THURSDAY, OCTOBER 8, 1891.

THE ICE AGE IN NORTH AMERICA.

The Ice Age in North America, and its Bearings upon the Antiquity of Man. By G. Frederick Wright, D.D., &c. With an Appendix on "The Probable Cause of Glaciation," by Warren Upham, F.G.S.A. With many new Maps and Illustrations. (London: Kegan Paul, Trench, Trübner, and Co., Limited, 1890.)

SWITZERLAND has been called the playground of Europe. The glacial epoch occupies a similar position in geology. Here the student, wearied with the precision of palæontology or of mineralogy, may revel in dreams of omnipotent glaciers, wrap himself in ice sheets, throw mental somersaults, swallow self-contradictory arguments, and be as blind to unpleasant facts as was Nelson at Copenhagen, when he put the telescope to his useless eye, and "spoke disrespectfully" of the signal of recall. To any sarcastic historian of the progress of geology, the literature of ice and its effects will be a boon, since it is so rich in unsound inductions and unstable hypotheses.

Dr. Wright's book, however, is, on the whole, a favourable exception to this general rule. Passages, no doubt, may be found here and there, to which exception might be taken—notably to his remarks on the subject of cirques, in which he regards with favour opinions which are hard to reconcile with expressions in other parts of the book, and rest largely on an erroneous statement—namely, that cirques "are confined to glaciated regions," and "as a rule . . . occupy positions where glaciers first appear." Still, in general his conclusions are supported by facts, very clearly and carefully described, so that we feel, even if occasionally not quite convinced, that his view is worthy of careful and respectful consideration.

But in the matter of ice the subject is long, and our space is brief. It will be better to abstain from criticism of details and give a short outline of those parts of Dr. Wright's book which will be of most interest to readers on this side of the Atlantic. As he states in his preface, his work deals not only with the Ice Age in North America, but also with the whole subject of the Glacial Period. So in its earlier part a considerable space is allotted to glaciers in general and their characteristics; in its later, to the effects of the Glacial Period in other parts of the world, its cause, its date, and its relation to the history of man. These, however, we shall pass over, and confine ourselves to the section dealing with glacial action on the North American continent.

After a sketch of the existing glaciers on the Pacific coast, Dr. Wright gives the results of a study of the Muir glacier in latitude 58° 50′, by the side of which a small party, of which he was a member, camped out for a month. This glacier is about a mile wide where it comes down to the sea, terminating in ice cliffs 300 feet, and sometimes a little above 400 feet in height. The rise inland is gradual—perhaps about 100 feet per mile—and the main body of the glacier occupies a vast amphitheatre, with diameters ranging from 30 to 40 miles. From a number of observations it appeared that the stream of ice entered the inlet, where the cross section was about five million square feet (5000 feet wide by 1000 deep), at an average rate of 40 feet a day (70 feet in the centre and 10 near the margin). It was, however, evident that this glacier, for some time past, had been retreating; indeed, fresh striations and *débris* could be traced to more than 2500 feet above its present surface. Dr. Wright also found below the end of the ice the dead stumps of a forest of cedar trees, erect, and rooted in a clayey soil, but buried beneath glacial gravel. Probably this was deposited by streams, flowing from the advancing ice, which afterwards overrode the mass.

Dr. Wright estimates the amount of sediment which is now being washed down from the basin of the Muir Glacier as equal to nearly one-third of an inch per annum over the total area (1200 square miles) which it occupies. In regard to the vexed question of the excavatory powers of glaciers, Dr. Wright expresses himself, as a rule, cautiously, ascribing to them the formation of true rock-basins under favourable circumstances, but laying stress upon the fact that, in the lower part of their course, where they are beginning to spread out over the lowlands, they can pass, as in the case mentioned above, over quite incoherent materials, without disturbing them. It also seems to follow from his remarks that he regards glaciers as agents of abrasion rather than of erosion, in which we have no doubt he is correct. As another indication of his general caution and candour, we may note that he is careful to point out that striated stones and rock surfaces do not always prove the former presence of a glacier, and may not even have been produced by the action of ice.

A large part of the book is devoted, as a matter of course, to a description of the glaciated area in North America. The boundary of this, as Dr. Wright explains, is sometimes distinctly marked by a terminal moraine, at others it is less definite, being only vaguely indicated by scattered *débris*. But in his opinion—and here he expresses the opinion of the majority of American geologists—there was a time when a large part of Northern America east of the Rocky Mountains was buried beneath a mass of ice. There is, indeed, a driftless area in Wisconsin, which may have formed a kind of *jardin* on a gigantic scale, in this huge *mer de glace*, but, speaking generally, the whole region of the great lakes was covered by an ice-sheet which came down to the sea at Long Island and traversed the northern part of Pennsylvania; thence its irregular frontal margin can be traced to the south-west, until, in the valley of the Mississippi, it reaches almost as far south as the 37th parallel of latitude. Of the various indications of this vanished ice-sheet, the smoothed and striated surfaces of rock, the moraines and boulder clays, the "kames" and "drumlins," Dr. Wright gives careful descriptions and illustrations, usually taken from photographs, so that the evidence is presented as clearly as is possible to the reader. To the last-named phenomena—the "kames" and "drumlins"—and some curious hollows which he calls "kettle-holes," Dr. Wright devotes much attention. The first he regards as indicative of lines of drainage in the closing stage of the Ice Age; the second, as early terminal moraines, modified in shape by the subsequent passage of the ice over them, and so anterior in date to the kames. The kettle-holes occur among morainic deposits, and are thus explained:—As the ice is retreating, a mass of it may be insulated; as this melts, the superincumbent material tends to slip towards the edges, and thus to form a ring of *débris*, by which, after the ice has disappeared, a hollow is inclosed. Dr. Wright also adopts the opinion, maintained by Prof. Claypole, the late Prof. H. C. Lewis, and others, that one effect of the advance of this great mass of ice was to obstruct the flow of all rivers which take a northerly course, and thus to convert their valleys into lakes.

But into a discussion of this interesting question, and of the cause of the glacial epoch, to which a considerable space is devoted, we must not now enter. We must also pass over the questions relating to the date of the glacial epoch and its relation to the first appearance of men, merely stating that Dr. Wright inclines to regard the latter as pre-glacial, but the former as less remote than is generally supposed. It must suffice to say that he appears to be a careful observer, and generally a cautious reasoner, though slightly too prone to quote the remarks of others without due criticism; so that, on the whole, his book presents us with a good summary of the results of investigations into the glacial geology of North America, and will be valuable for purposes of reference on this side of the Atlantic. T. G. BONNEY.

THE TOTAL REFLECTOMETER AND THE REFRACTOMETER FOR CHEMISTS.

Das Totalreflectometer und das Refractometer für Chemiker, ihre Verwendung in der Krystalloptik und zur Untersuchung der Lichtbrechung von Flüssigkeiten. Von Dr. C. Pulfrich, Privatdocenten an der Universität Bonn, und Assistenten des physikalischen Instituts. With 4 Lithographic Plates and 45 Figures in the Text. (Leipzig: W. Engelmann, 1890.)

THIS book contains an exhaustive account of one of the latest devices in physical optics for investigating the refractive power of uniaxial and biaxial crystals. The idea of making use of the principle of total reflection for this purpose is not new. Wollaston, at the beginning of the century, brought forward a method in which the crystal plate under examination was attached to a glass prism; but, owing to the experimental difficulties involved in this process, it met with little practical application. The instrument constructed by Kohlrausch in 1878, in which the crystal plate was immersed in a strongly refractive liquid, was a distinct advance, and has been much used. Within the last ten years, also, Wollaston's apparatus has been considerably improved by Fussner and Liebisch. Both these instruments, however, have still many inconveniences, and it is the claim of the author that the method which he has devised, and which forms the subject of the present work, is free from these.

To give some idea of this method, without entering into practical details, it will be sufficient to state that it consists essentially in the replacement of the prism of the Wollaston instrument by a glass cylinder, to the upper plane surface of which the crystal plate is attached. The cylinder can be rotated about its long axis, so that the refractive phenomena in all azimuths can be observed. This is the distinguishing feature which forms the chief advantage of the new method. Thus, by illuminating the crystal plate from the side at grazing incidence, and slowly rotating the cylinder, the whole extent of the limiting curves of total reflection comes under observation. By a special method of illumination from all sides the limiting curves may be received on a screen beneath the cylinder and made visible to a number of observers; *e.g.* in the case of a uniaxial crystal the appearance on the screen will be the sectional curves of the wave-surface, a circle and an ellipse corresponding to the ordinary and extraordinary rays.

The method was first suggested by the author four years ago. The object of the present work is to give a complete account of the series of measurements and observations which have been made with the instrument since that time with a view to testing its usefulness and trustworthiness. After some preliminary observations on the theoretical principles involved in the method of total reflection, the author gives a detailed description of the construction of the new instrument and the methods of observation by which it is possible in a single crystal section to ascertain the position of the axes of elasticity, to measure the optic axial angle for different colours, and to determine the principal refractive indices. Of special interest is the section on the appearances in the direction of the optic axes of biaxial crystals. Observations made on a plate of asparagine, cut parallel to the optic axial plane, showed distinctly the effects due to the internal and external conical refraction, thus supplementing Lloyd's experiments in demonstrating the general correctness of the Fresnel wave-surface. The last section of the book deals with the refraction of liquids, and contains a description of the refractometer for chemists, which is a simplified form of the total reflectometer, in which a prism replaces the cylinder. Altogether, a perusal of the work leaves the impression that the invention of this ingenious and yet comparatively simple method for investigating the refractive power of doubly refractive media marks a decided advance in physical science; and the author appears to have quite substantiated his claim to have made the total reflection method, which has long been recognized as theoretically the most promising, also a thoroughly practical one. G. T. P.

A WEATHER RECORD OF THE FOURTEENTH CENTURY.

Consideraciones temperiei pro 7 annis, per Magistrem Wilhelmum Merle, socium domus de Merton. Reproduced and Translated under the supervision of G. J. Symons, F.R.S. (London: Edward Stanford, 1891.)

IN January 1337, barely forty-five years after the death of Roger Bacon, and ten years after the accession of King Edward the Third, William Merle, a Fellow of Merton College, and Rector of Driby, in Lincolnshire, commenced a journal of the current weather as experienced partly at his rectory "in Lyndesay, near the north-east coast," and partly at Oxford. This journal he continued month by month for seven years, or up to three years before his death, the notices of the last four years being considerably amplified over the earlier entries; and the original manuscript, still preserved in the Bodleian Library, has now, thanks to the initiation of Mr.

G. J. Symons, been reproduced in *facsimile* by photography, translated from the monkish Latin of the original text by Miss Parker, and published in a handsome small folio volume, of which one hundred copies have been printed. It is probably, as stated on the title-page, the earliest known weather journal in the world.

The manuscript consists of nine and a half pages of abbreviated Latin, written on vellum in a distinct and easily decipherable text, and is apparently in excellent preservation. It is bound up with a number of other manuscript treatises (one of which is also by Merle) dealing with weather prognostication, astrological lore, and other subjects which, according to the scientific views of the day, were nearly related branches of knowledge. Some of these treatises were collected, and some written by, William Reed, who was Bishop of Chichester from 1369 to 1386, and who bequeathed them to scholars of Merton, "being of his kin." Subsequently, the volume passed into the possession of Sir Kenelm Digby, who, in 1634, presented it, together with other manuscripts, to the Bodleian Library. It is interesting and not uninstructive to note how modest a figure is cut, in this scientific record of the fourteenth century, by the few pages of original observation amid the mass of speculative writings in which they are buried; and how in the nineteenth century they alone retain all their pristine value, and are resuscitated with all the honours of *facsimile* reproduction, while the learned treatises on the conjunctions of the planets, the lunar mansions, and rules for prognosticating the weather, are left undisturbed in the musty dignity in which they have reposed for more than five centuries.

As already remarked, Merle's entries are at first very brief, the notice of each month's weather seldom exceeding two lines of the manuscript. Thus for January 1337 we find:—

"In January there was warmth with moderate dryness, and in the previous winter [or the previous part of the same winter?] there had not been any considerable cold or humidity, but more dryness and warmth."

Gradually, however, the notes expand, and it is not a little interesting to trace how by degrees the journalist's growing interest in his probably novel undertaking leads him to record more and more in detail the facts that present themselves to his daily observation. Thus from a brief general summary of the characteristic weather of the month, as illustrated in the above quotation, at the end of the year he proceeds to record the character of each week, and towards the end of the third year (1339) he begins to notice the weather of a few special days. From the beginning of 1340 greater amplification is indulged in: the monthly notes often expand to six or eight lines, and in the final year of the record (1343) sometimes to from ten to fourteen lines. In illustration of these more detailed entries, the notice for July 1343 may be quoted:—

"July.—Considerable heat on the first five days, and it was great on the 3rd and 4th. On the 4th, two or three hours before sunset, heavy thunder began with more vivid lightning than I think I had ever seen, which lasted until midnight, with heavy rain. 5th, light thunder about sunset. On the 6th day and throughout the second week it was gloomy, and there was a slight fog occasionally. 12th, light rain; 14th, gloomy; 15th, and three following days, considerable heat; 19th, rain which penetrated a good deal; 20th, light rain; 22nd, rain; 25th, heavy rain, with heavy thunder in the night, and also in the morning of the following day. All the remainder was rainy, with fog, and rain in small drops, and it was gloomy the whole time. 28th in the night, and 29th in the morning, thunder, with heavy rain. There was lightning with the last two thunderstorms."

For the last four years, indeed, Merle's notes are sufficiently ample to allow of a fair estimate of the weather of those years in comparison with that of the present day, and perhaps some such comparison may be instituted by those who have at command the ample registers of our own time for the same part of Lincolnshire. Seeing how great have been the changes wrought in the character of the surface of the country, by the clearing of forests, drainage, and the extension of agriculture, such a comparison may possibly furnish matter of great interest.

The fourteenth century is sadly memorable for the disastrous famines and pestilences that then desolated England, and above all for the "Black Death," which half depopulated the realm, and was nowhere more fatal than in East Anglia. But this last did not make its first appearance until the end of 1348, about a year after Merle's death, and nearly five years after the conclusion of his journal, which ends abruptly with January 1344; and although a severe famine is recorded in 1335, and another in 1353, it does not appear that any of the years included in his register was especially disastrous. The famine of 1335 is said to have been due to excessive rain, and we may perhaps hazard the surmise that the recent memory of this visitation was the stimulus that induced Merle to record these interesting notes, which good fortune has preserved for us through five and a half centuries.

H. F. B.

OUR BOOK SHELF.

The South Italian Volcanoes. Being the Account of an Excursion to them made by English and other Geologists in 1889, under the auspices of the Geologists' Association of London, with Papers on the Different Localities by Messrs. Johnston-Lavis, Platania, Sambon, Zezi, and Madame Antonia Lavis; including the Bibliography of the Volcanic Districts, and Sixteen Plates. Edited by H. J. Johnston-Lavis, M.D., F.G.S., &c. Pp. 342. (Naples: F. Furchheim, 1891.)

IN this useful volume, Dr. Johnston-Lavis has issued reprints of his report on the Italian excursion made by the members of the Geologists' Association under his direction, and of his abridged sketch of the geology of Vesuvius and Monte Somma, already noticed in this journal. These reprints are accompanied by several interesting original papers—namely, one on the thermomineral and gas springs of Sujo, near Roccamonfina, by Dr. Johnston-Lavis himself; one on the geology of Acireale, by Signor G. Platania; another entitled "Notes on the Eolian Islands and on Pumice-stone," by Dr. L. Sambon; and lastly a chapter on "The Travertine and Acque Albule in the neighbourhood of Tivoli," by Signor Pietro Zezi. These various memoirs occupy 88 pages of the volume, the remainder being devoted to a very useful bibliography of Italian vulcanology, compiled by Dr. Johnston-Lavis and Madame Antonia F. Lavis.

Not the least valuable portion of the work is the series of beautiful photographs taken by Dr. Johnston-Lavis from well-selected points of view, and admirably reproduced as small quarto plates. These plates are striking illustrations of what can be accomplished by instantaneous photography as an aid to vulcanological study. Among them are very instructive views of explosive outbursts from the craters of Stromboli and Vulcano. In the case of the small explosions from the first-mentioned volcano, the ejected fragments are seen in the midst of the steam-clouds; and in the case of the more violent eruptions from Vulcano several phases in the same outburst have been caught at intervals of a few seconds. Those who already know this very interesting district will be glad to have their recollections revived by these admirable plates; and those who have never had the pleasure of visiting the South Italian volcanoes may obtain from these remarkable photographs a much better idea of the localities than any descriptions or drawings can possibly give.

Buried Cities and Bible Countries. By George St. Clair, F.G.S. (London: Kegan Paul, Trench, Trübner, and Co., 1891.)

EVERYONE knows that recent archæological research has brought to light a vast number of facts which are directly or indirectly connected with ancient Hebrew history. The object of the author of the present work is to set forth the more important of these facts, and to explain their significance. He deals with the results of exploration in Egypt, Palestine, and Mesopotamia; and he has a chapter on Jerusalem, with regard to the topography of which he has been led to conclusions different from those of other writers. The book has been prepared for the benefit of persons "who have no time to follow the course of exploration, and no taste for technical details"; and readers of this class will find in it much that will be to them both new and interesting. The value of the text is increased by good maps, plans, and other illustrations.

Food, Physiology, &c. By William Durham, F.R.S.E. (London and Edinburgh: A. and C. Black, 1891.)

THIS is the third volume of a series by Mr. Durham, entitled "Science in Plain Language." The author does not pretend to say anything new, but he has brought together, and arranged clearly, a mass of facts which will no doubt be of interest, and may be of practical service, to many readers who have neither time nor inclination for the study of more elaborate treatises. He begins with the consideration of solid and liquid foods, then gives some account of the constituents of food, and finally sketches the structure and functions of the bodily organs.

Blackie's Science Readers. (London: Blackie and Son, 1891.)

THE aim of this series is to arouse the interest of children in the common objects of the natural world, and to give them some insight into the processes by which articles of ordinary use are produced. The idea is excellent, and has been very successfully worked out. The series consists of five little volumes, the first two of which present some "lessons on common objects." From the third volume the reader will learn something about the simple principles of classification; about substances used in arts and manufactures; about phenomena of earth and atmosphere; and about matter in three states—solids, liquids, and gases. The fourth and fifth volumes—by the Rev. Theodore Wood—deal with animal and plant life. The facts set forth have been carefully selected, and they are presented in a bright, easy, natural style which cannot fail to make them at once intelligible and attractive. Good teachers will find the series of real service in helping them to foster in the minds of their pupils a love of accurate observation and independent reasoning.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Comparative Palatability.

WITH the view of supplementing the experiments carried out last year by Mr. F. Finn and myself (NATURE, vol. xlii. pp. 571, 572), I have been feeding, during August and September, specimens of the common frog and toad.

Among Hymenoptera, *Bombi* are readily taken by frogs. I have records of *B. lapidarius* (drones and workers), *terrestris* (queens and workers), and *muscorum* (drones and workers). On one occasion only a freely-feeding frog refused to attack for the second time a large queen of *terrestris*, which had stung its mouth. Many of the insects were, however, thus taken at the second attempt. The common wasp was eaten eagerly by frogs and toads. I was again unfortunate in not taking any *Chrysididæ*. *Sirex gigas* was attacked both by a frog, for which it seemed too large, and by a toad, under whose lip it appeared to insert its ovipositor. Neither animal ventured to seize it again—certainly for an hour or so. I was then obliged to abandon the observation. I could get no large ichneumons.

Of Lepidoptera, *Vanessa urticæ* was taken by frogs and toads, and *V. io* by a frog. Three or four specimens of *Pieris rapæ* and *napi* would be taken in succession by a frog, which also ate *P. brassicæ*. The insects' flutterings did not seem to matter: more than once they were taken on the wing. A toad once took *P. rapæ*. I was surprised to see a frog seize a dead specimen of this butterfly, which had been lying for several hours in the inclosure. It was partially swallowed, but rejected after some seconds—having unfortunately been taken together with some cedar needles. *Plusia gamma* was eaten eagerly by both frogs and toads. Hairy caterpillars (*e.g.* of *Orgyia antiqua* and *Spilosoma* sp.) were taken by a frog. Smooth green larvæ were eaten greedily.

Of Diptera, *Eristalis tenax* was eagerly seized by frogs and toads. A red-tailed, long-winged fly was eaten by a frog.

Blatta orientalis was taken without hesitation; as were, of course, earthworms.

Of three frogs under observation, only one was of much working value. This specimen (a male) became in a fortnight so tame as to attempt to take the handle of the butterfly-net with which I placed the insects, &c., in the inclosure. This fact recalls Mr. E. B. Poulton's observation, that his tree-frogs seized the end of the forceps with which food was given them.

It is, perhaps, worthy of notice that the larvæ of the blow-fly, though eaten eagerly by toads, are frequently passed whole from the body; and would, therefore, seem to be with difficulty digested.

Want of time has prevented my experimenting, as I had wished to do, with *Salamandra maculosa*. Mr. F. Finn offered a specimen to ducks, which will eat the small newt, and found that though more than one bird observed it, and one even ran towards it, it was not touched. The observation extended over more than an hour.

E. B. TITCHENER.

Mote House, Mote Road, Maidstone, September 25.

Alum Solution.

DANS le no. 1141 de votre excellente Revue, M. Napier Draper demande pour quelle raison la solution d'alun a été universellement adoptée pour l'absorption des radiations de grande longueur d'onde. Ce n'est point pour répondre à cette question que je vous écris, car, pas plus que votre correspondant, je ne connais d'expériences directes suffisamment exactes desquelles il résulterait que la solution d'alun absorbe plus que l'eau pure. Je hasarderai, cependant, une explication: l'eau est un des liquides transparents les plus absorbants; l'alun occupe un rang analogue parmi les solides; en dehors de toute vérification, si l'absorption sélective de chacun de ces corps s'exerce sur une partie différente du spectre, on peut supposer que leur mélange exerce une absorption plus complète que chacun des corps pris isolément.

A cette occasion, je prendrai la liberté de relever une erreur que l'on a fréquemment commise dans ces derniers temps au sujet de l'absorption des radiations infra-rouges par l'eau. On a coutume de définir le rendement d'un foyer de lumière par le rapport de l'énergie située dans la partie visible du spectre à l'énergie totale rayonnée par le foyer. Sans insister sur ce que cette définition a de défectueux (je traiterai prochainement cette question dans la *Revue générale des Sciences*), je rappellerai qu'on mesure d'ordinaire le rendement en recevant successivement sur un radiomètre quelconque (pile de Melloni, bolomètre, radiomicromètre de Boys) la radiation totale du foyer, et la radiation qui a traversé une certaine épaisseur d'eau ; on admet que les radiations obscures ont été retenues, et on fait le quotient de ces deux quantités. Aucun physicien, je suppose, ne croit que l'absorption par l'eau commence à l'endroit précis où cesse la vision, et devient immédiatement totale, mais on pense en général que le résultat ainsi obtenu est assez approché.

Or nous pouvons déterminer directement le rendement photogénique d'une source en mesurant la superficie des courbes d'énergie rayonnante visible et invisible. En partant des nombres de M. Langley, on trouve ainsi, pour le rendement d'une lampe à gaz une valeur comprise entre 1 et 2 pour cent. D'autre part, les recherches de M. Knut Ångström ont montré que l'absorption par l'eau est presque nulle pour $\lambda = 1\mu$, et n'est totale qu'à partir de $\lambda = 2\mu$ environ. Une couche épaisse d'eau laisse passer près de 10 pour cent de l'énergie rayonnante invisible. La méthode ordinaire donnerait donc, pour le rendement d'une lampe à gaz, 11 à 12 pour cent, c'est à dire une quantité six fois trop forte.

Je ne quitterai pas ce sujet sans faire remarquer le singulier usage en vertu duquel la puissance de la radiation solaire est rapportée à la *minute*, tandis que toutes les puissances possibles—cheval, *horse-power*, watt, ainsi que toutes les radiations—sont exprimées par rapport à la seconde. Il serait temps de faire disparaître cette anomalie.

Ch. Ed. Guillaume.

Pavillon de Breteuil, Sèvres, France,
25 septembre, 1891.

Weather Cycles.

With reference to this most interesting question, may I be allowed to call attention to the following figures? Having had to consult Dr. Rutty's "Natural History of Dublin," 1772, vol. ii., I casually found on p. 353 of that volume, in his remarkable detailed registry of the weather in Dublin for a long series of years, the following remark: "It has been remarked that the following years were memorable for great frosts in England, viz. 1638, 1661, 1684, 1708, 1716, 1739." Now the intervals between these dates are 23, 23, 24, 8, 23. He further remarks, on p. 368:—"It is to be observed that whereas since the great frost of 1739, until the latter end of the present summer, 1744, we had generally an unusual prevalence of dry weather, in autumn our usual wet weather returned." It may be remarked that the interval of 23 years is about double the sun-spot period, and furthermore that the years mentioned by Rutty correspond roughly with years of sun-spot minima or maxima as given in Wolf's Catalogue, mentioned by Guillemin in his work "Le Ciel" (1877), p. 104. This correspondence would appear as follows:—

Sun-spot Year.	Interval.	Great colds.	Interval.
1639·5 min.		1638	
	20·5		23
1660 min.		1661	
	25		23
1685 min.		1684	
	20·5		24
1705·5 min.		1708	
	12·5		8
1718 min.		1716	
	20·7		23
1738·7 min.		1739	
	16·8 } 22·8		15 } 23
1755·5 max.		1754	
	6·0		8
1761·5 max.		1762	

J. P. O'Reilly.

Royal College of Science for Ireland, Stephen's Green,
Dublin, September 25.

Occurrence of the Ringed Snake in the Sea.

The readiness with which the British snake (*Tropidonotus natrix*) will enter fresh water is well known. Its occurrence in the sea seems anomalous, and therefore I venture to submit the following details.

The specimen in question was seen on September 7, from a small boat on the east coast of the Isle of Wight, while about a thousand yards distant from the shore, and about midway between Shanklin and Luccombe Chines. When first seen it was swimming straight out to sea—viz. in an easterly direction. The sea was calm and a strong current was flowing from the south, so that the creature was swimming across the current. At first it took no notice of the boat, but as the boat was rowed towards it, it changed its course and swam directly away from the boat. It was soon captured, and found to be uninjured and in good condition. Upon dissection it proved to be a male; the entire alimentary canal was absolutely empty. The internal organs were free from disease or other abnormality. It measured 33 inches in length. It is most probable that this snake entered the sea about a mile from where it was obtained, as the beach is bounded by almost perpendicular cliffs, some 300 feet high, at that place.

J. Cowper.

A Rare Phenomenon.

Mr. Wilson's letter in your issue of September 24 (p. 494), recalls what I myself saw on the same evening. On Friday, the 11th, I was returning with a friend to town after a day's ramble in Epping Forest. We caught the 8.36 p.m. train at Epping, which is due at Woodford at 8.59, and was, I think, only a few minutes late. Just as the train was nearing Woodford Station, my friend and myself simultaneously noticed a luminous band, such as that observed by Mr. Wilson, and extending from the horizon almost to the zenith. Our first unreflecting thought was to refer it to the revolving light at the Naval Exhibition, only it did not revolve, and the direction was quite wrong. The fact that both of us thought of this is indicative of the appearance which the luminous beam bore. The night was clear and starlit, and I observed that the point in the horizon from which the beam rose was almost under the Great Bear, but a little to the left as I faced it. We saw it only for a minute or two before it was hidden from us by the shed of Woodford Station, in which station we stayed for what seemed a long while. When we got into the open country again, the phenomenon had disappeared. I may add, that my own eye being unfortunately defective for red, I asked my companion if he noticed any red tinge in the light, and he answered that it seemed quite white.

Burlington House. Herbert Rix.

The narrow luminous band described in Nature, September 24 (p. 494) was seen here on Friday, the 11th inst., between 8.30 and 9 p.m., at the same time at which it was seen by Mr. Wilson in the county Westmeath, but about twenty-two hours later than it was seen by Prof. Copeland in Aberdeenshire. It passed close south of Cassiopeia, and nearly through the zenith. Half an hour later it had drifted 8° or 10° southward, and had become very faint.

There can be little doubt that the very rapidly moving "comet" seen by Mr. Eddie at Grahamstown, South Africa, on October 27, 1890, was a phenomenon of this kind.

J. L. E. Dreyer.

The Observatory, Armagh, September 28.

The Heights of Auroras.

The rare part of the phenomena described by your correspondents is the extreme narrowness of the auroral arches seen on the 10th in the north of Scotland, and on the 11th at Ryde. I take all the other descriptions on the 11th to refer to one arch —a different one from that seen at Ryde; and it was a much wider one, and therefore less unusual, its width having been about 5° as seen here. Your correspondents do not give its width, except that, as seen from Nottingham, it was evidently very broad, and is not stated to have been an arch at all, though I should suppose it was one. The observation at Nottingham Forest, compared with those further north, gives a good opportunity for ascertaining the height of the top of the aurora; but, as Mr. A. Marshall has not given the altitude of the base of the aurora as seen from Nottingham, there are no materials for calculating the height of that. I made several observations of the position of the central line of the arch. I might specify that at 9.25 it was at R.A. 20h. 42m., Decl. + 33½°, and R.A. 0h. 43m., Decl. + 33°, and it moved very slowly.

Is it not time some systematic effort was made to calculate the heights of auroras? A good many observations have been made on this point, showing great variation in height; and yet, beyond the conclusion that it seems probable they may be seen at lower elevations nearer the magnetic pole than elsewhere, we know nothing as to whether they vary in height with the place, the time, or the nature of the auroras. Now is the time, seeing that auroras appear to be becoming more numerous than they have been for many years past. T. W. BACKHOUSE.

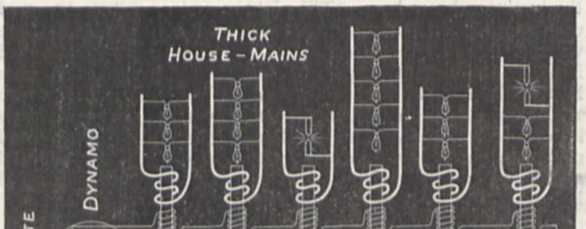
West Hendon House, Sunderland, October 5.

SOME NOTES ON THE FRANKFORT INTERNATIONAL ELECTRICAL EXHIBITION.[1]

III.

From One Hundred to Twenty Thousand Volts.

THE incandescent lamp having, by 1885, reached a fair degree of perfection, it appeared that the one need still remaining, in connection with the distribution of the electric light over a large area, would be supplied by the use of transformers. For a transformer with many convolutions of fine insulated wire on one coil, and a few convolutions of thick insulated wire on the other, would transform a large pressure and small current into a small pressure and large current; hence, if such a transformer were placed in each house, it would be possible to light up even a scattered district by a comparatively fine wire from a central station, whereas previously it had seemed



TRANSFORMERS IN SERIES
GAULARD AND GIBBS' ORIGINAL ARRANGEMENT.
FIG. 2

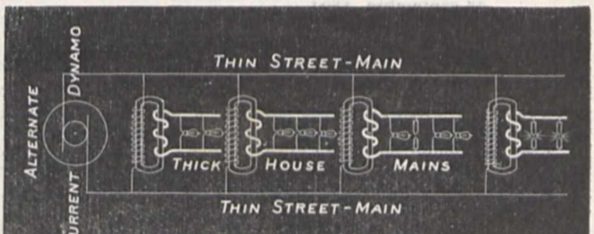
that it would be necessary to use copper conductors many square inches in cross-section to light many houses even when at no great distance from one another.

Hence, in the autumn of 1885 we find Messrs. Gaulard and Gibbs making preparations at the Grosvenor Gallery, Bond Street, for establishing there the pioneer central station for London.

But the method they adopted was that of placing the transformers in series, as seen in Fig. 2, and this system has the great disadvantage that the brightness of the electric lamps in a house cannot be kept automatically constant when other lamps in the same house are turned on or off. There are, of course, two conditions to be fulfilled in electric lighting: one, that turning on or off lamps in one house shall not affect the brightness of the lamps in any other house; the other, that turning on or off lamps in one room shall not affect the brightness of the lamps in any other room of the same house. With transformers in series, the first condition is satisfied by keeping the alternating current which passes through the fine wire or *primary coil* of the transformer perfectly constant; but this does not render the potential difference between the wires from the *secondary circuit*, or house mains, independent of the current in this secondary circuit—that is, independent of the number of lamps turned on in the house. Consequently, the series arrangement of transformers adopted by Messrs. Gaulard and Gibbs, while rendering the lamps in one house independent of those in another, did not attain the same result for lamps in different rooms of the same house.

Complaints, therefore, became general. Various unsuccessful devices were tried to remedy this evil, when an application was received from Mr. Sebastian Ziani de Ferranti to be allowed to try a transformer which he had designed. The application was accepted, for Mr. Ferranti, although quite young, was already known as having constructed an ingenious alternate-current dynamo, and in February 1886 the charge of the Grosvenor Gallery central station passed over into Mr. Ferranti's hands.

The new engineer recommended that the system of placing the transformers in series should be totally discarded, and that a parallel arrangement should be adopted



TRANSFORMERS IN PARALLEL
ARRANGEMENT NOW ADOPTED
FIG 3.

in its place, as in Fig. 3, because a well-made transformer had this important property—that if the potential difference at the terminals of the primary coil were kept constant, the potential difference between the terminals of the secondary coil would also remain nearly constant whatever were the current passing through this circuit; so that if the pressure between the street mains were always kept the same, the brightness of the lamps would hardly be affected either by turning on or off lamps in the same or in any other house.

Placing the transformers in parallel, however, would necessitate working at a low pressure, said the press, and would rob the transformer system of all its value, for "it is surely not proposed for one moment to work a parallel system where the primary has a difference of potential of 2000 volts." However, that is exactly what Mr. Ferranti not only proposed to do, but what he actually carried out on a large scale, so that his mains by 1888 stretched from Regent's Park to the Thames, and from Chancery Lane to Hyde Park, supplying current to some 20,000 glow-lamps. The Board of Trade had made regulations, about 200 volts being the maximum pressure permitted in a house; Parliament had passed the Electric Lighting Act of 1882, containing clauses rendering the development of the electric lighting industry well nigh commercially impossible; but Mr. Ferranti overcame all these legalities by bridging his mains from house-top to house-top, instead

[1] Continued from p. 524.

of putting them under the streets and himself under the control of the authorities.

But every corner at the Bond Street central station had soon to be utilized; a dynamo weighing tons had on one occasion to be lifted into position over a steam-engine necessarily kept always running to maintain a constant supply of current to the houses. New customers were added daily to the list, more and more current had to be generated nightly, in the face of engineering difficulties, and in the teeth of injunctions against smoke, injunctions against dust, and injunctions against noise.

A fresh start became imperative, so it was decided to build at Deptford, 6 miles away from Bond Street, a vast generating station, which should be the largest in the world, and to use the Grosvenor Gallery, and probably fresh sites to be obtained in town, merely as transforming stations. In the mains between Deptford and London it was decided to employ 10,000 volts, to be reduced to 2400 in London, and the power then distributed by the existing overhead mains, and again reduced to 100 volts on entering the houses, as before.

The scheme was a far-reaching one; permission was asked from the Board of Trade by the London Electric Supply Corporation, the outcome of the original Grosvenor Gallery Syndicate, to run wires along 27 railways and through 30 parishes; two dynamos, each to furnish 1250 horse-power at 10,000 volts, were built with special engines to drive them, as seen in Fig. 4, and a cable laid to London. But on starting the dynamos, when they were completed, it was found that the insulation of the cable would not stand 10,000 nor even 5000 volts; and for a time power was supplied direct from Deptford to the houses in London, one transformation at the houses themselves being alone effected.

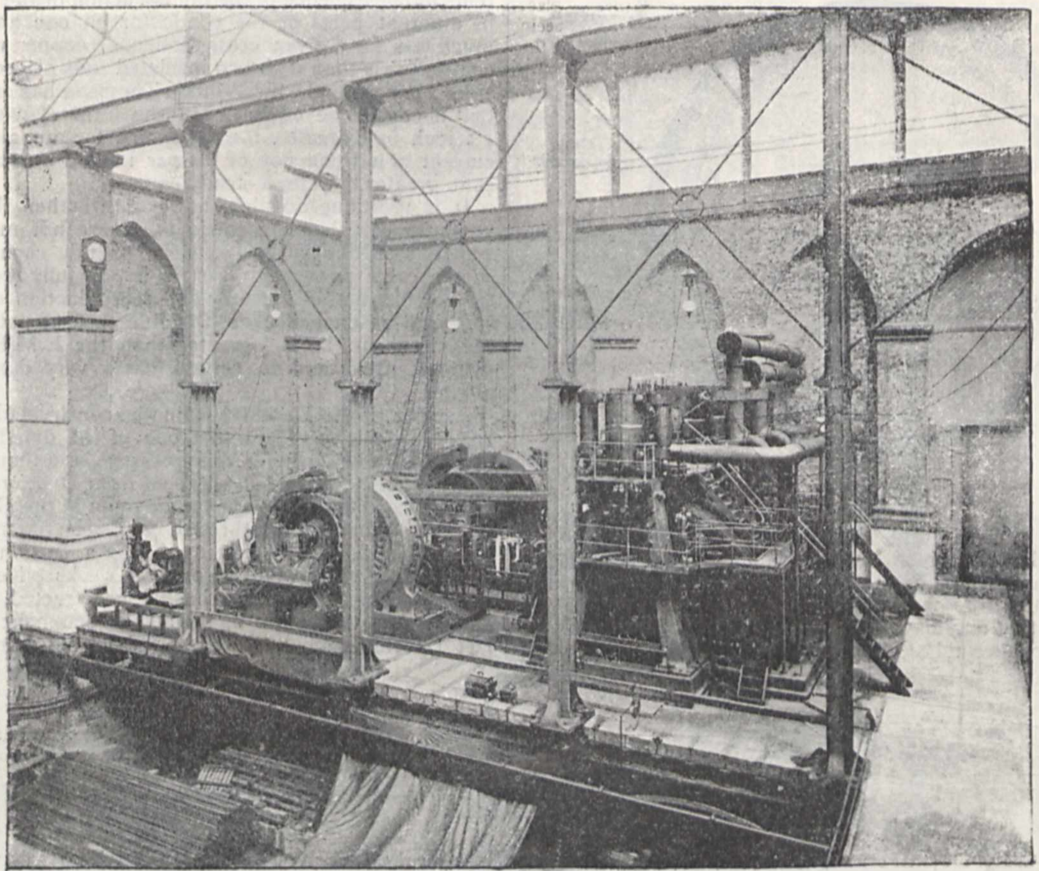


FIG. 4.—Two 1250 horse-power dynamos (opened for inspection) at Deptford.

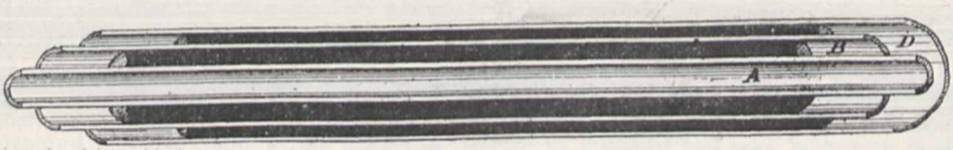


FIG 5.—Longitudinal section of the Ferranti main. A, inner copper tube; B, outer copper tube; D, iron protecting tube; waxed paper insulation shaded black.

Then Mr. Ferranti carried out his original intention of constructing the main of two concentric copper tubes, to serve respectively as the going and return conductor. The inner copper tube, 20 feet long, seen in section, A,

Fig. 5, has brown paper soaked in ozokerit rolled round it to a thickness of about five-eighths of an inch. Outside this is slipped a larger copper tube, B, Fig. 5, and the whole is drawn through a taper die under great pressure, which has the effect of forcibly compressing the paper and consolidating the mass. Next, more brown paper

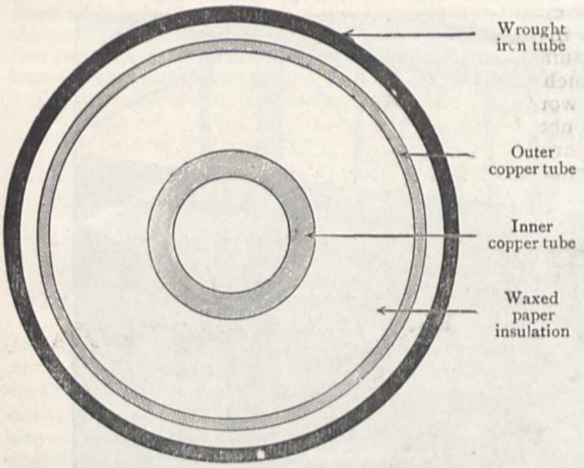


Fig. 6.—Cross-section of the Ferranti main; full size.

soaked in melted ozokerit is rolled on, to a thickness of one-eighth of an inch, and the whole slipped loosely into an iron tube, D, Fig. 5, which protects the cable subsequently from mechanical injury. To fill up any air spaces that may have been left between the iron and the outer copper tubes, the 20-feet section is placed over a

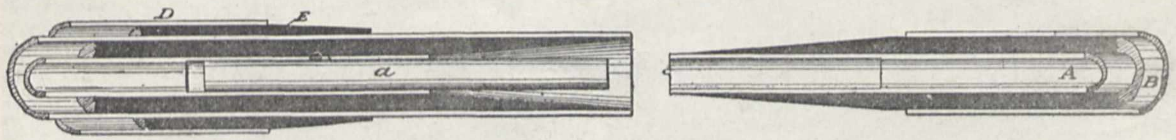


Fig. 7.—Ends of two pieces of main, tapered ready for jointing. *a*, copper rod to make electric connection between inner copper tubes; E, waxed paper coned like a pencil.

fire, and melted wax pumped in between the two through a tube inserted in a hole drilled in the middle of the iron tube.

Fig. 6 shows a cross-section of the finished main full size, and as the sectional area of the metal in each of the copper tubes is about a quarter of a square inch, the main can transmit about 2000 horse-power at 10,000 volts.

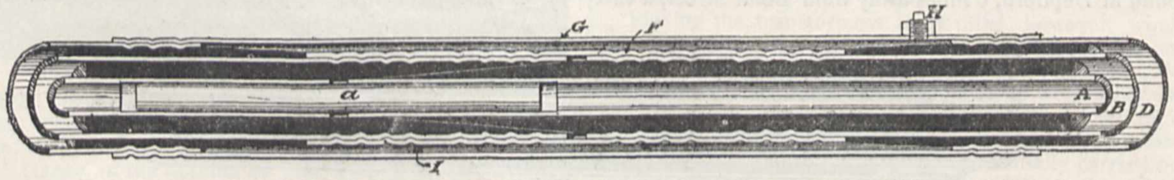


Fig. 8.—Ferranti main, jointed. F, copper sleeve slipped over two ends of outer copper tubes, and then corrugated with special tool; G, iron sleeve slipped over two ends of iron tubes, and corrugated with special tool; H, screw-hole to run in melted wax, I.

The main being constructed in lengths of only 20 feet, some 1500 joints have had to be made in 6 miles of main, or 6500 joints altogether in the five mains which have been laid from London to Deptford. These joints have been made without solder, in the way shown in Figs. 7 and 8, pressure alone between the copper tubes having been relied on to maintain good contact.

The object of using concentric tubes is twofold—first, as the outer copper tube is kept practically at the potential of the earth, it is impossible to get a severe shock unless the inner tube is touched, and this, of course, can only be done by first cutting through the outer; second, the effective increase of the resistance and of the self-induction which occurs with rapidly alternating currents in consequence of the mutual action of the currents in different parts of the conductor on one another is much less for a given cross-section of copper with concentric tubes than with two insulated rods placed side by side. For example, Sir William Thomson has calculated that if copper be employed in the form of a solid rod, 1·2 inch in diameter, the resistance for an alternating current of a frequency of 80 per second will be 31 per cent. greater than for a steady current.

It is very questionable, however, whether these advantages of using concentric tubes are not more than compensated for by the large electrostatic capacity that such a cable possesses. For, as is now fully recognized, the combination of capacity and self-induction can by a species of resonance cause the difference of potential in the circuit to be far greater than the E.M.F. of the dynamo itself, and in certain cases very dangerously greater.

As soon as the Deptford main was constructed to stand 10,000 volts, it was found that one of the dynamos seen in Fig. 4 broke down at this pressure, and therefore for many months the current was sent from Deptford at only 5000 volts; next, the transformer room at the Grosvenor Gallery was burnt down through carelessness, some £8000 worth of transformers destroyed, and a portion of London left in darkness for two or three weeks. New transformers were hastily, too hastily, constructed, and the current was turned on again at the commencement of last December; but after a few days the transformers were, one after another, short-circuited by the electric current sparking from the primary coil to the iron core of the transformers, and all the houses on the London Electric Supply Corporation's system again left in darkness during the nearly perpetual night of a densely foggy winter. The Metropolitan Electric Supply Company—which also distributes an alternating current by means of transformers, but from several central stations in the heart of London itself, and therefore requiring to use only 1000 volts and a single transformation—came to the rescue in certain districts, but in others the householders had to be left to their fate, as it would have been far too expensive to run special mains from the Metropolitan Company's stations merely as a temporary expedient.

Finally, in March of this year, current was again turned on from Deptford, at the pressure originally proposed, viz. 10,000 volts. It was not, however, supplied from the dynamos illustrated in Fig. 4; but, instead, Messrs. Deprez and Carpentier's plan of transforming up and transforming down again, illustrated in Fig. 1, p. 522, was employed. For, by this time, two dynamos, formerly at the Grosvenor Gallery, each of 600 horse-power, had been taken to Deptford and erected there, as seen in Fig. 9; new steam-engines, more powerful than those formerly employed at the Grosvenor Gallery, having been constructed to drive them.

These dynamos generate the current at 2400 volts, then, by means of transformers at Deptford, this is raised to 10,000 volts. On the power arriving in London, the pressure is transformed down again to 2400 volts, and at the houses there is a further transformation of this 2400 volts to 100 volts. There are, therefore, no less than three transformations of pressure between the dynamo terminals at Deptford and the lamps in the houses in London.

Regarded as a gigantic experiment in electrical engineering, the Deptford scheme has achieved a gallant victory, for, with a buoyancy that no disaster could crush, and with the determination of a Napoleon to conquer every mechanical and electrical obstacle in the way, Mr. Ferranti has step by step succeeded in distributing current to quite distant parts of London at a pressure which in 1885 was regarded as quite impracticable, and for the last seven months he has been sending the power to London at a pressure which, even at the end of last year, was deemed simply visionary.

But as a commercial undertaking the Deptford transmission is a dreary failure, since what is the advantage of transmitting the current 6 miles that is in any way commensurate with the capital already expended? When power can be obtained very cheaply, from a rapid river for example, it may be highly remunerative to transport it in some such way as is now being done between Lauffen and Frankfort. But can power be obtained so much more cheaply at Deptford than in London to make it worth while transmitting it over 6 miles? Land undoubtedly costs much less down the river than in the heart of London, coal can be very easily brought to a generating station on the banks of the Thames, and the water might perhaps be employed to work condensing steam-engines; but such economies can only compensate for a fraction of the yearly interest on the capital expended on the Deptford scheme. Indeed, even if the station at Deptford had been built with rigid economy, and only large enough for the present demand, it is questionable whether the loss of power in three transformations of the pressure would not eat up much of the saving that could be effected by having the generating station quite out of London.

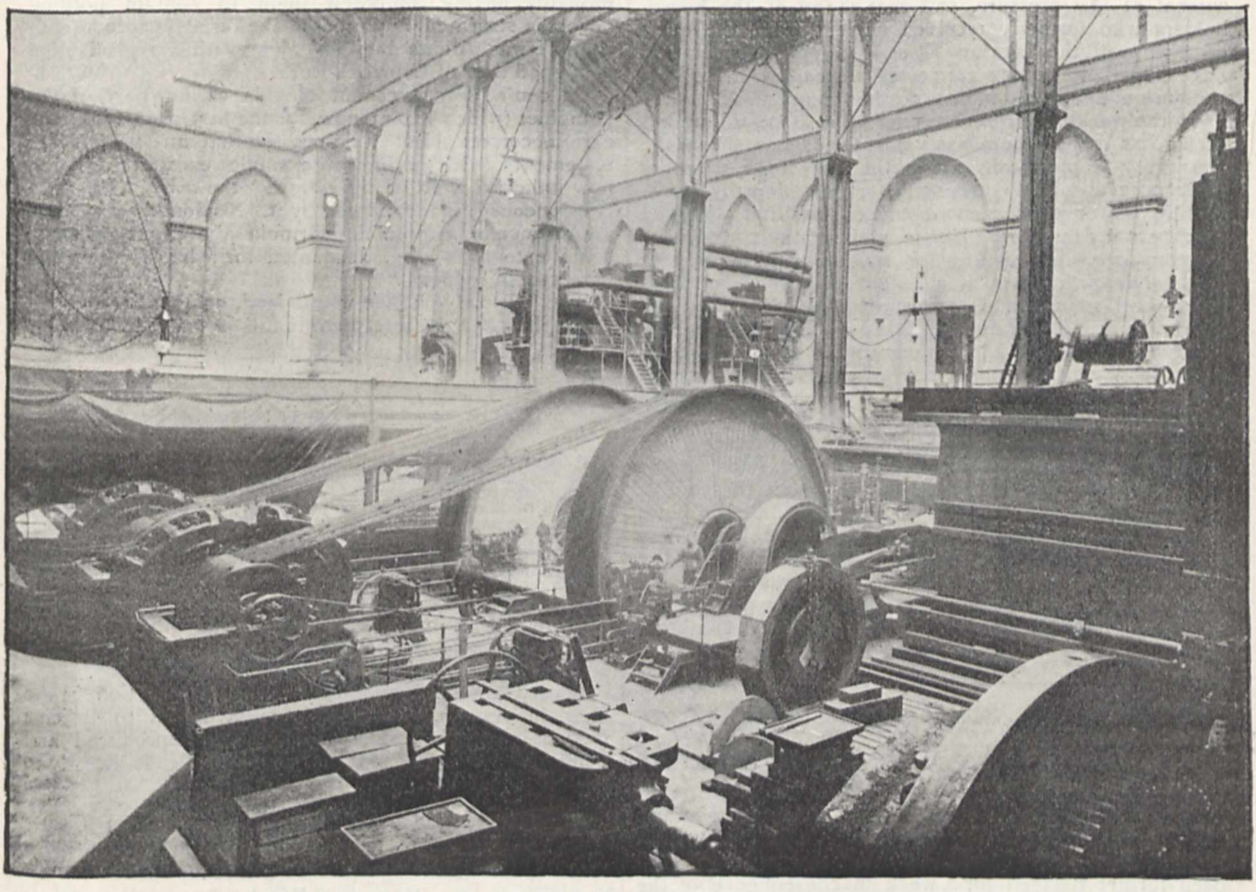


Fig. 9.—Two of the Grosvenor Gallery dynamos re erected at Deptford and driven by new steam-engines. Two 1250 horse-power dynamos at the back.

As it is, however, the London Electric Supply Company have been so engrossed with the electric lighting of London in the *future*, that they have practically ignored the present wants of the householder; the vast building at Deptford has been constructed to carry a second story of boilers and engines, when it is very doubtful if even the present story can be wholly utilized for a long time to come; rows of boilers and furnaces were erected some two or three years ago to supply steam to drive dynamos which are not yet made; tens of thousands of pounds have been expended on machinery to be employed in constructing two ten-thousand horse-power dynamos, and the armature of one of them, 43 feet in diameter, has had to be left abandoned only half finished, because there is neither money nor present need for such a dynamo at Deptford.

And while all these provisions for the future electric lighting of London on a vast scale were slowly proceeding, the present customers were left sometimes for hours, sometimes for days, and occasionally even for weeks in darkness: what wonder is it, then, that all over London there have have been growing up central stations supplying a direct current at low pressure, and that many of the householders who formerly received current from the overhead wires of the London Electric Supply Corporation have had their houses connected instead with the low-pressure underground mains of other companies?

To the world at large, however, the Deptford undertaking has been of immense value, for it has shown the possibility of practically using the very high potential differences absolutely necessary for economically transmitting power over such distances as that between Lauffen and Frankfort. Hence, maintaining 20,000 volts between *bare* wires running for 109 miles along the side of the Neckar railway, at a height of only 16 feet from the ground, sounds much less startling now than did Mr. Ferranti's proposal made and acted on five years ago to bring only one-tenth of this pressure, by means of india-rubber covered conductors, into locked transformer rooms built of brick in the basement of the houses supplied with current from the Grosvenor Gallery.

In fact, the results that have been attained through Mr. Ferranti's undaunted courage, and the well-filled purses of his friends, have led people to look on a pressure of 20,000 volts as they regard a velocity of 70 miles an hour, so that to day, in order to prevent boys climbing up any one of the 3000 ordinary telegraph poles which carry the wires from Lauffen to Frankfort, it is thought sufficient to merely paint a skull and cross-bones on every post as an indication of the deadly fate that awaits the climber.[1]

(*To be continued.*)

[1] We have to thank the *Electrician* and the *Electrical Review* for some of the illustrations used in this article.

ON VAN DER WAALS'S TREATMENT OF LAPLACE'S PRESSURE IN THE VIRIAL EQUATION: IN ANSWER TO LORD RAYLEIGH.

My dear Lord Rayleigh,—As you are aware, I did not see your letter of September 7 (Nature, 24/9/91) till a fortnight after its date; and my reply has been further delayed for a week in consequence of the closing of Edinburgh University Library at this season. Even now I can refer only to the German version of Van der Waals's pamphlet.

Partly on account of its unfamiliar language, but more especially on account of a very definite unfavourable opinion expressed by Clerk-Maxwell (Nature, 15/10/74) I did not attempt to read the pamphlet when it appeared; and it was not till 1888 that, in consequence of some hints from Dr. H. Du Bois, I hastily perused it in its German form.

The passage which you quote from my paper (where, by the way, the printers have unfortunately put *resistance* for *resilience*) is certainly not a very accurate description of Van der Waals's method, but it represents faithfully the difficulties which I felt on first reading the pamphlet. I said that Van der Waals's "justification of the introduction of the term a/v^2 into an account already closed, as it were, escapes me." And I am not surprised that it did so. For the statement of Clerk-Maxwell had prepared me to look for error; and when, at the end of Chap. VI., I met with the formula

$$p(v - b) = \mathrm{R}(1 + at),$$

which, a couple of pages later (nothing but general reasoning intervening), somehow developed itself into

$$\left(p + \frac{a}{v^2}\right)(v - b) = \mathrm{R}(1 + at),$$

I naturally concluded that this was the matter adverted to. I spoke of the first of these equations as a "closed account," because of the process by which b had been introduced. To this point I must presently recur.

I had not examined with any particular care the opening chapters, to which your letter chiefly refers; probably having supposed them to contain nothing beyond a statement and proof of the Virial Theorem (with which I was already familiar) along with a reproduction of a good deal of Laplace's work.

Of course your account of this earlier part of the pamphlet (which I have now, for the first time, read with care) is correct. But I do not see that any part of my statements (with perhaps the single exception of the now italicized word in the phrase "the *whole* procedure is erroneous") is invalidated by it. No doubt, the sudden appearance of a/v^2 in the formula above quoted is, to some extent at least, accounted for; but is the term correctly introduced?

The formula you give would lead, on Van der Waals's principles as to the interpretation of $\frac{1}{3}\Sigma(m\mathrm{V}^2)$, to

$$v(p + \mathrm{K}) = \mathrm{R}(1 + at),$$

or

$$v\left(p + \frac{a}{v^2}\right) = \mathrm{R}(1 + at).$$

But how can the factor $(v - b)/v$, which Van der Waals introduces on the left in consequence of the finite diameters of the particles, be justifiably applied to the term in K as well as to that in p? Yet to apply it so is essential to Van der Waals's theory; for without it the resulting equation will not give a cubic in v, and cannot therefore be applied to the isothermals for which it is required. And, in any case, it could scarcely be said that the K term, after being manipulated in this manner, is, in any strict sense, "extracted from the term $\Sigma(\mathrm{R}r)$."

A very strange thing appears, in this connection, in the German version. A result, due it seems to Lorentz (which, in ignorance of his work, I had reproduced and published in the first part of my paper), leads directly to the equation

$$pv = \mathrm{R}(1 + at)\left(1 + \frac{b}{v}\right);$$

which is then put in the confessedly approximate form

$$p(v - b) = \mathrm{R}(1 + at).$$

Of this it is remarked:—"was genau mit dem obigen Resultate [that obtained by the use of the factor $(v - b)/v$] übereinstimmt." It is obvious that, when we have to divide both sides by $v - b$, we ought to restore the proper factor on the right; and thus that the equation ought to take the final form

$$p + \frac{a}{v^2} = \mathrm{R}(1 + at)\,\frac{v + b}{v^2},$$

instead of the more convenient form

$$p + \frac{a}{v^2} = \frac{\mathrm{R}(1 + at)}{v - b},$$

in which Van der Waals employs it. But then it would not give the required cubic in v!

I think that the mere fact of Van der Waals's saying (in a passage which is evidently applicable to his own processes, though it is applied only to that of Lorentz) "die ganze Rechnung doch nur bis auf Grössen der ersten Ordnung (wie b/v) genau ist" throws very grave doubt on the whole investigation. For in the most interesting part of the critical isothermal of CO_2 the fraction b/v cannot be looked upon as a small quantity of the first order. In fact, without raising the question, either of Van der Waals's mode of interpreting the term $\frac{1}{2}\Sigma(mV^2)$ or of the paucity of constants in his equation, the above consideration would of itself render the results untrustworthy. Van der Waals has most opportunely and effectively called attention to an exceedingly promising mode of attacking a very difficult problem, and his methods are both ingenious and suggestive; but I do not think that his results can be regarded, even from the most favourable point of view, as more than "*Guesses at Truth.*"

For, if we take the experimental test, there can be no doubt that (as I have stated in § 65 of my paper) "Van der Waals's curves cannot be made to coincide with those of Andrews." And I think I have given reasons for believing that "the term of Van der Waals's equation, which he took to represent Laplace's K, is not the statical pressure due to molecular forces but (approximately) its excess over the repulsion due to the speed of the particles." Of course I mean by this that, when Van der Waals, comparing his equation with experiment, assigns a numerical value to his term a/v^2, he is not justified in regarding it as the value of Laplace's K; though that quantity was, he tells us, the main object of his inquiry.

Believe me yours very truly,

P. G. TAIT.

St. Andrews, September 28.

THE EXISTING SCHOOLS OF SCIENCE AND ART.

AT a meeting of influential science and art teachers held at the Charterhouse School of Science and Art, Goswell Road, on the 3rd instant, the position of existing schools, with regard to the fierce opposition offered by highly endowed Polytechnics, was calmly and broadly discussed.

For many years, under the system not only recognized but encouraged by the Science and Art Department, schools have been established in London and the provinces. The aid afforded by the Department has mainly been (1) to contribute largely to the building fund of schools intended for the exclusive teaching of science and art subjects, and (2) to remunerate by Government grant the services of the teachers engaged. The regulations of the Department provide that such aid is given to any centre where the need of it is apparent. It is, however, perfectly well known that the teacher, in the majority of cases, was the person upon whom the duty fell to organize the classes and set the ball rolling, and it would be difficult to mention any school or institute in which the motive spirit was not a teacher.

By recent Acts of Parliament a great impetus has been given to that side of science and art instruction known as technical education. Funds which in past times could only have been raised by persistent begging are now forthcoming almost as a matter of routine. In the provinces there is every sign that the authorities having the administration of the grant of public moneys intend to recognize existing schools. In London it is not so Schemes for the erection of new buildings are pushed forward without due regard to those institutions already doing a good work. At the meeting of teachers already referred to several instances were cited. The People's Palace, erected almost in the very shadow of the Bow and Bromley Institute, has, by reason of its endowment, greatly hampered and harassed the older institution. The West London School of Art succumbed two years ago to the attack of the Regent Street Polytechnic; and now the St. Martin's School of Art, one of the best known centres of instruction in the metropolis, has closed its doors. Without endowment it could not compete with its more favoured rivals. The closing of this school is the more to be regretted because of the high tone of the work carried on within its walls.

Unfortunately, it cannot be denied that many so-called schools of science and art are simply carried on as "grant-earning" establishments, and the country would lose little or nothing if they were closed at once. But there are others affording excellent science and art instruction; and though these may not be affected by the present Polytechnics, it is evident that the schemes yet in embryo for the erection of other buildings will, if not properly checked, raise an undignified competition with the older schools. It is therefore a matter of great public importance that the established institutions should not be overlooked by the London County Council. If new buildings are deemed to be necessary, the old school of science and art should be treated as the nucleus of the enlarged scheme.

Two points of error seem to be apparent in the plan of campaign of the supporters of Polytechnics—(1) that educational work must be associated with recreation; and (2) that technical education has a very limited area, and that science and art education in its fullest sense is unnecessary.

"Schools of art," said a gentleman to me recently, "are dead." Surely nothing could be more absurd. As I understand technical education, it is the application of general principles to a specific purpose. Schools of science and art—*i.e.* schools for the study of science as science, and art as art—should be encouraged as much as before. This can be done without interfering with the specific application of such study to a particular purpose.

With regard to the question of recreation, I think it would be found that, although those institutes which make much of athleticism and such matters attract the largest proportion of students, the attendance *pro ratâ* in the class-rooms, and the results obtained there, would not favourably compare with an institute carrying out a purely educational programme. At the meeting referred to, one teacher stated that, although at a Polytechnic with which he had been connected only seven students entered the class, scores of young men could be found in the billiard-room and gymnasium. At the Science and Art Institute, Wolverton, one of the best and most practical schools in the country, it was decided to close the billiard-room in consequence of the serious effect it had upon the attendance of students at the classes. I am personally acquainted with the science and art work carried on at the Regent Street Polytechnic. Excellent as it is, it would be still better if it could be relieved of the recreative element.

The London County Council has shelved for a time the appropriation of the funds provided by the Excise Act, 1890, for the promotion of technical education. But the matter must soon come up again. Healthy competition is excellent, but in this matter it is clearly not to the interest of the public that its money should be used for pushing on a new venture as a competitor to, and in antagonism with, an existing institution. The best butcher's shop in London would stand a poor chance if a rival establishment run with money raised by taxation, and not of necessity expected to pay its way, opened its doors on the opposite side of the road; and this is practically the state of affairs. The teachers, moreover, have a perfect right to be heard on this question. Devoting their best years to the training necessary for science and art teaching, it may be urged that they have a moral, if not a legal, claim to be considered.

In concluding, I would point out that the exponents of technical instruction are too keen on "centralization."

Let us have large buildings with costly apparatus and every convenience, but do not entirely crush the small schools. To the working man with limited time and means, weary with his day's toil, a modest school close at hand is of greater service than a huge building six miles away involving railway fare and loss of time. By careful arrangements such smaller schools can be preserved, and largely used as "feeders" for the institutes of magnitude.

The whole matter, therefore, of science and art schools and future Polytechnics should be referred to duly qualified men. There is no reason why existing machinery should not fit in with the new plant to make an harmonious whole. OLIVER S. DAWSON.

NOTES.

THE autumn meeting of the Iron and Steel Institute was opened at the Royal Arsenal, Woolwich, on Tuesday, the greater part of the day being devoted to an examination of the various departments of the Arsenal. On Wednesday papers were discussed, and to-day visits are to be made to the Naval Exhibition, the Enfield Small Arms Factory, and the Thames Iron Works. We hope to print next week an account of the proceedings.

AN exhibition of cone-bearing trees and shrubs, asters, and sunflowers, and a conference upon them, were opened in the Royal Horticultural Society's Gardens, Chiswick, on Tuesday. Large numbers of conifers were sent from various parts of the country, no fewer than 30 collections coming from Scotland. The first prize was awarded to the Dowager Marchioness of Huntly for her collection of conifers, the second to Lord Wimborne. The largest araucarian cones were sent from Lady Fortescue's, at Dropmore, Maidenhead, where there is an araucaria 68 feet high—the tallest male araucaria in this country. Kew Gardens contributed about 200 different conifers. On Tuesday papers were read on asters and sunflowers. The conference on conifers began on Wednesday, and is being continued to-day.

A COMMISSION of engineers representing the various European Powers is to meet shortly at Cairo to consider the question of a storage reservoir, and to advise the Egyptian Government on the subject. The Commission will be required to select a site to the north of Wady Halfa, or within the present limits of Egypt.

THE organizers of the International Folk Lore Congress are to be congratulated on the success of their undertaking. The attendance was good; many excellent papers were read; and there were animated and suggestive discussions on most of the problems which are now of especial interest to students of folklore. Mr. Andrew Lang, as President, delivered the opening address, in which he presented a most interesting statement of what he conceives to be the fundamental principles of the science. Admirable addresses were also delivered by Mr. Sidney Hartland, Prof. Rhys, and Sir Frederick Pollock, who presided respectively over the Sections devoted to folk-tales, mythology, and institutions and customs. The members of the Congress dined together at the Criterion Restaurant on Tuesday evening.

STUDENTS of psychology and philosophy will read with regret Prof. Croom Robertson's "valedictory" words in *Mind*, from the editorship of which the state of his health makes it necessary for him to retire. For sixteen years he has done his work as editor with conspicuous ability and success. A second series of the Review will be begun next quarter. It will be under a co-operative direction which promises, Prof. Croom Robertson thinks, "a far more effective covering of the ground of psychology and philosophy than has hitherto been attained."

THE seventh of the series of One Man Photographic Exhibitions is now being held at the Camera Club. It is open to visitors from 10 a.m. to 4 p.m. on presentation of cards, which can be obtained from members or from the Hon. Secretary. The exhibition consists of photographs by Mr. Ralph W. Robinson.

WE learn from the *Botanical Gazette* that Mr. O. F. Cook, Instructor in Biology at the University of Syracuse, U.S.A., intends starting about November 1 in charge of an expedition to Liberia and other parts of Africa, with the object of studying the natural history of the country, especially the plants and insects. Mr. Cook will be glad to hear from anyone who would like to have material from that region.

YESTERDAY evening a meeting of the Medical Society, University College, London, was held in the Botanical Theatre, University College. Dr. W. H. Gaskell, F.R.S., delivered an address on a new theory of the origin of Vertebrates, deduced from the study of vertebrate anatomy and physiology.

THE Belgian Minister of Public Instruction offers a prize of 25,000 francs for the best memoir on the meteorological, hydrological, and geological conditions of the countries of equatoria Africa, regarded from the sanitary point of view. The subject must be studied with special reference to the welfare of Europeans resident in the Congo State.

IN the Proceedings of the Academy of Natural Sciences of Philadelphia for 1891, some parts of which have just reached us, there is an excellent memoir of the late Dr. Joseph Leidy, by Dr. Henry C. Chapman. It is followed by a list of Dr. Leidy's numerous writings.

IN a valuable paper on the "Rapakiwi," J. J. Sederholm, o the Geological Survey of Finland, has furnished petrographers with a trustworthy description of the mode of occurrence and minute structure of a granitic rock which has excited much interest, but has hitherto been very imperfectly understood. The official maps of the district where the Rapakiwi is found, with the accompanying memoirs, were published about a year ago; and the last number of Tschermak's *Mineralogischen und Petrographischen Mittheilungen*, now edited by Dr. F. Becke contains a full discussion of the petrological peculiarities of the rock. Writing from the famous laboratory of Heidelberg, Herr Sederholm naturally adopts the nomenclature of Prof. Rosenbusch, and it would appear from his description that the Rapawiki will have to take its place among the numerous types of "granophyre" (using this term as Rosenbusch does, and not as originally defined by Vogelsang) which constitute links between the plutonic granites and the volcanic rhyolites. The excellent photographic illustrations accompanying the memoir give an admirable idea of the peculiar nodular structure of the rock, which has attracted so much attention to it. In the same journal, we find a second memoir by Herr Sederholm, on the Archæan rocks of South-West Finland, describing a varied series of igneous rocks, and discussing the effect of dynamometamorphic action upon them. The general conclusions of the author agree with those to which the study of similar rocks in other districts has led Lossen, Roland, Irving, Lehmann, Williams, Reusch, and Teall.

EXCELLENT arrangements have been made for the establishment of a good system of technical instruction in Essex. An organizing joint committee of the County Council and the Essex Field Club was lately appointed to deal with the question, and funds were placed at its disposal. This body has now issued a preliminary schedule of subjects to be taught. Local technical instruction committees are invited to select from the list one or more subjects which they may deem specially suitable for their respective neighbourhoods. When several such bodies, representing adjacent districts, have chosen a particular subject, the organizing committee will select a teacher or lecturer, and endeavour to arrange a circuit for him comprising the centres needing his services, apparatus and illustrations being provided by means of the fund for that purpose. By this means the aid of thoroughly qualified and equipped instructors may be obtained by the local committees at a cost considerably less than would be incurred if each centre were to act independently.

STRENUOUS efforts are being made in Scotland to secure that the country shall be supplied with a sound and adequate system of technical instruction. An important public meeting will be held at Edinburgh, on Thursday, October 29, for the consideration of the subject. Lord Elgin will preside, and it is expected that several members of both Houses of Parliament, and others interested in the question, will take part in the proceedings. The following are the provisional agenda:—(1) Chairman's address; (2) report on action taken up to this time by Town and County Councils—(*a*) in England, (*b*) in Scotland—with reference to the application of the sums available for technical education under the Local Taxation (Customs and Excise) Act, 1890; (3) the relation of the Local Taxation Act to technical (including commercial and agricultural) education; (4) report on various agencies already available for technical instruction in Scotland—(*a*) in rural districts, (*b*) in towns; (5) the amendments necessary in the Technical Schools (Scotland) Act, 1887.

THE Nicholson Institute, Leek, of which Sir Philip Magnus is President, has issued its Calendar for the session 1891–92; and an admirable Calendar it is, presenting many varied elements of interest. In the technical school connected with the Institute there will be classes for the study of wood-carving, modelling, bleaching, hygiene, and other subjects; and in the "science department" instruction will be given in botany, physiology, physiography, machine construction and drawing, and practical plane and solid geometry.

AN Agricultural and Mechanical College is about to be established at São Paulo, in Brazil, an endowment of 200,000 dollars having already been promised, and the further aid of the Government secured. The Presidency of the College has been offered to Prof. L. H. Bailey, the American botanist.

IN the Report for 1891 of the Governors of the Baltimore Fishing School, an interesting sketch of the history of the institution is given. The progress of the school encourages the Governors to believe that its success will prove of great advantage to Irish fisheries. They point out, however, that its operations are not on the enlarged scale originally contemplated; and to all who can appreciate the importance of the youth of the Irish coasts being trained in remunerative industrial pursuits, the Governors appeal for contributions to enable them to extend their work. The boys are thoroughly instructed in everything that pertains to the labours of fishermen. They also receive the literary education usual in such establishments; and a special class has been formed for the teaching of elementary navigation in connection with the Science and Art Department. At the last examination in this subject twenty-four pupils presented themselves. Of these, not one failed, twenty-two passing in the first division, and two in the second.

WE have received from the Meteorological Council a copy of the "Meteorological Observations at Stations of the Second Order" for the year 1887, containing observations and results for 66 stations. At 21 stations the observations taken at 9h. a.m. and 9h. p.m. are printed *in extenso*, and the whole work is on the same plan as in the volume for 1886 (NATURE, vol. xliii. p. 20), viz. the barometer observations are given without reduction to sea-level, and the differences between the dry and wet bulb thermometer readings are given as the "depression of wet-bulb." The maximum and minimum thermometers are read at 9h. p.m., and the readings entered to the day on which they were read. The rainfall is measured at 9h. a.m., and the amount registered entered to the previous day. Fog is only entered when the observer is quite enveloped in it. This work has been continued in a more or less complete form since 1866 (when, however, there was only one station); and the summaries contain, *inter alia*, very useful *résumés* of the state of the weather and wind-distribution, and afford excellent materials for preparing a revised climatology of the British Isles. The work is accompanied by a key map, showing the distribution of the stations, and indicating those which belong to the Royal and Scottish Meteorological Societies: it will be seen that all districts are well represented except, perhaps, on the more exposed western coasts and islands. A special table is also given, showing the number of hours of bright sunshine in each month for those stations at which sunshine-recorders exist.

THE Chief Signal Officer of the U.S. Army has, just before the transfer of the Meteorological Service to the Agricultural Department, issued three atlases, bearing upon the meteorology of the United States, showing—(1) The isobars, isotherms, and winds for each month from January to December for the years 1871–73, a period prior to the regular publication of the monthly charts. The data used include all the materials possessed by the Smithsonian Institution. (2) The probability of rainy days, prepared from observations for 18 years (1871–88). The average number of such days for all months and for each station has been calculated, and the percentages thus obtained are graphically shown on the charts. The data show great differences of distribution of rainfall in localities not far distant from each other; the influence of the prevailing direction of the wind in increasing the number of rainy days is particularly noticeable in the Lake region. (3) The average monthly cloudiness for the period 1871–88. Cloud observations show indirectly the relative amount of sunshine, as it may be assumed, within reasonable limits, that the complement of cloudiness will be sunshine. The investigation of this element is useful in determining the suitableness of certain localities for health resorts, or for the ripening of crops, and the charts may be considered as standard cloud maps of the United States.

DR. KING, Director of the Botanical Survey of India, has issued a Report on the working of the Botanical Survey in Assam and Burmah, for which 2000 rupees are annually allowed, with a view to arranging a plan for working by native collectors. Dr. King visited Assam in the latter half of last year, and found the local authorities ready to afford every assistance. Two native collectors were secured, and set to work near Golaghat, and in the Khasia Hills. The Conservator of Forests also sent a large number of specimens to the Herbarium at Calcutta, and a Eurasian collector was employed for a time in Cachar. Some interesting plants were also obtained from the base of the Eastern Himalayas. Fairly good work was done in Upper Burmah by a native collector, and his specimens are now in course of being arranged at the Calcutta Herbarium. The collecting agencies continue working during the present year.

DR. PRAIN, the Curator of the Herbarium of the Calcutta Botanical Gardens, accompanied the surveying ship *Investigator* during part of her operations in the Bay of Bengal last year. By a special arrangement, Dr. Prain was put down on the Great Coco Island for a few days, and was also enabled to pay short visits to the Little Coco and to Rutland Islands. Except for the visit made by Dr. Prain under similar circumstances the previous year, the Great Coco had not before been explored by a botanist, and the Little Coco and Rutland Islands were this year visited for the first time. Accounts of these visits are to be officially published in due course.

A number of small expeditions in the Chin Hills and on the Bhamo frontier of Upper Burmah have been arranged for next cold season. In the Chin country, a column will explore the Chinboh country, and four other columns will visit the Baunghshe, Tashon, Tlangton, Kanhow, and Nwengal tribes. In order to effect a settlement of the Kachyen tribes, columns will be sent out from Bhamo, Mogoung, and Myitkynia. An expedition will also proceed to explore the amber-mines and the india-rubber tracts, and, if practicable, join hands with Assam.

To estimate the relative merits of different kinds of points for lightning conductors, Dr. Hess recently collected and examined nineteen heads of conductors that had been struck by lightning (*Electrot. Zeits.*). His conclusions are as follows: (1) the fusion of points of lightning conductors by lightning causes no danger of fire through scattering of fused drops, for this does not occur; (2) fine and smooth points receive the lightning stroke in concentrated form, while sharply angled and ribbed, also blunt points, divide it into threads; (3) platinum needles and tips have no advantage over copper points; (4) there are lightning strokes which are capable of making brass wire 7·2 mm. (say 0·29 inch) thick, incandescent. Unbranched copper conductors should therefore never be thinner than 7·0 mm.

In submitting to the Wellington Philosophical Society some "Coccid Notes" lately, Mr. W. M. Maskell expressed regret that entomologists generally did not devote more attention to the Coccidæ. He believed he was the only person in New Zealand who had published anything on the subject. In the Coccidæ there was infinite variety—a variety of life-history, habits, and customs that seemed greater than that afforded by any other branch of entomology. He gave instances of peculiarities in these insects—wonderful vitality in some cases, and the boring habits of one particular insect after it had thrown off legs, mouth, &c.—all tending to prove that these little despised creatures were more interesting for study than "all the butterflies"

Farmers in many parts of Victoria seem to be fully alive to the necessity of adapting their methods to the conditions under which they have to carry on their work. Mr. David A. Crichton, in a report printed in the latest Bulletin (No. 12) of the Victoria Department of Agriculture, says that, although farmers are supposed to be too conservative in their practice to do much in the way of new industries, he has been agreeably surprised to find that a very large number are anxious to try crops other than cereals. Fruit culture in particular is attracting great attention, and he feels confident that before long it will become one of the staple agricultural industries of the colony. He is doing his bes to stimulate this particular industry, and, in addition to the information afforded by his lectures, he makes it a practice to visit as many places as possible, to advise upon the selection of sites for orchards and vineyards, and give practical lessons in pruning, training, and other matters. He finds that this assistance is highly appreciated, and his services are in great demand in this respect Mr. Crichton's position in connection with the Victoria Department of Agriculture is that of "the fruit and special industries' expert."

Mr. John H. Cooke is publishing in the *Mediterranean Naturalist* an interesting series of observations on the geology of the Maltese islands. In the September number he refers to Cala Hein, a little bay between Comino and Cominotto. On a bright day, he says, this bay presents an endless succession of the most brilliant colours, "which commences with a deep blue, and from thence passes through every conceivable gradation of green, orange, and white, after attaining the last of which it again graduates onward in the distance to that cerulean blue that is so characteristic of Mediterranean waters." The setting of the picture is not less effective than the picture itself. Around the bay are many caverns, which have sombre-looking entrances and wildly-fantastic shapes. The sides of these caverns are full of interest for geologists, as "they literally teem with the remains of creatures that formerly lived and died in the waters in which the islands were built up."

Mr. W. Prentis, of Rainham, Kent, describes in the October number of the *Zoologist* an interesting case of a wild duck's forethought. A mowing machine was set to work round the outside of a field of lucerne bordering a marsh, diminishing the circle each time round the field, leaving about two acres in the centre. A wild duck was seen by the shepherd to fly from the piece of lucerne that was left with something in her beak, and, happening to fly near him, she dropped a three parts incubated egg. She was again observed by the shepherd, and also by the sheep-shearer, carrying another egg in her beak, this time over the marsh-wall towards the saltings; and again she was seen for the third time carrying an egg in her beak in the same direction. Next day, when the field was "finished" by the removal of the last piece of lucerne, the wild duck's nest from which the eggs had been removed was discovered.

Mr. W. H. Harris, Ealing, records in *Nature Notes* (September 15) a remarkable instance of "frugality" in bees. The recent extremely rainy weather seems to have suggested to his bees that there would probably soon be an end of honey-making. Accordingly, although there was "a crate of fairly filled sections above the stock-box," they adopted vigorous measures to prevent future inconvenience. "It is a positive fact," says Mr. Harris, "that my bees, not content with ejecting larvæ of both drones and workers, proceeded to suck out the soft contents of the corpses, leaving only the white chitinous covering, which had not hardened sufficiently to prevent the workers from piercing it with their mandibles, and then inserting their tongues."

Messrs. R. Friedländer and Son, Berlin, send us the latest of their catalogues of botanical books. This list, besides various works on the distribution of plants and on botanical exploration, includes a great number of writings on the floræ of different parts of the world.

Messrs. Kegan Paul, Trench, Trübner, and Co. announce the following books on scientific subjects:—"Colour Blindness and Colour Perception," by F. W. Edridge-Green, M.D., with three coloured plates (International Scientific Series); "Descriptive Catalogue of the Nests and Eggs of Birds found breeding in Australia and Tasmania," by A. J. North, with 21 full-page plates; "English Folk Rhymes," by G. F. Northall; the following volumes of a series, "Modern Science," to be edited by Sir John Lubbock—"The Cause of an Ice Age," by Sir Robert Ball, F.R.S., "The Horse: a Study in Natural History," by William Henry Flower, C.B., "The Oak: a Popular Introduction to Forest Botany," by H. Marshall Ward, F.R.S., "The Laws and Properties of Matter," by R. T. Glazebrook, F.R.S.;—"On Seedlings," by Sir John Lubbock, with numerous figures in text; "How to Use the Ophthalmoscope," elementary instruction in ophthalmoscopy, by Edgar A. Browne, fourth edition, completely revised; "Principles of Political Economy," by Arthur Latham Perry; "Moral Order and Progress," an analysis of ethical conceptions, by S. Alexander, second edition (Trübner's Philosophical Library); "Chemistry of the Carbon Compounds, or Organic Chemistry," by Prof. Victor von Richter, authorized translation by Edgar F. Smith, new and enlarged edition.

Two more papers by Prof. Curtius, upon the reactions of the hydrate of his recently isolated hydrazine or diamidogen, $\begin{matrix} NH_2 \\ | \\ NH_2 \end{matrix}$, are contributed to the most recent numbers of the *Journal für praktische Chemie*. The earlier communication describes, for the first time, the neutral sulphate of hydrazine

$(N_2H_4)_2 \cdot H_2SO_4$. Hydrazine is found to form two sulphates—an acid one, $N_2H_4 \cdot H_2SO_4$, and the neutral one now described. The acid sulphate is a beautifully crystalline salt—an account of which was given in NATURE, vol. xliii. p. 205. It is distinguished by its high melting-point, 254° C., and its difficult solubility. The neutral sulphate now described is obtained by evaporation of the solution formed by neutralizing hydrazine hydrate with dilute sulphuric acid, first, over a water-bath, and finally, as the new salt is very deliquescent, *in vacuo*. It crystallizes in large brilliant tables, melting at 85°. It is precipitated in a most curious manner from its aqueous solution by alcohol, separating as an oil, which, on being stirred with a glass rod, and in contact with a small crystal of the salt, immediately solidifies to a fine mass of crystals, which, like those obtained by evaporation, consist of anhydrous $(N_2H_4)_2 \cdot H_2SO_4$.

THE second and much longer communication describes an important series of new compounds, the ketazines, obtained by the action of hydrazine hydrate upon ketones. The simplest of these new substances, the one obtained by the action of hydrazine hydrate upon acetone, is represented by the formula $(CH_3)_2C{=}N{-}N{=}C(CH_3)_2$. When hydrazine hydrate is dropped upon acetone, a most violent reaction occurs, resulting in an explosion unless the acetone is surrounded by a freezing mixture. When thus moderated, however, the substance above formulated is produced together with water, the reaction occurring according to the following equation:—

$$2\,(CH_3)_2CO + N_2H_4 \cdot H_2O = (CH_3)_2C{=}N{-}N{=}C(CH_3)_2 + 3H_2O.$$

By allowing the product to remain for some hours in contact with caustic potash the water is removed, and upon distillation the new ketazine passes over in the pure state. It is a clear liquid possessing a sharp odour somewhat resembling that of the alkaloid coniine. It boils without decomposition at 131°. By employing other ketones, such as methyl ethyl ketone, diethyl ketone, and others of the same type, a large number of these ketazines have been prepared. Those containing fatty radicles are liquids, and those containing aromatic groups are solids. The lowest members only dissolve in water, the solubility rapidly diminishing with increase of carbon atoms. Acids decompose them in the cold, with assimilation of water, into their constituents; towards alkalies, however, they are comparatively stable. Light exerts a decomposing action upon them, specimens placed in bright sunshine rapidly becoming yellow. Reducing agents, such as sodium amalgam, are without action upon them, and they appear further to be incapable of reducing either Fehling's solution or (except after long boiling) ammoniacal solutions of silver salts.

THE additions to the Zoological Society's Gardens during the past fortnight include two —— Cormorants (*Phalacrocorax*, sp. inc.) from New Zealand, presented by the Earl of Onslow, G.C.M.G.; a Vervet Monkey (*Cercopithecus lalandii* ♀) from South Africa, a White-fronted Lemur (*Lemur albifrons* ♀) from Madagascar, presented by Captain R. C. Stevenson; a Golden Agouti (*Dasyprocta agouti*), a Garden's Night Heron (*Nycticorax gardeni*), a —— Heron (*Ardea*, sp. inc.) from Surinam, presented by Mr. Frank Fisher; a Common Paradoxure (*Paradoxurus typus*) from India, presented by Miss Bason; two Blackcaps (*Sylvia atricapilla*), two Lesser Whitethroats (*Sylvia curruca*), two Goldfinches (*Carduelis elegans*), a Marsh Tit (*Parus palustris*), British, presented by Mr. J. Young, F.Z.S.; three Common Vipers (*Vipera berus*), British, presented by Messrs. A. H. R. and F. R. Wollaston; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mrs. Gwynne; an Indian Civet (*Viverricula malaccensis*) from India, presented by Mr. Herbert Courtney Hodson; two Chilian Sea Eagles (*Geranoaëtus melanoleucus*) from Chili, presented by Mr. H. Berkeley James, F.Z.S.; two Grey-breasted Parrakeets (*Bolborhynchus monachus*) from Monte Video, presented by Mr. J. C. Wallace; two Nightingales (*Daulias luscinia*), two Common Whitethroats (*Sylvia cinerea*), a Blackcap (*Sylvia atricapilla*), British, presented by Mr. J. Young, F.Z.S.; four Yellow Wagtails (*Motacilla raii*), British, presented by Mr. W. Swaysland; a Common Cormorant (*Phalacrocorax carbo*) from Scotland, presented by Mr. F. T. Barry, M.P.; fifteen Striped Snakes (*Tropidonotus sirtalis*) from North America, presented by Mr. J. Gray; a Solitary Thrush (*Monticola cyanus*), European, a Macaque Monkey (*Macacus cynomolgus*) from India, deposited; a Sharpe's Wood Owl (*Syrnium nuchale*) from West Africa, a Testaceous Snake (*Ptyas testacea*) from California, two Quebec Marmots (*Arctomys monax*) from North America, two Scaly Doves (*Scardafella squamosa*) from South America, purchased; a Ruddy-headed Goose (*Bernicla rubidiceps*) from Falkland, received in exchange.

OUR ASTRONOMICAL COLUMN.

PHYSICAL APPEARANCE OF PERIODIC COMETS.—Comets possess no personal characteristic appearance; but Mr. Barnard, writing to the *Astronomical Journal*, No. 246, suggests that it may be possible to arrange those of short period according to their physical peculiarities. To the first class he would assign those comets which are large, round, and very gradually brighter in the middle, with no special condensation, and of a very diffused nature. They have no nucleus or tail, and are so decidedly periodic that, trusting to this peculiarity, Mr. Barnard predicted that the comet discovered by Swift in November 1889, and D'Arrest's comet at its return last year, were of short period. The most distinctive members of this class of comets are D'Arrest's, Swift's 1880, Brooks's 1886, and Swift's 1889. There are few nebulæ that resemble this class. A much larger and less exclusive class contain comets which are comparatively small, and which have an indefinite central brightness or nucleus. Many of the parabolic comets resemble these, and there are hundreds of nebulæ exactly like them in telescopic appearance. To this class are assigned comets Faye; Wolf, 1884 III.; Finlay, 1886 VII.; Brooks, 1889 V.; Spitaler, 1890. It is possible that the peculiarities of these two distinct classes of short-period comets may furnish some information as to their relative ages.

DISCOVERY OF TEMPEL-SWIFT'S COMET.—Mr. Barnard found this comet on September 28, and Mr. W. F. Denning discovered it independently two days later about 4° south-west of its computed position. The comet passes perihelion in November. Its position, according to M. Bossert's ephemeris, is as follows:—

Ephemeris for Paris Midnight.

1891.	Right Ascension. h. m. s.	Declination. ° ′	Brightness.
Oct. 6	21 6 2	+ 3 24·8	7·01
,, 8	6 19	3 54·0	
,, 10	6 55	4 24·7	7·77
,, 12	7 51	4 56·9	
,, 14	9 9	5 30·8	8·61
,, 16	10 48	6 6·4	
,, 18	12 49	6 43·9	9·54
,, 20	15 13	7 23·4	
,, 22	18 0	8 4·8	10·54
,, 24	21 12	8 48·3	
,, 26	24 50	9 34·0	11·64
,, 28	28 56	10 22·2	
,, 30	21 33 30	11 12·8	12·83

The comet is therefore in Equuleus at the present time, and moving towards Pegasus.

PHOTOGRAPHIC DEFINITION.

I.

IT is a matter of some interest to determine what are the limits to the definition obtainable in photographs. In examining this question, three distinct classes of problems present themselves—namely:—

(1) Those depending on the wave-length of light, and the action of a perfect lens on such wave-lengths.

(2) The various aberrations of real lenses.

(3) The qualities of the different sensitive surfaces on which the pictures are formed.

Taking these divisions of the subject in the order given, I will inquire first what is the limit to photographic definition on the supposition that the lens has no aberration of any kind, *i.e.* that all the waves which reach it from any point arrive at the image of that point in the same phase.

The image thus formed consists, as is well known, of a bright disk surrounded by alternate dark and bright rings, the intensity of the illumination of the rings decreasing rapidly at each successive ring, reckoning outwards from the centre.

In order that the images of two neighbouring points may appear separated from one another, the central disks of their images ought not to overlap. If the disks are just in contact, it is possible that they would appear as a double object in the photograph, and this may be taken as the limit of the defining power of a lens. (See Airy "On Light," and Lord Rayleigh "On the Theory and Manufacture of Diffraction Gratings," *Phil. Mag.*, 1874.)

But, in ordinary photography, objects at very various distances have to be simultaneously represented, and it is to the definition attainable under these circumstances that I wish now to direct attention.

On referring to the papers above-mentioned, it will be seen that the diameter of the central disk is

$$1{\cdot}219\,\frac{\lambda F}{A},$$

where λ is the wave-length of light,

F the focal length of the lens,

A the aperture of the lens.

This gives the effective diameter of the image of a point truly in focus when not far removed from the axis of the lens.

Points nearer to, or further from, the lens than that which has its image on the plate will be represented on the latter by round patches of light; these being the sections by the plate of the cones of rays which have for their summits the geometrical foci of the points, and for their slant the radius of the aperture ÷ focal length.[1] Thus, if e is the distance before or behind the plate of the focus of a point, it will be represented on the plate by a patch of light of diameter

$$e\,\frac{A}{F}.$$

This diameter can be diminished by the use of a diaphragm, *i.e.* by diminishing A, but this at the same time increases the diameter of the images of points whose foci are on the plate. And the resulting average definition will be improved by diminishing A until the patch of light, representing the point most out of focus, has the same diameter as the diffraction disk of the image point in focus.

If we suppose the photographic plate to be placed at such a distance from the lens that the focus of the nearest object is as much behind the plate as the focus of very distant objects is in front of it, we shall have, to determine the diameter of the stop giving the best average definition, the following equation:—

Putting F = principal focal length,

D = distance of nearest object,

$q = 1{\cdot}219\lambda$,

$$q\,\frac{F+e}{A} = e\,\frac{A}{F+e};$$

$$\therefore\quad q\,\frac{(F+e)^2}{e} = A^2\ .\ .\ .\ .\ .\ .\ .\ (1)$$

but, by the ordinary formulæ, connecting the conjugate foci of lenses, we have, if $D = F + g$,

$$2eg = F^2;$$

$$\therefore\quad e = \frac{F^2}{2g} = \frac{F^2}{2(D-F)};$$

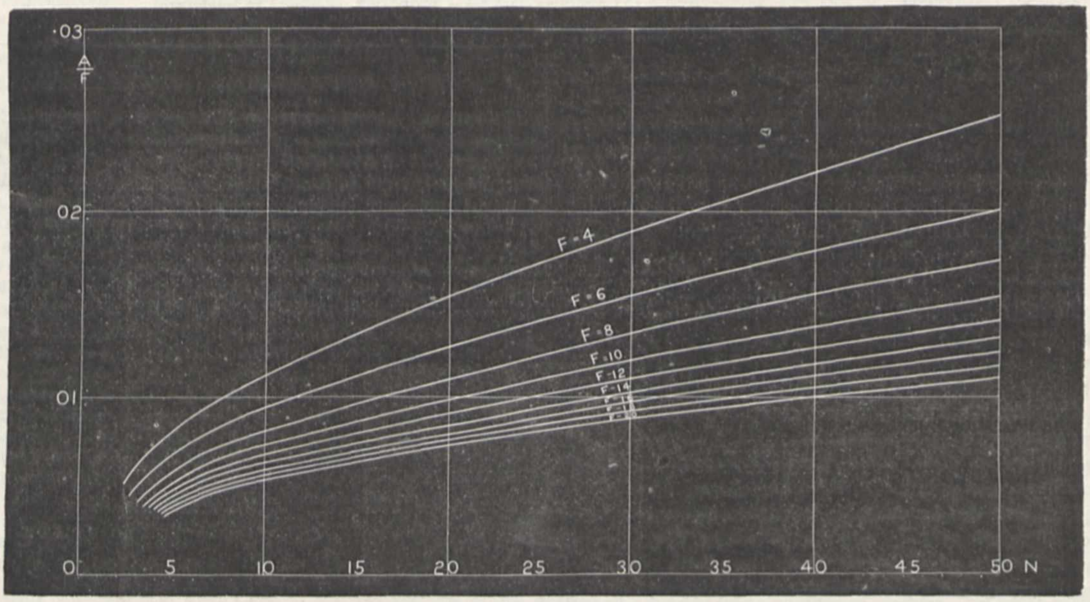


FIG. 1.

[1] This is an approximate statement only. The true expression involves an investigation of the intensity of the light immediately in front of and behind a caustic.

whence, substituting for e in (1) we have

$$A = \sqrt{\frac{q}{2}}\ \frac{2D - F}{\sqrt{D - F}} \quad . \ . \ . \ . \ . \ . \ . \ (2)$$

Let the nearest object be at n times the focal length of the lens. Then, putting nF for D,

$$A = \sqrt{\frac{qF}{2}}\ \frac{2n - 1}{\sqrt{n - 1}} \quad . \ . \ . \ . \ . \ . \ (3)$$

This gives the value of A as a linear quantity; it is usual, however, to reckon the diameter of stops as fractions of the focal length.

Dividing, therefore, (3) by F,

$$\frac{A}{F} = \sqrt{\frac{q}{2F}}\ \frac{2n - 1}{\sqrt{n - 1}} \quad . \ . \ . \ . \ . \ . \ (4)$$

From (4) the accompanying table has been computed, giving $\frac{A}{F}$ for various values of F and n. (Fig. 1 gives the same graphically.)

Table showing ratio of aperture to focal length which gives the best average definition when the nearest object to be photographed is at "n" times the focal length of the lens, and distant objects are also in view.

	$\frac{A}{F}$									
F	n=5	n=10	n=15	n=20	n=25	n=30	n=35	n=40	n=45	n=50
in.										
4	·00785	·0110	·0130	·0150	·0171	·0188	·0201	·0216	·0229	·0244
6	·00640	·00920	·0106	·0124	·0139	·0152	·0165	·0177	·0188	·0199
8	·00554	·00775	·0092	·0107	0121	·0132	·0142	·0154	·0162	·0172
10	·00495	·00699	·00825	·0096	·0108	·0118	·0129	·0137	·0146	·0154
12	·00452	·00635	·00735	·0087	0098	·0108	·0117	·0125	·0133	·0141
14	·00420	·00580	·00695	0081	·0091	·0100	·0108	·0116	·0123	·0130
16	·00392	·00552	·00651	·0076	·0085	·0093	·0101	·0108	·0115	·0122
18	·00370	·00520	·00615	·0071	·0080	·0088	·0095	·0102	·0109	·0115
20	·00350	·00494	·00584	0068	·0076	·0084	·0090	·0097	·0103	·0109

I have not before seen it pointed out that the ratio $\frac{A}{F}$, which gives the best average definition, alters with the value of F.

If α is the least angular distance between two points (as seen from the centre of the lens) which are shown as separate points on the photograph, α must at any rate not be less than $\frac{q}{A}$, or

$$\sqrt{\frac{2q}{F}}\ \frac{\sqrt{n - 1}}{2n - 1},$$

showing that, if the foreground is kept at a distance proportional to the focal length of the lens, the definition improves with an increase of the focal length.

On the other hand, if the nearest object is at some fixed distance, D, from the lens, we have as the limit for α,

$$\sqrt{2q}\ \frac{\sqrt{D - F}}{2D - F},$$

an expression which increases with F, so that for a given picture taken from a fixed position, definition will be gained by the use of a short focus.

The gain, however, in this respect is not great, for in practice D is always a considerable multiple of F, and writing

$$\sqrt{\frac{1}{4D + \frac{F^2}{D - F}}} \text{ for } \frac{\sqrt{D - F}}{2D - F},$$

it will be seen that when D is many times F, $\frac{F^2}{D - F}$ may be neglected in comparison with 4D.

Thus, in ordinary cases the limit for α is $\sqrt{\frac{q}{2D}}$, and is independent of the focal length of the lens employed.

If we inquire how close the nearest object may be to the lens when a view containing also distant objects has to be photographed with a definition reaching a certain standard, we have, on the above supposition,

$$D = \frac{q}{2\alpha^2};$$

and if we put $\alpha = 1'$, which is often taken as the least angle separable by the unaided eye, and λ as $\frac{1}{50000}$ inch,

$$D = 150 \text{ inches},$$

showing that if the picture is to appear as well defined as the natural objects themselves, to the eye placed at the position of the lens, no object in the view must be nearer the latter than about 13 feet.[1]

Though, as above stated, the focal length does not affect the definition, when the right-sized stop is used, it does the rapidity with which a picture may be taken, for the intensity of the light on the plate is measured by $\frac{A^2}{F^2}$ or $\frac{q}{2F}\ \frac{(2n - 1)^2}{n - 1}$.

That is, in these circumstances, the exposure is inversely as the focal length.

All that has been hitherto said refers to the definition in the central parts of the plate.

The definition for the oblique pencils is necessarily worse. For even if it were assumed that the lens was perfect for oblique pencils, the points out of focus would be no longer represented by circular areas, but by the elliptic projections of these circles on the plane of the plate.

The assumption, however, that a lens is perfect for oblique pencils is too far removed from actual fact to make it worth while to consider the results to which such a supposition would lead.

The definition for the marginal parts of the photograph depends on the various aberrations which all combinations of lenses suffer from in some degree, but which in well-made examples are completely, or almost completely, corrected for direct pencils.

These aberrations are (1) spherical, (2) chromatic, (3) astigmatism, (4) curvature of field.

The effects of the two last are much the most important, and will be considered first.

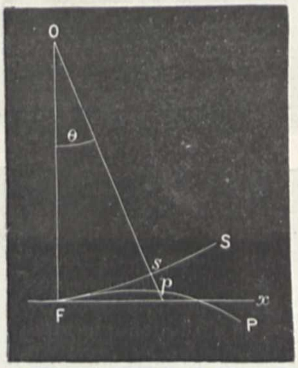


FIG. 2.

Let O (Fig. 2) be the optic centre of the lens, OF the axis of the lens, and F the principal focus.

Let Fx be the plane of the plate, FP and FS the curves on which the primary and secondary foci respectively lie.

Let Op be the axis of a pencil inclined to OF at an angle θ, and meeting FP and FS in p and s. Then sp measures the astigmatism of the lens for a pencil of obliquity θ.

Putting y_p and y_s for the ordinates of the curves FP and FS at p and s, it will be seen that a point distant θ from the axis of the lens, will be represented on the plane of the plate by an oval patch of light whose axes are $A\frac{y_p}{\cos\theta}$ and $A\frac{y_s}{\cos\theta}$ in directions parallel and perpendicular to Fx; A, as before, being the aperture of the lens.

Any formula depending on the actual data of real combinations of lenses, and giving the values of y_p and y_s in terms of radii of curvature and refractive indices, &c., of the lenses composing them, would be a very unmanageable thing for the

[1] I have verified this with a lens of 10-inch focus.

purpose in hand; but I give the curves in question obtained experimentally for seven lenses of different types in my

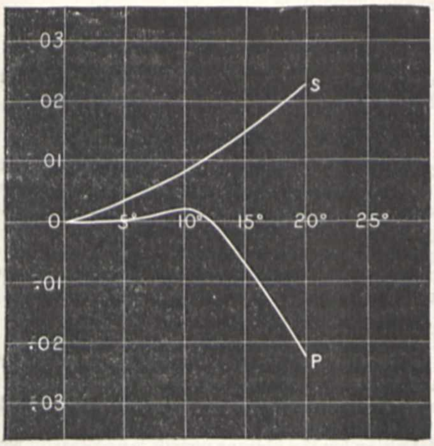


FIG. 3.—English Portrait.

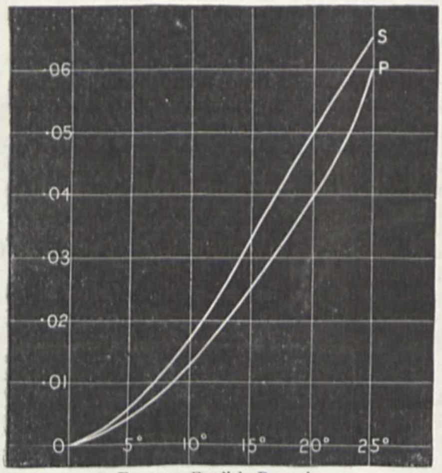


FIG. 4.—English Portrait.

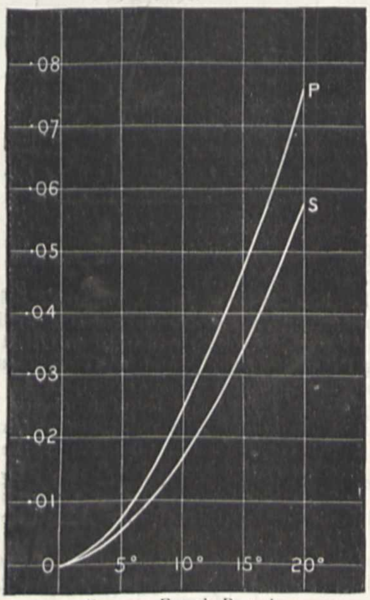


FIG. 5.—French Portrait.

possession (see Figs. 3 to 9). All these lenses except Fig. 5 are by makers reputed to be the best.

[Fig. 5 is peculiar in having the usual relative positions of the primary and secondary foci reversed.]

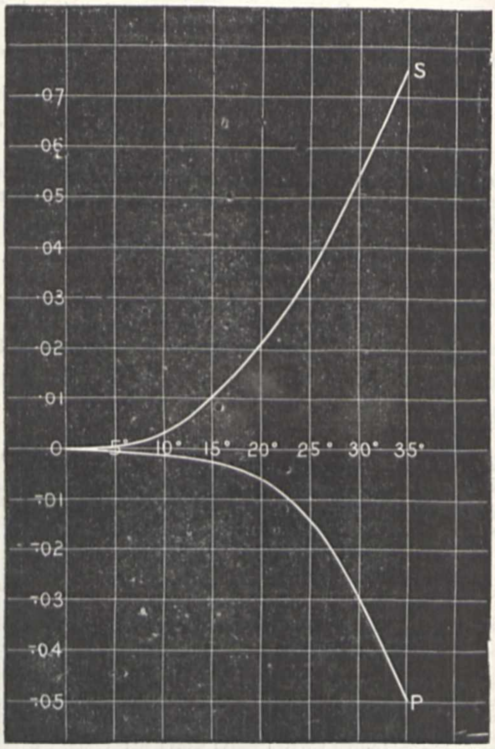


FIG. 6.—English Wide-angle Doublet.

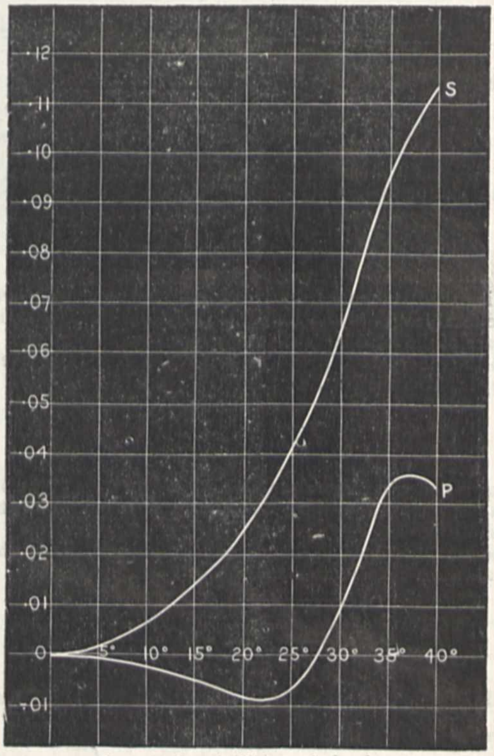


FIG. 7.—French Wide-angle Doublet.

The ordinates of these curves are the distances of the primary and secondary foci from a plane through the principal focus at right angles to the axis of the lens, and are expressed as fractions of the focal length.

The abscissæ are the inclinations (in degrees) of the pencils to the axis.

Suppose, now, that the plate is placed at a distance e behind

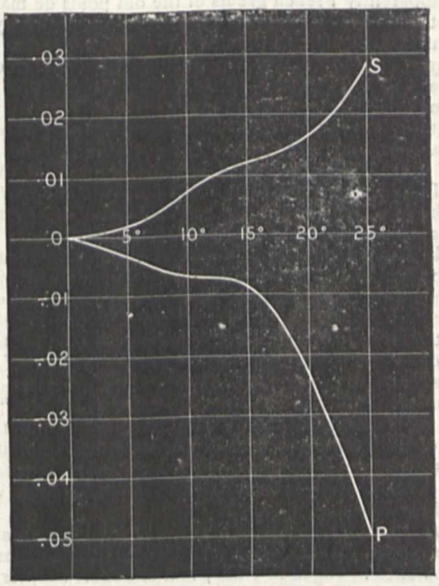


Fig. 8.—Rapid Rectilinear.

the principal focus, e being half the distance between the foci for direct pencils of the nearest and distant objects.

The worst defined point in the centre of the picture will then be represented as having a width $\frac{A}{F}e$ nearly, while at the obliquity θ this width becomes $\frac{A}{F}\frac{e \pm y}{\cos\theta}$ nearly, according to whether the

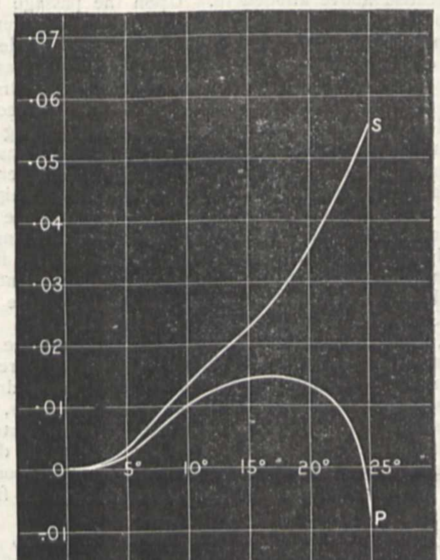


Fig. 9.—Triplet.

image under consideration is that of the most distant or the nearest point.

Hence, unless y is small compared with e, the definition for rays of obliquity θ will be sensibly worse than in the centre of the plate, and a reference to the curves for y_p and y_s shows at a glance that this must be the case even at 10° for all the lenses unless the general standard of the definition is lowered by a large value of e.

As an example of the use of the curves, let us take the rapid rectilinear No. 6, and compare the definition at 20° obliquity with that at the centre, supposing that the nearest object is at a distance of 25F.

This gives $e = \cdot 02$F nearly, and at 20° $y_p = -\cdot 023$F, $y_s = +\cdot 016$ F, hence we have as follows:—

	At 20° from axis		
	due to primary focus.	due to secondary focus.	At centre.
Width of image of nearest point... $\frac{A}{F\cos\theta}\times$ ...	·043 ...	·004 ...	·02
Distant do. ...	·003 ...	·036 ...	·02

This shows that while the nearest points at this obliquity are represented by long ovals placed as if radiating from the axis, the most distant points become similar but rather smaller ovals with their long axes at right angles to the former, and that the length of the ovals is about twice the diameter of the image formed by the direct pencils.

In the same way the definition, as far as it depends on astigmatism and curvature of field, at any obliquity may be found for any lens for which y_p and y_s are known.

Lauriston Hall, September 9. A. Mallock.

(To be continued.)

THE KOH-I-NUR—A CRITICISM.

The true history of the Koh-i-Nur diamond, if it could be written, would be a singularly interesting one. But the historian would have a difficult task. The pages that I purpose writing will be devoted to the criticism, possibly the refuting, of some fallacies that hang round the subject; but they will not deal with some other historical difficulties that I have not space even to indicate, but which do not belong to those portions of the history for criticism on which the following pages are designed.

The period in the history of the Koh-i-Nur that has attracted the notice of all modern writers on the diamond, and to a degree, I think, somewhat beyond its importance, is the five or ten minutes during which the French diamond-dealer, Tavernier, held in his hand the most important of the Crown jewels of the Emperor Aurungzebe. It was a great diamond, and the record Tavernier has handed down in his "Voyages," of its weight, its form, and its history, will have to be critically dealt with.

It may be at once stated that the disputable point regarding this diamond is whether it was a certain ancient diamond of fame in India, or one much larger than this ancient stone, that had been found not very long before Tavernier was present at the Court of Aurungzebe. For the larger stone I shall retain the name of "the Great Mogul"; for the older and more famous one the title of the Koh-i-Nur. Some hold that Tavernier saw and handled the Koh-i-Nur; others that his own story is correct, and that it was the Great Mogul that he described. And I should add that some, in addition to this latter view, believe the Great Mogul ought to be called the Koh i-Nur.

In order to clear the ground, I may say that while attaching no very great importance to the question as to which of the two first views is the correct one—and I must add also, valuing at a somewhat low estimate the historical or technical accuracy of Tavernier's statements on this and many other matters—I, some thirty-five years ago, came to the conclusion that the diamond Tavernier saw was probably the Koh-i-Nur, and that he muddled its history with the other and larger diamond that I showed to have been probably at the time in the keeping of Shah Jahan, the captive father of Aurungzebe. The merits of the question will be discussed in their proper place; but while holding myself open to conviction if any new arguments can be brought forward against my view, I may state that none yet announced have shaken that opinion.

Until the fifteenth century there appears to have been one and only one very large diamond known in India or in the world. I might have said until the sixteenth century but that there is a record of two and an unauthenticated rumour of a third during that century, the largest of which, however, was very likely the Koh-i-Nur. But that one large diamond of the earlier time had been a famous stone for centuries. Legends had gathered round it, and tradition had linked the legends with authentic history in the dawn of the fourteenth century. The tale was told briefly by Prof. H. H. Wilson in the sketch of the Koh-i-Nur which he contributed to the official catalogue of the Exhibition of 1851. No more competent person could have performed the task than the great Orientalist and Sanscrit scholar, with his large experience of Hindoo customs and modes of thought. And he wrote the notice with the statements before him that had been collected in the bazaars of India by order of the Company at the time when the Koh-i-Nur became a Crown jewel of the Queen.

The latest historian of the Koh-i-Nur, however, dismisses this curious tradition and its distinguished narrator by the somewhat flippant remark that "it has afforded sundry imaginative writers a subject for highly characteristic paragraphs."

The gentleman who writes in this tone of the eminent custodian of the East India Company's Library cannot be expected to treat Mr. King or any other man of learning less contemptuously; but his qualifications for dealing with the subject at all from a wider point of view than that of the old French diamond-dealer will, perhaps, be fairly called in question by the readers of the following pages.

Yet Dr. Ball, of the Science and Art Department in Dublin, has had Indian experience on the Geological Survey, an office that ranks deservedly high even among the great departments of the Indian public service. He has, furthermore, recently thought the Indian part of Tavernier's "Voyages" worthy of a fresh translation, which he has effected with judgment and with notes, the topographic part of which, at least, appears to be of considerable value and interest; and he has otherwise been an author on subjects that came before him in India as a geologist and a sojourner.

It is probably a sort of loyalty to the author whom he has deemed worthy of so much of his time and industry that blinds him in his advocacy of Tavernier's statements, notwithstanding their manifold inconsistencies and absence of scholarlike quality. I hope, while criticizing his hypotheses and statements regarding the Koh-i-Nur, I may not in any respect quit a judicial attitude to appear in that of a partisan.

The great diamond to which allusion has been made emerges in history in the first years of the fourteenth century. It was in 1300 A.D. in the hands of the Rajahs of Malwa, an ancient Ráj that had at one time spread over Hindostan, and in all the vicissitudes of a thousand years had never bent to a Muhammadan conqueror, until the generals of the Delhi Emperor Alá-ud-dín Muhammad Shah overran its rich territory, and carried away the accumulated treasure of Ujjein in the first decad of the fourteenth century.

The date of 1304 is that given by Ferishtá for this conquest, and then it was that the great diamond takes its place in history. In 1526 the invasion of India by Babar was crowned by his victory on the famous battle-field of Panaput. Babar himself—in those memoirs that rank only after the "Commentaries" of Cæsar as the most interesting records penned by a great conqueror—describes the reception by his son Humayún of the great diamond among the treasures which he was sent forward to secure at the strong fortress at Agra. Babar gives the weight of the diamond as being computed at 8 mishkals, and in another place he compares the Muhammadan weights with those of the Hindoo system, putting the mishkal as equivalent to 40 of the little Hindoo units of weight, the rati. The diamond, then, weighed near about 320 of these ratis. There are several lines of investigation for determining the weight of the mishkal; and without here entering on a long but interesting discussion of this weight, it will suffice to say that the most important of them converge on a value of from 73 to 74 troy grains. If the mishkal weighed 73·636 troy grains, 8 such mishkals would be 589·088 grains. The weight of the Koh-i-Nur diamond in the Exhibition of 1851 was 589·52 troy grains. It may be added that this latter weight is equivalent to $186\frac{1}{16}$ English carats of 3·1682 troy grains, and would require, to make up the 320 ratis, a rati of the value of 1·8425 troy grains.

It is very remarkable how numbers closely corresponding to one or other of these values for the weight of a great diamond, in carats or ratis, will recur in the subsequent discussion. Thus Anselm de Boot, in commenting in the early years of the seventeenth century upon some observations on Indian diamonds made in the previous century by Garcias de Orto (a Portuguese physician at the Viceregal Court of Goa), states the largest diamond Garcias had seen to have weighed 187½ carats. Garcias puts its weight at 140 mangelins. His translator (into Latin), Le Cluze, interprets the 140 mangelins as equivalent to 700 grains (apparently French grains of the old poids de marc). But De Boot evidently either had some separate authority for his statement that the largest diamond Garcias had seen weighed 187½ carats, or had the means of reckoning more correctly than Le Cluze the value in Dutch or in Portuguese carats of the 140 mangelins of Garcias. Garcias was in India for thirty years in the reign of Akbar, a reign that, commencing three years earlier and ending three years later, covered "the spacious times of great Elizabeth"; and if any European of the many visiting India at that time would have had special opportunity of seeing the great diamond in the treasury of Babar's grandson, it would have been the body-physician of the Portuguese Viceroy. Dr. Ball has got into a hopeless mess in an endeavour to discredit observations of mine, and of my late learned friend Mr. King, regarding this allusion of De Boot's to a diamond weighing 187½ carats. Dr. Ball is quite mistaken in supposing that he is the first person who had an acquaintance with De Boot's sources of information, with Le Cluze's translation of Garcias into excellent Latin, and with the commentators who edited De Boot and largely plagiarized from Le Cluze. In his "Natural History of Precious Stones," Mr. King gave, in 1866, an account of all these persons and their writings, but that accomplished scholar would certainly never have fallen into so absurd an error as Dr. Ball has rushed into in connection with De Boot's allusion to a 187½-carat diamond.

Garcias, like Le Cluze, was a botanist, and his treatise was on Indian botany. He, however, devoted a few pages to the precious stones in vogue in India, and one short chapter is given to the diamond. De Boot transcribed, with omissions, these chapters of Garcias, and with misprints that probably arose from the statements he made, and even the pages he incorporated, being in the form of notes culled by him from a great variety of sources, of which Garcias was only one. Among the misprints or misapprehensions in De Boot's very remarkable book on stones and gems, is that by which he always substitutes the name of Monardes, a writer on the botany of the New World, in lieu of that of Garcias, an error the source of which Mr. King explained in the treatise above alluded to. Upon the passage in which De Boot refers to the great diamond, and which runs thus: "Nunquam tamen majorem (adamantem) illo qui pendebat 187½ ceratia, cujus mentionem facit Monardes, inventum fuisse puto," Adrian Tull, a Belgian physician who edited the treatise of Anselm de Boot, adds a note to the chapter, correcting the name Monardes for that of Garcias, and then quoting from Le Cluze another note introduced at the end of his translation of the chapter, to the effect that he, Le Cluze, had never himself seen a larger diamond *in Belgium* than one which weighed 190 *grains*. Dr. Ball quotes this note in the Latin of Le Cluze to show that De Boot did not know what he was writing about, and still less that Mr. King and, of course, myself did, inasmuch as we had fastened upon De Boot's singular statement without due study of our authors. It is the writer of the "true history" of the Koh-i-Nur who has not gone to the authorities. Had he done so, he would have found in the 1605 edition three notes on this passage by Le Cluze. In the first he analyzes Garcias's 140 mangelins into "septingenta grana, sive unciam unam, drachmam unam, scriptula duo, grana quatuor. Nam mangelis, ut ante dixit noster auctor, quinque grana pendit, et septuaginta duobas granis dragma constat." His next note alludes to the diamonds he had seen himself in Belgium; and the third is upon certain crystals known as Bristol diamonds, found three miles from that city.

Passing from this curious aberration of Dr. Ball's, we may ask, What did De Boot mean by alluding in a second passage to the diamond Garcias had seen in India as weighing 187½ carats? As I have said, it is barely possible he had means external to Garcias's statement in his book of knowing the weight of this diamond. The weights summed together by Le Cluze were apothecary weights, varying somewhat in different localities in Western Europe from the corresponding divisions of the French ounce of 576 French grains, equivalent to 472·1875 troy grains. The weight of the diamond on the French system would be 573·776 grains troy according to Le Cluze's reckoning, In terms of the old Netherlands ounce of 474·75 grains, current in Antwerp, it would be 576·95 troy grains. But none of these are carat grains. De Boot, on the other hand, in estimating the 140 mangelins as 187½ carats, took the mangelin not at the 5 carat grains of Garcias, but at 5·3568 such grains, taking probably 1⅓ carat as the measure of the mangelin instead of 1¼ carat, the former being one among the several values which this variable unit had in different places.

The 187½ carats of De Boot would, on the value of the Amsterdam carat, 7½ of which equal an engle, which was the sixteenth part of the Dutch troy mark, give a weight for the diamond in question of 593·437 troy grains: the weight of the Koh-i-Nur having been 589·5 troy grains. It is very difficult to ascertain with accuracy the values of the different units—marks, ounces, carats—in the different countries and cities in the seventeenth century; but it is probable that even the mere 4 grains, or little more than a carat, difference between De Boot's estimate of the 140 mangelins and the traditional weight of the Koh-i-Nur would disappear if we possessed these data in a more complete form. There can be little doubt that Le Cluze was in error in taking the apothecary weight instead of carat weight in translating the grains of Garcias.

It may be asked, Why devote so much consideration to this casual statement of De Boot's? The answer is twofold. The astronomer has patiently searched in the records of early observations for any that might indicate the position at a former epoch of a new-found planet; and so, where the silence about an object of historical interest has been scarcely broken through two or three centuries, one tests any observation of the casual wayfarer in the domain of literature that may perhaps shed a ray of light on it. The other reason is that, if not disposed to resent, one is at least desirous to refute, attack on those who can no longer give their own answer to assailants of a new generation, who perhaps may not bring to an investigation the learning or the patient temper of those who have gone from us, and carried great stores of scholarly learning into the silence. Whether I am right or wrong in the explanation I have offered of De Boot's conversion of Garcias's 140 mangelins into 187½ carats, I trust that at any rate I have shown cause for the statement by Mr. King that "it seems as if he (De Boot) had heard of the Koh-i-Nur; it being scarcely probable that two stones should be co-existent of that extraordinary weight."

In dealing with another of those coincidences in weight to which allusion was made, and one example of which has just been discussed, we get on the delicate ground of the degree of confidence to be placed in Tavernier's facts and figures, and the not less delicate ground of a theory about the Koh-i-Nur, started by Dr. Ball, before which the other strange vicissitudes and hairbreadth escapes of that old talisman pale into insignificance.

We have made sufficient acquaintance with the historic Indian diamond to leave it for a while, in order to introduce that other greater stone which we have designated as the "Great Mogul."

Bernier, from personal contact with whom Tavernier no doubt derived much of what had an historical character in his volumes, describes the gift by Emir Jumla, a Persian adventurer of great ability in the service of the King of Golconda, of a large diamond to the Emperor Shah Jahan, "ce grand diamant que l'on estime sans pareil." It was an appeal to his cupidity, and to a real connoisseur's passion for precious stones, at a time when the Emir was effecting a change in his allegiance from Golconda to Delhi—in fact, appealing to a new master to induce him to assail the old one.

In 1665, Tavernier, who was no less a courtier than a dealer, was invited by Aurungzebe to present himself at his Court to inspect his jewels.

The Emperor, seated on the peacock throne, could see the ceremony that was conducted in a small apartment at the end of the hall. Tavernier describes the patient circumspection with which he was shown the various stones and jewels by a Persian custodian. First and foremost among them was the great diamond, "qui est une rose (a rose-cut stone) ronde (rounded but not necessarily circular in form) fort haute d'un côté." There was a small crack at the edge below, and a little flaw within. It was of fine water, and weighed 319½ ratis, which Tavernier states to be equivalent to "280 de nos carats," the rati being ⅞ of a carat, which, however, would give 279·58 carats. Such was the only great diamond that he saw, and as he first described it.

He proceeds to give his version of its history. It was the stone given by the Emir Jumla to Shah Jahan; but he adds that, whereas it had then a weight of 900 ratis or 787½ carats, it was worked down by a Venetian diamond-cutter, Hortensio Borgis, till it had only the 280 carats weight above noted. The word *égrisée* is that used; Dr. Ball interprets it as entirely *ground* down. But, though this is the most rational meaning of this technical word, it would, as Mr. King has remarked, have taken more time than the few months which intervened between the gift and the eclipse of Shah Jahan for the mere grinding down to have been accomplished by the processes in use in the seventeenth century, and especially in India. Undoubtedly, therefore, Hortensio must have availed himself of the cleavage property of the diamond to aid him in his grinding process. Tavernier goes on to say, "Après avoir bien contemplé cette grande pierre, et l'avoir remise entre les mains d'Akel-Kan, il me fit voir un autre diamant," &c., &c.; and he then describes a number of stones and pearls, of which he gives the weights, some more or less approximately, some definitely, in ratis or in melscals (or mishkals). The melscal he also states as giving 6 to the ounce, which I think is probably a mistake for 6⅔ to the ounce. Finally, he says that he had held all the jewels in his hand, and considered them with sufficient attention and leisure to be able to assure the reader that his description of them is exact and trustworthy, as was that of the thrones which he previously had ample time to inspect. It will be noted he does not say he weighed any of the stones; nor does his doing so seem compatible with his description of the scene.

But in another chapter near the end of the same book he gives a brief enumeration of the finest precious stones he had, in his long travels, known. The diamond described in the earlier chapter is alluded to now with slight but immaterial variations or corrections as to weight; but Tavernier here states that he was allowed to weigh the stone, and he further adds that it had the form of an egg cut through the middle. Dr. Ball truly notes that this process may be performed in one of two ways—longitudinally, or transversely; and that the Koh-i-Nur in 1850 represented the longitudinally bisected demi-egg, but, he naïvely adds, "This difference of form, as I shall explain, was the result of the mutilation to which it was subject."

Tavernier's statement that the diamond was "fort haute d'un côté" seems, indeed, hardly to accord with any other than a longitudinal section of the egg.

But then, as if to make his description inexplicable, Tavernier appends to this later chapter—written or edited probably by another hand four or five years after the event of his handling the stone—a rude sketch of the great diamond that he saw. It may be conceived as an extremely inaccurate sketch from memory of a semi-egg-shaped stone seen "end on," or of a cross-cut half-egg seen from any point of view; but, except for the trace of a small undercut face in his projection, it has not any resemblance to the Koh-i-Nur. In width, his sketch is very slightly larger than the length of the Windsor diamond, but in no other dimension does it at all compare with that stone as it was in 1850.

Then there is the question of weight. Babar's diamond, we have seen, weighed about 8 mishkals, or, in Indian weights, about 320 ratis (gold ratis). This would correspond to 240 pearl ratis, or may be represented as 224 of the Deccan ratis of Ferishtá.

The diamond Tavernier saw weighed, he said (was he merely told so, or did he really weigh it?), 319½ ratis, only half a rati different from Babar's diamond. But Tavernier's ratis were not those which Babar reckoned by, and his carats (*nos carats*) must (*pace* Dr. Ball) have been French carats. Dr. Ball supposes he has contributed to the published data of this tangle of contradictions one new fact in a final determination of Tavernier's carat, and, by implication, of his rati also. Tavernier gives the weight in carats of the yellow diamond of the Grand Duke of Tuscany, now in the Schatzkammer at Vienna. The weight of this stone being accurately known, and being also given by Tavernier as 139½ carats, it is not difficult to determine the value of this particular carat to be 3·037 troy grains. This is in fact identical with the Florentine or Tuscan carat, as Dr. Ball points out.

That gentleman assumes from this that Tavernier always employed this carat in his calculations. Such, however, is quite incompatible with his expression on other occasions, when he speaks of "*nos carats.*" It is clear that Tavernier took the weight of this Florentine diamond from some trustworthy Tuscan source, giving it in Florentine carats. In fact, it is an illustration of what seems to be indicated as his habit in many other instances. He gives the weights of stones he mentions in ratis or mangelins, or in mishkals, and proceeds to state the equivalent weights in terms of *nos carats, i.e.* of the Paris carat; for no Frenchman would designate any carat other than one current in France by such a term.

It would be a tedious task to inflict on a reader the minute detail of calculation and reference to statistical authorities that would be involved in a critical study of Tavernier's assertions regarding Indian and other weights, or Dr. Ball's incursion into that study.

But one fundamental error must be alluded to, that vitiates the accuracy of Dr. Ball's calculations. He is possessed of the singular belief that, in the seventeenth century, Tavernier would have been familiar with the French ponderary system known as the *système transitoire* or *usuel*, which was introduced by the law of May 1812 into France, in temporary substitution for the old livre (*poids de marc*) of 9216 French grains, and its subdivisions.

It is quite unnecessary to follow the results of this error; for the only interest as regards our inquiry concerns the significance of the 319·5 ratis which Tavernier states the great diamond of Aurungzebe to have weighed. 320 ratis was the Hindoo equivalent, in Babar's time, of the 8 mishkals of Babar's diamond, and the Koh-i-Nur in 1850 weighed those 8 mishkals.

Tavernier says that the 319·5 ratis correspond to 280 French carats (*nos carats*). Here, then, is a second of those marvellous coincidences in numbers to which we have already made allusion—I may call them impossible coincidences, unless they apply to one and the same diamond.

Dr. Ball sees, apparently, no difficulty in the recurrence of any number of these identical figures as representing the weights of huge diamonds. For his explanation of the matter is that the diamond Tavernier handled was, as the French merchant asserted, the stone that Bernier mentions as the gift of Emir Jumla to Shah Jahan; that it did weigh 319½ ratis, but that these were ratis of Tavernier's standard, equivalent, in fact, to 0·875 of a carat, whereas Babar's ratis were only 0·578 of a carat. Dr. Ball's assertion, however, is that this great diamond is the Queen's Koh-i-Nur, but that after Nadir Shah's time it had become diminished by successive chippings performed on it by needy princes, who in succession owned it, and turned its severed fragments to account, until finally, and presumably before it fell into the hands of Runjit Singh, this great Mogul diamond had shrunk in magnitude from its asserted 280 carats to 186 carats—from the 319½ ratis of Tavernier's reckoning to the 320 ratis on Babar's reckoning; in a word, it had become reduced by this astounding process to the precise 8 mishkals of the Koh-i-Nur in 1526. So here is a third coincidence that we are called on gravely to accept as serious history.

The only originality, however, involved in this singular view of history, and the way to write it, is the reason assigned for the whittling down of the diamond from the asserted 280 carats to 186 carats. Several ingenious persons have indulged before in speculations as to the synthesis of one big diamond to be called the Koh-i-nur from several smaller ones scattered about the world, with a fine scorn of shape and weight and "water" in the component fragments, and of any historical ground whatever for their hypotheses. The late Mr. Tennant, of the Strand, even engaged the services of the great Russian diamond in this mosaic, ignorant, apparently of the facts that, like the Koh-i-Nur, it is an Indian-cut stone of about 194 carats weight, and is of a brownish-yellow hue.

But the coincidences in weight of various phantom diamonds with that which Babar recorded do not come to an end even with this crowning wonder, as I shall presently show.

Perhaps some one may, in parenthesis, ask what evidence there is for the breaking up of a great diamond by owners who clung to the Koh-i-Nur with a tenacity second only to their own hold on life. To this the answer is very simple. Not one fact or plausible argument is adduced to support it. Dr. Ball's imagination is its argument; and, indeed, I cannot find one single contribution of *fact* from that gentleman to the history of the Koh-i-Nur that has any novelty at all. There remains, however, a question that has to be answered, whether this mutilation theory be ever so wild or were ever so sane. If Tavernier saw the Great Mogul diamond, where was the old Hindoo stone? or if it was, as I have supposed, the Hindoo Koh-i-Nur that Tavernier handled, where was the Great Mogul?

Tavernier saw no second diamond of the first rank in magnitude. But there were two great diamonds somewhere—Babar's and Mir Jumla's, or, as I have designated them, the Koh-i-Nur and the Great Mogul. One or other of these Tavernier has described: where was the one he did not see?

It is now thirty-five years ago that I suggested the answer. Supposing, as I did and do, that Tavernier handled the Koh-i-Nur, I indicated the prison-palace of Shah Jahan as the repository of the Great Mogul. But, whichever diamond it may have been that the French traveller saw, the other was assuredly among those splendid stones that the old Emperor told the son who had usurped his throne that he would pound to dust if their surrender was insisted on. Anyone read in Indian history needs not to be told that the threat never had to be fulfilled; that Aurungzebe, content with the realities of power, cared little for the splendours that environed it, and left his captive father in the enjoyment of the allurements and the external pomp and vanity of a sovereign's surroundings, including the collection of jewels and precious stones in which his soul delighted. On his death they were brought to Aurungzebe by his sister Jehanira, who had shared her father's captivity.

It matters nothing to the subsequent history of the Koh-i-Nur whether it or the Great Mogul was the stone that remained in the custody of the fallen Emperor. But I have maintained that it was more probable that Shah Jahan should have retained the diamond that may be styled his private property, as having been given him by the Emir Jumla; and that therefore the stone seen in Aurungzebe's possession would in every probability have been the diamond of Babar, which, like the peacock throne and other gorgeous adornments of the presence chamber, would, as a Crown jewel, have remained in the imperial treasury.

Of course, this view of the matter involves great misgivings as regards Tavernier's accuracy. It involves his having applied to the only big diamond he saw the stories he had heard, from Bernier, no doubt, and from others, regarding that other great diamond given by the Emir Jumla to Shah Jahan. It further involves his having attempted to represent in a drawing a diamond he had seen several years before, but in a drawing so absolutely unlike the Koh-i-Nur as to be hardly recognizable as representing the Queen's diamond, and even less the diamond that he himself described, as he saw it, among the treasures of Aurungzebe.

The Great Mogul diamond had been cut by a European cutter. But, so far as it is of any value at all as evidence, Tavernier's drawing suggests a characteristically Indian-cut stone, much resembling in form and facetting the Russian diamond known as the "Orloff," which I have inspected, and can aver to be Indian in its cutting. The Koh-i-Nur, too, to which I personally gave careful attention in 1851, was no less unquestionably Indian in its facetting. Models in plaster-of-Paris made directly from the diamond confirm this; and traces of the original faces of the diamond, besides two large octahedral faces, appear to have been worked into the design of the facetting. The rows of facets were obviously put on so as to humour the original form of the stone and diminish its weight as little as possible; and notably they were thus skilfully arranged in regard to the upper edge of one of two large octahedral faces that has erroneously been described as a cleavage plane due to a fracture after the cutting had been performed. In fact, it and another large face, forming the base of the crystal, had not the lustre of cleavage surfaces, but wore the aspect of faces that had so far undergone attrition, probably in a river-bed, that the angle between them was no longer quite the true octahedral angle. The facets in general presented an imperfect adamantine lustre, and appeared slightly rounded, the result, probably, of the imperfect processes employed by the native Hindoo lapidary, especially in very early times.

Even Tavernier's drawing rudely indicates three rows of facets, put on in a manner that hardly consists with the fashion of a rose-cut diamond of European workmanship.

With my profound scepticism as to the critical value of Tavernier's arithmetic, I have ventured to think that the simplest explanation of all these instances of marvellous recurrence in various forms of the numbers representing the weight of the Koh-i-Nur is best explained by supposing that Akhil Khan gave Tavernier the traditional weight of the Babar diamond which he had placed in his hand, and that the French merchant translated this weight into carats, not as from the old ratis of Babar's or even of Akbar's day, but from the pearl ratis, of one or other value, with which he had become acquainted in the bazaars of India. Tavernier's rati, as calculated from the Paris carat on the ratio of $\frac{7}{8}$, should have a value of 2·77088 troy grains, and as drawn from his various statements of equivalent weights it varies from 2·4066, in one case 2·750, to 2·797 troy grains. His mishkal also he puts at $\frac{1}{6}$ the French ounce, *i.e.* 78·7 troy grains; which should, however, probably have been $6\frac{1}{2}$ ounces to the mishkal, and the rati of Tavernier is entirely dissimilar to any known rati of ancient or modern India.

The $319\frac{1}{2}$ ratis is readily explained on this hypothesis; and it is really too large a demand on our credulity to believe that two of the largest diamonds in the world should be severally of $319\frac{1}{2}$ ratis and 320 ratis, though of different units of value, when a simpler explanation is able to dispose of the anomaly.

I have said that the marvellous coincidences of weight imported into the Koh-i-Nur history do not come to an end with Babar's 8 mishkals, with Anselm de Boot's $187\frac{1}{2}$ carats, with Tavernier's $319\frac{1}{2}$ ratis, nor even with Dr. Ball's miraculous chipping process, resulting in a reduction of the Great Mogul diamond to the identical weight of the Koh-i-Nur in 1850. The original diamond of Babar had to be accounted for, and its ghost had to be laid. So another coincidence had to be imported into the narrative, or rather into the romance. Another diamond had to be found, also with the precise weight of the Koh-i-Nur, and this Dr. Ball has ready to hand. The Darya-i-Nur, or "Sea of Light," reposes in the treasury of the Shah. Sir J. Malcolm saw it, and casually stated its weight as given to him at 186 carats. Now Sir J. Malcolm, during his residence at the Court of the Shah, not only was acquainted with the marvellous treasures in jewels brought by Nadir from the palace of Delhi, but he was enabled to have *facsimile* drawings of them made.

By the kindness of his son, General Malcolm, I possess the tracings of this dazzling wealth of jewellery. The Darya-i-Nur is a large flat diamond with bevelled edges, and in the form of a long rectangle. When Malcolm knew it, it was set in a glorious galaxy of mighty rubies. He could therefore have only known its weight from hearsay evidence, and the recorded carats were most likely the echo of those associated with the fame of the Koh-i-Nur. Now, I have no hesitation in asserting this Darya-i-Nur to be an old acquaintance of those familiar with Tavernier's pages. Unless two diamonds, flat, bevelled, and of identical dimensions, can be shown to co-exist, of above 200 carats weight, the stone known as the Golconda diamond or the Table diamond is no other than the Darya-i-Nur.

It happens fortunately to be one of the few stones described by Tavernier to the form and weight of which, as given by him, we can attach complete confidence. He had a lead model made from it in order to negotiate its sale: and he gives its weight as $176\frac{1}{8}$ mangelins, or $242\frac{5}{16}$ "de nos carats." This gives its weight at 767·42 troy grains, or 240 English carats, this particular mangelin being, on Tavernier's estimate of $1\frac{3}{8}$ of a carat, about 4·357 troy grains. Tavernier having had a lead model made of this remarkable flat diamond, he figures it no doubt with much exactitude. A copy of his figure and of the tracing of the Darya-i-Nur is subjoined, in which it will be seen that if the unsymmetrical end be cut off and the sides more accurately squared, so as to make the diamond a symmetrical rectangle, the figures of the two stones become identical in form and dimension. A card cut to represent the "Golconda" diamond, and the parts of it as described, gave the ratio of

the Golconda : the Darya-i-Nur = 10 : 8·5,

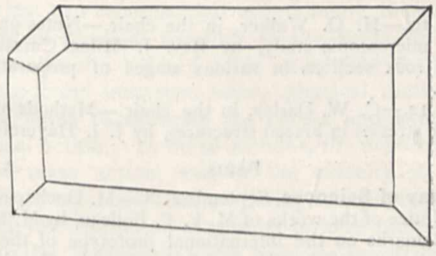
that is to say, the portion trimmed away was about 15 per cent.

The remaining stone would thus have a weight of about 214 English carats, and if 4 carats be allowed for the bevelling and squaring of the stone, the present weight of the Darya-i-Nur should be about 210 English carats.

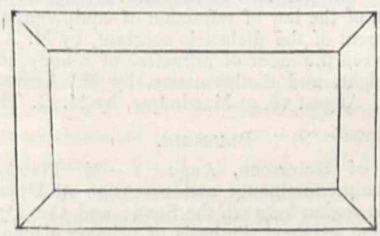
I trust I have thus laid this last phantom raised by the author of the "true history." But the final problem as to the Great Mogul diamond still remains.

If the Queen's proud trophy of the final conquest of India is indeed the great Koh-i-Nur, the old Malwa diamond descending to Her Majesty from the possession of Patan and Mogul dynasties of Delhi; carried off to Persia and named by Nadir; seized as the potent talisman of empire by Ahmed Shah, and held by his Durani descendants till it came back to India, the companion of the exile of Shah Sujah, and then torn from him by the grim Lion of Lahore—true to its destiny as "the possession, ever, of him that was the strongest,"—if this be indeed the stone that, from early times to 1850, preserved its form and weight of 8 mishkals, where was, and where is now, the Great Mogul diamond that Bernier told of? The answer is, I believe, the simplest and the most natural: It is, where the historian would look for it, in the treasury of Teheran. One large diamond, standing high upon an elliptic base, is there, or was there, in Sir John Malcolm's day. Its long diameter is much larger, and its shorter diameter smaller, than that of the diamond figured by Tavernier.

I do not assert it to be the Great Mogul. I assert merely that it probably is that great diamond; and I hope that in what has been said in the criticisms I have here offered upon the writers on the Koh-i-Nur I have averred nothing that does not rest on proof; that I have offered no conjecture that is not supported by reasonable probability; and that I have made no assault on any theory or fact asserted to be such by others, without at least offering some justification for my criticism in the reasons and facts I have been able to adduce.



Golconda Table Diamond.



Darya-i-Nur.

A true history of the Koh-i-Nur has still to be written. I hope I have, in these criticisms, done something to clear the way for the writer of it. Other avocations and duties may prevent my undertaking the interesting task. At any rate, if it should ever be mine to perform it, I trust the result will at least bear some verisimilitude to a true history.

N. STORY-MASKELYNE.

SCIENTIFIC SERIALS.

A LARGE portion of the number of the *Botanical Gazette* for July is occupied by an instalment of Mr. John Donnell Smith's "Undescribed Plants from Guatemala"; several of the new species are figured. New parasitic or saprophytic Fungi—Hyphomycetes and Uredineæ—are described in this number by Mr. R. Thaxter, and in that for August by Mr. J. C. Arthur. In the latter, M. T. Holm continues his study of some anatomical characters of North American Gramineæ, and Mr. F. Lamson Scribner contributes a sketch of the flora of Orono, Maine.

THE numbers of the *Journal of Botany* for August and September contain the conclusion of Mr. G. Murray's important paper on the Algæ of the Clyde sea-area, accompanied by a map showing the various depths. This paper has now been issued separately. In his notes on Mycetozoa, Mr. A. Lister describes species found in various herbaria not included in Dr. Cooke's "Myxomycetes of Great Britain"—three of them new

The paper is illustrated by five plates. Three new British species of *Hieracium* are described by Mr. E. F. Linton and Mr. W. H. Beeby.

SOCIETIES AND ACADEMIES.

Sydney.

Royal Society of New South Wales, August 5.—H. C. Russell, F.R.S., President, in the chair.—On the microscopic structure of Australian rocks, by Rev. J. Milne Curran.—The Chairman presented the Society's bronze medal and a money prize of £25, which had been awarded to Father Curran for this paper.—Prof. Anderson Stuart exhibited his new instrument for demonstrating the nature of such waves as those of light.

August 10.—H. O. Walker, in the chair.—Notes on slicing rocks for microscopic study, by Rev. J. Milne Curran, illustrated by rock sections in various stages of preparation for mounting.

August 12.—C. W. Darley, in the chair.—Methods of determining the stresses in braced structures, by J. I. Haycroft.

Paris.

Academy of Sciences, September 28.—M. Duchartre in the chair.—Notice of the works of M. P. P. Boileau, by M. Maurice Lévy.—Remarks on the international prototype of the metre, by M. Foerster.—Observations of four asteroids, discovered at Nice Observatory on August 28 and September 1, 8, and 11, by M. Charlois. The positions on the dates of discovery are given.—Verification of the law of refraction of equipotential surfaces, and measurement of the dielectric constant, by M. A. Perot.—Relation between the index of refraction of a body, its density, molecular weight, and diathermancy, by M. Aymonnet.—On the cyclone of August 18, at Martinique, by M. G. Tissandier.

Brussels.

Academy of Sciences, August 1.—M. Plateau in the chair.—On the predominance and extension of Upper Eocene deposits in the region between the Senne and the Dyle, by M. Michel Mourlon.—Direct synthesis of primary alcohols, by Dr. P. Henry.—On circular sections in surfaces of the second degree, by Prof. Cl. Servais.—On the curvature of lines of the order p possessing a multiple point of the order $p-1$, by M. A. Demoulin.—Preliminary notes on the organization and development of different forms of Anthrozoaires, by M. Paul Cerfontaine. The author describes a new Cerianthus from the Red Sea, and names it *Cerianthus brachysoma*. He has also studied in detail the tentacles of *Cerianthus membranaceus*, and the variations of these organs during successive stages of individual evolution, and relates an interesting case of regeneration observed in *Astrodes calycularis*.—Researches on the lower organisms, by M. Jean Massart.

Göttingen.

Royal Scientific Society.—The *Nachrichten* from June to August 1891 contain the following papers of scientific interest:—

June.—Karl Heun, Berlin, mathematical note on the integration of the equation for the motion of Gauss's bifilar pendulum.

July.—Fr. Schilling, note on an interpretation of the formulæ of spherical trigonometry when complex values are assigned to the sides and angles of a spherical triangle.

August.—Eduard Riecke, on the molecular theory of piezoelectricity and pyroelectricity.—Tammann and W. Nernst, on the maximum vapour tension of hydrogen liberated from solutions by metals.—Tammann, the permeability of precipitate-films.—Eduard Riecke, on a surface connected with the electrical peculiarities of tourmaline. — David Hilbert, the theory of algebraic invariants of forms with any number of variables.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Universal Atlas, Part 7 (Cassell).—Food, Physiology, &c.: W. Durham (Black).—British Edible Fungi: Dr. M. C. Cooke (Paul).—South Africa, from Arab Domination to British Rule: edited by R. W. Murray (Stanford).—An Elementary Hand-book on Potable Water: F. Davis (Gay and Bird).—The Birds of the Sandwich Islands, Part 2: Wilson and Evans (Porter).—Differential and Integral Calculus: T. H. Miller (Percival).—Physiography: J. Spencer (Percival).—Geodesy: J. H. Gore (Heinemann).—Electricity and Magnetism: A. Guillemin; translated by Prof. S. P. Thompson (Macmillan).—Annuaire de l'Observatoire Municipal de Montsouris, 1891 (Paris, Gauthier-Villars).—Stones for Building and Decoration: G. P. Merrill (New York, Wiley).—Taxidermy and Zoological Collecting: W. T. Hornaday (Paul).—Dynamics of the Sun: J. W. Davis (New York).—The Man of Genius: Prof. C. Lombroso (Scott)—Ninth Annual Report of the Fishery Board for Scotland, Three Parts (Edinburgh).—Richerche Sperimentali Intorno a Certe Scintille Elettriche constituite da Masse Luminose im Moto: Prof. A. Righi (Bologna).—Proceedings of the Liverpool Geological Society, Part 3, vol. vi. (Liverpool).—Mind, No. 64 (Williams and Norgate).—Journal of the Royal Statistical Society, September (Stanford).—Journal of the Royal Agricultural Society, 3rd series, vol. ii., Part 3 (Murray).

DIARY OF SOCIETIES.

London.

THURSDAY, October 8.

Camera Club, at 8.30.—Paper by Captain Abney.

MONDAY, October 12.

Camera Club, at 8.30.—Lantern Evening.

THURSDAY, October 15.

Camera Club, at 8.30.—Bacteria Photographed: Andrew Pringle.

CONTENTS.