## THURSDAY, JULY 19, 1877

### THE "INFLEXIBLE"

IN our last number we sketched in outline the scientific principles and considerations which lie at the foundation of the important question now at issue regarding the *Inflexible*.

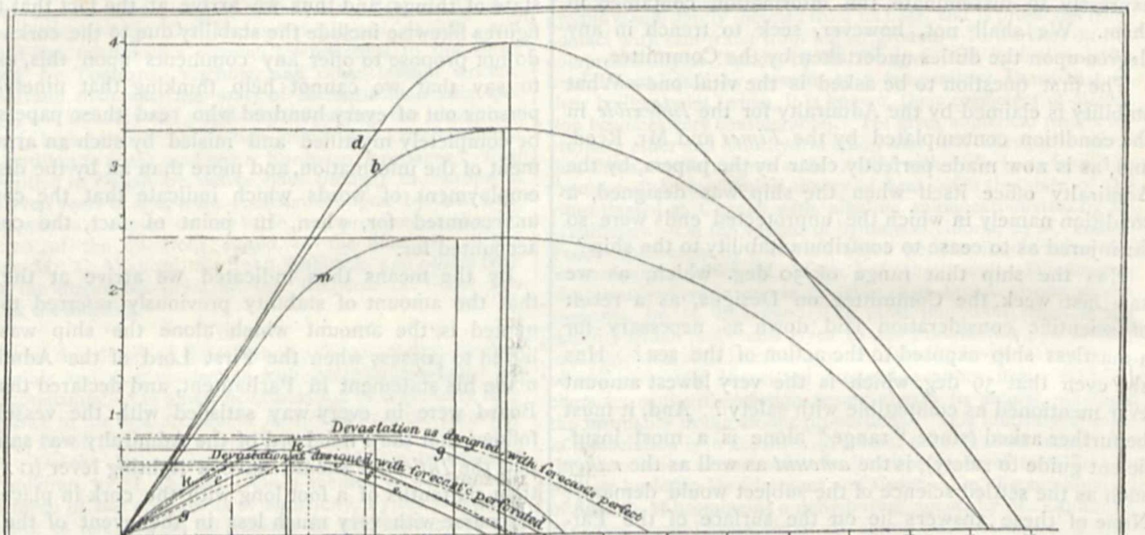
We now proceed to consider the case as set forth in the papers which have since been presented. The fact of a Committee having been appointed to investigate it is no reason for our passing over in silence these papers, which have been already laid upon the table of Parliament expressly to disseminate the information contained in them. We shall not, however, seek to trench in any degree upon the duties undertaken by the Committee.

The first question to be asked is the vital one—What stability is claimed by the Admiralty for the *Inflexible* in the condition contemplated by the *Times* and Mr. Reed, and, as is now made perfectly clear by the papers, by the Admiralty office itself when the ship was designed, a condition namely in which the unprotected ends were so far injured as to cease to contribute stability to the ship?

Has the ship that range of 50 deg. which, as we saw last week, the Committee on Designs, as a result of scientific consideration laid down as necessary for a mastless ship exposed to the action of the sea? Has she even that 39 deg. which is the very lowest amount ever mentioned as compatible with safety? And, it must be further asked (since "range" alone is a most insufficient guide to safety), is the *amount* as well as the *range* such as the settled science of the subject would demand? None of these answers lie on the surface of the Parliamentary papers, and some of them are not to be found there at all; but by what Dr. Schliemann would call "the pickaxe method" of investigation, we can get at part of them, and infer the remainder with all necessary certainty. The most difficult and unfortunate part of the inquiry is due to the fact that the *apparent* answers that do lie on the surface are not the true ones, although they bear every outward semblance of being so. The scientific world is turning to these papers for one piece of information before and beyond all others, namely, this very stability of the *Inflexible* with "unarmoured ends giving no stability;" and when they get to p. 10 there they find it plainly set forth, in tabular form, and in full detail: "Maximum stability 6,532 foot-tons; angle of maximum stability 13½ deg.; range 30 deg.;" and reading this they begin to say to themselves that although the ship is, on the showing of her constructor, below the standard of safety which everybody has either laid down or accepted, yet that she is not so alarmingly deficient as they had supposed from the pronounced tone of her critics. Give her, say they, the benefit of the cork (in which the whole Admiralty has so suddenly acquired extraordinary faith), and although she would then be somewhere near the recognised limits of safety, and would be liable to the cork being more or less rapidly got rid of under shell fire, still the case is not so very bad after all. But unfortunately on reading further they come on the following page upon an ugly foot-note by Mr. Barnaby, bearing date June 25—one week after the occurrence of the *Inflexible* debate in Parliament—and in this foot-note is disclosed the fact that the figures which we have quoted, and which are given at p. 10 as expressive of the case when the unarmoured ends give no stability, turn out to be the figures applicable to the case when the cork is in place, and when the unarmoured ends are contributing the stability which arises from the cork! This is not clearly shown on the surface of the foot-note, but it has to be discovered in a roundabout manner. It can, however, be reached in this way. The foot-note implies that the cork was allowed for in the calculations of a certain curve given on the sheet of curves appended (Curve *e* in our present diagram), and by comparing this curve with the figures given on p. 10, we find that they express the same state of things, and thus we arrive at the fact that these figures likewise include the stability due to the cork. We do not propose to offer any comments upon this, except to say that we cannot help thinking that ninety-nine persons out of every hundred who read these papers will be completely mystified and misled by such an arrangement of the information, and more than all by the distinct employment of words which indicate that the cork is unaccounted for, when, in point of fact, the cork is accounted for.

By the means thus indicated we arrive at the fact that the amount of stability previously referred to and quoted is the amount which alone the ship was believed to possess when the First Lord of the Admiralty made his statement in Parliament, and declared that the Board were in every way satisfied with the vessel. It follows that the First Lord of the Admiralty was satisfied that the *Inflexible* should have a righting lever (G Z) less than six-tenths of a foot long with the cork in place, and of course with very much less in the event of the cork being blown out. We are not able to state—for the Admiralty does not give the information—how much stability the constructors and the Admiralty actually believed the ship to possess in the case under notice (viz., the case of the unarmoured ends really contributing no stability), but it is impossible to peruse these papers and to come to any other conclusion than that unless this information was advisedly kept back it ought to have been given. We cannot ourselves, of course, profess to say how much of the measure of stability which we have quoted was due to the cork, but we are able to approximate pretty closely to it by aid of the diagram which Mr. Reed prepared, and which is printed with the papers. That diagram Mr. Barnaby has marked "wrong," and no doubt it does differ a little from Mr. Barnaby's own diagram in its estimate of the amount of stability with the cork in. Indeed Mr. Reed only put his curve of stability forward as approximate, and as being in his opinion "pretty near the truth;" but this diagram may be taken as a sufficient approximate indication of the amount of stability actually due to the cork itself, and by taking that away from the Admiralty diagram we get what the Admiralty themselves would probably allow to be the state of the ship with the cork out. We have performed this operation, and we find that the *Inflexible* is, with the cork out, left almost absolutely without any stability whatever, even when she is judged by the diagram which the Admiralty themselves supply, and upon which they rested when the first Parliamentary debate took place.

Subsequently, that is on June 25, Mr. Barnaby put forward a memorandum (the foot-note before referred to) showing that his own previous diagram might be extended a few degrees by introducing such considerations as that the four guns had been increased by 20 tons each (in a ship of over 11,000 tons !), that the armoured deck was to be broken through to let the cables be stowed lower, and that an allowance might be made for immersed materials ; and by these means some little show of a curve of stability, even with the cork out, is obtained ; but the amount of it must be exceedingly small, as the range claimed is only 17 deg., and the curves which we give in the accompanying engraving indicate by analogy how very small the *amount* of stability must in this case be with so extremely limited a range. We feel bound, however, to demur to any such change in the Admiralty calculations, both because of the doubt which must inevitably rest on the alteration of calculations made to meet a public inquiry already commenced, and because the actual changes made in this case are in themselves improper. It is improper to make new allowances now ostensibly for the enlarged guns, because still larger guns were originally contemplated in the design, as the printed Papers clearly show ; it is improper to alter the cables to meet the present state of things, as they were obviously wisely placed in the first instance ; and it is improper for the sake of a small nominal increase in the apparent amount of stability of this ship to introduce novelties of calcula-




Range of Stability

DESCRIPTION OF CURVES.

| | | Angle of Max. Stab. | Max. G. Z. | Range. | Metacentre above C.G. |
|---|---|---|---|---|---|
| | | ° | Feet. | ° | Feet. |
| *b* | Ship complete, Cork in place ... ... ... ... | 31·2 | 3·28 | 74·3 | 8·25 |
| *d* | As in *b* but in light condition ... ... ... | 31·7 | 3·935 | 71·5 | 8·53 |
| *e* | Fully equipped ; ends riddled | 13·5 | ·568 | 30·0 | 2·0 |
| *f* | As in *e* but coal between decks (800 tons) removed ... | 15·4 | ·534 | 32·2 | 3·09 |
| *g* | As in *e* but in light condition ... ... ... | 20·8 | ·794 | 36·8 | 2·22 |
| *k* | As in *e* but supposing the water in ship when upright locked ... ... ... ... ... | 13·9 | ·705 | 32·0 | |
| *m* | As in *k* but supposing main deck kept free of water ... | 27·4 | 2·42 | 71·5 | |

tion which will defeat all comparison now and hereafter between the *Inflexible* and other ships, and which will absorb into the substance of the calculations for this one ship those small outlying margins which together make up the dividing ground between safety and risk. For these good and sufficient reasons we stand upon those calculations on which Mr. Ward Hunt rested when he addressed Parliament on the subject, and we are obliged to state that, according to those official calculations, the *Inflexible* is practically without stability when the unarmoured ends have ceased to furnish any.

The next question which arises is, how is the ship circumstanced with the cork in ? and the answer to that we have incidentally had before us already. Even by introducing such considerations as Mr. Barnaby adduces in the foot-note he only claims for his curve of stability with the cork in a range of 35 deg., and with the range so increased the righting lever G Z cannot much exceed six-tenths of a foot in amount. It will be easy for the reader of these remarks to imagine, without our assistance, the slight alteration of curve *e*, to which we are here pointing, and when they bring the eye down from the large levers of stability indicated by the curves *d*, *b*, and *m*, to the curve *e*, even when thus enlarged, they will see what a striking difference there is between the ship with her unarmoured ends intact and the ship with those ends broken into by the sea. The fact is that even in this state with the curve of stability extended by the devices to which we have already objected, the ship is in an unsafe condition when judged by the only standards that at present exist for our guidance, viz., those laid down in the Report of the Committee on Designs of 1871.

We have now almost exhausted all that the Parliamentary Papers contain on the essential question at issue, but before concluding this part of our subject we must advert to the case of the *Devastation*. The *Devastation*, as designed, had an unarmoured forecastle, which extended down to within about a foot of the water's surface, and with this forecastle perfect, she had a range of stability of 43½ deg. Mr. Barnaby alleges that with the forecastle perforated, her range of stability would be brought down to 35 deg., and that the maximum angle of stability would be 9 deg. It seems to us that the curves given show something more and something different to that which the table in the text gives. They show something more, because it is clear on inspecting that one which relates to the *Devastation* with forecastle perforated, that although the maximum appears to be reached at 9 deg., there is no perceptible diminution in the magnitude of the stability for nearly another 9 deg. The curve seems to be parallel to the base line, from 9 deg. up to 16 or 17 deg., and the state of the *Devastation*, therefore, is totally different from that which might be expected from the announcement in the text, that 9 deg. is the angle of maximum stability. But even allowing all the stability which the *Devastation* is shown to possess by these curves, we are ourselves disposed to believe that when this ship was altered, the alteration should have been so made as to add to the stability of the ship when the unarmoured bow is perforated. Now the Reports of the Committee on Designs show that the changes made after Mr. Reed left the service were of a kind which, while adding materially to the stability of the ship under ordinary circumstances, burdened her with a large amount of top weight, which, together with the lowering of the armoured freeboard, must have diminished the stability with the bow perforated. In view of this consideration, we should much like to have seen the *Devastation's* curve of stability as it would now be with the unarmoured bow perforated, for we are disposed to think that the production of that curve would disclose a much less satisfactory condition of things as regards stability in the *Devastation* as she now is, than existed at the time of her design; and, as we have already intimated, this ship appears to us not to have possessed even at first all the stability desirable. We are aware that the Committee on Designs expressed a contrary opinion, but we doubt if they duly considered the state of the vessel with the forecastle perforated, and we believe that if they had they would probably have looked less complacently upon the change that was made than they actually did. If we are right in this view of the *Devastation*, Mr. Reed must no doubt bear the blame of having cut this ship's stability somewhat finer than it should have been, and all that could be admitted in mitigation of the fact is that which he would probably plead, viz., that this ship was the first great monitor of the kind that had ever been designed; that calculations of stability at considerable angles of inclination had not then come into vogue; and that he left the service long before the *Devastation* was completed for sea; and therefore that it was not from his hands that she passed into actual service.

But it is no part of our business to distribute blame in such matters, and all that we can say on the point is that we are not satisfied even with the *Devastation*, and that whatever deficiency of stability she might have possessed as originally designed, that deficiency must have been made worse by the modifications which the ship has since undergone.

We must now say a few words upon the question of the cork, and how far it may really be relied upon for giving to the *Inflexible*—not a safe measure of stability when the unarmoured ends have ceased to furnish any otherwise, for we have seen that with all the cork in place the stability is still too little for safety—but some sort of chance of escaping that capsizing which must befall her at sea with the cork gone. The great advocate of cork is the present Director of Naval Ordnance, Admiral Boys; but if the arguments which he sets forth in these printed papers are in truth the foundations on which the hope of the Admiralty is based, their confidence must be very ill-grounded indeed. Admiral Boys is good enough to set forth in a formal manner the reasons which induce him to think the cork can be depended upon. There is one at least to which science at once supplies an answer.

He mentions "the difficulty of striking a ship at or below the water-line, particularly one of the *Inflexible* type, that will scarcely ever roll." Scarcely ever roll! A more astonishing, and may we say a more utterly groundless remark, was probably never made upon a serious subject. It has been established beyond all question or cavil that one of the most fruitful causes of rolling is great stability, and that mastless ships of great stability will roll more than any other. Now, in her uninjured state, which is the state in which she will enter upon an action, the *Inflexible* will have very large stability, and may be expected to roll heavily in a seaway. A glance at our engraving this week will show that the length of the righting lever, G Z, is sometimes over three feet, and in one case (with certain stores consumed) nearly four feet, whereas not one of all the typical curves given in our last number gives a G Z that much exceeds three feet, while all but one are much below that. It is true that the admission of water at the ends may have a perceptible effect in diminishing the rolling, but it would be premature to presume any great results in this respect. And apart altogether from the rolling of the ship, every seaman, including Admiral Boys, must be well aware that the ends of the steadiest ships in the world get much exposed by the mere falling away of the water in a seaway. With regard to Admiral Boys other assumptions they are more out of our province, but we may remark that the enunciation of such views by a high officer of the Admiralty is calculated to fill common minds with astonishment and apprehension. Naval architecture, instead of being a complex and difficult branch of science, would be of all arts the most simple and easy, if all that had to be done were to keep safe the under-water parts of ships that never roll; to encounter projectiles that cannot fulfil the object for which they are employed; and to engage vessels armed in the most suitable manner, and managed in the most suitable way, to let us escape!

The conclusions we have arrived at are, that the *Inflexible* is not a safe ship for battle in her present state;

that the objections brought against her have been much too lightly treated; and that the disclosure of her condition, with the circumstances that have followed it, have excited just surprise and dissatisfaction. The subject must be anxiously watched through its future stages.

## THE NEW METEOROLOGICAL COUNCIL

THE final stage of the labours of the Treasury Committee, to which we have made frequent reference, has now been reached. The Royal Society has been appealed to to nominate the new council; they have done so, and the Government has accepted the nominations, which are as follows:—Prof. H. J. S. Smith, Savilian Professor of Geometry in the University of Oxford and Keeper of the University Museum (Chairman); Prof. Stokes, Lucasian Professor of Mathematics in the University of Cambridge, and Secretary of the Royal Society; Dr. Warren de la Rue, Mr. F. Galton, and Gen. Strachey, Member of the Indian Council. In addition to these there is Capt. Evans, the Hydrographer of the Navy, as an *ex-officio* member.

The new Meteorological Council, then, like the old Meteorological Committee, is composed of Members of the Royal Society, who severally hold distinguished positions in special departments of science, and who collectively represent considerable administrative ability. The addition to the new Council of two distinguished mathematicians and physicists, such as Professors Smith and Stokes, will be generally regarded with satisfaction, particularly when it is considered that it is to the mathematician and physicist that meteorologists must always look for information and guidance on many matters affecting the intricate and difficult problems with which they, in the position the science has attained, must now deal.

It is, however, matter of general surprise among meteorologists, or we should rather say of wide-spread regret, that the New Council will resemble the old Committee in having no meteorologist upon it. The omission, so far as concerned the Meteorological Committee, was a serious one, and led to mistakes; so far as concerns the new one it will be well if it does not seriously mar its usefulness and retard the foundation of the future science of physical meteorology. At the same time it is only just to point out that because the science is of the future, the choice of the Royal Society was small, and that considerations not on the surface may have had to be borne in mind. However this may be, there is no doubt that the Royal Society and the new Council have accepted a great responsibility, and that the action of the latter will be most keenly watched. The Royal Society, in a report to the Government, has stated:—

"The Council of the Royal Society is of opinion that the most practical method of advancing meteorology is to endeavour by research and experiment to place that science on a firm basis. They are also of opinion that this can be done only by the devotion of the time of scientific men to the necessary research and experiment."

Men of science, therefore, will be justified in looking both for research and experiment from the new council in addition to the dreary piles of observations which have cumbered all scientific libraries for the last half-century.

And here is the rub. Will the busy—not to say already over-worked—members of the Council adopt this "practical method," and conduct researches? or do they propose to content themselves by going into the market with the 1,000*l*. which is given for *research*, and, be it remarked, not for mere *observations*? In the latter case it is to be hoped that their advances will be met in no narrow spirit; for if the new council only fosters research and experiment, it will be a great gain.

While, on the one hand then, we have a right to expect results of a high order from the new Council, on the other we are glad to see they are to be no longer an unpaid body. Besides the 1,000*l*. devoted to research there is another 1,000*l*. devoted to the payment of the members. This sum is to be spent partly in retaining fees and partly in payment for attendance.

The vote asked for the present year and agreed to on Tuesday is 10,000*l*., and the Secretary of the Treasury then stated that the Committee had recommended an expenditure of 4,000*l*. a year by the Meteorological Council, and, in the judgment of the Treasury, the recommendation was one that ought to be adopted. A supplementary vote will be asked for this at an early date.

As regards the meteorological societies, on whom must devolve the practical working out of the large problem of the comparative climatology of the various districts of the United Kingdom—the working out of this problem being beyond the scope of the operations of the New Council just as certainly as it is beyond the resources originally placed at its disposal—we cannot but suppose that the Government have, in handing over the administration of the meteorological grant to the New Council, made provision that a portion of the additional 4,000*l*. will be spent in adequately aiding these societies in doing important national work which they are in a position to do so economically, and which, judging from the past, they can do so effectively.

This now seems to be the Treasury view, for in the warm debate very properly raised by the Scotch members in favour of the claims of the Scottish Meteorological Society, Mr. W. H. Smith stated that, as to the tests that ought to be applied in such cases, special regard ought to be paid to two points. The object to be attained ought to be distinctly national, and not one in which particular individuals or classes were concerned, and security ought to be taken that the persons who sought assistance were contributing largely to promote the object in view. There is no doubt that the Scottish Society satisfies both these requirements.

The Chancellor of the Exchequer also stated that it would be for the Council to consider how far they could avail themselves of the services of the Scottish Meteorological Society in the conduct of their business, and on what terms that assistance should be rendered.

The Council have lost no time in entering upon their duties, and it is devoutly to be wished that some sign may soon be given that if its constitution is not what was generally looked for, it is still well qualified to discharge its functions and to merit the confidence of meteorologists, although they have had so little to say to its appointment.

## THE "*POLARIS*" EXPEDITION

*Narrative of the North Polar Expedition U.S. Ship "Polaris," Captain Charles Francis Hall, Commanding.* Edited under the Direction of the Hon. G. M. Robeson, Secretary of the Navy, by Rear-Admiral C. H. Davis, U.S.N. U.S. Naval Observatory, 1876. (Washington: Government Printing-office, 1876.)

THIS is a handsome record of one of the most memorable, and in some respects most successful of Arctic expeditions. Though dated 1876, a note dated March, 1877, is prefixed, stating that the concluding chapters have been prepared by Prof. J. E. Nourse. We have already (vol. viii., 217, 435 and *passim*) given so full details concerning this expedition that we need do little more now than notice the publication of this record by the U.S. Government. It contains a full general narrative of the expedition drawn up not only from the official records of the responsible officers, but from the diaries kept by many of the subordinate officers and men, many of the latter being unusually intelligent. Indeed it formed part of the instructions to the expedition in its outset that as many of the officers and men as were able should keep diaries, which were to be handed over to the U.S. Government on the return of the expediti n, a praiseworthy feature, we think, which might be advantageously copied by all similar expeditions.

Captain Hall himself is spoken of justly in the narrative in the highest terms of praise. His enthusiasm for Arctic exploration had become almost a religion with him, and had he lived there seems little doubt that much more would have been accomplished than even there was. He had qualified himself by two long residences among the Esquimaux for enduring all the hardships incident to Polar exploration; and while his main aim was geographical discovery he had a sufficient knowledge of and love for science to induce him to do all in his power to look after its interests in connection with his expedition. To quote the work before us, Hall "possessed judgment and sagacity altogether too large and comprehensive not to be fully alive to the importance of its promotion; and not to know that every accession, whether of law or fact, to its domain, tended to the benefit of mankind." We believe that the narrative of Hall's second residence among the Esquimaux will shortly be published for the first time.

The present work will correct some misconceptions that became current at the time that the news of the fate of the *Polaris* expedition reached this country. It was stated, for example, that the ship was not well fitted for her work; but the fact is that everything was done to strengthen her and otherwise adapt her for the special work she had to do that the United States naval authorities could suggest. Hall himself said that no better equipped expedition ever set out for the Pole. The expedition was in every sense a government one, 50,000 dollars having been appropriated for it, and it was governed by the naval discipline of the United States. True, this latter point does not seem to have been rigidly carried out, Hall himself not having been a naval officer, and perhaps a little too soft to be so strict as he ought to have been. Explicit, but sufficiently elastic instructions were given him, and the scientific instructions prepared by the United States National Academy are given in the Appendix. The latter are exceedingly detailed and carefully drawn up, and embrace every department of science. The scientific results of this expedition are, we believe, of very great importance, and we are glad to learn that a portion of them, at least, have just been published by the chief of the scientific staff, Dr. Emil Bessels..

The idea of the expedition was due solely to Capt. Hall, and it was only through his enthusiastic agitation that the United States Government were persuaded to equip it. His death was a great loss to the cause of Arctic exploration, and we may say to science; and it is a relief to find that after rigid inquiry on the part of the Government it has been concluded that his death arose from purely natural causes. Our readers may remember that a handsome tablet was placed at the head of his grave by our own recent expedition.

The present volume, we have said, is a handsome one, and compares favourably with the unattractive blue-books issued by our own Government in similar cases. There are a large number of attractive illustrations and maps, the former, however, executed by a very roundabout process; they are wood-engravings painted in oil from original sketches by Mr. Emil Schumann and Dr. Bessels, photographed on wood and engraved. One may be inclined to fear that their truthfulness will be apt to suffer during this long process. A very fine life-like portrait of Capt. Hall forms a frontispiece.

## POLLUTION OF RIVERS

*A Treatise on the Law Relating to the Pollution and Obstruction of Watercourses, together with a Brief Summary of the Various Sources of Rivers Pollution.* By Clement Higgins, M.A., F.C.S., Barrister-at-Law. (London: Stevens and Haynes, Law Publishers, Bell Yard, Temple Bar, 1877.)

THE pollution and obstruction of rivers by sewage and the refuse of manufacturing processes is, in a country like this, densely populated and depending mainly on its manufactures, a matter of the gravest importance, justifying indeed, a saying of Earl Beaconsfield's, which has met with a good deal of ridicule, that the motto of his government should be *sanitas sanitatum omnia sanitas*. We have on the one hand to preserve as rigidly as possible the purity of our streams, and on the other to interfere as little as can be with those manufactures which are so great an element in the production of our national welfare.

It is now some years since the Government issued a Royal Commission to inquire into the best means of carrying out the problem, and that the matter is now in a fair way to solution is mainly due to the labours of the late Commission, of which Dr. Frankland was the chemical member.

The five Reports presented by it to Parliament, take rank, indeed, as a classical research into the subject, and have an interest to countries other than our own. The Rivers Pollution Act of 1876 is based on the recommendations of the Commission, and it is not too much to say that without them legislation would have been impossible.

The Act in question constitutes four classes of offences as follows:—To pass into any stream (1) any solid matter so as to interfere with its due flow, or to pollute its waters;

(2) any sewage matter; (3 any poisonous, noxious, or polluting liquid from any manufacturing process; and (4) any poisonous, noxious, or polluting solid or liquid matter from any mine.

Mr. Higgins justly remarks that "the successful working of the Act will much depend upon the meaning of the word '*polluting*' as therein used, by those with whom its interpretation rests." In order to understand the drift of this remark it is necessary to observe that the Act of 1876 virtually gives no standard of purity, though the Commission of 1868 recommended an extensive and somewhat stringent list of standards. We think that on the whole the Act is right in the omission, as a suggestion made by Mr. Crookes in his evidence before the House of Lords in 1873, namely, "that the river itself should be the standard of purity, and that no liquid should be allowed to be sent into a river if the liquid contains a greater percentage of impurity than the river itself," seems to be a very feasible standard and one easily and quickly referred to. Again, as Mr. Crookes pointed out, the standard would naturally improve, as nothing worse than the river at any given point would enter it, whence in the course of nature amelioration would ensue, while the process being gradual would give the manufacturer or township time to improve their waste or sewage, and one of the most disastrous sources of trouble the injury to the water-course from the casting into it of solid refuse would be at once prohibited; as would pollution by actually poisonous matter, such as arsenical and other liquids.

It appears to us that guided by *competent* chemical evidence there ought to be no difficulty in obtaining legal decisions as to the polluting or harmless character of any liquid that may be called in question, while as to solid matters, of any kind whatever, the mere fact of their entry into a stream ought to be an offence without reference to their character. On the whole we think the act, though perhaps partaking too much of the "permissive" character, which is so prominent a feature of modern legislation, to be one which, if conscientiously used with due consideration to the facts of each individual case ought to work great good. In the race for wealth we are perhaps too little apt to think of the future. The brooks and running streams like the land we live on are not ours to do as we like with, but like an entailed estate are only held in trust for the next heir, and like national or family honour should be handed down to posterity pure and unsullied.

Mr. Higgins has devoted great care to his treatise on the Act, and his chemical training has evidently stood him in good stead, the numerous references to cases bearing on the various points show a laborious study of the legal aspects of the case and will add greatly to the value of the work in the eyes of the legal profession, for whose information it is primarily intended.

R. J. FRISWELL

## OUR BOOK SHELF

*The Cradle of the Blue Nile; a Visit to the Court of King John of Ethiopia.* By E. A. De Cosson, F.R.G.S. Two vols. With Map and Illustrations. (London: John Murray, 1877.)

ALTHOUGH Mr. De Cosson did not go over any new ground in his tour, and although he was unable even to carry out his original plan, we are sure that most readers will find much that is new and certainly interesting in his volumes. He went slowly southward from Massowah to Lake Tzana, north-west to the lower Bahr-el-Azrek, down the Nile to Berber, and across to Saakim. He won the favour of King John, of whom he speaks as an able, well-meaning ruler, and was thus able to see much of the life of the people, and learn much of the antiquities and the character of the country that otherwise he would have missed. To any one wishing to obtain an attractive account of the past history and present condition of Abyssinia, we strongly commend Mr. De Cosson's work.

*The Tiber and its Tributaries, their Natural History and Classical Associations.* By Strother A. Smith, M.A. Map and Illustrations. (London: Longmans and Co., 1877.)

THE idea of this work is, we think, a happy one, and its execution successful. The object is to gather under one head everything of interest relating to the Tiber. This has necessarily involved a great amount of research, and the result will be welcomed both by the student of history, the "scholar," and the geographer. Considerable space is devoted to the inundations of the Tiber, and also to its birds and its fishes. Two nicely-coloured plates are devoted to the muræna, the mullet, the lamprey, and the sturgeon. The Tay, at Perth, we should inform Mr. Smith, is no more an "estuary" than the Thames at London Bridge, unless the word is applied to all that part of a river reached by the tide.

*A Short Account of the Principal Geometrical Methods of Approximating to the Value of π.* For the Use of Colleges and Schools. By the Rev. G. Pirie, M.A. (Macmillan, 1877.)

*Elements of Geometry Based on Euclid.* Book I. For Elementary and Middle Class Schools. By E. Atkins, B.Sc. Collins's School Series. (Glasgow: Collins, 1877.)

*Takimetry. Concrete Geometry in Three Lessons. Accessible, Inaccessible, Incalculable.* Translated by D. W. Gwynne, M.D., from the French of E. Lagout. (Glasgow: Collins, 1877.)

THE little pamphlet first named does not attack the problem from the circle-squarer's point of view—the use of the word "approximating" sufficiently points this out—but gives an interesting account of what was done for the question between the times of Archimedes and Huyghens. A few elementary propositions lead up to what was attempted by Willebrord Snell ("Cyclometricus," 1621) and elegantly effected by Huyghens. Mr. Pirie's object is to correct what he deems a defect in our present works on Trigonometry, and to supply a few simple propositions "on the threshold of the subject." We can recommend the book as one suitable for being put into the hands of sixth form pupils. A few references are supplied to fuller sources of information upon the quadrature of the circle.

Mr. Atkins's book seems to differ but little from the ordinary forms of Euclid as now printed. One feature is the addition of short side-notes drawing attention to the objects of the successive steps of the construction and proof. There is a short collection of sixty exercises grouped under the propositions upon which they depend. Some of these appear to us wrongly placed, and a few incorrectly printed. The work is neatly got up and of a handy form.

If all that is said of takimetry by its admirers be true a revolution in mathematical instruction may be speedily expected. "With one hundred lessons of takimetric instruction any one can very well learn geometry, algebra, arithmetic, and mechanics." "The classical geometry of Euclid *disguises its object, its utility*, and thus, for a considerable time, yields a barren and discouraging result, whilst takimetry is able, on the other hand, to produce the miracle of an astonishing progress." In the Fundamental Takimetry (introductory to Takitechny) objects are classified into square, round, pointed, and truncated forms. The three lessons of Takimetry are (1) equivalence; (2) resemblance; (3) the three squares of a right-angled triangle (*i.e.*, "Euc.," i. 47). The subject requires only three lectures, each of an hour's duration. Amongst the subjects for measurement are the accessible, the inaccessible, and the incalculable (*i.e.*, those which depend upon the circle). There is much that is good in this book, though in its present form it is overweighted with a mass of extraneous matter. By aid of the prettily-coloured figures (there are models, also, we are told, to accompany the book) a considerable knowledge of mensuration, we think, might be imparted even to dull boys. We could take exception to the translation in many places.

## 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.*]

### Museum Reform

"J. P." suggests, under the head of "Museum Reform" (vol. xvi. p. 183), the idea of a conference of museum keepers, out of which a permanent union among museum officers might result. I am of opinion that this idea is an excellent one, and that the administration of the museums of all countries would gain a great deal if an opportunity to museum officers were offered to interchange their opinions and to communicate to each other their different practical experiences. Perhaps some arrangements and rules might *generally* be accepted, as, *e.g.*, to labelling specimens, exchanging duplicates, publishing annual reports in a journal *ad hoc*, &c. A. B. MEYER

Dresden, July 9

### Koenig's Tuning-Forks

THE letter of Herr Koenig inserted in NATURE (vol. xvi. p. 162) did not come under my notice till July 8. On October 27 last year I counted all the 64 sets of beats in Herr Appunn's tonometer, one through 15, the rest through 20 seconds with a pocket chronometer which was gaining less than 4 seconds a day, and found every set of beats perfectly true. The perfection of the consonances, more than 80 of which I tested mechanically, by observing the beats that arose on flattening one of the two consonant notes, seemed to me to eliminate all possible error of irregular counting. The suggestion now is that the beats were perfectly regular and uniform, and that no exception need be taken to my counting, but that Herr Appunn's pendulum was originally incorrect, to such an extent that what appeared to him 80 beats in 20 seconds, were only 79·27, and that my chronometer was not sufficient to detect the error. If this were the case all the numbers on Herr Appunn's tonometer would have to be reduced by as nearly as possible 9 in 1,000, which would make them agree with Herr Koenig's. I shall therefore have to re-test the tonometer with a larger chronometer and if possible count each set of beats for a longer time. I shall not be able to undertake this examination at present, but I shall not neglect doing so, and will inform you of the result. It is right, however, to say that on July 9 and 10 I received communications from Prof. McLeod showing that his improved instrument for counting vibrations gave results almost exactly agreeing with Herr Koenig's numbers. The marked difference of Herr Appunn's and Herr Koenig's numbers will I hope lead to such an examination of the subject as will result in an accurate determination of pitch that can be generally accepted. ALEXANDER J. ELLIS

Kensington, July 11

P.S.—I have this morning received a letter from Herr Appunn, in which he tells me that the letter of Prof. Helmholtz, quoted by Herr Koenig, was received eleven or twelve years ago, and that the error of Herr Appunn's pendulum there pointed out was corrected more than ten years ago. He also refers me to pp. 46-7 of Prof. W. Preyer's tract "Ueber die Grenzen der Tonwahrnehmung," Jena, 1876, in which, by a calculation there detailed, Prof. Preyer shows that the absolute pitch of two of Herr Koenig's forks, which should have been 128 and 256, were 129·1 and 258·2; and says that "the determination is as exact as possible, so that the first decimal place can be fully trusted." I made another fork to be 258·4, and I know by comparison of several specimens that Koenig's forks do not always agree within more than ·2 vibrations. A. J. E.

July 16

### On a Fish-sheltering Medusa

WHILE collecting some three weeks since on the south shore of Killary bay in Connemara, I observed that out of a number of the common *Aurelia aurita* moving about in a rocky inlet below me, one was invariably accompanied by a small fish, of about an inch or an inch and a quarter in length, which had established itself inside of the hemispherical disc.

Occasionally the Medusa turned in its pulsations, so as to bring the umbrella undermost, when the fish would shoot hastily out, but the Medusa had no sooner righted itself, than the fish returned, and seizing its opportunity, swam in between the marginal tentacles, and close up to the fringes of the actinostome, remaining distinctly visible through the pellucid disc.

I afterwards noticed several other Aurelia similarly attended, but was not able, unfortunately, to identify the fish, which invariably darted off at the most distant approach of a landing-net—it appeared, however, so far as I could judge, to be the young of one of the larger species. Perhaps some of your readers could contribute suggestions on that point.

Associations of a similar character have, I know, been frequently observed in the case of the Physalidæ and other Acalephæ, but not, so far as I am aware, in connection with this species. E. LAWLESS

### The Earth and Moon

I HAVE only now (July 12) noticed Prof. Tait's remark respecting a sentence, or rather half a sentence, which he quotes from an article of mine in the *Cornhill Magazine* for June. It runs thus: "What mathematicians call the moving force exerted by the earth on the moon is eighty-one times greater than the corresponding force exerted by the moon on the earth." This admits of an interpretation implying gross ignorance on my part—ignorance, viz., of the fact that the moon pulls the earth just as strongly as the earth pulls the moon. It also admits of an interpretation accordant with fact, for the moving force exerted by the earth on each unit of mass in the moon is eighty-one times greater than the corresponding force exerted by the moon on each unit of mass in the earth. I do not think anyone is likely to believe that I made the mistake imputed to me by Prof. Tait, in this instance, any more than that I made an equally absurd blunder which he attributed to me in your columns several months ago, or that he himself made the ludicrous blunder attributed to him (in jest) by my humorous friend, Prof. Nipher, of St. Louis. But as a mere matter of fact, I may point out that the half-sentence quoted by him is completed by a half sentence leaving no doubt as to my real meaning, and is immediately preceded by the statement that "the moon pulls the earth just as strongly as the earth pulls the moon."

London, July 12 RICHD. A. PROCTOR

### Blue and Yellow Crocuses

REFERRING to Mr. W. B. Tegetmeier's letter in NATURE, vol. xvi. p. 163, I can say that I once had a pony born and bred on Dartmoor, which had never seen oats until it came into my father's stable in the fourth year of its age, and it refused them. We induced it to eat oats by mixing them with hay and gradually reducing the quantity of hay until the oats predominated.

Penzance, July 10 THOS. CORNISH

### Japanese Mirrors

MORE than eleven years ago, in February, 1866, I published in *The Reader* (since extinct), a letter giving, I venture to think, a complete explanation of the phenomena exhibited by certain Japanese mirrors (through a mistake as to their nationality I called them Chinese); and as your readers appear to be unacquainted with this, perhaps I may be allowed to reproduce the substance of my former letter. In order to ascertain whether any variations in the form of the surface of the mirror, which was very slightly convex, affected the question, I looked for any distortions that might be produced in the image of the ground-glass globe of a gas lamp, as the point of incidence moved across that portion of the polished surface on the back of which was a raised figure. Let A and B be two lines on the surface immediately over the two edges of such a figure. Then as the limb of the image approached A, it became flattened; when it had passed A it expanded to more than its original size; indeed between A and B the image was sensibly larger than when viewed from any other part of the mirror. When the limb approached B it was again flattened, and beyond B it resumed its original dimensions. This clearly proves that the portion of the surface of the mirror between A and B was, if not actually plane, at least less convex than the rest of the surface; and as upon this supposition the figures when thrown upon a screen should appear *brighter* than the rest of the image, which is exactly what occurs in fact, there can be no doubt that this is sufficient to account for the peculiarity in question. In all probability the mirror had warped in cooling, except in the thicker portions where the raised figures existed. J. PARNELL

Hadham House, Upper Clapton, July 6

### Printing and Calico Printing

YOUR correspondent, Mr. Henry Cecil, is under a singular misapprehension as to the inventor of cylinder machine calico printing, and the date of its first practical application. Mr. Isaac Taylor was certainly not the originator of cylinder printing; and that art was developed long before he, "in 1855 or 1856 superintended its application at Manchester." Mr. Taylor, it is true, obtained several patents for inventions connected with cylinder-printing—one, I think, for a form of pentograph, and another for the use of thin sheet copper instead of thick cast cylinders of that metal. These, so far as I know, never succeeded in practice, and it is highly probable they brought their gifted inventor loss instead of gain; but that result was not due to "the inevitable compliment of piracy." Who the inventor of cylinder printing was it would be hard definitely to determine. Nearly a century and a half ago a patent was granted for an invention which embodied the leading principles of the modern machine, and from that time downwards the apparatus gradually developed and perfected in the hands of innumerable practical inventors.

THE WRITER OF CALICO PRINTING IN THE "ENCYCLOPÆDIA BRITANNICA"

## *LOCAL MUSEUMS*

THE importance of local museums is gradually but unmistakably forcing itself upon the country. It may take much time to foster any united action, without which any definite progress is very improbable, but year by year is adding to the ranks of those who are wise enough to see and have influence enough to advocate their value as a part of the educational stock-in-trade of the nation. We rejoice to see that Mr. Chamberlain has enrolled himself among their advocates in the House of Commons. On Monday he drew attention to the fact that the public expenditure for the promotion of science and art was exclusively confined to London, Edinburgh, and Dublin. The amount of the estimate this year, he said, for museums, art galleries, and parks in the metropolis amounted to nearly 400,000*l*., and that for Edinburgh and Dublin to nearly 50,000*l*. To those sums the provinces had to contribute twice over. Birmingham contributed about 4,000*l*., and had to find about 8,000*l*. a year besides for her own local art institutions. It might be said with truth that a national collection should be placed in the metropolis at the expense of the nation, but that argument did not apply to the expenditure on the public parks and still less to that which the Bethnal Green Museum involved. He did not complain of such expenditure. It produced most admirable results, adding as it did to the pleasure and happiness of great masses of the people, and tending to elevate and refine their minds. It was, too, in some sort a commercial investment, as it was calculated to enable artisans the better to compete with those of other nations. What he complained of was that the principle had not been carried far enough. He was anxious to see established in every one of our great centres of population and industry museums devoted to art and manufactures appropriate to each particular district. To show how highly these institutions were appreciated in the provinces, he mentioned that in Birmingham the local museum which had been established by private subscriptions was visited annually by 300,000 persons, and as the population of the town was only 370,000, the attendance was immensely greater than was shown by the returns of the number of visitors to our metropolitan institutions. Results equally extraordinary could be quoted from other provincial towns in which such museums existed. He further stated that although provincial communities were at present legally able to tax themselves to the extent of 1*d*. in the pound for the purpose of establishing museums and libraries, in Birmingham all this money went to the free library, and they had therefore no means of establishing an industrial museum.

We are glad also to see that the Government is now alive to the importance of this action, for, although Lord Sandon in his reply begged that the matter might not be pressed upon them at the present moment, he reminded the House of the great advantage which the country derived from the South Kensington Museum, which was now, in fact, a gigantic circulating museum. Almost all the principal objects in the museum, except those of great rarity or delicacy, were sent on their travels at different times through the provinces, and in this way aid was already given to local museums. The country derived enormous advantage from this vote. Local exhibitions were frequent, and loans from the South Kensington Museum for these exhibitions were very numerous. Eight museums had these objects sent to them, and a great deal had been done as the hon. gentleman wished. The South Kensington authorities were anxious to follow that course, but he could not say whether they would be able next year to do more in that direction. Their hands were to some extent tied by the necessity of economy, but the matter would receive the best attention of the Government, and he hoped that next session they might be able to go further.

Of course, neither Mr. Chamberlain nor Dr. Lyon Playfair allowed the subject to drop without pointing out that the British Museum and the National Gallery had no circulating system in operation, that in fact Lord Sandon was quite justified in adopting that line of argument with regard to the South Kensington authorities; but that many of the London galleries and museums were of no use to the provinces. The British Museum, for instance, and the National Gallery were practically of no use except to London, yet every one knew that they contained many duplicates which would be most valuable to the provinces, and the offer of some important pictures was sometimes declined on behalf of the National Gallery. Nor was this all. Dr. Playfair pointed out that in France the Minister of Education was responsible for all the museums, and constantly sent collections into the provinces; but in England, the management of the galleries was, so to say, dislocated, and not under one authority or one Minister. Why did he not go further and point out the recommendation of the Duke of Devonshire's Commission?

## *THE VELOCITY OF LIGHT*[1]

THE correct determination of the velocity of light is a result on which so much in physical science depends that there is good reason for us to give a description of the details of the apparatus used for the purpose of obtaining the exact value. Until the time that M. Cornu undertook experiments with this object in view the generally received value of the velocity in question was 298,000 kilometres per second. This depended on the experiments of M. Foucault, who used a rotating mirror on which the rays of light from cross-wires fell, and while the mirror was in a certain position were reflected by it to a concave mirror at a distance of 13½ feet, having the revolving mirror at its centre of curvature and so fixed as to return the rays of light to the latter, which reflected them to the point of departure. While however the rotating mirror was in rapid motion, a ray of light reflected by it to the distant mirror and back from it, would not, unless the passage of light were instantaneous, reach the rotating mirror until the latter had moved from its position of first reflection, and would not therefore return to the point of departure, but to some point near it, depending on the angle through which the rotating mirror had moved in the time between its reflecting the ray to the concave mirror and the return of that ray. By placing the cross-wires in the principal focus of a convex lens the rays proceed in a parallel beam, and on returning form an image of the wires, removed from the wires themselves, a distance depending on the angular velocity of the mirror and the velocity of light. The cross-wires and their images are rendered visible by viewing them by means of a diagonal reflector of plain glass in front, which at the same time allows sufficient light to pass through to illuminate them.[2]

In 1849 M. Fizeau devised a method differing from that just described by which he measured the time a ray took to travel from Suresnes to Montmartre and back. The apparatus consisted of a toothed wheel which could be rotated with a known velocity, and having the teeth and intervals equal in size. A pencil of rays was sent through the interval between two teeth to a reflector at Montmartre 28,334 feet distant, which caused the ray to return on itself. So long as the wheel is at rest and the rays pass through an interval, they will be returned through that same interval, but when the wheel turns with sufficient velocity a tooth takes the place of an interval before the ray has time to return from Montmartre and get through, and is therefore interrupted. By rotating this wheel faster the next interval will take the place of the preceding one on the return of the ray, which will again get through, and so on passing and being interrupted as the velocity of rotation increases.

It is obvious, then, that if we know the number of teeth on the wheel and the number of turns per second, say at the instant of reappearance of the spot of light after a disappearance, we shall know the interval between the passing away of the ray by the edge of one tooth to its return by the corresponding edge of the next; and this is the time the ray has taken to traverse the distance to the reflecting station and back, and from this the velocity of light follows. From these experiments M. Fizeau obtained a velocity of 315,000 kilometres = 196,000 miles per second.

At that time the velocity of light deduced from the observations of eclipses of Jupiter's satellites, combined with the then accepted solar parallax, was 190,000 miles per second, closely agreeing with M. Fizeau's result; later determinations of solar parallax have given a smaller result than former ones, and consequently the velocity of light deduced therefrom becomes reduced, which again closely agrees with M. Foucault's direct determination.

In the year 1874 the Council of the Paris Observatory, on the proposition of M. Leverrier and M. Fizeau, decided on the execution of experiments for the direct determination of the velocity of light, and offered the use of the scientific apparatus at the observatory for the purpose, together with funds for the construction of the necessary instruments. To M. Cornu was entrusted the execution of the operations; and after due consideration and experimental comparison, he adopted the method of M. Fizeau in preference to the revolving mirror of M. Foucault. A preliminary series of experiments were carried out in 1871 and 1872, between the École Polytechnique and Mont Valérien, a distance of 10,310 meters, giving a result of 185,370 miles per second as the velocity of light, with a probable error of less than $\frac{1}{100}$. M. Cornu then commenced more careful experiments between the Observatory and the Tour de Montlhéry, a distance of 22,910 metres. The principle of M. Cornu's arrangement we have already described, it being the same as that of M. Fizeau, but the details of the apparatus are somewhat elaborate, and in his Memoir occupy seven large sheets of plates; we can, however, to a certain extent describe them. Rays of light from a highly luminous source issuing from a small hole in a diaphragm, pass through a convex lens, and after reflection at an angle of 45 from the surface of a plain piece of glass, are brought to a focus at the circumference of the toothed wheel; the light then traverses an object-glass of fourteen inches diameter, and the parallel rays travel to the reflecting station; here they are received by an object-glass of six-inch aperture, and about six feet focal length, at the principal focus of which is a reflecting mirror of silvered glass. From this mirror the rays are returned to the toothed wheel, where an image is formed coinciding with the original image of the hole in the illuminated diaphragm, the rays if not intercepted by a tooth, pass onwards, and the greater part of them traverse the diagonal reflector of plain glass and an eyepiece beyond, through which the image formed by reflection from the distant station is viewed. So far we have given an outline of the optical part of the apparatus as well as we can without the use of the drawings by which the details can only be made intelligible. We next come to the toothed wheel, and here certain conditions must be fulfilled: first a velocity of rotation must be obtained capable of admitting a considerable number of orders of extinction; secondly, the motive power must be such that the observer can easily control the velocity of the wheel; thirdly, there must be a means of recording the velocity at any instant of time. The motive power is a weight which drives a train of wheels which rotate the toothed wheel, the latter being constructed of aluminium from $\frac{1}{10}$ to $\frac{1}{15}$ millimetre in thickness; wheels of different diameters were used varying from 35 to 48 millimetres. The velocity is recorded on the surface of smoked paper on a roller of about one metre in circumference and half a metre in length, turning on a horizontal axis. The records are made by the action of electro-magnets on light tracers pressing against the surface of the smoked paper. The velocity of the cylinder carrying the paper is such that a line 20 mm. is traced in a second, and during each revolution the tracers are moved on horizontally 15 mm. One of the tracers is put into action at every second by electric communication with a standard clock; a second is put into action at every $\frac{1}{10}$ second by a trembler governed by the pendulum of the clock; the third moves at each fortieth or four hundredth revolution of the toothed wheel, and the fourth is under the control of the observer. Each of the four tracers is continually in contact with the smoked surface of the paper, and so long as it is not moved sidewise by the electro-magnet, traces a continuous line round the cylinder, but on the passing of a current round the

[1] "Détermination de la Vitesse de la Lumière d'après des Expériences exécutées en 1874, entre l'Observatoire et Montlhéry." Par M. A. Cornu. (Paris: Gauthier Villars, 1876.)

[2] From the experiments of M. Foucault in 1862 a velocity of 298,000 kilometres = 185,157 miles per second was deduced.

electro-magnet, the tracer makes a short mark at right angles to line, and a zig-zag line caused by the vibration of the tracer, back to its original position; the first two lines, therefore, show seconds and tenths of seconds, the third, the instants of completion of forty or 400 revolutions, according to the desire of the operator, of the toothed wheel; a comparison, therefore, at once gives the number of revolutions per second, while on the fourth line are marked the instants of disappearance or reappearance of the light, and the velocities at those instants are then at once known. To make an experiment the aperture in the diaphragm is illuminated by a lime-light or sometimes with sunlight by means of a heliostat. The necessary adjustments in the direction of the rays of light to the distant station are then made by bringing the distant collimator into the centre of the field of the observing apparatus, and the point of light—the luminous echo—is made to accurately coincide with its original at

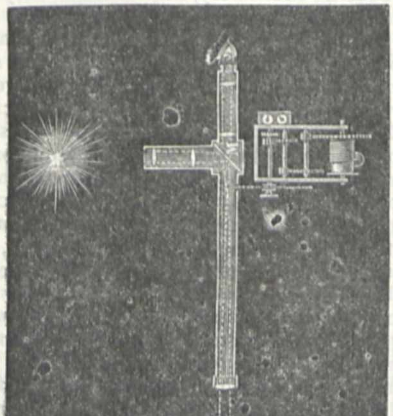


FIG. 1.—Plan of M. Fizeau's Apparatus.

the circumference of the toothed wheel. Particulars of the experiment, as to number of teeth of wheel, direction of rotation, &c., are entered on the paper on the cylinder, and the latter is then set in motion; the observer then sets the toothed wheel going and watches the luminous echo, and on its disappearing touches a key which sends an electric current to the electro-magnet controlling the fourth tracer, which therefore registers the instant the velocity is sufficient to cause a disappearance. As the velocity of the wheel increases, the luminous echo again appears and the key is pressed; a further increase in velocity causes another disappearance and so on to the higher orders, each of which is registered. The velocities at the different instants are read off by a micrometer to $\frac{1}{100}$ of a second.

It is obvious that the state of the air must have a great effect on the definition of the luminous echo, and that although the observation appears extremely simple still there may be large errors due to irregular refraction of the air, causing a motion of the point of light, and a large amount of patience must be required. Two careful surveys showed the distance between the two stations to be 22909·77 metres, and the mean velocity obtained from a large number of observations after the various corrections were made was 300,400 metres per second of mean time.

The Memoir of M. Cornu contains a large amount of theoretical matter and formulæ of corrections which of course we cannot reproduce here.

We may, however, refer to the principal causes of error. The first is a personal error depending on the sensibility of the eye of the observer in determining the disappearance and reappearance of the light at the toothed wheel, and also depending on the intensity of the luminous source; secondly, accidental inequality in the size of the teeth of the wheel; thirdly, irregularity of motion; fourthly, excentricity of wheels; fifthly, optical errors due to imperfections in the adjustment of the lenses and reflector. The first of these is small and can theoretically be reduced indefinitely by increasing the velocity of the

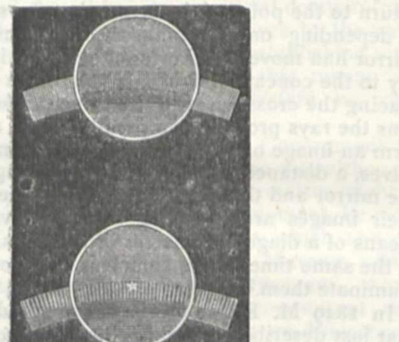


FIG. 2.—Details of Toothed Wheel.

toothed wheel and thereby observing the higher orders of extinction and reappearance of the light, but M. Cornu desires to be rigorously exact, and therefore the effect of this and the other errors is carefully calculated.

Considerable care was exercised in the choice of stations, and those adopted were fixed upon chiefly on account of the distance between them being more easily ascertained from previous triangulation. This distance was determined by Cassini and La Caille in 1740, the result being 22910·196 metres. From the observations of Delambre the same distance was computed to be 22909·34 metres; and 22910 metres, which is nearly the mean adopted by M. Cornu.

The corrected results of the experiments gave a velocity of 300,350 kilometres a second, but this was obtained in air, and therefore 82 kilometres must be added to this result to give the velocity *in vacuo*; and as the result of his experiments M. Cornu adopts a velocity *in vacuo* of 300,400 kilometres = 186,638 miles per second of mean time, with a probable error of $\frac{1}{1000}$, or 300 kilometres.

If from this value we deduce the solar parallax, we find the latter to be 8″·881, assuming the time required for light to travel from the sun to us to be 6m. 13·2 sec., as obtained from observations on Jupiter's satellites, and the radius of the earth 6378·233 kilometres.

Again, the sun's parallax deduced from M. Cornu's values of the velocity of light in conjunction with the value of aberration is, with Bradley's estimate of 20″·25, 8″·882, and with Struve's, of 20″·445, 8″·798. These values of parallax compare favourably with determination by other methods, of which we give a few examples. The value given by the transits of Venus in 1761 and 1769 was 8″·5776 computed by Encke, but increased to 8″·891 by Mr. Stone on a redetermination. By the record of an observation of the occultation of $\psi_2$ Aquarii on October 1, 1672, M. Leverrier obtained 8″·866; by meridian observations of Mars at Greenwich in 1862, 8″·932; by the latitude of Venus obtained from transits of 1761 and 1769, combined with present latitudes, M. Leverrier finds 8″·853; from the discussion of meridional observations of Venus in an interval of 106 years 8″·859; by the opposition of Mars in 1860 by M. Liais 8″·760; by opposition of Flora in 1873 by Prof. Galle 8″·873. Judging from these results the velocity of 186,638 miles per second is not very far from the mark, and the care in selection of methods and in computing results can scarcely be surpassed.

G. M. S.

## *EVOLUTION OF NERVES AND NERVO-SYSTEMS*[1]

NERVE-TISSUE universally consists of two elementary structures, viz., very minute nerve-cells and very minute nerve-fibres. The nerve-fibres proceed to and from the nerve-cells, thus serving to unite the cells with one another, and also with distant parts of the animal body. Moreover, nerve-cells and fibres, wherever we meet with them, present very much the same appearances. Here, for instance, is a sketch of highly magnified nerve-tissue as we find it in the human brain, and here is one of my own drawings of nerve-tissue as I have found it in the jelly-fish; and you see how similar the drawings are—notwithstanding they are taken from the extreme limits of the animal kingdom within which nerve-tissue is known to occur.

Nerve-cells are usually found collected together in aggregates which are called nerve-centres or ganglia, to and from which large bundles of nerve-fibres come and go. These large bundles of nerve-fibres are what we see with the naked eye as nerves, permeating the body in all directions. When such a bundle of nerve-fibres reaches a ganglion, or collection of nerve-cells, it splits up like the end of a rope which has been teased out, and the constituent fibres pass into and out of the nerve-cells, so interlacing with one another in all directions, as here diagrammatically represented. More true to nature is this diagram, which represents a magnified section of human brain—the human brain being itself nothing more than a collection of very large ganglia.

To explain the *function* of nerve-cells and nerve-fibres, I must begin by explaining what physiologists mean by the word "excitability." Suppose this to represent a muscle cut from the body of a freshly-killed animal. So long as you do not interfere with it in any way, so long will it remain quite passive. But every time you stimulate it either with a pinch, a burn, or, as represented in the diagram, with an electrical shock, the muscle will give a single contraction in response to every stimulation. Now it is this readiness of organic tissues to respond to a stimulus that physiologists designate by the term excitability.

Nerves, no less than muscles, present the property of being excitable. Suppose, for instance, that this is another muscle prepared in the same way as the last one, except that together with the muscle there is cut out the attached nerve. Every time you pinch, burn, or electrify any part of the nerve, the muscle will contract. But you will carefully observe there is this great difference between these two cases of response on the part of the muscle; viz., that while in the former case the muscle responded to a stimulus *applied directly to its own substance*, in the latter case the muscle responded to a stimulus *applied at a distance from its own substance*, which stimulus was then *conducted* to the muscle by the nerve. And here we perceive the characteristic function of nerve-fibres, viz., that of conducting stimuli to a distance. This is the function of nerve-*fibres*; but the function of nerve-*cells* is different, viz, that of accumulating nervous energy, and at fitting times of discharging this energy into the attached nerve-fibres. The nervous energy when thus discharged from the nerve-cells acts as a stimulus to the nerve-fibre; so that if a muscle is attached to the end of the fibre it contracts on receiving this stimulus. I may add that when nerve-cells are collected into ganglia they often appear to discharge their energy spontaneously, without any observable stimulus to cause the discharge; so that in all but the lowest animals, whenever we meet with apparently spontaneous action, we infer that ganglia are probably present. But the point which most of all I desire you to keep well in mind this evening is the distinction which I here draw between muscle and nerve. A stimulus applied to a nerveless muscle can only course through the muscle by giving rise to a visible wave of contraction, which spreads in all directions from the seat of stimulation as from a centre. A nerve, on the other hand, conducts the stimulus without undergoing any change of shape. Now in order not to forget this all-important distinction, I shall always to-night speak of muscle as conducting a visible wave of *contraction*, and of nerve as conducting an invisible or molecular wave of *stimulation*. Nerve-fibres, then, are functionally distinguished from muscle-fibres—and also, I may add, from protoplasm—by displaying the property of conducting invisible or molecular waves of stimulation from one part of an organism to another—so establishing physiological continuity between such parts *without the necessary passage of contractile waves*.

I will now conclude all that it is necessary to say about the function of nervous tissue by describing the mechanism of reflex action. Suppose this to represent any peripheral structure, such as a part of the skin of some animal, this a collection of nerve-cells or ganglion, and this a muscle. The part of the skin represented is united to the nerve-cells composing the ganglion by means of this in-coming nerve-trunk, while the nerve-cells in the ganglion are united to the muscle by means of this out-going nerve-trunk. Therefore when any stimulus falls on the skin where this in-coming nerve-trunk takes its origin, the nerve-trunk conveys the stimulus to the nerve-cells in the ganglion. When the nerve-cells receive the stimulus they liberate one of their characteristic discharges of nervous energy, which discharge then passes down this out-going nerve and so causes the muscle to contract. Now this particular kind of response is called response by reflex action, because the stimulus wave does not pass in a straight line from the seat of stimulation to the muscle, but passes in the first instance to the ganglion, and is from it *reflected* to the muscle. This, at first sight, appears to be a roundabout sort of a process, but in reality it is the most economic process available; for we must remember the enormous number and complexity of the stimuli to which every animal is more or less exposed, and the consequent necessity that arises in the case of highly organised animals of there being some organised system whereby these stimuli shall be suitably responded to. Or, to adopt a happy illustration of Prof. Bain, the stimuli are systematised on the same principle as the circulation of letters by post is systematised; for just as in the case of the letters there is no

[1] Abstract of a Lecture delivered at the Royal Institution on Friday evening, May 25, 1877. By George J. Romanes, M.A., F.L.S., &c.

direct communication between one street and another, but every letter passes first to the central office; so the transmission of stimuli from one member of the body to another is effected exclusively through a centre or ganglion.

Those among you who are acquainted with Mr. Herbert Spencer's writings are doubtless well aware how strong a case he makes out in favour of his theory respecting the genesis of nerves. This theory, you will remember, is that which supposes incipient conductile tissues, or rudimentary nerve-fibres, to be differentiated from the surrounding contractile tissues, or homogeneous protoplasm, by a process of integration which is due simply to use. Thus, beginning with the case of undifferentiated protoplasm, Mr. Spencer starts from the fact that every portion of the colloidal mass is equally excitable and equally contractile. But soon after protoplasm begins to assume definite shapes, recognised by us as specific forms of life, some of its parts are habitually exposed to the action of forces different from those to which other of its parts are exposed. Consequently, as protoplasm continues to assume more and more varied forms, in some cases it must happen that parts thus peculiarly situated with reference to external forces will be more frequently stimulated to contract than are other parts of the mass. Now in such cases the relative frequency with which waves of stimulation radiate from the more exposed parts, will probably have the effect of creating a sort of polar arrangement of the protoplasmic molecules lying in the lines through which these waves pass, and for other reasons also will tend ever more and more to convert these lines into passages offering less and less resistance to the flow of such molecular waves—*i.e.*, waves of stimulation as distinguished from waves of contraction. And lastly, when lines offering a comparatively low resistance to the passage of molecular impulses have thus been organically established, they must then continue to grow more and more definite by constant use, until eventually they become the habitual channels of communication between the parts of the contractile mass through which they pass. Thus, for instance, if such a line has been established between the points A and B of a contractile mass of protoplasm, when a stimulus falls upon A a molecular wave of stimulation will course through that line to B, so causing the tissue at B to contract—and this even though no *contractile* wave has passed through the tissue from A to B. Such is a very meagre epitome of Mr. Spencer's theory, the most vivid conception of which may perhaps be conveyed in a few words by employing his own illustration—viz., that just as water continually widens and deepens the channels through which it flows, so molecular waves of the kind we are considering, by always flowing in the same tissue tracts, tend ever more and more to excavate for themselves functionally differentiated lines of passage. When such a line of passage becomes fully developed, it is a nerve-fibre, distinguishable as such by the histologist; but before it arrives at this its completed stage—*i.e.*, before it is observable as a distinct structure—Mr. Spencer calls it a "line of discharge."

Such being the theory, I will endeavour to show how it is substantiated by facts. And here it becomes necessary to refer to my own work. You are all, I suppose, acquainted with the general appearance of a Medusa, or jelly-fish. The animal presents the general form of a mushroom. The organ which occupies the same position as the stalk does in the mushroom is the mouth and stomach of the Medusa, and is called the polypite; while the organ which resembles in shape the dome of the mushroom constitutes the main bulk of the animal, and is called the swimming-bell. Both the polypite and the swimming-bell are almost entirely composed of a thick transparent and non-contractile jelly; but the whole surface of the polypite, and the whole *concave* surface of the bell, are overlaid by a thin layer or sheet of contractile tissue. This tissue is not exactly protoplasm and not exactly muscle, but something between the two. It constitutes the earliest appearance in the animal kingdom of anything resembling muscular tissue. The thickness of this continuous layer of incipient muscle is pretty uniform, and is nowhere greater than that of very thin paper. The margin of the bell supports a series of highly contractile tentacles, and also another series of bodies which are of great importance for us to-night. These are the so-called marginal bodies, which are here represented, but the structure of which I need not describe. Lastly, it may not be superfluous to add that all the Medusæ are locomotive. The mechanism of their locomotion is very simple, consisting merely of an alternate contraction and relaxation of the entire muscular sheet which lines the cavity of the bell. At each contraction of this muscular sheet, the gelatinous walls of the bell are drawn together; the capacity of the bell being thus diminished, water is ejected from the open mouth of the bell backwards, and the consequent reaction propels the animal forwards. In these swimming movements systole and diastole follow one another with as perfect a rhythm as they do in the beating of a heart.

The question as to whether the Medusæ possess a nervous system is a question which has long occupied the more or less arduous labours of many naturalists. Until

Fig. 1

lately, however, there has been so little certainty on the subject that Prof. Huxley—himself one of the greatest authorities on the group—thus defined the standing of the question in his "Classification of the Animal Kingdom:" "No nervous system has yet been discovered in any of these animals." The cause of this uncertainty is to be found in the fact that the transparent and deliquescent nature of the tissues of the Medusæ renders adequate microscopical observation in their case a matter of extreme difficulty; so much so that, looking to the quantity and quality of the labour which has been bestowed on the question, I doubt whether the latter would ever have been satisfactorily settled by the histological methods alone. But those of you who were present at my lecture last year will no doubt remember that by employing methods other than the histological, I was able to set this long-standing question finally at rest. For you will no doubt remember my having told you that on merely cutting off the extreme marginal rim of the bell I was surprised to find that the previously active motions of the animal suddenly and entirely ceased; the paralysis caused by this simple operation was instantaneous, enduring, and complete. On the other hand, you may remember, the severed margin which had just been taken from the swimming-bell invariably continued its rhythmical motions with a vigour and a pertinacity not in the least impaired by its severance from the main organism. For hours, and even for days after the operation, these motions persisted; so that the contrast between the death-like quiescence of the mutilated swimming-bell and the active contractions of the thread-like portion which had just been removed from its margin, was a contrast as striking as it is possible to conceive.

These experiments, then, conclusively proved that in the marginal rim of the Medusæ there is situated an intensely localised system of nervous centres, or ganglia, to the functional activity of which the rhythmical motions of the swimming-bell are exclusively due.

(*To be continued.*)

## ON ELEMENTARY INSTRUCTION IN PHYSIOLOGY[1]

THE chief ground upon which I venture to recommend that the teaching of elementary physiology should form an essential part of any organised course of instruction in matters pertaining to domestic economy, is that a knowledge of even the elements of this subject supplies those conceptions of the constitution and mode of action of the living body and of the nature of health and disease, which prepare the mind to receive instruction from sanitary science.

It is, I think, eminently desirable that the hygienist and the physician should find something in the public mind to which they can appeal; some little stock of universally acknowledged truths, which may serve as a foundation for their warnings, and predispose towards an intelligent obedience to their recommendations.

Listening to ordinary talk about health, disease, and death, one is often led to entertain a doubt whether the speakers believe that the course of natural causation runs as smoothly in the human body as elsewhere. Indications are too often obvious of a strong, though perhaps an unavowed and half unconscious, undercurrent of opinion that the phenomena of life are not only widely different in their superficial characters and in their practical importance, from other natural events; but that they do not follow in that definite order which characterises the succession of all other occurrences, and the statement of which we call a law of nature.

Hence, I think, arises the want of heartiness of belief in the value of knowledge respecting the laws of health and disease, and of the foresight and care to which knowledge is the essential preliminary, which is so often noticeable; and a corresponding laxity and carelessness in practice, the results of which are too frequently lamentable.

It is said that, among the many religious sects of Russia, there is one which holds that all disease is brought about by the direct and special interference of the Deity, and which, therefore, looks with repugnance upon both preventive and curative measures, as alike blasphemous interferences with the will of God. Among ourselves, the "Peculiar People" are, I believe, the only persons who hold the like doctrine in its integrity, and carry it out with logical rigour. But many of us are old enough to recollect that the administration of chloroform in assuagement of the pangs of childbirth was, at its introduction, strenuously resisted upon similar grounds.

I am not sure that the feeling, of which the doctrine to which I have referred is the full expression, does not lie at the bottom of the minds of a great many people who would yet vigorously object to give a verbal assent to the doctrine itself. However this may be, the main point is that sufficient knowledge has now been acquired of vital phenomena to justify the assertion that the notion that there is anything exceptional about these phenomena receives not a particle of support from any known fact. On the contrary, there is a vast and an increasing mass of evidence that birth and death, health and disease, are as much parts of the ordinary stream of events as the rising and setting of the sun, or the changes of the moon; and that the living body is a mechanism the proper working of which we term health; its disturbance, disease; its stoppage, death. The activity of this mechanism is dependent upon many and complicated conditions, some of which are hopelessly beyond our control, while others are readily accessible and are capable of being indefinitely modified by our own actions. The business of the hygienist and of the physician is to know the range of these modifiable conditions, and how to influence them towards the maintenance of health and the prolongation of life; the business of the general public is to give an intelligent assent and a ready obedience based upon that assent, to the rules laid down for their guidance by such experts. But an intelligent assent is an assent based upon knowledge, and the knowledge which is here in question means an acquaintance with the elements of physiology.

It is not difficult to acquire such knowledge. What is true, to a certain extent, of all the physical sciences, is eminently characteristic of physiology—the difficulty of the subject begins beyond the stage of elementary knowledge, and increases with every stage of progress. While the most highly trained and best furnished intellect may find all its resources insufficient when it strives to reach the heights and penetrate into the depths of the problems of physiology, the elementary and fundamental truths can be made clear to a child.

No one can have any difficulty in comprehending the mechanism of circulation or respiration, or the general mode of operation of the organ of vision; though the unravelling of all the minutiæ of these processes may, for the present, baffle the conjoined attacks of the most accomplished physicists, chemists, and mathematicians. To know the anatomy of the human body, with even an approximation to thoroughness, is the work of a life, but as much as is needed for a sound comprehension of elementary physiological truths may be learned in a week.

A knowledge of the elements of physiology is not only easy of acquirement, but it may be made a real and practical acquaintance with the facts, as far as it goes. The subject of study is always at hand in oneself. The principal constituents of the skeleton, and the changes of form of contracting muscles, may be felt through one's own skin. The beating of one's heart, and its connection with the pulse may be noted; the influence of the valves of one's own veins may be shown; the movements of respiration may be observed; while the wonderful phenomena of sensation afford an endless field for curious and interesting self-study. The prick of a needle will yield, in a drop of one's own blood, material for microscopic observation of phenomena which lie at the foundation of all biological conceptions; and a cold, with its concomitant coughing and sneezing, may prove the sweet uses of adversity by helping one to a clear conception of what is meant by "reflex action."

Of course, there is a limit to this physiological self-examination. But there is so close a solidarity between ourselves and our poor relations of the animal world, that our inaccessible inward parts may be supplemented by theirs. A comparative anatomist knows that a sheep's heart and lungs, or eye, must not be confounded with those of a man; but so far as the comprehension of the elementary facts of the physiology of circulation and of respiration and of vision goes, the one furnishes the needful anatomical data as well as the other.

Thus, it is quite possible to give instruction in elementary physiology in such a manner as not only to confer knowledge, which, for the reason I have mentioned, is useful in itself; but to serve the purposes of a training in accurate observation, and in the methods of reasoning of physical science. But that is an advantage which I

[1] A paper read at the Domestic Economy Congress, by Prof. Huxley, F.R.S.

mention only incidentally as the present conference does not deal with education in the ordinary sense of the word.

It will not be suspected that I wish to make physiologists of all the world. It would be as reasonable to accuse an advocate of the "three R's" of a desire to make an orator, an author, and a mathematician of everybody. A stumbling reader, a pot-hook writer, and an arithmetician who has not got beyond the rule of three, is not a person of brilliant acquirements; but the difference between such a member of society and one who cannot either read, write, or cipher is almost inexpressible; and no one nowadays doubts the value of instruction, even if it goes no further.

The saying that a little knowledge is a dangerous thing is, to my mind, a very dangerous adage. If knowledge is real and genuine, I do not believe that it is other than a very valuable possession, however infinitesimal its quantity may be. Indeed, if a little knowledge is dangerous, where is the man who has so much as to be out of danger?

If William Harvey's life-long labours had revealed to him a tenth part of what may be made sound and real knowledge to our boys and girls—he would not only have been what he was, the greatest physiologist of his age, but he would have loomed upon the seventeenth century as a sort of intellectual portent. Our little knowledge would have been to him a great, astounding, unlooked-for vision of scientific truth.

I really see no harm which can come of giving our children a little knowledge of physiology. But then, as I have said, the instruction must be real, based upon observation, eked out by good explanatory diagrams and models, and conveyed by a teacher whose knowledge has been acquired by study of the facts, and not the mere catechismal parrot-work which too often usurps the place of elementary teaching.

It is, I hope, unnecessary for me to give a formal contradiction to the silly fiction, which is assiduously circulated by fanatics who not only ought to know, but do know, that their assertions are untrue, that I have advocated the introduction of that experimental discipline which is absolutely indispensable to the professed physiologist, into elementary teaching.

But while I should object to any experimentation which can justly be called painful, for the purpose of elementary instruction, and while, as a member of a late Royal Commission, I gladly did my best to prevent the infliction of needless pain for any purpose, I think it is my duty to take this opportunity of expressing my regret at a condition of the law which permits a boy to troll for pike, or set lines, with live frog bait, for idle amusement; and, at the same time, lays the teacher of that boy open to the penalty of fine and imprisonment if he uses the same animal for the purpose of exhibiting one of the most beautiful and instructive of physiological spectacles, the circulation in the web of the foot. No one could undertake to affirm that a frog is not inconvenienced by being wrapped up in a wet rag, and having his toes tied out; and it cannot be denied that inconvenience is a sort of pain. But you must not inflict the least pain on a vertebrated animal for scientific purposes (though you may do a good deal in that way for gain or for sport) without due licence of the Secretary of State for the Home Department, granted under the authority of the Vivisection Act.

So it comes about, that in this present year of grace 1877, two persons may be charged with cruelty to animals. One has impaled a frog, and suffered the creature to writhe about in that condition for hours; the other has pained the animal no more than one of us would be pained by tying strings round his fingers, and keeping him in the position of a hydropathic patient. The first offender says, "I did it because I find fishing very amusing," and the magistrate bids him depart in peace; nay, probably wishes him good sport. The second pleads, "I wanted to impress a scientific truth, with a distinctness attainable in no other way, on the minds of my scholars," and the magistrate fines him five pounds.

I cannot but think that this is an anomalous and not wholly creditable state of things.

## *OUR ASTRONOMICAL COLUMN*

D'ARREST'S COMET.—M. Leverrier notifies the discovery of the periodical comet of D'Arrest by M. Coggia at Marseilles, on the 8th inst., nearly in the position assigned by M. Leveau's calculations. It was also detected at Florence by M. Tempel, on the 10th.

The comet was discovered by the late Prof. D'Arrest at Leipsic on June 27, 1851, and observed till October 6. The elliptical character of the orbit was pointed out by the discoverer early in August, and his conclusions were verified by the calculations of Vogel and Villarceau shortly afterwards, the latter astronomer commencing, while the comet was yet under observation, a series of elaborate computations of the effect of planetary perturbations upon its motion, which were continued by him until taken up by Leveau. With the aid of Villarceau's ephemerides the comet was detected on its ensuing return to perihelion at the Royal Observatory, Cape of Good Hope, and observed from December 5, 1857, to January 18, 1858. Oudemans, in a memoir published by the Royal Academy of Sciences at Amsterdam in 1854, had also carried forward the elements to this appearance, his results indicating that while the normal positions of 1851 were best represented by a mean motion which would bring the comet to perihelion again on December 5, 1857, there yet remained an uncertainty to the extent of eighty-five days in the length of the revolution. Villarceau, in the *Comptes Rendus de l'Académie des Sciences*, 1852, December 6, considered the period fixed within narrower limits, one of his sets of elements assigning November 28, 1857, for the next perihelion passage, on which day the Cape observations show that it actually occurred. At the second return in the spring of 1864 the comet was not observed, and a very heavy work was involved in the preparation of an ephemeris for 1870, owing to the large perturbations due to the action of Jupiter in 1861, the comet having in April of that year approached the planet within 0·36 of the mean distance of the earth from the sun, and the two bodies remaining in proximity for a considerable time; it was therefore necessary to determine the effect of this near approach to the most powerful of the planets with every possible precision, a long work successfully accomplished by Leveau, who found on continuing the calculation of the perturbations of Jupiter, Saturn, and Mars, to June, 1870, the following material changes in the elements at the perihelion passage in November, 1857.

| | | | |
|---|---|---|---|
| | ° ′ | | ° ′ |
| Long. of Perihelion | − 4 32 | Angle of Eccentricity | − 1 52 |
| ,, Ascend. Node | − 2 12 | Mean anomaly ... | + 10 10 |
| Inclination ... ... | + 1 43 | Mean motion ... | − 15″·82 |

So that the period of revolution was lengthened sixty-eight days, the comet arriving at perihelion on September 22, 1870. The effect of these perturbations was to alter the geocentric place at this time, no less than 14°·6 in right ascension, and 7°·6 in declination. At all three returns the comet has been a faint object, and it was particularly so in 1870, when it was, nevertheless, sufficiently observed, Prof. Julius Schmidt, profiting by his favourable position at Athens, to follow it until nearly the end of the year.

The following are the dimensions of the orbit of D'Arrest's comet in the present year, according to the elements of Leveau.

| | | | |
|---|---|---|---|
| Semi-axis major ... | 3·54139 | Perihelion distance | 1·31809 |
| ,, minor ... | 2·75651 | Aphelion ,, ... | 5·76469 |
| Semi-parameter ... | 2·14559 | Eccentricity ... | 0·6278048 |

The period of revolution is 2434·2 days, or 6·664 years, therefore nearly identical with that of Biela's comet up to 1852. The comet passed its perihelion on May 10. It will not arrive at its least distance from the earth until October 20, but the theoretical intensity of light diminishes from the present time, indeed has been on the decrease since the middle of May; the comet may be a test object on the borders of the constellations Eridanus and Orion in September and October.

It is probable that this comet had been revolving in its present restricted orbit for many years previous to its discovery in June, 1851. It certainly does not furnish a parallel case to that of Brorsen's comet, which was detected at its first perihelion passage after the attraction of the planet Jupiter had impressed upon it the actual form of orbit in May 1842. The nearest approach of D'Arrest's comet to Jupiter during the revolution immediately preceding discovery, took place at the end of September 1849, when the distance was 1·136.

THE BINARY STAR $\alpha$ CENTAURI.—Mr. Ellery communicates to the Royal Astronomical Society recent measures of this, the finest and most interesting of all the revolving double-stars. Taking means the following epochs result from the Melbourne measures:—

| | | | | |
|---|---|---|---|---|
| 1876·72 | Position | 51°·1 | Distance | 4″·3 |
| 1877·25 | ,, | 69·1 | ,, | 3·13 |

Mr. Maxwell Hall (NATURE, vol. xv. p. 510) supplies the following:—

| | | | | |
|---|---|---|---|---|
| 1877·14 | Position | 64°·4 | Distance | 3″·3 |

Mr. Ellery states that the distance in 1862 was 10″, but this must be an over-estimate with the meridian instrument; a mean of seventy micrometrical measures by Mr. Eyre B. Powell at Madras, gives for 1862·2, a distance of only 6″·79, a result no doubt entitled to great confidence. The first *minimum* of distance appears to have occurred in 1856. Capt. Jacob's measures for 1856·27 giving 3″·89, and a maximum of about 10″ followed in 1868-70. It is to be hoped that the star will now be frequently measured micrometrically with all possible precision, though the brilliancy and closeness of the components may render such measures difficult. A practised computer should be able to throw some light on the real nature of the orbit from the data already in our possession, but the continued regular measurement of the star at this critical period cannot fail to be of great importance in extending our knowledge of the motion in this system. The reliable estimate of its distance resulting from the observations of Henderson, Maclear, and Moesta, vastly increases the interest attaching to it.

## GEOLOGICAL NOTES

GEOLOGICAL SURVEY OF THE UNITED KINGDOM.—The gradual progress of the English and Scottish Geological Surveys has brought the members of the two corps almost within sight of each other. The line of demarcation between the two kingdoms being nearer the base of operations from Scotland, has been sooner reached from that side. From Berwick-on-Tweed south-westwards the work has been carried up from the north to the English border through the range of the Cheviot Hills, and down the valley of Liddesdale to the Solway. To prevent any subsequent risk of the lines from either side not fitting accurately, the officers on the Scottish border are at present engaged in running their boundaries into Cumberland and Northumberland for such a short distance as may be required to leave them in a position where they can be easily taken up by the advanced guard of the English Survey. When this and some few isolated areas are completed, the whole of the south of Scotland between the Tay and Clyde and the English border will have been geologically surveyed, and the Scottish staff will then be engaged on both sides of the flanks of the Highlands. Already ground has been broken, and some progress has been made on the north side of the Grampian chain. On the English side the mountainous lake district is all surveyed, while the work is so well advanced in Cumberland that it may probably be completed up to the Scottish border by the end of this year. Considerable progress has likewise recently been made on the eastern side in pushing the survey northwards in Northumberland, though a considerable tract of that country still remains unmapped towards the Cheviots and Tweed. Among the south-eastern counties the survey is advancing through Norfolk, Suffolk, and Cambridgeshire, while in the south-west some of the maps which were made in the early days of the Geological Survey are being re-surveyed and brought up to date in West Somerset and Devon.

GEOLOGICAL SURVEY OF CANADA.—The Report of this Survey for 1875-76 has just arrived. In size, general interest, and geological value, it fully equals its well-known predecessors, while in regard to maps, sections, and other illustrations, it even surpasses them. Briefly told, its story is this—The Philadelphia Exhibition absorbed much of the time and thought which would otherwise have been expended on the field-work, laboratory, and museum duties of the officers. But the director need not regret this temporary suspension of the usual operations of his staff, for there can be no doubt that the display of rocks, minerals, and fossils, made by Canada at the Centennial Exhibition, so universally admired brought the mineral resources of the dominion and the skill of its geological survey before the world with such prominence as could hardly have been attained even with the ablest maps and memoirs. Mr. Selwyn's own labours from April to November, 1875, embraced an exploration of parts of British Columbia where likewise Mr. George M. Dawson, who has lately been appointed to the Canadian Survey, has been actively employed. Prof. Macour, besides his geological work, made a careful botanical survey of the region traversed, and his detailed narrative appears in this Report. Mr. Ellis was sent into the North-west Territory to make a series of borings. Mr. Bell explored the country between James Bay and Lakes Superior and Huron; while in the eastern parts of the Dominion detailed surveys were made in the coal-fields of Nova Scotia, in New Brunswick, and in Cape Breton. Besides these explorations others were continued by Mr. Vennor in Ontario. Of these Mr. Selwyn remarks that they prove the existence in Western Quebec and Eastern Ontario of a massive red orthoclase gneiss without visible stratification, lying probably unconformably under the vast crystalline masses containing *Eozoon*. He suggests that what is called Lower Laurentian may have to be termed Middle, the fundamental red gneiss becoming the Lower.

EXCREMENTITIOUS DEPOSITS IN THE ROCKY MOUNTAINS.—A recent paper to the Philadelphia Academy by Mr. Henshaw, on the excrementitious deposits in the Rocky Mountain region, sustains Prof. Cope's view that they were made by big-eared rats, a species of *Neotoma*, probably *N. cinerea*. They consist of vegetable matter, sometimes with a bitumen-like look, and varying from this appearance to that of pill-like excrements. In a crevice of the rocks one deposit had a depth of two feet, and contained also some small twigs and "birds'" feathers. "The mass was evidently the accumulation of years, and had served as a nest. Throughout was a large amount of hard droppings from which the urine had passed, and whose nature was unmistakable. The urine, charged with a certain amount of excrementitious matters, had filtered through to form the singular deposits." Water or the urine has carried the portions it could dissolve down the faces of walls, and deposited it on shelves where no animals without wings could reach, and sometimes on the roofs of cavities. All the regions where these deposits occur are inhabited by the *Neotoma*, which is essentially a vegetarian.

## NOTES

THE inaugural meeting of the Domestic Economy Congress was held at the rooms of the Society of Artists in Birmingham, on Tuesday last. In the absence of Lord Leigh the Mayor took the chair. Prof. Huxley gave a short address, in which he maintained that our so-called education does not fit a man for understanding his social duties. Public opinion was now beginning to take a different view of what education ought to be. Those who supported this Congress were among those who felt most strongly on the subject, and the influence on Government from discussions and meetings would be successful in the long run. The real business of the Congress commenced yesterday, when, amongst the papers read, were:—"Nursing," by Mrs. W. E. Gladstone; "Infant Life," by the Countess of Ebersburg; "Nursing in Connection with Education," by Miss Helen Taylor; "Elementary Instruction to Children in Physiology," by Prof. Huxley; and "Warming and Ventilation," by Capt. Galton. We give elsewhere an account of Prof. Huxley's paper, and we commend the latter part of it especially, not only to those who are interested in the teaching of physiology in schools, but to all who have given attention to that "burning question" of the day—vivisection.

THE annual session of the French Association for the Advancement of Science will take place at Havre on August 23 next, so that the members of the British Association will be able to take part in the proceedings, and a good attendance of these is anticipated. Members wishing to visit Havre are desired to write to the secretary of the French Association, 76, Rue de Rennes, Paris. Dr. Gilbert, local secretary in Havre, will engage rooms if required by previous notice. The president this year is Dr. Broca, Professor to the Faculty of Medicine, and Director of the Anthropological School of Paris. The maritime situation of Havre will supply every opportunity for a number of interesting excursions, especially for members of the British Association. The principal object of the deliberations of the society will be to determine how to secure a large and effective representation of science during the International Exhibition next year. A delegation of the French Society will be sent to the session of the British Association as was arranged last year.

ON Monday the 9th inst. the Haberdasher's Company voted 250 guineas as a donation to the building fund of the new City of London College. This institution is the outcome of the Metropolitan Evening Classes for young men, originally established at Crosby Hall, Bishopsgate Street, in 1848. Since that time it has pursued a most successful career, and now has more than 1,400 members.

THE Albert Medal of the Society of Arts has recently been presented to Sir George B. Airy, K.C.B., "for eminent services rendered to commerce by his researches in nautical astronomy and in magnetism, and by his improvements in the application of the mariner's compass to the navigation of iron ships."

WE understand that the International Meteorological Congress which was to have met at Rome in September, has been postponed to next year.

THE sad death of Dr. James Bryce furnishes another instance of the fact that though geology is in itself an invigorating pursuit it has its own share of risks. This long-known writer had left his home in Edinburgh on Tuesday morning the 10th inst. for a geological tour in the Western Highlands. He had reached as far as the Falls of Foyers on Loch Ness, whither he had undertaken to conduct a scientific excursion from Inverness a little later. On Wednesday, after leaving at the hotel a note addressed to his daughter announcing his safe arrival so far on his journey, he strolled out hammer in hand to make some further observations among the granitic crags of that neighbourhood to which he had already given some attention. He was never seen again alive. A few hours afterwards his lifeless body was found on a slope of *débris* at the foot of a shattered cliff of rock. His hammer lay a few yards higher up at the base of the crags. It is supposed that either from the concussion of his hammer or from some other cause a portion of the cliff had fallen away, crushing his temples, and killing him instantaneously. Dr. Bryce's early researches among the basalt of the north of Ireland, his work on Arran and Clydesdale, his papers on the Secondary rocks of the West Highlands, and his labours in connection with Scottish earthquakes have made his name familiar to geologists in this country. He was seventy-one years of age.

WE regret to announce the death, on the 15th instant, of another well-known geologist, Mr. John Williamson, of Scarborough, the discoverer of the celebrated Gresthorpe plant-beds. His labours in bygone years, when field-workers like him were very scarce, will long be borne in remembrance. He was born in 1784, so that he was ninety-three years of age.

THE Portuguese African explorers, Major Serpa Pinto and Capt. Brito Capello, have returned to Lisbon after visiting Paris and London (whither they had gone to obtain some necessary articles), and are to start for Loanda in the *Zaire*. According to the *Diario de Noticias*, they have got together a magnificent outfit, and M. de Abbadie, the eminent French explorer, pronounced this expedition the best and most scientifically formed that had yet gone out to Africa. He has given them the remarkable universal theodolite with inverse action, of his own invention, which he has called the "Abbas," and with which he made geodetic measurements in Abyssinia; which further he used in Algiers in observation of the transit of Venus. They also take a new apparatus invented by Father Perry for the study of terrestrial magnetism, one of the best equatorials of the Polytechnic School of Paris, a sextant of great delicacy, &c. M. Serpa Pinto has previously made extensive journeys in Africa, to Lake Nyassa and to near the Victoria Falls of the Zambesi.

WE note that a public meeting is to be held at the Mansion House to-day in aid of the "African Exploration Fund," recently commenced by the Council of the Royal Geographical Society to promote the continuous and systematic exploration of the interior of Africa.

AN engineer of St. Petersburg, M. de Kern, announces the discovery of a new metal, which he calls *Davyum*. It is found in the residues got from extraction of platinum. To isolate the element it is necessary, after having precipitated the ruthenium, to treat the mother lye with nitrate of ammonia. A red precipitate is produced, which on calcination yields davyum. This metal is easily attackable by aqua regia, and much less by boiling sulphuric acid; potash precipitates it in a yellow state, and sulphuretted hydrogen in a brown, passing into black through desiccation. With sulphocyanide of potassium the chloride of davyum gives a red coloration. From theoretical considerations developed by M. Mendeléeff, M. de Kern considers davyum to rank between molybdenum and ruthenium. On this supposition its equivalent should be near 100; and he proposes to test this experimentally.

M. V. OBERMAYER, of Vienna, has proved by experiments that the internal friction (viscosity) of hard black pitch is subject to the same laws as fluid friction. He determined the coefficients of internal friction by three different methods:—1. Pressure of cylindrical plates; 2. Deformation of parallelepipedal plates; 3. Distortion of cylindrical plates. No gliding of the black pitch occurs on the metal plates, between which the pitch plates are cast. For soft bodies, the internal friction appears not to follow exactly the laws of fluid friction.

ON July 13 the French Minister of Public Works visited the works of the 1878 Exhibition, which are in course of progression at the Champ de Mars. The number of workmen engaged in actual working was 1,137. Not less than 700 were employed at the Central Pavilion.

A SINGULAR accident has been recorded by the *Journal Officiel*. M. Gastard, of Paris, had placed a number of cartridges on a table. Some solar rays having been concentrated by an "eye" in the glass of a window, a terrific explosion took place. Similar catastrophes are more common than is generally supposed in summer, the windows of railway carriages, igniting sometimes overdried plants, or even leaves fallen on railway embankments. It is known also that fires sometimes occur in Algerian forests through drops of water suspended to the leaves and forming lenses.

IT is now about a quarter of a century since the first submarine cable was laid, and the telegraph system may now be said to embrace all parts of the world, offering a certain completeness as an object of study. In an interesting *brochure* recently sent us, "Recherches sur la Loi du Mouvement Telegraphique International," M. Madsen sets the problem,—Is there a determinate relation between the international telegraphic movement and the commercial traffic, and what is the mathematical expression of this relation? He arrives, from a comparison of statistics, at a law which may be approximately expressed by the equation, $T = \frac{1}{d}[\sqrt{VN} + N_1 + N_2]$; in which $T$ denotes the number of telegrams between two countries, $d$ the distance between their commercial "centres of gravity," $V$ the value (in pounds sterling) of their commerce with each other, $N$ the tonnage of the ships sailing between them, $N_1$ the tonnage of ships of the one country ($L$), but sailing between the other $L_1$ and other countries; $N_2$ the tonnage of ships belonging to $L_1$, but sailing between $L$ and other countries. The law has various applications, some of which M. Madsen points out.

AN ingenious new registering thermometer devised by M. Hervé Mangon, is described in *La Nature*. A long and fine capillary tube bent on itself and containing mercury, is supported in an iron frame; it passes through the stopper of a bell jar and terminates with a fine point in a mercury dish placed in one scale of a balance; the other scale contains a vessel of glycerine communicating by glass and india-rubber tubing with another glycerine vessel on the same level in an adjoining frame. When, on rise of temperature, mercury is forced out into the vessel, the balance is depressed on one side and an electric contact made, affecting an electro-magnet in the registering apparatus, which is composed of M. Redier's double wheel-work with differential train (which we must not stay to describe). When the depression referred to has occurred a suspended float in the second glycerine vessel descends, and raises the glycerine in the first, increasing the weight in that scale. The curve obtained (from a pencil on moving paper) is of zigzag form, the wheel-work being in constant motion, now to the right, now to the left.

M. BERTRAND having lately made an appeal to possessors of letters from Gauss, with a view to publishing the complete works of the eminent geometer, the grand-daughter of Laplace has responded with five interesting letters. One of them, written in 1807, presents Gauss at the outset of his career, deprived of his fortune and threatened with extreme measures if he did not pay 2,000 francs as a war contribution to the French army occupying Göttingen. In his distress he applies to Laplace, thinking his intervention might prove effectual. At the same time he describes the equally sad position of his colleague Harding. Laplace, unable to influence Napoleon, pays the 2,000 francs, and begs his friend not to disquiet himself further. Meanwhile Gauss obtains the sum from Olber, and now he is in a position to succour Harding. Two years later he paid Laplace back the sum he had borrowed.

AN analysis has been lately made by Dr. Alder Wright (*Chemical News*) of two samples of wine, "ruby" and "white," from the Auldana vineyards, South Australia, with a view to determine the proportion of iron as a natural constituent. The average amount obtained by one method was, in both cases, 0·00130, by another method 0·00146, the iron being calculated as FeO (in the former case it is thought there may have been a little loss through incineration, &c.). Two circumstances are noted; first, that contact of the grape-juice with ironwork of any kind is studiously avoided in the manufacture; secondly, that the soil of the Auldana vineyard is exceptionally ferruginous, and as iron is taken up from the soil by vegetation, this seems a probable cause for the occurrence of iron in the finished wine. The identical character of the values, too (the wines being of different vintages) makes it improbable that the source of the iron is outside the grape-juice.

AN interesting experiment with regard to the speed of pigeon flight was made the other day. A carrier pigeon having been let off in Dover simultaneously with the starting of the express for London, reached the latter place twenty minutes in advance of the train. This corresponds to a distance-difference of eighteen miles.

WE observe that the recent enlightened decision by the Senate of London University with regard to admission of women to degrees in medicine meets with a good deal of hostile criticism from some of our leading medical contemporaries. We feel sure that no Trades' Union spirit will be allowed to prejudice what must generally be recognised as a step in the right direction.

AT a meeting of members of the Birmingham Natural History and Microscopical Society, held at the Midland Institute on July 13, a committee was appointed to make arrangements for another marine excursion, somewhat similar to the one made by this society in 1873 to Teignmouth, but this time it is to be to Arran and the Western Islands of Scotland. The dredging will be carried on in Lamlash and Brodick Bays. At the same time excursions will be made on land to Arran and the adjacent localities, all of which, we believe, yield a number of rare specimens, both botanical and geological; so that the members of each section no doubt will find this an enjoyable and an interesting excursion. It will take place in the first week in September.

A WONDERFUL white aquamarine has been found in Perthshire which, when cut, has produced one of the most brilliant gems ever seen. It is said by many competent judges to be equal to her Majesty's celebrated Koh-i-noor, its refraction being very great both by day and night. It is of a pure pellucid liquid white, and is known as the Scotch Koh-i-noor. Its hardness is 8·0, and specific gravity 2·76. Mr. Bryce M. Wright, F.R.G.S., is its possessor.

THE additions to the Zoological Society's Gardens during the past week include a Sambur Deer (*Cervus aristotelis*), a Spotted Porcine Deer (*Cervus minor*) from India, presented by H.R.H. the Prince of Wales, K.G.; a Slow Loris (*Nycticebus tardigradus*) from Malacca, a Prehensile-tailed Paradoxure (*Paradoxurus prehensilis*) from Burmah, presented by Mr. W. H. Richardson; a Leadbeater's Cockatoo (*Cacatua leadbeateri*) from Australia, presented by Mrs. Shand; a Red Howler (*Mycetes seniculus*) from New Granada, four Axis Deer (*Cervus axis*) from India, a Merian's Opossum (*Didelphys dorsigera*) from South America, deposited; two Striped Hyænas (*Hyæna striata*), born in the Gardens, a Black-necked Swan (*Cygnus nigricollis*), hatched in the Gardens.

## *THE STÄLLDALEN METEORITE*[1]

In the Scandinavian North, so extraordinarily rich in mines and quarries, there have been found during the last few years a number of new minerals, by which many a mine and even many an inconsiderable opening scarce known in its own parish has become world-famous in mineralogical literature. Several of these *finds* are of great interest in a systematic aspect—for instance, the discovery of *barytite*, a new, exceedingly basic variety of felspar containing baryta; of *ganomalite*, the first natural silicate of lead which has been discovered; of *ekdennite*, a new mineral containing antimonic acid, from the mines of Langban; and of *homilite*, a new, beautifully crystallised silicate of boron, containing water, from Brevig. Others again give us a highly unexpected insight into the nature of the chemical forces which are in activity in the interior of the earth—for instance, the Wermland minerals, *manganosite*, or protoxide of manganese, and *pyrokroite*, or hydrated protoxide of manganese, which afford evidence of a powerful reducing action. The latter mineral has during last year been found at a new locality—the mines of Nordmark.

However important these newly-discovered minerals may be, they do not awaken so keen an interest as the stones which from time to time fall from the heavens, and afford us specimens of the matter to be found in spaces so remote that rays of light require thousands of years to reach them. A new and highly instructive contribution to our knowledge of meteorites has been obtained in Sweden through the fall of the meteorite, which took place at Ställdalen, near Nya Kopparberg, in Örebro län, on June 28, 1876, at 11·50 A.M., from a fireball which was visible over a large part of middle Sweden. In the neighbourhood of Stockholm the meteor appeared as an indistinctly-defined fireball, followed by a long streak of fire. The ball was first visible below the zenith in the north-east or north-north-east, and went from hence towards the horizon in the west, where it generally appeared to fall in the immediate neighbourhood of the spectator, sometimes with, sometimes without, the throwing out of sparks. In the town of Gefle the fire-red ball, followed by a streak of the same colour, was seen moving from north-east to south-west. At the neighbouring promontory, Harnäs, it was first seen of the size of a large star, speedily increasing, however, leaving a long streak of fire behind it, and finally disappearing without noise, falling, according to the supposition of the spectator, behind some neighbouring buildings.

At Malmköping the meteor appeared to proceed from the northern heavens towards the west, leaving behind it a fine white streak, which was distinguishable for two minutes. At a height of 25° above the horizon it disappeared without falling asunder. At Linköping the nucleus of the meteor was pear-shaped, of blinding whiteness, followed by a streak of fire which was strongly luminous notwithstanding the clear bright sunshine, and about eight times longer than the nucleus. It was first observed pretty high up in the north-east, but afterwards sank to a height of 10° above the horizon in the west, where it broke up without noise into a number of star-sparks. In Skara the meteor, followed by a beautifully luminous streak of fire, appeared to fall asunder, throwing out sparks strongly at the same time, after having gone from east to west with an apparent diameter of half that of the moon. In Hedemora two fire-balls were seen, one close behind the other, falling from the zenith towards the west, leaving behind them a light grey streak. A minute after the meteor passed from the field of view, a loud explosion was heard, which is also mentioned in reports from the town of Falun and from Gustafs and Stora Tuna parishes. In Mora no explosion was heard, but here the meteor, which left in its path a stream of fire of a deep violet colour, was seen to fall asunder in the south-south-east, with a strongly luminous fire rain, the fire drops of which, however, were extinguished before they reached the horizon. In Karlskoga a fireball of a blinding clear reddish white lustre was seen high up in the zenith. Hence it sank towards the north-north-west to a height of 30°, and afterwards parted into three or four smaller pieces, which speedily went out and resembled the stars which fall from a rocket. The meteor left behind it a white smoke, which in the calm air remained in the direction of the fall about a minute, and afterwards dispersed. In the neighbourhood of Karlstad, the meteor was thought to fall in the north east. It was compared to a falling star rocket. It was very bright, with a white nucleus, having fire-red edges, and passing when bursting asunder to a blinding white, the separate pieces being clearly visible. Its apparent size was compared to that of the full moon, and after its disappearance a white streak remained for some seconds in the sky. In Hoböl parish in Dalsland there was seen in the sky a pointed fire-ball, resembling in form a soda-water flask, at first pretty high in the heavens, afterwards approaching the earth, dividing into two parts and disappearing without any detonation after the lapse of half a minute. At Lysekil the meteor appeared to fall perpendicularly in the north-west, and spring asunder without any noise some few feet above the surface of the water. According to a statement in the newspapers the meteor in question was simultaneously seen at Christiania. In Denmark and Finland it was not visible.

From a careful and critical examination of these statements, and many others which have been collected, it appears that the meteor in question, possibly with the neighbourhood of $\gamma$ Cephei as radiation point, proceeded in a somewhat oblique direction to the place where the stones fell on the meteor bursting asunder. If with a point 40 kilometres south of Ställdalen as a centre, a circle be described through Christiania, the westernmost place where the phenomenon was observed, its circumference intersects Orust in the south, the neighbourhood of Stockholm in the east, and Gefle in the north-east, and includes all the places where the meteor was visible. At Stockholm, Hedemora, Karlskoga, and Lysekil, the meteor is said to have been visible first in the north-east, somewhat below the zenith, and if the direction is noted where it disappeared in the neighbourhood of the horizon, this direction in general corresponds very well with the direction from the place of observation to the place of fall. The meteor thus went under the horizon or disappeared in its neighbourhood at Stockholm to west, at Gefle to south-west, at Mora to south-south-east, at Lysekil to north, at Malmköping to north-west, and so on.

The meteor thus reached the end of its short luminous path in the region where the fall took place. It became luminous at a height which cannot, after making allowance for errors of observation, be reckoned at less than 300 to 400 kilometres, but was probably greater. At this height the atmosphere, notwithstanding its extreme tenuity, is capable by its resistance of heating red hot a body moving with *cosmic* speed, as of 75 kilometres per second, and if the composition of the atmosphere at this height be the same as at the surface of the earth the meteor will meet with sufficient oxygen to maintain a lively combustion of the combustible matters which enter into the composition of the meteor. It appears to me that we have here an explanation of the considerable height in the atmosphere at which meteors first become luminous—an explanation which is so much the more probable as we now not only know a number of carboniferous meteorites, but also by the meteorite fall at Hessle, in Upland, have distinct proof that the common meteorites may be accompanied, and perhaps are generally accompanied, by an easily combustible carboniferous dust. Only through such a supposition can we obtain an explanation of the large size of these meteors when compared with the stones which fall, as well as of their strong illuminating power, which clearly shows that the light arises from the glowing of solid masses, and not merely from the compressed and heated gases which the meteor has collected before it.

The statements regarding the size of the Ställdalen meteor are very various. The most probable are those which give it a diameter of six minutes, which, supposing the distance to be 250 kilometres, would give the fireball a diameter of 436 metres, or nearly 1,500 feet. In comparison with this size the stones that have fallen are surprisingly small, which yields a further support to the supposition that the main mass of the meteor consisted of substances which had already high up in the air been dissipated in the form of gas or undergone combustion. In the case of the Ställdalen meteor there is also the exceedingly remarkable circumstance that the fireball was not visible in the region where the path of the meteor struck the earth and where the meteorites fell, although this place lay nearly in the centre of the area where the meteor was visible as a luminous fireball, and although the sky here too was clear and cloudless with the exception of the little dark cloud which the meteor collected before it in its path through the air. It was probably this cloud which prevented it from being seen in the region which lay in the direction of the fall. Although no fireball was seen here, loud detonations were heard and some light streaks of cloud were visible in the zenith, from which, according to some, faint flashes of fire

[1] Abstract of an Address by Prof. Nordenskjöld at the Anniversary of the Royal Swedish Academy of Sciences.

resembling lightning were seen darting. Whistling, rumbling, and rattling noises were also heard. The sound was thought, for the most part, to come from the west or south-west. It was not heard in Karlskoga, which lies to the south, but far to the north and north-west. At Falun it was supposed that a fall of rock had taken place in a mine, and at Grandgrufvan, at Ludvika, the sound was heard as of a peal of thunder at a depth of sixty metres underground. At other places a dynamite magazine was thought to have exploded, or it was taken for a loud clap of thunder.

In the neighbourhood of a workman who was cutting trees in a wood several branches of a tree were broken off by a stone weighing nearly a kilogram, in a way which clearly showed no great falling velocity, which was further confirmed by the stone making a hole in the ground only a decimetre in depth. Another person saw a stone fall close beside him, and immediately took it up. It was not at all warm. A girl saw a stone weighing two kilograms fall to the ground "so that the earth smoked." Several fell in the Lake Björken or were picked up in the neighbourhood soon after. One weighing 8½ kilograms fell in a rye-field. In falling it had gone in two pieces and made an eight-inch deep hole in the cultivated soil. The largest stone weighed 12½ kilograms.

The number of the stones that have been found, however, amounts only to eleven, with a total weight of thirty-four kilograms. They were scattered within an oval two kilometres broad, whose larger axis had a length of eight kilometres. The largest stone was found in the south-west end of the oval, in a meadow surrounded by wood. It is probable that larger stones have fallen farther into the wood, and thus escaped observation. The stones are of very irregular form, and on their surface are full of the depressions peculiar to meteorites. On the surface they are, as usual, covered with a blackish fused crust of very variable thickness, being so thick on some of the fractured surfaces as to completely conceal the colour and inequalities of the main mass, and on other similar surfaces so thin that the colour and crystalline structure of the main mass may be clearly distinguished. Sometimes the crust is completely wanting, so that the surface of the stone, with the exception of an inconsiderable blackening, resembles a fresh fracture. The stones are thus fragments which have been formed at different times, and exposed for different periods to the action of the glowing envelope. The largest stones are covered in many directions with black friction surfaces which are more clearly marked on these meteorites than on any I know. These too have probably been formed in our atmosphere, and show that with the great pressure produced by the resistance of the air, cracks have been formed in the meteorite along which its different parts before springing asunder rubbed against each other during the rotation of the irregularly-formed mass, whereby the uneven surfaces have been smoothened, and coloured black by the heat developed during friction, the projecting metallic particles flattened, &c. On breaking in pieces the meteorites in question, they are found to consist of a coarse breccia-like mixture of grey and of nearly black portions, little differing from each other in chemical composition. It is remarkable that the grey mass when heated becomes dark, and thereby in appearance quite like the black, which appears to show that some of the breccia-like pieces found in the stones had been heated, while this does not appear to have been the case with the other part. Different pieces of the Ställdalen meteorites thus appear to have been exposed to the action of very different temperatures before they were united into the mass, hard, tough, and difficult to break up, which formed the meteorite.

The stones that fell at Ställdalen have been carefully analysed by Mr. G. Lindström, assistant in the mineralogical department of the Riks Museum, who found them to consist of nickel-iron; a silicate decomposed by acids, chiefly olivine; a silicate indecomposable by acids, probably bronzite; magnetic pyrites, and inconsiderable quantities of phosphide of nickel-iron; of a phosphate, and of chloride of iron. The first-named substance, a metallic alloy of ninety per cent. iron and ten per cent. nickel, is not known of terrestrial origin, but distinguishes most meteorites, and makes it possible to separate with certainty the meteorites which have fallen at Ställdalen from all other minerals occurring in the quarter. The two other main constituents again, olivine and bronzite, are also wanting in our granites, gneisses, and common slaty rocks, but are found commonly entering into the composition of a number of rocks which by most of the geologists and mineralogists of the present day are considered to be of plutonic origin. Many circumstances, however, indicate that these rocks, which in remarkably regular layers cover extensive regions of the earth's surface, *often, but not always*, consist of stratified tuff-like formations which during the enormous duration of geological periods have assumed a crystalline structure. The resemblance between them and various constituent parts of the meteorites is so striking that the question must be seriously and impartially discussed whether a part of the plutonic rocks are not of cosmic origin. By this I mean that it gradually fell to the earth even after its surface formed an abode for animals and plants, and that under favourable circumstances it collected so as to form proper stratified so-called plutonic rocks, in which, through subsequent chemical changes, so great a development of heat has sometimes taken place that volcanic and plutonic incandescent craters have arisen in the interior of the earth.

Many observed facts may be quoted in support of this view, if it for the present appears very strange on account of the great changes it would bring about in the prevailing ideas of the history of the formation of the heavenly body which we inhabit. We have perhaps here the true solution of the many disputed questions raised by the discovery of meteoric iron at Ovifak, in Greenland, a simple explanation of the abundant occurrence of magnesia in certain geological formations, and of many other geological phenomena difficult of explanation according to theories now prevalent.

## UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Mr. W. N. Shaw, B.A., Emmanuel College, 16th Wrangler, 1876, and 1st Class Natural Sciences Tripos (Distinguished in Physics), 1876, has been elected to a fellowship in his College.

LONDON.—The following have passed the recent examination for the degree of Doctor of Science in the branches specified:—

Branch IV.—Inorganic Chemistry.—J. M. H. Munro, Royal College of Science, Dublin.

Branch VI.—Electricity (treated experimentally).—O. J. Lodge, University College.

Branch VIII.—Physical Optics, Heat, Acoustics (treated mathematically).—J. F. Main, Trinity College, Cambridge.

Branch X.—Comparative Anatomy.—A. M. Marshall, B.A., St. John's, Cambridge, and St. Bartholomew's Hospital.

Branch XIV.—Geology.—W. Saise, Royal School of Mines.

## SOCIETIES AND ACADEMIES

### LONDON

**Geological Society**, June 20.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Messrs. George Alexander Gibson, Henry P. Gurney, John Higson, and Francis Stevenson, were elected fellows of the Society.—The following papers were read:—On a hitherto unnoticed circumstance affecting the piling up of volcanic cones, by R. Mallet, F.R.S.—The steppes of Southern Russia, by Thomas Belt, F.G.S.—The glacial period, by J. F. Campbell, F.G.S.—The action of coast-ice on an oscillating area, by Prof. John Milne, F.G.S., of the Imperial College of Engineering, Tokio, Japan.—On points of similarity between zeolitic and siliceous incrustations of recent formation by thermal springs and those observed in amygdaloid and other altered volcanic rocks, by Prof. A. Daubrée, F.M.G.S.—On the cretaceous Dentaliadæ, by J. S. Gardner, F.G.S.—On a number of new sections around the estuary of the Dee which exhibit phenomena having an important bearing on the origin of boulder-clay and the sequence of glacial events, by D. Mackintosh, F.G.S.—Discovery of silurian beds in Teesdale, by W. Gunn, F.G.S., and C. T. Clough, F.G.S., of H.M. Geological Survey.—On the superficial geology of British Columbia, by George Mercer Dawson, F.G.S., Assoc. R.S.M., of the Geological Survey of Canada.—The exploration of the ossiferous deposit at Windy Knoll, Castleton, Derbyshire, by Rooke Pennington, F.G.S., and Prof. W. Boyd Dawkins, by Prof. W. Boyd Dawkins, F.R.S.—Description of the fossil organic remains from Bendigo, by M. Carl August Zachariæ, communicated by the president.—Notes on some recent discoveries of copper ore in Nova Scotia, by Edwin Gilpin, F.G.S.—Glacial drift in the North-eastern Carpathians, by R. L. Jack, F.G.S., and John Horne, F.G.S., of the Geological Survey of

Scotland.—On terminal curvature in the south-western counties, by W. A. E. Ussher, F.G.S., of H.M. Geological Survey.—On the chronological classification of the granitic rocks of Ireland, by G. H. Kinahan, M.R.I.A., communicated by Prof. Ramsay, F.R.S.—The Cambrian rocks of South-east Ireland, by G. H. Kinahan, M.R.I.A., communicated by Prof. Ramsay, F.R.S.

PHILADELPHIA

**Academy of Natural Sciences.**—A valuable list of the fresh-water fishes of Northern Indiana, by Dr. D. S. Jordan, is published in the *Proceedings* for 1877, with remarks on many forms of novelty or interest. This is followed by a critical account of the genera of North American fresh-water fishes, by Dr. Jordan and Mr. C. H. Gilbert. One list gives the whole of the genera in the order of their original description, with full references.

VIENNA

**Imperial Academy of Sciences**, April 19.—Contributions to the cosmic theory of meteorites, I. Proof of identical meteorite paths, by M. Niessl. Two detonating meteorites, on April 10, 1874, in Bohemia, and April 9, 1876, in Hungary, had apparently the same point of emergence, and observations gave for both a velocity corresponding to a hyperbolic path.—On the action of alcoholic caustic potash solution on ether-like nitro-bodies, by MM. Hess and Schwab.—On the application of the microscope to quantitative determinations, by M. Jonstorff.—On the history of creation of our planetary system, &c., by M. Sedlitschka.—On some remarkable phenomena in Geissler tubes (fourth paper), by MM. Reitlinger and Urbanitzky. Seeking the causes and laws of the repulsions and attractions, they experimented with various gases rarefied in Wüllner's cylindrical tubes (without capillary part), noting simultaneously with a multiplier the changes in the induced current. They were led to the conviction that it is a case of reciprocal action between accumulation of static electricity on the approximated conductors and current electricity in the tubes, and that the chemical character of the gases has a great influence on the apparent progress of the phenomenon.

April 26.—On iron cyanide compounds, by M. Skraup. This relates to superferrid-cyanide of potassium.—On a new derivative of sulpho-urea, sulphydantoinic acid, by M. Maly.—Theory of circular polarisation, by M. V. Lang.—On *Phymatocarcinus speciosus*, Reuss, by M. Bittner.—A geological profile from Osmanich am-Arcer, on the Sveti Nikola-Balkan, to Ak-Palanka, on the Nisava, by M. Toula.

PARIS

**Academy of Sciences**, July 9.—M. Peligot in the chair.—The following papers were read:—On the alcoholate of chloral, by M. Wurtz. The dehydratation of crystallised oxalate of potash occurs in vapour of the alcoholate as easily as in air; not so in vapour of hydrate of chloral (proving that the latter contains water).—Reply to M. Roudaire's last note on the Algerian inland sea, by M. Naudin. He insists specially on the erosive force the current would have both in its primary state and in time of flooding. The troubled water of the coast, too, would enter and deposit much sediment.—On electric transmission through the ground by means of trees, by M. Du Moncel. Trees are all, more or less, conductors, their conductivity depending on the quantity of liquids in them. The roots act as electrodes. The resistance of a tree, commencing with its leaves, and supposing contact only with a few of them, varies from 200,000 to 400,000 kilometres of telegraph wire (in round numbers). That of the trunk, at a height of 7 to 8 m. hardly exceeds, in strong trees, 3,000 kil. in connection with the ground, and varies from 2,000 to 7,000 kil. between small metallic electrodes. Thus, contact of telegraph wires with leaves need not give much anxiety. The resistance of ordinary houses being about sixteen to twenty times that of trees, the latter, if not under the former in height, may be considered a protection, but as rain usually falls in thunderstorms and diminishes the difference of conductivity between trees and house, a protective effect of trees may only lie in their superior height.—Treatment by sulphocarbonates of vines of Orleans and Saint Jean-le-Blanc, by M. Gueyrand.—On the quasi-circular movements of a point subject to the attraction of a fixed centre, by M. Boussinesq.—On the diamagnetism of condensed hydrogen, by M. Blondlot. Palladium charged with hydrogen M. Blondlot finds to be *less* magnetic than palladium uncharged; which accords with the facts that palladium is weakly magnetic and hydrogen diamagnetic. Graham's opposite experience is thought due to some disturbing cause, probably impurity of the acid used in charging the palladium by means of electrolysis; the least trace of a ferruginous body gives a deposit on the palladium, which would explain Graham's results.—Photometric researches on coloured flames, by M. Gouy. If the quantity of salt introduced into the flame be doubled, the increase of brightness of each line is at most equal to what would be produced by doubling the thickness of the flame, and it is nearly always inferior.—On a new metal, *davyum*, by M. Kern (see p. 236).—On the oxidability of sulphide of manganese, by MM. De Clermont and Guiot.—On a new general method of synthesis of hydrocarbons, acetones, &c. Third note by MM. Friedel and Crafts.—Action of bromine on pyrotartaric acid; second memoir by M. Bourgoin.—On the determination of carbonic acid in blood serum, by M. Fredericq.—Researches on bitter almonds, by M. Portes. Young bitter almonds contain amygdaline; they have always a different composition from sweet almonds; the embryo alone contains emulsine, and it appears pretty late; the amygdaline is localised in the teguments of the seed; its origin is still unknown; by degrees it quits the teguments and penetrates into the cotyledons by the radicle.—On the nickelised iron of Santa Cattarina, by M. Lunay.—On some physiological facts observed in Drosera, by M. Ziegler. It has been observed that Drosera is sensible to the physical action of salts of quinine after excessive indirect animal contact. Many other bodies have this property, and among them is *urea*. Like quinine, urea does not cause any action in normal droseras, but on being united with certain other bodies, it produces contraction (*e.g.*, granules made of a mixture of urea and iron with white wax give contraction; but granules of wax with urea alone, or with iron alone, have no effect).—Comparative study of cupric preparations introduced into the stomach and the blood, by MM. Feltz and Ritter. Insoluble albuminate of copper ingested into the stomach in considerable quantity has hardly any effect; soluble albuminate causes disorders at least as grave as the ammoniacal sulphate in distilled water. Sulphate of copper dissolved in syrupy glycerine is much more poisonous than in aqueous glycerine.—Treatment of rheumatism, gout, and various nervous states, with salicylic acid and its derivatives, by M. Sée. It seems beneficial in some cases.—On testing for salicylic acid, by M. Marty.—On external use of salicylic acid, by M. Grellot.—The advantages of immediate and early trepanations, by M. Gross.

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