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THURSDAY, JULY 25, 1872

THE LAST ATTACK ON DARWINISM*

THIS volume, which in bulk, general appearance, and typography bears a close resemblance to the earlier editions of the "Origin of Species," seems got up to stand by its side on the bookshelf, an ever ready antidote to the pernicious doctrines of Mr. Darwin and his supporters. After a careful perusal we must confess that it may seriously damage Mr. Darwin's reputation with those who have never read his works; but we are quite sure that no one who has studied the "Origin of Species," and been convinced of the general accuracy of its statements and conclusions, will have their convictions at all shaken by Dr. Bree's argument. As, however, it is just the work to be read by those who have only a second-hand knowledge of Mr. Darwin's works, we feel it to be a duty to call attention to the very careless manner in which the book is written,—its numerous errors, misrepresentations, and misconceptions, and its extensive use of declamation and opinion as sufficient answers to Mr. Darwin's elaborate observations, carefully selected facts, and cautious inductions.

In a work of purely adverse criticism, the first duty of an author is to quote his opponent's words with scrupulous accuracy. Yet, in the very first page of his book, Dr. Bree misquotes Dr. Hooker; at p. 3 and again at p. 9 he repeats this misquotation; and he devotes eight pages to proving that what Dr. Hooker did not say is erroneous. The quotation is from the Presidential Address at Norwich. The words actually used, and to be found in the authoritative report, are:—"So far from Natural Selection being a thing of the past, it is an accepted doctrine with *almost* every philosophical naturalist; including, it will always be understood, a considerable proportion who are not prepared to assent that it accounts for all that Mr. Darwin assigns to it." Dr. Bree omits the word *almost*, and then sets himself to convict Dr. Hooker of misrepresentation, by showing that with *some* "philosophical naturalists" it is not an accepted doctrine.

On p. 2 Dr. Bree makes a misstatement, almost equally glaring, of another author's view. He says, "And Mr. St. George Mivart has proved, and I think incontestably, that it (Natural Selection) *has not a basis of truth;*" and refers the reader to "Genesis of Species," 1871. But in this volume we find (at p. 5) the author's statement, that the object of his book is "to maintain the position that 'Natural Selection' *acts, and, indeed, must act*, but that still, in order that we may be able to account for the production of known kinds of animals and plants, it requires to be supplemented by the action of some other natural law or laws yet to be discovered."

A little further on Dr. Bree discusses Herbert Spencer's "First Principles;" and how far he is likely to elucidate that philosopher's views may be seen by the following curious blunder. At p. 48 he tries to explain to his readers what Spencer means by "the integration of matter," and quotes the following passage from his "First Principles:"—

"Every mass, from a grain of sand to a planet, radiates heat to other masses, and absorbs heat radiated by other masses; and in so far as it does the one it becomes integrated, while in so far as it does the other it becomes disintegrated."

Dr. Bree appears to have been afraid that his readers would hardly be of sufficient mental calibre to comprehend this passage. He therefore elucidates it as follows: "Integration of matter, therefore, is the absorption of heat, and heat, we are told by Tyndall, endorsed by Spencer, is 'tremulous motion'—therefore, integration of matter is the absorption of motion." We think Dr. Bree has hardly done justice to his readers by merely turning Spencer's statement topsy-turvy, and showing them that a "good rule will work both ways;" he should further have illustrated the subject by what that philosopher terms a concrete example, and explained that, in his view, water is *integrated*, when, by absorbing heat, it changes into steam, and *disintegrated* when by radiating heat it becomes solid ice!

If the supposed fallacies of such men as Hooker and Spencer, who, in Dr. Bree's opinion, are mere satellites of Darwin, are thus ruthlessly exposed, we can hardly expect the chief conspirator himself to receive much mercy. In his "Animals and Plants under Domestication," vol. ii. pp. 250-255, Mr. Darwin carefully discusses the various views as to the causes of variability, and arrives at the conclusion that variability is *not* an ultimate fact necessarily contingent on reproduction (p. 253), and that variability of every kind is directly or indirectly caused by changed conditions of life (p. 255). Dr. Bree, however, referring to the same chapter of the same work, gives his view of the writer's meaning in the following passage:—"But Mr. Darwin goes further. He says there is an inherent tendency in the constitution of the organism to vary, independent of, but modified by, its conditions." At p. 191 Dr. Bree states, as if on Mr. Darwin's authority, "that tortoise-shell cats are so coloured as a rule only in the males;" and at p. 192, that Mr. Darwin "does not believe" exactly what Mr. Darwin says he does believe. But these are only errors of the pen in the haste of argumentative composition; a less excusable mistake is made at p. 212, where, after quoting a passage from Mr. Darwin about mimicry, Dr. Bree says:—"This passage implies that an insect can imitate the organisation of another insect, by means of a knowledge that such organisation is safer from enemies than that in which nature had clothed it. A more unsound, unphilosophical, unproved, reckless statement is not to be found, &c. &c. . . . It is only just to say that the above theory did not originate with Mr. Darwin. It is the *sole production* of the fertile brain of *Mr. Wallace.*" Here we have a misrepresentation and a misstatement. No expression of Mr. Darwin or myself can be taken to mean that we believed in a voluntary knowing imitation of the organisation of one insect by another. In my article on "Mimicry" I have expressly disclaimed this view. As to the latter part of the quotation, the first words of Mr. Darwin's paragraph headed "Mimicry," and which Dr. Bree must have had before his eyes, are:—"This principle was first made clear in an admirable paper by *Mr. Bates!*" A little farther on, my

* "An Exposition of Fallacies in the Hypothesis of Mr. Darwin." By C. R. Bree, M.D., F.Z.S, Senior Physician to the Essex and Colchester Hospital. (London: Longmans, Green, and Co., 1872.)

theory of birds' nests and the colour of female birds is noticed with strong disapproval; and a crushing array of facts is adduced as being opposed to my statement that "when both sexes are coloured in a strikingly conspicuous manner the nest is of such a nature as to conceal the sitting bird." The whitethroat, thrush, snipe, skylark, and hedge-sparrow are adduced as opposed to my views; but as they must all be coloured in a *strikingly conspicuous* manner if they are to be of any use to Dr. Bree or his hypothetical schoolboy, the reason why they are cited remains a mystery to me. Two pages farther on we have more misquotations or blunders. At p. 229 we are told that Nitzsch's "feather tracts" are those parts of the body which have the skin uncovered! while at p. 230 we find that it is the brilliant rays *absorbed* by feathers that produce the vivid, varied, and beautiful colouring of birds! At p. 259 it is stated that "inconceivably minute changes" are alone utilised by natural selection--a misrepresentation which no word of mine or Mr. Darwin's will justify. At p. 261 we have this passage:—"Mr. Wallace adopts Mr. Darwin's view, that there is no such thing as instinct at all, in the sense in which we understand the word. He considers it the 'result of small contingent consequences, as produced by natural selection.'" As the "he" in this sentence appears to refer to Mr. Wallace, and the last ten words are given as a quotation, I felt rather ashamed of myself for writing what I could not the least understand. But a careful examination of my paper shows me that I have said nothing about the "result of contingent consequences;" neither can I find anything of the kind in Mr. Darwin's writings on "instinct." We must pass it over, therefore, as one of the ingenious paraphrases by which Dr. Bree endeavours to elucidate a difficult subject.

In a large folded frontispiece we have "The Descent of Man, after Darwin's Theory," and this is explained at p. 325; but here, too, Mr. Darwin has not been read aright, for "man's ancient ancestor, with cocked ears, tail, prehensile feet, both sexes bearded and hirsute, males with great canine teeth," is placed between Marsupials and Lemurs, whereas Darwin places it after the origin of the catarrhine monkeys, in fact, at Fig. 15 of Dr. Bree's diagram. Our author makes a great point of this, and says:—"From such a Darwinian creation were descended the lowest of the quadrumana, the lemurs."

At p. 331 we have another of our author's enigmatical sentences:—"If an optician makes an object-glass, he does so in reference to the objective, the lens." I had previously imagined that the objective *was* the object-glass; but at p. 351 I was still more puzzled by reference to the "final law of the pendulum" and the "final law of the screw"—two things I had never before heard of.

We think we have now shown that this book contains so large a number of errors, misrepresentations, and misconceptions as to render it quite untrustworthy. We proceed to give a few instances of the author's copious use of declamation, assertion, and opinion, of doubtful facts and illogical arguments.

Of declamation and assertion we have an abundance, the following being a favourable specimen:—"The system of Darwin is eminently illogical, and must fall. It is an hypothesis which draws large but unsound deductions from the rare and abnormal deviations, leaving the real field untouched and unexplored. It is founded upon the exceptions, not the rules of nature. It is utterly opposed to design, to the teachings of animal mechanics, to the grand and beautiful and everlasting proofs upon which the teleologist loves to dwell. It is a cold, unsound, unphilosophic, degrading system of assumed probabilities, which, if true, would be ten times more wonderful than anything assumed or believed by the most strict and rigid disciple of special creation. Nay, still further, if proved in every point to be true, it would still leave the fact of special creation in all its wonderful mystery. The organic cannot be formed from the inorganic; nor could the organic, if it were so formed, be endowed by any physical force with the laws and properties of life. Go on still in speculation, and I ask, Whence the inorganic—its beginning, its ending, its grand and inexplicable laws, which the physicist in vain attempts to correlate with the vital? Whence gravitation, and what? the sidereal system and its movements? the Spirit that breathes through illimitable space, and lives through an eternity of time?"

A large portion of the volume is occupied with quotations from Agassiz, Haughton, Flourens, Owen, and other opponents of Darwinism; and Dr. Bree complains that these authors have hardly been noticed and not replied to by Darwin or his supporters. But the reason of this is explained in the pages of the present work (where we may suppose their best passages are quoted), by the almost entire absence of argument directed to the essential points of Mr. Darwin's theories, and the immense preponderance of loose assertions, in support of which no evidence is given. Thus, Agassiz asserts that "the differences" among domestic animals "are not of the same kind as the differences we observe among wild animals;" that "the differences we observe among wild animals are the result of a *creative power*;" that "domestication *never* produces forms which are self-perpetuating;" that "*at all times* the world has been inhabited by as great a diversity of animals as exists now;" and other similar assertions, almost all of which are controverted by accumulated facts in the works of Mr. Darwin. Chapter xxviii. is entirely devoted to an account of Agassiz's views of design, and supposed *proofs* of a creative mind at work in every step of the development of a group of animals. The facts will appear to most naturalists thoroughly consistent with the theory of evolution and that of natural selection; while the arguments involve a view of the Deity which most philosophical thinkers will find it hard to accept. Agassiz compares the Creator to an engineer, and speaks of Him as "*constantly* and *thoughtfully working* among the *complicated structures* that He has made." This idea is not that of an *omniscient* Creator, but of some inferior being with an intellect vastly superior to man's, but yet limited. "*Thoughtfully working*" implies effort to understand and overcome difficulties; whereas *thought* at all, as we think, is utterly opposed to the conception of omniscience.

Another chapter is devoted to Prof. Haughton's theory of "Least Action in Nature;" and here, again, all the established facts are perfectly consistent with, and almost necessary deductions from, evolution and natural selec-

tion. But it is the mere wide general assertions which Dr. Bree quotes with greatest approval as destructive of Darwinism. Thus: "There is no evidence in nature of birds with imperfect wings; no proof of a succession of blunders before perfection was attained. All is perfect, and all was always perfect." And again: "In every arrangement of bones, muscles, joints, and parts of animals, the motion *must* be such as it would be on the hypothesis that the muscles were a living, intelligent thing, trying to save itself trouble." This last may be true, but it is certainly not necessarily true; and as to imperfect wings, what are those of the Cassowary and Apteryx, which have no known function whatever?

The article of M. Flourens against Darwin is given in an appendix, and his facts as to the crossing of quadrupeds are said to be absolutely fatal to the whole theory of natural selection. But these facts are of a very imperfect and scanty character, and are almost wholly negative; and they are fully noticed in Mr. Darwin's elaborate discussion of the difficult question of hybridity, although Dr. Bree assures his readers that these facts were "never contradicted or *even noticed by Mr. Darwin!*" Under the heading "Flourens," in the index to "Animals and Plants under Domestication," are four references, and the works, "Longévité Humaine" and "De l'Instinct," are referred to; while Dr. Bree himself seems to be unaware of the existence of anything but the "Criticism on Darwin," which has been long ago most admirably answered by Prof. Huxley.

We will now give a few examples of the facts and arguments adduced by Dr. Bree himself. At p. 90, he tells us that Mr. Darwin "has given figures of different sized skulls and jaw-bones, scapulæ and clavicles (of pigeons), differing just as much from each other as the same bones in different sized Englishmen would do; and nothing more!" And on the next page he assures us that a Colchester pigeon-fancier told him, that if he allowed his short-beaked tumblers to fly out of doors they would revert to a state of nature, and that, *in a few weeks*, the beautiful small beaks would be as long and as coarse as those of any other bird! On which Dr. Bree triumphantly remarks—"Of course they would." At p. 131 he tells us, that although young song birds will learn other birds' notes with which they may be associated, yet *if kept quite alone* they will sing their own natural song, "*as several who have tried the experiment assure me.*" This is directly opposed to the experiments on this very point of Daines Barrington, quoted by me in "Contributions to the Theory of Natural Selection," 2nd Ed. p. 221, and it would therefore have been a valuable contribution to our knowledge of this difficult subject if the experiments alluded to had been given in detail, not vaguely referred to. At p. 143 it is stated that the bees' cell "is one of the finest examples in nature of what is termed the principle of 'least action;' that is to say, the greatest amount of space is gained by the least amount of material." This is certainly not true, for the cell being suspended from *the top* and *equally thick throughout*, must be too strong at bottom if strong enough at top. There is therefore waste of material. This objection was published nine years ago, in the "Annals of Natural History" for October 1863, and it has never been answered.

On the imperfection of the geological record Dr. Bree is very strong. He says that Mr. Darwin "asks us to imagine that an ape-like man became evolved in the lower tertiaries, the remains of which or of his descendants have never been discovered. Such a demand upon the credulity of mankind was never, I believe, before seriously made, unless we were told that geese were transmuted barnacles" (p. 180). This is, of course, a sufficient answer to Sir Charles Lyell's careful investigation of the subject, and especially to his most suggestive table of old fossil mammals, given in the twentieth chapter of his "Elements of Geology."

Mr. Mivart and Prof. Owen are both applauded so far as they oppose Darwin, but as both of them believe in some form of development, they are, in Dr. Bree's opinion, almost equally involved in error. Mr. Mivart's doctrine of evolution, he thinks, cannot stand, and "looks too much like Mr. Tegetmeir's pigeons, made to order." It is, however, no doubt offered with the best intentions, "as a means of reconciling scientific and religious thought,"—"two lines which, Mr. Spencer remarks, are running parallel *and* gradually approaching each other!" (We doubt the accuracy of this quotation from Mr. Spencer, but we are near the end of the book and have learnt not to expect accuracy.) Prof. Owen has, in Dr. Bree's humble opinion, "surrendered the outposts of our defence to the believers in the Darwinian hypothesis." As to Sir Charles Lyell, the charm of his works is gone for Dr. Bree, and he reflects with melancholy what the future will think of the great geologist's transmutation of thought, and with regret that such a man could, "in the maturity of his age and fame, have forsaken the 'principles' of his youth, of his manhood, and of his prime." The researches of M. Gaudry in Greece are of no use whatever; for the various forms of elephant, rhinoceros, horse, and pig, which he and Sir Charles Lyell believe to be intermediate forms, differ no more from one another than do English from Americans, and only prove a "slight variation!"

These are the kind of observations, this the kind of reasoning, by which Dr. Bree thinks to stem the tide of belief in Darwinism. At p. 269, Prof. Huxley is severely criticised for having written the following passage: "The mixture of ignorance and insolence which *at first* characterised a large proportion of the attacks with which Mr. Darwin was assailed, is no longer the sad distinction of anti-Darwinian criticism." This, Dr. Bree, with his usual curious logic, asserts is manifestly untrue, *because* some of the highest men in science, such as Agassiz, Flourens, Owen, Haughton, &c., oppose Darwinism. Why then did Dr. Bree not let well alone—leave the battle in the hands of these redoubted champions, and not give Prof. Huxley the opportunity of retracting his statement, on the ground that although the *insolence* of the first opponents of Darwinism may have vanished, their *ignorance* has returned?

In conclusion, I must again repeat that the only reason for devoting so much space to a book so little worthy of its title or its author, is the wish to warn such as are not well acquainted with Mr. Darwin's works from implicitly relying either on Dr. Bree's facts and arguments, or on the accuracy of his representation of those of Mr. Darwin and his supporters.

ALFRED R. WALLACE

OUR BOOK SHELF

Experimental Chemistry. Founded on the work of Dr. J. A. Stöckhardt. By C. W. Heaton, F.C.S. (London: Bell and Daldy.)

MANY students of chemistry have had reason to be grateful to Dr. Stöckhardt for his work on the Principles of Chemistry. For many years it was almost the only representative of its class; for it enabled students to acquire a considerable and useful knowledge of chemistry by teaching them to work experimentally at the subject, instead of merely reading about it. One of the great merits of his book, and which also belongs to the volume now under consideration, is that, although the number of experiments described is large and well selected, yet they do not necessarily require anything but extremely simple apparatus. This work, therefore, we believe will be found useful to a numerous and increasing class of students, who, though hindered by limited means and want of opportunity, wish to acquire some knowledge of chemistry.

The work is divided into four parts: the first treating of the General Principles; the second on the non-metallic elements; the third on the metals; and the fourth on Organic Chemistry. With the commencement of Part I. we confessed we were rather startled. The student is at once taken into a sort of half discussion as to whether matter has any existence or not, and the conclusion come to is that the problem will probably be for ever in dispute. This to a young beginner would scarcely impress him with the definite and unchangeable facts of Physical and Chemical Science.

Part I., on General Principles, is, we think, too advanced and complicated for the class of students by whom it is likely to be used. In fact, we should imagine that a student commencing the book and working by himself would find this part very up-hill work. For instance, before having studied any of the properties of the elements, he has to become acquainted with the various methods of fixing the atomic weights, the classification by atomicity, variations of atomicity, isomorphism, &c. In our opinion it would be almost better for a student to commence at the second part, that is, with the study of the non-metallic elements, in doing this, however, some little reference to Part I. would be necessary for the explanation of the meaning of symbols, &c., and he might then return to the complete study of Part I. The first part contains a number of definitions, several of which are not so good as they might be. It is said, for instance, that in a mixture the properties of the different ingredients are always perceptible. Gunpowder is given as an instance of a mixture; but in this the yellow colour of the sulphur and the white colour of the potassic nitrate are certainly not perceptible. Again, the definition of an acid is the following:—"An acid is composed of hydrogen with one of those radicles (p. 86) which are called acid radicles. The hydrogen can be replaced by metals, in which case one of the compounds called salts is formed. Acids redden litmus, and are commonly sour." On referring to p. 86, the exact definition of an acid radicle is not to be found; it is, as nearly as can be expressed, according to the author's ideas, the residue of an acid from which the hydrogen is abstracted. The definition of an acid, then, seems to be a body that contains hydrogen replaceable by metals, which is sometimes sour and reddens litmus. Surely definitions of a rather more definite and complete character might have been selected. Further in the book (p. 298) the author thinks it is often more convenient to regard the inorganic acids as hydrates, that is containing the radicle hydroxyl (HO), and of course uses this radicle throughout the organic acids. If the student accepts the two definitions he will have a double set of radicles, which would probably lead to much confusion.

The second part of the book is devoted to the non-metallic elements, the properties of which are studied by means of simple and instructive experiments, which are generally well described; the same is also the case with the next part, on the metals, and we then pass to the organic section of the work.

The field of Organic Chemistry is now so large that in the small space here devoted to it, a brief description only can be given of some of the more important compounds. It is also difficult in this section to arrange experiments which can easily be performed by students. It is thus necessary to confine the description of such classes of substances as the alcohols, the aldehydes, acids, haloid ethers, &c., to a very few pages. The arrangement, too, is peculiar, the experimental part of the organic work beginning with the study of cellulin, starch, sugar, &c., passing afterwards to the study of the more simple compounds, such as ethylic alcohol, acetic acid, &c.,—which seems rather like reversing the order of things. In a subsequent edition it would, perhaps, be better to adopt the modern system of classification, which would probably give the student a far better and more comprehensive knowledge of the subject. The book is, on the whole, one which, with a little reservation, can be safely recommended to students who wish to study Chemistry in the experimental way rather than simply to cram it up by reading. There is some room for improvement in the woodcuts, which in some instances are not artistic, and might be replaced by engravings of more modern and convenient apparatus.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Ocean Currents

I HAVE just read Mr. Ferrel's letter (NATURE, June 13), in which he refers to mine of April 25, and the proof therein adduced by me to show the physical impossibility of oceanic circulation being the result of differences of specific gravity. Unless Mr. Ferrel means (and I hardly think he does) that six foot-pounds of energy can perform 9,025 foot-pounds of work provided only sufficient time be allowed in which to perform that work, then I do not suppose there is any reader who may have glanced over my article on the subject who will not readily admit that Mr. Ferrel's reasoning has no direct bearing whatever on my argument.

The slope from the equator to latitude 60° is six feet. The total amount of work which gravity can perform upon a pound of water in carrying it down this slope is, of course, six foot-pounds. And this holds equally true whether the pound of water moves down the slope in, say one month, or takes 1,000 years to perform the journey; because the amount of work performed by gravity depends not upon the time which a body takes in descending, but upon the distance through which the body descends. In the present case six feet is the distance, consequently six foot-pounds is the amount of work performed upon the pound of water in its passage down the slope from the equator to lat. 60°.

Mr. Ferrel assumes that the velocity of the general movement of the water advocated by Dr. Carpenter does not exceed one mile per day, and that consequently the resistance to motion must be small. Undoubtedly the slower the motion the less the resistance; but so far as the argument under consideration is concerned, it is a matter of indifference whether we suppose the velocity of the water to be a mile per minute, a mile per day, or only a mile in 1,000,000 years; because it is found that when the water from the equatorial regions reaches, say, lat. 60°, it, as a matter of fact, is not moving eastwards relatively to the earth's surface with a velocity of several hundreds of feet per second, but with a velocity of only a few feet per second, perhaps not more than three feet at the utmost. In this case the water has lost 760 feet per second of velocity which it possessed when it left the equator. Each pound of water has therefore lost 9,025 foot pounds of energy. What has become of all this energy? It has all been consumed in overcoming resistance during the motion of the water from the equator to lat. 60°. But, be it observed, it has

been consumed, not in overcoming resistance to the poleward motion of the water, but in overcoming the resistance to eastward motion. The energy consumed is deflecting energy, not impelling energy.

According to Dr. Carpenter's theory the pound of water has in virtue of gravity only six foot-pounds of energy to carry it from the equator to lat. 60° against all the resistances to its poleward motion; but it so happens that before the water reaches lat. 60° no less than 9,025 foot-pounds of energy is consumed in overcoming resistance to eastward motion. But if it requires 9,025 foot-pounds of energy to overcome the resistance to eastward motion, how is it possible that Dr. Carpenter's six foot-pounds of energy can overcome the resistance to the poleward motion? The velocity of the motion of the water polewards is as great as, if not greater than, the velocity of the motion eastwards, consequently the resistance to the motion of the water poleward must be as great as the resistance to the motion eastward. But if so, then the work of the resistances to poleward motion is 1,500 times greater than the work of gravity. The work of gravity being only six foot-pounds, whereas the work of the resistances is 9,025 foot-pounds.

One of two things must therefore follow as a necessary consequence: (1) either the work of the resistances to poleward motion is 1,500 times greater than the work of gravity, or (2) the work of the resistances to poleward motion is 1,500 times less than the work of the resistances to eastward motion. But either conclusion is equally fatal to the gravitation theory.

It seems to me that until the advocates of this theory manage to escape from this dilemma, it is needless to argue further on the matter. For, unless it can be shown that the work of the resistances is not greater than the work of gravity, the much disputed question as to whether or not difference of specific gravity can be the cause of a *general* interchange of equatorial and polar water must be regarded as finally settled in the negative.

I cannot help thinking but that Mr. Ferrel is misled by his supposed analogy between a slope produced by the influence of the attraction of the moon and that produced by difference of specific gravity. Although a slope of 9 feet in a quadrant resulting from difference of specific gravity is insufficient to produce motion of the water, nevertheless, the sea will easily regain its level after the attractive force of the moon is withdrawn, even though the height to which the surface of the ocean is raised may not exceed a single inch. The reason of the difference in the two cases must be obvious to any one who will reflect on the matter. I have already in my paper in the Phil. Mag. for Oct. 1871 alluded to this reason, and will have occasion again to refer to it at greater length.

I may notice that by a typographical error in my article the velocity of rotation at lat. 60° was stated to be 773 feet per second instead of 763 feet per second.

Edinburgh, July 18 JAMES CROLL

The Melbourne Telescope

MR. ELLERY has been so good as to send me an enlargement of the lunar photograph taken with the great Melbourne telescope, to which you allude at p. 228, No. 142 of your Journal. This picture, Mr. Ellery tells me, was taken on the second evening of trial; it is very beautiful, although not so *critically* sharp as several I have obtained with my Newtonian equatorial of 13 in. aperture, and a little more than 10 ft. focal length. This sharpness, however, is a mere question of the shadiness of the atmosphere; and I feel persuaded that pictures will be taken with the Melbourne telescope far surpassing any hitherto procured. In my telescope the focal image varies from 1 in. to $1\frac{2}{10}$ in. in diameter, according to the distance of the moon from the earth. The primary picture of the Melbourne telescope (an enlargement of which has been sent to me) is $3\frac{3}{10}$ in. in diameter; hence the structure of the collodion and minute defects in it are of much less importance than when smaller instruments are used.

The employment of the great Melbourne telescope for astronomical photography cannot fail to be of great advantage to astronomy, and I should be very glad to see a similar instrument at work in England, notwithstanding its too much abused climate.

WARREN DE LA RUE

P.S.—As soon as the Melbourne picture has been mounted and protected, I will place it in the Astronomical Society's rooms for inspection.

On the Rigidity of the Earth and the Liquidity of Lavas.

IN his letter upon the Rigidity of the Earth and the Liquidity of Lavas in the number of NATURE for July 11, Dr. Sterry Hunt has replied to my challenge to propose an explanation of the connection between mountain ranges and trains of volcanoes consistent with a rigid globe, other than that to which I refer it, viz. the production of fusion through a diminution of pressure due to the partial support of the mountains by the lateral thrust which has upraised them. Dr. Hunt suggests that liquefaction may take place beneath such ranges, through *increased* pressure promoting the liquefaction of the water-impregnated mass; and quotes the late Archdeacon Pratt as maintaining the existence of a greater pressure beneath mountain ranges.

In reply, I may be allowed to ask Dr. Hunt for a reference to such an expression of Pratt's opinion. I cannot call to mind any passage of his to that effect. The result of his calculations of the attraction of the Himalayas upon the plumb-line showed that they do not attract so much as they ought to do, and he explained this by supposing a deficiency of matter beneath the mountains. His own explanation of the phenomena, as given in the fourth edition of his Theory of the Earth, finished very shortly before his lamented death, is, "that the varieties we see in mountains and plains and ocean beds in the earth's surface, have arisen from the earth having been once a fluid or semifluid mass, and that in solidifying the mass has contracted unequally, so as to form hollows where the contraction has been greatest, into which water flowed and formed seas and oceans, and to leave high table-lands and mountain-ranges where the contra tion has been less." (He speaks here of contraction in the vertical direction.) A geologist will, I suppose, receive this as a very incomplete explanation; but the material point is that the Archdeacon was led to adopt it because he had discovered a deficiency of matter beneath the Himalayas. This seems incompatible with Dr. Hunt's view (both with regard to Pratt's opinion on the subject, and with regard to the fact itself) that there is an increased pressure beneath mountain ranges.

It will now appear that my "speculation" upon the origin of volcanic action was suggested by the proved deficiency of matter, and consequently probable diminution of pressure; and not that the idea of diminished pressure was invented to account for volcanic action. I have merely proposed a connection between lateral pressure and diminished density which seems most natural, namely, that the same pressure which upraised the mountains continues partially to support them. And I cannot see how it can do otherwise. For the abutments of the mountains having approached by contraction of the crust, cannot again recede without expansion, which cannot take place. Dr. Hunt's view of the liquefaction of lavas, to my mind, requires explanation. Admitting that pressure promotes aqueous liquefaction in heated rocks, when rocks so liquefied began to rise in a volcanic mass, would they not be brought under diminished pressure, and would they not become immediately solidified, so that they could not reach the surface in a fluid state?

Moreover, since liquefaction, according to this view, is increased by pressure, the interior parts of the earth being under greater pressure than the more superficial strata, ought, at least to that depth where water is present, to be more liquid, and this would be incompatible with the supposition of a rigid globe which Dr. Hunt favours.

Harlton, Cambridge O. FISHER

The Method of Least Squares

WILL you allow me to call the attention of Mr. J. W. L. Glaisher to the following sentence from Encke, Berliner Jahrbuch 1853, p. 311. "Ich werde mir deshalb erlauben, völlig dem Gange den Lagrange genommen hat folgend, wie könnte man sich erdreisten, bei der ungemeinen Klarheit, Einfachheit und Tiefe des grossen Meisters, eine irgend bedeutende Anderung vorzunehmen, den Theil der Abhandlung hier wiederzugeben, welcher den Beweis für das arithmetische Mittel enthält, und selbst Sätze, die im Grunde schon die Methode der kleinsten Quadrate in sich begreifen." Also to article 17, Corollary, of the Memoir of Lagrange.

This is not the place to discuss the doctrine of the Method of Least Squares; but I may say that in my judgment the method rests on the assumption of the principle of the arithmetical mean, an assumption which is justified by an universal experience.

Having made this assumption, the rule that the sum of the squares of the remaining errors is a minimum follows very simply : *vide* the late Memoir of Hansen, art. 3.

With regard to the practical application of the Method of Least Squares, I think the whole honour of its introduction belongs to Gauss. The rules which he and his scholar Encke have given for the application of the method, and for executing the numerical operations, are so complete and perfect that but little more can be desired.

Washington, July 4 ASAPH HALL

Solar Rainbow

ON the 10th instant at about half-past seven in the evening I saw part of a well-defined rainbow about 5° west of the zenith, the convexity of the bow towards the setting sun, which at the time was about 3° above the horizon. Light clouds were passing beneath the bow. There was no rain.

Brighton, July 15 GEORGE DINNOW

Hive Bees *v.* Mechanism

I HAVE never followed Huber through his wonderful researches into the astounding working proceedings of hive bees—that elder Huber, who, by the way, aided by so admirable a spouse, brought his researches to so successful an issue, notwithstanding his blindness. Hence my excuse, if what I attempt to describe as being original to my own sense of observation, prove not so to others. I think it is conceded universally that amongst other leguminous plants peas have ever been secure from cross fertilisation, one variety with the other, in so far as natural influences, insect agency, &c., are concerned. Our stocks of garden peas, though known to run weedy and grow inferior when cultivated too long upon one kind of soil, very rarely, if ever, sport or vary as other plants placed in juxtaposition of species, especially varieties, are known to do. So decidedly has this fact been confirmed, that invariably sweet peas, even when it is desirable to grow them true to name, are sown in rows, side by side, whites, scarlets, blues, &c., with the utmost impunity. And this is wholly owing to the fact that the floral envelopes are so securely wrapped around the pistil and the stamens, that these parts cannot be reached without the exertion of more power than the strength bees and similar winged insects are supposed ordinarily to possess ; unless, indeed, mechanism be called to their aid—a science in itself, but which, nevertheless, has been resorted to in the instance to which I would direct attention. Here then the hive-bees methodically bare the stamens by sheer mechanical force, and rob each of its load of pollen by sense of touch alone. And this they do in this wise. Alighting on the *ala* or "wings" of each bloom, they first of all press their heads up under the base of that part of the papilionaceous corolla called the "standard," or *vexillum*, and extract what nectar they find. Then, with their little heads firmly pressed therein, and holding fast by the four fore legs, they exert their power, thus artificially contrived, by treading down both the aforesaid wings and the "keel," or *carina*, which so securely envelopes the sexual organs, that they protrude, so that the anthers are laid bare, when they generally rub the pollen off the stamens on to their hairy bellies, only occasionally using one hinder leg to aid them. It will be seen that they do this with their hinder legs and body, at a time when their heads are entirely hidden from view. I have tested these parts in regard to the pressure needed to disclose the pollen thus, and find that a pressure of half to three quarter ounce is necessary ; and computing the weight of an individual bee to be about the sixteenth of an ounce, we see what an amount of power must be exerted in this hind-before, or blindfold manner, by these interesting little creatures.

I should add, however, though I have been a selecter of sweet peas for more than a dozen years in other parts of the country, as the selection sold by some seedsmen with my signature attached confirms, I nevertheless have not previously believed in any power possessed by insect agency to thus destroy selections. Here, at Valentines, however, being only seven miles from London, it would appear that the bees, like town sparrows, are unusually "wise in their generation," and that, owing to scarcity of honey-yielding materials, they are driven to such wonderful feats as I have explained.

WILLIAM EARLEY

The Gardens, Valentines

The Red Rocks

IF the peroxide of iron was deposited (as in the Swedish lakes) as brown hydrous peroxide, and if long boiling in the laboratory may be considered analogous to evaporation in an inland sea, then it would appear from the following extract from Watts's "Dictionary of Chemistry" that there is no difficulty in accounting for the colour of the red rocks :—

"A remarkable insoluble modification of ferric hydrate is produced by boiling the ordinary yellow hydrate to $2\,Fe_2\,O_3\,3\,H_2O$ (precipitated from the chloride by ammonia) in water for seven or eight hours. The colour then changes from ochre-yellow to brick-red, and the hydrate thus altered is scarcely acted upon by strong boiling nitric acid, and but very slowly by hydrochloric acid. In acetic acid, or dilute nitric or hydrochloric acid it dissolves, forming a red liquid, which is clear by transmitted, but turbid by reflected light ; is precipitated by the smallest quantity of an alkali salt or a sulphate ; and on addition of strong nitric or hydrochloric acid, yields a red granular precipitate, which redissolves on diluting the liquid with water."

The change of colour from brown to red is readily obtained by boiling the hydrate in a flask for several hours, as described above. The change is gradual, and before becoming finally red the precipitate is of a chocolate colour, corresponding with that sometimes observed in the red sandstones.

Small pieces of white sandstone, introduced into the flask during the boiling, are of course coloured red, and resemble red sandstone when taken out and dried. R. D. P.

Instantaneousness of Lightning

DURING a recent night thunderstorm I got out my colour-top, with the usual disc of so-called primary colours arranged to blend into grey or white on rotation, in order to show to my children the instantaneousness of the lightning, and that by its light the disc would, as I had no doubt, appear stationary in one or several successive positions according to the character of the flash, as it does by the light of an ordinary electric spark from a Leyden jar or induction coil. On trying the experiment, however, by turning the disc (about forty times in a second) at a window in a dark room opposite to the cloud in and from which the discharges were taking place, I found that this was only very partially the case. When the direct stroke was actually visible, or only slightly veiled by cloud, the effect I looked for was produced, the bands of colour standing out clear and apparently motionless ; but at other times during the apparently (to the eye) prolonged flash, the colours blended so as to indicate a continuous fainter light in addition to the occasional instantaneous appearance of definite colour and form due to the intermittent light of the discharge. Of course I satisfied myself that there was no other light to account for this, the night and the room being very dark in the intervals of the flashes, and I repeated the experiment in another night storm (on the 11th) with just the same result. The effect appears to be due to the retention of light in the cloud by phosphorescence, and, so far as I can find on inquiry, does not seem to have been noticed before.

If my view is correct, it would explain the fact that distant lightning at night, when no cloud is in sight, is apparently so much sharper than when nearer.

A propos to the subject, a letter to the *Times* describing the storm of the 11th inst., speaks of the lightning "flickering with a perceptible rustle"—a curious instance of transference of impression from one sense to another, the visual sensation of flickering being exactly analogous to the auditory sensation of rustling. B. W. SMITH

Hampstead, July 20

Severe Thunderstorm

A HEAVY thunderstorm passed over this neighbourhood this afternoon. One of the flashes was followed by a smart "snap" like that of the discharge of a large Leyden jar, or the explosion of a heavy percussion cap. The interval between this noise and the crash of the thunder was about half or three-quarters of a second. I have not unfrequently noticed a hissing noise immediately preceding a violent clap of thunder, but never anything so distinctly resembling the sound of the ordinary electric spark.

ALEX. BEAZELEY

Willesden Lane, Kilburn, July 23

TECHNICAL EDUCATION

ON Saturday last a meeting was held at the rooms of the Society of Arts, of scientific men and others interested in the promotion of technical education among the working classes, and especially among the skilled artisans of this country. The chair was taken by H.R.H. Prince Arthur, who showed in the remarks which he addressed to the meeting an admirable appreciation of the extreme value of the subject to the future prosperity of the country. After the Secretary of the Society of Arts had explained the proposal of Captain Donnelly that the Society should establish technological examinations for the purpose of testing the practical knowledge and skill required in the application of the scientific principles involved in each art and manufacture, H.R.H. said:—

"The Society of Arts are endeavouring to encourage among those who are practically employed in various industries the study of art, and an accurate knowledge of its application in each branch of manufacture. The Society will not, however, attempt to teach the practice of those arts. Their object is rather to lay a sound foundation of all the principles on which those practices may be carried out to the best advantage. The Society wish and desire to give certificates, prizes, and scholarships to those who show that to practical skill as workmen they have added an accurate knowledge of natural principles in those matters. I am certain the necessity and advantage of combining scientific principles with practice will be seen by all. I only wonder that we are but just beginning to undertake this task. The machinery for these technological examinations already exists, and, in fact, the skeleton of industrial universities is ready to our hands. What we now require is funds to clothe it with and give it life, and to enable it to carry out its work—that is, to obtain sufficient prizes to encourage and reward the deserving youth of this country. If others would only do what Sir Joseph Whitworth has done by his noble endowment for mechanical engineering, we might soon hope to see our most sanguine expectations realised."

Little by little the country is gradually waking up to understanding the principles on which alone its future greatness as a manufacturing nation can rest, Many will probably echo H.R.H.'s wonder that "we are but just beginning to undertake this task."

VOLCANOES OF CENTRAL FRANCE

THE conduct of the war against the Goths having brought Procopius into Campania, he is thus led to give an account of Vesuvius. In the text before me the mountain is called Bebios, and ash is said to have fallen at Byzantium; but undoubtedly Vesuvius is meant, and there seem to be good reasons for suspecting that "Byzantium" should be "Beneventum." Both Besuios (= Vesuios) and Beneventum could be so written as easily to be mistaken for the names now in the text. I cannot indeed pretend to say when, I must leave that to the critics.

Procopius, "De Bello Gothico," l. ii. c. 4: "At that time Mount Vesuvius was roaring, but there was no eruption; however, in consequence of the roaring, an eruption was confidently expected and the surrounding population were in great fear. This mountain is about seventy stadii from Naples, bearing northwards from that place (τετραμμενον αὐτῇ πρὸς Βορρᾶν ἄνεμον). [The real bearing is about 10° S. of E. Is he confounding Naples with Stabiæ, where Pliny died?]* It is naturally escarped,—wide-spreading below, but the top steep and very rugged; and on the summit of Vesuvius, in the centre, there is a cavern, which appears to be remarkably deep, and it would seem as if it extended quite to the extremities (ἔσχατα) of the mountain; and any one who is bold enough to look down may see fire in it. It is of no consequence to the inhabitants when no flame is seen above it; but when the mountain sends forth an explosive roaring it generally happens that not long afterwards a vast quantity of dust is thrown up, and if this destructive shower meets a traveller on the road he can by no means escape with his life; and if it descends on houses, they fall, crushed down by the mass of ash; and if a violent wind happens to be blowing, the ash is carried to a great height, out of people's sight, and is transported wherever the wind may carry it, and it may fall in some country at a very great distance. And they say that formerly, when it fell in Byzantium [qu. Beneventum?], the people there were so alarmed that they have made it a rule (ἔγνωσαν) in general assembly from that day to this to propitiate the deity by yearly supplications (λιταῖς). At another time (they say) it fell in Tripolis in Lybia. And they say that this roaring first (?) took place a hundred or more years ago, and that latterly it occurred much more frequently, but with decreasing violence.* They say, too, that when Vesuvius happens to eject this dust, the country where it falls is sure to produce abundant crops. On this mountain especially, beyond all others, the air is very light, and highly conducive to health, and time out of mind medical men have unhesitatingly sent thither their consumptive patients. Such, in the main, are the circumstances in connection with Vesuvius."

Vesuvius lies about 40° 47′ N. by 14° 26′ E., Byzantium about 41° N. by 28° 59′ E.; so a difference of 14° 33′ of longitude on the parallel of 41°, or, by a rough estimate, a distance of 750 miles. Is it possible that ashes ever fell at that distance in such quantities as to terrify the people, and drive them to institute Rogations? Is it likely that a professional historiographer, a resident in the very town where the alarm is said to have occurred, and where the Rogations had been instituted and constantly observed, could have first heard of these facts in Italy and from Italians? If this is scarcely credible, it suggests that we should read Beneventum. It is about thirty miles from the mountain—a distance not so great but that such events might have occurred there. He who wishes to have a vivid picture of them, let him read Pliny's second letter to Tacitus (vi. 20) describing what happened to himself at Misenum. The erroneous reading is somewhat countenanced by the fact that the statement seems intended to be an instance of great distance; whereas probably the heavy fall of ash and the consequent Rogations alone caused Beneventum to be mentioned, and Tripolis was the only case of distance.

While, therefore, the geologists are searching for ash at Vienne, the archæologists might oblige us by searching the annals of the principal Sees round Vesuvius, especially Beneventum. There were also Neapolis, Surrentum, Salernum, and perhaps other Sees, which our bishop might have held, but the scattered rays of light seem best to converge on Beneventum. At a much later date it is described as an Imperial vicariat; did it bear any such relation to the empire in the middle of the fifth century? Did it possess walls, a spacious forum, and a resident nobility? Could people have seen, or have fancied they saw, deer taking refuge in the forum? Is there any record

* It has occurred to me to suggest πρὸς Βάριον ὅρμον for πρὸς Βορρᾶν ἄνεμον This would not be an unnatural reference in the secretary of a Byzantine general, if the Greek emperors already used that port for military purposes.

* My translation of this passage is open to criticism. I have even ventured to read ἀπισχυρισάμενον for ἀπισχυρισάμενοι, and I give an unusual sense to the word, by analogy to ἀποπλουτέω, ἀπομανθάνω, ἀποκραιπαλίζομαι &c. The context seems to me imperatively to demand it. The Latin translation renders the text, as it stands, thus:—"According to them it is now a hundred years or more since the prior roaring occurred; the memory of the other is much more recent; moreover they affirm that it cannot but be, that, &c." Can θᾶσσον be translated by "more recent"? and why such violent asseveration about a simple agricultural fact? The difference does not affect our question; but it is of some importance in its bearing on the History of Vesuvius.

or mention of a Bishop Mamertus, or of some intrusive bishop who may have been Mamertus? Labbe's "Sacrosancta Concilia" (at the British Museum or the London Library) might be consulted to see whether any hint is given of Mamertus having been in Italy, either in the letters of Leo, or (more probably) in those of his successor, Hilary (iv. pp. 1032—1047).

Procopius got his information during the first Gothic war (A.D. 535—540); the second lasted from A.D. 544 to A.D. 548. He died about A.D. 565, at the age of 60. Ought the "century or more ago" to be computed from the date of his information, or from the time when he wrote? This might make a difference of 10 or 20 years. If the former, then the commencement (?) and violence of the eruptions are thrown back to the early part of the fifth century. In any case they must have preceded rather than followed the interval from 455 to 463. Where did Prof. Daubeny get his date of A.D. 472? Certainly not here.

It is therefore very likely that about A.D. 455 Vesuvius may have been in a state of violent activity. In that year Rome was sacked. These are precisely the conditions which the hypothesis requires. Procopius was told that the Rogations were occasioned by such eruptions at about that date. If, then, Mamertus was the sole (?) author of them, he must have been present where the Terrors occurred. Is there positive evidence of the fact? If no positive evidence, for or against, can be found, it will then be time enough to go into other questions of probability. Here for the present I conclude.

HENRY NORTON

WATER ANALYSIS

II.

IN the last article* we assumed that the object of estimating the organic carbon and nitrogen has been fully understood by the reader; but in order to render all chance of our meaning being misunderstood in the remarks which are to follow, it will be as well to briefly capitulate the reasons which render these determinations so valuable. Carbon and nitrogen form with hydrogen the principal constituents of all organised bodies, and hence are found in all the excreta of animals. Nitrogen is found in animals to a much greater extent than in plants, while in the inorganic world this latter element is scarcely found at all, and carbon only in the form of carbon, coal or carbonic acid, none of which bodies are likely to be found in water except the last, the first two being insoluble. It hence follows that if a large quantity, or indeed any quantity, of carbon and nitrogen be found in water, their source can only have been organic in its nature, and if the proportion of nitrogen to carbon be more than one to five, the source is almost absolutely certain to have been animal.

Accordingly, when we find these two elements existing in waters in the proportions just indicated, we are justified in assuming the presence of some form of animal or organic contamination in them; and of all forms in which this contamination can exist, sewage is the most probable. It will thus be seen that when chemists assert that a water is contaminated with sewage, they do so on grounds the truth of which is easily demonstrable. That such waters containing effete animal matters are injurious, no arguments of ours will be required to prove; no persons of authority in sanitary matters have presumed to assert that such waters are harmless.

With regard to the method of obtaining evidence as to this contamination, however, considerable difference of opinion exists, and in the former paper we have endeavoured to show the worthlessness of the early processes of estimating organic matter by ignition of the residue, and by treatment with permanganate of potash; and though this last process is condemned by all, it is still in use.

We have also given reasons for regarding with distrust the results obtained by the use of Chapman, Wanklyn, and Smith's method of indirect determination of organic matter from the amount of ammonia evolved by the water, as we maintain that it cannot be shown that the ammonia evolved bears any distinct relation to the amount of organic matter present, and that with many waters it is difficult to obtain an accurate estimate of the ammonia thus evolved.

The process we have now to consider bases its claims to confidence on the fact that it gives an absolute determination of the quantity of carbon and nitrogen present in the water, and that it does do this can, we think, be proved without much difficulty.

The process is based upon the fact that when a body containing carbon and nitrogen is heated in contact with cupric oxide to bright redness, the carbon is converted into carbonic anhydride at the expense of a portion of the oxygen of the cupric oxide, while the nitrogen is liberated, partly in the free state and partly in the forms of its lower oxides, the quantity of these latter being reduced as much as possible by causing them to pass over the surface of red-hot metallic copper, which abstracts the oxygen and leaves the nitrogen free. In other words, the substances are obtained and estimated by what is known to chemists as a "combustion."

As far back as 1864 an unsuccessful attempt had been made by Weltzien to apply this process to the estimation of carbon in water. The failure was due to the very minute quantities available for estimation, and to the fact that the water was rendered acid with sulphuric acid before evaporation, a proceeding which directly tended to vitiate the results, even had no other obstacle intervened, as the acid gradually concentrated during evaporation, ultimately became sufficiently strong to char and decompose much of the organic matter present.

It was not until Dr. Hermann Sprengel had placed in the hands of chemists a new and powerful means of research in the shape of the admirable air-pump which bears his name, that it was possible to estimate directly the minute amounts of carbon and nitrogen which water, as a rule, contains.

In March 1868 Messrs. Frankland and Armstrong published a method of water analysis which they had elaborated, after eighteen months' work at the subject.

A quantity of water proportionate to the amount of ammonia found,* and varying from one litre in the case of a town water-supply, to 100 cubic centimetres in the case of a much polluted water, is introduced into a flask, and 15 cubic centimetres of a saturated solution of sulphurous anhydride are added, and the water boiled briskly for three minutes; the water is then removed from the source of heat, and a portion introduced into a hemispherical lipless glass dish of about 100 to 120 cubic centimetres capacity; this is placed on a steam bath, and to the first dishful two or three drops of a moderately strong solution of ferric chloride are added. Should the water leave but a small solid residue, or contain little calcic carbonate, a few drops of a solution of sodic sulphite should also be added. The dish is then covered with a cap of filtering paper stretched over a ring of cane, and the evaporation continued, the rest of the water being kept warm in the flask, and added from time to time to the dish. The laboratory in which this operation is performed should be kept free from dust, and no ammonia should ever be allowed in it.

The rationale of the process so far is very simple, and is as follows:—The first boiling with sulphurous acid expels the free carbonic acid in the water, and also any that

* See NATURE, vol. vi. p. 104.

* If the ammonia be less than 0·1 part per 100,000, 1 litre should be used; if more than 1·0, a hundred cubic centimetres or less. (See Sutton's "Volumetric Analysis." J. A. Churchill, New Burlington Street, 2nd Edition, 1871, pp 246-295.)

may exist in combination as calcic carbonate, and during the evaporation the remaining sulphurous acid reduces and expels the nitrous and nitric acids present, in which the ferric chloride greatly helps it; the sulphuric acid which is thus formed is neutralised by the calcic sulphite (formed during the preliminary expulsion of carbonic acid) with liberation of fresh sulphurous acid, or should no calcic salt be present, the sodic sulphite is added to effect this.

As soon as the whole of the water is evaporated, the residue is carefully detached from the dish by means of a flexible steel spatula, and thoroughly mixed with some fine cupric oxide; it is then introduced into a stout piece of combustion tube about 430 millimetres long, one end of which has been closed, after having been very carefully cleansed, and which has about 30 millimetres of its length filled up with coarsely granulated cupric oxide.* After the mixed residue and cupric oxide have been introduced, the tube is filled up to within 100 millimetres of the open end with granular cupric oxide; a tightly-rolled cylinder of copper gauze covered with sheet copper, and which has been recently ignited and cooled in a current of hydrogen, is then put in, lastly a few millimetres more of granular cupric oxide, and the tube is then drawn out in the blowpipe flame, put into a combustion furnace, connected with a sprengel pump, and, while the anterior part of the tube is being heated to redness, thoroughly exhausted of air. The pump is then stopped, and the heat gradually carried backwards until the whole of the combustion tube has been heated to redness. Any gas which may come off is collected from the bottom of the Sprengel pump over mercury, and as soon as the gas ceases to be evolved, the furnace is allowed to cool slightly, and the tube again exhausted. The gas pumped off is transferred to an apparatus for the analysis of gases, and measured, after the absence of sulphurous anhydride has been insured by the introduction into the gas of a drop of a solution of dipotassic dichromate. The carbonic anhydride is then absorbed by potassic hydrate, and the gas again measured, the difference being the carbonic anhydride. To the residual gas a minute bubble of oxygen is added to decompose the nitric oxide, should any be present, and after the excess of oxygen has been absorbed by pyrogallic acid, the gas again measured consists of nitrogen; to this half the difference between the two last readings is added, as this represents the nitrogen which had existed as nitric oxide, and the result is the total nitrogen.

All the above data are reduced to measures of weight by the use of the formula. Log. $\frac{\cdot 0012562}{(1 + \cdot 00367t)760}$ which gives the weight of cubic centimetres of nitrogen in grams. This table of logarithms is carried out for each tenth of a degree from 0° to 30°C.

To those uninitiated in gas analysis, the above may sound very complicated; but in practice it is found extremely simple. The whole analysis, including the calculations, can easily be carried out in half an hour, and the combustion itself need not occupy more, as a rule, than forty-five minutes.

The value of the above process is at once perceived when it is remembered that by it 0·0000005 gram. carbon and 0·0000001 gram. nitrogen are distinctly measurable quantities. The methods of analysis involving the use of gasometric measurements are by very far the most accurate in the whole range of a science whose very foundations rest on the possibility of accurately weighing and measuring varying quantities. The great capabilities of this method of inquiry have received a splendid demonstration in the researches of Sir Benjamin Brodie on Ozone; and Profs. Williamson and Russell, recognising the peculiar excellence of these methods, have endeavoured with considerable success to make them applicable to a more extended range of work, in connection with which it will only be necessary to remind the reader of the careful determinations of the atomic weights of cobalt and nickel made by the latter chemist.

Notwithstanding these advantages, the application of this method to the determination of the organic constituents of water has encountered the most strenuous opposition in many quarters.

But until a more absolutely certain method of determining the quantities of carbon and nitrogen shall have proved Frankland and Armstrong's process to be in error, it would, indeed, be the height of folly to adopt in its place a method which, like the Albumenoid process, only professes to give a fraction of the nitrogen present (and has absolutely failed to prove that it even gives any known fraction whatever); while it does not even attempt to estimate the carbon at all. Again, over 60 per cent. of the errors given by Frankland and Armstrong in their paper are minus errors, and thus directly tend to favour the Albumenoid process when analysis of waters by the two methods are compared; for the Albumenoid process almost invariably gives less quantities of ammonia than Frankland and Armstrong's does of nitrogen. It must be borne in mind that these absolute errors are almost certainly due to errors in weighing the small quantities of substances used to test the process, quantities so small that they are admitted by the objectors to be only about one-tenth of those usually used in organic analysis.

But in the case of water no weighing at all occurs. A measured quantity of water is taken, and the quantity of carbon and nitrogen which happen to exist in that measure is ascertained, whether it be small or great.

Again, it has been urged that ammonia is lost during the evaporation. This was found to be the case; but it was a difficulty only requiring to be known in order to be remedied. Accordingly, solution of salts of ammonia made acid with sulphurous acid in one case, and with metaphosphoric acid in the other (the last acid replaces the first when sewage is operated on), were evaporated and the nitrogen determined, and from the loss found two tables* constructed, in which each alternate term was an absolute determination, and the intermediate ones calculations. The wonderful constancy with which the numbers alter for each strength of solution shows how accurate the determinations must have been in order to obtain them.

Another valuable proof of the trustworthiness of a process is to be found in the degree of agreement existing between duplicate determinations made with it; and when the process is examined in this way it certainly passes the test in the most satisfactory manner, and the following examples will show how well duplicate determinations agree with each other:—

		Parts per 100,000. Organic Carbon	Organic Nitrogen.
Grand Junction Water Company . .	1	·185	·030
,, ,, ,, . .	2	·172	·030
East London ,, ,, . . .	1	·157	·026
,, ,, ,, . . .	2	·148	·030
New River ,, ,, . . .	1	·239	·042
,, ,, ,, . . .	2	·231	·042
The Don at Alford	1	·115	·024
,, ,, ...	2	·112	·026
Lady Well Spring (Dundee) . . .	1	·029	·035
,, ,, ,, . . .	2	·023	·033

Indeed, the numbers given in the Registrar-General's monthly reports exhibit so remarkable an agreement among themselves that any unprejudiced judge must admit the accuracy of the method by which they were obtained. We shall consider the remainder of the subject in a concluding article.

* Made by igniting and oxidising short pieces of copper wire; it can be obtained from dealers in chemicals.

* See Sutton's "Volumetric Analysis," p. 276, 2nd Edition.

THE BLIND FISHES OF THE MAMMOTH CAVE AND THEIR ALLIES*

THE blind fish of the Mammoth Cave has from its discovery been regarded with curiosity by all who have heard of its existence, while anatomists and physiologists have considered it as one of those singular animals whose special anatomy must be studied in order to understand correctly facts that have been demonstrated from other sources; and, in these days of the Darwinian and development theories, the little blind fish is called forth to give its testimony, pro or con.

Before touching upon this point, however, we must call attention to the structure of the fish and its allies, and to others that are either partially or totally blind.

In the lancelet (*Branchiostoma*) and the hag (*Myxine*) the eye is described "as simple in form as that of a leech, consisting simply of a skin follicle coated by a dark pigment, which receives the end of a nerve from the brain." Such an eye speck as this structure gives would only answer for the simple perception of light. In the young† of the lampreys (*Petromyzon*) the eye is very small and placed in a fold of the skin of the head, and probably of little use, as these young remain buried in the sand; but as they attain maturity, and, with it, the parasitic habits of the adult, their eyes are developed to a fair size, thus reversing the general rule in the class.

In most other fishes the eyes are developed to a full and even remarkable extent as to size and perfection of sight in water. In *Anableps*, or the so-called four-eyed fish of the fresh waters of Central and South America, which belongs to a closely allied family with our blind fish, the *Cyprinodontidæ*, the eyes are not only fully developed, but are divided into an upper and lower portion in such a way, by an opaque horizontal line, as to give the effect of two pupils, by which the fish probably sees as well when following its prey on the surface with its eyes out of water, as when under water. But it is in the interesting family of Cat fishes (*Siluridæ*) that we find the most singular arrangement of eyes in perfect adaptation to the diversified modes of life of the numerous species. In this family the eyes assume nearly every possible modification from partial and even total blindness to perfectly developed eyes, and these organs are placed in almost every conceivable position in a fish's head; from the ordinary large eyes on the side, to small ones on top of the head, enabling the fish to see only what is above; to the oval eyes on the side, in some just back of the mouth, situated in such a way that the fish can only see what is in close proximity to its jaws or even below them. Many genera of this family found in South America,‡ Africa,§ and Asia,‖ have the eyes so small and buried under the skin, or protected by folds or cartilage, as evidently to be of no more use than simply to distinguish light from darkness.

Among the most interesting forms of this family, in this respect, is the genus described by Prof. Cope under the name of *Gronias nigrilabris*. This fish is very closely allied to our common bull pout or horned pout, and of about the same size (ten inches in length). It was taken in the Conestoga river in Lancaster Co., Pennsylvania, where it is "occasionally caught by fishermen and is supposed to issue from a subterranean stream said to traverse the limestone in that part of Lancaster Co., and discharge into the Conestoga." We quote the following from Prof. Cope's remarks on the fish:*—

"Two specimens of this fish present an interesting condition of the rudimental eyes. On the left side of both a small perforation exists in the corium, which is closed by the epidermis, representing a rudimental cornea; on the other the corium is complete. Here the eyeball exists as a very small cartilaginous sphere with thick walls, concealed by the muscles and fibrous tissue attached, and filled by a minute nucleus of pigment. On the other the sphere is larger and thinner walled, the thinnest portion adherent to the corneal spot above mentioned; there is a lining of pigment. It is scarcely collapsed in one, in the other so closely as to give a tripodal section. Here we have an interesting transitional condition in one and the same animal, with regard to a peculiarity which has at the same time physiological and systematic significance, and is one of the comparatively few cases where the physiological appropriateness of a generic modification can be demonstrated. It is therefore not subject to the difficulty under which the advocates of natural selection

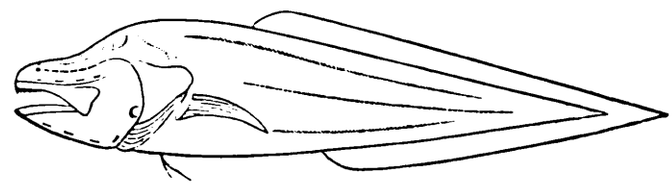

FIG. 1.—Cuban Blind Fish: Lucifuga subterraneus.

labour, when necessitated to explain a structure as being a step in the advance towards, or in the recession from, any *unknown* modification needful to the existence of the species. In the present case observation on the species in a state of nature may furnish interesting results. In no specimen has a trace of anything representing the lens been found."

When we remember that the lens of the eye in *Amblyopsis* has been found, even though the eye is less developed in all its parts than in *Gronias*, it is probable that a careful microscopical examination would show its existence in this genus also.

It is interesting to note that this fish is black above (lighter on the sides and white below), notwithstanding its supposed subterranean habits, and that all the other members of the family having rudimentary or covered eyes are also dark coloured, while the blind fishes of the Mammoth Cave and of the caves in Cuba are nearly colourless. This want of colour in the latter fishes has been considered as due to their subterranean life. If this be the cause, why should the blind cat-fishes retain the colours characteristic of the other members of the family living in open waters?

The fishes which in a general way, so far as blindness, tactile sense, and mode of life are concerned, come the nearest to the blind fishes of the Mammoth Cave, are those described by Prof. Poey† under the names of *Lucifuga subterraneus* and *L. dentatus*.‡ These fishes having

* Reprinted from the *American Naturalist*, a sequel to "The Blind Crustacea of the Mammoth Cave." See NATURE, vol. v. pp. 445, 484.

† These young lampreys have been described under the generic name of Ammocœtes, and it was not until 1856, when Prof. Müller discovered the fact of a metamorphosis in the lampreys, that their true position was ascertained. Prof. Müller has traced the history of the common European species, and shown that it is three or four years in attaining its perfect form. With this fact before us and with the early stages of the Myxinoids still unknown, have we not some reason for suspecting that the lancelet may yet prove to be a larval form of the Myxinoids, notwithstanding that it is said to lay eggs? Why should we not suspect the existence in the very lowest vertebrates of something akin to alternate generations, or of larvæ capable of reproduction? Without having any facts to support such an assumption, except that, on general principles, the young of Myxine would probably be very much like Branchiostoma, and that its young is not known, while Branchiostoma has only been found in waters where some species of Myxinoid exists, I think that before the position of the lancelet is firmly established, we must know the embryology of the Myxinoids; for should the lancelet prove not to be the young of the Myxinoids, it must necessarily form a distinct class of animals, perhaps as near to the molluscs as to the vertebrates.

‡ *Pimelodus cyclopium* of Humboldt, *Helogenes*, *Agoniosus* and other genera.

§ *Eutropius congensis*.

‖ *Ailia*, *Shilbichthys*, *Bagroides*, and other genera.

* Proceedings of the Academy of Natural Sciences of Philadelphia for 1864, p. 231.

† Memorias sobre la Historia Natural de la Isla de Cuba, por Felipe Poey. Tomo 2, pp. 95-114. Pls. 9, 10, 11. Habana, 1856-8.

‡ This species was afterwards referred to the genus *Stygicola* Gill, on account of the presence of palatine teeth which are wanting in the other species. There are also several other good characters, to judge from the figures of the head, skull, and brain given by Poey, that would warrant the reference of the fish to a distinct genus from *L. subterraneus*.

the broad, flattened, fleshy head, with minute cilia, without external eyes, and inhabiting caves so similar in structure to the Mammoth Cave, make a comparison of them with the fishes of the Mammoth Cave most interesting. This is greatly enhanced by the fact that the Cuban fishes belong to a family of essentially marine habit, quite far removed from *Amblyopsis*. The fresh water ling (*Lota*), belonging to the same great group of fishes (though to a distinct family or sub-family) containing the cod on the one hand and the Cuban blind fish on the other, is probably the nearest fresh water relative of the Cuban fish, but the nearest representative yet known is the marine genus *Brotula*, one species of which is found in the Carribean Sea.

In the Cuban blind fish we find ciliary appendages on the head and body quite distinctly developed, evidently of the same character as those of *Amblyopsis*, and answering the purpose of tactile organs. These cilia are in the form of small but plainly visible protuberances (reminding one of the single fleshy protuberance over the opercular opening just back of the head in *Amblyopsis*). There are eight of these on top of the head of a specimen I hastily examined, received from Prof. Poey by the Museum of Comparative Zoology, and quite a number arranged in three rows on each side of the body, showing that tactile sense is well developed in this fish: though it is rather singular that the barbels on the jaws, so usually developed as organs of touch in the cod family and its allies, are entirely wanting in this fish.

The brain of *Lucifuga subterraneus*, as represented by the figures of Poey, differs very much from that of *L. dentatus* and of *Amblyopsis*. In all, the optic lobes are as largely developed as in allied fishes provided with well developed eyes. In *Lucifuga subterraneus* the cerebral lobes are separated by quite a space from the round optic lobes, which are represented as a little larger than the cerebral lobes, and also of greater diameter than the cerebellum; this latter being more developed laterally than in either *L. dentatus* or *Amblyopsis*. The three divisions of the brain are represented, from a top view, as nearly complete circles (without division into right and left lobes), of which that representing the optic lobes is slightly the largest. In *L. dentatus* the procencephalon and the optic lobes are represented as divided into right and left lobes, as in *Amblyopsis*, and the cerebellum does not extend laterally over the medulla oblongata as in *L. subterraneus*, but, as in *Amblyopsis*, is not so broad as the medulla, and, projecting forwards, covers a much larger portion of the optic lobes than is the case in *L. subterraneus*.

The Cuban blind fish has the body, cheeks, and opercular bones covered with scales. As in *Amblyopsis*, the eyes exist, but are so imbedded in the flesh of the head as to be of no use. The outline cut here given (Fig. 1), copied from Poey, is very characteristic of the form of the fish, but does not exhibit the fleshy cilia or details of scaling.

The first notice that I can find of the Mammoth Cave blind fish is that contained in the "Proceedings of the Academy of Natural Sciences of Philadelphia," vol. i. p. 175, where is recorded the presentation of a specimen to the Academy by W. T. Craige, M.D., at the meeting held on May 24, 1842, in the following words:—

"A white, eyeless crayfish (*Astacus Bartoni?*) and a small white fish, also eyeless (presumed to belong to a sub-genus of *Silurus*), both taken from a small stream called the River Styx in the Mammoth Cave, Kentucky, about two and one-half miles from the entrance."

Dr. DeKay, in his "Natural History of New York Fishes," p. 187, published in 1842, describes the fish, from a poor specimen in the Cabinet of the Lyceum of Natural History of New York, under the name of *Amblyopsis* spelæus*.† De Kay's description is on the whole so characteristic of the fish as to leave no doubt as to the species he had before him, though the statement that it has eight rays supporting the branchiostegal membrane (instead of six), and that the eyes are "large" but under the skin, must have been due the bad condition of his specimen, and to his taking the fatty layer covering the minute eyes for the eyes themselves, as pointed out by Prof. Wyman. Dr. DeKay places the genus with the Siluridæ (cat-fishes), but at the same time questions its connection with the family, and says that it will probably form the type of a new family. In 1843 Prof. Jeffries Wyman* gave an account of the dissection of a specimen in which he could not find a trace of the eye or of the optic nerve, probably owing to the condition of the specimen, as he afterwards† found the eye spots, and made out the structure of the eye. When describing the brain, Prof. Wyman calls attention to the fact of the optic lobes being as well developed as in allied fishes with well developed eyes, and asks if this fact does not indicate that the optic lobes are the seat of other functions as well as of that of sight. He also calls attention to the papillæ on the head as tactile organs furnished with nerves from the fifth pair.

Dr. Theo. Tellkampf‡ was the first to point out the existence of the rudimentary eyes from dissections made by himself and Prof. J. Müller, and to state that they can be detected in some specimens as black spots under the skin by means of a powerful lens. Prof. Wyman afterwards detected the eye through the skin in several specimens. Dr. Tellkampf also was the first to call attention to the "folds on the head, as undoubtedly serving as organs of touch, as numerous fine nerves lead from the trigeminal nerve to them and to the head generally."

It is also to Dr. Tellkampf that we are indebted for the first figure of the fish,§ and for figures illustrating the brain and internal organs. The descriptions of the anatomy of the fish by Drs. Tellkampf and Wyman are all that have ever been written on the subject of any importance, with the exception of the description of the eye by Dr. Dalton, whose paper, in the *New York Medical Times*, vol. ii. p. 354, I have not seen. Prof. Poey gives a comparison of portions of the structure with that of the Cuban blind fishes.

Dr. Tellkampf proposed the name of *Heteropygii*‖ for the family of which, at the time, a single species from the Mammoth Cave was the only known representative, and makes a comparison of the characters with those of *Aphredoderus Sayanus*, a fish found only in the fresh waters of the United States, and belonging to the old family of Percoids, but now considered as representing a family by itself, though closely allied to the North American breams (*Pomotis*), and having the anal opening under the throat, as in the blind fish.

Dr. Storer,¶ not knowing of Dr. Tellkampf's paper, proposed the name of *Hypsæidæ*, for the blind fish, and placed it between the minnow and the pickerel families, in the order of Malacopterygian, or soft-rayed fishes. According to the system adopted by Dr. Günther, it stands as closely allied to the minnows, *Cyprinodontidæ* (many of which are viviparous and have the single ovary and general character of the blind fish), and the shiners, *Cyprinidæ*, of the order of Physostomi. Dr. Tellkampf, in discussing the relations of the family, points out its many resemblances to the family of *Clupesoces*, and its differences from the Siluroids, Cyprinodontes, and Clupeoids, with which it has more or less affinity, real or supposed. Prof. Cope in his paper on the Classification

* Obtuse vision.
† Of a cave.

* *Silliman's Journal*, vol. 45, p. 94.
† Proceedings Boston Soc. Nat. Hist., vol. 4, p. 395. 1853.
‡ Muller's Archiv. fur Anat., 1844, p. 332. Reprinted in the New York *Journal of Medicine* for July, 1845 p. 84. with plate.
§ The only other figures of the species, that I am aware of, are the simple outlines given in Poey's "Mem. de Cuba," the woodcut in Wood's "Illustrated Natural History," and the cut in Tenney's "Zoology." None of these figures are very satisfactory.
‖ From the advanced position of the terminus of the intestine being so different from the position which it has in ordinary fishes
¶ "Synopsis of the Fishes of North America," published in 1846.

of Fishes* places the *Amblyopsis* in the order of Haplomi with the shore minnows, pickerel and mud fish, and in an article on the Wyandotte Cave,† he says that the Cyprinodontes (shore minnows) are its nearest allies. This arrangement by Prof. Cope places the Haplomi between the order containing the herrings and that containing the electric eel of South America, all included with the garpike, dog fish of the fresh waters (*Amia*), cat fishes, suckers, and eels proper, &c. in the division of Physostomi as limited by him.

Prof. Agassiz‡ in 1851 stated that the blind fish was an aberrant form of the Cyprinodontes.

Thus all those authors who have expressed an opinion as to the position which the fish should hold in the natural system, have come to the same conclusions as to the great group, division, or order, into which it should be placed. For all the terms used above, when reduced to any one system, bring *Amblyopsis* into the same general position in the system; its nearest allies being the minnows, pickerels, shiners, and herrings; and unless a careful study of its skeleton should prove to the contrary, we must, from present data, consider the family containing *Amblyopsis* as more nearly allied to the Cyprinodontes, or our common minnows having teeth on the jaws, than to any other family, differing from them principally by the structure of the several parts of the alimentary canal and the forward position of its termination.

I have thus far mentioned only one species of blind fish from the cave, the *Amblyopsis spelæus*. The waters of the cave not only contain another species of blind fish, differing from *Amblyopsis* in several particulars, especially by its smaller size and by its being without ventral fins, which I have identified as the *Typhlichthys subterraneus* of Dr. Girard; but also a fish with well developed eyes, as proved by the account given by Dr. Tellkampf, and by the drawing of a fish found by Prof. Wyman, in 1856, in the stomach of an *Amblyopsis* he was dissecting. It is very much to be regretted that the specimen is not now to be found, and that it was so much acted on by the gastric juice as to destroy all external characters by which it could be identified from the drawing which Prof. Wyman made of it, which is of about natural size. Dr. Tellkampf remarks on the fish with eyes are as follows:—

"Besides the colourless blind fish, there are also others found in the cave, which are black, commonly known by the name of mud fish. I saw a dark-coloured fish in the water, but did not succeed in catching it. The latter are said to have eyes, and are entirely dissimilar to the blind fish."

The name "mud fish," given to this fish with eyes, and the statement that it is of a dark colour, together with the drawing by Prof. Wyman of the fish found in the stomach of the blind fish, showing the position of the dorsal fin to be the same as in the fish commonly called mud fish in the fresh waters of the Middle, Western, and Southern States, perhaps indicates the fish with eyes to be a species of *Melanura*. This fish is called mud fish from the habit of burying itself in the mud, tail first, to the depth of two to four inches, and of remaining buried in the mud in our western ditches during a time of drought. This habit, perhaps, in a measure fits it for a subterranean life. The occurrence of a fish belonging to the same family with the blind fish, but with well developed eyes, in the subterranean streams in Alabama, however, renders it probable that the cave fish with eyes may be the same or an allied species, and the drawing by Prof. Wyman would answer equally as well for it.

The fact that the *Amblyopsis* succeeded in catching a fish of probably very rapid and darting movements, shows that the tactile sense is well developed, and that the blind fish must be very active in the pursuit of its prey; probably guided by the movement which the latter makes in the water so sensibly influencing the delicate tactile organs of the blind fish that it is enabled to follow rapidly; while the pursued, not having the sense of touch so fully developed, is constantly encountering obstacles in the darkness.

In describing the habits of the blind fish, Dr. Tellkampf says:—

"It is found solitary, and is very difficult to be caught, since it requires the greatest caution to bring the net beneath them without driving them away. At the slightest motion of the water they dart off a short distance and usually stop. Then is the time to follow them rapidly with a net and lift them out of the water. They are mostly found near stones or rocks which lie upon the bottom, but seldom near the surface of the water."

Prof. Cope, in describing the habits of the blind fish which he obtained in a stream that passes into the Wyandotte Cave, though he entered it by means of a well in the vicinity of the cave, says that—

"If these *Amblyopses* be not alarmed they come to the surface to feed, and swim in full sight like white aquatic ghosts. They are then easily taken by the hand or net, if perfect silence be preserved, for they are unconscious of the presence of an enemy except through the sense of hearing. This sense is, however, evidently very acute, for at any noise they turn suddenly downward, and hide beneath stones, &c., at the bottom. They must take much of their food near the surface, as the life of the depths is apparently very sparse. This habit is rendered easy by the structure of the fish, for the mouth is directed upwards, and the head is very flat above, thus allowing the mouth to be at the surface."

The blind fish has a single ovary, in common with several genera of viviparous Cyprinodontes. In three female specimens of *Amblyopsis* which I have opened, the ovary was distended with large eggs, but no signs of the embryo could be traced. In these three specimens it was the right ovary that was developed, and this was by the side of the stomach, and did not extend beyond it. The number of eggs contained in the ovary was not far from one hundred in the specimen examined. As the embryos develop, the mass probably pushes further back in the cavity and also extends the abdominal walls. That this fish is viviparous is proved by the statement made by Mr. Thompson before the Belfast Natural History Society* that one of the blind fishes from the cave, four and a half inches long, "was put in water as soon as captured, where it gave birth to nearly twenty young, which swam about for some time, but soon died. These, with the exception of one or two, were carefully preserved, and fifteen of them are now before us (at the meeting, I wish they were here), they were each four lines in length."

It is singular that no mention is made regarding these young, as to the presence or absence of eyes, and, as if it was fated that this important point should remain unnoticed as long as possible, it is equally singular that Dr. Steindachner omitted to examine some very young specimens which he received from a friend a few months since and sent to the Vienna Museum, where they will remain unexamined until he returns there. I saw the Doctor only a week after these, to me, interesting specimens had been sent abroad, and he was as grieved as I was disappointed at my being just too late to take advantage of them.

At what time the young are born has never been stated, but judging from such data as I can at present command, I think that it must be during the months of September

* *American Naturalist*, vol. v. p. 579, 1871.
† *Indianopolis Daily Journal* of September 5, 1871. Reprinted in Ann. Mag. Nat. Hist., Nov. 1871.
‡ *Silliman's Journal*, p. 128.

* Annals and Mag. of Natural History, vol. xiii. p. 112, 1844.

and October. Specimens collected during those months would probably contain embryos in various stages of development, the examination of which would undoubtedly lead to most interesting results.

The largest specimens I have seen of *Amblyopsis* are several males and females, each from four to four and a half inches in length, which seems to be about as large as the fish grows, though Dr. Günther mentions a specimen in the British Museum of five inches in length. The largest specimen captured of late years is said to have been taken during the summer of 1871, and sold for ten dollars to a person who was so desirous of securing the precious morsel that he had it cooked for his supper. The smallest specimen I have seen was one and nine-tenths inches in length. F. W. PUTNAM

NOTES

ADDRESSES of sympathy with Dr. Hooker, in the difficult position in which he is placed with regard to the management of Kew Gardens, are flowing in on all sides, from those interested in the maintenance of Kew as a scientific establishment. At a meeting of the leading botanists and horticulturists held last week at the office of the *Gardener's Chronicle*, resolutions were unanimously passed expressing the sympathy of the meeting with Dr. Hooker, and in favour of an address to Mr. Gladstone, calling attention to the eminent services rendered by him to science, and pointing out that it is absolutely essential for the efficient management of the establishment at Kew that the Director must have complete control over the subordinate officials, free from any interference in matters of detail from his official superiors. The same course has been followed by the Council of the Royal Horticultural Society, and by its various committees. At a Council of the Royal Botanic Society, specially summoned for the purpose on Saturday last, similar resolutions were passed. The Council of the Meteorological Society has sent to Mr. Gladstone, Lord Derby, Sir John Lubbock, and Dr. Hooker, copies of a resolution in the same direction.

AN interesting *soirée* was held at the Royal Albert Hall on Thursday evening last, under the auspices of the Society of Telegraph Engineers, with the aid of the Postal Telegraph Department. A model of every kind of telegraph instrument which has been generally used for commercial purposes from the opening of telegraphs to the present time was exhibited. Each apparatus was connected up with wires proceeding from the central hall to the galleries, and thus the actual practical working of the telegraph system was made apparent to those present in a very effective manner. A descriptive lecture was at the same time given by Mr. W. H. Preece, of the Postal Telegraph Department, who lucidly explained the action of the electric current in producing the simple elementary signals. The admirable manner in which the lecturer made himself heard in every part of the vast assembly was a general subject of remark. The most interesting feature of the entertainment was the direct and instantaneous working with India. Kurrachee, the terminus of the Indo-European line in India, a distance of upwards of 5,000 miles, was the town selected, and, in reply to a message of inquiry, that station said, "Here, Kurrachee," and followed it up with the announcement that "locusts are swarming in Scinde." After the conversations held with India from the Albert Hall were concluded, the Grand Vizier of Persia, who was at Teheran, sent to the Albert Hall a warm message of congratulation to the Prince of Wales.

Les Mondes speaks with contempt and surprise of the treatment which science meets with at the hands of the British Government, referring particularly to the refusal of the latter to grant a sum for the purpose of enabling scientific men to make marine explorations on the coasts of Europe and India, and to the conduct of Mr. Ayrton towards Dr. Hooker. The latter is spoken of as a gentleman "eminent, talented, universally honoured for his integrity, loved for his courteous manner and the goodness of his heart, and who has devoted to the service of the State a life not only laborious but illustrious."

THE new French Association for the Advancement of Science intends to hold its first meeting at Bordeaux in the month of September next, commencing on the 5th.

WE are sorry to see the following paragraph in *Les Mondes* of July 18:—"No one has yet responded to the appeal we made to the French *savants* to take part in force (*en nombre*) in the Brighton meeting, which opens on August 14. We shall not organise this scientific excursion unless we are able to count upon a sufficient number of adherents. There is no time to lose." We hope it is not too late, and that something like a representative party may be yet organised from among all classes of French men of science. We are sure we can promise they will be heartily welcomed and hospitably treated at Brighton.

THE Annual Meeting of the British Medical Association will be held in Birmingham on the 6th, 7th, 8th, and 9th days of August next.

MR. W. A. TILDEN, D.Sc. Lond., Demonstrator of Practical Chemistry to the Pharmaceutical Society, has been appointed Chemical Master in Clifton College.

BY the provisions of the late Dr. William J. Walker's foundation, two prizes are annually offered by the Boston Society of Natural History for the best memoirs, written in the English language, on subjects proposed by a Committee appointed by the Council. For the best memoir presented, a prize of sixty dollars may be awarded; if, however, the memoir be one of marked merit, the amount may be increased, at the discretion of the Committee, to one hundred dollars. For the memoir next in value a sum not exceeding fifty dollars may be given; but neither of these prizes is to be awarded unless the papers under consideration are deemed of adequate merits. Memoirs offered in competition for these prizes must be forwarded on or before April 1, 1873, prepaid, and addressed, "Boston Society of Natural History, for the Committee on the Walker Prizes, Boston, Mass." Each memoir must be accompanied by a sealed envelope enclosing the author's name, and subscribed by a motto corresponding to one borne by the manuscript. The subject of the Annual Prize of 1873 will be "On the Development and Transformations of the Common House Fly."

THE Academy of Sciences and Belles Lettres of Caen has offered a prize of 4,000 francs for a paper on "The part played by leaves in vegetation." What is wanted is an account of exact experiments and new facts calculated to clear up, invalidate, confirm, or modify the doubtful points in the received theories. Papers must be sent in before December 31, 1875, addressed to M. Travers, secretary of the Academy, Caen.

ON Thursday last Mr. Thomas Baring, M.P., F.R.S., distributed prizes and certificates to the students who had passed the examinations connected with the educational courses of lectures delivered at the London Institution during the past session. In the examination on Prof. Huxley's lectures, "On the Physiology of Bodily Motion and Consciousness," the first prize was gained by H. B. Hyde, jun.; the second by Miss Caroline Lloyd; and the third by A. J. Wallis. In the examination on Dr. Odling's course, "On Elementary Chemistry," the first, second, and third prizes were carried off by H. Louis, A. J. Richardson, and Miss Eleanor F. Garrett. In the examination connected with the lectures "On the Theory of Music," delivered by Mr. E. J. Hopkins, the first prize was obtained by Alfred Hare, and the second by Miss Frances S. Voysey. In

the examination on Prof. Bentley's course, "On the Classification of Plants," Miss Eleanor F. Garrett gained the first prize, Miss Elizabeth J. Garrett the second, and L. T. Thorne the third. Sixty-six Pass Certificates were granted, and no fewer than twenty-two young ladies were among the successful candidates.

WE are glad to see from the report on the teaching of Practical Physiology in the London schools, which appears in the *Medical Times* of Saturday, that the means and method for teaching this subject to University College, the only one yet reported on, are in every way satisfactory.

Harper's Weekly records the death of Mr. Coleman T. Robinson, of New York, a gentleman well known for the attention he has paid to the subject of American lepidoptera. A man of wealth, and able to gratify his tastes in this direction, he acquired, by his own efforts and by purchase, a very extensive cabinet, embracing over twenty thousand specimens, which not long ago he presented to the American Museum of Natural History in the Central Park. Mr. Robinson published quite extensively upon American lepidoptera—a paper upon the sphinges of Cuba being perhaps the most important. In some of his labours he was associated with Mr. Grote.

WE hear from America of the death of Mr. Robert Swift, who had paid great attention to terrestrial and marine mollusca. His valuable cabinet of shells will be presented by his daughter to the Philadelphia Academy of Sciences. He had already given to the Museum of the Smithsonian Institution an extensive series of specimens of the birds of St. Thomas and Porto Rico.

A MONUMENT has been erected to Boerhaave, the great naturalist and physician, at Leyden in Holland. The statue is 11 ft. 8 in. high, and stands on a pedestal of 10 ft. in height. The figure represents him as a professor lecturing.

WE learn from the *Times of India* of June 28 that the *Madras Mail* and other journals in the Madras Presidency are altogether discontented with the recent judgment of the Court of Inquiry as to the cyclone, to which we referred last week. The *Mail* has subjected the judgment to a searching criticism, and, after comparing it with the evidence, concludes that the Court throughout has erred egregiously on the side of severity. It seems to be thought that the Board of Trade will either reverse or modify the Court's judgment.

DR. E. DECAISNE, in a note communicated to the Academy of Sciences on the 3rd of June, shows that from the triple point of view of the fecundity of marriages, of the absolute number of births, and of the excess of births over deaths, France occupies the lowest place of all the European States. In Prussia 100 marriages give 460 children; in France the same number of marriages give only 300 children. In Prussia the number of births for each 100 of the population generally is 3·98 per annum; in France there are only 2·55. In Prussia the excess of births over deaths in each million of the population is 13,000 per annum, whilst in France it only amounts to 2,400. "If we admit," says Dr. Decaisne, "as a conclusion from the above figures, that the doubling of the population of France, despoiled of two of its finest provinces, and by unheard of disasters, will require 170 years to be effected, whilst that of Prussia requires only 42 years, Great Britain 52, and Russia 66, some estimate may be made of the amount of the evil that has befallen this country." He does not hesitate to say that, as the first step towards the restoration of the former power of France, the first thing that should occupy her statesmen is the reconstitution, the reorganisation of human life; and everyone should throw aside that false patriotism which has been the cause of so much ill.

AT its last session the Congress of the United States made an appropriation of 75,000 dollars for the continuation of Professor Hayden's geological exploration of the Territories; and on the 15th of June the Professor had already established his camp at Ogden, Utah, preparatory to prosecuting his researches. The present session seems likely to be marked by still further additions to our knowledge of the physical and natural history of the central and western regions of the United States.

ACCORDING to the *Yale College Courant*, a new era in the educational development of the Japanese has been entered upon in the opening of an exhibition of curiosities of nature and art in Yedo in the beginning of April last. The formation of collections of this kind is usually characteristic of an advanced stage of culture; and in imitating the European and American example in this respect the Japanese show their great superiority to the Chinese and other Oriental nations. The exhibition referred to was opened in a temple sacred to the spirit of Confucius, and situated in the grounds of the old Chinese college. This institution was the chief seat in Japan of the study of Chinese literature, but has been closed for some years, as the study of the Chinese has now become obsolete. The exhibition, to which a charge for admission of about two cents was made, was projected by the Japanese themselves; and although small, yet, according to the writer in the *Courant*, it was really very good and well selected. The specimens were those mainly pertaining to the fauna and flora of Japan, embracing reptiles, fishes, insects, and birds, the last being well stuffed and mounted. Specimens of timber, in polished slabs, were exhibited; and the cases of insects were filled with a very great variety of species. To the wonders of nature were added numerous art curiosities, mainly of old and rare patterns of articles of lacquered bronze.

A CORRESPONDENT of the *Chemical News* asks why the Cavendish Society has for years ceased to publish works of scientific value, and suggests that some of the works of its founders and officers—Faraday, Graham, Hofmann, Daubeny, Miller, Stenhouse—as well as others, are so scarce, that their publication by the Society would be a boon to many, and would remunerate it for its trouble in publishing them.

GENERAL MYER, the indefatigable head of the United States Signal Service, has proposed, according to *Harper's Weekly*, to take the occasion of various balloon ascensions during the present season, especially from Boston, to make observations in regard to the temperature, barometric pressure, and the currents of the higher altitudes of the atmosphere. Sergeant Schaeffer, of the corps, has been detailed for this purpose, and has been in training for some time preparatory to his important work.

WE learn from the Fifth Annual Report of the Peabody Institute of Baltimore, for the year ending June 1, that upwards of 13,000 dollars have been spent on books and binding, and that during the year 120 lectures have been delivered at the Institute—30 being popular lectures, and 90 what are called class lectures, designed for more minute instruction in special branches of knowledge." Of the popular lectures Prof. W. H. Miles gave ten—mostly geological—on such subjects as "Revelations of the Microscope and of the Deep-Sea Soundings," "Coral and the Coral Islands," "Glaciers, Rivers, and Oceans," "The Geological History of Man," &c. The class lectures are divided into six courses, of which two are scientific—the one consisting of twenty lectures on Physiology by Prof. F. T. Miles, of Maryland University, and the other twenty lectures on Sound and Heat by Prof. H. C. White, of St. John's College, Baltimore. The average number in each class, independent of single admission, was thirty-seven, the charge for a course of twenty lectures being only 3 dols. During the year 3,883 volumes besides pamphlets have been added to the library, the number of readers having been 2,582.

JUDGING from the Report of the Exeter Science School in connection with the Government Department of Science and Art, it seems altogether in a very satisfactory condition. There are classes for Physical Geography, Mathematics, Acoustics, Light and Heat, Chemistry, Animal and Vegetable Physiology, Geology, Mineralogy, Metallurgy, Botany, Building Construction, Theoretical Mechanics, Machine Construction, and Drawing. From the number who have passed the examinations, it would appear that the classes must have been well attended, and several of the students have most creditably distinguished themselves.

THE following is a list of the electric lights in England and France with the dates at which they were erected :—Dungeness, Jan. 1862; Cape La Heve, France, South Light, Dec. 1863; North Light, Nov. 1866; Cape Grisnez, France, Feb. 1869; Souter Point, England, Jan. 1871; South Foreland, 2 lights, Jan. 1872. It is interesting to see that England took the lead in this matter of the adaptation of electric illumination to light-house purposes, and it must also be remembered that although the first electric light was only erected in 1862, yet that in 1859 experiments were made under the supervision of the late Prof. Faraday which were very successful.

MR. WILLIAM F. DENNING, of Bristol, writes us that the sun's surface has recently been in a very disturbed condition. On observing the sun on the afternoon of the 12th instant with an old 4in. metallic-mirror reflecting telescope, he noticed a large scattered group of spots in the north-eastern quadrant of the disc. This group contained no less than twenty-seven individual spots, one of which was of considerable dimensions. It was situated on the eastern portion of the group, and was constituted of three well-defined umbræ and a large irregular penumbra, which on the east side was very dark, and on the exterior edge pierced with a train of minute dark spots. In the other quadrants Mr. Denning noticed four groups and one isolated spot surrounded by penumbra in the north-western quadrant. These groups (though insignificant in regard to the dimensions of the spots which composed them) contained twenty-one spots in all; so that, including the large cluster before referred to, there were forty-eight dark spots seen altogether. Several groups of faculæ were also perceptible in the vicinity of the margin of the disc.

THE first number of the *Journal of the Society of Telegraph Engineers* contains, besides a list of members, and the rules and regulations, a record of the proceedings of the Society since its formation, including reports of the papers read, and the discussions which followed. The members already number about 280, and among them are the names of some of the most eminent scientific men of the time, the President being Charles W. Siemens, and the Vice-Presidents Lord Lindsay and Frank I. Scudamore, C.B. The society "is established for the general advancement of Electrical and Telegraphic Science, and more praticularly for facilitating the exchange of information and ideas among its members," and consists of members, associates, students, and honorary members. Besides the President's address, in which he justly maintains that such a special society "is necessary for the more rapid development of a new and important branch of applied science," the report contains a paper by Mr. R. S. Culley on Automatic Telegraphs, and a sketch of the Progress of Sea Telegraphy by Captain Colomb, R.N. The latter half of the volume consists of "Abstracts and Extracts," bearing on the department with which the society is concerned.

A NOVEL kind of magazine has made its unpretending appearance—*Loose Leaves*, a magazine conducted at the Church Stretton Private Asylum. It is written almost entirely by members of the asylum, and we have seen many madder publications proceeding from those who consider themselves sane. As an effort to occupy the minds of the unfortunate inmates of such establishments, the attempt is commendable, and worthy of all success and imitation.

HISTORICAL ECLIPSES

MR. J. R. HIND, writing from Mr. Bishop's Observatory, Twickenham, to the *Times*, supplies the following interesting sketch of the Eclipses recorded in History :—

"It is well understood that the historical eclipses, especially those of the sun, have an important bearing upon our knowledge of the elements of the moon's motion, as affording the means of testing the accuracy of those elements when carried back to very remote times. I send you a brief account of some results I have deduced in a systematic examination of these eclipses, making only such a selection therefrom as may possibly possess interest for the general reader. I shall omit any reference to the purely astronomical conclusions to which I have been led, which would be out of place in your columns, and, indeed, would extend this communication beyond reasonable limits. It may, however, be desirable to state that I have employed the last value of the secular acceleration of the moon's mean motion given by Prof. Hansen, of Gotha, the author of the latest lunar tables, and have combined other important elements as determined by him with the results of M. Leverrier's tables of the sun. From recent investigations it appears by no means improbable that we may have to rely wholly upon the ancient eclipses in fixing the true amount of acceleration in the motion of our satellite.

"I shall follow the chronological order in the subjoined remarks upon some of the better known eclipses of history. These form a part only of the phenomena I have rigorously examined upon the same system of calculation.

"1. The Nineveh Eclipse of B.C. 763, June 15.—The discovery of the record of this eclipse on one of the Nineveh tablets in the British Museum was announced by Sir Henry Rawlinson in the *Athenæum* of May 18, 1867, to which I refer for details of its bearing on the sacred and profane history of the period. In the actual state of our knowledge it is the *terminus a quo* for researches on the historical eclipses, and I believe I am correct in saying its value in an astronomical point of view is greater than that attaching to the famous eclipse predicted by Thales to the Ionians, as mentioned by Herodotus. The underlining of the inscription appears to indicate a phenomenon of unusual character, or that the eclipse was total in or near Nineveh. Adopting for the position of the city the longitude and latitude deduced by the Astronomer Royal for the pyramid of Nimrud, I find the calculated southern limit of totality would pass a few miles south of Nineveh, leaving a very large partial eclipse at that city. Very trifling corrections in the lunar elements employed would suffice to bring the total eclipse over it. In this longitude the duration of totality on the central line would be 4m. 20s., the middle of the eclipse at half-past 9 local time.

"2. The Eclipse of B C. 689, January 11.—The idea that the retrogression of the shadow on 'the dial of Ahaz' during the illness of Hezekiah may have been connected with a solar eclipse has given rise to much discussion, and several writers have endeavoured to point out how the occurrence might thus be explained. Of the eclipses to which attention has been directed, the above has perhaps appeared the more probable. It was an annular eclipse, and at Jerusalem the sun would present the form of a luminous ring for 7½ minutes, the middle at 10h. 18m. In Babylon it would have the same appearance for seven minutes. It seems hardly probable that the eclipse could have occurred much later in the day, though more than one author has considered the circumstance essential for the explanation of the retrograde motion of the shadow on the ancient form of sun-dial by an eclipse. I must leave the reader to judge how far the expression 'the wonder done in the land' may relate to such a phenomenon, which is, of course, a very rare one in a particular locality.

"3. The eclipse of Thales, B.C. 585, May 28.—This eclipse, which, as Herodotus informs us, terminated the six years' war between the Medes and Lydians under Cyaxares and Alyattes, when during a battle 'day was suddenly turned into night,' has greatly exercised the chronologist and the astronomer, and although, misled by imperfect tables of the lunar motions, they have fixed upon other eclipses from time to time, it has been known for some years past that the date distinctly assigned by Pliny (the fourth year of the 48th Olympiad) is the correct one. My new calculation throws the shadow precisely over the tract of country where with the greatest probability it has been supposed the contending armies were situated, and in addition it indicates a circumstance which I believe has not resulted from any previous calculation, and which may not be without its

chronological import, viz., that the eclipse was total in Nineveh for between three and four minutes shortly before sunset. The date of the final destruction of Nineveh is closely connected with the eclipse of Thales.

"4. The Eclipse of Xerxes, B.C. 478, February 17.—Much difficulty has been experienced by chronologists with regard to an eclipse which occurred, according to Herodotus, in the early spring, when Xerxes was setting out from Sardis on his expedition against Greece. It is certain there was no such phenomenon in the year B.C. 480, to which this event is usually referred, and in examining the eclipses about this period I have found only one that can apply. There is no doubt that the sun was very largely eclipsed at Sardis on the morning of February 17, B.C. 478. A direct calculation for this place shows that more than 94-100ths of the sun's diameter would be covered, the greatest phase ten minutes after 11, local time. The eclipse was annular, and Sardis appears to have been just outside the annulus. One other eclipse only was visible in eastern Europe about this year, it occurred B.C. 479, October 2, and has been considered to be the one which occurred at the time Cleombrotus consulted the oracles at Sparta. Its magnitude there is found to have been about 6-10ths, the greatest eclipse at 0h. 50m. If the eclipse of B.C. 478 be truly the one recorded by the historian, the date of the battle of Salamis will be required to be brought down two years.

"5. The Eclipse of Agathocles, B.C. 310, August 15 (Diodorus, Justin).—On the morning after the fleet of Agathocles sailed from Syracuse for Africa, the historian tells us the sun was eclipsed to such a degree (*tantum fit solis deliquium*) that the stars everywhere appeared as at night. Though Agathocles could hardly have been more than 100 miles from Syracuse, it is uncertain in which direction he had sailed, or whether he was rounding Sicily on the north or south side, and this circumstance detracts from the scientific value of the record. My calculation throws a central line near the African coast, so that the fleet, if sailing southwards, would be near the northern limit of totality.

"6. The Eclipse on the Passage of the Rubicon by Cæsar (Dion), B.C. 51, March 7.—This would appear to have been a very notable phenomenon on the Rubicon and in Northern Italy generally. The eclipse was annular, and the annular phase continued 6m. 30s. At Rome there would be a partial eclipse, about three-fourths of the sun's diameter being covered. A line drawn from 9° 24′ E., and 43° 26′ N., to 14° 39′ E., and 46° 15′ N., will define the course of the central eclipse across Italy, and the ring-formed appearance of the sun would extend to about 1° 35′ north and south of this line. The Rubicon would be placed about midway between the central line and the southern limit. Near Ariminum the middle of the eclipse occurred at 0h. 50m. By some writers (including the Abbé du Fresnoy, in his valuable 'Tablettes Chronologiques,') the eclipse is dated B.C. 50; the above, however, is the correct year.

"A great eclipse has been referred to the year B.C. 43 or 44, soon after the death of Julius Cæsar, and it is instanced by Baron de Zach and M. Arago as the first annular eclipse upon record. Calculation shows that there could not have been an eclipse, annular or otherwise, visible in Italy in either of those years, nor, indeed, for several years before or after. The phenomenon alluded to was, no doubt, of a meteorological character, and this would appear from the passage in Suetonius, one of the authors quoted upon the subject.

"7. The Eclipse of Herod (Josephus).—The lunar eclipse which I take to be the one recorded by the Jewish historian during Herod's last illness occurred B.C. 1, January 9. On this occasion the moon passed nearly centrally through the earth's shadow, entering in at 11h. 23m. P.M. mean time at Jerusalem, and emerging at 2h. 57m. A.M. on the 10th; the total eclipse continued 1m. 39s. This is the date recognised by Calvisius and recently supported by Mr. Bosanquet. An eclipse in B.C. 4 on the night between March 12-13, which other chronologists have supposed to be the one referred to, was partial only, and did not commence till 1 A.M.; little more than half the moon's diameter was immersed in the earth's shadow at greatest phase.

"8. The Eclipse of Phlegon in the 202nd Olympiad (Eusebius) A.D. 29, November 24.—Total on a line crossing the Black Sea rather west of Odessa in Sinope, thence near the site of Nineveh to the Persian Gulf. At Jerusalem a partial eclipse; about 11.10 A.M. eight-tenths of the sun's diameter would be covered; at Heliopolis (Baalbec) also partial—nine-tenths. At a point on the central line near Sinope the totality would continue 1½ minutes. Humboldt mentions that this eclipse had been calculated by Wurm, but I have not met with his results. It is the only solar eclipse that could have been visible in Jerusalem during the period usually fixed for the ministry of Christ.

"The moon was eclipsed on the generally received date of the Crucifixion, A.D. 33, April 3. I find she had emerged from the earth's dark shadow a quarter of an hour before she rose at Jerusalem (6.36 P.M.): but the penumbra continued upon her disc for an hour afterwards.

"9. The Eclipse of 113, May 31.—Kepler, after endeavouring to ascertain the date of a total eclipse mentioned by Plutarch as having 'recently occurred about noon,' when the darkness was like that of night, and stars were seen in all directions, states he had found none which accorded better with the description than the above. On submitting it to calculation on the modern elements, the central line appears to have passed too far north—over central Germany. I have not succeeded in discovering the date of this eclipse, though I have accurately examined several at the close of the first and beginning of the second century.

"10. The Eclipse of 418, July 19.—Very large at Constantinople, according to Philostorgius, who relates that at the eighth hour of the day the sun was so far eclipsed that the stars appeared, and a comet which had not been previously perceived became visible during the obscurity, and was watched for more than four months afterwards. According to my calculation the central line passed somewhat to the south of Constantinople, where ninety-five hundredths of the sun's diameter would be covered. At a very short distance below that point the eclipse would be total. This is the second occasion upon which the discovery of a comet during a total, or nearly total, eclipse of the sun is recorded in history.

"11. The Eclipse of 671, December 7, on the attempted removal of the pulpit of Mahomet from Medina.—Prof. Ockley, in his 'History of the Saracens,' mentions on the authority of several Arabian writers, a large solar eclipse which occurred about the 52nd year of the Hegira. The Caliph Moawiyah having formed the intention of removing the Prophet's pulpit from Medina to his residence at Damascus, his people proceeded to do so, 'when immediately to their great surprise and astonishment the sun was eclipsed to that degree that the stars appeared.' Baron de Zach refers the eclipse to 674, October 4, but in this he is certainly mistaken—I believe through a wrong assumption as regards the moon's latitude. The correct date would appear to be 671, December 7. The eclipse of this day was annular on the central line. At Medina the greatest phase occurred at 10h. 43m., when 85-100ths of the sun's diameter would be obscured. In the clear skies of that part of the world such a degree of eclipse might be sufficient to bring out the brighter planets or stars. No larger eclipse, visible at Medina, occurred about this epoch.

"12. The Eclipse of 840, May 5.—Among the causes which are said to have brought on the *maladie de langueur* that terminated the life of Louis le Débonnaire was 'the fright which a total eclipse of the sun had occasioned him.' It is related that the King was taken ill at Worms, and having been removed to Ingelheim, near Mayence, he died there on June 20. I find the northern limit of totality in this eclipse passed about 100 miles south of Worms, and on the central line in this longitude the total eclipse continued 5m. 25s., an unusually long interval for the latitude of Central Europe. The middle occurred at 1.15 P.M., with the sun at an altitude of 57°. The phenomenon under such circumstances must have been a very imposing one, and well calculated in those days to inspire alarm.

"I have already described in your columns the track of the total eclipse of 1140, March 20 (William of Malmesbury) across this country, and merely refer to it now to add, that if any one of your readers is aware of its being recorded as total in London, he might be doing an astronomical service by making the fact generally known.

"13. The Eclipse of 1133, August 2 (William of Malmesbury), a great solar eclipse, considered as foreboding evil to Henry I. of England.—The central line traversed Scotland from Ross to Forfar, and the eclipse was, of course, large in every part of the country. It would be total in Northumberland. In the centre of Forfarshire totality continued 4m. 20s. Berwick-upon-Tweed was about 20 miles within the south limit.

"During the existence of the kingdom of Jerusalem there is mention of an eclipse which would appear to have been total in the city or its immediate neighbourhood, and has been variously dated from the election of Godfrey of Bouillon in 1097. I am

inclined to think it must be to the eclipse of August, 1133, that the record applies, though previous or subsequent events may have been mixed up with it by the historian. Continuing the calculation of the track of total eclipse after leaving this island, I find it would enter Palestine near Jaffa, and pass over Jerusalem and Hebron, where the sun would be hidden 4¼ minutes about 3 P.M., and from Nablous on the north to Ascalon on the south the country would be in darkness for nearly the same interval. The magnitude of the eclipse of 1187, September 4, was rather more than 9-10ths at Jerusalem, the central line passing between eight and nine degrees to the north; in the eclipse of 1191, June 23, the magnitude was about 7-10ths.

"14. The Eclipse of 1433, June 7, long remembered in Scotland as "the black hour."—It was a remarkable eclipse, the moon being nearly in perigee and the sun not far from apogee. The central line traversed the country in a south-easterly direction, from Ross to Forfar, passing near Inverness and Dundee. Maclaurin mentions that in his time a manuscript account of this eclipse was preserved in the University of Edinburgh, wherein the darkness is said to have come on about 3 P.M., and to have been very profound. By direction calculation for Edinburgh I find the total eclipse commenced at 3h. 3m., and continued 3m. 41s. At Inverness totality continued 4m. 32s. The after course of this eclipse was north of Frankfort on the Main and Munich, over the Dardanelles, south of Aleppo, and thence nearly parallel to the course of the Euphrates to the north-east border of Arabia. The totality was observed in the Turkish dominions according to Calvisius.

"15. The Eclipse of 1598, February 25.—Maclaurin says the memory of this eclipse was preserved among the people of Scotland, and 'that day they termed Black Saturday.' He adds:—'There is a tradition that some persons in the north lost their way in the time of this eclipse, and perished in the snow'—a statement the probability of which our experience of recent phenomena by no means tends to support. The central eclipse may be described as having passed about five miles south of Stranraer to the Bass Rock, a little south of Edinburgh, or, more precisely, over Dalkeith. Totality came on at Edinburgh at 10h. 15m., and continued 1m. 30s. The duration was the same at Douglas, Isle of Man. From the rapid motion of the moon in declination the course of the central line was a quickly-ascending one, in latitude on the earth's surface, the total eclipse passing off within the Arctic circle. Kepler must refer to another eclipse which was observed by Jessenius at Torgau, on the Elbe, though he gives the above date.

"16. The Eclipse of 1652, April 8, to which reference is also made by Maclaurin as 'still famous among the populace of Scotland, and known among them by the appellation of Mirk Monday.'—The central line passed over the south-east of Ireland, near Wexford and Wicklow, arrived on the shores of Scotland near Burrow Head, Wigtonshire, and running within a few miles from Edinburgh, Montrose, and Aberdeen, left the island at Peterhead. Greenock and Elgin would be situate near the north limit, and the Cheviots and Berwick upon the south limit of totality. The eclipse was observed at Carrickfergus, Ireland, by Dr. Wyberd. I find by direct calculation for this place that it was only just within the north limit of totality, which would commence at 10h. 8m. 30s., and continue 44 seconds. This short duration may partly explain a curious remark of Dr. Wyberd, that when the sun was reduced to 'a very slender crescent of light, the moon all at once threw herself within the margin of the solar disc with such agility that she seemed to revolve like an upper millstone, affording a pleasant spectacle of rotatory motion.' Wyberd's further description clearly applies to the corona.

"I believe it has been generally supposed that the last total eclipse of the sun visible in England was that of 1715, May 3, so well recorded by Halley in the 'Philosophical Transactions' of the Royal Society, and I was under this impression myself until, on calculating the elements of the eclipse of 1724 (May 22), observed at Paris, and by the French King at the Trianon, I discovered that before reaching France the belt of totality must have traversed the south-west of England, and it now appears that the totality did not pass by us unrecorded.

"I am indebted to the Astronomer Royal for referring me to an account by Dr. Stukeley, who observed the eclipse from Salisbury Plain. The duration of totality in that locality would be rather less than three minutes. The eclipse of 1724 is therefore the last that has been total in England, and as I have shown in a previous communication, there will be no other till August 11, 1999, and that will be confined to the south-west corner of the country."

ON PHOTOGRAPHIC IRRADIATION IN OVER-EXPOSED PLATES*

THE most cursory observer of any of the recent corona photographs must have remarked the apparent eating-in of the prominences over the limb of the dark Moon. A more careful examination of the photographs shows that the whole limb of the Moon is more or less eaten into, and that the indentations under the prominences are only exaggerations of a phenomenon which is present at all parts of the limb, but which varies in intensity according as the dark limb of the Moon is projected on a brighter or less luminous background.

In all over-exposed photographs of luminous objects upon a dark background, the brighter parts of the picture are found to be surrounded by a nebulous haze or border of light, which increases the diameter of the image formed by the luminous objects at the expense of those which are less luminous.

This nebulous haze has often been spoken of as "the extension of the chemical action," but without begging the question of its cause, we propose to speak of it as photographic irradiation. It has been found to vary with the time of exposure, and the relative brightness of the object and its background.

On examining the effects of photographic irradiation in a decidedly over-exposed picture, it will be seen that the nebulous fringes round luminous objects are distinctly divided into two parts—an inner and very marked border of light, following the contour of the luminous objects, and an outer and much less definite haze, thus:—

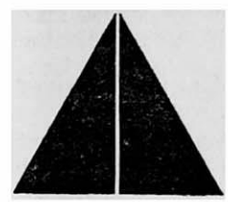

FIG. A

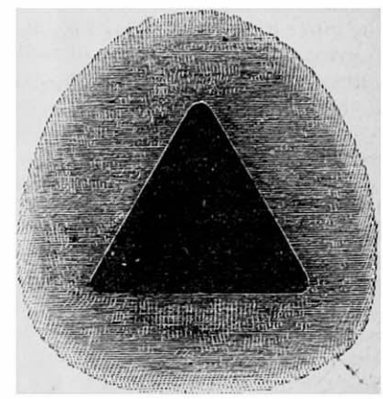

FIG. B

where Fig. A represents a normal photograph, and Fig. B a decidedly over-exposed plate from the same object.

The inner border of light fades gradually from the inside outwards, and it is very difficult, and indeed impossible, to tell where the true image of the luminous object ends, and its photographic irradiation begins. While, on the other hand, the boundary between the outer and inner fringes (or halos) of irradiation is more definitely marked, although it would be difficult to say with any absolute precision, at what point the inner fringe terminates.

Our first experiments were devised in order to test whether reflections from the back surface of the plate played any part in the production of the fringes; for this purpose plates of ebonite and the so-called non-actinic yellow glass were prepared.

In the over-exposed photographs taken on ebonite, it was found that the outer haze had entirely disappeared; while in the photographs taken on plates of yellow glass the outer haze is still distinctly to be traced, though it is much fainter than on an ordinary white glass plate with the same exposure.

By placing a piece of wetted black paper at the back of an unground plate the outer haze may be greatly reduced, while it was found that by grinding both the back and the front surfaces of a yellow glass-plate, and covering the back with a coating of black varnish, the outer haze may be rendered quite imperceptible, while, however, the inner border of irradiation still remains as before.

From these experiments we may conclude that the outer haze is produced by reflections from the back of the plate; and the action of the wetted black paper in reducing the outer irradiation may be explained by the consideration that the change of refractive index in passing from the glass to the film of water behind, is much less than in passing from glass into air. There is, consequently, less reflection at the back surface of the plate; most

* By Lord Lindsay and Mr. A. Cowper Ranyard. Reprinted from the Monthly Notices of the Royal Astronomical Society, June 14, 1872.

of the light emerges into the film of water, and is then absorbed by the black paper.

Fig. C represents a photograph taken upon a yellow glass plate, with a backing of wet black paper, but otherwise exposed under similar conditions to the photograph represented in Fig. B.

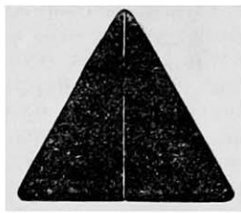

Fig. C

The outer irradiation halo may therefore be entirely avoided for the future in any corona or other necessarily over-exposed photographs by the use of the opaque plates. If, however, it is considered important that the negatives should be capable of being copied by transmitted light, the outer halo may still be to a great extent avoided by the use of transparent glass plates with a backing of wet black paper or black varnish.

Secondly, as to the inner and more definitely-marked irradiation edge which remained and seemed to be unaffected by the precautions that had served to rid us of the outer halo. Since the inner fringe was equally to be found on an opaque and on a transparent plate, we felt ourselves justified in seeking for its cause in front of the first or upper surface of the prepared plate; that is, it must be referred either to some action taking place within the thickness of the collodion, or to the optical imperfections of the instrument.

In order to determine whether the scene of action lay within the thickness of the collodion, we placed an ivory ruler with a bevelled edge in immediate contact with the collodion film. The plate with the ruler upon it was then exposed within the camera, so that the image of an incandescent platinum wire fell partly upon the collodion film, and partly upon the ivory ruler. If the scene of action lay within the collodion film, we might expect the inner irradiation fringe to extend itself under the edge of the ruler, while if it were due to the optical imperfections of the lens, the image of the wire would be cut off sharply by the edge of the ruler.

On removing the plate from the camera, and before the ruler was shifted from its place on the collodion, the whole was exposed for a few seconds to the action of the light from a gas-burner, in order that the position occupied by the edge of the ruler might be faintly printed upon the collodion film. On developing the plate, it was found that the image of the wire was sharply cut off at the place occupied by the edge of the ruler, as in Fig. D.

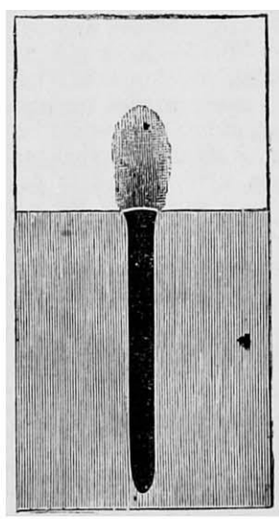

Fig. D

The very faint action extending inwards under the ruler being evidently due to the want of perfect opacity in the ivory, it seems, therefore, to be clearly proved that the inner irradiation edge is not caused by any chemical or other action taking place within the thickness of the collodion; but must be referred to the optical imperfections of the instrument which throws the image upon the collodion film.

It is instructive to remark that the photographic image of the wire is not cut off by an absolutely straight line at the edge of the ruler, but it is slightly convex, and is separated from the faint action which has apparently taken place through the ruler by a very narrow bright line, which appears to indicate the presence of a small capillary film of liquid along the edges of the ruler, forming a minute cylindrical lens. At the point where the collodion was acted upon by the light, the minute cylindrical lens appears to have been interfered with, and depressed inwards towards the ruler; we may therefore conclude that the collodion film is slightly swelled or thickened by the action of the light upon it.*

The cause of the inner irradiation-edge seems to be that every point of a luminous object is not represented by a simple point of light in the luminous image; in other words, the circle of least diffusion of any pencil is a curve of sensible area, of which the central and most intense portions imprint themselves first upon the collodion.

Some further experiments were made in order to test whether the size of the circles of least diffusion was chiefly owing to chromatic aberration (in which case the difficulty might be got rid of by the use of reflectors); for this purpose a bath of solution of sulphate of copper was placed in front of a gas-burner, and the triangular diaphragm shown in Fig. A was then placed between the sulphate of copper bath and the lens of the camera; but the blue screen thus formed seemed to have very little effect in altering the breadth of the irradiation-fringe, only slightly retarding the rate of its formation; a similar result was obtained on placing a piece of yellow glass in front of the diaphragm; in this case, however, the formation of the fringe was still further retarded.

Photographers have long known that by making use of stops they can obtain a much sharper image. By way of experiment, we cut off the edges of the lens with a circular stop, and found that the inner irradiation-fringe was thus greatly decreased. It seems, therefore, fair to argue that the aberration of oblique pencils exceeds in magnitude the other disturbing causes, and that it will be well, in making preparations for the photographic observation of the transit of Venus, to avoid as much as possible all oblique pencils.

We would, therefore, place our photographic plates in the primary focus, and thus avoid the necessarily deep curves of any arrangement of lenses which may be used for enlarging the image. Whether it would be best to make use of a reflector or a refractor, remains to be settled by further experiment, but our present experiences would lead us to vote in favour of the reflector.

We cannot conclude without returning our best thanks to Mr. H. Davis, who has rendered us able assistance in carrying out the foregoing experiments.

SCIENTIFIC SERIALS

Annalen der Chemie und Pharmacie, February and March 1872.—This double number is unusually bulky; it contains no less than 252 pages, and abounds with much interesting matter. The effect of the new management seems to be evident, as the last paper published in this number was only received on January 14; formerly some two or three months generally elapsed from the time of the reception of a paper before its publication. The number of the papers renders it impossible in the short space at our disposal to give more than a passing glance at some of the more important. Amongst them we notice three papers in continuation of Linnemann's researches; these treat of the synthesis of normal butyric acid, of butyl alcohol, and on some of the

* We are at present unable to find any explanation of the slight apparent thickening of the end of the image of the wire where it abuts upon the ruler, but the same thickening is to be found in all the plates. It may be well to remark that it appears evident from slight indication in the negatives which it would be difficult to render in a woodcut, that the true edge of the ruler coincides with the inner side of the white line (or with the side away from the image of the wire). The convexity of the end of the image of the wire cannot, therefore, be regarded as indicating even a slight chemical encroachment. The slightly tapered appearance of the other end of the image of the wire is due to the fact that the platinum incandescent wire is cooled at its points of contact with the thick copper wires of the circuit.

normal butyl derivatives. The author prepared the normal butyric acid from iodide of propyl, which he obtained from the products of fermentation. This was boiled with potassic cyanide, and the product treated with alcoholic potash, yielding potassic butyrate; from this it is easy by well-known methods to prepare the alcohol and its derivatives, many of which have been carefully studied and are described in these pages.—Gorup-Besanez contributes a paper on the ozone reactions in the neighbourhood of the evaporating houses of salt springs (Gradirhausern); he finds that when large quantities of water evaporate spontaneously or in a current of air, ammonic nitrate and ozone are formed in appreciable quantities, and that the nearer to the evaporating surface the ozone paper was placed the more intense was the ozone reaction.—Schorlemmer has contributed a paper on the normal paraffines. He has examined many of them, such as pentan or amyl hydride, and hexan and heptyl hydride; he finds that by the action of chlorine on these pure bodies in each case two isomeric chlorides are obtained, and from which a primary and secondary alcohol can be produced, which yield a ketone and an acid by oxidation.—The next paper is by Patera "On the means of protecting textile fabrics, &c., from fire." The author's only objection to the use of tungstate of soda, which he considers very efficient, is on account of its price. He proposes as a substitute a mixture of four parts of borax and three parts of magnesic sulphate, which is freshly prepared and dissolved in 20 to 30 parts of warm water; the fabric to be protected is dipped in the solution, wrung out and dried. A second substitute is a mixture of ammonic sulphate and gypsum. These mixtures can be used for such things as crape, tulle, muslin, canvas, wood, and rope.—Wartha has a short note on the action of potassic hydrate on anthraquinone. He finds that at high temperatures these two react and form small quantities of alizarin.—Sintenis contributes a long and interesting paper "On our knowledge of the benzyl ethers;" and Popoff another on the oxidation of the ketones, both of which deal very thoroughly with their respective subjects.

THE *American Journal of Science and Arts* for June opens with a paper on the early stages of the America Lobster (*Homarus americanus* Edw.) by Mr. S. J. Smith illustrated with a plate. Dr. J. J. Woodward contributes some Remarks on the nomenclature of Achromatic Objectives for the Compound Microscope, and Prof. A. M. Mayer a description of a new form of Lantern-Galvanometer. Mr. S. W. Ford describes some new species of Primordial Fossils, and Mr. F. B. Meek some new fossils from the Cincinnati Group of Ohio; and a further important contribution to palæontological science is found in Prof. O. C. March's paper on the Structure of the Skull and Limbs in Mosasauroid Reptiles, with descriptions of new genera and species, illustrated by four plates. The new genera described are *Lestosaurus*, four species, and *Rhinosaurus*, one species. The continued articles from preceding Nos. are Prof. Verrill on Radiata from the Coast of N. Carolina, and Prof. Norton on Molecular and Cosmical Physics.

THE *Scottish Naturalist* for July consists mainly of short notes of observations and discoveries relating to Scottish Natural History, chiefly Zoology. We find also the following articles of somewhat greater length:—Description of a new Hemipter, *Anthomyia Sonchi* (the sow-thistle fly), by Mr. Jas. Hardy; on the Nest of *Formica rufa* and its inhabitants, by Dr. Buchanan White; Notes on Scottish Hemiptera, also by the Editor; On the "Yellow Fins" of the Allan-water, by Dr. W. C. M'Intosh, and the continuation of the Lists of Scottish Lepidoptera and Coleoptera by Drs. Buchanan White and Sharp.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, June 20—"On the present amount of Westerly Magnetic Declination (Variation of the Compass) on the Coasts of Great Britain, and its Annual Changes." By Staff-Captain Frederick J. Evans, R.N., F.R.S.

The rapidly accelerating value within the last few years of the westerly magnetic declination over the whole area of the United Kingdom and the adjacent seas, as observed at the fixed magnetic observatories of Greenwich, Kew, Brussels, Paris, and also at Christiana in Norway, is a subject of importance in practical navigation as affecting the compass bearings derived from charts and those laid down for the guidance of pilots.

The attention of the Hydrographic Department of the Admiralty has been constantly directed to this interesting physical fact; and as the duties of Her Majesty's surveying vessels employed on our shores between the years 1866—1870 embraced nearly the whole extent of coast line, advantage was thus taken to determine, with great attention to accuracy, the magnetic declination at widely spread and favourable localities.

The observations thus made by the surveying officers of H.M. navy are given in detail, with the corrections for secular change, to Jan. 1, 1872,* for which epoch a chart of the British Islands, exhibiting the lines of magnetic declination of equal value, is also appended. By comparing these lines with the corresponding lines given in the Declination Chart for 1842-5, Phil. Trans. for 1870, art. xiv., "Contributions to Terrestrial Magnetism," No. xii., by General Sir Edward Sabine, the annual decrease of the westerly declination, in the interval 29·5 years, over various geographical districts is thus shown:—

Shetland Islands and N.E. coast of Scotland, between 56th and 60th parallels	8′·24
E. coast of England, between 56th and 51st parallels	7′·78
S. coast of England, between 51st and 49th parallels	7′·34
Dungeness to Scilly Islands, with the Channel Islands	
(Greenwich Observatory	7′·14)
Irish Channel, between 52nd and 54th parallels	7′·10
Hebrides and W. coast of Scotland, between 56th and 58th parallels	6′·85
Ireland, S.W., W., and N.W. coast, between 52nd and 55th parallels	6′·26

It is thus seen that in the area included by the shores of the United Kingdom, the change was greater on the eastern than on the western side; as also that in the higher parallels of latitude of this area the change was greater than in the lower parallels.

By a further comparison of results as observed within the last ten to twelve years, at the same stations within the same geographical districts, the following approximate values of the present rate of annual change (westerly declination, decreasing) are obtained:—

Shetland Islands and N.E. coast of Scotland...	11′·2
E. coast of England (Bridlington)	10′·3
S. coast of England (Plymouth)...	7′·9
Scotland, W. and N.W. coasts	9′·5
Ireland, S.W. coast	6′·6

These values are in satisfactory accordance with those obtained in the interval (1865-71) at the following fixed magnetic observatories:—

Greenwich ...	8′·33	mean annual decrease of westerly declination.
Kew... ...	8′·08	" " "
Stonyhurst ...	7′·85	" " "

"On the Physical Nature of the Coagulation of the Blood," by Alfred Hutchison Smee.

"On the Detection of Organic and other Nitrogenised Matter existing in the Atmosphere," by Alfred Hutchison Smee.

"Contributions to Terrestrial Magnetism, No. XIII.," by General Sir Edward Sabine, K.C.B., V.P.R.S.

The author presents this paper as the companion of No. XI. of his "Contributions to Terrestrial Magnetism," which contained the Magnetic Survey of the Southern Hemisphere from 40° S. lat. to the extreme limit towards the Southern Pole, as does the present memoir, No. XIII. of the same series, the three magnetic elements from 40° N. lat. to the furthest attained limit of the Northern Pole. In both papers the mean epoch is the same, viz., 1842·5. Where it has been possible to do so, corrections to this mean epoch have been obtained and applied to earlier and later observations.

The determinations are derived from observers of all countries, and are arranged in zones, each of 5° of lat., passing round the globe. The table thus formed contains between 3,000 and 4,000 stations at which the magnetic elements have been determined. The observers are named, and references are made to the sources from whence their observations are taken. The paper is accompanied by maps of the resulting Isogonic, Isoclinal, and Isodynamic Lines, executed at the Hydrographic Office.

"On the Law of Extraordinary Refraction in Iceland Spar," by G. G. Stokes, Sec. R.S.

* A mean value of 19° 40′ being assumed for the westerly magnetic declination at Greenwich Observatory for this epoch.

It is now some years since I carried out, in the case of Iceland spar, the method of examination of the law of refraction which I described in my report on Double Refraction, published in the Report of the British Association for the year 1862. A prism, approximately right-angled isosceles, was cut in such a direction as to admit of scrutiny, across the two acute angles, in directions comprising respectively inclinations of 90° and 45° to the axis. The directions of the cut faces were referred by reflection to the cleavage planes, and thereby to the axis. The light observed was the bright D of a soda-flame.

The result obtained was that Huyghens's construction gives the true law of double refraction within the limits of errors of observation. The error, if any, could hardly exceed a unit in the *fourth* place of decimals of the index, or reciprocal of the wave-velocity, the velocity in air being taken as unity. This result is sufficient *absolutely to disprove* the law resulting from the theory which makes double refraction depend on a difference of inertia in different directions.

I intend to present to the Royal Society a detailed account of the observations; but, in the meantime, the publication of this preliminary notice of the result obtained may possibly be useful to those engaged in the theory of double refraction.

Paris

Academy of Sciences, July 8.—M. Becquerel presented a memoir on the influence of pressure upon the phenomena of endosmose and exosmose.—M. E. Becquerel presented a report upon the recent memoir by MM. F. Lucas and A. Cazin on the duration of the electric spark.—M. Wurtz communicated a note by M. G. Salet on the primary spectrum of iodine.—M. H. Sainte-Claire Deville presented a note by M. L. Cailletet on the compressibility of liquids under high pressures, giving the coefficients of compressibility of various fluids at certain temperatures and pressures, and describing the apparatus by means of which these results were obtained.—M. Flammarion presented some remarks on a part of a recent note by M. de Fonvielle on some observations made during the ascents of the balloon Lea, relating especially to the halo observed round the shadow of the balloon, and accepting the explanation of M. Tissandier.—M. Becquerel presented a memoir on some effects of slow actions produced during a certain number of years. In this paper the author described certain products, having their analogues in nature, formed by slow action in a vessel hermetically closed for twenty years. They include crystals of arragonite and of rhombohedric carbonate of lime, crystals of arseniate of lime, glauberite—with potash instead of soda, crystals of carbonate of lead, and malachite.—M. T. Schlœsing presented a second note on the solution of carbonate of lime by carbonic acid, and M. Wurtz a note by M. C. Lauth in reply to a recent note by MM. Girard and De Laire on the manufacture of aniline colours.—M. Bardy also forwarded a note on the last-mentioned subject.—Analyses of a new variety of amblygonite from Montebras, of amblygonite from Hebron in Maine, and of wavellite from Montebras, by M. F. Pisani, were communicated by M. H. Sainte-Claire Deville.—M. Wurtz communicated a note by MM. C. Friedel and R. D. Silva on a third bichlorinated propylene.—M. E. J. Maumené presented a memoir on two new acids produced by the oxidation of sugar, in illustration and support of his general theory of chemical action.—M. Balard communicated a note by M. J. Riban on the aldanes, or aldehydes condensed with elimination of water, the agents employed by him for the removal of the water being sodium or zinc. For these bodies he proposes the name of aldanes.—A third part of MM. Berthelot and Longuemine's thermochemical researches upon bodies formed by double decomposition was read. The substances experimented on were protochloride, perchloride, oxychloride, and protobromide of phosphorus; and the results obtained by treating these bodies with water and with potash are here stated.—M. C. Bernard described the evolution of glycogene in the eggs of birds, in continuation of his previous communications on glycogenesis in animals.—An extract from a letter of the Abbé David to M. Milne-Edwards containing some zoological observations made in the province of Tché-Kiang, was read. The author notices a new species of *Ibis* (*I. sinensis*), a new Falcon (*F. sacroïdes*) a new *Elanus* (*E. sinensis*), and a new Salamander of the genus *Cynops* (*C. orientalis*). He also mentions the occurrence of some other birds, and of a great freshwater tortoise attaining a weight of 200 to 300 pounds, supposed to be *Chiria indica*.—MM. Jamin and De Laurès presented a note on the alterations of weight undergone by the human body in baths, in which they confirm the results of M. Durrien, according to which the weight of the body is maintained or increased by absorption so long as the temperature is low or moderate, but diminished by immersion in warm water.—M. Bernard presented a fifth note by M. Paul Bert on the influence exerted by changes in barometric pressure upon the phenomena of life.—MM. P. van Tieghem and G. Le Monnier presented a joint note describing the zygospores of *Mucor phycomyces;* and M. Duchartre a paper by M. Duval-Jouve on a new species of the genus *Athenia* (*A. Barrandonii*) from the south of France.—M. Milne-Edwards communicated a note by M. H. Filhol on the carnivora and chiroptera, of which the fossil remains are found in the deposits of phosphate of lime at Caylux, Fregols, and Concots. The author describes the jaw of a cat, which he names *Pseudelurus Edwardsii;* a jaw serving as a link between the cats and mustelidæ, upon which he founds a new genus, and which he names *Ælurogale intermedia*; and two jaws of dogs, described as *Canis caylucensis* and *C. Gaudryi*. At Fregols there is a breccia composed entirely of the bones of bats, which the author refers to *Rhinolophus*, under the name of *R. antiquus*.

PAMPHLETS RECEIVED

English.—Proceedings of a Joint Meeting of the Malvern, Bath, and Woolhope Field Clubs.—Discussion of the Anemometrical Results furnished by the Anemometer at Sandwick Manse, Orkney, 1863-68.—On the Law which regulates the frequency of the pulse: A. H. Garrod.—Report of the Winchester and Hampshire Scientific and Literary Society, 1870-71.—Journal of Mental Science, July.—Memoirs of the Geological Survey of England and Wales.—Man in the Crag.—Scottish Naturalist, July.—Quarterly Journal of Microscopical Science, July.—Naval Science, No. 2.—Quarterly Journal of Science, July.—The Glacial Geology of Lancashire and Cheshire: T. M. Reade.—Vaccination and the Vaccination Laws: Rev. W. H. Rothery.—Journal of the Society of Telegraph Engineers, No. 1.—On the Change of Climate during the Glacial Epoch: Jas. Geikie.—Introductory Lecture delivered at the University of Glasgow: A. Dickson.—Route for Steamers from Aden to the Straits of Sunda.—On the Winds of the North Atlantic.—Strictures on Darwinism: H. H. Howorth. Part I.—Extracts from the Opening Address of the President of the Botanical Society of Edinburgh: Sir W. Elliot.—Annual Address to the Victoria Institute: Rev. J. Kirk.—Explosive Agents applied to Industrial Purposes: F. A. Abel.—Remarks to accompany the Monthly Charts of Meteorological Observations.—Grevillea, No. 1: M. C. Cooke.

American and Colonial.—Die neue entdeckte Geyser-gebiete am Oberen Yellowstone u. Madison Rivers: F. V. Hayden.—Illustrated Catalogue of the Museum of Comparative Zoology at Harvard College. No. 6; T. Lyman—Annual Report of the Trustees of the Museum of Comparative Zoology at Harvard.—The Eozoon Limestone of Easteren Massachusetts: J. B. Perry.—Fifth Annual Report to the Trustees of the Peabody Institute, Baltimore.—On the Structure of the Skull of Mosasauroid Reptiles: Prof. O. C. Marsh.—Preliminary description of *Hesperornis regalis:* Prof. O. C. Marsh.—Statement relating to the Home and Foreign Trade of Canada: W. J. Patterson.—Monthly Record of Results of Observation in Meteorology and Terrestrial Magnetism at Melbourne, March 1872.—Supplement to Fifth Annual Report of the U.S. Geological Survey of the Territories: F. V. Hayden.—Popular Science Monthly, Nos. 1-3.—Mineral Statistics of Victoria, 1871.

Foreign.—Sulla determinazione dei pesi molecolari delle sostanze saline: Dr. E. Paterno.—Osservazione dell' eclisse totale del 12 Dec., 1871, a Poodacottah nell' Indostan: Prof. L. Respighi.—Zeitschrift für Ethnologie, 1872. No. 3.—Journal d'Anthropologie, 1872. No. 3.—Cronica Scientifica: P. Tacchini.

CONTENTS

Errata.—Vol. vi., p. 221, col. 2, line 34 from bottom, for "Chladin," read "Chladni;" and p. 222, line 22 from top, and also in Contents, for "Allen," read "Alexander."