THURSDAY, AUGUST 29, 1895.

SIR SAMUEL BAKER AND NORTHERN AFRICA.

Sir Samuel Baker: a Memoir. By T. Douglas Murray and A. Silva White. 8vo. Pp. xii. 447, with six illustrations and nine maps. (London: Macmillan and Co., 1895.)

North Africa. Stanford's Compendium of Geography and Travel. (New series). *Africa.* Vol. i. By A. H. Keane. 8vo. Pp. xvi. 639, with seventy-seven illustrations and nine maps. (London: E. Stanford, 1895.)

A SUMMARY of our present knowledge of Northern Africa, and a memoir of the late Sir Samuel Baker, may be appropriately considered together, for Baker's main title to fame rests on the work he did in that region; and had his experience been properly utilised, the most interesting part of it might not have been lost to civilisation and closed to scientific inquiry.

Samuel White Baker came of an old Devonshire family, members of which have done good work for their country since the time when Sir John Baker served Henry VIII. as Attorney-General, Chancellor of the Exchequer, and Speaker of the House of Commons. Baker was born in London on June 8, 1821, and spent most of his early life at Enfield. He was destined for a commercial career, and in 1842 placed in his father's office in Fenchurch Street. But the work was utterly uncongenial to him. His marriage kept him quiet for a time, but not for long; for next year he gave up business and went to Mauritius, where the family had estates. In 1846 he went for a shooting expedition to Ceylon, and was so impressed by the possibilities of the island, which then had a very bad reputation, that he resolved to found a colony in it. In 1848 he led a party of settlers to Newera Eliya, where 1000 acres of land had been bought from the Government. This was cleared, and a settlement made. Baker remained there till 1855, and during his stay did a good deal of big-game shooting. In 1856 his wife died, and as he had previously lost three of his children, he became very depressed, and actually resolved to enter the Church. This scheme came to nothing, and Baker accepted instead the post of manager of the Dobruscha Railway, the construction of which had been just begun. This kept him busy in 1859 and 1860, and raised in him the keen interest he afterwards felt in the Eastern question. It was in the next year, when Baker was forty years of age, that he resolved on an expedition into Africa to try to meet Speke (whose sister had married Baker's father) and Grant, and carry out some explorations to supplement theirs. In order to gain experience of the people and to learn the languages required, he made a preliminary excursion up the Atbara to some of the Abyssinian sources of the Nile. He left Khartum on his main expedition on December 18, 1862, reaching Gondokoro in the following February. Here he met Speke and Grant, who returned northward in Baker's boats, while he and his heroic wife continued their journey southward along the Nile valley, and through Unyoro till they reached the Albert Nyanza at Bakovia. The discovery of this lake was the greatest achievement of the expedition; but it was only the accident of the condition of the weather, that robbed them of the discovery of the snow-clad peaks of Ruwenzori. They had reached a point whence, in clear weather, the mountain ought to have been as visible "as St. Paul's dome from Westminster Bridge," as Stanley said. They returned to Europe in 1865, and in 1869 went back to the Soudan on an expedition to suppress the slave trade. Baker had all a Devonshire Quaker's horror of this trade. The view that slavery was a kind of secondary larval structure, necessary in a certain stage of national progress, and later on to be absorbed or thrown off, was not then recognised. Baker simply regarded it as an unholy thing, which was to be crushed by any means or at any cost. He accordingly went for it with the pluck of a bull-dog, and just about as much judgment. He was given a commission to go to the Soudan to break up the gangs of slave raiders. He had an independent command, but could do little of permanent value without the assistance of his colleague, the Governor of Khartum; but this worthy official, as well as Baker's native assistants and the supreme authorities in Cairo, all believed in the slave trade in theory, and carried it out in practice. Ismail Pasha alone seems to have been sincere, and not to have endeavoured to thwart the efforts he was ostensibly supporting. Thanks, however, to Baker's indomitable pluck and energy, and his tact with the men, this Quixotic expedition was carried through with a certain measure of success. Its commander alone benefited much by it, for he secured a great reputation as a leader of men, and learnt better to understand both the Soudan and the slave trade. He returned to Europe in 1873, recognising the futility of trying to effect a social revolution over several millions of square miles by shooting a few score of the agents in a trade, of which the principals lived unpunished in Cairo and Khartum. He realised that the only useful course was to improve the industrial conditions, so as to render slavery unnecessary. Had Baker been sent back to the Soudan, and allowed to work on these lines, the subsequent revolt might have been avoided. But the task was entrusted to other hands, and unfortunately Gordon's peculiar genius was less successful with Mohammedan fanatics than it had been with the stolid Chinese.

After Baker's return he settled at Sandford Orleigh in Devonshire, where he lived till his death, except that every winter he made expeditions to some warmer clime. He was always ready, like a knight-errant of old, to rush forth to relieve the inhabitants of some village on the Brahmapootra from the tigers that preyed upon them. He was fond of sport to the last; even after he had become too unsteady to be a match for anything worse than the worn-out old tigers who have had to turn "man-eaters."

The story of Baker's life is pleasantly told, and even in less competent hands could not have failed to be interesting. The editors have wisely left Baker to relate most of it by quoting copious extracts from his letters. Explanatory chapters help the reader to understand the condition of African geography at the time of his journeys, and to appreciate the relative importance of his work. These chapters seem to be judicious and well informed. Our main regret is that we do not hear enough of Baker as a sportsman and a naturalist. One chapter is devoted to this, but we doubt if it does full credit to Baker's work in this field. His valuable contributions to natural history are barely referred to; his important services to gunnery and his improvements in cartridges are not mentioned. We should have been glad to have seen more space devoted to this, at the cost of condensation of the political writings, some of which are hardly likely to add to his reputation. For when we remember the conditions under which he shot, the clumsy old muzzle-loaders and the badly-mixed powders he used, and the accuracy and fulness of his observations upon the habits of animals, we cannot but reckon Baker as the greatest of English sportsmen.

While Baker's memoir gives an account of the political conditions of the Soudan from 1860 onward, Prof. Keane's admirable summary of the present knowledge of North African geography completes the sketch in other departments. He divides North Africa into six divisions, viz. the Atlas (including Morocco, Algiers and Tunis), the Sahara, the Soudan and the Niger Basin, Egypt and Nubia, and Italian North-East Africa (including Abyssinia and Somaliland). Each of these districts is described separately, an account being given of its general physical geography, of its history, as far as this is known, of its ethnography, and natural history. The ethnographical sketches are especially well done, while the political histories are the most detailed. The natural history is the least satisfactory part of the book. The geology is mostly quoted second-hand, or is taken only from geographical instead of from geological papers. Some of the botanical records are certainly quite untrustworthy, as when on p. 533 *Casuarina* is reported on the banks of the Webi Shebeyli, whereas it occurs only on the ends of the promontories on the eastern coasts. The nine maps are admirably clear, while full of information. The volume is in every way a great improvement on the preceding editions. The immense increase in the material to be summarised, has made the task a difficult one. This enormous growth of knowledge applies, however, to five out of the six districts described. It is only in one that progress has been stopped, and of which the new edition has nothing fresh to report, except paper delimitations in Europe and reaction in Africa. All Junker's collections, the greatest ever made in the equatorial provinces of Egypt, were lost by the closing of the Soudan. It is to be hoped, however, that European officials will not much longer prohibit our representatives in the field from taking action, and again opening to progress the lands where Gordon's death and Baker's life-work added their names to the roll of our national heroes. J. W. G.

BIO-OPTIMISM.

The Evergreen. A Northern Seasonal. Published in the Lawnmarket of Edinburgh by Patrick Geddes and Colleagues. (London: Fisher Unwin, 1895.)

IT is not often that a reviewer is called upon to write art criticism in the columns of NATURE. But the circumstances of the "Evergreen" are peculiar; it is published with a certain scientific sanction as the expression of a coming scientific Renascence of Art, and it is impossible to avoid glancing at its æsthetic merits. It is a semi-annual periodical emanating from the biological school of St. Andrews University. Mr. J. Arthur Thomson assists with the proem and the concluding article ("The Scots Renascence"), and other significant work in the volume is from the pen of Prof. Patrick Geddes. It may be assumed that a large section of the public will accept this volume as being representative of the younger generation of biological workers, and as indicating the æsthetic tendencies of a scientific training. What injustice may be done thereby a glance at the initial Almanac will show. In this page of "Scots Renascence" design the beautiful markings on the carapace of a crab and the exquisite convolutions of a ram's horn are alike replaced by unmeaning and clumsy spirals, the delicate outlines of a butterfly body by a gross shape like a soda-water bottle; its wings are indicated by three sausage-shaped excrescences on either side, and the vegetable forms in the decorative border are deprived of all variety and sinuosity in favour of a system of cast-iron semi-circular curves. Now, as a matter of fact, provided there is no excess of diagram, his training should render the genuine biologist more acutely sensitive to these ugly and unmeaning distortions than the average educated man. Neither does a biological training blind the eye to the quite fortuitous arrangement of the black masses in Mr. Duncan's studies in the art of Mr. Beardsley, to the clumsy line of Mr. Mackie's reminiscences of Mr. Walter Crane, or to the amateurish quality of Mr. Burn-Murdoch. And when Mr. Riccardo Stephens honours Herrick on his intention rather than his execution, and Mr. Laubach, rejoicing "with tabret and string" at the advent of spring, bleats

> "Now hillock and highway
> Are budding and glad,
> Thro' dingle and byway
> Go lassie and lad,"

it must not be supposed that the frequenters of the biological laboratory, outside the circle immediately about Prof. Patrick Geddes, are more profoundly stirred than they are when Mr. Kipling, full of knowledge and power, sings of the wind and the sea and the heart of the natural man.

But enough has been said of the artistic merits of this volume. Regarded as anything more than the first efforts of amateurs in art and literature—and it makes that claim—it is bad from cover to cover; and even the covers are bad. No mitigated condemnation will meet the circumstances of the case. Imagine the New English Art Club propounding a Scientific Renascence in its leisure moments! Of greater concern to the readers of NATURE than the fact that a successful professor may be an indifferent art editor, is the attempt on the part of two biologists—real responsible biologists—writing for the unscientific public, to represent Biology as having turned upon its own philosophical implications. Mr. Thomson, for instance, tells his readers that "the conception of the Struggle for Existence as Nature's sole method of progress," "was to be sure a libel projected upon nature, but it had enough truth in it to be mischievous for a while." So zoologists honour their greatest! "Science," he says, has perceived "how false to natural fact the theory was." "It has shown how primordial, how organically imperative the social virtues are; how love, not egoism, is the motive which the final history of every species justifies." And so on to some beautiful socialistic sentiment and anticipations of "the dominance of a common civic ideal, which to naturalists is known as a Symbiosis." And Prof. Geddes writes tumultuously in the same vein—a kind of pulpit science—many hopeful things of "Renascence," and the "Elixir of Life."

Now there is absolutely no justification for these sweeping assertions, this frantic hopefulness, this attempt to belittle the giants of the Natural Selection period of biological history. There is nothing in Symbiosis or in any other group of phenomena to warrant the statement that the representation of all life as a Struggle for Existence is a libel on Nature. Because some species have abandoned fighting in open order, each family for itself, as some of the larger carnivora do, for a fight in masses after the fashion of the ants, because the fungus fighting its brother fungus has armed itself with an auxiliary alga, because man instead of killing his cattle at sight preserves them against his convenience, and fights with advertisements and legal process instead of with flint instruments, is life therefore any the less a battle-field? Has anything arisen to show that the seed of the unfit need not perish, that a species may wheel into line with new conditions without the generous assistance of Death, that where the life and breeding of every individual in a species is about equally secure, a degenerative process must not inevitably supervene? As a matter of fact Natural Selection grips us more grimly than it ever did, because the doubts thrown upon the inheritance of acquired characteristics have deprived us of our trust in education as a means of redemption for decadent families. In our hearts we all wish that the case was not so, we all hate Death and his handiwork; but the business of science is not to keep up the courage of men, but to tell the truth. And biological science in the study still faces this dilemma, that the individual in a non-combatant species, if such a thing as a non-combatant species ever exist, a species, that is to say, perfectly adapted to static conditions, is, by virtue of its perfect reactions, a mechanism, and that in a species not in a state of equilibrium, a species undergoing modification, a certain painful stress must weigh upon all its imperfectly adapted individuals, and death be busy among the most imperfect. And where your animal is social, the stress is still upon the group of imperfect individuals constituting the imperfect herd or anthill, or what not—they merely suffer by wholesale instead of by retail. In brief, a static species is mechanical, an evolving species suffering—no line of escape from that *impasse* has as yet presented itself. The names of the sculptor who carves out the new forms of life are, and so far as human science goes at present they must ever be, Pain and Death. And the phenomena of degeneration rob one of any confidence that the new forms will be in any case or in a majority of cases "higher" (by any standard except present adaptation to circumstances) than the old.

Messrs. Geddes and Thomson have advanced nothing to weaken these convictions, and their attitude is altogether amazingly unscientific. Mr. Thomson talks of the Gospel of the Resurrection and "that charming girl Proserpina," and Baldur the Beautiful and Dornröschen, and hammers away at the great god Pan, inviting all and sundry to "light the Beltane fires"—apparently with the dry truths of science—"and keep the Floralia," while Prof. Geddes relies chiefly on Proserpine and the Alchemy of Life for his literary effects. Intercalated among these writings are amateurish short stories about spring, "descriptive articles" of the High School Essay type, poetry and illustrations such as we have already dealt with. In this manner is the banner of the "Scots Renascence," and "Bio-optimism" unfurled by these industrious investigators in biology. It will not appeal to science students, but to that large and important class of the community which trims its convictions to its amiable sentiments, it may appear as a very desirable mitigation of the rigour of, what Mr. Buchanan has very aptly called, the Calvinism of science. H. G. WELLS.

THE GLYPTODONT ORIGIN OF MAMMALS.

Studies in the Evolution of Animals. By E. Bonavia, M.D. (London: Constable, 1895.)

IN his preface the author writes that: "Having completed the 'Flora of the Assyrian Monuments and its Outcomes,' I was looking about for something to take up next as a subject of study. In the furriers' windows I was attracted by the leopard and tiger skins, which by degrees became objects of interesting study and speculation." In the true interests of zoology, it is to be deplored that his attention was not attracted by some other subject.

The key-note to the startling theory propounded in this volume is to be found in a sentence on page 131, where it is stated that: "The Glyptodonts, or other armoured animals of a similar nature, were the *originals* from which all existing mammals, including marsupials, descended."

This astounding statement is largely based on the belief that the rosettes on the skins of the jaguar and leopard are the remnants of the rosette-sculpture on the bony carapace of the glyptodonts, the author stating (p. 124) that these markings "are *inherited* from ancestral plate-impressions of some extinct glyptodontoid form, and have *not* been evolved by a process of natural selection."

How the author can conceive that the *Felidæ* are descended from any glyptodont-like form (by which it may be presumed an edentate is meant) will pass the comprehension of any anatomical zoologist; but all will endorse his remark (p. 163) that "one would indeed require to have lived a good bit of time to witness a Glyptodon changing into a Jaguar." This, however, is by no means all. Later on the author finds evidence of glyptodont affinities in the bosses on the skin of Rhinoceroses, and remarks (p. 217) that "the giant armadillo has its hind feet ungulate, its hoofs are almost exactly like those of the Malayan Tapir; and in some rhinoceroses the incisor teeth are wholly wanting, and that part of the jaw is contracted, not unlike that of the Glyptodon." If this means anything, it means that rhinoceroses are evolved from a veritable edentate glyptodont; and it is thus a pity the author did not enlighten us how the full dentition and claws of a jaguar were also to be derived from such a type.

It would be mere waste of space to state how marsupials enter the scheme, but it may be mentioned that the loss of the primeval carapace of ordinary mammals is attributed (p. 209) to a deficiency of carbonate of lime in the water and plants on which they subsisted. It will also be a surprise to zoologists to learn (p. 142) that the coloration of the Indian black-buck is due to its having lost its armour on the ventral sooner than on the dorsal surface. And equal wonderment will be experienced when they read (p. 300) that dolphins are near relatives of Plesiosaurs, and that the author doubts whether "there are any good reasons for supposing that Ichthyosaurs were *not* mammals"!

In another chapter the author is led, from the study of monstrosities, to the conclusion that horses are more nearly allied to the Artiodactyla than they are to either rhinoceroses or tapirs!

Many more similar instances might be quoted, but it will suffice to say that if the author be right, all zoologists are hopelessly in the wrong in their views on mammalian affinity.

Among the redeeming features in the book will be found many interesting observations on the coloration of cats and horses, and the author appears to have made out a fairly good case for the derivation of the striping of the tiger from the spots of a leopard-like type. Many of the figures of animals, especially the skins of leopards, are admirable examples of photography, and would be well worth reproduction in other works.

R. LYDEKKER.

OUR BOOK SHELF.

Le Cause Dell' Era Glaciale. By Luigi de Marchi, Libero Docente di Meteorologia nella R. Universita di Pavia. (Pavia: Fratelli Fusi.)

THIS work does not fulfil the expectations raised by its title. It is a prize essay of 220 large octavo pages, divided into three sections. The first treats of the climatic conditions of a glacial invasion, and here the author agrees with a number of German writers whom he quotes, in considering that a glacial epoch is due to a lowering of mean animal temperature and a diminution of the annual range, accompanied by an increased rainfall in summer. The next section treats of the temperature of the air. We find a large collection of empiric formulæ, taken for the most part from German authors, some of which are based on assumptions which appear to be far from satisfactory, and which certainly cannot be verified in the exhaustive way which one would wish before applying them to find the temperature in the Glacial Age. Among these there is one more important than the others, in which *t*, the mean annual temperature at any given locality, is expressed in terms of no less than *fifteen* physical quantities, such as the supposed temperature of an ideal sky,[1] the absolute radiating power of this sky, the transmissive powers of the atmosphere for radiation from earth and water, and for sun-heat, and last, but not least important, "a term of correction which expresses the effect of the physical and meteorological condition of the locality," and this term may, according to the author, oscillate between $-6°$ C. and $+6°$ C.

The third section, entitled "The Cause of a Glacial Age," contains the author's deductions from this formula. He uses it to disprove the hypothesis that the Ice Age was due to a change in the obliquity, but he cannot apply it to discuss Croll's theory, because it only takes account of the *total* annual heat received. Hence he refers to previous writers for his criticism on Croll. Similarly the geographical hypothesis is dismissed as insufficient, so that the way is cleared for the author's own hypothesis, viz. that the Ice Age was caused by a general lowering of temperature which arose from a diminution of the atmospheric transparency, which can only be explained (p. 183) as the effect of a general diffusion into the atmosphere, over the whole surface of the earth, of a gas, vapour, or dust which absorbs, or reflects towards space, a part of the heat which comes from the sun. "But since the glacial epoch also presupposes an extraordinary rainfall, among the many hypotheses which may be framed, one spontaneously presents itself, viz. that a great mass of aqueous vapour was launched against and diffused into the atmosphere." Owing to the lowering of temperature due to want of transparency, the vapour would fall as snow, and this precipitation would go on until the mass of vapour injected into the atmosphere is entirely or in great part eliminated.

The author quotes an Italian writer, who suggests that the action of volcanos in the age preceding the Ice Ages affords a possible explanation of the (supposed) launching of these vast masses of aqueous vapour into the atmosphere.

[1] "Not Ferrel's hypothetical temperature of space, but (following Pouillet, Frölich, and Preuter) the temperature of an ideal surface, of which the radiating power is equivalent to that of the whole atmosphere, and of all the celestial bodies, except the sun." This temperature is taken as equal to $-45°\cdot4$ C. for all parts of the globe, the poles as well as the equator.

Leitfaden für histologische Untersuchungen. By Bernhard Rawitz. Second edition. (Jena: Gustav Fischer, 1895.)

HISTOLOGICAL methods have become so perfected, microscopic appliances so modified, and staining reagents so numerous, that it is necessary to have good reference books for use in laboratories. Although there are a number of such works, amongst which we may mention Lee's "Vade Mecum," Sims Woodhead's "Manual," and Fletcher's edition of Von Kahlden's "Practical Pathological Histology," the appearance of a new edition of Rawitz's compendium will be welcomed by all who were familiar with the first edition, which was published six years ago. It resembles Von Kahlden's book in arrangement, but while this latter has been compiled specially for pathological investigations, Rawitz's "Leitfaden" is essentially intended for the biologist and physiologist, and forms a suitable supplement to its morbid counterpart. When reviewing Dr. Fletcher's translation of Von Kahlden's book, some time back, we regretted the omission of various matters relating to section-cutting, embedding and staining, an omission which is excusable on the ground that in a work on practical pathological histology a sound knowledge of these subjects might be taken for granted. Rawitz gives excellent descriptions of all our recognised modern methods, and a careful account of paraffin embedding and paraffin cutting, which will prove useful to all who wish to become familiar with what is undoubtedly the best method for general histological purposes. His directions for working with celloidin are equally good, and since this method is somewhat neglected in this country the beginner will find a number of hints which Dr. Fletcher might well have included in his translation. The completeness with which the various methods of fixation, hardening, and staining have been enumerated is admirable, and we gain the firm conviction that the author has only included what is sound, and in careful hands certain to give good and trustworthy results. Chapter xi. (part 1) contains some useful information on the art of drawing and "reconstructing" microscopical objects. The "Leitfaden" may be recommended without hesitation to the histologist as a book of reference for use in the laboratory: it will save time, and seldom cause disappointment.

A. A. K.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The University of London.

I AM anxious to make it clear that what Sir John Lubbock has sprung upon us is a radical change in the procedure of Convocation.

The object can only be, it appears to me, to obtain a reversal of its policy. As a political expedient it is, therefore, very similar to the action of those politicians who for analogous reasons would change the constitution of the House of Lords.

Sir John now defines what he calls his "suggestion" in the following words:—"That in voting on the new Charter, members of Convocation should do so 'as at a senatorial election,' *i.e.* by voting papers." I call this a radical change in the procedure of Convocation.

I put aside the not immaterial point that as a Statutory Commission is a delegation from Parliament, the result of its labours will not be embodied in a Charter, but will be virtually in effect an Act of Parliament when approved by that body.

Sir John has made the following statements about his "suggestion":—

(1) "I am not asking that any privilege which they do not at present possess should be conferred upon my constituents, but only supporting what is now their *legal right* . . . This right I know they highly value" (NATURE, July 18, p. 269).

(2) "It is the *law* at present" (NATURE, August 8, p. 340).

The words which I have put in italics are definite and explicit, and are, of course, in flat opposition to my repeated statement that Sir John's suggestion amounts to a fundamental and, indeed, revolutionary change of procedure. This change consists in extending the mode of voting in a senatorial election to other matters. Now the mode of voting at a senatorial election is prescribed by the 21st clause of the Charter, which is printed in NATURE for July 25, p. 296. It embraces two very important points. First, the right of absent members to vote at all is not absolute but only *permissive*. The words are: "Power to the Convocation, *if it shall think fit*, to enable absent members of the Convocation to vote on such nominations . . . by voting papers." Secondly, this permissive right is strictly limited by the words "*but not so to vote on any other matter*."

It is upon this vital discrepancy between Sir John's statements quoted above and the provisions of the Charter that I think it is imperative that he should give some explanation. This demand on my part he is pleased to call an "attack." Well, however that may be, he at least owes it to himself to meet it.

I trust, however, that I have now made it clear, and even to Sir John, that his "suggestion" is not the law, but that, further, it involves the abrogation of a portion of the Charter. I think as a member of Convocation that in making such a proposal without consulting that body he has exceeded his functions as our Parliamentary representative. At any rate it must, I think, be admitted that he is making short work of the "right" which his "constituents highly value." (NATURE, August 8, p. 340.)

I am unwilling to prolong a painful discussion. But as Sir John is pledged to bring forward his "suggestion" in Parliament, which of course can incorporate it in the Bill, if it thinks proper, it seems to me of extreme importance to dissipate his contention that it is already the "law." W. T. THISELTON-DYER.

Kew, August 23.

The Nomenclature of Colours.

THE interesting article of Mr. J. H. Pillsbury, published in your last number, recalls to me a passage in my autobiography, which, though it is already in print, will not be issued until after my death. As bearing on the question Mr. Pillsbury raises, this passage may, perhaps with advantage, be published in advance. The plan suggested aims at no such scientific nicety of discrimination or naming as that he proposes, but is one which is applicable with the means at present in use. It is, as will be perceived, based on the old theory respecting the primary colours; but whatever qualification has to be made in this, need not affect the method described. The passage is as follows:—

"I mention it here chiefly for the purpose of introducing an accompanying thought respecting the nomenclature of colours. The carrying on of such a scheme would be facilitated by some mode of specifying varieties of tints with definiteness; and my notion was that this might be done by naming them in a manner analogous to that in which the points of the compass are named. The subdivisions coming in regular order when 'boxing the compass,' as it is called, run thus:—North, north by east, north-north-east, north-east by north, north-east; north-east by east, east-north-east, east by north, east. Applying this method to colours, there would result a series standing thus:—Red, red by blue, red-red-blue, red-blue by red, red-blue (purple); red-blue by blue, blue-red-blue, blue by red, blue. And in like manner would be distinguished the intermediate colours between blue and yellow and those between yellow and red. Twenty-four gradations of colour in the whole circle would thus have names; as is shown by a diagram I have preserved. Where greater nicety was desirable, the sailor's method of specifying a half-point might be utilised—as red-red-blue, half-blue; signifying the intermediate tint between red-red-blue and blue-red by red. Of course these names would be names of pure colours only—the primaries and their mixtures with one another; but the method might be expanded by the use of numbers to each: 1, 2, 3, signifying proportions of added neutral tint subduing the colour, so as to produce gradations of impurity.

"Some such nomenclature would, I think, be of much service. At present, by shopmen and ladies, the names of colours are used in a chaotic manner—violet, for instance, being spoken of by them as purple, and other names being grossly misapplied. As matters stand there is really no mode of making known in words, with anything like exactness, a colour required; and hence many impediments to transactions and many errors. In general life, too, people labour under an inability to convey true colour-conceptions of things they are describing. The system indicated would enable them to do this, were they, in the course of education, practised in the distinguishing and naming of colours. If, by drawing, there should be discipline of the eye in matters of form, so there should be an accompanying discipline of the eye in matters of colour."

Were some authoritative body to publish cards representing these various gradations of colour, arranged as are the points of the compass, each division bearing its assigned name, as above given, such cards might serve as standards; and any one possessing them would be able to indicate, within narrow limits, to a shopkeeper or manufacturer, the tint he or she wanted. Of course to complete the method it would be needful that there should be a mode of indicating gradations of intensity, and if the numbers 1, 2, 3, were appended to indicate the degrees of impurity by mixture with neutral tint, *a*, *b*, *c*, might be used to signify the intensity or degree of dilution of the colour.

Very possibly, or even probably, this idea has occurred to others, for it is a very obvious one. HERBERT SPENCER.

The Mount, Westerham, July 23.

Clausius' Virial Theorem.

THE above-named theorem, which appeared in the *Phil. Mag.* for August 1870, much as it is now used in connection with the kinetic theory of gases, received little, if any, attention in England for some time after its introduction. Apparently the theorem was accepted without hesitation or discussion, and, as far as I can learn, neither on its first introduction or since has it received any adverse criticism, or, in fact, any criticism whatsoever. My object in writing this letter is, in the first place, to direct attention to the arguments used by Clausius to establish his theorem, which appear to me to be unsound, and secondly, by applying a simple test case, to show that the theorem itself is not true.

Clausius first proves the following equation.

$$\frac{m}{4t}\int_0^t \frac{d^2(x^2)}{dt^2}dt = \frac{m}{2t}\int_0^t x\frac{d^2x}{dt^2}dt + \frac{m}{dt}\int_0^t \left(\frac{dx}{dt}\right)^2 dt.$$

If for the moment, for the sake of simplicity, we divide both sides of the equation by $\frac{m}{2t}$, we get

$$\frac{1}{2}\int_0 \frac{d^2(x^2)}{dt^2}dt = \int_0^t x\frac{d^2x}{dt^2}dt + \int_0^t \left(\frac{dx}{dt}\right)^2 dt,$$

and this may be written

$$ux = \int_0^t xdu + \int_0^t udx.$$

In this form it is easy to see that each term may be graphically represented by an area, and the equation simply expresses the fact that the rectangular area xu is equal to the algebraic sum of the areas $\int_0^t u dx$ and $\int_0^t x du$. It is obvious that for periodic motion the rectangle xu will vanish when a suitable value is given to t; but so also will the areas $\int_0^t u dx$ and $\int_0^t x du$. So that when $xu = 0$ we get, either

$$\int_0^t u dx = 0 \text{ and } \int_0^t x du = 0; \text{ or } \int_0^t u dx = -\int_0^t x du.$$

Again, in what Clausius calls "stationary motion" when xu does not vanish periodically, although we can make the expression $\frac{m}{2t} ux$ vanishingly small, by taking t very great, it is obvious that if the areas $\int_0^t u dx$ and $-\int_0^t x du$ are not equal before multiplying them by $\frac{m}{2t}$, the expressions so obtained are not so afterwards. Moreover, and finally, it should be observed that the expression $m\int_0^t u dx$ *does not represent kinetic energy*; to represent which the expression should be $m\int_0^t u du$. The above considerations seem to me to entirely upset Clausius' demonstration.

In the tenth edition of Maxwell's "Heat" (p. 323), Lord Rayleigh has given an illustration of the manner in which he supposes the "virial" to act in opposition to kinetic energy, and we may take his illustration as a simple test of the theorem. He supposes two bodies, each of mass m, to revolve in a circular path with a constant velocity about their centre of gravity. Here, as there is no pressure, the so-called virial equation takes the form

$$\Sigma \tfrac{1}{2} m v^2 = \tfrac{1}{2} \Sigma R r.$$

In the above equation v, the velocity, is constant, and $R = mf$. If we take ρ as the radius of the circle, then $r = 2\rho$, and the equation becomes

$$\tfrac{1}{2} v^2 \Sigma m = \tfrac{1}{2} \times 2\rho f \Sigma m.$$

Hence

$$\tfrac{1}{2} v^2 = \rho f;$$

which equation *does not represent the ordinary law of centrifugal force*. Lord Rayleigh omitted to notice that

$$\Sigma R = \Sigma m f = f \Sigma m = 2mf.$$

When, however, we throw overboard all ideas of "virial," and look upon the term $\tfrac{1}{2}\Sigma R r$ in the so-called "virial equation" as simply representing work and equal to $\tfrac{3}{2} p V$, also an expression for work, then the equation

$$\Sigma \tfrac{1}{2} m v^2 = \tfrac{3}{2} p V + \tfrac{1}{2} \Sigma R r$$

is certainly true. But there seems no possible advantage to be obtained in splitting the right-hand member into two equal terms, instead of writing the equation

$$\Sigma \tfrac{1}{2} m v^2 = 3pV; \text{ or } \Sigma \tfrac{1}{2} m v^2 = \Sigma R r;$$

in either of which forms—the first for preference—it is applicable to ideal gases. For natural permanent gases the equations become, either

$$\Sigma \tfrac{1}{2} \beta m v^2 = 3pV; \text{ or } \Sigma \tfrac{1}{2} \beta m v^2 = \Sigma R r,$$

and not

$$\Sigma \tfrac{1}{2} \beta m v^2 = \Sigma \beta R r,$$

as given in my letter (p. 221) on "Argon and the Kinetic Theory." C. E. BASEVI.

London, W., August 14.

Incubation among the Egyptians.

ARTIFICIAL incubation, like many another practice supposed to be peculiar to modern civilisation, is but a revival from very ancient times. Diodorus, an author who wrote about forty years before the commencement of the Christian era, tells how the Egyptians of his time, with their own hands, bring eggs to maturity, and how the young chickens thus produced are not inferior in any way to those hatched by the usual means.

The practice, probably with methods differing little from those of ancient times, survives to the present day among the fellahs of Egypt. In suitable places ovens are erected, and the proprietors go round the neighbouring villages collecting eggs. A sufficient number having been collected, they are placed on mats strewed with bran, in a room about 11 feet square, with a flat roof. Over this chamber, which is about 4 feet high, there is another built about 9 feet in height. The roof, which is vaulted, has a small aperture in the centre to admit light during the warm weather; below it another opening of larger dimensions communicates with the oven below. In the cold weather both are kept closed, and a lamp is kept burning within. Entrance is then obtained from the front of the lower chamber. In the upper room fires are made in troughs along the sides, and the eggs are placed on the mats below in two lines, corresponding to and immediately below the fires. The fires are lighted twice a day, the first time to die about midday, the second to last from about 3 p.m. to 8 p.m. The first batch of eggs are left for about half a day in the warmest situation, after which they are moved to make room for others, until the whole number in hand have had the benefit of the position. This is repeated for six days. Each egg is then examined by a strong light. All eggs that at this stage are clear are rejected, but those that are cloudy or opaque are restored to the oven for another four days. Then they are removed to another chamber, where there are no fires, but the air is excluded. Here they lie for five days, after which they are placed separately, about one or two inches apart, and continually turned. This last stage generally takes six or seven days. During this time a constant examination is made by placing each egg to the upper eyelid, when a warmth greater than that of the human skin is a favourable sign. The duration of the process generally extends over twenty-one days, but thin-shelled eggs often take only eighteen days. The average heat required is 86° F. Excessive heat is prejudicial. In Egypt the best time is from February 23 to April 24.

J. TYRRELL BAYLEE.

Mountain Sickness.

I HAVE just come back from a journey in the region of the Andes, and in looking over the numbers of NATURE, which had accumulated during my absence, I came across the extract, which you make in your notes of February 21, from the *Revue Scientifique*, on the subject of mountain sickness. I cannot agree with M. Kronecker's statement that beyond three thousand metres mountain sickness attacks all persons as soon as they indulge in the least muscular effort, as I made the acquaintance of many people, mostly railway men, living and working at altitudes of fourteen or fifteen thousand feet on the Oroya line and the Southern Railway of Peru, who had never experienced *soroche*, or mountain sickness. As far as my own experience goes, in three journeys across the Andes and several mountain ascents, including one to the top of the crater of the Misti, 19,300 feet above sea level, I had only one attack of *soroche*, and that was at the end of a ride on an oil engine from sea level to fourteen thousand feet in nine hours. But this was so complicated with suffocation by the oil fumes and scorching by the heat of the furnace while running through the fifty-seven tunnels on the line, that I cannot say how much was mountain sickness and how much was not. At any rate, I was perfectly well the next morning, and rode over a pass nearly seventeen thousand feet high without the slightest inconvenience. As regards the danger of a prolonged sojourn, my experience teaches me that it is almost entirely due to personal idiosyncrasy and unwise eating and drinking. A healthy person whose lungs and heart are all right, who does not over-eat and is very moderate in the use of stimulants, will not suffer from mountain sickness after the first few hours, and in many cases will not suffer at all if the ascent is sufficiently gradual. Of course very violent exertion produces distress by reason of the deficiency of oxygen. I do not think that there need be any difficulty about the officials of the proposed Jungfrau railway, if steady men, not of a full habit of body, are selected. I never heard of any trouble from mountain sickness among the Peruvian railway men unless they over-stimulated, and yet they are accustomed to go in a day from sea level to 15,764 feet on the Oroya line, and to 14,666 feet on the Southern line, and return to sea level on the following day. I may add that I have made both these journeys myself without the slightest inconvenience, and have been able to walk and ride without any trouble at the end of them.

London, August 20. GEORGE GRIFFITH.

How was Wallace led to the Discovery of Natural Selection?

THE reviewer of Osborn's "From the Greeks to Darwin" (*antea* p. 362) says that Marshall quotes the fact of Wallace's being led "to the discovery of natural selection as he lay ill of intermittent fever at Ternate," and refers one to the abridged form of the "Life and Letters of Charles Darwin" for this statement. Having only the original edition in three volumes, from the year 1887, at my disposal, wherein I cannot find it, I would draw attention to my having published the fact as far back as 1870 ("Charles Darwin and Alfred Russel Wallace. Ihre ersten Publicationen über die Entstehung der Arten, nebst einer Skizze ihres Lebens und einem Verzeichniss ihrer Schriften." Erlangen, E. Besold, 8vo, pp. xxiii. and 56, on page xviii.) The remarks to be found there are based upon a letter of Mr. Wallace's dated November 22, 1869, and now before me, a passage of which runs thus:—

"The paper No. 9 ['on the law which has regulated the introduction of new species' A.N.H. 1855] should be read along with No. 19 ['on the tendency of varieties to depart indefinitely from the original type' P.L.S. 1858]. When I wrote it I was firmly convinced of the derivative origin of species, but had not arrived at an idea of the process. When I wrote No. 19 at Ternate [in the year 1858] I did not [know] what were Mr. Darwin's views or the nature of the work he was engaged on, except generally that it was on 'Variation.' I hit upon the idea of 'Natural Selection' (though I did not give it that name) while shivering under the cold fit of ague, and I was led to it by Malthus' views on population applied to animals. As soon as my ague fit was over I sat down, wrote out the article, copied it, and sent it off by the next post to Mr. Darwin. It was printed without my knowledge, and of course without any correction of proofs. I should, of course, like this fact to be stated."

This I did in my pamphlet of 1870 on the page quoted, and on page 39, and I hope Dr. Wallace will forgive me for now making known the whole of his highly interesting statement *in his own words*. Of course I am not sure whether he did not tell or write the same to some one else, though I am not aware that it has been published.

Ordinary mortals dream nonsense in their fits of fever, a philosopher of Dr. Wallace's standing conceives original ideas!

A. B. MEYER.

Zoological Museum, Dresden, August 19.

THE letter to Prof. Newton, published in the abridged "Life of Darwin," was written in 1887. I had entirely forgotten that I had written on the same subject to Dr. Meyer in 1869, or that he had published anything in reference to it. That letter probably contained my earliest statement on the subject, and it agrees substantially with my later statements.—A. R. WALLACE.

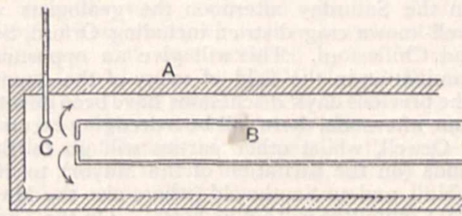
A Problem in Thermodynamics.

SIEMENS taught us how, by using the heat of the gases escaping from a furnace to heat the gas and air before entering the furnace, we could obtain temperatures limited only by the fire-resisting quality of the materials of which the furnace is constructed. Now, it occurred to me whether on the same principle very low temperatures might not be reached. My idea is this: If compressed air is expanded to atmospheric pressure, the gas does work in overcoming the resistance of the atmosphere, and is cooled to a corresponding amount.

Suppose, for instance, the gas is compressed to 1/100 of its volume, then 1 cubic metre would perform, in expanding against the atmospheric pressure of 1 kil. per 1 square centimetre, or 10,000 kilos per square metre, an amount of work equal to $10,000 \times 0{\cdot}99 = 9900$ kilgr.-metres, and absorb $\frac{9900}{424}$ units of heat. Now, 1 cubic metre of air weighs 1·24 kil., and, having a specific heat of 0·24, the temperature of the expanded air would be lower 78° than before expanding.

Now suppose A is a tube of a material impervious to heat—that is, a perfect non-conductor—and B a tube made of a perfect conductor of heat; the tube A being closed at one end, and B having a small opening in the end.

Now, if a continuous supply of compressed air is kept up in tube B, this air will come down in temperature, and, passing along between A and B, cools the compressed air before it expands.



I should be glad if any of your readers could give me the theoretical minimum of temperature produced at C.

Essen-Ruhr, Germany. E. BLASS.

A Remarkable Flight of Birds.

ON September 30, 1894, about 3 p.m., I was observing the sun through an 8-inch telescope. I noticed some dark figures of birds passing, like shadows, across the sun. I was using a dark glass, and the birds were, consequently, only visible when seen against the bright solar disc. The silhouettes of the birds were very sharply and clearly cut. Every few seconds a bird would emerge from the darkness, pass slowly across the sun and disappear on the other side. I watched them for over ten minutes without any decrease in their numbers. The whole number of birds must have been enormous, otherwise it would have been impossible for some of them to have passed as frequently as they did between my telescope and the sun. The birds were flying in a southerly direction, and were quite invisible to the naked eye. I was, therefore, unable to determine their distance, but should think they must have been two or three miles away, for the telescope was in focus for the birds and sun at the same time. I do not know what birds they were. Comparing the spread of their wings with the solar disc, I should say their wings subtended an angle of about two minutes. The place from which I observed them was Shere, a village between Guildford and Dorking. I am told that such a flight of birds has not before been recorded in this country, and have been urged to publish an account in the hope that other astronomers, who may have seen a similar thing, may be led to mention the fact.

Shere, Guildford. R. A. BRAY.

THE IPSWICH MEETING OF THE BRITISH ASSOCIATION.

IN our last article we gave a general outline of the local arrangements for the Meeting. The programme, as a whole, is now fairly complete. A slight alteration has been made with reference to the soirées; the first will be given by the Ipswich Scientific Society and the Suffolk Institute of Archæology jointly, and the second by the Mayor of Ipswich (Mr. J. H. Bartlet,). The fitting up of the Section Rooms is proceeding rapidly, and arrangements are being made for the darkening of those in which a lantern will be used. In the case of Sections A and B, which meet in the same building, only the room allotted to Section B will be fitted up with dark blinds and a lantern screen, and the Sections will be asked to exchange rooms on days when papers requiring lantern illustration are read in Section A. The same arrangement will be made as to Sections D and K, which meet in the two rooms at the Masonic Hall. For the President's address in these Sections, the Lyceum Theatre, which is a short distance off, will be placed at the disposal of the Sectional Committees, as the Masonic Hall rooms may be hardly large enough to contain all those who would probably wish to be present on these particular occasions. For a similar reason, Section G, which meets in the Co-operative Hall, will be asked to allow the President's addresses in Sections A and B to be delivered there. A spacious room adjoining the main street, and within two minutes' walk of the reception room, will be set apart for a ladies club-room.

The excursions will be of a more varied character than usual. On the Saturday afternoon the geologists will visit the well-known crag district, including Orford, Sudbourne, and Chillesford. This will give an opportunity for the examination in the field of many of the deposits to which the previous days' discussions have been devoted. On the same afternoon, there will be a dredging excursion down the Orwell, whilst other parties will go to Bury St. Edmunds (on the invitation of the Mayor), to Helmingham Hall, and to Southwold (where also the Mayor and a Local Committee will act as hosts). On the Thursday afternoon after the meeting, there will be another dredging expedition, and also an excursion to Colchester (on the invitation of the Mayor), to the Flint Napping Works at Brandon, and to the Broads, on which occasion the party will be entertained *en route* by the Mayor of Yarmouth. The geologists on this day will go to the Norfolk coast to examine the Glacial and Pliocene deposits in the neighbourhood of Cromer, where arrangements will be made so that those, who wish, may stay the night. Other short afternoon excursions will be made near Ipswich whenever time allows.

The programme of work in the Sections is rapidly filling up. In Section A, the President, Prof. W. M. Hicks, will take as the subject for his address, "The Fluid Theories of Ether and Matter." On the Friday a joint sitting will be held with Section B, when Prof. A. Schuster will open a discussion, in which Lord Rayleigh and Mr. Crookes are expected to take part, on the evidence to be gathered as to the simple or compound character of a gas from the constitution of its spectrum. On the same occasion, Captain W. de W. Abney and Mr. C. H. Bothamley will read papers on orthochromatic photography. There will also be important discussions in Section A, on the question of a new practical unit of heat, introduced by a paper from Mr. E. H. Griffiths, and on the objective character of combination tones, opened by Prof. Rücker. Other papers to be read in the Section will be on the teaching of geometrical drawing in schools, by Prof. O. Henrici, on the electrification and diselectrification of gases, by Lord Kelvin and Messrs. Maclean and Galt, on vertical (earth-air) electrical currents, by Prof. Rücker, on the events that go on within molecules, by Dr. Johnstone-Stoney, on the velocity of light in a rarefied gas through which a current is passing, by Messrs. Edser and Starling, on a dynamical top, by Mr. G. T. Walker, and on Boltzmann's minimum theorem, and the question of reversibility in the kinetic theory of gases, by Mr. E. P. Culverwell.

In Section B, the President, Prof. R. Meldola, will deal in his address with the relations of physiology and chemistry. The Monday will be devoted chiefly to papers dealing with the relation of chemistry to agriculture, which are already anticipated locally with considerable interest, on account of the large stake the district has in agriculture. Prof. Warington will be amongst those to read papers on the question. The Tuesday will be given up to papers on organic chemistry.

In Section C, the address of the President, Mr. Whitaker, will be devoted to the subterranean geology of the Eastern Counties, as exhibited in various deep borings and wells. Mr. Whitaker will also have a paper on the latest results in the boring for coal, now being made at Stutton. The other papers on local questions will probably deal mainly with newer Tertiary geology; Ipswich being a capital centre for the study of our Pliocene and Pleistocene deposits. Besides the local papers, communications have been promised from certain of the foreign visitors, on the correlation of our British Tertiary deposits with their continental equivalents. A paper by M. Gustave Dollfus, of Paris, on the extent of the Tertiary seas of Western Europe, will give his views of the physiography of the south and east of England in Pliocene times, and is likely to lead to some discussion. Glaciation, as was to be expected at Ipswich, will occupy a good deal of time. Prof. Sollas will exhibit the "pitch-glaciers," by which he has produced in the laboratory many of the obscurer phenomena of glaciation. Mr. Robert White communicates a paper on the glaciation of tropical South America.

Of the miscellaneous communications likely to be brought forward, we can only mention a few. Mr. Joseph Francis, the engineer to the New River Company, will have one on the method adopted to ascertain the direction of the dip in the Palæozoic rocks met with in the deep borings at Ware and Cheshunt. It may be observed that while there is no difficulty in obtaining the amount of the dip, when a solid core is brought up, it has always been a difficult problem how to obtain the far more important data as to its direction. Papers are also expected from Prof. Nicholson and Mr. Marr, on the phylogeny of the graptolites; from Messrs. Garwood and Marr, on zonal divisions of the Carboniferous system; from Mr. T. V. Holmes, on the ancient physiography of South Essex; from Messrs. Reid and Ridley, on the Arctic and Palæolithic deposits at Hoxne. Others, on American palæontology, have been promised by Profs. Claypole and Marsh.

Section D meets this year under the presidency of Prof. W. A. Herdman, and, for the first time in the history of the Association, it will be a section of zoology alone. Botany now forms a separate section, and although physiology is nominally attached to Section D for this meeting, it will in fact be unrepresented. The work of Section D will be largely devoted to questions of marine fisheries and marine zoology. On the Friday of the meeting, Prof. McIntosh will open a discussion on fishery questions, and an interesting debate is expected. Prof. Haddon will read a paper on the Royal Dublin Society's Fishery Survey; Dr. Bashford Dean, of New York, will give a paper on apparatus for catching oyster spat and its failure in practice, and will also exhibit an interesting collection of eggs and larvæ; Prof. Herdman will give an exhibition of lantern slides illustrative of fishery problems, and will explain the method of "zoning" of shores, &c., and, in conjunction with Prof. Boyce, will give a paper on oysters and typhoid. Other papers will be read by Prof. Miall, on pupation in insects; by Prof. Ritter, of New York, on budding in Tunicata; by Prof. Lloyd Morgan, on experiments on instinct in young birds; by Dr. H. O. Forbes, on the Antarctic continent, and on seals; and by Dr. Otto Maas, of Munich, Prof. Gilson, of Louvain, Prof. Howes, Mr. Moore, Mr. Hoyle, Dr. Hurst, and others on various subjects.

The following is the provisional programme for Section G:—Thursday, 12.—Address by the President, Prof. Vernon Harcourt; light railways in agricultural districts, by Major-General Webber; congelation of soil for foundation purposes, by M. Gobert; Bentley coal borings (a local work), by R. C. Rapier. Friday, 13.—The growth of the port of Harwich, by W. Birt; notes on improvement of Maas in connection with Hook of Holland route, by the President; Snowdon tram-road, by Sir Douglas Fox; notes on autumn floods of 1894, by W. H. Symons; river weirs and flood prevention, by F. G. M. Stoney. Saturday, 14.—Dredging operations at Mersey Bar, by A. G. Lyster; carbonic anhydride refrigerating machinery, by E. Hesketh; deodorising sewage by Herzite process at Ipswich, by J. Napier.—Monday, 16, will be devoted to electrical papers, among which will be the following:—Induction telegraphy, notes on further advance, by W. H. Preece; glow lamps, by W. H. Preece;

modern applications of electricity to traction, by P. Dawson; the chloride battery, by W. H. Earle; extension and development of the telephone in agricultural districts, by Major-General Webber; telephony, by A. R. Bennett; the field telegraph in Chitral campaign, by P. V. Luke; a new portable photometer, by W. H. Preece and A. P. Trotter. Tuesday, 17.—Interim report of committee on standardising; modern flour-milling machinery, by F. W. Turner; paper-making machinery, by Mr. Mason; printing without use of movable types, by J. Southward; incandescent gas lamps, by C. Cooke; B.A. Standard small screws, by R. B. Compton; uniform factor of safety in steam boilers, by J. Key.

The provisional programme for Section H is as follows:—Thursday, September 12.—Address by Prof. Flinders Petrie; skulls of the aborigines of Jamaica, by Sir W. H. Flower; skulls of the Neolithic invaders of Egypt, by Dr. J. G. Garson; Andamanese, by Morris Portman; Neolithic invaders of Egypt, by Prof. Flinders Petrie. Friday, September 13.—Worked flints from South Africa, by H. W. Seton Karr; flint and metal working in Egypt, by Prof. Flinders Petrie; flints found at Thebes, by Gen. Pitt Rivers; plateau flints of North Kent, by B. Harrison; graving tools from terrace gravels of the Thames valley, by H. Stopes; Palæolithic projectiles, by the same; megaliths of Tripoli, by Swainson Cooper; kitchen midden at Hastings (report), by W. J. Lewis Abbott. Saturday, September 14.—North-west tribes of Canada (report), by Prof. E. B. Tylor; Samoyedes of the Arctic tundras, by A. Montefiore; language illustrating primitive warfare, by Rev. Hartwell Jones; ethnographical survey (report), by E. Sidney Hartland; deviations of children (report), by Dr. Warner. Monday, September 16.—Cannibalism, by Captain Hinde; folk-lore of Ipswich, by Miss Layard; ethnographical conclusions, by G. Laurence Gomme; general conclusions, by Edward Clodd; folk-lore illustrated, by Prof. Haddon; religious origin of dances, by Mrs. Grove. Tuesday, September 17.—On interference with the civilisation of other races, by Lord Stanmore, Prof. Douglas, Prof. Haddon, and Dr. R. N. Cust, and letters of the late R. L. Stevenson; southern Arabians, by Theodore Bent; the Eskimo, by F. Linklater and J. A. Fowler. Wednesday, September 18.—Lake village of Glastonbury (report), by Dr. R. Munro; prehistoric Greek idols, by Arthur Evans; Neolithic station of Butmir, by Dr. R. Munro.

A SOUVENIR OF "CHALLENGER" WORK.

A MEDAL has been prepared as a souvenir of the scientific work connected with the *Challenger* expedition. The medal, which is in bronze, is three inches in diameter, and was modelled by Mr. Birnie Rhind, sculptor, from designs by Mr. William S. Black, both of Edinburgh. It was cast in Paris, and is being presented by Dr. John Murray to the naval officers of the expedition, the contributors of memoirs to the report on the scientific results of the expedition, and to members of the civilian scientific staff, as a souvenir of *Challenger* work.

The accompanying illustrations have been reproduced from two photographs of the casts forwarded to us by Mr. Black, and show the two sides of the medal. On the front of the medal, the head of Athena with owl occupies the centre, and is placed on the globe, which in turn is surrounded by a border of water indicating the voyage of the expedition around the world. Out of the water rises Neptune, with trident and a trawl disclosing the treasures of the deep-sea. The decoration of the border is completed with a dolphin and two mer-

maids supporting a ribbon with the words "Voyage of H.M.S. *Challenger*, 1872–1876."

The back of the medal bears the crest of the *Challenger*—a mailed warrior throwing down the gauntlet to Neptune, whose trident appears above the waves. This central figure is surrounded by a scroll bearing the words, "Report on the Scientific Results of the *Challenger* Expedition, 1886–1895." The name of the recipient of each medal is engraved around the edge.

It is hardly necessary to say that the medal has been very much appreciated, and appears to have been received with special satisfaction by foreign contributors to the *Challenger* Report, who regard it as a pleasing recognition of their assistance in the great work which has now been completed.

DR. FRIEDRICH W. G. SPÖRER.

IN a recent number of NATURE we unfortunately had to record the loss of an astronomer, Dr. Friedrich Tietjen, who devoted himself to computation, or, we should say, to that branch of astronomy which deals with the methods of calculation, and with the reduction of the observations themselves.

It is our lot to-day to say a few words about another hard worker in astronomical science, whose end has followed too soon after that of Dr. Tietjen. This devoted student of astronomy has been an energetic observer in the same degree that Dr. Tietjen was an ardent computer. We refer to Dr. Friedrich Wilhelm Gustav Spörer, the former chief assistant of the Astro-Physical Observatory at Potsdam, and who died on July 7 last.

Dr. Spörer was born in Berlin on October 23, 1822, and after spending some time at the Friedrich-Wilhelms Gymnasium, he entered the University of Berlin, making mathematics and astronomy his chief studies. On December 14, 1843, he gained his doctor's degree, the subject of his thesis being the comet of 1723. In the following years he worked under Encke's direction at the Berlin Observatory, and in 1846, after having made his Staats exam., went as a teacher of mathematics and natural science to the Gymnasium at Bromberg. In 1847 he proceeded to Prenzlau, and two years later to Anclam, at which latter place he taught for twenty-five years, and became eventually Pro-rector.

It was during his leisure hours there that Dr. Spörer was able to turn his attention to astronomical observations, his instrumental equipment being of a very inferior kind. Notwithstanding this hindrance, he was able, however, by great diligence and perseverance, to make useful observations with regard to the statistics of the solar spots, which have made his name known to every worker of solar physics. Through the attention of Prof. Schellbach, who was the teacher of the then Crown Prince Friedrich Wilhelm, afterwards Kaiser Friedrich, Dr. Spörer was equipped with a good 5-inch telescope, with which he continued to make his solar observations by the known method of projection. His Anclam observations appeared from time to time in numerous articles contributed to the *Astronomischen Nachrichten*, and also in two larger papers which came out in the years 1874 and 1876 in the *Publicationen der Astronomischen Gesellschaft*. The chief value of these pieces of work lies in the careful determination of the elements of rotation of the sun, and also in the more accurate settlement of the then empirically known law of Carrington, namely, the decrease in the velocity of rotation of the sun-spots according to increase of solar latitude.

In the year 1868, accompanied by Prof. Tietjen and Dr. Engelmann, Dr. Spörer took part in the astronomical expedition to observe the total eclipse of the sun visible in the East Indies. Six years later (1874) he received the appointment as observer at the Potsdam Astro-Physical Observatory, and in the same year continued his solar observations from the top of the tower of the Military Orphan Asylum, until the completion of the observatory.

There Dr. Spörer, with untiring energy and with the same ardour that he displayed in Anclam, did a great amount of work in collecting data on the subject of sun-spots. The publications of the Astro-Physical Observatory (years 1879–1894) contain four valuable papers by him, giving a rich quantity of accurate observations that will remain a classical work for the study of the proper motion of the solar spots.

In 1882 Dr. Spörer became chief assistant, and this position he held until October 1894, when he retired for a well-earned rest.

From Dr. Spörer's observations of solar spots, the most important deductions that have been made may be summed up as follows:—

(1) That the period of rotation of the apparent surface of the sun about the axis, is not the same for every part.

(2) That the velocity of the spots is greater nearer the equator than further away from it, and that this velocity can be approximately represented by a formula.

(3) That the variation in latitude is periodical, and that there are two series of spots. We learn thus that the true sun-spot cycle is one extending over twelve to fourteen years, and that another begins in high latitudes before the former has ceased.

(4) His observations of the quantity of spotted area between the years 1856–1880, show a length of period of eleven years, this being the time between two consecutive maxima.

The maximum is reached when the mean latitude of the spots is about 16° north and south. A retreat then takes place from about 30° to 16°, that is, 14° in four years, and a further retreat from 16° to 8°, that is, 8° in eight years; or, in other words, we get a change of latitude of over 3° a year to begin with, and one of ½° a year to end with.

Such results as these, which have here only been briefly summarised, are of fundamental importance, and form valuable data for those attempting to investigate the conditions of atmospheric circulation at the surface of our sun. Since the observations have been made consecutively by such a diligent observer, and extend over a considerable period of time, they are strictly of a uniform nature, and in consequence they are comparable *inter se*.

Happy in his work, and endowed with a strong constitution, Dr. Spörer was free from the ailings of old age up to his last day. It was when on a journey to visit his children that he was suddenly seized with paralysis of the heart, without ever having had any previous sign of illness, and died quietly and without pain.

His loss not only affects the astronomical world, but his large circle of friends, all of whom will mourn deeply such a sudden and unexpected bereavement.

W. J. S. L.

NOTES.

Considerable activity has been displayed at the Plymouth laboratory of the Marine Biological Association during the present summer, and general satisfaction has been experienced by the naturalists who have visited the station for the purpose of research. Progress has been made with the series of dredging operations in the outlying grounds of the neighbourhood. The unsettled weather of the past two months has been a somewhat unfavourable condition in these expeditions; but it is expected that these operations may be carried on regularly and with increased success during the autumn months. The following naturalists have occupied tables at the laboratory during the summer: Prof. Weldon, F.R.S., Mr. G. P. Bidder, Mr. W. Garstang, Mr. T. H. Riches, Dr. Albrecht Bethe, Mr. W. J. Beaumont, Mr. Gilchrist, and others.

A well-marked earthquake disturbance was felt at Zermatt on Wednesday, August 21. Many houses were severely shaken.

Among the deaths of eminent scientific men abroad, we notice the name of Dr. F. Hoppe-Seyler, professor of physiological chemistry in Strassburg University, and also that of Dr. S. Moos, professor of otology in Heidelberg University.

We regret to record the death of Dr. J. S. Bristowe, F.R.S., whose work on the "Theory and Practice of Medicine" is recognised as a classic, while his other contributions to scientific literature give him a high place among medical worthies. Dr. Bristowe had filled the offices of President of the Medical Society, of the Pathological Society, and of the Neurological Society. He was elected into the Royal Society in June 1881.

THE Assistant Clerk to the Geological Society, Mr. F. E. Brown, died suddenly on Sunday, August 4. The Society loses in him an invaluable official, who was ever rigid in the exact performance of all his duties, and combined with strict business-like habits a courtesy and patience which endeared him to his colleagues and to the Fellows generally.

THE eleventh Congress of Americanists will be held in the City of Mexico, on October 15-20. The meeting has for its principal object the progress of ethnographical, linguistic, and historical studies of the two Americas, especially with reference to the period prior to the discovery of the New World. Among the matters which will be discussed at the forthcoming gathering are the following:—The relations existing between different American peoples before the discovery; maps of the Atlantic and Pacific Oceans in the sixteenth century; medical natural history of the Ancient Mexicans; public instruction in Mexico in early times, and from the conquest of Mexico to the middle of the sixteenth century; mines and metallurgy before the conquest of Mexico; interpretation of the symbolic dances of the Azetics; different forms of arrows and their use among the natives of Central America; recent researches with regard to the first appearance of man in America; relationships between the Esquimaux and other native races of North America; prehistoric man in Mexico; the stone carvings in Central America; the pottery of Nicaragua and Costa Rica; the chronological classification of the monuments of Mexico and Central America; the human inhabitants of caves and grottos; Indian hieroglyphics; names of animals in the native languages of Central America; the decipherment and comparison of the hieroglyphics of ancient races of Mexico; the use of hieroglyphic writing since the conquest of Mexico, and the importance of its study in connection with the Mexican and Mayan languages. The President of the Congress is Sr. J. Baranda, and the Secretary, Sr. T. S. Santos, to whom all memoirs and other communications should be addressed at the Bibliothèque Nationale, Mexico.

DURING the latter part of last week the area of high barometric pressure that had prevailed over the greater part of the British Islands gave way to small disturbances, which either approached from the Atlantic, or were formed immediately over this country, causing severe thunderstorms over England and Ireland, while lightning was also visible in Scotland. In the storm of Thursday night (22nd inst.) the lightning was extremely brilliant in London, the flashes during part of the time being almost continuous. Considerable quantities of rain fell in many localities, and in some of the English districts much damage was done by hail.

THE problem solved by Edison's kinetoscope has been successfully attacked along a different line by MM. A. and L. Lumière. The film which in the kinetoscope takes the impressions of moving objects is passed before the eye with a continuous motion, and it is only illuminated for about a 7000th of a second at the instant at which each successive picture is fully in view. Hence the total illumination is exceedingly feeble. A very bright object is necessary; the eye has to be brought close to the moving film, and the number of impressions per second must be at least thirty in order to give continuity. MM. Lumière's "kinematograph," which is not subject to these disadvantages, is described in the *Revue Générale des Sciences*. The principal features of this instrument are a mechanism whereby the film is at rest during illumination, and an arrangement for projecting the images upon a screen, so as to be visible to a large meeting. Under these circumstances, fifteen images per second are all that is necessary. The film is at rest for two-thirds of the time of passage of each image. During the remaining third the film is grasped and pulled forward as far as the next image by a set of teeth attached to a frame whose motion is governed by a cam worked by a revolving handle. The same apparatus also serves as a camera for taking the photographs, and for printing transparencies from the negative film. For this purpose two films are passed over the rollers, the negative and the film to be printed on, and exposure is made for a very short time as each negative image is placed in the field. An exhibition was given on July 11, at the offices of the *Revue Générale des Sciences*, at which the evolutions of cuirassiers, a house on fire, a factory, street scenes, and a dinner-party were shown on the screen, and were much admired.

A NUMBER of observations referring to a shower of dust in connection with snow in Indiana and Kentucky, are brought together and discussed in the *Monthly Weather Review*. The dust does not appear to have been the nuclei of snowflakes, but was intermingled in the air with the snow, and fell during an interval between two snow-storms. An examination of numerous samples showed that the dust was made up largely of silt, mixed with organic matter. A number of freshwater algæ were distinguished, though they appear to have been dead and dried for some time. There were also groups of diatoms, fungi, animal and plant hairs, fibres of grasses, shreds of woody tissue of some shrub or tree, and many other objects in the samples examined. Everything indicated that the material came from the bottom of some dried-up lake, pond, or marsh, or some river-bottom. To afford information upon the belief that this fine material is very valuable as a fertiliser, an examination of the dust was made from that point of view. The analyses showed that the material is no better fertiliser than any other good surface soil. The dust was almost identical with the so-called "loess" formation, which covers very extensive areas in Illinois, Indiana, Nebraska, and other adjoining States; its depth in some places amounting to a hundred feet or more. This is interesting, because there is a long-standing controversy as to the origin of the loess formation of the North-west. Certain portions of the loess formation of Asia are known to be wind deposits, and there is very strong presumptive evidence, now borne out by the examination of the samples of dust, that much of the loess of the Western States is also a wind deposit. Special interest is thus attached to the dust-storm referred to, on account of the bearing of the observations on the question of the formation of agricultural soils, and especially the loess, which is the lightest and finest of all. This light soil is easily raised and carried by the strong winds of the western plains of America; instances have occurred in which six inches of surface soil have been blown away from freshly cultivated fields in the course of a single wind-storm. Prof. Cleveland Abbe is of the opinion that the dust caught between the two layers of snow in Indiana, probably did not differ materially from that which is daily present in the atmosphere of that region, but its presence on the top of a layer of snow rendered it easy to gather the dust-fall without contamination with the soil already existing. So this dust formation, or loess, when it has once settled upon the ordinary soils, becomes a new ingredient in their composition, and is therefore well worth further study.

A USEFUL bulletin, on the pasteurisation of milk and cream for direct consumption, has been issued from the Agricultural Experiment Station of the University of Wisconsin. It is drawn up by Dr. H. L. Russell, the bacteriologist attached to the station, and contains much interesting matter. There can be no doubt whatever that the pasteurisation of milk is a most important hygienic measure, destroying as it does an average of about 99·7 per cent of the microbes present in milk, amongst which are the diphtheria and typhoid microbes, as well as those organisms associated with gastric and intestinal disturbances so common in young infants during the summer. It is claimed that the introduction of pasteurised milk among the poor people of New York, through the philanthropic efforts of Mr. Nathan Straus, has done much to reduce the infant mortality in that city during the hot summer months. The practical side of the question has not been lost sight of by Dr. Russell, and the results of his experiments on the efficient production and distribution of pasteurised milk on a commercial scale are carefully brought together. The subject is one of great importance, both from a hygienic as well as commercial point of view, and we may surely hope that before long our dairy authorities will take the matter up, and that we shall follow, though tardily, the example already set us by our neighbours in France and Germany, where pasteurised milk may be purchased across the counter.

THE volume of "British Rainfall" for 1894, compiled by Mr. G. J. Symons and Mr. H. Sowerby Wallis, from observations made at more than three thousand stations in the British Isles, has just been published. As in previous years, the volume contains articles upon various branches of rainfall work, and upon rainfalls of exceptional interest.

DR. TH. WOLF has contributed to the *Verhandlungen der Gesellschaft für Erdkunde zu Berlin* (Bd. xxii. Nos. 4 and 5, 1895, pp. 246–265, pl. iii.) a detailed sketch of the Galapagos Islands, describing their geology, in some detail, with shorter accounts of the botany and zoology. He denies that there are any grounds for Dr. Baur's theory that the islands were once connected with the mainland of South America.

WE have on our table the *Journal* of the Royal Agricultural and Commercial Society of British Guiana, containing two papers of scientific interest, viz. "Cane Cultivation in the Straits Settlements," by Mr. F. Campen, and "A Journey to the Summit of Roraima," by Mr. J. J. Quelch; also the *Journal* of the Institute of Jamaica, which, though mostly taken up with matters of historical interest, contains several notes on local natural history topics, and a note on the discovery of aboriginal Indian remains in the Port Royal Mountains, already described in these columns by Mr. J. E. Duerden (p. 173).

THE report of the Royal Prussian Meteorological Institute for the year 1894 draws attention to two points: the completion of the arrangements for magnetic observations at the Potsdam Observatory, and the conclusion of a number of balloon ascents made during the year. The results of these ascents will be made the subject of a special investigation; one of the balloons, sent up with registering instruments only, reached an altitude of over sixty thousand feet. The report shows that many important publications have been issued, both officially, and in various periodicals, by members of the staff; some of these papers have been noticed in our columns. The laboratory experiments carried on by the Institute are of a high order, and have attracted the attention of scientific men in various countries.

THE Royal Horticultural Society's *Journal* for August has in it several important papers. There is a report of the Primula Conference, held a short time back with the idea of increasing and improving the culture of the various species of Primula by procuring new plants from remote regions; by practising the most successful methods of culture; and by producing hybrids. A paper on the botanical work done on the genus Primula since the last conference in 1886 was contributed by Mr. J. G. Baker, F.R.S., and this is printed with one on the culture and classification of Primulas, by Mr. H. Selfe-Leonard, and another on the Auricula, by Mr. J. Douglas. Among the other papers in the *Journal*, we notice a long and very valuable description of the plants and gardens of the Canary Isles, by Dr. Morris, C.M.G., and a paper on the culture of roses under glass, by Mr. F. Cant.

DR. K. SAPPER has supplemented his recent memoir, "Bemerkungen über die raümliche Verteilung und morphologischen Eigentümlichkeiten der Vulkane Guatemalas" (*Zeit. deut. geol. Ger.*, Bd. xlv. 1893), by a further account of the topography of some of the less-known volcanos. (*Petermann's Mitth.* Bd. xli. No. 5, 1895, pp. 105–109, pl. vii.) In spite of the fact that the volcanos of Guatemala have been repeatedly examined during the last half-century, and described in Dollfus and Montserrat's classical work, many of them were almost unknown. Dr. Sapper now describes the volcanos of Acatenango, 3950 m., which consists of five craters in line; San Pedro, 3050m., on which no trace of recent volcanic action remains, for the mountain is wooded to the summit, and the crater has been destroyed; and a group of western volcanos. He was anxious to explore the previously unknown Lacandon, which if proved to be volcanic would fill up a gap in the chain. He was unable to ascend the mountain, but saw sufficient to render it almost certain that Lacandon is a volcano of the first order.

THE Madras Government Museum is, to judge from the Administration Report for the year 1894–95, a very progressive institution. Mr. Edgar Thurston, the superintendent, appears to be sparing no efforts to make the museum more valuable for educational purposes, and for reference in connection with natural history, economic, and other subjects, and also more attractive to the ordinary sight-seer. The increase in the number of visitors to the museum during the year—from 311,112 to 368,282—shows that his efforts are appreciated. We notice with interest, that an entirely new departure was made, during the year covered by the report, by the commencement of a detailed anthropological survey of the races, castes, and tribes which inhabit Southern India. The Madras Government express in the report their satisfaction that the survey has been set on foot. Mr. Thurston has already collected sufficient evidence to make it clear that his investigation will prove of great interest and value.

WHEN Mr. Alfred Daniell's "Text-Book of the Principles of Physics" (Macmillan) appeared, eleven years ago, it was at once hailed as an original work, and a decided acquisition to the literature of physics. The third edition, which is now before us, maintains the characteristics of the original issue. At the time when the work was designed, it was possible for a medical student to obtain the degree of Doctor of Medicine without any adequate knowledge of physics. "That arrangement," Mr. Daniell then wrote, "is self-evidently opposed to common-sense, and to the exigencies of physiological study and of medical practice; such an anomaly cannot, it may be anticipated, endure much longer. Before many years are over, it will be universally acknowledged in practice, as it already is in theory, that knowledge of natural philosophy is an essential part of the mental equipment of the medical student and of the properly-trained medical man." It is satisfactory to be able to record that Mr. Daniell's prognostication was fulfilled in 1892, when the new regulations of the General Medical Council came into force, and it is also gratifying to know that medicine is every day becoming more truly scientific in its methods and objects. Mr. Daniell's work is by no means only suited for a medical class-room; it is alike useful to all students of science. The leading principles of physical science are set forth in the pages of the book in language the precision and accuracy of which make the volume welcome to all who study physics.

WE have received from the Deutsche Seewarte the first supplement to the principal catalogue of its valuable library, which now contains some seventeen thousand works relating to meteorological and kindred sciences, and includes the important collection of the late Prof. H. Dove. It is arranged under subjects, with the titles under each entered according to authors or institutions, while an alphabetical index at the end facilitates the reference to the subject catalogue. Opinions differ as to the best method of publishing such a work, the strictly alphabetical arrangement, such as followed by Prof. G. Hellmann in his excellent *Repertorium der Deutschen Meteorologie*, or the Royal Society's catalogue of scientific papers, possesses great advantages, and obviates the necessity of indexing one book under several sections; but as the Seewarte originally adopted another method, it has perhaps done well to keep to the same plan, and has rendered good service to science by its careful preparation and timely publication of the catalogue. The first part was issued in the year 1890.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*, ♂) from India, presented by Mr. Hugh H. Collis; a Macaque Monkey (*Macacus cynomolgus*, ♂) from India, presented by Mr. E. Laundy; a Vervet Monkey (*Cercopithecus lalandii*, ♀) from South Africa, presented by Mrs. Edward Webb; two Brown Capuchins (*Cebus fatuellus*) from Guiana, presented by Major W. S. D. Liardet; two Black-eared Marmosets (*Hapale penicillata*) from South-east Brazil, presented by Mrs. H. V. Friend; a Suricate (*Suricata tetradactyla*) from South Africa, presented by Mr. J. Lewis; a Purple-capped Lory (*Lorius domicella*) from Moluccas, presented by Mr. T. Bailey; two Tarantula Spiders (*Mygale*, sp. inc.) from Trinidad, presented by Mr. J. Hoadley; six Grey Parrots (*Psittacus erithacus*) from West Africa, deposited; a Collared Fruit Bat (*Cynonycteris collaris*), a Ypecha Rail (*Aramides ypecaha*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

REAPPEARANCE OF SWIFT'S COMET.—The *Edinburgh Circular*, No. 44, publishes a telegram from Kiel announcing that Comet Swift was seen by Mr. E. E. Barnard, at the Lick Observatory, on the 20th and 21st inst. The comet is described as faint, and its position and daily motion are given as follows:—

	Local Mean Time.		R.A.		Decl.
	h. m.		h. m. s.		° ′ ″
1895, August 21,	11 23·7	...	0 30 11·4.	...	+5 38 55
Daily Motion		...	+2 48	...	+ 10

THE LATITUDE VARIATION TIDE.—One of the most interesting outcomes of the recognition of the variability of the earth's axis of rotation has been the search for the tide, corresponding to the latitude variation. The separation of the axis of rotation from the axis of figure must cause at any point on the earth's surface successive divergences of the sea-level, from that which would exist if the figure of the earth remained a fixed ellipsoid of revolution. This consideration naturally led to the inquiry whether a small oscillation in the mean sea-level could be actually detected, having the same period as the displacement of the pole. The earliest results published were those obtained by Dr. Bakhuyzen (*Astr. Nach.* No. 3261), who used the tidal observations for the years 1855 to 1892, registered on a mareograph at the Helder, and these results showed a satisfactory agreement with those deduced from astronomical observations.

In the meantime Mr. A. S. Christie has been at work on the records made at the United States Coast Survey mareograph stations, and his results, embodied in a paper read before the Philosophical Society of Washington, are now before us. The paper is divided into two sections, the first of which is devoted to the derivation of the formulæ necessary for the elimination of the effects of other tides, and the second contains the results of the application of these formulæ.

The observations employed are obtained from two series, made at stations in the vicinity of San Francisco, namely, at Fort Point (1856–70) and Sausalito (1877–91). Mr. Christie has also used a similar series made at Pulpit Harbour, Penobscot Bay, Maine (1870–88). It will be sufficient to give here the final result arrived at by combining the results at San Francisco and Pulpit Harbour. The period deduced is 431 ± 4 days, and the value of the half-range tide is 15 ± 2 mm.; while the dates at which the critical phases of the tide occurred are:—

	San Francisco.		Pulpit Harbour.
Min.,	1872, July 15 ± 15 days	...	1878, August 22 ± 10 days.
Max.,	1873, Feb. 15 ± 15 ,,	...	1879, March 25 ± 10 ,,

Dr. Bakhuyzen's value of the half-range is 8·2 mm., a result that does not differ greatly from the mean here given, 15 mm., or from either of the two results, 17·4 mm. and 12·5 mm., on which this value rests.

Reduced to the latitude of Berlin, we have another comparison between the investigations of the American and Dutch astronomers, and the results are still fairly satisfactory, as shown below:—

Julian Date of Maximum Latitude of Berlin.

Bakhuyzen,	from astronomical observations	2405141 Julian
,,	from discussion of Helder tides ...	201
Christie,	from San Francisco tides	153 ± 16

It seems possible, therefore, that this difficult question of the motion of the earth's pole may be attacked by two quite separate processes.

THE SOLAR PARALLAX FROM MARS OBSERVATIONS.—With the view of making a new and trustworthy determination of the solar parallax, a scheme was suggested in 1892 by the authorities of the Washington Observatory for the observation of the difference of declination at the time of meridian passage between Mars and a number of selected stars. The horizontal equatorial parallax of Mars reached in that year a maximum of 23″·4, a sufficiently favourable condition, though the small altitude of the planet in the northern observatories was likely to introduce considerable uncertainty in the amount of refraction. Among the observatories that replied to the invitation of Washington to take part in this scheme are those of Gotha and the Cape of Good Hope. The result of the combination of the two sets of observations has recently been published by Dr. Paul Harzer, and are of especial interest, since Gotha lies nearly on the northern limit of the region in which observations of Mars could be made with sufficient accuracy.

It was a part of the original suggestion—to which some exception was taken at the time—that in addition to the method of fixing the declination of the centre of Mars by the employment of a pair of wires, separated by about 16″ to cut off equal segments from the northern and southern limbs of the disc, a reflecting prism should be mounted outside the eyepiece, and that half the observations should be made with, and half without the use of this additional apparatus. The result of the precaution is shown in the following figures, in the case of the two observers who took part in the series:—

	Dr. Harzer.		Dr. Rohrbach.
Mars stars ...	+0″·253±0″·039	...	−0″·383±0″·129
Mars	−0·270±0·091	...	−0·523±0·262

These figures imply that Dr. Harzer placed the stars too low and the planet too high with reference to the threads, Dr. Rohrbach, in both cases, too high.

The observations were continued from June 22 to September 23, and when combined in three groups, formed on the assumption that the error of the ephemeris is constant throughout each group, the resulting values of the solar parallax are—

Group	I.	...	$\pi=8''\cdot680\pm0''\cdot081$
,,	II.	...	$=8\cdot890\pm0\cdot089$
,,	III.	...	$=8\cdot828\pm0\cdot065$

or combined into one, $\pi=8''\cdot799\pm0''\cdot044$.

The complete combination of the whole series formed into 20 normal places, and in which the possible variation of the error of the ephemeris is also sought, gives $\pi=8''\cdot800\pm0''\cdot039$, and the value of $d\delta$ is expressed in the form

$$d\delta=\frac{-1''\cdot147+0''\cdot288t}{\Delta},$$

where t and Δ are reckoned from August 7·000, and the unit for t is 50 days.

THE SUN'S PLACE IN NATURE.[1]

X.

THE NEW CLASSIFICATION OF THE STARS.

I NOW pass to the new classification of stars which has been suggested by the totality of the facts which I have so far brought before you.

Although the first observations of stellar spectra were made by Fraunhofer, we owe to Rutherfurd the first attempt at classification. In December 1862 he wrote as follows :[2]

"The star spectra present such varieties that it is difficult to point out any mode of classification. For the present I divide them into three groups. First, those having many lines and bands and most nearly resembling the Sun, viz. Capella, β Geminorum, α Orionis, Aldebaran, γ Leonis, Arcturus, and β Pegasi. These are all reddish or golden stars. The second group, of which Sirius is the type, presents spectra wholly unlike that of the Sun, and are white stars. The third group, comprising α Virginis, Rigel, &c., are also white stars, but show no lines ; perhaps they contain no mineral substance, or are incandescent without flame.

"It is not my intention to hazard any conjecture based upon the foregoing observations ; this is more properly the province of the chemist, and a great accumulation of accurate data should be obtained before making the daring attempt to proclaim any of the constituent elements of the stars."

This classification was followed up by Secchi, who practically adopted Rutherfurd's three groups, changing, however, the word group to type, and adding a fourth. On this point Dr. Gould, in his memoir[3] of Rutherfurd, writes as follows :

"I cannot forbear calling attention to the classification, essentially the same, subsequently published by Secchi without reference to this or to any of the other labours of Rutherfurd, and which is generally cited under Secchi's name." (See "Scheiner," p. 258, and "Translation," pp. 235-236.)

In these and other subsequent classifications—and of course we must classify our stars if we are to speak about them with intelligence, and to understand the relations of one body or system of bodies to another—it has been taken for granted that nebulæ have nothing whatever to do with stars, and that all the stars lie along one line of temperature, the highest temperature being at one end, and the lowest at the other. Such, at all events, is Vogel's view. Now we have to conclude that nebulæ are stars to be, and that some apparent stars are really nebulæ ; and I think I have shown you sufficient justification for the idea that the undisturbed nebulæ are of relatively low temperature ; hence we have bodies getting hotter as well as bodies getting cooler, and both must be provided for.

In 1873 Dr. Vogel brought out a new and much more detailed classification considerably extending the number of groupings employed by Rutherfurd and Secchi. This classification is based on the assumption that all stars began by being very hot, and that the various changes observed in the spectra are due to cooling,[1] and the presence of bright lines is considered as a matter of secondary importance only, and gives rise to subgroupings only.

Dr. Scheiner has quite recently accepted this statement. He appeals to his new observations of the spectrum of magnesium as a "direct proof of the correctness of the physical interpretation of Vogel's spectral classes, according to which Class II. is developed by cooling from I., and III. by a further process of cooling from II." (*Astronomy and Astro-Physics*, 1894, p. 571.)

Pechüle was the first to object to Vogel's classification, mainly on the ground that Secchi's types 3 and 4 had been improperly brought together.[2] Now the views I have brought before you cut at the root of such a classification as this.

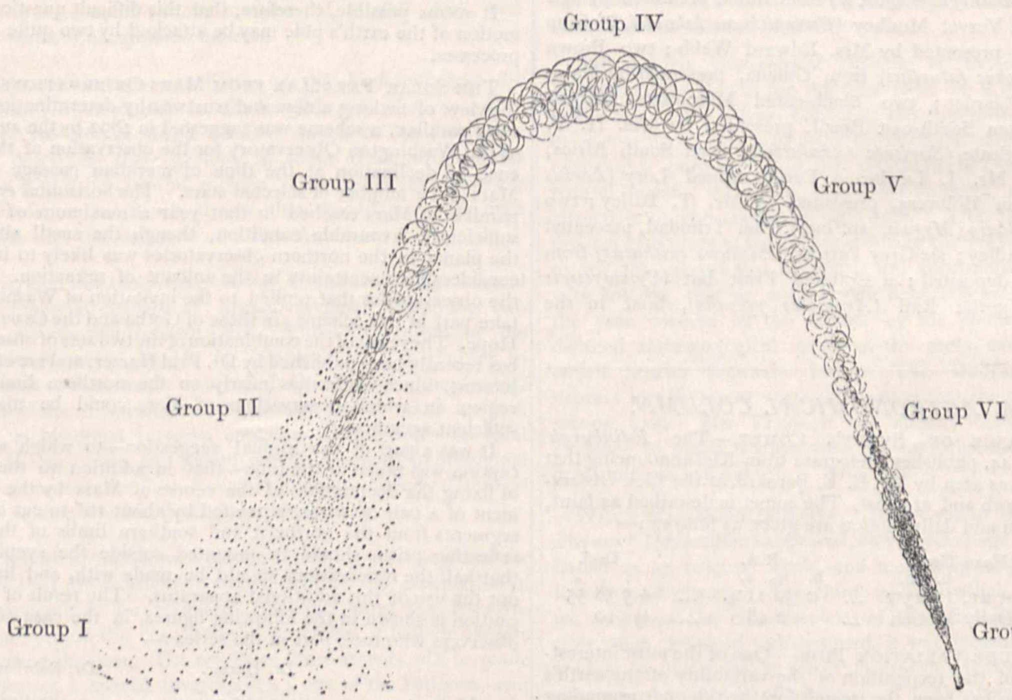


FIG. 38.—Temperature curve.

It is perhaps worth while in passing to point out that in the course of lectures I gave here in 1886 I stated, taking the then classification as a basis[3] :—

"On the nebular hypothesis, supposing . . . . that we started with ordinary cometary materials, then, on the beginning of a central condensation which in time is to become a star, as Kant and Laplace suggested, such central condensation should then give us a star of the fourth class. As the energy of condensation increased and the temperature got higher, the spectra would change through the third and second classes, till ultimately, *when the temperature was highest*, the first class spectrum would be reached. *On the slackening down of the temperature* of the now formed star, the spectra of the second, third, and fourth classes would then be reproduced, but, of course, now in the direct order."

[1] Revised from shorthand notes of a course of Lectures to Working Men at the Museum of Practical Geology during November and December, 1894. (Continued from page 329.)

[2] *American Journal of Science*, vol. xxxv. p. 71.

[3] Read before the National Academy, April 1895.

[1] "Selon la théorie il faudra que tôt ou tard toutes les étoiles de la première classe deviennent de la seconde, et celles-ci de la troisième." Dunér.

[2] The details of Vogel's classification and Pechüle's criticisms are given in my "Meteoritic Hypothesis," pp. 345-6.

[3] Lockyer, NATURE, vol. xxxiv. 1886, p. 228.

We now know that this classification will not do, since all reference to bright lines is omitted, and every one now agrees that they must take the first place, and this is one of the great teachings of the views I have been bringing forward for the last ten years.

The idea which one arrives at by a discussion of all the spectroscopic facts is that we begin with a condition in which meteorites in swarms and streams are very far apart, and we get from the collisions of these a spectrum which gives us bright flutings and lines, in other words the spectrum of the nebulæ; when they get a little more dense, we get the bright-line stars; and as they get still more dense, we find the star with a mixture of bright and dark flutings. Then we get still more condensation and dark lines, and then the highest temperature of all; after which begins a descent on the other side, till at last we end in cool, dark bodies like the earth and moon.

This seems to be the classification which is necessitated by the consideration of all the facts, and it is, moreover, one which seems to give us possibilities of an explanation of the phenomena of new stars and variable stars, and many other things without going into the region of the unknown and impossible.

It also lands us in the so-called temperature curve along which I ventured to place the various classes of nebulæ and stars some time ago. I am glad to say that so far no valid objection has been made to it.

It will be noticed that in the classification I have suggested I use the word "group," first employed by Rutherfurd; it is one which ought never to have been changed.

With regard to this subject, Prof. Keeler, one of our most important authorities in this matter, agrees that a classification which depends on this temperature curve certainly has advantages over other systems. He writes [1]:—

"Prof. Lockyer's system of stellar classification provides for both an ascending and a descending branch of the temperature curve, and in this respect it certainly has advantages over other systems which claim to have a rational basis."

I am also more glad than I can say that Prof. Pickering, who has now given many years, with the aid of appliances beyond all precedent, to the study of these questions, has arrived at conclusions strikingly similar to my own.

In the first place he includes the nebulæ as well as the stars in his system; but it is right that I should add that he does not commit himself to any statements relating to the relative temperature of the different groups, although he distinctly accepts the idea of evolution, or what he terms an order of growth.

He writes (*Astronomy and Astro-Physics*, 1893, p. 722):

"In general, it may be stated that, with a few exceptions, all the stars may be arranged in a sequence, beginning with the planetary nebulæ, passing through the bright-line stars to the Orion stars, thence to the first type stars, and by insensible changes to the second and third type stars. The evidence that the same plan governs the construction of all parts of the visible universe is thus conclusive."

Prof. Pickering's results may be shown in tabular form, but first it will be well to show the general differences between the more recent classifications:—

	Secchi.	Vogel.	Lockyer.
Nebulæ	Not classified.	Not classified.	Group I.
Bright-line stars ...	,,	,,	
Mixed fluting stars ..	Type III.	Class IIIa.	,, II.
Dark line stars (ascending)	,, II.	,, II.	,, III.
Broad hydrogen stars ...	,, I.	,, I.	,, IV.
Solar stars	,, II.	,, II.	,, V.
Carbon absorption stars...	,, IV.	,, IIIb.	,, VI.

In his classification, Prof. Pickering begins with the earliest stages, taking the planetary nebulæ and such nebulæ as that of Orion; he then comes to the bright-line stars, and then to such stars as those of Orion, and ultimately places the Sun, as I also do, after the spectrum of such a star as Sirius. There are practically two departures in his classification from that given by myself. One is that what I call the bright and dark fluting group of stars, represented by several of the red, and brightest, stars in the heavens, he makes cooler than the Sun. And the class of stars which I group together and call Group VI., in which we get mainly the absorption of carbon in the atmosphere, he omits altogether, possibly for a very wise reason, as they are certainly the most difficult stars to tackle; but you see the divergences in his classification from mine are small as compared with those between Dr. Vogel and myself, and he, I repeat, like myself, attributes the variation to an "order of growth."

This premised, the differences of sequence between Prof. Pickering and myself may be shown as follows:—

Lockyer.	Pickering.
I.	I.
II.	
III.	III.
IV.	IV.
V.	V.
VI.	II.

Prof. Pickering, in the Draper Catalogue, combines like stars under the different letters of the alphabet. The distribution of these letters in relation to my Groups is as follows:—

	Lockyer.	Pickering. (Draper catalogue.)
Nebulæ	I.	P. (Planetary Nebulæ.)
Bright-line stars...		O.
Mixed fluting stars	II.	M.
Dark-line stars (ascending) ...	III.	B. H. I. K. (?)
Broad hydrogen stars	IV.	A.
Solar stars	V.	F. G. K. L.
Carbon absorption stars ...	VI.	N.

It will be seen that certain groups are represented by more than one letter, but it is to be noted that here again Prof. Pickering and myself have arrived at very nearly similar results, for generally a different letter with him represents a sub-group with me. This will be gathered from the subjoined table.

Table showing the subdivisions of Groups III. and V.

Group.	Pickering.
III. α	H.
III. β	I. (some Q.)
III. γ	B.
V. α	F.
V. β	G.
V. γ	K. L.

With regard to Prof. Pickering, then, I have chiefly to justify the place I have given to the stars of my Group II., which I place after the nebulæ and bright-line stars, and he places after the Sun.

I fancy that one of the reasons which has led Prof. Pickering to this conclusion is to be found in the assumption that strong indications of calcium and iron can only mark one stage of growth, while I think it is certain they must mark two.

We know they mark the present stage of the Sun's history, and taking meteorites as we find them, a relatively low temperature would provide us with more calcium and iron vapours to act as absorbers round each one than anything else.

Now we have strong indications of calcium and iron absorption in such stars as α Herculis as well as in the Sun, but the general appearance of the spectra of these stars is so different that both Secchi and Vogel have classified them apart, and so indeed does Prof. Pickering.

But the reason that I classified these stars also in different groups, and one on the rising and the other on the descending arm of the temperature curve, was that in those like α Herculis we have enormous variability as well as bright lines and flutings indicative of sparse swarms, while in those like the Sun the production of such phenomena is almost unthinkable. The special variability of stars of my Group II. (Secchi's type III.) and the production of bright lines at maximum is now freely acknowledged. On this point Prof. Pickering remarks [1]:—

"Long period variables in general are of the third type, and have the hydrogen lines bright when near their maxima, as stated above. This property has led to the discovery of more

[1] *Astronomy and Astro-Physics*, 1894, p. 60.

[1] *Astronomy and Astro-Physics*, 1893, p. 721.

than twenty objects of this class, and no exception has been found of a star having this spectrum whose light does not really vary. Of the variables of long period which have been discovered visually, the hydrogen lines have been photographed as bright in forty-one, the greater portion of the others being too faint or too red to be studied with our present means."

As said before, it seems impossible to imagine how our Sun, as it proceeds along its "order of growth," should change into a body with such characteristics as these. But on this point we must wait for more large scale photographic spectra; in other words, more facts.

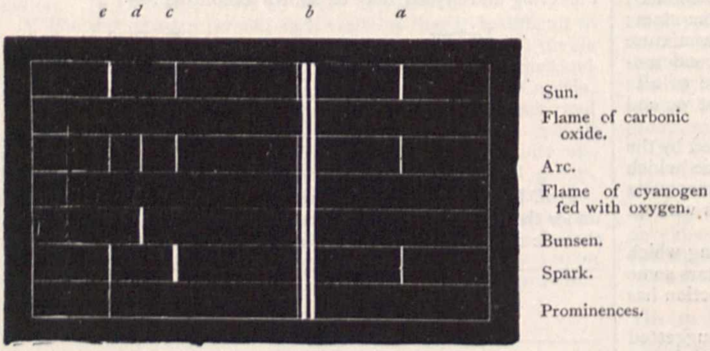


FIG. 39.—Showing the various intensities of the lines of magnesium as seen under different conditions.

Associated with this change in the order of evolution, Prof. Pickering classes the chief stars in Orion, such as Bellatrix, characterised by spectra containing hydrogen and a few other dark lines of unknown origin, as early forms. On this point I may also quote the following from Prof. Campbell (*Astronomy and Astro-Physics*, 1894, p. 475):—

"In conclusion, I think we can say, from the foregoing observations, that the spectra of the Wolf-Rayet stars are not closely related to any other known type. They appear to have several points in common with the nebular and Orion type spectra; but the last two appear to be much more closely related to each other than to the Wolf-Rayet spectra. It is therefore difficult to place these stars between the nebulæ and Orion stars. They certainly do not come *after* the Orion stars, and one does not like to place them before the nebulæ. We can probably say that the bright lines are chromospheric, owing their origin to very extensive and highly-heated atmospheres, but showing very little relation, in constitution and physical condition, to that of our own Sun. For the present, at least, this type of spectrum must be considered as distinct from every other known type, just as the nebular spectrum is distinct, and like the nebular spectrum containing lines whose origin cannot now be assigned."

Although Dr. Vogel and others apparently still hold in the main to the classification which assumes that all stars were created hot, and that nebulæ have nothing to do with them; that, in short, every star began in the highest stage of temperature, so that the whole history of every star in the heavens has been a process of cooling, there are signs of wavering here and there. Some of the definitions are being "edited" and re-edited to fit the facts which the photographic record is pouring in upon us. I may take, as an instance, the following statement made by Dr. Scheiner with reference to α Cygni, which is classified by Dr. Vogel as a solar star.

"These figures plainly show that the spectrum of α Cygni, in spite of the large number of its lines, has no resemblance with that of the sun. While it is possible to identify most of the lines with solar lines in respect to their position, yet the total lack of agreement as to intensity of the lines makes many of these identifications worthless."

The "figures" referred to are micrometer measures of a photograph. My experience in these matters is that it is a pure waste of time to measure a photograph until it has been compared with others to which it is important to refer it, enlarged up to the same scale. In this I think I carry Prof. Keeler with me (*Astronomy and Astro-Physics*, 1894, p. 485). "The coincidence of . . . lines is shown more beautifully by inspection of . . . photographs than by any process of measurement." Thus a comparison of the spectra of α Cygni and of the Sun *which Dr. Vogel classes together*, shows at once the dissimilarity pointed out above without any measurement whatever. I am glad, however, to find that Dr. Scheiner now regards the identification as "worthless," because it is such differences as these which have compelled me to reject Dr. Vogel's classification.

Dr. Scheiner then goes on:—

"The magnesium line at λ 4481 is the strongest in the entire spectrum. The other strong lines coincide for the most part with the fainter solar lines. The presence of numerous iron lines can be scarcely doubted, but here again we have the peculiar phenomenon that the fainter, instead of the stronger, lines occur. We may conclude from all these facts that very different conditions as to temperature must prevail in α Cygni from those in the stars of class I*a*." (Scheiner's "Astronomical Spectroscopy," Frost's translation, p. 247.)

Much of the work of the future, which eventually must smooth down all differences between stellar classifications, must consist of the study of single lines in the spectra of different stars, and I am rejoiced to find that the Potsdam observers are at length beginning to take this matter up. Dr. Scheiner, one of the Potsdam assistants, has, as seen above, called attention to the behaviour of the line 4481 of magnesium, and agrees that the variations in the line observed are due to differences of temperature, and that therefore it may be used as a stellar thermometer.[1]

But for this work an acquaintance with the literature of the subject is desirable. Had Dr. Scheiner been acquainted with it, I am certain he would have done me the honour to quote, or at all events to refer to, a communication I made to the Royal Society (16 years ago!), pointing out that the line in question was visible only at high temperatures, and that such work would help us in the study of "the atmospheres of the hottest stars."[2] In the same connection, in the "Chemistry of the Sun," published in 1887, I gave the diagrams, here reproduced, indicating the lines, visible at various temperatures in the laboratory, and in the Sun and prominences.

Having said so much on the different classifications of stars, and indicated, I trust judicially, that the one suggested by the meteoritic hypothesis is so far holding its own, I now pass on to some recent work which was undertaken to test it by a limited photographic survey. In the first instance I had used the eye observations of others; a study of spectra, entirely photographic, it was hoped would enable an independent estimate to be formed as to the validity of the hypothesis.

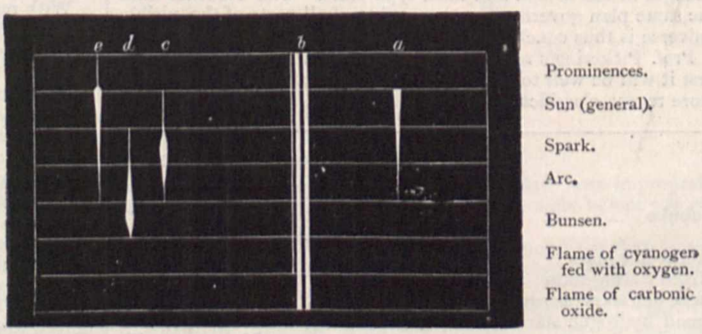


FIG. 40.—The various intensities of the lines of magnesium arranged in order of increasing temperatures. The lines marked *a b c d e* in the diagrams have the following wave-lengths:—5209·8, 517 (*b*), 4703·5, 4570·3, 4481.

The conclusions I came to in the first instance were necessarily based on observations made by others, for the reason that my own work up to that time had been chiefly directed to the Sun.

So soon, however, as my solar work rendered it necessary to determine the Sun's true place among the stars in regard to its

[1] "Astronomical Spectroscopy," p. viii.
[2] *Roy. Soc. Proc.* vol. xxx. p. 22, 1879.

temperature and physical conditions, arrangements were made to photograph the spectra of stars and nebulæ, in order to test the view, employing a quite new basis of facts; this new basis of the inquiry consists of 443 photographs of 171 of the brighter stars.

Having this new and accurate basis of induction, the objects were to determine whether the hypothesis founded on eye observations is also demanded by the photographs, and in the affirmative case to discover and apply new tests of its validity, or otherwise.

The results as yet obtained are not sufficient to permit a discussion of all points bearing upon the new classification, but most of the crucial ones are certainly covered by the photographs already obtained.

The main instrument employed in the work has been a 6-inch refracting telescope, with an object-glass made and corrected for G by the Brothers Henry. This was at first used in conjunction

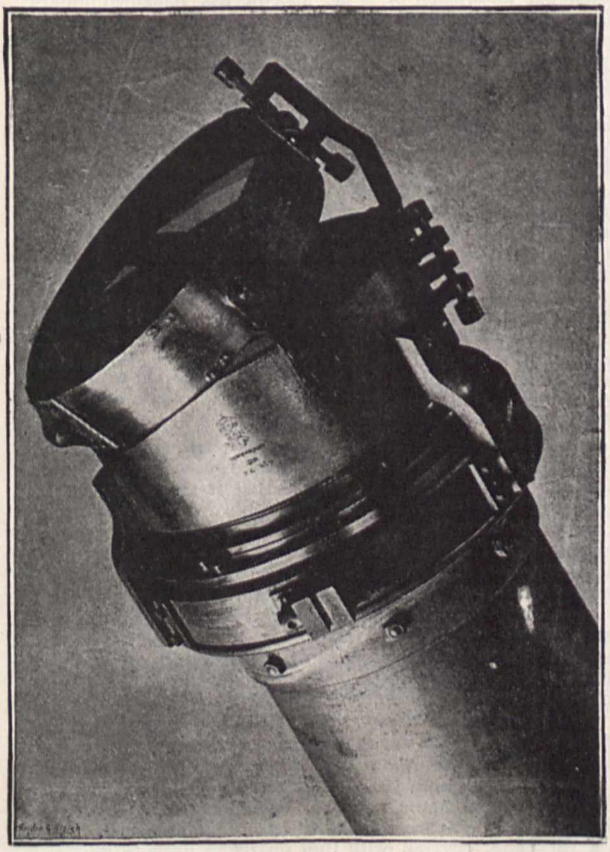


FIG. 41.—Objective prism fitted to object-glass.

with a prism of $7\frac{1}{2}°$ of dense glass by Hilger. The object-glass and prism are fixed at the end of a wooden tube, which is attached to the side of the 10-inch equatorial, at such an angle that the spectrum of a star falls on the middle of the photographic plate when its image is at the centre of the field of the larger instrument. The camera is arranged to take plates of the ordinary commercial size, $4\frac{1}{4} \times 3\frac{1}{4}$ inches. The spectra obtained with this instrument are 0·6 inch long from F to K. An excellent photograph of the spectrum of a first magnitude star can be obtained with an exposure of five minutes. Afterwards a 6-inch prism, with a refracting angle of 45°, obtained from the Brothers Henry, was used with the Henry 6-inch object glass. The spectra obtained with the latter are two inches long from F to K, and the definition is exquisite. In some photographs the calcium line at H is very clearly separated from the line of hydrogen, which occupies very nearly the same position. It is unnecessary to swing the back of the camera in order to get a perfect focus from F to K. The deviation of the prism is so great that it would be very inconvenient to incline the tube which supports it at the proper angle to the larger telescope. When photographing the spectrum of a star, therefore, the star is first brought to the centre of the field of the large telescope, and the proper deviation is then given by reading off on the declination circle. This method has been found to work quite satisfactorily.

With this combination the exposure required for a first magnitude star is about twenty minutes. The method of mounting the prism is shown in Fig. 41.

For the fainter stars, the 6-inch prism of $7\frac{1}{2}°$ has been adapted to a Dallmeyer rectilinear lens of 6 inches aperture and 48 inches focal length. At times prisms of $7\frac{1}{2}°$ have been used on a 10-inch equatorial.

Since the spectrum of a point of light such as a star is a line so fine that the spectral lines would not be measurable, it is necessary to give it breadth. This is done by adjusting the prism so that the spectrum lies along a meridian of R.A. and altering the rate of the clock.

J. NORMAN LOCKYER.

(To be continued.)

THE IRON AND STEEL INSTITUTE.

THE annual summer meeting of the Iron and Steel Institute was held in Birmingham last week, commencing Tuesday, the 20th inst., and extending over Friday, the 23rd inst. Sir David Dale, the President, took the chair at the sittings for the reading of papers, and it may be said here that the meeting was remarkably successful throughout, being one of the pleasantest and most instructive gatherings that has been held for a long time past; both Mr. Brough, the Secretary of the Institute, and the local committee are to be congratulated on the excellence of their arrangements.

There were twelve papers down for reading and discussion, of which the following is a list:—

"On the Direct Puddling of Iron," by E. Bonehill (Marchienne-au-Pont, Belgium).

"On the Production of Iron by a New Process," by R. A. Hadfield, member of Council (Sheffield).

"On the Thermo-Chemistry of the Bessemer Process," by Prof. W. N. Hartley, F.R.S. (Dublin).

"On the Hardening of Steel," by H. M. Howe (Boston, U.S.A.).

"On the Mineral Resources of South Staffordshire," by H. W. Hughes (Dudley).

"On the Iron Industry of South Staffordshire," by D. Jones, Secretary of the South Staffordshire Ironmasters' Association (Shifnal).

"On the Iron Industry of the South of Russia," by George Kamensky (St. Petersburg).

"On Cooling Curves and Tests of Cast Iron," by W. J. Keep (Detroit, U.S.A.).

"On the Analysis of Ferro-Chromium," by E. H. Saniter (Wigan).

"On Small Cast Ingots," by R. Smith-Casson (Birmingham).

"On Tests of Cast Iron," by T. D. West (Sharpsville, Pennsylvania).

"On Nickel Steel," by H. A. Wiggin (Birmingham).

The papers of Mr. West and Mr. Keep were taken as read, all the others being read and discussed.

On the members assembling on Tuesday morning, in the Council House of Birmingham Corporation, they were welcomed by the Mayor, and by the members of the local reception committee.

The first paper taken was that by Mr. D. Jones, on the iron industry of South Staffordshire. This was an interesting contribution, but mainly historical in its character. It dealt with the rise and progress of the iron industry of the district from its earliest days, and, in treating of more modern times, pointed out how the production of wrought-iron had decreased as steel had taken its place, although a good deal of puddled iron is still produced in the district. The paper of Mr. Hughes, on the mineral resources of South Staffordshire, was very much of the same character, and gave, in a convenient form, many facts relating to the subject.

M. Bonehill's paper on the direct puddling of iron was next read. This process appears to be a revival of, and doubtless an improvement on, a method of puddling which was proposed, and to a limited extent carried out, in the earlier years of the century, but which never obtained any great hold in the iron industry. It consists, briefly, in running molten iron from the blast furnace into a reservoir, and from thence letting it flow into the puddling furnace, the latter being of larger description than is generally used. It is obvious that with this process, as compared to the ordinary method of feeding the puddling furnace with cold pig, there is a saving of fuel, inasmuch as the metal does not require melting; on the other hand, the difficulty of getting a uniform product, owing to the inability to mix various kinds of pig, has to be overcome. Apparently the author has been successful in the latter respect, although how he has accomplished his end was not stated in the paper; the tests given, however, indicate that a superior quality of iron is produced.

Mr. Kamensky's paper on the iron industries of South Russia was, like the two first contributions, of an historical nature. In this case, however, there was less of ancient history in the memoir, and necessarily so, as the production of iron in Russia, as an industry of importance, is of essentially modern growth. It is true that iron-making has been carried on in Russia for a long time past, but it is only within the last year or two that any great strides have been made. Now, however, there are several works in operation, and it appears likely that more will follow; so Russia may in her turn put in a claim for a share of the opening markets of the world. This is a fact that British steel-makers may perhaps look on not altogether with satisfaction; but it is inevitable. Only by increased exertion can British manufacturers maintain their position in the markets of the world; but there is one point, however, worthy of attention. If Russia is about to start many steel works, large quantities of plant and machinery will be required. It is proposed that the Institute shall next year hold its summer meeting in Russia. The suggestion is a bold one, but is worthy of consideration, for it is only by pushing abroad that steel makers can hope to keep abreast of the times. The days are past when the manufacture of iron and steel was almost entirely centred in England. Now there are works all over the world, under intelligent and scientific management. It is unreasonable to expect that we, in England, will continue to originate all new and valuable processes, and it is well, therefore, that English manufacturers should go abroad to reap the advantages of foreign research and practice; just as foreign manufacturers have in times past, and are still, reaping the advantage of English experience and study.

The reading and discussion of the above four papers constituted the business of the first sitting. The afternoon of that day—Tuesday, the 20th inst.—was devoted to visits to works. One party proceeded to the Staffordshire Steel and Ingot Iron Company's establishment at Bilston, where the operations of rolling sections and plates were witnessed. A large quantity of basic steel is produced at these works; and the method of dealing with the basic slag, which is largely used for agricultural purposes, was inspected with interest by the members. Another party visited the Electric Construction Company's works at Wolverhampton; whilst, again, others distributed themselves amongst various works in Birmingham.

On assembling again on Wednesday morning the first paper taken was that contributed by Prof. Harley, on the thermochemistry of the Bessemer process. This was an exceedingly interesting paper, which those engaged in subjects of this nature would do well to read in full in the *Transactions* of the Institute. The author commenced by saying that the flame issuing from the mouth of a Bessemer converter was first investigated by Sir Henry Roscoe in 1863 (see Manchester Literary and Philosophical Society's *Proceedings*, vol. iii. p. 57, and *Philosophical Magazine*, vol. xxxiv. p. 437); by Leilegg (see *Sitzungsberichte Kaiserl. Akademie der Wissenschaften*, Wien, vol. lvi. part ii.); and by Marshall Watts in 1867 (see *Philosophical Magazine*, vol. xxxiv. p. 437); by Tunner (see Dingler's *Polytechnisches Journal*, vol. clxxviii. p. 465); by J. M. Silliman Rowan (*Philosophical Magazine*, vol. xli. p. 1); by von Lichtenfels (see Dingler's *Polytechnisches Journal*, vol. cxci. p. 213); by Spear Parker (*Chemical News*, vol. xxii. p. 25); by Kupelwieser (*Oesterreichische Zeitschrift für Berg-und Hüttenwesen*, No. 8, p. 59, 1868); by Brunner and Wedding in 1868 (see *Zeitschrift für das Berg-Hütten-und Salinenwesen im preussischen Staate*, vol. xxvii. p. 117, 1869); and also by A. Greiner in 1874 (see *Revue Universelle des Mines*, vol. xxxv. p. 623). Up to the present time the nature of the spectrum, the cause of its production, its sudden disappearance when decarburisation of the metal takes place, and the connection between the decarburisation of the metal and the extinction of the spectrum, have not been satisfactorily explained. According to Roscoe, Leilegg, Kupelwieser, and Spear Parker, the spectrum is characterised by bands of carbon or of carbon monoxide, which disappear when all the carbon is burnt out of the metal. On the other hand, Simmler, Brunner, von Lichtenfels, and Wedding hold that the spectrum is not due to carbon, or to carbon monoxide, but to manganese and other elements in pig iron. Dr. Marshall Watts had come to the conclusion that it was not the spectrum of carbon in any form, nor of manganese, but that of manganetic oxide. Leilegg proved that carbon monoxide yields a continuous spectrum, which causes the bright spectrum of the Bessemer flame; but he also attributed certain lines, or bands, to the high temperature of the carbon monoxide. Marshall Watts established the fact that six lines of the spectrum of iron were present in the Bessemer spectrum; Greiner observed in flame from highly manganiferous pig iron the spectrum of manganese. The author concluded this part of his paper by pointing out the fact that notwithstanding the great advance which has been made in spectroscopy during the last twenty years, our knowledge of flame spectra has remained almost stationary, although much attention has been directed to the spectra of the elements as we obtain them at higher temperatures by vapourising substances in the electric arc, and by the transmission of electric sparks.

Prof. Hartley next proceeded to describe a method of accurately investigating the Bessemer flame. He pointed out that the determination of wave-length of lines and bands by eye observation only, with instruments of the usual form, is laborious under the most advantageous conditions, but it is especially so when the spectra are constantly changing; and it becomes practically impossible when the lines and bands to be measured are in the ultra-violet. Spectra which are recorded by photography are capable of being more accurately measured at leisure by very simple means; moreover, they constitute a permanent record; and for accurate observations, determinations of wave-lengths are absolutely essential. The author next went on to describe a modification of the instrument he had originally designed for this purpose. This is described in the *Proceedings* of the Royal Dublin Society, and also in Thorpe's "Dictionary of Applied Chemistry," article "Spectroscope." This instrument was especially designed for use in steel work, particularly for studying the spectra of flames and heated gases of open-hearth furnaces. It was therefore desirable that it should give a fair amount of dispersion at the less refrangible end of the spectrum. A train of four quartz prisms was at first arranged, and a camera was fitted with a rack and pinion movement to the frame holding the dark slide, so that as many as thirty spectra could be photographed on one plate. The stand, however, was found to be too light. Instead of four quartz prisms, a single prism of calcite may be employed if the surfaces are well protected from dust; the prism table was fixed so that it could be placed in almost any required position. The camera was of metal with an eye-piece behind the frame for the dark slide, so as to make it available for visual observation. In a circular box at the end of the camera, which was reduced in size, the dark slide can be fixed at any angle, as it is rotated by means of a toothed wheel. The prisms move automatically with the camera, and in order to secure the minimum angle of deviation to the mean rays photographed there is a condensing lens of 3-inch focus. There is a slit plate, covered with thin quartz to exclude dust and dirt, and upon this the image of the flame was projected. A metal plate, with a V-shaped piece cut out at one end, slides over the slit plate, and serves to shorten or lengthen the slit and secure a greater or smaller number of spectra on one photographic plate. In some cases a photograph was taken every half-minute, from the commencement to the termination of the "blow." This could be accomplished only by the use of the arrangement described, as the plates were no more than 3 inches by 2½ inches. The instrument was focussed by a photograph of sun spectra.

The author also described an ingenious arrangement consisting of yellow cloth, with armholes and sleeves fitted with elastic, by means of which he carried on development of the photographs without use of a dark room. By this apparatus it was shown that a large number of lines in the spectrum of the Bessemer flame were coincident with lines in the solar spectrum, and the position of the lines and edges of bands with respect to the sodium line was recorded, being measured with a micrometer screw and microscope. Enlargements were made in which the spectra were magnified ten diameters. Several interpolation curves were drawn by which linear measurements were reduced to oscillation-frequencies, and by means of Barlow's mathematical tables these were reduced to wave-lengths which are the reciprocals of the oscillation-frequencies. The author then went on to describe some of the difficulties met with in obtaining measurements of bands, due to alterations in width, or to their becoming less distinct at the edges. The question is dealt with in "Flame Spectra of High Temperatures," *Philosophical Transactions*, 1894, part 1.

Prof. Hartley had carried out experiments at Crewe, and at Dowlais, in South Wales. Results obtained by photography of the spectrum of the Bessemer flame were given in the paper. For the details we must refer our readers to the original memoir. As the author pointed out, the Bessemer spectrum is a complex one, which exhibits differences in constitution during different periods of the blow, and even during different intervals of the same period. Watts had observed that the spectrum differs in different works, owing to variations of temperature and the composition of the metal blown. After discussing the various opinions held by previous investigators as to the utility of spectrum analysis in steel making—on which subject inquirers are by no means agreed—the cause of the non-appearance of lines at the termination of the blow is discussed. Prof. Hartley then proceeded to what was perhaps the most interesting part of his paper, namely, the temperature of the Bessemer metal and of the flame, and the use of the spectrum as an index of temperature. Watts concluded that though the temperature of the flame was above the melting point of gold, it was below that of platinum. Le Chatelier (*Comptes rendus*, vol. cxiv. p. 670) was of opinion that the temperature of the Bessemer converter during the boil is 1330° C., at the finish 1580° C., while the steel in the ladle is at 1640° C. There is no measure of the temperature at the hottest period of the boil, and unless the metal in the converter is cooled during the last minute of the blow, which some of the author's photographs indicated, it was difficult to understand how its temperature could be raised by the addition of the cooler spiegeleisen and ferro-manganese. The rise of temperature at this period could be accounted for by the after-blow. Of course when the metal is charged with oxygen, the additional spiegeleisen, containing carbon and manganese, would cause the combustion of these elements. When the oxyhydrogen flame spectra of the manganese, magnetic oxide of iron, and ferric oxide are photographed, the number of lines and bands in the spectra are not more numerous than with a Bessemer flame spectrum of only half a minute's exposure, although the above spectra may have received any exposure from thirty to eighty minutes. When a substance emits a spectrum composed of bands and lines, it is evidence of the presence of the substance in the flame in a state of glowing vapour; when the same substance emits two spectra, one differing from the other by the largely increased number of bands or lines, it is evidence that either the substance is more copiously vapourised, or that the temperature of the vapour is higher. When a simple spectrum changes to one of a more complex character, the alteration is due to an increase in temperature, other things being equal. Similarly when a spectrum extends through the visible rays into the ultra-violet region, and an increase is observed in the number and intensity of the ultra-violet rays, nothing but an increase of temperature will serve to account for the change in the spectrum. No increase of material in the flame would increase the refrangibility of the rays emitted by its vapour; hence the study of the ultra-violet spectra of flames by the photographic method becomes an important line of investigation.

After pointing out the difficulty of ascertaining the maximum temperature of any flame (as such temperature may exist over but a very small area), and giving an instance, the author states that Le Chatelier's recent measurements of the temperature of furnaces have given numbers considerably lower than those usually accepted. Langley's estimate of the temperature of the Bessemer flame at 2000° C.—because platinum appears to be rapidly melted in it—is not to be relied upon. Le Chatelier finds that the metal is not fused but dissolved in drops of molten steel. Marshall Watts observed that the sodium lines 5681 and 5687 may be employed as an index of temperature, since they are present in the spectrum of any flame containing sodium the temperature of which is hot enough to melt platinum, but they do not appear at lower temperatures. The Bessemer flame does not show this double line, but only the D lines, neither does it show lithium orange lines, which appear at a somewhat lower temperature. It may therefore be concluded that the flame is not hot enough to produce these lines. The proportion of sodium in the Bessemer flame is evidently very small from the narrowness and want of intensity of the D lines, and the fact that they are not seen reversed in any spectrum; hence, though the temperature may be high enough, the quantity of material present is not sufficiently large to yield the lines 5681 and 5687.

We have not space to follow the author in all the interesting details of his reasoning, but we have perhaps said enough to indicate his line of thought. He later points out that, judging by the number of lines and bands belonging to iron and manganese, which have been photographed in the spectrum of the Bessemer flame, the temperature must in any case nearly approach that of the oxyhydrogen flame, even if it does not very generally exceed it. The paper concluded with particulars of the heat of combustion of the oxidisable impurities in pig iron. He calculates, as far as data are available, the absolute heating effect of such oxidation. The temperature retained according to these calculations amounts to 1454° C. above that of molten cast iron. This, however, is a theoretical value, and allowance must be made for the specific heats of the gases, the metal, and the slag, which are greater at the elevated temperatures than at the temperatures at which the numbers representing specific heats were determined. The specific heat of the converter must be considerable, but it must be remembered that it is already heated to the temperature of the molten metal; but even if we allow that 50 per cent. of the heat is absorbed, or conveyed away, we should then have the temperature 727° C. above that of the molten pig iron; and thus, with grey iron, at 1220° C. the metal may have acquired a temperature of more than 1947° C., which is very considerably above the melting point of platinum.

The discussion which followed the reading of this paper was interesting, but no new points of importance were added. Mr. Bauerman considered that the author was right in laying stress on the temperature of the flame as well as on the materials in the converter. Mr. J. Stead pointed out that some of the calculations were made in cases where the composition of the metal was very different to that common in England. Mr. Tucker also pointed out the difficulty in arriving at any conclusion owing to the variation in metal used, and he referred to the effect of a temperature of dissociation which might be obtained if the metal were sufficiently rich in silicon. His own experiments supported those of Prof. Hartley, that the temperature was certainly at times considerably above the melting point of platinum, and he was inclined to think that the temperature of dissociation was often reached.

The next paper was also one of considerable scientific interest. It was Mr. Howe's contribution on the hardening of steel, and was read in abstract by Mr. Brough, the Secretary of the Institute. As the paper had been received so recently, copies of it had not been distributed, and it was manifestly impossible to discuss a memoir of this abstruse nature at first sight, especially as the paper was not read in full. It was therefore wisely determined to have the text corrected, after which the paper will be distributed, and its discussion taken at the next meeting in May. For the present, it will suffice to say that the author deals largely with the vexed problem of the allotropic state of iron. It would have been a pity to have discussed the paper on the spot, as neither Prof. Roberts-Austen nor Prof. Arnold were present; neither had M. Osmond been able to send his usual written contribution. In fact, the only person present whose name has become at all prominently identified with the states of iron treated was Mr. Hadfield, who spoke briefly, saying that he had not had time to master the paper. We will, therefore, defer our abstract of this memoir until the time comes to give an account of the next meeting.

Mr. R. A. Hadfield's paper on the production of iron by a new process was next read. The author's object has been to obtain a pure iron; for which purpose he had had recourse to aluminium as an agent. The first result was that he made an alloy of iron and aluminium very rich in the latter constituent, there being no less than 36 per cent. present. In spite of being a failure, so far as the object in view was concerned, a very interesting result was obtained; for although there was no more than a trace of carbon present, the alloy was hard enough to scratch glass. Proceeding on the same lines, however, and working with ferrous oxide and granulated aluminium, a sample of iron containing 99·75 per cent. of that metal was finally obtained at the very moderate cost of about eighteen pence per pound.

Mr. Saniter's paper, describing a new method for the analysis of chrome and ferro-chromium, was the last read at this sitting. This is a further extension of Mr. Stead's modification of Dr. Clarke's process, and has the great advantage of reducing the time occupied in the analysis.

On the afternoon of this day there were several excursions, the chief of which was to Worcester, where the works of the Royal Porcelain Company were inspected. Another party visited the Round Oak Iron and Steel Works, while others proceeded to the glass works, fireclay works, small arms factories, and to other works in and around Birmingham. In the evening there was a very successful reception and entertainment in the Edgbaston Botanical Gardens.

The final sitting of the meeting was on Thursday of last week, when a paper by Mr. Henry Wiggin, on nickel steel, was first taken. In this contribution the advantages of nickel steel as a constructive material were brought forward; its great tensile strength combined with excessive ductility being dwelt upon. Another advantage possessed is freedom from corrosion, as compared with ordinary steel. Instances were given of the nickel steel containing 3¼ per cent. of nickel, which had a tensile strength fully 30 per cent. higher than ordinary steel, and an elastic limit at least 75 per cent. higher. The author does not give any details in regard to cost, which is naturally higher than that of ordinary steel; but speaking upon the subject generally, he was of opinion that the additional price that would have to be charged would generally be more than compensated for by increased efficiency. In the discussion, Mr. W. Beardmore, of Glasgow, said he had been making large quantities of nickel steel for the last two years. This was for armour-plates, but he was now preparing a series of tests to submit to Lloyd's with a view to introducing the material for marine purposes. Mr. Jeremiah Head, who had lately visited America, said that at the works of Mr. Carnegie he had seen large quantities of nickel steel produced at a cost, he was told, of about £7 a ton; but there natural gas of great richness was available. Mr. Thompson, of New York, who had been largely engaged in the manufacture of nickel steel, said that in America 50,000 to 75,000 tons of this material had been produced during the last three years. A German chemist had found that with an alloy of 15 per cent. of nickel almost a new metal was made having a tensile strength of 244,000 lbs. to the square inch, and an elastic limit as high in proportion. He estimated that to build a large battleship of nickel steel would add but 2 per cent. to her cost, whilst the efficiency would be doubled. Mr. Thomas Turner afterwards pointed out that nickel steel was supposed to have a wide range of extension and contraction with variations of temperature, so that if a ship went to the polar regions it might become even feet shorter in its length.

Mr. Smith-Casson's paper, on small cast ingots, was next read. The author claims to have got very good results by casting ingots together from the bottom. This was the last paper read at the meeting.

Thursday afternoon was devoted to an excursion to Stratford-on-Avon, whilst on the following day, Friday, an excursion was made to Kenilworth and to Warwick, where members and their friends were entertained at the Castle by Lord and Lady Warwick.

THE SPECTRUM OF HELIUM.[1]

IN the *Chemical News* for March 29 last (vol. lxxi. p. 151), I published the results of measurements of the wave-lengths of the more prominent lines seen in the spectrum of the gas from clèveite, now identified with helium. The gas had been given to me by the discoverer, Prof. Ramsay; and being from the first batch prepared, it contained other gases as impurities, such as nitrogen and aqueous vapour, both of which gave spectra interfering with the purity of the true helium spectrum. I have since, thanks to the kindness of Profs. Ramsay and J. Norman Lockyer, had an opportunity of examining samples of helium from different minerals and of considerable purity as far as known contamination is concerned. These samples of gas were sealed in tubes of various kinds and exhausted to the most luminous point for spectrum observations. In most cases no internal electrodes were used, but the rarefied gas was illuminated solely by induction, metallic terminals being attached to the outside of the tube.[1] For photographic purposes a quartz window was attached to the end of the tube, so that the spectrum of the gas could be taken "end on."

My examinations have chiefly been made on five samples of gas.

(1) A sample from Prof. Ramsay in March last. Prepared from clèveite.

(2) A sample from Prof. Ramsay in May last. Prepared from a specimen of uraninite sent to him by Prof. Hillebrand. Gas obtained by means of sulphuric acid; purified by sparking.

(3) A sample from Prof. Ramsay in June last. Prepared from bröggerite.

(4) A sample from Prof. Lockyer in July last. Prepared by a process of fractional distillation from a sample of bröggerite sent by Prof. Brögger.

(5) A sample of gas from Prof. Ramsay, "Helium Purissimum." This was obtained from mixed sources, and had been purified to the highest possible point.

In the following table the first four samples of gas will be called:—(1) "Clèveite, R."; (2) "Uraninite, R"; (3) "Bröggerite, R"; and (4) "Bröggerite, L." Only the strongest of the lines, and those about which I have no doubt, are given. The wave-lengths are on Rowland's scale.

The photographs were taken on plates bent to the proper curvature for bringing the whole spectrum in accurate focus at the same time. The spectrum given by a spark between an alloy of equal atoms of mercury, cadmium, zinc, and tin, was photographed at the same time on the plate, partially overlapping the helium spectrum; suitable lines of these metals were used as standards. The measurements were taken by means of a special micrometer reading approximately to the 1/100,000th inch, and with accuracy to the 1/10,000th of an inch. The calculations were performed according to Sir George Stokes's formula, supplemented by an additional formula kindly supplied by Sir George Stokes, giving a correction to be applied to the approximate wave-lengths given by the first formula, and greatly increasing the accuracy of the results.

Wave-length.	Intensity.	
7065·5	5	A red line, seen in all the samples of gas. Young gives a chromospheric line at 7065 5.
6678·1	8	A red line, seen in all the samples of gas. Thalén gives a line at 6677, and Lockyer at 6678. Young gives a chromospheric line at 6678·3.
5876·0	30	The characteristic yellow line of helium, seen in all the samples of gas. Thalén makes it 5875·9, and Rowland 5875·98. Young gives a chromospheric line at 5876.
5062·15	3	
5047·1	5	A yellow-green line, only seen in "Helium Puriss." and in "Bröggerite, R," and "L." Thalén gives the wave-length as 5048.
5015·9	7	A green line seen in all the samples of gas. Thalén gives the wave-length 5016. Young gives a chromospheric line at 5015·9.
4931·9	3	
4922·6	10	A green line, seen in all the samples of gas. Thalén gives the wave-length 4922. Young gives a chromospheric line at 4922·3.
4870·6	7	A green line, only seen in "Uraninite, R." Young gives a chromospheric line at 4870·4.
4847·3	7	A green line, only seen in "Uraninite, R." Young gives a chromospheric line at 4848·7.
4805·6	9	A green line, only seen in "Uraninite, R." Young gives a chromospheric line at 4805·25.
4764·4	2	There is a hydrogen line at 4764·0.
4735·1	10	A very strong greenish blue line, only seen in "Uraninite, R."
4713·4	9	A blue line, seen in all the samples of gas. Thalén's measurement is 4713·5. Young gives a chromospheric line at 4713·4.
4658·5	8	A blue line, only seen in "Uraninite, R."
4579·1	3	A faint blue line, seen in "Uraninite, R." Lockyer gives a line at 4580, from certain minerals. I can see no traces of it in the gas from Bröggerite. A hydrogen line occurs at 4580·1.

[1] From the *Chemical News*, August 23.

[1] *Journal of the Institution of Electrical Engineers*, part 91, vol. xx., Inaugural Address by the President, William Crookes, F.R.S., Jan. 15, 1891.

Wave-length.	Intensity.	
4559·4	2	Young gives a chromospheric line at 4558·9.
4544·1	5	
4520·9	3	A faint blue line, seen in "Uraninite, R." Lockyer gives a line at 4522, seen in the gas from some minerals. Young gives a chromospheric line at 4522·9. It is absent in the gas from Bröggerite.
4511·4	5	A blue line, seen in "Uraninite, R," but not in the others. It is coincident with the strong head of a carbon band in the CO_2 and Cy spectrum.
4497·8	2	There is a hydrogen line at 4498·75.
4471·5	10	A very strong blue line, having a fainter line on each side, forming a close triplet. It is a prominent line in all the samples of gas examined. Young gives the wave-length 4471·8 for a line in the chromosphere, and Lockyer gives 4471 for a line in gas from Bröggerite.
4435·7	9	Seen in "Helium Puriss."
4437·1	1	Young gives a chromospheric line at 4437·2.
4428·1	10	These two lines form a close pair. I can only see them in "Uraninite, R." No trace of them can be seen in the gases from other sources. Young gives chromospheric lines at 4426·6 and 4425·6.
4424·0	10	
4399·0	10	A strong line, only seen in "Uraninite, R." Absent in the gas from the other sources. Lockyer gives a line at 4398 in gas from certain minerals. Young gives a chromospheric line at 4398·9.
4386·3	6	Seen in all the samples of gas. Young gives a chromospheric line at 4385·4.
4378·8	8	These two lines form a pair seen in "Uraninite, R," but entirely absent in the others.
4371.0	8	
4348·4	10	Seen in "Uraninite, R." Lockyer finds a line at 4347 in the gas from certain minerals.
4333·9	10	Probably a very close double line. Seen in "Uraninite, R," and "Clèveite, R." Not seen in the other samples. Lockyer gives a line in the gas from certain minerals at 4338.
4298·7	6	Only seen in "Uraninite, R." Young gives a chromospheric line at 4298·5.
4281·3	5	Only seen in "Uraninite, R."
4271·0	5	Only seen in "Uraninite, R." The strong head of a nitrogen band occurs close to this line.
4258·8	7	Seen in all the samples of gas.
4227·1	5	Only seen in "Uraninite, R." Young gives a chromospheric line at 4226·89
4198·6	9	These three lines form a prominent group in "Uraninite, R," they are very faint in "Clèveite, R," and in Bröggerite, L," but are not seen in "Bröggerite, R."
4189·9	9	
4181·5	9	
4178·1	1	An extremely faint line. Lockyer gives a line at 4177, seen in the gas from certain minerals, and Young gives a chromospheric line at 4179·5.
4169·4	6	Seen in "Helium Puriss."
4157·6	8	A strong line in "Uraninite, R," very faint in "Bröggerite, R," and "L," not seen in "Clèveite, R."
4143·9	7	Strong in "Clèveite, R," in "Helium Puriss.," and in "Bröggerite, L." It is faint in "Uraninite, R," and not seen in "Bröggerite, R." Lockyer gives a line at 4145 in gas from certain minerals.
4121·3	7	Present in all the gases except "Clèveite, R."
4044·3	9	Present in "Uraninite, R," and "Clèveite, R." Absent in the others.
4026·1	10	These lines form a very close pair, seen in all the samples of gas, except "Bröggerite, R." Lockyer finds a line in Bröggerite gas at 4026·5.
4024·15	6	
4012·9	7	Seen in all the samples of gas.
4009·2	7	Seen in "Helium Puriss."
3964·8	10	The centre line of a dense triplet. Only seen in "Clèveite, R," in Helium Puriss.," and "Bröggerite, L." Hale gives a chromospheric line at 3964.

Wave-length.	Intensity.	
3962·3	4	Seen in all the samples of gas.
3948·2	10	Very strong in "Uraninite, R," very faint in "Clèveite, R," and not seen in the others. Lockyer finds a line in gas from Bröggerite at 3947. There is an eclipse line at the same wave-length.
3925·8	2	Seen in "Helium Puriss."
3917·0	2	Seen in "Helium Puriss."
3913·2	4	Only seen in "Uraninite, R," and "Helium Puriss." Hale gives a chromospheric line at 3913·5.
3890·5	9	A very strong triplet, seen in all the samples of gas. Lockyer finds a line having a wave-length 3889 in gas from Bröggerite. Hale gives a chromospheric line at 3888·73. There is a strong hydrogen line at 3889·15
3888·5	10	
3885·9	9	
3874·6	6	Only seen in "Uraninite, R."
3867·7	8	Seen in "Helium Puriss."
3819·4	10	Seen in all the samples of gas. Deslandres gives a chromospheric line at 3819·8.
3800·6	4	Seen in "Helium Puriss."
3732·5	5	Seen in "Helium Puriss." Hale gives a chromospheric line at 3733·3
3705·4	6	Seen in all the samples of gas. Deslandres gives a chromospheric line at 3705·9.
3642·0	8	Only seen in "Uraninite, R."
3633·3	8	Seen in "Helium Puriss."
3627·8	5	Only seen in "Uraninite, R."
3613·7	9	Seen in "Helium Puriss."
3587·0	5	Seen in "Helium Puriss."
3447·8	8	Seen in "Helium Puriss."
3353·8	5	Seen in "Helium Puriss."
3247·5	2	Seen in "Helium Puriss."
3187·3	10	The centre line of a close triplet. Very faint in "Clèveite, R," and "Uraninite, R," and strong in "Helium Puriss." and in "Bröggerite, L." It is not seen in "Bröggerite, R."
2944·9	8	A prominent line, only seen in "Helium Puriss." and in "Bröggerite, L."
2536·5	8	Seen in "Helium Puriss." A mercury line occurs at 2536·72.
2479·1	4	Seen in "Helium Puriss."
2446·4	2	Seen in "Helium Puriss."
2419·8	2	Seen in "Helium Puriss."

Some of the more refrangible lines may possibly be due to the presence of a carbon compound with the helium. To photograph them, a long exposure, extending over several hours, is necessary. The quartz window has to be cemented to the glass with an organic cement, and the long-continued action of the powerful induction current on the organic matter decomposes it, and fills the more refrangible end of the spectrum with lines and bands in which some of the flutings of hydrocarbon, cyanogen, and carbonic anhydride are to be distinguished.

There is a great difference in the relative intensities of the same lines in the gas from different minerals. Besides the case mentioned by Prof. Kayser of the yellow and green lines, 5876 and 5016, which vary in strength to such a degree as to render it highly probable that they represent two different elements, I have found many similar cases of lines which are relatively faint or absent in gas from one source and strong in that from another source.

Noticing only the strongest lines, which I have called "Intensity 10," "9," or "8," and taking no account of them when present in traces in other minerals, the following appear to be special to the gas from uraninite :—

4735·1
4658·5
4428·1
4424·0
4399·0
4378·8
4371·0
4348·4
4198·6
4189·9
4181·5
4157·6
3948·2
3642·0

The following strong lines are present in all the samples of gas:—

7065·5
6678·1
5876·0
5015·9
4922·6
4713·4
4471·5
4386·3
4258·8
4012·9
3962·3
3890·5
3888·5
3885·9
3819·4
3705·4

The distribution assigned to some of the lines in the above tables is subject to correction. The intensities are deduced from an examination of photographs, taken with very varied exposures; some having been exposed long enough to bring out the fainter lines, and some a short time to give details of structure in the stronger lines. Unless all the photographs have been exposed for the same time, there is a liability of the relative intensities of lines in one picture not being the same as those in another picture. Judgment is needed in deciding whether a line is to have an intensity of 7 or 8 assigned to it; and as in the tables I have not included lines below intensity 8, it might happen that another series of photographs with independent measurements of intensities would in some degree alter the above arrangement.

In the following table I have given a list of lines which are probably identical with lines observed in the chromosphere and prominences:—

Wave-lengths observed of helium.	Intensities.	Wave-lengths of chromospheric lines,[1] Rowland's scale.
7065·5	10	7065·5
6678·1	10	6678·3
5876·0	30	5876·0
5015·6	6	5015·9
4922·6	10	4922·3
4870·6	7	4870·4
4847·3	7	4848·7
4805·6	9	4805·25
4713·4	9	4713·4
4559·4	2	4558·9
4520·9	3	4522·9
4471·5	10	4471·8
4437·1	1	4437·2
4428·1	10	4426·6
4424·0	10	4425·6
4399·0	10	4398·9
4386·3	6	4385·4
4298·7	6	4298·5
4227·1	5	4226·89
4178·1	1	4179·5
3964·8	10	3964·0 H[2]
3948·2	10	3945·2 H
3913·2	4	3913·5 H
3888·5	10	3888·73 H
3819·4	10	3819·8 D
3732·5	5	3733·3
3705·4	6	3705·9 D

W. CROOKES.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

MR. ADOLPH SUTRO, well known as the builder of the famous Sutro tunnel on the Comstock lode in Nevada, and now Mayor of San Francisco, has just offered the State University Regents thirteen acres of land within the city limits, on which to erect buildings for the affiliated colleges of the University. In addition to this, he will deed to the Trustee of the city thirteen acres adjoining, as a site for the Sutro library of over 200,000 volumes. The gift is valued at £300,000, and will be worth £400,000 when the contemplated improvements are made.

THE Clothworkers' Exhibition, awarded by the Oxford and Cambridge Schools Examination Board to the best candidate in physical science at the examination held for higher certificates, has been gained by T. W. Fagan, Denstone College, Staffordshire. The exhibition, which is of the value of £52 10*s*. a year, is tenable for three years by the holder as a non-collegiate student at either Oxford or Cambridge.

MR. W. M. GARDNER, Assistant Lecturer in Dyeing in the Yorkshire College, Leeds, has been appointed Head Master of the Chemistry and Dyeing Department of the Bradford Technical College.

THE Calendar of the Durham College of Science, Newcastle-upon Tyne, for the session 1895–96 has just been published, and also separate prospectuses of the day and evening classes.

SIR A. ROLLIT asked the First Lord of the Treasury on Tuesday whether the Government intended, and when, to propose legislation in pursuance of the report of the Gresham Commission or the University of London. In reply, Mr. Balfour said that legislation will be impossible on the subject in the course of the present Session, and he was unable to say what action will be taken by the Government.

THE operations of the City and Guilds of London Institute are divided broadly into two branches, viz. the educational work of three London Colleges, and of the Technological Examinations. The new edition of the programme of the latter, including regulations for the registration and inspection of classes in technology and manual training, has come to hand. It is more bulky that any of the previous programmes of the examinations, which fact may be taken as an indication that the Institute is growing with the extension of technical education. The technical subjects in which examinations are held now number sixty-three. A practical examination for "electric wiremen" has been added, and a corresponding addition has been made to the syllabus for the preliminary examination in electric lighting. The syllabuses of several other subjects have been modified, and that in wood-work has been rewritten.

THE forty-second Report of the Department of Science and Art has just been received. A noteworthy point shown by the statistics contained in it is the diminution in the number of science schools, classes, and students under instruction, brought about by the abolition of grants for second-class passes in the Elementary Stages of Science subjects. As compared with the previous year, the number of schools in 1894 had decreased by 152—from 2754 to 2602; the number of pupils had decreased by about 10,000—from 193,431 to 183,120; and the number of classes in different branches of science had decreased by 908—from 10,341 to 9433. This diminution is attributed to the changes in payments on results, "and also probably to the opening of numerous technical classes by the local authorities in different parts of the country, which have drawn away the students from the classes in pure science. The decrease in the number of schools and classes is owing partly to the same cause, and partly to the amalgamation of smaller schools, or to their absorption in the more prosperous and better supported schools in their neighbourhood, many classes in which instruction of a very elementary nature only had been given being at the same time closed." A determination has been made of the average ages at which students in the Department's science classes obtained successes in the Elementary and Advanced Stages, It was found that the average age in Day Schools for a student to obtain a first-class elementary success was about 14, and for a second-class Advanced about 15¾, while in the evening classes the ages were respectively about 18 and ·21. In addition to statistics, and information as to science instruction and technical education, the Report contains the reports of the work of the Geological Survey of the United Kingdom, and of the Committee on Solar Physics.

[1] "A Treatise on Astronomical Spectroscopy," by Dr. J. Scheiner, translated by E. B. Frost. Boston, 1894.

[2] The wave-lengths to which the initials D and H are added are wave-lengths of lines photographically detected in the spectrum of the chromosphere by Deslandres (D) and Hale (H). Their photographs do not extend beyond wave-length 3630. Prof. Lockyer (*Roy. Soc. Proc.* vol. lviii. p. 116, May 1895) has already pointed out fourteen coincidences between the wave-lengths of lines in terrestrial helium and in those observed in the chromosphere, the eclipse lines, and stellar spectra.

SCIENTIFIC SERIALS.

American Journal of Science, August.—The earth a magnetic shell, by Frank H. Bigelow. This paper gives the vectors of the polar magnetic field at the earth due to the sun, together with certain deductions from their intensity and distribution. Unless the magnetic permeability of the interior of the earth is less than 1, which is highly improbable, the polar vectors obtained must be interpreted as stream lines flowing round an obstacle in the interior of the earth. In other words, the outer stratum of the earth is permeable to the external magnetic forces, while the nucleus is not; that is to say, the earth is a magnetic shell. The diameter of this impermeable nucleus is calculated at 6340 miles, and the thickness of the shell at 790 miles. The external polar field is concentrated in two belts, one of which is the auroral zone round the poles, and the tropical belt at the two tropics. It is a pity that most magnetic observatories are placed on the mid-latitude depression. Since both the magnetic and the electromagnetic vectors represent cosmical forces of the same type as gravitation, connecting the sun with the planets, they should be taken into account in general theoretical astronomy, or the celestial mechanics of the solar system. It is possible that certain irregular motions as yet unexplained may be accounted for on the basis of these additional forces.—On the velocity of electric waves, by John Trowbridge and William Duane. The apparatus used for photographing successive sparks whose images were thrown on the plate by a revolving mirror, was substantially the same as previously described; but the dielectric used was plate glass, and the terminals were made of cadmium. The average value for the velocity of electric waves travelling along two parallel wires was $3{\cdot}0024 \times 10^{10}$ cm. per second, a value which differs from the velocity of light by less than 2 per cent. of its value, and from the ratio between the two systems of electromagnetic units by even less.—On the distribution and the secular variation of terrestrial magnetism, by L. A. Bauer. The distribution and the secular variation appear to be closely related, they obey similar laws, and seem to be connected in some way with the rotation of the earth. The following are some of the laws traced by the author: The mean declination along a parallel of latitude is always westerly, the minimum occurring near the equator. The mean inclination along a parallel of latitude follows quite closely the law: $\tan I = 2 \tan \phi$ where I is the inclination and ϕ the geographical latitude. The minimum range in declination, and the minimum average secular change from 1780 to 1885 along a parallel of latitude occurred near the equator, the values generally increasing upon leaving the equator. The corresponding values in the case of inclination were maxima, and decrease upon leaving the equator.—Complementary rocks and radial dykes, by L. V. Pirsson. "Complementary rocks" are such that if the basic types are combined with the accompanying acid types, they give the composition of the main type of magma with which they are associated.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 1.—Dr. E. Lindon Mellus gave the results of experimental lesions of the cortex cerebri in the Bonnet Monkey. The experiments were confined to the left hemisphere, and consisted in the removal of minute portions of the cortex (generally about 16 sq. mm.) representing the centres for movements of the hallux and thumb, as well as several centres within the facial area. The animals recovered from the operation without any sign of sepsis, and were killed from ten to thirty-five days after the operation, the brains and cords hardened in Müller's fluid, and stained by the Marchi method. Numerous association fibres, both coarse and fine, connecting the lesion with the surrounding cortex, were found degenerated. These were always most numerous in the immediate neighbourhood of the lesion, and mostly distributed to the two central convolutions.

From lesions in the hallux centre degenerated association fibres were distributed to both central convolutions to the level of the inferior genu of the fissure of Rolando, to the parietal lobule, to the posterior portion of the superior frontal convolution, to the lobulus paracentralis, precuneus, and the gyrus fornicatus. Degenerate fibres crossed in the middle third of the corpus callosum and were distributed to corresponding portions of the right cortex, the degeneration on the right side being considerably less than on the left. In the lower levels of the left internal capsule the degeneration was scattered over the area of the middle third of the posterior limb, being somewhat anterior to its position in higher levels. From the posterior limb of the internal capsule most of the fine degeneration passed into the optic thalamus, while the coarse passed on into the crus, where it was found in the middle third. Many coarse degenerate fibres passed from the crus into the substantia nigra. At the decussation of the pyramids the tract divides, the larger portion crossing to the opposite lateral column, while the smaller goes to that of the same side. The amount of degeneration passing to the lateral column of the same (left) side varies from a third of all the degeneration in one case to about a twentieth in the others. In each case a few degenerate fibres remain in the left anterior column after the completion of the decussation. The amount varies in different cases, and is not apparently dependent on the proportion of degenerate fibres passing to the lateral column of the same side. The relations and extent of the degenerated areas remain unchanged throughout the cervical and dorsal cord. The degeneration in the crossed tract of each side is evenly scattered over its entire area, the two sides only differing in the density of the degeneration. In the lumbar region the degeneration in each crossed tract and in the left anterior column begins to go out, and, in the only case examined at that level, the degeneration had not all disappeared at the level of the third sacral root.

In lesions of the thumb centre (ascending parietal convolution just above the inferior genu of the fissure of Rolando) degenerated association fibres were distributed to the central convolutions from the border of the longitudinal fissure nearly to the fissure of Sylvius. To a less degree, but in varying amounts, degenerate fibres were traced to the posterior portions of the middle and inferior frontal convolutions, to the supra marginal and angular gyri, the upper or posterior portion of the superior temporal convolution, the precuneus and lobus quadratus and paracentralis and the gyrus fornicatus. Degenerate fibres crossing in the middle third of the corpus callosum were distributed to the corresponding convolutions of the right side, though less in amount and area of distribution. There was a remarkable variation in the size of the fibres distributed to the central convolutions of both hemispheres, being coarse above the level of the lesion and fine below, thus corresponding with the measurements made by Bevan Lewis of the corpuscles of the fourth layer of the cortex in this region. The arrangement and distribution of the degeneration in the posterior limb of the left internal capsule was the same as in lesions of the hallux centre, and there was the same passage of fine degeneration from the capsule to the thalamus. The amount of coarse degeneration passing from the crus to the substantia nigra was much greater than in lesions of the hallux centre, varying from a half to nearly the whole of the degeneration reaching the crus. In one case only was there a division of the degenerated tract at the decussation of the pyramids such as was observed in lesions of the hallux centre, and the amount of degeneration passing to the left lateral column was less than in either of the hallux cases. This was also the only case in which a few degenerate fibres remained in the left anterior column after the completion of the decussation. In two cases some degeneration was found in the right capsule and crus occupying the same position and following the same course as the degenerate fibres in the left capsule and crus, but its direct connection with the lesion could not be demonstrated. From the level of the seventh cervical root downward the degenerate fibres steadily and gradually disappeared, and at the level of the third dorsal root there were none left, thus confirming the results obtained by excitation of the nerve roots.

The lesions within the facial area were, with one exception, along the upper border of the fissure of Sylvius. The single exception was in the ascending frontal convolution near the inferior genu of the fissure of Rolando. In all these experiments the degenerate association fibres were mostly distributed to the central convolutions, but in some instances to the posterior portions of the middle and inferior frontal, the superior and inferior temporal convolutions, and the supra marginal gyrus. The degeneration in the corpus callosum was mostly in the anterior half of the middle third, and the distribution of degenerate fibres to the convolutions of the right hemisphere more nearly corresponded to that of the left than in lesions of the hallux or thumb centre. In all the lesions of the facial area the degenerations in the uppermost levels of the capsule were in the anterior portion, gradually moving backward in the lower levels until they were found in the same position (the middle third of the posterior limb) as the degenerations resulting from lesions of the hallux and thumb centres. In this backward movement of the facial fibres in the capsule there is necessarily a level in which they envelope the genu, which would account for the fact that they are generally described as occupying that position. As in the other lesions, most of the fine degeneration passed from the internal capsule to the thalamus. In the crus the degeneration was scattered pretty evenly over the area of the middle third, exactly corresponding to the situation of the pyramidal fibres in the other experiments, and not occupying the position usually assigned to them, mesial to the pyramidal fibres. No degeneration was found in the accessory bundle to the fillet. As in the other experiments, degenerate fibres were found passing from the crus to the substantia nigra. The remaining degenerate fibres began to leave the left pyramidal tract at the junction of the pons and medulla, passing as single degenerate fibres to the facial nucleus of one or the other side. Below the level of the facial nuclei these fibres passed to the motor nuclei of the glossopharyngeus and vagus on both sides, the majority crossing the raphe to reach the nuclei on the opposite side. Occasional fibres were observed which apparently passed to some termination dorsal to these nuclei. This movement of degenerate fibres continued as far as the sensory decussation. A few degenerate fibres (probably thumb or finger fibres) remained in the pyramid and crossed in the decussation to the right lateral column, and disappeared in the lower cervical or upper dorsal region. In some of the facial lesions there were appearances of degeneration in the right internal capsule, but its connection with the lesion could not be demonstrated.

PARIS.

Academy of Sciences, August 19.—On matches tipped with explosive mixtures, by M. Th. Schlœsing. The author has experimented with a number of mixtures of substances with the view of finding a paste endowed with the properties of that mixture containing white phosphorus, and not having its poisonous character. The results show that it is necessary to use potassium chlorate, red phosphorus, ground glass, and glue or its equivalent, and that it is by no means a simple matter to find a perfect substitute for the paste used in tipping common matches.—On the storms and earthquakes in Austria during June, by M. Ch. V. Zenger. It is shown that during this period: (1) Solar activity has been very great. (2) Magnetic perturbations have been very ample and frequent. (3) Earthquakes and cyclonic storms of extraordinary violence have coincided with the appearance of numerous and brilliant meteorites, and with the passage of numerous shooting stars.—On equilateral hyperbolæ of any order, by M. Paul Serret.—On permanent deformations and the rupture of solid bodies, by M. Faurie.—On the conducting power of mixtures of metal filings and dielectrics, by M. G. T. Lhuillier.—Researches on the combinations of mercury cyanide with chlorides, by M. Raoul Varet. A thermochemical study on the combinations of mercury cyanide with the chlorides of sodium, ammonium, barium, strontium, calcium, magnesium, zinc, and cadmium. The solutions of these double salts do not give the isopurpurate reaction with a picrate at 30°, and hence the cyanogen remains wholly in combination with the mercury at this temperature. On boiling, however, there is evidence of interchange of a small proportion of cyanogen for chlorine.—Thermal researches on cyanuric acid, by M. Paul Lemoult. As in the case of phosphoric acid, the addition of each of three equivalents of alkali is marked by a different evolution of heat; the acid is a tribasic *mixed* acid.—Heat of combustion of some β-ketonic ethereal salts, by M. J. Guinchant.—Determination of heat disengaged in alcoholic fermentation, by M. A. Bouffard.—On the gum of wines, by MM. G. Nivière and A. Hubert.—On the migration of phosphate of lime in plants, by M. L. Vaudin.—Origin and rôle of the nucleus in the formation of spores and in the act of fecundation, among the Uredineæ, by M. Sappin-Trouffy.

BERLIN.

Physiological Society, July 5.—Prof. Munk, President, in the chair.—Prof. H. Munk spoke on contractures he had observed in monkeys after removal of the motor areas of the cerebral cortex.—Prof. Gad reported some experiments of Prof. Nicolaides (of Athens), which had demonstrated the presence of fat granules in the pyloric gastric glands and in Brunner's glands.

July 19.—Prof. du Bois Reymond, President, in the chair.—Dr. Schultz demonstrated micro- and macro-scopically the contraction of the unstriated muscle fibres of the stomach of Salamander. It was seen that the excised strips only contract when they are cut out in the direction of the long axis of the fibres, not when the fibres are cut through at right angles to their axis. Dr. Rawitz had stained the lymphatic glands in the mesentery of *Macacus cynomolgus* by his "additive" method. He found the nuclei of the cells were generally placed excentrically, and contained a minute round chromatin patch. The linin network was marked by minute nuclei at the points of intersection and attachment. The structure of the plasma was quite indeterminate, but it appeared to contain a small round body, 2 to 3 μ in diameter, which stained somewhat deeply, and which he regarded as van Beneden's "attraction sphere." Dr. Schultz had examined the optical properties of unstriated muscle-fibres of vertebrates in polarised light. It was found that although single fibres were not doubly refracting, a thicker layer of them was so quite distinctly. From this he concluded that the *single* fibres are in reality doubly refracting, but too feebly so to be perceptible. The double refraction became less during contraction, from which he concluded that, in accordance with von Ebner's theory, the anisotropic property of the fibres is due to differences in their internal tensions, the latter being greater in a transverse than in a longitudinal direction.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Erdmagnetische Messungen in Österreich: J. Liznar (Wien).—Durham College of Science, Calendar (Reid).—Die Schöpfung des Menschen und Seiner Ideale: Dr. W. Haacke (Williams and Norgate).—Elements of Coordinate Geometry: Prof. S. Loney (Macmillan).—A Laboratory Manual of Organic Chemistry: Prof. Lassar-Cohn, translated by Prof. A. Smith (Macmillan).—Astronomische Mittheilungen von der Königlichen Sternwarte zu Göttingen: Prof. W. Schur, Vierter Theil (Göttingen, Kaestner).—Symons's British Rainfall, 1894 (Stanford).—Forty-second Report of the Department of Science and Art (Eyre and Spottiswoode).

PAMPHLETS.—Geological Survey of Alabama; Report upon the Coosa Coal Field: A. M. Gibson (Montgomery).—Plants and Gardens of the Canary Islands: Dr. D. Morris (Spottiswoode).

SERIALS.—Botanische Jahrbucher für Systematik, Pflanzengeschichte und Pflanzengeographie, Einundzwanzigster Band, 3 Heft (Williams and Norgate).—Journal of the Institute of Jamaica, April (Kingston).—L'Anthropologie, Tome 6, No. 4 (Paris).—Quarterly Journal of Microscopical Science, August (Churchill).—Journal of the Royal Horticultural Society, August (117 Victoria Street).—Longman's Magazine, August (Longmans).

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