THURSDAY, JANUARY 5, 1888.

ELECTRICITY FOR PUBLIC SCHOOLS AND COLLEGES.

Electricity for Public Schools and Colleges. By W. Larden, M.A. (London: Longmans, Green, and Co., 1887.)

THIS is a book which possesses many good points, but which becomes, on close acquaintance, painfully disappointing, and even irritating, to the reader. The author has undoubtedly spared no pains to make it full of information; but its very fullness becomes bewildering, owing to the way in which the material is cut up and put together. One might almost imagine that it had been reduced from a much larger work, chiefly by means of deletions, and without the rounding-off of the angularities which such a process would inevitably develop.

The science of electricity and magnetism is, without question, an experimental science; and the author of the present work does not offer his book to his readers as a book on the mathematical side of this experimental science; but as an elementary book suitable for higher schools and for Colleges. He confines himself as to mathematics by assuming "no more mathematical knowledge than is usually possessed by the higher boys in a classical school." Under these conditions we should expect to have a book containing exact and well-finished descriptions of experiments and apparatus, along with explanations of phenomena and with theory brought down to correspond with our present knowledge on this most fascinating subject. Expectations or hopes such as these are very far from being realized in the book before us.

The amount of material collected by the author is undoubtedly very great. The number of instruments and machines referred to and described is enormous. The descriptions are, however, often unsatisfactory, nor are they written with any attempt at finish or good taste The book is supposed to be for the use of well-educated beginners; but we think it would be difficult to find a worse model for boys or young men as to the writing of descriptions of apparatus or of experiments. Often the heading of a paragraph has half the duty to perform; the remainder may be done by a diagram, which is lettered in a tantalizing way, as if a description had been intended.

Here, for example, are the descriptions of two of the most important frictional electric machines:—

"II. *The Common Plate Machine.*—In this there is nothing essentially different from the cylinder machine. A glance at the figure will explain all. There are generally two rubbers; and in this form of machine they cannot well be insulated, if required; so the machine cannot be used as a source of both + and − electricity. Instead of glass, ebonite plates may be used, the rubbers being of amalgamated silk.

"III. *Winter's Plate Machine.*—In this the rubber and the points of the prime conductor are more widely separated; and the prime conductor can therefore acquire a higher level (or potential) of charge without discharge over the glass to the rubber. The rubber can be insulated or not, as required. A curious feature is an addition to the prime conductor in the shape of a large ring of brass inclosed in baked wood. This ring increases the 'capacity' of the prime conductor."

The description of Winter's machine is not even supplemented with a figure; and we doubt if any student reading the description will form the faintest conception of the nature of the machine or of the "ring of brass inclosed in baked wood." "Amalgamated silk," too, is a shortened expression, which is, to say the least, as inappropriate as it is uncommon. These descriptions have not been specially chosen for inadequacy. There are numbers no more complete than these.

Probably the chapter which will be found most satisfactor by learners is the long and important Chapter X., which deals with electro-static potential. This, with the exception of the first two or three sections, is very complete and well given. The subject is explained with great clearness, and with abundant reference to numerical calculation.

The chapters on dynamo-electric machines and on motors may also be considered fairly good for an elementary text-book. The learner will obtain in these chapters a sufficient account of the principles of these machines, given with satisfactory clearness.

In his treatment of the subject of units, and particularly of the electro-magnetic units, the author is singularly unhappy. In an elementary book, or in any book on this subject, whether elementary or advanced, it must be considered a fundamental mistake to omit a full and clear explanation of the foundation and derivation of the absolute electro-magnetic unit of resistance; and it is utterly unsatisfactory to give, as a definition of the unit of resistance, the remark merely that "Ohm's law defines the unit of resistance as that through which unit electromotive force gives unit current." It was not in this way that the absolute unit of resistance was fixed upon, and the original definition is certainly worthy of the attention of the student. Taking the statement given above, however, and turning to "Ohm's law" for information, the learner finds no statement of this law in words, but merely the following:—

"Ohm's law is that—

$$C \text{ is proportional to } \frac{E}{R},$$

or

$$C = k\frac{E}{R}."$$

Such a statement as this might perhaps, if reproduced for the benefit of an examiner, serve to conceal the ignorance of the individual under examination, and might leave the examiner so uncertain that he would be obliged, though unwillingly, to award half marks to the answer; but to the student it can do no real good.

There is but one other remark on the electro-magnetic unit of resistance, and it is almost equally infelicitous with what is quoted above. It is contained in a "note" on "Determination of Units," and is to the effect that "resistance can be measured by observation of the heat evolved when a known current flows through the conductor in question." A very slight acquaintance with possibilities in experimenting would dispel any such idea.

In connection with explanations regarding units we meet here the customary sections on "Dimensions of

Units," but it seems to us that too much importance is commonly given to the well-known table of dimensions. Without very full and very clear explanations of the whole subject (and these are not to be found in the book before us), the table and the remarks given in connection with it are worse than useless; they only serve to confuse an intelligent pupil whose own common-sense will carry him safely through any calculation which he can be reasonably expected to make.

We cannot avoid calling attention, before closing this notice, to some most painful defects in style, because it is, we are of opinion, of the utmost importance that students should be trained from the very beginning to write and speak with due respect to ordinary proprieties of literary composition. It is not to the faults of English grammar and to uncouthness of language that we would call attention most particularly, though such faults abound. Thus, on p. 16 the student is recommended to "puzzle this out"; and on p. 72 we find the following sentence:—

"There *will be* a fall of potential, or an electrical hill, from this body A down to the walls, &c. (*sic*). Such *was* the case if only we change the sign of the charge in Chapter IV. § 14."

On p. 248 we find the following:—

"*Failure of a Smee's Cell to decompose Water.*—If the back E.M.F. *e* of an electrolytic cell *would be* greater than the *E.M.F.* E of the battery, then such a battery *will* fail to drive a current through, and decompose, such a cell."

The worst fault in style is, however, the introduction, on every page of the book, often in every line, of contractions of all sorts. Thus we have, through the whole of the electro-static part, + and − for "positive" and "negative"; and we have bodies "+ly electrified" and "−ly electrified." Throughout the electro-chemical part we scarcely once have hydrogen, or zinc, or sulphuric acid mentioned by name, but always H, Zn, and H_2SO_4. This becomes confusing, to say the least, when the author is dealing with the "*Connection between E.M.F.'s and 'Heats of Combination.'*" Here, "if we represent by H_{Zn} the heat in calories evolved by the solution of 1 gramme of Zn in dilute H_2SO_4, then H_{Zn} is called the heat of combination of zinc with dilute H_2SO_4."

Worse, perhaps, is the use of "ΔV" and "the algebraic sum of the different ΔV's" where "ΔV" stands for the words "difference of potentials." This is done everywhere throughout the first part of the book; and by and by, when we are introduced to ΔV's producing E.M.F.'s, and to "Thomson and Peltier E.M.F.'s," human patience absolutely fails. In the list of abbreviations we are told that a second of time is denoted by 1″ "sometimes." It ought to be *never*.

It is scarcely necessary to say that, were there no merits in the book before us, it would hardly be worth while to enter into a discussion of its faults. But the work is really of great value, and were the materials somewhat rearranged, the writing improved, nine-tenths of the paragraph breaks taken away, and the multitude of notes incorporated with the text, it would prove a most important text-book in electricity and magnetism.

INDO-CHINA AND THE INDIAN ARCHIPELAGO.

Miscellaneous Papers relating to Indo-China and the Indian Archipelago. Reprinted for the Straits Branch of the Royal Asiatic Society. Second Series. Two Volumes. (London: Trübner and Co., 1887.)

THE new series of papers relating to Indo-China, like its predecessor, consists of reprints from various periodicals which are not within the reach of ordinary readers. Thus in the present volumes we find papers of great interest, and some of considerable importance, reproduced from the Journal of the Royal Geographical Society of forty years ago, from the Journals of the Asiatic Society of Bengal, of the Royal Asiatic Society, and from the publications of various Dutch Societies. With regard to the latter, it may be said that they are the most valuable papers in the volumes, for the Dutch have long studied with great assiduity the land and people under their rule in the Malay Archipelago. Their scientific services, in Java especially, are recruited from Holland with the utmost care; the members are spread over the scattered Dutch possessions from Northern Sumatra to New Guinea; they are constantly studying the problems presented to them by man and Nature around them; and the consequence is that the *Verhandelingen* of the Society of Arts and Sciences at Batavia, the *Indische Tijdschrift*, and other publications in the mother country, as well as in Java, are full of papers written by skilled and qualified persons who have devoted special attention to subjects connected with the Malay Archipelago. The editor of these volumes is indebted to these Dutch publications for such papers as that on the rocks of Pulo Ubin, by Mr. J. R. Logan, the greatest English student of this region that ever lived, although there are certain members of the Straits Civil Service who promise to rival him; for Mr. Groeneveldt's "Notes on the Malay Archipelago and Malacca," a modest title under which is concealed a learned examination of a vast quantity of Chinese literature with a view to ascertaining what the Chinese knew about the region; Father Borie's account of the Mantra tribe, and several others.

The experiment of collecting in this way from various sources the papers relating to a particular region is, we believe, a novel one. In this instance it appears to be a success. Here in four volumes, obtainable at a moderate price, we have the contents—so far as they relate to the Malay Peninsula and Archipelago, and appear to a skilled editor to be of permanent value—of more than a score of periodicals, many of which are quite inaccessible to ordinary students, and which, even in London, could only be examined in the British Museum, the India Office, and possibly the Royal Asiatic and Royal Geographical Societies. The Council of the Straits Society, which advanced the funds for this excellent undertaking, is to be congratulated on its public spirit, and we trust it will not lose, even in a pecuniary sense. Whether it does or not, it has placed every student of the region within which its members labour under an obligation by the production of these volumes. Other learned Societies in various parts of the globe might well emulate this example, for there is nothing more laborious or bewildering than to hunt through old periodicals without adequate indexes and without an exact reference, for a valuable paper. The number of papers of permanent value in these old periodicals is very small: subsequent researches have thrown them out of date; the mere efflux of time has proved some of them to be useless; many deal with temporary subjects, which are now of no importance to anyone. Such a periodical, for example, as the old *China Repository*, printed partly in Canton—the Canton of the old days—now fetches an absurd price. Sets have been sold in recent years at from £30 to £50; yet all that it contains of value now could be placed in two volumes such as these before us. The demand for special works of this character, however, is too small to induce any publisher to incur the risk of producing them; and hence it is that we are thrown back on the learned Societies, which represent the students of to-day, to place within our reach the labours of past generations of scholars, and of the literary and intellectual fathers that begat them. This, however, is a question for the Societies themselves, for their own members must feel more acutely than anyone else the truth of these observations.

We have already mentioned a few of the papers of scientific interest in the present series. If Mr. Logan's paper on the peculiar rocks of Pulo Ubin, an island near Singapore, is not out of date at present, it probably soon will be if the long-promised survey of the part of the Malay Peninsula under British influence is to be thoroughly carried out. Dr. F. Stoliczka has a short paper on some species of Malayan Amphibia and Reptilia, and a longer one on the land-shells of Penang; while Father Borie describes the Mantras, amongst whom he laboured as a missionary for some years. This is one of the aboriginal tribes of the peninsula, which were driven inland by the great Malay invasion of the twelfth century. Of these, the Karens inhabit the north and part of Burmah, the Semangs the States of Kedah, Perak, and Selangore, the Mantras the region lying between the latter territory and Mount Ophir, the Jakons and Sambinbangs the southern part of the peninsula. The writer describes the manners and habits of the people in some detail. A most interesting paper, and one of the longest, is Dr. Friederich's account of the language, literature, religion, and castes of the people of Bali, an island which occupies a peculiar relation in the history of the civilization of the Malay Peninsula and Archipelago. The editor in his introduction describes that position in these words:—

"The continued existence, in unabated vitality, of a nationalized Hinduism, blended with pre-Hindu customs and practices, among a spirited and vigorous people is not only . . . a kind of commentary on the ancient condition of the natives of Java, it allows us also to draw a fair inference as to the kind of Hinduism at one time prevailing in other parts of Malaysia less favoured by historical records, where ruthless Islam has since obliterated to a great extent the traces of other creeds, traditions, and institutions. It is, indeed, essential to a proper understanding and estimate of the religious and social condition of the various and wide-spread Malayan tribes that the influence which Hindu civilization has, in a greater or lesser degree, exerted upon them, should as far as possible be investigated."

It should be mentioned that the last number of the Proceedings of the Dutch Geographical Society contains a paper on the same subject by Count Limburg Sturm, who visited the island last year.

Finally, there are certain "Notices on Zoological Subjects," and "Descriptions of Malayan Plants," reprinted from an English periodical published at Bencoolen nearly seventy years ago, with a note to the letter by Sir Joseph Hooker and Mr. Hervey, correcting the terminology. In the preface the editor quotes part of a letter from Sir Joseph pointing to a speedy investigation of the flora of the Malay Peninsula, for which he has urged the Colonial Government to contribute funds. Seeing that Dr. Rost has had to go back to 1820 for an account of the flora, it seems almost time that Sir Joseph Hooker's advice should be taken by Sir Cecil Smith and the Legislative Council of the Straits Settlements.

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873-76, *under the command of Capt. George S. Nares, R.N., F.R.S., and the late Capt. F. T. Thomson, R.N.* Prepared under the superintendence of the late Sir C. Wyville Thomson, Knt., F.R.S., &c., and now of John Murray, one of the Naturalists of the Expedition. Zoology—Vol. XXII. (Published by Order of Her Majesty's Government, 1887.)

VOLUME XXII. contains the Report, by Dr. Günther, Keeper of the Department of Zoology in the British Museum, on the deep-sea fishes collected during the cruise.

Originally it was intended to fix an arbitrary depth as distinguishing between the shore and deep-sea fishes, and accordingly, in the author's previous Report on the shore fishes of the *Challenger* Expedition, all those fishes captured at a less depth than 350 fathoms were treated as more or less littoral forms. However, the subsequent Norwegian and North American explorations brought to light instances of fishes with an unmistakably bathybial organization occurring at a much shallower depth than the forms discovered by the *Challenger*; or, on the other hand, showed that certain littoral forms descend not only to 100 but even to beyond 300 fathoms.

In the present Report, the 100-fathom line is adopted as the boundary at which, with the extinction of sunlight, the bathybial fauna commences, sporadically, no doubt, and largely mixed with surface forms.

The material which forms the subject of this Report consisted of 794 specimens, of which 610 were obtained during the voyage of the *Challenger*, 88 on the cruises of the *Knight-Errant* and *Triton*, and 96 from other sources. These specimens are referred to 266 species, 177 falling to the share of the *Challenger*, and 14 being due to the exploration of the Faroe Channel. The number of new species discovered by the *Challenger* amounts to 144, whilst by the deep-sea exploration of the Faroe Channel 10 species have been added to the fauna of the British seas.

In the introduction we have a history of our present knowledge of the fish-fauna of the deep-sea, some account of the characteristics of deep-sea fish, and an account of their vertical and horizontal distribution.

While no distinct bathymetrical zones, characterized by peculiar forms, can be defined, yet the following table clearly shows that the abundance of fish life decreases with the depth. There have been found between

Fathoms.	Species.
100–300	232
300–500	142
500–700	76
700–1500	56
1500–2000	24
2000–2900	23

While no doubt this decrease in numbers is partly due to the extreme difficulty of investigating the deep-sea fauna, it cannot but be also regarded as pretty certain that, while locally abundant as to individuals, the number of species found is but small.

The descriptions of the new genera and species, though abounding with interesting details in reference to the many strange forms described, cannot with the space at our disposal be even summarized,—they are such as would have been expected from the known skill and judgment of Dr. Günther ; but we must find room for some allusion to the Report on the structure of the phosphorescent organs, on the head of Ipnops, by Prof. H. N. Moseley, and on the structure of the phosphorescent organs of fishes, by Dr. R. von Lendenfeld.

In *Ipnops murrayi* the eyes as well as the optic nerves are completely absent, but a pair of symmetrical luminous organs are to be found on either side of the median line of the upper flattened surface of its head, the upper wall of the skull where it covers them being completely transparent.

These phosphorescent organs are composed of hexagonal columnar masses, arranged with considerable regularity in rows, and resting inferiorly on a pigmented connective-tissue layer. Each hexagonal column is composed of a number (from thirty to forty) of transparent rods, disposed side by side at right angles to the outer surface of the organ, and with their bases applied against the concave surface of large hexagonal pigment cells, one of which forms the base of each hexagonal column. The basal pigment cells are also hexagonal in outline, and are cup-like, concavo-convex in form, and of the same breadth as the hexagonal columns. These organs receive a rich blood supply, and there appears little room for doubt but that the nerve supply comes from the fifth nerve. No trace of any other nerve supply has been found. From a comparison of these organs with those of a similar nature in other fish, the author concludes that they are but highly specialized and enormously enlarged representatives of the phosphorescent organs on the heads of such allied Scopelids as *Scopelus rafinesquii* and *S. metopoclampus*.

The Report of Dr. von Lendenfeld is of a more general character, treating as it does of the phosphorescent organs of most of the known phosphorescent fishes, though not alluding to those of Ipnops. These organs are classified into the regular ocellar organs and the irregular glandular organs. Both these classes are again subdivided in reference to their form or position ; and in conclusion we have a comparison of the different phosphorescent organs of fishes, and of these as compared with similar organs in other animals.

Dr. von Lendenfeld sums up his investigations as follows:—(1) The phosphorescent organs of fishes are more or less modified glands which have partly been developed from simple slime-glands in the skin, and partly in connection with the slime-canal system ; (2) the typical clavate cells are modified gland-cells ; (3) the accessory reflectors and sphincters are developed from the skin around and below the gland ; (4) the large suborbital organs are innervated by a modified branch of the trigeminus, and the other organs by the ordinary superficial nerves.

A splendid atlas of plates accompanies this volume. Of these, sixty-six represent the new species described by Dr. Günther, and several of them are folding plates ; two illustrate the anatomy of the phosphorescent organs of *Ipnops murrayi ;* and the remaining five are drawn by Dr. von Lendenfeld and illustrate in a very beautiful manner his Report on the phosphorescent organs just alluded to.

SALINE DEPOSITS.

Die Bildung des Natronsaltpeters aus Mutterlangensalzen. By Dr. Carl Ochsenius. (Stuttgart : E. Koch, 1887.)

THIS book is a very valuable contribution to the history of saline deposits in general, but it is especially useful on account of the author's detailed description of the salt-beds of Chili and Peru, to the study of which he mainly devotes his attention. He discusses the various theories which have from time to time been advanced to account for the formation of Chili saltpetre (sodium nitrate), and shows that it must be regarded as the product of the action of oxidizing guano on certain mother-liquors containing carbonate of soda.

The salt-beds on the west coast of South America are found in the rainless district which stretches from Payta (near Amotape), in Peru, as far south as the twenty-sixth parallel. This region forms a narrow strip along the coast-line, and rarely exceeds twenty-five miles in width. It is bounded on the east by a chain of the Andes, and in the southern portion of the district the coast is fringed with low-lying hills, known as the coast Cordilleras. The author considers that, before the upheaval of the Andes, salt began to deposit in certain bays, which had been wholly or partially shut off from the sea by the gradual formation of an intercepting bar. Then, while the process of evaporation was still incomplete, the district was raised by volcanic action, and the mother-liquors from the salt lakes eventually escaped, running down into the valleys, and, where they encountered no obstacle, reaching the sea. The coast Cordilleras acted as a barrier in the southern portion of the district ; while in the northern part the liquors doubtless returned to the sea. The volcanoes which produced the aforesaid upheaval exhaled immense volumes of carbonic acid gas, and the author considers that a portion of the sodium chloride in the mother-liquors was thus converted into sodium carbonate. (The co-existence of borates goes far to confirm the source of carbonic acid.) The coast in this part of Chili is studded with small islands containing deposits of guano rich in ammonia. The guano dust is carried by the prevailing west winds far into the country, and would fall into the mother-liquor lakes, where, on exposure to the air at a warm temperature, it would gradually oxidize to nitrate, and, acting on the sodium carbonate, would form sodium nitrate (Chili saltpetre).

The "caliche" (crude saltpetre) is most variable in appearance and in the percentage of nitrate which it contains. The various substances, other than sodium nitrate, which are found in the Tarapaca and Atacama deposits are described at length by the author, who compares them with those which are found at Stassfurt, and he traces in the comparative prominence of the more soluble salts in the Chilian deposits a further confirmation of his theory that the nitre-beds are formed from mother-liquor salts.

The book is well indexed, and is supplied with a map and several sections of the district described.

J. I. W.

OUR BOOK SHELF.

Tenerife, and its Six Satellites. By Olivia M. Stone. In Two Vols. (London: Marcus Ward, 1887.)

A GOOD book on the Canary Islands, which have been of so much service to many an invalid, has long been wanted; for, as Mrs. Stone says, many parts even of the best-known islands of Tenerife and Gran Canaria are untrodden ground to English people, and are but little known to persons of any other nationality. Mrs. Stone supplies all the information that can be needed by the most exacting visitor to the islands, or by persons who may wish to read about them at home. As she has already shown in her "Norway in June," she has excellent powers of observation, and knows how to give a clear and effective account of all that she sees in her travels. In the present work her descriptions are all the more vivid because they were written "on the spot," when everything she wished to set down in her narrative was still fresh in her mind. To the Island of Hierro, to which she and her husband seem to have been the first English visitors, she devotes a good deal of attention; and what she has to say about that "solitary, happy, singular" island is full of interest, and would alone have justified her, if justification had been necessary, in making her travels in the Canary Islands the subject of a book. In an appendix she presents a useful epitome of all necessary expenses connected with her tour.

Through Central Asia. By Henry Lansdell, D.D. (London: Sampson Low, 1887.)

THIS is a popular edition of the author's well-known "Russian Central Asia, including Kuldja, Bokhara, Khiva, and Merv." He has omitted many whole chapters and most of the notes, thinking it best that the present edition should consist chiefly of a personal narrative. Any student who may desire fuller information regarding Central Asia is referred to the original work, in which Dr. Lansdell gives 4300 species of fauna and flora in about twenty lists with introductions, adds a bibliography of 700 titles, and treats more or less fully of the geography, economy and administration, ethnology, antiquities, history, meteorology, geology, zoology, and botany of all parts of Russian Turkistan, Kuldja, Bokhara, Khiva, and Turkmenia, down to the frontier of Afghanistan. To the new and abridged edition he has added an appendix on he delimitation of the Russo-Afghan frontier.

LETTERS TO THE EDITOR.

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

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

The Star of Bethlehem.

REFERRING to the hypothesis in your last week's issue, that the star of Bethlehem was Venus, I would point out that 1180 synodical periods of Venus (*i.e.* $1180 \times 583{\cdot}92 = 689{,}025$ mean solar days) take us back from October 28, 1887—when Venus was at her maximum brilliancy as a morning star—to only May 3 of the year 1 A.D. instead of December 25 of the year 1 B.C. For the number of days from October 28, 1887, to December 25, 1 B.C., is 689,155 (viz. $1887 \times 365{\cdot}2425 = 689{,}213 - 64 + 6 = 689{,}155$). This would appear to show, either that the birth of Christ took place about May 3, or that Venus at her maximum brilliancy as a morning star was not the star of Bethlehem. I should be glad of your remarks on this.

JOHN T. NICOLSON.

20 Thirlestane Road, Edinburgh, December 26, 1887.

I INFER from the article entitled "The Star of Bethlehem" (NATURE, December 22, 1887, p. 169) that the writer supposes the craze he deals with did not exist until Venus became a morning star. It was equally prevalent here when, early in the year, she was an evening star, as the following fact will show. On May 21, 1887, a lady wrote me as follows:—"Will you kindly tell me what people mean about '*a wonderful star*'? All our servants are talking about it. . . . Some call it '*the star of Bethlehem.*' . . . I hear it is '*wonderfully bright!*'"

Torquay, December 26, 1887. WM. PENGELLY.

IN regard to the so-called "star of Bethlehem," Prof. C. A. Grimmer, in "Life from the Dead," No. 69 (August 1879), p. 267, wrote:—"It will be seen in 'Cassiopeia's Chair,' and will be accompanied by a total eclipse of the sun and moon. The marvellous brilliancy of the 'star of Bethlehem' in 1887 will surpass any of its previous visitations. It will be seen even at noonday, shining with a quick flashing light the entire year, after which it will gradually decrease in brightness, and finally disappear." E. COATHAM.

January 2.

On some Apparent Contradictions at the Foundations of Knowledge.

IN Chapter III. of Mr. Herbert Spencer's "First Principles" (p. 47, under heading, "Ultimate Scientific Ideas"), are treated the subjects of space and time. Here contradictions and difficulties of an apparently insuperable character are encountered in the attempt to define the nature of space and time, and the existence of these difficulties is frankly acknowledged. But with all the respect that is here due, it appears difficult to admit that these apparent contradictions are necessary, and in regard to space, in the first place, it will be my object here to suggest a remedy.

I will first quote some passages from the "First Principles" (5th edition) relating to this question, viz. as follows:—

"Thus as space and time cannot either be nonentities, nor the attributes of entities, we have no choice but to consider them as entities. But while on the hypothesis of their objectivity, space and time must be classed as things, we find on experiment that to represent them in thought as things is impossible" (p. 47).

It will be observed here that we encounter the apparent contradiction that those are classed as things which it is found impossible to represent in thought as things.[1]

[1] Experiment would then indirectly say that space and time were *not* things.

Then it is remarked, "To be conceived at all, a thing must be conceived as having attributes" (p. 47); and yet the author admits that it is impossible to assign any attribute to space (p. 48). So that it would appear from the last impossibility that space is *not* a thing (or entity).

It is added, "All entities, which we actually know as such, are limited" (p. 48). But, on the other hand, it is allowed that, "Of space and time we cannot assert either limitation or the absence of limitation" (p. 48).

It is observed also as follows:—"Nor are space and time unthinkable as entities, only from the absence of attributes" (p. 48). This would involve the conclusion apparently that that is considered to be an entity which is absolutely "*unthinkable*" as such.

Must there not be some flaw here, and some solution possible?

I have to propose—and this may appear very bold at first sight—that space is a non-entity. I must explain my meaning more fully. The first question or difficulty will be, How can we conceive of space (a void) or even talk of it, if it be a non-entity or nothing? In fact, on p. 177 is the remark, "Nothing cannot become an object of consciousness."

In reply to this, I would venture to suggest that under certain conditions, nothing can become an object of consciousness, viz. *by contrast with something*. We can be conscious of an absence. Darkness can become an object of consciousness by contrast with light. So space in itself—which I contend is nothing—is an object of consciousness [1] by contrast with matter.

We consider space to be an entity, I fancy, because of our experience with palpable air, &c., which (for convenience, but inaccurately) is called space. Space *per se*, an absolute void, we have no experience of. We measure all so-called spaces with matter—standards made of matter. We estimate how much solid matter is absent in a room (for instance), which we call its "volume." Mathematical lines are unconsciously figured as material no doubt from our habit of drawing them; and the spaces of triangles, &c., are usually filled out with solid matter.

It would be ridiculous (as it seems) to ask what would happen if a void disappeared. It cannot disappear because it is *already* nothing.

In regard to matter, we can conceive a certain volume of it, a certain volume added to that, &c.; and no doubt we cannot easily limit the conceivability thus extending to a larger volume. But we are not forced (by necessity as it were) to conceive an infinite volume of any entity or actually existing thing; and it appears that a void is excluded from the category of the unknowable, as we cannot expect to know anything about nothing.

Why do we hear of the creation of matter speculated about (as an inadequate attempt at explanation), but the creation of space regarded as absurd? [2] Because the first is an entity and the second is not. A non-entity cannot be supposed to be created, or it is absurd to ask the question.

One may encounter difficulties of explanation *by assuming too much to exist*—too much to explain, it appears. So I account for some of the startling contradictions supposed to exist at the basis of knowledge. What is nothing, if a void be not nothing? In order to be face to face with nothing and contrast it with something, we should not have to abolish a void, I venture to think.

Another matter seems important. On p. 34 ("First Principles") is the following, viz.:—"Did there exist nothing but an immeasurable void, explanation would be needed as much as now. There would still arise the question, How came it so? If the theory of creation by external agency be an adequate one, it would supply an answer; and its answer would be—Space was made in the same way that matter was made. But the impossibility of conceiving this is so manifest, that no one dares to assert it. For if space was created it must have been previously non-existent. The non-existence of space cannot, however, by any mental effort be imagined. . . . We are unable to conceive its absence either in the past or in the future."

[1] It appears that in order to assert an existence there must be a conception of non-existence as a contrast; otherwise the word "existence" would seem to have no distinct meaning. If matter be an existence, its absence (or a void) must be a non-existence. In other words, an absolute void (vacuity) is contemplated as the absence of existence.

[2] The author remarks of space, on p. 48, as follows:—"The only attribute which it is possible for a moment to think of as belonging to it, is that of extension; and to credit it with this implies a confusion of thought. For extension and space are convertible terms."

In regard to the commencing passage, viz. "Did there exist nothing but an immeasurable void, explanation would be needed as much as now," it might be asked, When would you be satisfied with an explanation? Explanations must finish somewhere; they finish at existences, I should fancy, and cannot extend to their absence. It is this demanding explanation perpetually, without conceived limit, that leads to the contradictions and attempts at defining nothings—as seems manifest. Extraordinary as this view taken by the author appears, it is consistent with his assumption that an absolute void is an existence or thing, whereby it is put on the same footing as matter. But observe to what this further leads.

First, the inconceivable existence of an infinite thing without attributes is assumed. Second, its non-existence cannot "by any mental effort be imagined." This means, in my view, that all attempts to imagine it *more* nothing than it is, are futile. What better definition of nothing could we have than that we cannot assert it to have "either limitation or the absence of limitation," or it is "unthinkable" as an entity "from the absence of attributes"?

Well, in this way, actual existence of something which is put on the same footing as matter seems to be made a necessity for an infinite past time; as (unlike matter in this respect) we cannot even *imagine* change here—in fact, the original creation of this thing (a void) no one dares to assert." In the same way, no one would venture to assert the creation of a mathematical line, or a mathematical plane, *i.e.* the creation of extension [1] of one, two, or three dimensions.

From the author's conclusion that space is an entity, it may be reasoned, then, that, since we must apparently have one existence for an infinite past time, we may as well have two, or include matter. Hence, with all the deference which the views as a whole in the "First Principles" demand, I would point out that in this way support is given to the idea of existence for an infinite past time (impossible to grasp fairly, as the author concedes)—which, as I contend, is not warranted by the facts.

S. TOLVER PRESTON.

30 Rue de la Clef, Paris, December 1887.

Christmas Island.

HAVING read with much interest the description of Christmas Island by Captain Aldrich and Mr. Lister, I have endeavoured to interpret some of the facts there given in the light of my own examination of similar islands in the Western Pacific. As pointed out by Captain Wharton, the complete casing of an island, 1200 feet in height, with coral rock is somewhat unusual. This may find its explanation in in the absence of stream-courses and ravines, a circumstance from which I infer that the island has not been exposed sufficiently long, since its upheaval, to the denuding agencies. When its surface has been extensively carved out by the action of running water, the old volcanic peak, which these upraised reefs envelop, will in all probability be exposed. Christmas Island, therefore, has still the early part of its story to unfold.

The three tiers of cliffs evidently mark pauses in the elevation. As they appear to decrease in height with the ascent, it would seem that older lines of cliffs on the upper slopes of the island have been removed to a great extent by denudation. The principal features of the movement of upheaval appear to resemble those of which similar upraised coral islands give evidence in the West Indies, Western Pacific, and other regions of elevated coral reefs. Protracted elevatory movements of from 100 to 300 feet are separated by long pauses, during which cliffs are worn back by the waves, and the reefs grow seaward: hence the terraced profiles of these islands. I have pointed out that in the Solomon Group these protracted movements consist of a succession of small upheavals of usually 5 or 6 feet at a time.

17 Woodlane, Falmouth. H. B. GUPPY.

A Mechanical Cause of the Lamination of Sandstone not hitherto noticed.

THE lamination of sedimentary rocks is usually attributed to the successive deposition of sediment of varying degrees of fineness or coarseness. Currents of water have a selective action

[1] The author remarks that "Extension and space are convertible terms" (p. 48). I may express my agreement with the author as to the inadequacy of the theory of the "creation" of matter, as an explanation.

on the materials that are swept along by them, by which grains of one size and weight are laid down at one time, and of another size and weight at another. Changes in the nature of the material in suspension also occur through which the deposit may be at different times more siliceous, argillaceous, or calcareous. This is doubtless in most cases a true explanation of the cause of lamination in rocks, but it is not a full one, nor does it account for stratification such as I am about to describe.

In sand dunes composed entirely of siliceous grains such as are seen on the west coast of Lancashire between Liverpool and Southport, a strong false-bedded lamination is often beautifully developed. This is best seen when the sand-hills are moist from recent rainfall, and the talus has been cut away by a high tide, leaving a vertical face of sand to the shore side. After this has occurred a gentle wind will weather out the structure of the sand-hill in a remarkable manner. The layers often stand out several inches in projecting mouldings and fillets, while the finer laminæ are wonderfully developed. I have often minutely

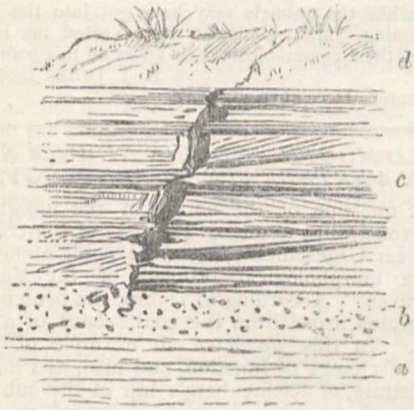


FIG. 1.—View of sand-dune, showing the bedding and laminæ weathered out by denudation. *a*, shore; *b*, loose talus; *c*, vertical cliff of sand; *d*, surface of sand-dune.

examined the constitution of these beds, but have been unable to detect any difference in the sizes of the constituent grains of the several beds or laminæ. What makes the fact more striking is that the grains are generally and in many cases much rounded. An examination, however, shows that the laminæ projecting from the face of the sand-cliff are much harder and more solid than the portions between them that have weathered back. They can, in fact, be broken off in pieces by the fingers without crumbling. The grains of sand, I must observe, are only temporarily bound together by the capillary attraction of the water.

The explanation which suggests itself to me is that the grains of sand, according to the state of the weather during deposition, are at one time more completely aggregated than at another. The shore sand, I have noticed, is greatly affected by the state of the water it is laid down by. In one place may often be seen a stretch of hard fine sand, while in another at the same level the sand may be soft, both being at the same point of saturation. It is well known to builders that pouring water on loose sand tends to solidify it, therefore it is most probable that the state of the weather influences the solidity of aggregation of the surface of the sand dunes and assists to build up layers of different density.

Between the projecting fillets already described as weathered out of the sand cliffs the sand is looser and more porous, and, drying faster, falls away from the face at a greater rate than the compacted beds. In sand heaped together by the wind there are few, I should think, would *a priori* look for much internal structure; yet here are the most undoubted evidences to the contrary which are generally passed by, being looked upon as a matter of course not demanding further thought! If we consider in what way the constituent grains naturally arrange themselves by gravity, we shall, I think, get an additional clue to the cause of lamination. Even if the grains were as round as shot they would by gravitation tend to arrange themselves in parallel planes, the upper grains falling into the interstices of those next below them, so—

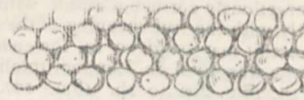


FIG. 2.

If, on the contrary, the grains have a long and short axis, they will tend to lie with the longer axis parallel to the plane of deposition, so—

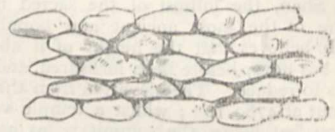


FIG. 3.

With irregular fragments the arrangement will not be so perfect, but they also will tend to be laid down in definite planes.

An examination of specimens of laminated sandstone shows that a fracture vertical to the plane of lamination exhibits a more jagged surface than a fracture parallel to the plane of lamination. This it is that gives the strength to sandstone to resist transverse stress.

It is thus seen that nature adopts the same principle to build up sandstone that a mason does to build a wall. From the way in which the particles arrange themselves a natural "bond" is produced. The grains "break joint," as it is technically called—that is, the joints are not vertically over each other—while the planes of deposition correspond to the "courses" of a wall. The principle here explained I have seen well exhibited in conglomerates formed of flattish oval pebbles. The mode of aggregation of the particles of a sedimentary rock, due to the ordinary dynamical laws governing deposition, and independent of the coarseness or fineness of the grains of successive layers, is an important factor in its constitution, which seems hitherto not to have attracted much attention. T. MELLARD READE.

Total Solar Eclipse of October 29, 878.

IN NATURE for March 11, 1875, vol. xi. p. 365, a computation is given of this eclipse, based on an entry in the "Annales Fuldenses," which runs thus: "Sol quoque in 4 kal. Novembris post horam nonam ita obscuratus est per dimidiam horam, ut stellæ in cœlo apparent, et omnes noctem sibi imminere putarent."

The computer found that the sun rose on that day at Fulda at 7h. 12m. apparent time, 6h. 57m. mean time, that the partial phase began at 0h. 56m., and ended at 3h. 24m., totality commencing at Fulda at 2h. 9m. 32s. local mean time, and continuing 1m. 41s. till 2h. 11m. 13s. He seems to have been puzzled, however, by the statement of the annalist that the darkness occurred "post horam nonam," observing plausibly enough that the ninth hour from sunrise would be 4 p.m.

It is shown, however, in Dr. Smith's "Dictionary of Christian Antiquities," vol. i. p. 793, that the day then employed by the Church was the natural day extending from sunrise to sunset, which was conceived to be divided into twelve hours (shorter of course in winter than in summer); so that the first hour was the twelfth part of the natural day, which began with sunrise; the sixth hour that which ended when the sun crossed the meridian, and so on.

The question, then, which arises is this: At what point of local mean time did the ninth natural hour end at Fulda on October 29, 878?

The sun rose at 7h. 12m. apparent time: this would give a semi-diurnal arc of 4h. 48m., or 9h. 36m., as the duration of the natural day, one-twelfth part of which, or 48 minutes, would be the length of the natural hour. As nine such hours would contain 432 minutes, it is clear that the ninth hour after 6h. 57m. the local mean time of sunrise would end at 2h. 9m., and the half-hour of darkness mentioned would have extended from 2h. 9m. to 2h. 33m. As the computer reckoned that totality lasted from 2h. 9m. 32s. to 2h. 11m. 13s., the obscuration would have been gradually passing away during that period.

The coincidence between the record and the calculation is a very striking one, and testifies at the same time to the veracity of the Benedictine monks of Fulda, the trustworthiness of the lunar and solar tables, and the accuracy of the computer who brought out so marvellously correct a result without knowing that it agreed exactly with the true meaning of the record.

No doubt equal credit may be given to the computer's statement that this eclipse was total in London, totality continuing at St. Paul's from 1h. 16m. 20s. to 1h. 18m. 10s. local mean time.

C. S. TAYLOR.

Height of T'ai Shan.

A FORMER student of mine, Mr. S. Couling, has recently ascended T'ai Shan, the loftiest of the sacred mountains of China, and one of the most ancient and popular places of pilgrimage. He believes that the height of it above the surrounding plain has never before been measured, and has sent me his observations to reduce. The elevation from the plain to the summit comes out at 4780 feet; whilst a temple vaguely stated to be about 400 feet below the summit is, as ascertained by barometer, 4485 feet above the plain.

SILVANUS P. THOMPSON.

The Shadow of a Mist.

LIVING on the Blue Mountains at an elevation of 5000 feet, I am frequently astonished at the ever varying beauty of the mists and clouds. But a short time ago it was my good fortune to see the shadow of a mist, itself not visible.

On the evening of November 16, shortly after 7 o'clock in the evening, I was watching the electric light with which the military authorities were experimenting at Port Royal, 15 miles distant in a straight line. The light at times was so brilliant that the shadow of a person standing 20 feet from the house was distinct on the white-painted front, even when he held a lamp partially turned down close to his body on the side next the house. Rain was falling, but so slightly that there was no need for an umbrella. No mist or cloud was visible in the direct line to Port Royal. and yet a net-work of shadow was thrown on the house, the meshes of which were 3 or 4 inches in width. The shadows were all in motion, moving from east to west, in the direction of the scarcely noticeable breeze; individual portions of the meshes disappearing and re-forming as they moved, so that it was quite dazzling to look at the shadow, reminding me of the ripple on water as seen against a strong light. A puff of tobacco smoke had a shadow only when an inch or two from the house, so that the mist must have been much denser, and yet it cannot have been of any great breadth, or the shadow would have been uniform instead of reticulated. No doubt many of your readers can explain this appearance, which to me seemed so singular.

W. FAWCETT,
Director of Public Gardens and Plantations.

Cinchona, Gordon Town P.O., Jamaica, December 1, 1887.

The Ffynnon Beuno and Cae Gwyn Caves.

IT would seem that so long as the controversy with regard to the contents of these caves is confined to Dr. Hicks, Prof. Hughes, and Mr. W. G. Smith, the points at issue will never be decided. Dr. Hicks argues most needlessly for the *pre*-Glacial age of the cave deposits; Prof. Hughes calmly assumes that the outside deposits are *post*-Glacial; and many geologists must be heartily tired of hearing these two gentlemen contradict one another without defining what they mean by the terms Glacial and post-Glacial.

The fact is that the St. Asaph drift (to which Prof. Hughes now admits the outside deposits belong) is part of the later Glacial series of Northern England; and Prof. Hughes has no right to call it post-Glacial without defining what he means by that term. Most people call them Glacial deposits. If therefore the cave-deposits are older than this drift, they are not necessarily *pre*-Glacial, as Dr. Hicks maintains, but only anterior to what Mr. Mellard Reade terms the marine low-level boulder-clays. Now many think that these clays and their associated sands are coæval with, or newer than, the so-called post-Glacial river-gravels of Southern England. It is not surprising therefore that the cave fauna should be the same as that of the river-gravels, and it is perfectly needless to compare it with the fauna of the Cromer Forest bed.

In Lincolnshire the same marine shells occur in sands and gravels beneath the latest sheet of boulder-clay, and a gravel beneath the same clay at Burgh has yielded teeth and bones of *Elephas antiquus*, *Rhinoceros leptorhinus*, and *Bos primigenius.* These beds are on the same line of latitude as St. Asaph, and are probably of the same age as that drift; but it may be that neither of them are older than the oldest river-gravels of the Cam or Thames valleys.

It has been repeatedly pointed out that the terms Glacial and post-Glacial cannot be used as conveying any idea of relative age except along one and the same parallel of latitude, and it is rather surprising that the Woodwardian Professor of Geology should seem to be unaware of this. If by post-Glacial Prof. Hughes means later Glacial or newer Pleistocene, everyone will probably agree with him, but he confuses the issue by his bad choice of terms.

The palæontological evidence is really of no value—the argument leads nowhere; what we want is an expression of opinion by some geologist who has seen the locality and the recent excavations, regarding the explanation proposed by Prof. Hughes, viz. that the present position of the bones beneath the marine drift is due to the falling in of the roof of the cave near one entrance, while the animals may have got into the cave by another opening. Many geologists have visited the locality—will some of them communicate their views on this point?

A. J. JUKES BROWNE.

Southampton, December 28, 1887.

THE OLD MOUTH AND THE NEW: A STUDY IN VERTEBRATE MORPHOLOGY.

"THE question of the nature of the mouth," says Prof. Dohrn in one of the first of his celebrated "Studien zur Urgeschichte," "is the point about which the whole morphological problem of the Vertebrate body revolves." According to Dohrn, the present mouth of Vertebrates arose from the coalescence of a pair of gill-clefts. In this we have an example of Dohrn's principle of change of function, and also, as I hope soon to demonstrate, of Kleinenberg's law of the substitution of organs. I do not now wish or intend to give an account of the researches by which Dohrn showed that the mouth in some cases first arises as a pair of lateral invaginations of epiblast, still less of my own small contribution to this question, which consisted in recording the facts that the mouth also resembles a gill-cleft in some other particulars.

It suffices here to say that these researches have not yet been refuted, and that the view that the present mouth of Vertebrates is, so to speak, a new structure, rests on a very sound foundation.

With the blastopore as the foundation of mouth and anus I have here no concern, nor have I any sort of sympathy with the upholders of a theory which has been condemned and rejected by embryologists such as Lankester, Kleinenberg, and Salensky.

The problem I have to discuss is, granted that the present Vertebrate mouth is a new[1] structure, what traces, if any, are to be found of the old mouth? It is conceivable, and I strongly emphasize the point, that the old mouth might have disappeared, even from the development, without leaving a trace behind.

We seem to be gradually getting out of the idea that ontogeny is even a fair repetition, much less a perfect one, of phylogeny, for absolutely rudimentary organs (organs performing no function at all) are only retained as larval or embryonic organs, as the basis or *Anlage* of other organs, or, finally, because they are inseparably connected with the development of other organs. Of the latter a fair case, it seems to me, is to be seen in the rudiment of the parietal eye in the higher Vertebrates. This organ, functionless except in a few fishes and reptiles, possibly only reappears in the development because it is intimately connected in some way or other with the paired eyes.

A still better example is, I think, to be met with in the

[1] It is rather paradoxical to speak of a thing as new which has existed in its present form for untold millions of years.

rudiments of the gill sense-organs and ganglia described by Prof. Froriep in Mammalia. (Of these I hope to give a fuller account in connection with other work.) I find them in lizards, crocodiles, and birds; and there can be little doubt that they exist as rudiments in all animals above fishes and amphibia. Their recurrence has its explanation in that they probably form the *Anlage* for certain portions of the cranial ganglia.

It was Dohrn who first hinted, in his work on "Der Ursprung der Wirbeltiere," published in 1875, that the hypophysis cerebri represented the last remains of the old mouth, and that it must have opened on the dorsal surface, after passing between the crura cerebri.

This idea he soon gave up, and indeed, in the work above mentioned, he inclined to the view that the opening lay somewhere in the region of the medulla oblongata. Since then he has relinquished, for the time, the search for the old mouth, and has advised others to do the same.

His first hypothesis has more recently been advanced as new by Prof. Owen and Mr. J. T. Cunningham. Both of these writers hold very slightly different views from those originally suggested by Dohrn.

Some of the statements which I am about to make appear on the surface to bear slight resemblance to Cunningham's views, but, as I hope will be seen, nothing could be further from the truth. Cunningham, starting from Balfour's well-known, and now universally accepted, belief that the spinal cord and brain were once an open plate, advocated, as the latest discovery of Vertebrate morphology, the view that the infundibulum, whose walls consist of nervous matter and nothing else, is the vestige of the old mouth which pierced the brain.

One cannot but marvel at the rashness of an hypothesis which annexes, without more ado, a portion of the nervous system, and proclaims it to all the world as the remnant of a former passage from the exterior to the stomach of the animal!

Cunningham overlooks entirely the nature and exceedingly complicated development of the processus infundibuli, or nervous portion of the hypophysis.

Although, thanks to Rabl-Rückhard and others, we have obtained a certain amount of light on the nature of the pineal gland or epiphysis, the body (hypophysis), at the opposite end of the third ventricle still remains one of those organs on which all sorts of speculations may be made, with impunity. Some of the explanations offered are in accordance with certain facts of its development. Others, on the contrary, accord with no known fact of embryology.

The nervous part—or, as I shall call it, the *neural hypophysis*—has been considered by Rabl-Rückhard as a gland secreting cerebro-spinal fluid. I must, however, express a strong opinion that such a glandular function is extremely improbable, for the conversion of a piece of nervous tissue into a gland is absolutely without parallel.

Goette and Wiedersheim both regard the nervous part as a remnant of a sense-organ; against which view *a priori* little or nothing can be said. The mouth part or *oral hypophysis* was finally classed by Dohrn as the rudiment of a pair of gill-clefts—a supposition not wholly unsupported by its developmental history. It has also, not unnaturally, been looked upon as a remnant of a mouth-gland.

Prof. Hubrecht made it the basis of his comparisons of Nemertea and Vertebrata, and saw in it the remains of the Nemertean proboscis, the Vertebrate notochord being the homologue of the proboscis sheath—comparisons which appear to me to be as little capable of support as those of the same investigator between the Vertebrate and Nemertean nervous systems.

And so, after all, on turning to Wiedersheim's latest book, "Der Bau des Menschen," we read: "The hour of the release of the hypophysis cerebri from its obscure position has not yet struck, and the problems it presents are rendered more difficult in that it develops from two different points—from the brain (infundibulum) and from the epiblast of the primitive pharyngeal involution."

For what we know of the facts of its anatomy and development we are mainly indebted to five distinguished morphologists: Profs. W. Müller, Goette, Mihálkovics, Kölliker, and Dohrn. In the following very brief summary I partly follow Kölliker's account (in his valuable "Entwickelungsgeschichte des Menschen," 1879), which, for the time it was written, is by far the most complete we possess.

My own researches on Sharks, Ganoids, Dipnoi, Cyclostomata, Amphibia, Lizards, Snakes, Crocodiles, Birds, and Mammals, mainly confirm Kölliker, who, in his turn, has taken the greater portion of his account from the beautiful classic of Mihálkovics.

The hypophysis cerebri is composed of two parts: the one, *neural hypophysis*, derived from the nervous system; the other, *oral hypophysis*, from the epiblast in the region of the mouth.

The oral hypophysis is formed early in development as an epiblastic involution towards the end of the notochord, *i.e.* towards the hypoblast, and in the direction of the base of the brain. In some cases it may even grow in the direction of a process of hypoblast immediately below the anterior end of the notochord. But, except in Myxine, it never fuses with the hypoblast. It afterwards becomes pinched off from the pharynx, and gets thus to lie on the floor of the skull, becoming finally converted into a compound gland-like organ.

The neural hypophysis, or hinder lappet of the hypophysis, on the other hand, develops ventrally as a process of the basal portion of the thalamencephalon, or hinder part of the fore-brain. At first composed of tissue of exactly the same character as the rest of the thalamencephalon, it becomes solid below and converted into indifferent tissue; the portion of the process which remains hollow, and forms the base of the infundibulum, alone retains a nervous structure. Kölliker records that in pig embryos of 3 centimetres in length longitudinal bundles of nerve-fibres pass into the developing neural hypophysis, or processus infundibuli as it is called, from the base of the thalamencephalon.

In most cases, especially in Mammalia and also in Dipnoi, the neural hypophysis becomes closely and almost inseparably connected with the oral hypophysis. Usually the *Anlage* of the oral hypophysis lies in the region of the mouth epiblast; in Petromyzon and Myxine it lies in front of and outside the mouth. The process by which it got into the mouth involution cannot be explained without numerous figures.

According to Dohrn, the oral hypophysis arises in Petromyzon as an invagination of epiblast in front of the mouth between the oral and nasal depressions. It grows towards the base of the infundibulum, and comes into close relationship with the end of the notochord, *i.e.* with a structure derived from hypoblast, while it approaches a special process of hypoblast itself, with which, however, in Petromyzon it does not fuse. In Myxine, although the development is unfortunately not yet known, we may assume that this fusion is effected, for in that animal it opens throughout life into the gut (see figure, *O.M.*).

In Ammocœtes it gives off a certain number of gland-follicles, which, according to Dohrn, become pinched off in the Petromyzon. While I am not yet quite convinced of the certainty of this latter point, I find, in Myxine, numerous small glandular follicles opening into the oral hypophysis. In Petromyzon and Myxine the neural hypophysis is present, and, as I believe, not rudimentary. It appears to supply nerve-fibres to the oral hypophysis.

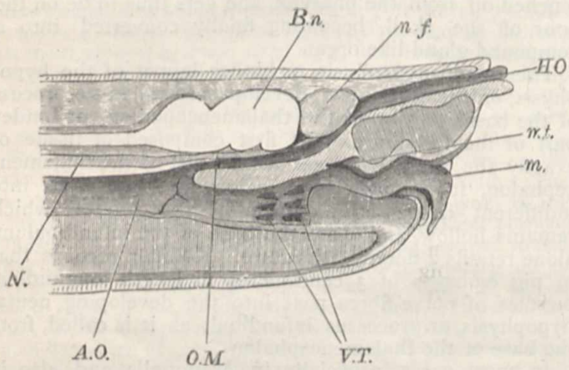
Dohrn finds in Hippocampus traces of a paired origin of the oral hypophysis. This is important.

I propose to divide the oral or glandular hypophysis into two parts, viz. a duct or main oral hypophysis and a glandular part or glandular hypophysis. The whole structure is without doubt in nearly all cases rudimentary, and of little or no functional importance. A mass of information bearing upon it has recently come to light in the study of the developmental history of Annelids, chiefly at the hands of Kleinenberg and Salensky.

From the results of Kleinenberg's work, more especially, we are placed in a position to compare the structure and development of the hypophysis with those of certain organs in the worms. To my mind, the comparison which follows is one of the neatest in the whole range of comparative morphology; I would therefore, before proceeding further, give a brief *résumé* of Kleinenberg's results so far as they here concern us.

In the first place, he records how the larval stomodæum or mouth is replaced in a very complicated manner by the Annelid permanent mouth or *Schlund*. The latter is formed as a paired involution of the stomodæum, *i.e.* of the epiblast, and this he considers to have originally represented stomodæal glands. It encroaches upon and swallows up the old mouth, and, finally fusing with the hypoblast, it opens into the gut.

The replacement of the larval mouth in Annelids by a new structure was already known, but Kleinenberg describes the steps of the process in great detail, and he



Myxine glutinosa. Head in longitudinal section × 2. *H.O*, opening of hypophysis; *m.*, mouth opening; *m.t.*, median tooth; *V.T.*, ventral teeth; *n.f.*, one of the folds of the nasal sensory membrane; *Bn.*, brain; *O.M.*, opening of hypophysis into gut; *A.O.*, œsophagus; *N.*, notochord.

states that this mode of mouth substitution by means of a paired involution is of very wide occurrence in the Chætopods.

In it we have a direct parallel to the substitution of the old Vertebrate mouth by a pair of gill-clefts, but, in truth, we have something more.

Another phenomenon of extreme interest is the formation of the special mouth (or *Schlund*) nervous system. *This apparatus is only concerned with the innervation of the permanent* Schlund, *and takes no share in the innervation of the hypoblastic alimentary canal.* It arises as a special process of the hinder part of the subœsophageal ganglion: this grows towards the developing Schlund, becomes closely attached to the latter, fuses with it and gets pinched off from the larger portion of the subœsophageal ganglion, which is left as the first ganglion of the ventral chain.

I must here digress in order to discuss the question of the presence or absence of any representative of the supraœsophageal ganglion of Annelids in Vertebrates,[1] and here again Kleinenberg comes to our assistance.

I have myself devoted a good deal of attention to this point, and have arrived at the conclusion (held also, I believe, by Prof. Dohrn) that there is no likelihood at all of our finding an area in the Vertebrate brain which was ever pierced by the œsophagus—pierced so as to divide the brain into a supraœsophageal and a subœsophageal portion, which might be compared respectively to such divisions of the Annelidan nervous system. At first sight, this appears like an admission that the Annelidan theory of the origin of Vertebrates is untenable. But such is not the case.

From a large number of researches, including those of Bergh, Salensky, and Kleinenberg, we know that the supraœsophageal ganglion of Annelids certainly arises independently of the ventral chain, and that it only later becomes connected with the latter by the development of the circumœsophageal collar.

Kleinenberg's brilliant researches also teach us that the permanent Annelidan nervous system arises through substitution, and partial or entire disappearance of whole larval nervous apparatuses and sense-organs. And, indeed, after reading his beautiful work, one is fully prepared for one of the closing statements in it—that possibly the supraœsophageal ganglion is entirely absent in Vertebrates.

Personally, I have no hesitation at all in accepting this as probably true; but the grounds for my belief, or some of them, I can only hint at here. They arise out of as yet unpublished developmental researches. Briefly stated, I see in the development of the gill-clefts, with their special sense-organs and ganglia[1]—all of which lie in the region which is under the control of a system comparable to the ventral nerve-cord of Annelids—a probable cause of the disappearance of the supraœsophageal ganglion in the ancestors of Vertebrates,—in a similar way to that in which, according to Kleinenberg, the dislodging and destruction of the special larval ganglionic centres takes place in the Annelid.

I believe that in the ancestors of the Vertebrates, by the development of the eyes, and of the important gill sense-organs and ganglia, the ventral chain came to obtain control over a very extensive system of ganglia, sense-organs, and muscles; and, having already a control over the mouth or *Schlund*, it entirely deposed the supraœsophageal ganglion (and its sense-organs). The entire *raison d'être* of the latter being thus disposed of, it naturally degenerated and finally disappeared.

If it be admitted that the supraœsophageal ganglion of Annelids is absent in Vertebrates, and that the brain and spinal cord of the latter may be compared directly with the ventral cord of Annelids, then a whole host of direct structural relationships between Annelida and Vertebrates may be established. Kleinenberg expresses his opinion that the spinal ganglia of Vertebrates have their parallel in the parapodial ganglia of Annelids,—a comparison which, as I shall elsewhere show, is entirely justifiable for the spinal ganglia and for certain portions of the cranial ganglia also.

Let me now briefly review the conditions demanded of any structures in the Vertebrate which are to be homologized with the permanent mouth of Annelids. Such ought to arise as a paired involution of epiblast (though it is conceivable *a priori* that the paired character might be lost). This involution must fuse with, and open into, the cavity of the hypoblast. It must also give rise to certain glands, and it must have a special nervous system of its own derived from the hinder part of the first ventral ganglion or its homologue—which nervous system must supply it alone, and no other part of the alimentary canal.

All these conditions are fulfilled by the complex called hypophysis cerebri.

In at least one case (Hippocampus) the oral hypophy-

[1] I postpone the consideration of Prof. Semper's views on this point, and on the nature of the mouth in Annelids and Vertebrates.

[1] The cranial ganglia of Vertebrates are far more complicated morphologically than has hitherto been recognized. In addition to parts which appear to correspond morphologically to the posterior root ganglia of the spinal nerves plus the sympathetic ganglia, they also contain the special ganglia which are formed in connection with the gill sense-organs.

sis[1] is known to arise as a paired epiblastic involution (Dohrn). In the Cyclostomata it is formed as an epiblastic involution (possibly paired) at the extreme anterior end of the body. In one Vertebrate alone, Myxine (*vide* figure), it still opens into the hypoblast; in all others it approaches the hypoblast in development, but does not fuse with that layer. It always lies in very close relationship with the extreme end of the notochord—that is, with the end of a structure derived from the hypoblast.

In adult Petromyzon, in which the tube of the oral hypophysis has the same relationships as in Myxine, except that the posterior opening into the hypoblastic sac is absent, it nevertheless has an astonishing length, and ends blindly very close to the gut. In Myxine and Petromyzon, tubular glands are developed in connection with it. In all the higher Vertebrates, in which the oral part is very rudimentary, it always has a distinct glandular character.

And now, what of the last condition? This also is satisfactorily met. In all cases the oral hypophysis has a special, and indeed large, process of nervous matter (the processus infundibuli, or neural hypophysis), which is derived from the posterior part of the fore-brain, from the base of the infundibulum. This process is concerned with the innervation of the oral hypophysis alone. In Myxine and Petromyzon alone, so far as my researches extend (possibly also in Protopterus), this nervous system is not rudimentary. In most Vertebrates the neural hypophysis, which, as Kölliker aptly remarks, is at first composed of the same cell elements and fibres as the rest of the brain, degenerates, and in very many full-grown animals forms a mass of tissue, the structure of which many observers have compared to that of the suprarenal bodies (known to be masses of degenerated tissue).

The neural hypophysis is thus the most remarkable structure in the whole of the Vertebrate central nervous system. Though degenerated, it still clings to the traditions of its ancestry, for even, as it were, in its death it is closely and almost inseparably connected with the rest of the hypophysis, especially in Mammalia and in Dipnoi. In Myxine alone, of all Vertebrates, the old mouth still retains some of its functions as a mouth; it conducts the water of respiration to the gills. In this case, even, changes have occurred, for the nose[2] (see figure, *n.f.*) has got partly involved in the passage of the old mouth. If it be true that the nose was originally a branchial sense-organ—which view, in spite of Gegenbaur, I still maintain—its assumption of a position in the passage of the old mouth in Myxine is, on purely physiological grounds, intelligible.

It is well known that that which I call the old mouth in Myxine is purely respiratory, conducting water into the gills; and what then could be more likely than that one of the branchial sense-organs should be, as it were, told off to do duty at its entrance. It is certain, from Goette's and Dohrn's researches, that these passages in Myxine and Petromyzon are the representatives of the oral hypophysis. I have gone over and extended these observations, and can fully confirm Dohrn in nearly every point, and all I claim here is the identification of the hypoblastic opening in Myxine as the (modified) opening of the old mouth into the gut.

If the above morphological comparison can be maintained (and I believe it can), the importance of its bearing on the morphology of Vertebrates can hardly be over-estimated.

A number of other problems and conclusions arise out of all this, but I reserve the consideration of these for a much more exhaustive work, in which the literature of the subject will receive full attention. J. BEARD.

Anatomisches Institut, Freiburg i/B., November 16.

[1] I believe it is very frequently paired, though not at its point of origin.

[2] In Petromyzon, Dohrn finds that the nose is at first a special depression apart from the hypophysis invagination. The latter lies between the nose and mouth.

TIMBER, AND SOME OF ITS DISEASES.[1]

III.

HAVING now obtained some idea of the principal points in the structure and varieties of normal healthy timber, we may pass to the consideration of some of the diseases which affect it. The subject seems to fall very naturally into two convenient divisions, if we agree to treat of (1) those diseases which make their appearance in the living trees, and (2) those which are only found to affect dead timber after it is felled and sawn up. In reality, however, this mode of dividing the subject is purely arbitrary, and the two categories of diseases are linked together by all possible gradations.

Confining our attention for the present to the diseases of standing timber—*i.e.* which affect undoubtedly living trees—it can soon be shown that they are very numerous and varied in kind; hence it will be necessary to make some choice of what can best be described in this article. I shall therefore propose for the present to leave out of account those diseases which do injury to timber indirectly, such as leaf-diseases, the diseases of buds, growing roots, and so forth, as well as those which do harm in anticipation by injuring or destroying seedlings and young plants. The present article will thus be devoted to some of the diseases which attack the timber in the trees which are still standing; and as those caused by fungus parasites are the most interesting, we will for the present confine our attention to them.

It has long been known to planters and foresters that trees become rotten at the core, and even hollow, at all ages and in all kinds of situations, and that in many cases the first obvious signs that anything is the matter with the timber make their appearance when, after a high gale, a large limb snaps off, and the wood is found to be decayed internally. Now it is by no means implied that this rotting at the core—"wet-rot," "red-rot," &c., are other names generally applied to what is really a class of diseases—is *always* referable to a single cause; but it is certain that in a large number of cases it is due to the ravages of fungus parasites. The chief reason for popular misconceptions regarding these points is want of accurate knowledge of the structure and functions of wood on the one hand, and of the nature and biology of fungi on the other. The words disease, parasitism, decomposition, &c., convey very little meaning unless the student has had opportunities of obtaining some such knowledge of the biology of plants as can only be got in a modern laboratory: under this disadvantage the reader may not always grasp the full significance of what follows, but it will be at least clear that such fungi demand attention as serious enemies of our timber.

It will be advantageous to join the remarks I have to make to a part description of some of the contents of what is perhaps one of the most instructive and remarkable museums in the world—the Museum of Forest Botany in Münich, which I have lately had the good fortune to examine under the guidance of Prof. Robert Hartig, the distinguished botanist to whose energy the Museum is due, and to whose brilliant investigations we owe nearly all that has been discovered of the diseases of trees caused by the Hymenomycetes. Not only is Prof. Hartig's collection unique in itself, but the objects are classical, and illustrate facts which are as yet hardly known outside the small circle of specialists who have devoted themselves to such studies as are here referred to.

One of the most disastrous of the fungi which attack living trees is *Trametes radiciperda* (Hartig), the *Polyporus annosus* of Fries, and it is especially destructive to the Coniferæ. Almost everyone is familiar with some of our common Polyporei, especially those the fructifications of which project like irregular brackets of various colours from dead stumps, or from the stems of moribund trees;

[1] Continued from p. 207.

well, such forms will be found on examination to have numerous minute pores on the under side or on the upper side of their cheese-like, corky, or woody substance, and the spores which reproduce the fungus are developed on the walls lining these many pores, to which these fungi owe their name. *Trametes radiciperda* is one of those forms which has its pores on the upper side of the spore-bearing fructification, and presents the remarkable peculiarity of developing the latter on the exterior of roots beneath the surface of the soil (Fig. 11).

This is not the place to discuss the characters of species and genera, nor to enter at any detail into the structure of fungi, but it is necessary to point out that in those cases where the casual observer sees only the fructification of a Polyporus, or of a toadstool, or of a mushroom (projecting from a rotting stump or from the ground, for

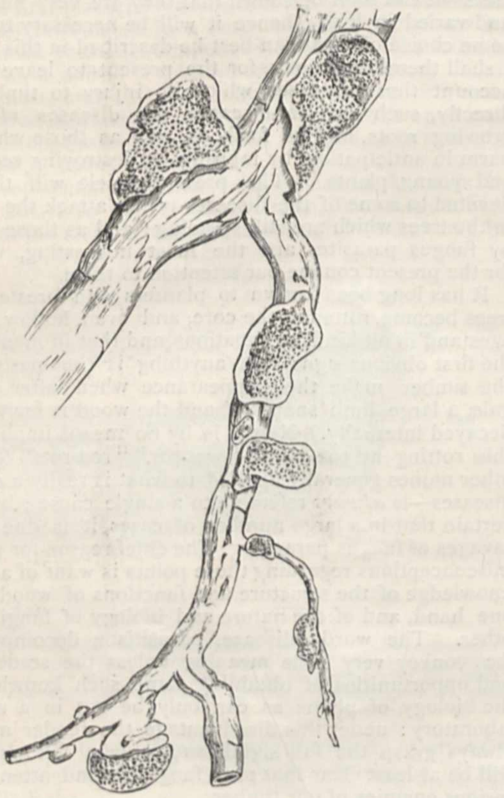


Fig. 11.—Portion of root of a spruce-fir, with fructification of *Trametes radiciperda* (after Hartig). Each fructification is a yellowish-white mass of felt-like substance spread over the root, and with minute pores, in which the spores are produced, on its outer surface; the mycelium which has developed it is in the interior of the root.

instance), the botanist knows that this fructification is attached to, and has taken origin from, a number of fine colourless filaments woven into a felt-like mass known as the mycelium, and that this felt-work of mycelium is spreading on and in the rotten wood, or soil, or whatever else the fungus grows on, and acts as roots, &c., for the benefit of the fructification.

Now, the peculiarity of the mycelium of this *Trametes radiciperda* is that it spreads in the wood of the roots and trunks of pines and firs and other Conifers, and takes its nourishment from the wood-substance, &c., and it is to the researches of Hartig that we owe our knowledge of how it gets there and what it does when there. He found that the spores germinate easily in the moisture around the roots, and put forth filaments which enter between the bark-scales, and thus the mycelium establishes itself in the living tree, between the cortex and the wood (Fig. 12). It is curious to note that the spores may be carried from place to place by mice and other burrowing animals, since this Trametes is apt to develop its fructification and spores in the burrows, and they are rubbed off into the fur of the animals as they pass over and under the spore-bearing mass.

When the mycelium obtains a hold in the root, it soon spreads between the cortex and the wood, feeding upon, and of course destroying, the cambium. Here it spreads in the form of thin flattened bands, with a silky lustre, making its way up the root to the base of the stem, whence it goes on spreading further up into the trunk (Fig. 12).

Even if the mycelium confined its ravages to the cambial region, it is obvious, from what was described in Articles I. and II., that it would be disastrous to the tree; but its destructive influence extends much further than this. In the first place, it can spread to another root

Fig. 12.—Piece of root of spruce-fir, with the mycelium of *Trametes radiciperda* (after Hartig) enlarged about 3 times. The white mycelium spreads in a fan-like manner over the surface beneath the cortex, as seen in the figure where the latter has been lifted and removed (*a*). Here and there the mycelium bursts through the cortex in the form of white protuberances (*b*), to form the fructifications.

belonging to another tree, if the latter comes in contact in the moist soil with a root already infected; in the second place, the mycelium sends fine filaments in all directions into the wood itself, and the destructive action of these filaments—called hyphæ—soon reduces the timber, for several yards up the trunk, to a rotting, useless mass. After thus destroying the roots and lower parts of the tree, the mycelium may then begin to break through the dead bark, and again form the fructifications referred to.

Since, as we shall see, *Trametes radiciperda* is not the only fungus which brings about the destruction of standing timber from the roots upwards, it may be well to see what characters enable us to distinguish the disease thus induced, in the absence of the fructification.

The most obvious external symptoms of the disease in a plantation, &c., are: the leaves turn pale, and then yellow, and die off; then the lower part of the stem begins to die, and rots, though the bark higher up may preserve its normal appearance. If the bark is removed from one of the diseased roots or stems, there may be seen the flat, silky, white bands of mycelium running in the plane of the cambium, and here and there protruding tiny white cushions between the scales of the bark (Fig. 12); in advanced stages the fructifications developed from these cushions may also be found. The wood inside the diseased root will be soft and damp, and in a more or less advanced stage of decomposition.

On examining the timber itself, we again obtain distinctive characters which enable the expert to detect the disease at a glance. I had the good fortune to spend several pleasant hours in the Münich Museum examining and comparing the various diseases of timbers, and it is astonishing how well marked the symptoms are. In the present case the wood at a certain stage presents the appearance represented in the drawing, Fig. 13. The general tone is yellow, passing into a browner hue. Scattered here and there in this ground-work of still sounder wood are peculiar oval or irregular patches of snowy white, and in the centre of each white patch is a black speck. Nothing surprised me more than the accu-

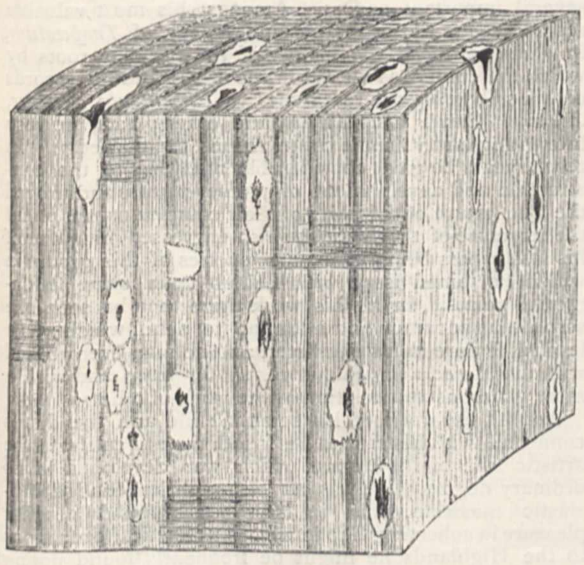


FIG. 13.—A block of the timber of a spruce-fir, attacked by *Trametes radiciperda*. The general colour is yellow, and in the yellow matrix of less rotten wood are soft white patches, each with a black speck in it. These patches are portions completely disorganized by the action of the mycelium, and the appearance is very characteristic of this particular disease. (After Hartig.)

racy with which Prof. Hartig's figures reproduce the characteristic appearance of the original specimens in his classical collection, and I have tried to copy this in the woodcut, but of course the want of colour makes itself evident.

It is interesting and important to trace the earlier changes in the diseased timber. When the filaments of the fungus first begin to enter the wood, they grow upwards more rapidly than across the grain, piercing the walls of the cells and tracheides by means of a secretion—a soluble ferment—which they exude. This ferment softens and dissolves the substance of the walls, and therefore, of course, destroys the structure and firmness, &c., of the timber. Supposing the filaments to enter cells which still contain protoplasm and starch, and other nutritive substances (such as occur in the medullary rays, for example), the filaments kill the living contents and feed on them. The result is that what remains unconsumed acquires a darker colour, and this makes itself visible in the mass to the unaided eye as a rosy or purple hue, gradually spreading through the attacked timber. As the destructive action of the fungus proceeds in the wood, the purple shades are gradually replaced by a yellowish cast, and a series of minute black dots make their appearance here and there; then the black dots gradually surround themselves with the white areas, and we have the stage shown in Fig. 13.

These white areas are the remains of the elements of the wood which have already been completely delignified by the action of the ferment secreted by the fungus filaments—*i.e.* the hard woody cell-walls have become converted into soft and swelling cellulose, and the filaments are dissolving and feeding upon the latter (Fig. 14). In the next stage of the advancing destruction of the timber the black dots mostly disappear, and the white areas get larger; then the middle-lamella between the contiguous

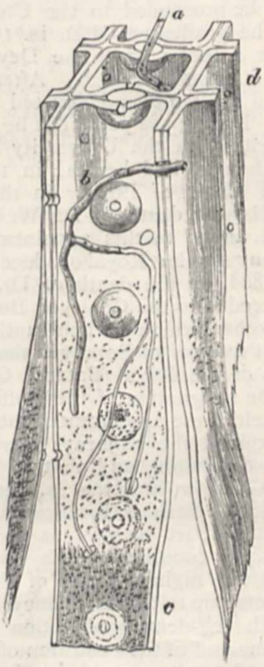


FIG. 14.—Sectional view of a tracheide of the spruce-fir, attacked by the hyphæ (*a*, *b*) of a *Trametes*, highly magnified (after Hartig). The upper part of the tracheide has its walls still sound, though already pierced by the hyphæ; the lower part (*c*) has the walls completely delignified, and converted into cellulose, which swells up and dissolves. The middle-lamella is also undergoing diss lution. The holes in the walls have been bored by hyph e.

elements of the wood becomes dissolved, and soft places and cavities are produced, causing the previously firm timber to become spongy and soft, and it eventually breaks up into a rotting mass of vegetable remains.

It will readily be understood that all these progressive changes are accompanied by a decrease in the specific gravity of the timber, for the fungus decomposes the substance much in the same way as it is decomposed by putrefaction or combustion, *i.e.* it causes the burning off of the carbon, hydrogen, and nitrogen, in the presence of oxygen, to carbon-dioxide, water, and ammonia, retaining part in its own substance for the time being, and living at its expense. H. MARSHALL WARD.

(*To be continued.*)

PROFESSOR ALEXANDER DICKSON.

THE close of 1887 has been marked by a long death-roll in the ranks of science. In the company of botanists it has been especially heavy, and now the sad news of the tragically sudden death of Prof. Alexander

Dickson, at the early age of fifty-one, comes upon us with startling unexpectedness. Two days before Christmas Prof. Dickson left Edinburgh, in his usual health and vigour, for Hartree House, his Lanarkshire residence. During the following days he spent much of his time in the favourite pastime of curling, which he much enjoyed. On Friday last, December 30, 1887, he was in exceptionally good spirits on the ice; his side was winning a close match, and he entered keenly into the excitement of the moment, when, without warning, he dropped dead in the act of making a shot.

Alexander Dickson was born in Edinburgh on February 21, 1836, the second son of David Dickson, of Hartree and Kilbucho, extensive estates in Lanarkshire and Peebles-shire, to which he afterwards succeeded, his elder brother having predeceased his father. Educated when a boy at home, he proceeded to the University of Edinburgh, where he graduated M.D. in 1860, obtaining a gold medal for a thesis on "The Development of the Seed-vessel of *Caryophyllaceæ*." After graduating, he soon abandoned medicine, and devoted himself to botanical pursuits. During the year 1862 he acted as Deputy Professor of Botany in the University of Aberdeen for Prof. Dickie, then in bad health; in 1866 he was appointed to the Chair of Botany in the University of Dublin, vacant by the death of Dr. W. H. Harvey; and a year later he added to this appointment that of Professor of Botany in the Royal College of Science for Ireland. In 1868, on the death of Dr. G. W. Walker-Arnott, he succeeded to the Chair of Botany in the University of Glasgow, which he held until 1879, when he was appointed Professor of Botany in the University, and Regius Keeper of the Royal Botanic Garden, in Edinburgh, upon the resignation of these offices by Dr. J. H. Balfour. He held these appointments at the time of his death. He received the honorary degree of M.D. from the University of Dublin, and that of LL.D. from the University of Glasgow, and was a member of various learned Societies. Besides his scientific life he had another important part to play as a laird with large properties in three counties, and he was a model landlord. He had the highest ideas of the duties of his position, and acted up to them. Money, time, and energy were given with self-denying devotion to the improvement of his farms and of the condition of his tenants, and no better-ordered estates could be found than those which he controlled. He was a Deputy-Lieutenant of Peebles-shire, and took an active share in all the functions which his position entailed.

By the death of Alexander Dickson the botanical world loses one of its best morphologists. He wore the mantle of the old French school typified in Mirbel, Richard, St. Hilaire, and Payer, of which Baillon is at present the foremost French representative; and at a time like the present, when it is a fashion to decry morphology, his loss falls all the more heavily. No botanist in this country had so full and accurate a grasp of organography. His published papers, numerous and valuable as they are, afford but an imperfect idea, significant indeed, of the wealth of his knowledge, and the keenness of his perception. Those who came in contact with him will remember the fascination of his discourse, and the surprising variety and aptness of the illustrations which he could bring up one after the other to support his own views or confound those of an opponent. In all his scientific work the strong conservatism of his nature found expression. His cautious and logical mind did not allow of his following with enthusiasm rash speculations of the more ardent botanical workers; and the flood of literature on botanical subjects which is poured out year by year had no terrors for him, as he acted upon the principle, which many will agree is a sound one, that, if you leave the literature until it is a year or two old, what is worth reading sifts itself. The soundness of his judgment upon scientific problems may in some measure be traced to the influence of the precept and example of that glorious band of real teachers, which at the time of his University career made Edinburgh a centre of attraction in the intellectual world; and a good illustration of his force of mind is to be found in his attitude towards the much-discussed question of the growth of the cell-wall. Having satisfied himself that the apposition theory was a sufficient explanation, he consistently opposed Nägeli's intussusception theory during the years when it was all but universally accepted; and now the botanical world has come round again to regard an apposition theory as that which has the better basis in fact.

All organographic questions had a peculiar interest for Dickson. A considerable portion of his own work was devoted to the elucidation of the true nature of the flowers in *Coniferæ*. As the result of his researches on *Dammara* and observations on other *Coniferæ*, he adopted Baillon's view of the carpellary nature of the integument in *Pinus*, and, notwithstanding the defection by Strasburger, who originally supported this view, he continued to maintain it. Phyllotaxis was a subject to which he devoted great attention, and upon which he published several important papers. Amongst his most valuable researches are those on the embryogeny of *Tropæolum*, in which he traced the history of the peculiar roots by which the embryo is nursed in the seed; and the records of his embryological researches in *Pinguicula*, *Ruscus*, *Zostera*, *Phœnix*, *Delphinium*, and other plants, are very interesting and valuable contributions to knowledge. In recent years he gave considerable attention to the construction and development of pitcher-plants, and proved the true nature of the parts of their complex organs; and the structure of the *Hepaticæ* also engrossed him, one of his last papers being upon some species of this group, in which he joined issue with Leitjeb upon some fundamental points. In all his work there may be seen the scrupulous accuracy and attention to detail which was a leading feature in his character, and no man ever worked with more care and jealous regard for truth and with a more generous appreciation of the work of others.

Amidst the work of his scientific life and the duties connected with his estates he found time to cultivate the artistic side of his nature, which was developed in no ordinary degree. He was an accomplished and enthusiastic musician, and in later years found peculiar pleasure in collecting Gaelic airs. At botanical excursions to the Highlands he might be frequently found noting down an air as it was droned by a gillie or whistled by a herd, and he amassed a considerable number of these airs, which at one time he thought of publishing. He was also a very skilful draughtsman, and his drawings in chalk on the slate were quite a feature of his lectures.

In the discharge of every duty he was most conscientious, and his unostentatious kindliness attracted everyone who had dealings with him. Quiet and retiring in disposition, he was endeared to all by the nobility of his character and his sympathetic nature. As Professor his students loved him; as laird his tenants loved him. It has been said of him he could never lose a friend, for he never could say an unkind word or omit to do a kind action, and in this estimate all who knew him will concur. The news of his death will be heard with sorrow by a wide circle of friends, and bring sadness to many a heart which will mourn for one who had fine generosity of the kind that lets "not the right hand know what the left hand doeth."

NOTES.

THE Municipal Council of Paris proposes to establish in the Faculty of Sciences a new professorship devoted to the philosophy of biology, and especially to the teaching of the doctrines of Darwin. This distresses some of the older French zoologists, but "their reign," a correspondent writes to us from Paris, "is coming to an end, and notwithstanding their obstinate and unintelligent opposition, Darwinism is the creed of all the younger French naturalists. The only trouble with regard to the new professorship will be to put the right man in the right place."

PROF. BONNEY'S course of lectures on geology begins at University College on Wednesday, January 11, and will be continued on the Thursdays, Tuesdays, and Wednesdays following. The course on economic geology begins on the following Friday, and will be continued on that day in each week.

A COURSE of about six lectures on "Photographic Chemistry" will shortly be delivered by Prof. R. Meldola, F.R.S., at the Finsbury Technical College. The course will begin on Wednesday, January 18, at 7.30 p.m., and be continued on successive Wednesdays. The object of the lecturer will be to develop the scientific principles upon which modern photography is based, so as to enable professional and amateur photographers to keep abreast of recent advancements in the subject. Those who attend the lectures will have the opportunity, if they desire it, of receiving practical laboratory instruction in the testing and valuation of photographic chemicals.

AT the opening meeting of the session of the Society of Telegraph-Engineers and Electricians, on Thursday, the 12th instant, the new President, Mr. Edward Graves, will deliver his inaugural address.

IN the abstracts of the Proceedings of the Chemical Society (Jan.-Dec. 1887) we find the following list of grants made from the Research Fund of the Society during the year:—£25 to Prof. E. H. Rennie, for the further study of the red colouring-matter of *Drosera whittakeri*; £25 to Mr. Holland Crompton, for the study of the action of nitric acid on copper-zinc and copper-tin alloys with the object of determining whether the metals exist in combination or admixed; £10 to Mr. C. H. Bothamley, for experiments on the use of dyes in photography, and especially on the sensitizing action of the dye; £25 to Mr. W. P. Wynne, for the determination of the nature of the products formed on oxidizing nitric oxide by admixture with oxygen; £10 to Mr. A. Wynter Blyth, for the study of the constitution of butter-fat.

METEOROLOGY is indebted to Dr. J. Hann for an exhaustive discussion of the distribution of atmospheric pressure over Central and Southern Europe, based upon the monthly and yearly means at 205 stations, for the thirty years 1851-80. Very few such discussions have been undertaken since the appearance of Buchan's great work, about twenty years ago. The author insists on the application of the correction for gravity, which up to the present time has been generally neglected. Only a few observations for France have been used in this discussion, but a work of a similar nature is in hand for that country by M. Angot. Charts are also drawn for each month and for the year, showing the isobars for every ·02 inch at the sea-level, and also for four months and the year at the level of 500 metres above the sea. The work forms Part 2, vol. ii. of Dr. Penck's "Geographische Abhandlungen" (Vienna, 1887).

THE Chief Signal Officer of the United States has issued a circular, dated December 6, stating that as, in his belief, the great value of simultaneous maps consists in showing the general features of the weather of the northern hemisphere, he has decided to reproduce daily a chart showing the general outlines of the pressure and wind for certain selected stations, although as before stated (NATURE, Dec. 8, p. 137), he cannot guarantee their continuance for any great length of time. The charts have commenced with October 1, 1886, and are based on the observations taken at noon, Greenwich time; the temperature observations are not represented, owing to the limited means available for such work.

THE Central Physical Observatory of St. Petersburg has issued a very useful table, showing for all its telegraphic stations the normal temperatures for 7 a.m. for each month, calculated from the number of years available for each place. From these values the Observatory constructs diagrams showing the annual march of the temperature, and from these curves the normal temperature may be calculated very closely for each day of the year. These data enable the Observatory to introduce into its Daily Weather Reports the departure of the temperature day by day from its normal value. The normal temperatures for foreign stations are to be similarly dealt with subsequently.

THE Swedish Government has given notice that in the beginning of the year 1888, a fog signal will be established near Hallands Wäderö lighthouse, on the eastern shore of the northern approach to the Sound, Kattegat. The signal will be a steam syren, which, during thick or foggy weather, will give *two blasts every minute*, in the following manner; a low note of *seven seconds* duration, an interval of *three seconds* silence, then a high note of *three seconds* duration, followed by an interval of *forty-seven seconds* silence.

THE Government *Gazette* of the Colony of Lagos of July 30 last contains monthly meteorological means for the year 1886. The observations are made at the Colonial Hospital, lat. 6° 27′ N., long. 3° 26′ E.

ON December 11, about 5.30 p.m., a brilliant meteor was seen in and around Christiania. It moved slowly in a south-easterly direction, and disappeared behind a bank of clouds. Its light, of a yellow-green colour, was very intense. The passage occupied about five seconds.

EARTHQUAKES on December 16 and 17 are reported from Prinpolje and Plewlje, in Bosnia. At Werny, in Turkistan, a shock occurred at midnight on December 16. A shock was noticed at Geneva on December 19 between 5 and 6 p.m. A telegram from Mexico states that a sharp shock occurred there at half past 7 on January 2.

A NUMBER of highly interesting experiments upon the behaviour of passive iron towards nitric acid when placed in a powerful magnetic field have recently been made by Messrs. Nichols and Franklin (*Amer. Journ. of Science*, December 1887). About 8 cubic centimetres of nitric acid, specific gravity 1·368, were poured upon a gramme of powdered iron contained in a perfectly clean test-tube. This test-tube was immersed in water contained in an outer glass vessel, and the temperature of the contents of the tube could be accurately ascertained by means of a thermometer suspended in it. The whole apparatus was then carefully arranged between the poles of an electro-magnet specially constructed to give a field as uniform as possible. Before actuating the magnet it was found that the iron remained perfectly passive in presence of the nitric acid until the temperature was raised to 89°, when the usual explosion consequent upon loss of passivity occurred. But on repeating the experiment when a powerful current was traversing the coils of the electro-magnet, effervescence commenced at once, and at 51° the explosion occurred in a most violent manner, projecting most of the liquid out of the tube. The remainder of the liquid, however, remained quiescent until the iron was touched with the thermometer-bulb, when a second explosion occurred. In a third experiment the magnet was not actuated until the apparatus was heated to 60°; but the moment the current was allowed to pass the explosion occurred instantaneously. Hence it appears that the action of the magnet is to lower the temperature of transition from the passive to the active state. In attempting to determine the cause of this singular phenomenon, it was found that when two iron bars placed parallel to the lines of force in the magnetic field were submerged in any liquid capable of attacking iron, the ends of one bar and the central portions of the other being alone allowed to come into actual contact with the liquid, the bar with ends exposed became in relation to the other as zinc to platinum, so that on connecting the bars by wires a permanent current was found to flow. Hence it is supposed that, in case of a single mass of iron, local currents will be set up between those parts in which magnetic poles are induced and the intermediate parts, and Messrs. Nichols and Franklin are of opinion that these local currents are the cause of the curious behaviour of passive iron in the magnetic field.

A MEETING was held at Philadelphia on December 12 to celebrate the hundredth anniversary of the birth of Thomas Hopkins Gallaudet, the pioneer of the movement for the instruction of the deaf in America. A short biographical sketch of Gallaudet was read, and one of his poems was recited by four deaf girls in the sign-language. Prof. Graham Bell delivered an address, which was interpreted into the sign-language as rapidly as it was spoken, and, according to *Science*, was greatly appreciated by the many deaf persons in the audience. The two sons of Gallaudet, both of whom are engaged in continuing the work of their father—one as the President of the deaf-mute College at Washington, the other as a pastor for the deaf—were present, and made remarks suitable to the occasion.

MESSRS. MACMILLAN AND CO. will publish immediately a new Treatise on Algebra, by Mr. Charles Smith, of Sidney Sussex College, Cambridge, whose previous text-books on Conic Sections, on Solid Geometry, and on Elementary Algebra have been very favourably received. The new book is designed for the use of the higher classes of schools and the junior students in the Universities. One important change is made from the usual order adopted in English text-books on algebra, in that some of the tests of the convergency of infinite series are considered before such series are made any use of. A knowledge of the elementary properties of determinants being of great and increasing practical utility, Mr. Smith has introduced a short discussion of their fundamental properties, founded on the treatises of Dostor and Muir. No pains have been spared to insure variety and interest in the examples, which have been selected from numerous examination-papers and from the mathematical journals.

MESSRS. MACMILLAN AND CO. have in the press a treatise on Higher Arithmetic and Elementary Mensuration, by Mr. P. Goyen, Inspector of Schools in New Zealand. Feeling the defect in most text-books of arithmetic that the worked-out types are all of the simplest character, while the exercises which follow them abound in difficulties, Mr. Goyen has worked out an immensely large number and variety of graduated types, and taken great pains to adapt the exercises to them. In the mensuration, wherever the geometrical proof of a rule is quite simple, it is given. A chapter on surds is inserted, because a knowledge of surd operations is useful in mensuration, and is required in many public examinations.

A GOOD address on some sociological aspects of sanitation was lately delivered before the Philosophical Society of Glasgow by Dr. James B. Russell, President of the Society. This address has now been published. It contains some excellent remarks on the extent to which the State has a right to limit individual freedom in the attempt to establish the conditions of public health.

THE Journal of the Straits Branch of the Royal Asiatic Society (No. 18), just received, contains the Malay text, with an English translation, of "Raja Donan," a Malay fairy-tale. This is one of a series of *cheritras*, taken down, word for word, from the lips of Mir Hassan. Among the other contents of the number are an essay (continued from No. 17) towards a bibliography of Siam, and an English, Sulu, and Malay vocabulary.

THE editorship of the well-known Brunswick scientific journal, *Globus*, changed with the new year, or, rather, with the commencement of the fifty-third volume on December 19. Dr. Emil Deckert takes the place of Dr. Richard Kiepert, and at the same time an alteration is made in the sub-title. Where, formerly, this read, "*mit besonderer Berücksichtigung der Anthropologie und Ethnologie*," it now reads "*mit besonderer Berücksichtigung der Ethnologie, der Kulturverhältnisse, und des Welthandels*," and in an address to the reader the editor and publishers explain that the alteration represents a corresponding alteration in the programme. As before, every effort will be made to supply abundant information of a geographical and ethnological character; but as German national interests have largely developed and extended in the last few years, in future a good deal more attention will be devoted to questions connected, as we understand it, with German possessions and German interests abroad. The practical effects of thus enlarging the scope of the journal are not apparent in the number before us, but it may be hoped that *Globus* will not lose its character as a popular educator in geography and the allied subjects. Why a similar journal has not been established in this country is a mystery.

AN American journal devoted to geology and the allied sciences has just been started. It is called the *American Geologist*, and will for the present be published at Minneapolis, Minn.

MR. A. SIDNEY OLLIFF, of the Australian Museum, Sydney, writes to the January number of the *Entomologist* about giant Lepidopterous larvæ in Australia. The larva of *Chalepteryx collesi*, a large moth which was unusually abundant during the past summer in the vicinity of Sydney, often, he says, attains the length of 7 inches, and is robust in proportion. This moth feeds on various Eucalypti, and is of a rich satiny-brown colour; each segment, except the first, is furnished with eight yellow verrucose spots, which emit long brown bristles; the anal extremity, a yellow band on the first segment, and two additional verrucose spots on the second and third segments also give rise to bristles. The cocoon, as well as the larva of this species, is armed with fine and exceedingly sharp bristles, which, if carelessly handled, readily penetrate the skin, causing considerable irritation. The larva of the beautiful swift (*Zelotypia stacyi*) measures 8 inches when full grown, and Mr. Olliff has seen several *Cossus* larvæ of similar dimensions.

IN the *Entomologist* for January, Mr. Alfred Bell offers some suggestions about post-Glacial insects. So far as his experience goes, insect remains are by no means common, and belong chiefly to the Coleoptera. He gives thirty species, nearly all of which belong to this division of the insect world. As Mr. Bell points out, however, it does not follow that Lepidoptera were not present during the post-Glacial period, since they occur in beautiful preservation in deposits of much older date in England and on the Continent. The nature of the post-Glacial soils was not favourable to the preservation of soft-bodied animals. "Hence," says Mr. Bell, "if anyone knows of Lepidoptera retained in a fossil state, it will be of real service to science if he will say where they were found, and under what conditions."

IN the January number of the *Zoologist*, Mr. Allan Ellison has an interesting article on the autumnal migration of birds in Ireland. He says that the migration movement of last autumn in Ireland was in all respects a most exceptional one. Some of the migrants appeared unusually early, and all in much larger numbers than Mr. Ellison had ever before observed. On October 8 he saw the first flocks, both of starlings and redwings. On the same day, and for about a week after, immense numbers of golden plovers were passing over, flying towards the west and south-west in large V-shaped strings. This was about the usual time for starlings and redwings, but early for golden plover. On the 11th again both redwings and starlings were constantly passing. On the 16th he observed a great host of fieldfares, many thousands in number, winging their way across the sky towards the south-west. From October 17 to the beginning of November the starling migration was at its height, the flocks being much larger and more numerous than he had ever observed in former years. He saw four within a quarter of an hour on the afternoon of the 18th. At 4 p.m. on the 22nd, the largest flock he ever saw passed over. It was in the form of a column, perhaps nearly a mile long, and must have numbered thousands, spanning the sky from horizon to horizon for more than half a minute, and was followed in a short time by two smaller flocks. All the latter part of October skylarks were from time to time flying over, generally large straggling flocks or scattered individuals, flying nearly out of sight, but their call-notes being distinctly audible. Mr. Ellison hopes that those who are favourably situated for observing the arrival of winter birds will report whether they have noticed a corresponding abundance of migrant this season.

THE new number of *Mind* opens with an able and suggestive article on pleasure, pain, desire, and volition, by Mr. F. H. Bradley. Mr. J. McK. Cattell has an interesting paper on the Psychological Laboratory at Leipzig. Mr. T. Whittaker writes on individualism and State action; and Mr. D. G. Ritchie on origin and validity.

PARIS is soon to have a Museum of Religions. M. Guimet, of Lyons, who has been a great traveller, has been engaged for years past in collecting altars, priests' robes, and other objects relating to religious ceremonies. These objects he presented some time ago to Paris on condition that a building should be specially devoted to them. This building, close to the Trocadéro Palace, has just been finished, and the collection will soon be transferred to it.

THE additions to the Zoological Society's Gardens during the past week include two Spotted Ichneumons (*Herpestes nepalensis*), a —— Fox (*Canis* ——) from Afghanistan, presented by Lieut.-Colonel Sir Oliver B. C. St. John, K.C.S.I.; a Common Otter (*Lutra vulgaris*), British, presented by Mr. Edward Hart; a Red-throated Diver (*Colymbus septentrionalis*), British, presented by Mr. Charles A. Howell; two Greater Sulphur-crested Cockatoos (*Cacatua galerita*) from Australia, presented by Master Rankin.

OUR ASTRONOMICAL COLUMN.

BRAZILIAN RESULTS FROM THE TRANSIT OF VENUS.—M. Cruls, in a note to the Paris Académie des Sciences, states that the reports of the various expeditions sent out by the Brazilian Government to observe the transit of Venus in 1882 are almost entirely printed, and will shortly appear. Three stations were occupied, viz. S. Thomas in the Antilles, Olinda in Brazil, and Punta-Arenas in the Straits of Magellan. The Baron de Teffé was in command of the first expedition; M. J. d'O. Lacaille of the second, and M. Cruls himself of the third. The duration of the transit at Punta-Arenas was nearly the mean duration, both ingress and egress being slightly accelerated. The two more northern stations had the duration much shortened, ingress being retarded, and egress accelerated. The chief observer at each station was supplied with an equatorial of 6·3 inches aperture; and at S. Thomas two other telescopes of 4·5 and 4·1 inches respectively were also used. At Olinda likewise there was a second telescope in use, of 4·5 inches aperture. The method of projection was employed in order to get rid of the physiological effects produced by the intensity of the solar light, and in combining the observations made with telescopes of different apertures, weights were given to them proportional to the square of the diameters of the object-glasses, in accordance with the results obtained from the experiments of MM. Wolf and André. The resulting parallax from the internal contacts is 8″·808.

THE ASTEROIDS.—Prof. Daniel Kirkwood, of the Indiana University, has just issued a short essay on the asteroids or minor planets, this group of tiny bodies being entitled on many grounds to more particular consideration than it has yet generally received. The first part of the essay gives a brief sketch of the history of the discovery of the first five asteroids, together with the names of the discoverers and date of discovery of all as yet known to us, and a table giving the elements of their orbits. Prof. Kirkwood makes it clear that the numbers of those still unknown are practically inexhaustible, for if Leverrier's estimate be correct, that the quantity of matter contained in the group cannot be greater than one-fourth the mass of the earth, it would yet require no fewer than 72,000,000 bodies as large as Menippe to make up this amount. Fortunately the rate of discovery appears limited to ten or a dozen per annum, so that there is no immediate danger of our being overwhelmed by the impossibility of following up some few millions of orbits. The second part of the work deals with questions relating to the origin of the group, and with certain relationships apparent in their orbits, particularly with regard to the irregular distribution of the asteroids in their ring, certain districts being left entirely void, viz. those where the asteroid would have a period commensurable with that of Jupiter. Prof. Kirkwood has on former occasions repeatedly shown how Jupiter would tend to eliminate bodies revolving in these positions by increasing the eccentricities of their orbits until their perihelion distances fell within the body of the sun itself, and he has accounted for the gaps in the ring of Saturn upon a similar principle. Prof. Kirkwood is of opinion that several of our periodic comets may have been originally members of the asteroid family. All the thirteen comets whose periods correspond to mean distances within the asteroid zone have direct motion, and inclinations similar to those of the minor planets, and their eccentricities are generally less than that of other known comets; whilst five of these comets have periods respectively corresponding to some of the most marked gaps in the asteroid zone.

Prof. Kirkwood makes no reference to the importance of certain members of the group as affording means for the determination of the solar parallax, which many astronomers will consider to be their most useful function, and as compensating for the enormous labour, both of observation and computation, involved in following the paths of so great a number of wanderers. And it would have been exceedingly useful if he had supplemented his other tables by one showing those asteroids which have only been observed during one opposition. Some of those theoretically the most interesting have not been observed for several years, and are practically lost to us, and it would seem a matter of more pressing importance at the present time that these should be picked up again, if possible, rather than fresh additions should be made to a list already unmanageably long.

OLBERS' COMET.—The following ephemeris for Berlin midnight, by Dr. Krueger (*Astr. Nach.* No. 2818), is in continuation of that given in NATURE for 1887 December 15:—

1888.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Jan. 6	16 50 20	1 22·7 S.	0·2486	0·3821	0·45
8	16 54 8	1 43·4 S.			
10	16 57 51	2 3·4 S.	0·2583	0·3854	0·43
12	17 1 29	2 23·6 S.			
14	17 5 3	2 41·2 S.	0·2679	0·3884	0·40
16	17 8 31	2 59·2 S.			
18	17 11 55	3 16·6 S.	0·2773	0·3910	0·38
20	17 15 14	3 33·4 S.			
22	17 18 29	3 49·6 S.	0·2866	0·3932	0·36

The brightness on August 27 is taken as unity.

Dr. E. Lamp succeeded in seeing the comet for a short time on December 12, and concludes, from a very rough comparison with a star that the ephemeris then required a correction of + 8s. in R.A., but was practically right in Decl.

The Clinton Catalogue.—The *Sidereal Messenger* for December announces that the great catalogue of 30,000 stars, upon which Dr. Peters and his assistant, Prof. Borst, have been engaged for several years past, is virtually completed, and ready for the press, and its publication is expected during the present winter. In the prosecution of this work Prof. Borst has gathered the stars from the various astronomical publications of the last fifty years, and reduced them to the epoch of the forthcoming catalogue.

Occultations of Stars by Planets.—Herr A. Berberich calls attention in the *Astronomische Nachrichten*, No. 2814, to the importance of observations of occultations of stars by the planets, and supplies a list of stars which may possibly be occulted by either Venus, Mars, Jupiter, or Saturn, during the course of the present year. Such observations have been extremely rare, yet they would prove extremely important, for they would throw light on the extent and density of the planetary atmospheres, and would afford a means in the cases of Mars and Venus for the determination of parallax and diameter. Herr Berberich adds that in the case of the three outer planets the occultation of a star by the primary would afford a specially favourable opportunity for the determination of the positions of the satellites, since micrometer measures of their places as referred to the occulted star would be free from many errors to which the direct comparison of the planet and its satellites is exposed.

The following stars may possibly undergo occultation during the next fortnight:—

Planet.	G.M.T. of Conjunction in R.A. h. m.	Star. °		Mag.	Pl − ✻ Δδ ′	Max Duration. m.
♀	Jan. 5 16 29·2	S.D. − 17	No. 4187	9·7	−0·13	6·0
♀	9 18 1·4	18	4279	9·5	+1·05	5·8
♂	12 3 41·4	4	3445	9·3	−0·18	7·4
♀	12 8 32·3	19	4401	9·3	+0·84	5·7
♀	14 18 40·8	19	4441	9·5	−0·19	5·6
♀	15 1 31·9	20	4446	9·5	+0·38	5·5
♀	17 23 22·5	20	4635	9·3	−0·57	5·4

The maximum duration is the interval between immersion and emersion for a central occultation.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 JANUARY 8-14.

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

At Greenwich on January 8

Sun rises, 8h. 7m.; souths, 12h. 6m. 49·1s.; sets, 16h. 7m.: right asc. on meridian, 19h. 16·8m.; decl. 22° 17′ S. Sidereal Time at Sunset, 23h. 18m.

Moon (New on January 13, 9h.) rises, 2h. 14m.; souths, 7h. 34m.; sets, 12h. 44m.: right asc. on meridian, 14h. 43·4m.; decl. 10° 21′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	7 55	11 40	15 25	18 50·0	24 27 S.
Venus.....	4 36	9 1	13 26	16 10·3	18 29 S.
Mars......	0 11	5 53	11 35	13 1·9	4 14 S.
Jupiter....	4 16	8 38	13 0	15 47·4	19 4 S.
Saturn....	17 28*	1 19	9 10	8 27·1	19 39 N.
Uranus...	0 23	5 55	11 27	13 4·3	6 8 S.
Neptune..	12 51	20 31	4 11*	3 42·5	17 56 N.

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

Occultation of Star by the Moon (visible at Greenwich).

Jan.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
9	η Libræ	6	6 18	6 46	341 298

Jan.	h.	
9	11	Jupiter in conjunction with and 4° 12′ south of the Moon.
10	0	Venus in conjunction with and 2° 16′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m
U Cephei	0 52·4	81 16 N.	Jan.	10, 22 22 *m*
ζ Geminorum	6 57·5	20 44 N.	,,	9, 3 0 *m*
			,,	14, 3 0 *M*
R Canis Majoris	7 14·5	16 12 S.	,,	11, 22 53 *m*
			,,	13, 2 9 *m*
U Monocerotis	7 25·5	9 33 S.	,,	11, *m*
S Cancri	8 37·5	19 26 N.	,,	9, 23 12 *m*
R Leonis	9 41·5	11 57 N.	,,	8, *m*
R Ursæ Majoris	10 36·7	69 22 N.	,,	8, *M*
T Ursæ Majoris	12 31·3	60 6 N.	,,	12, *m*
W Virginis	13 20·3	2 48 S.	,,	8, 22 0 *m*
δ Libræ	14 55·0	8 4 S.	,,	11, 4 34 *m*
U Coronæ	15 13·6	32 3 N.	,,	8, 0 45 *m*
U Ophiuchi	17 10·9	1 20 N.	,,	8, 4 38 *m*
		and at intervals of		20 8
T Vulpeculæ	20 46·7	27 50 N.	Jan.	11, 0 0 *m*
			,,	12, 1 0 *M*
Y Cygni	20 47·6	34 14 N.	,,	10, 21 7 *m*
			,,	13, 21 0 *m*
W Cygni	21 31·8	44 53 N.	,,	14, *M*
δ Cephei	22 25·0	57 51 N.	,,	8, 4 0 *M*
			,,	11, 22 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl.	
Near ξ Virginis	174	9 N.	January 11.
,, ζ Boötis	220	14 N.	Very swift; streaks.
,, β Boötis	222	42 N.	Very swift; streaks.

DUNÉR ON STARS WITH SPECTRA OF CLASS III.[1]

I.

In publishing a few days before his death the last part of his discoveries relating to the spectra of stars of the third class, D'Arrest pronounced the opinion that henceforward there would be nothing essential to add to the knowledge then possessed of the stellar spectra of this class in the northern heavens. When D'Arrest died, 123 well-developed objects of Class III.*a* were known, and counting all the objects known, 150; the stars known in Class III.*b* were 23. Actually, the well-developed stars of III.*a* are 214, and if all are reckoned, 475; the stars of III.*b* are 55 at least.

The number of objects in Class III. with which we are acquainted has been tripled by recent researches, but, besides, the relation between the numbers of the stars in the two lower classes has been considerably altered, considering that at present there are 8·5 stars III.*a* instead of 6·5, to 1 star III.*b*. However, we should commit a serious error if we drew the conclusion that in reality the spectra III.*b* were not more than nine times rarer than III.*a*. On account of the enormous width of the bands, one is able without any difficulty to recognize the nature of a spectrum III.*b* in very faint stars, which one is not able to do in III.*a*, unless in the rare objects of this class in which the bands are more marked and broader than usual.

I find this opinion confirmed by the fact that the researches of M. Vogel give more than 200 new spectra III.*a*, and have scarcely led to an acquaintance with one new spectrum III.*b*. It is very probable therefore that we are already acquainted with all these stars to the magnitude of 7·5 inclusive; this is rendered still more probable by the following table, which gives the number of the stars III.*a* and III.*b* belonging to different magnitudes:—

Magnitude.	Class III.*a*. Observed.	Class III.*a*. Calculated.	Class III.*b*. Observed.	Class III.*b*. Calculated.
1·0–1·9	2	1	0	0
2·0–2·9	5	3	0	0
3·0–3·9	9	11	0	0
4·0–4·9	31	28	0	1
5·0–5·9	88	90	2	2
6·0–6·9	134	380	11	8
7·0–7·9	151	—	18	24
8·0–8·9	37	—	14	—
9·0–9·9	18	—	10	—

[1] We have already referred generally to M. Dunér's important memoir published in the Transactions of the Swedish Academy. We now give a translation of his general conclusions.—Ed.

In the columns headed "Calculated" are the numbers obtained by multiplying by 4/3 the numbers of the stars in the classes of different magnitude given in the "Wunder des Himmels," by Littrow, 5th ed. p. 577, deduced from the *Durchmusterung* of Argelander, and then dividing the result by 15 and 750 respectively. For the stars III.*a* the agreement is almost perfect up to the magnitude of 5·9 inclusive, and for III.*b* up to 6·9; up to 7·9 the agreement is pretty good, whilst after that the numbers observed are more and more in arrear of the numbers obtained by calculation. We may conclude therefore that our knowledge of the spectra III.*a* is almost complete up to the magnitude of 5·9 inclusive, and of spectra III.*b* up to 7·5 inclusive. The researches of M. Vogel have not added any new star III.*a* with a magnitude higher than 5·0, and only very few between 5·0 and 6·0, and, as I have already said, no new star III.*b* above the magnitude 7·5, although he has examined all the stars up to this limit of magnitude between − 2° and + 20° of declination. As to the difference existing between "observation" and "calculation" in the case of the III.*a* feebler than 6·0, we must remember that as yet no systematic spectroscopic research has been made of the stars between − 2° and − 23°, nor between + 20° and the North Pole. Consequently the number of stars III.*a* between 6·0 and 7·5 will probably be much increased before very long, and will approach the theoretic number. On this account I imagine the stars of III.*b* are fifty times rarer than those of III.*a*.

The list of these rare stars is probably already very complete for that part of the sky visible in Europe, for the nearer the researches of an astronomer are to the present time the feebler are the stars with spectra of this class which he discovers (Secchi 6·7, D'Arrest 7·0, Vogel 7·1, Duner 8·3, Pickering 9·1). The conclusions, therefore, which we can draw as to the manner in which these stars are distributed over the heavens deserve some confidence. Such a research is very interesting. We have already seen that the principal bands in these spectra owe their origin to the presence of a carbon compound in the atmosphere of the stars. It is important to know whether there is a certain direction in the heavens in which these stars are more numerous than in others, especially when we consider that the same substance is present in comets, which come from interstellar space. I have made such a research, and have come to the conclusion that the objects in question are grouped similarly to stars in general, being closer together in the neighbourhood of the Milky Way. Setting out from the position of the Pole of the Milky Way given by Heis, R.A. = 12h. 42m., Decl. = + 26° 8′, for the equinox 1900·0, I have calculated the quantities P, or the distances of the stars from this Pole, given in my catalogue. But to have my list a little more complete for the part of the heavens invisible in Europe, I have calculated the same quantity also for the following stars, whose spectra have been examined by M. Pechülé ("Expédition Danoise pour l'Observation du Passage de Venus," 1882, pp. 40–43).

Star.	Magnitude.	R.A. 1900·0. h. m.	Decl. 1900·0. ° ′	P. °
65 Schj.	8	5 40	− 46 30	60·6
103 Schj.	8	7 54	− 49 43	80·6
125	7·5	9 51	− 41 7	78·6
126	8·5	9 57	− 59 45	87·0
128	7	10 8	− 31 30	71·5
130	6·5	10 31	− 39 3	72·6

By the help of the P's found, I have obtained the following table, which indicates the numbers of those stars which are between the different limits of distance of the Poles, boreal or austral, of the Milky Way.

Limits of Polar Distance. ° °	No. of Stars.	Mean Magnitude.
0–35	3	6·6
35–60	8	6·6
60–70	8	7·2
70–80	13	7·4
80–90	29	8·3

It is at once seen that there is an immense accumulation between 80° and 90° of polar distance, and that the polar regions are totally empty up to 19° distance from the Pole; and this relation would doubtless become still more striking if our knowledge of these stars which are invisible in Europe was more complete; for, whilst the two Polar regions are for the most part visible, a great part of the Milky Way is always below our horizon. Besides the number of stars in the different zones, I have also calculated their mean magnitudes, and it will be seen that for them, as well as for other stars, there is this rule—that in the Milky Way the faint stars are much closer together than in the neighbourhood of its Poles.

One might perhaps suppose that there is a certain portion of the Milky Way where the stars III.*b* are more frequent than elsewhere. In order to decide this it is necessary first to calculate for each star the quantity which has the same relation to the Milky Way as the right ascensions have to the equator; and then make a table, on the distribution, having this quantity as its foundation. Such a research cannot, however, lead to good results as long as our acquaintance with the stars between 25° of south declination and the South Pole is almost *nil*. I will only say, then, that there is a great number of these stars around R.A. 305°, Decl. + 40°, but almost an equal number around R.A. 85°, Decl. + 25°. Now both these points are precisely those in which, in the northern hemisphere, the stars are closest together. It seems that they are grouped almost according to the same laws as all other stars, and that, properly speaking, there is no region where stars of the Class III.*b* abound.

A similar research of the stars III.*a* could not give exact results, as our acquaintance with these stars below magnitude 6·0 is still too imperfect. However, the researches which M. Pechülé undertook, with the aid of the *Uranometria Argentina*, on the distribution of the coloured stars, render it probable that these also are closest together in the neighbourhood of the Milky Way.

I have already said that in all probability the spectra of fixed stars must be subject to variations on account of the diminution in the temperature of stars which must take place sooner or later, and I observed that it is precisely on the supposition of such a diminution that the classes of M. Vogel are based. There are, however, eminent *savants* who have combated the correctness of this opinion, and who have formed ingenious hypotheses to prove the possibility that the sun, and consequently the stars also, may regain the heat which emanates from them. But it would be too much to say that these theories have victoriously withstood criticism, and the spectroscopic examination of the stars has given results fatal to them. Although the spectra of stars may be divided into very distinct classes, according to their characteristics, there are, on the other hand, numerous spectra of all possible grades between any two classes, so that it may be difficult, if not impossible, to decide to what class a star belongs, and that even when it is sufficiently brilliant for all the details of its spectra to be distinctly recognized. Besides, we see that the more the star resembles the first class, the brighter is its violet part, whereas the violet part becomes fainter and fainter or even invisible when the spectrum resembles that of α Orionis (III.*a*). On that account it seems certain that the spectra owe their characteristics to the greater or less degree of incandescence of the stars, so that the temperature of stars of Class III. must be relatively low.

Doubtless these changes do take place in the stellar spectra, although we must suppose that, as regards the spectra of the first two classes, they are almost exclusively secular, and operate so slowly that millions of years may pass before they become apparent.

It is different with stars of Class III. These being probably already much cooler than the others, we may reasonably expect that the changes will take place more rapidly, and perhaps also that from time to time temporary augmentations in activity will take place on their surface, followed by periodic changes in their spectra.

In the course of his observations Secchi arrived at the conclusion that the colours and spectra of these stars were subject to remarkable changes in a very short period. My observations led to the same conclusion, if observations from the years 1866 to 1874 may be trusted without reserve. For, without counting the few and unimportant discrepancies which I discovered between the aspect of several spectra and the descriptions given by earlier observers, I found that there are forty stars which have been comprised in Class III., among which there is scarcely one which now belongs to it, and there are some which ought to have been transported from one sub-class into the other. But, for reasons which I will here explain, such a conclusion would certainly be too hasty.

On the one hand, Secchi's observations date from a time which we may call the infancy of spectrum analysis, and the instruments employed were very imperfect; on the other hand, he was the first to introduce a classification of the stars according to their spectra. Therefore it is easily understood that Secchi was only able to seize gradually the characteristics of the different types (thus it was not until late that he introduced the fourth type); and again, he once changed the order so that the second and third types changed numbers. On account of this change, some errors may have found their way into his publications. Some even may be explained without having recourse to this supposition. All the spectra which I have excluded from the third class are, according to Secchi, indeterminable, except two, which I consider intermediate between II.*a* and III.*a*, and the two stars R.A. = 9h. 18m., Decl. = − 21° 50′, and R.A. = 18h. 14m. 40s., Decl. = +25°·2, which Secchi found independently of Schjellerup's catalogue of red stars. It might happen then that with a clear sky faint bands might be perceived,[1] and as to the last we may well suspect that there is some gross error in their positions, judging from what Secchi says as to the manner in which he discovered the first of them.[2] Such a supposition would not be admissible for the star 249 Schj. This star is situated, according to Herschel, amongst a mass of stars, and Secchi says of it, "Stella di 9ª rossa con rigoni nello spettro 4° tipo certamente" ("Memoria Seconda," p. 52). I have often examined the cluster in which this star was situated, but without perceiving it. We may therefore believe that it is variable of long period.

I still have to refer to the stars which ought to have been transferred from one *sub*-class to another. In the spectra of these, variability seems to me quite inadmissible, the two sub-classes being, as I shall try to prove soon, co-ordinated, and not successive phases of development which every star must undergo. I suppose that at the commencement of his observations of spectra of the third class, when Secchi met with stars III.*b* not very well marked, he did not think them different from III.*a*, and he did not perceive the difference until after having seen several spectra of this class as pronounced as those of 78 and 152 Schj. On the spectrum of the latter he still says in 1867, "In conclusione è tipo di *a* Ercole ma con zone vere mancanti" ("Catalogo," pp. 14, 15).

However, neither Secchi nor even D'Arrest examined a sufficient number of spectra III.*b* to thoroughly understand their characteristics. Both appear to admit that there are fundamental differences between spectra belonging to it. For instance, Secchi says of the spectrum of star 136 Schj., "E difficile dire se sia proprio del 4° tipo" ("Memoria Seconda," p. 42), and of the stars discovered by Wolf and Rayet with bright lines which are not hydrogen,[3] and dark bands in the spectra, and therefore certainly belonging to Class II.*b*, he says, "Accenneremo qui soltanto che esse appartengono al 4° tipo, ma sono di quelle a zone molto irregolari" ("Sugli spettri prismatici delle stelle fisse," p. 194), and "Ad ogni modo sono di 4° tipo, e le righe paiono del carbonio diretto" (*l.c.* p. 216). D'Arrest speaks as follows of the star 74 Schj., "Irregular spectrum of type IV." (*A.N.* 2016); and of the star 155*b* Schj., "Very remarkable spectrum, &c." (*A.N.* 2009). By collecting all my observations on all the spectra of this class it is seen that not one of them really departs from what may be called the normal spectrum. Doubtless there are in different stars notable differences in the darkness of the flutings, and in the brilliancy of the intervals, but all this does not prevent all the spectra being formed according to one constant type, as happens with Class III.*a*. Besides, Secchi seems to think that the aspect of a spectrum may change completely with the kind of spectroscope used. He says of the spectrum of 132 Schj., "Tipo 4° ben deciso, . . . oculare cilindrico. Coll' oculare piccolo sferico tutto questo era sparito e si credette tipo 3°." When instruments are used which give so vague an appearance to an exceptionally well-defined spectrum, presenting essential characteristics, it is easy to commit serious errors in judging of the spectra examined. Therefore I cannot see that the discrepancies which exist between Secchi's observations and mine are a sign of variations in the stellar spectra, although no doubt it is prudent to occasionally examine the stars concerning which these discrepancies have arisen.

It is quite a different case with the discrepancies that I have found between my observations and those of D'Arrest, who was supplied with excellent instruments, and was a most careful and skilful observer. It is necessary therefore to examine more closely into the cases in which differences exist. There are three, two of which concern the stars 24034 Ll. = Weisse XII.ʰ 793 and DM. + 60° 1461 = A. Oe. 13681. D'Arrest says that the latter has "a bright well-marked spectrum of type III." (*A.N.* 2044) and the former "a clear, fluted spectrum, the flutings being very distinct although pretty fine, III." (*A.N.* 2009). I found both nearly white, and their spectra II.*a*, or continuous. It is true that I examined the positions of these stars by the help of the two catalogues in which they are, and obtained the same positions, nevertheless one is tempted to believe that D'Arrest made some error in the identification of these stars, especially as he did not observe them several times. This supposition is quite inadmissible, however, for the star DM. + 36° 2772 = Ll. 30500, for here D'Arrest expressly says (*A.N.* 2009), "8·3 mag. with beautiful column-like spectrum. It is one of the stars accompanying the great Hercules nebula." I have calculated the position of this star with the help of the catalogue and of Ll., and besides that I examined all the stars in the neighbourhood of the great cluster in Hercules without finding one of Class III.*a*. The star DM. + 36° 2772 is of orange colour, but its spectrum is continuous, or at most II.*a* very poorly developed. As regards this spectrum I shall not even attempt to explain the difference between what I have seen, and the description given by D'Arrest. A variability of the spectrum seems really probable, and the star is certainly deserving of much attention. Besides this star there are others whose spectra I found very feebly developed, whilst D'Arrest says that they are beautiful or even superb. This also may be regarded as a sign of variability, and a fact which also supports this supposition is that D'Arrest has made his observations under atmospheric conditions generally regarded as similar to those found at Lund with a spectroscope of similar construction to one of ours, and a telescope very little larger than the one which I used. But it is remarkable that whereas I have often found expressions used by D'Arrest to describe spectra stronger than I should have used, the contrary is of very rare occurrence. It is possible, therefore, that the differences are only apparent, and that either D'Arrest's observations were made under exceptionally favourable circumstances, or mine under very unfavourable ones. The latter supposition is scarcely probable however, for when such differences have occurred I repeated the observation several times; and besides, Vogel's observations on the stars between − 2° and + 20° agree almost without exception with mine. It appears, moreover, that very often D'Arrest only made one observation of the same star; and, without depreciating his researches, it seems to me more probable that there are small and rare inexactitudes in the observations, than that such great changes have taken place in the stars themselves in the short period of ten years.

My researches already contradict the hypothesis that important changes in the stellar spectra take place so rapidly. My observations embrace a period of six years, and a much larger number of objects than either D'Arrest or Secchi examined. But there is no spectrum in which my latest observations have differed sensibly from my first ones. It is true that my first observations on the spectrum of R Crateris are in direct opposition to the last, the former making its spectrum III.*b* and the latter III.*a*. But that is in no way a sign of variation in the spectrum. When the bands in the spectrum III.*a* of a faint star are exceedingly broad in the green-blue, it is easy to fall into the error of regarding it as III.*b*. At the time of M. Vogel's first observation he also believed that the spectra of stars DM. + 14° 2525 and DM. + 17° 3940 were III.*b*, and it is true that these two spectra, especially the last, are strikingly like the spectra III.*b* in spectroscopes of small dispersion.

It was in order to escape such errors that I determined in the spectra of most of the faint stars of Class III.*b* the approximate wave-lengths of their principal flutings; the wave-length of band 6, and also that of band 10, being a sure mark that the spectra belonged to this section of Class III. This deceptive appearance generally disappears when the star is examined with spectroscopes of considerable dispersion. I am therefore at present of opinion that, excluding the new stars and perhaps η Argûs, which seems to belong to the same category, we have no reason to believe that great and rapid changes take place in the stellar spectra, although it must be confessed that the observations of certain stars, especially DM. + 36° 2772, are such as to render such changes very probable.

(To be continued.)

[1] M. Vogel has as a matter of fact seen feeble bands in the spectrum of one of them − 60 Schj., while in the case of ten stars his observations confirm my own; in the spectrum of another star M. Pechülé has not seen any bands.

[2] "Trovata cercando 124 Schj." The position of this star differs by 27m. and 40′ from that of the star in question.

[3] In his observations at Vienna with the great refractor, M. Vogel was able to see the lines of hydrogen either C or F in the spectra of all three stars; they were, however, feeble in comparison with the other bright lines.

THE ART OF COMPUTATION FOR THE PURPOSES OF SCIENCE.

I.

THE art of computation as distinguished from the science of arithmetic it so generally neglected in our ordinary courses of education, that most men and almost all women feel the greatest difficulty and repugnance in dealing with figures. The causes of and remedies for this deficiency are discussed at some length in a paper "On teaching Arithmetic" (*Journal of Education*, May 1885), and the following remarks refer specially to the requirements of students of science.

I must apologize for the use in proving my case of some names of high and well-deserved repute. Instances are given, as far as possible, which have been publicly acknowledged or corrected, with the full admission that this paper is itself a house of glass, and that any stone may impinge even upon Newton, since, as Lord Lytton tells us, "that great master of calculations the most abstruse could not accurately cast up a sum in addition. Nothing brought him to an end of his majestic tether like dot and carry one."

In 1867 Mr. Stone pointed out two numerical errors in Leverrier's determination of the solar parallax.

Prof. J. D. Van der Plats writes (*Chemical News*, July 30, 1886):—"The verification to which I have submitted the calculations of M. Stas seems superfluous seeing that it deals with the experiments of a *savant* who has never had an equal in exactitude. It may perhaps astonish some as much as it did me to find that the original memoirs contain numerous arithmetical mistakes, as well as typographical errors, of which some are considerable."

Mr. J. Y. Buchanan writes (NATURE, vol. xxxv. p. 76):—"There is a statement in NATURE for November 11 that the weight of the column of water between 20 fathoms and 70 fathoms from the surface under the westerly equatorial current is only 88 per cent. of the weight of the same column under the easterly counter equatorial current. I regret that a serious arithmetical error occurs in the calculation on which this statement was founded. There is no such considerable difference of weight in the two columns of water." Suppose at the equator the Guinea current flows from west to east at the rate of 40 knots in twenty-four hours, and that the equatorial current flows at 30 knots in twenty-four hours in the opposite direction. The opposite directions of the two currents cause an additive and subtractive difference in the tangential velocity of the particles of water due to the rotation of the earth, and therefore an apparent difference in the acceleration due to gravity of about 1/46000, or a pressure equal to that of an additional 1/13 of an inch of water on the column of 50 fathoms.

On page 84 of the *first* edition (the second has been corrected) of Prof. Huxley's admirable "Physiography," we read:—"The weight of air on a square mile is about 590,129,971,200 lbs., and the carbonic acid which it contains weighs not less than 3,081,870,106 lbs., or about 1,375,834 tons. The weight of the carbon in this carbonic acid is 371,475 tons."

This short statement contains excellent examples of many of the common arithmetical slips and errors.

The first number is ten times too great, and not quite accurately calculated from the data $(5280)^2 \times 144 \times 14{\cdot}73 = 59,133,431,808$. Multiplying this by $\frac{{\cdot}05321}{100}$, the proportion by mass of carbonic acid in the air, we obtain 31,464,899; here, besides a slip, the number is again multiplied by ten. The pounds are reduced to tons correctly, but there is a slip in the reduction to carbon, since

$$\frac{1,375,834 \times 3}{11} = 375,227.$$

Many more instances might easily be brought forward, but the above will suffice to prove that even the highest attainments in science are too often accompanied by inaccuracy in arithmetic. The causes of this defect have been frequently discussed, but, with the exception of De Morgan and his pupils, little advance in the methods of teaching arithmetic seems to have been made since the days of Recorde and Cocker.

The teachers of arithmetic in our public and higher-grade schools are usually good mathematicians who, in their own school-days, have been hurried through the hated subject to higher work, and have had no subsequent experience in the practical computation required in the laboratory, workshop, or counting-house. When compelled to work out a sum for themselves, the theory is supplied by their knowledge of algebra, and the practical work by a table of logarithms. When brought face to face with the fact that their pupils dislike and are very weak in arithmetic, they fall back upon the stock argument that they teach arithmetic as a training for the mind, and not as a useful art. In too many cases it is to be feared that they are not teaching arithmetic at all.

The great majority of the text-books in common use seem to be defective from the point of view of a student of science in at least three points.

More than half the rules and examples are devoted to money, and arithmetic is treated as though it applied only to pounds, shillings, and pence.

Secondly, few give any suggestion as to the use of tables in lightening arithmetical work, and a boy leaves school disgusted with long rows of figures in which he sees no utility, and without any idea to how large an extent the work could be lightened.

Lastly, the various methods of dealing with approximate quantities are omitted, and a painstaking boy calculates vast collections of figures of which only two or three have any meaning.

Thus Prof. Huxley gives the tenth figure, 6, in the expression for the amount of carbonic acid on a square mile, ignoring the facts that while the percentage of carbonic acid varies in the first figure, its density is not known to the fourth, and the pressure of the air varies in the second.

It is convenient to bear in mind the following simple rules, due, I believe, to De Morgan. If two numbers, a and b, each true to the first decimal place, are multiplied together, the result is true to $\frac{a+b}{20}$ only; a second true decimal in *each* number makes the result ten times more correct, and so on. In dividing a/b where each is true to the first place, the result is true to $\frac{a+b}{b^2 \times 20}$ and so on. Any attempt at greater accuracy in calculation than is indicated by these results should be avoided, since it only precludes the use of cheap and handy tables, tires the calculator making him more liable to error in the important figures, and tends to give quite a false idea of the accuracy of the experiments on which the calculations are based; unless, indeed, we take seriously the answer of Dulong when asked why he always gave his results to eight figures, "I don't see why I should erase the last decimals, for, if the first figures are wrong, possibly the last are correct."

The natural tendency of the human mind, even if controlled by mathematical and scientific training, is to exalt the accuracy of one's own experiments. This is well shown by Prof. Ramsay and Dr. Young in discussing the vapour-tension of liquid benzene (Proc. Phys. Soc., January 1887):—

"A curve was drawn to represent these (experimental) relations, and from it three points were chosen, 0° C. 26·54 mm., 40° C. 180·2 mm., and 80° C. 755 mm. The constants for the formula $\log p = a + b\alpha^t$ are $a = 4{\cdot}72452$, $\log b(-) = 0{\cdot}5185950$, $\log \alpha = \bar{1}{\cdot}996847125$." *Nine* places of decimals are given with apparent confidence, when (1) only three of the whole number of experiments were made even in duplicate; (2) the last pressure, 755, was obtained not by experiment at all, but by extrapolation from a freehand curve, the highest experiment being 79°·6 and 743·1 mm.; (3) a difference of $\frac{1}{8}$° at low temperatures produced no change in pressure which was appreciable by the apparatus used. With the above-mentioned constants the author's calculate for 60° C. 388·51 mm. Using their data and a table of four-figure logarithms, I find $a = 4{\cdot}7239$, $b = -3{\cdot}3$, $\log \alpha = \bar{1}{\cdot}99684$, which gives for 60° C. 390 mm. Regnault gives 390·1 mm.

Under suitable conditions the observation of one quantity can be made with great exactness. It is possible that Sir George Airy estimated 1/100 of a second in a day, or 1/8,640,000; that a balance can be made to estimate 1/1,000,000 of the load, though those of Stas were only accurate to 1/825,000; and that Sir J. Whitworth measured the 1/1,000,000 of an inch. These cases, however, are exceptional, and give quite a wrong idea of the accuracy attainable in ordinary observations and experiments, when several operations, each liable to error, have to be performed, and various corrections introduced by calculation from extraneous data.

The more closely we examine work of the highest accuracy the more convinced we become of the truth of the statement of Thomson and Tait (p. 333): "Few measurements of any kind are correct to more than *six* significant figures." Thus the number of inches in a metre was found by Capt. Kater in 1821 to be 39·37079, and by General Clarke in 1866 to be 39·37043; this fundamental datum therefore is affected by a doubt of nearly 1/100,000, which of course affects all results dependent on it. In 1856 Miller found that a cubic foot of water at 62° F. weighs 62·321 lbs. From Kater's result a cubic foot contains 28·3153 cubic decimetres, and the mean of a large number of experiments, especially those of Lefevre Gineau, and Kupffer, make the cubic decimetre of water at 4° C. to weigh a kilo = 2·20462125 lbs. according to Miller. Hence a cubic foot of water at 4° C. weighs 62·4255 lbs.; and taking the expansion from Förster (1870), which is nearly identical with that used by Miller, the weight at 16°·67 C. becomes 62·355 lbs.; or about 1/2000 heavier than Miller's determination. But these are the results obtained by picked men under all conditions to insure the greatest accuracy. Results which agree to two or three in the fourth figure show an exceptionally good chemist, while a physicist must be careful indeed to obtain numbers concordant to the fifth figure.

For practical purposes, then, calculations in science may be divided into two classes. The great majority of experiments in physics, chemistry, biology, geodesy, mensuration, navigation, and crystallography are not to be trusted beyond the fourth or fifth figure. Hence a similar accuracy in calculation is all which is required. Some few experiments in each branch—such as the work of Kater, Regnault, Stas, some observations in astronomy, and a few reductions in sociology—may require six or eight figures to be accurately dealt with.

In pure mathematics, of course, numerical results may be pushed to any extent compatible with even the partial sanity of the calculator.

The following suggestions are intended to assist such of my readers as are not mathematicians in working sums of each class by the aid of tables.

Mechanical aids, such as slide rules, arithmometers, and the like, are purposely omitted, since they would require a paper to themselves. The objection to the larger and more powerful is that they are expensive and complicated; that they require a good deal of practice on the part of the operator to give accurate results; and that they are not readily adapted to work shorter sums than they are intended for. On the other hand, a slide rule is an almost indispensable servant when once one has learnt the use of it for dealing rapidly with comparatively small numbers; for large numbers it becomes very cumbrous.

The two cardinal points in approximate working are the short methods of multiplying and dividing decimals suggested by Oughtred in 1631, and strengthening the last figure retained when the first omitted is above 4. For greater accuracy it is well to mark all strengthened figures, and to allow for an excess or defect of them; as a further security one figure beyond what is required may be calculated.

Tables of the multiples from 1 to 9 of numbers which frequently occur are of great assistance especially when the calculator is tired. They are easily made by repeated additions or by the use of the convenient "automatic multiplier" of Mr. Sawyer, which is merely a modern adaptation of Napier's bones.

The due use of complements and reciprocals saves a good deal of time in subtraction and division.

Tables for general use and special purposes are very numerous. For our present purpose they fall naturally into three classes. Five kinds of tables should be in the hands of all calculators:—

1. Multiplication tables such as those of Crelle, by the aid of which three figures may be dealt with at once with greater certainty than is usually the case with one. Tables of primes and factors are not much required for scientific purposes.

2. Reciprocals, which reduce division to the short multiplication of decimals, render the addition of fractions easy, and assist chemists in percentage compositions.

3. Squares, cubes, square roots, cube roots. For most purposes in chemistry and physics a small table up to 100 is sufficient, especially when aided by the following convenient method of approximating to a cube root. If a^3 be the nearest exact cube to the given number N, $N = (a \pm b)^3 = a^3 \pm 3a^2b + 3ab^2 \pm b^3$, or if b be small, $\pm b = \frac{N - a^3}{3a^2}$. Thus to find $\sqrt[3]{28}$, $a = 3$, $b = \frac{28 - 27}{27}$ $= \cdot 037$, $\therefore \sqrt[3]{28} = 3\cdot 037$ instead of 3·0366.

De Morgan's edition of Barlow is very convenient, and suffices for all ordinary purposes.

4. Common logarithms to four and five figures. Four-figure tables are perhaps most convenient on one face of a card. Hoüel's reprint of Lalande, with some changes and many valuable additions, is cheap and most convenient in form; it quite suffices for all common work.

For the reasons already mentioned seven-figure tables are unnecessarily cumbrous and expensive for ordinary work. They should never be put into the hands of beginners, as is now the usual practice. Experience shows that boys learn the method of using and appreciate the value of logarithms far more readily than is generally supposed.

5. Gauss's sum and difference logarithms are valuable in dealing with certain trigonometrical formulæ and with questions of expansion.

In the second class may be placed those general tables which are less commonly required, such as:—

1. Powers of 2 and other numbers. Cohn tells us that some varieties of Bacterium multiply by fission every hour, hence by the end of a day one individual would increase to 2^{24} = 16,777,216. We may therefore cease to wonder at the rapid spread of some forms of infection.

2. Factorials are required in solving permutations and combinations, and therefore in all questions relating to probabilities. Hatchett recommended that a systematic examination of all possible alloys of all the metals should be undertaken. He forgot to remind anyone who attempted to follow his advice that if only one proportion of each of thirty common metals were considered, the number of binary alloys would be 435, of ternary 4060, and of quaternary 27,405. If four multiples of the atomic weight of each of the thirty metals be taken, the binary compounds are 5655, ternary 247,660, quaternary 1,013,985.

3. The sums of arithmetical series are so readily obtained that they are rarely tabulated.

4. Geometrical series are required in certain social questions, such as the increase of population and the output of coal. Tables of the sums of these series when the ratio is nearly one are common, and of considerable use in some scientific problems.

5. For some purposes it is convenient to express numbers in a scale different from the common decimal one.

Thus (Clerk Maxwell, "Elementary Electricity," p. 180) a series of resistance coils are best arranged according to the powers of 2, since the smallest number of separate coils is required, and they are most readily tested. The same is true for a set of weights. Thus to express from 1 to 100 gm. 9 weights are ordinarily provided; 9 weights in the scale of 2 will express up to 511 gm., while 7 weights suffice for 100 gm., since 100 in the scale of 2 is expressed by 1100100.

6. The curious theory of trees due to Profs. Cayley and Sylvester (B. A. Report, 1875) seems to promise the possibility of computing the number of possible compounds formed by elements of given valency. Thus x atoms of tetravalent carbon will combine with monad hydrogen to form N compounds.

x	N	x	N	x	N.
1	1	6	18	11	1346
2	1	7	42	12	3326
3	2	8	96	13	8329
4	4	9	229		
5	9	10	549		

If of the first thirteen paraffin hydrocarbons alone there are 13,952 possible forms each with its own series of derivatives, there seems little chance of chemists having nothing to do for some time to come.

7. Natural logarithms are required by some formulæ, and are at times more convenient than common logarithms.

According to Haughton ("Animal Mechanics," p. 282), the study of the action of certain muscles requires the use of natural logarithms.

The ratio of the mean absolute pressure P to the initial absolute pressure p in a steam-cylinder at the given rate of expansion r is expressed by $\frac{P}{p} = \frac{1 + \text{nat. log } r}{r}$.

Weldon supposed (B. A. Report, 1881), that some power of the atomic weight (X) of each of the first fourteen elements in Mendelejeff's classification is a simple multiple of the same power of the atomic weight of lithium. Or $X^x = m7^x$

$$l. X = 1·9459 + \frac{l. m.}{x}$$

It is easy to see that since x may be any whole number, and m any small whole number, X may have any value whatever within the limits of errors of experiment; or the relation is fanciful rather than real.

8. Values of the definite integral $\frac{2}{\sqrt{\pi}}\int_0^x e^{-x^2} dx$ representing the probability curve, upon which the whole science of the adjustment and comparison of quantitative experiments is based.

Tables of the third class, which offer special facilities to those engaged in any one kind of work, are very numerous.

The physicist has Rankine's "Rules and Tables," Everett's "Units and Physical Constants," Hospitallier's "Formulaire de l'Electricien," and many others.

The chemist has Biedermann's "Kemiker Kalendar," the "Agenda du Chemiste," and various tables for analysis, such as those at the end of Fresenius.

The needs of both physicists and chemists are more or less supplied by Landolt and Bornstein's "Tabellen," the "Annuaire du Bureau des Longitudes," and my own more portable "Numerical Tables and Constants in Elementary Science."

I know of no such numerical compendium dealing with biology, but have often felt the want of one.

To sum up briefly the points which have been so far touched upon. The great majority of numerical problems which really occur in scientific work only require four figures to be accurately dealt with; hence a little ingenuity will generally bring them within the range of small tables. They should be worked out neatly, and as briefly as is consistent with the requisite accuracy; all useless figures should be rigorously excluded as misleading. Some few problems require the use of more powerful tables. Six-figure tables, such as those in Weale's series, and Collins's Logarithms for practical men, are little used, and inconvenient in practice. Seven-figure tables, such as Callet, Hutton, Babbage, Chambers, Schrön, Bremiker, Bruhns, Sang, so far as numbers go are nearly equally good; they differ chiefly in the trigonometrical ratios, which lie outside our present subject, and also considerably in price. SYDNEY LUPTON.

(*To be continued.*)

SOCIETIES AND ACADEMIES.

LONDON.

Zoological Society, December 20, 1887.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of November 1887.—Mr. Sclater read a letter from Dr. H. Burmeister containing a description of a supposed new Humming-bird from Tucuman. Mr. Sclater proposed to call this species, of which the type was in the National Museum of Buenos Ayres, *Chætocercus burmeisteri*.—The Secretary exhibited, on behalf of Major Yerbury, a pair of horns of the Oorial (*Ovis cycloceros*), which formerly belonged to the Royal Artillery Mess at Fort Attock, and were stated to have been originally obtained in the Chitta Pahar Range, a few miles south of Attock. These horns were apparently of the form lately described by Mr. A. O. Hume as *Ovis blanfordi*.—An extract was read from a letter received from Mr. H. M. Phipson, of the Bombay Natural History Society, offering some living Snakes for the Society's collection.—Mr. F. E. Beddard read a paper on Hooker's Sea-lion, *Otaria* (*Arctocephalus*) *hookeri*, based upon the specimens of this species recently received by the Society, one of which had lately died. The author called attention to the external features, visceral anatomy, and osteology of this Sea-lion, in comparison with the corresponding characters of other species of the group.—Mr. G. A. Boulenger read the description of a new genus of Lizards of the family Teiidæ, founded on a specimen presented to the British Museum by Mr. H. N. Ridley, who had obtained it in the forest of Iguarasse, Pernambuco. The author proposed to name this Lizard *Stenolepis ridleyi*.—A communication from the Rev. H. S. Gorham, entitled a "Revision of the Japanese species of *Endomychidæ*," was read. In this paper three new genera and thirteen new species were characterized and described. Additional observations were made upon the species previously known to inhabit Japan. The new species were based on specimens obtained by Mr. George Lewis during his last journey to the islands in 1880–81.—Mr. G. A. Boulenger gave an account of the fishes obtained by Surgeon-Major A. S. G. Jayakar at Muscat, east coast of Arabia, which had been presented by him to the British Museum. The collection contained specimens of 172 species, many of which were unrepresented in the national collection, and fifteen of which were apparently new to science.—Mr. H. Druce read a paper containing descriptions of some new species of Lepidoptera Heterocera, from Tropical Africa.

EDINBURGH.

Royal Society, December 19, 1887.—Sir Douglas Maclagan, Vice-President, in the chair.—Mr. John Murray communicated a paper on the height and volume of the dry land, and the depth and volume of the ocean. The mean height of the land above sea-level is 2250 feet. Only 2 per cent. of the ocean is included inside a depth of 500 fathoms. Seventy-seven per cent. lies between depths of 500 and 3000 fathoms. The mean depth of the ocean is 12,480 feet. If all the land were utilized to fill up hollows on the earth's surface, the sea would cover it to a uniform depth of 2 miles.—Sir W. Turner read a paper on the pineal gland in the walrus. The gland is excessively developed backwards, being visible from above without any dissection of the brain. The author contrasted it with the same gland in the lizard which is prolonged forwards and ends in the pineal eye. The cerebral lobes in the lizard are small, while those of all mammals are large. He suggested that the development of the lobes may have carried the gland backwards, and caused atrophy of the prolongation ending in the pineal eye. The atrophy, on the other hand, might have been caused by ossification extending over the aperture where the eye is situated.—Dr. Byron Bramwell described a method which he and Dr. Milne Murray had used successfully to record the exact time-relations of cardiac sounds and murmurs.—Prof. Crum Brown submitted a paper by Prof. Letts on the benzyl phosphines.—Dr. H. R. Mill read a criticism by Dr. Guppy on the theory of subsidence as explaining the origin of coral reefs.—Prof. Tait discussed the compressibility of water and of different solutions of common salt. Perkins proved sixty years ago that water becomes less compressible as the pressure is raised. At high pressures then it may be roughly assimilated to an extremely compressed gas. If the gas be regarded as consisting of hard spheres, the curve representing the relation between pressure and volume is approximately hyperbolic. The first asymptote of the hyperbola indicates what must be added to the external pressure to give the whole pressure to which the liquid is subject. The second indicates the ultimate volume to which it could be reduced by an infinite pressure. Applying this to the experimental results given to the Society in July last, the author showed that the pressure in water under ordinary circumstances is somewhere about thirty-two tons' weight per square inch; and the ultimate loss of volume under infinite pressure is about 25 per cent.

PARIS.

Academy of Sciences, December 26, 1887.—M. Janssen, President, in the chair.—Annual address, by M. Janssen. After brief reference to the losses sustained by the Academy during the year by the deaths of the illustrious *savants* MM. Paul Bert, Gosselin, Boussingault, and Vulpian, the President passed on to speak of recent scientific progress in France. Special mention was made of the magnificent Observatory just completed at Nice, for which the munificent founder, M. Bischoffsheim, receives the Arago Medal, now for the first time awarded. Allusion was also made to the isolation of fluorine effected by M. Moissan, and to the development of stellar photography, declared to be an "invention d'origine toute française." Nevertheless reference is made to the preliminary work of the English and American labourers in this field, Rutherfurd, Warren de la Rue, Bond, and Gould.—The Presidential allocution was followed by the announcement of the prizes for the year 1887, by the Secretary, M. J. Bertrand, who also read a paper on the life and work of the distinguished engineer, Stanislas Charles H. Laurent Dupuy de Lome.

Subjoined are the names of the successful competitors for the annual prizes. *Geometry:* Prix Francœur, M. Émile Barbier; Prix Poncelet, M. Appell. *Mechanics:* Extraordinary Prize of 6000 francs, divided between MM. Héraud, Dubois, Rouvier, and Moisson; Prix Montyon, M. Paul Vieille; Prix Plumey, M. Guyou. *Astronomy:* Prix Lalande, M. Dunér; Prix Valz, M. Perigaud; Prix Janssen, the late M. Kirchhoff. *Physics:* Grand Prize for the Mathematical Sciences, M. Willotte; Prix La Caze, MM. Paul and Prosper Henry. *Statistics:* Prix Montyon, MM. Victor Turquan, de Saint-Julien, and G. Bienaymé. *Chemistry:* Prix Jecker, MM. Arnaud and A. Haller; Prix La Caze, M. Moissan. *Geology:* Prix Delesse, M. Gorceix. *Botany:* Prix Barbier, MM. Edouard Heckel and M. Schlagdenhauffen; Prix Desmazières, MM. Ardissone and Dangeard; Prix Montagne, M. Boudier. *Anatomy and Zoology:* Grand Prize for the Physical Sciences, M. Raphael Dubois. *Medicine and Surgery:* Prix Montyon, Drs. Henri Leloir and E. Motais, and MM. Nocard and Mollereau; Prix Bréant, MM. Galtier, Chantemesse, and Widal; Prix Godard, M. Azarie Brodeur; Prix Chaussier, Dr. Jaccoud; Prix Serres, M. Alexandre Kowalevsky; Prix Lallemand, MM. Pitres, Vaillard, and Van Lair. *Physiology:* Prix Montyon, M. Ch. E. Quinquaud; Prix L. La Caze, Dr. Ch. Rouget. *Physical Geography:* Prix Gay, MM. Alfred Angot and Wilhelm Zeuker. *General Prizes:* the Arago Medal, M. Raphael Louis Bischoffsheim; Prix Montyon (Unhealthy Industries), Dr. Edouard Heckel; Prix Trémont, M. Jules Morin; Prix Gegner, M. Valson; Prix Petit d'Ormoy (Mathematical Sciences), the late M. Laguerre; Prix Petit d'Ormoy (Natural Sciences), M. Balbiani; Prix Laplace, M. Jules E. R. de Billy.—Honourable mention was made of the two English physiologists, Drs. Augustus D. Waller and E. Waymouth-Reid, for their memoir on the excised heart of mammals, published in the *Comptes rendus* for May 31, 1887. This study contains a number of new and highly interesting facts regarding the electric phenomena of the heart, the duration of the regular action of its four parts after excision, and the slowness acquired under certain circumstances by the wave of cardiac contraction.—Amongst the more important prizes offered for competition under the usual conditions during the years 1888 and 1889 are the following:—*Geometry:* Grand Prize for the Mathematical Sciences, to complete the theory of algebraic functions of two independent variables; Prix Bordin, to complete in some important particular the theory of the movement of a solid body. *Mechanics:* Prix Fourneyron, theoretic and practical essay on the progress of aërial navigation since 1880. *Astronomy:* Prix Damoiseau, to complete the theory of the irregularities occurring at long intervals in the motion of the moon caused by the planets. *Physics:* Grand Prize for the Mathematical Sciences, to complete in some important particular the theory of the application of electricity to the transmission of labour. *Agriculture:* Prix Vaillant for the best work on the diseases of cereals. *Anatomy and Zoology:* Grand Prize for the Physical Sciences, a complete study of the embryology and evolution of any animal, at the option of the candidate; Prix Bordin, comparative study of the auditory apparatus in warm-blooded Vertebrates, mammals and birds. *Physical Geography:* Prix Gay, to prepare monthly charts of the surface currents in the Atlantic, with a survey of the movement of drift ice in the waters about the Arctic regions; Prix Gay, to determine by a comparative study of their respective faunas and floras the relations formerly existing between the Polynesian Islands and the neighbouring lands.

Astronomical Society, November 9, 1887.—M. Flammarion, President, in the chair.—The President read a paper on some observations on the relative colours of stars, which he had made in 1875 by means of a specially constructed sextant in which the images of two stars wide apart could be brought into the same field.—M. Detaille read a paper on the photography of the solar spectrum with a direct-vision spectroscope, and stated that this subject was quite within the reach of amateurs, on a small scale of course, and presented many interesting points. He showed some negatives and positives obtained with a small instrument.

December 14.—M. Flammarion, President, in the chair.—The meeting was opened by the distribution of the calendar reform prizes, amounting, in medals and money, to the value of 5000 francs (an anonymous gift):—1st prize, 1500 francs, M. Gaston Armelin, of Paris; 2nd prize, 1200 francs, M. Hanin, of Auxerre; 3rd prize, 1000 francs, M. Francis de Roucy, of Compiègne; 4th prize, 800 francs, M. Barnout, of Paris; 5th prize, 250 francs, M. Remy Thouvenin, of Nancy; 6th prize, 250 francs, M. Blot, of Clermont (Oise).—M. Flammarion read a paper on some probable common proper movements of certain stars. In looking over the catalogue of the Paris Observatory, he had observed that several stars in Taurus—namely, Lalande 8178, 8209, 8237, 8256, 8297, 8404—had no motion in declination, and had all about the same proper motion in R.A. The same remarks apply to θ^1 and θ^2 Tauri. The two stars γ Leporis and Lalande 10931 seem also to be connected.—Colonel Laussedat, Director of the Conservatoire des Arts et Métiers, exhibited a curious binocular glass, constructed for Louis XIV. by Father Seraphin in 1681. This huge instrument comprises three rectangular bows which slide into each other. The length of the whole affair is no less than 3 metres 10 centimetres.—M. Neuville, in a letter, notices that the minimum of Algol seems longer than 6 minutes as given by several authors. He adopts 18 minutes, and gives a probable size of Algol's dark companion.—MM. Paul Henry and Detaille remark that Webb gives 18 minutes as the duration of Algol's minimum.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Birds of Wiltshire: Rev. A. C. Smith (Porter).—Arithmetic Papers: S. J. D. Shaw (Deighton, Bell, and Co.).—Major Lawrence, F.L.S., 3 vols.: Hon. E. Lawless (Murray).—Catalogue of the Fossil Mammalia in the British Museum; Natural History, Part v.: R. Lydekker (London).—Prodromus of the Zoology of Victoria, Decades 1–14: F. McCoy (Melbourne).—The Theory and Use of a Physical Balance: J. Walker (Clarendon Press).—Journal of Anatomy and Physiology, January (Williams and Norgate).

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