Nature June 5th 1884.

Charles William Siemens

From a Photograph by Van der Weyde

London Published by Macmillan & Co

Nature

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

VOLUME XXX

MAY 1884 to OCTOBER 1884

*" To the solid ground
Of Nature trusts the mind which builds for aye."*—WORDSWORTH

1912. 1942.

London and New York

MACMILLAN AND CO.

1884

LONDON:
R. CLAY, SONS, AND TAYLOR, PRINTERS
BREAD STREET HILL, E.C.

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*"To the solid ground
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THURSDAY, MAY 1, 1884

SCIENCE AND MANUFACTURES

THE occurrence in one week of meetings held by the Iron and Steel Institute, and by the Institution of Mechanical Engineers, seems to offer a fitting occasion for further remarks on the connection between science and art, between practical construction and theoretical investigation. A few months ago, in an article on the same subject, it was pointed out how these two branches of knowledge were found to work in harmony for the ends of each—science instructing art, art supporting and ministering to science; and this truth was illustrated by a variety of examples. Others which have occurred since that time may be touched upon before we conclude. At this moment we are anxious to insist once more on the need which exists to draw this union between art and science closer than it has ever been drawn before, and to remove all obstacles which may stand in the way of its fullest realisation.

The necessity for this union lies in the fact that England has an industrial supremacy to maintain, and that year by year its maintenance becomes more difficult in the face of keen and jealous competition. Whatever may be said in Parliament, all practical men are aware that the great tide of prosperity, promised last year by the President of the Board of Trade, has not yet begun to flow; that on the contrary there was never perhaps a time when the special industries of England were more depressed, or their outlook more gloomy. The fact that the steel-rail makers of England have banded themselves with those of France and Belgium into an association for the maintenance of remunerative prices speaks volumes, not only as to the severity of competition, but as to the sources from which that competition comes. On the other side we see the iron-masters of America extending their output year by year, and her manufacturers entering into competition with us in neutral markets, while jealously excluding us from their own.

What is to be the remedy for this state of things? How is the demand for manufactured articles, and for the raw materials out of which those articles are made, to be once more equalised with the supply? Unless some vast market, such as China or Central Africa, can be opened up to European commerce, the only chance seems to lie in a new departure; in some great cheapening of production, or cheapening of transport, comparable to that which was effected by the development of railways. Now what is the physical fact lying at the basis of railway locomotion? It is simply this, that iron laid in the form of a track offers a resistance to rolling which, as compared with an ordinary road, is insignificant, whilst at the same time it offers a resistance to sliding large enough to utilise to the full the vast tractive power of the modern locomotive. The first point had long been known; the second was seized by the practical genius of George Stephenson, and enabled him at once to solve the problem of high speed locomotion. In so doing he owed nothing to science; but science might have discovered the fact, and would have done so with small trouble, if the idea had been put into her head—if, in fact, there had been in England that union of theory with practice which it is our present aim to advocate.

What is wanted now is that science shall point out some other fact of nature, new or old, which practice may seize upon, turn to her own ends, and make the basis of some new industrial development. It is easy to indicate various directions which such a development might take. Thus there is great need of some system of light railways which can be laid down on ordinary roads, and so cheaply that the traffic available on such roads may be sufficient to pay a fair return on the capital. It is impossible to calculate the advantages which would spring from the wide extension of such "third-class railways," as they are called in Germany. Again, the storage of power, such as that of the tidal wave, with cheap and ready means for giving it out when and where it is needed, offers a wide field for invention, and may lead to the most fruitful results. The transmission of power to long distances, whether by electricity, compressed air, or otherwise, is a somewhat similar problem, which at present occupies the attention of many engineers and men of science. Lastly, the more homely subject of house-building offers at this moment special inducements to constructive genius. If houses could be built, by the use of iron or otherwise, at, say, half their present cost, the problem of sheltering our poor would be solved; unsafe and ruinous tenements would disappear, and a demand would set in for building materials and labour such as the world has never known.

Here, however, the question arises, Supposing that science and art should combine successfully for any such purpose, is it in England that the development will take shape?

At the time of the last industrial epoch, that of the introduction of railways, it would have been safe to prophesy that this would be the case. It is by no means so certain now. As regards cheap transport, for instance, the most promising recent invention in this field, viz. the caustic soda condenser previously described by us, was brought out in Germany. Other improvements in the same field, such as the portable railways of De Cauville, the rack railway of Riggenbach, the cable tramway of Hallidie, the fireless engine of Francq, the iron sleepers which are rapidly becoming universal in Germany, have all taken their rise either on the Continent or in America. The storage of power, in its only practical form, that of the secondary battery, owes its origin to Planté and Faure. The transmission of power is being worked out by Siemens in Berlin, and by Deprez and Tresca in Paris. Lastly, as to building, no one can travel abroad without seeing that as regards scientific architecture, England stands far nearer the bottom than the top in the scale of civilised nations.

What is the reason of this? Why is England thus lagging behind in the race? The answer is not far to seek. In America, in France, above all in Germany, the union between science and art is far more close and cordial than with us. Every practical constructor or manufacturer is anxious to know all he can of science, every scientific professor desires to mix practice with his theory. Thus on the one hand we find ordinary engineers drawing on all the resources of mathematics for the solution of such problems as the proper section of rails or the resistance of trains; on the other hand we see Clausius, perhaps the greatest of German physicists, devoting two long papers to investigate the working theory of the dynamo machine. But a concrete instance will make our meaning clearer. Within the last few days we have inspected a safety lamp, of which some thousands have already been sold for the German mines. It has many points of excellence, but we need only dwell upon one. It is well known to be most important that a miner's lamp should be locked in such a way that he cannot, if he will, open it; and it has been found very difficult to provide any simple kind of lock which it is beyond the resources of a clever workman to tamper with. In this lamp the difficulty is got over by making the upper part screw into the lower, while inside the lamp there is a catch or pawl, which, as in a common ratchet, prevents the screw from being turned the opposite way. Hence, that the lamp may be unscrewed, the pawl must be drawn out of place. In the overseer's office this can be accomplished by means of a powerful horse-shoe magnet. The pawl has a tail, which is attracted by the magnet when the latter is placed in contact with the side of the lamp. The tail moving towards the magnet, the pawl moves in the opposite direction, and so allows the upper part of the lamp to be unscrewed, while the lower is held as if in a vice by the same magnetic power.

Now here we have a simple and beautiful contrivance for effecting an important practical object. It is merely the application of a well-known scientific principle to solve a special problem in construction; but it never could have been invented except by one to whom the resources of science and the needs of art were equally familiar—who was at once a physicist and an engineer. Now it cannot be questioned that in England we can boast many of the highest authorities in science, many men of the highest skill in practical construction; but the union of the two is comparatively rare, and yet it is this very union—the application of the scientific spirit to the things of common life, as so well illustrated in the excellent paper by Prof. Newcomb, published elsewhere—which is the vital necessity of the age.

The fault is not all on one side. Science sometimes looks down on Practice as a rough, prosperous mechanic, interested in nothing but his work and his wages, while Practice sneers at Science as a fine gentleman, too much absorbed in crotchets to be worth any attention or respect, and who, if he had not some one to look after him, would shortly be in the workhouse.

As an instance we may take the magnetic balance lately described by Prof. Hughes. This beautiful instrument promises at least to supply a want long felt by the makers and users of iron—the want of some method of "mechanical analysis"—some means of determining the physical and chemical properties of a given material—without testing it to destruction, as is now unavoidable. But whilst thus appealing on the one hand to manufacturers, the invention appealed on the other hand to electricians, as offering a ready index of the magnetic qualities of a metal. By the latter it has been welcomed, and is being used, as, for instance, by Mr. Preece, for the testing of telegraph-wire; but, so far as we know, not a single manufacturer or engineer has thought it worth while to encounter the small amount of trouble and expense which would be needed to test thoroughly the capabilities of the instrument in determining the mechanical properties of finished iron or steel.

We by no means wish to imply that no progress is being made in the direction here pointed out. The work undertaken by the City and Guilds Institute, the foundation of scientific colleges, such as those at Birmingham, Sheffield, Leeds, Nottingham, and elsewhere, the appointment of a Committee on Technical Education, the delivery of scientific lectures at the Institution of Civil Engineers—these are all signs that the gap existing between art and science is at last recognised, and that endeavours are being made to draw them together. Moreover, the old "rule-of-thumb" engineer is rapidly passing away, and a new generation is springing up, who, if they do not possess much science themselves, are at least alive to its value. The testing machine, for instance, is becoming a recognised institution in large workshops, where not many years ago it would have been scouted as absurd. In the skilful hands of a practical engineer, Mr. Wicksteed of Leeds, it has been made to record its own variations of stress by a self-drawn diagram, and this record seems likely to throw fresh and unexpected light on the physical problems of extension and rupture. The same gentleman has both discovered and applied a new and most remarkable phenomenon in friction; the fact, namely, that if we give a rotary motion to a body which is in contact with another, not only is the friction diminished in the direction of motion, but the friction in the perpendicular direction is also diminished, apparently in at least an equal degree. Hence, for instance, by rotating the leather packing of an hydraulic ram, it becomes quite free to move in its cylinder in obedience to a difference in pressure on one side or the other. Here we have, once more, science helping art, and art in return throwing light upon the path of science.

These facts, and others like them, are encouraging signs, but we must repeat that something more than signs is needed. The work must be not only begun but finished, the bonds of union must be drawn close, and that quickly, or England will find that it is too late, and that she is once more ready to do the work of the world just when the world has left her no work to do.

FORSTER'S "STRATA OF THE NORTH OF ENGLAND"

A Treatise on a Section of the Strata from Newcastle to Cross Fell. By Westgarth Forster. Third Edition, revised and corrected to the present time by the Rev. W. Nall, M.A., with Memoir. 8vo. (Newcastle-upon-Tyne, 1883.)

THE position of Forster's "Strata" among the classics of geological literature in England is so well defined that a reissue would be welcome to many readers, as although the progress both of coal and metal mining during the long interval that has elapsed since the appearance of the last edition in 1821 has done much to supplement, and in some instances modify, the author's evidence, it must ever remain as a splendid monument of geological investigation as carried on in the earlier years of the century. Unfortunately, in the present issue the editor has carried out his duties in a very thorough-going fashion; to use his own words, "Some alterations have been made in this edition of the 'Strata.' Parts I. and II. have been revised and rearranged; Part III. has been partially recast; some of the old sections have been extended, and other sections have been given; obsolete matter has been expunged, and new matter in the form of notes has been added."

If the editing had been confined to the last-mentioned additions, or rather if all the alterations had been supplied as footnotes or in the form of appendixes, such a course would have been perfectly justifiable, and the value of the text would have been enhanced; but from the course adopted of shifting the original text backwards and forwards to bring it into harmony with more modern views, and rearranging the sections even to the extent of renumbering the beds of limestone in the lead-measures, and the intercalation of new subdivisions in the limestone series not contained in the original, the work has become so strangely metamorphosed that any one taking it for what it professes to be, namely, Westgarth Forster's "Strata," will be liable to be strangely misled, unless he carefully compares it with the original text. This is much to be regretted, as the editor's work has evidently been a labour of love, and it is strange that he should have so ill-used his favourite volume.

The editor has, however, done one good service deserving grateful mention by supplying a memoir of the author, which is, however, eccentrically interpolated between the original table of contents and the text. From this we gather many interesting particulars of the life of one who may be regarded as the prototype of the Sopwiths, Bewicks, and other mining engineers in the north of England, who have become famous not only in their original districts, but in all parts of Europe and America. It is somewhat surprising to learn how in the year 1807 the material for the first edition of the "Strata" was collected by Forster, who for that purpose resigned the agency of the Allendale lead mines. The volume was issued in 1809 in the same year with William Smith's first geological map of England, and at once became exceedingly popular; and thenceforward the author was recognised as one of the leading men in his profession, and was fully engaged in many surveys until his retirement in 1833. During this active period of twenty-three years he worked in nearly all the mineral districts of England and Wales, with the exception of Cornwall and Devon, and also visited Spain and North America. The American trip was made in 1831, in pre-steamboat days, in the fine packet-ship *Napoleon*, making a fairly good voyage of thirty-two days across the Atlantic. The districts visited were Pottsville and Mauch Chunk, in the anthracite district of Pennsylvania, which had then been discovered only eight years, and the Phœnix Copper Mines in Connecticut.

The later years of his life were clouded by misfortunes due to losses in working some lead mines in Wales, and before the spring of 1829 he had spent nearly all that he possessed in abortive trials, at a period of extreme depression in the lead trade. In 1833-34 failing health led him to retire from active work, and on November 9, 1835, he died at Garrigill, in Cumberland, in his sixty-third year. In the author's words, Forster rendered valuable service to the sciences of mining and geology, and for that service, if for no other reason, his name will continue to be remembered for a long time to come. H. B.

OUR BOOK SHELF

A Manual of Chemistry. By Henry Watts, B.A. (London: Churchill, 1883.)

THIS work is stated by the author to be intended for a student commencing the study of chemistry, and, as he states in his preface, this volume commences with a short sketch of the more important elementary bodies, the principal laws of chemical combination, and the representation of the constitution and reaction of bodies by symbolic notation. In addition to this there is a large section on chemical physics, including the mechanical properties of gases and the chief phenomena of heat, light, magnetism, &c. For an elementary work, as intended by the author, it is somewhat dense, and would be certainly apt to frighten a beginner in chemistry. The sections on physics alone, comprising Part I., occupy very nearly 150 pages, and within this narrow space we find that in the domain of light we have refraction, reflection, circular polarisation, &c., treated at considerable length. In magnetism and electricity we have a very complete and exceedingly condensed mass of information, certainly much too complete and condensed for an elementary text-book. In the purely chemical section, forming Part II., the work is extended so as to include a considerable chapter on crystals and the more recent extensions of the atomic theory, and also to the so-called rare metals, which we find treated at considerable length. As to the arrangement of the chemical part, the method adopted in "Miller's Chemistry" of arranging the elements under the terms metals of the alkaline earths, &c., has been adopted, which is a very excellent method of arrangement for teaching purposes, as it allows of elements with similar properties being compared. There is evidently throughout the whole of the book a tendency to condense far too much into a small space. It would be an exceedingly difficult book indeed to be put before an *absolute* beginner. The explanatory part is reduced apparently as much as possible, although a great many facts are crammed in, certainly in good order; but still a beginner requires very much more explanation of facts than is to be found in this book. On that account, and being more an epitome of facts than explanations, especially in the chemical portion, it is scarcely possible to criticise it. The arrangement is very excellent and the details are well up to date. We notice that ozone has been put in in the form of an addendum: surely its position is closely in connection with oxygen. It is very liable in this position to be overlooked, or at any rate neglected, by a student. As there is such a considerable amount of attention given to the rare metals, especially vanadium, many of its compounds being detailed, it is somewhat surprising that davyum, though perhaps not yet absolutely settled, is not mentioned along with them. On looking carefully through the book, a number of points occur in which more explanation, or even an explanation of formulæ, would be very advantageous; but on the whole Mr. Watts is to be complimented on having produced a very complete, though certainly not quite elementary, manual on the science.

Arithmetical Chemistry. By C. J. Woodward, Birmingham and Midland Institute. (London: Simpkin, Marshall, and Co., 1884.)

THIS is almost a book of questions selected from the Cambridge and Oxford Local, University of London, Science and Art Department, and other examination papers. It is divided into headings on laboratory calculations, where, after an example of a volumetric or gravimetric analysis, a number of exercises and questions follows, and gas analysis with corrections of gases for pressure, &c., and determinations of vapour densities, specific and atomic volume, specific heat, calorific power, calorific intensity, kinetic theory of gases, and diffusion. The explanations are in most cases short and to the point, but the immense number of examples and exercises given tend to make it a "getting-up" book for examinations rather than a book to work with in the laboratory.

Experimental Chemistry. By J. Emerson Reynolds. Part III. (London: Longmans, Green, and Co., 1884.)

PROF. REYNOLDS, in the first and second parts of this little work, has departed somewhat from the usual method adopted in chemical books for junior students. The first and second parts deal entirely with the non-metals and their compounds, acids, &c.; while the third part is devoted to metals. It is divided into numbered experiments for the student to perform in rotation, and should be exceedingly valuable to medical and other students who have only a short time at disposal for practical chemistry. There is no attempt at systematic analysis, but the experiments are sufficiently logically arranged to enable a student who gives his attention to them to be able to perform any simple qualitative analysis. At the same time each experiment is very fully explained, and the reactions expressed in most cases with equations. Part III. is supplemented by a series of analytical tables at the end, which, however, are not very clear. They are certainly somewhat too complex for the class of student for which the book is intended. On the whole, however, it is a very excellent work.

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.*]

Reply to Mr. Grubb's Criticisms on the Equatorial Coudé of the Paris Observatory

I HAVE just received the last number of the *Scientific Transactions of the Royal Society of Dublin*, containing a description of an instrument analogous to the equatorial constructed on a principle which I published in 1871 in the *Comptes Rendus*. Since it seems to me indispensable after I have studied Mr. Grubb's memoir to reply to it publicly, I beg you to insert this note in NATURE. In order to give greater importance to the modifications imagined by himself, the author, Mr. Grubb, submits the instrument imagined by me to a very severe criticism, and attributes very severe defects to it. There is no doubt that there is a considerable difference between the instruments in question. One of them, in fact, as experiments carried on during two years have proved, lends itself to the execution of all possible astronomical researches under the best possible conditions for securing precision, while the other, according to the description given in the publication above cited, renders impossible a very great part of the researches to which an equatorial is specially destined. Further on I shall insist on this difference, but I wish first of all to reply to Mr. Grubb's objections and to show that they are in all points contrary to the fact.

To give weight to his argument Mr. Grubb examines a case of the construction of an instrument of 27 inches aperture, and he anticipates in the construction the following difficulties, which he considers insurmountable:—(1) The optical difficulty of constructing a large plane mirror. (2) The practical difficulty of procuring a disk of the necessary dimensions. Mr. Grubb affirms that there is no glass-works capable of making a disk of glass so large. (3) The difficulty of moving a mirror of which the weight, according to Mr. Grubb's calculations, will be very nearly half a ton. (4) The dearness of the instrument, which would cost more than an ordinary equatorial, plus dome and observatory.

I will discuss these points one by one. (1) The construction of plane mirrors is a settled question nowadays. Many astronomers have been able to convince themselves that in the equatorial coudé of 11 inches aperture the introduction of a plane mirror of 16 inches in no way marred the definition. The brothers Henry, who constructed this mirror, have just made another of 40 inches diameter, which leaves absolutely nothing to be desired in this respect. This difficulty, moreover, is so little felt or feared by our opticians that the price of a similar mirror for an instrument coudé is only about a quarter of the price of an object-glass, in spite of the great difference of their respective surfaces. Thus, for an object-glass of 27 inches, the price of which is 70,000 francs, that of a plane mirror of 38 inches is 18,000 francs. For an object-glass of 40 inches, price 200,000 francs, the corresponding mirror of 58 inches only costs 40,000 francs. I admit, nevertheless, that in this respect Mr. Grubb may have had apprehensions. In the past, in fact, serious difficulties have been met with in the production of plane mirrors, but my own personal experience has enabled me to realise the real cause of this want of success. Up to the present time, to satisfy preconceived ideas it was believed that to establish rapidly an equilibrium of temperature it was necessary that the thickness of the mirror should be small. Then, under the influence of a tightening, however slight, or only a flexion, the mirrors were deformed unequally, and consequently produced an obvious alteration in the beauty of the images. The brothers Henry, studying the same question by different processes, have arrived at the same conclusions. In giving to the disk a sufficient thickness, the production of a plane surface is not more difficult than any optical surface whatever. The means of verification are so delicate that in a mirror of 40 inches diameter an error of 1/50,000 of a millimetre can easily be determined and eliminated. So if there be a sphericity in the mirror, the radius of curvature will have at least 1600 leagues, that is to say, about the radius of curvature of a bath of mercury, and in taking still greater pains it will be possible to go further and to reach an exactitude such that the radius of curvature might be twice as great. Under these conditions it is not possible, as one can readily see, that there can be any appreciable imperfection. The problem is therefore solved, as I said above.

(2) The impossibility of finding glass-works capable of furnishing large disks for mirrors, pointed out by Mr. Grubb, does not exist. We have in reserve at the Observatory a disk for a plane mirror of 1·22 m. diameter, weighing 650 kilos. The mirror just finished by the brothers Henry has a diameter of 40 inches, with a thickness of ·17 m.; this weighs 380 kilos. The glass-works of St. Gobain are prepared to furnish blocks weighing a ton, the diameter 58 inches, to anybody who will order them.

(3) The third point in Mr. Grubb's criticism has arisen from some error in calculation. Mr. Grubb makes me say that it is necessary to give the mirror a thickness equal to 1/5 of the diameter, and then making some calculations with this false datum, he finds that the mirror necessary for an equatorial coudé of 27 inches would weigh 8½ tonnes, and it is this result that misleads him. Here are the facts. I have never said that the thickness should be ·18 of the diameter. I have given ·18 as a *maximum*. Allow me to quote textually what I have said (*Journal de Physique*, August 1883): "Des recherches effectuées avec des miroirs de 0·08 m. m'ont démontré que pour prevenir dans un miroir toute déformation causée par la flexion ou un leger serrage, il faut que l'épaisseur du verre soit 0·18 du diamètre. Peut être avec des miroirs plus grands sera-t-il possible de réduire notablement cette épaisseur; en tout cas, la fraction 0·18 doit être considérée comme un maximum."

In my equatorial coudé for example, the mirror has a thickness equivalent to 1/6 of the diameter. In taking 1/7 for a mirror of 38 inches destined for an object-glass of 27 inches, one has still nothing to fear, for its weight would scarcely reach 250 kilos., that is to say, the half of the weight indicated by Mr. Grubb. For an instrument which weighs already between 8 and 9 tonnes, this addition of 250 kilos., or of 280 kilos. if we adopt the proportion of 1/6, is so small as not to be worth mentioning. From a mechanical point of view the displacement of the cylinder carrying mirrors of 250 kilos., or even a tonne, considering both the movement and the stability of the installation, does not offer the least difficulty. It is a mechanical problem so elementary that not only the first-class artists of all countries but even ordinary constructors would be able to solve it without any great effort of intelligence. I am only astonished that a man of Mr. Grubb's reputation should have stopped to consider such a detail.

(4) We now come to the question of expense. I am curious to know whence Mr. Grubb could have got his information, because it differs so absolutely from the facts. In spite of the addition of two supplementary mirrors, the price of the new instrument is less, or at all events not greater, than an ordinary one. The simplicity of the construction is such that the saving in the mechanical part covers the expense of the optical one. As I have already pointed out in the *Comptes Rendus* in 1871 and 1873, and in the *Journal de Physique* already quoted, the considerable expense of rotating domes, &c., is entirely avoided. I am greatly astonished, therefore, that Mr. Grubb makes on this point, so thoroughly studied, an objection so little founded. If he had, moreover, looked at the drawing which I published in the *Journal de Physique* of last year, he would have seen that it is almost identical with that which he has communicated to the Dublin Society, so far as the general arrangements for sheltering the observer and instrument are concerned. M. Gautier has been good enough to furnish me with the prices.

Dimensions Inches	Ordinary Equatorial Francs	Equatorial Coudé Francs
12	48,000	44,000
18	81,000	79,000
27	183,000	183,000

These prices are for instruments of the most complete kind.

The objects which I wished to attain were (1) to realise an instrument more stable than the ordinary ones, and to render possible the measurement of large angular distances; (2) to establish an arrangement which permits the astronomer to explore the whole sky and to regulate himself, with the most perfect convenience, all the movements of his apparatus; (3) to avoid the necessity of those monumental domes, of which both the building and the movement are always so costly; (4) to realise an instrument which, in spite of the introduction of two supplementary mirrors, would be optically more perfect. Indeed, being able to give the telescope a much greater focal distance than in the common large instruments, the achromatism may be rendered more perfect. I believe that I have succeeded in satisfying these various conditions, and that I can appeal to the judgment of several of the most eminent astronomers who have studied the instrument. Mr. Grubb makes a last reproach rather curiously. He is astonished that I have not as once applied the new system to the construction of a large instrument, for he adds, "No one finds any difficulty in working an equatorial of 11 inches." I do not know whether this opinion of Mr. Grubb is based on his personal experience, but I do know that all astronomers do not share his optimism on this point. To cite only one instance. In all the observatories where the discovery of comets or small planets is in question, telescopes of 6 or 7 inches aperture at the outside are employed. Continued work of this kind with a larger instrument is accompanied with fatigue. Now in my instrument, whatever be the nature of the research, a precious economy of time and energy is secured. If we have not in Paris a larger telescope on my plan, I must say that my regret is greater than that of Mr. Grubb, and that the lack of it does not depend on me.

In a subsequent letter I propose to discuss Mr. Grubb's counter proposal. M. Lœwy

Paris Observatory, April 10

On the Motion of Projectiles

Experiments made in 1866 showed that the resistance of the air to the motion of projectiles varied only slightly for such forms of heads as were likely to be used in practice, and that it was of no importance whether the apex of the shot was pointed or rounded off.

From what I have said (Nature, vol. xxix. p. 527) it is evident that before we can calculate the trajectory of shot we must know something of the quality of the gun from which the shot is supposed to be fired. For if trajectories of shot fired from the experimental guns were calculated by the use of the tabular values of K_v, it is plain that the calculated ranges would err in *excess* for the 3-, 7-, and 9-inch guns, and in *defect* for the 5-inch gun. As Mr. Ristori, using tabular coefficients, finds his calculated less than his experimental range, it may be that his gun gave a degree of steadiness above the average, as the 5-inch gun did. It is also necessary to be correctly informed of the exact *initial direction* taken by the shot, because, owing to the recoil of the gun, there is a "jump," which gives a greater elevation to the direction taken by the shot than the elevation of the axis of the gun. It is also a difficult matter to obtain the correct *initial velocity* of a small-arm bullet. For I am assured on the highest authority that the cotton threads of which my screens are composed, would be likely to cause unsteadiness in the motion of a small bullet, and if so, the measured velocity would be lower than the velocity of the undisturbed bullet. And if wire screens were used, the evil would be increased. From the excellent work done by Robins with a *small* ballistic pendulum, I am disposed to think that that instrument might be advantageously used in experiments with *small*-arm bullets.

As my tabular values of K_v were determined from the average results given by 3- to 9-inch shot, I did not venture to put them forward as being applicable to the calculation of the motion of small bullets. But recently Major McClintock, R.A., Assistant Superintendent Royal Small-Arms Factory, has used them for that purpose. He says:—"The accuracy of rifle-bullet trajectories calculated by means of Prof. Bashforth's tables, has been tested by firing a large number of rounds through paper screens placed at different points along the range. The rifle used in the experiment was the Martini-Henry, and the screens were erected at intervals along a 500-yards and a 1000-yards range. The result of the experiment was most satisfactory, the mean heights of the bullet-holes in the screens agreeing closely with the heights found by calculation."

As is well known, guns vary greatly in the degree of steadiness they impart to elongated shot, and this often changes with the initial velocity of the shot. It is important, therefore, to have a ready way of changing the tabular coefficients K_v in the ratio of $1 : \kappa$, when the coefficients will become κK_v, where κ is a constant for that round. In the calculation of the general tables, and in using the tables of values of integrals denoted by X, Y, and T, we have to deal with the product $\frac{d^2}{w} \times \left(\kappa K\right) =$

$\left(\frac{d^2}{w}\kappa\right) \times K_v$. Hence, if we calculate the value of $\left(\frac{d^2}{w}\kappa\right)$ at first, we may proceed without further change to use the general tables, or to calculate trajectories, using the tabular values of K_v, just as when no change is required in the value of the tabulated coefficients. For the 3-, 5-, 7-, and 9-inch guns, the value of κ, in the cases we have considered, would be 1·08, 0·92, 1·05, and 1·08 respectively. There was one case where the studs did not act, when K_{950} was found to be 335·1 instead of 75·0, which gives $\kappa = 4{\cdot}47$. The smaller the value of κ the steadier will be the motion of the shot. Mr. Ristori might calculate another range, using $\kappa = 0{\cdot}92$ given by the 5-inch gun, if he be satisfied that he has got the correct initial velocity and initial direction of the shot. If the calculated and measured ranges did not now quite agree, the remaining correction of κ might be found perhaps with sufficient accuracy by proportional parts. This seems to be the best way of expressing the degree of steadiness of shot when they are so light as to have their motion disturbed by cutting the threads of screens. But for heavy shot the most direct way is to fire through equidistant screens as already explained.

When I made experiments with low velocities in 1879, I was furnished with some range tables of the 6·3-inch Howitzer, where, it was said, my coefficients did not give correct results. As the difficulty in obtaining good results increases as the velocity decreases, I took the trouble to make a thorough comparison between the calculated and experimental results in the above case. I also made use of some German range tables in the same way, where the value of $d^2 \div w$ was only a trifle larger. The results of these comparisons may be found at pp. 45–49 of my Final Report, 1880, which appear to me quite satisfactory.

So long as there are guns good, bad, and indifferent, it is clear that no single set of coefficients can give ranges suited to all cases. But ranges, &c., calculated by the help of my coefficients, may serve as a standard of comparison applicable to every case, and so give a measure of the steadiness of the shot.

April 23 FRANCIS BASHFORTH

The Dry and Wet Bulb Thermometers "Froude"

A SO-CALLED thermometer "froude" has been used in France for many years, and its use, on special occasions, has gradually extended to other countries. It consists of an ordinary thermometer fastened to a string two to four feet in length. If such a thermometer be swung through a circle whose radius is the length of the string, it is evident that it will attain the temperature of a large mass of air unless the results are vitiated by friction with the air, oxidation, centrifugal action, or other causes. Careful comparisons, at high and low velocities, with a thermometer properly exposed, have given entirely reliable results.

Some time ago the writer attempted to use this instrument as a wet bulb with such results that it was decided to fasten two thermometers together, one with its bulb one and a half inches below the other. The latter may be easily wet by immersion without wetting the dry. This arrangement has worked admirably, and has been in constant use the past winter in determining temperature and humidity conditions both in city and country, in courts and rooms. Every one who has undertaken humidity observations with a wet bulb thermometer, especially in cold calm weather, has found it very difficult to obtain good results even after an hour's waiting after wetting. With this instrument it is an easy matter in every instance to obtain an absolutely correct result in two or three minutes. This is shown by the fact that repeated wetting and swinging will give the same result as often as tried. If ice is on the bulb, it is best, after swinging for several observations, or until the clear glare of ice has gone, to wet in water above freezing so as to remove the old ice and form a new coat. Distilled water should also be used if possible.

With this instrument it has been found that, in order to get even approximate results of humidity above grass ground, in clear calm weather, with temperature below freezing, the observation must be made upon a knoll, never in a valley or upon a plain with rising ground on any side. It has also been found that under the above conditions in the morning a height of twenty to thirty feet is essential, even upon a knoll, in order to obviate the effect of the gradual accumulation through the night of damp air above sod. H. A. HAZEN

April 14

Extraordinary Darkness at Midday

THE extraordinary darkness that occurred here suddenly on the morning of the 26th is deserving of record, as being the most intense that is remembered by any of the inhabitants.

The early morning was bright, and no change was noticed until close upon 11 a.m., when the sky became rapidly darker in the west-south-west. The wind was blowing from the north-east at 11 a.m. with a velocity of five miles an hour, and it scarcely changed at 11.40, when it increased to seven miles an hour, and veered at once to south-west, and then moved more slowly round through west and north, back again to east at 1 p.m., when its velocity was only three miles an hour.

At 11.30 the darkness was so great that it was found impossible to read even bold print (small pica) close by the window, and at this time a dense black cloud with a slightly yellowish tinge hung over the south-west sky; the blackness being most intense at 10° above the horizon. At 11.35 it became somewhat lighter, and at 11.40 the rain began to fall, and in forty minutes 0·114 inch of rain-water was collected in our rain-gauges, the whole being almost as black as ink, and full of fine carbon in suspension. Hail that fell a mile off to the south-west by south, and both hail and snow that fell on the hills two miles to the west, were also black. At Preston, fourteen miles to the south-west, the darkness was very marked, but five and a half miles to the north-east nothing very particular was noted.

Stonyhurst Observatory, April 28 S. J. PERRY

Intelligence in Animals

I THINK it was about the year 1844 that the Duke of Argyll desired my late father, his factor, to preserve game in the district of Kintyre, Argyllshire. If any steps in this direction had been taken by other proprietors, they were very irregular. My memory goes back to about 1846 and 1848, and at that time the grouse of Kintyre "sat like stones"—they might be shot to dogs from the first to the last day of the season; in fact it was often difficult to get the birds up. With this preservation, grouse increased enormously—and therefore the food supply of the people—to such an extent that the late Sir John Cuningham and my father shot, on one 12th of August, seventy-two brace of grouse. Sir John was a very old man, and insisted on loading his own gun, an old muzzle-loader. My father never shot hard. Now I do not believe any two men with two guns and loaders could do this in the same district with all the improvements in arms and dogs; whilst I have heard my father say that seven brace was a good bag when he was young, before game-preserving.

Grouse yet sit pretty well in Kintyre, and I believe this is the case because it was one of the last districts to preserve and shoot; but the birds are every year becoming wilder, and now in the month of September it is useless to take dogs on the hill, and for two years we, like others, have had to drive them.

I account for this by an alteration in instinct, and I am as sure as any one can be, from observation and the opinion of competent persons, that it is *progressive instinct in successive generations*. Formerly the great enemies of the grouse were ravens, that took their eggs and young birds; foxes, polecats or martin cats, and wild cats, that took them at *night* on the ground; and hawks, that took them on the wing during the day. When man stepped in and altered the balance of Nature, the

> Bird that up and flew away,
> He lived to breed another day.

No hawk was there to knock him down. He found from experience that flying away before man and his dog came near gave him safety; and his children that inherited the wit or instinct or power of turning heather into nerve-force or intelligent thought—or whatever the straw-splitters like to call it—lived; whilst his brother, that inherited the qualities which kept him hiding in the heather, was shot when forced up.

I had this summer ample corroboration of this theory. About eight years ago I was shooting in the island of Rum; the grouse were not preserved and were extremely tame, so tame in September and October that I had to run after them to make them take the wing, and it was new to dogs. Last year I again shot in the island, and I observed the same tameness in one part of the island, but in another district I observed the grouse were larger, darker, and much wilder. I was puzzled with this until I found out that the late tenant had three years before turned down some English grouse, and in the district where they were so turned down the grouse were very wild.

Knockrioch, April 28 DUNCAN STEWART

IN NATURE (vol. xxix. p. 596) there is a letter signed James Graves, in which he says, "as to the magpie or any other bird being able to fix dates exactly to the day, it is unproved and incredible." I do not know what may be the case in regard to birds-nestbuilding, but I can give two instances of the regularity with which birds arrive at certain localities *en route* northward, whatever may be the state of the weather. During a ten years' residence on the shores of Hudson's Bay, the first Canada goose of the spring migration was seen and generally shot on April 23. At Toronto on Lake Ontario, large flocks of a pretty little plover called the "black-heart," from a black patch on its breast, pass along the islands, flying northward, on St. George's Day (April 23), and are seldom or never seen even a day before or a day after that date. The poor little birds have a sad time of it for six or eight hours, as a number of sportsmen go out for the occasion and knock them down by the half-dozen or more at every shot. In this case, as in the other, wind and weather appear to cause no difference JOHN RAE

4, Addison Gardens, April 25

THE ABSORPTION OF WATER BY PLANTS

AN ingenious instrument has lately been described by Dr. J. W. Moll (*Archives Neerlandaises*, i. xviii.) under the name of the Potometer. It is a modification of Sachs' apparatus for determining the amount of water which a cut branch absorbs in a given time. I have been for some years in the habit of using a form of Sachs' instrument, differing in principle from Moll's apparatus, but resembling it in being especially adapted for making observations at short intervals of time. As the subject of transmission of water through wood is now attracting attention among physiologists, it seems worth while to describe my instrument.

A short piece of thick indiarubber tube is slipped over the cut end of a branch and firmly attached to it by wire ties; the other end of the rubber tube being securely fitted to a glass tube which is filled with water. The other end of the glass tube is closed by an indiarubber cork through which passes a coarse thermometer tube. The apparatus is now fixed so that the free end of the thermometer tube dips into water. As the leaves evaporate the water in the glass tube is sucked up by the cut end of the branch, the loss being constantly made up by a current flowing in through the thermometer tube. If then we can estimate the rate of this current we shall know the rate of absorption of water. This is very simply done by allowing, for a few seconds, the thermometer tube to suck in air instead of water; when a column of air a few millimetres in length has been drawn in, the end of the tube is again immersed in water, and the bubble travels rapidly along the thermometer tube, when its speed is measured by means of a chronograph.

This method appears no doubt to be a rough one, and is open to objections; but I believe that it does not give rise to serious errors, and it certainly demonstrates extremely well small changes in the rate of absorption by a cut end of the branch. By means of my instrument observations can be made at very short intervals; for instance, four readings were taken in 1′ 50″; it is therefore well adapted for observing rapid changes in the rate of absorption.

I reserve a full discussion of the merits and demerits of the instrument for a later publication.

Experiments, April 1884.—When a branch is first fitted to the instrument the rate of absorption is extremely rapid, owing to causes which need not here be considered, but after a time the rate of absorption (which diminishes with great rapidity) becomes constant. A branch of Portugal laurel (*Cerasus lusitanica*) was cut at 9 30 a.m., and was not fitted to the apparatus until 10.15 a.m.

The following table shows clearly the rapid decrease in the rate of absorption:—[1]

Times h.	m.	Rate of Absorption
10	18	71
	20	53
	25	40
11	14	26
	35	25
	52	25

Sachs has called attention to the diminution in the absorption which occurs when cut branches are placed in water, and has shown that the absorbing power can be renovated by cutting a fresh surface at the base of the branch. This effect is well shown with my instrument.[1]

The above-mentioned branch of Portu al laurel which had been placed in water at 9.30 a.m. gave a relative rate of absorption of twenty-four at 12.28; at 12.30, between 6 and 7 cm. were cut off, and the branch was again fitted to the machine, the operation lasting one and a half minutes.

Time p.m. h.	m.	Rate of Absorption
12	28	24
	30	fresh surface cut
	31½	branch replaced
	33	35
	35	30
	39	28
	45	26
	54	26

When the rate of absorption has become constant, any variation in dampness or dryness of the air causes variations in the transpiration of the leaves, and therefore in the rate of absorption. These changes are well shown by my instrument. The following experiment, made with a branch of Portugal laurel shows the amount and rapidity of the increase in absorption caused by exposing the leaves to the sun shining through window-glass:—

Time a.m. h.	m.	Rate of Absorption
10	44	14
	49	14
	55	14
11	0	15
	5	15
	6¼	14
	Blind drawn up	
	7½?	14
	9	20
	12½	27

Thus in six minutes the rate of absorption had nearly doubled. A similarly rapid effect is seen when the sunlight is cut off, when the rate of absorption falls.

Time h.	m.	Rate of Absorption
12	5	33
	10	32
	10½	blind down
	12½	27
	19½	20
	25	19
	29	18

That is, the rate of absorption diminished in the proportion of 100:56 in twenty-four minutes, when the sunlight was cut off. In the same way the effect of opening a window and thus increasing the evaporation for the leaves, is a once visible in increased rate of movement in the bubble.

Cut Stem of Ivy

Time a.m. h.	m.	s.	Rate of Absorption
10	33	0	32
11	0	0	31
	9	0	window and door opened
	10	40	34
	11	30	37
	12	0	37
	18	0	33
	19	0	window and door shut
	21	30	31
	22	30	29
		40	31

[1] In this and the following tables I have not given the actual quantities of water absorbed, merely the relative rates of absorption.

[1] It need not here be discussed whether the particular phenomenon her described is the same as that described by Sachs.

Effect of Disturbance.—The fact observed by Baranetzky, that a small disturbance, such as a slight shake, increases the transpiration of the leaves, is also easily seen, and my observations show that in some cases the effect may be very transitory. It need not occur in the use of my instrument, since the plant remains quite untouched, the only necessary movement being the removal and replacement of a vessel of water to allow the entrance of a bubble.

Effect of Sudden Diminution of the Evaporating Surface.—I have been astonished to find with what rapidity the rate of absorption diminishes when the evaporating surface is diminished by cutting off a twig from the branch under experiment.

A branch of Portugal laurel was absorbing (April 10) with great regularity.

Time p.m. h. m. s.	Rate of Absorption
5 5 0	23
47 0	23
48 40	a small branch cut
49 15	21
50 30	20
52 0	21
58 0	20
6 4 0	20
11 0	20
12 0	another branch cut off
20	18
13 0	17
14 40	16
26 0	16
35 0	16

When the first branch was removed nearly the whole of the permanent diminution was visible in 35″; in this case the distance from the base of the branch to the place where the small branch was severed was 28 cm.

In the second experiment half the permanent effect was produced in 20″; and here the distance of the cut-off twig from the cut end of the main branch was 45 cm.

But far more remarkable results were obtained when a long stem of ivy was used for the experiment.

A stem of ivy was removed from the tree on which it grew on April 13, and was placed with its cut end in water until the following day, when it was fitted to the apparatus.[1]

April 14.—The rate of absorption remained fairly constant from 10.30 to 11.45 a.m., when the first branch was removed.

Time a.m. h. m. s.	Rate of Absorption
10 33 0	32
11 42 0	31
45 0	branch cut off
15	29
46 20	23
53 0	26

The branch which had been cut off left the main stem at a distance of 13 feet 3 inches from the basal end, so that change in the rate of absorption was transmitted at the rate of 0·88 feet per second.

In a second trial the following results were obtained:—

Time h. m. s.	Rate of Absorption
3 1 0	23
6 0	23
11 30	23
13 0	stem cut
15	18
14 0	17
15 30	15
18 0	15

The point at which the stem was severed was 16 feet 5 inches from the basal end, so that here a change in absorption travelled at the rate of 1·1 foot per second. The same result followed on two other occasions, but here the rates of transmission were slower—

15 feet 5 inches in 30″,
9 feet 10 inches in 30″.

In the latter of these two cases the part removed was not a branch of the stem, but a branch belonging to a neighbouring stem which had grown into lateral union with it, so that here the transmission of the change must have taken place laterally from the branch to the stem, and then longitudinally along the latter.

Effect of partly severing the Branch.—My instrument is well adapted for testing the transmitting capacity of a branch which has been partly cut through. I have been much surprised at the fact that cuts to the depth of half or more than half the diameter of the branch produce practically no diminution in the rate of absorption. Indeed a slight increase is often visible owing to the disturbance which must occur when the branch is cut with the saw.

A branch of yew ·55 inch in diameter was sawn (A) to the depth of ·25 inch, (B) so deep that the bridge of *wood* through which the water had to pass was not more than ·1 inch in thickness in radial direction.

Time p.m. h. m.	Rate of Absorption
3 26	99
32	99
34	cut A made
35	98
40	103
47	101
4 3	99
37	99
38½	cut B made
40	100
43	97
46	100
50	100

It was only when the bridge remaining after cut B was narrowed, by sawing on both sides at right angles to the former direction, that the rate of absorption fell. The same thing was shown in another branch of yew, the woody part of which was 10 mm. in diameter, and which was sawn to a depth of 8 mm., leaving a bridge 2 mm. thick in radial direction. Even this deep section did not diminish the rate of absorption. With the Portugal laurel the same thing was observed; and here the possibility of recovery from a temporary diminution in absorption was shown in two cases.

The branch with its bark on measured 11 mm. in diameter, and it was traversed by an excentric cylinder of old brown wood 1·8 mm. in diameter.

The following figures give the result of section:—

Time a.m. h. m.	Rate of Absorption
11 0	18
9	18
14	cut A
16	16
20	18
30	17
12 0	17
6	18
8–9	cut B
10	18
11	19
17	17
30	18
35	cut C
36½	9
38	9
42	10
48	12
55	14
p.m. 1 15	16

[1] The stem was fixed so that its distal end was some few feet above the base immersed in water.

It will be seen that the two first cuts produced no effect comparable to that caused by C. Cut A was 3 mm. deep, cut B was also 3 mm. deep on the opposite side, so that, after B had been made a bridge, 5 mm. in radial direction remained; nevertheless the rate of absorption was undiminished. Cut C was made by increasing B to the depth of 5 mm. from the bark, so that the bridge of more or less central wood finally left was 3 mm. in radial thickness, and even then the diminution was only temporary. The cuts were made about 7 cm. from the basal end, and the same distance from the first branch. Another branch of Portugal laurel showed the same thing. The wood of the branch at the point of section was about a centimetre in diameter, and contained a large proportion of old brown wood. The external envelope of white wood was cut away with the exception[1] of a bridge measuring roughly 3 × 3·5 mm. in cross section.

The result is shown in the following table:—

Time p.m. h. m.	Rate of Absorption
1 34	15
2 13	15
16	cut
19½	10
21½	11
26	13
32	14
39	13
47	14
53	15

Here again we have a diminution followed by gradual rise.

When the little bridge of younger wood was severed, the fall in rate of absorption was rapid.

Time p.m. h. m.	Rate of Absorption
5 27	16
28½	bridge severed
32	0·12
47	0·08

Thus the absorption fell to one-twentieth of the original amount; that it did not quite cease may be accounted for by the fact that the younger circumferential wood was not completely cut through.

My apparatus would be also suitable for such experiments as those of Dufour (*Arbeiten d. Bot. Inst. in Würzburg*, 1884, Band iii.), in which he showed that sharply bending a stem, such as a hop-bind, does not prevent the passage of the water of transpiration, whereas water could not be mechanically forced through the bent stem. Dufour also repeated Hales' experiments in which the transpiring branch was cut half through on two opposite sides, the points of section being an inch or two apart. When this had been done, so that the continuity of all *cavities* of vessels and cells was broken, he found that the transpiration-stream could still pass, because the continuity of the cell-*walls* remained unbroken. I give a single experiment of this kind to show that my instrument is well adapted for such work (April 15):—

Time h. m.	Rate of Absorption
11 32	25
34	25
36	25
37½	first cut
39	22
49	24
55	second cut
12 2	0·84
22	0·69

Both cuts penetrated to the centre of the branch. The first was one and a half inch from the base, the second half an inch below the first cut, and on the opposite side of the branch.

Dufour's experiments would seem to show that the great depression in absorption which occurred on making the second cut may have been only a temporary phenomenon; this and other kindred questions I hope soon to be able to work out. FRANCIS DARWIN

Cambridge, April 17

[1] The young wood was not well severed, and a small amount remained in continuity.

WHAT IS A LIBERAL EDUCATION?[1]

I DO not intend, in the present paper, to enter upon the disputed question between the advocates of classical culture on the one hand, and those of scientific training on the other; because it seems to me that the line on which the two parties divide is not that which really divides the thought of the day. If we look closely into the case, we shall see that the objects of a higher education may be divided into three classes, instead of the two familiar ones of liberal and professional. In fact, what we commonly call a liberal education should, I think, have two separate objects. With the idea of a professional education we are all familiar: it is that which enables the possessor to pursue with advantage some wealth-producing specialty. Although, in accordance with well-known economic principles, it is designed to make the individual useful to his fellow-men, the ultimate object in view is the gaining of a livelihood by the individual himself. On the other hand, the object had in view in what is commonly known as culture is not the mere gaining of a livelihood, but the acquisition of those ideas, and the training of those powers, which conduce to the happiness of the individual. From this point of view culture may be considered an end unto itself.

The third object which we have to consider is only beginning to receive recognition in the eyes of the public. It is the general usefulness of the individual, not merely to himself and to those with whom he stands in business relations, but to society at large. Modern thought and investigation lead to the conclusion, that man himself, the institutions under which he lives, and the conditions which surround him, are subject to slow, progressive changes; and that it depends very largely on the policy of each generation of mankind whether these changes shall be in the way of improvement or retrogression. During the next fifty years all of us will have passed from the stage of active life, and the course of events will be very largely directed by men who are still unborn. The happiness of those men is, from the widest philanthropic point of view, just as important as the happiness of those who now inhabit the earth; and, in the light of modern science, we now see that that happiness depends very largely upon our own actions. We thus have opened out to us an interest and a field of solicitude in which we need the best thought of the time. The question is, What form of education and training will best fit the now rising generation for the duty of improving the condition of the generation to follow it?

Let it be understood that we are now speaking, not of the education of the masses, but of that higher education which is necessarily confined to a small minority. So far as I am aware, that fraction of the male population which receives a college education is not far from 1 per cent. To that comparatively small body we must look for the power which is to direct the society of the future, and by their acts to promote the well- or ill-being of the coming generation. Our duty to that generation is to so use and train this select body as to be of most benefit to the men of the future. What is the training required? I reply by saying that I know nothing better for this end than a wide and liberal training in the scientific spirit and the scientific method. The technicalities of science are not the first object; and, so far as they are introduced, it is only

[1] From *Science*.

as media through which we may imbue the mind with certain general and abstract ideas. If called upon to define the scientific spirit, I should say that it was the love of truth for its own sake. This definition carries with it the idea of a love of exactitude—the more exact we are the nearer we are to the truth. It carries with it a certain independence of authority; because, although an adherence to authoritative propositions taught us by our ancestors, and which we regard as true, may, in a certain sense, be regarded as a love of truth, yet it ought rather to be called a love of these propositions, irrespective of their truth. The lover of truth is ready to reject every previous opinion the moment he sees reason to doubt its exactness. This particular direction of the love of truth will lead its possessor to pursue truth in every direction, and especially to investigate those problems of society where the greatest additions to knowledge may be hoped for.

Scientific method we may define as simply generalised common sense. I believe it was described by Clifford as organised common sense. It differs from the method adopted by the man of business, to decide upon the best method of conducting his affairs, only in being founded on a more refined analysis of the conditions of the problem. Its necessity arises from the fact that, when men apply their powers of reason and judgment to problems above those of every-day life, they are prone to lose that sobriety of judgment and that grasp upon the conditions of the case which they show in the conduct of their own private affairs. Business offers us an example of the most effectual elimination of the unfit and of "the survival of the fittest." The man who acts upon false theories loses his money, drops out of society, and is no longer a factor in the result. But there is no such method of elimination when the interests of society at large are considered. The ignorant theoriser and speculator can continue writing long after his theories have been proved groundless, and, in any case, the question whether he is right or wrong is only one of opinion.

I ask leave to introduce an illustration of the possibilities of scientific method in the direction alluded to. Looking at the present state of knowledge, of the laws of wealth and prosperity of communities, we see a great resemblance to the scientific ideas entertained by mankind at large many centuries ago. There is the same lack of precise ideas, the same countless differences of opinion, the same mass of meaningless speculation, and the same ignorance of how to analyse the problem before us in the two cases. Two or three centuries ago the modern method of investigating nature was illustrated by Galileo, generalised by Bacon, and perfected by Newton and his contemporaries. A few fundamental ideas gained, a vast load of useless rubbish thrown away, and a little knowledge how to go to work acquired, have put a new face upon society. Look at such questions as those of the tariff and currency. It is impossible not to feel the need of some revolution of the same kind which shall lead to certain knowledge of the subject. The enormous difference of opinion which prevails shows that certain knowledge is not reached by the majority, if it is by any. We find no fundamental principles on which there is a general agreement. From what point must we view the problem in order to see our way to its solution?

I reply, from the scientific standpoint. All such political questions as those of the tariff and the currency are, in their nature, scientific questions. They are not matters of sentiment or feeling which can be decided by popular vote, but questions of fact, as effected by the mutual action and interaction of a complicated series of causes. The only way to get at the truth is to analyse these causes into their component elements, and see in what manner each acts by itself, and how that action is modified by the presence of the others: in other words, we must do what Galileo and Newton did to arrive at the truths of Nature. With this object in view, whatever our views of culture, we may let science, scientific method, and the scientific spirit be the fundamental object in every scheme of a liberal education.

S. NEWCOMB

THE KRAKATOA ERUPTION[1]

THE inquiry, instituted in consequence of a Government resolution of October 4, 1883, into the nature, the extent, and the consequences of the volcanic eruptions of Krakatoa, has led to various remarkable results of which a short account is given here. A detailed report is in course of preparation, but will not appear for some months, as the making of numerous illustrative maps and plates will take much time. The inquiry did not extend solely to the islands of the Straits of Sunda, but also to the coast countries of the Lampong districts, Bantam and Batavia, which were partly or entirely destroyed. In the Straits of Sunda the islands of Merak, Toppershoedje, Dwars in den Weg (Thwart the Way), Seboekoe, Sebesi, Lagoendi, Krakatoa, Taboean, Prince's Island, the Monnikrotsen (the Monk's Rocks), and Meeuwen Island (Mew's Island), were visited; further, the coast strip from Ketimbang to Kalianda, and inland as far as Kesoegihan, besides the foot of the Radja Bassa; the coast of Hoeroen to Telok Betong, and the environs of the capital; the southern part of Semangka Bay (the northern part was inaccessible through pumice-stone), the kampoengs Tampang and Blimbing, near the Vlakken Hoek, Java's First Point (Java Head), and the coasts of Tjiringin and Anjer to Merak. The voyage, which lasted seventeen days, was made by the *hopperbarge* (small steamer) *Kediri*, commander 't Hoen, given for the inquiry by the temporary chief of the Batavian Harbour Works. About the causes of eruptions there is usually not much to be said, yet in this case something has been ascertained. Krakatoa, namely, lies with a few other volcanoes on a rent or fissure in the crust of the earth which runs across the Straits of Sunda, and of which I indicated the probable existence for the first time three years ago. Along such a fissure little shiftings of the earth's crust are possible, by which a pressure is exercised upon the molten substances below the crust. It is also possible that along such a rent—however tightly closed by the neighbouring stone-layers—the water may more easily than elsewhere flow to the regions under the earth. If this water comes in contact with the molten substances, steam at high temperature and high pressure is formed, and this steam may be considered as the chief motor of most, if not all, volcanic eruptions.

Many circumstances, therefore, combine to make eruptions take place in preference near fissures, provided water (either rain or sea-water) can penetrate in sufficient quantity. We must conclude from the 200 years' quietude of the volcanoes in the Straits of Sunda that the water affluence during that time was but small, and only became larger within the last years. Now it happens that during the last years a great many earthquakes took place along this fissure, of which the lighthouse on "Java's First Point" in particular suffered greatly. The most violent earthquake took place September 1, 1880; the upper part of the tower was rent, and had to be broken off afterwards. These earthquakes were probably the result of subterranean subsidences, and I think I may assume that through those subsidences modifications took place along the fissure through which the sea-water could ooze in greater quantity than before. Within the three last years the pressure of the steam formed became sufficiently strong to force the lava, out of the much deeper-lying lava strata, upwards through the crater of Krakatoa, and the eruption took place when, at last, the violence of the steam was enabled to force its way through the lava to the crater and the surface. The steam carried with it a

[1] Translation of a Short Report on the Eruption of Krakatoa on August 26, 27, and 28, 1883.

quantity of lava, which was mostly shot as fine dust out of the crater. The porous nature of the ejected substances —pumice-stone was almost the only substance formed—is doubtless to be ascribed to the steam, which was blown with great force through the lava. I must keep the more detailed description of the way in which the eruption proper was prepared and took place for a future time, as drawings are requisite for a right understanding of the matter. I must still observe here, however, that through the Krakatoa eruption our notions about the shape and extent of subterranean regions will probably be much modified. If it may be assumed that there exists a connection between our eruptions, the heightened activity of the volcanoes in the Indian Archipelago since that time, and the earthquakes in Australia which took place simultaneously with and succeeded the eruptions of the last days—in any case a remarkable coincidence—then much larger dimensions will have to be allowed to those regions than the present geologists are accustomed to assign to them.

Krakatoa is the only point which has been active. There are reports that Sebesi and the Radja Bassa have also shown activity, but this is inaccurate.

Of the old Krakatoa there is no detailed survey; the English and Dutch sea-charts, both on a small scale, besides a couple of sketches taken by Buijskes in 1849, and by me in 1880, alone give some idea of the surface-formation of the island. From the two sketches may be seen that the island had three tops—the northern, called Perboewatan (in some reports Roewatan), was the lowest of the three, and showed streams of lava on various sides; this is the point which first began to be active in May 1883, and which probably also ejected pumice-stone in 1860. The centre top bore the name of Danan, and was active also in August 1883. The southern peak, the mountain Rakata properly speaking (which was corrupted into Krakatoa), was by far the highest point of the whole island, and, according to the sea-chart, 822 metres high. This point is also an old crater, but was *not* active in 1883.

On May 20, 1883, the Perboewatan began suddenly to show signs of activity; that nothing was known before then of an approaching eruption must be ascribed to the fact that Krakatoa was uninhabited, and only visited occasionally by Lampong fishermen, who went no further than the coast. Otherwise it would seem inexplicable that no previous signs of it should have been noticed. The eruptions lasted with various degrees of violence, and with intervals, till August 26; while latterly also the crater of Mount Danan became active. Though in themselves not unimportant, these eruptions were insignificant compared to what followed. On August 11 trees were still growing on the peak, so that the destruction of vegetable life was then still limited to the immediate neighbourhood of the craters. On the 26th the explosions much increased in violence, and they reached their maximum on the 27th, at 10 a.m. They then diminished in strength, but lasted still the whole night of Monday to Tuesday, till they suddenly ceased on the 28th, at about 6 a.m.

About the eruptions from May till August 26 little of importance has, on the whole, become known; all I have been able to collect will be mentioned in the detailed report.

The eruptions of August 26 and 27 were accompanied by violent detonations and air vibrations. During those days almost incessantly a rumbling sound was heard which resembled the noise of thunder at a distance, the explosions properly speaking were accompanied by short detonations which can best be compared to heavy cannon shots, but the most violent detonations were still shorter and more rattling, and cannot be compared to any other sound.

The sounds of the explosions in May were heard in a north-west direction at Moeara Doea in Palembang, and at Bintoehan, in the division of Kauer in Benkoelen, respectively 230 and 270 kilometres from Krakatoa; the transmission of the sound on August 27 surpasses, however, all which is known of the kind. The explosions were heard in Ceylon, in Burmah, at Manilla, at Doreh in the Geelvink Bay (New Guinea), and at Perth on the west coast of Australia, besides all the places which lie closer to Krakatoa than the above-mentioned. If a circle is drawn from Krakatoa with a radius of 30°, 1,800 geographical miles, or 3,333 kilometres, the circle will go exactly over the furthest points where the sound was heard. The furthest distance between the points east and west where the sound was heard is therefore 60° (the diameter of the circle) or one-sixth of the whole circumference of the earth. The surface of this circle, or rather of the spherical segment, comprises more than one-fifteenth of the surface of the earth. In historic times no eruption is known of which the sound was transmitted over such an enormous area. At the eruption of Tombora in Sumbawa, in 1815, the radius of the circle within which the sound was heard, was but half the size, namely 15°, the surface being therefore 3·93 times smaller.

If a circle with the same radius, namely 30°, is drawn round the earth, taking Amsterdam as the centre, the circle would have the following course. The northern point lies 82° north latitude, thus north of Spitzbergen; from there the circle runs to the middle of Novya Zemlya, thence along the Ural Mountains to Orenburg, Tiflis, Damascus, Jerusalem, Suez; crosses the Tropic of Cancer at about 15° east longitude from Greenwich, reaches the most southern point at 22° north latitude in the Desert of Sahara, crosses the Tropic of Cancer once more at 5° west longitude from Greenwich, runs close along Ferro, includes the Canary Islands and Azores, besides the greater part of Greenland, and runs back to the starting point north of Spitzbergen. In various places it was observed that the strongest detonations were heard at different hours, and also that in places in the neighbourhood of Krakatoa little or nothing was heard of the sound, whereas it was heard very distinctly in places further removed. Thus for example the loudest report was heard at Buitenzorg at a quarter to seven, at Batavia at half-past eight, at Telok Betong at ten o'clock on the morning of the 27th. This was caused principally by the direction of the wind; it appears clearly from the reports that the sounds were loudest on the side of Krakatoa whither the wind blew, and the fine ash particles were blown. But this does not yet explain the fact that the sound was sometimes better heard in places that were further off than in those that were nearer when those places lay in the same direction from Krakatoa, such as, for example, Anjer, Serang, and Batavia. This phenomenon is to be ascribed solely to the great quantity of ash particles which were present in the lower atmosphere. If one assumes for example the presence of a thick cloud of ashes between Krakatoa and Anjer, this would act on the sound waves like a thick soft cushion; along and above such an ash cloud the sound may be propelled very easily to further removed places, for instance Batavia, whereas at Anjer, close behind the ash cloud, no sounds or only faint ones would be heard. Other explanations, such as by the interference of sound, seem to me less probable though not entirely impossible.

Besides these sound vibrations, very long air waves were formed during the explosions, which did not manifest themselves by any sound, but had nevertheless an important effect. The most rapid of these vibrations communicated themselves to the buildings and walls of rooms, so that objects which hung on the walls or from the ceiling were set in motion. Thus at Batavia and at Buitenzorg, a distance of 150 kilometres from Krakatoa, the doors and windows began to rattle, clocks stood still, ornaments on cabinets fell down, and hanging lamps were thrown out of their fastenings, and fell shattered to the ground with their chimneys and globes.

But not only at this distance was the air vibration perceptible. At Batoe-Radja in Palembang (250 kilometres from Krakatoa) rents appeared in the pradjoerit's barracks at three o'clock in the night; even at Palembang, 350 kilometres from Krakatoa, several Government buildings had to be immediately vacated as a crash was feared; and even in the Alkmaar country in Pasoeroean, 830 kilometres from Krakatoa, the walls were rent in the house of the administrator and the machinist. All this was caused by air vibrations, *not* by earthquakes, for it is a remarkable fact that these have nowhere been observed with certainty in this eruption.

Finally in the most violent explosions, air waves of an astonishing length were formed. As the Meteorological Institute at Batavia no longer possesses a self-registering barometer, those waves would have passed unobserved at Batavia, had they not fortunately been recorded by the indicator of the gas-works. This apparatus is self-registering, and continually marks on a paper, wound round a turning cylinder, the pressure of the gas. As the large gasometer was set in motion on August 26 and 27 by the pressure of the air waves, these oscillations were marked by the indicator, and the line of pressure shows that day, not the normal curves, but a number of sharp points. As hour lines are marked on the paper, the time at which these oscillations occured can be accurately fixed, and if the time be deducted which the wave requires to travel from Krakatoa to Batavia, the moment is also known when the wave was formed, and the explosion took place (omitting a correction for the time which elapses between the moment of pressure on the gasometer and the moment this is recorded by the indicator, a lapse of time which is unfortunately not exactly known). The barometer experiments in Europe and America show that those large air waves possess almost as great a velocity as sound, from which it follows that they require seven minutes to travel the distance between Krakatoa and Batavia.

I have concluded from this that the most violent explosions took place at the following hours:—August 27, 5h. 35m., 6h. 50m., 10h. 5m., and 10h. 55m., Batavia time. By far the most violent of these four was the explosion of 10h. 5m. Then also an air wave was propelled from Krakatoa which spread in a circular form round this point as pole along the surface of the earth, and travelled no less than three and a quarter times round the whole circumference of the earth. The velocity was, as already observed, about the same as that of sound, although these were waves of a gigantic length (the length of the lowest sound waves being about 20 metres, that of the Krakatoa air wave more than a million metres).

The eruptions, which took place at first above the sea, probably became submarine about ten o'clock on August 27. Before that time only more or less damp ashes were ejected, after that also a large quantity of mud or mire, being volanic sand mixed with sea-water. The giving way of the northern part of the mountain must have preceded these submarine eruptions, as appeared from the time at which the large tide wave, which probably originated through this subsidence, overflowed the Vlakken Hoek. This catastrophe caused a great change in the group of islands of Krakatoa. To the north-west of Krakatoa lies Verlaten Island, to the north-east Long Island, and west of the latter lies the Poolsche Hoedje. This small island has disappeared, the two others still exist, and are even larger than before, through the ejected substances which have settled on and around the island; but the greatest change has been undergone by Krakatoa itself. The whole northern part with the craters Perboewatan and Danan, and half of the peak have sunk in the depths. There only remains the southern part of the peak, which has been cut in two from the very top, and forms on the north side a magnificent precipitous cliff more than 800 metres high. Through the downfall therefore a volcano rupture has been formed which is probably unique in the world. A coloured drawing of this remarkable rock will be added to the large report.

The size of Krakatoa was formerly 33½ square kilometres; of that 23 square kilometres have subsided, and 10½ square kilometres remain extant. But on the south and south-west side the island has been increased by a large ring of volcanic products, so that the size of New Krakatoa is now, according to our survey, 15⅓ square kilometres. The size of Long Island was formerly 2·9 and is now 3·2 square kilometres. Verlaten Island has become much larger; it was formerly 3·7 and is now 11·8 square kilometres in size. Of the Poolsche Hoedje nothing remains.

In the place where the fallen part of Krakatoa once stood there is now everywhere deep sea, generally 200, in some places even more than 300 metres deep. It is remarkable that in the midst of this deep sea a rock has remained which rises about 5 metres above its surface. Close to this rock, which is certainly not larger than 10 metres square, the sea is more than 200 metres deep. It is like a gigantic club, which Krakatoa lifts defiantly out of the sea.

The volcanic products of the preceding year consist almost exclusively, as we have already said, of pumice-stone; only here and there among the pale gray material a solitary piece of darker coloured steatite or a vitreous piece of obsidian appears. Although the stone masses in the crater were doubtless liquid, a stream of lava could nowhere pour down, because everything was shot out of the crater in larger and smaller pieces, and generally in powder.

The chemical composition of the ejected substances is not yet sufficiently known, but from the analyses that have hitherto been made it would appear that all the substances do not contain the same quantity of silica; probably that the large pieces floating on the top of the molten mass were somewhat more acid than the lava that lay deeper in the crater and was ejected as powder. The ashes collected by myself at Buitenzorg contained, according to the analysis made at Batavia, 60 per cent; a piece of pumice-stone, collected on the Island Calmeyer, 68 per cent.; a small piece of obsidian from Krakatoa over 68 per cent.; and fine yellow ashes from the east coast of Krakatoa even 70 per cent. of silica. There was found moreover alumina 14 to 16 per cent., protoxide of iron 6 per cent., chalk 4 per cent., soda 4 to 6 per cent., and a little magnesia.

In the microscopic examination of the ashes collected at Buitenzorg there was found—(1) *glass* in innumerable irregular fractured particles, generally completely permeated with vacuoles round or oval; in some particles the glass threads are bent. Those glass particles, microscopic pieces of pumice-stone, are always present in large abundance. (2) *felspar*, very fresh and clear, sometimes with distinct polysynthetic twin lines, generally, however, in single crystals; all seems to be plagioclase, as the analysis shows no potash. As inclusions in the felspar are found glass, apatite, augite, and magnetite. (3) *pyroxene*, partly green and then extinguishing obliquely, therefore monoclinic augite, partly coloured brown, and then, as it appears, extinguishing in parallel lines; it is not yet quite certain whether these last brown augites, which are present in much larger quantities than the green ones, belong to a rhombic pyroxene (bronzite or hypersthene), or are brown monoclinic augites which lie in the preparations on the orthopinacoid. This would be fortuitous; but if the orthopinacoid is much more developed in those crystals than the klinopinacoid, it is not surprising. I also believe I observed in one brown crystal oblique extinction, while sometimes transitions from brown to green tints occur. Inclusions are glass, apatite, magnetite. (4) *magnetite* in grains and octahedra, as the oldest component part. The quantity of magnetite decreases in the ashes in proportion as they fell further from Krakatoa.

If the molten mass had slowly cooled, an ordinary augite-andesite or andesite-steatite (with rhombic pyroxene), would have originated.

The thickness of the ejected substances diminishes on the whole as the distance from Krakatoa increases; the coarser material fell principally within a circle, with a radius of 15 kilometres drawn round Krakatoa, although pieces of the size of a fist were still thrown at a distance of 40 kilometres. Within the circle of 15 kilometres' radius the thickness of the layers of volcanic substances is 20 to 40 metres. At the back of the Island of Krakatoa, the thickness of the ash mountains at the base is in some places even 60 to 80 metres, but diminishes in thickness upwards, so that, in the deep clefts, which have already been hollowed out by the water, the old surface of the mountain and the fallen trees appear.

The thick layers of ashes were cooled at the top at the time of my visit, but were still very hot below, so that in the deep ravines hot water and steam appeared everywhere; also at Verlaten Island, Long Island, the Islands Steers and Calmeyer, and even at Sebesi, steam was seen to escape here and there. At Krakatoa there are, besides, stems of trees which have been carbonised by the hot ashes and continue to smoulder close to the fracture, where the air can penetrate, so that at night a little fire-glow and smoke may be observed. These small fires specially gave rise to the report that Krakatoa was still active.

The ascent of the mountain from behind, on the pumice-stone elevations, is difficult, but possible; the innumerable crevices, into which one must constantly descend, make the climbing up in the great heat and the total want of shade very fatiguing. The ascent may be made from the north-west, close along the rupture till about 20 metres from the top, which, according to our measurements, lies 831 metres above the sea; the surroundings of the top are rent and constantly crumbling away.

Between Krakatoa and Sebesi there is a large quantity of ashes and pumice-stone which has filled up that entire part of the sea, and projects in two places above the surface. To these two points the names of Steers Island and Calmeyer Island have been given. They do not rise more than a few metres above the sea, have much to suffer from the beating of the waves as they only consist of loose material, and will soon be washed away. The sixteen small craters between Sebesi and Krakatoa, reduced in later reports to six or four, have never existed. The smoking volcanic accumulations have been mistaken for active craters which, at first, from a distance was not unlikely to happen.

The finer ashes were blown eastward (east-south-east), to near Bandoeng (250 kilometres from Krakatoa), in a north-north-west direction to Singapore and Bancalis, respectively 835 and 915 kilometres from Krakatoa, in a south-west direction as far as Kokos Island (Keeling Island), 1,200 kilometres from Krakatoa; how far the ashes were projected west, north, and south is unknown; the surface comprises at least 750,000 square kilometres, that is, almost as large an area as Sweden and Norway, larger than the Austro-Hungarian Empire, also larger than the German Empire with Denmark (including Iceland), the Netherlands, and Belgium together, and twenty-one times the size of the Netherlands.

Evidently the prevailing wind-currents, *i.e.* south-east and north-east, have carried the particles along, which causes the outline of the surface covered with ashes to be irregularly curved.

Finer particles still have fallen even beyond this line into the sea, as appears from reports of ships; and the finest of all, mixed with a quantity of vapour, remained a long time floating in the upper air-currents, and, propelled by the wind, have made a journey round the world. The vapour was condensed to water, and froze in the cold currents; the refraction through the innumerable ice crystals caused the beautiful dark red glow which was observed the last months in so many places in Asia, Africa, Europe, and America; while the ash particles partly obscured the sunlight, or gave the sun blue and green tints at its rise and setting.

If one considers that the volume of the solid ejected substances already amounts to several cubic kilometres, and that the volume of ejected gas substances was perhaps hundreds of times as large, the hypothesis of a cosmic ice cloud to explain the air phenomena seems to me quite superfluous.

That the ash particles, as a matter of fact, were carried very far in the upper air-currents, has already appeared from snow which fell in Spain, and rain in the Netherlands, in which the same components were found as in the ashes of Krakatoa; and that the particles must moreover have been projected *very* high at the last eruption may be concluded from the report that, on the 20th May, during one of the first eruptions, the steam cloud—according to the measurements taken on board of the German man-of-war, *Elisabeth*, which left Anjer that morning at nine o'clock—must have reached a height of at least 11,000 metres. During the much more violent explosions of August 26 and 27, the height, if the above report may be relied on, may very well have reached 15 to 20 kilometres.

I found that on calculating as accurately as possible the quantity of ejected solid substances, they reached 18 *cubic kilometres*. In doubtful cases the lowest figure was always selected, so that 18 kilometres may be too low, but not too high, a computation. The possible margin amounts in my estimation to not more than 2 or 3 cubic kilometres.

However large a quantity this may be, it does not nearly reach that which the Tombora produced in 1815, and which Junghuhn estimates at 317 cubic kilometres; this computation, however, rests on but few data, so that in my opinion a quantity of 150 to 200 cubic kilometres will come nearer the truth. But even in that case the number is eight to eleven times larger than ours, which is not astonishing, as at that time at Madura, a distance of more than 500 kilometres from the Tombora, the sun was totally obscured *for three days*, whereas the darkness here only lasted *a few hours*.

Of these 18 cubic kilometres, which represent a weight of more than 36×10^{12} kilogrammes, no less than 12 cubic kilometres, or two-thirds of the whole ejected quantity lies within the circle with a radius of 15 kilometres drawn round Krakatoa. As the sea between Krakatoa and Sebesi was not deeper than 36 metres, and the thickness of the volcanic ejections amounts to almost the same, the navigation there has become quite impossible. A little further the thickness diminishes considerably. From 15 to $22\frac{1}{2}$ kilometres from Krakatoa, the average thickness amounts to no more than 1 to $1\frac{1}{2}$ kilometres; within this ring lies Sebesi, which now only presents a heap of ashes, with a few projecting stumps of trees; nothing here remains of the four populated kampoengs which formerly stood on the plain opposite the small island Mengoenang (Huisman's Island), all has been washed away, and is covered with a layer of ashes 1 metre thick. From $22\frac{1}{2}$ to 40 kilometres, the average thickness of the ashes amounts to 0·3 metre, then to 50 kilometres 0·2 metre. At a still greater distance from Krakatoa the thickness speedily diminishes to 2, 1, and half a centimetre, but the finer the ashes become the more the direction of the wind is perceptible. An "ash map" will be added to the detailed report.

One more very remarkable phenomenon during the eruption was the formation of powerful sea waves, which flowed over the low-lying coast districts of the Straits of

Sunda, and destroyed a number of kampoengs and more than 35,000 lives.

There is much uncertainty about the time at which these waves originated, and this is not surprising. The number of Europeans who witnessed the catastrophe on the coast places in the Sunda Straits and on board ships was but small; besides, most of them were in a state of great alarm, so that observations with a timepiece were exceptional. Most of the estimates of time were no more than a rough calculation, which, especially after the darkness set in, were not very trustworthy. This explains what came under my notice, that a time computation of the same event in the same place by two different people showed a discrepancy of an hour and a half.

Besides, a remarkable fact must be taken into account, namely, that the largest wave, the only one which spread great distances along the north coast of Java and to the south-west, and which surpassed all other waves by far in height, was almost seen nowhere; at Tjiringin alone this wave was seen to approach before the darkness began, and this was about ten o'clock on the morning of the 27th. Anjer was already destroyed at 6 a.m., and then abandoned. At Telok Betong, and in the lighthouses on the Vlakken Hoek, and at Java's First Point, the wave was not seen because it was pitch dark. Even in the lighthouse, 40 metres above the sea on Java's First Point, nothing was seen of the wave, and the destruction of the coast country was only discovered the next morning when it became light.

As the great darkness at Bantam set in soon after the great detonation of 10h. 5m.—the same explosion which gave rise to the great air wave—and as the wave had only time, before the darkness set in, to reach the neighbouring Tjiringin, which lies 47 kilometres from Krakatoa, this tide wave cannot have arisen much before 9h. 50m. or 9h. 55m. At the Vlakken Hoek, 103 kilometres from Krakatoa, it appeared at about 10h. 30m., which agrees with our time computation, if it be taken into account that the velocity of the waves towards the Vlakken Hoek must have been greater on account of the greater depth of the sea than towards Tjiringin.

It is very probable that shortly before ten o'clock a subsidence of the hollow crater walls of one or both of the active craters took place, that through this the water gained access in large quantities, and that then half of the peak, which had been *previously undermined and fractured by the eruptions*, also disappeared in the depths. The cause of the great wave motion must no doubt be sought for in the subsidence of the peak. Of the northern part of the island, after the many eruptions, not much more than a hollow shell can have remained, the subsidence of which could not have caused waves of great importance; nor could the rush of the water produce great waves, but rather a suction towards Krakatoa, and this may be the cause of the water on the coast *first* retreating in various places before the great flood advanced.

The peak itself, however, was still massive, and I have calculated that the part which fell of this mountain alone, without Danan and Perboewatan, possessed a volume of at least 1 cubic kilometre. If this cubic kilometre be suddenly plunged into the sea, the same quantity of water must be displaced, which must give rise to a circular wave round Krakatoa.

There have been, however, other smaller waves; one as early as Sunday evening, August 26, at 5h. or 5h. 30m., two more in the night, and on Monday morning at 6h. a wave which destroyed Anjer. It is difficult to account for the small waves by assuming that parts of the mountain gave way, because, if so, probably the sea would have gained access also, and mud eruptions would have occurred much sooner, unless it be supposed that mud was ejected, but nowhere far enough to reach inhabited places, which is not quite impossible.

If no subsidences of the mountain had yet taken place, there only remains the assumption that these waves were caused by the enormous masses of ejected matter falling into the sea. As has been said above, 12 cubic kilometres of stones and ashes are lying close round Krakatoa; this quantity has been thrown there from May 20, but certainly, for the greater part, during the violent explosions of August 26 and 27. Assuming that, for instance, about 1 cubic kilometre of these substances was thrown into the sea at a time, waves must of course have been the result, which, as I have calculated, must have reached a considerable height.

The large wave of ten o'clock specially ran up a great height against the precipitous cliffs of the Sunda Straits, according to our measurements as follows:—15 metres up the lighthouse at the Vlakken Hoek; at Beneawang (Semangka), uncertain; at Telok Betong, before the house of the Resident, 22 metres; at the Apenberg (Goenoeng Koenjit), 24 metres; at Kalianda, up a sloping plain, 24 metres; on the south side of Thwart the Way, ± 35 metres (not measured); on the south side of Toppershoedje, 30, on the north side, 24 metres; at Merak itself the height cannot be ascertained with certainty, the old house of the engineer stood only 14 metres above the sea; about 2 kilometres south of Merak, 35 metres; north of Anjer, on the coast opposite Brabandshoedje, 36 metres. The height, therefore, varies everywhere, and depends on the situation of the places, their distance from Krakatoa, their being more or less sheltered, and the steepness of the coast. At Sebesi there is no trace left of the tide wave, as everything is thickly covered with ashes, which fell after the wave. At Seboekoe the height amounts from 25 to 30 metres, but no measurement was taken.

The big wave which was propelled from Krakatoa at about 9h. 50m. spread over great distances, among others as far as Ceylon, Aden, Mauritius, Port Elizabeth in South Africa, and even to the coast of France. The velocity of the waves varies greatly, of course, since it increases with the depth of the sea; I shall not be able to give a detailed summary till the tables of all the self-registering tide apparatus shall have been collected. For the Indian Archipelago, and a few points beyond, I found the following numbers:—

Places	Velocity per hour in miles	Average depth of the sea in metres
Island Noordwachter	37	37
Tandjong-Priok (Batavia)	36	35
Undeepwater Island	33	29
Dendang (Billiton)	31	26
Tandjong-Pandan (Billiton)	32	27½
Tjilamaja (Krawang)	31	26
Oedjoeng Pangka (near Soerabaja)	29¼	23
Pasar Manna (Benkoelen)	113	344½
Padang	109	320½
Mauritius (Port Louis)	(364)?	(3575)?
Port Elizabeth	306	2526

In our Archipelago the velocity is small, owing to the shallowness of the sea, but in the deep sea, on the route to Mauritius and the Cape, it increases considerably, *i.e.* amounts to more than 300 knots an hour, a velocity which is alone to be compared to that of the lunar tide wave and the earthquake waves of Simoda, in Japan, of December 23, 1854, and of Tacna, in Peru, of August 13, 1868.

From the velocity of the wave the average depth of the sea between the places along its path can be determined; I have put together in the foregoing table those various degrees of velocity, but they can only be trusted when the height of the wave is small with respect to the depth of the sea, which in our Archipelago is not strictly the case. However the numbers agree pretty well with the sea-chart. In the time computation of Mauritius there appears to be a mistake, as the average depth cannot be so different from that of Port Elizabeth.

After these terrible events Krakatoa slowly calmed down, not however without having still violently roared in the evening and night of August 27 to 28. The detonations were scarcely less strong at Buitenzorg from ten to one o'clock than in the morning. But after the 28th nothing more was heard of the mountain. The tidal registrations at Tandjong Priok exhibit still a few small oscillations till August 30 at twelve o'clock in the day, but after that the condition of the water also became normal. Notwithstanding this I found that there must have been a serious eruption a considerable time after August 28, and shortly before I visited the island.

On October 11 I left Batavia with my staff, and after having visited various points in the Straits of Sunda, arrived at Calmeyer on the 15th. We stopped a few hours in order to survey the island, which is a perfectly bare bank of pumice-stone, divided into seven parts by encroachments of the sea; the temperature was 42° Celsius, a heat which almost stupefied us. Here already my attention was drawn to the fact that the white or pale gray pumice-sand was covered by a 0·2 metre thick layer of darker coloured very fine ash, which exhibited numerous fissures on the surface, produced in the process of drying, and therefore had probably fallen there as wet mud. I did not, however, then attach any special importance to this phenomenon.

On the 16th I arrived at Krakatoa and remained there till the 18th. When in surveying the mountain on the 17th we had climbed to the top, and began the steep descent on the south side, I observed with astonishment that on the ordinary gray pumice-stone material two black streaks were visible, which began 600 metres above the sea, therefore about 200 metres lower than the top, and could be traced in a tolerably straight line over a length of 1300 metres till 100 metres above the sea. These black streaks proved to be two mud streams, which had flowed down the slope of the mountain and had covered the white pumice-stone to the thickness on an average of 0·2 to 0·3 metre and a breadth of 1 to 5 metres. The most remarkable fact was, however, that these mud streams were not only traced down the back of the mountain but had also flowed into the deep ravines of pumice-stone material, as can be distinctly seen. Therefore those mud streams did not arrive there till the crevices in the pumice-sand already existed, and as several weeks must have been required for the water to hollow out these ravines the mud eruption cannot immediately have followed the eruptions of August. The very fine dark gray mud was still damp at the time of my visit, and could be kneaded with the hand, which also proves that the streams were of recent origin.

In this eruption very curious objects were ejected, *i.e.* very smooth, round balls resembling marbles, to the size of 1½ to 6 centimetres in diameter. They are full of acids, they contain 55 per cent. carbonate of lime, 26 per cent. silica, 11 per cent. alumina, and 5 per cent. water. These calcareous lumps of marl must be derived from layers of marl which exist at the bottom of the Straits of Sunda in the neighbourhood of Krakatoa, and the slime or dust of which has been shot out of the crater in a rapid revolving motion. The balls, which are rare, are never found inside but only on the top of the pumice-stone dust, generally half sunk in the sand; they evidently belong to the last ejections. Whether the mud streams also contain lime I have not been able to ascertain, as a piece brought as a sample has unfortunately been lost.

The last mud eruption, which must have been very important, since on Calmeyer, 12 kilometres from Krakatoa, the upper black layer is 0·2 metre thick, and the mud must have been thrown over the top, which is 830 metres high, to the back of the mountain, whence it poured down, probably took place only six days before my arrival, namely on October 10, at about 9h. 30m. in the evening, because on that evening at about ten o'clock a considerable tide wave arrived at Tjikawoeng in Welkomstbaai (Welcome Bay), the only tide wave which was observed since August 28. A rumbling sound in the direction of Krakatoa was then heard in that place, as well as a little more northward at Soemoer. The wave overflowed the shore to a distance of 75 metres beyond the tide-mark at Tjikawoeng, but has not been observed at other points of the coast, as the devastated coast country was not yet inhabited and was quite abandoned at night. We find in this another proof that the falling down of large quantities of ejected substances round Krakatoa suffices to form important waves in the Straits of Sunda.

The eruption on October 10 seems to have been the last. But this eruption was scarcely noticed, and it is therefore possible that subsequent feebler volcanic actions may have remained quite unobserved. When I visited Krakatoa there was nowhere any sign of activity. On October 18 we left Krakatoa, and we arrived on the evening of the 19th at Vlakken Hoek, where nothing was noticeable. It is not likely, therefore, that the rumbling sounds which were heard that evening at Tangerang and Mauk coming from the west should have proceeded from Krakatoa. It would, however, be very interesting to visit Krakatoa once more in order to be able to trace whether any more changes have taken place since October 18.

Though there is no fear of any serious eruption of Krakatoa after the terrific activity of the volcano and the subsidence of the greater part of the island, still much that is interesting may be learnt yet from less important subsequent volcanic actions, as we see in the instance of the lumps of marl.

With the detailed report a large map of Krakatoa will appear, as well as maps of Calmeyer, the devastated parts of Merak, Java's First Point, Sebesi, Seboekoe, Telok Betong, and Kalianda; moreover tables indicating the pressure at the gas-works at Batavia, and of the self-registering tidal apparatus at Tandjong Priok and at Soerabaja; a small "ash map" and other supplements, and finally a few coloured drawings of Krakatoa and the devastated districts, where in a few moments tens of thousands of people lost their lives on the memorable 27th of August, 1883. R. D. M. VERBEEK

Buitenzorg, February 19, 1884

THE LATE MONSIEUR DUMAS

AT the funeral of this eminent chemist addresses were given by the representatives of various official bodies. From these we subjoin the following extracts, affording as they do an idea of the estimation in which M. Dumas was held by his contemporaries, and of the position to which he is entitled in the science of the present century.

M. le Comte d'Haussonville, Director of the French Academy, said:—

"Who was more worthy than Jean Baptiste Dumas of the high distinctions conferred on him by the Academy of Sciences! By us he was welcomed at a time when his name already ranked amongst the most illustrious of our times, when he had already been hailed as a master by associates destined soon to become masters in their turn. To their authoritative voice, rather than to me, must belong the duty of recording the signal services rendered to science by our regretted colleague, whose mortal remains lie at our feet. They will tell you with a fulness far beyond my power how, under the first inspiration of his soul, he understood how to vary his experiments and verify his assumptions. And with what supreme delight, says one of our *confrères*, who had the honour to receive him into the Academy, he pierced with eagle-eye into the depths of the divine laboratory, beyond which there is naught but the infinite, the unfathomable, the unapproachable! Speaking of his own work, he himself thus expresses himself:—'Above the sphere of phenomena which we study, and where such a vast field of discovery still lies open before us, there is still a higher sphere inaccessible to our methods. We begin to understand the life of bodies; that of the soul belongs to another order.' My years, near enough to those of the venerable seer by whose death we are overwhelmed with grief, enabled me to assist at one of M. Dumas' earliest triumphs. It was before the year 1848, when in his official capacity he ascended the tribune of the House of Deputies in order to expound the whole mechanism of minting in connection with a law then under discussion. Notwithstanding the dryness of the subject, I still remember how we remained for two hours captivated by the charm of his natural eloquence. In taking leave of so great a memory, permit me to repeat the glowing words recently uttered by M. Dumas himself on the occasion of the death of his distinguished colleague, M. Regnault: 'The Academy, faithful interpreter of posterity and sole heir of your renown, hastens to render a public homage of affection to your person, of thanks for the great and noble work of your life, of respect for your brilliant services, awaiting the time when science and your country shall pay their debt to a name worthy of every honour.'"

M. J. Bertrand, Perpetual Secretary of the Academy of Sciences, said:—

"M. Dumas has been our universal teacher. His lectures at the Athenæum, at the College of France, at the Central School, at the School of Medicine, the Faculty of Sciences, and Polytechnic, had so many attractions, he understood so well how to inspire his audience, he indicated the path of progress so clearly, and made each discourse so finished and perfect in itself, that all alike withwithdrew resolved not to miss the following lecture.

"In the history of reformed chemistry no name will assuredly eclipse that of M. Dumas. Eager to disseminate his ideas, skilful in placing his proofs in a clear light, his wise and lofty intellect surveyed from a high standpoint the main routes of science, acting ever as a faithful guide to all who, younger than himself, considered that they honoured themselves in proclaiming him their master."

M. Rolland, as President of the Academy of Sciences, naturally entered into some detail on the scientific work of M. Dumas. Among other things he said:—

"The scientific work of J. B. Dumas is immense, and his labours have long shed a lustre on his name. In his thirty-second year he had already joined the Academy of Sciences, of which he subsequently became one of the most eminent and respected members. I cannot attempt here to mention all the numerous discoveries due to his genius, by which he has so potently contributed to the establishment of modern chemistry, herein showing himself the worthy successor of Lavoisier.

"I will therefore restrict my remarks to the second period of his career, during which, as Perpetual Secretary of the Academy of Sciences, he enabled us better to appreciate his subtle and lofty intellect, his profound knowledge of men and things. Hence his authority was unanimously recognised by his colleagues, whose councils and labours were so often controlled by him with admirable tact and prudence under peculiarly delicate circumstances. If to these rare gifts be added a fluent speech, a kindly and sympathetic feeling from which he never departed, it will be understood how highly prized was the combination of these exceptional qualities, how valued by the Academy, where Dumas so often played the part of guide and director.

"These eminent virtues had long been esteemed and utilised by other societies also, such as those of the Central School, of the Friends of Science, and others, over whose labours he had presided for many years. He was also intrusted with the presidency of several international Commissions, where were discussed many important questions in connection with a uniform system of weights, measures, and currency, as well as with the determination of electric units. There might perhaps be reason rather to regret the manifold occupations and public duties which absorbed so much of his time, diverting him from the prosecution of purely scientific researches, where so much might still be hoped from such a powerful genius. For, in spite of a long life devoted to incessant work, our *confrère* had to the last preserved his strong intellect and mental activity.

"But however great and varied were the labours constantly claiming his attention, he never neglected his more personal duties. His was the life of a true patriarch, ever encircled by his children and children's children, who cherished his fair name and ever rejoiced in the constant solicitude of a tender and devoted father.

"Dumas had married the daughter of Alexander Brongniart in the year 1825. All who, like myself, had the privilege to be welcomed in that happy circle, can testify to the intimate and devoted character of their union down to these last days. I may here be permitted to express to the bereaved widow our warmest sympathy in the loss which to-day deprives her of the affectionate support of such a well-beloved husband.

"But the time has come to bid a last farewell to the mortal spoils of our illustrious colleague, whose memory shall ever remain engraved on our hearts, whose name is eternally enrolled amongst those of the great thinkers by whom the nineteenth century has been most honoured."

Following M. Rolland came M. Wurtz, who spoke on behalf of the Faculties of Science and Medicine.

"To those already deposed on the remains of M. Dumas, the University," M. Wurtz said, "adds other wreaths in supreme homage to the teacher by whom we have all profited, to the *savant* who has shed a lustre on our times, to the worthy citizen who has left a void in the hearts of all. Ours is a public mourning, and above the voices which we hear around us I seem to hear the great voice of France, which in Dumas suffers an irreparable loss. For a period of sixty years he served her with distinction under the most varied circumstances.

"To a piercing genius, an intuition leading to great discoveries and broad views of the universe, Dumas added the choicest gifts of eloquence, of a clear and graceful style, gifts which make the orator and the writer. He was the ideal of a French *savant*, and history will award him a place not far removed from that of his admired master, Lavoisier.

"Born at Alais in 1800, he began life as a chemist's assistant in Geneva. But he was scarcely twenty years of age when, jointly with Prévost, he published some researches on various physiological subjects, and notably some experiments on the blood, which have held their ground to the present time. After his arrival in Paris in 1821 he devoted himself exclusively to chemistry, and soon felt himself competent to undertake such grave work as the independent development of organic chemistry, and the reform of mineral chemistry. And if during the last fifty years chemistry has broken new ground, and become, so to say, transformed under our eyes, this has been accomplished in virtue of a programme he was the first to trace, and the foundations of which were laid by his own discoveries. The ideas at that time current had been drawn from the relatively simple study of mineral compounds. All combinations, it was assumed, are formed of two direct elements, themselves either simple bodies or compounds in the first degree. This so-called 'dualism' in chemistry, traceable to Lavoisier, had been adopted and developed by Berzelius, but was overthrown by Dumas. Studying in 1834 the action of chlorine on organic compounds, he detected in this simple body 'the remarkable power of replacing hydrogen atom by atom.' Such was the first announcement of a law which, supported by thousands of analogous cases, now forms the point of departure for the theory of substitutions and its consequences, associated with the name of Dumas. This conception was developed in a series of memoirs dealing with chemical types, and was later on generalised and simplified by Charles Gerhardt.

"Dumas' studies embraced every branch of the science—discovery and description of mineral and organic compounds, analysis of numerous substances and improvement of the methods of analysis themselves, determination of atomic weights. With the penetration of inventive genius he introduced into all his researches that firm grasp of the subject, that accuracy in details, that critical spirit which are the essential conditions and necessary instruments of all scientific investigation.

"And how shall I speak of his theoretical views expressed on a great variety of special subjects, and embodied either in his great 'Traité de Chimie Appliquée aux Arts,' or in his admirable 'Leçons de Philosophie Chimique'? Merely to mention one point, to Dumas we are indebted for a first attempt at a classification of simple non-metallic bodies, an attempt which has still its value.

"Let me also remind you that, after enriching physiological chemistry at the outset of his career, he soon after endowed physics with a new method for determining the densities of vapour, in continuation of the work begun by his master, Gay-Lussac.

"But a complete idea of his influence and authority cannot be had without reference to his career as a teacher. On his arrival in Paris he opens a course of lectures at the Athenæum. Later on he founds, jointly with Lavallée, Ollivier, and Péclet, the Central School of Arts and Manufactures, where he conducts the chemical class for a quarter of a century. In 1832 he replaces Thenard at the Polytechnic, and the same year is appointed Assistant Professor to the Faculty of Sciences. In 1841 he becomes at once Titulary and Dean of the same Faculty, having three years previously obtained the Chair of Organic Chemistry in the Faculty of Medicine. It was here perhaps that his talents as a teacher achieved their greatest triumphs. He was at that time at the most brilliant period of his creative genius, and he set forth the great ideas then animating him with sympathetic warmth and persuasion, with inimitable clearness and wealth of illustration.

"Such, in a few words, has been the preponderating part played by M. Dumas in science and instruction. And although during his last years he withdrew from public life, it was only to devote himself to work of another order. He was equal to every undertaking imposed on him, the soul of the many committees over which he presided, the ornament of the Academic celebrations which he honoured by his presence and addresses. And after such a long and glorious life what remained except a peaceful end in the midst of his family circle, and in the full enjoyment of all his faculties? But such a commanding figure cannot pass into forgetfulness. Your memory, Dumas, shall be perpetuated, your name transmitted from age to age. You shall live in your works, in the example you have given, in the immortal productions and rare qualities of your mind: *Forma mentis æterna*."

THE EARTHQUAKE

EARTHQUAKES are so rarely observed in England, that an exceptional interest attaches to that of April 22, an interest far in excess of that due to its intrinsic importance. Fortunately the earthquake is exceptional in another sense. It is seldom that a shock results in so small a loss of human life in proportion to the damage done to houses.

The daily London press, for a few days after the occurrence, gave much information as to the range of the earthquake, and the nature and amount of the damage done; further details are given in the local papers of the Eastern Counties, but we are still sadly in want of definite statements upon many matters of great importance. In this article we shall notice only a few points of interest, reserving for a later issue, it is hoped by the aid of fuller knowledge, a more complete account of the phenomena, to be illustrated by a map showing the area of disturbance.

The shock was most severely felt near the north shore of the estuary of the Blackwater, and for about six miles inland to the north, in the direction of Colchester. The geology of this district is simple. Nearly all the country is occupied by London Clay; over the marshy land of the Colne, and the flats separating Mersea Island from the mainland, there is a covering of recent alluvial deposits; over parts of the higher land of Mersea Island there are patches, from a quarter of a square mile to one square mile in area, of Glacial gravel, the remnants of a great sheet of similar material which once overspread the London Clay and joined the large area of similar gravel near Colchester. This town is mostly built on gravel, which rises to a greater height, and occurs in considerable thickness, to the south-west of the town—over Lexden and Stanway Heaths; further to the west and south-west this gravel passes under Boulder Clay. Underlying the whole of the Tertiary beds of the east of England there is a continuous bed of Chalk, from 600 to 1000 or more feet in thickness. Below the Chalk there is a bed of Gault Clay of varying thickness. But here our certain knowledge of the geological structure of the country ends. Rocks of Silurian, Devonian, or Carboniferous age have been proved at various points under the east of England—at Harwich, Ware, Turnford, Tottenham Court Road; rocks of probably Triassic age have been found at Crossness and Richmond. Still further west and north-west the older rocks have been proved at Burford and Northampton. Over Central England the Jurassic and Triassic rocks cover a wide area, but from beneath these the older rocks appear in numerous places.

One of the most interesting questions connected with the recent earthquake is to ascertain whether there be any relation between the known range of these older rocks and the range of the earthquake over areas far distant from its central spot. At first it seemed certain that such was the case. The shock was plainly felt at Bristol, Wolverhampton, Birmingham, and Leicester—all places on or near to the outcrop of the older rocks. Numerous intermediate localities have since been mentioned, many not being connected, so far as we yet know, with the near existence of older rocks; but the far distant places still make it probable that some such connection exists.

It seems therefore likely that the wider and more general range of the earthquake is connected with the range of the Palæozoic rocks, whereas the local phenomena depend very largely upon the nature and thickness of the Secondary and Tertiary rocks. It is therefore important that those who study in detail the effects of the earthquake on the spot should do so with the aid of the Geological Survey Map of the district, which was surveyed by Mr. W. H. Dalton. The map and explanatory memoir are both published; in them the nature of the drift deposits are fully explained.

Almost all earthquakes have a very striking effect on springs and wells, sometimes causing a permanent change, at other times having merely a temporary influence. It is somewhat remarkable that so little has been recorded upon this point. A strong spring at West Mersea, which issues at the base of the Glacial gravel, where this bed rests upon London Clay, is said to have ceased to flow for a short time, and to have been discoloured when the water returned. Any residents in the district who have the opportunity of inquiring into similar cases, which doubtless occurred, will do good service by noting the facts.

Dr. J. E. Taylor's letter, which appears elsewhere, contains much valuable information, such as might well be collected from neighbouring areas; his observations as to the twisting of chimneys, &c., and as to the direction in which that twist occurred, is a case in point.

Information is also wanted as to the angle at which the shock emerged from the ground at various points around the central area, in order that the depth from the surface at which the shock originated may be known. There should be no difficulty in collecting data for this in a district where so many buildings are cracked and shattered. The direction of the cracks, and the angles which these cracks make with a horizontal line, should be carefully noted.

Another point upon which much uncertainty at present exists is the direction in which the wave travelled from its point of origin. The swinging of chandeliers, the swinging of pictures on certain walls and not on others, pendulums which stopped or not according to the direction of the swing, are all important helps towards deciding this question. Of course it is always dangerous to seek for knowledge of this kind some time after the event, but in many cases it may be possible to speak with absolute certainty of the facts.

Some observers speak decidedly of two distinct shocks; this probably was the case frequently, though seldom noticed. The rumbling sound so frequently accompanying earthquake shocks was in many cases noticed in Suffolk and Essex. It is rarely mentioned elsewhere, but is said to have been heard at Chelsea, Reading, and Bristol.

As regards the actual area affected by the shock, there is perhaps much yet to learn. It is recorded along the south side of the Thames from Herne Bay to London, and again at Reading. It was felt at Maidstone and Croydon, and again along the south-east coast from Hastings to Portsmouth and Ryde. But at present we know of no observations in the central parts of Kent, Surrey, or Sussex. W. TOPLEY

WE have received the following further communications in reference to the earthquake :—

As all facts connected with the earthquake shock on Tuesday may prove of more or less value, I beg to communicate the following. The house which I occupy is situated in the centre block of buildings constituting Inverness Terrace, on the western side. Under this block of houses runs the Underground Railway, but a distance of one hundred paces from my house. During the daytime the passage of trains is wholly unperceived, but during the night, when heavy luggage-trains run, a very perceptible vibration is experienced, and in the stillness a distinct rumbling is heard. On the morning of Tuesday I was engaged reading, when my attention was called to what I supposed to be the passage of a train ; but the peculiarity of the motion speedily undeceived me. The sensation was that of being borne rather on water than on solid earth, and as I had already had experience of an earthquake shock in India, I suspected that this disturbance I was feeling was of the same nature. I immediately looked at my watch and noted the time as being thirty-two minutes past nine o'clock. As no one of the other three inmates of the house had perceived anything unusual, I thought no more about the matter until I saw the announcement in the evening papers of what had happened. I then went to the watchmaker's and found that my watch was just fifteen minutes too fast. I am therefore able, with fair approximation to accuracy, to fix seventeen minutes past nine o'clock as the time at which the vibration ceased at this point. W. C. B. EATWELL

69, Inverness Terrace, Kensington Gardens, W., April 24

ON Wednesday morning last, the day after the earthquake, I determined to start upon its track. In Ipswich here, little or no visible harm has been done ; but no sooner had I arrived at Colchester and commenced to walk through the town, from the chief station to the Hythe, than abundant evidence of the ruin wrought by it was visible. Chimneys were totally thrown down, and the brickwork had crashed through the frail roofs. Others were standing, but they looked as if they had been struck by lightning. Their upper parts were splintered and laterally expanded. I could not help noticing that nearly all the houses whose chimneys were wrecked were the oldest—hardly any of the modern, cheaply-built cottages being affected, contrary to my expectation.

At Wivenhoe I found the appearance of the town best expressed by the remark already made : "It looked as if it had been bombarded." That was the first idea which rose in my mind.

Hardly a house was untouched, inside or out. The *newest* houses seemed to be externally least affected, but they made up for this inside. They looked as if they had been given a few half turns, and then shaken up. The plaster had been detached from all the walls, the roofs were rent and loosened all along the cornices, and the framework of the windows was everywhere splintered or free. The battlements of the grand old church had been thrown down, and about fifteen tons of rubbish lay among the crushed headstones and the delicate and abundant grave flowers. Here again there was evidence of a semi-rotatory motion on the part of the earthquake. The beautiful Independent Chapel is so utterly wrecked within and without that it will all have to come down. The streets were full of bricks, mortar, and tiles, although with characteristic English tidiness and diligence the terror-stricken inhabitants were already clearing away the debris. I noticed several houses with rents at the bases of their walls, and in such of the chimneys as remained standing they were frequent. One thing struck me, the rents sprang at an angle of about 30° at the bases of the buildings, whilst in the chimneys this was increased to from 40° to 45°.

The old ferryman related his experience after the manner of an old salt. He was just bringing his boat to the shore when the shock occurred—"it seemed just like *three seas*," he said—a capital and vivid expression to convey an idea of the wave-motion.

Crossing the river I made my way through Fingrinhoe village, and on to Langenhoe. I did not see a single house on the road, large or small, for a distance of about four miles, that had escaped untouched. The fine old Jacobean hall at Fingrinhoe has lost the upper part of the western side of the front elevation. Here I found some of the chimneys that had been left standing *twisted* on their pediments. I carefully noted this on the way, and on examining those of the massive chimneys of the rectory at Langenhoe, the torsion was very plainly visible. The twist had come from the south, for the faces of the chimneys which had previously looked in that direction were now turned almost south-easterly. I did not set out a minute too soon to note these circumstances, for all the builders of the countryside were already abroad, and in a few days all the evidences of earthquake action of the greatest value to seismologists will have been completely obliterated. Thus I found a very intelligent builder from Colchester on the lawn of the Langenhoe Rectory, giving orders for having the twisted chimneys removed, and I have no doubt they were all taken down within twenty-four hours. He had been driving all over the disturbed countryside, and told me that wherever the big chimneys had been left they were twisted from the south-south-west to the north-north-east, especially in the contiguous villages of Peldon and Abberton. This, I think, settles the original direction of the earthquake wave, and also establishes its rotatory character.

Langenhoe Church is an utter ruin, and all that yet stands will have to come down. It is a sad sight to see this picturesque, ivy-clad old church—standing so prettily overlooking the creeks where the ancient Danish Vikings landed in the dawn of our modern history but a comparatively few years before the church was built—now so utterly ruined. The porch on the north side is of brick, and a modern structure. Two large rents run up, one on each side of the doorway, at an angle of about 32°. They run from opposite directions, and meet just above the keystone of the arch. Here another large rent parallel with the ground traverses the masonry. It seemed to me that the first earthquake shock which rent the brickwork sprang from the western corner, and was reflected so as to form the opposite rent, after striking and lifting up and forming the parallel crack abovementioned.

The battlements of Langenhoe Church, like those of Wivenhoe, have been shaken down. But while those of Wivenhoe were thrown upon the ground chiefly on the *west* side, those of Langenhoe Church were thrown on the nave—that is, in an opposite or *easterly* direction. They crashed through the roof and carried a gallery with them, the concussion meantime bursting out the upper part of the chancel end. Am I right in thinking that this pitching forward of the loosened rubbish in opposite directions, as exemplified in these two churches, taken in connection with the overwhelming proof of rotatory motion, indicates that the movement of the earthquake had swerved right round between Wivenhoe and Langenhoe? In that case does it not also suggest the *local* character of the earthquake?

Langenhoe and the adjacent villages, with the Isle of Mersea close by and in full view, appear to form the focus of the disturbance. So far as I have been able to learn, the clocks stopped by the shock were those facing the north.

I see the newspapers refer to various cracks and fissures in the ground at Langenhoe, Abberton, Mersea, and elsewhere, as having been caused by the earthquake. I saw numbers of them, but in every instance they were the ordinary cracks which always appear in the London Clay during a drought, or after a spell of dry weather like that of the last three weeks. In none of the instances I saw had the fissures anything to do with the earthquake.

The local character of the area of chief disturbance is not only indicated by the different directions in which the rubbish was thrown from the battlements of Wivenhoe and Langenhoe Churches relatively, but also by the fact that whilst the western side of Mersea Island suffered severely, the eastern side was only slightly affected in comparison.

Museum, Ipswich, April 26 J. E. TAYLOR

THE earthquake was felt here very plainly, and I am able to give some evidence as to the amount of oscillation experienced at the moment when the wave passed under Cambridge. I happened to be looking at my marine aquaria at rather more than twenty minutes past nine on Tuesday morning (I regret I did not notice the *exact* time, but that was about it), and the water in them distinctly moved. The oscillation was not violent, as if produced by a concussion in the air, such as an explosion would cause, but rather as if the table on which the aquaria stand had been tilted up to the extent of an inch, and in the direction of a line running east and west. I was looking more particularly at a very shallow aquarium in which I keep shrimps, mussels, and sand-loving annelids, and one portion of which has less than a quarter of an inch depth of water. This was tilted up so much that the sand at the shallow end was quite uncovered by the water, and my first thought was that evaporation had taken place during the preceding night to such an extent as to endanger the lives of the nereids and other creatures; I therefore went hastily for some fresh water, but upon returning with it in a minute I found the water at its normal level, and I had no necessity to pour any fresh in. I remember, too, that I was sensible of a slight giddiness at the time, and the house and everything in it seemed to be moving. The sensation indeed was much like being on shipboard. I had no suspicion of the real cause, but thought it was a slight faintness, as I had not then breakfasted.

Mill Road, Cambridge, April 23 ALBERT H. WATERS

THE following memoranda may be of interest:—On January 8, 1869, I was with Prof. Dawkins, engaged in examining the late Mr. Whincopp's collection at Woodbridge, Suffolk. On my way home I was delayed three hours at Bury St. Edmund's in consequence of a luggage-train having broken down to the eastward. While there I was told that an earthquake had been felt that day at Thurston, Elmswell, and Haughley, places between Ipswich and Bury. It was reported that a workman, sitting eating his luncheon on the bank, saw the rails move. Mentioning this when I returned home, I was told that the policeman in this village had felt a shock. I therefore interviewed him and made the following note:—"January 15, 1869: P.C. Redhouse, when near the 'Hare and Hounds'" (which is a few hundred yards south of my house) "on Sunday morning the 3rd, about 2 a.m., heard a sound like heavy distant guns, which seemed to shake him and to make him reel. He was walking fast, and stopped. There was no shake after the sound. He thought there were six or seven reports in a couple of seconds. The movement was from north to south. There were three sounds before he stopped, and three afterwards. He did not regain his steadiness for two or three chains' distance. The sounds were very heavy, and he went home in alarm." I was awakened the same night by a tremor of the bed. This occurred a week before the shock in Suffolk. The late earthquake was preceded at Langenhoe by a slighter one on February 18.

A yacht captain at Wivenhoe happened, on the 22nd inst., to witness the effects from the top of a ladder. Hearing a rumbling sound, he looked about him and saw the church and all the houses rocking about, some one way and some another, "like a lot of pleasure-boats at the seaside with a gentle swell on." This seems to show that the length of the wave could not have been great, but that it must have been in opposite places withi a few hundred yards. Knowing the district well, it strikes me as remarkable that the strength of the shock should have been so much localised, while the distance over which it was slightly felt was so extended. O. FISHER

Harlton, Cambridge, April 28

ALTHOUGH this Observatory does not possess a seismograph, yet the passage of yesterday's earthquake wave was recorded by the magnetographs, although I am not aware the shock was felt by any one in this neighbourhood. It was registered at 9.17–18 a.m. G.M.T., and from the fact that the disturbance of the horizontal force magnetometer was the greatest, we infer that the terrestrial movement was rather north and south than east and west. G. M. WHIPPLE

Kew Observatory, Richmond, Surrey, April 23

PROBABLY one of the extreme limits of the action of the earthquake of April 22 was at Street, Somerset, ten miles beyond the Mendip main anticline. There it was *certainly* felt by an invalid lady, who mentioned it at midday dinner, only a few hours after, no news having been received, of course, from other parts. Has there been any certain record of it north of the concealed Palæozoic ridge across the North Midland counties?

York, April 28 J. EDMUND CLARK

NOTES

AT the meeting of the Executive Committee of the City and Guilds of London Institute held on Tuesday, the following appointments were made at the Central Institution, Exhibition Road:—To the Professorship of Chemistry, Henry Armstrong, Ph.D., F.R.S., of the Technical College, Finsbury; to the Professorship of Engineering, W. C. Unwin, D.Sc., of the Royal Engineering College, Cooper's Hill; to the Professorship of Mechanics and Mathematics, Olaus Henrici, Ph.D., F.R.S., of University College, London; to the Professorship of Physics, Oliver Lodge, D.Sc., of University College, Liverpool.

IN a crowded house on Tuesday last the Convocation of the University of Oxford passed the much-debated statute allowing women to enter for "certain of the honour examinations of the University." The statute has been opposed on very different grounds. The old Conservative Oxford School (fast becoming extinct among the resident teachers) of course objected to any change in favour of the higher education of women; with them went a portion of the High Church party, who look with disfavour on any proposal tending to bring women into intellectual competition with men. Others, again, opposed the statute on the ground that it was unfair to men, who have to keep certain terms and pass certain examinations within a specified time if they wish to enter for an honour school, whereas the statute allows women to enter for honours without the same preliminary examinations, and without restrictions as to time and residence. Others again feared an influx of young ladies into Oxford, as likely to destroy the manliness of the undergraduates and spoil the natural modesty of the lady students. To these arguments the success which the present halls for ladies in Oxford have met with is the best answer. Their presence has not revolutionised the University; they have not been a stumbling-block to discipline nor a rock of offence to the Church. The women's examinations, conducted by the delegates, were exactly on the same subjects, and the papers were set by the same men, as in the men's honour examinations before this statute passed. Now the same papers will serve for both, trouble will be saved, and the women who obtain honours will win a certificate universally recognised throughout the country. Oxford is to be congratulated on Tuesday's vote.

THE Rede Lecture at Cambridge University will be delivered on May 28 by Mr. Francis Galton, the subject of the lecture being "The Measurement of Human Faculty."

We are informed that tickets have been applied for as follows for the Montreal meeting of the British Association:—Members elected prior to October 1882, 379; Members elected since October 1882, 181; Associates (relations of Members), 120; total, 680.

The International Geological Congress will hold its meeting in Berlin this year, towards the end of September.

The International Polar Conference concluded its labours last Thursday.

In reference to the recent sunsets a correspondent writes that Graham's Island was in eruption, throwing out vast quantities of steam, ashes, and cinders from July 19 to August 16, 1831, and in connection therewith sends us the following extract from a letter written from Malta, January 28, 1832 (see *Phil. Trans.* 1832):—"In the month of August a singular appearance was witnessed in the heavens, many evenings successively, both here and in Sicily. Soon after sunset the western sky became of a dark, lurid red, which extended almost to the zenith, and continued gradually diminishing in extent and intensity even beyond the limit of twilight. This phenomenon, too, was attributed to the volcano, and was supposed by many people, whom it greatly alarmed, to be portentous of some impending calamity." Our correspondent also sends us the following old translation of Virgil's "Georgics," Book i. line 542:—

"He, too, bewailing her unhappy doom
When fell her glorious Cæsar, pitied Rome,
With dusky redness veiled his cheerful light,
And impious mortals feared eternal night;
Then, too, the trembling earth and seas that raged,
And dogs and boding birds dire ill presaged;
What globes of flame hath thundering Etna thrown,
What heaps of sulphur mixed with molten stone,
From her burst entrails did she oft expire,
And deluge the Cyclopean fields with fire."

The Kew Committee of the Royal Society have affiliated to the Department for the examination and verification of scientific instruments a branch which will rate watches for either makers or the public on very moderate terms.

The Council of the Royal Geographical Society have decided to appoint for one year an inspector, to inquire thoroughly into and report on the state of geographical education at home and on the Continent. In addition to studying the best methods of geographical teaching—chiefly probably in Germany and Switzerland—he will be required to collect and report on the best text-books, maps, models, and appliances. His honorarium will be 250*l.*, to include travelling expenses, but not the purchase of books, &c., which will be defrayed by the Society on the selection being approved by the Council.

Science in Japan has recently suffered a severe loss by the death of Dr. A. J. C. Geerts, which took place at Yokohama towards the end of last year at the early age of forty. He had been for fifteen years in the employment of the Japanese Government, and a few weeks before his death his services had been recognised by the Emperor, who conferred on him the Order of the Rising Sun. Dr. Geerts was originally Professor of Chemistry in the School of Military Medicine at Utrecht, and in 1868 was offered by the Japanese Government the post of Professor of Natural Science at the Medical School then recently established at Nagasaki. After occupying this position for five years he was nominated adviser to the Department of Hygiene and Public Health in Tokio, and was also charged with the establishment of a chemical laboratory at Kiôtô. In 1877 he established a similar institution in Yokohama, where his duties consisted chiefly in the testing of foreign drugs imported for sale amongst the Japanese, and this position he held at the time of his death. Like every other European in the Japanese Government service whose duty compels him to stand between his own countrymen and the natives, and to hold an even balance between the claims of both, his work was frequently of a harassing and unpleasant description; nevertheless he found time to write numerous works on Japan. His papers on Japanese mineral products, communicated during a number of years to the two learned societies in Japan, are of much value. He also published a Japanese Pharmacopœia, an account of the numerous mineral springs in Japan, and finally he commenced, and actually published, two volumes of an encyclopædic work entitled "Produits de la Nature Japonaise et Chinoise," in which he intended to describe the names, history, and application "to arts, industry, economy, medicine, &c., of substances derived from the three kingdoms of nature, and which are employed by the Japanese and Chinese." The formidable nature of this title is in no degree diminished when we come to examine the torso of the work itself. Ordinary men, who bear in mind that human life and human powers are limited, can only stand amazed at the conception of this work; for the author not only ransacked all that had ever been written on China and Japan in Europe, but also examined the whole of Chinese and Japanese literature before he sat down to write even the most insignificant article. In the section "Iron" alone one finds about 200 references to works in all literatures and of all ages. Each section contains the Japanese and Chinese legends respectin the origin and discovery of the production which formed its subject, the places where it has been or is now found, the primitive modes of obtaining it, the various qualities ascribed to it, its employment in arts and industry, &c. From this method of writing, it was inevitable that the work should bear the appearance of a hotch-potch, an *omnium gatherum* of fact and myth; but we could at least feel sure that in each section all that had ever been known of the subject was given. The work was really beyond the power of any single individual, and, if it were to be brought to an end at all, should have been executed on some extensive plan of cooperation similar to that employed in Dr. Murray's English Dictionary. As an example of the minute care bestowed on each point, it may be mentioned that in dealing with "Jade" the author gives two Latin synonyms, two Chinese, thirteen Japanese, a Spanish, a Manchu, a Turkish, a Persian, an Arab, and a Maori synonym!

Lieut. B. Baden-Powell, Scots Guards, made an ascent in his own balloon from Aldershot on Monday last week. The weather at the time of starting (4.30) was threatening and the wind fresh from the north-east. On rising to a height of 4000 feet, a lovely cloudscape was seen, the sky overhead being clear and blue, and a sea of clouds stretching around with very distinct horizon. Below, the earth could be seen through the haze, on which the shadow of the balloon was thrown, a bright halo surrounding the car. The descent was made at a quarter to six, about twenty miles off.

Mr. H. O. Forbes writes:—In a note received from the ex-Governor of Timor (now in Lisbon) I learn that a violent earthquake was experienced in Dilly on November 11, which destroyed the hospital and also damaged the church and other edifices, but without loss of life.

The last number of the *Journal of the Straits Branch of the Royal Asiatic Society* (Singapore, 1883) has the continuation of Capt. Kelham's notes on the ornithology of the Straits Settlements and the western States of the Malay Peninsula; also a collection of Malay proverbs, by Mr. Maxwell. Mr. Cameron contributes a paper on the Patani, the most considerable river of the peninsula, which flows northwards into the Gulf of Siam. An article of extraordinary interest is that on *latah*, a nervous disorder among Malays, or rather the native name applied to those who labour under the disorder. "It includes all persons of a peculiarly nervous organisation, ranging from those who, from their mental constitution, seem absolutely subservient to another's will, down to those who appear merely of a markedly excitable temperament." Numerous examples of the effect of this mysterious mental affection are added by the writer, Mr. H. A. O'Brien.

WE gladly notice the issue of three new numbers of the *Encyclopædia of Natural Sciences*, from the publishing house of Eduard Trewendt, in Breslau. The tenth number is now out of the *Alphabetical Manual of Zoology, Anthropology, and Ethnology* (part i. l. 36), which with this new instalment has completed its "F," and entered on its "G." The number referred to contains very valuable articles contributed by Gustav Jäger, Reichenow, von Mojsisowics, Roeckl, von Hellwald, Sussdorf, and others. Nos. 19 and 20 of the second part of the collective work have also appeared, both belonging to the *Dictionary of Chemistry*, edited by Ladenberg. Among other valuable articles in No. 19, by Ladenburg, Biedermann, Weddige, and Jacobsen, "Azoverbindungen," by Heumann, and "Benzœsäure" by Weddige, are treated with special completeness. In No. 20 are articles by Engler, Drechsel, Biedermann, and others. These two numbers bring the *Dictionary of Chemistry* to the end of "B." We again wish all success to this comprehensive collective work on the natural sciences.

MM. HENRY are experimenting with a system of photography for double-stars, in order to determine their distance and position angle. They have already obtained good results on about twenty stars in various constellations.

M. LEVEAU has been appointed Astronome Titulaire at the Paris Observatory, in place of the late M. Yvon Villarceau.

IN a small pamphlet published at Saigon ("Memoire sur les Poissons de la Rivière de Hué," C. Guilland et Martinon), M. Tirant, the Administrator of Native Affairs, has given a catalogue of the fish to be found in the river of Hué, the capital of Annam, and in the adjacent lagoons. These latter are exceedingly numerous, running parallel to the sea for miles, and are filled during the rainy season by the overflow from the rivers. They are employed as reservoirs for the fish supply of the capital. In them, and in the river itself, Dr. Tirant states he procured seventy new species of fish.

INFORMATION has recently been received in Paris of the death of M. Bruel, one of the most enterprising of French explorers in Cochin China. He was murdered by pirates on January 18 in Cambodia, on the frontier of the Laos country.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♂) from Continental India, presented by Mr. A. MacDonnett Green; a Common Fox (*Canis vulpes*), British, presented by Miss Bertha Haig; a Herring Gull (*Larus argentatus*), European, presented by Mr. R. Morton Middleton, jun.; a Green Lizard (*Lacerta viridis*), European, presented by Mr. J. H. Leech; three Russell's Vipers (*Vipera russelli*) from Ceylon, two Indian Rat Snakes (*Pytas mucosa*), an Indian Python (*Python molurus*), an Indian River Snake (*Tropidonotus quincunciatus*), two Indian Cobras (*Naia tripudians*) from India, presented by Mr. Gerald Waller; a Gray Ichneumon (*Herpestes griseus*) from India, a Short-headed Phalanger (*Belideus breviceps*) from Australia, three Lesser Birds of Paradise (*Paradisea minor*), two Red-sided Eclectus (*Eclectus pectoralis*) from New Guinea, a Chattering Lory (*Lorius garrulus*), a Three-coloured Lory (*Lorius tricolor*) from Moluccas, deposited; a Dusky Parrot (*Pionus violaceus*) from Guiana, received in exchange; a Smooth-headed Capuchin (*Cebus monachus*) from South-East Brazil, a Severe Macaw (*Ara severa*) from Brazil, two Schlegel's Doves (*Chalcopelia puella*), a Buffon's Touracou (*Corythaix buffoni*) from West Africa, a Diademed Amazon (*Chrysotis diademata*), a Yellow-shouldered Amazon (*Chrysotis ochroptera*) from South America, a Banded Aracari (*Pteroglossus torquatus*) from Central America, received on approval; a Mediterranean Seal (*Monachus albiventer*) from the Mediterranean, two Chinchillas (*Chinchilla lanigera*) from Chili, an Anaconda (*Eunectes murinus*) from South America, purchased.

OUR ASTRONOMICAL COLUMN

THE SOUTHERN COMET (ROSS, JANUARY 7).—Adopting Mr. Tebbutt's elements copied into this column last week, we have the following positions of the comet observed at Melbourne and Windsor, N.S.W., for 6h. Greenwich mean time:—

1883	R.A. h. m. s.	N.P.D. ° ′	Log. distance from Earth	Log. distance from Sun
Dec. 16	17 4 30	93 56·9	0·0561	9·6151
18	17 12 4	94 3·2	0·0232	9·5758
20	17 21 35	94 34·4	9·9864	9·5393
22	17 33 53	95 44·7	9·9463	9·5102
24	17 49 54	97 50·8	9·9046	9·4934
26	18 10 31	101 6·2	9·8651	9·4930
28	18 36 2	105 29·2	9·8332	9·5090
30	19 5 47	110 35·5	9·8144	9·5376

This ephemeris, founded upon an orbit which is certainly not open to material correction, enables us to decide that the supposed comet which was seen in Tasmania on the mornings of December 25 and 27, rising a few minutes before the sun, could not have been the comet detected by Mr. Ross on January 7, which on those mornings would not rise (at New Norfolk, for instance) till upwards of forty minutes after the sun; on December 25 the sun rose there at 4h. 21m., the comet at 5h. 2m.

It is not easy to reconcile the estimate of brightness at Melbourne on January 11 with that of Mr. Tebbutt on January 19. Mr. Ellery writes to the *Observatory* that on the former evening the comet disappeared in a faintly illuminated field, simultaneously with a tenth-magnitude star, while Mr. Tebbutt considered it on January 19 to be just beyond unassisted vision; yet the ratio of the theoretical intensity of light on the former date would be to that on the latter as 2·9 to 1.

The comet appears to have been well above the horizon in European latitudes before daylight, previous to the perihelion passage. Between December 17 and 21 it rose at Greenwich about 5h. 40m. a.m., but the presence of the moon would have rendered its discovery difficult. It was nearest to the earth on the morning of January 1, the distance being then 0·646 (the earth's mean distance from the sun = 1).

THE ASPECT OF URANUS.—At the sitting of the Academy of Sciences of Paris on April 21, M. Perrotin presented a note on the aspect of Uranus, from observations made with the 15-inch equatorial at the Observatory of Nice. On March 18 he had remarked, in company with Mr. Lockyer, a bright spot near the lower limb of the planet, as seen in the inverting telescope. Further observations showed that it was near the equator of Uranus. It was a very difficult object, and much uncertainty existed as to its exact position; it was better seen as it approached the limb. It was observed on April 1 about 11h., at the northern extremity of the equatorial diameter, and on the next night about 10h. 40m., at the southern extremity: it occupied the same position on April 7 at 10h. 30m., and April 12 at 11h. These observations, M. Perrotin adds, made at the limits of visibility, required very favourable conditions, and being aware of the possibility of illusion in such a case, he invites the attention of observers possessed of powerful optical means, in order to control his own impressions. The appearance and the indeterminateness in the duration of the phenomenon on April 1, when the images were best, rather point to a luminous belt than to a single spot, which introduces uncertainty in the times of the observations; with due regard to this, M. Perrotin finds a fair agreement with the assumption of a rotation not differing much from ten hours. On April 12 Mr. Trépied was present, and confirmed the impressions received by the Nice astronomer; he also remarked in the bright part a condensation which had previously escaped notice.

By "diamètre equatorial" we presume M. Perrotin refers to the diameter in the plane of the orbits of the satellites.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following courses of lectures are being delivered this term :—

Mathematics—Prof. Stokes, Optics; Prof. Darwin, Theory of Potential and Attractions; Trinity College, Mr. Glaisher, Theory of Errors, Mr. Ball, Higher Solid Geometry, Mr. J. J. Thomson, Dynamics of a Rigid Body, Mr. Rowe, Higher Integral Calculus and Abel's Theorem, Mr. Forsyth, Thermodynamics; St. John's College, Dr. Besant, Analysis, for Schedules II. and III., Mr. Pendlebury, Laplace's and Bessel's Functions, Mr. Webb, Elementary Rigid Dynamics; Pembroke College, Mr. Burnside, Hydrodynamics; Emmanuel College, Mr. Webb, Elasticity.

Physics—Trinity College, Mr. Trotter, Electricity and Magnetism, Mr. Glazebrook, Elementary Physics; Cavendish Laboratory, Mr. Shaw, Elementary Physics; St. John's College, Mr. Hart, Elementary Electricity, Practical Physics, Cavendish Laboratory; Advanced Demonstrations in Light and Sound; and Elementary Demonstrations in Optics and Electricity.

Chemistry—Prof. Liveing, Course of Examinations and Personal Instruction of those who have attended his general course in the last two terms; Mr. Main, General Course, including Carbon Compounds; Mr. Pattison Muir, Non-Metals, and Elementary Organic; Mr. Sell, Elementary Chemistry; Mr. Scott, Gas Analysis; Mr. Lewis, Catechetical Course.

Practical Chemistry—Demonstrations for 1st M.B. by Mr. Sell and Fenton; Demonstrations in Qualitative Analysis (Sidney College), Mr. Neville; Practical Courses at St. John's and Caius College Laboratories.

Mineralogy—Prof. Lewis; and two Courses of Elementary Demonstrations.

Mechanism—Prof. Stuart, Differential and Integral Calculus for Engineering Students; Mr. Lyon, Machine Construction and Heat; Mr. Ames, Surveying and Levelling.

Physiology—Elementary, Prof. Foster; Structure and Function of the Central Nervous System, Mr. Langley; Advanced Physiology of Respirations, Dr. Gaskell; Preparation for 2nd M.B., Mr. Hill.

Human Anatomy—Prof. Macalister, Anatomical Basis of Anthropology, Advanced; Demonstrations and Dissections.

Elementary Biology, Mr. Sedgwick; Morphology of the Vertebrata, Mr. Sedgwick; Mollusca and Tunicata, Mr. Weldon; Mammalia, Mr. Gadow.

Botany—Prof. Babington, Structural and Systematic; Morphology, chiefly Cryptogamic, advanced, Dr. Vines; Demonstration Lectures on Physiology, Mr. F. Darwin; Demonstrations in Systematic Botany, Mr. Potter.

Geology—Stratigraphy, Local, Prof. Hughes; General Course, Carboniferous to Recent, Dr. R. D. Roberts; Palæontology, Elementary, Mr. T. Roberts; Microscopic Petrology, Mr. A. Harker; Climatology, Mr. E. Hill; Metamorphism, Mr. Marr; Field Lectures, Prof. Hughes.

SOCIETIES AND ACADEMIES

LONDON

Royal Meteorological Society, April 16.—Mr. J. K. Laughton, M.A., F.R.A.S., vice-president, in the chair.—J. Y. Davidson and T. Wright were elected Fellows of the Society. —The following papers were read:—On the origin and course of the squall which capsized H.M.S. *Eurydice*, March 24, 1878, by the Hon. Ralph Abercromby, F.R.Met.Soc. It will be remembered that the *Eurydice*, which was a full-rigged corvette, when passing Ventnor in the Isle of Wight, running free before a westerly wind, with all sails set, was struck by a sudden squall from the north-west; and before sail could be shortened she went on her beam ends, and the lee ports being open, she filled and foundered. The author has investigated the character of the weather preceding and following the day in question, and finds that the squall was one belonging to the class which is associated with the trough of V-shaped depressions. The squall, which originated in the north of England, swept across the Isle of Wight at a rate of about thirty-eight miles an hour. The V-depression was of an uncommon class, in which the rain occurs after the passage of the trough, and not in front of it, as is usually the case. The weather generally for March 24 was unusually complex, and of exceptional intensity, and for this reason some of the details of the changes cannot be explained.—Waterspouts and their formation, by Capt. J. W. C. Martyr.—The weather forecasts for October, November, and December, 1883, by C. E. Peek, M.A., F.R.Met.Soc. This is a comparison of the weather indicated in the forecasts of the Meteorological Office with that actually experienced at Rousdon in Dorset.—On certain effects which may have been produced in the atmosphere by floating particles of volcanic matter from the eruptions of Krakatoa and Mount St. Augustin, by W. F. Stanley, F.G.S. In this paper were given details of a microscopical examination which had been made of some dust that fell, to the thickness of about two inches, upon the deck of the bark *Arabella*, in lat. 5° 37′ S., long. 88° 58′ E., at about 1000 miles from Krakatoa, and supposed to be from the eruption of that volcano. The dust under examination was contained upon a single microscopic slide. For the convenience of discussion of the subject the visible forms were separated into eight different kinds of particles:—(*a*) Small masses and single crystals of mineral matter visible by polarised light only. These were principally of augite and of certain felspars. (*b*) Very thin chips and scales of the above. (*c*) Very small masses of dense ordinary pumice. (*d*) Fractured chips of the above with one thin edge. (*e*) Light apparently *overblown* pumice in relatively large thin plates. (*f*) Fractured parts of *e*, but of larger bubbles traversed by seams upon which septa normal to the surface formerly existed. (*g*) Fractured parts of *e*, but of larger plates, with a thicker seam on one edge or on one corner only. (*h*) Thin glassy plates of *e*, formerly of relatively much larger size. These are of equal thickness throughout, and generally with one hollow surface. The particles *a* and *b* form only about half to one per cent. of the mass, the whole of the remainder being of the different forms of pumice described. The particles *g* and *h*, as being much the lightest in proportion to their extent of surface, were most dwelt upon. These particles, which the author termed *bubble-plates*, are of irregular, angular forms. They measure under the microscope, in different directions, from about ·5 to ·05 mm. The thickness of the plates is fairly uniform, varying between ·001 and ·002 mm. When there is a seam on one edge, the plate is smaller, and thickens towards the seam. By taking the interior part of a large mass of pumice and breaking it up into fine dust, some similar forms may be discovered. These plates being of quite transparent, volcanic glass (obsidian), they are invisible under the microscope, by direct light; but being placed in a medium of higher refractive power, as Canada balsam, they become clearly defined under oblique illumination, above a spot lens, with careful adjustment. Mr. Stanley suggested that these thin plates were from overblown bubbles of volcanic glass such as forms the mass of pumice; that most probably they were projected from about the centre of the volcanic chimney, where they could maintain a melting temperature until they reached the higher atmosphere; under which conditions the internal steam in each separate bubble would expand in volume through release of external pressure until the bubbles burst in the very thin fragments shown. These thin forms of bubble-plates, having great surface in comparison with their very small masses, were such as were eminently adapted to float in atmospheric currents to great distances. As such particles would descend with their convex sides downwards, they would also be especially adapted to reflect the sun's rays, when the sun sank to the horizon, whereas when the sun was at greater altitude his rays would pass through them nearly unobstructed. It was therefore proposed that the after-glow so often observed since the eruptions of Krakatoa and Mount St. Augustin was possibly due to reflection from these thin plates.

DUBLIN

Royal Society, March 17.—Section of Physical and Experimental Science.—Howard Grubb, M.E., F.R.S., in the chair. —On the success of an instrument for completing the optical adjustment of reflecting telescopes, by G. Johnstone Stoney, M.A., D.Sc., F.R.S., vice-president of the Society. The author had been astronomical assistant to the late Earl of Rosse, and while in charge of his observatory became impressed with the importance of increasing both the degree of accuracy and the facility with which reflecting telescopes can be adjusted. At the Cheltenham meeting of the British Association in 1857 he described an instrument designed to attain these ends, but had no opportunity of testing its performance till two years ago, when a twelve-inch mirror came into his possession of exquisite defining power, figured by the late Mr. Charles E. Burton, B.A., F.R.A.S. This mirror is mounted as a Newtonian telescope.

The collimator proposed in 1857 was made for it last autumn by Mr. Howard Grubb, M.E., F.R.S., and its performance has been fully tested with the most satisfactory results during the present observing season. The new collimator is a short telescope of eleven inches focus and two inches aperture, which, when used, is to be inserted into the eyepiece-holder of the large telescope. A spark between platinum points is produced in the focus of this instrument by a small Rhumkorff's coil such as those sold with toy apparatus, and the light of the spark emerging from the collimator is reflected by the small mirror of the Newtonian, and so reaches its large mirror. By pushing the eyepiece and platinum points of the collimator a little inside its focus, the beam of light, as it passes down the large telescope, is rendered slightly divergent, and falls normally on the large mirror. If everything is in perfect adjustment, the beam of light will then, after reflection by the large mirror, retrace its steps, and, recentering the collimator, will form an image coincident with the spark. Any want of adjustment is at once betrayed by the image in the field of view of the collimator not being coincident with the spark. On commencing the night's observing, the mirrors of the large telescope are first adjusted in the usual way. The collimator is then put into the eyepiece-holder, and if the telescope has been tolerably well adjusted, the image of the spark will be found not farther from the spark than a quarter or a third of the field of view of the eyepiece of the collimator. The adjustment is then completed in the following way:—The eyepiece-holder, instead of being rigidly attached to the tube of the telescope, is mounted on a triangular plate fastened to the side of the telescope by screws acting against springs at the corners. By these screws the line of collimation of the eyepiece-holder can be slightly altered, and by moving them the image of the spark is made to coincide with the spark. The instrument is then in a condition of optical adjustment vastly more perfect than has hitherto been attainable with reflecting telescopes. This whole process occupies less than half a minute, and is so easy of application that the author is in the habit of repeating it every time the telescope is moved to a fresh object. He is rewarded by having the last degree of refinement applied to the adjustment of his telescope in using it upon every object, an advantage the importance of which will be appreciated by every astronomer who uses a sufficiently fine mirror and is working on a sufficiently good night.—Mr. J. Joly, B.E., read a paper entitled "Notes on a Microscopical Examination of the Volcanic Ash from Krakatoa." The ash examined was part of some which fell on board the Norwegian barque *Borjild* while she lay at anchor off the great Kombuis Island on August 27, 1883. Her position was some 75 miles to the north-east of Krakatoa, a strong south-westerly gale prevailing at the time. She was hence most favourably placed for receiving good samples of the dust. A specimen of the floating pumice, picked up by the *Borjild* in the Straits of Sunda, was compared with the ash. Microscopically they were found to present the same features. Two species of pyroxene occur—a monoclinic and a rhombic variety. The first was augite; the second presents many of the optical characteristics of hypersthene. Both contain much magnetite. A triclinic felspar is very abundant, showing many different crystalline shapes. The identity of many of these with the triclinic felspar is shown by their occurrence, twinned with and superimposed upon crystals presenting undeniable plagioclastic characteristics. They show small angles of extinction. The presence of sanadine appeared doubtful. Iron pyrites was found in the ash, both embedded in vitreous fragments and free, as aggregations of cubes, showing the striations at right angles for adjacent faces. Magnetite is abundant. The frequency of lines of growth on the felspars seemed indicative of a comparatively tranquil formation. Most of the crystals showed a fine coating—much pitted and reticulated—of vitreous matter. A sudden mechanical separation from a viscous magma would explain this appearance, which somewhat resembled that produced by rapidly separating two flat surfaces compressing a viscous substance. Organic remains were found abundantly in both ash and pumice. A foraminiferal shell, very perfect, was found in the ash, and another in the pumice. Fragments, apparently of some algæ, were found plentifully in the former.—Dr. R. S. Ball, F.R.S., exhibited Mr. Common's photograph of the great nebula in Orion.—Prof. G. F. Fitzgerald, F.R.S., exhibited Ayrton and Perry's new spring ammeter.

Section of Natural Science, W. Frazer, F.R.C.S.I., in the chair.—On spherical or globular phosphorites of Russian Podolia, by Prof. J. P. O'Reilly, C.E.—Catalogue of Vertebrate fossils from the Siwaliks of India, by R. Lydekker, B.A., F.G.S., F.Z.S. Communicated by V. Ball, M.A., F.R.S.—On the action of waves on sea-beaches and sea-bottoms, by A. R. Hunt, M.A., F.G.S. Communicated by Prof. A. C. Haddon, M.A., F.Z.S. After detailing the conflicting views put forward by various authors, Mr. Hunt discusses Mr. Scott Russell's theory of oscillatory waves being converted into waves of translation, with observations and experiments to disprove it. The author then treats of the action of waves, currents, and wind currents on beaches, shingles, and sandbanks as observed in the neighbourhood of Torquay, and describes experiments conducted in a specially constructed tank.

Edinburgh

Royal Physical Society, April 23.—Dr. Traquair, F.R.S., in the chair.—Mr. Hugh Miller, A.R.S.M., read a paper on boulder-glaciation and striated pavements. Starting from local observations made near Edinburgh by Charles Maclaren and Hugh Miller upon the glaciation *in situ* of boulders and boulder pavements in the till, the author has been led to the conclusion that boulder-glaciation *in situ*, registering the ice-movement during the formation of the till, is extremely common. The glaciation of the county of Northumberland, to which he referred in passing, may be roughly divided into upland-glaciation, valley-glaciation, and glaciation of the seaboard. All these are registered equally well in the striation of the larger boulders (whether singly or in groups) as in that of the rock below. He confirmed the older observations that the glaciating agent was the same in both the rocks and the boulders, adducing the fact as strong evidence of the glacier origin of the deposit. That floating ice should striate in fixed directions so many blocks lying in soft mud at the sea-bottom he regards as impossible. As registering changes in ice-movement, the intercrossing of erratics, and a distinction between successive boulder-clays, this widespread glaciation of boulders *in situ* may prove of general importance and a distinguishing mark of the true till.

Paris

Academy of Sciences, April 21.—M. Rolland in the chair.—Letter of condolence to the family of the late M. Dumas from the *savants* of Geneva.—On a theorem of Kant relating to the celestial mechanism, by M. Faye.—On the scale of temperatures and on molecular weights, by M. Berthelot. The author endeavours to show that a profound study of specific heats tends to establish the fact that heat, which resolves compound molecules into their elements, has also the effect of resolving the highly complex groups of particles which constitute the bodies hitherto regarded as *elementary*.—On the optical identity of the crystals of herderite of Ehrenfriedersdorf with that of the State of Maine, by M. Des Cloizeaux.—Account of a young gorilla recently brought from the Gaboon and now in the menagerie of the Natural History Museum, Paris, by M. Alph. Milne-Edwards. This specimen is described as of a much more ferocious character than the chimpanzee or orang-utan, and greatly inferior in intelligence even to the gibbon.—Note accompanying the presentation of the marine charts and hydrographic documents offered to the Academy by the Depot of Charts and Plans on behalf of the Department of Marine, by M. de Jonquières.—On the separation of phosphoric acid in arable lands, by M. de Gasparin.—On the speed attained by Lapps with their snowshoes; extract from a letter addressed by M. Nordenskjöld to M. Daubrée. From the result of races instituted for the purpose of determining this point, an average speed of over six miles per hour was verified at Quickjock in Lapland.—Further observations on the present appearance of the planet Uranus as observed at the Observatory of Nice during the month of April, by M. Perrotin.—Changes observed in the rings of Saturn, by M. E. L. Trouvelot. From continued observations made since the year 1875 at the Meudon Observatory the author is able definitely to confirm the conclusion already arrived at, that the rings, so far from being fixed, are extremely variable.—On surfaces of the third order, by M. C. Le Paige.—On uniformly inclined surfaces and proportional systems, by M. L. Lecornu.—On the principle of the prism of greatest thrust, laid down by Coulomb in the theory of the limited equilibrium of sandy masses, by M. J. Boussinesq.—On the diffusion of light through unpolished glass or metal surfaces, by M. Gouy.—On the propagation of sound through gases, by M. Neyreneuf.—On the boiling-point of oxygen, air, nitrogen, and the oxide of carbon under atmospheric pressure, by M. S. Wroblewski.—On a metallic radical, by M. P. Schutzenberger.—Determination of the densities of the vapours of the chloride of glucinium, by MM. L. F. Nilson and Otto Pettersson.—On the neutral molybdate of didymium, and on the equivalence of didymium, by M. Alph. Cossa.—On the curves of solubility of salts, by M. A. Étard.—On the bark of *Xanthoxylum caribæum*, Lk., as a febrifuge, by MM. Heckel and Fr. Schlagdenhauffen.—On the application of the digester for the destruction of microbes in liquids, by M. L. Heydenreich.—On some siliceous spicules of living sponges obtained from the dredgings executed during the last expedition of the *Talisman*, by M. J. Thoulet.—On the generic relations of *Orbulina universa* with Globigerina, two illustrations, by M. C. Schlumberger. From the comparative study of these organisms the author infers that the dimorphism of the Foraminifera is an initial character resulting from two original forms.—On the action of heat on the phenomena of vegetation : (1) on the development and direction of the roots ; (2) on the heliotropism of certain plants, by M. A. Barthélemy.—On marine and fresh-water deposits considered from the economical standpoint, according as they are or are not sulphuretted ; alluvia of the Durance, by M. Dieulafait.—New report on the diamantiferous deposit at Grâo Mogol, province of Minas Geraes, Brazil, by M. Gorceix.—On the bones of the head of the Simœdosaurians, and on the various species of this extinct reptile found in the Cernay formations in the Rheims district, by M. V. Lemoine.—Note on the crepuscular phenomena observed at the Imperial Observatory of Rio de Janeiro during the winter months of 1883-84, by M. L. Cruls. —Note on the scientific mission to Cape Horn 1882-83 in connection with the question of the periodicity of barometric oscillations, by M. Ch. V. Zenger.

BERLIN

Physical Society, March 21.—Dr. Frölich spoke of some modifications of Wheatstone's bridge which had been applied to the measurement of the electric resistance of galvanic elements and batteries. Wheatstone's bridge consisted, as was known, of a wire quadrilateral and two wire diagonals. Of the two diagonals one contained a battery of constant electromotive force, the other the galvanometer. In these circumstances the resistances of the four lateral wires showed the proportion $W_1 : W_3 = W_2 : W_4$. For the purpose of measuring the resistance in a galvanic battery, the arrangement was so far empirically changed that the battery to be measured was inserted in one of the lateral wires. A second empirical method consisted in inserting the galvanometer into one diagonal wire and interrupting the second ; the battery to be measured was placed in a lateral wire. Dr. Frölich showed that both arrangements were only modifications of Wheatstone's bridge. The way in which these modifications originated might be conceived by supposing that the bridge was formed of cords, and that the angles of the square were successively shifted ; the proportion which applied to Wheatstone's bridge would still hold in the new case. Dr. Frölich laid down a general law applicable to all individual cases. If in a Wheatstone bridge an element be inserted into each wire, while one diagonal wire contained the galvanometer, and the other was interrupted, if, moreover, on opening this wire, the electromotive force in the other diagonal remained unchanged, then the proportion above stated between the resistances of the lateral wires would still hold. Whether this general law included such a case as could be applied practically and with certainty to the measurement of the resistance of elements must be determined by experience.—Dr. Frölich then gave a report on the continuation of his measurements of solar temperature. At a former sitting he communicated the measurements he had made during the previous year. These measurements yielded an almost equal result on June 29 and July 1, an increase of solar heat of 6 per cent. over this last estimate on August 14, and a value pretty nearly equal to that of July 1 in the middle of October. Since then doubts had been expressed as to whether the calculated increase of solar heat in August corresponded with the fact, seeing that the amount of the difference was not so much greater than might be accounted for by assuming an error, not easily avoided, in an observation. In opposition to this consideration, Dr. Frölich contended that, even if it were claimed that the difference would have to be three times greater than any error in observation which might probably occur, the increase in August had such a high degree of probability in its favour that one might bet 22½ to 1 for its accuracy. All doubt, however, on the matter was completely removed by two measurements Dr. Frölich made on February 19 and March 17. Both measurements yielded pretty nearly equal values of solar heat, and one was 15 per cent. higher than the estimate of the middle of October last year. In this case the probable error was surpassed eight times. Dr. Frölich was of opinion that the increase of solar heat in August was connected with an assumed formation of sunspots, and seeing that the spots were bound up with the magnetism of the earth he made inquiries with a view to ascertaining the state of the terrestrial magnetism at that time. From the average of the reports collected by him he found that in correspondence with the increased solar heat in the middle of August there was a diminution of the earth's magnetism.—At the close Dr. Frölich produced a large lump of magnesium as the product of an electrolytic industry. The piece was wrought in a factory according to a patented method based essentially on the melting of chloride of magnesium, and decomposing it in the melted state by an electric current.

VIENNA

Imperial Academy of Sciences, March 20.—L. Martin, on the polydimensional argument.—R. von Drasche, on some new and less-known ex-European Ascidia. — T. Latschenberger, on testing and determining ammonia in animal fluids.—W. Fossek, synthesis of dyad alcohols by action of alcoholic potash on aldehydes.—On the action of phosphorus trichloride on aldehyde, by the same.—F. Wiesner, on geotropic curvature of roots.—F. W. Dafert, synthesis of glycuronic acid from mannite (sealed packet).—K. Olszewski, determination of density and of coefficient of expansion of liquid oxygen.—Determination of the temperature of solidification of some gases and liquids, by the same.

April 3.—A. Adamkiewicz, preliminary communication on new stainings of the spinal cord, part ii. ; results obtained by staining the diseased spinal cord with saffronine.—M. Loewit, contributions to theory of blood-coagulation, part i. ; on the coagulating power of the blood-disks.—A. Lustig, contributions to development of gustatory buds.—T. V. Tanowski, on direct substitution-products of azobenzene and on an asymmetrical trinitroazobenzene.—E. Witlazil, on polymorphism of *Chætophorus populi*, L.—M. Strainsky, on tides and their reaction on the configuration of the earth's surface.—R. Benedikt and K. Hazura, on morin.—R. Benedikt and P. Julius, on diresorcin and diresorcinphthaline.—K. Hazura and P. Julius, on resorcin-ether.—P. Julius, on a new reaction of benzidine.

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