THURSDAY, FEBRUARY 2, 1888.

THE COMPOSITION OF WATER.

DUMAS, in his well-known memoir on the gravimetric composition of water, which every student is taught, and rightly so, to regard as one of the classics of chemistry, states that of all analyses presented to a chemist that of water is the one which offers the greatest uncertainty. Critics of a certain type may possibly take exception to the literal accuracy of this remark. No one, however, will gainsay the statement that, in view of the momentous issues which depend upon our knowledge of the composition of water, this knowledge is not by any means so exact as the state of contemporary science demands. It is, of course, not merely the question of the quantitative composition of water, but the far more important matter of the relative values of the atomic weights of hydrogen and oxygen. Of all stoïchiometrical constants required by chemists nowadays, those of hydrogen and oxygen are infinitely the most important. Every chemist knows what is dependent on these ratios, and he knows too that the difficulties which their direct determination involves are well-nigh insuperable.

All the discussions within recent years on the validity of Prout's law have tended to show that so far as experimental work is concerned, the question may now be said to hang upon these particular values. It is not too much to say that, if any chemist could succeed in showing by irrefragable experimental evidence that the atomic weight of oxygen was exactly sixteen times that of hydrogen, he would in the present state of scientific opinion at once succeed in inducing his brethren to accept Prout's law and all its far-reaching consequences as articles of their chemical creed.

It may be worth while to examine very briefly the nature of the ground upon which the present accepted values for the relative atomic weights of hydrogen and oxygen are based. It will be generally conceded that the evidence upon which chemists have almost exclusively relied is that afforded by Dumas' gravimetric analysis of water, and by Regnault's determination of the specific gravities of oxygen and hydrogen.

Dumas' work was published in 1843. His method was identical in principle with that employed by Dulong and Berzelius for the same purpose, and consisted, as is well known, in heating copper oxide with an unknown weight of hydrogen, and determining (1) the loss of weight suffered by the oxide, and (2) the weight of the water formed. The decrease in weight of the copper oxide was assumed to represent the weight of the oxygen evolved, and the difference between this weight and that of the water formed was held to be the amount of hydrogen which had combined with that of the oxygen.

In all, nineteen experiments were completed, in which quantities of water varying from about fifteen to eighty-six grammes were formed. Treating the results in the manner adopted by Meyer and Seubert—that is, in accordance with the equation

$$x = \frac{b_1 + b_2 + b_3 + \ldots + b_n}{a_1 + a_2 + a_3 + \ldots + a_n} = \frac{[b]}{[a]},$$

in which $a =$ weight of oxygen used, and $b =$ weight of water formed—it follows that $[a] = 840{\cdot}16$ grammes, and $[b] = 945{\cdot}44$ grammes, whence the ratios H : O = 1 : 15·96.

When, however, we come to examine more nearly the details of the method of determination, we find that these ratios are certainly affected by errors of which the magnitude cannot be even approximately known. In the first place, the sulphuric acid solution employed to generate the hydrogen must have contained dissolved air, the effect of which would be to lower the ratio of the oxygen. This fact was not indeed unnoticed by Dumas, but its effect could not be estimated with any certainty. Moreover, it seems almost impossible to prepare hydrogen from zinc and sulphuric acid without the formation of more or less sulphur dioxide, the last traces of which can only be removed by prolonged exposure to potash solution. Copper is one of the few metals that have the power of forming a hydride, and although this hydride, like the palladium hydride, is more or less readily decomposed by heat, the affinity of hydrogen for copper may be still traceable even at moderately high temperatures. Melsens, working in Dumas' laboratory, found that the reduced copper did actually retain hydrogen, and in amount varying with the temperature to which it had been heated. The weight of the condensed water must have been increased, as Berzelius pointed out, by the air which it eventually dissolved. Now the effect of all these errors would be to lower the value for the atomic weight of oxygen. Of course there may have been errors working in the opposite direction of which we know nothing, but it is reasonably certain that the net result of the constant errors, so far as these can be ascertained, is to give too small a value for oxygen. Above all, there are the fortuitous errors, such as those caused by differences in the power of surface-condensation of the vessels employed; errors of weighing and of reduction to a standard atmosphere, &c.; which, although theoretically allowed for and eliminated by a sufficiently frequent repetition of the experiments, may, on the whole, tend to operate in a given direction. Lastly, there is a source of error of the same order in a circumstance which, as there is a certain touch of pathos in them, may be stated in Dumas' own words:—

"Il faut même ajouter que la durée nécessaire de ces opérations, en m'obligeant à prolonger le travail fort avant dans la nuit, en plaçant les pesées vers deux ou trois heures du matin dans la plupart des cas, constitue une cause d'erreur réelle. Je n'oserais pas assurer que de telles pesées méritent autant de confiance que si elles avaient été exécutées dans des circonstances plus favorables et par une observateur moins accablé de la fatigue inévitable après quinze ou vingt heures d'attention soutenue."

There is, above and beyond all, a fundamental flaw in the principle of the method, of which Dumas himself was fully conscious. After having declared that of all analyses presented to a chemist water is the one which offers the greatest uncertainty, he goes on to state to what this uncertainty is due:—

"En effet, 1 partie d'hydrogène se combine avec 8 parties d'oxygène pour former de l'eau, et rien ne serait plus exact que l'analyse de l'eau, si l'on pouvait peser l'hydrogène et peser l'eau qui proviendrait de sa combustion. Mais l'expérience n'est pas possible sous cette forme. Nous sommes obligés de peser l'eau formée, et l'oxygène qui a servi à la produire, pour en déduire, par différence, le poids de l'hydrogène qui en fait partie. Ainsi, une erreur de 1/900 sur le poids de l'eau, ou de 1/800 sur le poids de l'oxygène, affecte d'une quantité égale à 1/90 ou à 1/80 le poids de l'hydrogène. Que ces erreurs étant dans le même sens viennent à s'ajouter, et l'on aura des erreurs qui iront à 1/40."

Let us now turn our attention to the evidence afforded by Regnault's determinations of the densities of oxygen and hydrogen. Prof. Le Conte has detected some slight numerical errors in Regnault's reductions (*Phil. Mag.* [4] 27–29), and when the necessary corrections are made it follows that the density of oxygen is 1·105612, and that of hydrogen 0·069269; whence, on the assumption of Avogadro's law, we have the ratio O : H = 15·9611 : 1. This result is in such striking agreement with Dumas' value that it is generally held to afford the strongest corroboration of it. The number given for oxygen is probably among the most accurate of Regnault's determinations of gaseous densities; the subsequent results of Von Jolly, which are alone comparable in character with those of Regnault, when reduced to the geographical position of Regnault's laboratory have not materially altered the value. The number given for hydrogen is certainly not entitled to the same degree of confidence. Indeed, it has been stated that Regnault was himself of this opinion, on account of the great difficulty of procuring hydrogen free from air. It is hardly necessary to point out that even an extremely minute admixture of air would tend to lower the relative value of the atomic weight of oxygen. Moreover, the hydrogen in the course of its preparation must have been saturated with moisture; and although, of course, all precautions at that time known were taken to dry the gas, it is quite certain that it could not have been absolutely free from traces of water. The experiments of Dixon have shown how extremely difficult it is to dry a gas perfectly, and it is now recognized that the ordinary methods of desiccation still leave appreciable traces of moisture in it. The effect of this moisture in the case of hydrogen would be to increase its density, whereas in the case of the oxygen it would tend to decrease it. On the other hand, oxygen and hydrogen when measured under the standard conditions of temperature and pressure are not, strictly speaking, under exactly comparable conditions, and the assumption of the validity of Avogadro's law is not mathematically correct.

Within recent years the question of the composition of water has been again attacked, and with a fuller knowledge of the various sources of error which the progress of science has shown to be present in the older methods. Julius Thomsen found that 1 litre of dry hydrogen, measured under standard conditions of temperature and pressure, when burnt with oxygen gave, as the mean of eight concordant experiments, 0·8041 grammes of water. Accordingly, 2 litres of hydrogen, on combining with oxygen, would give 1·6082 grammes of water. Assuming the validity of Gay-Lussac's law, and using Regnault's values for the weights of the gases at standard temperature and pressure, the calculated weight becomes—

2 litres hydrogen	=	0·1791 grammes
1 litre oxygen	=	1·4298 ,,
water	=	1·6089 ,,

The difference is 0·7 milligramme. But the question may be immediately asked, "Is Gay-Lussac's law actually valid?" The work of Regnault and Amagat on the relation of volumes of gases to heat and pressure indicates that, as ordinarily stated, it cannot be absolutely valid. Dr. A. Scott has recently put the question to the test of experiment, and, from a long series of trials in which large volumes of gases were caused to combine, he finds that the most probable ratio is 1·994 : 1 (Proc. Roy. Soc., 1887, 398). Taking Regnault's data as before, we have—

1·994 litres hydrogen	=	0·1876 gramme
1 litre oxygen	=	1·4298 ,,
water	=	1·6084 ,,

which differs only by 0·2 milligramme from Thomsen's result.

Now, from Regnault's densities of oxygen and hydrogen, as recalculated by Prof. Le Conte, it follows that the weights of equal volumes of the gases are as 1 : 15·9611, which, on the basis of Dr. Scott's ratio for the combining volumes, gives—

$$O = 16{\cdot}009.$$

Prof. J. P. Cooke and Mr. T. W. Richards, of Harvard College, have recently presented us with a further contribution to the subject (Proc. Amer. Acad. of Arts and Sciences, xxiii. 149), which merits very special attention, not only on account of the intrinsic excellence of the experimental work of which it is an account, but also because it is here attempted to obviate certain of the sources of error which have already been pointed out as inherent in Dumas' method. The method adopted by the American chemists was to pass a known weight of hydrogen over heated copper oxide and to weigh the amount of water formed. It will be seen that the essential feature in this method is that the weight of the hydrogen is known whilst that of the oxygen is obtained by difference; in contradistinction to the method of Dumas, where the weight of the oxygen was known and that of the hydrogen found by difference. The preparation of this hydrogen and the determination of its weight were, however, problems which required the highest manipulative skill. Obviously, everything depends upon the purity of the hydrogen. A glass globe of about 5 litres capacity and weighing about 570 grammes was so provided with stopcocks that it could be evacutated by the air-pump. The vacuous globe was weighed against a similar globe, in the manner already adopted by Regnault, filled with hydrogen, and its weight again determined. The weight of hydrogen taken was about 0·42 gramme. The hydrogen was then driven over the heated copper oxide by a current of dry air, and the water formed collected partly in a weighed tube, and partly by means of sulphuric acid and phosphoric oxide. The hydrogen was obtained by three different methods: (1) by the action of sulphuric acid upon zinc ; (2) by means of caustic potash and aluminium ; and (3) by electrolysis. In all, sixteen experiments are given. The results are stated in the following table :—

Series.	No. of Experiments.	Sum of the weights of hydrogen. grammes.	Sum of the weights of water. grammes.	Atomic weight of oxygen. max.	min.	Calc. from sums.
I.	5	2·0876	18·7406	15·977	15·937	15·954
II.	5	2·0803	18·6740	15·962	15·942	15·953
III.	6	2·5350	22·7541	15·967	15·937	15·952

The final mean is O = 15·953 ± 0·0017.

This very bald account does but scanty justice to the beauty and simplicity of the methods adopted by Prof. Cooke and Mr. Richards, and to the manipulative skill and patience with which they carried them out. With respect to the bearing of their result on Prout's hypothesis, the question seems to them to narrow itself to this point : Is the hydrogen they have made use of the typical hydrogen element ? They are inclined to believe that the theoretical question in regard to Prout's law has been settled so far as analytical work can solve the problem. On this point, however, we are at issue with them. That statement implies a finality about our present quantitative methods which we have no right to assume. It implies, too, that the methods employed by the authors have yielded as close an approximation to the typical element as we are ever likely to obtain. Their method in principle no doubt removes one fundamental objection to Dumas' plan of work, considered as an experimental process, but it by no means removes all the sources of error, and anybody who will patiently sift out these sources and seek to appreciate their net effect must admit that the ultimate tendency is to apparently lower the relative value of the atomic weight of oxygen.

If we have regard to this fact, and if we consider too what we may call the volumetric evidence, as given above, it seems premature to assume that the ultimate question has actually been narrowed down to the point to which Prof. Cooke and Mr. Richards are disposed to conclude that they have brought it. T. E. THORPE.

PHYSICAL GEOGRAPHY OF THE SEA.

Handbuch der Ozeanographie. Von Dr. Georg von Boguslawski und Prof. Dr. Otto Krümmel. Two Vols. (Stuttgart : Engelhorn, 1884–87.)

THESE volumes belong to a series of geographical hand-books—each written by an acknowledged master of the subject—brought out on a uniform plan, under the editorship of Prof. Dr. F. Ratzel, by the well-known Stuttgart publishing firm of Engelhorn.

Dr. Ratzel's own volume on Anthropogeography, Dr. Hann's on Climatology, and Dr. Heim's on Glaciers have already appeared ; and now, under the somewhat novel name of "Oceanography," the physical geography of the sea has been exhaustively treated. The editor has divided the subject into two parts, and given each to a specialist. In Volume I., Prof. Dr. G. von Boguslawski, of the Hydrographical Department of the German Admiralty, treats of the distribution, physical condition, and chemical composition of sea-water ; while in Volume II., Dr. Otto Krümmel, Professor of Geography in the University of Kiel, discourses on the motions of the ocean. Each part is complete in itself, but the index to both is given only in the second volume.

It is somewhat difficult to give an idea of the multifarious contents of this exhaustive treatise. In Volume I., Dr. Boguslawski begins with an account of the shape and area of the different oceans, and then naturally describes successively the physical character of their various coast-lines, and the depths of the sea all over the world. But we must remark on the poverty of illustration and the absence of maps which characterize this and so many other first-rate German books. Here we have actually no map, however rough, to show graphically the depth of the oceans. The unfortunate reader who wants to get his information as easily and quickly as possible has first to read through about 100 pages of closely-printed type, and then to try and picture to himself the relief of the floor of the sea.

After discussing ocean depths, the author devotes a short chapter to the chemical composition of salt water ; and then a somewhat longer space to the density or specific gravity of the sea. Colour and transparency are next discussed, and the last 200 pages are occupied with what is called maritime meteorology.

Of this space only thirty-four pages are devoted to wind and storms, and very rightly, as these subjects would require a special volume for their proper treatment. The remainder is occupied with a minute account of the temperature of the ocean, both on the surface and at various depths ; and with a notice of the distribution of ice in high latitudes. Here too, the value of the admirable text is greatly diminished by the absence of maps.

So far no mathematics have been required, but it is impossible to treat of the motions of the sea without algebraical formulæ. Dr. Krümmel, however, uses great judgment in only giving the formulæ of motion, which only involve simple algebra, and not the investigation of the formulæ, that would require much higher analysis.

In the second volume he begins with the consideration of waves. The theory of wave-motion, both in deep and shallow water, he gives first, mostly following Airy ; while experimental illustrations, and observations on the actual length, height, and speed of waves follow next. Breakers and rollers are then discussed, together with their influence on the abrasion of coast-lines. Earthquake and volcanic waves are illustrated by a self-recorded tidal trace from South Georgia, which will be new to English readers ; and the section ends with a capital account of stationary waves, *sêches*, and of the curious tidal phenomena in the Straits of Euripus, which so puzzled the ancient Greeks.

The author then turns to tides, dealing mostly with the theories of Laplace, Whewell, Ferrel, Airy, Thomson, &c., but very wisely ending with a chapter on "unsolved problems." The difficult subject of the vertical circulation of the ocean is next discussed, and an admirable account given of the cold *aufreibwasser*—up-rubbed water—of tropical weather coasts. This uneuphonious term is applied to the cold water that is found close to many tropical shores off which the wind blows steadily. For instance, near Cape Guardafui, when the south-west monsoon blows off shore, cold water is found near the land, but when the north-east monsoon blows on shore nothing but warm water can be discovered. The theory is that an off-shore wind blows, or rubs, the sun-heated surface water to leeward, and that the proper level of the sea is maintained by cold water welling up from below. Mr. J. Murray, of the *Challenger*, has discovered a similar effect in the long, narrow, deep waters of Loch Ness. With a south-west wind the coldest water is at the south-west end of the lake, but when the wind changes to north-east the lowest temperature is found at the north-east extremity.

The remainder of the work is taken up by a description of the currents of the ocean. The theory, of course, is fully given, and we may note that the author uses Ferrel's formula for the deflection of a moving particle to the right, through the influence of the earth's rotation, which has been accepted in every country except England. The long detail of the currents in different oceans of course contains little novelty, but is illustrated by an excellent map in blue and red of the direction and velocity of these well-known cold and hot streams.

Both of these volumes are to a certain extent uncritical compilations, for the results of various experiments and observations are merely recorded, without any comment on the varying quality of the work. We have already commented on the absence of maps; and the instruments used in oceanic research might well have been much more copiously illustrated. Still this work is a most valuable addition to the literature of the subject, and we wish that it could be translated into English.

There is no text-book of the subject in England, beyond School-Board primers, except the work of Maury; and this, in spite of a fascinating style, is too fanciful, and too much out of date, to be of any use.

Though the volumes now under review can never be popular in the ordinary sense of the word, still they would be invaluable to scientific men and others, who though not specialists wish to study in a compact and available form the present state of knowledge of one of the most interesting branches of modern research.

RALPH ABERCROMBY.

BULLETIN OF THE UNITED STATES FISH COMMISSION.

Bulletin of the United States Fish Commission. Vol. VI., for 1886. (Washington: Government Printing Office, 1887.)

THE immense number of short articles in this volume are as usual classified in a topical synopsis of the contents. The largest class is that of articles concerning the fisheries, the next in size contains those concerning aquiculture, the next those concerning natural history, and there are two other classes headed U.S. Fish Commission -General, and Miscellaneous. Of the biological articles Mr. John A. Ryder contributes only three, and the reader regrets there are not more from his hand. One is on the early development of the toad-fish, *Batrachus tau*, whose eggs are described as adherent, being fixed to the under surface of submerged boulders. The young toad-fish have this unique peculiarity, that when the egg-membrane bursts they are not set free but the lower surface of the yolk-sac remains firmly fixed to the adherent portion of the membrane, and this adhesion continues until the yolk-sac has become almost entirely intra-abdominal. The second of Mr. Ryder's papers is on the cleavage of the blasto-disk in the ovum of *Raja erinacea*; and the third on the intra-ovarian gestation of the viviparous *Sebastes marinus*: this last is based upon the examination of a gravid specimen obtained by the *Albatross*.

The few articles on the reproduction and generative organs of eels are of little value, as the information contained in them is not up to date. One, for instance, is a translation of a paper by Prof. Pavesi, published in 1880, and therefore of course treating as probabilities propositions concerning the testes which were proved in 1881 by Otto Hermes.

Among the aquicultural articles there are a great many on the shad-hatching work of the Commission, most of them detailing statistics of the operations of 1886. In one of these Marshall McDonald announces that for the entire period of the Commission's work up to and including 1882, 200,000,000 of young shad were produced, while for 1886 alone the total was 90,000,000, and this last number was fifteen times as great as the number of adult shad captured for market in one season. In another report by the same writer we find that the cost of production of shad-fry was $127·66, or about £25, per million. The exact effect of the artificial production of shad-fry on the supply of the adult fish is not estimated, but in one place we find that the catch in the Potomac was much larger in 1886 than in 1885; and in another that in Connecticut pollutions and sewage are diminishing the number of shad in the rivers.

Evidence is given that shad are now fairly abundant on the whole coast of California, apparently from plantings in the River Sacramento, but no regular run of shad seems to have been produced in that river; and we find statistics of plantings in 1886, in the Columbia and Colorado, from which a better result is expected. But of course the Bulletin is not the place to look for a connected and logical discussion of the operations carried out and their results. The publication contains occasional notes and statistics which are interesting to those who are familiar with the matters to which they belong, and which place on record facts which form materials for a connected study.

Of the very large amount of information comprised under the heading Fisheries, we cannot say more here than that it includes details and statistics not only of American fisheries but of those of all parts of the world.

OUR BOOK SHELF.

Flour Manufacture: a Treatise on Milling Science and Practice. By Friedrich Kick; translated by H. H. P. Powles. (London: Crosby Lockwood and Son, 1888.)

THE art of flour-milling, which of late years has undergone changes in its method of the most marked character, has at no time been productive of anything like a copious technology; and, in the attempt to supply this deficiency, it was natural that Mr. Powles should turn his regard towards Austria, where the manufacture of flour had engaged the attention of scientific experts long before the necessity for systematic inquiry into its processes became obvious in this country. The publication of Dr. F Kick's supplement to his treatise "Die Mehlfabrication," which tabulated the improvements in machinery for preparing and grinding cereals introduced up to the year 1883, placed at the disposal of the translator a manual complete in its investigations into the nature of grain from the miller's technical standpoint, and into the best means of reducing it to flour. It is true that the book does not concern itself with the construction of the mill building nor with the motive power to be employed; but, from this point onward, the leading principles which should guide the milling engineer are carefully and accurately related, and their application justified when necessary by mathematical demonstration; the *rationale* at the same time being within the comprehension of the practical miller. Of this method the chapters on "balancing millstones" (p. 113), and on "disintegrators" (supplement, p. 25), afford admirable examples. The various operations of grain preparation, grinding, and of bolting, sifting, and dressing the meal, with descriptions and plates of the best known machines employed, are fully detailed, whilst the controversy between the advocates of "high" milling and "low" milling is discreetly adjusted by the author in the incidental remark that "which of these methods is to be used can only be settled by the local demand, if, as is generally the case, the mill works for the home market."

It is, however, to those portions of the work which relate to roller-mills that the reader at the present time will probably turn in the first instance. He will find here, not only information as to the various kinds in use and as to the manner in which they have been found to perform their work, but an intelligible account of the operations involved in the reduction of cereals by rollers, and good reason shown why the time honoured millstones have become almost entirely discarded in the manufacture of wheaten flour.

The book is very fully illustrated by woodcuts throughout the text, and by some thirty supplementary sheets of diagrams; whilst a preliminary chapter contributed by Dr. August Vogel, of Vienna, on the histology of farinaceous grains, adds completeness to the work.

We congratulate the translator on his introducing to the English reader a volume of the utmost value to millers and engineers, and of great interest to many other persons more or less concerned with this important industry.

Elements of Chemistry: a Text-book for Beginners. By Ira Remsen. (London: Macmillan and Co., 1887.)

OPINIONS no doubt differ much as to what is simple enough for a beginner. A good deal depends on the age of the beginner. We hold, in opposition to the author in his preface, that the present production is not well adapted for very young pupils.

There is a good deal of promise in the book which might be better fulfilled, and there is an attempt to cover far too large a field, with the result—not intended by the author—that it reads more like a book on general chemical information than an elementary introduction to chemistry.

Metals and non-metals are dealt with under "family" groups, and most of their common, and many uncommon, compounds described, generally with formulæ, and this in cases and with equations which cannot be termed simple; for instance, technical processes like soda-making, or bleaching powder, or potassium chlorate, or nitro-benzene, &c. Otherwise the order and arrangement of matter and the questions attached to each section are most excellent, and the book would be most useful even for general reading, exercise, and information on the chemistry of common things to the great mass of partially informed, ordinarily well educated, people of any age. To the senior boys of public schools, who have already had a little instruction in science, this book would be really useful, as taking them in a different manner over ground already partially covered, widening their general knowledge, and cultivating the main thing, "*thinking.*"

A Primary Geometry, with Simple and Practical Examples in Plane and Projection Drawing, and suited to all Beginners. By S. E. Warren, C.E. (New York: Wiley and Sons; London: Trübner, 1887.)

THIS work bears as motto, "Geometry should be begun as early and as simply in behalf of industrial life as arithmetic is in behalf of business life"; and its object is, accordingly, to contribute to a general earlier beginning of the study of geometry. "The truths of *form*, as needed in *drawing*, have been made prominent, while not neglecting elementary ones of *measure*."

The text treats of straight lines, triangles, regular figures, areas, lines and planes in space, the elementary bodies, and projections of elementary solids, the subject being considered in a common-sense fashion without much use of purely geometrical proofs. Having perused a very large portion of his book without detecting any flaw, we consider the author competent for the task he has undertaken, but we do not take kindly to such presentments of geometry. We believe, however, the book to be well adapted to junior pupils as an introduction to the study, and also to artisans and others who are likely to be able to grasp the illustrations given better than they would purely geometrical proofs for which their antecedents have not prepared them.

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 Duke of Argyll's Charges against Men of Science.

I REGRET to find that the Duke of Argyll has once more evaded the point at issue. The question is one not of *formulas* but of *facts*. If the statements upon which his Grace bases the severe strictures of his "Great Lesson" were *true*, I for one should take no exception to any "metaphorical or rhetorical expression" by which he chose to enforce his lesson.

Three months have elapsed since the Duke's attention was directed to the discussions which during the last seven years have taken place upon the subject of Mr. Murray's theory of coral reefs—and especially to that one in which the Director-General of our Geological Survey, and the most eminent of American geologists, Prof. J. D. Dana, bore the leading parts; the Duke has been referred to the scientific journals in which this and the other discussions have been carried on; and the fact has been pointed out to him that all the principal text-books of geology, foreign as well as British, which have been published since the theory was announced, have given it a prominent position in their pages. In the face of these facts, is the Duke of Argyll still prepared to maintain that, with respect to the theory in question, there has been "a grudging silence as far as public discussion is concerned"; that there has been "a silence of any effective criticism"; and that "no serious reply has ever been attempted"? If his Grace admits that he was mistaken in making these assertions, is he prepared to withdraw them and also the comments which he has based upon them?

Instead of doing anything of the kind up to the present, the Duke of Argyll has fathered two stories about the wrong-doings of geologists—both of which stories have as little foundation in fact as his statements in "the Great Lesson."

The first of these stories is related in very circumstantial terms, but without any authority being given for it. It is said that a Fellow of the Geological Society offered a certain paper, which the authorities of the Society refused; and it is asserted that the reason of their refusal was that the paper "was not orthodox," and "they probably smelt heresy." Now the Duke of Argyll is well aware that every Fellow of the Geological Society has the right to present papers for reading, and that the responsibility for accepting or refusing papers rests in the first instance with the President; but he, in the case of exercising his veto, is bound to report the fact, and the reasons for his action, at the next meeting of the Council. The records of the Society show that no such paper was ever offered to it; that the President never exercised his right of veto; and that the Council never discussed the grounds of the supposed refusal. The Duke of Argyll has been informed of these facts, but he has not yet retracted the very serious charge which he has made affecting the honour and good faith of the President and the other twenty-two members of the Council of the Geological Society.

In the case of the second story circulated by the Duke of Argyll, the authority is given. The complaint is made that since 1862 "advanced geologists" have "ignored" views which "tend to dethrone" their own "pet theories." Anyone who chooses to refer to the *Philosophical Magazine* for 1862 will see that the "pet theories" in question are those relating to the antiquity of man; that the "advanced geologists" implicated in the charge must have been the late Sir Charles Lyell, Prof. Prestwich, and those who have followed up their researches and arguments; and that the "views" which they "ignored" were the suggestions which I described in my last letter!

JOHN W. JUDD.

The Total Eclipse of the Moon of 1888 January 28, as observed at Birr Castle Observatory, Parsonstown.

THE total eclipse of the moon on Saturday last was, like its predecessor in 1884 (see NATURE, vol. xxx. p. 589, and Trans. Royal Dublin Society for October 1885), favoured by a very clear sky during the whole time of its progress, so that very extensive observations of the changes of the moon's heat in consequence of the passing over of the earth shadow could be made. The apparatus used was essentially the same as that used before; yet the two old thermopiles had been replaced by two new ones especially made for this occasion by the Earl of Rosse.

The observations began at 7h. 19m. M.T. Greenwich, and were, as much as possible, uninterruptedly continued till 15h. 45m.

During this time 638 distinct readings of the galvanometer were obtained, which, when fully reduced, will enable a very satisfactory heat-curve to be drawn. A few preliminary results, reduced to zenith, I communicate at once.

Galvanometer.		
739·4	...	1h. 10m. before first contact with penumbra.
663·4	...	24m. ,, ,, ,,
624·1	...	First contact with penumbra.
252·1	...	,, shadow.
34·9	...	22m. before beginning of total phase.
30·2	...	22m. after ,, ,,
231·9	...	Last contact with shadow.
545·6	...	,, penumbra.
540·8	...	1h. 34m. after last contact with penumbra.

From these figures it will be seen—

(1) That the heat radiated by the moon begins to decrease a considerable time before the first contact with the penumbra.

(2) That 22m. before the beginning of totality the heat is only 4·7 per cent. of the value obtained 1h. 10m. before the first contact with the penumbra. Unfortunately an unforeseen stoppage of the driving-clock prevented the observations from being carried on closer up to and during the total phase.

(3) That in spite of the rapid fall on approach to totality, the heat, after the last contact with the penumbra, does not at once increase to anything like the value observed at corresponding times before the first contact.

It is worth remarking that points 2 and 3 are confirmatory of the results arrived at in 1884. OTTO BOEDICKER.

Birr Castle Observatory, Parsonstown, January 30.

"Elementary Chemistry," and "Practical Chemistry."

I CRAVE leave from the Editor for space in which to reply, on my own behalf and on that of my fellow-authors Messrs. Slater and Carnegie, to the charges brought by "H. E. A." in NATURE of January 19 (p. 265) against our method of teaching chemistry. At the outset I thank "H. E. A." for the patience which, as he publicly announces, he has shown in waiting for the publication of these books, and I condole with him in his disappointment. Like him, I too am waiting patiently; I trust my disappointment will be less bitter.

One of the important points in our plan of chemical teaching is the connection of the work in the laboratory with the student's reading and lecture-work. To emphasize this connection, and to make our course run fairly smoothly, we have published two books, one to be used in the laboratory, the other to be used in the lecture-room and in reading in connection with the whole work of the student. "H. E. A." acknowledges the advantages of this division, but throughout his review he ignores the statement distinctly made by us, that one book is complementary to the other and that both must be used together. He confines his remarks almost wholly to one of our books, viz. the "Practical Chemistry"; and yet he condemns our system of teaching. On this ground alone I claim that his review is misleading and unfair. I go further, and assert that "H. E. A." has condemned our system without acquainting himself with its essential features. He says that "in the earlier part of the 'Practical Chemistry' Messrs. Muir and Carnegie do not sufficiently bear in mind their own intention, and that much of the matter would find a more fitting place in the companion volume." No one reading this would suppose that almost every experiment used in Chaps. I. to VIII. of the "Practical Chemistry" is also used in Chaps. I. to IX. of the "Elementary Chemistry." Yet this is the case. In one book the experiments are described, along with others, in such terms as allow attention to be concentrated on their results and on the reasoning on these results; in the other book the experiments are described in detail in order that the student may repeat them in the laboratory. In another part of his review "H. E. A." says that most of the subjects dealt with in the third part of the "Elementary Chemistry" "ought never to have been introduced into an 'Elementary Chemistry.'" He has here made a slip: it is the third part of the "Practical Chemistry" which includes subjects not touched on in the other book. This correction involves a point of some importance. Although the preface to our "Practical Chemistry" states that the book forms part of a course of elementary chemistry, yet the student who uses both books will see that the course of work laid down in the practical book carries him much beyond the limits of treatment adopted in the other volume. There are numerous direct and indirect indications of this in the book itself, which those for whom the work is intended will not fail to notice. One cannot put the whole of one's book into the preface. I admit that it would have been better had we indicated in the preface to the "Practical Chemistry" that many experiments in Parts II. and III. are difficult to perform, and require skill and training; but I assert that the nature of the experiments themselves, the references to the original papers to be read before conducting these experiments, and the suggestions as to other work to be done preparatory to Parts II. and III. respectively, suffice to indicate to the student, although not necessarily to the reviewer, the character of the work described in the later chapters of the "Practical Chemistry."

Chapter I. of Part III. of the "Practical Chemistry" involves a repetition of some of Stas's determinations of the atomic weight of silver. "H. E. A." says that this chapter should have been included in Part I., and he adds, "the remaining chapters ought never to have been introduced into an 'Elementary Chemistry,'" kindly informing his readers that these chapters are included "because of the senior author's well-known tendency to worship physical constants." I venture to remind "H. E. A." that no election has taken place to the office of supreme pontiff of chemistry. Were that official in existence, I feel inclined to think he would admit that accurate determinations of atomic weights—and "H. E. A." allows these in the most elementary part of the course—are determinations of constants which have physical as well as chemical meanings.

"H. E. A." says that in the "Practical Chemistry" there is an "entire absence of anything approaching to a *systematic* arrangement." The boldness and baldness of the assertions made by the reviewer encourage me to meet this statement with a direct denial. There *is* a systematic arrangement in the whole book, or rather in the whole scheme embodied in both books. Because "H. E. A." fails to discover that plan which finds favour with him, it does not follow that systematic arrangement is absent. To say that the arrangement is not that which one would like to see adopted is fair criticism; but to imply that there is no alternative between one's own system and chaos is to expose one's own ignorance. And what is the feature of that system of practical chemistry in which alone the reviewer thinks the student can find salvation? He says, "in a properly chosen series of experiments everything should be proved; no assumption should be necessary." In another part of the review he tells us that "air and the phenomena of combustion should be first studied: the composition of air should be determined, and oxygen should be *discovered* by the student . . . The composition of water should next be qualitatively ascertained." I should be deeply indebted to "H. E. A." if he would kindly describe experiments on these subjects, suited to beginners in chemistry, in which no assumptions are made, and which convey sound teaching. He must not get over the difficulty by cleverly hiding the assumptions made, and so appearing to make none; everything must be proved, and proved by experiments which the beginner can satisfactorily conduct. I hold, and I am convinced that the history of science bears me out, that all scientific reasoning starts with certain assumptions, and that in every particular train of reasoning assumptions are made. If the beginner can be taught to recognize the assumptions which are involved in his reasoning on experimental data, he will do well. In the "Practical Chemistry" we have tried to emphasize the assumptions which the beginner must make. In our opinion the fatal thing is to cover over and hide away the assumptions; by doing this, the student acquires a habit of confounding hypotheses with facts, and so unconsciously he slides into loose methods of reasoning. I fancy I can detect the effects of such a method in the whole review: has not "H. E. A." tacitly, probably unconsciously, assumed that chemical truth abides with him and with him only?

We thank "H. E. A." for indicating some points in the descriptions of certain experiments which might be improved, and also for reminding us that the drawings of apparatus are not as good as they might be. These things can and will be improved. The mistake in the description of the diffusion-experiment, on p. 30 of the "Elementary Chemistry," to which "H. E. A." alludes, has been already pointed out to us, and a slip has been inserted in all copies except the first few hundred correcting this mistake. We cannot congratulate the reviewer, nor do we think he will be inclined on second thoughts to congratulate himself, on the trifling quibbles in which he has indulged regarding one of our experiments on the electrolysis of water.

Cambridge, January 23. M. M. PATTISON MUIR.

"Physical Science and the Woolwich Examinations."

I AM afraid that the moderation of your article on the regulations for admission to the military colleges may give some readers the impression that science is merely being discouraged more or less seriously in their examinations. The fact is, however, that it is being ousted with absolute certainty, for hardly anyone can afford to take up an optional subject which is at a disadvantage of 1000 marks. Severity of competition has within the last few years quite doubled the number of marks qualifying for admission to Sandhurst, and it will soon be impossible, even if it is not so at present, for a candidate to gain a place if he takes up any subject other than Latin, French, German, or mathematics.

This making all the men fit square holes whether they are round or not can hardly be for the advantage of the service, and one's curiosity is aroused as to the reason for such retrogressive changes—whether it is due, as has been asserted, to the action of head masters who do not desire to accumulate or encourage new-fashioned lore; or whether the military authorities really opine that to an officer who may have to deal with telegraphy, to choose a camping-ground, or perhaps direct a search for water, Latin is half as important again as electricity or physical geology.

Is it really too much to expect that they might insist first on a thorough knowledge of those parts of an ordinary education which are specially necessary or helpful to an officer, and then treat the unessential subjects on an equality as far as possible, and let a boy do in his preparation as he will when a man—adequately fulfil the duties of his position, and then follow his own bent? W. A.

January 30.

"The Art of Computation for the Purposes of Science."

HAVING read with much interest Mr. Sydney Lupton's second article on this subject, I think it right to draw his attention, and that of your readers, to Table III. of my book of five-figure and other logarithms published by Messrs. C. and E. Layton in 1870.

This table was framed by me for the purpose of enabling computers who occasionally require to use logarithms to ten places to get same with as little trouble as possible, and without shifting to any other book. In fact, I believe results can be got from my table almost as quickly as from the voluminous and beautiful volume of George Vega.

For instance, referring to Mr. Lupton's example, I find from my table and the instructions that log 1·0542482375 = log 1·05 + log 1·0040459405—this by simple division; then—

By part A log 1·05 = 0·0211892991
By part B log 1·0040459405 = 0·0017535845

log 1·0542482375 = 0·0229428836

correct by Mr. Lupton's solution from Vega.

My whole table is contained in eight octavo pages, and I believe is in as narrow a compass as is consistent with utility.

I may add that in the preliminary part of my book will be found a method of finding the logarithms of all numbers by nothing more than simple multiplication.

The late Prof. Augustus De Morgan, when I showed him this Table No. III., I well remember, replied: "It is very good indeed, but you will get no one to look at it," showing how rarely logarithms are ever required for any practical use beyond five, or at the most seven, figures. E. ERSKINE SCOTT.

6 Bond Court, Walbrook, London, E.C., January 18.

THE articles of Mr. Sydney Lupton on the above subject, which have appeared in recent numbers of your paper, do not profess to be complete; still, as their declared object is to assist those who are not mathematicians to work sums by the aid of tables, it seems to me that the best methods should not be passed over in silence, while others that are practically obsolete are discussed at length.

I beg of you therefore to allow me to call attention to the labours of the late Peter Gray, F.R.A.S., in the direction of supplying facilities for computing logarithms and antilogarithms. He contributed papers on the subject to various magazines; notably a series (with a table for formation of logarithms and antilogarithms to twelve places) to the Journal of the Institute of Actuaries in 1865. His most important work on this subject was, however, published as an independent volume in 1876. It is entitled "Tables for the Formation of Logarithms and Antilogarithms to Twenty-four or any less number of places"; and it contains, besides the tables, an explanatory introduction and an exhaustive historical preface. The published price is only 7*s*. 6*d*., and it is therefore not beyond the reach of those who require such tools.

Weddle's method, the last mentioned by Mr. Lupton, consists in multiplying the given number down to unity, by means of a series of factors of the form $1 - (\cdot 1)_n \times r$, where r may take any integral value from 1 to 9. The logarithms of the factors are then obtained from a previously prepared table, and the complement of the sum of these logarithms is the logarithm of the given number. Weddle also used his method conversely, to calculate antilogarithms.

Hearn, of the Royal Military College, Sandhurst, improved upon Weddle's method, by substituting factors of the form $1 + (\cdot 1)^n \times r$ for the computation of antilogarithms, r, as before, ranging in value from 1 to 9; but he retained the factors $1 - (\cdot 1)^n \times r$ for computing logarithms.

Gray's improvements on Hearn were twofold. In the first place, he gave r the range from 1 to 999, taking for factors $1 + (\cdot 001)^n \times r$, and he thereby brought within narrow compass the arithmetical work involved. In the second place, by a simple arrangement of the calculations, he showed how to use factors of the form $1 + (\cdot 001)^n \times r$, instead of $1 - (\cdot 001)^n \times r$, for computing logarithms as well as antilogarithms; and thus, not only made the operations more convenient, but also caused one set of preparatory tables to he sufficient.

The principal table in Gray's book above-named consists of the logarithms to twenty-four places of all the possible factors $1 + (\cdot 001)^n \times r$, up to that limit. An auxiliary table contains, also to twenty-four places, the logarithms and their complements of the natural numbers 1 to 9, these being frequently required to "prepare" the given number. A smaller table to twelve figures only appeared, as already mentioned, in the Journal of the Institute of Actuaries, and was subsequently published separately by Messrs. C. and E. Layton; but as the twenty-four-figure table can be worked quite easily to any extent up to that limit, there is no particular advantage in the smaller one.

By means of Gray's tables the work of forming logarithms and antilogarithms is reduced to a minimum, and the process is so simple that any arithmetician can perform it, the more especially as many numerical examples are given in the introduction.

London, January 23. GEORGE KING.

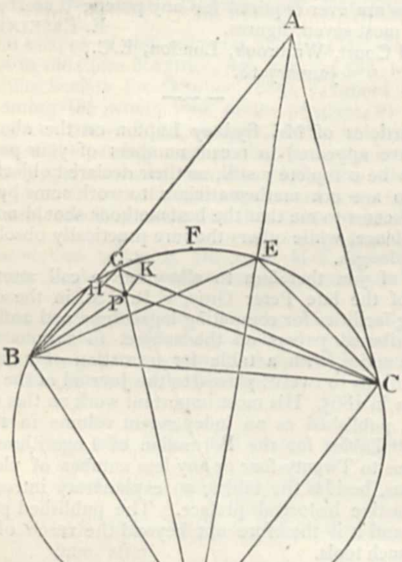
Note on a Problem in Maxima and Minima.

To find a point such that the sum of the straight lines joining it with the angular points of a given triangle shall be a minimum.

This problem was proposed by Fermat to Torricelli, who solved it, and sent it to Vincent Viviani, who also solved it, but called it a problem "quod, ut vera fateor, non nisi iteratis oppugnationibus tunc nobis vincere datum fuit."

The solution is given in Gregory's "Examples of the Differential and Integral Calculus," and in Todhunter's "Differential Calculus," pp. 240–42.

Yet it can be solved in the most elementary manner.



Let ABC be the triangle. Describe an equilateral triangle on BC on the side remote from A. Describe a circle round the triangle BCD. Join AD. Then E is the point required. Join BE, CE.

(1) It follows, from Euc. vi. D, that

$$BE + EC = ED,$$
$$\therefore BE + EC + AE = AD,$$

and evidently $\angle BEC = BEA = AEC = 120^\circ$.

(2) Let F be a point on the circumference BC.

$$BF + FC = FD \quad \text{(Euc. vi. D)},$$
$$\therefore BF + FC + FA = FD + FA > AD.$$

(3) Let P be a point not on the circumference. Join DP, and produce it to the circumference at G. Let fall the perpendiculars PH and PK, on GB and GC respectively.

$$\text{By Euc. i. 26, } GH = GK = \tfrac{1}{2}GP.$$
$$\text{Since } \angle GPH = 30^\circ = GPK,$$
$$\therefore BH + KC = PD,$$
$$\therefore BP + PC > PD,$$
$$\therefore BP + PC + PA > PD + PA > AD.$$

(4) It also follows from the above that if $\angle A = 120^\circ$, then the point required is $A \equiv E$.

If $\angle A > 120^\circ$, the point A will be within the circle, and A itself will be the point required. R. CHARTRES.

Note on the Dimensions and Meaning of J, usually called the Mechanical Equivalent of Heat.

THE title "mechanical equivalent of heat" tends to make one consider that J means the ratio of a quantity of mechanical energy to an equivalent quantity of heat; but since heat *is* mechanical energy (in a molecular form) it follows that J on this supposition is equal to *unity*, and therefore unnecessary.

Another way in which J is sometimes regarded is as the ratio between the ordinary *units* of heat and work; that is to say, in England, it is the ratio of the British thermal unit to a foot-pound, viz. the number 772. This definition makes it a simple number, the number of work units in a heat unit, a number which depends on the units of heat and work employed, and is different in France and England.

Now although J generally has one or other of these significations—that is, must be either unity or some pure number—yet people speak of the *dimensions* of J as being, not zero, but $\frac{\text{Work}}{\text{Mass} \times \text{Temperature}}$.

It is evident that there must be some confusion here, a confusion arising from the fact that most people when *talking* of quantities *mean* only so many times the units of those quantities, and so are not always sufficiently careful about the definitions of the various quantities which they introduce.

Now if we confine our attention to quantities themselves, independently of any systems of measurement, we shall be led to a perfectly consistent mode of regarding J, a way moreover in which it will have the required dimensions $\frac{\text{Work}}{\text{Mass} \times \text{Temperature}}$.

A British thermal unit is the heat required to raise a pound of water at freezing-point through 1° F., and Joule discovered that the mechanical equivalent of that amount of heat was about 772 foot-pounds.

Hence if we wish to consider the work necessary to raise any other mass of water at freezing-point through any small range of temperature, we have only to notice that the quantity $\frac{\text{Work}}{\text{Mass} \times \text{Range of Temperature}}$ is constant, and equal to $\frac{772 \text{ foot-pounds}}{1 \text{ pound} \times 1^\circ \text{ F.}}$.

This quantity is very fitly denoted by J, and might, if thought convenient, be called *a Joule*.

But this quantity is the *specific heat of water*, according to the definition that specific heat is the heat required to raise a mass through a small range of temperature *divided by* the mass and the range. So that we have arrived at these conclusions: a quantity of heat is the same thing, whether expressed in British thermal units, or in foot-pounds, or in terms of any other standard; and the specific heat of water at 0° C. is denoted by the letter J.

Indeed it may be said that the result of Joule's experiments is the determination of the specific heat of water in absolute measure. Again, if c is the ratio of the specific heat of any substance to that of water, the full expression of its specific heat is cJ; that is, its specific heat is some multiple or fraction of a *Joule*.

The first law of thermo-dynamics will then be expressed as—

$$dQ - p \cdot dV = cJm \cdot d\theta + m \cdot dI,$$

where $dQ - pdV$ is the total energy supplied, $cJmd\theta$ is the amount of new energy evidenced by increase of temperature, and mdI is the increment of the latent energy of the body.

Coopers Hill, Staines, January 19. ALFRED LODGE.

The Temporary Thermo-Current in Iron.

IN the *Philosophical Magazine* for January, Mr. Herbert Tomlinson has proposed an explanation of the remarkable fact that in an iron wire, heated red hot by a burner, an electric current is produced when the flame is shifted along the wire (see Wiedemann's "Galvanismus," ii. 453).

As his explanation is inadequate, perhaps I may be excused again drawing attention to this subject. Briefly his explanation is as follows:—That, as the portion of the wire in the flame rises in temperature, it, thermo-electrically speaking, becomes in fact like a different metal, and that then, on shifting the flame, the junction with the unaltered wire on the side moved towards becomes hotter than before, while the one on the other side falls in temperature, thus presenting the ordinary case of a thermo-couple with junctions at different temperatures. Now this explanation entirely overlooks the fact that, by the first assumption, just as fast as the temperature on one side rises, the wire there changes into the "second state," and correspondingly changes back on the other side as the temperature falls there; so that, as far as this explanation goes, there ought to be no current whatever, for thus both junctions must always be at the same temperature.

When I first noticed this current, which from considerations to follow I have ventured to call the "temporary thermo-current," it appeared to me to be due to the difference in the temperature-slope (or gradient) along the wire in front from that behind the flame, as it heats more rapidly in front than it cools behind, and to the electromotive force being a function of the slope, *i.e.* of $\frac{d\theta}{dx}$. But this hypothesis did not stand the test of experiment, as I have shown in a paper published in the Proceedings of the Royal Dublin Society, July 1886. So that as there *is* a current, we must suppose the "second state" to be not only a fuuction of the temperature, but also of the time, *i.e.* that the wire changes into (or from) the "second state" more slowly than it is possible for it to change in temperature. So that the electromotive force at any point depends on the rate of change of the temperature slope, or equals $\phi\left(\frac{d}{dt}\frac{d\theta}{dx}\right)$. In support of this it will be found that if the flame be steadily moved along very slowly no current is produced—at all events less than would be otherwise expected; and, secondly, that the maximum current is got by moving the flame the fastest consistent with the condition of keeping the wire red hot.

It is with the view of emphasizing this dependence on the time that the term "temporary thermo-current" seems appropriate. FRED. T. TROUTON.

Physical Laboratory, Trinity College, Dublin.

Causes influencing the Bathymetrical Range of Deep-Sea Fishes.

YOU refer (p. 219) to the fact that Dr. Günther has adopted the 100-fathom line as the boundary at which with the extinction of sunlight the bathybial fauna commences. This selection of 100 fathoms as the limiting horizon is of much interest in connection with the theory that the shallow-water marine fauna is greatly influenced by wave-currents. In a letter you published in 1885 (NATURE, vol. xxxii. p. 390) I indicated 100 fathoms as the depth to which wave-action of some sort must extend, as evidenced by the character of the deposits at the mouth of the English Channel. Dr. Günther now shows that the deep-sea fishes do not rise above that horizon. But, although the 100-fathom horizon agrees very well with the apparent limit of wave-action, it does not seem to agree with the most recent experiments on the penetration of sunlight in water.

So recently as November last you recorded the fact that during the past year Prof. Forel found that the greatest "depth-limit of absolute darkness" from March to July in the Lake of Geneva was 100 metres (NATURE, vol. xxxvii. p. 88). If experiments in a fresh-water lake may be taken as a guide to light-penetration in the ocean, 50 fathoms will be nearer the limit than 100. In this case the bathymetrical range of the bathybial fauna cannot be much influenced, if at all, by the presence or absence of sunlight. This view is moreover fortified by the fact that, though the deep-sea forms do not usually ascend above the 100-fathom line, the shallow-water forms go far below it; and there is no reason why they should not do so; for, although a form unfitted to withstand wave-currents cannot face them, there is nothing to prevent a flat-fish, fully equipped in this respect, from passing at will from the disturbed to the tranquil horizon, and *vice versâ*. A. R. HUNT.

Torquay, January 10.

Wind Force at Sea.

IN reference to a letter on the above subject in NATURE (p. 274), I beg to acquaint your readers that Capt. Barker's wish that anemometers should be used more on board ship has been endeavoured to be met by an instrument designed by myself on the sail principle. It has now been in use on some ships at sea for long voyages for five years, and daily observations have been obtained and sent home of the data observed, of pressure, direction, and velocity of the winds met with.

Regarding the further inquiry of ascertaining the rainfall at sea, this has now been carried on for about ten years by means of a rain-gauge designed by myself on the pivot principle, and it has been used by many vessels in all the great seas.

The daily observations have been sent home and are now on hand, and about five years of the returns have already been announced, and a further compilation of the data may be prepared when the materials become sufficient.

It may be added that the late Capt. Symington, of the s.s. *Hankow*, amongst his meteorological observations took the rainfall by rain-gauge on his ship for twenty years or more. The marine anemometer and rain-gauge above mentioned were exhibited at the Liverpool Exhibition in 1886, and at the Meteorological Exhibition of last year. W. G. BLACK.

Edinburgh, January 21.

Untimely Insect Development.

SOME of your readers may be interested in a case of untimely insect development, caused no doubt by the phenomenal mildness of the weather in this part of the country during the last few days. Last evening a perfect imago of the common tortoiseshell butterfly (*Vanessa urtica*) was found inside my house on the wall of my nursery. It is fully developed in every way, and the only thing in its appearance at all abnormal is that the antennæ are bent back and lie between the wings, which are in the erect position usual in repose. The insect has evidently only just emerged from the pupa, and is in a torpid condition, only just flapping its wings when touched. The nursery is a warm room looking to the south, and has a fire in it all day.

St. Albans, January 10. JOHN MORISON.

Weasels killing Frogs.

SEEING a note in NATURE (December 29, 1887, p. 208), about weasels killing frogs, I thought that the following fact would be a further confirmation.

I was walking near the village of Clifton Hampden in August last, when I saw a weasel, carrying a good-sized frog in its mouth, come cautiously out of the rank grass by the road-side; directly the weasel perceived me, it dropped its prey on the road and retreated to the cover of the grass. The frog was dead. I kept silence, and the weasel left its hiding-place, and advanced a few steps, but again retreated. Soon, after several advances and retreats, it rushed out, seized the frog with its teeth, and running across the road disappeared in the long grass on the other side.

January 20. M. S. PEMBREY.

"British and Irish Salmonidæ."

THE author of "British and Irish Salmonidæ" calls in question the justice of three criticisms in my review of that book. In reply to his first objection, I have to point out that my quotation of the sentence referred to was, as Mr. Day has himself noted, made to draw attention to its grammatical errors, and therefore the omission of a few words which affected the sense but not the construction was of no consequence at all. I omitted the words intentionally, to shorten the quotation, and gave no opinion on the statement contained in the sentence: the statement which is implied rather than expressed is perfectly correct.

With regard to the second point, the statement in the text of the book which I questioned is as follows:—"The main principle is to employ thin layers of well-packed and pressed moss in trays with perforated bottoms, *the eggs being separated from the moss by muslin, mosquito-netting, swans' down, calico, or butter-cloth*, and that each tray contains two or three layers." In all the methods of packing salmonoid eggs in which moss is employed, the descriptions I have read state that the eggs are placed in direct contact with the moss, and Mr. Day does not justify the statement above quoted by referring to another statement in his notes, that for shorter journeys eggs are thrown off the frames on to swans' down. I doubted, and still doubt, if there is any method practised in which layers of moss are used, and are separated from the eggs by muslin or similar material.

With regard to the third point, it is true that on p. 249 of his book, in the chapter on *S. fontinalis*, Mr. Day refers to Brown Goode's "Game Fishes of the United States," and to the statement in that work that *S. namaycush* has, as its nearest relative, *S. fontinalis*. But I think a more direct reference to a speciegraphical description of *S. namaycush* might have been expected in a footnote referring to errors in the descriptions of this fish by certain writers. My remark about the omission of such reference was not made under the impression that S. *namaycush* was not a char, for I am aware that it is described as such in recent American reports on pisciculture, and have no doubt that such description is correct. But reference to speciegraphical determinations are rare in such reports, and I think readers of Mr. Day's book would have been glad of the references which he now supplies in his letter. YOUR REVIEWER.

MODERN VIEWS OF ELECTRICITY.[1]

PART III. (*continued*).

VI.

LET us now pass in review the various facts and experiences which have led us to a dual view of electricity; a kind of two-fluid theory, but in a very modified form.

First, there are the old experiments which vaguely suggest the separate existence of negative electricity, such as:—

(1) The wind from a point whether positive or negative; so that a candle gets blown always away from it, whether the point be on the prime conductor and the candle held in hand, or whether the point be held in the hand and presented to the candle or prime conductor; so, also, that a point whirligig turns the same way, whether supported on the prime conductor, or whether attached to the earth and placed near it.

(2) Phenomena connected with the spark discharge, such as Wheatstone's old experiment on what he called the velocity of electricity, with the three pair of knobs; and the double burr produced in cardboard when pierced with a spark, suggesting that something has pierced it both ways at once.

Then there are the more recently observed facts; as, for instance:—

(3) The fact that an electrostatic strain scarcely affects the volume of a dielectric; thereby at once suggesting something of the nature of a shearing or distorting stress, which alters shape but not size; a displacement of positive outwards and simultaneous negative inwards.

(4) The facts of electrolysis, and the double procession of atoms past each other in opposite directions.

(5) The phenomena of self-induction, and the behaviour of a thick wire to an alternating current. The delay also in magnetizing iron, and especially the possibility of permanent magnetism; combined with

(6) The absence of momentum in an electric current, or moment of momentum in an electro-magnet, as tested by all mechanical means yet tried.

I admit at once that many of these are mere superficial suggestions which may hardly bear examination and criticism. Only (3), (4), (5), and (6) can be at all seriously appealed to; but (5) and (6), in conjunction, seem to me to afford a sort of provisional and hypothetical proof, which (3) greatly strengthens.

At this point we must for the present again leave the question.

Representation of a Magnetic Field.

The disturbance called magnetism, which we have shown to be something of the nature of a spin—a rotation about an axis—is conspicuously not limited to the steel or iron of the magnet: it spreads out through all adjacent space, and constitutes what is called the magnetic field. A map of the field is afforded by the use of iron filings, which cling end to end and point out the direction of the force at every point.

These lines of force so mapped are to be regarded as the axes of molecular whirls. They are continuous with similar lines in the substance of the steel, and every line really forms a closed curve, of which a portion is in the steel and a portion in the air. In a wire helix, such as Figs. 16 or 29, the lines are wholly in the air, but in one part of their course they thread the helix, and in another part they spread out more or less between its faces.

But according to Ampère's theory of molecular currents there is no *essential* difference between such a helix and a steel magnet; directly the currents in the molecules of the magnet are considered, everything resolves itself into chains of molecular currents, threading themselves along a common closed curve or axis. Each atom, whether in the steel or in the air, is the seat of a whirl of electricity, more or less faced round so as on the average to have its plane at right angles to the lines of force. The simplest plan of avoiding having to consider those only partially faced round, is to imagine the whole number divided into a set which face accurately in the right direction, and a set which look any way at perfect random, and to neglect this latter set.

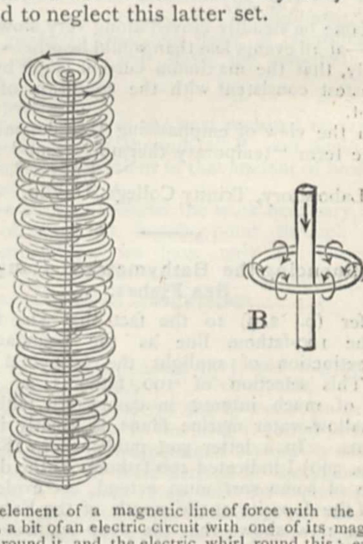


FIG. 30.—A, an element of a magnetic line of force with the electric whirl round it; B, a bit of an electric circuit with one of its magnetic lines of force shown round it, and the electric whirl round this; each magnetic line of force round a current being an electric vortex ring.

Well now try and picture a chain of whirls like beads spinning on a wire threading them all, and think of the effect of a material fluid thus rotating. Obviously it would tend to whirl itself fatter, and to shorten its length. An assemblage of such parallel straight whirls would thus squeeze each other laterally, or cause a lateral pressure, and would tend to drag their free ends together, causing a longitudinal tension.

Such whirls cannot in truth have free ends except at the boundary of a medium—as at the free surface of a liquid. Magnetic whirls are in reality all closed curves;

[1] Continued from p. 110.

but inasmuch as part of them may be in a mobile fluid like air, and part of them in a solid like iron or steel, it is convenient to distinguish between their two portions; and one may think of the air whirls alone as reaching from one piece of iron to another and by their shortening tendency or centrifugal force pulling the two pieces together.

The arrangement shown in Fig. 31 illustrates the kind

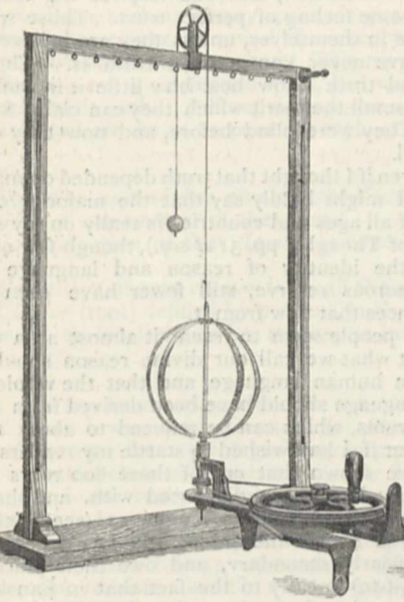


FIG. 31.—A "shape of the earth" model which, when whirled, exerts a tension along its axis, pulling up the weight attached to it, and a pressure at right angles, by reason of its bulging out.

of force exerted by a spinning elastic framework along and perpendicular to its axis of rotation.

One can easily see this effect of a whirl in a tea-cup or inverted bell-jar full of liquid. Stir it vigorously, and leave it. It presses against the walls harder than before, so that if they were elastic they would bulge out with the lateral pressure; and it sucks down the top or free end of its axis of rotation, producing quite a depression or

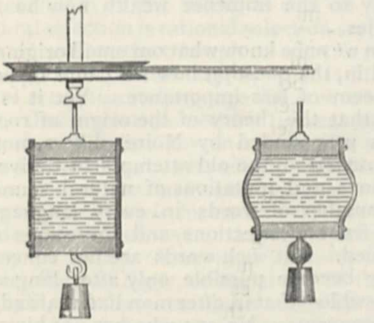


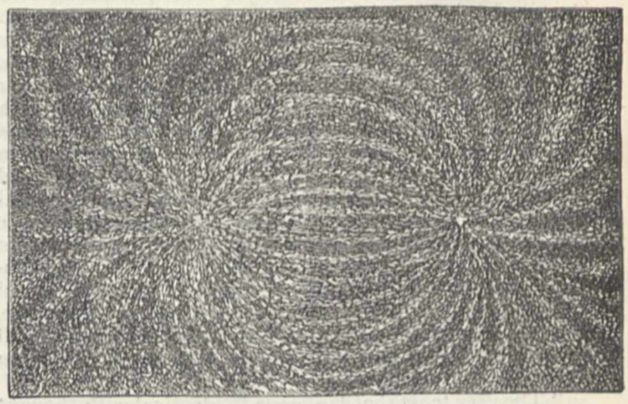
FIG. 32.—An elastic-walled cylindrical vessel full of liquid hanging from a whirling table, and, by reason of centrifugal force, raising a weight and bulging out laterally when spun, thereby illustrating a tension along the axis of rotation and a pressure in every perpendicular direction.

hollow against the force of gravity. Or, as a more striking illustration, make the following apparatus.

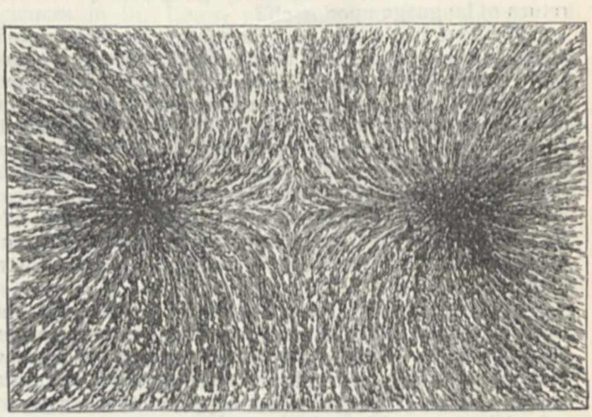
Two circular boards joined by a short wide elastic tube or drum: a weight hung to the lower board, the top board hung from a horizontal whirling table, the drum filled with water, and the whole spun round. The weight is raised by the longitudinal tension; the sides bulge out with the lateral pressure.

There is no need for the whole vessel to rotate. If the liquid inside rotates, the same effect is produced.

Imagine now a medium composed of a multitude of such cells with rotating liquid inside: let the cells be either very long, or else be joined end to end so as to make a chain—a series of chains side by side—and you have a picture of a magnetic medium traversed by a field of force. End-boundaries of the field will be dragged together, thus representing magnetic attraction; while, sideways, the lines of force (axes of whirl) squeeze each



Attraction.



Repulsion.

FIG. 33.—Attraction and repulsion. The tension along the lines of force or axes of rotation drags the one pair of poles together; and the pressure in directions perpendicular to the axis of rotation due to the centrifugal force of the whirls drives the other pair apart.

other apart, thus illustrating repulsion. This is Clerk-Maxwell's view of an electro-magnetic medium, and of the mode in which magnetic stress, and magnetic attractions and repulsions between bodies, arise.

Wherever lines of force reach across from one body to another, those bodies are dragged together as if pulled by so many elastics (Fig. 33); but wherever lines of force from one body present their *sides* to those proceeding from another body, then those bodies are driven apart.

OLIVER J. LODGE.

(*To be continued.*)

LANGUAGE-REASON.

THE inclosed letter on "Reason-Language" was written to an American friend, and has been published in an American paper in Chicago. I thought it might possibly interest the readers of NATURE.

Oxford, January 22. F. MAX MÜLLER.

"You tell me that my book on the 'Science of Thought' is thoroughly revolutionary, and that I have all recognized authorities in philosophy against me. I doubt it. My book is, if you like, evolutionary, but not revolutionary; I mean it is the natural outcome of that philosophical and historical study of language which began with Leibniz, and which during our century has so widely spread and ramified as to overshadow nearly all sciences, not excepting what I call the science of thought.

"If you mean by revolutionary a violent breaking with the past, I hold on the contrary that a full appreciation of the true nature of language and a recognition of its inseparableness from thought will prove the best means of recovering that unbroken thread which binds our modern schools of thought most closely together with those of the Middle Ages and of ancient Greece. It alone will help us to reconcile systems of philosophy hitherto supposed to be entirely antagonistic. If I am right—and I must confess that with regard to the fundamental principle of the identity of reason and language I share the common weakness of all philosophers, that I cannot doubt its truth—then what we call the history of philosophy will assume a totally new aspect. It will reveal itself before our eyes as the natural growth of language, though at the same time as a constant struggle of old against new language—in fact, as a dialectic process in the true sense of the word.

"The very tenet that language is identical with thought—what is it but a correction of language, a repentance, a return of language upon itself?

"We have two words, and therefore it requires with us a strong effort to perceive that behind these two words there is but one essence. To a Greek this effort would be comparatively easy, because his word *logos* continued to mean the undivided essence of language and thought. In our modern languages we shall find it difficult to coin a word that could take the place of *logos*. Neither *discours* in French, nor *Rede* in German, which meant originally the same as *ratio*, will help us. We shall have to be satisfied with such compounds as thought-word or word-thought. At least, I can think of no better expedient.

"You strongly object to my saying that there is no such thing as reason. But let us see whether we came honestly by that word. Because we reason—that is, because we reckon, because we add and subtract—therefore we say that we have reason; and thus it has happened that reason was raised into something which we have or possess, into a faculty, or power, or something, whatever it may be, that deserves to be written with a capital R. And yet we have only to look into the workshop of language in order to see that there is nothing substantial corresponding to this substantive, and that neither the heart nor the brain, neither the breath nor the spirit, of man discloses its original whereabouts. It may sound violent and revolutionary to you when I say that there is no such thing as reason; and yet no philosopher, not even Kant, has ever in his definition of reason told us what it is really made of. But remember, I am far from saying that reason is a mere word. That expression, 'a mere word,' seems to me the most objectionable expression in the whole of our philosophical dictionary.

"Reason is something—namely, language—not simply as we now hear it and use it, but as it has been slowly elaborated by man through all the ages of his existence on earth. Reason is the growth of centuries, it is the work of man, and at the same time an instrument brought to higher and higher perfection by the leading thinkers and speakers of the world. No reason without language—no language without reason. Try to reckon without numbers, whether spoken, written, or otherwise marked; and if you succeed in that I shall admit that it is possible to reason or reckon without words, and that there is in us such a thing or such a power or faculty as reason, apart from words.

"You say I shall never live to see it admitted that man cannot reason without words. This does not discourage me. Through the whole of my life I have cared for truth, not for success. And truth is not our own. We may seek truth, serve truth, love truth; but truth takes care of herself, and she inspires her true lovers with the same feeling of perfect trust. Those who cannot believe in themselves, unless they are believed in by others, have never known what truth is. Those who have found truth know best how little it is their work, and how small the merit which they can claim for themselves. They were blind before, and now they can see. That is all.

"But even if I thought that truth depended on majorities, I believe I might boldly say that the majority of philosophers of all ages and countries is really on my side (see 'Science of Thought,' pp. 31 *et seq.*), though few only have asserted the identity of reason and language without some timorous reserve, still fewer have seen all the consequences that flow from it.

"Some people seem to resent it almost as a personal insult that what we call our divine reason should be no more than human language, and that the whole of this human language should have been derived from no more than 800 roots, which can be reduced to about 120 concepts. But if I had wished to startle my readers I could easily have shown that out of these 800 roots one-half could really have been dispensed with, and has been dispensed with in modern languages (see 'Science of Thought,' p. 417), while among the 120 concepts not a few are clearly secondary, and owe their place in my list (*ib.* p. 619) merely to the fact that in Sanskrit they cannot be reduced to any more primitive concept. To dance, for, instance, cannot be called a primitive concept; perhaps not even to hunger, to thirst, to cook, to roast, &c. Only it so happens that in Sanskrit, to which my statistical remarks were restricted, we cannot go behind such roots as NÆT, KSHUDH, TÆSH, PAK, &c. It is in that limited sense only that such roots and such concepts can be called primitive. The number of really primitive concepts would be so alarmingly small that for the present it seemed wiser to say nothing about it. But so far from being ashamed of our modest beginnings, we ought really to glory rather in having raised our small patrimony to the immense wealth now hoarded in our dictionaries.

"When we once know what our small original patrimony consisted in, the question how we came in possession of it may seem of less importance. Yet it is well to remember that the theory of the origin of roots and concepts, as propounded by Noiré, differs, not in degree, but *toto cœlo* from the old attempts to derive roots from interjections and imitations of natural sounds. That a certain number of words in every language has been derived from interjections and imitations no one has ever denied. But such words are not conceptual words, and they become possible only after language had become possible—that is, after man had realized his power of forming concepts. No one who has not himself grappled with that problem can appreciate the complete change that has come over it by the recognition of the fact that roots are the phonetic expressions of the consciousness of our own acts. Nothing but this, our consciousness of our own repeated acts, could possibly have given us our first concepts. Nothing else answers the necessary requirements of a concept, that it should be the consciousness of something manifold, yet necessarily realized as one. After the genesis of the first concept, everything else becomes intelligible. The results of our acts become the first objects of our conceptual thought; and with conceptual thought, language, which is nothing if not conceptual, begins. Roots are afterwards localized, and made the signs of our objects by means of local exponents, whether suffixes, prefixes, or infixes. What has been scraped and shaped again and again becomes as it were 'shape-her',' *i.e.* a shaft; what has been dug and hollowed out by repeated blows becomes 'dig-her',' *i.e.* a hole. And from the concept of a hole dug, or of an empty cave, there is an uninterrupted progress to the most abstract concepts, such as empty space, or even nothing. No doubt, when we hear the sound of cuckoo, we may by one jump arrive at the word cuckoo. This may be called a word, but it is not a conceptual word, and we deal with conceptual words only. Before we can get at a single conceptual word, we have to pass through at least five stages:—

"(1) Consciousness of our own repeated acts.
"(2) *Clamor concomitans* of these acts.
"(3) Consciousness of that *clamor* as concomitant of the act.
"(4) Repetition of that *clamor* to recall the act.
"(5) *Clamor* (root) defined by prefixes, suffixes, &c., to recall the act as localized in its results, its instruments, its agents, &c.

"You can see from my preface to the 'Science of Thought' that I was quite prepared for fierce attacks, whether they came from theologians, from philosophers, or from a certain class of scholars. So far from being discouraged, I am really delighted by the opposition which my book has roused, though you would be surprised to hear what strong support also I have received from quarters where I least expected it. I have never felt called upon to write a book to which everybody should say *Amen.* When I write a book, I expect the world to say *tamen*, as I have always said *tamen* to the world in writing my books. I have been called very audacious for daring to interfere with philosophy, as if the study of language, to which I have devoted the whole of my life, could be separated from a study of philosophy. I have listened very patiently for many years to the old story that grammar is one thing and logic another; that the former deals with such laws of thought as are observed, the latter with such as ought to be observed. No, no. True philosophy teaches us another lesson—namely, that in the long run nothing is except what ought to be, and that in the evolution of the mind, as well as in that of Nature, natural selection is rational selection; or, in reality, the triumph of reason, the triumph of what is reasonable and right; or, as people now say, of what is fittest. We must learn to recognize in language the true evolution of reason. In that evolution nothing is real or remains real except what is right; nay, in it even the apparently irrational and anomalous has its reason and justification. Towards the end of the last century, what used to be called *Grammaire Générale* formed a very favourite subject for academic discussions; it has now been replaced by what may be called *Grammaire Historique.* In the same manner, *Formal Logic*, or the study of the general laws of thought, will have to make room for *Historical Logic*, or a study of the historical growth of thought. Delbrück's essays on comparative syntax show what can be done in this direction. For practical purposes, for teaching the art of reasoning, formal logic will always retain its separate existence; but the best study of the real laws of thought will be hereafter the study of the real laws of language. If it was really so audacious to make the identity of language and reason the foundation of a new system of philosophy, may I make the modest request that some philosopher by profession should give us a definition of what language is without reason, or reason without language? "F. M. M."

FERDINAND VANDEVEER HAYDEN.

We reprint from the American journal *Science* (January 6) the following article on Dr. Hayden, whose death we lately announced:—Prof. Ferdinand Vandeveer Hayden, M.D., Ph.D., LL.D., who died in Philadelphia on the morning of December 22, was born in Westfield, Mass., September 7, 1829. Early in life he went to Ohio. In 1850 he was graduated from Oberlin College, and soon afterward read medicine at Albany, N.Y., receiving his degree from the Albany Medical College in 1853. He did not begin the practice of medicine, but in the spring of the year of his graduation was sent by Prof. James Hall of Albany, with Mr. F. B. Meek, to visit the Bad Lands of White River, to make collections of the Cretaceous and Tertiary fossils of that region. This was the beginning of his explorations of the West, which continued with little interruption for more than thirty years.

In the spring of 1854, Dr. Hayden returned to the Upper Missouri region, and spent two years in exploring it, mainly at his own expense, although he was aided a portion of the time by gentlemen connected with the American Fur Company. During these two years he traversed the Missouri River to Fort Benton, and the Yellowstone to the mouth of the Big Horn River, and explored considerable portions of the Bad Lands of White River and other districts not immediately bordering upon the Missouri. The large collections of fossils he made were given partly to the Academy of Sciences in St. Louis, and partly to the Academy of Natural Sciences of Philadelphia.

As one of the members of the Geological Survey has recently said, these collections and researches mark the commencement of the epoch of true geologic investigation of the Great West. The collections attracted the attention of the officers of the Smithsonian Institution; and in February 1856, Dr. Hayden was employed by Lieut. G. K. Warren, of the United States Topographical Engineers, to make a report upon the region he had explored; so that the results of his labours during the three previous years were utilized by the Government. This report was made in March of the same year, and in May following he was appointed geologist on the staff of Lieut. Warren, who was then engaged in making a reconnaissance of the North-West. He continued in this position until 1859, when he was appointed naturalist and surgeon to the Expedition for the exploration of the Yellowstone and Missouri Rivers, by Capt. William F. Raynolds of the Corps of Engineers of the United States Army, with whom he remained until 1862. The results of his work while with Liutenant Warren were published in a preliminary report of the War Department, and in several articles in the Proceedings of the Academy of Natural Sciences of Philadelphia for the Years 1857 and 1858, and more fully in a memoir on the geology and natural history of the Upper Missouri, published in the Transactions of the American Philosophical Society, Philadelphia, 1862. This paper also included chapters on the mammals, birds, reptiles, fishes, and recent mollusca of the region in which his geological investigations were carried on. During this period also he found time to make notes upon the languages and customs of the Indian tribes with which he came in contact. These notes were embodied in "Contributions to the Ethnography and Philology of the Indian Tribes of the Missouri River," published in the Transactions of the American Philosophical Society, Philadelphia, 1862; in a "Sketch of the Mandan Indians, with some Observations illustrating the Grammatical Structure of their Language," published in the *American Journal of Science* in 1862; and in "Brief Notes on the Pawnee, Winnebago, and Omaha Languages," published in the Proceedings of the American Philosophical Society, Philadelphia, 1869.

In May 1862, Dr. Hayden was appointed acting-assistant surgeon of volunteers by the Surgeon-General of the United States Army, and was sent to Satterlee Hospital in Philadelphia. He was confirmed by the United States Senate as assistant-surgeon and full surgeon of volunteers on the same day (February 16, 1863), and sent to Beaufort, S.C., as chief medical officer, where he remained for one year, when he was ordered to Washington as assistant medical inspector of the Department of Washington. On February 19, 1864, he was sent to Winchester, Va., as chief medical officer of the army in the Shenandoah valley. Here he remained until May 1865, when he resigned, and was brevetted lieutenant-colonel for meritorious services during the war. During the remainder of the year 1865 he was employed in work at the Smithsonian Institution. It was during this year that he was elected Professor of Geology and Mineralogy in the University of Pennsylvania,—a position he held until 1872, when the increased executive duties in connection with the Geological Survey of the Territories induced him to resign it.

In the summer of 1866 he undertook another expedition to the Bad Lands of Dakota, under the auspices of the Academy of Natural Sciences of Philadelphia, for the purpose of clearing up some doubtful points in the geology of that region, and returned with large and valuable collections of vertebrate fossils, which were described in a memoir published by the Academy of Natural Sciences of Philadelphia in 1869. From 1867 to 1879 the history of Dr. Hayden is the history of the United States Geological and Geographical Survey of the Territories, of which he was geologist-in-charge, and to the success of which he devoted all his energies during the twelve years of its existence. In this time more than fifty volumes, together with numerous maps, were issued under his supervision. One of the results of his surveys, and the one in which he probably took the greatest interest, was the setting aside by Congress of the Yellowstone National Park. The idea of reserving this region as a park or pleasure-ground for the people originated with Dr. Hayden, and the law setting it apart was prepared under his direction. The work of the Geological Survey of the Territories had its consummation in the Atlas of Colorado, which increased greatly our knowledge of one of the most interesting portions of the Great West. In 1879, after the disbanding of the Survey of the Territories, Dr. Hayden received an appointment as geologist on the newly organized United States Geological Survey. For about three years he was occupied in the completing of the business of the Geological and Geographical Survey of the Territories, and the preparation of the final results of that survey. His health had already begun to fail, but early in 1883 he asked to be relieved from the supervision of the printing of the reports, and during the three following seasons he undertook field work in Montana. By the latter part of the year 1886 his health had become so poor that he was confined most of the time to his bed. He then resigned his position as geologist, closing an honourable connection with the Government that included twenty-eight years of actual service as naturalist, surgeon, and geologist. To the general interest in science excited by the enthusiastic labours of Dr. Hayden in his geologic explorations, is due in a great degree the existence and continuance of the present United States Geological Survey.

In 1876 the degree of LL.D. was conferred upon him by the University of Rochester, and in June 1886 the same degree was conferred upon him by the University of Pennsylvania. Dr. Hayden was a member of the National Academy of Sciences and of many other Societies scattered throughout the country. He was also honorary and corresponding member of a large number of foreign Societies.

As to Dr. Hayden's personal character, those who were personally associated with him know best how genial he was, and how sincere and enthusiastic his desire to forward the cause of science. Although impulsive at times, he was generous to a fault. His subordinates all knew that each one stood upon his own merits, and that due credit would be awarded to his successful efforts. The same spirit actuated him in respect to those not immediately connected with him. His views are expressed as follows in one of his earliest reports, when speaking of those who had preceded him: "Any man who regards the permanency or endurance of his own reputation will not ignore any of these frontier men who made their early explorations under circumstances of great danger and hardship."

His ideas were broad and liberal. He aimed to make a thorough astronomical, topographical, geological, and botanical survey of the Great West, with a view to the development of its mining and agricultural resources. The greater part of his work for the Government and for science was a labour of love.

To the foregoing notice some token of recognition and regret on the part of brother geologists on this side of the Atlantic may perhaps be fittingly appended by one who knew Dr. Hayden personally, was familiar with his writings, and had wandered in his footsteps among the solitudes of the Far West. The first impression which the late geologist made on those who came to know him was one of gentleness, almost of timidity. They could hardly help asking themselves, "Can this be the man who has so successfully won over the blustering Congressmen to grant him year after year such large appropriations for his western surveys; who has organized such wonderful expeditions; who has gone through such hardships, and in an incredibly short space of time has made such excellent reconnaissances and published such voluminous Reports and admirable maps?" It was some time before one could see the real underlying secret of his success. This was undoubtedly a quiet enthusiasm for science, supported by an undemonstrative but indomitable courage, and a determination to gain the proposed end, cost what it might in bodily and mental endurance. No one who has not been in some measure admitted behind the scenes of political wire-pulling in the States, can realize what had to be undertaken by the man of science who would obtain and retain an annual subsidy from Congress for scientific investigation in the days when Hayden carried on his explorations. There were other rival claimants for Parliamentary aid who were doing similar work, under other Government Departments. There was likewise the wide outside circle of scientific men who had no State employment, and some of whom thought themselves at least as deserving of it as those who fortunately had gained it. Then there were the Gallios of Congress, who cared nothing about science of any kind, those who grudged money spent out of their own States, those who required to see on their drawing-room table a well got up Annual Report with pictures and maps before they could be made to believe that the money was well bestowed. And the weeks and months of early summer, so precious for field work, had to be passed in the lobbies of the Capitol, making sure that there would be no failure in the granting of the appropriation. The most wearisome and profitless part of his year was this "lobbying" at Washington. But Hayden had no choice in the matter. He must either go through with that part of his work or abandon his western surveys altogether. This alternative has not always been borne in mind by those who have judged of him.

There can be no doubt that among the names of those who have pioneered into the marvellous geology of Western North America, that of F. V. Hayden will always hold a high and honoured place. This place will be his due, not only because of his own personal achievements in original exploration. His earlier work exhibits much of that instinctive capacity for grasping geological structure which is the main requisite for a field geologist. He had a keen "eye for a country." But he likewise possessed the art of choosing the best men for his assistants, and the tact of attaching them to himself and his corps. In this way he accomplished much excellent work, keeping himself latterly rather in the background so far as actual personal geological investigations were concerned, and contenting himself with the laborious task of organization and supervision, while he encouraged and pushed forward his coadjutors.

The abolition of his Survey and the appointment of one of his rivals to the post of Director of the reconstituted Geological Survey of the United States, was a blow from which he does not seem ever to have recovered. He was treated, however, with great generosity by the new Director, and had a share of the large annual appropriation to enable him to complete his Reports. He was urged to condense these voluminous works, and to present a concise and readable account of what he and his fellow-workers had done for the geology of the far West. But he had no literary proclivities, and in the end gladly surrendered the task of writing for the more congenial employment of renewing his personal acquaintance with the geology of the Western Territories. Perhaps among those, and there must be many, who personally knew and esteemed him, there may be one competent and willing to compile or complete the summary which he never completed, and thus to erect to his memory a more fitting and lasting monument than one of brass or marble. A. G.

NOTES.

We regret to announce the death of Dr. Asa Gray, the most eminent of American botanists. He died at Cambridge, Massachusetts, on Monday, aged seventy-seven. Next week we shall give some account of his services to science.

Mr. George Godwin, F.R.S., well known as the editor of the *Builder*, died on January 27. He was seventy-three years of age. Among his writings were several works in which, with great earnestness, he pressed upon the attention of the public the evil consequences springing from the neglect of sanitary laws.

Mr. George Robert Waterhouse, late Keeper of the Department of Geology in the British Museum, died at his residence, Curton Lodge, Putney, on January 21, in his seventy-eighth year.

We have also to record the death of the well-known botanist, Dr. J. T. I. Boswell, who was for many years Curator to the Botanical Society in London, and a Lecturer at the Charing Cross and Middlesex Schools of Medicine.

The Medals and Funds to be given at the annual meeting of the Geological Society on February 17 have been awarded as follows:—Wollaston Medal to H. B. Medlicott, F.R.S.; Murchison Medal to Prof. J. S. Newberry, M.D., of New York; Lyell Medal to Prof. H. Alleyne Nicholson, M.D., D.Sc.; Wollaston Fund to John Horne, F.R.S.E.; Murchison Fund to E. Wilson of the Bristol Museum; Lyell Fund to Arthur H. Foord, and T. Roberts, B.A.

The Academy of Sciences at Turin has awarded the great Bressa prize of 12,000 francs (£480) to M. Pasteur.

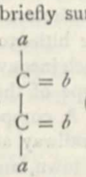
The eighteenth International Congress of Orientalists will meet in Stockholm on September 2 next, and be opened by King Oscar in person, attended by the whole of the Royal family. The Congress will sit till September 6, when the members will visit Christiania as guests of the King, in whose name they will be entertained in the Norwegian capital for two days. They will then proceed to Gothenburg, where the Congress will be dissolved. In honour of the Congress a bibliography is to be issued, containing the portraits in heliography of all living Orientalists, and a *résumé* of the works published by each. The work is to be most sumptuously got up. The editor is Count Carlo Landberg.

The following arrangements have been made for the Penny Science Lectures at the Royal Victoria Hall: February 7, by Dr. Percy Frankland, "Germs in the Air, and what they do for us"; February 14, by E. Wethered, "Earthquakes and Volcanoes"; February 21, by F. R. Cheshire, "Insects as Florists and Fruit-makers"; February 21, by E. Hodder, "Incidents in the Life of Lord Shaftesbury."

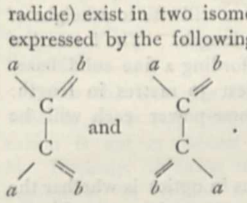
Information has been received of the arrival of H.M. surveying-ship *Egeria*, Capt. P. Aldrich, at King George's Sound, after a very successful deep-sea sounding cruise across the Indian Ocean. Between latitudes 10° and 35° S.,—a belt 1500 miles wide,—not a single sounding has heretofore been obtained in this ocean, and it is therefore satisfactory to learn that forty-three soundings, all of them accompanied with several sets of temperature observations, have been now obtained. The *Egeria's* track was from Sunda Strait to Mauritius, thence south to latitude 38°·5, and thence to Western Australia.

Memorials are being sent from various public bodies in Hampshire to the Lord President of the Council, requesting that the proposed Forestry School for England may be established in that county. It is pointed out that the extensive Crown lands of Hampshire are peculiarly well fitted for scientific and practical forestry.

A paper of exceptional interest was read by Prof. Victor Meyer at the meeting of the Chemical Society of Göttingen held on January 24. In it were embodied some remarkable speculations upon the shape of the ultimate atoms of carbon. These ideas are the outcome of his recent work upon the oxims of benzil and certain other complicated organic compounds, and may be briefly summed up as follows. Certain compounds of the type



(where a represents a monad and b a dyad radicle) exist in two isomeric modifications which can only be expressed by the following different geometrical arrangement:



This necessitates an expansion of the theory of Van t' Hoff and Wislicenus, according to which, by rotation of one of the carbon atoms in the first case, the latter would be the only stable form; there are cases in which this rotation is free to occur, and cases like the present where it is prevented. From a consideration of the geometrical isomers of the benzyl cyanides, Prof. Meyer further shows that the valencies of carbon may be displaced out of their normal positions at the corners of a regular tetrahedron by the unequal attractions of unlike radicles. Finally, as the only means of accounting for all these varied phenomena, Prof. Meyer expresses his conviction that the atoms of carbon are spheres, each surrounded by an ether-shell, which forms the seat of the four valencies. On account of their probable electrical connection he terms these valencies "electrules," and considers that the electrules of the same atom are in isochronous oscillation, and therefore, in accordance with the law, repel each other but are attracted by the dissimilar oscillations of other atoms. In consequence of this repulsion they must take up the regular tetrahedral position, from which, however, under the circumstances mentioned above, they may be slightly displaced. In course of the discussion which ensued, Prof. Riecke followed up the subject with a preliminary notice of his own researches upon the shape of odd-valent atoms, and showed that, in case of nitrogen, phosphorus, and other pentad elements, the ether-shell is probably not spherical, but an ellipsoid; three valencies being situate upon the great circle at the angles of an equilateral triangle, whilst the other two are located at the poles. If this indeed be the case, the tri- and penta-valency of the nitrogen group will be completely and very simply explained.

IN a paper contributed to the Royal Society of Edinburgh, and to be published in a forthcoming volume of the Society's *Transactions*, Mr. A. Crichton Mitchell gives the results of an experimental inquiry, made at the Edinburgh University Physical Laboratory, into the thermal conductivity of iron, copper, and German silver. The method employed was that of Forbes. The experiments were, for the most part, a repetition of those made by Prof. Tait about ten years ago; the main difference being that the bars used were nickel-plated in order to prevent oxidation, and thus render the estimation of the amount of surface loss of heat a matter of greater certainty than hitherto. The results arrived at confirm, in the main, those of the previous work, on the same bars, already mentioned, and are of some importance in deciding the question as to whether thermal conductivity increases or decreases with increase of temperature. The principal conclusion in the paper is, that in iron, thermal conductivity increases with increased temperature, and that therefore iron can no longer be looked upon as an exception to the rule followed by the other metals, viz. that their conductivity increases with temperature.

THE first rack-railway in France was opened lately at Langres (in Haute-Marne), which is perched on a hill 1460 feet high, and is the principal seat of the French manufacture of fine cutlery. The two railway-stations which have hitherto served it are several kilometres from the town, and much inconvenience has been felt. The new line is made on the type of the Righi railway, and rises to the heart of the town. A company has recently been formed to construct a funicular railway at Hong Kong, for ascent of the hill overlooking the town, and much frequented by the inhabitants. This line will rise to a height of 500 metres, and will be 1600 metres in length. The formation of the ground is exclusively granite, affording a fine solid base. There will be three viaducts, the largest 30 metres in length. Two compound engines of forty horse-power each will be installed at the upper station.

ONE of the most important questions in optics is whether the velocity of propagation, and therewith the wave-length of light, depend on its intensity or not. A determination of wave-length from prismatic decomposition is not capable of great accuracy. Dr. Ebert, of Erlangen, in studying the subject, preferred a more accurate method based on interference phenomena. Using eight variously coloured light sources (lithium, hydrogen, sodium, &c.), varied in intensity between the values 1 and 250, he established the constancy of the wave-length and velocity of propagation to nearly a millionth within those limits of intensity. Considering, with these results, that the great brightness of the sun does not destroy the coincidences of the Fraunhofer lines with lines of our terrestrial light sources, even with the greatest dispersions, Dr. Ebert thinks his affirmation (of independence of intensity) is generally valid within ordinarily occurring limits of brightness.

MR. M. W. HARRINGTON has contributed to the *American Meteorological Journal* for December the results of an interesting inquiry as to whether the rainfall is increasing on the plains. The subject is one of importance, as an annual increase of the rainfall would increase the agricultural capacity of a large territory. In order to come to a definite conclusion he has used two long series of observations representing the average conditions at the epochs of 1850 and 1880. The author compares the lines of equal rainfall for the two periods, and shows that, if there had been an increase, any one line should have travelled westwards in the interval between the two epochs. The result of the inquiry shows an apparent advance along the zone included between the parallels of 35° and 45°, and a regression above and below these latitudes, in other words that there has been apparently a consistent increase of rainfall toward the plains.

M. A. LANCASTER, Meteorologist at the Royal Observatory of Brussels, and Inspector of Meteorological Stations in Belgium, has published a paper on the climate of that country in 1887, based on the observations at Brussels, and three other stations at the west, north, and east limits of the kingdom, the stations selected being typical of all other points of observation. The weather for each month is discussed at considerable length, and the results are compared with the normal values. The observations show that the mean barometer is highest in December and lowest in October; the absolute maximum temperature during the year at Brussels was 91°·0, and the minimum 15°·4. The prevalent winds are from west-south-west to south-south-west. The rainfall during the year amounted to 24·42 inches, being considerably below the average, viz. 28·78 inches. The rainfall diminishes with the distance from the sea, excepting in the neighbourhood of the forests; the amount increases notably in the Ardennes.

DR. FINES has published, in the fifteenth *Bulletin Météorologique* of the Department of the Pyrénées-Orientales, the results of experiments carried on at the Perpignan Observatory:—(1) To test the theory of M. Kammermann, of Geneva, for the prediction of spring frosts by comparison of the readings of the wet-bulb and minimum thermometers (explained in the *Archives des Sciences physiques et naturelles*, vol. xiv. p. 425); the result being that the data, so far as Perpignan is concerned, do not bear out the character of accuracy attributed to them elsewhere. (2) To compare the results of wind-velocity given at each instant by a Bourdon's anemometer (presented to the Academy of Sciences, July 30, 1882) with the records of Robinson's velocity anemometer. The result of this inquiry shows a mean increase in the maximum velocities of over 21 per cent. by the use of Bourdon's instrument as compared with the means of the greatest velocities obtained by Robinson's anemometer.

WE have received from the Imperial Observatory at Tokio the monthly summaries and means of the observations of the meteorological service in Japan for the year 1886, accompanied by charts showing the tracks of central areas of high and low barometer and by synoptic weather charts for each month. The stations are supplied with good instruments (in some cases with self-recording apparatus), and thee obsrvations which have now been regularly taken for about eleven years under the present organization compare favourably with those of European systems. The climate is generally of the oceanic type—the highest mean temperature at Tokio occurring in August. The winds are governed by the monsoon seasons; during the first three months of the year the prevalent directions at Tokio are north and north-west. From April to June the winds shift round to the south, through east. In July and August southerly winds predominate, but in September a sudden change to the north occurs, and continues generally until December. Warnings of wind and weather are issued to various stations; the general percentage of success for both elements during the year 1886 was 80. Rainfall maps for each month and for the year are also given.

WE have received the *Annuaire* for the year 1888, published by the Bureau des Longitudes with Messrs. Gauthier-Villars, Paris. It contains, besides the tables usually expected in works of this class, much useful information as to the monetary systems of the various nations of the world, mineralogy, meteorology, and other subjects. We may especially note an excellent account, by Admiral Mouchez, of the International Astronomical Congress which met in Paris in April 1887, to prepare the way for the execution of a photographic chart of the heavens.

THE Syndics of the Cambridge University Press have undertaken the publication of a collected edition of the mathematical papers of Prof. Cayley. These papers, originally contributed to the Royal and other Societies and to various mathematical journals, will be arranged for publication by Prof. Cayley himself, who will add notes containing references to the writings of other mathematicians on allied subjects. It is expected that the edition will extend to ten quarto volumes; it is intended to publish two volumes each year until the completion of the work.

"MY TELESCOPE; a Simple Introduction to the Glories of the Heavens," is the title of a little half-crown work on astronomy by "A Quekett Club Man," whose kindred volumes on "The Microscope" have been so successful. It will be issued in a few days by Messrs. Roper and Drowley.

A NEW geological map of the Government of Kutais, by MM. Simonovitch and Sorokin, has been published at Tiflis by the Mining Department.

AT a recent meeting of the Asiatic Society of Japan (reported in the *Japan Weekly Mail* of November 19), Mr. Batchelor read a paper on the *Kamui*, or gods of the Ainos of Yezo. He enumerated under thirteen heads these deities as they appear to be arranged in the Aino mind. These are: (1) the chief of all the deities, the possessor of heaven and the maker of worlds and places; (2) the progenitor of the Aino race, and presider over the affairs of men, who is the only human being worshipped by the people; (3) the sun and moon (the stars are not worshipped); (4) the fire-god, worshipped because of its general usefulness in cooking, healing, purifying, &c.,—sometimes spoken of as the "messenger" or mediator between gods and men; (5) the goddesses who preside over springs, lakes, rivers, and waterfalls,—they are worshipped as benefactors of mankind, particularly in alluring fish to ascend and descend the rivers; (6) the sea-gods, two in number, one being good and one evil,—the latter is the originator of all storms, and the direct cause of shipwrecks and death from drowning at sea; (7) bears, the most powerful animals known to the Ainos, as well as the most useful, supplying them at once with food and clothing; (8) the autumn salmon, the largest fish ascending the rivers,—it is not worshipped, but the term *Kamui* or deity is applied to it; (9) many birds, some of good, others of ill, omen, hough not worshipped, are called deities. The same term is applied to beautiful localities, to high mountains, to regions full of bears or rivers full of fish, to large trees, to cool breezes on a warm day, to men of official rank, to devils, evil spirits, and reptiles. When applied to anything good, the term *Kamui* expresses the quality of usefulness, beneficence, divinity; when applied to anything evil, it implies dread, hatefulness, and such like. Applied to animals, it represents the greatest, fiercest, or most useful; to men, it is a mere title of respect. Subsequently, in the course of the discussion, Mr. Batchelor said that the facts of the Aino religion were very simply stated. They had one chief god, and all the others were officers or messengers of this supreme being; there was no lightning- or thunder-god. These were the facts, but he could not explain them. The Ainos, he said, regarded the sun as a body in which a deity resides, "distinguishing, so to speak, between a body and a soul."

THE fossil head of a mammoth has just been unearthed in the Montmartre cemetery in Paris. The distance between the tusks is nearly 2 feet. Further excavations are being made in the hope that the remainder of the skeleton may be discovered.

ABOUT 20 cwt. of bones of prehistoric animals have been found in the bear cave near Rübeland, in the Harz Mountains. Only a part of the cave has yet been explored.

THE University of Upsala has recently been presented with the fossil skeleton of a whale, found in a layer of marl at a depth of 10 feet in the province of Halland, in the south of Sweden. The skeleton, which is almost perfect, is that of a whale which has been called *Eubalæna svedenborgii*, from some portions of a whale skeleton found last century in the province of West Gothia, and now also in the Museum at Upsala. The skeleton is the only one complete ever found. It is that of a young whale.

ON January 8, about 4 p.m., a magnificent meteor was seen at Porsgrund, in the south-east of Norway. It moved rapidly in an easterly direction towards the constellation Taurus. It was square in appearance, but the corners were rounded, the colours being intense green and violet, increasing in strength as the meteor disappeared behind a hill. The size was about that of the full moon. No sound was heard, nor did it leave a train. The passage occupied about two seconds.

THE fog which lately prevailed over our islands and the North Sea extended far into the heart of Norway.

ON December 27, about 11.30 p.m., a severe shock of earthquake was felt at Solum, in the province of Bratsberg, in the south-east of Norway. The shock was so severe that beds seemed lifted from the floor, and the occupants fled in terror into the open. The shock lasted several seconds, and was accompanied by several deep detonations. Large cracks were afterwards seen in the earth. The motion was from east to west.

A SEVERE earthquake occurred at Algiers on January 8. It was noticed throughout the whole province. In one village a house fell in, and the church and the school-house were damaged.

FROM the Consular Report on the Trade of France for 1886-87, it is apparent that the desire for technical education is not at present widespread throughout that country. M. Lockroy, thinking that one of the great causes of the depression in trade, from which France, like England, has been suffering for some years, was the almost total absence of technical education in France, and that the usual remedies were of little or no avail unless aided by a sounder education of tradesmen and merchants, founded a new department in the Ministry of Commerce to supervise the carrying out of his plans. By his help and that of other supporters of his scheme, technical schools have so increased and multiplied, that there are at present ninety of these institutions in Paris and the provinces subsidized by the State. Very few of them are self-supporting, and the number of students in attendance is lamentably small. Students now, it is said, are not anxious to attend, but it is thought that if technical schools received the power of conferring degrees equivalent to the lower degrees in a University, the students would come more readily. It is also urged that these schools should be freed from Governmental control, and be handed over to the mercantile bodies that in France correspond to our Chambers of Commerce, who understand local needs and local industries better than any department of State. Most of these institutions are behind the age, and the collections at the Conservatoire des Arts et Métiers, Paris, are not so full as those in other countries, and the building itself is in a half-ruinous condition. If the other establishments are inferior to this, as the Report seems to imply, perhaps it is not so difficult to account for the paucity of students and their lack of interest as the Ministry of Commerce seems to thinkit is.

THE additions to the Zoological Society's Gardens during the past week include two Poë Honey-eaters (*Prosthemadera novæ-zealandiæ*) from New Zealand, presented by Capt. Brabazon J. Barlow, s.s. *Tamui*; a Brazilian Hangnest (*Icterus jamaicai*) from Brazil, presented by Mr. Geo. D. Morce; a White-bellied Sea Eagle (*Haliaetus leucocephalus*) from Newfoundland, presented by Mr. Geo. M. Johnson; three Egyptian Cobras (*Naia haje*), three Cerastes Vipers (*Vipera cerastes*), two Hissing Sand Snakes (*Psammophis sibilans*), a Clifford's Snake (*Zamenis cliffordi*), an Egyptian Eryx (*Eryx jaculus*), a Blunt-nosed Snake (*Dipsas obtusa*) from Egypt, presented by Capt. W. G. Burrows; twenty-one Horrid Rattlesnakes (*Crotalus horridus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

AMERICAN OBSERVATORIES.—The January number of the *Sidereal Messenger* states that the University of California has allotted $19,000 for the current expenses of the Lick Observatory during the present year. The Observatory has received an accession to its staff in Mr. Charles B. Hill, formerly of Chabot Observatory. The equipment of the Observatory has also been furthered by the arrival of the 36-inch photographic corrector and the micrometer for the great telescope. The micrometer is by Fauth and Co.

A new Observatory has been opened in connection with the Syracuse University, New York. This Observatory, erected in memory of Mr. C. D. Holden, a former graduate of the University, was dedicated on November 18, 1887, Prof. Newcomb pronouncing the inaugural address. The new institution possesses a transit instrument by Troughton and Simms, of 3 inches aperture, a chronometer by Dent and Co., a chronograph by Fauth and Co., and an 8-inch equatorial by the Alvan Clarks. Prof. John R. French is the Director.

At the Washburn Observatory, Prof. Brown, the new Director, who was formerly at the Naval Observatory, Washington, is engaged at Prof. Auwers' request in the determination of the fundamental star-places of the Zusatz-sterne in Auwers' system.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 FEBRUARY 5–11.

(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 February 5

Sun rises, 7h. 35m.; souths, 12h. 14m. 13·6s.; sets, 16h. 53m.: right asc. on meridian, 21h. 14·6m.; decl. 16° 0′ S. Sidereal Time at Sunset, 1h. 54m.

Moon (between Last Quarter and New) rises, 1h. 20m.; souths, 6h. 24m.; sets, 11h. 19m.: right asc. on meridian, 15h. 23·5m.; decl. 13° 16′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	8 10 ...	13 6 ...	18 2 ...	22 6·3 ...	12 58 S.
Venus.....	5 30 ...	9 32 ...	13 34 ...	18 31·8 ...	21 58 S.
Mars	23 17*...	4 41 ...	10 5 ...	13 40·0 ...	7 44 S.
Jupiter....	2 50 ...	7 6 ...	11 22 ...	16 5·6 ...	19 55 S.
Saturn....	15 20 ...	23 15 ...	7 10*...	8 17·5 ...	20 14 N.
Uranus ...	22 33*...	4 5 ...	9 37 ...	13 4·2 ...	6 7 S.
Neptune..	11 1 ...	18 40 ...	2 19*...	3 41·6 ...	17 54 N.

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

Feb.	h.	
6 ...	1 ...	Jupiter in conjunction with and 4° 2′ south of the Moon.
7 ...	6 ...	Neptune stationary.
8 ...	21 ...	Venus in conjunction with and 1° 24′ south of the Moon

February 11-12.—A partial eclipse of the Sun: not visible in Europe.

Saturn, February 5.—Outer major axis of outer ring = 46″·1; outer minor axis of outer ring = 16″·0; southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4 ...	81 16 N. ...	Feb. 9,	20 19	*m*
Algol	3 0·9 ...	40 31 N. ...	,, 10,	1 30	*m*
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,, 5,	22 43	*m*
			,, 7,	1 59	*m*
S Hydræ	8 47·7 ...	3 30 N. ...	,, 9,		*M*
T Virginis	12 8·9 ...	5 24 S. ...	,, 7,		*M*
δ Libræ	14 55·0 ...	8 4 S. ...	,, 8,	2 50	*m*
U Coronæ	15 13·6 ...	32 3 N. ...	,, 8,	2 26	*m*
V Coronæ	15 45·5 ...	39 55 N. ...	,, 5,		*m*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 9,	1 29	*m*
		and at intervals of		20 8	
X Sagittarii... ...	17 40·5 ..	27 47 S. ...	Feb. 9,	5 0	*M*
Z Sagittarii... ...	18 14·8 ...	18 55 S. ...	,, 6,	0 0	*m*
β Lyræ...	18 46·0 ...	33 14 N. ...	,, 7,	21 0	*m*
			,, 11,	2 0	*M*
U Aquilæ	19 23·3 ...	7 16 S. ...	,, 11,	5 0	*m*
η Aquilæ	19 46·8 ...	0 43 N. ...	,, 11,	21 0	*M*
S Sagittæ	19 50·9 ...	16 20 N. ...	,, 6,	4 0	*m*
			,, 9,	4 0	*M*
Y Cygni	20 47·6 ...	34 14 N. ...	,, 6,	20 9	*m*
			,, 9,	20 3	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near Capella	74 ...	43 N.	
,, λ Draconis	165 ...	73 N.	
,, θ Draconis	240 ...	62 N. ...	February 6.

GEOGRAPHICAL NOTES.

AT Monday's meeting of the Royal Geographical Society, Admiral Mayne gave an account of recent explorations in British North Borneo. The paper of most original interest was, however, that of Mr. Maurice Portman, on the exploration and surveys of the Little Andaman. As an official on the Andaman Islands, Mr. Portman made it his business to conciliate the natives of the Little Andaman, who were regarded as quite intractable, and had been severely punished several times for murdering shipwrecked sailors. After a great deal of trouble and much risk, Mr. Portman succeeded in making friends with the natives, with the result that he and those who accompanied him could visit the island with impunity. He has thus been able to collect much welcome information both concerning the island and its highly interesting inhabitants. He completely surveyed the island, and has thus been able to make important corrections on our maps. At the north end the island consists of mangrove swamp and low belts of sandy soil, on which the aborigines have their huts. On the west and south-west the land rises into low hills of a coarse sandstone, running more or less north and south. The timber appears to be much the same as that of the South Andaman, though Mr. Portman saw no padouk and very few bamboos. The rocks are chiefly lime and sandstone, with a good deal of actual coral rock on the east and south coasts. In one place, south of Daogulé Bay, Mr. Portman noticed an outcrop of igneous rock. He found no minerals of importance. This island is about 27 miles long by 15 miles broad, and is encircled by a fringing coral reef. The products of the sea are the same as at the Great Andaman; but the Tubiporine family of coral, particularly *Tubipora musica*, occur in profusion. Dugong and turtle are very plentiful. On the South Sentinel Island, about 12 miles west of the Little Andaman, the turtle appear to have their breeding-station. This island, which is composed entirely of coral rock, is infested by large iguanas, and the *Birgus latro*, or cocoa-nut-stealing crab (which certainly does not live on cocoa nuts there, as there are none). In rough weather landing is almost impossible on the coast of the Little Andaman, and even in fine weather there are heavy ground-swells and tide-rips. On the north coast large isolated reefs and ledges exist, which make navigation dangerous. With regard to the aborgines of the island, Mr. Portman is of opinion that the whole of the Little Andaman Island is peopled by one race, calling themselves Ongés. These people are subdivided into tribes, who adhere more or less to their own villages, and who quarrel and fight with each other considerably. They appear healthy; their principal diseases being chest complaints, colds, fever, and itch. In physique they compare favourably with the inhabitants of the Great Andaman. Their manners and customs differ somewhat from those of the Great Andaman people, the principal differences being the following:—Instead of small lean-to's, they build large circular huts, some measuring as much as 35 feet in height, and 60 feet in diameter. In these huts the various families sleep on charpoys of wood and cane matting, raised from 6 to 18 inches off the ground, and about 2 feet 6 inches square. Their habits are more cleanly, particularly as regards their huts, and the manner of preparing their food, which is invariably cooked. They cook, dry, and store in baskets, a small fish like a sprat, and this, with the boiled seed of the mangrove, seems to be their principal food, which they supplement with what they can. Their canoes, utensils, ornaments, and bows, are different from those of other Andamanese, and the women wear a tassel of yellow fibre in place of the leaf. They do not smear their bodies over with red ochre, or tattoo themselves, nor do the women keep their heads clean shaved. They are by no means expert in the use of a canoe in rough water, and do not harpoon turtle or dugong, though very fond of the former. They have no religion of any kind, and Mr. Portman learnt nothing of their traditions or superstitions, from which they seem even more free than their neighbours.

Mr. C. M. Woodford has recently returned from a two years' visit to the Solomon Islands, with extensive collections of mammals, birds, reptiles, Lepidoptera, &c. Nearly six months were spent on Guadalcanar, an island the interior of which has never been previously explored. Ascents were made of several rivers, the furthest point reached being about fifteen miles from the coast; but the hostility of the bushmen prevented the ascent of Mount Lammas.

OUR ELECTRICAL COLUMN.

Mr. Willard Case, of Auburn, N.Y., U.S.A., whose extremely interesting paper on a thermic voltaic cell was read before the Royal Society on May 6, 1886, is systematically pursuing his studies to obtain electric energy direct from carbon without passing through the intermediate stage of heat. A paper read on January 10, 1888, in New York, narrates his latest experiments. Jablochkoff tried to do it by immersing plates of carbon and iron in fused nitre. Mr. Case has been using chlorate of potassium and chlorine peroxide (perchloric acid), and with the latter has obtained an E.M.F. with certain forms of carbon varying from 0·3 to 1·24 volt.

In 1869, Dr. Gore proposed a thermo magnetic generator of electricity (Proc. R.S. 1868–69, p. 261), in which an increasing or decreasing magnetic field was produced by heating and cooling an iron wire placed as a core to a coil of wire. Mr. Edison has recently endeavoured to make this principle practical, but M. Menges, of the Hague, has been more successful. The difficulties to overcome are waste of heat, energy, and consumption of time, in heating and cooling. The results obtained at present are, however, poor, though encouraging.

M. Tereschin, following Quincke's examples and directions, has found with water, methyl and ethylic alcohol, bisulphide of carbon, ether, oil of turpentine, and rape oil, a considerable transport of mass in capillary tubes in the direction of the positive current (*Beiblätter* No. 10, 1887); and Prof. Horace Lamb, in the *Phil. Mag.* for January, prints the admirable paper on the subject which was read before Section A of the British Association at Manchester last September, in which he criticizes the work of Wiedemann and Helmholtz, and explains the phenomenon on the assumption of Quincke, that there is a contact potential difference between the fluid and its solid boundaries, and his own conclusion that there is a sliding coefficient for a fluid in contact with a solid. This transport of mass, due to currents, and the electromotive forces produced by the passage of liquids through capillary tubes, and porous diaphragms are facts undeveloped and unapplied at present.

Considerable attention is being devoted to the heating and fusing of wires by currents. Short lengths of fine wire are used in nearly all electric light equipments as safety valves or cut outs; but the law determining the behaviour of these fuses was little known. Mr. Preece has written two papers for the Royal Society. Profs. Ayrton and Perry introduced the subject in a recent paper read at the Society of Electrical Engineers, and Mr. Cockburn has brought the whole subject before that Society, where it has been well threshed out. For fine wires, viz. those under ·010 in., the fusing current varies with the diameter; but for wires over ·010 in., is given by the equation—

$$C = ad^{3}/_{2}.$$

The constant a has been determined for all metals. The behaviour of tin, which is very commonly used, is peculiar. When it approaches the temperature of fusion, its surface oxidizes and coats the wire with a thin skin, which acquires a higher temperature and allows a greater current to flow before fusing. Mr. Cockburn breaks through this skin with a weight—a pellet of lead; while Mr. Preece prevents the skin forming by covering the wire with shell-lac, which acts as a flux and prevents oxidation.

Major King, U.S.A., has recently made a mammoth electro-magnet out of two Rodman guns, weighing about 60 tons. It was excited by a powerful dynamo, and the armature resisted a pull of nearly 10 tons. The field was felt and watches were stopped at very great distances.

Von Bernardo's system of welding by directing an arc itself along the crack, fissure, or edge of the metal to be welded is attracting great attention on the Continent. Prof. Rühlmann, of Chemnitz, has read a very interesting paper before the Electrotechnical Society of Berlin. A carbon rod is the positive and the metal to be fused the negative pole of the arc. The arc acts like a blow-pipe flame. It is eminently adapted to repair cracks and leaks in boilers, heaters, and condensers, to repair tools and generally to cover the ground of soldering and welding.

A novel mode of forming electrolytically deposited copper tubes is attracting considerable attention. The copper is slowly deposited in a thin coating on an iron mandril kept constantly rotating in the bath. As the copper forms it is pressed by an agate burnisher, which compresses the molecular structure into a hard and solid mass of great tensile strength. Such copper has reached a breaking strain of 40 tons on the square inch. The process is due to Mr. W. Elmore.

A curious experiment is mentioned by the *Electrician* (January 27). A disk of soft iron has a spindle put through it so that it can be spun like a top. When at rest or moving slowly the disk is attracted by the poles of a magnet; but when it turns with sufficient velocity it is repelled by the magnet. The reaction of the induced currents in the mass of the metal is greater than the magnetic attraction.

H. F. Weber has cast doubts on the dull red rays being the first luminous rays to appear. He says that the carbon filaments, platinum, gold, and iron give a "gray glow," which is evident at temperatures much below that of dull red, viz. 525° C. Gold gives this gray effect at 417°, iron at 377°, and platinum at 390°.

THE PROPOSED TEACHING UNIVERSITY FOR LONDON.

The following is the text of the petition which has been drawn up by the Association for Promoting a Teaching University for London:—

To the Queen's Most Excellent Majesty in Council.

The Humble Petition of the Association for Promoting a Teaching University for London

Showeth—

1. That the Association for Promoting a Teaching University for London was formed in 1884, and has enrolled up to the present time about 250 members, each of whom was specially invited to join on the ground of eminence, or of experience in matters affecting University teaching in London, or of being

actively engaged in educational or administrative work in one of the institutions in which such teaching is given.

2. That your petitioners have been engaged for the last three years in examining the state and requirements of University education in London, and in conferring with the persons responsible for the teaching and administration of the institutions in which such education is carried on. They have thus been led to the conclusion that there exists in the metropolis and its suburban districts a general and growing demand for the development of University education. They are convinced that this demand cannot be met while higher education in London remains in its present unorganized state, and while the various institutions giving University instruction are deprived of the means of common discussion and concerted action. For the teaching given in these institutions their respective governing bodies are alone responsible, and each of these bodies for the most part acts in educational matters on the advice of its own teachers; but there is no common centre, such as a University would supply, where these governing bodies and their teachers could meet for purposes of conference, and wherein measures for the better organization of teaching could be discussed and settled. It is matter of experience to those who have taken part in the administration of such institutions that they suffer from the want of public recognition and support—a want due, not to defects in their work, but mainly to the anomaly of their position as institutions performing some of the functions without having the *status* of a University.

3. That the severance from the work of teaching of the work of examination for degrees, and the assignment of such examination to the existing University for London as its sole function, has had an injurious effect upon University education in London. The restraint exercised over efficient institutions through examinations held by a body which is neither responsible for their teaching nor in communication with their teachers acts as a fetter upon education, and gives undue consequence to examinations and their results. Examinations so arranged are less efficient than they might be made as a test of real merit, and tend to encourage dissipation of intellectual energy. In the Faculty of Medicine, although a systematic course of study in a recognized school is now required by the existing University of London, the want of due relations between the examining institution on the one hand and the teaching bodies and professional authorities on the other has led to unsatisfactory results.

4. That the evils above mentioned cannot be fully remedied but by the establishment in London of a Teaching University—that is to say, a University which (1) provides for the student in all the subjects included in its Faculties the best attainable teaching with the necessary aids and appliances; (2) requires a regular course of attendance on such teaching as a preliminary to graduation; and (3) secures to the teacher a direct and adequate representation in its councils, and a due share in its administration.

5. That such a University may be formed without trenching upon the province of the existing University of London, the functions of which are entirely different, and without superseding any institution now giving genuine University instruction in the metropolis. A Teaching University for London would incorporate or associate such institutions without injury to their individual life, as the Victoria University has incorporated Colleges in the North of England.

6. That the metropolis, regarded as the seat of a Teaching University, possesses for students in every Faculty, but especially in the Faculties of Laws and Medicine, advantages which cannot be equalled in any other place in the United Kingdom. Such a University, once established, would supply the motive power for various amendments in the University education of London, which are generally admitted to be needful, such as the greater concentration of the teaching of particular subjects in the earlier scientific stages of medical education; the foundation of additional chairs, attached either to particular institutions or to the University, for the further prosecution of special studies; the promotion of new Faculties; the encouragement of general education as a preliminary to the training for all professions; and, finally, such a presentation to the public of the needs of higher education in London as might secure from the corporate or private munificence of the metropolis the endowments necessary to enable it to keep pace with the growth of population and with the progress of learning.

7. That your petitioners, wishing to promote the foundation of a Teaching University in London, have held conferences with representative London teachers of University rank in the Faculties of Arts, Science, and Medicine, and have submitted to them the following statement of the objects to be aimed at in the foundation of such a University:—

(1) The organization of University teaching in and for London, in the form of a Teaching University, with Faculties of Arts, Science, Laws, and Medicine.

(2) The association of University examinations with University teaching, and the direction of both by the same authorities.

(3) The conferring of a substantive voice in the government of the University upon those engaged in the work of University teaching and examination.

(4) Existing institutions in London of University rank not to be abolished or ignored, but to be taken as the basis or component parts of the University, and either partially or completely incorporated, with the minimum of internal change.

(5) An alliance to be established between the University and the professional societies or corporations, the Council of Legal Education as representing the Inns of Court, the Royal College of Physicians of London, and the Royal College of Surgeons of England.

8. That these conferences have resulted in three reports, each embodying a substantial approval of the objects above stated and of the proposals of your petitioners for the purpose of effecting them.

9. That your petitioners have also held conferences with committees of the Senate and Convocation of the existing University of London, and that, simultaneously with the action taken by your petitioners, and, as they believe, in consequence thereof, the questions at issue have, it is understood, been under the consideration of the Senate and Convocation. Their deliberations have resulted in a report, which has been communicated to your petitioners, and recommends various changes in the constitution of the Senate as the governing body of the University, the establishment of Faculties and Boards of Studies, and the introduction on the governing body of representatives of such Faculties.

10. That such proposals differ from the proposals of your petitioners in the following, among other, respects:—

(1) In not requiring, as a preliminary to graduation in the Faculties of Arts and Science, a regular course of instruction in some recognized teaching institution. Such a course of instruction, however, continues to be required by the existing University as a preliminary to graduation in the Faculty of Medicine.

(2) In admitting Colleges not situated within the London district, the effect of this provision being that the suggested Faculties and Boards of Studies could meet but seldom, and thus would not afford to the teaching institutions of the metropolis adequate means of common discussion and concerted action.

(3) In the absence of any sufficient conditions for securing that the associated Colleges shall be doing effective University work.

(4) In not providing for the direct representation upon the governing body of the associated institutions, or of University teachers.

(5) In granting an unduly large representation to the graduates of the University.

11. That it appears to be difficult, if not impossible, for the University of London, consistently with its relations towards institutions situated elsewhere, and towards private students, to accept modifications which would enable it to fulfil the objects above mentioned.

12. That your petitioners have also held conferences with committees of the Councils of University College, London, and King's College, London, respectively, and have submitted to them the above statement of the objects of the Association. The subject having been subsequently brought by the committees before their respective Councils, these Councils adopted resolutions expressing their approval of the objects above stated, and have since determined to petition Your Majesty to the same general effect as is set forth in this petition.

13. That an alliance between the Teaching University above described and the chief professional societies and corporations of the metropolis, such as the Inns of Court, the Royal College of Physicians of London, and the Royal College of Surgeons of England, would be desirable as securing professional interests in

the arrangements for graduation, and in simplifying and re-arranging examinations in the Faculties of Laws and Medicine. Your petitioners have accordingly opened communications with the above-named bodies regarding this subject. They understand, however, that the Royal College of Physicians and the Royal College of Surgeons are disposed to seek conjointly for independent powers of granting degrees in a Faculty of Medicine. Your petitioners deprecate any severance of the machinery for granting degrees in London from academic influences. Many serious defects of University education in London are due to such a severance.

14. That, with a view to avoid multiplication of bodies conferring a diploma or a licence to practise, it is expedient that the possession of the conjoint diploma of the two Royal Colleges above named should be a preliminary condition for obtaining a medical degree in the University, the conferring of such diploma remaining, as at present, the function of the said Royal Colleges.

15. That the objects above set forth would, in the opinion of your petitioners, be most readily accomplished by the issue of a charter to a body of persons suitably constituted to be the governing body of a new University in and for London; such body to consist of the following persons :—

(1) The Chancellor of the University; the first Chancellor to be appointed by Your Majesty, and named in the charter.

(2) Members to be named by Your Majesty in the charter. Vacancies to be filled by the Lord President.

(3) Members chosen by the governing bodies of University College, London; King's College, London; and such other Colleges as may be associated with the University.

(4) Members chosen by the governing bodies of the professional societies and corporations hereinbefore referred to, if associated with the University.

(5) Members chosen by the professors or teaching staff of associated institutions doing University work, and assembled in the Faculties, whether of Arts, Science, Laws, or Medicine, to which they respectively belong, such members to be in number not less than one-third of the whole governing body.

16. That power should be given to the governing body of the new University to accept the application for association with the University of any teaching institution in the metropolis, the conditions of such association to be—(*a*) that the institution is giving instruction of a University character; (*b*) that it has established a complete curriculum, and possesses a sufficient teaching staff in at least one of the recognized Faculties; (*c*) and that it has furnished proofs of its means and appliances for teaching being established on a satisfactory basis.

Your petitioners therefore humbly pray Your Majesty to be pleased to grant a charter to a body of persons appointed as is described in this petition, or to such other person as Your Majesty may be pleased to select, constituting a University in and for London upon the principles and for the purposes hereinbefore stated, and having power to grant its own degrees in the Faculties of Arts, Science, Laws, and Medicine, and that Your Majesty will be pleased to make such orders in the premises as to Your Majesty, in your Royal wisdom and justice, may seem meet.

And your petitioners will ever pray, &c.

Executive Committee of the Association for Promoting a Teaching University for London:—W. Grylls Adams, M.A., F.R.S., J. W. Cunningham, Sec. King's College, J. Curnow, M.D., F.R.C.P., Sir Dyce Duckworth, M.D., F.R.C.P., G. Carey Foster, B.A., F.R.S., M. Berkeley Hill, M.B., F.R.C.S., W. H. H. Hudson, M.A., LL.M., J. Marshall, LL.D., F.R.S. (Chairman), Norman Moore, M.D., F.R.C.P., H. Morley, LL.D., W. M. Ord, M.D., F.R.C.P., F. Pollock, M.A., R. S. Poole, LL.D., W. J. Russell, Ph.D., F.R.S., T. E. Scrutton, M.A., LL.B., Rev. Henry Wace, D.D., G. C. W. Warr, M.A., A. W. Williamson, LL.D., F.R.S., Gerald F. Yeo, M.D., F.R.C.S., Sir George Young, LL.D.; Secretary, F. C. Montague, M.A., 12 New Court, Carey Street, W.C.

THE TOTAL ECLIPSE OF THE MOON, JANUARY 28.

THE weather on the night of January 28 proved decidedly unfavourable for those astronomers in London and the neighbourhood who had prepared to observe the occultations of small stars by the eclipsed moon. The sky, which had been beautifully clear in the morning, but which had become partially clouded towards evening, had cleared again a little before the commencement of the eclipse, thus raising hopes which were destined to be disappointed, for the clouds returned, and, with the exception of two or three short breaks, the moon was enveloped in cloud more or less dense during the entire duration of the total phase. Very full preparations for the observation of the occultations had been made at the Royal Observatory, Greenwich, but only the observers at the four largest telescopes were able to see even one of the predicted phenomena. At the Cambridge Observatory a similar disheartening experience was recorded, and at Mr. Crossley's Observatory, Halifax, it was quite cloudy, but in the west and south of England, and in Ireland at Dublin and Belfast, the conditions for observing were very favourable. On the Continent, at Vienna no observations could be made, the moon being enveloped in thick haze; at Paris and Berlin the sky had been overcast, and there had been a fall of snow before the eclipse, but the latter half of the eclipse was well observed at the former station, and some good results were obtained at thelatter during a clear interval about a quarter of an hour after the commencement of totality. At Moscow the eclipse was seen in a very clear sky, and at Madrid it was partially clear.

The following table gives the number of observations obtained at those Observatories from which accounts have been received up to the present time :—

Observatory.	Aperture of telescope, inches	No. of Stars observed. Dis.	Reap.
Royal Observatory, Greenwich	24	3	7
,, ,, ,,	12¾	1	4
,, ,, ,,	6	3	1
,, ,, ,,	6½	0	3
Col. Tupman's—Harrow	18	3	2
Mr. Penrose's—Wimbledon	6	3	2
Mr. Brodie's—Fernhill, I. of W.	8½	9	5
Mr. Stothert's—Bath	6	11	13
Cambridge Observatory	12	0	2
Miss Petrie—Bradford	6	1	1
Mr. Backhouse's—Sunderland	—	1	0
Glasgow Observatory	9	6	7
Mr. Heath—Edinburgh	3¼	3	1
Dunsink Observatory	—	17	18

Observatory.	Aperture of telescope, inches	No. of Stars observed.
Stonyhurst Observatory	8	1
,, ,,	5½	12
,, ,,	4	4

The 8-inch refractor at Stonyhurst was devoted to spectroscopic observations during the greater part of the eclipse. It had been in the programme to make similar observations at Greenwich had the night proved favourable, and also to take a series of photographs showing the progress of the eclipse. Three photographs were secured, but the clouds prevented all spectroscopic work. Dr. Copeland also at Dun Echt had intended to make spectroscopic observations, but was almost completely thwarted by snow-squalls.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 19.—"On the Secondary Carpals, Metacarpals, and Digital Rays in the Wings of existing Carinate Birds." By W. K. Parker, F.R.S.

In a paper "On the Morphology of Birds," already sent in to the Royal Society, but not yet published, I have described certain additional parts in the wings of Gallinaceous birds.

One of these lies on the radial side of the first metacarpal; the other two are on the ulnar side of the second and third metacarpals.

These parts, which at first caused me considerable surprise, being wholly unexpected by me, are only part of what I have since found in other families.

During the past year I have worked out the development of the skeleton in the Duck tribe ("Anatidæ"), in the Auk tribe ("Alcidæ"), and in the Gull tribe ("Laridæ"), and to some degree in some other families. The subject appears to me to be of great interest, and I have, through various English and American friends, obtained many scores of embryos and young birds, &c., that I may be able to trace these parts in every main group of the class. Normally, both the existing Carinatæ and Ratitæ, and such extinct forms as have been worked out—*Archæopteryx*, *Hesperornis*, *Ichthyornis*—show that the primary form of the bird's wing is simply *tri-digitate*. In this I agree with Baur, who has helped me greatly in this matter, both by his valuable papers and also by personal discussion with me.

The normal "manus" of a Carinate bird contains two permanently distinct carpals: three carpals that lose their independence by anchylosis with the metacarpals, and three digital rays extending from the three fused metacarpals.

In some birds, *e.g.* the Passerinæ, the *pollex* of the first digit has only one phalanx attached to its short metacarpal, the second only two, and the third only one, phalanx. In others, Plovers, Gulls, Cormorants, &c., an additional or *ungual* phalanx is found on the first and second digit; and in some birds, *e.g. Numenius*, during their embryonic state, a small semi-distinct nucleus is seen on the end of the aborted phalanx of the third digit.

In my as yet unpublished paper I have mentioned a sub-distinct tract of very solid fibro-cartilage, which evidently corresponds with what has been called "præ-pollex" by Kehrer and others.[1]

I am satisfied, now, that this very notable part is the remnant of the skeleton of the *spur*, so remarkably developed in the Palamedidæ, certain Geese, Plovers, and Jacanas.

This part therefore need not interfere with the consideration of the *true secondary digital* parts.

Among the last communications received by me from Dr. Baur, I find in print what I had already learned from him orally.

In some "General Notes" published in the *American Naturalist*, September 1887, p. 839, I find the following paragraph:—"The oldest Ichthyopterygia had few phalanges and not more than five digits; the radius and ulna were longer than broad, and separated by a space. Later, through the adaptation to the water, more phalanges were developed, more digits appeared, mostly by division of the former, or by new formation on the ulnar side. I have never found a new digit developed on the radial side."

These are most important facts, some of which—namely, the bifurcation of the digital rays—I had received some light upon before, both from Dr. Gadow and from Prof. D'Arcy W. Thompson.[2]

I find that the *carpus*, *metacarpus*, and digital rays are all apt to increase in number beyond what is normal.

Long ago I found, in one of the Palamedidæ, *e.g. Chauna chavaria*, *two ulnar carpals*, apparently an "ulnare" proper, and "centrale." More recently, in the embryo of a more normal Chenomorph—the Falkland Island Goose (*Chloëphaga poliocephala*)—I found the ulnare nearly divided into two segments.

On the other side of the carpus in an embryo Kestrel (*Falco tinnunculus*) and in a young Sparrow-hawk (*Accipiter nisus*), I found a "radiale" in two pieces, the outer of which in the latter was degenerating into the large "*os prominens*" which is found in the tendon of the "tensor patagii" muscle of rapacious birds.

In the embryos of Gulls, Auks, Guillemots, &c., the large "distal carpal" of the index or second digit sends forward a long wedge of cartilage towards an additional metacarpal nucleus. Evidently this is the rudiment of another carpal seeking to be attached to its own intercalary metacarpal.

Further on, on the large second digit, the flat dilated part of the proximal phalanx, on its ulnar side, also, is developed from a distinct tract of true cartilage, but soon loses its independence; it forms the plate on which some of the *primary quills* are fixed.

Still further on, on the ulnar side, near the small well-developed ungual phalanx of the embryo, but later, after hatching, a small oval cartilage appears, and *is ossified independently*.

A similar tract of cartilage is formed on the pollex or first digit, also, but is somewhat smaller than that on the second; it is on the ulnar side and near the ungual phalanx.

In the feeble third digit I only find a rudimentary secondary metacarpal, on the ulnar side; this is very constant throughout the *Carinatæ*; and sometimes, as I have already mentioned, there is a small rudiment of a second phalanx on that digit which, in the Lizard, has *four* phalanges.[3]

[1] "Beiträge zur Kentniss des Carpus und Tarsus der Amphibien, Reptilien, und Saüger," *Berichte der Naturforschenden Gesellschaft zu Freiburg i. B.*, vol. i. 1886 (Heft 4 und Taf. 4).

[2] See his paper on the hind limbs of Ichthyosaurus, &c., *Journ. Anat. Physiol.*, vol. xx. pp. 1–4 (reprint).

[3] The figures of these parts, and also of the rest of the developing skeleton in these birds—Ducks, Auks, Guillemots, &c.—are ready for publication.

In seeking for evidence of the manner in which these high and noble hot-blooded feathered forms arose from among the Archaic Reptilia, I think that something has been gained in what I have stated above.

The skull brings evidence of the same sort during its development, and it is to ancient long-beaked forms, and not to modern short-faced types of Reptilia, that we must look for any near relationship of the Reptiles in the Birds.

In the Guillemot (*Uria troile*) I have satisfied myself that there has been a considerable amount of *secular* shortening of the beak (rostrum and fore-part of mandibles), and if we look at Dr. Marsh's figures of *Hesperornis* and *Ichthyornis* we shall see what long bills these toothed birds possessed.

But there is no part of a developing bird's skeleton that is not rich with suggestive facts of this kind, as I propose to show in due time.

January 26.—"On the Emigration of Amœboid Corpuscles in the Star-fish (*Asterias rubens*)." By Herbert E. Durham, B.A., lately Vintner Exhibitioner, King's College, Cambridge. Communicated by Dr. P. H. Carpenter, F.R.S., F.L.S.

When small particles (*e.g.* Indian ink) are introduced into the body cavity of a specimen of *Asterias rubens*, they are soon ingested by the amœboid corpuscles of the cœlomic fluid; the latter are carried in various directions by the currents set up by the cilia of the cœlomic epithelium. In the dermal branchiæ these granule-laden corpuscles were observed to adhere to the wall of the branchia, and migrate by amœboid movement to the exterior. [Where such migration is proceeding very actively, a perforation filled by a plug of the corpuscles is formed.] Arrived at the exterior, the corpuscles retain an irregular shape for a while, then they become spherical, swell up, and disintegrate.

Besides corpuscles thus laden with foreign granules, corpuscles containing refringent sphærules (sphæruliferous corpuscles, "Plasma-Wanderzellen") were observed in the extruded material: emigration of such corpuscles was also noted to take place in specimens kept in captivity in glass vessels. Hamann's observation that "Plasma-Wanderzellen" occur in the branchiæ of Echinids helps to confirm the view that this a normal process: further observations are necessary to elucidate its significance. [Dr. Hartog's statements as regards the outward current in the water-tube (stone-canal), were confirmed by the presence of corpuscles in the pore-canals of the madreporite.]

With regard to the other point, it seems clear that minute foreign bodies introduced into the system of a star-fish can be, and are, got rid of by scavenging corpuscles.

"Note on the Madreporite of *Cribrella ocellata*." By the same.

The dogma laid down by Ludwig is that the cavity of the water-vascular system is isolated from other cavities. In a series of sections carried through the madreporite, &c., of *Cribrella ocellata*, it was seen that a few pore-canals of the madreporic plate open directly into the "Schlauchförmiger Kanal"; the ampulla into which the water-tube (stone-canal) dilates being also connected by an opening with the "Schlauchförmiger Kanal": this latter space being enterocœlic in origin, it is interesting to compare the arrangement in Crinoids.

Zoological Society, January 17.—Dr. A. Günther, F.R.S., Vice-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 December 1887, and called attention to a small Fox from Afghanistan, presented by Lieut.-Col. Sir O. B. C. St. John, which should probably be referred to the species shortly noticed by Blyth as *Vulpes griffithi*. It was, however, somewhat doubtful whether the species was really distinct from *Vulpes leucopus*, Blyth, the small Desert Fox of Western India.—Mr. Francis Day exhibited and made remarks on some hybrid fishes from Howietoun, and on a British specimen of the Spined Loche.—Mr. Oldfield Thomas read a report on a collection of Mammals obtained by Emin Pasha in Central Africa, and presented by him to the Natural History Museum. The collection contained 115 specimens belonging to 39 species. The great mass of the collection had been obtained in a district called Monbuttu, just within the Congo Basin. A new Flying Squirrel, of small size, was named *Anomulurus pusillus*, and a new Tree-Hyrax, *Dendrohyrax emini*, after its discoverer.—Capt. G. E. Shelley read a paper on a collection of birds made by Emin Pasha in Equatorial Africa. The series had been formed partly in the Upper Nile district and partly in the Monbuttu country in the

Congo Basin, and contained examples of four species new to science, proposed to be called *Indicator emini*, *Spermospiza ruficapilla*, *Ploceus castanops*, and *Glareola emini*.—Dr. A. Günther, F.R.S., read a report on a collection of Reptiles and Batrachians from Monbuttu, sent by Emin Pasha. The author enumerated seventeen specimens, of which nine were almost generally distributed over the African region; of the remainder, seven were known from various parts of West Africa. One Tree-Snake was described as new, and called, after its discoverer, *Ahætulla emini*.—Mr. Edgar A. Smith, read an account of the Shells collected by Dr. Emin Pasha on the Albert Nyanza, Central Africa. Of the five species of which examples were obtained, three were referred to new species. It was stated that fifteen species of shells were now known from Lake Albert, of which seven were peculiar to it.—Mr. Arthur G. Butler gave an account of the Lepidoptera received from Dr. Emin Pasha. The collection contained examples of 155 species, of which thirteen Butterflies, and two Moths were new to science.—A communication was read from Mr. Charles O. Waterhouse, containing an account of the Coleoptera from Eastern Equatorial Africa received from Emin Pasha. One of the species was new to science, and six of them had previously been received at the British Museum from West Africa only.

Geological Society, January 11.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—On the law that governs the action of flowing streams, by R. D. Oldham.—Supplementary notes on the stratigraphy of the Bagshot Beds of the London Basin, by the Rev. A. Irving. This paper contained the results of field-work during the year 1887. Additional notes on the stratigraphy of the Bracknell and Ascot Hills were given, justifying the reading of the country as shown in Figs. 1 and 2 of the author's last paper (Q.J.G.S., August 1887), the examination of this line of country having been extended as far as Englefield Green. Sections of the beds of the Middle Group as they crop out at Cæsar's Camp, Swinley Park, Ascot, and Sunningdale, were described and correlated with the 76 feet of beds which constitute that group in the Well-section at Wellington College. The stratigraphy of the hills known as Finchampstead Ridges has been worked out from numerous sections on their flanks; and the strata of the Bearwood Hills were correlated directly with them. All along the northern margin a general attenuation of (*a*) the Lower (fluviatile) Sands, and of (*b*) the Middle (green earthy) Sands was shown to occur, and in some places on the northern margin they are found to have entirely thinned away, admitting of distinct overlap at more than one horizon. The second part of the paper dealt with the Highclere district, where the author believes he has established the full succession of the three stages of the Bagshot formation, a section being given across the valley south of Highclere Station, showing the succession of the whole Eocene series (with the *Ostrea bellovacina* bed for its base) as it is developed there. Some important conclusions were drawn as to the Tertiary physiography of the South of England; and the revised tabulation of the Tertiaries put forward by Prof. Prestwich at the Society's last meeting was referred to as supporting some of the main points for which the author has contended. The reading of this paper was followed by a discussion in which the President, Mr. Monckton, Mr. Herries, and Mr. Drew took part.—The red-rock series of the Devon coast section, by the Rev. A. Irving.

Chemical Society, January 19.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—Morindon, by T. E. Thorpe, F.R.S., and W. J. Smith. Morindin, the active colouring-matter of A'l, the root-bark of *Morinda citrifolia*, yields 48·4 per cent. of morindon on hydrolysis. This latter substance is a methylanthracene-derivative of the composition $C_{15}H_{10}O_5$, and differs from all of the eight known compounds of the same formula.—Manganese trioxide, by T. E. Thorpe, F.R.S., and F. J. Hambly. The authors have repeated Franke's experiments (*Journ. für prakt. Chem.*, 1887) on the so-called volatile oxides of manganese, and have been unable to obtain any evidence of the existence of the blue gaseous manganese tetroxide. They find, however, that manganese trioxide exists, and can be formed by the action of a solution of potassium permanganate in sulphuric acid on dry sodium carbonate.—Note on Chatard's process for the estimation of small quantities of manganese, by the same.—Contributions to the theory of the vitriol-chamber process, by G. Lunge. The theory has been recently advanced by Raschig (*Liebig's Annalen*, 241, 161) that the vitriol-chamber process consists in the formation in the first instance from nitrous acid and sulphurous acid of dihydroxylaminesulphonic acid, which, being acted on by nitrous acid, yields sulphuric acid and nitric oxide, the latter being reconverted into nitrous acid. This theory is regarded by the author as untenable on all points, since it completely ignores the existence of nitrosyl sulphate (chamber-crystals), whilst nitric oxide, oxygen in excess, and water do not yield nitrous acid, but nitric acid. In the author's view it is not NO, but N_2O_3 which acts as a carrier of the oxygen in the vitriol-chamber, and the principal reactions are: $2SO_2 + N_2O_3 + O_2 + H_2O = 2SO_2(OH)(ONO)$; $2SO_2(OH)(ONO) + H_2O = 2SO_2(OH)_2 + N_2O_3$. Much NO is present in the front chambers along with N_2O_3; it is formed by a secondary reaction from nitrosyl sulphate, $2SO_2(OH)(ONO) + SO_2 + 2H_2O = 3SOH_2SO_4 + 2NO$, and is principally absorbed by the direct reaction, $4SO_2 + 4NO + 3O_2 + 2H_2O = 4SO_2(OH)(ONO)$; none of it can pass into NO_2 (which does not occur at all in normally working chambers), but some of it may pass into HNO_3, which is at once acted on by SO_2;—$SO + HNO_3 = SO_2(OH)(ONO)$. Thus the normal vitriol-chamber process is not as hitherto understood an alteration of reductions and oxidations, but it is a *condensation* of nitrous acid, or of NO with SO_2 and O_2 to nitrosyl sulphate, and a splitting up of the latter into N_2O_3 and sulphuric acid.

EDINBURGH.

Royal Society, January 6.—Sir W. Thomson, President, in the chair.—Mr. J. T. Bottomley described and exhibited a practical constant-volume air thermometer. This instrument has been designed by Mr. Bottomley so as to be more sensitive, more accurate, and, at the same time, of much greater range than air thermometers hitherto in use. Mr. Bottomley also exhibited some glass globes with internal cavities produced by cooling.—Prof. Tait communicated a paper by Dr. G. Plarr on the roots of $\epsilon^2 = -1$; and a paper by Prof. Burnside on a simplified proof of Maxwell's theorem.—Prof. Tait also read a paper on the Thomson effect in iron.—Dr. Thomas Muir read a paper on vanishing aggregates of determinants. He has obtained the general theorem of which a particular case was discovered lately by Kronecker, and attracted much attention in Germany.—Prof. Crum Brown communicated a paper by Dr. Griffiths on the Malpighian tubes and the "hepatic cells" of the *araneina* and the diverticula of the *asteridea*.

PARIS.

Academy of Sciences, January 23.—M. Janssen, President, in the chair.—Remarks in reference to M. J. Bertrand's recent note on the law of probability of error, by M. F. Tisserand. A solution is given of the problem, "To determine the function $\psi(x_1-x_2, x_1-x_3, \ldots . x_1-x_n)$, where $x_1, x_2, \ldots . x_n$ indicate n arbitrary quantities independent one of the other, in such a way that this function is symmetrical in relation to $x_1, x_2, \ldots . x_n$."—The paper is followed by a communication from M. F. Tisserand on the law of probability as applied to target-firing.—On some notions, principles, and formulas, which come into play in several questions connected with algebraic curves and surfaces, by M. de Jonquières. A rapid summary is given of these principles, &c., some of which have been established by the author himself, some by other mathematicians.—Note on the second volume of the "Annales de l'Observatoire de Bordeaux," by M. M. Lœwy. This volume is largely occupied with the important observations undertaken for the purpose of revising the positions of the stars in Argelander-Oeltzen's catalogue. It contains the precise co-ordinates of about 3500 stars belonging to the southernmost region of the northern hemisphere.—Contributions to the history of the problematical organisms of old marine basins, by M. Stanislas Meunier. The paper deals with the so-called Bilobites, regarded by some palæontologists as mere physical tracings, by others as real organic remains. Several arguments are advanced in favour of the latter opinion, which is regarded as fairly well established, although not yet rigorously demonstrated.—On the rapidity with which the report of fire-arms is propagated, by M. Journée. All the facts here described tend to show that a projectile possessing a greater velocity than that of the report produces, during its passage through the air, a continuous sound analogous to the explosion of gunpowder.—On the mean distances of the planets from the sun, by M. Roger. It is shown that, apart from certain deviations within a defined limit, these distances form a geometric progression modified by a periodic irregularity. In a future communication the connection will be pointed out between this law and the theoretic views advanced by the author on the formation of the planetary system.—Summary of the solar observations made at Rome during the last quarter of the year 1887, by M. P. Tacchini. The diminution of spots already noted in September was continued during the two ensuing months, so that the mean was even less than in the previous quarter. The protuberances also were less frequent.—On the phases of Jupiter, by Dom E. Siffert. Most of these observations, which were taken at the Observatory of Grignon, are tabulated for the period from March to December, 1885.—On the application of the Cremonian quadratic substitutions to the integration of the differential equation of the first order, by M. Léon Autonne. In this paper the author develops, for the integration of the differential equation of the first order, the method based on the employment of the Cremonian quadratic substitutions, and applies this method to some special cases of a simple and comprehensive character.—Electric solution of algebraic equations, by M. Félix Lucas. It is shown how, by means of electricity, the solution of equations of any degree p, whose real or imaginary coefficients are given numerically, may be reduced to that of equations of degrees lower than p.—Action of vanadic acid on the alkaline fluorides, by M. A. Ditte. The present paper deals with the fluorides of sodium and ammonium, whose composition is shown to be analogous to that of the fluoride of potassium.—Action of hydrochloric acid on cupric chloride, by M. Engel. The results are tabulated of a series of experiments with the hydrochlorate of cupric chloride. In this substance the chloride of copper appears to be in the anhydrous state, all the water being ultimately combined with the hydrochloric acid.—On the alcoholic fermentation of galactose, by M. Em. Bourquelot. From these experiments, undertaken to determine the true character of the action of the yeast of beer on galactose, the author concludes that pure galactose does not ferment in the presence of the yeast at 15° to 16° C., but that it undergoes alcoholic fermentation when glucose, lævulose, or maltose are added.—On two new genera of Epicarides, by MM. A. Giard and J. Bonnier. The specimens here described live in the fresh waters of the Dutch East Indies, and are regarded as the types of two new genera, Probopyrus and Palegyge, whence are respectively derived the genera Bopyrus and Gyge. They are here named *Probopyrus ascendens*, Semper, and *Palegyge borrei*, G. and B.

BERLIN.

Physiological Society, January 13.—Prof. du Bois Reymond, President, in the chair.—Prof. Fritsch described the detent-joint of a Sheat-fish (Siluridæ). In this fish, as found in the Nile, the adjusting and fixing of the dorsal and pectoral fins is provided for by the various shape and arrangement of the surfaces of the joints, which take the form of hooks and detents. The speaker explained the above arrangements by means of drawings and preparations, by means of which it was easily seen that when once the dorsal spine is fixed it will withstand a very considerable force. These spines constitute a protective mechanism against other predatory fishes, and accounts for the numerical development of these fishes in the Nile. The speaker stated his inability to accept Sörensen's view that the detent-joints of these fishes are organs for the production of sound.—Dr. Joseph had studied the minute structure of the axis-cylinder in the nerves of the electric organ of *Torpedo marmorata* treating them with osmic acid and various staining reagents. By making a careful series of transverse sections he has become convinced that not only the medullary sheath, but also the axis cylinder, possesses a fan-like structure, and that the longitudinal fibrils run in the meshes of the radiating fibres, and are the true conducting tissue of the nerve. The diameter of the axis is six or seven times as great as that of the sheath.—Dr. Weyl had subjected silk to a thoroughly chemical examination, and obtained values for its percentage composition, after purification by treatment with caustic soda, which corresponded with those given twenty-five years ago by Cramer; according to these, silk may be taken as belonging to the proteid class of bodies. Raw silk, and to a greater degree that which has been purified by soda, is soluble in fuming hydrochloric acid; if this solution is poured into alcohol, a white cloudiness is produced, which speedily increases in intensity, and on cooling gives rise to a solid white gelatinous mass. The percentage composition of this new substance obtained from silk, and called by the speaker seroin, is, as regards its carbon and hydrogen, the same as that of silk, but it contains less nitrogen. It possessed in all cases the same composition, so that it is undoubtedly a distinctly characterized chemical substance, and is neither pure silk nor some closely related proteid formed by a splitting-off of ammonia. When treated with dilute acids, seroin yields the same products of decomposition as does fibroin—namely, large quantities of leucin and tyrosin, by which it is characterized as being a proteid. Dr. Weyl hoped shortly to resume this investigation in the direction of a general consideration of the proteid group.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Voltaic Electricity: T. P. Treglohan (Longmans).—Practical Physics for Schools and the Junior Students of Colleges, vol. i. Electricity and Magnetism: Stewart and Gee (Macmillan).—Behind the Tides: C. B. Radcliffe (Macmillan).—Pflanzen-Teratologie: M. T. Masters; German by U. Dammer (Leipzig).—Practical Amateur Photography: C. C. Vevers.

CONTENTS.

PAGE