THURSDAY, NOVEMBER 15, 1883

THE "AUSTRAL" JUDGMENT

THE inquiry into the sinking of the mail-steamer *Austral* in Sydney Harbour has probably attracted more attention than any other case which has come before the Wreck Commissioner's Court since it was established. Not merely those specially interested in or connected with shipping but the public generally were desirous of knowing how it happened that a magnificent steamship of the most recent construction should have foundered at anchor in smooth water and in a dead calm. It is satisfactory, therefore, to find that the causes of the accident have been discovered, and that they do not affect the reputation of the ship, nor the credit of her designers. The circumstances of the accident are briefly these :—The *Austral* had completed her second outward voyage, had discharged nearly all her cargo, and had partially refilled her coal-bunkers. A collier came alongside to continue the coaling, and the work was proceeded with during the night. In order to facilitate coaling, and to keep the interior of the ship clean, coal-ports had been formed in the sides, the height of these ports above water when the ship was upright being about five feet. The coaling was rapidly done, and no proper supervision was exercised by any of the officers of the ship; consequently a considerable weight of coal was introduced on the starboard side without any corresponding weight being placed on the port side, and the ship was gradually heeled over. At length such an inclination was reached that the sills of the after coaling-ports were brought to the sea-level; water began to enter the ports and to pass freely into the interior of the ship, and in fifteen to twenty minutes from the time the alarm was given she sank. No one appears to have observed the dangerous proximity of the coal-ports to the water until it was too late to save the vessel. Had there been ordinary care and watchfulness the accident would not have occurred.

This last statement can be made with certainty in view of the scientific evidence respecting the stability of the *Austral* given in the course of the inquiry. After the vessel had been raised and brought home the owners commissioned a competent naval architect, Mr. Elgar, to thoroughly investigate her conditions of stability at the time of the accident, and under various circumstances. As a basis for this investigation an inclining experiment was made on the vessel, and the vertical position of her centre of gravity was ascertained. Simple calculations enabled the investigator to pass from the experimental condition of the ship to all other conditions brought under review, and to place before the Court ample materials for answering the question—Was she a stable vessel? This answer was distinctly in the affirmative; indeed there is no room for doubting that with proper management, and the occasional use of the water-ballast with which she was provided, the *Austral* possessed sufficient stability. It is unnecessary to enter into details as to her "stiffness" and range of stability in various conditions of lading; but it may be worth stating that, according to the evidence, had the coal-ports been closed and all weights on board secured, she would have been practically *uncapsizable* at the time of the accident.

It would be out of place here to discuss the finding of the Court as regards the responsibility or blameworthiness of the owners, officers, and other persons connected with the management of the ship. One broad general principle laid down by the Commissioner in his judgment may be considered with propriety, since it affects not merely the owners of the *Austral*, but shipowners as a body. Mr. Rothery is strongly of opinion that shipowners should cause investigations to be made of the stability of their ships, and should furnish captains with the results of these investigations for information and guidance. In the case of the *Austral* no such investigations were made until after the accident, and what happened with her is the common case with ships of the mercantile marine. There has been a remarkable advance in the applications of scientific methods to merchant-ship construction of late years, and the consideration of problems of stability has been forced upon the attention of shipbuilders and shipowners in many cases. But the adoption of the Commissioner's opinion would involve a much greater extension of scientific method and exact calculation than has yet taken place.

Shipbuilders necessarily have no control over the loading of the ships they build; and in most merchant ships the stability is practically determined by the nature and distribution of the cargoes carried. Up to the present time exceedingly little information is on record as to the actual stability of laden merchant ships; and their loading usually has to be done under very hurried and difficult conditions by men possessed of great practical experience, but having little or no acquaintance with the principles of stability. Owners have hitherto been content to depend almost exclusively on experience with previous vessels in determining the dimensions of new ships, and have not set much store on the result of scientific calculation. Builders, on the other hand, recognising their want of control over the working of the vessels, have refrained, for the most part, from making detailed calculations of stability. Even the leading firms have chiefly confined attention to experimental and other investigations which would be useful in preparing subsequent designs; and in most cases the owners have not had communicated to them any facts which may have been ascertained respecting the stability of ships. Mr. Rothery maintains that all this should be changed: that fuller investigations should be universally made, and the results furnished by the owners to the captains.

The great, if not paramount, importance of due consideration being given to the stability of merchant ships, and particularly of cargo-carrying steamers, is recognised by the most eminent authorities. Mr. Rothery in his recommendation indorses what has been said and written repeatedly of late years. But while there is a very general assent to the proposition that something should be done to secure a due amount of stability and to prevent improper or excessive loading, there is not a similar agreement respecting the means to be employed. For example some of the professional witnesses at the *Austral* inquiry expressed doubts as to the wisdom of placing in the hands of merchant-ship captains the results of calculations for stability expressed in the forms of "metacentric diagrams" or "curves of stability." These gentlemen feared that to the average ship-captain such curves and diagrams would be unintelligible, and therefore of no practical value. It must be admitted that there is some force in this contention; but on the other hand it is obvious that a very moderate amount of instruction ought to suffice to make information of the kind intelligible and useful to an educated seaman.

It may be worth while to mention what is the established practice in the Royal Navy in this matter. Each of Her Majesty's ships is provided with a "Statement of Stability," in which appears a record of the "metacentric heights," corresponding respectively to the "fully laden" and the "extreme light" conditions of the vessel. There is also a record of the calculations of stability at various angles of heel; the angle at which the stability attains its maximum and that at which it vanishes being noted. In cases where special precautions are needed special standing orders are given. For instance, in some low freeboard ships it is stringently ordered that a certain maximum load draught shall not be exceeded, because any diminution of the corresponding freeboard would cause an objectionable decrease in the range and area of the curve of stability. Again, in some vessels, as coals and stores are consumed, the stability is considerably diminished, and then orders are given that the ship shall not be lightened beyond a certain minimum draught, that draught being maintained if necessary by the admission of water-ballast. All these regulations are based upon careful experiment and detailed calculations. In the original design of the ships close attention is bestowed upon the question of their sufficient stability; and when the vessels are completed, an experimental check is put upon the intentions of the design, any necessary corrections being made in the original calculations. But it is right to remark that war ships are much more easily dealt with than merchant ships, because definite positions are assigned in them by the designer for all the weights carried—whether they be armour, or guns, or coals, or ammunition, or outfit. Hence it is possible to state distinctly what is the stability in the fully laden condition, and what are the extremes of possible variations in stability as coals, stores, &c., are consumed. In merchant ships, as was remarked above, the designer and builder have no corresponding control over stowage, and in practice very considerable variations in stowage necessarily occur. Leaving this difference aside for an instant, it may be stated that in the Royal Navy the information given on "Statements of Stability" is highly valued and well understood by naval officers. This result is, no doubt, attributable in a large degree to the fact that at the Royal Naval College for many years past classes have been arranged wherein naval officers receive instruction in the elements of naval architecture, and especially in the methods of interpreting the various statements and drawings issued by the Admiralty to the ships of the fleet. Similar instruction could not fail to be of service to officers of the mercantile marine, and the Admiralty have made provision in the Regulations for the admission of a certain number of such officers annually; but as yet no advantage has been taken of the permission. Either in this way or in some other, instruction must be obtained by merchant captains if they are to exercise an intelligent control over the loading of their vessels, and to insure the provision of sufficient stability.

It seems very probable that one result of recent occurrences and discussions will be the grant of greater freedom to shipbuilders in choosing the dimensions for new ships than has been customary hitherto. And it may be anticipated that increasing attention will be bestowed upon investigations of stability in connection with new designs. But whatever improvements may be made in the general practice of shipbuilders, the responsibility for management and loading must always remain with the owners and commanding officers of merchant ships. Ill-advised action on their part might render futile all the precautions of the designer. He may have secured what seems a good margin of stability, on the basis of some hypothetical arrangement of a certain dead weight which was supposed to be the maximum a ship would carry; and yet in practice some more critical condition of loading may arise which must be dealt with by those in charge of the vessel.

Having regard to the very considerable variations in the character of the cargoes carried by the great majority of merchant ships on their several voyages, it appears to be highly important that owners and captains should have placed in their possession full information respecting the stability of their ships; and that they should be able to make intelligent use of this information. One of the most valuable pieces of information which a captain could obtain for a laden ship would be her "metacentric height," and there seems no reason why an intelligent officer who had been furnished with a "metacentric diagram," and understood its use, should not experimentally determine for himself before leaving port what measure of "stiffness" his ship possessed, and at what vertical position the centre of gravity was placed (if the conditions of loading were of an unusual character). He would then have a more certain assurance of the sufficiency or otherwise of the stability of the ship than he could otherwise possess; and this assurance might easily be made to extend not merely to the initial stability but to the stability at large angles of inclination. It may be urged that it is too much to hope for any such experiments, or for such an advance in knowledge; and that in the stress of business time cannot be found for such elaborate inquiries. Possibly one may be too sanguine to indulge this hope; but inclining experiments of the kind indicated are neither lengthy nor costly operations, and their value as indications of the probable safety or danger of laden ships cannot well be over-estimated.

The necessity for carefully considering the stability of merchant ships is not a matter of dispute. All concerned may be assumed to desire some practical solution of the problems involved in securing sufficient stability. And on a review of the whole subject it will probably be admitted that all three classes interested—the shipowner, shipbuilder, and ship-captain—must accept their several responsibilities while working towards a common end. The shipowner may be presumed to know best the special requirements to be fulfilled in any new design. It is the duty of the designer to make sure that appropriate dimensions and proportions are secured in association with the fulfilment of these requirements, or to point out the impossibilty of such an association. And, finally, upon the skilful and intelligent conduct of the captain must necessarily depend in a great degree the safety and success of the vessel during her career. In order that the best results may be obtained in face of the difficulties incidental to the design and management of many modern types of ships, the standard of knowledge must be raised in all three classes. W. H. WHITE

THE "ENCYCLOPÆDIA BRITANNICA"

Encyclopædia Britannica. Ninth Edition. Vol. xv. Loo-Mem. Vol. xvi. Men-Mos. (Edinburgh: A. and C. Black, 1883.)

AMONG the most important scientific articles in vol. xv. of the new edition of the "Britannica" are those on Medicine, Mechanics, and Mammalia.

The concise but comprehensive epitome of the history of medicine which Dr. Payne has contributed is the only history of the kind in the language. In Germany there are in this subject, as in almost every other branch of learning, excellent text-books; and the author acknowledges his obligations to Häser's "Lehrbuch der Geschichte der Medicin und der epidemischen Krankheiten." In France, Daremberg's "Histoire des Sciences Médicales" is also well known. But in England there has been no serious attempt to write a history of medicine since the publication of Freind's letters to Mead (1725); even these only dealt with a portion of the subject, and were written or at least begun under the disadvantage of confinement in the Tower. There have been a few valuable contributions to the subject, such as Dr. Greenhill's articles in Smith's "Dictionary of Classical Biography," and Dr. Munk's Roll of the College of Physicians, but nothing more.[1]

Is this neglect justifiable? In other branches of natural history and natural philosophy an acquaintance with the successive steps by which modern knowledge has been won is almost necessary for clearly comprehending the result. A history of astronomy, of electricity, or of physiology would be not only of interest but of practical value to the student of each of these subjects. But a history of medicine, however important as a chapter in the development of human intellect and the progress of civilisation, is scarcely any help towards understanding either the principles or the practice of the art of healing. A modern physician finds some knowledge of chemistry and of physics indispensable; botany and zoology are not without important bearing on his professional studies; a knowledge of German is of great practical use; but he may be ignorant of all medical literature above fifty years old without any loss, except the loss of the intellectual pleasure which every educated man should take in the past history of his profession.

That this is the case seems evident from the utter neglect of the older medical classics in medical education, notwithstanding occasional murmurs from the few who have earned the right to murmur by having read them, and from others—a neglect which exists not only in practical England and America, but no less in the learned German and the conservative French schools. This neglect is only confirmed by occasional glimpses of the said classics, and it is illustrated by the fact that we owe even the sketch of the labours of two thousand years which forms the subject of this review to the demands of an encyclopædia.

Nor is the reason far to seek. Modern medicine has scarcely anything but its aim in common with the art of the ancients. The attempt of the older physicians was to find some comprehensive explanation which would account for all the diseases of mankind, and their practical method was the application of certain remedies, recommended by the crudest experience, or more often by some such dogmatic criterion as that of "signatures." The authority of the ancients was regarded as independent of proof. In like manner naturalists used to study the worthless gossip of Pliny, and Milton recommended Columella as a school-book because of the practical importance of husbandry; indeed in England we still teach geometry from an ancient Greek text-book, and Euclid will be the last to follow Aristotle and Galen, Dioscorides and Celsus, into learned oblivion. But the object of modern medicine is not to explain but to investigate, to ascertain what is amiss, and to deal with it as directly as possible, on the principles of physics and of chemistry, guided by experiment and checked by skilled statistics. Homœopathy is only the last of the "systems" of medicine; not more arbitrary than many others, and, like the rest, not so much a wrong solution of a scientific problem as an answer to a question which cannot reasonably be put.

The art of rational medicine must therefore depend upon a knowledge of the body and its functions, on the power of discovering its physical conditions, and on acquaintance with the physico-chemical laws to which it is subject; just as the art of navigation depends on a knowledge of astronomy and of meteorology. But even the rough outlines of anatomy were only made out during the sixteenth and seventeenth centuries, and the discovery of its minuter details, so well begun between 1650 and 1700, was only resumed and carried to its present degree of completion by the achromatic microscopes of the last fifty years. Morbid anatomy dates from Morgagni. Physiology had no true existence before Harvey's discovery of the muscular contraction of the heart and the circulation of the blood in 1628. It was retarded rather than helped by premature application of mechanical laws, and did not make important progress again until the birth of chemistry in the last thirty years of the eighteenth century. If anatomy may be dated from the dissections of Vesalius, physiology from the vivisections of Harvey, and chemistry from the laboratory of Lavoisier, we cannot fix the beginning of modern medicine earlier than the introduction of mediate auscultation by Laennec in 1819.

Interest, however, will always belong to the history of medicine, apart from the practical value of the older medical literature. The study of the dreary succession of the Greek "sects," of the Galenical and Arabian "schools," and of the subsequent iatro-chemical, iatro-mechanical, Brunonian, and other "systems," is of service to warn too eager speculation from the errors of

[1] Dr. Edward Meryon's "History of Medicine" was never finished. Dr. Adams's editions of Hippocrates and of Paulus Ægineta, Croke's of the "Regimen Sanitatis Salernitanum," and Payne's of Linacre's translation, "De Temperamentis," are scholarly works "Lives of British Physicians" and "The Gold-headed Cane" are not ungracefully written. "The History and Heroes of the Art of Medicine" is a very poor compilation. A brilliant essay on the subject will be found at the end of "Poems" and other remains of the late Dr. Frank Smith (Smith and Elder, 1879).

past ages. Here and there, "apparent rari, nantes in gurgite vasto," records of real observation: the aphorisms of Hippocrates, or the clinical pictures of Sydenham. Occasionally a good style commends an almost valueless treatise, as in the case of Celsus and Fracastori. More often we are attracted by some amusing gossip, some shrewd remark, or some interesting historical allusion, to epidemics or to wars, to the deaths of kings and conquerors, or to the daily accidents of contemporary life. Such are Caius's account of the sweating sickness, Ambrose Paré's description of his treatment of gunshot wounds in Savoy and at Rouen, and the "cases" recorded by Dutch surgeons of the seventeenth century. Nay, apart from utility and from such chance rewards as these, there will always be those who take the genuine delight of a book-worm in old authors because they are old, those who have the respectable appetite for information which is omnivorous, and students of the human mind for whom acquaintance with its dullest wanderings is fruitful.

It is therefore well that English readers should have at least an outline of medicine in the past, and this want has been admirably supplied by Dr. Payne. Wisely abandoning all endeavours to include the biographical part of his subject, tempting as the excursion must often have seemed, and leaving on one side the curious history of medicine as a profession, its connection with the Church, the differentiation of its several branches, its varied social position, and the growth and decay of the great colleges and schools of medicine, he has aimed only at presenting within the narrow limits allowed (about thirty-seven columns quarto) a view of the changes of medical theories, and of the slow progress and frequent retrogression of the medical art. Beginning with an appreciative sketch of Hippocratic medicine, the important work of the Alexandrine physicians is next indicated, the scientific scope and character of Galen is described, and the obscure line of tradition of classical medicine is traced down to the mediæval school of Salerno. The vast, but thankless and little explored, field of Arabian medicine is then rapidly surveyed, and its dominion in Western Europe explained as being really little more than that of a corrupt Galenism. The revival of learning at the beginning of the sixteenth century was probably a misfortune to medicine, for when the Italian scholars, and our own Linacre and Caius translated the works of Galen into good Latin, these medical "classics" shared in the glory which surrounded the language of the New Testament and of Plato. The first steps of anatomy were in contradiction of statements by Galen, the first discovery of physiology was a refutation of his whole system. Yet the baneful influence of his great name, like that of the still greater name of Aristotle, lasted long after his claim to implicit credence had been disproved. As the ancient system was worn away, its place was eagerly striven for by the feebler systems of Paracelsus, Van Helmont, Borelli, Sylvius, Stahl, Hoffmann, John Brown, and Hahnemann in a long succession of three hundred years.

With the morbid anatomy of Morgagni, Baillie, and Laennec, and the physical diagnosis introduced by the latter great physician, the modern era of rational medicine began, in which sects and systems are mere survivals—superstitions—of an unduly prolonged middle age. At this point Dr. Payne's heart and paper seem to fail together. He ends, much as Gray's bard ended his prophetic outline of English history, in a fine confused view of a period of light and splendour, illustrated by the names of Rokitansky and Virchow, Czermak and Helmholtz, Bright, Graves, Addison, Stokes, and Trousseau. It was no doubt wise not to attempt an account of the triumphs of the new era, but we hope that the learned author of this article may make it the foundation of a complete history of medicine, fuller and more exact than Daremberg's, lighter and brighter than those of Sprengel and Häser. We also venture to suggest to the editor of the "Encyclopædia Britannica" that an article dealing with the curious and interesting history of medicine as a profession should be obtained from the same pen, under the heading, say, of "Physic, History of the Practitioners of."

We have scarcely left room for finding fault, and little room is needed. But to redeem our encomium from the charge of blindness, we may ask why the history of the school of Salerno is given after that of Arabian medicine; what evidence there is apart from his name that Bernard Gordon of Montpellier (1307) was a Scot; and what possible aptitude there is in a comparison between two such different persons as the impudent, drunken vagabond who called himself Paracelsus and the great German reformer who lived at the same time.

Lastly, while we fully admit the justice of connecting the introduction of auscultation and of chemical and microscopical examination of morbid fluids with the introduction of a knowledge of morbid anatomy—for this connection was, in fact, the *novum organum* of medicine from 1820 onwards—yet we think that there should also have been indicated, however briefly, the still newer method which has characterised the history of yet more recent medicine, namely, the method of number and measurement, by which to the stethoscope and the test tube have been added the clinical thermometer, the compte-globule and the sphygmograph. Perhaps future historians of medicine (particularly if they should write "primers" or "outlines" "for examination purposes") will divide the nineteenth century into four periods: the first (1800-1820) introductory, the second (1820-1850) the period of morbid anatomy and of physical diagnosis, the third (1850-1880) the period of morbid histology and of quantitative investigation; while the last, we may hope, will be called the period of experimental medicine, in which laboratories shall do the same service for pathology and therapeutics which they have already done for physiology.

There appears, under the head of "Mechanics," another of those mathematical dissertations which, each complete in itself, are to be found at such frequent intervals in the volumes of the new edition of the "Britannica." The author of the part of this article which treats of theoretical mechanics is Prof. Tait, and those who are familiar with his writings will be able to form an estimate of the way in which the treatment of the subject is conceived and carried out.

The science of mechanics in its widest range rests on Newton's Three Laws of Motion, and on that other passage in the "Principia" dealing with the activity of an agent, the full significance of which, when interpreted by the light of modern discoveries, was first made clear by Professors Thomson and Tait. An examination of Newton's original statement shows that in his view "equilibrium is not a balancing of forces, but a balancing of the effects of forces. When a mass rests on a table, gravity produces in it a vertically downward velocity which is continually neutralised by the equal upward velocity produced by the reaction of the table, and these forces . . . are equal because they produce in equal times equal and opposite quantities of motion."

As regards our knowledge of force as distinguished from its mechanical measure as change of momentum, we are reminded that our idea of force, originally derived from the muscular sense, "may be a mere suggestion of sense corresponding (no doubt) to some process going on outside us, but quite as different from the sensation which suggests it, as is a periodic shearing of the ether from brightness, or a periodic change of density of air from noise."

In discussing still further the nature of force, Prof. Tait points out that our belief in matter, the most certain of all objective realities, is largely based on the property of the unchangeability of its aggregate amount. "The only other thing in the universe which is conserved as matter is conserved is energy. Hence we conclude that energy is the true physical reality, and force, which is merely the space-rate at which energy is transformed, must be regarded like other expressions, such as rate of interest, death-rate, gradient of heat, as an expression introduced for convenience, and not necessarily because of an objective reality attached to it."

Remembering the dual nature of all force as being exerted between two bodies, we have, as another reading of the Third Law, "Every action between two bodies is a stress."

With regard to potential energy, which must depend in some hitherto unexplained way, like kinetic energy, on motion, Prof. Tait says: "The conclusion which appears inevitable is that, whatever matter may be, the other physical reality in the universe which is never found unassociated with matter, depends, in all its widely varied forms, upon motion of matter."

After explaining Newton's Laws, the author deals with the principles of kinematics, and then with statics and kinematics of various material systems, with different degrees of freedom, inserting amongst the analytical proofs several of those elegant geometrical constructions for which he is so well known. Whilst the nature of the article precludes a thorough exposition of the higher and more involved parts of the subject, he has succeeded in presenting illustrative problems of all the great divisions in mechanics, which afford some insight into the nature of the special parts of the subject to which they refer.

This most useful article, which exhibits the state of knowledge in theoretical mechanics at the present time, concludes with a list of the principal works on mechanics.

Following Prof. Tait's article, and under the heading of "Applied Mechanics," we have the reprint of an article by the late Prof. Rankine, contributed by him to the volume of the "Encyclopædia Britannica" which was published in 1857.

In this article Prof. Rankine has dealt with the principles of the subject very much on the same lines as in his larger published work on "Applied Mechanics." It is needless to say that nothing that Rankine wrote on the theory of mechanics can ever become antiquated or obsolete. He possessed such a firm grasp of the foundations of the subject, that it seems impossible to believe that on these points he could commit an error. But since that time many new discoveries have been made in mechanics, as in other sciences, to which we find no reference in the present articles. Of these perhaps the most important are the later developments of graphical statics, and the kinematical analysis of Prof. Reuleaux. The former subject, which really dates its origin from the time of the discoveries by Rankine of the Theory of the Extension of the Funicular Polygon, and by Clerk Maxwell of the Theory of Reciprocal Figures, has received at the hands of Culmann and others developments which are now proving themselves of the greatest importance in engineering design. Of the higher parts of these more modern methods no information is given, either in the article before us, or in the extremely clear and simple theory of Frames, which appears in Prof. Jenkin's article on "Bridges," in the fourth volume of this "Encyclopædia," or in any other place in the work, and having regard to the importance of the subject, we cannot but regret its absence.

We believe that had the work of Reuleaux been published earlier, Rankine would have been one of the first to recognise its beauty and value.

The whole article displays the power of logical arrangement and method, as well as the condensed style which is so characteristic of all Rankine's writings, and makes them such difficult reading for beginners. These will probably prefer his "Applied Mechanics," for purposes of study, to the article before us. But as an exposition, in small compass, of the leading principles of that science, it is altogether admirable as far as it goes, whilst its value is increased by the numerous articles in this "Encyclopædia" on special, more technical parts of the subject, such as that of Prof. Jenkin, already quoted, and that of Prof. Unwin on "Hydraulics," and others which are promised in forthcoming volumes.

The article on "Mammalia," by Prof. Flower, is an extremely well condensed and intelligibly written essay on the highest class of vertebrate beings, for which, as the author notes, there has never been a generally accepted vernacular designation. Still the class known to zoologists as Mammals is one rigidly defined, and one that obeys the strictest rules of logic in its definition, despite Kant's remarks on the impossibility of defining strictly natural objects. It is easy to imagine the mammary glands reduced to a state of extreme simplicity, but among living mammals this never occurs, nor is there any gland to be confounded with them in any other vertebrate form. The article opens with a chapter on the general anatomical characters of the class, in which an immense amount of accurate information is compressed into a small space. Many of the figures illustrating the details of the osteology are taken from Prof. Flower's well-known work on this subject. In the chapter on classification, the recent arguments of Prof. Huxley in favour of passing over all known forms of birds and reptiles and going straight to the amphibia for the progenitors of the mammalia are quoted with approval; and that author's subdivision of the class into three sub-branches—Prototheria, Metatheria, and Eutheria—is adopted. The history of the distribution of the mammals in time and space follows; and then we have the characters of the different orders and families, and of the principal forms of the class. In this section of the memoir the illustrations, taken from the best sources, are especially to be praised, and in many instances the information as to rare or new species is brought well up to date. This seems to us especially so in the interesting group of the bats and insectivora, for which Prof. Flower acknowledges his indebtedness to Dr. G. E. Dobson, but in the portion devoted to the order Primates, an order which Prof. Flower makes to include the lemurs, the monkeys, and man, we read the little that is written under the impression that it was but introductory to a good deal that was to follow, and when we turned over to p. 446 we found the essay was finished and that we had arrived at the index; even this bears marks of a forced compression, for while the earlier letters are fairly done, the last in the index have evidently had a lot "squeezed" out.

One other article relating to zoology in this volume is also by Prof. W. H. Flower, on the "Mammoth." He alludes to the derivation of this name as being by some ascribed to a Tartar origin, by others that it is a corruption of the Arabic word *Behemoth*, or great beast, but on the authority of Prof. Sayce it is a corruption of the Biblical Behemoth, Arabic behîmat.

The scientific articles in vol. xvi. are so numerous and important that it is impossible for us to give them satisfactory notice in the space at our disposal; we can do no more than name the more important. From Prof. Dittmar we have Metallurgy and Metals; Prof. Chandler Roberts and Mr. R. A. Hill contribute the article on Mint, in which all aspects of the subject are fully as well as interestingly treated; while Mining, by Dr. Le Neve Foster, is both practical and scientific. Meteorology, of course, has been undertaken by Mr. Buchan and Prof. Balfour Stewart, and forms an admirable exposition of the present condition of a science of great and growing complexity; Mr. Buchan treating of instruments and phenomena, while Prof. Stewart deals with the science that underlies the subject. The article on Micrometer is by Dr. David Gill; while it is natural to find Dr. W. B. Carpenter's name attached to that on Microscope. Prof. Heddle contributes an elaborate and profusely illustrated article on Mineralogy. Molecule has a triple authorship, Rev. H. W. Watson, Mr. S. H. Burbury, and Prof. Crum Brown, both its physical and chemical aspects being thus fully treated. The article on Mollusca in this volume, by Prof. Ray Lankester, is as complete and masterly and richly illustrated as that on Mammalia in the previous volume. Under Moon we have a short article on the lunar theory, by Prof. Simon Newcomb; other aspects of the subject have been dealt with under Astronomy. Mr. P. Geddes has a careful and wonderfully exhaustive article on Morphology; and Mr. R. M'Lachlan finishes off the volume with a somewhat tiny article on Mosquito. There are many other smaller articles in all departments of science,—Prof. A. Newton, for example, doing all birds,—and several important ethnologico-geographical articles, as Mexico, by Mr. E. B. Tylor and Prof. Keane, and Mongols, by Prof. Douglas and Prof. Jülg. We hope in a future number to be able to refer in detail to some of the articles mentioned.

LETTERS TO THE EDITOR

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

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

Living Scorpions, Mygale, and Protopterus

WILL you allow me to use your columns in order to ask any of your readers residing in tropical localities, who may be generous enough to wish to help a naturalist in his researches, to send to me *living* specimens of *large* Scorpions (not less than three inches in length), and *living* specimens of large Mygale (birdsnesting spider); also I would beg for *living* Earthworms of large size from African, Indian, American, and Australian localities. Any of these animals can be sent in a small tin box in which a few holes are perforated; the tin box being packed in a much larger wooden box with hay or loose paper. Damp moss should be placed with the Scorpion or Mygale. Each specimen should be inclosed in a separate tin box, since these animals are cannibals. The holes in the tin box containing an Earthworm should be very few, and the amount of damp moss very great. Earthworms would travel best in a Wardian case, should the opportunity offer—not loose, but in the above-mentioned tin box.

I would further take this opportunity to ask for information concerning the best way of keeping the African Lepidosiren, or mud-fish (*Protopterus annectens*), in confinement. I require to ascertain (1) its natural food, (2) the temperature of the waters in which it naturally lives, (3) whether these are stagnant or rapidly running, (4) whether anything is known as to habits in the breeding season, and if this season immediately precedes or succeeds the dry season.

Some of your readers in this country or in Africa may have gained experience on these points, and would greatly help me in an attempt to breed the mud-fish by communicating with me.

E. RAY LANKESTER

11, Wellington Mansions, North Bank, N.W.

Electricity in India.—The Green Sun

[THE following letter has been sent us for publication by Sir William Thomson, to whom it is addressed :—]

For nearly a month the air has been in a state of electrification, which seems to me so interesting that I thought you would probably like to hear of it at once without my waiting to complete my observations. Unfortunately I cannot tell the exact date at which it began, but August 31 showed positive electricity all day apparently. On September 1 and 2, I was not able to get any measurements, but on the 3rd at 1.10 p.m., I got negative readings from −28 to −17 div., wind light, S. by W. By 2.45 it had changed to +6. Next morning at 10.5 a.m. it varied from −136 to −44; this was on the roof. I then took it to the ground, to a place quite open, and found readings from −460 in gusts of wind to −162 when the wind was light. The wind was fresh, westerly. Up to 1h. 14m. it continued negative, but at my next reading, 3 p.m., it was +35, and remained steadily positive, the wind having now gone round to the east (sea breeze).

5th, 6 a.m., positive, from 9 a.m. to 2.5 negative, and thereafter positive.

This continued with the exception of the 9th, when it was positive all day till the 13th.

On the 20th the reading at 9.55 was −34, but at 11.55 it was +44, the wind in the meanwhile having changed from west (land wind) to east (sea breeze). A similar state of affairs still continues.

During all this time the weather in Madras has been fine, and for some days at first, when I made very special inquiries, I found that no rain had fallen within 100 miles of Madras. It may perhaps be worth mentioning that from the 8th to the 14th we have had the strange phenomenon of a bright green sun at sunrise and sunset, the sun appearing as a rayless globe, at which you could easily look, and yet so sharply defined that sunspots could be well seen with the naked eye. On the 22nd again, two days after the electricity had gone to positive, the green sun reappeared, and has now changed to a sort of golden green. I do not say that there is any connection between the two, but they seem worth mentioning together. I have got a large number of observations which I will reduce as soon as possible, and send to the Royal Society of Edinburgh, but there is no use doing this till things return to their normal state. It is worth pointing out that observations made at intervals of six hours might have entirely failed to find the negative electricity. I usually, for convenience, take observations at 9 a.m., before leaving for college, and the next would be at three, and both these are always positive. I have not got the exact scale of my electrometer, but I find that 100 Daniell's cells give only 24 div. of a deflection. I am very much disappointed that I have not got my new instrument yet. Had I had it I would have been able to get simultaneous observations carried on at Madras and at a place forty miles to the west, which might have given valuable results. My present instrument, though working better than before, needs constant recharging. For example, at noon yesterday the earth reading was 1750, and at six this morning it was only 1440. C. MICHIE SMITH

Christian College, Madras, September 26

Unusual Cloud-Glow after Sunset

YESTERDAY evening a most extraordinary sunset effect was seen here, which made a deep impression on all who observed it. The sky was nearly clear when the sun set at 4.18, and the air transparent. A few cirrocumulus fleeces became lighted up with a pink and then with a deep red colour immediately after sunset. A very peculiar greenish and white opalescent haze now appeared about the point of the sun's departure, and shone as if with a light of its own, near the horizon. The upper part of this pearly mist soon assumed a pink colour, while the lower part was white, green, and greenish-yellow. About 4.35 the sky from near the horizon towards the zenith had begun to turn to a brilliant but delicate pink, and some pink cirrus-like streaks stretched apparently horizontally towards the south-east. The coloured portion of the sky spread out like a sheaf from the horizon, and apparently consisted of a very high, thin filmy cirrus disposed in transverse bands or ripples, close together, and very delicate in form, outline, and tint. Below the pink, and between it and the point where the sun had set, remained the very curious, opalescent, shining, green and white vapour, hanging, as it were, vertically, and changing very little during many minutes. The borders of the pink sheaf were definite, and finely contrasted with the deep blue sky. As darkness came on, the pink glow seemed to increase in brightness, and at five o'clock cast a fine weird light over the hills. The moon was now bright in the south-east, and began to cast dark shadows. About five the colour slowly receded from the part nearest the zenith towards the horizon, and as it retired left a clearly visible filmy ripple of cirrus of a faint gray tint. At 5.25 the greater part of the colour was gone, and the cloud remained bright only near the horizon. At 5.32, however, it began to grow again, and in a short time (5.40) the whole extent of the film was again glowing bright pink, producing a most striking effect in contrast with the silvery moon, dark sky, and bright stars in the north and east. The pink light then slowly withdrew towards the horizon, remaining bright and deep coloured low down till 5.50. At 5.58 the last pink disappeared. The whole phenomenon from first to last was in the highest degree peculiar and striking. It was remarkable, first, for the interval which elapsed between the time of sunset and the time at which the cloud became bright, next for the light, filmy character of the cloud, thirdly for the bright green glow near the place of sunset, fourthly for the small transverse ripple form of the cloud, fifthly for the permanence of shape and immobility of the cloud, sixthly for the very long endurance of the coloured reflected sunlight after sunset, one hour and forty minutes, and seventhly for the second illumination, which began more than an hour after sunset. It was certainly due to cirrus or a higher kind of cloud, because (1) parts of the illuminated sky stretched in long streaks southwards, and the glow remained long in these streaks, resembling very high cirrus; (2) when the light left the sky the first time, the part which had been illuminated remained visible as silvery gray cloud ripples, before the second after-glow rekindled it, and (3) because the colour became very gradually darker as time went on, and because the recessions of light both times were towards the place of sunset. A similar very high cirrus had also been specially marked long after sunset on November 8, and about the time of sunrise on November 9. The night following this rare display was exceedingly clear and fine. This evening (November 10) the light, high cirrus, all but invisible in full daylight, with its delicate ripples, assumed the pink tint about fifteen minutes after sunset, showing the upper air to be in the same abnormal condition as yesterday, and the phenomenon was feebly repeated. It would be interesting to ascertain the approximate height of cirrus on which sunshine remains one hour and forty minutes after sunset at this time of year. F. A. R. RUSSELL

Dunrozel, Haslemere, Surrey, November 10

Shadow-Beams in the East at Sunset

THE phenomenon of beams of shadow meeting in the east at sunset, treated of in the pages of NATURE some months since (at which time you did me the honour of inserting a letter of mine), was beautifully witnessed here to-day and yesterday. Both days were unusually clear; there was, nevertheless, a "body" in the air, without which the propagation of the beams could not take place. Yesterday the sky was striped with cirrus cloud like the swaths of a hayfield; only in the east there was a bay or reach of clear blue sky, and in this the shadow-beams appeared, slender, colourless, and radiating every way like a fan wide open. This lasted from 3.30 to about 4.30. To-day the sky was cloudless, except for a low bank in the west; in the east was a "cast" of blue mist, from which sprang alternate broad bands of rose colour and blue, slightly fringed. I was not able to look for them till about 4.30, when the sun was down, and they soon faded. I have not before seen this appearance so far north, but on the south coast, where I first saw it, I think it might often be witnessed. It is merely an effect of perspective, but a strange and beautiful one.

Stonyhurst College, November 12 GERARD HOPKINS

The Java Eruption

THE accompanying paragraph may be of interest in connection with the Java catastrophe. I may mention that from the 28th of last month, when I first noticed it, there has been an exceptional red glow after sundown, and a strange green tint in the sky, while till the last few days the moon has had a distinctly green tint; this green tint has been noticed in many parts of India. F. C. CONSTABLE

Karachi, October 16

A FLOATING LAVA BED.—Sir,—It may be interesting to some of your scientific readers to know that the steamship *Siam*, on her voyage from King George's Sound to Colombo, on August 1, when in lat. 6° S., 89° E., passed, for upwards of four hours, through large quantities of lava, which extended as far as could be seen (the ship was going eleven knots at the time). The lava was floating in a succession of lanes from five to ten yards wide, and trending in a direction north-west to south-east. The nearest land was the coast of Sumatra (distant 700 miles), but as there was a current of fifteen to thirty miles a day, setting to the eastward, the lava could not have come from there, and I can only imagine it must have been an upheaval from somewhere near the spot. I may mention the soundings on the chart show over 2,000 fathoms. There was a submarine volcano near the spot in 1879.—EDWARD ASHDOWN, Commander, P. and O. steamship *Siam*. (*Sind Gazette Bulletin*, October 12.)

Towering of Birds

WHEN shooting in Fifeshire last October I fired at a partridge at a distance of about forty yards; the bird flew on for a short distance, and then began to rise, not in the manner in which a towering bird generally ascends, but soaring as if it did so voluntarily. After rising to the height of 100 or 150 yards very much after the fashion in which some hawks soar, its flight was suddenly deflected downwards obliquely for a considerable distance, when it swerved, and came towards the ground in a different direction, alighting as though it were in possession of its natural powers, some hundreds of yards from the place whence it rose. On going to the spot where it had settled, it was found to be alive and crouching in the long grass. The keeper ran in and placed his hand on it, when the bird struggled and tried to get away; he killed it seeing that it was wounded. On examining the bird immediately after I found that it had been struck by two pellets of No. 6 shot, one of which had penetrated the pectoral muscles, but had not injured the cavity; the lungs and other viscera were uninjured. The other pellet had entered behind and below the left eyeball, and, passing forward, had emerged on the other side, passing above the upper mandible. The brain was uninjured, but the lower part of the left eyeball was cut and distended with blood. There was no other injury. No doubt the shock had confused the bird, and caused its strange flight, which, though upward, was very different in its character from that of ordinary towering where the lungs are perforated, and unconsciousness is the result of the circulation of non-aërated blood. J. FAYRER

Meteors

PERMIT me to point out to Mr. J. M. Hayward (NATURE, Nov. 8, p. 30) that his observation of the large meteor of November 4 possesses no scientific value, inasmuch as he has omitted to mention the important features of its appearance. The time is given as "just now" (or November 4), and the broad path of fire which this fine meteor discharged upon its course must have been situated *somewhere* in the south-east, for your correspondent states he saw it "on turning to the south-east."

I had endeavoured to show in NATURE of the preceding week (Nov. 1, p. 6) that these delightfully vague forms of expression as applied to meteors are wholly inadequate, and, as such, cannot receive any attention at the hands of those who investigate these phenomena.

Had Mr. Hayward given us the essential details of his observation, it might have proved very valuable, for a large meteor (perhaps identical with the one he refers to) was observed at many places on the night of November 4. As recorded at Chelmsford, Bath, and Bristol the paths were:—

Time.		Mag.	From α	From δ	To α	To δ	Observer.
1883.	h. m.		°	°			
Nov. 4	10 14	= ♀	8	S. 20	355	S. 30	H. Corder, Chelmsford.
Nov. 4	10 12	6 × ♃	33	S. 6	9	S. 23	J. L. Stothert, Bath.
Nov. 4	10 12	> ♃	36	N. 1½	16	S. 19	W. F. Denning, Bristol.

The several estimates of brilliancy are very discordant, but the time and paths agree so closely that there is little doubt the observations refer to the same meteor.

Another fine meteor was seen here on October 26 at 9h. 17m. It gave a succession of four lightning-like flashes. Path from α 288 δ 56° + to α 333° δ 59° +. This was not the only fireball visible that night, for I see by NATURE (November 8, p. 44) that "On October 26 at about 7 p.m. a splendid meteor was seen in the district of Hernösand, Sweden." It appeared "with a blinding white lustre in the zenith and travelling very rapidly down to the horizon." In this case again we have to deplore the extremely vague manner of the description. Had the precise direction of flight been given, it would have been interesting to determine whether this fireball belonged to the same stream as the equally fine one recorded at Bristol on the same night. W. F. DENNING

Bristol, November 10

THE meteors during October have been numerous, and the most of them proceeded from some point in Auriga. With the exception of about nine days of unfavourable weather, I have seen several meteors night and morning throughout October, but they were generally small and transient. I have counted fifty-two from 10 p.m. of October 3 to 4·30 a.m. of the 4th, many of them large and of several seconds' duration. The largest of these passed slowly from the first bright star on the left of Capella, in Auriga, to a point about 1° below α Cygni. The smallest of them blinked rapidly before the eye in the zenith over the Milky Way, which, this night, was the principal theatre of their display. From 3.30 to 4.30 a.m. I counted forty of the fifty-two meteors. From 1 a.m. to 4 of October 8 I observed very brilliant meteors. One at 2.25 a.m. darted from about 1° above Capella and disappeared at a point ½° from Phad in the Plough, without exploding and without leaving any trace of light behind. It was as large as Venus. At 2.40 a.m. a very large and brilliant meteor dashed out from a point midway between Capella and the first bright star to its right in Auriga, and sped along above the Pleiades and Aries through the Square of Pegasus, and exploded 3° beyond it, leaving no fire in its wake. October 15, 11.38 p.m., a very unusual meteor sailed slowly from β Ceti to within 1° of Betelgueux, in the right shoulder of Orion. After travelling two-thirds of its journey, it exploded into four, three of which formed the head of an arrow, and the fourth adorned its tail, all the four sending out bright nebulous light behind them. At 2.50 a.m., October 26, a large ball of fire (bolide), apparently seven inches in diameter, illumined the heavens with great brilliancy as it descended from about midway between the third and fourth bright stars on the left of Capella, exploding twice during the last half of its journey, and disappearing just as it reached the moon. It had no tail. It was seen by some of the Paisley night police, and one of them was frightened that it would dash the moon out of the heavens. This bolide had no detonation in either of its two explosions, and the last of it was only about the size of Jupiter. One policeman describes it as a large fiery ball of the size of the full moon, but this is an exaggeration. The extraordinary meteor of October 15, after its explosion, was described by an observer as a well-formed arrow of flaming fire, followed by a ball of fire with a tail. To me it appeared to resemble the head and body of a fish, as well as the form of an arrow. DONALD CAMERON

Mossvale, Paisley, November 6

ON the evening of Saturday last, at 10.12 p.m., a remarkable meteorite was observed close to Trinity College, Glenalmond, in Perthshire. It presented the appearance of a bright spherical ball, which moved horizontally from east-north-east to west-south-west at a height roughly estimated at 300 feet. When it began to curve downwards it disappeared from view, but it left behind it a luminous trail of great brilliancy, which was seen for fully forty seconds, its brilliancy gradually diminishing till it entirely faded away. W. BESANT LOWE

Trinity College, Glenalmond, Perth, November 12

"Anatomy for Artists"

I AM quite unable to do as your correspondent "An Art Student" suggests, for the second edition of the above-named book has been just issued. I may add, however, that the reasons which led me deliberately to adopt the plan alluded to in regard to the illustrations of the bones still remain, in my opinion, sound, and I trust that the majority of my readers of the past, present, and future editions have not been and will not be "discouraged" by the effort which I desire them, for their own sakes as students, to make. JOHN MARSHALL

10, Savile Row, W., November 12

P.S.—It seems that *I* ought to have two "letters of reference" attached to myself, for I am not "Dr." but "Mr." Marshall.

Earthquake

NATURE on October 25 contained notices of shocks of earthquake which were felt at a quarter to one o'clock on the night of October 19 (11h. 20m. Greenwich M.T.) at Cadiz and other places on the coast of Andalusia. I have information that about 17h. 45m. later these shocks, which were travelling from east to west, had apparently reached Bermudas. In a letter just received from ex-Chief Justice Darrell, dated October 22, he remarks:—"A very unusual event occurred here on the 20th of this month, in a shock of an earthquake, which however was slight; no life was lost, nor serious damage done to buildings; but the shock, which lasted less than a minute, at about a quarter past one p.m. was universally and unmistakably felt throughout the colony. It is said to be only the third time that any earthquake has been experienced in Bermuda in the last forty years." A quarter past one in Bermuda would be about four and a half minutes past five at Greenwich, requiring, if the shocks originated in the same wave, a rate of transmission of about 158 geographical miles an hour, or 2·6 miles per minute; less than half the rate at which the great shocks of 1755 and 1761 crossed the Atlantic from Lisbon to Barbados, which is given by Mallet as 7·3 miles, or 6·3 geographical miles per minute.[1]

J. H. LEFROY

"Partials"

IN your number of Nov. 1, p. 6, I noticed an article the object of which was to account for the existence of "partials." Were the theory therein set forth correct, we should have a constant number of "partials" for any given "fundamental" tone of constant force regardless of its source; whereas it is a well-known fact that, while the tones of some instruments are rich in "partials," those of other instruments have but few.

CROMWELL O. VARLEY

Cromwell House, Bexley Heath, Kent

SCIENCE AND ENGINEERING

IN the address delivered by Mr. Westmacott, President of the Institution of Mechanical Engineers, to the English and Belgian engineers assembled at Liége last August, there occurred the following passage:—"Engineering brings all other sciences into play: chemical or physical discoveries, such as those of Faraday, would be of little practical use if engineers were not ready with mechanical appliances to carry them out, and make them commercially successful in the way best suited to each."

We have no objection to make to these words, spoken at such a time and before such an assembly. It would of course be easy to take the converse view, and observe that engineering would have made little progress in modern times, but for the splendid resources which the discoveries of pure science have placed at her disposal, and which she has only had to adopt and utilise for her own purposes. But there is no need to quarrel over two opposite modes of stating the same fact. There *is* need on the other hand that the fact itself should be fairly recognised and accepted, namely, that science may be looked upon as at once the handmaid and the guide of art, art as at once the pupil and the supporter of science. In the present article we propose to give a few illustrations which will bring out and emphasise this truth.

We could scarcely find a better instance than is furnished to our hand in the sentence we have chosen for a text. No man ever worked with a more single-hearted devotion to pure science—with a more absolute disregard of money or fame, as compared with knowledge—than Michael Faraday. Yet future ages will perhaps judge that no stronger impulse was ever given to the progress of industrial art, or to the advancement of the material interests of mankind, than the impulse which sprang from his discoveries in electricity and magnetism. Of these discoveries we are only now beginning to reap the benefit. But we have merely to consider the position which the dynamo-electric machine already occupies in the industrial world, and the far higher position which, as almost all admit, it is destined to occupy in the future, in order to see how much we owe to Faraday's establishment of the connection between magnetism and electricity. That is one side of the question—the debt which art owes to science. But let us look at the other side also. Does science owe nothing to art? Will any one say that we should know as much as we do concerning the theory of the dynamo-electric motor, and the laws of electro-magnetic action generally, if that motor had never risen (or fallen, as you choose to put it) to be something besides the instrument of a laboratory, or the toy of a lecture-room. Only a short time since the illustrious French physicist, M. Tresca, was enumerating the various sources of loss in the transmission of power by electricity along a fixed wire, as elucidated in the careful and elaborate experiments inaugurated by M. Marcel Deprez, and subsequently continued by himself. These losses—the electrical no less than the mechanical losses—are being thoroughly and minutely examined in the hope of reducing them to the lowest limit; and this examination cannot fail to throw much light on the exact distribution of the energy imparted to a dynamo machine, and the laws by which this distribution is governed. But would this examination ever have taken place—would the costly experiments which render it feasible ever have been performed—if the dynamo machine was still under the undisputed control of pure science, and had not become subject to the sway of the capitalist and the engineer?

Of course the electric telegraph affords an earlier and perhaps as good an illustration of the same fact. The discovery that electricity would pass along a wire and actuate a needle at the other end was at first a purely scientific one; and it was only gradually that its importance, from an industrial point of view, came to be recognised. Here again art owes to pure science the creation of a complete and important branch of engineering, whose works are spread like a net over the whole face of the globe. On the other hand, our knowledge of electricity, and specially of the electro-chemical processes which go on in the working of batteries, has been enormously improved in consequence of the use of such batteries for the purposes of telegraphy.

Let us turn to another example in a different branch of science. Whichever of our modern discoveries we may consider to be the most startling and important, there can I think be no doubt that the most beautiful is that of the spectroscope. It has enabled us to do that which but a few years before its introduction was taken for the very type of the impossible, viz. to study the chemical composition of the stars; and it is giving us clearer and clearer insight every day into the condition of the great luminary which forms the centre of our system. Still, however beautiful and interesting such results may be, it might well be thought that they could never have any practical application, and that the spectroscope at least would remain an instrument of science, but of science alone. This however is not the case. Some thirty years since Mr. Bessemer conceived the idea that the injurious constituents of raw iron—such as silicon, sulphur, &c.—might be got rid of by simple oxidation. The mass of crude metal was heated to a very high temperature; atmospheric air was forced through it at a considerable pressure; and the oxygen uniting with these metalloids carried them off in the form of acid gases. The very act of union generated a vast quantity of heat, which itself assisted the continuance of the process; and the gas therefore passed off in a highly luminous condition. But the important point was to know where to stop; to seize the exact moment when all or practically all hurtful ingredients had been removed, and before the oxygen had turned from them to attack the iron itself. How was this point to be ascertained? It was soon suggested that each of these gases in its incandescent state would show its own peculiar spectrum; and that, if the flame rushing out of the throat of the converter were viewed through a spectroscope, the moment when any substance such as sulphur had disappeared would be known by the disappearance of the corresponding lines in the spectrum. The anticipation, it is needless to say, was verified; and the spectroscope, though now superseded, had for a time its place among the regular appliances necessary for the carrying on of the Bessemer process.

This process itself, with all the momentous consequences, mechanical, commercial, and economical, which it has entailed, might be brought forward as a witness on our side; for it was almost completely worked out in the laboratory before being submitted to actual practice. In this respect it stands in marked contrast to the earlier processes for the making of iron and steel, which

[1] Mallet's Fourth Report, British Association, 1858.

were developed, it is difficult to say how, in the forge or furnace itself, and amid the smoke and din of practical work. At the same time the experiments of Bessemer were for the most part carried out with a distinct eye to their future application in practice, and their value for our present purpose is therefore not so great. The same we believe may be said with regard to the great rival of the Bessemer converter, viz. the Siemens open hearth; although this forms in itself a beautiful application of the scientific doctrine that steel stands midway, as regards its proportion of carbon, between wrought iron and pig iron, and ought therefore to be obtainable by a judicious mixture of the two. The basic process is the latest development, in this direction, of science as applied to metallurgy. Here, by simply giving a different chemical constitution to the clay lining of the converter, it is found possible to eliminate phosphorus—an element which has successfully withstood the attack of the Bessemer system. Now, to quote the words of a German eulogiser of the new method, phosphorus has been turned from an enemy into a friend; and the richer a given ore is in that substance, the more readily and cheaply does it seem likely to be converted into steel.

These latter examples have been taken from the art of metallurgy; and it may of course be said that, considering the intimate relations between that art and the science of chemistry, there can be no wonder if the former is largely dependent for its progress on the latter. I will therefore turn to what may appear the most concrete, practical, and unscientific of all arts—that, namely, of the mechanical engineer; and we shall find that even here examples will not fail us of the boons which pure science has conferred upon the art of construction, nor even perhaps of the reciprocal advantages which she has derived from the connection.

The address of Mr. Westmacott, from which I have already taken my text, supplies in itself more than one instance of the kind we seek—instances emphasised by papers read at the meeting where the address was spoken. Let us take, first, the manufacture of sugar from beetroot. This manufacture was forced into prominence in the early years of this century, when the Continental blockade maintained by England against Napoleon prevented all importation of sugar from America; and it has now attained very large dimensions, as all frequenters of the Continent must be aware. The process, as exhaustively described by a Belgian engineer, M. Mélin, offers several instances of the application of chemical and physical science to practical purposes. Thus, the first operation in making sugar from beetroot is to separate the juice from the flesh, the former being as much as 95 per cent. of the whole weight. Formerly this was accomplished by rasping the roots into a pulp, and then pressing the pulp in powerful hydraulic presses; in other words, by purely mechanical means. This process is now to a large extent superseded by what is called the diffusion process, depending on the well known physical phenomena of *endosmosis* and *exosmosis*. The beetroot is cut up into small slices called "cossettes," and these are placed in vessels filled with water. The result is that a current of endosmosis takes place from the water towards the juice in the cells, and a current of exosmosis from the juice towards the water. These currents go on cell by cell, and continue until a state of equilibrium is attained. The richer the water and the poorer the juice, the sooner does this equilibrium take place. Consequently the vessels are arranged in a series, forming what is called a diffusion battery; the pure water is admitted to the first vessel, in which the slices have already been nearly exhausted, and subtracts from them what juice there is left. It then passes as a thin juice to the next vessel, in which the slices are richer, and the process begins again. In the last vessel the water which has already done its work in all the previous vessels comes into contact with fresh slices, and begins the operation upon them. The same process has been applied at the other end of the manufacture of sugar. After the juice has been purified, and all the crystallisable sugar has been separated from it by boiling, there is left a mass of molasses, containing so much of the salts of potassium and sodium that no further crystallisation of the yet remaining sugar is possible. The object of the process called osmosis is to carry off these salts. The apparatus used, or osmogene, consists of a series of trays filled alternately with molasses and water, the bottoms being formed of parchment paper. A current passes through this paper in each direction, part of the water entering the molasses, and part of the salts, together with a certain quantity of sugar, entering the water. The result of thus freeing the molasses from the salts is that a large part of the remaining sugar can now be extracted by crystallisation.

Another instance in point comes from a paper dealing with the question of the construction of long tunnels. In England this has been chiefly discussed of late in connection with the Channel Tunnel, where, however, the conditions are comparatively simple. It is of still greater importance abroad. Two tunnels have already been pierced through the Alps; a third is nearly completed; and a fourth, the Simplon Tunnel, which will be the longest of any, is at this moment the subject of a most active study on the part of French engineers. In America, especially in connection with the deep mines of the western States, the problem is also of the highest importance. But the driving of such tunnels would be financially if not physically impossible, but for the resources which science has placed in our hands, first, by the preparation of new explosives, and, secondly, by methods of dealing with the very high temperatures which have to be encountered. As regards the first, the history of explosives is scarcely anything else than a record of the application of chemical principles to practical purposes—a record which in great part has yet to be written, and on which we cannot here dwell. It is certain, however, that but for the invention of nitroglycerine, a purely chemical compound, and its development in various forms, more or less safe and convenient, these long tunnels would never have been constructed. As regards the second point, the question of temperature is really the most formidable with which the tunnel engineer has to contend. In the St. Gothard Tunnel, just before the meeting of the two headings in February, 1880, the temperature rose as high as 93° Fahr. This, combined with the foulness of the air, produced an immense diminution in the work done per person and per horse employed, whilst several men were actually killed by the dynamite gases, and others suffered from a disease which was traced to a hitherto unknown species of internal worm. If the Simplon Tunnel should be constructed, yet higher temperatures may probably have to be dealt with. Although science can hardly be said to have completely mastered these difficulties, much has been done in that direction. A great deal of mechanical work has of course to be carried on at the face or far end of such a heading, and there are various means by which it might be done. But by far the most satisfactory solution, in most cases at least, is obtained by taking advantage of the properties of compressed air. Air can be compressed at the end of the tunnel either by steam-engines, or, still better, by turbines where water power is available. This compressed air may easily be led in pipes to the face of the heading, and used there to drive the small engines which work the rock-drilling machines, &c. The efficiency of such machines is doubtless low, chiefly owing to the physical fact that the air is heated by compression, and that much of this heat is lost whilst it traverses the long line of pipes leading to the scene of action. But here we have a great advantage from the point of view of ventilation; for as the air gained heat while being compressed, so it loses heat while expanding; and the result is that a current of cold and fresh air is continually issuing from the machines at the face of the heading, just where it is most wanted. In consequence, in the St. Gothard, as just alluded to, the hottest parts were always some little distance behind the face of the heading. Although in this case as much as 120,000 cubic metres of air (taken at atmospheric pressure) were daily poured into the heading, yet the ventilation was very insufficient. Moreover, the high pressure which is used for working the machines is not the best adapted for ventilation; and in the Arlberg tunnel separate ventilating pipes are employed, containing air compressed to about one atmosphere, which is delivered in much larger quantities, although not at so low a temperature. In connection with this question of ventilation a long series of observations have been taken at the St. Gothard, both during and since the construction: these have revealed the important physical fact (itself of high practical importance) that the barometer never stands at the same level on the two sides of a great mountain chain; and so have made valuable contributions to the science of meteorology.

Another most important use of the same scientific fact, namely, the properties of compressed air, is found in the sinking of foundations below water. When the piers of a bridge, or other structure, had to be placed in a deep stream, the old method was to drive a double row of piles round the place and fill them in with clay, forming what is called a cofferdam. The water was pumped out from the interior, and the foundation laid in the open. This is always a very expensive process, and in rapid streams is scarcely practicable. In recent times large bottomless cases, called caissons, have been used, with tubes attached to the roof, by which air can be forced into or out of the interior. These caissons are brought to the site of the proposed pier, and are there sunk. Where the bottom is loose sandy earth, the Vacuum process, as it is termed, is often employed; that is, the air is pumped out from the interior, and the superincumbent pressure then causes the caisson to sink and the earth to rise within it. But it is more usual to employ what is called the Plenum process, in which air under high pressure is pumped into the caisson and expels the water, as in a diving bell. Workmen then descend, entering through an air lock, and excavate the ground at the bottom of the caisson, which sinks gradually as the excavation continues. Under this system a length of some two miles of quay wall is being constructed at Antwerp, far out in the channel of the River Scheldt. Here the caissons are laid end to end with each other, along the whole curve of the wall, and the masonry is built on the top of them within a floating cofferdam of very ingenious construction.

There are few mechanical principles more widely known than that of so-called centrifugal force; an action which, though still a puzzle to students, has long been thoroughly understood. It is, however, comparatively recently that it has been applied in practice. One of the earliest examples was, perhaps, the ordinary governor, due to the genius of Watt. Every boy knows that if he takes a weight hanging from a string and twirls it round, the weight will rise higher and revolve in a larger circle as he increases the speed. Watt saw that if he attached such an apparatus to his steam engine, the balls or weights would tend to rise higher whenever the engine began to run faster, that this action might be made partly to draw over the valve which admitted the steam, and that in this way the supply of steam would be lessened, and the speed would fall. Few ideas in science have received so wide and so successful an application as this. But of late years another property of centrifugal force has been brought into play. The effect of this so-called force is that any body revolving in a circle has a continual tendency to fly off at a tangent; the amount of this tendency depending jointly on the mass of the body and on the velocity of the rotation. It is the former of these conditions which is now taken advantage of. For if we have a number of particles all revolving with the same velocity, but of different specific gravities, and if we allow them to follow their tendency of moving off at a tangent, it is evident that the heaviest particles having the greatest mass will move with the greatest energy. The result is that, if we take a mass of such particles and confine them within a circular casing, we shall find that, having rotated this casing with a high velocity and for a sufficient time, the heaviest particles will have settled at the outside and the lightest at the inside, whilst between the two there will be a gradation from the one to the other. Here, then, we have the means of separating two substances, solid or liquid, which are intimately mixed up together, but which are of different specific gravities. This physical principle has been taken advantage of in a somewhat homely but very important process, viz. the separation of cream from milk. In this arrangement the milk is charged into a vessel something of the shape and size of a Gloucester cheese, which stands on a vertical spindle and is made to rotate with a velocity as high as 7000 revolutions per minute. At this enormous speed the milk, which is the heavier, flies to the outside, while the cream remains behind and stands up as a thin layer on the inside of the rotating cylinder of fluid. So completely does this immense speed produce in the liquid the characteristics of a solid, that if the rotating shell of cream be touched by a knife it emits a harsh grating sound, and gives the sensation experienced in attempting to cut a stone. The separation is almost immediately complete, but the difficult point was to draw off the two liquids separately and continuously without stopping the machine. This has been simply accomplished by taking advantage of another principle of hydromechanics. A small pipe opening just inside the shell of the cylinder is brought back to near the centre, where it rises through a sort of neck and opens into an exterior casing. The pressure due to the velocity causes the skim-milk to rise in this pipe and flow continuously out at the inner end. The cream is at the same time drawn off by a similar orifice made in the same neck and leading into a different chamber.

Centrifugal action is not the only way in which particles of different specific gravity can be separated from each other by motion only. If a rapid "jigging" or up-and-down motion be given to a mixture of such particles, the tendency of the lighter to fly further under the action of the impulse causes them gradually to rise to the upper surface; this surface being free in the present case, and the result being therefore the reverse of what happens in the rotating chamber. If such a mixture be examined after this up-and-down motion has gone on for a considerable period, it will be found that the particles are arranged pretty accurately in layers, the lightest being at the top and the heaviest at the bottom. This principle has long been taken advantage of in such cases as the separation of lead ores from the matrix in which they are embedded. The rock in these cases is crushed into small fragments, and placed on a frame having a rapid up-and-down motion, when the heavy lead ore gradually collects at the bottom and the lighter stone on the top. To separate the two the machine must be stopped and cleared by hand. In the case of coal-washing, where the object is to separate fine coal from the particles of stone mixed with it, this process would be very costly, and indeed impossible, because a current of water is sweeping through the whole mass. In the case of the Coppée coal-washer, the desired end is achieved in a different and very simple manner. The well known mineral felspar has a specific gravity intermediate between that of the coal and the shale, or stone, with which it is found intermixed. If, then, a quantity of felspar in small fragments is thrown into the mixture, and the whole then submitted to the jigging process, the result will be that the stone will collect on the top, and the coal at the bottom, with a layer of felspar separating the two. A current of water sweeps through the whole, and is drawn off partly at the top, carrying with it the stone, and partly at the bottom, carrying with it the fine coal.

The above are instances where science has come to the aid of engineering. Here is one in which the obligation is reversed. The rapid stopping of railway trains, when necessary, by means of brakes, is a problem which has long occupied the attention of many engineers; and the mechanical solutions offered have been correspondingly numerous. Some of these depend on the action of steam, some of a vacuum, some of compressed air, some of pressure-water; others again ingeniously utilise the momentum of the wheels themselves. But for a long time no effort was made by any of these inventors thoroughly to master the theoretical conditions of the problem before them. At last, one of the most ingenious and successful among them, Mr. George Westinghouse, resolved to make experiments on the subject, and was fortunate enough to associate with himself Capt. Douglas Galton. Their experiments, carried on with rare energy and perseverance, and at great expense, not only brought into the clearest light the physical conditions of the question (conditions which were shown to be in strict accordance with theory), but also disclosed the interesting scientific fact that the friction between solid bodies at high velocities is not constant, as the experiments of Morin had been supposed to imply, but diminishes rapidly as the speed increases—a fact which other observations serve to confirm.

The old scientific principle known as the hydrostatic paradox, according to which a pressure applied at any point of an inclosed mass of liquid is transmitted unaltered to every other point, has been singularly fruitful in practical applications. Mr. Bramah was perhaps the first to recognise its value and importance. He applied it to the well known Bramah press, and in various other directions, some of which were less successful. One of these was a hydraulic lift, which Mr. Bramah proposed to construct by means of several cylinders sliding within each other after the manner of the tubes of a telescope. His specification of this invention sufficiently expresses his opinion of its value, for it concludes as follows:—"This patent does not only differ in its nature and in its boundless extent of claims to novelty, but also in its claims to merit and superior utility compared with any other patent ever brought before or sanctioned by the legislative authority of any nation." The telescope lift has not come into practical use; but lifts worked on the hydraulic principle are becoming more and more common every day. The same principle has been applied by the genius of Sir William Armstrong and others to the working of cranes and other machines for the lifting of weights, &c.; and under the form of the accumulator, with its distributing pipes and hydraulic engines, it provides a store of power always ready for application at any required point in a large system, yet costing practically nothing when not actually at work. This system of high-pressure mains worked from a central accumulator has been for some years in existence at Hull, as a means of supplying power commercially for all the purposes needed in a large town, and it is at this moment being carried out on a wider scale in the East End of London.

Taking advantage of this system, and combining with it another scientific principle of wide applicability, Mr. J. H. Greathead has brought out an instrument called the "injector hydrant," which seems likely to play an important part in the extinguishing of fires. This second principle is that of the lateral induction of fluids, and may be thus expressed in the words of the late William Froude:—"Any surface which in passing through a fluid experiences resistance must in so doing impress on the particles which resist it a force in the line of motion equal to the resistance." If then these particles are themselves part of a fluid, it will result that they will follow the direction of the moving fluid and be partly carried along with it. As applied in the injector hydrant, a small quantity of water derived from the high-pressure mains is made to pass from one pipe into another, coming in contact at the same time with a reservoir of water at ordinary pressure. The result is that the water from the reservoir is drawn into the second pipe through a trumpet-shaped nozzle, and may be made to issue as a stream to a considerable height. Thus the small quantity of pressure-water, which, if used by itself, would perhaps rise to a height of 500 feet, is made to carry with it a much larger quantity to a much smaller height, say that of an ordinary house.

The above are only a few of the many instances which might be given to prove the general truth of the fact with which we started, namely, the close and reciprocal connection between physical science and mechanical engineering, taking both in their widest sense. It may possibly be worth while to return again to the subject, as other illustrations arise. Two such have appeared even at the moment of writing, and though their practical success is not yet assured, it may be worth while to cite them. The first is an application of the old principle of the siphon to the purifying of sewage. Into a tank containing the sewage dips a siphon pipe some thirty feet high, of which the shorter leg is many times larger than the longer. When this is started, the water rises slowly and steadily in the shorter column, and before it reaches the top has left behind it all or almost all of the solid particles which it previously held in suspension. These fall slowly back through the column and collect at the bottom of the tank, to be cleared out when needful. The effluent water is not of course chemically pure, but sufficiently so to be turned into any ordinary stream. The second invention rests on a curious fact in chemistry, namely, that caustic soda or potash will absorb steam, forming a compound which has a much higher temperature than the steam absorbed. If, therefore, exhaust steam be discharged into the bottom of a vessel containing caustic alkali, not only will it become condensed, but this condensation will raise the temperature of the mass so high that it may be employed in the generation of fresh steam. It is needless to observe how important will be the bearing of this invention upon the working of steam-engines for many purposes, if only it can be established as a practical success. And if it is so established there can be no doubt that the experience thus acquired will reveal new and valuable facts with regard to the conditions of chemical combination and absorption, in the elements thus brought together.

WALTER R. BROWNE

THE LITERATURE OF THE FISHERIES EXHIBITION[1]

II.

THE depopulation of our littoral fisheries is the text of a paper on "Crustacea," by Mr. T. Cornish, who proposes to meet the difficulty by establishing a market for "middle-sized" Crustacea (and even fishes), other than those which we now eat, either as "luxuries or dainties." There is an amusing but authoritative air of originality about this paper. Mr. W. S. Kent, on the other hand, proposes the "Artificial Culture of Lobsters" as a remedy for the same evil, and recounts some interesting experiments made by himself—on a small scale—in which he succeeded in rearing the young lobsters taken captive. The leading developmental phases are set down for the guidance of others, but the account given is deficient in record of the earlier stages of the process. This is important, as the writer (presupposing

[1] Concluded from p. 36.

success such as has attended the artificial cultivation of the Salmonidæ) states, without apparent proof, that the cultivation might go on after the removal of the *eggs* from the parent. Should this be so, choice must then lie between the methods of Messrs. Cornish and Kent. The latter has overlooked the fact that our Irish lobster fisheries appear to be capable of much greater development, and we doubt how far an accusation brought against the "West-end chefs" is a logical one. We are at a loss to see the drift of Mr. K. Cornish's remarks, which form part of the discussion upon these two papers.

Early in the career of these meetings, our freshwater fisheries received attention at the hands of Sir Jas. Maitland, whose liberality in the matter of salmon-culture is well known in all fishing circles. The author, who regards the artificial propagation of the Salmonidæ as in its infancy, records the technique and results of a long practical experience, and indicates lines for future investigation, both as regards the migratory and non-migratory forms. He shows that by skilful attention he can rely upon hatching out 99 per cent. of Loch Levan trout ova, and, while discussing all sides of the question, he wisely points out that the object to be aimed at is "not to incubate the largest number of eggs in a given space," but so to manipulate them that "the largest number of healthy fish may result"—a statement involving difficulties for the study of which we must refer the reader to the paper itself. Intimately connected with this department is the question of the salmon-disease fungus, which forms the topic of a paper on "Fish Diseases," by Prof. Huxley. The author's investigations in the matter are well known to readers of NATURE, but all connected with freshwater fishing owe a debt of gratitude to the learned Professor for having thus sifted a voluminous literature upon the subject, and diagnosed in faultless style this pest. Its geographical limits are—for the first time—mapped out; the fuugus is shown to *cause*, and not merely accompany, the disease, and its propagation is conclusively shown to be favoured by causes which though unknown must necessarily be limited. Every inducement is given to the daily worker among these fishes to cooperate in the further study of the disease, in even the purely scientific aspects of which much yet remains to be done. The remarkable fact that the disease is in no way correlated with the "productiveness" of a river is fully demonstrated, and must carry its own lesson.

A somewhat analogous topic forms subject-matter for a paper on "The Destruction of Fish by Internal Parasites," by Dr. S. Cobbold. There is, however, the most marked contrast between it and that of Prof. Huxley, and we venture to say that the statements made on the first two pages and elsewhere, are calculated to frighten rather than encourage (by appealing to the experimental side) possible workers in a field for which the author claims so much. We are compelled to put this work down with a feeling of disappointment, the more so seeing that much of the space which might have been turned to better account is devoted either to a mere reiteration of statements made again and again by the author during the earlier sittings of the Conference, or to needlessly lengthy and verbose discussions upon minor points, to the exclusion of more important ones.

The all-important topic of "Food of Fishes" is attacked by Dr. Day. There is much in his paper that is of value, he having incorporated the observations of others with his own to the best advantage. The extreme importance of this subject is obvious to all concerned, but when—to say nothing of the question of inter-preying—we consider the extent to which it is known that the food of fishes may vary under conditions of most of which we know absolutely nothing, it is obvious that there opens up a field of labour, involving all sorts of side issues, work in which must necessarily be both prolonged and tedious. The paper, however, suggests certain lines along which a fruitful advance might be made. In the discussion which followed, the chairman (Prof. Huxley), taking a philosophic grasp of the question, resolves it into a balance in favour of "the ultimate store of food" furnished by "the Diatomaceæ which occur on the surface."

Mr. R. B. Marston, in an exceedingly practical paper on "Coarse Fish Culture," adduces reasons for which it is obvious that repopulation of our fresh waters must go on as matters stand, and can be very beneficially maintained. The question is one of growing importance, especially as it affects those who, although living far inland, still have the power of rearing good fish-food. We doubt, however, how far it is not possible to obviate certain of the difficulties mentioned, by more careful "nursing" alone. In advocating the introduction of the prolific Black Bass, the writer makes a statement, partly borne out by the experience of the Marquis of Exeter who first introduced the fish into Britain, but diametrically opposed by that of Sir Jas. Maitland—and which, if correct, is of great importance—viz. that it "thrives best in just those waters which are *not* suited to trout and salmon."

It is well known that the natural salmon stock of five of our largest rivers is practically exterminated, and that the fish present themselves annually at their unsavoury mouths, but to be baffled by causes, chief among which is that of pollution; in other cases, less markedly offensive, the fish are known to be slowly but certainly receding The Hon. W. F. B. Massey Mainwaring, in a paper upon "The Preservation of Fish Life in Rivers by the Exclusion of Town Sewage," first points out the main causes of actual death, and then proceeds to advocate the claims of the well-known A.B.C. process, exhibited by the Native Guano Company. For this he claims success, greater than that which has attended any such known chemical method, all at present pointing to irrigation and intermittent-downward-filtration, as the best solution of the difficulty. All the artificial breeding in the world cannot be of avail in waters thus becoming more deadly, and to the chemist the utilisation of waste offers a good field for work. There are other doubtful points about this paper, beyond the limits of a short notice, but it is sincerely to be hoped that when the present inquiry into the London sewage question terminates, the adoption of some treatment beneficial to our waters may perpetuate its action.

Closely allied are the interests of "Forest Protection," advocated by Mr. D. Howitz, the more especially as there is evidence to show that the disappearance of salmon has been at times associated with the clearing of forests. The author points out that, while the question has naturally more interest for other countries than our own, it is possible to maintain throughout the year, by the interaction of natural forces, a better equilibrium of life in shallow water. Although much yet remains to be done in this work, the arguments adduced are practical and weighty. The author advises the use of certain trees as being, from his own experience, preferable, the question of growth of timber not being overlooked.

All the aforementioned papers point indirectly to the "outcome" of the present movement, in so far as they suggest methods of improvement. Those which remain are either directly addressed to that subject itself, or to others demanding immediate attention.

Prof. Leone Levi brings forward a mass of statistical knowledge upon "The Economic Condition of Fishermen," stated to be "generally unsatisfactory." The paper abounds in useful information, not the least important being that which deals with the relationships existing between boat-owners and fishermen; the author also states that at present the workers are in proportion excessive "to the amount of production," and wisely recommends a "weeding" of those parasites—neither fishermen nor fools—said to exist. The "fortunes of the fisheries and agriculture in the last twenty years" are significantly compared; but this and other matters dealt with are beyond the limits of our present notice.

In "The Principles of Fishery Legislation" the Right Hon. G. Shaw Lefevre, proceeding to deal with the sea fisheries, exclusive of Crustacea and littoral forms, recalls the circumstances which led up to the passing of the Sea Fisheries Act of 1868—the result of an inquiry before a Commission of which he was himself a member. This Act, essentially one repealing restrictive legislation and giving increased liberty, has lately, as our readers doubtless know, been much under discussion, and the statistics here brought forward speak for themselves as to the wisdom and successful working of the laws then laid down. When we consider the state of the question, as reviewed by the author, we must admit that to alter would be to mar such statutes as these, unless prompted by fresh acquisitions to our knowledge. Speaking of the littoral species, the author shows that restrictive action has exercised no beneficial influence whatever upon our oyster fisheries, and in connection with this subject good evidence has been brought before the Conference to show that actual harm has often been done by premature legislation. These considerations all point to a conclusion, reiterated again and again in the papers before us, and affording consolation to all save a small faction, which pleads injury, but for what reason we know not. This valuable paper is supplemented by one upon "The Basis for Legislation on Fishery Questions," by Lieut.-Col. F. G. Solá, Secretary to the Spanish Commission. Much of this paper is necessarily taken up in discussing Spanish fisheries, but the moral points in the direction indicated above. Speaking of "an absolutely restrictive system," the writer ably remarks that, "under the shade of those abuses established, recognised, or tolerated by former laws, there will have grown up a crowd of well-to-do interests, which it is not possible to disregard." These words and those which follow, will bear all the consideration we can give them.

Setting aside the popular sensational aspect of the "Fish Markets" question, of which those in authority have lately heard enough, that of "Fish Transports and Fish Markets" demands early consideration and prompt action. His Excellency Spencer Walpole, in dealing with it, confines himself to that "internal traffic" in which lie many sources of evil. Speaking of the necessity for railway reform, the author does not, as might be imagined, advocate State management, but seeks solution of the "suicidal policy" now existing, by insuring—between land and water carriage—a "healthy competition." All we can hope is that the matter may be thus easily rectified, meanwhile the fact remains that the future of great and important fisheries must depend upon the issue. The author enters into a discussion of the market question, but as so much concerning this rests with the City Corporation we await their views. Despite the protest lodged by Mr. Sayer on p. 20, we cannot but regard the silence of, and want of concerted action among, the Billingsgate men, as an unhealthy sign.

The perils of a fishing life are patent to all, and when we hear a cry raised on all hands for increased harbour accommodation, and read that the failure of our fisheries is often due to want of weather forecasts, it is obvious that an important claim is established. Mr. Scott, in a paper on "Storm Warnings," brings a well-known experience to bear upon this matter, and compares our own condition and apparatus with those of other countries, notably the United States, Germany, and Holland. Our greatest need at present is shown to be want of observatories on the west coasts of Ireland and Scotland, and the author points out the significant fact that "storm signals are hoisted at 111 stations only over the whole United States, while we in these islands have nearly 140 for a much smaller area." Speaking of the famed American storm-warnings, the need of mid-ocean observatories is discussed, as the storms almost invariably "change their character *en route*." Much other valuable information is embodied in this paper.

Prof. Lankester, writing on "The Scientific Results of the Exhibition," after making some admirable remarks about the "so-called practical man" and other topics, sets up a plea for a zoological observatory or "station." While no one will fail to enter into the spirit of his paper, we are of opinion that the plan—as concerning fisheries alone—need not be so elaborate as that suggested by him. No subject has created a greater revolution in the minds and actions of fishermen of late, than the discovery of Profs. Sars and Malm that the eggs of certain of our deep-sea fishes develop at the surface, and even were this not so no one would gainsay Prof. Lankester's cry of "more zoology." When we read that "the herring fishery is a lottery," and that simply because we know nothing of the real nature and causes of the movements of those fishes, it is quite obvious in what direction our earliest observations must be pursued. For this purpose a transportable zoological laboratory, with proper boats and appliances, such as that used in the recent successful experiments in the Netherlands, would amply suffice, and we conceive of such as best embodied in "A National Fishery Society," for which Mr. Fryer urges a strong, and it seems to us an exceedingly just, claim. All modern advance in the fishing industry points to the conclusion that Governmental action must be slight but firm; this being so, both common sense and precedent show it to be absolutely necessary that some such mediating body as that which the author would have established, should exist. Such a society would, of necessity, acquire in time all necessaries for work and progress, but, until this stage at least is reached, Britain—whose waters are second to none—cannot hope to hold her own in the matter of International Fisheries. We heartily recommend our readers to reflect upon a speech, made by Mr. Birkbeck, M.P., Chairman of the Executive Committee, which follows the aforementioned paper.

Such are the aims and scope of the Literature of the Great International Fisheries Exhibition, and when the remaining publications are forthcoming it will form a collection upon which both the fishermen and all concerned must be congratulated. It has been impossible to do more than indicate the general line of work in this brief notice, no note having been taken of the extent to which certain papers overlap; it will be obvious, however, where abuse lies, where reform is needed, and along what lines the expected "outcome" must proceed.

The style of these books, produced by Messrs. W. Clowes and Sons, leaves nothing to be desired; the few typographical errors which occur being unavoidable in dealing with the technicalities of such an extensive subject.

NOTES

THE adjudication of medals for the present year by the Council of the Royal Society is as follows:—The Copley Medal to Prof. Sir William Thomson, F.R.S., for (1) his discovery of the law of the universal dissipation of energy; (2) his researches and eminent services in physics, both experimental and mathematical, especially in the theory of electricity and thermodynamics; a Royal Medal to Prof. T. A. Hirst, F.R.S., for his researches in pure mathematics; a Royal Medal to Prof. J. S. Burdon-Sanderson, M.D., F.R.S., for the eminent services which he has rendered to physiology and pathology, especially for his investigation of the relations of micro-organisms to disease, and for his researches on the electric phenomena of plants; the Davy Medal to Marcellin Berthelot, For. Mem. R.S., and Prof. Julius Thomsen for their researches in thermo-chemistry.

PROF. HUXLEY and Sir Joseph Hooker having been elected members of the Salters' Company, were present at a dinner given by the Company on Tuesday evening, and both took praiseworthy advantage of the opportunity to remind our "City men" of some wholesome truths. Prof. Huxley said he had no doubt that an immense field of usefulness lay open for the Guilds and the Corporation of London. Happily it was a field which was not altogether unploughed, and one in which the road had been practically shown towards doing an immense amount of good. He wished to express an opinion which he had formed with great care, and which he uttered with a full sense of responsibility, that the work which had been undertaken in the name of the City and Guilds of London, and which had at present resulted in the foundation of an institute for technical education, was one of the greatest works, if properly comprehended, which had ever been taken in hand, whether they viewed it with reference to the commercial prosperity of the country, to its social organisation, or to the preservation of the condition of political equilibrium; for at the present time the wealth and prosperity of the country were a cloud generated out of the application of physical science, and taking that science away the cloud would vanish like any other baseless fabric of a vision. The future predominance of the commercial power of England depended upon whether its merchants had the wisdom to appreciate the gifts which science gave them. If, however, these elements were disregarded, London would perish as surely as Carthage. The social state and the preservation of the condition of political equilibrium depended, he argued, upon a proper knowledge of science. The institution to which he had referred provided for all those requirements, and it was one of the greatest privileges of the office which he at present held that he should be associated with those engaged in the organisation of this system, and who, he trusted, would carry on the enterprise to a successful conclusion.

THE death is announced of the well-known American mineralogist, Mr. Lawrence Smith, at Louisville, Kentucky. Mr. Smith devoted himself mainly to the investigation of meteorites, and did much to increase our knowledge of these bodies. He was a corresponding member of the Paris Academy of Sciences.

CAPT. DAWSON and party of the British Circumpolar Expedition, which wintered at Fort Rae, Great Salt Lake, arrived safe and well at Winnipeg on November 2, having succeeded in crossing the height of land at Portage la Loche before the closing of the navigation by ice, which some of the resident authorities of the Hudson's Bay Company in the north-west thought they would be unable to do if detained on Slave Lake until the end of August.

M. CHARCOT, the chief surgeon of La Salpetrière, in Paris, has been nominated member of the Academy of Sciences.

IT has beeen arranged that the tercentenary of Edinburgh University shall be celebrated on April 16, 17, and 18 next.

THE results of the late Cambridge higher local examination were very discouraging as regards Group E (Natural Science). Only two out of sixty-six candidates gained a first class, and thirty-one failed. The following are extracts from the Examiners' reports:—Elementary Paper: The answers indicated an imperfect comprehension of principles, and an inadequate practical acquaintance with the subject-matter of the various sciences. In Chemistry the papers as a whole were markedly inferior to those of last year, showing want of knowledge of any practical arrangements for the simplest experiments. In Physics the work of all the candidates was very poor. The general want of clearness and definiteness of expression was very noteworthy. No marks were gained for answers to the numerical questions, and in but few cases were they attempted. In Physical Geography and Geology the answers were on the whole unsatisfactory. The candidates seemed to have studied the subject chiefly in books, for though one or two showed proofs of having acquired some practical knowledge in the Museum, nearly all, when describing the physiography and stratigraphical geology of an English district, gave indications that their knowledge was gained by reading, and not by actual observation in the field. In Physiology the answers of different candidates were very unequal. Some were extremely good, while a considerable number showed ignorance of the most rudimentary facts. There was very little evidence of a personal acquaintance with minute anatomy. In Zoology most of the answers were characterised by vagueness, want of precision, and a marked, often grotesque, ignorance of the meaning of the most ordinary technical terms. The reading of most of the candidates seems to have been very diffuse and unintelligent, while not one of the candidates had any real grasp of the principles of the subject. In Botany the answers were very weak. They indicated a tendency to neglect the external morphology and anatomy, and to pass on to special morphology and life-histories of the lower forms before the above-named branches of the subject had been properly mastered.

AMONG the lectures to be given at the London Institution during the coming season are the following:—December 3, Mr. G. J. Romanes, F.R.S., Instinct; 6, Rev. W. Green, the High Alps of New Zealand; 13, Prof. G. W. Henslow, the Glaciers of the Alps; 20, Prof. W. H. Flower, F.R.S., Whales; 27, Prof. H. Armstrong, F.R.S., Water (juvenile lecture); 31, Dr. Rae, F.R.S., the Eskimos and Life among them. January 3, Dr. Donald MacAlister, How a Bone is built; 7, Mr. H. Seebohm, Arctic Siberia; 10, Mr. Alfred Tylor, Celtic and Roman Britain; 17, Mr. H. Dixon, Explosives. February 7, Mr. Norman Lockyer, F.R.S., the last two Eclipses of the Sun; 18, Mr. J. Bryce, M.P., D.C.L., an Ideal University; 21, Prof. R. S. Ball, F.R.S., the Doctrine of Evolution applied to the Solar System; 25, Dr. E. B. Tylor, F.R.S., the Three Sources of History—Records, Monuments, and Social Laws. March 6, Prof. Schuster, F.R.S., the Aurora Borealis.

HERR CARL ROHRBACH of Leipzig has lately described a method of procuring a fluid having extraordinarily high refractive and dispersive powers. 100 parts of iodide of barium are mixed with 130 parts of scarlet biniodide of mercury. About 20 c.c. of distilled water are added to the powders, and they are then stirred up with a glass rod while heated in a test tube plunged into an oil bath previously warmed to 150° or 200° C. A fluid double iodide of mercury and barium is formed, which is then poured into a shallow porcelain dish and evaporated down until it acquires a density so great that a crystal of epidote no longer sinks in it. When cold, even topaz will float in it. It is then filtered through glass-wool. The fluid so prepared has a density of 3·575—3·588, boils at about 145°, and is of a yellow colour. Its refractive index is 1·7755 for the C line, and 1·8265 for the E line of the spectrum. For the two D lines of sodium the refractive indices are 1·7931 and 1·7933 respectively. So great is the disperson that, using a single hollow prism with a refracting power of 60°, the dispersion between the two D lines is almost exactly 2′ of angle.

THE latest official report of the Imperial German Post Office states that at the end of October the telephone was fully in operation in the following thirty-six cities and towns, within the Imperial postal territory (which does not include Bavaria or Würtemberg):—Aix-la-Chapelle, Altona, Barmen, Berlin, Beuthen, Brunswick, Bremen, Bremerhaven, Breslau, Burstcheid, Charlottenburg, Chemnitz, Cologne, Crefeld, Deutz, Dresden, Düsseldorf, Elberfeld, Frankfort-on-Main, Gebweiler, Geestemünde, Hamburg, Hanover, Harburg, Kiel, Königsberg, Leipzig, Magdeburg, Mayence, Mannheim, Mülhausen (in Alsace), Potsdam, Stettin, Strasburg, Sulzmatt, and Wandsbeck. In four other towns—Halle, Karlsruhe, M. Gladbach, and Rheydt—the arrangements for its introduction have progressed so far that it will most probably be in operation in them before the end of this year. It is therefore likely that by the end of 1883 forty towns within the Imperial German postal territory will possess the advantages of the telephone, against twenty-one last year, and seven in October, 1881.

THE programme of the Yorkshire College Students' Association for the present session is a varied and interesting one. A "Yorkshire College Photographic Club" has recently been formed, and has already a good roll of members, including several members of the College staff. A prize competition has been arranged, and the Society has every prospect of success. The secretary of the Photographic Club is Mr. W. O. Senior.

ONLY six months ago a Society of Natural Science was formed at Bournemouth, and already it has 103 members, the president being Prof. Allman, F.R.S. The Society being established upon the most comprehensive basis, recognises every department of physical science as coming within the scope of its investigations. It is open to all, without limitation of class or sex. During the past session various papers have been read, and during the summer months bi-weekly morning and evening walks were taken under the leadership of the appointed heads of sections for botany, entomology, marine and terrestrial zoology and geology. The Committee contemplate devoting part of its funds to be awarded annually as prizes for the best and most systematically arranged collections of natural history specimens, made solely by each exhibitor, as an inducement to the younger members to cultivate habits of careful observation and systematic study of nature. The Society held a very successful *conversazione* on the 7th inst. at Bournemouth, and so attractive was the exhibition connected therewith, that it was kept open the following day. Captain Hartley, chairman of the Bournemouth Improvement Commission, opened the *conversazione* by giving some account of the origin and objects of the Society. The exhibition was of a very varied and instructive character, and at intervals during the day short popular lectures were given on such subjects as air, sound, the moon, natural magic, while the Rev. G. H. West exhibited and explained from time to time various apparatus illustrating physical phenomena. Altogether the Society gives promise of a successful career.

MR. GEORGE MURRAY will deliver a lecture on the potato disea e at the Parkes Museum of Hygiene, 74A, Margaret Street, Regent Street, on Thursday, the 22nd instant, at 8 p.m.

SEVERAL members of the French Chamber of Deputies having contended that the transmission of telegrams was not so easy with underground wires as with aërial lines, M. Cochery has invited a number of opponents and electrical engineers to demonstrate on the lines now in existence, that the difference, if there is any, is quite immaterial.

AT a recent meeting of manufacturers and artisans convened by the Mayor at Coventry, resolutions were enthusiastically carried in favour of the adoption of a system of technical education in the city. It is proposed to provide a building for the consolidation and extension of the science classes, a lecture-hall, and reading-room, with a reference library of works appertaining to trade and manufactures, and to establish in connection with these three workshops for the practical teaching of mechanics (toolmaking, weaving, and watchmaking). It is estimated that about 4000*l*. will be needed for the building, and 3000*l*. for the fixtures and equipment of the building and workshops, in addition to which it will be necessary to provide an annual income of at least 300*l*. Subscriptions and donations exceeding 1000*l*. were promised at the meeting.

THE piercing of the Arlberg Tunnel, which will be 10,270 metres long, thus ranking third in the world, was expected to be completed to-day. The work began on November 13, 1880, on the western and eastern sides simultaneously, and has therefore lasted just three years, instead of four, as was calculated. Special trains will bring over two hundred invited guests from Austria, Italy, and Switzerland, to witness the final boring and the connection of the two galleries.

MR. G. J. SYMONS writes to the *Times* to say that Miss Eleanor Nunes, who had been keeping an extremely accurate record of the fall of rain at Langtree Wick, Torrington, Devon, died last spring, having left the sum of 100*l*. to him "to be applied to meteorological purposes." Mr. Symons announces that he is prepared to consider applications from all parts of the kingdom for rain-gauges to be sent gratuitously on loan subject to very easy conditions, and to send them to all accepted applicants who reside five miles from any rain-gauge now at work, and the same distance from any other applicant.

THE *Romando* has arrived at Cherbourg after a journey of two months, from Cape Horn. The results of the wintering have been important, and the crew is in good health.

THE diminution of credit rendered inevitable by the state of French finances will bear very little on the Budget of Public Instruction; the work of building the Meudon Observatory will not be stopped, and is proceeding favourably.

WE learn from a trustworthy source that there is again talk of transporting the Paris Observatory to some distance from the city, to a site in the vicinity of the new Flammarion Observatory.

THE Portuguese Government has appointed the explorers Capello and Ivens to proceed again on an expedition to West Africa, for the purpose of completing their map of the province of Angola, and of exploring the Congo. The explorers will leave by the packet on December 6.

NEWS has reached Europe of the assassination of M. De Brazza, but it is conjectured that this is the French explorer's brother, and not the explorer himself.

IN our note on the Royal Society last week, Dr. Warren De La Rue's name was given incorrectly.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♂) from India, presented by Mr. C. R. Browne; two Red-tailed Guans (*Ortalida ruficauda*) from Tobago, West Indies, presented by Mr. Alfred C. Priestly; two Gold Pheasants (*Thaumalea picta* ♂♂) from China, presented by Mr. H. W. Tyler; two Bar-breasted Finches (*Munia nisoria*) from Java, presented by Mr. J. Abrahams; a Kestrel (*Tinnunculus alaudarius*), British, presented by Mr. John Colebrook, F.Z.S.; two Long-eared Owls (*Asio otus*), European, presented by Mr. C. Purnchard; a Masked Parrakeet (*Pyrrhulopsis personata*) from the Fiji Islands, presented by Miss J. D. Smith; two Alligators (*Alligator mississippiensis*) from the Mississippi, presented respectively by Mr. Roland Bridgett and Mrs. M. E. Symons; a Peregrine Falcon (*Falco peregrinus*), European, a Goffin's Cockatoo (*Cacatua goffini*) from Queensland, deposited; a Bennett's Wallaby (*Halmaturus bennetti* ♀) from Tasmania, two Black Wallabys (*Halmaturus ualabatus* ♂♂) from New South Wales, a Yellow-footed Rock Kangaroo (*Petrogale xanthopus* ♂) from South Australia, a Mexican Eared Owl (*Asio mexicanus*) from Mexico, a Downy Owl (*Pulsatrix torquatus*) from South America, an Annulated Worm Snake (*Vermecilla annulata*) from Western Australia, purchased.

MOVEMENTS OF THE EARTH[1]

II.

Measurement of Time

IT has been shown how, by the application of geometrical and optical principles, the measurement of angular space has been carried down to the 1/100th of a second of arc, such a quantity being 1/129,600,000th part of an entire circumference, and when such an accuracy as this has been attained, and the altitude or the azimuth of the sun, or moon, or any other heavenly body can be correctly stated with this exactitude, it will be seen how much better off in the way of defining positions is the modern astronomer than was Hipparchus with his 1/3rd, and Tycho Brahé with his 1/4th of a degree. To do this, however, is not enough. It is not only necessary accurately to define the position of a heavenly body, it is necessary also to know at what particular time it occupied that position. The next thing to be done, then, is to see how far we moderns have got in another kind of measurement, no longer the measurement of arc—the measurement of angular distance—but the measurement of *time*.

The measurement of time, however, is not quite so simple a matter as was the measurement of space. A certain angular measurement of space, or the angular distance between two bodies, whether that distance be a degree, or a minute, or a second, is a very definite thing, having a beginning and an end; but time, so far as we can conceive, has neither beginning nor end; so that the problem of the measurement of time has to be attacked rather in a different way. Here again it will be as well that the matter should be studied historically.

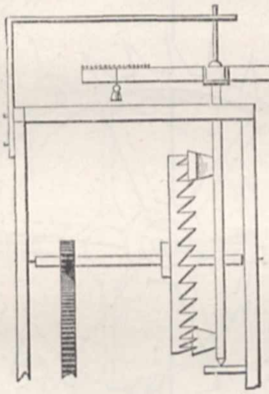

FIG. 18.—Ancient Clock Escapement.

What more natural than that man having got the idea of the flow of time, should have begun to measure it by the flow of water, or the flow of sand? The earliest time measurers were really made in this way; water or sand being allowed to drop from one receptacle to another. There were difficulties, however, in thus determining the flow of time. In the first place the thing was always wanting to be wound up, so to speak, something was wanted to continue the action, and to prolong it; and the first appeal to mechanical principles was made with that view.

The first real clock put up in England was put up in Old Palace Yard, in the year 1288, by the Lord Chief Justice of that time, who had to pay the expense of it as a fine for some fault he had committed. Its construction was somewhat after this wise. One method of dealing with the flow of time was to call in the aid of wheelwork; but, as is well known, if a weight acts upon a train of wheels the velocity increases as the rotation goes on. Therefore the science of mechanics was called in to supply some principle which could be applied to prevent this unequal velocity of a train of wheels. Consider the arrangement shown in Fig. 18.

The wheelwork train is capable of being driven by a falling weight. On the same axis as the smallest wheel, and therefore the one which turns most rapidly, will be seen another wheel provided with saw-like teeth. Then at the top is a weighted cross-bar, from the centre of which a perpendicular rod, provided with pallets, comes down to engage the teeth of the pallet-wheel. Now suppose the clock to be started. The weight is allowed to fall, and the wheels, including the pallet wheel, begin to revolve; then begins a reciprocating action between the swinging bar and the wheel with which it acts, because the pallets which act on the bar as they are on either side of the centre of motion really drive the bar first in one direction and then in the other. The teeth of the pallet wheel are continually coming into contact with the pallets of the swinging bar. First suppose that one of the teeth has encountered the upper pallet; it pushes this aside, and swings the bar in one direction. No sooner, however, has this been done than another tooth in the wheel at the bottom of the bar encounters the pallet and swings it in the opposite direction. In this way it is obvious that the bar is continually meeting and being met by the teeth of the rotating wheel, swinging first in one direction, and then in the other, the result of this reciprocal action being to prevent the increase in the velocity of the wheels which would otherwise take place.

It is in this way, then, by the performance at constant definite intervals of an equally constant definite amount of work, that the regularity of action of the clock is produced. The greater the distance of the weights on the cross-bar from its centre of motion, the longer will the bar take in swinging, the slower will be the action of the clock; so that the clock may be regulated by altering the position of these weights, bringing them nearer to, or removing them further from the centre of motion of the bar, according as it is desired to hasten or retard the action of the clock's mechanism. Yet at whatever distance from the centre of motion the two weights be placed, assuming always that they are both at the same distance from it, there is still this constantly-recurring performance, at equal intervals, of an equal amount of work which produces the regular action of the clock. This was the kind of clock then which was put up in Old Palace Yard. But that did not go well enough, giving such inaccurate results that Tycho Brahé had to discontinue its use. Fortunately some few years later two most eminent men, Galileo and Huyghens, had their attention drawn to this very problem. The first of these, Galileo, was at that time studying medicine. He happened one day to be in the Cathedral at Pisa, where, it will be remembered, they have a most beautiful lamp which swings from a great height in the cathedral. Galileo was at this time working at that branch of his medical studies which deals with the pulse, and he looked at this lamp and found that its swinging was perfectly regular. To-day perhaps it may seem very natural that this should be so, but Galileo had the advantage of being before us, and that is why it did not seem quite so natural to him. There was at that time no known reason why it should swing in perfect regular rhythm. He found that the lamp when swinging, no matter with what amplitude, took practically the same time for each swing, timing it by his pulse. His idea was that this would be an admirable method of determining the rate of a man's pulse, and the first clock on this principle was constructed from that medical point of view, being called a Pulsilogium. Some years afterwards, however, the extreme importance of such an arrangement from an astronomical standpoint became obvious, and very much attention was given to it. It is unnecessary to add that this swinging body is nowadays called a pendulum. The most perfect pendulum made in those early days is represented in Fig. 19.

The fundamental difference between that and the modern pendulum is that part of the pendulum between S and A was elastic. It was made elastic for the reason that although Galileo could not find any difference between the times of the oscillations of the lamp in Pisa Cathedral, according as its amplitude of swing was large or small, yet such a difference did exist, although it was only a slight one; and the only method of getting a perfect pendulum which should make its swing in exactly equal times, independent of its arc of oscillation, was to construct this so-called cycloidal pendulum. It was so named because in its swing its elastic portion was held by the curved guides seen in the figure, and made to bend in that particular curve. By this means the pendulum instead of swinging through the arc, K U R, was made to oscillate through D U L. But when the pendulum was at the points D and L, it was practically a shorter pendulum than when at rest. In other words, whilst the pendulum was swinging from U to D and from U to L, its curvature, and consequently its vibrating length was continually changing. In that way, by continually varying the length of the swinging part, it was found possible to make a pendulum which, independent of the length of its arc of oscillation, would make its swing in times which for all practical purposes were absolutely equal in length. That was the most

[1] Continued from vol. xxviii. p. 604.

perfect pendulum of that time. Nowadays, the cycloidal pendulum has been replaced by one which swings through a very small arc, and the continual shortening during the oscillation in the cycloidal pendulum is by this means dispensed with, whilst the friction also being much reduced, there is less interference from that source. With this very small swing the difference between the arc of the circle described and the cycloid in which the cycloidal pendulum swung is practically indistinguishable.

The great difference between the modern clock and the ancient one is that in the former the pendulum is interfered with as little as possible whilst swinging, and makes each swing under precisely similar conditions. To attain this is to have done much. In the first place, if the clock has a heavy weight, that weight will probably interfere a good deal with the swinging of the pendulum. The clockweight, therefore, must be as light as possible. Secondly, if the wheelwork is always in contact with the pendulum, this also will interfere with its free and natural movement. There must be, then, such an arrangement that the wheelwork shall be brought into contact with the pendulum only for the shortest possible time. Thirdly, it must be remembered that the different substances which it is most convenient to use in the construction of pendulums, vary their dimensions with the variations of the temperature and moisture of the air in which they are placed, and great care must be taken to eliminate any errors which might arise from such a source. How are these various conditions complied with? The first, that the clockweight must be small, is not difficult to adhere to; but it will be well to consider the way in which the second condition, that the action between wheelwork and pendulum shall be the least possible, is met. This is done by employing what is called an escapement. It is so named because the pendulum in its swing is allowed to escape from the wheelwork, and thus retain a perfect freedom. The particular form of escapement about to be described is that which, for a reason that will appear immediately, is called the dead-beat escapement (see Fig. 20).

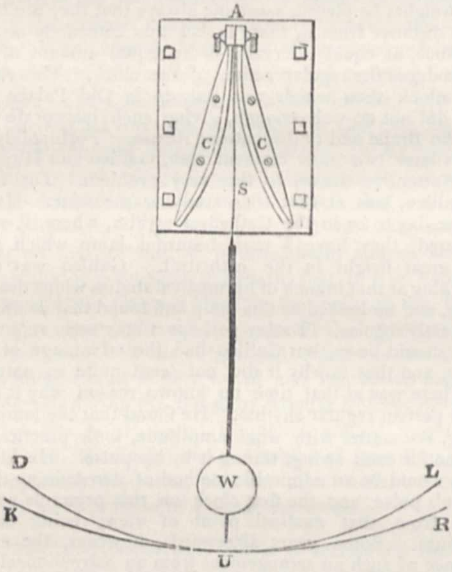

FIG. 19.—Cycloidal Pendulum.

The escape wheel is the modern representative of the toothed wheel of the old clock, whilst the projections W and D are modifications of the pallets on the swinging bar in that instrument. Let the pendulum move in the direction of the arrow. The tooth T has just been released, thus permitting the tooth V to engage the other pallet D. Now whilst the tooth remains on the pallet, the escape wheel remains locked, while the pendulum is quite free to swing, there being nothing to retard it save the very slight friction between the tooth and the surface of the pallet. The rotation of the escape wheels, however, brings the tooth on to the oblique edge of the pallet, and with it in this position the pendulum is aided in its forward swing. Then the pallet escapes, receiving an impulse, but since this is received almost as much before the pendulum has reached its vertical position as after it has passed that point, no increase or diminution in the time of its oscillation takes place. It is in this way that the second of our conditions is complied with, the wheelwork being effectually prevented from interfering with the regularity of the pendulum's swing. It is called the dead-beat escapement, because when the tooth falls on the circular portion of the pallet and locks the escape wheel, the seconds-hand fitted to it stops dead without recoil, because the arc of the surface of the pallet is struck from the centre of motion. In an astronomical clock a still more modern form of escapement, called the gravity escapement, is sometimes employed.

It will perhaps be convenient at this stage to compare the fineness of the division of time given by a clock of this description with the fineness of the division of the second of arc we have already discussed. There is, however, a little difficulty about this, because at present there seems to be no special reason why any particular unit of time should be selected. Ordinarily a day is divided into twenty-four hours, each of these twenty-four hours is subdivided into sixty minutes, these again being each divided into as many seconds. The origin of this division of time will be seen later on; for the present let the fact remain that it is so.

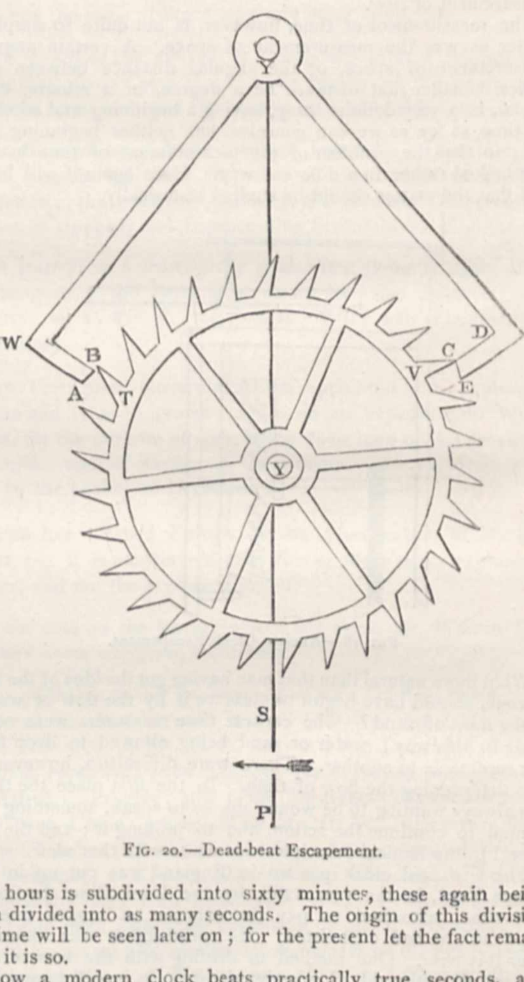

FIG. 20.—Dead-beat Escapement.

Now a modern clock beats practically true seconds, and astronomers after a little practice gain the power of mentally breaking that second up into ten divisions, each of which is of course one-tenth of a second, so that we can say that a day may be divided into 864,000 parts, and in this way institute a comparison of the fineness of the division of time with those minute measurements of angular space with which we so recently dealt.

It is a familiar fact that the length of a pendulum which vibrates seconds is some thirty-nine inches, and it is easy to understand that there are many conditions in which a clock of this kind, with its pendulum of more than a yard long, cannot be used. Not only indeed is there this inconvenient length of the pendulum, but it is necessary that the clock to which it belongs should be rigidly fixed in an upright position. The question therefore arises, is this clock which deals out seconds of such accuracy the only piece of mechanism that can record and divide our time, or is any other time-measuring instrument available? Fig. 21 shows part of such an instrument, known as the Chronometer, in which, whilst the principles necessary to be followed in the construction of the clock have been adhered to, the pendulum has been dispensed with, and the perfect stability and verticality of position so important to the clock, are here unnecessary.

In this instrument the pallets of the dead-beat escapement have been replaced by a detent, D. Let us consider the action. The escape-wheel, S, is advancing in the direction of the hands of a clock. One of its teeth meets the detent, and the wheel is locked. Then what happens is this: when the balance-wheel, R_1, swings, the circle, R_2, centred on it shares its motion. This, it will be seen, is armed with a little projection.

We left the escape-wheel locked. Now assume that the balance-wheel is swinging in the direction of the arrow. It carries the small circle with it, and the piece, P_2, in its motion, coming into contact with the end of the spring, seen projecting beyond the arm of the detent, raises it and the detent, so releasing the tooth of the escape-wheel. The slight retardation which the balance receives in consequence of this action is immediately compensated. The moment the escape-wheel moves on again, one of its teeth meets the projection, P_1, and the balance-wheel receiving this fresh impulse goes on to complete its swing. Then it returns and swings in the opposite direction, this time without acting in any way on the detent. When the balance-wheel made its first swing and the point P_2 met the projecting end of the spring, the

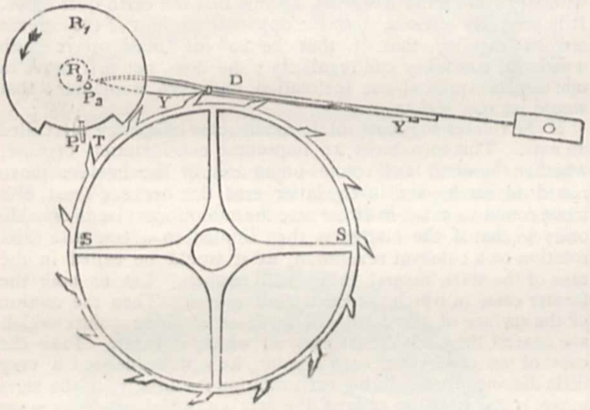

Fig. 21.—Chronometer Escapement.

latter could then only bend from the end of the arm with which the detent is provided and against which the point P_2 forced it. But on the return swing the spring is found capable of bending from the more distant point of its attachment to the shank of the locking-piece. It is therefore easily pushed aside; there is no change in the position of the detent, nor is any resistance offered to the motion of the balance-wheel, which goes on to complete its swing. Then another tooth is caught, the escape-wheel is again locked, and again released by the lifting of the detent. So the action goes on, the teeth of the escape-wheel being constantly detained and as constantly released by the action of the point P_2. The balance-wheel, it will be noted, receives its impulse only at every alternate swing, whereas in the clock the pendulum receives its impulse at each vibration.

Time then can be divided down to the 1/10th of a second, or as we expressed it, down to the 864,000th part of a day, not only by a clock, but also by this chronometer. Having obtained this 1/10th of a second by these instruments, the question arises as to whether it be possible to get a still finer division. It will be seen that a very much finer division than this can be obtained, the 1/100th part of a second being a measurable quantity; not that such a small fraction of time as this is ever necessary in astronomy, nor will it be until the present astronomical methods have ceased to exist. If it were possible to get all observations made by photography, then it would be worth while recording with such minuteness, because photography would always behave in the same way, whereas two observers never have the same idea as to the time of occurrence of any phenomena which they observe. Yet, although so great an accuracy as this is not attempted, it will be quite worth while to consider the means by which this exquisite fineness of the division of a second of time has been arrived at. We shall see that just in the same way as an appeal to mechanical principles resulted in an improvement in the construction of our clock, so this fineness in the division of time has been obtained by an appeal to the principles of electricity. Let it be assumed that the seconds pendulum of our clock swings with perfect accuracy and with absolute uniformity from second to second, in spite of changes of temperature and other perturbing influences; and having assumed this, let us see how electricity can be made to aid in the measurement of time. The instrument used is called a chronograph. It consists of a metal cylinder revolving by clockwork and covered with cloth, over which a piece of paper can be stretched. Below the cylinder and parallel with it is a track along which a frame carrying two electromagnetic markers or prickers is made to travel uniformly by the same clock that drives the cylinder. Wires connected with a battery lead from one of these magnets to a clock and from the other to a key, which can be depressed whenever an observation is made, and a current so sent to the magnet. The effect of this is to cause it instantaneously to attract its iron armature and cause the pricker with which it is connected to make a mark on the paper above.

The connection of the chronograph with the clock is as follows:—The bearing shown in the middle of the diagram (Fig. 22) is a continuation of the bearing on which the seconds hand of the clock is supported, and there is a little wheel which does its work quietly at the back of the clock in exactly the same way that the seconds hand does its work quietly in front of it. What

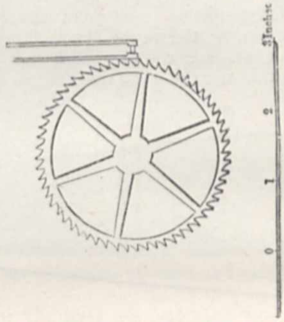

Fig. 22.—Electrical contact apparatus at back of clock.

that wheel does is this. Every time that each of its teeth—and there are sixty of them—comes to the top of the wheel it touches a little spring. That little spring then makes electrical contact, and a current is sent flowing through parts of the apparatus already described. Now the teeth in that wheel, being regularly disposed around its circumference, always succeed one another after exactly the same interval of time, and there is no difference or distinction from second to second, or from minute to minute. But suppose that before the clock is started one of these teeth is filed off, and so filed off that when the seconds hand points to 0 seconds, and the minute hand to a completed minute, this part of the wheel shall be at the top, and there shall be no electrical contact established, for the reason that the tooth of the wheel is not there to act on the spring. In that way it is easy to manage matters so that the beginning of each minute shall be distinguished from all the other fifty-nine seconds which make up the minute. Let the cylinder, covered with paper, revolve once in a minute. In that case, the electrical current will make a hole or a mark on that paper every second, and as matters are so arranged that the prickers shall be travelling along at the time that the dots are made upon the revolving paper they are thus made along a continuous spiral, and since we have supposed the cylinder to revolve once in a minute, the beginning of each minute will be in the same line along the spiral. Then, according to the length of the cylinder, a second of time will be obtained written in dots, sixty of them round the cylinder representing sixty seconds. Suppose now that a man with a perfect eye makes an observation, recording it by sending a current through the apparatus and making a dot on the paper. He will then have an opportunity of observing on the paper the precise relation of the dot which represents the time at which the observation was made to the other dots which represent the various seconds dotted out by the clock, and not only the exact distance of the observation prick from the nearest *second*, whether it be ½, or 1/10th, or 1/100th of the distance between that second and the next, but the omission of the record of the first second in the minute will give the relation that observation has to the nearest *minute*.

For the sake of simplicity the case of one observer making one observation has alone been considered; but if the work be properly arranged, then not only one electromagnet, but two, or three, or four, may be at work upon the same cylinder at the same time, each making its record, and that is how such work is being done at the Greenwich Observatory.

Observing Conditions

This power of measuring and dividing time then having been obtained, we seem to have reached our subject, "The Movements of the Earth." Yet even now there are one or two other matters which require to be discussed before we consider the movements themselves. The first of these is the important fact that the earth is spherical in its form. There have been many views held at different times as to the real shape of the earth, but the only view we need consider is that stated. In going down a river in a steamboat, or, better still, in standing upon the sea-shore at some place, such as Ramsgate, where there are cliffs, and where, consequently, one may get from the sea-level to some height above it, it is observed that when any ship disappears from our view by reason of its distance it seems to disappear as if it were passing over a gentle hill.

It does this in whatever direction it goes. This familiar fact is a clear proof that the earth is a sphere, and is so obvious that it may seem unnecessary to mention it, but it was as well to do so for a reason which will appear shortly. Besides this argument in favour of the spherical shape of the earth there is the argument from analogy: the moon is round, the sun is round, all the known planets are round. The stars are so infinitely removed from us that it cannot be determined whether they also

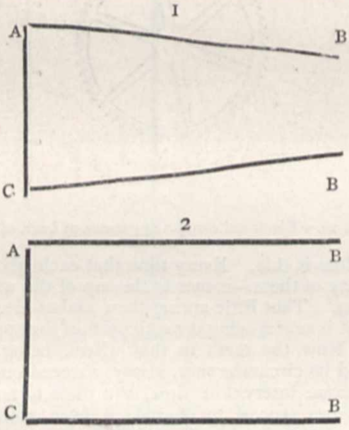

FIG. 23.—Model to illustrate parallax.

are spherical, but doubtless they are as round as the earth. This point of the tremendous distance of the stars is an important one to bear in mind. Their distance cannot be conveniently stated by thousands, nor even by millions of miles, it is something far greater than that. It may be asked why it is that such a statement can be thus positively made. For this reason: the stars have been observed now for many ages, and the historical records of ancient times show that the chief constellations, the chief clusters of stars visible in the heavens now, were seen then. In the Book of Job, for instance, there is a reference to the well-known constellation of Orion, and there is very little doubt that for thousands and thousands of years that constellation has preserved the familiar appearance of its main features. The constellation called Charles's Wain, or the Great Bear, was also known to the ancients. If the stars were very near to the earth this could not be. If they were close to us the smallest motion either of earth or star would at once change their apparent position, and would prevent this fixity of appearance, and the skies would be filled, not with the constellations with which we are so familiar, but with new and ever-changing clusters of stars. This constancy of the constellations, not only from century to century, but from era to era, clearly proves then that the stars of which they are made up must be at an infinite distance from the earth.

Let us consider the question of distance a little further. If two pieces of wood (see Fig. 23) joined together by a cross-piece be taken, a moment's thought will make it obvious that the angles which A B and C B make with the cross-piece A C, will vary with the distance of the body, which can be seen first by looking along A B and then by looking along C B. If these pointers be directed to a very near object in the room, they must be greatly inclined (as in 1). If something more distant be taken, there is less inclination, and if it were possible to sight St. Paul's by looking first along A B and then along C B, there would be still less. And if something at a still greater distance were sighted, say St. Giles's at Edinburgh, the inclination of A B and C B would be still smaller than it was in the case of St. Paul's, because St. Giles's is at a much greater distance. It follows then that in sighting an object so infinitely removed from us as a star, the light from it will be in a condition of parallelism, and A B and C B consequently be placed quite parallel in viewing it (see 2). That is another reason for saying that the stars are at this infinite distance from the earth. Why it is so important to insist on this point will appear very clearly by and bye.

Now suppose that in the centre of this lecture-theatre a little globe were hung to represent the earth, the walls of the theatre and the people in it representing the heavens surrounding the earth. Now in such a case it is clear that the appearances presented would be the same whether the heavens moved round the earth or the earth itself were endowed with motion. Let us, without making the assertion, assume that the earth does move. It is perfectly obvious, since the apparent motions of the heavens are so regular, that if that be so she must move with wonderful constancy and regularity; she does not first move in one direction and at one inclination, and then at another; that would be very serious.

If she rotates she must rotate round some imaginary line called an axis. This introduces an important consideration because, whether the earth itself rotates on an axis or the heavens move round the earth—and in the latter case the heavens must also move round an axis—in either case the motion must be an equable one; so that if the matter is thus limited to a constant axial rotation or a constant revolution, as it would be called in the case of the stars, several things will happen. Let us take the former case, in which the earth itself moves. Then the motion of the surface of the earth will be least at those points which are nearest the ends of the axis on which it turns. Take the case of an observer at such a point, he will be carried a very little distance round during each rotation; similarly, if the stars move, a star near the ends of the axis on which the stars move will be carried a very little distance round during each revolution of the celestial sphere.

Change the position of the man on the earth from the pole to the equator. Then he will be carried a very considerable distance round in each rotation of the earth: similarly with the stars; if they move, a star in the celestial equator will be carried round a very great distance during a revolution. That is the first point. Another point is that if we assume the earth to rotate we must carefully consider the varying conditions which are brought about by the different positions of an inhabitant of the earth under those circumstances. For instance take the case of a man at the equator, he looks at things from an equatorial point of view, and in the rotation of the earth he plunges straight up and straight down. Similarly, if the stars' daily revolution belongs not to the earth but to the stars, to an observer at the equator of the earth they would appear to move straight up and straight down; and now in dealing with this question and endeavouring to ascertain whether it be the earth or the stars which move it is most necessary to consider the relation of the movements or apparent movements of the stars to the place from which they are observed, and in so doing it is found that there is an immense difference between the conditions which obtain at the poles and at the equator with reference to the phenomena which are observable in each case.

Let us take a globe to represent the earth, and let London be considered the central point for our observations. Now at all places on the earth, in whatever direction we look, we see an apparent meeting of earth and sky; and supposing our observation to be made on an extended plain or at sea, the surface of the earth or sea may for simplicity's sake be considered as a plane bounded by the circle where the earth and sky seem to meet. This is known as the circle of the horizon. To represent this a piece of paper may be put over London on our globe (see Fig. 24), and London may be brought to the top. When that has been done, remembering that the stars are placed at so infinite a distance, the horizon which cuts the centre of the earth, and which is called the true horizon, may be considered as being practically the same thing as the small sensible horizon of London, represented by our piece of paper, when at the top of the globe, because the two planes will be parallel. For, whether a star be seen from the equator or from London, owing to its tremendous distance it will appear to occupy the same position in space. Now let the globe be made to rotate, then the inclination of the plane of the horizon of any place, of our horizon of London for instance, is continually changing during the rotation (Fig. 24). An exception, however, must be made with regard to the poles of the earth. At these two points the inclination will be constant during the whole of the rotation.

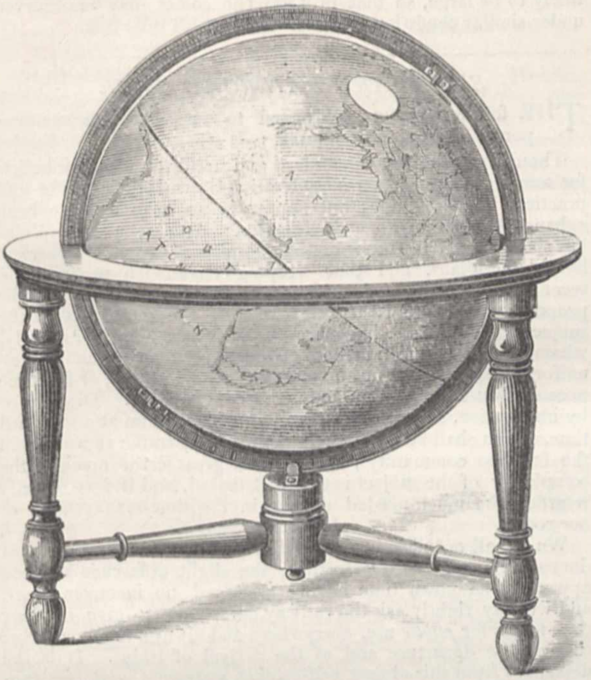

FIG. 24.—Diagram to show how the inclination of the horizon of London will change with the rotation of he earth.

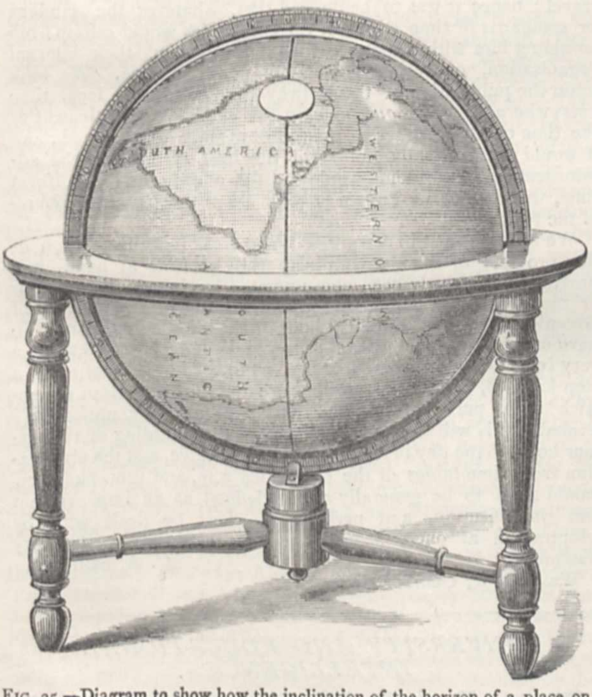

FIG. 25.—Diagram to show how the inclination of the horizon of a place on the equator changes in one direction only.

If now a point on the equator be brought to the top of the globe, it will be seen, as the globe is rotated, that the observer's horizon rapidly comes at right angles to its first position (see Fig. 25). This will show that the conditions of observation at different parts of the earth's surface are very different, and this whether it be the earth or the stars which move.

Let us now consider with a little greater detail the conditions which prevail in the latitude of London. Let London be again brought to the top of the globe. Let O (Fig. 26) represent an observer in the middle of the horizon, S W N E. Let Z be the zenith, which, of course, would be reached by a line starting from the centre of the earth, and passing straight up through the middle of the place of observation. S' is a star, and we want to define its position. How can this be done? Imagine first a line drawn from the observer to the zenith. Imagine next another line going from the observer to the star, or, what is the same thing, from the centre of the earth to the star. Then the angle inclosed by these two lines will give us the angular distance of that star from the zenith, or similarly we may take the angle included between imaginary lines joining observer with horizon and star, and thus obtain the star's altitude.

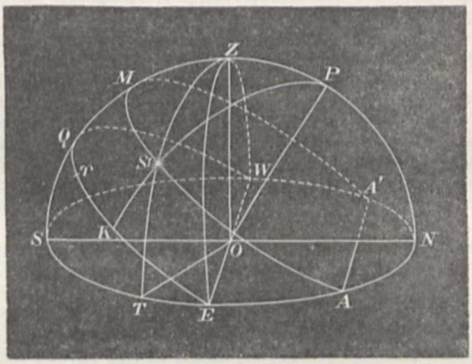

FIG. 26.—Observing condition at London.

Again, its position may be stated not only with regard to the zenith and to the horizon, but to some other point, say the north point. In that case a line or plane, ZEW, is imagined passing from the zenith through the observer, and the distance between E and N will give the star's angular distance from the north point of the horizon. Again, suppose it be desired to define the star's position with reference, not to the zenith, but with reference to the pole of the heavens, that point where the earth's axis if prolonged into space would cut the skies. In that case since P in our diagram marks the position of the pole, a line PS' will give what is called the polar distance of the star; and lastly, if the angular distance of the star from the equator of the heavens be required, since the prolongation of PS' would cut the equator, the distance from S' to the point of intersection will give the angular distance of the star from the equator; in other words its declination.

We have taken London, but of course each place on the earth has its sphere of observation with its zenith and the north, east, south, and west points. With regard to the axes of the earth and the heavens, they both possess north and south points, and in the heavens as in the earth, the equator lies midway between them.

J. NORMAN LOCKYER

(*To be continued.*)

OUR ASTRONOMICAL COLUMN

THE OBSERVATORY, CHICAGO.—Prof. G. W. Hough has issued his annual Report to the Board of Directors of the Chicago Astronomical Society, detailing the proceedings in the

Dearborn Observatory for 1883. The 18-inch Alvan Clark equatorial has again been employed in close observation of the great red spot and other phenomena of the planet Jupiter. Since the first observations at Chicago in September, 1879, it is stated that the red spot had not changed very materially in length, breadth, outline, or latitude. There had been a slow, retrograde drift in longitude, causing an apparent increase in the time of axial rotation. At the last opposition the deduced mean-rotation period was 9h. 55m. 38·4s. against 9h. 55m. 34·8s. in 1879.

Prof. Hough gives the following mean results of micrometrical measures of the red spot:—

	1879	1880	1881	1882
	"	"	"	"
Length ...	12·25 ...	11·55 ...	11·30 ...	11·83
Breadth ...	3·46 ...	3·54 ...	3·66 ...	3·65
Latitude ...	−6·95 ...	−7·14 ...	−7·40 ...	−7·52

The Chicago observer considers that while the spot has remained nearly stationary in latitude, the south edge of the great equatorial belt has gradually drifted south during the late opposition, until it is nearly coincident with the middle of the spot, and further, that "the two do not blend together, but are entirely distinct and separate." A depression formed in the edge of the belt (as shown in two drawings of the planet's disk, on December 29, 1882, and February 20, 1883), which corresponded in shape with the oval outline of the spot, the distance between the two being about a second of arc. The spot was extremely faint at the last observation for longitude on May 5. The equatorial white spot, first observed in 1879, was again visible during the last opposition; the rotation period 9h. 50m. 9·8s. deduced in the previous year, satisfying the observations.

The great comet of 1882 was micrometrically measured from October 4 to November 20, and sketches of the nucleus and envelope made. Subsequently to October 6 three centres of condensation were usually visible. As the comet receded from the sun, the head increased in length from 25" on October 4 to 139" on November 20. As late as March 6 there appeared to be three centres of condensation connected by matter of less density.

Difficult double-stars have been measured by Prof. Hough and Mr. S. W. Burnham, amongst them the interesting binaries, 40 Eridani (Σ 518), β Delphini, δ Equulei, and 85 Pegasi. Measures of the companion of Sirius gave for the epoch 1883.12 position 39°·9, distance 9"·04; the distance is diminishing about 0"·3 annually, so that in a few years it will be beyond reach of any except the largest telescopes. With the excellent measures obtained at Chicago more must soon be known as to the period of δ Equulei, reputed the most rapid of all binaries.

TEMPEL'S COMET, 1873 II.—The following are places for Greenwich midnight, deduced from M. Schulhof's elements:—

1883	R.A. h. m. s.	N.P.D. ° '	Log. distance from Earth	Log. distance from Sun
Nov. 16 ...	18 20 48 ...	113 42·1 ...	0·2867 ..	0·1288
18 ...	18 28 22 ...	113 52·1 ...	0·2877 ...	0·1287
20 ...	18 35 59 ...	114 0·6 ...	0·2888 ...	0·1286
22 ...	18 43 38 ...	114 7·6 ...	0·2900 ...	0·1287
24 ...	18 51 19 ..	114 13·1 ...	0·2912 ...	0·1289

This comet approaches pretty near to the orbit of the planet Mars; in heliocentric longitude 312° (equinox of 1878), corresponding to true anomaly 6°·1, the distance is 0·050.

D'ARREST'S COMET.—M. Leveau's ephemeris of this comet terminates on November 25. The following places are reduced from it to 6h. Greenwich M.T.:—

1883	R.A. h. m. s.	N.P.D. ° '	Log. dist. from Earth	Intensity of light
Nov. 16 ...	17 15 44 ...	105 13·3 ...	0·3637 ...	0·084
17 ...	17 18 51 ...	105 22·1		
18 ...	17 21 59 ...	105 30·8 ...	0·3628 ...	0·086
19 ...	17 25 8 ...	105 39·3		
20 ...	17 28 19 ...	105 47·7 ...	0·3620 ...	0·087
21 ...	17 31 30 ...	105 55·8		
22 ...	17 34 43 ...	106 3·7 ...	0·3611 ...	0·089
23 ...	17 37 57 ...	106 11·5		
24 ...	17 41 12 ...	106 19.1 ...	0·3603 ...	0·090

M. Leveau mentions that when Prof. Julius Schmidt last observed the comet at Athens in 1870 with a refractor of 0·17m. aperture the intensity of light was 0·150.

On November 16 the comet sets at Greenwich 2h. 10m. after the sun.

The planetary perturbations during the next revolution are not likely to be large, so that in 1890 the comet may be observed under similar conditions to those of 1870.

STANDARD RAILROAD TIME

THE following letter, addressed to our American contemporary *Science*, is of interest:—

Though the subject of standard and uniform railway time has for some years been under consideration by various scientific and practical bodies, it does not appear in any way to have been exhausted, even in its main features. Besides, a certain bias has shown itself in favour of the adoption of a series of certain hourly meridians, and thus keeping Greenwich minutes and seconds, when contrasted with the practicability of a more simple proposition. There is also a feature in the discussion of the subject which bears to have more light thrown upon it: namely, what necessary connection there is between the railway companies' uniform time and the mean local time of the people, or the time necessarily used in all transactions of common life. Directly or by implication, certain time-reformers evidently aim at a standard time, which shall be alike binding on railway traffic as well as on the business community; and to this great error much of the complexity of the subject is to be attributed, and it has directly retarded the much-needed reform in the time-management of our roads.

We say all ordinary business everywhere must for ever be conducted on local mean solar time, the slight difference between apparent and mean time having produced no inconvenience; and we may rightly ask the railway companies to give in their time-tables for *public* use, everywhere and always, the mean *local* time of the departure and of the arrival of trains. It is the departure from this almost self-evident statement, and the substitution and mixing-up in the time-tables of times referred to various local standards, which has in no small measure contributed to the confusion and perplexity of the present system. The people at large do not care to know by what time-system any railroad manages its trains, any more than they care what the steam-pressure is, or what is the number of the locomotive. All the traveller is interested in is regularity and safety of travel; hence it was to be desired that, whatever the standard or standards of time adopted, the companies would refrain from troubling him with a matter which only concerns their internal organisation, or which is entirely administrative. We look upon the publication of the railway time-tables, by local time everywhere, as a *sine quâ non* for the satisfactory settlement of the time question, so far as the public at large is concerned; and it would seem equally plain that the best system for the administration of railroads would be the adoption of a uniform time, this time to be known only to the managers and *employés* of the roads.

We are informed in *Science* of October 12 that the solution of the problem of standard railway time is near at hand, and probably has already been consummated by the adoption of four or more regions, each having uniform minutes and seconds of Greenwich time, but the local hour of the middle meridian. To have come down from several dozen of distinct time-systems to a very few and uniform ones, except as to the hour, is certainly a step forward, and, so far, gratifying; but why not adopt Greenwich time, pure and simple, and have absolute uniformity? Probably this will be felt before long. The counting of twenty-four hours to the day in the place of twice twelve, and the obliteration from time-tables of the obnoxious a.m. and p.m. numbers, would seem to be generally acknowledged as an improvement and simplification, and perhaps can best be dealt with by adopting it at once, accompanied by a simple explanatory statement.

C. A. SCHOTT

Washington, October 18

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—No election has yet taken place either for the Professorship of Botany or that of Rural Economy, which are now separated from each other. The Delegates of the Common University Fund have agreed to attach a Readership to the Chair of Botany, which will raise the income to 500*l.* a year. The

Professorship of Rural Economy will not be a resident one. The Professor will have to deliver twelve lectures. His stipend is 200*l*. a year.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, November 8.—Prof. Henrici, F.R.S., president, in the chair.—The following resolution, proposed by the President and seconded by Dr. Hirst, F.R.S., was carried unanimously, viz. :—"That the secretaries be requested to communicate to Mrs. Spottiswoode the expression of our sincere sympathy and the assurance of our deep sense of the loss which science has sustained by the untimely death of Mr. Spottiswoode."—The new Council was elected for the session 1883-84, viz. : Prof. Henrici, president ; Sir J. Cockle, F.R.S., and Mr. S. Roberts, F.R.S., vice-presidents ; Mr. A. B. Kempe, F.R.S., treasurer ; Messrs. M. Jenkins and R. Tucker, honorary secretaries ; other members, Prof. Cayley, F.R.S., Messrs. E. B. Elliott, J. W. L. Glaisher, F.R.S., J. Hammond, H. Hart, Dr. Hirst, F.R.S., W. D. Niven, F.R.S., Prof. Rowe, and Messrs. R. F. Scott and J. J. Walker, F.R.S. The Rev. J. J. Mylne and Mr. F. W. Watkin were elected members. —The following papers were communicated :—Symmetric functions, and in particular on certain inverse operators in connection therewith, Capt. P. A. Macmahon.—On a certain envelope, Prof. Wolstenholme.—On certain results obtained by means of the arguments of points on a plane curve, R. A. Roberts.—Third paper on multiple Frullanian integrals, E. B. Elliott.—Note on Jacobi's transformation of elliptic functions, J. Griffiths.—Symmedians and the triplicate-ratio circle, R. Tucker.

Linnean Society, November 1.—Frank Crisp, treasurer and vice-president, in the chair.—Messrs. T. E. Gunn and A. Hutton were elected Fellows.—A donation to the Society of several interesting letters of Linnæus (1736-1769) to G. D. Ehret, F.R.S., an eminent botanical artist of the last century, was announced by the Chairman, and a unanimous vote of thanks thereupon accorded to the Misses Grover and Mr. Chas. Ehret Grover for their valuable donation.—Mr. Crisp drew attention to specimens in fluid medium of *Limnocodium sowerbii*, as illustrative of Mr. P. Squires' method of preserving delicate and other medusæ.—Mr. H. Groves showed examples of *Chara braunii* from Ashton-under-Lyne, and Mr. Arthur Bennett of *Najas marina* and *N. alagnensis* from Hickling Broad, Norfolk, all being new to the British flora.—Mr. W. Fawcett exhibited *Testacella maugei* alive, the same being obtained by J. C. Mansel Pleydell in Dorset, and supposed to be indigenous to that county.—A paper was read on the changes of the flora and fauna of New Zealand, by Dr. S. M. Curl. He referred more particularly to the district of Rangitikei and to the alterations of the aspect of the vegetation within the last forty years. He likewise records his own experiments in the cultivation of trees, shrubs, and flowering plants introduced from widely different climes, remarking that while a few fail to grow with vigour, the majority by degrees adapt themselves to the altered conditions, and many valuable economic plants thrive accordingly.—Mr. J. Starkie Gardner read a paper on *Alnus richardsoni*, a fossil fruit from the London Clay of Herne Bay. The species has been described by Bowerbank and commented on by Carruthers, Ettinghausen, and many other authors who have written upon the plants of the Tertiary formation. Originally considered as allied to *Casuarina*, Dr. R. Brown suggested its affinities to the Proteaceæ, a view afterwards upheld by Carruthers and others. Ettinghausen thereafter regarded it as a product of a Conifer (*Sequoia*), and Saporta compared the fruit to that of *Dammara*. Mr. Gardner enters fully into the structural peculiarities of the fossil fruit in question, and satisfactorily demonstrates that it belongs to the Betulaceæ under the genus *Alnus*. —Another paper by Miss G. Lister was read, viz. on the origin of the Placentas in the tribe Alsineæ of the order Caryophylleæ. This communication is based on a series of observations on the development of a number of genera and species. She concludes that the capsule in the Alsineæ is developed on essentially the same plan as that of *Lychnis*, the difference in the various genera being merely dependent upon the relative height attained by the carpels on the one hand, and by the central axis on the other. This being so, we are bound to admit that if we accept, as we do, the carpellary origin of the placentas in *Lychnis*, the placentas in the Alsineæ, from *Sagina apetala*, which most resemble *Lychnis*, to *Cerastium triviale*, which most widely differs from it, are also carpellary.

Chemical Society, November 1.—Dr. Perkin, F.R.S., president, in the chair.—The following papers were read :—On the production of hydroxylamine from nitric acid, by E. Divers. Free nitric acid yields hydroxylamine when treated with tin, zinc, cadmium, magnesium, and aluminium. In the presence of hydrochloric or sulphuric acid the quantity with tin or zinc may be considerable. Without a second acid only traces can be detected. The author also discusses the action of nitric acid upon metals and the constitution of nitrites, in which he considers the metal to be directly united with nitrogen.—On the chemistry of lacquer (*Urushi*) (part i.), by H. Yoshida. Lacquer contains a peculiar acid, Urushic acid, extracted by alcohol, some gum resembling gum arabic, water, and a peculiar diastatic body containing nitrogen. The lacquer when exposed to moist air at 20° C. dries up into a hard lustrous varnish. This hardening is brought about by the action of the diastase upon Urushic acid, the latter being converted into oxy-urushic acid.—On some compounds of phenols with amidobases, by G. Dyson. The author has prepared and investigated anilin phenate, toluidin phenate, naphthylamin phenate, anilin β naphthate, toluidin naphtnate, rosanilin phenate, xylidin naphthate, rosanilin aurinate, anilin aurinate.—On the alleged decomposition of phosphorous anhydride by sunlight, by R. Cowper and V. B. Lewes. In a paper at the British Association, Southport, the Rev. A. Irving stated that phosphorous anhydride prepared by passing air over heated phosphorus is decomposed by sunlight into phosphorus and phosphoric anhydride. The authors find that phosphorous anhydride thus prepared consists of a mixture of phosphoric anhydride, phosphorous anhydride, and phosphorus.

Physical Society, November 10.—Prof. Clifton in the chair.—Dr. J. Blaikley read a paper on the velocity of sound in air, in which he described a modification of Dulong's method of measuring it by the wave-length in a pipe lengthened. Dulong did not allow for the partial tones, which are an important factor, whereas Mr. Blaikley does. By means of organ pipes of different diameters, the author has found the velocity to be about 320 metres per second. Mean result with four tubes: one of 54·1 mm. diameter, velocity = 329·73 metres per second ; one of 32·5 mm. diameter, velocity = 328·78 metres ; one of 19·5 mm. diameter, velocity = 326·9 metres ; one of 11·7 mm., velocity = 324·56 metres. The velocity diminishes as the tube is smaller in bore.—Mr. Bosanquet made a communication on the moment of a compound magnet, which he showed how to measure by the method already published by him. A compound magnet made up of eighteen small cylinders of magnetised steel placed end to end is hung in a cradle carried by a delicate bifilar suspension, and placed at right angles to the magnetic meridian. The deviation from zero produced by the magnet is noted ; then the magnet is divided into two parallel rows of nine cylinders along the cradle, and the deviation again noted. The tangent of the angle of deviation from the east and west line, multiplied by a constant, is the moment of the magnet. The author also pointed out that to define the condition of a permanent magnet it was necessary to know the difference of magnetic potential, the "resistance" of the metal, and the resistance of the external space. —Mr. W. Lant Carpenter read a paper on measurements relating to the electric resistance of the skin, and certain medical appliances. The author's experiments, made upon himself, showed that the resistance of the body amounts to thousands of ohms, but is mainly due to the condition of the epidermis. If this is dry, the resistance is high. By soaking the skin in salt and water, he reduced the resistance of parts of his body from 10,300 ohms to 935 ohms after 100 minutes' soaking. He infers that a large electrode should be used in applying electricity to the body, and that the skin should be soaked for twenty-five minutes previously. Mr. Carpenter also exhibited a "chain-band" of Mr. Pulvermacher, and a small voltameter by the same inventor, in which the liberated gases force some of the water up a graduated tube as a gauge of the current. The author drew attention to the necessity of seeing that the skin should be dry in handling some electric light machines, else disagreeable shocks might result. Prof. Ayrton believed that the danger of electric lighting currents lay rather in their discontinuity than their electromotive force. The Brush currents, which have proved fatal, are more discontinuous than those of the Gramme machine, &c. Adverting to the presence of electricity in the air as influencing health, he suggested that the influence might be studied by electrifying the air, say in a hospital ward. Mr. W. Coffin stated that statically electrifying patients had been tried at Bellevue Hospital, New York, without definite results.

PARIS

Academy of Sciences, November 5.—M. Blanchard, president, in the chair.—Funeral orations on the late M. Breguet, by M. Janssen and Admiral Cloué.—Notice by M. Daubrée of the death of Mr. Lawrence Smith, Corresponding Member for the Section of Mineralogy, who died at Louisville, Kentucky, on October 12.—On lighting by electricity, by M. Th. Du Moncel.—On one of the methods proposed by M. Lœwy for determining the right ascensions of the circumpolar stars, by M. F. Gonnessiat.—Remarks on M. Boussinesq's communication respecting the equilibrium of a ring subjected to normal pressure uniformly distributed, by M. Maurice Lévy.—Note on the decomposition of a number into five squares, by M. Stieltjes.—On the probability that a given permutation of n quantities is an alternating permutation, by M. Désiré André.—On the algebraic integration of linear equations, by M. H. Poincaré.—On a family of developable surfaces generated by the intersection of a given left curve at an angle depending exclusively on the coordinates of the point of intersection, by M. Lucien Lévy.—On the potential of the inductive force due to a closed solenoid with current of varying intensity; analogy with Felici's theorem of electromagnetism, by M. Quet.—On a new non-periodical galvanometer, by M. G. Le Goarant de Tromelin.—On the electric resistance of sulphur, phosphorus, and some other more or less insulating substances, by M. G. Foussereau.—On the influence of nitrate of soda and of nitrate of potassa on the cultivation of potatoes, by M. P. P. Dehérain.—Researches on the physiological properties of maltose, by M. Em. Bourquelot.—On the external application of metallic copper as a preservative against cholera, by M. Axel Lamm.—On the comparative toxic action of metals on microbes, by M. Ch. Richet.—Note on zooglœic tuberculosis, by MM. L. Malassez and W. Vignal.—On spermatogenesis amongst the edriophthalmous Crustaceans (genera Ligia, Idotea, Sphæroma, Gammarus, Talitrum), by M. G. Herrmann.—On internal sacculine, a fresh stage in the development of *Sacculina carcini*, by M. Yves Delage.—On the anatomical structure of the Phallusiadeæ, a family of Ascidians on the coast of Provence, by M. L. Roule.—On the intestinal cavity and sexual apparatus of *Spadella marioni*, by M. P. Gourret.—A second contribution to the history of the formation of coal, by M. B. Renault.—On a ferriferous meteorite which fell at Saint Caprais de Quinsac, Gironde, on January 28, 1883, by MM. G. Lespiault and L. Forquignon.—On the diurnal variation of the barometer at different altitudes, and on the existence of a third barometric maximum, by M. Ch. André.—Note on the periodicity of earthquakes, by M. Ch. V. Zenger.—On the employment of sulphuric acid in the treatment of animal matter infected by contagious elements, by M. Darreau.

BERLIN

Physiological Society, October 26.—In the course of his investigations into the functions of the cortex of the cerebrum, Prof. Munk had often had occasion to collect experiences on the subject of the appearance of general epileptic spasms resulting from irritation of the cortex of the cerebrum. By this means he had been enabled to confirm not only the older clinical conclusion of Mr. Jackson, that epileptic spasms always proceeded from one group of muscles, and then overtook in a perfectly definite series more distant groups, and at last the whole body, but likewise the accuracy of Herr Hitzig's observation, that in the case of more powerful or longer continued irritations of the motory parts of the cortex of the cerebrum, the contractions of the group of muscles belonging to the irritated spot ended in general epileptic spasms. An experimental epilepsy of this kind Prof. Munk could produce from any spot of the motory part (the sphere of feeling), and the groups of muscles therefore followed each other exactly in the series in which the centra were stratified beside each other in the sphere of feeling, so that first the parts situated nearest the irritated spot, and then more distant parts became affected, till at last the whole body was subjected to epileptic contractions. Sometimes the whole of the groups of muscles on one side was attacked before the other side began to be affected; frequently, however, the irritation and the epileptic attack passed over at an earlier stage from one side to the other. That the experimental epilepsy originated in the motory section of the cortex of the cerebrum seemed to Prof. Munk indubitably established by the two following facts:—Let a small piece, say the centre for the movements of the upper extremity of the right side, be excided, and let the centre of the eye-muscles be irritated till epilepsy set in, then would the spasmodic contractions propagate themselves successively to all groups of muscles with the exception of the right upper extremity, which would remain at rest throughout the epileptic attack. Let, again, the centre of the eye-muscles (a part specially suitable for such experiments) of an animal be irritated so that an epileptic attack supervened, and after a corresponding pause let the irritations be repeated in the same part with equal strength and duration, then, in the event of the spasms reaching the muscles, say of the head or neck, by suddenly removing the irritated part of the membrane the epilepsy would also be terminated. Both phenomena were explainable only on the assumption that the irritation of the motory cortex of the brain was the cause of the experimental epilepsy. The assertion was advanced by another observer, that epilepsy could be generated not only from the front section of the cerebral cortex, but likewise from the sphere of vision. This position Prof. Munk induced Herr Danilo to put to the proof, but in spite of numerous experiments no confirmation of it could be gained. Electric streams, of a force and duration such as, applied to any part of the sphere of feeling, would undoubtedly have given rise to epilepsy, were quite powerless in this respect when applied to the sphere of sight. Not till streams of much intenser force and very considerably longer duration were applied to the sphere of sight was an epileptic attack produced. In this case, too, it was obvious that the result was due to the neighbouring parts of the sphere of feeling becoming irritated through propagation of the effect or by communication. If now at the beginning of the epileptic attack the irritated part of the sphere of sight were removed, the attack would not thereby be stopped. Nor was it of any greater consequence in the way of producing an attack that by a cross cut the irritated sphere of sight was freed from its substratum, if only it retained connection with the front part of the cortex. Let, however, the sphere of sight, by means of a perpendicular sagittal cut, be separated from the sphere of feeling, then could no epileptic attack be any longer produced by irritating the former. These facts seemed to Prof. Munk to conclusively demonstrate that experimental epilepsy could be produced only by irritation of the motory parts of the cortex of the cerebrum. He laid stress, however, on the fact that his experiences and experiments referred only to "experimental" epilepsy.

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