THURSDAY, AUGUST 6, 1885

A POSSIBLE WINDFALL FOR SCIENCE

IN a recent article we referred to the question of the amalgamation, so to speak, of the astronomical and civil day, in connection with the introduction of world time or prime meridian time, suggested by the Washington Conference. We pointed out that there were various opinions touching the time at which the change should be made, but that the concensus in its favour is so strong that it is certain to be made some time or another.

Our contemporary *Science* has recently called attention to a point which, if carried out, will make the work complete at an annual saving on the outlay of the present of something like 20,000*l*.

How is this to come about? In this wise. Let us suppose four nations *A*, *B*, *C*, or *D*, who each support a national observatory chiefly for the benefit of its Marine. This benefit consists in telling the mariners at what instant, according to the time shown by the clocks of *A*, *B*, *C*, or *D*, any celestial event, useful to him for determining his place at sea, will happen.

Let four ships, one of them representing each one nation, be within a cable's length of each other in the middle of the Pacific when the time comes for making an observation to determine position. Four books will be used, the production of which has been enormously costly, as each consists almost entirely of figures which depend upon elaborate calculations.

If the books are rightly calculated and the captains are skilful, of course the same position will come out in each case.

Evidently this work has been done four times over, and it is equally obvious that the result should have come out the same if the position had been determined properly on either ship from data supplied by either book. Why is this? Because our nations, though they have accepted in common the art of printing, the art of binding of printed pages together to form a book, and Arabic numerals, have not accepted a common time.

To come down from our generalities the four ships might have belonged to Germany, France, the United States, and Great Britain, and the four books might have been the *Berliner Jahrbuch*, the *Connaissance des Temps*, the *American Ephemeris*, and the *Nautical Almanac*. Sympathetically with these four books, at least three different times might have been indicated by the chronometers. And here lies the point. *Because* these chronometers show the time at Paris, or Berlin, or London therefore the computations of each celestial event, using the same data, employing the same processes, have been undertaken by each nation.

But even this is not all. We have said that at least three different times might have been indicated, and on our supposition only three times would have been indicated, because the U.S. Marine actually use Greenwich time.

Now it is clear that the general introduction of world time or prime meridian time, with the idea of which we are beginning to be familiar, will do for time what the introduction of the Arabic numeral did for numbers—it will denationalise and generalise it whenever necessary; and each observatory, sooner or later, is certain to have a clock showing the prime meridian time of the earth as it has one already of the skies, and when this comes about it will be to the general advantage for all to deal with the common time for purposes common to the planet.

Now among these what can be imagined more planetary than those with which the mariner has to do, and if this be so why shall there not be one unique planetary ephemeris.

From the abstract point of view more than one ephemeris cannot be defended, though it may be pardoned if, as *Science* suggests, the nations, to save their *amour propre*, must have ephemerides for their several meridians "much the same as all patent medicine firms and pill vendors feel the need of an almanac and calendar for the conservation of individual interests: it saves themselves and their patrons the indignity of referring to somebody else's almanac, and advertises the fact that they are enterprising enough to have one."

We cannot believe that the feeling characterised above, though it exists, would stand in the way of such a vast saving of labour and such a general improvement as might be brought about by an International Ephemeris, provided the question were well ventilated and wisely discussed by a congress summoned *ad hoc*. On this point *Science* writes:—

"It is certain that the deliberations of such a congress could not fail to advise governmental co-operation in the preparation of the nautical almanacs now existing, national pride aside, and this might be done in a multitude of ways, most prominently in the case of the preparation of the data relating to the moon. Take, for example, the hourly lunar ephemeris and the lunar distances as printed each year in the *British Nautical Almanac* and the *American Ephemeris*. These data occupy about one-third of the entire number of pages of each of these publications; they are now prepared independently by the two offices, but are, when printed, substantially identical in both; and, further, the work being done at about the same time in the two countries, the results of the one do not serve any sufficient purpose as a check upon the accuracy of the other. The cost of this part of the almanac alone to each nation amounts to several thousand dollars annually, —an amount which might be reduced one-half by the preparation of these data conjointly, to say nothing of other immediate and favourable results which might be secured by such co-operation.

"The wisest conservatism would appear to suggest the annual publication by the nations conjointly of a single volume of astronomical predictions, which, in addition to other improvements, should combine all those desirable features not dependent upon individual meridians, and which in some degree characterise all the astronomical ephemerides of the several Governments. The contents and arrangement of the articles of such an ephemeris could only be determined by an international conference. While this may be little better than mere speculation, any one who has the four principal ephemerides in constant use will readily recognise how small a portion of each is employed, and, with extended interpolation-tables, how little the inconvenience of using the ideal ephemeris solely would be."

It is sufficiently obvious that this enormous simplification and improvement must come about some time or the other, and it is to be hoped that no very long time will be allowed to elapse before some Government stirs in the matter. We have already a permanent international organisation, which, if its functions were to be extended so as to include the measurement of time as well as of space, might consider the question without any large increase of its numbers; we refer to the *Commission du Mètre*, which already largely consists of astronomers. We point this out to show that there are no real difficulties in the way of a preliminary consideration of the matter—nay more, that there are ways of reducing the difficulties by the choice of a body which already exists, and exists too in France, where the idea of a neutral meridian still lingers. We believe that a serious practical discussion would show that the idea which lies at the root of the contention for a neutral meridian is as impossible now as it has been in the past with regard to other internationalisations, such as Roman letters and Arabic numerals. If this were so, a great step would have been gained.

The writer in *Science*, however, does not propose that the Governments should be urged forward by any idea of saving their share of the sum we have already mentioned, and quite rightly. The idea is thrown out that it should be spent in an international mountain observatory, where in turns astronomers of all countries could carry out their special researches. The idea is a most admirable one, and will commend itself to all who know how years, and we may even say centuries, are being lost by heartbreaking attempts to do at a low level important work which is really only practicable at a high elevation.

PROFESSOR TAIT'S "PROPERTIES OF MATTER"

Properties of Matter. By Prof. Tait. (Edinburgh: Black, 1885.)

THE subject of this excellent little book includes the mechanical properties of matter, and much that is usually treated under the head of Chemical Physics, such as Diffusion and Capillarity. It might be difficult to give a reason why the electric and thermal conductivities of mercury, for example, should not be included among its properties as much as its density and its capillarity; but the distinction is convenient, and to some extent sanctioned by usage.

In the introductory chapters the author expounds some rather peculiar views with perhaps more insistance than is desirable in an elementary work. The word "force" is introduced apologetically, and with the explanation that "as it does not denote either matter or energy it is not a term for anything objective." No one will dispute the immense importance of the property of conservation, but the author appears to me to press his view too far. As Dr. Lodge has already pointed out, if conservation is to be the test of existence, Prof. Tait himself does not exist. I forbear from speculating what Dr. Lodge will say when he reads on p 11 that "not to have its price is conclusive against objectivity."

Chapters IV. to VII. form an elementary treatise on Mechanics, in which even the learned reader will find much that is interesting in the way of acute remark and illustration. Under the head of Gravitation are considered Kepler's laws, the experimental methods for determining the constant of gravitation ("the mean density of the earth"), and the attempts (such as Le Sage's) which have been made to explain the origin of gravitation.

The succeeding chapters on the deformation of solids and the compression of solids, liquids, and gases, are perhaps the most valuable part of the work, and will convey a much needed precision of ideas to many students of physics whose want of mathematical training deters them from consulting the rather formidable writings of the original workers in this field. The connection of Young's modulus of elasticity, applicable to a rod subject to purely longitudinal pull or push, with the more fundamental elastic constants expressing the behaviour of the body under hydrostatic pressure and pure shearing stress respectively, is demonstrated in full. Prof. Tait remarks that "Young's treatment of the subject of elasticity is one of the few really imperfect portions of his great work ('Lectures on Natural Philosophy.'). He gives the value of his modulus for water, mercury, air, &c.!" A deficiency of explanation must be admitted, but I am not sure that Young's ideas were really confused. The modulus for solids corresponds to a condition of no lateral *force*, that for liquids to no lateral *extension*. The distinction should certainly have been pointed out; but the moduli are really comparable in respect of very important effects, which Young probably had in his mind—viz. the propagation of sound along a bar of the solid in one case, and in the other through a fluid, whether unlimited or contained in an unyielding tube.

As a great admirer of Dr. Young's work, I cannot resist adding that if in some respects his treatment of elasticity is defective, in others it is in advance of many modern writings. Witness the following passage:—"There is, however, a limit beyond which the velocity of a body striking another cannot be increased without overcoming its resilience, and breaking it, however small the bulk of the first body may be, and this limit depends upon the inertia of the parts of the second body, which must not be disregarded, when they are impelled with a considerable velocity. For it is demonstrable that there is a certain velocity, dependent on the nature of a substance, with which the effect of any impulse or pressure is transmitted through it; a certain portion of time, which is shorter, according as the body is more elastic, being required for the propagation of the force through any part of it; and if the actual velocity of any impulse be in a greater proportion to the velocity than the extension or compression, of which the substance is capable, is to its whole length, it is obvious that a separation must be produced, since no parts can be extended or compressed which are not yet affected by the impulse, and the length of the portion affected at any instant is not sufficient to allow the required extension or compression."

The theory of "bending" and of "torsion" are discussed in Chapter XI. When the section of the rod deviates from the circular form, the torsional problem becomes rather complicated; but a statement is given of some of the interesting results of Saint Venant's investigations. In his treatment of the compression of solids and liquids, the author is able to make valuable contributions derived from his own experimental work.

In the chapter on "gases," a long extract is given from Boyle's "Defence of the Doctrine Touching the Spring and Weight of the Air," in order to show how completely the writer had established his case in 1662. As to this there can hardly be two opinions; and Prof. Tait is fully justified in insisting upon his objections to "Mariotte's law." In Appendix IV. a curious passage from Newton is discussed, in which the illustrious author appears to speak of Mariotte sarcastically. It is proper that these matters should be put right; but Prof. Tait is hardly impartial enough himself to succeed in enlisting the complete sympathy of foreigners. Cases of glaring injustice should be rectified; but there will always be a tendency (from which Englishmen cannot claim to be exempt) to give a full measure of credit to one's own countrymen, if only because one is better informed concerning their labours.

There is one matter, suitable to an elementary work, which I should be glad to see included in a future edition, viz., the principle of dynamical similarity, or the influence of *scale* upon dynamical and physical phenomena. It often happens that simple reasoning founded upon this principle tells us nearly all that is to be learned from even a successful mathematical investigation; and in the very numerous cases in which such an investigation is beyond our powers, the principle gives us information of the utmost importance. An example will make this clear. The pitch of a tuning-fork of homogeneous steel is dependent upon the size and shape as well as upon the elastic quality of the material; but the matter is too difficult for rigorous mathematical treatment. If, however, it be asked, How does the pitch depend upon the *size* of the fork, the shape and material being given? we need no complicated mathematics at all. The principle of dynamical similarity tells us at once that the time of vibration is proportional to the linear dimension.

Another example might be taken from a reaction which Prof. Tait describes as specially complex—viz., collision. A glass ball drops upon a marble floor from a height of one foot. How does the size of the ball affect the strains during collision and the danger of rupture? The principle teaches that if the scale of time be altered in the same proportion as the scale of length, similarity is secured, so that the strains are equal at corresponding times and at corresponding places. Hence a larger ball is not more likely to break than a smaller one, unless in consequence of the greater *duration* of the strains. I feel sure that in Prof. Tait's hands this very important and fundamental principle might be made intelligible to the great mass of physical students.

It would lead us too far to refer in detail to the various subjects treated in the later chapters under capillarity, diffusion, osmose, transpiration, viscosity, &c., but there is one point that I should like to mention. The explanation on p. 249 of the behaviour under water of drops of ink and of solution of permanganate of potash assumes the existence of a capillary tension in the surface separating the two fluids. In my own experiments on jets with this very solution, I have never seen any tendency to break up into drops (as, according to Savart and Plateau, there would be in air), and have therefore supposed that the capillary force was *nil*, or at any rate very small. Moreover, theory shows that the force depends entirely upon the suddenness of transition between two media, which suddenness must be broken down almost instantaneously when two miscible liquids come into contact. As the matter stands there seems to be here some discrepancy, which, perhaps, Prof. Tait could elucidate.

In his preface the author holds out hopes of further volumes on the same plan, dealing with dynamics, sound, and electricity. The readers of the present work will, I am sure, join in the wish that the appearance of these may be delayed no longer than is absolutely necessary.

RAYLEIGH

GRISEBACH'S "VEGETATION OF THE EARTH"

Die Vegetation der Erde nach ihrer klimatischen Anordnung. Ein Abriss der vergleichenden Geographie der Pflanzen. Von A. Grisebach. Zweite vermehrte und berichtigte Auflage. 8vo. Vol. I., pp. 567; Vol. II., pp. 693. (Leipzig: Wilhelm Engelmann, 1884.)

FROM the date, and the statement on the title-page that this is an augmented and corrected second edition of a work which was published in 1871, it might be expected that it contains the results of much more recent investigation; but an examination of the present edition is very disappointing. Indeed, it is doubtful, to say the least, whether it deserves the descriptive title given to it; for the "Quellenschriften und Erläuterungen" do not appear to contain a single additional reference, and it is not easy to discover that it has a claim to be anything more than a reprint, with some trifling alterations, of the original edition of 1871. The author died in 1879, so that one naturally looked to see who was the editor of this edition, and it was only after much seeking that a clue was found in a foot-note on p. 15 of the preface. After the appearance of the "Vegetation der Erde," A. Grisebach continued to write annual reports on the progress of geographical botany, and these, together with other scattered articles, were published in a collective form in 1880 by his eldest son, under the title, "Gesammelte Abhandlungen und kleinere Schriften zur Pflanzengeographie." From the foot-note in question it appears that this son—a gentleman in the Consular service of his country, and presumably unacquainted, or imperfectly acquainted with botanical literature—edited the new edition of the "Vegetation der Erde," "based upon the corrections and additions left by the author." Now it is perfectly certain that Grisebach regarded the annual reports referred to as so many supplements to his greater work, and the substance of which he would doubtless have incorporated therein had he himself prepared a second edition. Since his death, too, considerable additional information on geographical botany has come to light; and, what is more, it has been collected and published in German by Drude, Engler, and others; yet, as already mentioned, the additions and corrections in the present edition are merely trivial, and cannot be said to enhance materially the value of the work. In a footnote to Grisebach's preface to Tchihatcheff's admirable French edition of the original work, reproduced here, it is stated that some additions of Grisebach's thereto are here intercalated in their respective places. This is very good, but why Tchihatcheff's copious annotations and additions, recognised and sanctioned, as it were, by the author himself, should be ignored in a second German edition, is incomprehensible, saving the assumption that both with respect to his father's annual reports and other sources, the son was wholly incapable of doing his father justice. It is a pity that the task of preparing a second German edition was not entrusted to a competent botanist, because the original work, apart from the uncompromising antagonism to Evolution that pervades it, still occupies an undisputed position in modern botanical literature. As it is, the French edition is not merely an advance on the original German—it is incomparably better than the second German edition. It is only, however, fair that some justification of such assertions should be given. Taking the chapter on Oceanic Islands as an example, it may be confidently stated that no additional information is given; yet there is no branch of geographical botany that has advanced more during the last decade than insular. On the other hand Tchihatcheff embodies nearly all that was known up to date. One slight alteration observed in this chapter is—Madeira is stated to be 50 German geographical miles nearer Europe than the Azores, instead of 150, as in the original. Then certain unfounded statements in refutation of the arguments of other botanists concerning the relationships of insular floras remain uncorrected. Thus, in allusion to Sir Joseph Hooker's demonstration ("Insular Floras," p. 7) that the vegetation of St. Helena has, on the whole, its nearest affinities in South Africa, it is objected, on the authority of Roxburgh, that three out of the five genera named by Hooker were originally introduced into the island from the Cape of Good Hope, whereas an examination of Roxburgh's enumeration of the plants of St. Helena reveals the fact that the indigenous, and endemic, St. Helena species of the genera in question were unknown to him, and his remarks apply only to actually introduced species. Again, to repeat in 1884 such statements as that the vegetation of Juan Fernandez has little systematic relationship with that of the Chilian or Antarctic floras and that *Pringlea antiscorbutica* is restricted to Kerguelen Island is unpardonable, because the contrary is now historical. Defects such as those pointed out are numerous, but as they are mostly due to the state of knowledge fifteen years ago, the author of the work of that date is not to be blamed for them; rather the present editor and publisher for offering the public an old book as new.

W. BOTTING HEMSLEY

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.*]

Nomenclature in Elasticity

THE word *stress* is used, sometimes in the sense of *load*, sometimes in that of *load per unit area*. Clearness, however, requires these two ideas to be kept perfectly distinct, and therefore to be denoted by separate terms. *Load* is surely expressive enough, or, if not, there is the more comprehensive word *force*: why then use *stress* synonymously? It would be far better to reserve *stress* to signify *load per unit area*. This Prof. Kennedy (p. 269) calls *intensty of stress*; but why not *stress* simply? The word *intensity* is not in itself suggestive ot anything distinctive, and is therefore useless.

Pressure and *tension* are terms used in the same loose manner, though, when intended to represent *force*, they sometimes have the word *whole* prefixed. Is it not better to say *force* when we mean *force*? We can then reserve *pressure* and *tension* as vector-synonyms of *stress* in the sense of *force per unit area*, which is indeed their usual *rôle*.

Another misused term is *resilience*, which sometimes denotes the *work* done in producing *proof* strain in a body (Rankine's definition), sometimes the *work* done *per unit volume* in producing *proof* strain, sometimes the *work* done *per unit volume* in producing *any* strain. I prefer, myself, the third definition: the second would then be the *proof resilience*, and the first might be called the *strain-energy*.

However, whatever terminology is finally agreed upon, let it be perfectly definite and consistent.

In his Fig. 1 (p. 269) Prof. Kennedy writes: "Breaking load, 18·85 tons per square inch." According to his own nomenclature, he should surely say: "*intensity of* breaking *stress* 18·85 tons per square inch," and this I should prefer to call simply the breaking stress—premising that for *tons* I should write *tons' weight*. In this case, as the diameter is ¾ inch, and therefore the section ·442 square inch, the breaking *load* is 8·33 tons' weight. Similarly in the other figures.

Christ Church, Oxford ROBERT E. BAYNES

Earthquake-Proof Buildings

MR. MUIR is quite correct as to the facts and date of the introduction of the aseismatic tables into Japan. In 1869-70 seven aseismatic tables for carrying the lighting apparatus were sent from here and erected in Japan, and Mr. Simpkins, who has recently returned from Japan, informs me that there are three in action at present. Two iron towers, 46 feet high, with this arrangement at their base, were also constructed and shipped for Japan, but the vessel was lost and no more were sent out, as the engineer in charge—Mr. Brunton—took an unfavourable view of their efficiency—his idea being that they would not work, as he considered that buildings of "great weight and solidity, thereby adding to their inertia and checking their oscillation, were best suited to meet the difficulty in Japan." Mr. Milne's experiments with aseismatic tables have borne out Mr. David Stevenson's original view as to their power of mitigating an earthquake shock. For fuller information see NATURE, vol. xxx. p. 193. D. A. STEVENSON

Edinburgh, August 3

A Mechanical Telephone

HAVING observed in this week's NATURE a notice of a "mechanical telephone" said to be brought from America, I may state that so far back as 1878 I experimented on the transmission of sounds by wires, and communicated the results obtained, from a large number of experiments, to the Physical Society of London in March, 1878; the paper being afterwards published in the *Philosophical Magazine* for August, 1878. These experiments are referred to by the Count du Moncel in his book on "The Telephone," published in 1879. I found no difficulty in carrying on a conversation through wires laid in various ways from room to room of a house; and musical sounds, breathing, and whistling were also readily transmitted, and through most unlikely arrangements, such as a common wire fence. Various materials were tried for the transmitting and receiving ends—disks of cardboard set in deepish rims being found to give excellent results with a No. 16 copper wire. In one of my experiments I found that the disks were not required, the wire itself picking up and transmitting the sounds. The results obtained were most interesting; but as the range was necessarily limited, it did not seem to me that there was much scope for practical application. W. J. MILLAR

100, Wellington Street, Glasgow, July 31

Electrical Phenomenon

ABOUT ten o'clock in the evening of July 23 a party of four of us were standing at the head of the avenue leading to this house, when we saw a feebly-luminous flash appear on the ground at a distance of some thirty yards down the avenue. It rushed towards us with a wave-like motion, at a rate which I estimate at thirty miles an hour, and seemed to envelop us for an instant.

My left hand, which was hanging by my side, experienced precisely the same sensation as I have felt in receiving a shock from a weak galvanic battery. About three minutes afterwards we heard a peal of thunder, but, though we waited for some time, we neither saw nor heard anything further.

The gardener, who was one of the four, thus describes what he saw:—I thought it was a cloud of dust blowing up the avenue, and before I could think how that could be when there was not a breath of wind, I saw you three gentlemen covered for a second in a bright light, and that was all. Another of the party says that he observed what seemed to be a luminous cloud running up the avenue with a wavy motion. When it reached the party it rose off the ground and passed over the bodies of two of them, casting a sort of flash on their shoulders. The distance traversed was about twenty yards, and the time occupied between two and three seconds. (My own estimate of distance and velocity makes the time occupied almost exactly two seconds.) The day had been extremely hot and sultry, as also had the preceding day been, the thermometer readings being sometimes 80° F. in the shade.

On asking the gardener for further particulars, he tells me that the distance traversed by the luminous cloud was about forty yards, and that, when it had gone about half the distance, he saw a flash of lightning in the direction of it, but sideways; also that the top of the cloud seemed to be three or four feet from the ground, and it gradually rose higher as it came along. When the cloud reached the party he saw one of them distinctly by its light, the night being otherwise quite dark at the time; and, lastly, that the cloud went a few yards beyond the party into the open space in front of the house, and then disappeared.

J. B. A. WATT

Marchfield, Davidson's Mains, Midlothian

Our Ancestors

DURING eight centuries—say to the time of the Norman conquest—one's direct ancestors amount to a far greater number than would at first be contemplated. Taking three generations to a century, one has father and mother (2), grandparents (4), great-grandparents (8). At the end of the second century the number of ancestors springs to 64. Following the calculation you will find that at the end of eight centuries one is descended from no less than 16,000,000 ancestors. Intermarriage of course would reduce this estimate, and there is no doubt it must have largely prevailed. But the figures are so enormous that, in spite of all, I venture to suggest that the words "All ye are brethren" are literally true.

$\left(\frac{1}{2}\right)^n$

CO-ORDINATION OF THE SCIENTIFIC BUREAUS OF THE U.S. GOVERNMENT

A MOVEMENT is on foot in the United States for rearranging the various scientific departments of the Government under one central authority, and a report on the subject has been made by a committee of the National Academy of Sciences, consisting of Gen. Meigs, and Professors Trowbridge, Pickering, Young, Walker, and Langley, appointed for the purpose. The Report is published at length in *Science*. After referring to the state of things in Europe in this respect, it gives a brief account of the method in which such bureaus are organised in other countries; discusses at some length the character of the work done by the coast and geodetic and the geological surveys, especially in those points where their provinces are similar, pointing out that two distinct and independent trigonometric surveys of the United States are now in process of execution; distinguishes between the military and meteorological work of the Signal Service, and recommends their complete separation; indicates the danger of duplication of work by the Coast Survey and Hydrographic Office, but is not prepared to recommend that the latter be detached in any way from the control of the Navy Department, nor that the hydrographic work of the Coast Survey, for over forty years conducted so satisfactorily, be separated from that organisation, but suggests the lines on which it thinks the Coast Survey should work; lays down the principle that the Government should not undertake any work which can be equally well done by the enterprise of individual investigators, and that such work should be confined to what will "promote the general welfare of the country;" urges the importance of a proper extension of the trigonometrical survey of the United States; and, finally, recommends either the establishment of a department of science, or of a mixed commission of nine members—two of them scientific civilians to be appointed by the president for six years, two scientific men from the army and navy, three heads of the principal scientific bureaus, together with the president of the National Academy, and the secretary of the Smithsonian Institution.

To the Department of Science, or to the supervision of this Commission, it would transfer the Coast Survey, the Geological Survey, and the Meteorological Bureau, and establishing a physical laboratory, add to it a Bureau of Weights and Measures, the functions of which are now performed by the Coast Survey. The province of the proposed Commission is amply defined.

In the course of the Report the Committee give an interesting sketch of the work accomplished by the Coast Survey.

The Coast Survey was originally organised for the purpose of constructing maps and charts of the coast and harbours for the benefit of commerce and navigation. Conflicting opinions respecting the proper management of the Survey led to the formation, in 1843, of a board of officers with the duty of reorganising the Survey. This board submitted a plan which was enacted by Congress into law, upon and under which law the Survey has hitherto been executed. This plan provided for the co-operation of military officers, naval officers, and civilians in the various parts of the work. Under it the work of the Coast Survey has been continued to the present time.

In recent times a great extension of the field of operations of the Survey has been made, apparently looking to a triangulation covering the entire territory of the United States. The maps published annually with the report of the Survey enable us to know the geodetic work it has executed. It appears, from the maps accompanying the report of 1882, that on June 30 of that year a chain of triangles had been extended throughout the entire length of the Atlantic and Gulf coasts, and throughout about half the Pacific coast. Besides these coast-lines, extensive regions in the interior are seen to be triangulated. In the north-east, the triangulation covers the greater part of the States of New Hampshire, Vermont, and Massachusetts, about half of Connecticut, and it also includes a considerable part of the State of New York.

The reconnaissance has extended westward from the New Jersey coast, so as to include the greater part of the State of New Jersey, and a long strip in Pennsylvania. From Pennsylvania, the extended line of primary triangulation follows the Alleghany Mountains into Northern Alabama, and is now being continued across the country to Memphis.

A triangulation of the Mississippi River was extended from its mouth nearly to Memphis, where it would meet the last-described chain of triangles. The chain connecting the Atlantic and Pacific coasts has been completed nearly across the State of Nevada, and the reconnaissance includes nearly half of Utah Territory. The line is also surveyed at various points in Colorado, Kansas, Missouri, and Illinois. Besides all this, isolated regions in Wisconsin, Indiana, Illinois, Ohio, Kentucky, and Tennessee have been reconnoitred by the Coast and Geodetic Survey, in a way indicative of a plan designed ultimately to cover the entire territory. As its appropriations for some years past have made provision for the collection of data for a general map of the United States, we may fairly regard the Coast and Geodetic Survey as having undertaken a trigonometric survey of the whole United States.

The general views of the Committee respecting the working of the departments of the Government are worthy of special attention. They conceive it to be a sound principle that Congress should not undertake any work which can be equally well done by the enterprise of individual investigators. The leading universities are constantly increasing the means of scientific research by their professors and students; and, while the Government may with propriety encourage and cooperate with them, there is no reason why it should compete with them. The scientific work of the Government ought not, therefore, to be such as can be undertaken by individuals. It should also be confined to the increase and systematisation of knowledge tending "to promote the general welfare" of the country. Within these two restrictions there is a large and increasing field, which is only partly occupied by the organisations now under consideration.

The attention of Congress should be directed to the fact that the administration of a scientific bureau or department involves greater difficulties than that of a purely business department. The connections between the work done and the results ultimately to be attained for the public are not at all obvious to the people and press, and thus the great benefit of vigilant watching and constant criticism is wanting. Again: its administration requires a combination of scientific knowledge with administrative ability, which is more difficult to command than either of these qualities separately. These difficulties are intensified by the absence of any central authority to control the work of a Government scientific organisation. Each head of a scientific organisation is now practically absolutely independent, and, in his individual judgment of what his organisation shall do, is controlled only by Congress itself, acting only through its annual appropriation bills. The Committee conceive that this state of things calls for measures of reform.

A feature of such reform will be the collection of the organisations now under consideration, together with such other scientific bureaus as Congress may see fit to include in the scheme, under one central authority, to be recognised as responsible for, and controlling generally, the scientific operations of the Government. Various forms of such an authority might be devised, the choice of which will some day be made by Congress. The best form would be, in the opinion of the Committee, perhaps, the establishment of a "department of science," the head of which should be an administrator familiar with scientific affairs, but not necessarily an investigator in any special branch.

"Your Committee," the Report concludes, "states only the general sentiment and wish of men of science, when it says that its members believe the time is near when the country will demand the institution of a branch of the executive government devoted especially to the direction and control of all the purely scientific work of the Government. In this day the pursuit of science itself is, visibly to all men of education, directly connected with the promotion of the general welfare.

"Should such a department be now impracticable, should public opinion not be now ready for it, the next best measure, in the opinion of scientific men, would be to transfer all such work or bureaus to some one executive department. Keeping in mind what has been said respecting the two classes of work under the Signal Service, we are of opinion that the functions of the several organisations under consideration could now be most advantageously divided among perhaps four bureaus, viz.:—

"1. The Coast and Interior Survey, to be concerned principally with geodesy and hydrography, and to consist of the present Coast and Geodetic Survey.

"2. The Geological Survey, to comprise the present Geological Survey with its organisation unchanged.

"3. The Meteorological Bureau, to which should be transferred so much of the present *personnel* and functions of the chief signal office as are not necessary to the military duties of that office.

"4. A physical observatory, to investigate the laws of solar and terrestrial radiation, and their application to meteorology, with such other investigations in exact science as the Government might assign to it. In this connection, attention is called to a resolution passed by the recent Electrical Conference in Philadelphia, requesting the establishment, by the Government, of a Bureau of Electrical Standards. We are of opinion that the functions of the Bureau of Weights and Measures, now performed by the Coast Survey, could be advantageously transferred to the proposed bureau, and extended so as to include electrical measures.

"The members of your committee are conscious that placing these bureaus under one department would not necessarily result in the proper co-ordination of their work, because the head of such department would probably find it impracticable to enter into the consideration of all details necessary to that purpose. It appears to us that the evils already pointed out require, in any case, the organisation of a permanent Commission to prescribe a general policy for each of these bureaus. The functions of this Commission would be:—

"1. To examine, improve, and approve the plans of work proposed by the several bureaus, and to revise their estimates in accordance with such plan. The performance of this duty would require consultation with their chiefs generally and separately respecting the character of their work, and they should be members of the Commission.

"2. To approve in detail the methods of expenditure of the appropriations.

"3. To recommend such measures as they deem necessary to the efficiency of the bureaus under their supervision. It should, however, be understood that this Commission is not charged with purely administrative responsibility. It prescribes what shall be done, and recommends any measures necessary to secure that object, but does not concern itself with administrative details.

"We submit the following as a suggestion for the formation and *personnel* of such a Commission:—

"The Commission shall consist of (1) the President of the National Academy of Sciences; (2) the Secretary of the Smithsonian Institution; (3) and (4) two civilians of high scientific reputation, not otherwise in the Government service, to be appointed by the President of the United States for the term of six years; (5) one officer of the Corps of Engineers of the army; (6) one Professor of Mathematics in the navy, skilled in astronomy—these two to be designated by the President of the United States for a term of six years—who, with (7) the Superintendent of the Coast and Geodetic Survey; (8) the Director of the Geological Survey; and (9) the officer in charge of the Meteorological Service; shall constitute the Commission of ——. The Secretary of the —— department shall be *ex officio* President of the Commission.

"The members of the Commission, for their services as such, shall each be paid by the United States compensation in the sum of —— dollars per annum. Their necessary transportation and travelling expenses shall be provided for as are those of the officers of the army and navy when travelling on public business or duty, to be paid out of the appropriations for the services under their supervision.

"The Commission shall meet in Washington, D.C., for the transaction of business, not less than four times a year; but the President of the Commission may convene it whenever in his judgment the exigencies of the service require a meeting.

"The Commission shall be attached to the office of the secretary of the department of ——, and under his superintendence shall exercise a general control over the plans of work of the Coast and Geodetic Survey, the Geological Survey, and the Meteorological Service, and shall have the charge and custody of all the archives, books, documents, drawings, models, returns, apparatus, instruments, and all other things appertaining to the Commission.

"The estimates of the heads of these bureaus or offices shall pass through the Commission for revision and approval; and, after the annual appropriations have been made, no money shall be expended under them, except after revision and approval by the Commission of projects submitted by these bureaus in compliance with such projects.

"If at any time public money is being spent by any of these bureaus not in accordance with the views of the Commission, the Commission shall notify the proper auditor of the fact."

Our readers are already aware that the Congressional Committee appointed to consider the organisation of the surveys and other scientific work of the Government made no report at the last Session of Congress. The Commission was, however, continued as a Commission of the succeeding Congress. The expired places of Messrs. Pendleton and Lyman were filled by new appointments from the members elected to the next Congress. A meeting of the reorganised body has been held, which adjourned until next November without coming to any definite conclusion respecting the measures to be finally proposed. Before adjourning, Major Powell was authorised to make public the testimony which he had laid before them on different occasions, and which covers most of the points to be acted on by the Commission.

Major Powell's statements naturally include a very detailed account of the methods, work, organisation, and expenses of the Survey over which he presides. He also submitted his views upon the best method of consolidating the geological and coast surveys with the other scientific bureaus of the Government. This is the really important question before the Commission, since upon its decision must turn the general efficiency of the Government scientific service for a long time to come. The necessity for some such consolidation is strongly felt in Congress as well as outside of it. The one danger to be avoided is that of some hasty plan being adopted which may suit the exigencies of the moment, but may not work well after those exigencies have passed.

One very strong reason for placing the scientific bureaus under one head, or in one department, is that scientific work has many features peculiar to itself, which require it to be conducted upon principles different in some respects from those which prevail in other departments. The head of an ordinary bureau or department of the government, and indeed every man in public life, is conversant only with offices and duties which there is no serious difficulty in satisfactorily filling, with the aid of that knowledge of men and of the world which he acquires through his daily intercourse with others. Such a person is accustomed to finding scores of candidates for every office, from whom a suitable selectionis always possible. The idea of an office for which there may be no applicants, or, if there are any, for which it is morally certain that the applicants are all unfitted, no matter how good their recommendations, is one which he finds it difficult to assimilate. Indeed, in the case of the purely scientific office, the ability to find the proper men must be a part of the life education of the man who is to make the selection. It is safe to say that the best officers who have served in the coast and geological surveys are men who, under the ordinary system of Government appointments, would never have been heard of in connection with the positions which they so ably fill.

The same thing is true of the administration of a scientific bureau. No uniform system can be devised which will apply to all the details of a great scientific work. When we go beyond the regular routine operations it is needful that the duties shall be accommodated to the man, and that in many cases a larger measure of liberty shall be allowed the latter than could be tolerated in the usual operations of a Government department. All this requires, on the part of the administrative head of the department, an appreciation of the subject which can only be acquired by long familiarity. If the head is not specially charged with mastering the peculiar methods of administration thus rendered necessary, the chances are that he will fall into one of two opposite errors: either he will leave the heads of the scientific bureaus to manage things in their own way, without any administrative control whatever, or he will exert his authority in such a way as to endanger the efficiency of the work. The former is undoubtedly the more natural course to take, and thus arise the friction and duplication of work which so seriously impair efficiency and discipline.

Yet another feature of Government scientific work is that it is far removed from that public criticism which is so conducive to efficiency in other branches of the service. It is difficult to conceive that such a state of things as was exhibited by the surveys of the territories ten years ago could have existed in the performance of any work with which the public were conversant. At that time we had at least two independent surveys of the territories, prosecuted by different departments of the Government and with nominally different objects, but which were practically identical in their actual work. The officers in charge were independently surveying and mapping the very same regions. At the time that Hayden's Atlas of Colorado was published, Capt. Wheeler was engaged in surveying Colorado and making maps of the territory substantially identical in their objects with those of Hayden. Both surveys were intended to cover the whole public domain.

Nothing quite so bad as this is likely to arise in the future. But there is still room for much duplication of work as well as taste through competition in getting possession of particular fields. As a general rule, the head of a department is quite ready to approve of any extension of work which any of his bureau officers may propose, and has not always time to learn that the same work is being done, or might be better done, by some other department. The annual provision which Congress has got into the habit of inserting into the appropriations for the Signal Office—"provided that hereafter the work of no other department, bureau, or commission authorised by law shall be duplicated by this bureau"—is not quite satisfactory: it leaves open the question whether any proposed work is "the work of any other department, bureau, or commission."

The report of the National Academy of Sciences proposes to remedy some of these evils by placing the general policy of the scientific bureaus under the control of a mixed commission, organised somewhat after the plan of the Lighthouse Board. If the bureaus are to remain separate, we see no better plan than this for securing the proper coordination of their work; but Major Powell points out certain difficulties in the way of its successful operation. His strongest objection is, that subordinate officers of various departments would have to practically control the work, thus reducing the heads of the departments to channels for transmitting instructions. If the proposed Commission were to assume any administrative control of the work, this objection would certainly be fatal. The official responsibility of the head of a department for the work of his bureaus should not be interfered with. But the report of the Academy expressly disclaims charging the Commission with any administrative responsibility. Its sole function was to prescribe the policy of the bureaus; that is, to decide what each one should do, and what each one should refrain from doing, the whole execution of the work decided upon being left completely in the hands of the regular authorities. We see no reason why this should be "irksome" to the heads of the departments. We also feel that Major Powell assigns undue importance to the influence of the single military officer proposed by the Academy as one of the nine members of the Commission. It is not so clear to us, as it seems to be to him, that one such officer could leaven the whole lump of the Commission with ideas of military discipline unsuitable to the conduct of a scientific bureau.

But however favourably we may view the plan of this Commission, we must hold that the consolidation of the bureaus under a single head, or in a single department, would give far more assurance of efficiency. Especially is this the case with the two national surveys. Their work now covers the same fields, and their mutual interdependence is such that they should work under a common plan. The Geological Survey requires for its proper execution certain geodetic and astronomical work, the execution of which is not within the proper province of the geologist. It is absolutelynecessary that this geodetic and astronomical work should be so planned and executed as to meet the wants of the Geological Survey, and at the same time it is the proper function of the geodetic survey. We are informed by Major Powell that he makes use of all the coast-survey results so far as they are available, but he does not indicate what fraction of his labour is thus saved; and it goes without saying that he has no authority, directly or indirectly, to require that the coast and geodetic survey shall do anything which he may want done.

Among the suggestions made by Major Powell was one that all the scientific bureaus should be placed under the general direction of the regents of the Smithsonian Institution. This does not appear to have been considered practicable, and was not further urged by the director himself. One of the possible plans is to place all these bureaus under the interior department. The principal objection to this course is that that department is already overloaded with work, so that its head could not give the proper consideration to the subject. Yet this is the simplest course, and would certainly be an improvement on the present state of things. The more effective course would be to form a separate department of science and public works. To this there seems to be no positive and serious obstacle except the difficulty of getting any measure of the sort enacted into a law. The question whether the head of the department should be a scientific expert or a public administrator is an ulterior one, which need not be discussed at present. In the latter case the question of its being regarded as a cabinet office would arise. There will be little hesitation in deciding this question in the negative.

THE LICK OBSERVATORY[1]

THE Lick Observatory, in its present condition on the summit of Mount Hamilton, California, is so nearly completed, with the exception of the great telescope, that the institution may now be sketched to advantage in its permanent form. In an early issue of *Science*, therefore, this enterprise will be traced through its various stages, from the inception onward. Astronomers have been slow to avail themselves of the great advantages of mountain elevation and isolation in the prosecution of astronomical research, partly because of the pecuniary outlay attending the necessary expeditions, but chiefly because some of the earlier expeditions to mountain summits were not attended with results of especial importance, and, on good theoretical grounds, the meteorological conditions of such stations appeared likely to be so unfavourable as to counterbalance fully the advantages to be derived from mere elevation.

[1] From *Science*.

And besides, the evidence derived from the two most famous expeditions—that of Prof. C. Piazzi Smythe to the Peak of Teneriffe and of Mr. William Lassell to Malta—was so contradictory in character as to afford very good ground for abandoning the hope of immediate advantage to astronomy from superior elevations.

It is not possible to say how far Mr. James Lick was acquainted with these endeavours of scientific men; nor need the immediate circumstances or events which impelled him to his extraordinary astronomical bequest be considered here. Prof. Newcomb points out the fact that his movement followed close upon the completion of the great Washington telescope in 1873, then the largest in existence. Had Mr. Lick known the opinions of the best astronomers on the subject of mountain observatories, and the likelihood of securing, on elevated and isolated peaks, results at all commensurate with the trouble and expense of occupying such stations, he would have found very little to encourage the project. In this case, however, as very often before, a little experience has proved to be worth more than an indefinite amount of scientific theorising. It has been said that the scheme of building "a powerful telescope, superior to and more powerful than any yet made," was the nearest of all to the heart of Mr. Lick: there is abundant evidence that this is true; and it may be also true that he regarded the Observatory as an appendage of the telescope. But the course of subsequent events has proved it a matter for sincere gratulation in astronomical circles that he ever regarded either the Observatory or the telescope at all; for, had not the prospective researches with the great telescope arrested his attention, there is very little reason for believing that, in so far as he was concerned, astronomical science would ever have been in a position to reap benefit from the splendidly equipped Observatory which already exists on the summit of Mount Hamilton.

That Mr. Lick was bound, heart and soul, in the project, not only of a great telescope, but of the best possible location for it, is evident from the fact that, when nearing his eightieth year, and although oppressed with physical infirmity, he resolutely undertook a waggon journey of some forty miles or more, reclining on a mattress, all for the sake of investigating a proposed mountain site in person. His solicitous concern for the enterprise was very marked. Those who knew him best say that, if his practical knowledge of astronomy had been greater, he would have given every penny of his vast fortune for the great telescope, and the Observatory and its endowment. He would have recognised, too, the great improbability of such an institution being completed within a period of a few short years, and would thus have been led to provide for the reasonable use of the instrumental equipment as fast as it was put in place on the mountain. The failure to make such provision constitutes the chief point of unfavourable criticism on the part of astronomers, and is in many respects unfortunate; but sundry advantages also have arisen from it, which may be recognised with more profit, particularly as this condition of things must remain unalterable until the great telescope is completed, and the entire institution comes under the administration of the University of California, in full accord with the terms of Mr. Lick's bequest.

Five years ago no one could have anticipated that the year 1886 must pass with the great telescope still unfinished. It is worthy of note, however, that, while the delay in obtaining the necessary glass for the objective has proved so great an embarrassment to the work of the opticians, it has not as yet sensibly impeded the progress of the construction of the Observatory itself. To this fact we alluded at p. 377 of the current volume of *Science*, stating as well the very reasonable grounds for the belief that the plans of the Lick trustees, in so far as they pertain to the construction of the great telescope and the conjoint Observatory, will be completely executed at the close of the year 1887. With its unparalleled instrumental equipment, and an unusual endowment for the prosecution of astronomical research; located where the sky is cloudless most of the year, and at such an elevation as to be above the clouds a great part of the remainder; and situate in a region, too, where the steadiness of the air permits astronomical measurement of the highest precision to proceed uninterruptedly throughout the entire night for months at a time,—the Lick Observatory is destined, under prudent management, to take its place at once in the foremost rank; and, although it is the first established mountain observatory, it may well expect to hold its own in the emulation of similar institutions which may subsequently be inaugurated at greater elevations.

TWILIGHT[1]

THIS essay, an extract from a more comprehensive work on the problem of twilight, which the author hopes to conclude in the course of this year, and embodying a lecture recently delivered by him both in Hamburg and Leipzig, describes the phenomena of twilight in general and of the remarkable sky-glows of the winter of 1883 in particular, with clearness, fullness, and exactness, and explains the physical causes of these phenomena from a special and mature study of that universally interesting field of observation, by numerous highly pertinent and illustrative experiments, and altogether in a manner which should bring home, even to the unscientific reader, a new sense and a new intelligence of the painting offered anew every morning and evening to the study and delight of man universally.

After relating and taking measure of the stupendous outburst of Krakatoa and the brilliant glows involving nearly the whole earth for a long period after that event, and comparing these two consecutive phenomena with the analogous phenomena of the outburst of "Graham Island" in 1831, followed by brilliant twilights and peculiar blue and violet sun colours, attracting the admiration, in particular, of Italy, France, and Germany, the book addresses itself to the task of investigating the physical laws concatening these two apparently heterogeneous phenomena, and why all volcanic outbursts are not attended by the same wonderful optic displays. While each particle of dust, smoke, or fog causes a bending or diffraction of the light, a collective effect, comprehending a brilliant development of colours, is produced only when all the particles of matter are of equal size and are distributed uniformly in space—a condition not even most remotely fulfilled in the case of ordinary smoke and fog. Diffraction includes the lateral dispersion of the light, which is all the more efficient the nearer the edges lie to each other, and therefore the smaller the particles are, and also the "interference" of like-coloured rays of light. When a red light falls, for example, on a fine glass thread or a diamond stroke scratched into glass, the shadow will consist not of one thin black line, but of a whole system of parallel stripes alternately dark and brilliant, *i.e.* black and red. When, again, a white light falls on the diamond stroke, the reflection shows a system of parallel stripes glowing in all the colours of the rainbow. In the case of a single line the development of colours is indeed so small as to be scarcely perceptible, but with many thousand lines of exactly the same breadth, and situated at exactly the same distance from one another, the reflex image is such that, taken up on a white screen, it is visible at great distances. Perfectly corresponding is the case with granules of dust. The shadow of a single granule of dust in red light consists of a system of concentric rings, alternately dark and redly luminous, which are all the broader the smaller is the granule. In white light, on the other hand, the shadow of the granule consists of alternately dark and bright rainbow coloured rings. If the dust granules are all of the same size, then will the like-coloured rings pretty nearly coincide, and, in the case of a sufficiently large number of granules, the reflex image will be composed of coloured rings of great luminousness. If, on the other hand, the dust-granules are of different size, then will all the different colours coincide, and, according to a well-known optic law, the image will be colourless. The image of a dust-cloud may, therefore, be rich in colours, poor in colours, or colourless, according as the particles of dust of which it is composed are of the same or of different size.

The experiments of Coulier and Mascart, extended by Aitkin, have demonstrated that in a perfectly moist air, no formation of fog is possible, however much the temperature is lowered, so long as the air is absolutely free of dust; and that the more air, sufficiently moist, is charged with such foreign particles, the more intense is the formation of fog under a sufficient lowering of the temperature or pressure of the air. Let filtered and completely moist air in a glass ball have its pressure diminished, then will only a few particles of fog reveal themselves to the most careful inspection, even under the powerful light of an electric lamp—particles of fog which, moreover, yield not the slightest coloured image. Admit now into this filtered air a few cubic millimetres of ordinary house air, then will a very fine, silvery, transparent fog at once form itself, of such slight density that even in the case of a considerable area of it the transparency of the atmosphere would be but very little affected. At the first moment of its formation let a reflected image of the sun, or the reflected light of an electric lamp, be viewed through it: the image will be seen surrounded by an intensely luminous blue or greenish light, with a broad, reddish ring, the colouring of which may range through all stages from brilliant purple red to the most delicate pale pink.

The phenomena of colour produced and explained by experiments of the above description are made to serve as the key to the more extensive but essentially identical phenomena composing the total process of twilight, which is distributed, like a spectacular play, into three acts with a prelude, and sometimes, though comparatively seldom, an afterlude—parts which, however, are not strictly distinguished in time, but occur to some extent simultaneously and overlap each other; as also to the comparatively unimportant deviations—apart from the intensity of colouring—from the normal course, which obtained in the remarkable sky-glows that arrested universal attention throughout the fall and winter of 1883.

[1] "Die Dämmerungserscheinungen im Jahre 1883 und ihre physikalische Erklärung." Von J. Kiessling, Professor am Johanneum zu Hamburg. (Hamburg und Leipzig. 1885.)

HENRY MILNE-EDWARDS

HENRY MILNE-EDWARDS was born at Bruges in October, 1800. Having completed his elementary studies in Belgium he attended medical lectures in Paris, where he took his diploma in medicine in 1823. While he retained an interest in medical and surgical pursuits until late in life, and was a member of the Academy of Medicine, Paris, of the Medical Societies of London, Edinburgh, &c., his earliest passion seems to have been for the study of natural history, and he soon abandoned the practice of his profession and devoted himself to scientific researches among the lower forms of animal life.

During the years 1826 and 1828, in company with his friend and fellow-labourer Audouin, the assistant to Lamarck and Latreille, he made a careful study of the various invertebrates to be met with on the coasts at Granville, around the Isles at Chansey, and as far as Cape Frehel. A member of the French Academy was, during 1828, engaged on some hydrographical work off this coast, and good-naturedly assisted the littoral zone workers, enabling them to use the dredge in somewhat deeper water than they could reach from a row-boat. The results of these investigations were laid before the Academy of Sciences in July and November, 1829, and formed the subject of an elaborate report presented to the Academy in November, 1830, by Cuvier, Dumerit, and Latreille, Baron Cuvier being the writer of the report. In this memoir, for the first time so far as we know, the idea of zones of marine life is promulgated; these were four in number. A considerable portion of the memoir is devoted to the subject of the bristles in Annelids and to a description and classification of the Annelids of the coast of France. The reporters did not hesitate to express their satisfaction with the work the two friends had done, calling the special attention of the Academy to the "efforts heureux par lesquels ces deux habiles naturalistes sont parvenus à enrichir la Faune française d'espèces si nouvelles et si curieuses, et la zoologie en général d'observations si intéressantes." These happy efforts were but the forerunners of others carried on, in the case of Milne-Edwards, throughout a lengthened life.

In 1841 Milne-Edwards was appointed to the Professorship of Natural History in the Collége Royal de Henri IV., and about the same time we find him holding the Chair of Zoology and Comparative Physiology at the Faculty of Sciences, of which Faculty he was afterwards the Dean. On his friend Audouin's death, he was made Professor of Entomology at the Museum, Jardin des Plantes.

A considerable number of original memoirs, the titles of which it is here unnecessary to detail, were published about this period by Milne-Edwards in the *Annales des Sciences Naturelles*. This famous periodical first appeared in 1824, under the editorship of Audouin, Brogniart and Dumas. In 1834 the second series, from which geology and mineralogy were excluded, commenced under the joint editorship, for the zoological portion, of Audouin and Milne-Edwards, so that for now fifty years the zoological department has been under his management.

While labours as important as they were numerous secured for H. Milne-Edwards a high position among men of science, his name was also universally well-known and made popular by his elementary works on zoology. His "Eléments de Zoologie" were published in 1834 and were reissued in 1851 as a "Cours élémentaire de Zoologie." This work had an enormous circulation in France, and has not only been translated into several other languages, but also, until almost the other day, it formed the stock-in-trade, either as to its text or its illustrations, of most of the many small elementary works on natural history published in Europe.

In 1838 Milne-Edwards was elected a member of the Academy of Sciences in the section of anatomy and zoology. He was made an officer of the Legion of Honour in 1847, and a commander of this Order in 1861. In 1862 he succeeded Isidore Geoffroy Saint-Hilaire as Professor of Zoology at the Jardin des Plantes, and in a year or two afterwards was made assistant director of the museum.

Of his more important works as distinct from his memoirs may be mentioned his "Histoire naturelle des Crustacés," 1834-40. In this he was assisted by his friend Audouin, and it long remained as a standard authority on this group.

The "Histoire naturelle des Coralliares," 1857-60, was commenced after Milne-Edwards's return, in 1834, from a collectingtour on the coast of Algeria; but in 1847, in order to satisfy the calls of his publishers, he associated Jules Haime, so well known for his memoirs on the Polyps in the Palæontographical Society of London and in the *Annales des Sciences Naturelles*, with him in this work; but the death of Haime in 1856 compelled Milne-Edwards to complete the work himself. It is in a few tender words dedicated to the memory of Jules Haime.

"Leçons sur la Physiologie et l'Anatomie comparée de l'Homme et des Animaux" were published between 1857 and 1881, in fourteen volumes. The series is dedicated to his friend, M. J. Dumas, to whom he had dedicated the first work of his early pen. These lectures will always possess an importance to the student, from the immense mass of details, accompanied with copious references to the labours of others, that are brought within a limited compass.

"Recherches anatomiques et zoologiques faites pendant un Voyage sur les Côtes de la Sicile, &c.," forms a splendid quarto volume of over 850 pages, which are illustrated with nearly 100 coloured plates. This work is, for the most part, a corrected report of a series of memoirs contributed to the *Annales des Sciences Naturelles* by Milne-Edwards, A. de Quatrefages, and Emile Blanchard.

There can be little question that the name of H. Milne-Edwards will always rank high among the naturalists of the first half of the nineteenth century, and for years he was incontestibly one of the leaders of zoology. He was among the first who, not content with the study of the dead forms of animal life, made prolonged visits to the sea-coasts to study the living forms and to investigate their habits. These were days before biological stations were thought of and when the details of geographical distribution were little known. That Milne-Edwards's study of the geographical distribution of the lower forms of Invertebrates led him to the theory of there being centres of creation was what, from a purely zoological point of view, might have been expected; and when larger and truer views burst upon the world through the genius of Darwin, Milne-Edwards's mind, already preoccupied, was never altogether able to take them in. By the student of biology Milne-Edwards will be remembered by his theory of the division of physiological labour, one which threw an interesting light on many an intricate problem.

H. Milne-Edwards was an excellent linguist. English he spoke like a native. In manner courteous, he was kindly and affable to all. His house at the Jardin des Plantes was for years the focus of attraction for all the men of science in or visiting Paris. He was the possessor of a splendid library, the treasures of which were most freely at the services of students. He was a member of most of the learned Academies of Europe and America, and the possessor of several orders of State. Full of years and service, he died in Paris on July 29 last. As Geoffroy Saint-Hilaire was on his death succeeded by his son Isidore, so, happily for zoology, Henry Milne-Edwards has, in his son Alphonse, handed down his name and place to one every way worthy of both.

RADIANT LIGHT AND HEAT

Preliminary Notions

IT has been known from time immemorial that a sufficiently hot body when left to itself gives out light and heat, and likewise grows cold. It has also been known that a body not sufficiently hot to give out light may yet be capable of giving out heat, cooling as it does so.

If the above facts be studied scientifically they at once give rise to a series of important issues, all of which we are now in a position to reply to. These may be put in the form of the following questions:

(1) Is radiant light a substance or, if not, what is it?
(2) With what velocity does it move through space?
(3) Is radiant heat physically similar to radiant light?
(4) What is meant by a hot body?
(5) In what manner is the issue of radiant light and heat related to the cooling of the body?

Of these five questions the second was the first to receive a solution, and this through the aid of astronomical observations.

Römer, a Danish astronomer, determined in 1675 the velocity of light by means of the eclipses of Jupiter's satellites. It so happens that the planes in which the earth and Jupiter move around the sun, as well as the plane in which Jupiter's satellites move around that planet, coincide very nearly with each other. As a consequence the first or nearest of Jupiter's satellites passes within the shadow of the planet at intervals of 42hr. 28m. 36s., and thus becomes obscured.

Now, if light were to travel instantaneously from Jupiter to the earth we should always see this obscuration at the moment when it took place. But even if light required time to travel, yet if the earth were always at a constant distance from Jupiter we should see the obscuration at a constant interval of time after its occurrence. Now Römer found that when the earth was furthest away from Jupiter there was a retardation in the time of the occurrence equal to 16m. 36s., as compared with that when the earth and Jupiter were nearest together.

It will be seen from the diagram (Fig. 1) that the

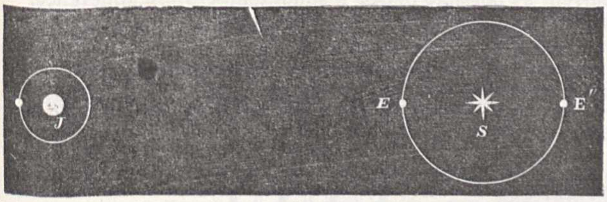


FIG. 1.

earth and Jupiter are nearest together when the earth is between Jupiter and the sun, and that the two are furthest apart when the sun is between the earth and Jupiter. Hence it follows that the difference in the distances from each other of the two planets in these two positions is equal to the diameter of the earth's orbit, or 183,000,000 of miles. If, therefore, light takes 996 seconds to cross this distance it ought to travel at the rate of 184,000 miles per second.

The velocity of light has likewise been determined by experiment. The arrangement for this purpose adopted by Fizeau is the one most easily understood. It consists of a toothed wheel, which may be made to revolve with great rapidity. Now a ray of light is made to pass through one of the intervals between the teeth, and to fall upon a reflecting mirror placed at a considerable distance off in such a manner that when the wheel is at rest the ray will be reflected back through the same interval. If, however, the wheel is in rapid motion it is possible that during the time which the ray takes to travel to the reflecting surface and back again the wheel may have moved so much that the ray is caught by the next tooth, and not allowed to pass through; while, if the motion be still more rapid, the ray may get through the next interval, and so on. Without entering more minutely into the conduct of the experiment, it will at once be seen that we have here the means of measuring the velocity of light.

By these and similar methods this velocity is now very accurately known, and is found to be about 187,000 miles, or 300,000 kilometres per second.

The evidence is very strong that all varieties of light, whether red, orange, yellow, green, blue, indigo, or violet move through vacant space with the same velocity.

Having thus briefly replied to the second of these questions, let me now return to the first, and inquire as to the nature of radiant light. We are able to conceive of two, and only two, varieties of progress in space. The one of these is the progress of actual matter, the other the progress of a form. An arrow discharged from a bow, or a bullet from a gun, represents the former of these, while the ever-widening circles which follow the plunge of a stone into a pool of water represent the latter. The progress which is visible when the wind blows along a field of corn or grass is another good illustration of a moving form. Here the corn or the grass is certainly not carried along, and if the wind is so carried, yet we cannot see the wind. What we see is an advancing form due to the oscillating motion of the various heads of corn or blades of grass. In like manner when a cannon or a gun is discharged at some distance from us the noise reaches our ear after a greater or less interval, depending upon the distance. Here it would be absurd to suppose that certain particles of air had been shot all the way from the cannon into our ear with the constant velocity of 1,100 feet per second—this velocity in the case of a gun or pistol being likewise the same as when the most powerful cannon is discharged. It is well known that in this instance a blow is given to the air, thus causing an arrangement of condensed and rarified particles which progresses with a certain definite velocity. The speed of progress of this form may either be determined by direct experiment, or by calculation founded on the well known properties of air—the two methods agreeing perfectly well together.

Now in many respects there is a strong analogy between sound and light, and these very questions which have been asked for sound are equally appropriate in the case of light. Can it be thought likely that hot bodies emit myriads of very small particles, which pass through space with the enormous velocity of 187,000 miles per second? or again, is it likely that this velocity should be precisely the same for all bodies and for all temperatures?

It is a singular circumstance that the illustrious Newton, to whom science owes so much, and one of whose achievements was a correct, or nearly correct, analysis of the conditions of undulatory motion in air, should nevertheless have become a powerful advocate of the corpuscular theory of light, thus lending his great authority to retard the progress of the rival theory, which represents light as an undulatory motion, similar in many respects to that which constitutes sound.

It is to Huyghens in the first place, and to Young and Fresnel in more recent times, that we owe the establishment of the undulatory theory of light upon so firm a basis that the older hypothesis is now entirely forgotten, or regarded only as a scientific curiosity.

There are two ways in which a theory may break down. Its various assumptions may display a great lack of living energy, or, in other words, may exhibit inability to expand themselves so as to incorporate a large volume of fact. Each new fact would thus imply the construction of a fresh assumption, so that there would be as many hypotheses as facts. A cumbrous structure of this kind, it is needless to say, would be utterly useless as a scientific instrument, and would finally fall to pieces from its own weight.

Another mode in which such a theory may break down is by the promulgation of some statement which is ultimately found to be contrary to fact. The corpuscular theory of light has broken down in both of these directions. For, in the first place, it had to be propped up by many fresh assumptions devised solely for the purpose of explaining fresh facts, and wholly useless in any other respect. In the next place one of its fundamental statements was ultimately contradicted by an appeal to experiment, carried out by M. Foucault, an eminent French observer. According to the corpuscular theory, or that of emission, the velocity of light ought to be greater in water than in air. On the other hand, according to the undulatory theory, the velocity in water is less than in air. If, therefore, it can be shown that light moves faster in air than in water then the undulatory theory is right; if the contrary, then the theory of emissions is right. Foucault succeeded in showing by an experimental method that light travels faster in air than in water, and this result has ever since been considered as decisive in favour of the undulatory theory.

We come now to our third question: Is radiant heat physically similar to radiant light? Here the difficulty is an instrumental one; the difficulty, in fact, of inventing something which shall do for dark heat what the eye can do for light.

At a comparatively early period Sir John Leslie devised his *differential thermometer*, with which he was able to obtain valuable results, to be hereafter alluded to. In this instrument we have two bulbs, A and B, filled with air, and connected together by a bent tube (Fig. 2) the lower

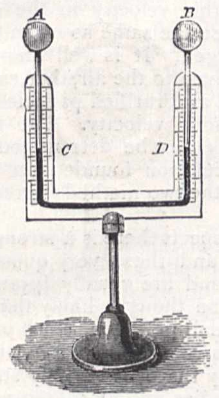


Fig. 2.

portion of which is filled with some coloured liquid, which ought not to be volatile. Let us begin by supposing that both bulbs are of the same temperature, and that under these circumstances the air is at the same pressure in both. The line between C and D, the surfaces of the liquid, in the two tubes will consequently be horizontal. Now suppose that the bulb A is heated, its air pressure is in consequence increased, and hence the liquid will be pushed down at C and up at D. In like manner if B is heated the liquid will be pushed down at D and up at C, and the change may be roughly taken as proportional to the difference in temperature between the two bulbs, this difference being supposed to be small. If, however, both bulbs are heated simultaneously, and to the same extent, there will be no motion in the liquid, inasmuch as there will be no difference in pressure of the air of the two bulbs.

In consequence of this mode of action the instrument has received the name of the differential thermometer;

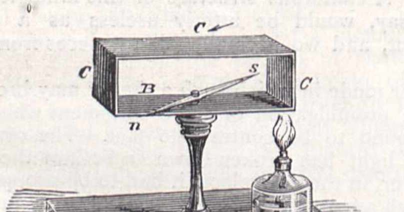


Fig. 3.

indeed, it is abundantly evident that what is measured is not the absolute temperature of A and B, but only the difference in temperature between the two.

Delicate as this instrument might at first sight appear to be, it forms but a poor substitute for the human eye, and had it not been for a new discovery, we should not have been able to make much progress in our knowledge of dark heat. The discovery alluded to is that of Seebeck, who found that in a circuit, composed of two metals soldered together, a current of electricity is produced when one of the junctions is heated, while the other is kept cool. If, however, both junctions be simultaneously heated to the same extent, no current is produced.

Here, then, we have an instrument similar in principle to that of Leslie, or, in other words, a new species of differential thermometer, and we shall now show that this arrangement is capable of being made extremely delicate as a measurer of small differences of temperature. The existence of a current of electricity is easily known by the motion of a magnetized needle, which tends to place itself at right angles to the direction of the current. Suppose now we have a circuit (Fig. 3), in which *C* denotes copper and *B* bismuth, and that we heat one of its junctions as in the Figure. We shall have, in consequence, a positive current following the direction of the arrow head, and the north pole of the needle will be pushed towards the observer as indicated in the Figure. When we make use of a magnet to measure a current we call our instrument a *galvanometer*. Our object, therefore, in this arrangement, is clearly to get as large a current as possible out of a small temperature difference, and then to measure this current by means of a galvanometer made as delicate as possible.

In order to obtain as strong a current as possible we must make use of a considerable number of junctions, as in Fig. 4, only in practice these junctions are very close

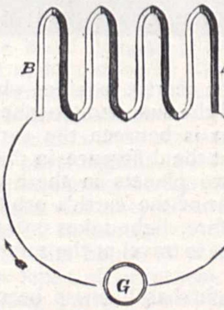


Fig. 4.

together. Here the heating influence is applied to the upper junctions, while the lower ones are kept cool. Another point is to select two suitable metals for our junctions—that is to say, metals the heating of which shall produce a powerful current. This is done by consulting a thermo-electric list of metals; in other words, a list such that the positive current shall go across the heated junction from the metal nearest the top to that nearest the bottom of the list.

The following is a series of this nature:

Bismuth.	
Nickel.	Silver.
Lead.	Zinc.
Tin.	Iron.
Copper.	Antimony.
Platinum.	Tellurium.

Now there is an important law which holds with reference to this series. If, for instance, we have a compound circuit, such as that in Fig. 5, connected with a galvanometer, we shall get the same current *in one direction* by heating through 1° C. the copper and tin junction, and also the tin and antimony junction, as we shall *in the opposite direction* by heating the antimony and copper junction. In other words, the various metals in the above list are to be regarded as being at so many different levels, and the strength of the current depends upon this difference of level, and not at all upon the exact number of halting places we make use of in going from the one level to the other. It thus appears that we shall get the greatest effect by selecting two metals near the opposite extremities of the list. Bismuth and antimony are generallly the metals chosen, and 36 or 49 junctions of these are frequently used, the metals being packed close together, but insulated from each other, and thus forming a sort of cube, each end of which contains, say, 36 junctions. These junctions are generally covered with lamp-black. If the one end of this be heated we shall have a current in the one direction; if the other end, we shall have one in the other direction; while, if both ends be heated simultaneously and to the same extent,

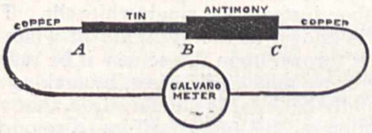


FIG. 5.

we shall have no current whatever. This arrangement forms what is termed a *thermopile*, and the cube of elements is generally encased in a brass covering presenting two terminals, in which the wires of the galvanometer are to be inserted and screwed tight. Inasmuch as this arrangement is generally used for viewing and measuring heat rays, a brass cone polished in the inside is often attached to the thermopile (Fig. 8) with the view of catching a large area of heat rays and reflecting them into the pile.

The galvanometer consists essentially of a magnet, which is delicately suspended by a very fine thread. Around it we have numerous coils of wire (but not in this case *very* numerous coils of *very* fine wire), which convey the current, each single coil counting separately in its action upon the magnet.

The various coils must, of course, be insulated from each other. A comparatively weak current will thus produce a visible effect if there be only a sufficient number of coils.

But yet the result so obtained is not the best, because we are having, after all, a strife between the influence of the current and that of the earth upon the small magnet. Assuming that the galvanometer was so placed to begin with that the magnet was in the magnetic meridian, then the current will tend to move the magnet to a position at right angles to this plane, while the earth's magnetic force will tend to keep it where it is. There is thus a strife between the two, and this will greatly interfere with the delicacy of the instrument. What we have

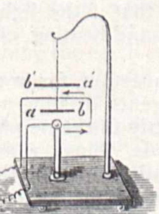


FIG. 6.

to do is so to counteract the earth's directive force that the little magnet may behave as if it was not under any external magnetic influence whatever.

A needle for which the directive effect of the earth's magnetic force is thus neutralised is said to be rendered *astatic*.

There are two ways in which this may be accomplished. We may use two needles of as nearly as possible the same strength, joined rigidly together with their poles in opposite directions, as in Fig. 6. Numerous coils of wire are wound around the lower needle, one of which we have exhibited. Here the upper current will tend to twist *b′* above the plane of the paper, while the lower current will act on *b′* in an opposite direction, this lower current, however, being further removed from the upper needle than the upper current, the latter will predominate, and the needle will, on the whole, be twisted round so as to place *b′* above the plane of the paper. Furthermore, the lower needle will be twisted round by both the upper and the under currents so as to place *a* above the plane of the paper, and hence the two needles will be twisted by the current in the same way, whilst the directive force of the earth's magnetism which opposes any motion of the needle will, by the arrangement above alluded to, be either altogether cancelled or rendered very small.

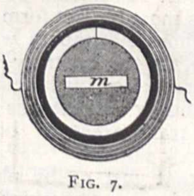


FIG. 7.

A galvanometer of this kind was employed by Melloni along with a thermopile as already described, and it was with these that he obtained the valuable results which we shall presently mention. But before dismissing this subject let us allude to some still further refinements made since the time of Melloni, which have contributed very greatly to increase the delicacy of this combination.

We have spoken about one way in which the effect of the earth's force may be neutralised, but we may likewise adopt the method of Sir W. Thomson, indicated in Fig. 8, where an external magnet, M, is so placed as to cancel the earth's action on the suspended galvanometer magnet which is supposed to be placed in the centre of G.

A still greater refinement consists in the joint use of both the methods now described. A system of two magnets

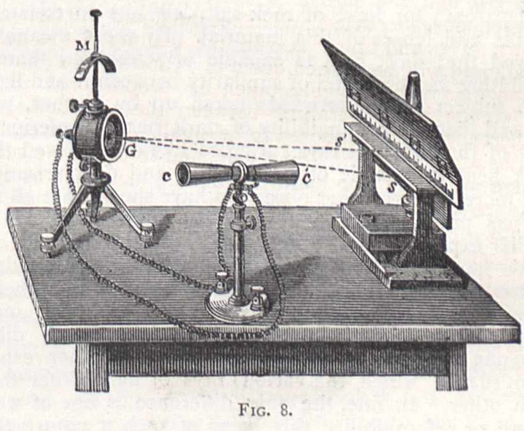


FIG. 8.

placed oppositely and united rigidly together is employed, a separate coil being made to surround each magnet. An external magnet is then so placed as to neutralize any directive force that may yet linger in the system. By this means very great delicacy almost amounting to instability may be obtained.

We shall conclude by mentioning an optical arrangement introduced by Sir W. Thomson, which greatly adds to the delicacy of the galvanometer. In this arrangement (Fig. 7) a small galvanometer magnet is attached to the back of a mirror, which mirror is suspended by a very fine thread.

Again (Fig. 8) there is a lamp behind the scale S S′, and a slit, or, better still, a round aperture below the scale, with a wire in its middle, is lighted up by the lamp, and a reflected image of this lighted aperture thrown by means of the magnet-mirror, already described, upon the scale. This image may be made to move over a large space of the scale for a comparatively small motion of the mirror. If the image be that of a round circle of light, with a wire in the centre, we shall be easily able to read the position on the scale of the image of the wire, and by this means measure the motions of the mirror with very great accuracy and delicacy.

Having thus described in detail the various arrangements tending to make the thermopile and galvanometer a very delicate instrument for measuring radiant heat, let us proceed to discuss the reply which this combination gives to the question raised.

Melloni, who it must be remembered did not work with the instrument in its most perfect form, soon began to find that very many of those substances which were transparent for light, were, on the contrary, nearly opaque for dark heat. As he continued his labours he had, however, the satisfaction of finding a substance that was as nearly as possible equally transparent for both—this substance being crystallized rock salt.

He next found that just as by placing together two screens of coloured glass, one of which absorbs the redder portion of white light, while the other absorbs all but the redder portion, we may virtually stop all the radiation, so by certain combinations of screens it was equally possible to stop all the radiation from a source of low temperature heat. In the one case the result was perceived by the eye, and in the other by the thermopile. By this means he found that green glass and alum formed a peculiarly opaque combination. He next tried the same combination for the solar rays, and found that when they were first intercepted by a screen of green glass, they had a very feeble power of passing through a second screen of alum. The similarity in the behaviour of the rays from these two sources led him to imagine that heat accompanied with light and low temperature heat, are not physically dissimilar.

Again, the discovery by Melloni of the *diathermancy* or transparency for heat of rock-salt, led him to construct prisms and lenses of this material. By these means he proved that dark heat is capable of refraction, thereby exhibiting another bond of similarity between it and light. The subject was afterwards taken up by Forbes, who showed that the refrangibility of dark heat is inferior to that of the luminous rays. Forbes likewise showed that dark heat is capable of polarization and depolarization, and more recently other observers have shown that all the various properties of light may be exhibited at will in similar experiments with dark heat.

We have thus strong evidence for believing that dark heat is similar to light, the difference between them being physiological rather than physical, or, to speak more exactly, rays of dark heat may be presumed to differ from one another and from rays of light in no other respect than that in which the various rays of light differ from each other. In fine, the only difference is one of wave length or refrangibility, this being of such a nature that dark heat is less refrangible and has greater wave-length than light.

It is desirable at this stage to say a few words about the spectrum which is obtained from a luminous source by means of a prism.

Let us suppose, for the sake of simplicity, that the luminous source is a thread or slit of light, and that, by means of a lens, after the manner of a photographer, we wish to obtain an image of this slit of light and throw it upon a white screen. This image will appear as a white luminous slit of light. If, however, we interpose a prism between the source of light and the screen we shall obtain a very different result. In the first place the rays will be much bent by the prism, so that the screen will have to be placed in a very different position in order to receive the image of the slit. In the next place all the rays which go to constitute the light will not be bent to the same extent, so that the image of the slit given by one constituent ray will be thrown upon a different portion of the screen from that given by another. Thus the red rays will be least bent, then the orange, the yellow, the green, the blue, the indigo, and the violet, these last forming the most refrangible of the rays that enter into the composition of white light. What we shall really have, therefore, will be a great number of images of the slit placed side by side without any interval between them, these images being red at the one extremity and violet at the other. We shall, in other words, be presented with a long parti-coloured ribbon instead of a single white slit. This ribbon forms what is known as the *spectrum* of white light, and if before it is thrown upon the screen it be reflected from a plane mirror we may easily show, by making the mirror oscillate rapidly backwards and forwards, that this ribbon when in motion reconstitutes itself into a colourless white. The spectrum has various properties. Part of it can affect the eye—we say part of it, for there are dark rays at either extremity which the eye cannot perceive.

Part of it can perform certain chemical changes. Here again we say part of it, because there are certain chemical changes which certain rays seem at first sight incapable of producing.

All of it is, however, capable of heating a substance upon which it falls and by which it can be absorbed. We have thus three effects—the luminous, the actinic, and the heating effects ; and certain portions of the spectrum are capable of exhibiting all the three.

If we take the action of the rays in blackening chloride of silver as a type of actinic influence we shall find that the maximum of the action is near, if not beyond, the most refrangible extremity of the visible spectrum.

If we take the effect upon the eye as our measure of light we shall find that the maximum is at the yellow, whilst if we take the heating effect of the spectrum under its usual circumstances of production we shall find that this has a maximum near the least refrangible extremity.

Now these considerations give rise to the following question : Is there only one thing present at one part of the spectrum, or are there three things ?

At first it was imagined by some of the physicists of a past generation that there was in reality more than one thing and that the light and heating effects were produced by different agents. It was, however, afterwards found that if you operate on any portion of the spectrum by reflexion, absorption, polarisation, or in any other way, all the various qualities of that region are affected in the same proportion, so that if the light effect is reduced by one-half the actinic and heating effects are reduced by one-half likewise.

This decides the question, for we cannot imagine two or three separate agents existing at the same place and each possessing exactly the same physical qualities as the other ; in other words, things which are not physically different from each other must be the same.

Thus we have now come to the conclusion that there is only one physical entity at any one part of the spectrum, and we have likewise been driven to see that in order to compare one part of the spectrum with another we must not use the eye, which has its own peculiarity, or some particular chemical substance which has likewise a partiality for certain rays.

What we have to do is to measure the amount of heat-energy possessed by the various parts of the spectrum, and this is done by allowing the rays in question to fall upon a suitable thermo-pile covered with lamp-black and then measuring the amount of heat to which they give rise by means of the indication of the galvanometer attached to the pile. A coating of lamp-black absorbs most of the rays, and if it is not absolutely perfect in this respect it is at any rate more perfect than any other substance.

We can have now a very clear conception of what takes place when we heat a body such as coal. At first it gives out a spectrum consisting of rays, all of which are less refrangible than those of the visible spectrum. Soon, however, as the coal continues to rise in temperature, it not only increases the number of such rays but takes on others of a more refrangible nature, entering into the visible spectrum when it begins to be red-hot.

Thereafter it pushes its way further and further into this spectrum, taking on successively yellow and green rays, blue, violet, and actinic rays as the temperature still rises, until at length it shines forth with the lustre of the electric light or of the sun.

Let us now proceed to reply to the fourth question, What is meant by a hot body? At first it was supposed that heat was a substance possessing mass but not weight, an imponderable, as it was termed, which insinuated itself between the particles of bodies, thus causing them to expand. This substance was further supposed to be rubbed out by friction and beaten out by percussion. It will be perceived that we have here a corpuscular theory of heat very similar to that of light, the one forming indeed the natural sequel to the other. The experiments of Davy, in which two pieces of ice both below 0° were made to melt one another by their mutual friction, and those of Rumford, made in boring cannon, sufficed, in the course of time, to convince physicists that heat cannot be a substance, inasmuch as the melting of the ice in Davy's experiments, and the heat produced in those of Rumford, would equally imply the creation in large amount of the matter of heat. It was therefore concluded by both these experimentalists that heat is not a substance but rather a species of energy. That is to say the only difference between a hot body and the same body when cold is that, in the former state the molecules of the body are in violent motion backwards and forwards, while in the last state this kind of motion is much less. This is the dynamical theory of heat at present universally held. In it heat is regarded as a kind of energy, so that when heat is produced by friction or percussion, a certain quantity of visible energy disappears from the universe, while at the same instant an equivalent quantity of heat-energy appears, or is created.

A little reflection will, however, show us that there is not here any *real* creation or annihilation, but merely the simultaneous disappearance of one kind of energy and the appearance of another; in fact, nothing more than a transmutation of energy. Joule was the first to prove the definite mechanical relation that exists between the visible energy which disappears and the heat which is generated, and according to his experiments, if a pound of water were to fall from a height of 772 feet under gravity, and if all its visible energy on reaching the earth could at once be converted into heat, the water would be found to have risen 1° Fahr. in temperature. It will at once be recognised that just as the material or corpuscular theory of heat fits into the corpuscular theory of radiant light, so does the dynamical, or energetic theory of heat fit into the undulatory or wave hypothesis. We may, in fact, imagine the little particles or molecules of heated bodies to be in a state of continual vibration resembling in this respect a bell, or the string of a musical instrument, except that their vibrations are much more rapid than those which constitute sound.

And just as the vibrations of a bell are carried off by the gaseous medium, *i.e.* the air which surrounds the bell, and ultimately affect our ear, producing the sensation of sound, so are the vibrations of molecules carried off by a medium (the ether) which surrounds them and ultimately affect our eye, producing the sensation of light. This train of thought enables us at once to reply to our fifth question, and to assert that there is a definite mechanical relation between the amount of heat which leaves a hot body as it cools, and the radiant energy which accompanies the act of cooling. And this definite mechanical relation may be stated in very simple language. If, for instance, a pound of water cools through 10° Fahr. then the radiant energy which it gives out in the process of cooling, if this should be made to impinge upon another pound of water, and be entirely absorbed by it, would heat it through 10°, so that while the one pound of water has become 10° cooler the other has been raised an equal amount in temperature.

We are now in a position to reply as follows to the questions proposed:

(1) Radiant light consists of an undulatory motion in a medium called ether.

(2) It moves with the velocity of 187,000 miles per second.

(3) Radiant heat is physically similar to radiant light, the only difference being that its wave length is greater, and its refrangibility less than those of light.

(4) A hot body is one whose molecules are in rapid motion.

(5) There is an equivalence in energy between the amount of radiant light and heat emitted by a hot body and the sensible heat which the body loses. Radiant light and heat may be termed *radiant energy*.

Without pretending to enter here into a philosophical discussion it is instructive to notice that all of these questions which were capable of being answered in two ways were answered wrongly at first.

Although this procedure of the human mind has delayed the correct solution of a very important series of questions, yet we in the present age cannot reasonably complain of what has taken place. It has given us a confidence in our present views that we could hardly have had if the question between two alternative views had not been threshed out in the past.

We can thus look to the future without dismay, and need not fear the gradual rising into strength of a school which shall call in question any of the very important conclusions at which we have now arrived.

Surely there is an advantage in being wrong first and right afterwards, especially when it was a past generation who went wrong and we ourselves who are right!

BALFOUR STEWART

(*To be continued.*)

NOTES

WE understand that Prof. Huxley, P.R.S., has agreed, at the request of the Lords of the Committee of Council on Education, to continue to act as Dean of the Normal School of Science and Royal School of Mines at South Kensington, and also to be responsible for the general direction of the biological instruction therein.

THE Senatus of the University of Edinburgh resolved at its last meeting that a lectureship of comparative embryology be instituted, and appointed Mr. George Brook, F.L.S., as lecturer, subject to the approval of the University Court. Mr. Brook has for some time been engaged in making investigations for the Fishery Board for Scotland.

THE Indian *Civil and Military Gazette* writing of the ornithological collection presented by Mr. Allan Hume of the Civil Service of India to the British Museum, says that its value and extent are only now beginning to be realised. It amounts to 62,000 skins of all kinds, and it has cost Mr. Bowdler Sharpe, of the Natural History Department of the British Museum, more than three weeks of uninterrupted labour to pack and send it away. Even now the work is not at an end, for the collection of eggs, which is no insignificant one, remains to be despatched. The gift, which represents the labour and learning of a lifetime is described by Mr. Sharpe as "the grandest collection of birds ever made."

FROM the Seventh Annual Report of Examinations in Technology, under the direction of the City and Guilds of London Institute for the Advancement of Technical Education, we notice that there is again a fair increase in the number of candidates who presented themselves, and a satisfactory proportional increase in the number of those who have passed. In 1884, 3,635 candidates were examined, of whom 1,829 passed. In 1885, 3,968 candidates were examined, of whom 2,168 have passed. Thus the increase of passes is six more than the total increase in the number of candidates. There is a slight falling off in the number of subjects in which the examinations have been held, owing to the fact that in four of the subjects, viz. :—Salt Manufacture, Oils and Fats, Silk Manufacture, and Mechanical Preparation of Ores, the number of candidates was below the minimum for which an examination is held. Applications for examination were received, however, in 46 out of the 47 subjects included in the programme. From the returns furnished in November last, it appears that 6,396 persons were receiving instruction in the registered classes of the Institute, as compared with 5,874 in the previous year. These numbers do not include the students in attendance at the technical classes of various schools and colleges at which the Professors do not accept payment on results. Two new subjects were this year added to the list, viz. :—Boot and Shoe Manufacture and Framework Knitting, in which subjects 69 candidates and 40 candidates respectively presented themselves. Nearly all these candidates received instruction in the recently-opened Technical School at Leicester. The percentage of failures on the results of the examinations in all subjects has decreased from 49·7 in 1884 to 45·3 in 1885. The proportion of failures is still large, showing the necessity of better instruction on the part of the teachers, and of more careful and sustained work on the part of the students. Of the inability of the majority of the candidates to make intelligible sketches, the examiners continue to complain; but it is hoped that this defect in the education of artizans will gradually be remedied as linear drawing comes to be more generally taught in our public elementary schools. During the past session, 263 classes have been held in different parts of the kingdom in connection with the Institute's examinations. Of the 6,396 students in attendance at these classes, 3,271 presented themselves for examination, and that of these 1,670 succeeded in satisfying the examiners. Last year, the number of candidates who passed from the registered classes of the Institute was 1,387, showing an increase of 283, which is a large proportion of the total increase, viz., 333 of successful candidates. This year, for the first time, Manchester heads the list of provincial centres from which the largest number of candidates have passed, the number being 147 as against 115 last year. A like number of candidates have passed from the Polytechnic Institution, London. Next in order of merit comes Glasgow, with 119 as against 139 last year, Bradford with 97 as against 90, Leeds with 84 as against 70 (55 from the Yorkshire College), Bolton with 75 as against 98, and Huddersfield with 72 as against 39. It is expected that about 750 of this year's successful candidates will gain a full Technological Certificate, in virtue of their having obtained from the Science and Art Department the necessary qualifying certificates in Science, in addition to their certificate in Technology. Of the 1,829 candidates who passed last year, 566 obtained the full certificate. This increase of 184 in the number of full certificates is a very satisfactory feature in this year's examinations. Compared with the total number of successful candidates, the percentage of those to whom full certificates will be awarded has increased from 31·2 to 34·5. From year to year, improvements suggest themselves in the working of these examinations, by which they are rendered more practical, and at the same time better adapted to the requirements of the students. The opening of the Central Institution, by affording new facilities for the training of technical teachers, will, it is hoped, do much towards improving the character of the instruction in the Institute's classes in connection with these examinations. Summer Courses for teachers, to be continued in subsequent years, have this year been held for the first time at the Central Institution, and the applications for admission to these courses show that the value of the instruction is likely to be fully appreciated by those for whom it is intended.

THE death is announced, at the age of fifty-five years, of Mr. Robert F. Fairlie, the well-known engineer; and also of Dr. Heinrich Wilhelm Reichardt, Professor of Botany in the University of Vienna.

AT the annual speech day at Reading School, on Tuesday, July 28, a new laboratory was opened by Dr. J. H. Gladstone, F.R.S. The Town Clerk (Mr. H. Day) read a statement to the effect that natural science had been taught in the school since the year 1872, but up to 1884 no adequate class-rooms had been fitted up or set apart for that purpose, except in a temporary way. Last year the Head Master submitted a scheme to the Trustees, and after the subject had been fairly thought over, three gentlemen, Messrs. G. W. Palmer, Alfred Palmer, and Walter Palmer, sons of the Member for Reading, volunteered to provide the accommodation recommended by Dr. Walker. The trustees gladly availed themselves of so generous an offer, and the result was that the school now possessed in that room—fitted up for chemical analysis, and in the adjacent lecture-room—excellent means of giving instruction in the usual branches of natural science. Dr. Gladstone then declared the laboratory open. Having praised its general arrangements, he congratulated the school on having obtained so magnificent a gift from the Messrs. Palmer, who were thus endeavouring to place chemistry upon an equal footing with the other studies carried on at that school. He would not go into the great controversy between things and words, but they would all agree that it was necessary that the knowledge of things should precede the knowledge of words, because the knowledge of words was only a kind of simulacrum unless the knowledge of things preceded it. A knowledge of chemistry was pre-eminently an experimental science, and they wanted that kind of training for all boys. Different studies gave a different training to the mind, and chemistry gave a training not only to the perceptive faculties, but also to the reasoning processes, and therefore chemistry had been wisely chosen to take an important part in the curriculum of that school.

THE foundation stone of the new buildings of the Sorbonne, which are to cost 22 millions, was laid on Monday by M. Goblet, French Minister of Education. The cellars and ground floor have already been built.

THE protracted season of midsummer heat throughout the United States has been broken, the *Times* correspondent states, by a series of drenching rains, accompanied by cyclones. A severe easterly storm began on Sunday, continuing throughout Monday, the wind changing to westward, and rains deluging the entire country east of the Mississippi. The heaviest rainfall, which was at Chicago, reached 5½ inches in the twelve hours ending Sunday at midnight. A universal report from all parts of the country tells of the vast damage done by the floods and cyclones. The rainfall on Monday evening at Philadelphia was nearly 3 inches. The cyclone started in Maryland about two o'clock on Monday afternoon, passing northward along the eastern border of Philadelphia at three o'clock. It wrecked houses and mills and destroyed cattle and crops in Maryland and Delaware, doing the severest injury along the Delaware river front of Philadelphia. Passing from south to north, a low, black, revolving ball of smoke moved at the rate of nearly a mile in a minute, crossing twice over the Delaware River, which is crescent-shaped. Five lives have been lost, six persons are missing, and about 100 injured. The damage done is estimated at half a million of dollars. Six hundred buildings were unroofed and the walls partly destroyed, railway cars blown from their tracks, trees uprooted, and several vessels wrecked. Two steamboats on the river had their upper works lifted off and destroyed, the pilot of one being drowned, while from the deck of another horses and a waggon were lifted by the wind and dropped into the river.

THE Government Astronomer of Hong Kong has published a notice with regard to typhoons, from which it appears that the earliest signs of these phenomena in the China seas are clouds of the cirrus type looking like fine hair, feathers, or small white tufts of wool travelling from east to north, a slight rise in the barometer, clear and dry but hot weather, and light winds. These are followed by a falling barometer, while the temperature rises still further. The air becomes oppressive from increasing dampness, and the sky presents a vaporous and threatening appearance. A swell in the sea, and also phosphorescence of the water, as well as glorious sunsets, are other signs useful to the mariner who is acquainted with the usual conditions in the locality. When the typhoon is approaching the sky becomes overcast, the temperature in consequence decreases, the dampness increases, and the barometer falls more rapidly, while the wind increases in force. Nearer the centre the wind blows so that no canvas can withstand it, and the rain pours down in torrents, but there is no thunder and lightning. Still nearer the centre there is less wind and rain, and the sky is partly clear, but the sea is tremendous. This is therefore the most dangerous position. Typhoons may be encountered in any season of the year, but are most frequent in August and September. They appear to originate south-east of the Philippine Islands. In August and September they frequently pass east of Formosa, or travel towards north-west up through the Formosa Channel, or strike the coast of China. Afterwards they usually recurve towards north-east and pass over Japan or across the sea north of Japan, but not with the violence that is characteristic of tropical storms. During the remainder of the year they most frequently cross the China Sea from east to west.

A TELEGRAM from St. Petersburg, dated August 3, states that despatches from Tashkend and Verny announce that there has been a severe earthquake at Pishpek (? Bish-uzek), damaging all the houses at that place. The shock extended to the settlements of Sukuluk and Belovodsk, which were laid in ruins. At Belovodsk a church fell in, many of the congregation assembled in it at the time being killed. Numerous fissures appeared in the ground. A later telegram from Verny reports that altogether fifty-four people were killed and sixty-four injured by the earthquake at Belovodsk and Karabolty. The shocks continue and the people are terror-stricken.

A TELEGRAM from Malaga states that a shock of earthquake occurred at Motril on the afternoon of July 30.

THE *Times* states that much uneasiness is being caused by the continued absence of tidings as to Mr. F. A. Gower, who lately carried on a series of experiments with a view to testing the adaptability of balloons to war purposes. Mr. Gower, who is well known to the scientific world as a joint patentee of the famous Gower-Bell telephone, had made Hythe the centre of his operations, and thence made several ascents. His final undertaking in this country was a successful aërial voyage across the Channel early in June. He continued his trial trips in France, and met with a misadventure while awaiting an opportunity of returning in a balloon to England. Undeterred by this, he made an ascent on July 18 from Cherbourg, and since that date nothing definite is known of his whereabouts. A pilot balloon which he had previously despatched has been found and sent on to Hythe; and a balloon has been picked up without a car some thirty miles off Dieppe. Sixteen days having now elapsed since the ascent and no message having been received from Mr. Gower, whose invariable practice it was at once to notify by wire his safety at either Cherbourg or Hythe, at both of which places he has left property, the gravest fears are entertained that he has been drowned. It may be mentioned that the experiments being carried on by Mr. Gower were within the cognisance of the Government, and have so far, it is believed, proved of a very satisfactory character.

ACCORDING to *Science* the daily papers announce that the U.S. commissioner of agriculture has established as a part of Riley's division a branch of investigation relating to economic ornithology, and has appointed Dr. C. Hart Merriam, a well-known ornithologist and secretary of the American Ornithologists' Union, a special agent to take charge of this part of the work. Dr. Merriam will make his headquarters at Sing Sing, N.Y., until Oct. 1, and after that at Washington. The scope of the investigation will cover the entire field of inter-relation of birds and agriculture, particularly from the entomologist's standpoint. The inquiry will relate primarily to the food and habits of birds, but will include also the collection of data bearing on the migration and geographical distribution of North American species. In this last inquiry the department hopes to have the co-operation of the Ornithologists' Union, Dr. Merriam being at the head of the Union's committee on migration.

DR. ELKIN, in charge of the heliometer of the Yale College observatory, has, *Science* says, been engaged for nearly a year and a half past in measuring the group of the Pleiades, his original plan being to measure with this instrument the same stars which Bessel measured with the Königsberg heliometer about fifty years ago. Dr. Elkin has taken advantage of all the improvements in the instrument and the methods of using it which have been developed in the last half-century; and, in addition to the successful carrying-out of his carefully elaborated plan of triangulation, he has also been able to extend his work to a large number of stars which Bessel did not measure. The position-angle and distance of the Bessel stars from the large star Alcyone are included in the work. The results of this very valuable work cannot be fully discussed and prepared for publication until the positions of certain stars of reference have been obtained from the work of other observatories where they are now being determined. Dr. Elkin has also obtained measures of the distances of a number of craters on the moon from neighbouring stars on thirty-six nights, near the times of first and last quarter. The positions of these craters on the moon itself had been determined; also series of measures made of the diameters of Venus, of the outer ring of Saturn, and of the satellite Titan referred to its primary. A registering micrometer has been devised, and, in the form constructed by the Repsolds, has proved a complete success, greatly increasing the amount of work which the observer can accomplish. Dr. Elkin proposes to devote the heliometer for a year and a half to come to investigations in stellar parallax. The plan of research mapped out and already commenced will, it is hoped, if carried to completion, furnish a reliable value of the relative parallax of stars of the first and eighth magnitude.

PROF. A. LANDMARK, chief director of the Norwegian Fisheries, has published some interesting particulars of his studies of the capability of salmon to jump waterfalls. He is of opinion that the jump depends as much on the height of the fall as on the currents below it. If there be a deep pool right under the fall, where the water is comparatively quiet, a salmon may jump 16 feet perpendicularly; but such jumps are rare, and he can only state with certainty that it has taken place at the Hellefos, in the Drams River, at Haugsend, where two great masts have been placed across the river for the study of the habits of the salmon, so that exact measurements may be effected. The height of the water in the river of course varies, but it is as a rule, when the salmon is running up stream, 16 feet below these masts. The distance between the two is 3½ feet, and the Professor states that he has seen salmon jump from the river below across *both* masts. As another example of high jumping, he mentions some instances of Carratunk waterfall, in Reumbec, in North America, where jumps of 12 feet have been recorded. Prof. Landmark further states that when a salmon jumps a fall nearly perpendicular in shape it is sometimes able to remain in the fall, even if the jump is a foot or two short of the actual height. This, he maintains, has been proved by an overwhelming quantity of evidence. The fish may then be seen to stand for a minute or two a foot or so below the edge of the fall in the same spot, in a trembling motion, when with a smart twitch of the tail the rest of the fall is cleared. But only fish which strike the fall straight with the snout are able to remain in the falling mass of water; if it is struck obliquely, the fish is carried back into the stream below. This Prof. Landmark believes to be the explanation of salmon passing falls with a clear descent of 16 feet. The professor believes that this is the extreme jump a salmon is capable of, and points out that, of course, not all are capable of performing this feat.

IN the new part of the *Transactions* of the Essex Field Club (vol. iv. part 1) the first and perhaps most interesting paper is Prof. Boulger's presidential address on the "Influence of Man upon the Flora of Essex."

ACCORDING to the *Chinese Recorder*, Dr. Wallace Taylor, a missionary doctor of Osaka, Japan, has made important discoveries regarding the origin of the disease *kakké*, or *beriberi*, as it is known in Ceylon. He traces it to a microscopic spore, which is often found largely developed in rice, and which he has finally detected in the earth of certain alluvial and damp localities.

WE have received from Denver the first volume of the *Proceedings* of the Colorado Scientific Society. Denver as a western mining camp, with an evil reputation, and Denver the capital of the State of Colorado, are places separated by ages of civilisation; but mining is prominent in both. The members of the Scientific Society appear from the list to be mainly civil or mining engineers, metallurgists, geologists, assayers, &c., and the papers are largely on these subjects, *e.g.* the estimation of arsenic, and of copper; the ore deposits of the Summit districts of Rio Grande county, Colorado (the principal paper in the volume), löllingite, &c. There are, however, other papers: there is the report by a commission of the society on the Artesian wells of Denver, a paper on extinct glaciers of the San Juan mountains, while one of the members, Mr. van Diest, read several papers on subjects connected with the Malay Archipelago, such as the formation of hills by mineral springs in the Island of Java, the geology of the Sumatra, and the method of mining there 250 years ago, the methods of smelting employed by the Chinese at Banka, &c. There is certainly plenty of vitality in the new society, and doubtless it will grow with the growth and strengthen with the strength of the magnificent State from which it takes its name.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus*) from India, presented by Mr. J. S. Stevens; two Turtle Doves (*Turtur communis*), European, presented by Mr. J. Hare; four Martinican Doves (*Zenaida martinicana*), a Moustache Ground Dove (*Geotrygon mystacea*), four Dominican Kestrels (*Tinnunculus dominicensis*), a Green Bittern (*Butorides virescens*) from the West Indies, presented by Dr. A. Boon, M.R.C.S.; a Golden Eagle (*Aquila chrysaetas*) from Perthshire, presented by Mr. Chas. J. Wertheimer; two Larger Hill Mynahs (*Gracul. intermedia*) from India, presented by Mr. Thomas Hudson; an Indian Python (*Python molurus*) from India, presented by Mr. Harrington Laing; four Proteus (*Proteus anguinus*), European, presented by Mr. Cook; a Red-headed Cardinal (*Paroaria larvata*), a Yellow Hangnest (*Cassicus persicus*) from South America, deposited; a Vulpine Phalanger (*Phalangista vulpina*), two Snow Birds (*Junco hyemalis*), a Northern Mocking-bird (*Mimus polyglottus*), bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK, 1885, *AUGUST* 9–15

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 9

Sun rises, 4h. 38m.; souths, 12h. 5m. 13·6s.; sets, 19h. 32m.; decl. on meridian, 15° 45′ N.: Sidereal Time at Sunset, 16h. 46m.

Moon (New on August 10) rises, 3h. 19m.; souths, 11h. 1m.; sets, 18h. 34m.; decl. on meridian, 15° 37′ N.

Planet	Rises h. m.	Souths h. m.	Sets h. m.	Decl. on meridian ° ′
Mercury	7 20	13 45	20 10	4 13 N.
Venus	7 3	13 47	20 31	7 55 N.
Mars	0 52	9 12	17 32	23 48 N.
Jupiter	6 45	13 34	20 23	8 48 N.
Saturn	0 57	9 6	17 15	22 29 N.

August 9, 10, *and* 11.—Principal nights for observation of the August (Perseus) meteors.

August	h.	
12	2	Jupiter in conjunction with and 2° 30′ north of the Moon.
12	9	Mercury in conjunction with and 1° 55′ south of the Moon.
12	12	Venus in conjunction with and 2° 13′ north of the Moon.

DR. PERKIN ON THE COAL-TAR COLOURS [1]

Anthraquinone Series

I MUST now draw your attention to the important class of colouring matter compounds obtained from anthracene or anthraquinone.

Alizarin and the other colouring matters related to it form one of the most important branches of the coal-tar colouring industry, and is one of special interest, because alizarin was the first instance of the production of a natural colouring matter artificially. It will be quite unnecessary for me here to say much about the madder root, which was the original source of alizarin, and was grown in such enormous quantities, but now is nearly a thing of the past; nor will I enter into the early chemical history of alizarin, and all the laborious work which was bestowed upon it by Dr. Schunck and others. As you are probably all aware, the relationship of alizarin and its formation from the coal-tar hydrocarbon anthracene was the result of the labours of Graebe and Liebermann, the researches which culminated in this being of a purely scientific nature. The original process for obtaining it has, however, not been found of practical value, but a new one in which sulphuric acid could be used in place of bromine was afterwards discovered by Caro, Graebe, and Liebermann in Germany, and by myself in this country, apparently simultaneously. A second process was also discovered by me, which was worked nearly all the time I was engaged in this industry. In this dichloranthracene was used instead of anthraquinone, and the product thus obtained yielded colours of a brilliancy which it has been found, even to the present time, difficult to match by the anthraquinone process.

At the time of the discovery of artificial alizarin, anthracene

[1] The President's Address at the annual meeting of the Society of Chemical Industry (not the Institute of Chemistry as stated last week). Continued from p. 307.

was not prepared by the tar distillers, as it had no application, and very little was known about it. It was discovered in 1832 by Dumas and Laurent. In 1854-55, when studying under Dr. Hofmann, I worked with it for some time, but my results were never published, because, owing to the erroneous formula given for it by Dumas and Laurent, which was accepted, my results would not fit in; nevertheless the information obtained afterwards proved of great value to me, although at the time the labour spent appeared to be lost labour, showing the value of research even when not successful. The formula of this hydrocarbon was not established until 1862, when it was studied by Dr. Anderson. This was only six years before the discovery of Graebe and Liebermann, and, had not the formula of anthracene been established before these chemists commenced their work, the relationship of alizarin to it would not have been discovered, and up this day it is possible that this artificial alizarin industry would not have been in existence. Researches like that of Dr. Anderson I have often heard spoken of slightingly, because they don't bear much on their surface; but who knows what such work may lead to? Earnest workers cannot be too much encouraged.

As anthracene was not a commercial product, it was necessary to experiment on its production before alizarin could be manufactured, and not only on the best methods of getting it, but also to get a rough idea of how much could be produced, because unless the hydrocarbon could be obtained in large quantities, artificial alizarin could not compete with madder. In our works at Greenford Green we commenced by distilling pitch; but afterwards tar distillers were induced to try to separate it from the last runnings of their stills by cooling and then filtering off the crystalline products which separated out, and in fact visits were paid to most of the tar distillers of the United Kingdom, others being corresponded with on the subject, and the result was that in a short time such quantities came in that the distillation of pitch was abandoned. And although much doubt and anxiety prevailed at first as to the possibility of getting a sufficient supply of this raw material, at the present day there are about 1000 tons of commercial product (about 30 per cent.) produced in excess of the requirements, the annual production in the United Kingdom being estimated at about 6000 tons 30 per cent., or nearly 2000 tons pure anthracene.

Although the colouring matter obtained from anthraquinone or dechloranthracene was at first simply considered as alizarin more or less pure, yet on investigating the matter it was soon found that it contained other colouring matter. To this I drew attention in 1870 (*J. Chem. Soc.* xxiii. 143, footnote), and in 1872 gave the analysis of a product which I named anthrapurpurin, followed by a more extended account a year afterwards (*J. Chem Soc.* xxv. 659, and xxvi. 425). This was called anthrapurpurin; because it is an anthracene derivative having the formula of purpurin, with which it is isomeric. In the latter paper I also referred to another colouring matter dyeing alumina mordants of an orange colour (*J. Chem. Soc.* xxvi. 425). It was also shown that anthrafluoric acid when fused with alkali gave a colouring matter behaving with mordants in the same way (*J. Chem. Soc.* xxvi. 20), and which has proved to be the same body. This latter reaction was afterwards more fully studied by Schunck and Roemer, and the colouring matter produced by it was shown also to have the formula of purpurin; they therefore called it flavopurpurin (*Ber.* ix. 678), so that the colouring matters formed have proved to be three in number—alizarin, anthrapurpurin, and flavopurpurin, all of which are valuable dyes, whereas in madder root there is only alizarin and purpurin, the latter being of only secondary value. This can now also be produced from anthracene. The researches which have been reached on the subject of the conditions under which these different colouring matters are formed, have led to the discovery of methods for their separate production, so that in artificial alizarine, which name commercially embraces all these colouring matters, both mixed and separate, we have more than a simple replacer of madder root, and as these colouring matters just referred to can be applied with the same mordants, varieties of styles of work can be produced by the calico printer and dyer which before were unknown. Anthrapurpurin is, I believe, of as great importance as alizarin itself, and used with it increases its brilliancy, and alone gives very brilliant scarlet shades.

Artificial alizarin was first produced commercially in this country by my firm at Greenford Green in 1869; when one ton was produced in 1870, forty tons were made in 1871, 227 tons, and so on increasingly. It was not produced on the Continent until 1871, when, according to Graebe and Liebermann, 125-150 tons were made. These weights do not apply to dry colour, but to paste.

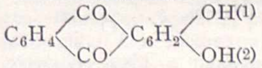
I cannot go into any lengthened account of the chemistry of this industry here; its development, however, has kept pace with theoretical investigations, in some cases it may be said to have forestalled it. For example, in the old methods of working, more anthrapurpurin than alizarin was produced; the conditions required to modify this were found out by experiment. According to all our previous knowledge as to the introduction of hydroxyl into a body by the fusion of its sulphonic acid with alkali, a monosulphonic acid should give a monohydroxyl compound, and a disulphonic acid a dihydroxyl compound. Therefore to produce alizarin, which is a dihydroxyl compound, an anthraquinone disulphonic acid was thought to be the proper thing to use. By experience this was gradually found to be incorrect, a monosulphonic acid being required to produce alizarin, a disulphonic giving anthra or flavopurpurin, the colouring matter not being due to the primary but to a secondary reaction as was afterwards shown by research—the mono and dioxyanthraquinones (the latter known as anthraflavic and isoanthraflavic acids) being the first products of the reaction, and then undergoing oxidation by the caustic alkali employed, and then yielding the corresponding colouring matter, a portion of the products, however, being at the same reduced back to anthraquinone.

A very important improvement preventing this loss by reduction was discovered by J. J. Koch, who found it might be avoided by the use of a small quantity of potassium chlorate with the alkali used in the fusion.

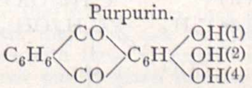
The amount of caustic soda used in this industry is very large at the Badische Aniline and Soda Fabrik—and, I believe, elsewhere—it is made on the spot; and I must say the cleanly way in which alkali is made in the above works contrasts very favourably with what I have seen in some of the alkali works in this country.

Like rosaniline, alizarin has now become a material for preparing other colouring matters. Of these there are two in use, viz., nitroalizarin, which gives orange-yellow shades with alumina mordants, and alizarin blue, a remarkable compound prepared from nitroalizarin by treating it with sulphuric acid and glycerine. This gives shades of colour like indigo. When first discovered, considerable difficulty was found in its application, on account of its insolubility; it has since been found to form a soluble compound with sodium bisulphite, and by this means its application has become much easier. The constitution of the colouring matter derived from anthracene may be represented as follows:—

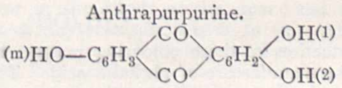
Alizarin.



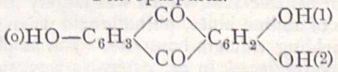
Purpurin.



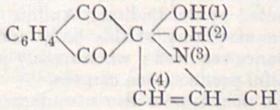
Anthrapurpurine.



Flavopurpurin.



Alizarin Blue.

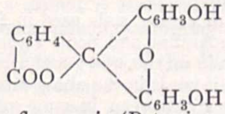


These colouring matters under the name of artificial alizarin are the most important of the coal-tar colours, their money value amounting to more than a third of the entire value of all the colours produced in the industry, and at present the price of artificial alizarin compound tinctorially is not more than one-fourth of that which madder or garancium ordinarily were before their production. There are now three works producing it in this country, but the bulk of that used still comes from Germany.

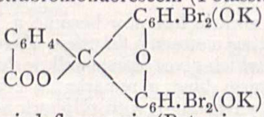
Phthalines.—The discovery of this class of bodies dates back to 1871, and was the result of the investigation of Baeyer. He found that phenols unite with a number of polybasic acids and with aldehydes, with separation of water when the mixture is heated alone, or with glycerol and sulphuric acid, the compounds formed not being ethers. Those produced when phthaiic anhydride is employed and which embrace those of practical value, are called phthalines. The first of these discovered by Baeyer was gallein (*Ber.* iv. 457), produced by heating pyrogallol with phthalic anhydride; its formula is $C_{20}H_{14}O_5$; by reduction it loses the elements of water and with hydrogen forming *cerulein.* These colouring matters, which for a long time remained unnoticed, are now being extensively used.

Later, in 1871, Baeyer discovered resophthalin, or fluorescein (*Ber.* iv. 555). This substance, which is remarkable for its yellowish green, fluorescence, dyes silk and wool yellow, but does not combine with mordants, and is not a very useful dyeing agent. But it was discovered by Caro in 1874, the subject being afterwards worked out jointly with Baeyer, that fluorescein when brominated yielded that beautiful dyestuff now called *eosine;* this was introduced into the market in July, 1874. Other substitution products were then studied, and the iodine product was found to give bluer shades of red than the bromine. One of the most beautiful colours of this series is the dichlortetraiodofluorescein, in which dichlorphthalic anhydride is used in its preparation. It is called phloxine. The methylic ether of eosine and its nitro derivative also have become commercial articles. These bodies are now manufactured in a practically firm condition. Their structure has been made out by research to be as follows:—

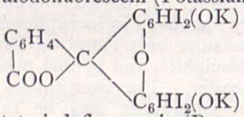
Fluorescein



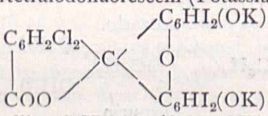
Eosinetetrabromofluorescein (Potassium Salt).



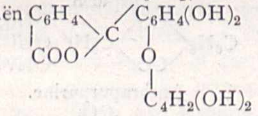
Tetraiodofluorescein (Potassium Salt).



Dichlortetraiodofluorescein (Potassium Salt).



Galliën



The introduction of these colouring matters had a great influence on the manufacture of phthalic acid. This acid, it will be remembered, was proposed a good many years since for the production of benzoic acid, which was largely in demand for the manufacture of aniline blues, phthalic acid when carefully treated with lime yielding calcium benzoate. But as phthalic acid was required to be produced in an extensive way, new experiments had to be made on the subject. The difficulties connected with this were surmounted by the Badische Aniline Fabrik, who are now the chief manufacturers of this body and its anhydride, which is the substance required; when freshly prepared it is one of the most beautiful products one can see.

Dichlorophthalic acid is now also manufactured for the preparation of the bluish shades of fluorescein derivatives already referred to. But this is not all; it was not only necessary to produce this anhydride in quantity, but it was necessary also to produce *resorcenol.* This substance was originally prepared from galbanum by fusing it with potash, or by distilling brazilin, &c., both technically impractical processes. It was afterwards produced by fusing various halogen derivatives of phenol and benzene sulphonic acid with alkali; these also were not practical processes. It was, however, eventually found that it could be produced by fusing metabenzenedisulphonic acid with potash, the original observation being made by Carrick; and by this process this product, which was a rare compound, is now manufactured and has become a common one, being produced in very large quantities.

Indigo Series.—Indigo is too well known a substance for me to make any remarks in reference to its history as a colouring matter, and with reference to the chemical side of the question I suppose few substances have had more work bestowed upon them than this body, so that I must confine any few remarks to its artificial formation. There is one point of interest, however, connected with indigo, and that is that it was the original source of aniline, this base being discovered in the products of its destructive distillation by Unverdorben, in 1826, as already referred to.

Notwithstanding the large amount of work which has been bestowed upon this colouring matter, its constitution has only been lately arrived at, and for this, and the methods of its artificial formation, we are indebted to the beautiful and laborious researches of Baeyer. The first process for its artificial production was patented by Baeyer in March, 1880. The process consists in preparing orthonitropropiolic acid and acting upon it in presence of an alkali, with a reducing agent, such as grape sugar, xanthate of sodium, &c.

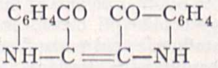
$$2[C_9H_5(NO_2)O_2] + H_4 = 2CO_2 + C_{16}H_{10}N_2O_2 + 2H_2O.$$

This process renders the application of artificial alizarin very easy to calico printing, because the products can be applied to the fabric and the reaction then completed, and thus the indigo is formed and fixed in the fibre, and this process is in use in some of the printworks of Mulhouse, where there is a continued though small demand for nitroorthopropiolic acid. Other processes have been discovered by Baeyer for the formation of Indigo; he has found that it can easily be formed from orthonitrobenzaldehyde by condensation with bodies containing CH_3CO group, such as acetone.

Hitherto this artificial formation of indigo has not met with much practical success. This does not arise from difficulties in its manufacture, but in its cost compared with natural indigo, which is a very cheap dyestuff.

So far as it has been manufactured, however, the possibility of this has been entirely dependant upon scientific research disconnected with its study. To prepare nitropropiolic acid it is necessary to begin with cinnamic acid as a raw material. This acid, until 1877, was only obtained from certain balsams, and was a very costly material. It was then discovered that it could be produced with comparative ease by the action of acetic anhydride and an acetate on benzaldehyde (*Journ. Chem. Soc.* xxxi. 428). Caro afterwards found that this process might be simplified by heating a mixture of benzylidene dichloride with sodium acetate, and it is by this process that it is now prepared.

The constitution of indigo Baeyer represents as follows:—



Several derivatives have been made which are interesting dyes, such as methyl indigo, tetrachlor indigo, etc.

Azo compounds.—The commencement of the history of the azo colours in an industrial sense has little to do with the theoretical side of the question, the early products being the offspring of empirical observations, and in no way connected with the theory of the diazo compounds, a condition of things very different from that now existing. Time will not allow me to enter into the beautiful work of Griess, much of which will be found in the *Philosophical Transactions* for 1864.

The first definite compound of this class, shown to possess dyeing powers, was a substance discovered by Prof. Church and myself, known first as nitrosonaphthalene, then as azodinaphthyldiamine, but now called amidoazonaphthalene. This substance, however, was of no practical value, because its salts, which are violet, cannot exist except in the presence of a certain amount of free acid. This substance has since been found of value in the preparation of the Magdala red.

The first substance of this class sent into the market was the phenylic analogen of amidoazonaphthalene by amidoazobenzene, which was discovered by Mène. It was introduced by Nicholson, who prepared it by a process which has not been published. It was afterwards patented by Dale and Caro, in 1863. This was a yellow dye, but did not demand success, because of its volatility. It has, however, since become useful for the manufacture of induline.

The first really successfull azo colour was Manchester and Bismark brown (triamidoazobenzene), which is produced by the action of nitrous acid on metadiaminobenzene.

The next important step took place in 1876, by the discovery of chrysoidine, by Caro and Witt. Independently this product is prepared by the action of diazobenzene on metadiamidobenzene.

About this time the subject began to be worked out on a scientific basis, and since then the number of diazo dyes produced is marvellous, and it will be useless for me to do more than to refer to one or two of the most important. About this period also the value of the sulpho group, began to be realised, and this has greatly added to the value of these dyes.

The first use of the sulpho group in relation to azo colours was in connection with amidoazonaphthalene, patented by myself in 1863.

During the early history of coal-tar colours, innumerable experiments were made with naphthalene derivatives to produce colouring matters, but no results of any value were obtained; the experiments were mostly made with naphthalamine. The first colouring matter that was obtained from it that was of value was Martin's yellow, a dinitronaphthol. After this came the Magdala red, which was not much used. The principal development of the coal-tar colours of late years has, however, been in connection with diazo reaction. In these reactions naphthol is much used, and this product, which a few years ago was unknown, is now manufactured by tons by fixing the naphthalene sulphonic acid with alkali, and is produced at a few pence per pound. Most of the azo colours produced from benzene derivatives are of a yellow or brown colour, but, by taking products of a higher molecular weight, colours of different shades of red are produced. The one which has commanded the greatest success is the scarlet, first known as Meister scarlet, produced by the acting of diazoxylene chloride on the disulphonic acid of β-naphthol; its constitution may be represented thus:—

$$C_6H_3(CH_3)_2-N_2-C_{10}H_4(OH)(HSO_3)_2.$$

And in the formation of bluer shades, diazocumene chloride is used. The cumedene used is now made from xylidene, by the beautiful reaction of Hofmann's, in which an alcohol radical associated with the nitrogen becomes that element, and enters with the hydrocarbon radical. These scarlets have had a very injurious influence on the cochineal market, and have in many cases displaced it.

If α-diazonaphthalene chloride be used instead of the xylene or cumene compounds, the colour known as Bordeaux is produced. Then, again, where derivatives of α-naphthol are used, different results are also obtained, so that great varieties of products can be produced. The preparation of these azo colours is a matter of much simplicity, and colouring matter being precipitated in bringing the products together, and, moreover, they can be produced in many theoretical quantities; hence they are remarkably cheap dyeing agents. The following are the formulæ of some of these azo dyers:—

Manchester Brown, Vesuvius or Bismarck Brown.—

$$NH_2C_6H_4-N_2C_6H_3(\overset{(m)}{N}H_2)_2HCl.$$

$$\text{Chrysoidine } C_6H_5.N_2.C_6\overset{(m)}{H_3}(NH_2)_2HCl.$$

$$\text{Fast yellow } KSO_3.C_6H_4.N_2.C_6H_4NH_2.$$

$$\text{Metaucher yellow } Na\overset{(3)}{SO_3}.N\overset{(1)}{H_4}N_2.C_6H_4NHC_6H_5.$$

$$\text{Orange } Na\overset{(4)}{SO_3}.C_6H_4.\overset{(1)}{N_2}.C_{10}H_6\overset{B}{(OH)}.$$

$$\text{Fast red } NaSO_3.C_{10}H_6.N_2.C_{10}H_6\overset{(B)}{(OH)}.$$

$$\text{Ponceau G. } C_6H_5.N_2.C_{10}H_4OH\overset{(B)}{(NaSO_3)_2}.$$

$$\text{Ponceau 2R } (CH_3)_2C_6H_5.N_2.C_{10}H_4OH\overset{(B)}{(NaSO_3)_2}.$$

$$\text{Bordeaux } C_{10}H_7.\alpha\text{-}N_2.C_{10}H_4OH\overset{(B)}{(NaSO_3)_2}.$$

From which it will be seen that the colour changes from yellow to red and claret by the increase of the molecular weight of the radicals introduced, and also by the relative position occupied by the group, &c.

Products of the quinoline series have of late been claiming attention in relation to colouring matters. It will perhaps be remembered that, in the early days of the coal-tar colour industry, a beautiful blue colour belonging to this series, discovered by Greville Williams (*Chem. News*, Oct. 11, 1860, 219), was introduced. This substance was called cyanine. The employment as a dye for silk at first produced quite a sensation, on account of the beauty of the colour; but unfortunately it was too fugitive to be of any practical value. Recent researches have shown that chryaniline is also to be regarded as a body of the chinoline class. Alizarin blue, and also the beautiful yellow dye obtained from acetonilide by Fischer, and known as flavanilin, are found also to belong to this class of substance.

Other colouring matters which have long been prepared from quinoline direct might be referred to did time permit. The peculiar green which is produced by the condensation of tetramethyldiphenylketone with quinoline is of interest, because the introduction of this quinoline has a very different influence on the resulting colouring matter to that of other groups containing amidogen—in fact, it appears to act more like the phenyl, as the green is very analogous to benzaldehyde green.

There is a very interesting new manufacture growing out of the coal-tar colour industry, and that is, the preparation of derivatives of quinoline as substitutes for quinine. I have mentioned that much work has of late been directed to the study of quinine itself, and although the artificial formation of this substance has not yet been discovered, new bodies have been obtained during these investigations which are thought to possess valuable medical properties. This is rather a remarkable development from this industry, seeing that it was owing to experiments made on the artificial formation of quinine that it owes its foundation.

There is another peculiar colouring matter I have not yet referred to, peculiar—as it contains sulphur. I refer to methyline blue, a very valuable dye, the constitution of which has been so well worked out by Bernthsen, but I feel I must be content with this slight reference to it.

As I have shown, the coal-tar colour industry originated in this country, where for some time it was solely carried on. The second impulse was from France in the discovery of magenta and its blue and purple phenylic derivatives, which were soon brought to a state of great purity in this country. The Hofmann violets were then discovered and produced also in this country, several other colours being perfected and largely used. By this time the manufacture of coal-tar colouring matter had made some progress in Germany and Switzerland; crude products in a cheap form were first made, but improvements soon followed.

The subject of these colouring matters was taken up with great earnestness in the German laboratories, so much so that it was stated at one time that this industry was acting injuriously to science, as it had diverted an undue amount of attention from other subjects. Time has, however, proved the groundlessness of this statement. This laboratory work, as well as research work generally, fitted a number of highly-trained chemists to enter the works, when they soon improved the processes, and thus they were able to produce products of a quality to compete with those of English manufacture, which had, owing to their purity, given superior and more reliable shades of colour in the hands of the dyer; and the result of the application of this scientific labour to this industry is that Germany produces products of the highest class and at the lowest price. The fact that Germany is now the head-quarters of this industry, raises the important question, Why has England allowed this state of things to come about? All the raw materials are produced in this country, both the products from coal and the other chemicals required, and, as we have seen, the industry originated and was first carried on here, and, in addition, we are the greatest consumers of the colouring matters. This fact is well worth considering, and it is many-sided. In my opinion, the Patent Laws, and the difficulty of preventing infringements from abroad, was one cause which may have prevented this country from maintaining its first position.

When speaking of the early history of the first coal-tar colour, mauvein, I referred to this class of infringement and how it was first met by the proceedings taken against the agents employed in this country, and this course was so far successful, but only pointed out how easily the law could be evaded if foreign manufacturers gave up responsible agents and sold direct to the consumers. Having no duties on such articles, no assistance could be obtained at the Customs, and the colouring matters were generally declared under the name of vegetable dyes or extracts, so that it was impossible to stop them entering the country, and even when found, owing to the onus of proof of their being manufactured by the patentee's process laying with the patentee, an almost insurmountable difficulty was raised, as in most cases no traces of the products used in the preparation were left in the colouring matter. The only other proceedings which could be instituted were against the consumer; here again the difficulties were practically insuperable.

In most cases the consumers were using the patentee's product to some extent, and it was impossible to know to what extent, in fact, without going into the many details connected with this point, it may be assumed that in most cases proceeding against a consumer of this kind of article is detrimental and practically useless.

The result of this infringement, by importation from abroad, is that a patentee had to compete against all other manufacturers with the exception of *his own countrymen.*

There can be but little doubt that this state of things has had much to do with preventing the development of this industry, and crippling enterprise in this country, as it prevented manufacturers even from working under royalties, there being no security whatever except in name. Again, the fact that a foreigner could take a patent in this country, manufacture in his own country, and send the product here, was a great source of loss and mischief to our trade. The new patent laws may probably alter this, but still the difficulty of importation in defiance of patent right still remains.

There is another matter which tells much against this country—namely, that we are not able to export colour to foreign countries upon the same conditions as foreign manufacturers can into this, because we are met with import duties which handicap us to a prohibitive extent, whereas the foreign manufacturer, being protected in his own country, may maintain his prices there and sell at a lower price in this country; but what is still more injurious, he may dispose of surplus production in this country at or even below cost price. The injurious effect of such a course upon our market can be easily understood by business men, and I need not go into it here. These are matters our manufacturers have to contend with, and cannot help themselves; there is, however, one matter in which they are undoubtedly at fault.

We find that in Germany the manufacturer understands the value of well-trained chemists, and sympathises with them; they also realise the value of theoretical chemistry—this is a condition of things we don't find in this country.

Unless I am mistaken, the coal-tar colour industry has acted as the great stimulus to the development of general chemical industries of Germany, and these, by starting with so much scientific aid as they have called to their assistance, have made an amount of progress during the last twenty-five years which is most remarkable. Up to that time England had been the seat of most of the large chemical industries, and the success which we have had appears to me to have produced a feeling of false security, and more attention has been paid by the heads of firms to the markets than to the chemistry of their manufactures.

I believe that thirty years ago there were very few chemists employed in chemical works, either in this country or on the Continent. Now there are very few without them; but in this country they are far less numerous and much less efficient than in Germany, and for this our manufacturers are to a great extent responsible. I am told that at some of our large chemical centres, the chemists, or so-called chemists, are sometimes paid not more than could be earned by a bricklayer. If such openings are put by manufacturers before young men, their parents are not likely to give them an expensive scientific training. If they get any they are not likely to continue it longer than enough to do analysis very imperfectly, say by studying for about nine months. An ordinary tradesman would not be considered efficient unless he passed a much longer apprenticeship than this, but I know teachers complain that it is difficult to get students who are to be works chemists to stay longer than this. The result is that when really efficient men are wanted, they are not to be found, and they have to be got from abroad. In my address to the Chemical Society last year, I referred to the past neglect of research at our chemical schools, so that I need not speak further on that aspect of the subject here, though it is an important one in relation to our industries.

There is no chasm, as we have already seen, between pure and applied chemistry; they do not even stand side by side, but are linked together, so that a technical chemist needs to be a thorough chemist, and unless we employ such men we must be at a great disadvantage in relation to foreign manufacturers.

This brings me to a subject which has occupied much attention of late, but I fear is much misunderstood by the public generally. I refer to the teaching required by technical men, or technical education. The general idea is that it should be carried out in what may be called its narrow sense. That is to say, that it should be in relation to the existing manufactures and the present methods employed in them. Whereas there can be no doubt it will be of small service unless it is carried out on a very broad and scientific basis. As it is, the processes which are *publicly* known and taught, are more or less antiquated, simply because improvements are naturally kept secret as long as possible, and therefore to spend a large amount of time in studying details of old processes would manifestly be a great waste of power, and I am glad to find that this view of the matter is held by some of our leading chemical manufacturers. Our chemical industries are now undergoing such rapid and radical changes, owing to the advances in scientific discovery, that this cannot be too much borne in mind. To train a young man as a technical chemist, I consider, requires first that he should have a thorough knowledge of chemical science and know how to use it by conducting research, that he should have a general knowledge of those sciences which are connected with it, such as physics, and of those subjects required by all manufacturers, such as engineering, mechanics, &c., and also study the way chemical operations are carried out on the large scale, not in one branch of manufacture only, but in many.

With men in our works so trained, and of course possessing the suitable natural qualifications, we might expect to see our chemical industries make good progress and keep well to the front, but such a course of study could not be gone through in twelve months, nor would men so qualified be content to receive the remuneration for their services which is now given.

The proposed course for technical chemists at the Central Institute of the City and Guilds of London Institute is to occupy three years, the students having already devoted one year to elementary chemistry this makes four years of study, and this is hardly sufficient. It is to be hoped that those who are to be principals, or to take responsible positions in works will avail themselves of the opportunities afforded by this Institute or get some similar course elsewhere, so that we may have efficient men in this country to advance our chemical industries, and also that the value of such chemists may be appreciated in this country.

The employment of well-trained chemists in Germany, and the division of labour which results from this, has no doubt been one of the chief causes of the great success not only in the colour but in other chemical industries. In this country it not infrequently happens that an inventor, or the head of a firm, feels that no one can do the work he is connected with like himself, which is probably quite true; but at the same time he forgets that one person is not able to attend to the details of a number of processes adequately from sheer want of time and strength; if, however, properly qualified men can be set over them, although they may be slower and less capable than himself, yet having less divided minds and more time they are able to work out the details under his direction with much more success than he could alone, as well as see things from other points of view, and then greater perfection will be attained. I think this is a matter deserving of careful attention on the part of our manufacturers.

I have now given a very brief, and therefore a very imperfect outline of the history of the coal-tar colour industry, an industry to which none other can be compared for its rapid progress. I have drawn your attention to the fact that it is the offspring of scientific resea ch, that in return, as I before stated, it has in many cases given a fresh impulse to research by giving the chemist new products, and also by opening up new subjects of theoretical interest for consideration, and from the fruits thus resulting again reaping further benefit. This linking together of industrial and theoretical chemistry has undoubtedly been the great cause of its wonderful development. We now have not only all the colours of the rainbow, but we have also the more sombre, but often not less useful, colours, and, moreover, there are also great varieties of products of similar colours possessing different properties which fit them for special uses. This industry is also one of no mean dimensions. I have not been able to get any very recent statistical information on this subject, but notwithstanding the great reduction of prices of the products of late years, yet owing to the extended development it has undergone, the value of the annual output has probably increased, and not declined, and from what information I have on the subject, I should say it is perhaps not less than 3,500,000*l*.

In my remarks I have also been led to refer to some of the points connected with the migration of this industry from this country to Germany and the probable influence our patent laws had upon this, to the matter of technical education, and the employment of high-class chemists in chemical work. This latter subject is undoubtedly of great importance, and requires the earnest consideration of our manufacturers. It is found profitable to employ chemists of this class on the Continent, surely it should be found equally profitable to employ them here. In conclusion, I am happy to say there are signs of the coal-tar colour industry returning to our country, in part at any rate, especially in relation to alizarin, for which there are now three large works in existence, and the production of other colouring matters is also increasing.

FAUNA OF TRANS-ALAY

IN the *Izvestia* of the Russian Geographical Society (xx. 6) we find an interesting paper by M. Grum-Grzimailo, who has journeyed in the mountains north of the Alay region of the Pamir, chiefly for zoological purposes. The immense cultivated loess-fields of Osh, devoid of trees, yielded poor zoological results. Only a few uncultivated places had in the spring a rich fauna: great numbers of birds, various *Colubridæ*, the *Pseudapus pallasii*, tortoises, immense numbers of *Bufo variabilis* were met with. Here the author gathered a very rich collection of Lepidoptera; also *Zamenis kaufmanni*, *Taphrometoron lineolatum*, *Elaphis dione*, *Eryx faculuj*, and many others. In the middle of May all these disappeared under the burning rays of the sun. On the way to Vadil several species which were not found later on were met with, such as the *Trigonocephalus halis*, the *Anthocaris pyrothoë*, and several others. The neighbourhoods of Vadil yielded nothing interesting at that part of the season (middle of June). Of vertebrates only two *Eremids* and one *Trigonocephalus hydrus* were found. Shankh-mardan and Jordan, on the contrary, gave a rich crop of insects, and M. Grzimailo remained there for ten days. On a rich Alpine pasturage, Artcha-bash, where Kirghizes are in the habit of staying, he found very rich zoological materials. The collections were enriched with a great number of rare species, such as *Pol. tamerlana*, *Colias eogene*, *Arctia erschoffi*, *Hol. jagorum*, which are common almost exclusively to the Himalayas and the South-West Thian-Shan, as also by several new species. On the snow-covered plateaux interesting specimens were found, and among them the *Megaloperdix nigelie* and the *Arctomys caudatus*. On the upper Kok-su, extending to a height of 12,000 feet, M. Grzimailo found a number of species which he did not see either before or afterwards during his journey, especially with regard to Lepidoptera. Vertebrates are few at this height: they were represented by the *Arctomys caudatus*, the eagle (*A. fulva*), one species of *Falco*, the *Frigilis graculus*, the *Pyrrhocorax alpinus ?*, *Pica*, *Caccabis huckar*, *Megaloperdix*, and *Columba*. On the pass itself the holes of the *Arctomys caudatus* are seen everywhere, as also holes of some *Arvicola*. The Lepidoptera are richly represented at that part of the summer, especially the two genera *Colias* and *Parnassius*. On the Djekaindy Pass it was the same; the *Lycænæ* were very numerous, so that on the space of 3 metres the author found fifteen species of them, of which three were unknown to him. Without mentioning localities of minor interest, the plateau between the Kara-su and the Aram is worthy of notice for the brilliant collections of Lepidoptera which were made there. One *Lacerta* was found at a height of 11,000 feet, a species of *Elaphis*, the *Canis melanotus*, the *Lepus lehmanni*, the *Ovis polii*; of birds, the Falconidæ were most usual; also the *Upupa epops*, the *Cuculus canorum*, species of *Columba*, the *Orthygion coturnix*, *Caccabis huckar*, *Corvus corax*, and many others, this last reaching the highest parts of the region. Another find of great interest must be mentioned. The late Mr. Fedchenko had already caught one female Lepidopteron, which was determined by M. Erschoff as *Colias nastes*. This species having been found formerly only in Labrador and Northern Lapland, the determination remained doubtful, the individual having been but a female. M. Grzimailo has happened to catch a number of both males and females, which really proved both to belong to *C. nastes*. It remains now to explain the strange extension of this species.

SCIENTIFIC SERIALS

Rendiconto della R. Accademia delle Scienze di Bologna, 1884-5.—On the geometrical construction of the central axis in a given system of forces, by Prof. F. P. Ruffini.—A fresh contribution to clinico-experimental studies, showing the depressing action of ipecacuanha administered in large doses in pulmonary affections, by Prof. F. Verardini.—On the velocity of the polarised rays in a body endowed with rotatory motion, by Prof. Augusto Righi.—On the physico-pathology of the suprarenal capsules, by Prof. Guido Tizzoni.—On *Perineo melus*, a new genus of parasite observed in the pig, by Prof. Cesare Taruffi.—On the antimoniates of bismuth, by Dr. Alfredo Cavazzi.—Action of gaseous phosphated hydrogen on the trichloride of gold dissolved in ether, in alcohol, and in water, by Dr. A. Cavazzi.—On conjugated conic sections, by Prof. Virginio Retali.—Some researches on the so-called syntomatic carbuncle in cattle, by Prof. Alfrede Gotti.—Observations on Jacobson's organ and on Stenson's duct in the camel, by Dr. Francesco Peli.—On the central termination of the optical nerves in mammals, by Prof. Giuseppe Bellonci.—On the paraboloid surfaces in the selliform rhombohedrals of dolomite and other anhydrous carbonates, by Prof. Luigi Bombicci.—Some general observations on the systems of functions, by Prof. Salvatore Pincherle.—On a monstrous fœtus requiring the operation of embriotomy for its delivery, by Dr. Cesare Belluzzi.—On the question of sex in *Tolyposporium cocconii*, by Dr. Fausto Morini.—On the fossil remains of Dioplodon and Mesoplodon occurring in the Upper Tertiary formations in Italy, by Prof. Giovanni Capellini.—Forensic experiments in traumatology with firearms, by Dr. Giuseppe Ravaglia.—Contributions to the chemical study of intestinal perforation in typhoid fever, by Prof. Giovanni Brugnoli.—On the mode of genesis of a polar globule in the ovarium of certain mammals, by Prof. Giuseppe Bellonci.—A systematic enumeration of the funguses in the province of Bologna, by Dr. Fausto Marini.—On the thermal emissive power of electric sparks, by Prof. Emilio Villari.—On the use of curvilinear coordinates in the theory of the potential and of elasticity, by Prof. Eugenio Beltrami.—An analytic method of determining the equation of time, by Prof Antonio Saporetti.

SOCIETIES AND ACADEMIES

BERLIN

Physiological Society, June 19.—Dr. J. Munk gave a brief sketch of the different views put forth respecting the formation of fat in the animal body, and then gave a short account of the now almost universally accepted view of Voit, who, on the basis of his very numerous experiments, laid down the doctrine that the fat in the animal body proceeded either from the alimentary fat, or, when this was not sufficient, from the albumen, which on its decomposition yielded products that by synthesis became transformed into fat, while the carbo-hydrates never yielded material towards the formation of fat in the animal body. Opposition to this doctrine was raised on the side only of agricultural chemists, who, by experiments on swine and geese produced direct demonstration that the deposition of fat was considerably increased by feeding with carbo-hydrates. In consequence of these experiments Prof. Voit admitted that omnivorous and herbivorous animals might in certain circumstances form fat out of carbo-hydrates; such, however, he maintained, was never the case with carnivorous animals and man; in them all fat was derived from the alimentary fat and the decomposition of albumen, both in his own experiments and in all hitherto published, and the fat was seen to be derivable from these two sources alone, even though only 12 per cent. of the decomposed albumen were taken for the formation of fat, and much more if, according to the theoretic calculations of Herr Henneberg, it was assumed that as much as 51 per cent. of the decomposed albumen might be utilised towards the formation of fat. Seeing now that Prof. Voit admitted that, in the case of omnivorous and herbivorous animals fat was produced from carbo-hydrates, the speaker set himself the task of establishing experimental conditions under which fat might be formed from carbo-hydrates in the case, likewise, of carnivorous animals. For these experiments he selected a dog, completely impoverished it of all fat by means of long fasting, and then gave it an aliment very rich in carbo-hydrates. The animal required to be young, or otherwise the loss of fat by fasting could not be complete, and if it were desired to obtain certainty respecting the attainment of perfect deprivation of fat, the decomposition of the albumen during the period of fasting would have to be traced by regular determinations of the quantity of azote in the urine and the excrement. Prof. Voit had (as was already known) proved that on account of its ready decomposibility, fat was a protection against the decomposition of albumen ; such would necessarily be the case in the fasting organism likewise, and the corporeal fat would necessarily protect the albumen from decomposition. In point of fact Dr. Munk found in fasting animals that when they were poor in fat the decomposition of albumen slowly abated in correspondence with the ever less abatement of the weight, whereas in the case of individuals rich in fat, the nitrogenous secretions in the last period, after the corporeal fat at disposal had been decomposed, did not only not abate, but even increased somewhat. The same cause as that followed by the nitrogenous secretion was also observed in the case of the elimination of sulphur. The exact process of albuminous decomposition during fasting thus offered an indication of the attainment of complete deprivation of fat in the body. The carrying out of the experiments was, however, attended with so many difficulties, that hitherto only one experiment had succeeded. It had reference to a large dog of three to four years old, weighing about 35 kilogrammes, which had been made to fast for thirty-one days, and was then fed daily with 200 grammes of meat, 100 grammes of lime, 400 grammes of starch, and 500 grammes of sugar, made into a preparation very acceptable to the dog. The gluten was occasionally added to the aliment to restrict the decomposition of the albumen. The experiment might be continued for twenty-five days ; in the two last days diarrhœa set in, and the dog was killed in order to determine precisely the contents of fat in the body. The weight of the body of the dog during the process of feeding had increased by four kilogrammes, and amounted at the end of the experiment to 27 kilogrammes. Of the albumen partaken only 800 grammes were left undecomposed in the whole body. At the outset, therefore, it could be inferred that the dog had formed and deposited a considerable quantity of fat—an inference which was confirmed by the examination of the body. The fat of the underskin tissue and of the mesentery was carefully cut out, melted, and weighed. Then the amount of fat on the muscles was determined in particular samples, and the fat on all the muscles of the animal calculated. The fat of the liver was directly ascertained, and, finally, an account was taken of the fat of the bones, the nerves, and the other organs, which was admitted to be only half the amount which other physiologists had obtained on the same parts in an individual whose skin, muscle, and liver fat corresponded with that of the dog examined in the present case. Certainly the quantity of fat thus found was considerably less than the quantity actually formed during the time of the experiment. From this sum of fat deposited by the dog there was now deducted the total amount of the alimentary fat which had been appropriated, and of the fat which might have been formed from the decomposed albumen (12 per cent.). The result was a remainder of over 900 grammes of fat formed by the dog, which was derivable neither from the alimentary fat nor from the decomposed albumen, and which had, therefore, to be attributed to the carbo-hydrates that had been copiously administered. The speaker instituted a second calculation, taking account of the assumption which had hitherto, however, never been proved or even rendered probable by a single experiment, but was purely a deduction from constitutional formulæ—the assumption, namely, that of the decomposed albumen as much as 51 per cent. might be utilised towards the formation of fat. But even under this supposition there still remained more than 400 grammes of fat formed by the dog which, contrary to the doctrine of Prof. Voit, must have been produced from the carbo-hydrates. In compliance with a suggestion thrown out in the discussion of the question the speaker had, furthermore, calculated as a fat-former the whole of the lime taken by the dog, although all experiments had demonstrated that lime in no case produced fat ; and yet, after that item had been fully taken account of, there were about 60 grammes of fat left that could be derived only from the carbo-hydrates. Dr. Munk therefore deemed it indisputably demonstrated by this experiment that in the case of carnivorous as well as omnivorous and herbivorous animals carbo-hydrates might in certain circumstances contribute towards the formation of fat.—Dr. Höltzke, following up a communication recently made by him respecting the influence of narcotics on pressure in the eye, reported experiments he had made concerning the influence of the blood-pressure on the intra-ocular pressure. The view had hitherto been universally accepted that the pressure in the eye was dependent on the blood pressure, and a series of experiences and experiments had been collected by way of proving this dependence. The nerves had likewise shown that they exerted an influence on the pressure in the eye, so far as they influenced the vascular system. Of the sympathetic in particular it was asserted that its paralysis induced an augmentation, whereas stimulation of the nerve caused a diminution of the intraocular pressure, and this converse process was said to be connected with the expansion and contraction of the vessels. Seeing, however, that some investigators maintained that the effect of the sympathetic on the pressure of the eye was exactly the opposite to that just referred to, the speaker had instituted new measurements by means of a manometer, utilising the second eye in the way of control. The result at which he arrived by this means was that the cutting of the sympathetic always entailed an abatement of the pressure to an average of 6 mm. mercury, and that stimulation of the peripheral nerve ending caused an increase of the pressure amounting to 14 mm. mercury. Stimulation of the supreme ganglion of the sympathetic had the same effect. If the veins of the neck were bound on the under side and the carotid was compressed then had neither the cutting nor the stimulation of the sympathetic absolutely any effect on the pressure in the eye—a proof that the influence of the sympathetic as above stated was only mediate, that the paralysis of the sympathetic induced the lowering of the ocular pressure only in consequence of the decrease of pressure in the vascular system and that the stimulation of the nerve caused the increase of the intra-ocular pressure only because of a rise of pressure in the blood. An experiment with a view to measuring the influence of the sympathetic on an atropinised eye did not yield perfectly decisive results, a circumstance which determined the speaker to investigate once more the influence of atropine on the ocular pressure. The result was somewhat different from that recently communicated. It was now ascertained with perfect certainty that the influence of atropine by itself was a diminution of the ocular pressure and therefore the contrary of that of eserine. Only when the pupil was powerfully expanded by the atropine did the pressure in the eye rise in correspondence with the other experiences that each expansion of the pupil was accompanied by an augmentation of pressure, and each contraction of the pupil was followed by an abatement of the ocular pressure. On the expansion and contraction of the pupil, the rise or the reduction of the blood-pressure became, in turn observable, and this latter again on its side generated a rise, or, as it might be, a fall of the pressure in the eye. This parallelism of the ocular and the blood-pressure the speaker had found to hold good in all his experiments. The pressure in the vitreous body invariably showed the same changes as did the pressure in the watery chamber.

CONTENTS

PAGE