaft* contains reports of the proceedings of some twenty of the observatories on the continent during the year 1881. At Berlin observations for the zone + 20° to 25°, were actively continued, upwards of 10,000 being made in the year. The 9-inch refractor was employed for comets and small planets, &c., the physical appearances of the comet 1881 III. receiving special attention. With the Declinograph 1200 small stars were observed, making, up to the end of 1881, 12,329 stars, mostly from the eleventh to the thirteenth magnitudes, thus determined, in connection with the identification and observation of the small planets. At Bonn the southern "Durchmusterung" furnished observations of upwards of 14,000 stars, so that rapid progress is being made with this work under the direction of Prof. Schönfeld. At Brussels astronomical physics, as well as meridian observations, have been attended to; the meteors of the August period were extensively observed over Belgium; Christiania was mainly occupied, under Dr. Fearnley, with the zone 65° – 70°, and the curious circumstance of the existence of four variable stars in this zone within a radius of 1° is recorded, the first in 20h. 59m. 20s. + 66° 8'·5, has been estimated by various observers from 5m. (Lalande) to 9m. (Argelander), the second is in 20h. 59m. 48s. + 67° 35'·9, the third in 21h. 7m. 33m. + 67° 54'·4, and the fourth in 21h. 11m. 49s. + 66° 0'·9, for 1855·0. Baron v. Engelhardt, at Dresden, has zealously observed the various comets of the year, and has made 111 observations of 19 minor planets, the principal instrument in the Baron's observatory is an equatorial refractor by Howard Grubb, of Dublin, aperture 306 mm. A new physical observatory has been erected at Herény, Hungary, by Eugen and Alexander von Gothard, the position of which is 12m. 49·8s. east of Berlin, with latitude 47° 16' 37"; the observatory is provided with a 10¼ inch equatorially mounted reflector by Browning, of London, observations were commenced in the second week of November, and chiefly consisted of the examination of star-spectra. At Keil an 8-inch refractor by Steinheil has been received: meridian observations here were largely devoted to circumpolar stars + 79° to 82°, but according to the present plan, the observations will be continued to the pole. Leipsic is now under the direction of Prof. H. Bruns. At Lund the zone undertaken by the observatory was continued, more than 5200 stars being determined. From the Observatory of Brera, Milan, Prof. Schiaparelli makes the welcome announcement that the late Baron Dembowski had confided to him all his astronomical manuscripts with the condition that they were to be utilised to the best advantage for the science. His measures of double stars, upwards of 20,000 in number, will be published under the auspices of the Accademia Reale dei Lincei; they are to form four volumes, of which the first will contain the measures made by Dembowski at Naples with his Plössl Dialyte in the years 1852-58; the second and third, the observations made at Galarate on stars of the Dorpat Catalogue, and the fourth, the measures of stars in W. Struve's appendix, the Pulkowa Catalogue, and double stars discovered by other astronomers, more especially by the eminent American observer, Mr. Burnham. The first volume is in course of preparation. At Plonsk Dr. Jedrzejewicz continues, in his private observatory, measures of double stars as his principal work. The passages of the red spot on Jupiter, by the middle of the disc, were micrometrically determined from November 25, 1880, to February 5, 1881, from 174 rotations, the period was found to be 9h. 55m. 34·41s. ± 0·13s., and at the same time the jovicentric latitude of the centre of the spot was found – 22°·8, and its length in degrees of the parallel 26°·4; the third and fourth comets of 1881 and Encke's comet were also observed for position. The physical observatory at Potsdam was in full activity, and in addition to the more special subjects of observation undertaken by this important establishment, an extensive series of observations of variable stars was secured in 1881. From Stockholm Dr. Hugo Gylden notifies his determination of the parallax of the star Bradley 3077, or No. 240 in Argelander's Catalogue of 250 stars, forming part of the seventh volume of the Bonn observations: the resulting value is 0″·283 ± 0″·0468; this star has considerable proper motion. Prof. R. Wolf communicates, from Zurich, he monthly numbers of days with and without sun-spots, and the relative numbers: in the whole year's observing-days, the sun was free from spots on five days, and exhibited spots on 297.

ATOMIC ATTRACTION

THE theory of universal gravitation, as I understand it, asserts that the mutual attraction exerted by any two bodies, A and B, is dependent only on their respective masses and on the distance between them, being entirely uninfluenced by the presence of other bodies even in the immediate neighbourhood of A or B. Thus at a given moment the Earth and Venus, being in certain definite positions, exert upon each other a certain force of attraction; the attraction thus taking place between the masses of the two planets would be unaltered by the removal of the Moon from the sphere of action; the gravitation of the Earth and the Moon does not therefore tie up any portion of the attractive energy of the Earth, and so diminish the force with which other bodies gravitate towards it.

A totally different assumption is usually made with regard to that form of attraction which gives rise to chemical phenomena. Here it is supposed that two or more atoms, having combined together, have thereby become incapable, at any rate in the majority of cases, of attracting others to any appreciable extent. Thus I imagine that most chemists hold the view that when hydrogen and oxygen combine together to form water they thereby exhaust, or nearly exhaust, their combining power, that the power of attraction residing in the oxygen atoms is all concentrated upon the hydrogen atoms, just as we might conceive all the attractive power of the Earth concentrated on the moon, thus leaving all other bodies in its neighbourhood free from the influence of gravity. We thus invest matter with two separate forms of attraction differing entirely in their mode of action, and having indeed nothing in common. It is however possible to a certain extent to assimilate chemical attraction and gravitation, and I propose here to discuss some of the results which ensue from the elaboration of this idea. Let us suppose then that the act of chemical combination in no wise alters the power of attraction which the combining atoms exert upon surrounding bodies, and let us see what effect this hypothesis has upon the explanation of various phenomena. In order to do this we must first render as precise as possible our notions of the construction of chemical compounds.

It is now known with certainty that the atomic and molecular volumes of substances are but slightly altered by combination, that is to say, that under comparable conditions the atom of any substance generally occupies about the same space with whatever atoms, similar or dissimilar, it may be combined. This fact seems to me to point to the conclusion that the atoms which make up a molecule are as close together as their periodic motions will permit, and are not merely held in certain positions of equilibrium by various opposing forces; for if the latter supposition were true, I fail to see how it would be possible for the same atom, together with its surrounding proportion of space, to have always the same volume. The immediate proximity of the several molecules in the liquid and solid states must also be assumed, in order to account for the invariability of molecular volumes.

The innumerable facts which have been brought to light by the efforts of those who have investigated the chemistry of the carbon compounds all lead one to suppose that there is some foundation for the ideas propounded by chemists concerning the position of the atoms, and that the constitutional formulæ ascribed to organic substances really represent the construction of the molecule. If this be so it certainly furnishes a further argument in support of the proximity of the atoms.

The assumptions contained in the preceding paragraphs are in no way opposed to the views generally held concerning molecular and atomic motion which we owe to the development of the science of heat. They merely state that there is no force of repulsion exerted between contiguous atoms, and that the vibratory or other movements are small compared with the size of the moving masses.

The object of the following remarks is to show that the hypothesis concerning chemical attraction mentioned above enables us to offer some explanation of the relative volatility of bodies. We all, I presume, look upon the maximum vapour tension of a substance at a given temperature as affording to a certain extent a means of estimating the attraction which its molecules exert among themselves; if there is considerable attraction there will be a low vapour tension, and with little attraction there will be a low boiling point. It follows from this that the attraction between the molecules of hydrogen is relatively extremely small; that in the case of oxygen and nitrogen it is also very small, though probably much larger than in the former case; the attraction mutually exerted by molecules of chlorine will be more considerable; while with bromine, iodine, and other liquid and solid elements it will be greater still. We must not however confound the attraction exerted between *atoms* of a substance with that between the *molecules*, for each atom attracts separately those of the contiguous molecule, so that the attraction between two molecules of bromine, for example, will be four times as great as between two atoms, and generally when the molecule of a substance contains n atoms the attraction between two molecules will be approximately n^2 times that between two atoms. This is of course even approximately true only when the distance between the two molecules is great relatively to their size; when the two molecules are close together the several interatomic attractions will be exercised over very different distances, and will therefore be very unequal in amount. Nevertheless, the above remark enables us to see that in some cases the apparent attraction, as estimated by the boiling-point, may be very misleading. In sulphur, for example, of which the molecule in the solid and liquid states is probably somewhat complex, we have a substance of high boiling-point, though the mutual attraction of the atoms may be comparatively small. The same is the case with carbon and many other substances.

Applying now the above considerations to a few actual cases, we shall see that the relative volatility of different substances is generally satisfactorily explained. Let us designate by $(h\,h)$ the attraction at unit distance between two atoms of hydrogen, by $(o\,o)$ the attraction between two atoms of oxygen, and generally by $(r\,s)$ the attraction at unit distance between any two atoms, R and S. Then in the case of water the molecular attraction will be represented by—

$$4A(h\,h) + 4B(h\,o) + C(o\,o),$$

where A, B, and C are factors dependent on the distances which separate the atoms; now we have seen that $(h\,h)$ and $(o\,o)$ probably have small values, but $(h\,o)$ is not small, hence the attraction between molecules of water should be far greater than that between molecules of oxygen, and the boiling-point much higher, a result which is in accord with fact. The boiling-point of water would probably be much higher than it is, were it not that the attractions between H and O are exerted over comparatively large distances, owing to the hydrogen of one molecule shielding its companion oxygen from the approach of other hydrogen. In the similarly constituted body, H_2S, the value of the molecular attraction will be—

$$4A(h\,h) + 4B(h\,s) + C(s\,s),$$

in which expression A, B, and C may be supposed to have values not differing excessively from those which hold good in the case of water (the sulphuretted hydrogen being supposed liquid). The value $(s\,s)$ is in itself small, and since the force is exerted between two atoms which cannot approach each other very closely, C is also small. The affinity of hydrogen for sulphur being also feeble, the whole value of the molecular attraction is small; sulphuretted hydrogen should therefore be an extremely volatile body, which is actually the case.

With hydrochloric, hydrobromic, and hydroidic acids we have for the molecular attraction the several values—

$$A(h\,h) + 2B(h\,cl) + C(cl\,cl)$$
$$A'(h\,h) + 2B'(h\,br) + C'(br\,br)$$
$$A''(h\,h) + 2B''(h\,i) + C''(i\,i).$$

As the three bodies are similarly constructed we may assume that A, A′, A″, &c., do not materially differ. As the third terms of these expressions increase the second terms diminish; we should therefore expect that there might be no great difference in the vapour-tensions of the three substances; experiment proves that they may be liquefied with about equal facility. It should be noticed that the thermal change accompanying the formation of any one of these gases, HCl, for example, is not a true measure of the attraction between the atoms, since it also includes the heat employed in separating the atoms of the original molecules H_2 and Cl_2.

We may also find a confirmation of the above views in the many homologous series of organic chemistry. In the alcohols of the ethyl series, for example, the larger the molecules the greater must be the attraction between them, and consequently the higher the boiling-point; this, as is well known, is in accordance with fact. In the case of isomeric alcohols, the influence of the position of the atoms comes conspicuously to the fore. It is clear that if the atoms of carbon of two different molecules cannot approach each other so nearly in the case of one isomer as in another, the attraction between the molecules will be less, and the boiling-point consequently lower. Now in secondary and tertiary alcohols the carbon atoms are more sheltered by each other, are, as it were, more removed from the exterior of the molecule than in primary alcohols; at the same time the boiling-points are lower, which is as it should be.

If we replace two atoms of hydrogen in an alcohol by one of oxygen we increase the attraction of the molecules, since we substitute a certain number of attractions ($h\,o$) and ($c\,o$) for the relatively small attractions ($h\,h$) and ($c\,h$); the increase of boiling-point which we should expect is confirmed by experiment. Many other examples might be brought forward, were it not that their discussion would transcend the limits of this article.

Before concluding I should like to draw attention to one question which is of importance. The use of the above hypothesis renders it difficult at first sight to account for the formation of definite chemical compounds; it seems that if any number of atoms of hydrogen are equally attracted by one of chlorine, the combination of one of them with that atom would not prevent the adherence of a second and a third forming H_2Cl, H_3Cl, &c. This difficulty is avoided by supposing that the chlorine atom is of such a form that only one atom of hydrogen can approach sufficiently closely to adhere permanently; such forms are difficult to imagine, though it may be remarked that an atom in the form of a ring offers in a certain sense a unique position to another which instals itself inside it. The existence of molecular compounds proves that the permanent adherence of other atoms is sometimes possible, and thus affords material support to the notion that the chemical affinity of an atom is not only exerted upon those atoms with which it is combined, but upon all others in its vicinity.

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THE GESTURE SPEECH OF MAN[1]

ANTHROPOLOGY tells the march of mankind out of savagery, in which different peoples have advanced in varying degrees, but all started in progress in civilisation from a point lower than that now occupied by the lowest of the tribes now found on earth. The marks of their rude origin, retained by all, are of the same number and kind, though differing in distinctness, showing a common origin to all intellectual and social development, notwithstanding present diversities. The most notable criterion of difference is in the copiousness and precision of oral speech, and connected with that, both as to origin and structure, is the unequal survival of gesture signs, which it is believed once universally prevailed. Where sign-language survives it is, therefore, an instructive vestige of the prehistoric epoch, and its study may solve problems in philology and psychology. That study is best pursued by comparing the pre-eminent gesture system of the North American Indians with the more degenerate or less developed systems of other peoples.

North America showed more favourable conditions for the development of gesture signs than any other thoroughly explored part of the world. In the pre-Columbian period the population was scanty, and so subdivided dialectically that the members of but few bands could readily converse with each other. The sixty-five families of the Indian language now known to have existed within the territory of the United States differed among themselves as radically as each differed from the Hebrew, Chinese, or English. In each of these families there were sometimes as many as twenty separate languages, differing from each other as the English, French, German, and Persian divisions of the Aryan linguistic stock.

The conditions and circumstances attending the prevalence, and sometimes the disuse, of sign-language in North America were explained. The report of travellers, that among Indians, as well as other tribes of men, some were unable to converse in the dark, because they could not gesture, is false. It is the old story of *aglossos* and *barbaros* applied by the Greeks to all who did not speak Greek, repeated by Isaiah of the "stammering" Assyrians, and now appearing in the term *slav* (speaker) as contradistinguished by the Russians from the Germans, whom they stigmatise as *njemez* (tongueless).

The theory that sign-language was the original utterance of mankind does not depend upon such tales or prejudices. After the immeasurable period during which man has been upon the earth, it is not probable that any existing peoples can be found among whom speech has not obviated the absolute necessity for gesture in communication between themselves. The signs survive for convenience used together in oral language, and for special employment when language is unavailable.

The assertions made that the sign-language of Indians originated from some one definite tribe or region supposes its comparatively recent origin, whereas the conditions favourable to its development existed very long ago and were co-extensive with the territory of North America occupied by any of the tribes. Such a solution would only be next in difficulty to the old persistent determination to decide upon the origin of the whole Indian race, in which most people of antiquity in the eastern hemisphere, including the lost tribe of Israel, the gypsies, and the Welsh, had figured conspicuously as putative parents. Numerous evidences were presented as to its antiquity and generality. But the signs are not now, and from the nature of their formation never were, identical and uniform.

An argument for the uniformity of the signs of Indians was derived from the fact that those used by any of them were generally understood by others. But signs might be understood without being identical with any before seen. There was evidence that where sign language was found among Indian tribes it had become more uniform than ever before, simply because many tribes had for some time past been forced to dwell near together at peace. The process of the formation and introduction of signs was the same among Indians as often observed among uninstructed deaf-mutes when associated together. There was a similarity of development between the sign language of mutes and Indians. The longer and closer the contact between Indians while no common tongue was adopted, the greater would be the uniformity of signs. The inference that there was but one true Indian sign language, just as there was but one true English language, was not correct, unless it could be shown that a much larger proportion of the Indians who use signs at all, than present researches show to be the case, used identically the same signs to express the same ideas, and also because the signs are not absolute and arbitrary, as are the words of English.

Are these signs conventional or instinctive? Sign language, as a product of evolution, had been developed rather than invented, and yet it seemed probable that each of the separate signs, like the several steps that lead to any true invention, had a definite origin arising out of some appropriate occasion, and the same sign might in this manner have had many independent origins due to identity in the circumstances, or, if lost, might have been reproduced. In regard to arbitrary or natural sounds, no signs in common use were in their origin conventional, and what appeared to be conventionality largely consisted in the form of abbreviation agreed upon. When the signs of the Indians had from ideographic become demotic, they might be called conventional, but still not arbitrary. Yet, while all Indians, as well as all gesturing men, have many signs in common, they use many others which have become conventional in the sense that their etymology and conception are not now known or regarded by those using them. The conventions by which such signs were established occurred during long periods and under many differing circumstances. Our Indians, far from being a homogeneous race and possessing uniformity in their language, religions, and customs, differ from each other more than all the several nations of Europe, and their semiotic conceptions have correspondingly differed. To insist that sign language was uniform were to assert that it is perfect. He next went on to prove the general ancient use of the system in North America. This fact might be recognised among tribes long exposed to

[1] Address by Col. Garrick Mallery, U.S.A., Chairman of the Sub-Section of Anthropology at the American Association (Cincinnati).

European influence and officially segregated from all others. Collections had been obtained from the Iroquois, Ojibwas, Alaskans, Apaches, Tuni, Pimas, Papagos and Maricopas, after army officers, missionaries, Indian agents, and travellers had denied them to be possessed of any knowledge on the subject.

The studies so far pursued led to the conclusion that at the time of the discovery of North America all its inhabitants practised sign language, though with different degrees of expertness, and that while under changed circumstances it was disused by some, others, in especial those who, after the acquisition of horses, became nomads of the great plains, retained and cultivated it to the high development now attained.

Instances were presented of the ascertained permanence of some Indian signs, and those of foreign peoples and deaf mutes. Though they, as well as words, animals and plants, have had their growth, development, and change, those which are general both among Indian tribes, and are also found in other parts of the world, must be of great antiquity. Many signs but little differentiated were unstable, while others that have proved to be the best modes of expression have survived as definite and established.

The Indian system as a whole was compared with those of foreign peoples—the ancient Greeks and Romans and the modern Italians being first considered. His researches during several years showed a surprising number of signs for the same idea which were substantially identical not only among savage tribes, but among all peoples that used gesture signs with any freedom. This remark applied to the collections of signs already obtained by correspondence from among the Turks, Armenians, and Koords, the Bushmen of Africa, the Fijians, the Redjangs and Lelongs of Sumatra, the Chinese and the Australians. In comparing the Indian sign language with deaf mute signs, it was noticeable that the Indians who had been brought to the Eastern States had often held happy intercourse by signs with white deaf mutes, who surely had no semiotic code preconcerted with any of the plain roamers. Many of their signs were identical, and all sooner or later were mutually understood. The result of all these comparisons was that the so-called sign language of Indians was not, properly speaking, one language, but that it and the gesture systems of deaf mutes and of all people constituted together one language—the gesture speech of mankind—of which each system is a dialect.

The most interesting light in which Indians, as other lower tribes of men, are to be regarded is in their present representation of the stage of evolution once passed through by our ancestors. Their signs, as well as their myths and customs, form a part of the palæontology of humanity. Their picture writings are now translated by working on the hypothesis that their rude form of graphic representation, when at the same time a system of idiographic gesture signs prevailed, would probably have been connected with the latter. Traces of the signs now used by the Indians are also found in the ideographic pictures of the Egyptian, Chinese, and Aztec characters.

Signs often gave to spoken words their first significance, and many primordial roots of language are found in bodily actions. Examples were given of English, Indian, Greek, and Latin words in connection with gesture signs for the same meaning, and the structure of the sign language was compared with the tongues of this continent, and with reference also to old Asiatic and African languages, showing similar operations of conditions in the same psychologic horizon.

The most obvious application of sign language for its practical utility depended upon the correctness of the view submitted, that it is not a mere semaphoric repetition of motions to be memorised from a limited traditional list, but a cultivable art, founded upon principles which can be readily applied by travellers. The advantage was not merely theoretical, but had been demonstrated to be practical by a professor in a deaf-mute college, who, lately visiting several of the wild tribes of the plains, made himself understood among all of them without knowing a word of any of their languages, and by another who had a similar experience in Italy and Southern France.

The powers of sign language were then compared with those of speech. It finds actually in nature an image by which any person can express his thoughts and wishes on the most needful subjects to any other person. Merely emotional sounds may correspond with merely emotional gestures, but whether with or without them would be useless for the explicit communication of facts and opinions of which signs themselves are capable. Notwithstanding frequent denials, they do possess abstract ideas. The rapidity of communication is very great, and can approach to that of thought. Oral speech is now conventional, and with the similar development of sign-language conventional expressions could be made with hands and body more quickly than with the vocal organs, because more organs could be worked at once.

But such rapidity is only obtained by a system of preconcerted abbreviations and by the adoption of absolute forms, thus sacrificing self-interpretation and naturalness.

Sign-language was superior to all others in that it permitted every one to find in nature an image to express his thoughts on the most needful matters intelligibly to any other person. The direct or substantial natural analogy peculiar to it prevented a confusion of ideas. Successful signs must have a much closer analogy and establish a *rapport* between the talkers far beyond that produced by the mere sound of words. If they had been elaborated by the secular labour devoted to spoken language, man could by his hands, arms and fingers, with facial and bodily accentuation, express any idea that could be conveyed by words. The very concepts of plurality, momentum, and righteousness could be clearly expressed by signs, and it is not understood why those signs could not have obtained their present abstract significance through the thoughts arising from the combination and comparison of other signs, without words. When highly cultivated, the rapidity of sign language on familiar subjects exceeds that of speech, and approaches to that of thought itself.

From the records of the ancient classic authors and also from the figures on Etruscan vases and Herculanean bronzes and other forms of archaic art, it is certain that a system of gesture-language is of great antiquity. Later, Quintilian gave elaborate rules for gesture which are specially notable for the significant disposition of the fingers still prevailing in Naples. The ancient and modern pantomimes were discussed, and also the gestures of speaking actors in the theatres, the latter being seldom actually significant or self-interpreting even in the expression of strong emotion. The same scenic gesture must apply to many diverse conditions of fact. Its fitness consists in being the same which the hearer of the expository words would spontaneously assume if yielding to the same emotions, and which, therefore, by association tends to induce sympathetic yielding. But the facts themselves depend upon the words uttered. A true sign-language would express the exact circumstances with or without any exhibition of the general emotion appropriate to them.

It is necessary to be free from the vague popular impression that some oral language of the general character of that now used by man is "natural" to man. There is no more necessary connection between ideas and sounds, the mere signs of words that strike the ear, than there is between the same ideas and signs for them which are addressed only to the eye. Early concepts of thought were of direct and material characters, as is shown by what has been ascertained of the radicals of language, and there does not seem to be any difficulty in expressing by gesture all that could have been expressed by those radicals.

It will be admitted that all the higher languages were at some past time less opulent and comprehensive than they are now, and as each particular language had been thoroughly studied, it had become evident that it grew out of some other and less advanced form. The discussion of philological subjects at the present day was varied by the suggested possibility that man at some time might have existed without any oral language. A proof of this assumption lay in the fact that uninstructed deaf mutes originated signs from time to time expressive of their wishes and ideas.

The doctrine of Archbishop Whately and Max Müller, that deaf mutes could not think until after instruction, was combated. No one now doubts that the deaf mute thinks after instruction either in gesture signs or in the finger alphabet, or more lately in visible speech. By this instruction he has become master of a new and foreign language, but that he obtained from signs. But no one can learn a foreign language unless he had one of his own, whether by descent or acquisition, by which it could be translated, and such translation could not even be commenced unless the mind had been already in action, and intelligently using the original language for that purpose. In fact the use by deaf mutes of signs originating in themselves shows a creative action of mind and innate faculty of expression beyond that of speakers who acquired language without conscious effort.

It may be conceded that after man had all his present faculties he did not choose between the adoption of voice and gesture, and never with those faculties was in a state where the one was used to the absolute exclusion of the other. The epoch, however, to which the present speculations relate is that in which he had not reached the present symmetric development of his intellect and of his bodily organs, and the inquiry is, Which mode of communication was earliest adapted to his simple wants and informed intelligence? With the voice he could imitate distinctively but few sounds of nature, while with gesture he could exhibit actions, motions, positions, forms, dimensions, directions, and distances, with their derivations and analogues. It would seem from this unequal division of capacity that oral speech remained rudimentary long after gesture had become an efficient mode of communication. With due allowance for all purely imitative sounds, and for the spontaneous action of vocal organs under excitement, it appears that the connection between ideas and words is only to be explained by a compact between speaker and hearer which supposes the existence of a prior mode of communication. This was probably by gesture. At least we may accept it as a clue leading out of the labyrinth of philological confusion, and regulating the immemorial quest of man's primitive speech.

SCIENTIFIC SERIALS

Verhandlungen des naturhistorischen Vereines der Preussischen Rheinlande und Westfalens, 1881. Zweite Hälfte.—We note here the following:—On some Anthozoa of the Devonian, by Prof. Schlüter.—The Stromatopora of the Rhenish Devonian, by Herr Bargatsky.—Geological sketch of a journey through Palestine and the Lebanon region, by Prof. von Rath.—On the building art of birds, reduced to its true value, by Prof Landois.—The beetle genus *Bruchus*, Linn., and especially *Bruchus pisorum*, Linn., by Herr Cornelius.—On new finds of saurian tracks in the Wealden Sandstone of the Bückeberg, by Herr Grabbe.—The Royal Mercury Works at Idria, by Herr Fabricius.—The zinc ore deposits of Wiesloch, by Herr von Decken.—Bone-remains from the Schipka Cave in Moravia, by Prof. Schaaffhausen.—Removal of an iron fragment from the eyeball with an electromagnet, by Dr. Samuelsohn.—Skulls from Kirchheim, by Prof. Schaaffhausen.—Influence of the use of transportable pneumatic apparatus on the circulation of a healthy man, by Prof. Finkler.—On a colossal femur of the horse, found in January, 1880, when removing part of a bank of the Wupper at Elberfeld, by Prof. Schaaffhausen.—On so-called cosmic dust from Dresden, by Prof. von Lasaulx.—New apparatus for continuous application of weak galvanic currents, by Prof. Finkelnberg.—On the earthquake of Ischia, March 4, 1881, by Prof. von Rath.—On eruptive gneiss in Saxony and Bavaria, by Dr. Lehmann.—Nerve-stretching; three cases, by Prof. Doutrelepont.

SOCIETIES AND ACADEMIES

London

Aëronautical Society, July 17.—A paper, upon the action of the pectoral muscle in the flight of a bird, was read by Mr. Fred. W. Brearey. He said that it behoved all experimenters in flight to reduce their theories into a demonstrable form. It had often been stated for instance that the power exerted by a bird in its flight had been greatly exaggerated, but no one had hitherto proved his assertion. It was capable however of satisfactory proof by demonstrating artificially the action of the pectoral muscle, by the aid of which weight became an accessory to power. When the bird committed itself to the air the upward pressure in the wings stretched the elastic ligament, which formed part of the muscle, to such an extent as to allow of the bird gliding upon the air without any exertion. The weight of the bird was the measure of this elasticity. It was said by some that at least the bird must possess the power by the downward stroke of the wing to raise its own weight. But Mr. Brearey said that this was not an absolute necessity, because the reaction of this elastic ligament aided the force of the down stroke. He proceeded to verify his assertion by the action of a model, with wings of four feet spread, under which he had attached an elastic cord passing under the body of the model. Upon committal to the air this just allowed of the wings being expanded, so that the model would glide downwards. He then detached the cord and wound up his power, calling attention to the fact that he had wound the india-rubber strands thirty-two times. He showed however that although this was sufficient to create a vigorous flapping of the wing when held in the hand, yet when committed to the air it had not the power to give one downward stroke, and therefore it could only glide as before. Holding it again with the cord attached and the power wound up the same number of times, he showed that it was unable to flap the wing, because the two forces were exactly held in equilibrium. There was a third factor wanted before it could fly—and that was weight. The model being liberated, flight was well sustained, and upon being set free several times without being wound up any further, it appeared able to fly with a very weak power. The same thing was observable with another model, composed entirely of a loose surface thrown into a wave action—his own invention. Mr. Brearey remarked that this economy in flight can only be obtained by something of the nature of wing action, and must be wholly wanting in any apparatus actuated by the screw.

Edinburgh

Royal Society, July 17.—Prof. Balfour, vice-president, in the chair.—Prof. Heddle read a paper on the sequence of rocks in the North-West Highlands, a point on which there had been and still was a great deal of controversy. The author had examined eighteen sections in the region around and to the north of Loch Maree, and had convinced himself that Murchison and Geikie were in the main correct. The succession of the rocks was found to be as follows:—Torridon Conglomerates, Lower Quartzite, Dolomite Series, "Logan" Rock, Upper Quartzite, Upper Gneiss. The dolomite does not extend so far west as the quartzite and Logan Rock, and is of no great lateral extent, but it stretches as a thin strip of shallow water deposit from end to end of the whole district.—Prof. Tait communicated a paper by Mr. Wm. Peddie on the rotation of plane of polarisation by quartz and its relation to wave-length. The spectrum of a ray of light which has been transmitted through the polariser, a piece of quartz, and the analyser, exhibits one or more absorption bands (the number depending upon the thickness of the quartz), which move along the spectrum as the analyser is rotated. By direct comparison of this spectrum with the ordinary solar spectrum in juxtaposition, the rotation for any Fraunhofer line can be estimated with considerable accuracy. The rotations were expressed in terms of the inverse even powers of the wave-lengths as far as the sixth.—Mr. W. W. J. Nicol, in a paper on the condition of ammonium salts when dissolved in water, explained the abnormal expansion of solutions of ammonium chloride and other ammonium salts by the partial dissociation on solution in water—an explanation suggested by the well-known fact that such salts become acid on boiling. This view of the matter seemed further to explain other anomalies in the behaviour of ammonium chloride solution—such for example as its surface tension investigated by Quincke, and its coefficient of absorption for carbon dioxide as determined by Mackenzie.—Mr. J. Y. Buchanan described a new form of solar calorimeter which he had used in Upper Egypt at the time of the last eclipse. The sun's rays were concentrated by suitable reflectors upon a glass tube, two inches long, which formed the upper end of a Liebig's condenser, and was mounted equatorially so as to follow the sun's motion. The heat was measured by the amount of water distilled in a given time. The results obtained were very satisfactory, agreeing with the results given by other methods.—Prof. Crum Brown read a continuation of the paper by Messrs. Laurie and Burton, on the heats of combination of the metals with the halogens, estimated from electromotive force observations. Their result for the heat of combination of zinc with iodine in the presence of water differed by barely 2 per cent. from Andrews' value. Other results did not agree so well; but this was hardly surprising where so many factors entered into the experiments. The most accurate method was no doubt to let a chlorine, iodine, or bromine cell with given poles run down in a calorimeter and estimate the heat so given out.—Professor Brown also communicated a long paper by Mr. W. L. Goodwin, on the nature of solution, in which the author made a careful investigation into the solution of chlorine in various liquids at different temperatures. Experiment showed that there was in many cases a temperature of maximum solubility, a fact which Mr. Goodwin explained as due to the formation at lower temperatures of a chlorine hydrate whose rate of increase of solubility with increase of temperature quite masked the simultaneous decrease of solubility of the gas until a temperature was approached at which the chlorine hydrate could no longer exist.—The second part of the description of new and little-known phanerogamous plants from Socotra, by Prof. Bayley Balfour, was received as read.—The chairman, in bringing to a close the hundredth session of the Society, gave a brief review of the session's work.

Berlin

Physiological Society, July 14.—Prof. Du Bois Reymond in the chair.—Dr. Friedländer spoke *à propos* of a paper by Dr. Baginski at the last meeting, on the cells of the stomach-wall, and presented some microscopical preparations. Dr. Brösicke gave a summary report on the results of his investigation of normal bone-tissue. With a very favourable preparation, a bone 200 years old, he could explain the nature of the "bone-corpuscles" observed in fresh bones, for he was in a position to inject them from the Haversian canals with a coloured mass. Thereby was proved the existence of lacunæ, which, by their outrunning parts, communicate with the Haversian canals. The entire bone-traversing system of cavities, lacunæ, their outrunners, and the canals, are inclosed in a proper skin, the limiting membrane, which Dr. Brösicke was able to isolate and investigate chemically. The limiting membrane hereupon showed reactions, which essentially distinguish it from the intercellular substance, and which entirely agree with the reactions of horn-tissue; it was therefore named the "Keratin-layer." The contents of the lacunæ are very different in different stages of development of the bone. In the embryo, the lacunæ are quite filled with protoplasm; later, the protoplasm retires from the intercellular substance, and a distinct interval between the latter and the protoplasm-cell can be observed; at this stage, probably, arises the "keratin-layer." At a further stage of development, the contents of the lacunæ are transformed into fat, the cells of which abundantly fill the cavities. The fat cells then fall asunder into detritus, which is gradually dissolved, so that the lacunæ remain empty, or, as the author supposes, filled with a gas, probably carbonic acid. The proper lime-containing bone-substance consists of fibres of the nature of connective tissue, which are bedded in lines in different directions, make up the layers of bone-material, and are held together by a structureless lime-containing cement-substance. This structure of the lime containing bone-tissue has been described before, and Dr. Brösicke has merely been able to confirm former data; but what is specially to be noted as new, among the results of the inquiry, is the demonstration of a limiting membrane clothing the entire system of cavities, and its keratin-like character.

Vienna

Imperial Academy of Sciences, July 6.—W. Biedermann, on the morphological changes of the lingual glands of the frog by stimulation of the glandular nerves.—H. Hammerl, on rainbows formed by liquids of different index of refraction.—F. Streintz, experimental researches on galvanic polarisation (first part).—R. Prescher, on the mucous organs of Marchantieæ.—G. Schmidt, on the internal pressure and energy of superheated steam.—S. Mayer, studies on the histology and physiology of the vascular system (preliminary communication).—T. V. Tanowsky, on the nitro-derivates of azobenzene-parasulphonic acid.—T. Kajaba, a contribution to the theory of polar planimeters used in practice.—F. Kreuter, a sealed packet with the inscription "On a new process of preservation of railway-sleepers."—T. Holletschek, on the orbit of the planet Ate (111).—G. Vortmann, on a new method for the direct determination of chlorine besides bromine and iodine.—Zd. H. Skraup, synthetical experiments on the chinoline series (part 4).—Zd. H. Skraup and G. Vortmann, on the derivates of dipyridyl.

Paris

Academy of Sciences, July 24.—M. Jamin in the chair.—The following papers were read:—New researches on the propagation of explosive phenomena in gases, by MM. Berthelot and Vieille. They study the behaviour of a great variety of mixtures, and find a very fair agreement between the theoretical velocity and that observed. The velocity of translation of the gaseous molecules, keeping all the kinetic energy which corresponds to the heat liberated, may be considered as a limit representing the maximum velocity of propagation. This velocity is diminished by contact of gases and other foreign bodies, also when the gas inflamed at first is too small and too quickly cooled by radiation, also when the elementary velocity of the chemical reaction is too weak (as with carbonic oxide).—Separation of gallium, by M. Lecoq de Boisbaudran. This relates to separation with cobalt, nickel, and thallium.—Dilator sympathetic nerves of vessels of the mouth and the lips, by MM. Dastre and Morat.—Theory of the diurnal motion of the axis of the earth, by M. Folie. He finds a diurnal precession and nutation which are far from insignificant and may become sensible to observation for circumpolars, even supposing the earth solid in the interior.—M. Faye made some remarks on Tom. I. of the Annals of the Observatory of Rio de Janeiro, sent by the Emperor of Brazil. — Observations of solar spots and faculæ, at the Royal Observatory of the Roman College, during the first half year of 1882, by M. Tacchini. The spots showed a secondary minimum in January, both in frequency and in size. There was increase till April, then rapid diminution. On no day were spots absent. The maximum will probably occur this year. The faculæ were pretty numerous from the first.—Latitudes of groups of solar spots in 1881, by M. Ricco (see Notes).—On the orbit of Japhet, by Mr. A. Hall.—Rapid solution of the problem of Kepler, by M. Zenger.—On the chemical work produced by the battery, by M. Tommasi.—The chromic acid couple as used by Favre (positive electrode platina) produces an exterior chemical work equal to about 65 calories. Substituting for the platina, carbon or spongy platina, one may get 20 calories more (*i.e.* about 85 calories).—On the variation of friction produced by voltaic polarisation, by M. Krouchkoll. He has found that polarisation by oxygen increases the friction, while polarisation by hydrogen diminishes. He describes his apparatus.—On the amplitude of telephonic vibrations, by M. Salet. On the iron plate of a Bell telephone were fixed two small glass discs giving Newton's rings. On speaking loudly to the telephone at 5m. or 6m. distance, the rings lose distinctness and disappear. To estimate the displacement by a continuous sound, a disc with slits was rotated before the instrument; with a certain velocity the rings return; and on then blowing through the disc, the sound proves to be in unison with that of the telephone. The amplitude of vibration of the telephone plate was estimated at two to three ten-thousandths of a millimetre.—Researches on the use of crusher-manometers, &c. (continued), by MM. Sarrau and Vieille. With the same density of charge, the maximum pressure of picrate of potash and dynamite are shown to differ considerably, though with one piston they had nearly the same crushing force.—Reproduction of calcite and of witherite, by MM. Miron and Bruneau.—On the vaporisation of metals in vacuo, by M. Demarçay. This was effected at comparatively low temperatures; the volatility of cadmium was proved at 160°, zinc at 184°, antimony and bismuth at 292°, lead and tin at 360°. The deposits in 24 to 48 hours were weighable (5 to 15 mgr.).—On the determination of astringent matters in wine, by M. Girard. He employs catgut, utilising its tendency to combine with those matters.—Law of congelation of benzenic substances in neutral substances, by M. Raoult. Acetones, aldehydes, ethers, hydrocarbons, and their derivatives, dissolved in a given weight of benzine in quantities proportional to their molecular weights, all lower the freezing-point of this liquid the same number of degrees.—Means of artificially conferring immunity against symptomatic or bacterian charbon, with attenuated virus, by MM. Arloing, Cornevin, and Thomas. — On Lieberkuehnia, a multinucleate rhizopod of fresh water, by M. Maupas.—On the fossil flora of Tong-King coal, by M. Zeiller.—New researches (physiological and therapeutical) on globularine, by MM. Heckel, Mourson, and Schlagdenhauffen. Globularetine is the purgative principle, and in a leaf-decoction the action is greater, on account of associated mannite.

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