THURSDAY, JANUARY 4, 1883

AUGUSTUS DE MORGAN

Memoir of Augustus de Morgan. By his Wife, Sophia Elizabeth de Morgan. With Selections from his Letters. (London: Longmans, 1882.)

"DE MORGAN is certainly no commonplace man." Whenever we read this sentence in Crabb Robinson's Diary we wonder how so acute an observer could have penned it. No one who has read the shortest article by De Morgan, or who has been in his company for however short a time, but would say that he was the very opposite of commonplace. Indeed the Diarist himself elsewhere records "De Morgan called. He is the only man whose calls, even when interruptions, are always acceptable. He has such luminous qualities, even in his small talk." This last testimony all who knew De Morgan will accept as true. Though nearly twelve years have passed away since the death of this eminent mathematician and logician, no account, so far as we know, has been given of his life and writings, save the appreciative notice by the late Prof. W. Stanley Jevons—whose writings so amply testify to the influence De Morgan's teaching exercised over him—in the present issue of the *Encyclopædia Britannica* (vol. vii. pp. 64-67, 1877), and the interesting sketch by Mr. Ranyard in the *Monthly Notices of the Royal Astronomical Society* for February 9, 1872, vol. xxxii. (erroneously cited as vol. xxii. in Jevons's article). It was, however, well known that a "life" was being drawn up by Mrs. De Morgan. This is the work now before us, in the preface to which the writer says, "my object has been to supply that part of my husband's life, the material for which would not be within the reach of another biographer."

Augustus De Morgan was born in the year 1806 (Mrs. De Morgan is not more explicit, but we learn incidentally from a letter—p. 394—that the exact date was June 27) at Madura, in the Madras Presidency.[1] His father was Lieut.-Col. De Morgan, who had held staff, and other appointments at several stations in India. Other members of his father's family also distinguished themselves in the service of the East India Company. His mother was the granddaughter of James Dodson, F.R.S., author of the *Antilogarithmic Canon* and other mathematical works,[2] a friend also and pupil of De Moivre; from her he appears to have inherited his musical talent ("his delightful flute") and his mathematical power. From his mother too we are told that De Morgan inherited his love of a city life.[3]

When Augustus was seven months old[4] the family came to England, and first settled at Worcester, but subsequently took up their residence at Barnstaple, and other towns in the West of England. After two or three journeys backwards and forwards, the father left Madras in 1816, having been ordered home ill with liver complaint, and died off St. Helena, leaving his widow with four surviving children. De Morgan gives in a half serious, half humorous way the idea "the victim" retained of his early schooling. At four years he learnt "reading and numeration" from his father. He always spoke gratefully of his father, but doubtless what he has written in his paper "On Teaching Arithmetic," had its rise in this early experience, "it is a very common notion that this subject is easy; that is, a child is called stupid who does not receive his first notions of number with facility . . . the subsequent discoveries of the little arithmetician, such as that six and four make thirteen, eight, seven, anything but ten, far from giving visions of the Lucasian or Savilian chairs, are considered tiresome, and are frequently rewarded by charges of stupidity or inattention. . . . Irritated or wearied by this failure, little manifestations of temper often take the place of the gentle tone with which the lesson commenced, by which the child, whose perception of such a change is very acute, is thoroughly cowed and discouraged, and left to believe that the fault was his own, when it really was that of his instructor."

When about nine years of age the Rev. J. L. Fenner was for a short time his teacher; from him the boy "learnt his first—fortunately not his last—notions of Latin and Greek, with some writing, summing, how to mend a pen, and the first four verses of Gray's 'Elegy,' with a wonderful emphasis upon the 'moping owl.' He thinks, too, that 'he pitied the sorrows of a poor old man'; but on this his memory is not so clear."[1] At Taunton, under the Rev. H. Barker, he was taught Latin, Greek, Euclid, Algebra, and a little Hebrew. Of his last teacher, the Rev. J. Parsons, of Redland, near Bristol, De Morgan always spoke with respect. "It was strange that among so many teachers the germ of mathematical ability should have been so long unnoticed. It could not be quite latent or quite unformed in the brain of a boy of fourteen; it can only be supposed that the routine of school teaching smothered and hid it from observation." It was whilst at Taunton that a friend, seeing the boy very busy in making a neat diagram with ruler and compasses, asked him what was to be done. He said he was *drawing mathematics.* "That's not mathematics," said his friend; "come and I will show you what is." The lines and angles were rubbed out, and the future mathematician, greatly surprised by finding that he had missed the aim of Euclid, was soon intent on the first demonstration he ever knew the meaning of. De Morgan himself writes of this time, "On referring to my own experience I find that I have always had the image of '*length without breadth.*' I remember when I first opened Euclid, at thirteen years of age, I am sure I had no bias to admit any thing which should make mathematics 'exist as a science': for I should have been better pleased if it had not existed at all, science or no science. I thought I had studies enough; and Walkingame, who I understood was

[1] De Morgan was proud of his birth in the sacred city of Madura, and at one time longed to visit his native country . . . his doing so when young was prevented by his defect of sight. From his birth both eyes were affected with the "sore eye" of India, and the left was saved (pp. 22, 5).

[2] He was also mathematical master at Christ's Hospital, and in connection with this "blot on the escutcheon," De Morgan writes that when quite a boy he asked one of his aunts "who James Dodson was," and received for answer, "we never cry stinking fish." He had to wait a few years to find out that his great-grandfather was the only one of his ancestors whose name would be held deserving of record.

[3] In the "Budget of Paradoxes" (p. 82) De Morgan applies to himself the lines:—

> "Ne'er out of town; 'tis such a horrid life;
> But duly sends his family and wife."

The memoir gives frequent illustrations of his dislike for even a short stay in the country (pp. 79, 94, 234).

[4] In the *Monthly Notices* "three months old" would appear to be incorrect.

[1] "Recollections by Mr. De Morgan," Appendix to Crabb Robinson's Diary, vol. iii. p. 540.

a cousin of Euclid, had given me no prejudice in favour of the family. But in the first glance at the book, when I came to 'a line is length without breadth,' I felt that I had gained expression for an idea which I distinctly possessed by image, but could not have put into words. And so, in a small way, I found that geometry *did* exist as a science."[1]

Before we leave these records of his school-days, we will cite some further remarks on the modes of instruction then in vogue—which, by his books, he more than any other writer helped to improve. When a boy arrives at school, "he is taught to *say* the table of numeration, and then proceeds through a number of rules . . . which, if he understand, it is well, but if not, nobody cares. . . . As to the reasons for the rules, the pupil cannot trouble his head (to use a common term for that much-avoided operation, thinking) about them, not knowing whether there are any at all, or whether the rules themselves came from the moon, or are a constituent part of that wisdom of our ancestors about which he sometimes hears. Should there be any natural defect in his mind, owing to which he finds it difficult to produce a correct result, knowing neither what he is to do, nor how to do it, there are several approved methods of proceeding. The best of these, unfortunately now somewhat exploded, is a flogging; which works on a principle recommended by physicians, of curing a disorder in a part which cannot be got at, by producing one in another which can. Next to this, comes the method of keeping the patient from all recreation until he has done what is required of him, it being considered the same thing in the end, whether he cannot work for want of means, or will not from want of application. It has been suggested to teach the principles involved in the rules, and thus to render the pupil their master instead of their slave; but to this plan, independently of its being an innovation, there are grave objections."[2]

At the age of sixteen years and a half he entered at Trinity College, Cambridge, and in 1825 gained a Trinity Scholarship. Devoting much of his time to music and to a rather wide range of reading (he had always "an insatiable appetite for novel-reading . . . let it be good or bad in a literary point of view, almost any work of fiction was welcome, provided it had plenty of incident and dialogue, and was not over-sentimental"[3]) he failed to attain the highest place in the Tripos, but came out Fourth Wrangler in 1827. Mrs. De Morgan notes that this failure, in a possibly fallacious test, was his own early, but unintentional, protest against competitive examinations, for which he felt excessive disapprobation even before his experience as a teacher showed him not only their mischievous effect upon mind and health, but their insufficiency to determine the real worth of a candidate for Honours (pp. 18, 56, 169). In connection with this subject we may mention that he had a great objection to marks in looking over examination papers. He said he could judge of the merits of the competitor from the whole work, but he could not reckon it up by marks, and he always refused to examine in this way.

[1] "On Infinity; and on the Sign of Equality." *Camb. Phil. Soc. Trans.*, vol. xi. Part 1.

[2] From a paper "On Mathematical Instruction," which, with four other papers by De Morgan, is reprinted (from the *Quarterly Journal of Education*) in the *Schoolmaster*, vol. ii., 1836. The five papers amply repay perusal even at the present date.

[3] In reference to this period of his life, he writes (1869, p. 393), "I read an enormous deal of fiction—all I could get hold of—so my amusement was not all philosophical."

Having conscientious scruples about the doctrines of the Established Church, he was prevented from proceeding to his M.A. degree and from sitting for a Fellowship, to which he would doubtless have been elected. "A strong repugnance to any sectarian restraints upon the freedom of opinion was one of De Morgan's characteristics throughout life." A further career at the University being thus closed against him, and having abandoned the study of medicine, he turned his thoughts to law, and entered at Lincoln's Inn. The establishment in 1828 of the London University—now University College—however gave him the opportunity of leaving the study of the Law, "which he did not like," for the teaching and pursuit of science. At the age of twenty-two, though much younger than any of the other thirty-one candidates[1] for the post, he was unanimously elected to the Chair of Mathematics. From this time, with the exception of an interval of five years,[2] he devoted himself with the greatest assiduity to the duties of the post until his final resignation in 1866.

It has been frequently remarked that De Morgan was unrivalled as a teacher of mathematics, and certainly no teaching in our University experience ever approached his in the faintest degree. Mr. Sedley Taylor writes:—

> De Morgan regularly delivered four courses of lectures, each of three hours a week, and lasting throughout the whole academical year. He thus lectured two hours every day to his College classes, besides giving a course addressed to schoolmasters in the evening, during a portion of the year . . . De Morgan was far from thinking the duties of his chair adequately performed by lecturing only. At the close of every lecture in each course he gave out a number of problems and examples illustrative of the subject which was then engaging the attention of the class. His students were expected to bring these to him worked out. He then looked them over, and returned them revised before the next lecture. Each example, if rightly done, was carefully marked with a tick, or if a mere inaccuracy occurred in the working it was crossed out, and the proper correction inserted. If, however, a mistake of *principle* was committed, the words 'show me' appeared on the exercise.[3] The student so summoned was expected to present himself on the platform at the close of the lecture, when De Morgan would carefully go over the point with him privately, and endeavour to clear up whatever difficulty he experienced. The amount of labour thus involved was very considerable, as the number of students in attendance frequently exceeded one hundred. . . . The claims which University or College examinations might be supposed to have on the studies of his pupils were never allowed to influence his programme in the slightest degree. He laboured to form sound scientific mathematicians, and, if he succeeded in this, cared little whether his pupils could reproduce more or less of their knowledge on paper in a given time . . . all *cram* he held in the most sovereign contempt. I remember, during the last week of his course which preceded an annual College examination, his abruptly addressing his class as follows: 'I notice that many of you have left off working my examples this week. I know perfectly well what you are doing; YOU ARE CRAMMING FOR THE EXAMINATION. But I will set you such a paper as shall make ALL YOUR CRAM of no use.' . . . De

[1] In a letter to Sir J. Herschel, August 9, 1862 (p. 312), De Morgan says, "I was picked out of fifty candidates."

[2] In consequence of a disagreement with the Council he resigned his Professorship, July 24, 1831. On the death of his successor, in October, 1836, he was requested to resume his office, and did so.

[3] The exercises were placed in a case of pigeon-holes hung on the wall near the entrance to the Mathematical Theatre.

Morgan's exposition combined excellences of the most varied kinds. It was clear, vivid, and succinct—rich too with abundance of illustration always at the command of enormously wide reading and an astonishingly retentive memory. A voice of sonorous sweetness, a grand forehead, and a profile of classic beauty, intensified the impression of commanding power which an almost equally complete mastery over mathematical truth, and over the forms of language in which he so attractively arrayed it, could not fail to make upon his auditors" (pp. 99, 100).

His pupils' affection, the memoir tells us, was not gained by any laxity of discipline, for he was strict, especially as to quietness and punctuality.[1]

Such arduous labours as these would amply suffice for the generality of teachers, but the remainder of his time was occupied with other work hardly less exhausting than these. In May, 1828, he was elected a Fellow of the Astronomical Society, and in February, 1830, he took his place on the Council. In 1831 he was elected Honorary Secretary; in which position he entered with zeal, we are told, into every question brought before the Society, and his place was not a sinecure. "It is not easy to say how much of the usefulness and prosperity of the Society . . . was due to his incessant energy and effort, and to his steady judgment at difficult junctures." Though his connection with the Society lasted for some thirty years, he would never undertake the office of President. "I will vote for and tolerate no President but a practical astronomer. . . . The President must be a man of brass—a micrometer-monger, a telescope-twiddler, a star-stringer, a planet-poker, and a nebula-nabber."[2] He was frequently employed as a consulting actuary,[3] and bestowed also much time and labour upon the subject of the decimal coinage.[4]

Passing from De Morgan's public labours we hurriedly glance at him as a writer, and here we cannot do better than quote Prof. Jevons: "From the above enumeration" of his mathematical and logical writings, "it will be apparent that the extent of De Morgan's literary and scientific labours was altogether extraordinary; nor was quality sacrificed to quantity. On the contrary every publication was finished with extreme care and accuracy, and no writer can be more safely trusted in everything which he wrote. It is possible that his continual efforts to attain completeness and absolute correctness injured his literary style, which is wanting in grace; but the estimation in which his books are held is shown by the fact that they are steadily rising in market price. Apart from his conspicuous position as a logical and mathematical discoverer, we may conclude that hardly any man of science in recent times has had a more extensive, though it may often be an unfelt influence, upon the progress of exact and sound knowledge."

His love of books was intense:[1] "the most worthless book of a bygone day is a record worthy of preservation." Evidences of his minute acquaintance with all sorts of out-of-the-way works present themselves in almost all his writings, but are especially conspicuous in that wondrous repertory of wit and wisdom, the "Budget of Paradoxes." De Morgan's peculiar dislike of conventional titles, "which are not what they seem to be," led him to decline the honorary degree of LL.D. of the University of Edinburgh, and accounts for his not allowing his name to be put up for the F.R.S.[2] "Whether *I* could have been a Fellow, I do not know; as the gentleman said, when asked whether he could play the violin, 'I never tried.'" In fact, as he writes in the "Budget," he was a man who could not *groove*.

The last occurrence connected with science which gave him pleasure was the foundation of the London Mathematical Society.[3] The idea of having such a society occurred to his son George and Mr. A. C. Ranyard, and on their mentioning the matter to Prof. De Morgan, he at once gave in his adhesion to their proposition, and with the countenance thus extended by himself and other leading mathematicians who were got together in reply to a circular issued by the two founders, the Society started into existence. Prof. De Morgan was the first president, and delivered at the first public meeting (January 16, 1865) an interesting and characteristic address. He continued to take a warm interest in the meetings (being a vice-president for the last time in the session 1869-70) until November 26, 1868, after which date severe illness prevented his further attendance.[4] The end came on March 18, 1871, "just after midnight he breathed his last."[5]

In the Vacation of 1837 De Morgan married Sophia Elizabeth, daughter of William Frend. This gentleman was a member of the old Mathematical, and subsequently of the Astronomical Society, had been Second Wrangler, and a Fellow of Jesus College, Cambridge. He sacrificed good prospects as a clergyman to his conscientious scruples about the doctrines of the Established Church, and was at this time Actuary of the Rock Life

[1] A student, who joined the class in 1859, has put at our disposal some notes he wrote during the session 1859-60: "The class begins at 9 o'clock, but however early we go the Professor is sure to be there. Only once or twice have I been early enough to see him coming. He has a large head, bald at the top, and with a tremendous halo of hair round the crown. He wears a black cloth suit and a parson's white neck-cloth. His coat is a swallow-tail, and his trousers, with fob pockets, scarcely reach his boots, of which the laces are often too long. As he shuffles along he seems to be counting the flagstones or rails, urged by a sort of centrifugal force to keep the outside kerb, as Dr. Johnson used to do. In the lecture-room when the bell has rung, he always goes through the same routine at commencing. First of all he takes out a large red silk pocket handkerchief, with which he wipes his spectacles; he then readjusts them with the bridge upside down, and though he has only one eye he can see as keenly as another man with both. He then turns back the cuffs of his sleeves, and, after passing his fingers through his hair, takes his compasses in hand and looks round the room at his class. He has been talking all the time, and by this is fairly launched on his subject. . . . He often indulges in jokes with manifest gusto. The other morning he was illustrating a point, when he said, 'This reminds me of an anecdote told me once by my old Cambridge tutor, Prof. Peacock. He had been for some time striving to instil into the mind of a rather obtuse student the difference between $4x$ and x^4. At last he timidly ventured to remark, "'I think, now, Mr. A., you clearly see the difference.'" "'Yes, I think I do, but between us don't you think, Professor, it is a needless refinement?"' I think the part of his lectures I have most enjoyed has been his treatment of the Theory of Probabilities. In this he seemed to revel." Then follow remarks similar to Mr. Taylor's, and he concludes: "No Professor takes more pains with his class, and all through the session he deposits, in the Library, Tracts written by himself on the particular branch then in hand." We have similar testimony from other quarters.

[2] A list of the offices he held is given (p. 270).

[3] He was never connected with any office, but his advice was sought by professionals whenever there arose a "nodus vindice dignus."

[4] A full account of his work in this direction occupies pp. 235-255 of the *Memoir*.

[1] He loved to surround himself, as far as his means allowed, with curious and rare books. He revelled in all the mysteries of watermarks, title-pages, colophons, catch-words, and the like; yet he treated bibliography as an important science.

[2] Why he did not care to "shine in the dignity of F.R.S."—*See* "Budget of Paradoxes," p. 18.

[3] There is a new Mathematical Society, and I am, at this present writing, its first president. We are very high in the newest developments, and bid fair to take a place among the scientific establishments." Then in contrast with the old Mathematical Society, "But not a drop of liquor is seen at our meetings, except a decanter of water: all our heavy is a fermentation of symbols; and we do not draw it mild,"—"Budget of Paradoxes," p. 236.

[4] In the recent Presidential Address it was announced that a "De Morgan Memorial Medal," of the value of £10 would be awarded triennially by the Society. The first award to be made in November, 1884.

[5] *Monthly Notices*, "at one o'clock in the afternoon."

Assurance Office. The marriage was a most happy one, and surrounded by a family of seven children, of whom three at least died before their father, De Morgan sought his happiness, as we have endeavoured to show, in his home, amongst his books, and in the earnest discharge of his professorial and other duties.

The Memoir is charmingly written, and abounds in graphic details, which bring clearly before the reader the picture of a simple, manly character that was unique in its idiosyncrasy. A prominent object in its production has been to tell the story of the Professor's connection with University College, and of the events which led to his leaving it. "After the lapse of sixteen years I trust that the narrative will provoke no revival of the somewhat acrimonious controversy which ensued."

Another feature of the work is the selection from De Morgan's extensive correspondence with contemporary scientific men; these letters are full of interest, and abound in utterances characteristic of the writer. Always effective and to the point, they are often very humorous: the humour, indeed, sometimes borders upon trifling.

Instead of thinking that Mrs. De Morgan has exceeded due limits in her selection, we would have welcomed a far larger number of specimens. On p. 333 De Morgan states that he had corresponded for thirty years with Sir W. Rowan Hamilton, but no specimens of the correspondence are given. In one or two cases Mrs. De Morgan has deviated from the general rule she laid down for herself, and to this deviation we are indebted for some very interesting letters from the late Sir Frederick Pollock, which enlighten one as to what was required to be read for the Senior Wranglership at the beginning of the present century. Is it too much to hope that another volume may be issued containing a further selection from the correspondence, and also a few of the more valuable of the early papers, such as those which appeared in the *Journal of Education*, and in the *Companion to the British Almanac?* We venture to give the following extract from a letter we received under date May 15, 1869, as an ordinary specimen of his writing to one who had no special claim upon the writer:—". . . I should decidedly object to the reference made to Barrow on the last leaf. 'Dr. Barrow with an orthodox dislike to give unnecessary credit to a Moslem author has misled. . . .' When was there an orthodox dislike to Mahometans being discoverers in science? And what possible reason is there for imputing any such feeling to Barrow, a man of most unimpeachable fairness, except in this, that when he had a congregation by the ears he would hold on for three hours until they prevailed on the organist to 'blow him down.' He had lived among the Turks at Smyrna and Constantinople, and was certainly not ill-inclined to a Mahometan, as such. But this much is enough: the imputation is quite new, or nearly so, and should not appear without proof in the *obiter dictum* of an historical writer who obviously makes it a theory to explain something he has found. . . . P.S.—I think that for one orthodox man who might be supposed likely to rob a Mahometan of geometry, I could find three who would have been more likely to toss it back again with the remark that such infidel stuff was only fit for Mahound and his slaves."

A list of writings is appended. In this we notice the following slips:—"Elements of Arithmetic," the dates should be "1st edition, 1830; 3rd, 1835"; on p. 403, in (18) for "No. 3" read § 3 of (15) supra"; p. 404, in (18) dele "1," and in (6) for "Trigonometry" read "Geometry; p. 405, (5) should, of course, be "$Pdx + Qdy + Rdz$"; p. 406, in 1849, insert "Remarks" after "Supplementary"; in (1), (2), we think the dates are inaccurate; p. 407, read "Al*f*onsine."

We remark also that no account is taken of communications to the Mathematical Society. These were ten in number, not counting the Opening Address, which forms the first number of the Society's *Proceedings*. The only papers printed are "A Proof that every Function has a Root" (No. vi., a mere notelet); "Remark on paper by Mr. Woolhouse 'on General Numerical Solution'" (No. xiv.); and "On the Conic Octagram" (No. x. pp. 26-29). In this last paper occurs the characteristic note: "This presentation of the second hexagon was actually suggested to me by observing that *Bloise Pascal* has two hexagrams, and the jocose inference that there ought to be two hexagons in the theorem (given in the paper). My own names are both octagrams; but though I bow before the coincidence, I have no suggestion to acknowledge." There is a fairly full "Index of Names, &c.," but we do not grasp the principle upon which it is drawn up, as some names are inserted and others left out. We failed at first to identify "Prof. John Adams" with "Neptune" Adams. There are also the following corrections to be be made:—Read "J. Baldwin Brown," "Arthur Cayley" (both here and in the "Budget of Paradoxes" the famous mathematician is called "George"), Hanssen (for Haussen), Encke (for Hencke), Royal Society (insert the most important reference to p. 172), Sedgwick, Rev. C. Simeon (not J.), insert John Taylor, p. 122, and dele "p. 124" under Sedley Taylor. On p. 306 we presume x should be $\dot{x}$, and on p. 286, for 1866 read 1865; the present writer succeeded George De Morgan as teacher in the session 1865-66.

R. TUCKER

FISHES OF SWITZERLAND

Faune des Vertébrés de la Suisse. Par Victor Fatio, Dr. Phil. Vol. IV. Histoire naturelle des Poissons. 1re partie I. Anarthropterygiens. II. Physostomes-Cyprinidés. 8°. pp. xiv. et 786, avec 5 planches. (Génève et Bale: H. Georg, 1882.)

AFTER an interval of nearly ten years Dr. Fatio has issued another volume of the series of excellent monographs, in which he gives the results of his researches into the vertebrate fauna of Switzerland. The first volume, published in the year 1869, contained the Mammals; the third (1872) the Reptiles and Batrachians; the second, which will be devoted to Ornithology, being still in course of preparation. The one now published, which is the fourth of the series, treats of a part of the Fishes, which class will be concluded in the fifth.

No one who studies this volume will be surprised at the long lapse of time which intervened between its appearance and that of the preceding. The author had not the advantage of being assisted in his work by collections already formed and available for the purpose, but had to collect the materials himself; a labour which, even in a small country like Switzerland, takes years to accomplish; especially as, for comparison's sake, he extended his researches to the fauna of the neighbouring countries. His descriptions are well elaborated and compiled from numerous observations; they include all the variations of age, sex, season, locality, &c., and particular notice is taken of those modifications, by which Swiss examples seem to be distinguished from those of Germany, France, Italy, &c. Thus, this work rises far above the level of a local publication, and is of as great a value to the student of European freshwater fishes, as to the Swiss naturalist.

The present volume treats of the Acanthopterygians and Cyprinoids only, 5 species of the former and 21 of the latter being admitted as permanent inhabitants of the country. Besides, the author distinguishes 3 sub-species, many varieties, and 3 hybrids; he also refers in shorter chapters to 13 other species and 6 hybrids, which are extra-limital, or may sooner or later be found straying into Switzerland.[1] This number of the freshwater fishes of Switzerland must appear small, when we consider that it comprises representatives of four of the principal river-systems of Europe, viz. the Rhine, Rhone, Po, and Danube; and there is no doubt that this comparative poverty is due to the altitude of the country, freshwater Acanthopterygians and Cyprinoids being generally more developed in the less rapid waters of warmer low-lying countries. The Rhine contributes the majority; 20 out of the 24 species[2] which inhabit the middle and lower sections of the river, ascending beyond the Falls of Schaffhausen. The Rhone is inhabited by 24 species, but, singularly, of these 11 only have been able to establish themselves above the Perte du Rhone, although the others freely enter the Saone or penetrate even into the upper Doubs, a river not included in Swiss territory. Of the 23 species found in the lower part of the Po, 15 reach the Swiss frontier; and this southern portion of the fauna is in its character so distinct from the northern, that 9 only of these 15 species are identical with Rhine fishes. Finally the fish-fauna of the Danube, which is stated to consist of 30 species, is represented in Switzerland by 3 only, the great altitude of the river Inn proving a most effectual barrier to the dispersal of the remainder. Moreover, these three species are common Central European types, and not peculiar to the Danube.

The author has taken great pains to ascertain the extreme limits of altitude, to which the several species can attain in the Alps. Two only go beyond the height of 2000 metres, viz. the minnow and miller's thumb, which are still found at respectively 2400 and 2200 m. The perch, the next in order, reaches an altitude of 2000 m., all the remainder living at, or below, 800 m. However, several have been successfully imported to altitudes varying between 1000 and 1700 m., thus the carp, tench, rudd, roach, and chub.

Of the fishes described in this volume, we wish to draw particular attention to two which, belonging to marine genera, and evidently being of marine origin, have acclimatised themselves in the fresh waters of Southern Europe, and penetrated into, or close to, the confines of Switzerland, viz. a goby (*Gobius fluviatilis* or *martensii*), which has ascended the Po, and a blenny (*Blenniu cagnata*), which occurs in abundance in Lago Maggiore as well as in the lake of Bourget in Savoy.

Hybrids are comparatively scarce in Switzerland. The author justly accounts for their scarcity by the physical peculiarities of his country; snow-fed, rapid rivers are less adapted for their production, than the slower and warmer waters of low countries, where a greater variety of species and a larger number of individuals are mixed together, sometimes within very narrow limits.

The volume is illustrated by five well executed plates, three of which are devoted to osteological, dental, and dermal details.

The author of a thoroughly original work like the present, cannot fail to differ from his predecessors in questions of specific distinctions and numerous other points of detail, but it is our duty to testify to the fair and calm spirit, in which such questions are discussed and treated by him; and we hope that, before many years, we shall have the pleasure of announcing to our readers the completion of so valuable a work as Dr. Fatio's Ichthyology of Switzerland.

[1] The second part of the Ichthyology is estimated to contain about 21 species.

[2] These and the following numbers refer to the Acanthopterygians and Cyprinoids only.

OUR BOOK SHELF

China. By Robert K. Douglas. (Society for Promoting Christian Knowledge, 1882.)

It may be said at once respecting this book that it is without exception the very best elementary work on China with which we are acquainted in any European language. The author has resided for many years in China, and is in the forefront of the Chinese scholarship of our time; his work is, therefore, not only accurate, but it places the reader abreast of the latest researches. One of the most remarkable of these is fully explained at pp. 359-60. The *Yih King*, or Book of Changes, is the work for which the greatest antiquity is claimed by the Chinese. Some writers have placed it as far back as between 300 and 400 B.C. However this may be, the key to its interpretation has been entirely lost, although the best native scholars of all ages, including Confucius himself, have attempted to explain it. M. Terrien de la Couperie (assisted, we believe, by Prof. Douglas himself, though this fact is not mentioned), has succeeded within the last few years in showing that "instead of being a mysterious depository of deep divinatory lore, it turns out to be a collection of syllabaries such as are common in Accadian literature interspersed with chapters of astrological formulæ, ephemerides, and others dealing with ethnological facts relating to the Aboriginal tribes of the country; but all taking the form of vocabularies, and therefore as impossible to be translated in the sense in which every commentator, from Confucius downwards, has attempted to translate them as 'Johnson's Dictionary' would be." Although we possess innumerable volumes on subjects connected with China, we have not until now a thoroughly trustworthy book covering the whole ground in a simple elementary manner. Some volumes recently published for popular reading on the countries of the East exhibit such lamentable ignorance, that we can only "gasp and stare" at their contents. Notwithstanding his own intimate knowledge of the subject, Prof. Douglas has consulted almost all that we have in our literature relating in any way to China, from Davis's Chinese poetry and Oppert's Susian texts, down to recent numbers of the English journals published in China. The two last chapters—that on "Language" and "Literature"—are models of clear and simple exposition of complicated subjects. Another excellence of the book is what we may call its perspective. The writer does not thrust any particular branch of his subject into undue prominence, to the detriment of the rest. The first chapter gives a brief sketch of Chinese history, the second of the system of administration; various chapters are then devoted to popular customs, to education, medicine, music, dress and food, architecture, honours, names, superstitions, religions, &c. There is also an excellent map. To the general reader who desires some accurate information respecting a country which is coming nearer to us every day, or to the student who wants a *vade mecum*, no better volume can be recommended.

LETTERS TO THE EDITOR

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

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

On the Occurrence of Great Tides since the Commencement of the Geological Epoch

MR. BALL says very truly that the fundamental question is, what traces of great tides ought we to expect to find if those great tides had really existed? Mr. Darwin says, coarse-grained rocks, and different forms of vegetation calculated to resist the action of the accompanying great winds. Mr. Ball, in reply, remarks that high tides in the Avon are accompanied by fine sediment. He thinks with others that the high tides would have produced a vastly greater amount of sediment than is being formed at present. I quite agree with Mr. Ball about the fine sediment, but I am not at all clear that high tides mean great marine denudation. By far the largest portion of the work done by the sea as a denuding agent is due not to the wearing action of currents, or to the pounding of materials on a beach at low water, but to the direct action of the sea on the cliffs. This force is estimated as about a ton per square foot on the average in winter, on the west coast of Great Britain. This undermines the cliff at the sea level, and then the top part falls partly by its own weight, but still more through the effect of the air compressed in the caves and cracks, which by its elasticity spread the blow over a very large surface through the crack and joints of the rock. Now to undermine cliffs with a given force of wind and wave, it seems clear that the maximum effect would be produced where the tides are very small, for there the force is constantly applied at the same spot. With a rise and fall of 100 feet, each portion of the cliff would be subjected to the force of the waves for so short a time that in all probability caves would never be formed at all, and the height of the tide would be an actual protection to the land. As a matter of fact, those places where the tides are highest show, as far as I know, no signs of excessive denudation. I spent two days last year on the Bay of Fundy, where the tides are higher than anywhere in the world, and I was very much struck with the absence of any evidence of great denudation due to the tide. The cliffs at the Joggins are about high-water mark, with a long beach which slopes very gradually. The force of the waves, such as they are, is spent in hammering this beach and grinding it into fine sand and mud; the mud is carried about in suspension by the tide, and the sand is shifted about, but the denuding effect is exceedingly small. The consequence is that the cliffs are pounded by the waves for such a short time each tide that they suffer mainly from atmospheric denudation, the sea doing little more than keeping their base clear, and in many places not even doing that.

Similar phenomena are presented by the highest tides in Great Britain—those on the Severn. Here at Clifton on the Avon the tide rises 30 feet; there are no waves; the banks are covered with a thick coating of mud and denudation is nil. At Aust Cliff, again, on the Severn, where the soft red marls are peculiarly liable to erosion, the height of the tide is again a protection. The cliff is about high-water mark, and the force of the waves is expended on the beach. The case is the same at Watchet, and a good many other places on the Severn. I do not know of any part of the Severn remarkable for excessive denudation, owing to the high tides. There is a strong resemblance between the Bay of Fundy and the Severn: there are the cliffs at high-water mark, the same long beaches, the same shifting sands and mud in suspension; similar causes have produced similar results. In narrow inlets like the Bay of Fundy and the estuary of the Severn, these high tides mean rapid currents and small waves; but along shores freely exposed to the ocean, the highest tides might be accompanied by very feeble currents. But if, as Mr. Darwin says, the high tides were accompanied by trade winds about $3\frac{5}{7}$ times as strong as the present ones, the battering power of the waves and the strength of the currents would be very greatly increased, and plains of marine denudation, it might be supposed, would be very rapidly formed. What would be likely to happen if such winds and waves began now to act on our shores? Should we have reason to expect that England would in a comparatively short time disappear beneath the waves? A very rapid destruction of our present cliffs would undoubtedly begin, though, as I pointed out before, this would be attributable mainly to the wind, and not to the tide; the cliffs would be driven back to about the ordinary high water mark, leaving a long shelving beach extending to a few feet below the low water mark. The cliffs would then for far the greater portion of each tide be entirely free from marine denudation, and their rate of wasting would depend on the power of the sea to tear up the long solid sloping beach, and restore comparatively deep water at the base of the cliffs. But this process is an exceedingly slow one, because there can be no undermining or assistance from compressed air, and I should anticipate that marine denudation would then be actually slower than it is at present. There would, of course, be abundance of very fine sediment formed during the first wearing back of the cliffs by the grinding of the materials between tide marks; but when once the cliffs had reached the high water line, the amount of sediment would depend chiefly on the amount of atmospheric denudation, supposing that the sea kept the base of the cliffs clear. We should, in fact, have a repetition of the phenomena presented to us now by the Bay of Fundy and the Severn. Thus far, then, it seems to me that no argument can be drawn from the fineness of the early sediments against the existence of high tides in the Geologic period; nor, on the other hand, does the quantity of sediment seem to me a strong argument in favour of it. But Mr Darwin's argument that the vegetation of the Carboniferous period could not possibly have held out against the violent winds which necessarily accompanied these high tides seems to me unanswerable. One has only to reflect on the effect produced by our present winds to feel convinced that if the winds and tides went together they were certainly Pre-Carboniferous, and almost equally certainly Pre-Devonian. J. G. GRENFELL

Clifton College, December 29, 1882

Sir George Airy on the Forth Bridge

As Sir George Airy's last letter may, like his first, provoke replies from distinguished American and continental engineers, it may save your correspondents' time and your own valuable space if I add a few final words in explanation.

1. Sir George says:—"The danger of buckling in a horizontal direction with a length of 340 feet, remains undiminished unless it is counteracted by bracing unknown to me." Now Sir George evidently has forgotten that some time ago he was furnished with photographs of a large model of the bridge taken with the view of showing the said bracing, and that his attention was specially directed to the point.

2. Sir George thinks "it desirable that attention should be called to the magnitude of the forces concerned," and speaks of a wind-pressure of 75 tons, and an end-pressure of 600 tons. Now he clearly has forgotten that before he wrote his first letter a "stress diagram" was sent to him, on which it was noted that the wind-pressure provided for was 2207 tons on each span, and that the estimated end-pressure on the strut referred to was 2380 tons.

3. Sir George holds the engraver responsible for some of the alarmist statements in his first letter. I must remark, therefore, that it was pointed out that the bridge would have been perfectly safe had the details of the design been as he assumed. For evidence that a 340 feet tubular strut of 12 feet diameter would not fail in the manner stated by him, he was referred to Hodgkinson's experiments as published in the *Philosophical Transactions*, and Clark's work on the Britannia bridge; and further, he was lent the *Transactions* of the American Society of Civil Engineers for last year, containing the most recent experiments on long wrought iron columns. Any or all of these documents would have shown him that the Forth bridge

struts, even if arranged as he first conceived them to be, could not have failed by flexure during the wildest hurricane.

4. Sir George finds fault with the connection of the brackets, and "can hardly imagine that trains could be run through at speed." I should have been pleased to have explained the connection to Sir George, but he has not sought to know anything about the details of the bridge, and, I am sure, would be much puzzled to give your readers even the vaguest possible description of the connection, which he nevertheless stigmatises as "not very perfect."

In conclusion, as Sir George has not done so himself, I would warn any young student who may have read the investigation contained in the appendix to the first letter, that the methods therein proposed would lead to an over-estimate of the strength of struts of ordinary proportions by from 200 to 300 per cent. This warning is the more necessary, as the general tenour of Sir George Airy's letter might make a student imagine that he erred, if anything, in the direction of excess of caution, whereas the application of the principles laid down by him would, in the case of the Forth bridge, result in the compression members being made only one-third of the strength considered expedient by Mr. Fowler and myself. B. BAKER

2, Queen Square Place, Westminster, S.W.

P.S.—It may interest some of your readers to know that the maximum force recorded during recent storms by our wind pressure plates at the Forth has been 20 lbs. per square foot, upon the small and light plate having an area of 2 square feet, and 12½ lbs. upon the large and heavy one, with an area of 300 square feet. The same ratio holds good down to pressures of 2 lbs. per square foot, and it appears pretty certain that the higher blasts are of such momentary duration and of such unequal distribution, that even a small sized railway bridge could never experience ordinary anemometer pressures. Other reasons for a reduced pressure on a large surface have been advanced by Dr. Siemens in a recent number of the *Comptes Rendus*. Nevertheless, in this instance of the Forth bridge we have assumed that a 56 lbs. hurricane will act simultaneously over the whole width of the Forth, with a resultant lateral pressure of no less than 8000 tons upon the main spans. We have further assumed that the said hurricane might blow down one side of the Forth, whilst a dead calm prevailed on the other side, and have even provided for the twisting action upon the piers and superstructure due to a 56 lbs. hurricane blowing *up* the Forth on one side, and *down* it on the other. To ascertain what lateral pressure a 56 lbs. hurricane would cause, we tested, both in air and in water currents, a large model of the bridge, with cross-bracing complete, and ascertained its equivalent in square feet of flat surface. Under any of the conditions of wind pressure enumerated above, combined with any distribution of the rolling load, the resultant stresses upon superstructure, holding down bolts and piers will be far within the safe working limits as determined by our experiments upon the respective materials.—B. B.

Altitude and Weather

IN NATURE, vol. xxvii. p. 176, you notice the remarkable warm and dry weather September 21 last on Ben Nevis, during an anticyclone, and, as at the foot the air was relatively cold and humid, you see in the heat and dryness on the mountain an effect of descending air currents. In this you are quite right, but I do not think you are right in estimating that this air was saturated at a certain height above Ben Nevis. The fact is this: the increase of temperature from a certain height above sea-level to the latter being *de facto* much less than the dynamical increase of a stratum of air, due to compression sinking down, a downward current of air will be generally warm, and relatively dry. It does not matter if it sinks along the slopes of mountains (as the foehn), or vertically, as modern meteorology considers it to be the case in anticyclones. There is only one great difference: the air currents down a slope may be, and often are, very violent, and only when they are so, their relative heat and dryness are felt, while the downward currents in an anticyclone are so gentle that they are seldom felt or directly registered, and that mostly the thermometer and hygrometer are our only means of detecting them. On account of their slow motions, the effect of these downward currents during anticyclones is little felt in valleys and plains, as (1) they are even more retarded near great land surfaces; (2) in the colder time of the year, especially when the ground is covered with snow, the radiation from the soil lowers the temperature of the lower strata. Thus during anticyclones in winter a very low temperature is generally experienced in plains and valleys, due to radiation, and a very high temperature and low humidity on isolated mountains, due to descending currents of air.

These conditions are best realised during protracted and considerable anticyclones, and it was Prof. Hann's merit to have explained this fact.[1] The exceedingly protracted anticyclone of December, 1879, in Central Europe, was especially favourable to the proof of the existence of descending currents, as the cold was great in the valleys, even in the high ones, like the Engadine and the Davos, but the air was warm and very dry on isolated mountains. An example from the best mountain observatory of that time, the Puy de Dôme, and the foot of it, will suffice, nine days, December 20-28, 1879, at 6 a.m.

	Feet.	Temp. F.	Relative humidity	Amount of Cloud
Puy de Dôme,	4813	38·8	38	1·3
Clermont (base)	1273	8·2	91	0·7

There is all reason to think that in these days there was no saturated stratum of air even considerably above, say the Puy de Dôme.

I must remark that Prof. Hann, in his last work, "Der Föhn in Bludenz," does not sustain his former opinion that great precipitations on the windward side of mountains is necessary to the appearance of a foehn on the leeward side. His opinion now is, that a considerable barometric gradient and the drawing in of air from considerable heights are alone necessary, for even if the air on the mountains is not abnormally warm, it will come down warm and relatively dry. A. WOEIKOFF

Ofizerskaja, St. Petersburg, December 15-27, 1882

The Fertilisation of the Speedwell

I FEAR that Dr. H. Müller's passage in Schenk's "Handbuch" would occupy too much space to be given here in full; but I can condense what he says into a few lines. Dr. Müller takes the *Veronica chamædrys* as representing a type of flowers in which the anthers have to be brought into a position to strike the body of the insect by the action of the insect itself. He finds the same arrangement in the *V. urticæfolia.* These flowers are visited by insects of various kinds, but their structure is, he thinks, explained only by what takes place when they are visited by *Syrphidæ*. When one of these insects visits such a flower, it hovers for some seconds before it, then settles upon the lower lobe of the corolla, without noticing the style which is coloured like the corolla, and which is now under the insect's body. It then crawls higher to reach the nectary, and in doing so bends down the stamens—which are also coloured like the corolla—until the anthers strike against the under part of the insect's body. The pollen thus obtained is carried to another flower, and brought into contact with the stigma when the insect first alights; and fresh pollen is again obtained by the attempts to reach the nectary. Dr. Müller either knows from observation or assumes that in the *V. chamædrys* anthers and stigma are mature at the same time. He attaches importance to the fact that both stamens and style are coloured like the corolla, and therefore appear to escape the observation of the insect; and the thinness of the base of the stamen is also noticed by him as one feature in the adaptation of the flower to the visits of *Syrphidæ*. He does not refer to the looseness of the corolla. Mr. Stapley's suggestion that this may play some part in the work of cross-fertilisation is an ingenious one, and calls for further research.

As to the *V. hederæfolia*, Dr. Müller mentions it as one of the plants that have a tendency to keep their flowers half-shut in cold and rainy weather, and thus to become cleistogamous.

I am sorry that I misunderstood Mr. Stapley's first letter upon any point; but he has misunderstood mine also, if he thinks I was not aware he wished to call attention "to the *adaptation* of the flower for cross-fertilisation." I wrote as briefly as I could, and naturally assumed that he would understand I was not thinking merely of the fact that Diptera drew down the stamens. ARTHUR RANSOM

Bedford, December 23, 1882

THE SACRED TREE OF KUM-BUM

THE dissipation of illusions is always a little painful, even after repeated experience of the process. I must confess, then, to some feeling of injury at learning from Mr. Keane's interesting review in NATURE, vol.

[1] *Zeitschr. für Meteorologie*, p. 129, 1876.

xxvii. p. 171, that Huc's "tree of ten thousand images" is nothing more than a common white lilac. Myths of this kind I have generally found to have some substratum of fact at the bottom. They can be rationalised, and mere explosion does not seem to be a satisfactory way of getting rid of them.

Now our knowledge of the indigenous vegetation of China is painfully limited. An immense portion of the flora is doubtless gone beyond recovery in the cultivated districts. Remnants of the primitive, wide-spreading forest remain, however, in the precincts of temples and monasteries, and these woods have always yielded novelties to botanists who have examined them. It had seemed, therefore, little short of certain that the sacred tree of Kum-bum would be something of considerable scientific interest if specimens of it could be got hold of.

The only edition of Huc at hand to refer to is Hazlitt's translation, published by Thomas Nelson and Sons in 1856. The well-known account of the tree will be found on pp. 324-6. According to Huc, the name Kum-bum, or as he spells it, Koun-boum, consists of "two Thibetan words signifying ten thousand images, and having allusion to the tree which, according to the legend, sprang from Tsong-Kaba's hair, and bears a Thibetan character on each of its leaves." Now, according to Kreitner, as quoted by Mr. Keane, "the Abbé Huc tells us that its leaves bear the natural impress of Buddha's likeness and of the Thibetan alphabet." As a matter of fact, he does not say anything like this. What he does say is as follows:—

"There were upon each of the leaves well-formed Thibetan characters, all of a green colour, some darker, some lighter than the leaf itself. Our first impression was a suspicion of fraud on the part of the Lamas, but, after a minute examination of every detail, we could not discover the least deception. The characters all appeared to us portions of the leaf itself, equally with its veins and nerves; the position was not the same in all; in one leaf they would be at the top of the leaf, in another in the middle, in a third at the base, or at the side; the younger leaves represented the characters only in a partial state of formation. The bark of the tree and its branches, which resemble that of the plane-tree, are also covered with these characters. When you remove a piece of old bark, the young bark under it exhibits the individual outlines of characters in a germinating state, and, what is very singular, these new characters are not unfrequently different from those which they replace."

Of the tree itself as Huc saw it some forty years ago, he gives the following account:—

"The tree of the Ten Thousand Images seemed to us of great age. Its trunk, which three men could scarcely embrace with outstretched arms, is not more than eight feet high; the branches, instead of shooting up, spread out in the shape of a plume of feathers, and are extremely bushy; few of them are dead. The leaves are always green, and the wood, which is of a reddish tint, has an exquisite odour, something like that of cinnamon. The Lamas informed us that in summer, towards the eighth moon, the tree produces huge red flowers of an extremely beautiful character."

Hazlitt's translation contains two woodcuts, one (p. 325) of the tree with its canopy, the other (p. 369) of a leaf with its markings. What the history of these illustrations is, there is nothing to show; Huc's book in the original French had, I think, none. The leaf with its markings has a by no means impossible appearance; whether the markings are like Thibetan characters, I cannot say. The outline of the leaf is not unlike that of a fuchsia, but it would not pass for a lilac.

I suspect, then, that there really was in Huc's time a tree with markings on the leaves, which the imagination of the pious assimilated to Thibetan characters. Perhaps it was the last local relic of some unknown endemic tree; in Hongkong I believe many of the endemic species are represented by but a few individuals. It may well have died and been replaced by a lilac, and the genuine markings by the fudged-up image of Budha "etched with some acid on the leaves."

It is disappointing that Szechenyi's expedition seems to have done nothing for botany. As Grisebach says, "We can only guess at the richness of the Chinese flora." Every now and then some one is induced to collect a few plants, and almost invariably they contain something new to science. A more extended knowledge of Chinese plants is now essential to a right understanding of the phyto-geographical facts of the north temperate flora. Unfortunately, the numerous Europeans who visit China are occupied with political, religious, or commercial business, with little time for subsidiary pursuits. But any of them who may chance to read these lines, may rest assured that they will be really doing a useful work by collecting and drying even a few *wild* plants in their respective neighbourhoods.

Kew W. T. THISELTON DYER

NORWEGIAN GEODETICAL OPERATIONS[1]

IN 1861 an Association was formed, under the auspices of Lieut.-General von Baeyer, having for its object the measurement of arcs of meridians, and parallels, in Europe. Most of the Continental nations joined this Association, and have carried out triangulations and spirit levellings of precision to further the objects in view. It is the intention of the Association to measure an arc extending from Palermo to Levanger in Norway, which will, however, probably be extended to the North Cape. The work before us is the report of the measurement of two base lines, and of their connection with the Norwegian triangulation which is to form part of the measurement of the above-mentioned arc. It was thought in 1862 that the existing Norwegian triangulation, supplemented and verified by some new work, would meet the requirements of the Association; but it was found, on investigation, that such was not the case, and moreover that the verifications could not be carried out, because the old trigonometrical stations could not be refound with any certainty. It was therefore decided to commence a new triangulation extending in a chain from the Swedish frontier (south of Christiana), where the chain is connected with the Swedish triangulation, to Levanger, where again a connection is to be made with another portion of the Swedish triangulation. The two base lines already mentioned are situated at the extremities of this chain of triangles, one at Egeberg, near Christiana, and the other at Rindenleret, near Levanger; both were measured during the summer of 1864, and Part I. is the report of these measurements.

The base measuring apparatus used is similar to that employed by Struve for the measurement of several base lines in Russia; it belongs to the Swedish Government, and was used for the measurement of their base lines. The apparatus consists of four cast-iron tubes, each approximately 2 toises[2] in length. One end of each tube is fitted with a small highly polished steel stud, and the other end with a "contact lever." The short arm of the contact lever terminates in a steel stud, which is intended to press against the fixed stud of the adjoining tube; the long arm moves on a scale. A measuring rod capable of varying its length to a slight extent is thus obtained, and this alteration in length can be measured with great delicacy, since the long arm of the lever greatly exaggerates it. This arrangement insures that the pressure between the rods is constant. Each tube is provided with two

[1] Publications of the Norwegian Committee of the European Association for the Measurement of Degrees. Geodetical Operations. Published in Three Parts. (Christiania, 1880 and 1882.)

[2] A toise is 2·13151116 yards as determined by Col. A. R. Clarke, C.B., R.E., F.R.S., &c.

thermometers, the bulbs of which are bent nearly at right angles to the stem, and are inserted into small holes in the tubes. In order to protect the tubes as far as possible from changes of temperature they are wrapped round with several thicknesses of cloth, and are further inclosed in a wooden box, out of which the two ends of the tube just project. During the measurement of a base line each rod is supported on two trestles, at one-fourth and three-fourths of its length, provided with screw arrangements giving slow motions laterally and in elevation. The rods are not, however, accurately levelled, and a correction has to be made for dislevelment. To measure the small angle of inclination each rod is fitted with a very sensitive level. One end of the level works on trunnions, the other is connected to a micrometer screw by means of which the level can be raised or lowered. The bed of the level is attached to the top of the box, but in such a manner that it can be adjusted truly parallel to the tube. The value of each micrometer division was determined by means of the meridian circle in the observatory at Christiana. It will be seen from the above that, as the measurement of a base line proceeds, the following readings are required for each rod: (1) the contact lever; (2) the thermometers; (3) the micrometer for inclination. These readings were taken and booked independently by two observers. Both base lines were measured twice, once in each direction.

Before and after the measurement of each base line each rod was compared with a *standard* rod, the exact length of which was known, namely:

$$= 1727{\cdot}96641\,(1 + 0{\cdot}000011476\,(t - 16^{\circ}{\cdot}25)) \pm 0.00058$$

expressed in Paris lines[1] based on Bessel's toise, t being expressed in degrees Centigrade. It was found that the rods were slightly diminished in length during the measurement of a base line (on an average 0·005 lines) owing to abrasion. An allowance was made for this diminution in length. The apparatus with which these comparisons were made consists of a massive cast-iron beam, turned up at both ends, and carrying two supports fitted with rollers upon which the rod to be measured rests. One end of this beam is fitted with a fixed steel stud, against which the contact lever of the rod under comparison bears; the other end carries a sliding scale, connected with a contact lever, and read by means of a micrometer microscope. A set of readings consisted in first measuring the standard rod, then each of the four measuring rods in succession, and lastly the standard rod again; the temperature of each rod was carefully noted. For a complete comparison twelve such sets of readings were taken.

The time occupied in measuring the Egeberg base was 18 days, and the observations for each measuring rod occupied 4 minutes; the Rindenleret base was measured more rapidly, namely, 2½ minutes per rod, due to the site being more level.

A considerable portion of Part I. is taken up in considering the errors to which the measurements of these base lines are liable, in estimating the allowances to be made to correct these errors, and in computing the probable errors of the final results. These errors are due: (1) to errors of observation in the actual measurement of the base lines; (2) to the error in the adopted length of the measuring rods.

Firstly, the errors to which the actual measurement of a base line is liable are as follows:—

A slight uncertainty attaches to the micrometer readings of the levels measuring the inclination of the rods. The probable error is computed to be

Egeberg base	± 0·350 lines
Rindenleret base	± 0·183 ,,

The errors due to the contact levers are next considered. It is shown that the error caused by the small uncertainty in the value of a degree of the scale over which the long arm of the lever moves, is too small to be taken into account, but the error caused by uncertainties in reading the scale is of sensible amount, and is computed to be

Egeberg base	± 0·015 lines
Rindenleret base	± 0·014 ,,

Further, the surface of the steel studs, at the end of the rods, is a portion of a sphere whose radius is considerably less than the length of a rod. Hence an error will occur each time a contact lever does not touch at the centre of the stud, that is if it makes an eccentric contact, and although every care was taken to obtain accurate contacts, it is considered that a correction of the following amounts should be made—

Egeberg base	− 0·351 ± 0·175 lines
Rindenleret base... ...	− 0·314 ± 0·157 ,,

The next source of error is that due to errors in alignment, these errors will always be negative, and are due to the uncertainty in placing the rods in the line given by the directing theodolite. This error is computed to amount to

Egeberg base	− 0·294 ± 0·101 lines
Rindenleret base... ...	− 0·262 ± 0·090 ,,

The computed variation of length of the rods due to alterations in temperature is vitiated by several errors. In the first place, the coefficient of expansion of the rods, as determined by Prof. Lindhagen, is affected by the small uncertainty, 0·000000015. Further, the correction for expansion is computed on the supposition that the thermometers do actually indicate the mean temperature of the rods at the time of taking the readings; but this is an assumption, and in fact it is estimated that the temperature indicated by the thermometers is the temperature the rod had 20·0 ± 5·9 minutes before taking the reading. This estimate is arrived at as follows: It will be remembered that each base line was measured twice; the difference between the two measurements is due to the various errors under consideration, and its probable value can therefore be computed; this computed value will contain, as an unknown, the time of which an estimate is required. Hence, by equating the computed difference to the actual difference the time can be found. The total error in the allowance made for expansion is found to be

Egeberg base...	± 0·085 + 0·525 $\wedge$
Rindenleret base	± 0·071 + 0·250 $\wedge$

where $\wedge$ = 20·0 ± 5·9 minutes.

Secondly, the errors due to the uncertainty in the accepted length of the rods are considered under four heads, namely: (1) the error in the length of the standard rod; (2) the error due to the bending of the beam of the comparing apparatus (some experiments were made to obtain data for the calculation of this error); (3) the error in comparing the rods with the standard; (4) the error due to the assumption that the diminution in length of the rods by abrasion is proportional to the length of time in use. The probable error of the accepted length of a rod during the measurement of the Egeberg base is computed to be ± 0·00081 lines, and during the measurement of the Rindenleret base ± 0·00071 lines.

Finally, the base lines had to be reduced to the sea-level; data had been obtained for this purpose by means of spirit-levelling operations. The reduction in the length of the base lines due to this cause is

Egeberg base	− 33·89 lines
Rindenleret base	− 0·852 ,,

Applying all these various corrections to the measured lengths of the base lines the final results are as follows:

[1] A Paris line is defined by 1 Paris line = $\frac{1}{864}$ toise, hence 1 Paris line = 0·088813 English inch.

Egeberg base ... ... 2025·28316 toises,

with a probable error of ± 0·00129, or $\frac{1}{1,570,000}$ of its length.

Rindenleret base ... ... 1806·3177 toises,

with a probable error of ± 0·00120, or $\frac{1}{1,500,000}$ of its length.

This is a high degree of accuracy as compared with older base lines (as for instance several base lines measured in France between 1798 and 1828, of which the probable errors are $\frac{1}{250,000}$); but this accuracy has frequently been attained of late years, and even surpassed, as, for instance, the base line of Madridejos, measured by General Ibañez in 1858, with a probable error of $\frac{1}{5,865,800}$.

Part II. is the account of the connection of the Egeberg base with the side Toass-Kolsaas, and Part III. that of the connection of the Rindenleret base with the side Stokvola-Haarskallen of the principal triangulation. The observations were made during 1864-66, but owing to an error at one of the stations, due to the bisection of a wrong object, further observations were made at that station in 1877. The connection in each case is very complete, and the work is well tied in. The centres of the trigonometrical stations were very carefully defined by letting an iron bolt into the rock, or, into a large block of stone; the centre of the face of this bolt, marked by a small hole, was the trigonometrical station. The signals, to which the observations were taken, consisted of an upright beam, to which was attached one or two boards about 0·75 m. square, which were painted white or black, and occasionally a vertical stripe 0·11 m. broad was painted on the centre of the board. At several of the stations the theodolite could be placed beneath the signal, and at such stations the signal was placed over the bolt, but in several cases, owing to the nature of the ground, or other causes, the trigonometrical station had to be placed at some distance from the signal, in one case as much as 54 Norwegian feet. In such cases the corrections to be applied to the observations were obtained by measuring a short base line, one end of which was the trigonometrical station, and the direction nearly at right angles to the line joining the station and the signal. Observations were taken from the ends of this base to the various points on the signal, which were bisected from the other stations, and these, together with the observed bearings to and from the other stations, enabled the necessary corrections to be made. The greatest correction thus required was 10′ 37″·34. But even at stations where the theodolite was placed beneath the signal, corrections were required to reduce the observations to the trigonometrical station, because different points on the signal were observed from the other stations, and these points were not vertically over the bolt. In these cases the corrections were computed in the following manner:—A piece of paper, mounted on a board, was placed horizontally on the ground over the centre of the station, and this centre marked on it. Then, by means of a small theodolite, the "traces" of the vertical planes passing through the various points observed to, were marked in pencil on the paper. The theodolite was now shifted, and the corresponding traces marked as before; the intersections of these traces gave a series of points vertically beneath the points on the signal to which observations had been made. From these points, the corresponding bearings to the various stations were plotted on the paper; and, lastly, perpendiculars were dropped, from the point representing the centre of the station, on to these bearings; the length of any one of these perpendiculars divided by the approximate distance to the corresponding station is the tangent of the correction to be applied.

Two instruments were used for measuring the angles; a 10″ universal instrument by Olsen, read by two micrometer microscopes, and a 12″ theodolite by Reichenback, read by four verniers. The errors of graduation of these instruments were investigated, and are given in a tabular form in Part II. Although, owing to the numerous observations taken to each object starting from different parts of the horizontal limb, the errors of graduation must have been eliminated to a very large extent, yet it was thought advisable to apply these corrections to the observations, in order to obtain a more accurate idea of the bearings of each station. The errors of the micrometer microscopes are also given in a table. The 10″ instrument was used at all, the 12″ theodolite appears to have only been used at two, stations. A third instrument, a 10″ universal instrument by Breithaupt and Sons, was used for the observations of 1877.

When observing, the instrument was first set at 0°, and a round of angles taken: the telescope was then reversed and the round taken again. The instrument was then set at 15° in the case of the triangulation connecting the Egeberg base, and at 20° (nearly) in the case of the Rindenleret base triangulation, and two rounds taken as before. The instrument was then again moved on 15° and 20° respectively, and so on. Thus in the first case forty-eight, and in the second thirty-six observations were taken to each station. In some few instances even a greater number were taken. The actual observations are not given in the Report, only the mean of four observations—two taken in the same position of the horizontal limb, and two in that position increased by 180°. The time occupied at each station averages four days; some stations were completed in two days.

The observations were compensated by the method enjoined by the Association for the measurement of degrees in Europe, namely, Bessel's method. The observed angles at each station are first compensated amongst themselves. A correction is then applied to each angle thus found, subject to the condition that the sum of the squares of these corrections for the whole triangulation is a minimum, and subject further to the geometrical conditions that the sum of the three angles of a triangle = 180 + spherical excess, and that the length of any side is the same by whatever *route* it is calculated. The necessary calculations are very laborious, and in the case of the Rindenleret base require the solution of simultaneous equations containing seventy-six unknowns. It is very questionable whether the result repays this labour; the method of compensation adopted for the Ordnance Survey, although perhaps not so rigid, compares favourably in this respect. The calculations for compensation are given very fully in the Report.

The Report is accompanied by plates showing the base measuring apparatus and the connecting triangulations.

ELEMENTS OF THE GREAT COMET OF 1882

(*Communicated by Vice-Admiral Rowan, Superintendent U.S. Naval Observatory*)

THE following elements were computed from three observations made at the U.S. Naval Observatory; the first and last being made with the Transit Circle, and the middle one compared with a known star which was afterwards observed on the Transit Circle:—

Wash. M.T.		App. α. h. m. s.		App. δ. ° ′ ″
Sept. 19·9697877	...	11 14 18·94	...	− 0 34 29·7
Oct. 8·7204363	...	10 28 6·63	...	− 10 40 22·6
Nov. 4·7009228	...	9 6 16·22	...	− 27 21 26·7

From these observations we deduce—

Perihelion Time = Sept. 17·2228200 Greenwich Mean Time.

$$\left.\begin{array}{rl} \Omega = & 346^\circ\ 1'\ 7''\cdot91 \\ \pi - \Omega = & 69\ 36\ 12\cdot79 \\ i = & 141\ 59\ 52\cdot16 \\ \phi = & 89\ 7\ 42\cdot70 \end{array}\right\} 1882\cdot0$$

$$\log a = 1\cdot9331366$$
$$\log q = 7\cdot8904739$$
$$\text{period} = 793\cdot689 \text{ years}$$
$$\delta\lambda \cos\beta = -0''\cdot06 \quad \delta\beta = +0''\cdot01$$

$$x = r\,[9\cdot9951411] \sin(170^\circ\ 42'\ 12''\cdot72 + v)$$
$$y = r\,[9\cdot9877234] \sin(262\ 46\ 57\cdot39 + v)$$
$$z = r\,[9\cdot4435130] \sin(\ 49\ 20\ 25\cdot11 + v)$$

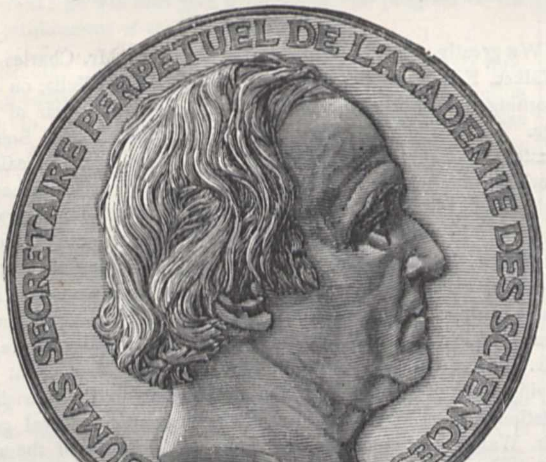
The observations as given were afterwards corrected for parallax by means of elements previously computed. These elements bear a considerable resemblance to Comet I., B.C. 371; and it may possibly be its third return, a very brilliant comet having been seen in full daylight A.D. 363.

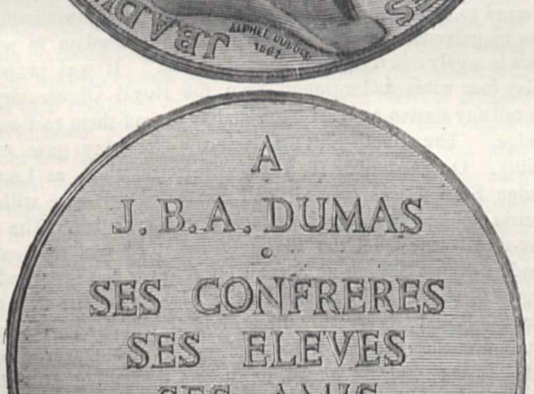
E. FRISBY,
Prof. Math., U.S.N.

Washington, Dec. 19, 1882

THE DUMAS MEDAL

WE recently (vol. xxvii. p. 174) gave the addresses at the Paris Academy of Sciences in connection with the presentation to M. Dumas of a medal in com-





memoration of the fiftieth anniversary of his election to the Academy. We are now able, by the courtesy of our French contemporary, *La Nature*, to reproduce an illustration of this medal, which was presented by M. Jamin in words both eloquent and touching, as a token of the "love and gratitude" of the distinguished chemists' *confrères*, pupils, and friends. The medal is the work of M. Alphée Dubois.

PROFESSOR VON GRAFF'S MONOGRAPH ON THE TURBELLARIANS[1]

THIS splendid folio monograph consists of two volumes, the one comprising the text of over 600 pages illustrated by woodcuts, the other twenty as beautifully executed partially coloured plates as have ever been turned out, all from the author's own original drawings. The publication of the work has been assisted by a grant from the Berlin Royal Academy of Sciences.

Ludwig von Graff is Professor of Zoology at the College of Forestry at Aschaffenburg, in Bavaria. Hs first memoir on Turbellarians was published in 1873, at which time he first made up his mind to work out from his own observation a revision of the Turbellarians. The present monograph is, as he tells us in the preface, the result of almost incessant work during the last five years. He has made numerous journeys to the Naples and Triest stations, and has also visited many other parts of the European coasts north and south, and the fresh waters in all directions, in order to pursue his investigations on living Turbellarians. He has thus been able himself to examine 70 out of the 168 species of Rhabdocœlida which are known with certainty. The work being thus founded on so wide a personal acquaintance with the forms of which it deals, is of especial weight and value; it constitutes a systematic monograph of the Rhabdocœlida, founded on a sound basis of anatomical structure, and embracing all species hitherto described by other observers, together with those discovered by the author himself (thirty new species).

It is doubtful whether the present work will be followed by a second part embracing in a similar manner all the known Dendrocœlida. The matter depends on the amount of ground which may be covered by Dr. A. Lang's forthcoming monograph on Turbellarians, in the "Fauna and Flora of the Gulf of Naples." If this monograph proves to be so comprehensive that a further one would be superfluous, then Prof. Graff will publish a quantity of material collected by him concerning the Dendrocælida, in three smaller memoirs on the Polyclada, the Triclada, and embryology respectively. The present work is appropriately dedicated to the memory of O. F. Müller and Sir John Dalyell. It is pleasing to find the great merits of the latter thus recognised by a foreign naturalist.

The author does not admit Sidonia = *Rhodope varanii*, which, in opposition to Dr. R. Bergh, he considers to be a nudibranch, or Dinophilus, which has lately been shown to lie near the Archiannelids amongst the Turbellarians; and in the definition he gives of the group excepts the Microstomida, which differ from all other Turbellaria in having a complete periœsophageal nerve ring, in being diæcious, and in multiplying asexually by budding.

Separating, as is now so usual, the Nemertines altogether from the Turbellarians, he divides the group into the Rhabdocælida and Dendrocælida. In the definition given of the two sub-orders, an interesting point of difference is brought out, namely, that in the former the yelk glands are always present in the form of a pair of compact glands, whereas in the latter they are always divided up into numerous separate follicles.

The Rhabdocælida are divided by the author into three groups: I. Acœla; II. Rhabdocœla; III. Alloiocœla, which are thus defined:—

[1] "Monographie der Turbellarien." 1. Rhabdocœlida. Dr. Ludwig von Graff. (Leipzig: W. Engelmann, 1882.)

1. Acœla. With digestive internal substance; without differentiation of a digestive tract and parenchym tissue. Without nervous system or excretory organs. All forms as yet known provided with an otolith.

2. Rhabdocœla. Digestive tract and parenchym tissue differentiated; a roomy body cavity usually present in which the regularly-shaped intestine is suspended by a small amount of parenchym tissue. With nervous system and excretory organ. Generative organs hermaphrodite (except in Microstoma and Stenostoma). Testes, as a rule, two compact glands. The female glands present as ovaries only, ovario-vitelligenous glands, or separate ovaries and yelk glands. Genital glands separated from the body parenchym by a special tunica propria. Pharynx always present, and very variously constructed. Otolith absent in most cases.

3. Alloiocœla. Digestive tract and parenchym tissue differentiated, but the body cavity much reduced by the abundant development of the latter. With nerve system and excretory organ. Generative organs hermaphrodite, with follicular testes and paired female glands, either ovaries only, or ovario-vitelligenous glands, or separate ovaries and yelk glands. Yelk glands irregularly lobular, rarely partially branched. Genital glands almost always without any tunica propria, lodged in the spaces in the body parenchym. Penis very uniform, and either without chilinous copulatory organs, or with these very little developed. Pharynx a pharynx variabilis or plicatus. Digestive tract lobular, or irregularly broadened out. All marine except one, or possibly two species.

Under the Alloiocœla come the genera—Plagiostoma, Vorticeros, Monotus, and others.

The work commences with a complete list of the literature on Turbellarians from the time of Trembley, who, in 1744, figured a black fresh-water Planarian to that of the publication of the last of Dr. Arnold Lang's important memoirs last year. The list is followed by a general treatise on the anatomy and physiology of the Rhabdocœlida. The account of the nematocysts of some forms is very interesting; their exact resemblance to those of Cœlenterata is fully borne out. *Microstonum lineare* appears to be the only species which, like Hydra and Cordylophora, possesses two kinds of nematocysts. The author thinks he has been able to detect on the surface of the cuticle, trigger hairs in connection with the nematocysts, like those in Hydroids. He considers the rhabdites or rod-bodies homologous with nematocysts, and refers, in connection with this question, to the nematocysts devoid of any thread which occur in many Cœlenterates, intermingled with fully developed ones. The structure of the pharynx is carefully gone into, and its different forms being of much use in classification, receive various names, such as *Pharynx bulbosus*, *P. plicatilis*, &c.

The water vascular system has been studied by von Graff with considerable success. It may consist of a single median main canal with a single posterior opening (Stenostoma) or a pair of laterally-placed canals with a similar single opening or two separate lateral canals with each a posterior opening (Derostoma), or there may be a pair of openings or a single one somewhat anteriorly placed. Ciliated funnel cells or flame cells such as exist in Cestodes, Trematodes, and Triclad Dendrocœles, have been discovered by von Graff also in the Rhabdocœlida. They do not, however, occur in connection with the tips of the ramifications of the water vascular canals, but almost entirely on the larger canals forming the networks. It is impossible here to follow the work further, through the interesting sections devoted to the development of Microstoma by budding, the habits of life and geographical distribution of the Rhabdocœlida. In connection with the discussion on classification, a table of the pedigree of Turbellaria is given, with Proporus as the ancestral starting-point. In this family tree the Dendrocœles are shown as derived from Acmostoma, a new genus of Alloiocœla, characterised by having a distinctly marked narrow ambulacral sole, the Polyclada directly, and the Triclada through Plagiostoma. The ascertained facts as to the structure of Turbellarians seem to point even more closely to their connection with the Cœlenterata. The presence of two kinds of nematocysts in one of the Rhabdocœla and possible occurrence in members of that group of trigger hairs, is a remarkable fact. Dr. Lang, believing that a part of the nervous system in Dendrocœles is truly mesenchymatous as in Ctenophora, and from other grounds concludes with Kowalewsky that the Polyclada are "creeping Cœlenterates which have many points of structure in common with the Ctenophora, some with the Medusæ. Such being the case, naturalists await with great impatience Kowalewsky's promised further information as to his extraordinary Cœloplana, supposed intermediate between Ctenophora and Dendrocœlida. The peculiar azygos character of the otolith in so many Dendrocœlida may perhaps be explained by the similar condition of the sense organ in Cœloplana. Prof. von Graff is much to be congratulated on the completion of this most important and admirable work.

H. N. MOSELEY

NOTES

WE greatly regret to announce the death of Mr. Charles V. Walker, F.R.S., at his residence at Tunbridge Wells, on the morning of December 24, 1882, in the seventy-first year of his age. Mr. Walker had been Telegraph Engineer to the South-Eastern Railway, since 1845. He had been a most zealous worker in the science of electricity, as the many works he leaves behind will testify. Indeed, he was one of the oldest telegraph engineers in the country, was the inventor of several useful appliances in connection with telegraphy, including the instruments by which the block system on railways is worked. His name is especially associated with the origin of the distribution of time by telegraph. On May 10, 1849, Mr. Glaisher wrote to Mr. Walker that he wished to talk with the latter about the laying down of a wire from the Observatory to the Lewisham Station, and on May 23 following, the Astronomer-Royal gave Mr. Walker a brief sketch of the use to be made of the wire referred to, his scheme, as he stated, being "the transmission of time by galvanic signal to every part of the kingdom in which there is a galvanic telegraph from London." It was proposed to lay four wires underground from the Royal Observatory to the railway station at Lewisham, and to extend them to London Bridge. The South-Eastern Railway Company gave every facility. On September 16, 1852, an electric clock at London Bridge Station was erected, and connected by wire with an electric clock at the Royal Observatory, Greenwich. The first time-signal sent from the Royal Observatory was received at London Bridge Station at 4 p.m. on August 5, 1852; and on August 9, 1852, Dover received a time-signal for the first time from the Royal Observatory direct, and it was made visible at certain first-class stations between London and Dover. After that the system rapidly spread, its success depending greatly on the scientific skill and enthusiasm of Mr. Walker. For some account of the subsequent development of the system, the reader may refer to the articles in NATURE, vol. xiv. pp. 50 and 110. Mr. Walker was treasurer of the Royal Astronomical Club for several years, and at the time of his death was president of the Society of Telegraph Engineers.

THE death is announced of Prof. Listing of Königsberg.

THE honour of Companion of the order of the Indian Empire has been conferred upon Surgeon-Major George Bidie, Superintendent of the Central Museum at Madras.

AT the last sitting of the year 1882, the Paris Academy of Sciences elected M. Bunsen, of Heidelberg, a Foreign Associate. M. Bunsen was already a Correspondent in the Section of Chemistry, and he will fill the place vacated by the death of M. Wöhler. It should be remembered that, contrary to the rule for members, who must be French citizens, and Associates, who must be foreigners, the correspondents of the Academy can be elected without any qualification of nationality; but none of them, either French subjects or foreigners, may live in Paris previous to their nomination. This rule is so strict that it is stated that an eminent man of science, wishing to become a candidate, removed his home from Paris to Versailles; and having been successful, returned to Paris, where he now lives.

THE Duc d'Aumale has been elected President of the Académie Française. M. Blanchard, the naturalist, will be President of the Academy of Sciences for 1883; he was vice-president during the past year. The vice-president for 1883, and future president for 1884 would be elected on Tuesday from the Mathematical Section. Before leaving the chair M. Jamin will, according to precedent, read a list of losses experienced by the Academy in 1882, and of the nominations made during the same period; he will also give a *résumé* of the progress of the several publications of the Academy.

ON Tuesday January 2, there was a gathering of people interested in educational progress, at No. 1, Byng Place, Gordon Square, to inspect the College Hall of Residence for Women Students which has lately been established there. Complete as this hall is in itself, we understand that it is only provisional until sufficient approval and support have been obtained to justify the opening of a building capable of accommodating a larger number of ladies. We may, however, regard it as embodying the idea of its founders, and as supplying in miniature a model of that comfortable and well-adapted academic residence which it is their object to provide for female undergraduates and art-students in London. The advantages to the members of this rapidly-increasing class of entering such a hall instead of taking separate lodgings or rooms in a boarding-house, or even living at home (in many cases) are not far to seek. Students in lodgings often suffer from neglect of health and under-feeding, while those who work at home are subject to interruptions and the strain of conflicting claims; and although they might avoid both these drawbacks in a good boarding house, they would still find that their residence was not adapted to the needs of student life. Whichever plan is adopted, girls generally lack opportunities of free intercourse with minds whose training has been about equal to their own, such as of late years they have been able to obtain at Oxford and Cambridge, and which is specially needed in London, the seat of the only English University that as yet admits them formally to degrees. Hence the three greatest benefits of the new hall will be: first, to bring the women students of London into social and intellectual fellowship, and thus to improve the quality of their work by encouraging conference on the subjects of study, without which it is hardly possible to acquire and test accuracy of thought; secondly, to diminish the causes of failure of health by care and good housekeeping; and thirdly, to increase the time at the disposal of students; thus, on the one hand, affording to the zealous worker opportunities of relaxation, which in different surroundings would be absorbed in housekeeping worries or other occupations, and, on the other hand, enabling the less enthusiastic to add to the quantity of their acquirements without increase of conscious effort. The Hall has been established chiefly in the interests of the students of University College (including the Slade School), but its usefulness is much enhanced by proximity to the London School of Medicine for Women, and the British Museum; for on this account we may fairly hope that it will contain numbers of students in the various departments of Literature, Science, and Medicine, and the Fine Arts. Liberality of thought and breadth of sympathy can hardly fail to be promoted, where subjects of interest are so varied amongst companions united by the common principle of serious study. Although the Hall was only opened last term, we notice with pleasure that all the rooms are already taken; hence there is reasonable ground for hope that the larger scheme of the Committee will before long be realised. That the interests of students of science will be well looked after may be gathered from the fact that the presidents of the Royal Society and of the British Association, Prof. Huxley, Dr. Gladstone, Mr. Samuelson, M.P., Prof. Carey Foster, and others are aiding the scheme. Sir John Lubbock is the treasurer of the Building Fund.

DR. VON HOCHSTETTER, for many years president of the Vienna Geographical Society has resigned this post and has been nominated honorary president for life. In his stead Count Hans Wilczek was elected president.

THE death is announced of Karl Winter, the well-known electrician. He died at Vienna on December 7 last.

PROF. W. GRYLLS ADAMS, F.R.S., will deliver a course of lectures on voltaic and dynamic electricity and magnetism, and their applications to cable-testing, electric lighting, &c., at King's College, London, during the ensuing session. A course of practical work in electrical testing and measurement with especial reference to electrical engineering will also be carried on under his direction in the Wheatstone Laboratory. The lectures will be given once a week on Mondays at 2 p.m., and the laboratory will be open daily (Saturday excepted) from 1 to 4. The work will begin on Monday, January 15.

THE French Senate has diminished by a million of francs (40,000*l.*), the Budget of Public Instruction for 1883. It is regarded as a warning given to the Lower House, not to spend with too free a hand the public funds for educational purposes.

THE continual rains are creating serious apprehensions in Paris, and the Seine has again reached the level of disastrous inundations. A similar calamity is befalling other cities in France, amongst which the foremost is Lyons. The calamity having been foreseen by the Hydrological Service, all measures have been taken to diminish as much as possible the extent of the disaster. *Akhbar* states that heavy rains have been experienced in Algiers, and even at Laghouat, where it has been received with a real exultation. The newspapers are full of the disastrous floods caused by the rise of nearly all the great rivers in the Central European plain.

WITH reference to a recent note to the effect that snow fell on November 11 in Madrid to the depth of 1 foot, Mr. Gillman writes that snow began to descend early that morning, but had ceased at midday. He nowhere found it deeper than 6 inches, but this was uniform in the streets and open country. In the night of 11th–12th, the minimum thermometer marked $-11°$ cent.; barometer on Sunday stood at 688 millims.

THE appearance within the last two years of two comets has been regarded as a most menacing portent by Chinese politicians. Their resemblance to flaming swords is regarded as emblematical of the vengeance of heaven on an unworthy nation. It is stated that in consequence of the last comet, an urgent decree has been promulgated in the name of the youthful monarch, stating that it is a clear indication that the officials are lax in making proper reports to the Throne, and have been keeping the Emperor in the dark as to pestilences and other calamities among the people. His Majesty has reason to believe that improper officials have been appointed; he has, moreover, subjected his Imperial heart to a rigorous examination in the seclusion of his palace, and he is much disquieted at the result. The people, he finds, are poverty-stricken, and await relief, and the present is a time of great anxiety and embarrassment. The crisis must be met with prompt measures and a reverent heart; the ministers are accordingly enjoined to exhibit loyalty and justice, and to strenuously guard themselves against the thraldom of official routine. They are to discover the real state of the country, and to make such dispositions as may give rise to all possible advantage, and eradicate all possible evil. If all this be done, we have the Imperial assurance that the people will live in peace and quietness, till heaven be in harmony with earth, and all harmful influences allayed. If decrees were always obeyed, the comet will have exercised a beneficent influence on the condition of the Chinese people.

ALL interested in photography will find much that is useful and curious in Mr. Baden Pritchard's Year-Book of Photography for 1883.

MR. E. ROBERTS has sent us his handy and useful Tide Table for 1883, containing the times of high water at London Bridge, and showing the possible overflows; to all Londoners interested in any way in their river, this table will prove serviceable. We have also from Mr. Roberts Tide Tables for the Indian ports, and Tide Tables for the port of Hongkong, in handy little volumes, containing many carefully compiled tables calculated to be of great service.

THE total number of visitors to the Royal Gardens, Kew, for the year 1882, was 1,244,167. This is 407,491 in excess of the numbers for 1881, which in its turn was greater by 111,254 than the number of visitors in any previous year. As in 1881 the Sunday visitors (606,935) were about equal in number to those on all the other days of the week put together (637,232).

A NEW natural history magazine in the Flemish language is announced. It is published at Ghent, and the title is *Natura Maandschrift voor Natuurwetenschaffen uitgegeven door het Natuurwetenschappelijk Genootschap von Ghent.* The editors are J. MacLeod, Ed. Remonchamps, and L. Backlandt. The natural sequence is that another Belgian magazine, in Wallon, will appear. The "gift of tongues" is daily becoming more and more a necessity for a working naturalist, and De Candolle's assertion that English is destined to become the language of science seems gradually more remote in realisation.

THE December number of the *Agricultural Students' Gazette*, Royal Agricultural College, Cirencester, contains an article by Sir J. B. Lawes on the future of agricultural field experiments, in which he points out that the time when isolated field experiments were of value has passed, and that now the questions to be solved in this way are such as can only be answered by carefully conducted experiments lasting over many years. Miss Ormerod contributes a paper on the Gooseberry Caterpillar, the larva of *Nematus Ribesii*, in which she suggests the best mode of preventing its ravages. A readable summary of the recent work of Leuckart and Thomas on the life-history of the Fluke is given by Mr. Ozame. The other papers in the number are on Contagious Diseases, by Prof. Garside; on the Harvest of 1882, by Prof. Little; on Butter-making, by Mr. Weber; besides much matter of more purely College interest. We notice that the College has commenced a series of field experiments on corn crops, in conducting which doubtless the advice of Sir J. B. Lawes will be followed. This *Gazette* in its new form promises to become of permanent value, and is exceedingly creditable to its editors, students of the Royal Agricultural College.

THE third expedition fitted out by the Milan Society for the commercial exploration of Africa, will leave early this month for Massana. The leader of the expedition is Signor Bianchi, who knows Abyssinia thoroughly. Count Salimboni accompanies him as engineer, and Prof. Licata as naturalist.

PROF. DOMENICO LOVISATO and Lieut. Bove, who jointly undertook the last Italian Antarctic expedition, are about to undertake another Antarctic journey for scientific purposes.

NEWS has been received from the German traveller, Robert Flegel, who was sent out to explore the Niger-Binue district. It appears that on April 10 last the traveller crossed the Binue River to the southern shore, and reached the large town of Wukari on April 13. By way of Bantadchi he proceeded, in four days' journey, to the decaying government city of Bakundi, in one and a half days more to Beli, and thence he reached Kontcha in the Adamnua district on May 26. From Kontcha to Jola is only a seven days' route. Flegel, whose health has much improved, strongly advises the establishment of a German station in that healthy and fertile country.

WE have on our table the following books:—Sydney Observatory, Double-Star Results, 1871-81 (Sydney); Der Electricität und der Magnetism, vol. i., Clerk Maxwell (Springer, Berlin); Cutting Tools, R. H. Smith (Cassell, Petter, and Galpin); A New Theory of Nature, D. Dewar (W. Reeves); Transactions of the Sanitary Institute, vol. iii. (Stanford); The Great Pyramid, R. A. Proctor (Chatto and Windus); Microbes in Fermentation, Putrefaction, and Disease, Ch. Cameron (Baillière, Tindall, and Co.); The Nebulæ, a Fragment of Astronomical History, A. E. Garrod (Parker); Relative Mortality of Large and Small Hospitals, H. C. Burdett (Churchills), To the Gold Coast for Gold, Burton and Cameron (Chatto and Windus); Physical Optics, R. T. Glazebrook (Longman); Essays in Philosophical Criticism, Seth and Haldane (Longmans); Year-Book of Photography, 1883, H. B. Pritchard (Piper and Carter); Report on the Oban Pennatulida (A. M. Marshall and W. P. Marshall); Catalogue of *Batrachia gradientia*, G. A. Boulenger (British Museum); The Brewer, Distiller, and Wine Manufacturer (Churchills); The Churchman's Almanak for Eight Centuries, W. A. Whitworth (Wells, Gardner, and Co.); Celtic Britain, Prof. J. Rhys (S.P.C.K.); Zoological Record, vol. xviii. 1881 (Van Voorst); Rankine's Useful Rules and Tables, sixth edition (Griffin); Madeira Spectroscopic, C. Piazzi Smyth (W. and A. K. Johnston); Ragnarok, the Age of Fire and Gravel, Ig. Donnelly (Sampson Low and Co.); The Electric Lighting Act, 1882, Clement Higgins and E. W. W. Edwards (W. Clowes).

THE additions to the Zoological Society's Gardens during the past week include a Black-eared Marmoset (*Hapale penicillata* ♂) from South-East Brazil, presented by Miss Tilleard; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. W. L. Brodie; a Rose Hill Parrakeet (*Platycercus eximius*) from Australia, presented by Mr. Geo. Lawson, F.Z.S.; a Black Tortoise (*Testudo carbonaria*) from St. Thomas', West Indies, presented by Viscount Tarbat, F.Z.S.; an Indian Cobra (*Naia tripudians*) from India, presented by Capt. Braddick; two Common Curlews (*Numenius arquata*), a Common Lapwing (*Vanellus cristatus*), a Golden Plover (*Charadrius pluvialis*), British, purchased.

BIOLOGICAL NOTES

ON A NEW GENUS OF CRYPTOPHYCEÆ.—It would appear that the interesting fresh-water genus of Algæ described by Bornet and Grunow as Mazæa (*vide* NATURE, vol. xxvi. p. 557) is without doubt the same as Nostochopsis of Wood. This genus of Wood was first briefly described in the *Proc. Amer. Philos. Soc.*, 1869, and more fully, and with good figures, in the "Fresh-water Algæ of the United States," 1872. The Philadelphia species, *N. lobatus*, Wood, is referred by its discoverer to the Rivulaceæ, and is apparently a different species from that described by Bornet and Grunow from Brazil.

FEMALE FLOWERS IN CONIFERÆ.—Quite recently Celakovsky has published a very elaborate criticism (on the structure of the female flowers in Coniferæ, as detailed in Eichler's well-known treatise). To this ("Zur Kritik der Ansichten von der Fruchtschuppe der Abietineen," &c. Prag, 1882), Eichler has replied in a paper read before the Gesellschaft der Nat. Freunde zu Berlin, in which he re-states the chief points of his proof and answers *seriatim* the objections brought against it. Dr. Peters sums these up as follows:—1. In all the vegetative buds of the pine, the two front leaves (Vorblätter) converge forwards towards the bract; it is hence improbable that in the fruit scale they should be turned backwards. Celakovsky, from the fact that in weak buds the former arrangement is somewhat modified, concludes that on the complete falling away of the bud from between the front leaves, these latter are enabled to push themselves backwards and cohere: an opinion not proved. 2. While in the vegetative bud, the leaf immediately following the front leaf falls backwards in abnormal fruit-scales, the portion interpreted as the next leaf falls forwards. To the representation of Celakovsky's, that owing to the fact that the front side being, in the course of development, preferentially assisted, the leaf of the assisted front side first reaches its development, Eichler opposes the statement that in the ordinary buds there is not a trace of such a preferential furtherance. 3. The part that is regarded as the third leaf of the bud cannot be a leaf, because it has its xylem on the dorsal, and its phloëm on its ventral surface. Celakovsky takes a twist of 180° for granted. This Eichler denounces as an evasion which would bring all serious scientific discussion to an end. 4. If the fruit-scale were formed by the growing together of two front leaves upon the hinder end of their axis, the latter if it developed further, would come to stand on the front side of the fruit-scale, but *de facto* it under such conditions stands behind. As Celakovsky however thinks that the middle piece of the front scale is half turned round, and is a leaf on the front side of the bud, to which both front leaves on the front side of the bud have adhered, by which means the axis comes to be posterior: therefore this opinion stands irreconcilably contradicted by his own supposition of the simultaneous pushing back of the front leaves. 5. The simplest explanation of the bud-arrangement, and of the bud itself, is got by supposing that the bract and the fruit-scale form together a single leaf which has produced an axillary bud. Here Eichler considers himself compelled to deny the charge of having set out with pre-formed notions. The change in his former opinions was brought about by a more intimate knowledge of the facts. 6. By pressure and excitation (Reiz) the axillary bud causes further changes in the fruit-scale, the formation of the keel and wings, while the central piece which is bounded by them, can separate itself from the side portions and assume the appearance of a special leaf. To Celakovsky's objection, that through the pressure of the bud-axis, only a circumscribed depression, and not a long furrow would be formed, there is this reply, that such a furrow must be produced by the growth of the scale past the early developed bud, and that this furrow can become wider as the scale becomes broader. 7. These keels (midribs) of the fruit-scale press past the bud on both sides, and hinder the development of the first lateral bud-leaves, so that the first bud-leaf now arises upon the hinder side. This explanation, characterised by Celakovsky as a forced hypothesis, is supported by the fact that the leaves could not become formed in a place where there is no room, and because on the other hand the two lateral bud-leaves show themselves if the mid-ribs are wanting or remain feebly developed (*Botan. Zeitung*, December 8).

THE TRACHEÆ IN LAMPYRIDÆ.—Heinrich Ritter v. Wielowiejski publishes in the November number of the *Zeitschrift für wissenschaftliche Zoologie* a very detailed account of the light-producing organs in *Lampyris splendidula* and *L. noctiluca*. His investigations were carried on at Jena, in Prof. Oscar Hertwig's laboratory. He sums up the most important results as follows:—1. The tracheal-terminal-cells of M. Schultze, which become black under osmic acid, are by no means—as their name would imply—the terminations of the respiratory tubes; for these branch out further on into brush-like masses of much finer capillaries, which are without the chitine spiral; they are very attenuated, and, making their way in the peritoneal layer (peritonealhaut), are numerously distributed to phosphorescent tissue. 2. The tracheal capillaries very rarely end abruptly (blind) in the phosphorescent organs, but most frequently anastomose with one another, forming an irregular meshwork. 3. The capillaries do not seem to enter into the structure of the parenchymatous cells, but rather course along their surface, often irregularly winding around and enveloping these. 4. The tracheal-terminal-cells are nothing more than the outer elements of the peritoneal layer at the base of the tracheal capillaries, which radiate in a brush-like fashion from a chitine-spiral-trachea. Their peripheral processes represent the extension of the latter upon the capillaries, and this relationship is homologous with certain embryonic stages of the tracheal system. 5. The tracheal-terminal-cells are not the seat or point of departure of the light-development. If this appears first in their vicinity, it is only a consequence of the fact that these structures have, owing to their affinity for oxygen, stored up in themselves a supply of this gas, and give it off in greater quantity to the neighbouring tissues. 6. The light-producing function is peculiar to the parenchyma-cells of the light-producing organs. It results from a slow oxidation of a substance formed by them under the control of the nervous system. 7. The ventral light-organ was found to consist of two layers, the parenchyma-cells of which are quite similar to one another in their morphological characters, but they differ from one another in the chemical nature of their contents. 8. The parenchyma-cells (is this the case with all?) seem connected with fine nerve-endings. 9. The light-organs are the morphological equivalents of the fatty-bodies.

THE STONES OF SAREPTA (ASIATIC RUSSIA).—The remarkable masses of stone found in the white sand of the Ergent Mountains at Sarepta have often caused people to inquire how they were formed. Some of them are found of the size of a hazel or walnut, and even larger; others are cylindrical, of the thickness of a half to one werschok (16 werschok = 28 inches), and a quarter to a half arschin (28 inches) long; others again target-shaped are more than a half arschin long, and one to four werschok thick. All the cylindrical ones, which are often also forked and root-shaped, exhibit, when they are broken across, a brown kernel with a white spot in its centre. Their surface is rough, and resembles a number of drops heaped one upon and beside another. When Alexander v. Humboldt visited Sarepta, the then director, Zwick, showed him these stone formations, Humboldt, while declaring that they were worthless recent things, was unable to say how they arose. Zwick, on the other hand, regarded them as very old and very problematical. Göbel also, who was afterwards shown these stones by Zwick, was unable to explain how they were formed. When Auerbach, the secretary of the Moscow Natural History Society, paid Alex Becker a visit twenty-eight years ago, he was brought to the place where these stone deposits were. He looked for an explanation of the formation of these stones and the reason of each stone containing a brown kernel. He was told that the stones were formed by roots. Auerbach said that these would form hydrochloric acid by decomposition. Becker now believes that he can with certainty assert that these formations arise round the roots of several plants that contain *milky* juice. *Tragopon ruthenicus*, *Scorzonera ensifolia*, and *Euphorbia gerardiana* grow plentifully in the white sand. Their long roots are inhabited and seamed by insects, and when their surface is once lacerated, their milky juice keeps perpetually flowing, and as it is sticky, the chalk-containing sand (the sand's colour is due only to the presence of chalk) settles firmly around the root. The root gradually dies, disappears, and there remains in its place a white, often hollow, kernel, together with the brown colour of the root-cortex. As the root is white under its cortex, the kernel also appears white, surrounded with the brown layer of the root-cortex. The round and target-shaped ones may originate from the milky juice running away into the sand, and therefore hardly any of them exhibit a brown kernel. Their guttiform surface can be explained by the drops of the milky juice. The cylindrical, forked, and root-shaped stones show clearly the form of the roots. *Euphorbia gerardiana*, to which these stone formations are chiefly ascribed, has very long roots, root-branches and root-fibres (Alec Becker, *Bull. de la Soc. Imp. des Natur. de Moscou*, 1882, No. i. p. 48).

AMERICAN RESEARCHES ON WATER ANALYSIS[1]

THE chemical results as to animal in contrast with vegetable organic matter in water, support, in general, the conclusions that have been usually drawn as to the source of organic matter, based on the more highly nitrogenous character of that from animal than that from vegetable *débris*. Still the necessity

[1] Concluded from p. 213.

for caution is shown; *e.g.* samples containing the refuse of canning tomatoes might have been erroneously thought contaminated with animal matter; others, containing a watery infusion of human fæces, with vegetable matter, &c.

Of the biological results under the same head, the most noteworthy is the well-marked pathological effect on rabbits of the injection of waters contaminated solely by such vegetable matter as would usually be thought harmless, *e.g.* peaty water. True, in the well-marked cases, the amount of organic matter present was large, but not beyond that in water sometimes used for drinking purposes. The Dismal Swamp water is an example; it has often been chosen for ship-supply, and has been spoken of as a source of supply for the city of Norfolk. On the theory (which has much in its favour) of disease caused by drinking water being due to the presence and action of living organisms, there might possibly be safety in drinking a peaty water, or water filtered through dead forest leaves, when fresh; danger, when, after some exposure, bacteria had been developed; and safety, again, perhaps, after the growth of these had fallen off, and more or less of the available organic matter had been consumed. Ship-captains say the Dismal Swamp water, after a time, becomes remarkably good and wholesome.

As to the putrescent or non-putrescent character of organic matter in water, the chemical evidence goes to prove (in opposition to Tidy's opinion) that the proportionate consumption of oxygen from permanganate within the first hour is rather greater for those waters containing vegetable than for those containing animal matter. Dr. Smart has expressed the opinion that gradual evolution of albuminoid ammonia indicates the presence of organic matter (vegetable or animal) in a fresh or comparatively fresh condition, while rapid evolution indicates that it is putrescent. His interpretations in this respect proved to be correct in a large proportion of cases, but not always.

The biological results under this head accord, on the whole, with the general belief that putrescent organic matter is more dangerous than that in a fresh or but slowly decomposing condition.

Prof. Mallet proceeds to state some *general conclusions with a view to sanitary application as to the value, separately and collectively, of the different processes of water analysis which have been under examination.*

It is not possible to decide absolutely on the wholesomeness or unwholesomeness of a drinking water by the mere use of any of the processes examined for the estimation of *organic matter*, or its constituents. Not only must such processes be used in connection with investigation of other more general evidence, as to the source and history of a water, but this should even be deemed of secondary importance in weighing the reasons for accepting or rejecting a water not manifestly unfit for drinking on other grounds.

There are no sound grounds on which to establish such general "standards of purity" as have been proposed, looking to exact amounts of organic carbon or nitrogen, "albuminoid ammonia," oxygen of permanganate consumed, &c., as permissible or not.

Chemical examination may be quite legitimately applied, first, to the detection of *very gross pollution* (as of a well from crushing of soil pipes), and secondly, to periodical examination of a water supply, so that suspicious changes from the ascertained normal character of the water may be promptly determined and their cause investigated. In the latter connexion there seems to be no objection to the establishment of *local* "standards of purity," based on thorough examination of the supply in its normal condition.

A careful determination of the nitrites and nitrates seems very important.

If he had to watch a large city water supply, the author would use all the three processes; each gives information which the others do not. Where only simple means were practicable, the albuminoid ammonia and permanganate processes might be employed together; but in no case should one only of these methods be resorted to.

Practical Suggestions as to the Use in their present form of the Chemical Processes Studied.—In general, water samples should be examined with the least possible delay after collection. Besides examination of a water in its fresh condition, samples of it should be set aside in half-filled but closed glass-stoppered bottles for (say) ten or twelve days, and one of these examined every day or two, to trace changes undergone.

In the case of the combustion process, however skilful the analyst, duplicate or even triplicate concordant results should be insisted on. To avoid the presence of ammonia from coal gas, in the atmosphere about the water-bath, the bath should be heated by steam brought in a small closed pipe from a distant boiler (preferably in another room), and the waste steam and condensed water should be carried off to a safe distance.

As to the albuminoid ammonia process, it would be well to adopt the rule that the distillation be stopped when, and not before, the last measure of distillate collected contains less than a *certain proportion*, say 1 per cent., of the whole quantity of ammonia already collected. To diminish the loss of amines or other volatile forms of nitrogenous matter, a separate distillation should be made with alkaline permanganate added *at once*, besides the usual course of treatment prescribed by Wanklyn, and the results of the two distillations compared. The details of the evolution of ammonia should always be given.

The Tidy form of the permanganate process is rather to be recommended than that of Kubel, if but one be used. The time during which the permanganate is allowed to act in the Tidy process should be increased to at least 12, better to 24 hours, *several* determinations, on different samples set aside at the same time, being made at (say) 1, 3, 6, 9, and 12 hours, to trace the progress of the oxidation.

Suggestions as to possible Improvements on the Processes examined deserving further Investigation—Combustion Process.—The author proposes to evaporate the water in a closed vessel immersed in a water bath, and connected with a good (water jet) air pump, a condensing worm being provided for the aqueous vapour, the feed to be managed through a nearly capillary tube with a glass stop-cock. The evaporation would thus be effected within a moderate time at a fixed temperature much lower than the boiling point. The loss of organic matter by simple volatilisation or oxidation would be greatly reduced; much less sulphurous acid would be required; the tendency to formation of sulphuric acid would be reduced to a minimum, and absorption of ammonia from the atmosphere about the dish quite prevented. In testing this last effect, two bulb tubes containing pure sulphuric acid might be interposed between the vacuum chamber and the pump.

For certain reasons it might be well to evaporate at first with the addition of a small excess of magnesia (as recommended by Lechartier), thus removing all ammonia, and then, the water having been brought down to a small volume, add a moderate excess only of sulphurous acid with a drop of a solution of ferrous salt (as directed by Frankland), and complete the evaporation to dryness—the whole in a jet pump vacuum, as suggested.

Further experiments as to the Williams method (copper-zinc couple) for removal of nitrates, are desirable.

From preliminary experiments, the author thinks nitrates and nitrites may be completely reduced by evaporating to a small bulk with no great excess of phosphorous or hypo-phosphorous acid, guarding against evolution of phosphuretted hydrogen by use of a low temperature, then adding magnesia in small excess, and completing the evaporation. The plan deserves to be carefully tested.

Albuminoid-ammonia Process, including Determination of free Ammonia.—To prevent (or at least largely reduce and make uniform) the loss of ammonia from imperfect condensation, the author would use a retort in a saline solution kept heated by steam (at say 102° or 105° C.), and condense in a glass worm surrounded by ice water, till the distillate should be brought to a uniform temperature not over (say) 5° C. It might be still better to distil in a completely closed apparatus, with a fixed difference of temperature between the retort and the far end of the condensing tube, with glass stopcock to draw off the distillate in successive measured portions, and a small safety-valve near the cold end.

In determination of free ammonia, it might be well to try a closed distilling apparatus connected with a (water-jet) air-pump, so as to maintain a partial vacuum within, keeping the retort at a fixed temperature much below 100° C., and collecting all the ammonia in a flask and one or two bulb tubes, with weak mineral acid placed between condenser and pump. There would be the disadvantage, however, that the progress of evolution of ammonia could not be easily traced by its collection in separate successive measures of the distillate; and it would be necessary to ascertain whether the application of the Nessler test would be at all interfered with by the sodium salts formed from the acid used.

To overcome, if possible, the most serious difficulty in the way of correct determination of free ammonia, viz. the ready breaking up of urea (and other amides) when present, on heating with sodium carbonate, it would be well to ascertain if Schlœsing's method for determination of ammonia admits of being applied to such excessively minute amounts of it as the water analyst is concerned with.

In conduction of the albuminoid-ammonia process proper, *i.e.* the distillation with alkaline pe manganate, the author would keep the original volume of liquid in the retort constant, by admitting ammonia-free distilled water through a capillary tube. with a glass stop-cock. When there is so much organic matter as to reduce, wholly or greatly, the usual charge of alkaline permanganate, he would first determine at about what a rate the reagent is used up, then progressively supply its solution, so as to keep the original strength as nearly as possible unaltered.

Permanganate Process.—The principle involved in the last paragraph applies also to this process. There should be a constant excess of permanganate all through the process. The process should be carried on at a pretty nearly fixed temperature (say 20° C. if the Tidy method be followed).

In conclusion, the author expresses a wish that more extended biological experiments should be made as to the effects of water variously polluted on the lower animals (other animals as well as rabbits), and that the action of water introduced into the stomach as well as hypodermically injected, should be tested. It would be well to have chemical examinations, on uniform plan, from time to time made of the water supply of the largest cities at periods when the general assent of medical men indicates unusual prevalence of, or exemption from, the classes of disease most probably capable of origination from the organic pollution of drinking-water. The author would especially suggest a combined chemical and biological inquiry as to the possible effects upon living animals of the ferment or ferments of nitrification in different stages of that process. Some minor questions connected with development of nitrites and nitrates from decomposing organic matter also deserve further examination.

LOCKYER'S DISSOCIATION-THEORY[1]

IN February, 1880, I took occasion, on the ground of my observations to the spectrum of chemically pure hydrogen, to take objection, to Lockyer's view, that calcium, at a very high temperature, is dissociated.[2] From the fact, *inter alia*, that of the two calcium lines, H' and H'', only the first is present in the spectra of so-called white stars photographed by Huggins, Lockyer proceeded to lay down the theory that calcium at a high temperature is decomposed into two substances, X and Y, of which the first gives the line H', the other the line H'', and that in the stars referred to, only the first is met with. Against this I urged that hydrogen, besides the four known and easily visible lines, has a remarkable line of very intense photographic power, which nearly coincides with Fraunhofer's H', and that one is the more warranted in regarding the supposed calcium-line observed by Huggins as a fifth hydrogen line, that the hydrogen lines in the spectra of those stars are developed in a striking manner, and also the ultra-violet star lines observed by Huggins, agree with the ultra-violet hydrogen lines photographically fixed by me.[3]

Lockyer, however, has not given up his idea of dissociation, but sought new proofs of it by the spectroscopic method.

He calls attention to the fact, *inter alia*, that in the spectrum of sun-spots, certain iron-lines appear broadened, and others not; that, moreover, many of them, as λ 4918 and λ 4919·7 do not occur in the spectrum of protuberances, which show other iron lines, but do in the spectrum of spots; that in the latter again, the iron lines are occasionally absent, which the former contain, and he proceeds to say: "there is, accordingly, no iron in the sun, but only its constitnents."[4]

This argumentation Liveing and Dewar[5] have already opposed, having proved that certain spectral lines of a substance, *e.g.* λ 5210 magnesium, and various calcium-lines, are only visible when certain foreign matters are present; in this case hydrogen on the one hand, and iron on the other; that accordingly the absence of certain iron lines in the spectra of the spots or protuberances may not be attributed to a dissociation, but to the absence of foreign matters which occasion the appearance of these lines in force.

Lockyer now takes his stand, however, on another fact, which is not explained by Liveing and Dewar's experiments, and which certainly seems to afford a firmer basis for his theory of dissociation than the facts referred to above. He says:[1]

"The last series of observations relates to the degree of motion of vapours in the sun-spots, which it is known, is indicated by changes in the refrangibility of lines. If all lines of iron in a spot were produced by iron vapour, which moves with a velocity of 40 km. in a second, this velocity would be indicated by a change of the refrangibility of *all* lines. But we find that that is *not* the case. We find not only different motions, which are indicated by different lines, but observe in the degree of motion the same inversions as in the breadth of the lines. This fact is easily explained, if we suppose dissociation, and *I know no more simple way of explaining it.*"

Lockyer cites as an example that in the spots of December 24, 1880, and January 1 and 6, 1881, a certain number of iron lines appeared bent, while others remained straight.

Now I believe it is possible to explain these facts on the basis of numerous observations in spectral analysis of absorption without needing to have recourse to the hypothesis of dissociation.

It is known that the position of the absorption-band of a substance depends very essentially on the dispersion of the medium in which it is dissolved or incorporated. One often observes that in strongly dispersive media the absorption-bands of a substance are displaced towards the red.[2] Now, the remarkable case often here occurs that certain absorption-bands are displaced with the increase of dispersion of the solvent, while others are not. Thus Hagenbach observed that, *e.g.*, the chlorophyll bands I. III. and IV. lie more towards red in alcoholic than in etheric solution, while the band II. in both solutions shows exactly the same position. I observed similar cases with uranian protoxide salts[3] and with cobalt compounds.[4]

Now Kundt has already called attention to the fact, that for absorption-spectra of gases the same rule holds good as for the absorption spectra of liquid substances. He adds, indeed: "It is only questionable whether, if, *e.g.* hyponitrate gas be mixed with various other transparent gases, the displacements of the absorption-bands are so considerable, that they can be perceived." This doubt, however, does not affect the rule supposed, but merely its experimental verification.[5] The supposition, then, is permissible that, in the same way as with liquids, added media also affect the position of absorption-bands in the case of gases, and that in this case, as in the other, displacements of certain bands occur, while the position of others remains unaltered.

When, therefore, in sun-spots, certain iron lines suffer a displacement, and others in the same place do not, the cause is not motion, but the admixture of a foreign, strongly dispersive gas, which acts on the displaced lines and not on the others. It follows from this, further, that curvatures of absorption lines of the sun-spots need not by any means be always explained as due to motion of the absorbing gases in the direction of the line of observation, but only where all lines of a matter participate in the curvature.

That bright lines of a luminous gas, also, in like circumstances, "by admixture of another non-luminous vapour, or one giving a continuous spectrum," may suffer a displacement, Kundt has already shown.

[1] A paper by Herr Hermann W. Vogel, read to the Berlin Academy on November 2, 1882. Communicated by the author.
[2] *Proc. Roy. Soc.*, xxviii. 157.
[3] *Monatsb. der Berliner Acad. der Wiss.*, 1880, p. 192.
[4] *Comptes Rendus*, t. xcii. 904.
[5] *Proc. Roy. Soc.*, 30, 93. *Wied. Beibl.*, iv. 366.

[1] Herr Vogel quotes a translation in *Naturforscher*.
[2] Kundt, *Jubelband Pogg. Ann.*, *p.* 620
[3] Vogel, "Pract. Spectralanalyse." Nördlingen bei Beck. P. 248.
[4] *Monatsb. der Akad. der Wiss.* of May 20, 1878.
[5] Kundt formerly doubted also the possibility of proof of an anomalous dispersion in gases and glowing vapours. Recently, however, he has succeeded in getting such proof in the case of sodium vapours (*Wied. Ann.* 10, p. 321).

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

MARCUS M. HARTOG, D.Sc., M.A., F.L.S., has been appointed to the Professorship of Natural History at Queen's College, Cork, vacant by the death of Prof. Leith Adams.

SOCIETIES AND ACADEMIES

London

Royal Society, December 14, 1882.—On the genus *Meliola*, by H. Marshall Ward, B.A., Fellow of Owens College, Manchester. The author has examined the life-history and structure of several species of these epiphytic fungi. The fungus consists of a much-branched mycelium, on which appendages and fruit-bodies occur. The *hyphæ* constituting the *mycelium*, consist of cylindrical cells, with hardened, brown cell-walls and protoplasmic contents; these are attached to the epidermis of the leaves, &c., of tropical plants by rudimentary *haustoria*, which do not pierce the cell-walls of the host, but are firmly adherent to the cuticle. The appendages consist of simple or branched setaceous outgrowths, which spring from the mycelium at various points, and are especially developed around the fruit-bodies from masses of hyphæ, which Bornet considered as forming a special part of the fungus, under the name of "receptacle"; these setæ cannot be considered as subserving any special function, however, and are certainly not tubes for the outlet of spores, as earlier observers have surmised. Other appendages occur in the form of small ovoid or flask-shaped lateral branchlets; some of these become free and subserve vegetative reproduction as *conidia*. The fruit-body, or *perithecium*, arises by continuous development of one of the pyriform lateral branchlets, and the author has studied its development very particularly. The short, ovoid, unicellular branchlet, after becoming separated from the parent hypha by a septum, suffers division into two cells by a septum running obliquely across it; of these two cells one produces the outer walls of the *perithecium*, by continuous cell-multiplication, whilst the other contributes the central portion, or *ascogonium*, by slower division of its contents.

The former cell, dividing up more rapidly, produces a layer of cells which envelope the latter by a process of "epiboly," and the outer cell-walls become hard, thick, and dark-coloured. The latter—ascogenous cell—divides up more slowly into a "core" of thin-walled cells, very rich in protoplasm. After complete envelopment, the cells of the "core" are recognised in vertical sections; certain of them become elongated, and form the earliest *asci*, while others become absorbed—together with inner cells of the enveloping layer—to provide nutritive material for the developing *asci* and their progeny.

The *asci* are produced successively, and are delicate clavate sacs, containing two to eight *spores*, each spore being divided by one, two, or three cross septa. The germination of the *spore* is also described and figured; it throws out an irregular germinal tube, which soon forms rudimentary *haustoria*, and grows forth as a mycelium, similar to that from which the *perithecium* was produced.

The author examines and criticises the views held by Bornet and Fries as to the systematic position of *Meliolas*; especially the opinion that they are to be considered as tropical representatives of the European *Erysipheæ*. He shows that the original cell from which the *perithecium* arises must be regarded as containing in itself the undifferentiated elements of an *Archecarpium* and *Antheridium*-branch (in the sense of De Bary and others), and that after the primary division into two cells, we must look upon one of these—the one which becomes more rapidly divided up—as the homologue of the *antheridium*-branch and enveloping tissues of the *Erysiphæ*; the more slowly divided cell—which produces the *ascogenous* core—being the equivalent of the *ascogonium*, &c., of those fungi. The details of successive phases of development are amply illustrated by figures and many peculiarities acquired by the group are carefully examined and described.

The author concludes that the *Meliolas* must be looked upon as a group developed along similar lines to those of the *Erysipheæ*, *Eurotium*, &c., but in which the sexual process has suffered still greater reduction or withdrawal, leading to those forms in which it is entirely suppressed.

With respect to the injurious action of these fungi on their hosts, the author decides that no direct parasitic action on the cell-contents takes place, but that injury results indirectly on account of the dense black *mycelium*, when strongly developed, depriving the leaves of air, light, &c.

Chemical Society, December 21, 1882.—Dr. Gilbert, president, in the chair.—The following papers were read:—On the condensation products of oenanthol (part ii.), by W. H. Perkin, jun. The author has studied the action of nascent hydrogen upon oenanthol; when this substance is dissolved in acetic acid and acted upon by sodium amalgam, heptylic alcohol is produced, also an aldehyde, $C_{14}H_{26}O$, and an alcohol, $C_{14}H_{28}O$; if the oenanthol is dissolved in ether, heptylic alcohol, a solid aldehyde melting at 29°·5 ($C_{14}H_{28}O$), and a second substance, $C_{21}H_{40}O$, are formed. By oxidising the aldehyde $C_{14}H_{28}O$ with silver oxide, a small quantity of an acid, $C_{14}H_{28}O_2$, boiling at 300°–310°, was obtained. The author has also studied the action of nascent hydrogen upon the aldehyde $C_{14}H_{28}O$, and discusses the constitution of these new bodies.—On the behaviour of the nitrogen of coal during destructive distillation; with some observations on the estimation of nitrogen in coal and coke, by W. Foster. It is usually stated in text-books that coal contains about 2 per cent. of nitrogen, which, during destructive distillation of the coal, comes off as ammonia. The author finds that this statement is not true. A Durham coal was used, containing 1·73 per cent. of nitrogen, and giving 74·5 per cent. of coke. If the total quantity of nitrogen in the coal be taken as 100, that evolved as ammonia is only 14·5 per cent.; as cyanogen, 1·56 per cent; as nitrogen in the coal-gas, 35·26 per cent.; left behind in the coke, 48·68 per cent.—On the absorption of weak reagents by cotton, silk, and wool, by E. J. Mills and J. Takamine. The reagents are sulphuric and hydrochloric acids, and caustic soda. This paper chiefly contains tables, with results calculated to five places of decimals.—On brucine, by W. A. Shenstone. Various observers have stated that brucine, when treated with dilute nitric acid, yields either methyl or ethyl, nitrate or nitrite. The author has studied the action of hydrochloric acid upon brucine quantitatively, and has proved that more than one molecule of methyl chloride is evolved from one molecule of brucine; he conclndes that brucine is strychnia, in which two atoms of hydrogen are replaced by two methoxyl groups, and its formula may be written, $C_{21}H_{20}(CH_3O)_2N_2O_2$.—Researches on the induline group, by O. N. Witt and E. G. P. Thomas. "Induline" is a term applied to all coloured compounds formed by the action of amidoazo compounds on the hydrochlorides of aromatic amines with elimination of ammonia. The authors have studied in the present paper the formation of amidoazobenzene, and its action on aromatic hydrochlorides, and especially on anilin hydrochloride.—Preliminary note on some diazo derivatives of nitrobenzylcyanide, by W. H. Perkin.

Meteorological Society, December 20, 1882.—Mr. J. K. Laughton, M.A., F.R.A.S., president, in the chair.—Three new Fellows were elected, and Capt. J. de Brito Capello and Mr. W. Ferrel, M.A., were elected honorary members.—The following papers were read:—Popular weather prognostics, by the Hon. R. Abercomby, F.M.S., and Mr. W. Marriott, F.M.S. The authors explain over 100 prognostics, by showing that they make their appearance in definite positions relative to the areas of high and low atmospheric pressure shown in synoptic charts. The method adopted not only explains many which have not hitherto been accounted for, but enables the failure, as well as the success, of any prognostic to be traced by following the history of the weather of the day on a synoptic chart. The forms discussed are:—cyclones, anticyclones, wedge-shaped and straight isobars. The weather in the last two is now described for the first time. They also point out (1) that prognostics will never be superseded for use at sea, and other solitary situations; and (2) that prognostics can be usefully combined with charts in synoptic forecasting, especially in certain classes of showers and thunderstorms which do not affect the reading of the barometer.—Report on the phenological observations for the year 1882, by the Rev. T. A. Preston, M.A., F.M.S. The most important feature of the phenological year was the mild winter. The effect of this upon vegetation was decidedly favourable; and had it not been for the gales—especially that of April 28—the foliage would have been luxuriant, and therefore free from insect attacks, but the contrary effect has been produced on insect life, for the scarcity of insects, especially butterflies and moths, has been the general remark of entomologists.—Mr. J. S. Dyason, F.R.G.S., exhibited a series of typical clouds in monochrone, and also a series of sketches of clouds in colour, made in June, July, and August, 1882.

Manchester

Literary and Philosophical Society—Microscopical and Natural History Section, December 12.—Prof. Roscoe in the chair.—Mr. James Heelis made some remarks upon the causes of the movement of the old Rhone Glacier with special reference to the power of gravity to produce such movement when considered in connection with the gradient down which the glacier has passed.—Prof. Osborne Reynolds, F.R.S., communicated and explained an elementary solution of the dynamical problem of isochronous vibration.—Mr. John Boyd exhibited a fine living specimen of *Argulus foliaceus*, a parasite of the carp.—Mr. Charles Bailey, F.L.S., made some remarks on the occurrence of *Selinum carvifolia* in Lincolnshire, and of *Potamogeton zizii* in Lancashire and Westmoreland, and mentioned the localities where he had met with them respectively.—Mr. R. D. Darbishire, F.G.S., gave an account of dredgings made by him in company with Dr. A. M. Marshall and Mr. Archer at Oban in September last, and exhibited specimens of a considerable variety of animals taken.—Prof. A. M. Marshall, M.A., gave a detailed description of three forms of Pennatulida met with during the dredging, and suggested the desirability of the section undertaking or taking part in similar excursions in future years.

DUBLIN

Royal Society, November 20, 1882.—Sections I. and III. Physical and Experimental Science, and Applied Science.—Rev. Gerald Molloy, D.D., in the chair.—The following communications were received:—Rev. H. M. Close, M.A., on the definition of force as the cause of motion, with some of the inconveniences connected therewith.—G. Johnston Stoney, D.Sc., F.R.S., and G. Gerald Stoney, on the energy expended in propelling a bycicle, parts 2 and 3.—Prof. W. F. Barrett, F.R.S.E., physical apparatus for class-teaching.—A. H. Curtis, LL.D., improved apparatus for exhibiting double reflection in the interior of a crystal.—Prof. G. F. Fitzgerald, F.T.C.D., recent advances in physical science, an account of Prof. Rowland's curved gratings for spectrum analysis.—Prof. Fitzgerald exhibited photographs of the solar spectrum taken by Prof. Rowland.

Section II. Natural Science.—Rev. A. H. Close, M.A., in the chair.—The following communications were received:—Prof. V. Ball, M.A., F.R.S., on some effects produced by landslips and movements of the soil cap, and their resemblance to phenomena which are generally attributed to other agencies.—Prof. A. C. Haddon, M.A., exhibition of marine invertebrates, belonging to the Natural History Museum, prepared at the Zoological Station, Naples, with remarks upon the various methods for the preparation of zoological specimens.—G. A. Kinahan, on the geology of Bray Head.

December 18, 1882.—Sections I. and III. Physical and Experimental Science, and Applied Science.—A. H. Curtis, LL.D.; in the chair.—The following communications were received:—G. F. Fitzgerald, F.T.C.D., on Dr. Eddy's hypothesis that radiant heat is an exception to the second law of thermodynamics.—Communicated by Howard Grubb, M.E., F.R.A.S.: (*a*) Notes on the transit of Venus, as observed at Armagh Observatory by Dr. Dreyer; (*b*) Notes on the transit of Venus, as observed at Cork Observatory by Prof. England; (*c*) Notes of the transit of Venus, as observed at Rathowen, Co. Westmeath, by Mr. W. E. Wilson.—G. Johnstone Stoney, D.Sc., F.R.S., on means of neutralising echoes in rooms.—G. Johnstone Stoney and G. Gerald Stoney, on geared bicycles and tricycles.—Dr. Otto Bœddicker, on the influence of magnetism on the rate of a chronometer (communicated by the Right Hon. the Earl of Rosse, F.R.S.)—Mr. Grubb informed the Society that Dr. Huggins had authorised him to announce that he had succeeded in photographing the corona of the uneclipsed sun by employing absorbing media.

PARIS

Academy of Sciences, December 18.—M. Jamin in the chair.—The following papers were read:—On a recent memoir, by M. Wolf, of Zurich, on the periodicity of sun-spots, by M. Faye. From further careful study (by a method described) of data for the last 120 years, M. Wolf concludes (1) that there is a period of 10 years; also (2) a period of 11 years, 4 months; and (3) that there is not a period of 12 years, imputable to the action of Jupiter. Spite of the great difference of the two periods, the interval between a minimum and the next maximum is the same in both, viz. 4½ years. After 170 years the phenomena recur in the same order, and with the same numerical values. M. Faye added some remarks by way of theory.—Statistics of preventive vaccination against *charbon* relating to 85,000 animals, by M. Pasteur. The figures (for Eure-et-Loire, where the ravages have been worst) show a marked reduction of the mortality from *charbon*; thus, of the 80,000 sheep vaccinated, only 0·65 per cent. died, whereas the average mortality of the past 10 years is 9·01 per cent.—Contribution to the study of rabies, by M. Bert. He gives results published a few years ago, but little known. *Inter alia*, inoculation with mucus from the respiratory passages of a mad dog caused rabies, but that with the salivary liquids did not. Reciprocal transfusion of blood between a healthy and a mad dog did not cause rabies in the former. The slaver of a mad dog, after filtering through plaster, was harmless, but the portion caught on the plaster caused rabies (which is thus probably due to a microbe).—On the functions of seven letters, by M. Brioschi.—Experiments with a new arrangement of the automotor elevating apparatus with oscillating tube, by M. de Caligny.—M. Faye presented the second and last volume of his "Cours d'Astronomie."—M. de Quatrefages announced the formation of a committee, headed by M. Milne-Edwards (who is now convalescent) for a monument to Darwin, as proposed in England.—On maize at different periods of its vegetation (continued), by M. Leplay.—M. Ladureau (in a memoir) stated that he has found, on an average, 1·80 cc. of sulphurous acid (free and combined) per cubic metre of air in the atmosphere of Lille.—The Secretary called attention to a new work on Galileo, by Signor Favaro, asked to be informed of any documents relative to Fermat (whose works are to be published by the Minister of Public Instruction), and read some telegrams on the transit of Venus.—Observations of the transit of Venus at Algiers Observatory, by M. Trépied. Bad weather marred the work. The spectrum, and photographs taken in the green, blue, and violet, showed no absorption by an atmosphere round Venus.—On the transit as observed at Rome, by M. Millosevich. He thinks the spectroscopic method the only one capable of giving good results, which admit of being tested, for the first contact.—On the great southern comet, as observed at the Imperial Observatory of Rio de Janeiro, by M. Cruls. On October 15 there were two nuclei, and he thinks the appearance of the tail due to two tails (corresponding to the nuclei).—On solar photometry, by M. Crova. By a method described, and by Bougner's method, he arrives at about 60,000 carcels for the intensity of the solar light on a clear day (at Montpellier), an estimate ten times those of Bougner and Wollaston.—Reply to M. Ledieu, &c., by M. Decharme.—On the sensation of white and complementary colours, by M. Rosenstiehl. The introduction of a coloured object in an illumination apparently homogeneous and colourless, at once shows the real lack of homogeneity in the combination of lights. There is often confusion between mixture of lights and mixture of sensations.—Researches on the duration of solidification of surfused substances, by M. Gernez. He worked with U tubes holding phosphorus. The course of the phenomenon is uniform. Previous heating of the phosphorus to different temperatures did not sensibly affect the velocity of solidification. M. Gernez studies the curve for velocity of solidification at different temperatures (43°·8 to 24°·9).—On the measurement of pressures developed in a closed vessel by explosive gaseous mixtures, by M. Vieille. The method (described) was to register the law of displacement of a piston of known section and mass; (results shortly).—On the crystallisation of hydrate of chlorine, by M. Ditte.—On chloride of pyrosulphuryl, by M. Konovaloff.—On the products of distillation of colophanry, by M. Renard.—Production of surgical anæsthesia, by combined action of protoxide of nitrogen and chloroform, by M. de Saint Martin. With protoxide of nitrogen (85 vol.), and oxygen (15 vol.) M. Bert got anæsthesia by operating under pressure. If 6 or 7 gr. chloroform be added per hectolitre, the effect is had quickly at ordinary pressure.—Passage of the bacterium of *charbon* from mother to fœtus, by MM. Strauss and Chamberland.—Physiological properties of oxethylquinoleïne-ammonia, by M. Bochefontaine. Like curare, it prevents passage of excitation from nerve to muscle, but, unlike curare, it makes the heart beat more slowly.—Experimental researches on spontaneous contractions of the uterus in certain mammalia, by M. Dembo.—On the formation of embryonal layers in the trout, by M. Hennegny.—Remarks on M. Lichtenstein's paper on pucerons, by M. Balbani.—Orographic note on the region of the Jura between Geneva and Poligny, by M. Bourgeat.—On a phenomenon of molecular mechanics, by M. Trève. He covers the tops of ivory balls, hung in a row, with metallic powder; when one end ball (say the left) is drawn back and let fall on its neighbour, the powder on the right half of the balls is thrown in the direction of the shock; but that on the last ball is thrown from the side opposite to the direction of the shock.

December 26.—M. Jamin in the chair.—The following papers were read:—Observations of the transit of Venus at the Naval Observatory of Toulon, by M. Mouchez. M. Rozet observed the black drop at second contact.—On two objections of Prof. Young of New Jersey, to the cyclonic theory of sunspots, by M. Faye. These are, the absence of visible traces of rotation in most spots, and the small difference of angular velocity in successive zones of the photosphere. M. Faye holds the unequal velocity sufficient to cause vortical movements of any calibre; and the general absence of agitation at the border of spots he attributes to the slowness of gyration there (our cyclones seen from above would show the same). He cites a number of positive indices of gyration.—Theory of the resistance of woven materials to extension, by M. Tresca. Such stuffs suffer elongations which increase less rapidly than the weights; and with equal weight, they show much greater elongation than those of the warp-threads composing them. The mode of interlacing of the threads explains these differences.—On the necessity of introducing certain modifications into the teaching of mechanics, and of banishing certain problems; *e.g.* the motion of the solid body of geometers, by M. Villarceau.—Considerations on the general theory of units, by M. Ledieu.—Separation of gallium (continued), by M. Lecoq-de-Boisbaudran. — Herr Bunsen was elected Foreign Associate in room of Wöhler, deceased.—Chemical studies on maize, &c. (continued), by M. Leplay.—Evolution of microscopic organisms in the living being, and in the dead body and morbid products, by M. Colin. Microbes are nowhere absent in the respiratory and digestive apparatus, and at many points they are prodigiously numerous. In normal conditions the liquids holding them are harmless, but they become dangerous after putrid alteration.—The first number of a new mathematical journal, *Acta Mathematica*, published at Stockholm (M. Mittag-Leffler, editor), was presented.—A telegram from Montevideo announced success of the transit observations at Santa Cruz.—Observation of the transit of Venus at Nice Observatory, by M. Mouchez. Five photographs were had, under difficult conditions.—Observation of the transit at Avila (Spain), by M. Thollon. They sought to observe Venus's atmosphere spectroscopically at a height of 1200 m., but bad weather prevented their getting any satisfactory results.—Photographs of the great comet of 1882 taken at the Observatory of the Cape of Good Hope, by Mr. Gill. Spite of long exposure (140 minutes for the sixth and last photograph), the stars at the centre of the image are remarkably distinct. More than fifty stars are seen through the tail. Mr. Gill does not doubt that stellar maps might be produced by direct photography of the heavens.—On the formula recently communicated to the Academy regarding prime numbers, by M. de Jonquières.—On the same, by M. Lipschitz.—Reply to a recent note by M. Lalanne on the verification and use of magnetic maps, by M. de Tillo.—Electrodynamic method for determination of the ohm; experimental measurement of the constant of a long coil, by M. Lippmann. —Measurement of the photometric intensity of spectral lines of hydrogen, by M. Lagarde. The curves from the values obtained show the inequality of intensity of the three lines, inequality variable with the induced discharge. With diminution of pressure, the curve straightens; at 6·5 mm., that for red is a straight line.—On the instantaneous pressure produced during combustion of gaseous mixtures, by MM. Mallard and Le Chatelier. With mixtures of H and O, the interior pressure exceeded by more than 2 atm. that corresponding really to the temperature of combustion; and this occurred in less than a ten-thousandth of a second. An explanation is offered.—On bisulphhydrate of ammonia, by M. Isambert.—On a case of physical isomerism of monochlorinated camphor, by M. Cazeneuve.—Biological researches on beet, by M. Corenwinder.—On the reduction of sulphates by sulphuraria, and on the formation of natural metallic sulphates, by M. Plauchud.—On the transformation of nitrates into nitrites, by MM. Gayon and Dupetit. They have isolated four distinct microbes capable of the action; one can live in chicken broth even when this is saturated with nitrate of potash, and decompose 10 gr. of the nitrate per litre daily. The microbes of chicken cholera, the bacterium of charbon, and the septic vibrion, effect denitrification much less easily. —On the poisonous principles of edible fungi, by M. Dupetit. Injecting, subcutaneously, the fresh juice of several such fungi into rabbits, &c., he observed symptoms of poisoning, followed by death. The chemical properties of the active principle recall those of soluble ferments, rather than of known alkaloids. A temperature of 100° renders the juice harmless.—Researches on the production of a general anæsthesia or a specially unilateral anæsthesia by a simple peripheric excitation, by M. Brown-Séquard. Irritation of the laryngeal mucous membrane with a current of carbonic acid will produce anæsthesia in all parts of the body, without passage of this gas into the blood.—On the physiological action of coffee, by M. Guimaraeo. The experiments (made on dogs at Rio) prove that coffee is at once a stimulant and a repairer. By permitting a greater expenditure and consumption of argotised substances, it evidently increases the power of work.—On the structure of cells of the mucous bodies of Malpighi, by M. Ranvier.—On the fœtal envelopes of Chiroptera of the family of Phyllostomides, by M. Robin.—On an usteria from great depths of the Atlantic, provided with a dorsal peduncle, by M. Perrier. This is a "find" of the *Travailleur* cruise, and is named *caulaster pedunculus*.—On the suctocitiates of M. de Merejkovsky, by M. Maupas. The type described, he says, has been long known.—Mineralogical analysis of the rock impasted in the meteorite of Atacama, by M. Meunier.

BERLIN

Physical Society, December 15, 1882.—Prof. Helmholtz in the chair.—Prof. Christiani demonstrated some acoustic experiments which he had incidentally made. In renovation of the Kœnig tuning-forks injured by the fire in the Physiological Institute, and which had to be freed from their coating of rust, and mounted on new resonance cases, one fork of the series, the fork *mi*$_3$, showed after tuning and sounding, when one side of it was turned towards the closed end of the case, a maximum of tone; it did not matter in which direction (right or left) the fork was turned round into the position referred to. Another fork *mi*$_3$ of the physical Institute in unison with the first, did not present the phenomenon, and when the forks and cases were exchanged, it appeared that the effect was connected with the new case. It was not explained. A second experiment, made by Prof. Christiani, was named by him "total absorption of tone." A singing flame was tuned approximately to the tone *mi*$_3$, and the resonance case bearing the tuning-fork *mi*$_3$ was held with its open end horizontal near the upper end of the chemical harmonica. The tone was unaffected. When, however, the same case, without tuning-fork, was brought to the same position relatively to the sounding chemical harmonica, the sound immediately ceased, and the flame burned quietly in the tube. Each time the tone of the flame ceased, when the mouth of a resonator adapted to the pitch was brought to the upper end of the tube, whereas the flame sounded again when the resonator was tuned to a different tone, or was loaded with a tuning-fork. Prof. Christiani means to investigate the phenomenon further.

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