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THE QUARTERLY
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I. THE DOLMEN-MOUNDS AND AMORPHOLITHIC MONUMENTS OF BRITTANY.

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Part I.

The Dolmen Mounds of Brittany; their History and Analogues.

The study of prehistoric archæology has now become a recognised branch of accurate if not exact science, by which valuable assistance is rendered towards the elucidation of many problems of universal interest in philosophy as well as ethnology. It increases our knowledge of the progress of human thought as exhibited by the habits, occupations, and arts of primitive man, whilst it also furnishes a clue to the intricate currents of the tide of ethnic migration; the first is supplemented by comparison with analogous modes of life, still found in existence amongst tribes of modern savages, whilst the latter throws light upon the primeval relationship of peoples, whose descendants are at present separated by broad intervening obstacles, whether seas, deserts, or other races of foreign stock.

Brittany offers to the ethnologist an unusually wide field for investigation in this direction; "*La clef de l'ethnologie de la France est en Bretagne*," says Professor Broca, and the numberless megalithic remains to be found in this province, especially in the departments of Finistère and Morbihan, offer promising mines of research. Brittany may well be called "*La Terre des grands souvenirs.*" Amongst these interesting monuments, M. de Freminville is a sure guide to their localities, as indeed he is to all the Breton antiquities, which he has made his especial study.

M. de Freminville divides the megalithic (sometimes wrongly termed cyclopean) remains under three heads:—1. Religious; 2. Sepulchral; and 3. Memorial; and he attributes them all to Celtic construction.

1. In the first group he places all the dolmens and demi-dolmens, which, following the popular ideas of his time, he supposes to have been Druidic altars, on which human sacrifices were performed, from the summit of which Arch-Druids preached, and inside of which they also had their habitation: where also an eternal pyre was kept burning, from which source the neighbouring Gauls daily obtained fire for their own domestic hearths, which were each night extinguished by the inexorable Celtic law of *couvre-feu*, the origin of which is lost in the gloom of prehistoric ages. He places also under this class the circular and other enclosures of upright stones, which he supposes to have been Druidic temples and sanctuaries.

2. The sepulchral group include the *menhirs* and alignments of *peulvens*, as also the confused assemblages of stones called in Celto-Breton "*Carneilloux*," which the Chevalier supposes to have been cemeteries, the grave-mounds or tumuli, to some of which, when composed simply of stones heaped upon one another (cairns), the distinguishing Celtic name of *galgal* is given.

3. To the memorial group are referred the isolated *menhirs* of extraordinary size, commemorative of victory or some momentous event.

Such were the ideas generally received some five-and-thirty-years ago, when De Freminville wrote; but since, a more perfect knowledge of various facts connected with these monuments has wrought a revolution of opinion, and the term Druid's altar is now obsolete, and no longer to be found in the antiquary's text-book.

After careful examination of numerous cromlechs and tumuli by excavation, the study and comparison of their varied contents has led the first archæologists of the day to conclude that *all dolmens* were originally covered with tumuli*, and that one and all of these and analogous structures are satisfactorily proved to be of a sepulchral character. The circles and alignments are, however, by some excluded from this category, but they are admitted to have had some connection with funeral rites and ceremonies.

* I include in the word *dolmen* all megalithic chambers, whether enclosed in mounds or deprived of their primitive coverings, and in treating of Breton monuments I insist on their tumular character as a principle of universal application. I know and admit of no so-called dolmen which does not or should not come under this rule. This megalithic structure is a tomb in every stage of dilapidation. It may be found totally enveloped or partially exposed, or wholly denuded, and in every intermediate state, in which may be detected the vestiges of the original tumulus. Instances of complete denudation are comparatively rare.—Rev. W. C. LUKIS.

It is to local museums that we must look for collections of the stone implements, ornaments, pottery, &c., found in the ancient sepulchres of the surrounding country; and in this respect the Bretons are not behindhand. The Museum formed by the Societé Polymathique of the Department of the Morbihan at Vannes is well worth a visit from every student in prehistoric archæology, and, although small, the collection of neolithic implements and ornaments is well assorted with special regard to the localities in which they were found. By inspection of the catalogue which enumerates and classifies these "*objets de l'age de la Pierre Polie,*" it appears that the museum contains 198 good specimens of stone celts, besides fragments, as well as flint knives, scrapers and flakes, hammer-heads, gorgets, torques, pendants, &c., all from the surrounding department. The other principal collections of relics of the stone period, from Brittany, are at St. Germain's, at Plouharnel (Madame Le Bail's), Mr. Lukis's Museum in Guernsey, and Mr. William Lukis's collection in Yorkshire. Our public collections in England are remarkable for their deficiency in Brittany examples, there being but one in the British Museum, and the Christy Collection, which is rich in celts from the Auvergne, has nevertheless none from Brittany. In the Blackmore Museum at Salisbury are a few specimens given by Mr. Barnwell, but this model museum possesses an instructive series of casts from those in the Vannes Museum.

In respect to pottery, the Morbihan collection is poor, whilst there is but a scanty show of flint-knives, flakes, and arrow-points; with regard to the arrow-points, Mr. Lukis says,—

"It is a circumstance which should be mentioned with reference to the flint arrow-points, that the members of the Polymathique Society of the Morbihan, after fifteen years' labours, have succeeded in finding two only, and this circumstance has led them to remark that these objects are very rare in the department; but I entertain a different opinion, and conclude that they have not searched carefully, for in one chambered barrow I have found *nine,* in another *three*, and in a third *one;* in each case after a previous disturbance of their contents. They are small objects and easily escape detection, more particularly when explorers neglect to use a sieve. The truth is that explorations of these buildings are generally carried on too rapidly, especially by those whose place of residence is at a distance, as well as by those who employ paid labourers, to whom, therefore, time is of great consequence."*

The French *savans* have since been more successful in finding these smaller relics. The accounts of the explorations of the various tumuli, published by the society from time to time in small pamphlets, are unusually interesting. It is indeed enough to make our island archæologists die of envy to read M. Galles's account of the mining and excavation of the huge tumulus of Mont San Michel, in September, 1862. There is an almost exciting description of the moment when, a shaft having been sunk from the summit, the small sepulchral chamber was first broken into, and the enthusiastic explorer, placing his lamp within the aperture, sees the glistening of the pòlished celts, jasper and turquoise beads, &c.; and how, when entering alone into the small kist, he hands out to his colleagues, Louis Galles (" *qu'Hoffman eût nommé l'homme aux dolmens* ") and M. Lallemand, one by one, these valuable relics, amongst other things, eleven jade celts and six-and-twenty small ones of fibrolite.

Unfortunately the members of the society could not always be on the spot during the progress of the excavations, and consequently many valuable celts and other ornaments from this same tumulus are in possession of the neighbouring farmers and peasantry, who are not unwilling to sell them, whilst the proprietors of the small inns at Carnac have some handsome specimens, which they keep to exhibit to tourist visitors, &c., and of the value of which they have rather an exalted opinion. "Helas!" exclaims M. René Galles, "Le dirai-je; nos paysans vendent aux Anglais les celtæ de leurs pères!"

It may here be noted, for the information of visitors to this locality, that there is a notorious manufacturer of pseudo-celts in the village of Carnac.

The tumulus of Tumiac, on the Arzon promontory, was one of the earliest attacked by the Vannes Society, where the find was rich and encouraging; similarly, Manné-Lud, at Locmariaquer, Le Moustoir, and the barrow in the grounds of Baron Walbrook, at Kercado, with others, have been opened with varied success; but perhaps the best worth recording is the discovery of the dolmen under the large mound named MANNÉ-ER-H'ROËK.

MANNÉ-ER-H'ROËK is a conspicuous tumulus outside the

* On a Remarkable Chambered Long Barrow, at Kerlescant Carnac, Brittany, by Rev. W. C. LUKIS, p. 6.

village of Locmariaquer, close to the sea-shore, and not far from the Pte. de Kerpenhir, at the entrance to the Gulf of the Morbihan. On the French naval charts it bears the name of "*Butte de César*," but the only name by which it is known to the native peasantry is *Manné-er-h'roëk* (Montagne de la fée). According to the French account, it is a long barrow, measuring over a hundred yards in length by 60 in breadth, whilst it has a height of 33 feet. Its longer axis is what would be termed orientated by those who see orientation in all these remains, although it is within two degrees of the S.E. and N.W. points of the compass. Mr. Lukis considers this tumulus to have been originally round, and that its elongated appearance at present is owing to its sides having been removed in the course of agricultural operations. At its base on the N.E. side are two fallen *menhirs* of considerable size, whilst some smaller stone, not far from the present base in the opposite direction, may perhaps be taken to indicate the remains of a *peristalith* which once surrounded the grave-mound.

In September, 1863, M. Lefebvre, the Préfet of the Morbihan, and M. R. Galles, caused this barrow to be explored; a party of soldiers under their direction soon sank a large crater about the middle of the mound, and it was found that it was a galgal built up in the usual way, of rough stones piled one on another, and not dissimilar to the analogous mounds at Tumiac and Mount St. Michel. Not far from the summit they found in the course of clearance some Roman coins and *débris* of Roman vases; deeper they found beads of enamel, jasper, and agate; until in the very centre they arrived at a sepulchral chamber 12 feet long, 8 feet broad, and 5 feet high. It was not a megalithic dolmen, but consisted of three roofing-slabs supported by dry walling, and, according to Mr. Lukis,* who groups the dolmens according to the construction of their roofs, may be termed a *ceiled* in contradistinction to the *vaulted* sepulchre. Outside the entrance to this rude cell were found the three fragments of a stone tablet with curious incised archaic sculpturings upon them. These appear to belong to an altogether different type of pattern to the elaborate ornamentation of Gavr' Inis, and more resemble the ruder engravings found on the slabs of Manné-Lud and on the large

* "On the Various Forms of Monuments commonly called Dolmens in Brittany, pointing out a Progress in their Architectural Construction, with an Attempt to Reduce them to Chronological Order." By the Rev. W. C. Lukis, M.A., F.S.A.—"Transactions of International Congress of Prehistoric Archæology." Third Session.

cap-stone of the Table des Marchands; the peculiar pattern, which is generally understood as representing the hafted celt, occurs in all the latter dolmens as well as in the chamber of the tumulus at Kercado. There may be some analogy between this memorial tablet being buried at the entrance to the chamber under the enormous galgal, and the ancient custom of the Scandinavians, as shown by the finding of rune-stones inside the cairn during the oldest period of the iron age; whilst the rich decorations and hieroglyphics beautifully inscribed on the sarcophagi of the Egyptian and Oriental tombs point in the same direction. The fact of some of these rude characters being surrounded with a species of cartouche or label in the case of the Manné-er-H'roëk inscription is worth noticing from the fact that it is also found according to Professor Stephens, of Copenhagen, enclosing Runic characters, but only of the most ancient description, whilst its frequent appearance on Egyptian and Phœnician monuments is well known. On the pavement of flat stones in the interior of the chamber were found exposed two celts and a ring, both of jadeite, with three jasper pendants. The ring is partly oval, flat, and polished, and is the only one of its kind in the Museum at Vannes. Somewhat similar rings have been found in Guernsey, where they are in Mr. Lukis's Museum. One of these shows signs of wear, and it is conjectured that they were probably worn as a gorget;* they are too small for the wrist. But perhaps the most remarkable feature in the exploration of this chamber was yet to follow, and that was the discovery under the pavement before-mentioned of the extraordinary number of 101 celts, 91 of which were of *fibrolite*, small, sharp, and perfect, whilst the larger ones of jade, diorite, &c., were more or less fractured; some of these last are noticeable as being pierced for the purposes of suspension.

Such an unprecedented find of celts has never before or since been equalled, at least in Europe; whilst the enormous proportion composed of that rare material called *fibrolite*, which is not known in Europe as a native substance, is also astonishing.

The significant facts, that all the small fibrolite celts are in good preservation and appear to have never been used, whilst the rougher and larger specimens show signs of wear,

* Flint Chips, p. 104. By E. T. STEVENS, Hon. Curator of the Blackmore Museum.

and were also apparently purposely* so fractured when deposited, are well worthy of our closest attention.

Now it is found that in all interior or sepulchral finds the celts made of fibrolite are at least 75 per cent of the whole number found, whilst the surface and miscellaneous finds produce but few of this material, being generally of diorite and porphyry; thus, wherever found, the fibrolite celt is indicative of sepulchral associations, and was evidently not made for daily use. A superstitious reverence probably attached to the rare stone, as in China at the present day the possession of a certain amount of jade† entitles the owner to a certain rank, and as the jade celt also confers certain rights of chieftainship in New Zealand. This celt may also have been used as an amulet or talisman, like the siger stone (victory-token) and life-stone (against wounds and death) used by the Norse and Swensk adventurers of Scandinavia, which were worn on their weapons or armour, or on the person in a small bag, and accompanied them to the tomb.‡

Although there is a slight difference in the exterior contour of the great Dolmen-mounds of Tumiac, St. Michel, and Manné-er-H'roëk, the first being most conical, and the second much the longest of the three, still they may be placed in the same class as to their structure, being all composed of alternate layers of sand and stones, the implements found inside of them being also similar. Therefore we may with tolerable certainty assign them to a contemporary period and the same race of builders.

At the same time Mr. Barnwell has rightly pointed out the different methods of interment practised in each mound; thus, in the chamber of Tumiac, the body had been placed in its natural state, whilst in that of St. Michel complete incineration had first taken place. In Manné-er-H'roëk, on the other hand, no human bones or least trace of them could be found by the explorers. Could this last have been a cœnotaph?

Whilst on the subject of *cremation* and *inhumation,* and as an additional proof that both were practised at the same

* The spoon, mat, pillow, and spears of a dead Kaffir are laid beside their owner in the grave; the shafts of the latter are always broken and the iron heads bent, perhaps from some vague idea that the spirit of the deceased will come out of the earth and do mischief with them.—Rev. J. G. WOOD, Africa.

† "To Heaven alone is offered a piece of blue jade, cylindrical in shape and a foot long, formerly used as a symbol of sovereignty."—"Sacrifice by the Emperor of China." *Vide* WILLIAMSON'S Journeys in North China; ch. xvi., "Peking," by the Rev. JOSEPH EDKINS, B.A.

‡ Professor STEPHENS.

time* we may notice the curious elongated tumulus of Manné-Lud in the same neighbourhood, being only a mile distant from Manné-er-H'roëk. Here in one chamber were found the remains of two human bodies, side by side, one of which had suffered incineration and the other simple interment. At the western extremity of this mound a magnificent megalithic dolmen has long ago been exposed, whilst at the eastern extremity the explorers found a curious arrangement of stones with an assemblage of burnt animal bones, as if an holocaust had been burnt in sacrifice. A parallel instance of finding the bones of animals occurs in an oblong barrow at Dalby Tyrstrup-hundred, South Jutland, where the skeleton of an ox was discovered in 1840; a golden head-ring was also found in the same mound.

According to Mr. Lukis, the practice of burning the dead was nearly unknown to the builders of the megalithic dolmens. The eastern portion of the tumulus of Manné-Lud is probably of later date than the dolmen at the west.

The next example of a dolmen under a tumulus, viz., that of Kercado, is taken as presenting a complete contrast to those of Manné-er-H'roëk, St. Michel, &c.; as those huge tumuli enclose but rude, small, and imperfect sepulchres, whilst this modest hillock, one-tenth of their bulk, contains within an highly finished megalithic dolmen, consisting of a square chamber and gallery of upright blocks supporting large cap-stones. On one cap-stone and on two of the side blocks are signs of rude ornamentation. It is suggested with great probability by M. Galles that this tumulus must have been despoiled of its contents previous to the visit of its late examiners in 1863, as but few objects of human workmanship were found within, whilst bones comparatively well preserved, with Roman pottery, were found within, indicating, according to the Marquis de Valory, that the Romans had used this aboriginal tomb as a place of burial during their occupation of Armorica. A minute jadeite celt was found, a larger one of diorite with *callaïs*† beads and some pendants of talc,

* A modern instance of several kinds of sepulture being practised by the same people at the same time is to be found in Thibet. The Llamas of Thibet, according to Huc, have four methods of disposing of their dead, viz.:—1. Combustion; 2. Immersion in rivers and lakes; 3. Exposure on the summits of mountains; and, 4, the most esteemed—Cutting up the bodies in pieces and giving them to the dogs. As to this last method, the poor people have the dogs of the suburbs for their mausoleum, but for persons of distinction a little more ceremony is used. There are convents where sacred dogs are kept on purpose to devour the corpses of rich Thibetans.

† Le nom de callaïs a été imposé par M. Damour au minéral qui forme nos grains de collier. Le couleur de cette matière est le vert-pomme, se rapprochant

agalmatolite, and mica-schist, interesting on account of their similarity with articles found in the lake dwellings of Switzerland.

Another unusual type of dolmen is exhibited by that exhumed at Kergonfals, in the Commune of Bignan, near Locmine. In the first place, it is in the interior of the department, at a considerable distance from the sea coast, and therefore it is an exception to the generally received supposition that the dolmens are universally found by the shore, as the majority certainly are; but this, with other examples, seems to prove that it is only the richer cultivation of the interior which has caused the demolition of the stone structures, whilst the exposure of the site and sterility of the soil have been the means of saving similar remains on the coast from the plough of the farmer. Again, the tumulus of Kergonfals is not built, as usual, on the summit, but on the side of a slight eminence.

In company with two other dolmens, *viz.*, Le Rocher in Plougemelen and Les Pierres Plates at Locmariaquer, the *allée couverte* forms an angle. The plans of all three dolmens exhibit a decided curve, although in different directions.

Another unusual feature at Kergonfals, also, is that the eastern extremity of the gallery is at a higher level than the chamber and adjacent passage, into which there is an abrupt descent; two portions, also, of dry walling divided the gallery when first explored. The interior find was not large, comprising three blunt quartzose cells of rude type and imperfect polish, two flint knives, and an empty pottery vase; but the explorers were fortunate in obtaining some human bones, from a careful observation of whose position an expert anatomist (Dr. Mauricet) was enabled authoritatively to state that they were the bones of a strong and athletic man, whose body had been placed in a sitting or crouching

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du vert de l'émeraude. Quelques echantillons sont comme marbrés de parties blanches et de parties bleuatres; d'autres sont maculés de veines et de taches brunes ou noires, par suite d'un mélange accidentel de matières argileuses. Le mineral est translucide, à peu près autant que la chrysoprase. Sa cassure est compacte comme celle de la cire. Il raie le calcaire, mais il est facilement rayé par une pointe d'acier. Sa poussière est blanche, infusible au chalumeau. Cette substance est un phosphate d'alumine hydraté comme la turquoise orientale, mais elle en diffère sensiblement, aussi bien par les proportions de ses principes constituants que par ses caractères extérieurs. M. Damour, d'après les différences appréciables que existent entre ces deux matières, les sépare dans la classification des espèces. Il emprunte à Pline le nom de callaïs, qu'il applique à notre minéral, et réserve celui de turquoise à la pierre précieuse de couleur bleu de ciel, si comme en joaillerie.—Voir la description de la callaïs par M. Damour; Comptes Rendus de l'Academie des Sciences, Tome lix., Séance du 5 Décembre, 1864.)

posture, with the head down on the knees; a decision which undoubtedly proves the tumulus of Kergonfals to have been a place of sepulture by inhumation.

This burial in a sitting or crouching posture seems to have been usual, at various periods, among the dolmen-builders, as similar examples have been found in Scandinavia, as at Goldhaon, in the Channel Islands, as at Du-Thus, opened by Mr. Lukis, where two skeletons were found, under the cap-stone of one of the small northern chambers, in a similar position. At Charlton Abbots, in Gloucestershire, were found as many as twelve, and at West Kennet, Wilts, six bodies, all in a sitting position; so, also, at Uley and Avening the same position is noticeable. Nor are there wanting modern instances of burial in the sitting posture, which is practised amongst the Japanese, Australians, and Esquimaux. Among the Kaffirs, also, the body is never laid prostrate, but the body is placed in the grave in a sitting posture, the knees being brought to the chin and the head bent over them.*

Some very slight traces of artificial working of the interior stones seem to have been considered as doubtful by the explorers. We cannot but regret, in reading Mr. Galle's account of this examination, that his party thought it necessary to remove several of the cap-stones in the course of their work, which, from our experience, can never be really necessary. He says:—"Nous nous décidâmes à regret à enlever les deux premières tables, apres avoir pris un croquis exact de l'état des lieux."

The better-known hollow tumulus of Le Rocher, which has been noticed above, was cleared out by the late M. Bain, the owner of the neighbouring property, as long back as 1844, and the few objects found therein are still preserved by his widow: these principally consist of a few fragments of pottery, one of which was the base of a vase, containing beads of blue jasper and dark jade, with a blade and arrow-head, both of flint.

The grotto itself, beneath the superimposed galgal, or cairn, consists of thirteen broad and flat cap-stones, supported by upright slabs, the interstices between which are built up with dry walling. The angle which the eastern gallery makes with the western portion is as nearly as possible in the centre of the structure; the angle is 125°, whilst that at Kergonfals is more acute, being 108° only; as usual,

* Some Arab tribes, in the Hadhramaut, according to Von Wrede, yet practise the ancient pre-Mohammedan (Himyaritic?) fashion of interment, with the knees drawn up to the head.

the *allée* itself and the stones composing it are larger in width and height at the western than at the eastern extremity. Here we may observe that this is one of the few monuments of its kind which it is a real pleasure to visit, as it is kept free from rubbish and filth, which generally render such grottoes so disgusting to penetrate, and when illuminated presents a striking appearance. The last time we visited this sepulchre we found it arranged with seats at the western chamber, and decorated with garlands in honour of some little *fête*, when it presented the appearance rather of some nymph's retreat than a dismal charnel-house. On a bright day, after entering this vault without lights, after allowing one's eyes to get accustomed to the gloom, the daylight through the entrance is quite sufficient to render the stones in the chamber itself visible, and under these circumstances the angle of the gallery, as seen from the interior, is impressive. There is some sculpturing on one or two of the side slabs in this dolmen, which is of the same type of pattern as those found at Les Pierres Plates.

This elaborate ornamentation, which is a marked characteristic of some of the dolmens, may help archæologists in determining the relative antiquity of the monuments in which it is found. The most ancient markings are most probably the so-called cup-markings, which have successfully baffled hitherto all enquirers as to their origin and meaning, and seem to be found in the New World as well as the Old. In the Ohio mounds, and at Orizaba, in Scandinavia, the Channel Islands, England, Scotland, Wales, Brittany, and Switzerland, they are to be seen; their interpretation remains a mystery. These cup-cuttings, in fact, hardly come under the head of Archaic sculpture.

Irregular lines and a species of net-work seem to be the earliest of any actual design or pattern, as at Kercado and Kerozille; next a pattern of pot-hooks, or boomerangs, as at the Dol-au-Marchand; all these last are rude and nearly effaced. Whilst speaking of the Dol-au-Marchand, the recent disfiguration of the western upright, which exhibits these most interesting Archaic sculpture, deserves severe reprobation. Some wilful hand has carved, with an iron tool, in the centre of the slab, the word GAZELLE, in large letters: the mere mention of this fact, and the thought that it is attributable to British visitors, is sufficient. The progress of sculpture, as developed at Pierres Plates and Gavr' Inis, is instructive. In the first the patterns are more regular, but in the last they are more elaborate, and seem even as intended to convey some information by means of their hieroglyphics.

One thing seems almost certain, and that is—that this elaborate decoration of the interior of long galleries,* leading to sculptured chambers, shows to us that these sepulchral chambers were intended to be visited subsequent to the interments; and this is the more likely, inasmuch as these galleries are so orientated (at least 66 per cent are so in Brittany) that, at some season of the year, the sun, on rising, would brightly illuminate the interior of the tomb. Ellis mentions an analogous custom amongst the Hovas, in Madagascar, at the present day. He says:—"The Hova chiefs manifest considerable solicitude about their graves; and I was told that one of the chief officers, who died lately at the capital, requested of his sons, shortly before his death, that after his interment they would occasionally remove the large stone slab that would form the door of his sepulchre, and let the sun shine in upon him." †

Mr. Lukis's classification of ceiled and vaulted sepulchres in Brittany has already been mentioned; he supposes that their various forms indicate not merely a prolonged residence of their builders in the country, but also a progress in their constructive science: thus several of their forms may have

* Compare Dr. Palmer's account of the stone houses in Easter Island:—"At the south-west end of this island, at the sea edge of the Terano Kau Crater, are a number, say eighty or more, of houses of great age, now unused, mostly in good preservation, which are built in irregular lines, as the ground permits, their doors facing the sea. Each house is oblong-oval, built of layers of irregular flat pieces of stone, the walls about 5½ feet high. The doors are in the side, as in the present grass huts, and of about the same size. The walls are very thick, 5 feet at least, which makes the entrance quite a passage. On entering, the walls are found to be lined with upright slabs, say 4 feet high, but not so broad. Above these, small thin slabs are arranged like tiles, overlapping and so gradually arching till the roof opening is able to be bridged over by long thin slabs of some 5½ or 5 feet, which are not more than 6 inches in thickness and 2 feet in width. The inner dimensions of the 'hall' are about 16 paces long by 5 paces wide and the roof is fully 6½ feet high inside under the centre slabs. The passage leading to it is paved with slabs, under which is a kind of crypt or blind drain which extends to the distance of about 6 feet outside, where also it is covered with flat slabs, and is of the same dimensions as the passage. It is carefully built of stone squared and dressed; it ends abruptly and squarely. In these drains, I was informed the dead men heated were kept till required for the feasts. Outside the hall, and at right angles to it, are smaller chambers, which do not communicate with it, and each of which has a separate door from the outside. We were told that these were generally the women's apartments. The upright slabs which lined the hall, and those of the roof, were painted in red, black, and white, with all manner of devices and figures, some like the geometric figures of the Mexicans, some birds, rapas, faces, symbolic figures of Phallic nature, &c. There was no appearance of pavement in the halls, and in many of them enormous quantities of a univalve—a maritime *Neritina*—which had been used for food."—*Vide* "A Visit to Easter Island in 1868," by J. Linton Palmer, F.R.C.S., Surgeon of H.M.S. "Topaze;" Journal of the Royal Geographical Society, vol. xl., 1870.

† Ellis, Three Visits to Madagascar, p. 312.

been suggested by others which have preceded them, and sometimes the side-chambers appear to be additions subsequently made to an older sepulchre, composed simply of a chamber and entrance-passage. There are many remarkable examples in Brittany of alterations, enlargements, and additions to the first building, whilst others indicate their side-chambers to have been originally planned, and to form important features of the structure; the numerous diagrams, plans, and elevations of typical examples which illustrate Mr. Lukis's paper fully bear out his theory.

The dolmen-mounds of Brittany may be classified according to the characteristics of their internal structure, as follows:—

(I.) The round barrow containing an ordinary kist-vaen, either rectangular or polygonal, covered in with one or more cap-stones. In Brittany this class does not appear to belong to the earliest period; for instance, the round tumulus in the Forest of Carnoet, in Finistère, which was explored in 1843, contained gold, silver, and silver-plated bronze ornaments, as well as flint arrow-points, whilst bronze weapons were found in another simple kist-vaen at Kerlivit, near Douarnenez.

(II.) The round barrow containing the ordinary megalithic dolmen, consisting of a chamber, with a narrow covered way or passage leading to it: this class is common everywhere throughout the province, and Mr. Lukis takes this typical form as a basis from which all the other striking varieties of ground plans observable in Brittany are deducible.

(III.) The same as the above, but containing two or more chambers, in some cases additional and in others original, as at Keriaval and Kludyer.

(IV.) In this class there are also several chambers, with a common gallery of communication; but, although the gallery is megalithic, the chambers are built up, and of the bee-hive vaulted type. A good example of this unusual type is to be found, partially exposed, at the Pte. du Rosmeur, near St. Guenolé.

(V.) Long barrow, containing long and narrow chambers; megalithic, as at Garren Dol and Parc-ar-Dolmen (Finistère).

(VI.) Same type as the above, but with tolmen, or holed entrances. A good specimen of this class has been recently destroyed near Kerlescant; fortunately before its entire destruction it was described by Mr. W. Lukis. There is a similar monument, as yet unexplored, at Kerléarec, north of the Chateau du Lac.

(VII.) Round barrow, containing a megalithic dolmen, consisting of a chamber with the gallery at a sharp angle to it. *Vide* description already given of Kergonfals.

(VIII.) Same type; gallery forming an angle with sculptured stones, as at Le Rocher and Les Pierres Plates.

(IX.) Round barrow, containing two or more megalithic dolmens, either parallel, as Plouharnel and Kerlan, or at right angles to one another, as Les Grottes des Kerozille.

(X.) Long barrow, containing a megalithic dolmen at one extremity, and other smaller kists, megalithic and built up, apparently of a secondary or additional period, *e.g.*, Manné Lud and Le Moustoir.

(XI.) Round barrow, containing long and straight avenue, square chamber, and blocks of noble proportions, highly decorated; notably Gavr' Inis.

(XII.) Immense tumuli, containing comparatively insignificant kists, partially megalithic, partially dry-walled; for instance Mt. St. Michel (Carnac), Manné-er-H'roëk (Locmariaquer), and Tumiac (Arzon).

The last five divisions are all more or less sculptured, whilst cup-markings alone are found on some of the others.

There are also found long barrows without any internal structure: these are not unfrequently accompanied by a menhir, as at Kerlescant; and in the same barrow were found two rows of small-sized blocks of stone, a species of revetment, perhaps marking the limit of the original barrow. Mr. Lukis attributes these barrows to a late period, but this will be alluded to in connection with the neighbouring alignments.

Before approaching the subject of the dolmen-builders, it may be as well to give a short description of analogous structures in the Peninsula South of France, as well as to make some mention of those which are found in such numbers in the North of Africa. The cromlechs in the British and Channel Islands are too well known to need any allusion being made to them.

In the Peninsula the cromlechs, when denuded, are known under the name of *Antas* (a term about which there has been much disputing, but which, after all, seems to signify ancient altars used as landmarks); those partially enveloped in the tumulus, or on the summit of a mound, are termed *Mamunhas* (corruption of Mamua or Mamôa—tumulus); and when covered in, as the *allées* and *grottes* of Brittany, they are termed *Furnas*.

In the year 1734 over three hundred of these remains are mentioned as existing in Portugal, but in 1868 * M. da Costa could only enumerate forty-two, of which twenty-eight are in the Province of Aleutejo, twelve in Beira, two in Traz-os-Montes, two in Minho, whilst none remain either in Estremadura or D'Algarve.

The largest aggregation of these antas appears to be at Contado d'Alcogulo, the property of M. Le Cocq, where there are five remaining together. The only stone implements described by M. da Costa were found here, and consist of half-a-dozen rude greenstone celts and a quartzite muller. With the exception of four, all the above are denuded and ruined antas ; the exceptions are two furnas near Vizella in Minho, the Mamunha de Mamaltar in Beira, and the Mamunha de Carrazedo in Traz-os-Montes. This last is chiefly remarkable from the curious hollowed circular mark, presumedly artificial, on one of its supports.

There is also one curious monument mentioned, as composed of two rows of stones, near a menhir between Cepaes and Fafe, in Minho. As this is the sole description of the monument, and no dimensions mentioned, it is difficult to judge of its composition. It may be analogous to two rows of small vertical stones in the long barrow at Kerlescant, already mentioned, or there may formerly have existed an avenue of stones. Unfortunately it appears that the monument has been destroyed, and the stones made use of in the construction of the neighbouring convent of Pombeiro.

The largest and most perfect furna in the Spanish Peninsula is, however, that of Antéquera, near Malaga. This monument seems to be composed of five fine cap-stones, supported by uprights, ten on either side, of large dimensions.

The Algerian megalithic remains are much more numerous, and form vast assemblages of grave-vaults, but the structures themselves are on a much smaller scale than those of France, England, and Scandinavia.

Messrs. Férand, Bertrand, Veltnez, Bourjot, Letourneux, and Bourguignat, have all written on the subject of the African dolmens, but the name of one writer best known to English readers, not as an archæologist indeed, but as a general, is that of General Faidherbe, who was in command of the northern army of France during the late war ; not very long since he gave a description of the necropoles of Constantine.

There are four principal groups of cromlechs, not to

* Descripçao de Alguns Dolmins ou Antas de Portugal, par F. A. Pereira Da Costa. Lisboa, 1868.

mention smaller ones, in the Province of Constantine, *viz.*, Roknia, Mazela, Bou-Merzoug, and Djebel-Mehmel. These consist of some thousands of megalithic tombs, arranged in straight lines, sometimes forming enclosures. All of these tombs have evidently been covered originally with tumuli, now destroyed by atmospherical agency, and in nearly every case the ancient *enceinte* of the tumulus can be traced.

General Faidherbe, who explored five apparently intact tombs at Mazela, was not successful in finding either pottery or implements. At this place the blocks composing the cromlechs are of a more regular description than the ruder ones of Roknia. At this latter place, however, M. Bourguignat * was more fortunate in his exploration of twenty-eight of these monuments, selected at various points of the necropolis. The larger tombs here contained one or two bodies, whilst the smallest alone contained the remains of three persons. Ornaments of bronze and one of silver-gilt were found in five of the sepulchres and vases of rude pottery, placed generally near the head of the skeleton. M. Bourguignat assigns these tombs to a period at least 1000 years before the Christian era, and he suggests an ingenious and novel method of calculating the ages of these structures by the layers of innumerable snail-shells found within them. At Roknia the monuments are kist-vaens, formed of four upright supports, supporting a cap-stone. At Mazela the single cap-stone, in many cases, is supported by dry-walling, whilst around them circles of stones, generally laid flat, are oftener found than at Roknia.

At Roknia the tombs are situate within a crater, where hot springs, now extinct, once existed. There is evidence that the tombs were built whilst these springs were yet in action. The neglect of the Romans and Carthaginians to utilise these hot springs, whilst they formed bath establishments at neighbouring thermal sources, is pointed out as a proof that these springs were also extinct in the time of the Roman occupation, and that, therefore, the construction of these tombs was also prior to that period.

At Bou-Merzoug the late Mr. Christy found flint flakes and arrow-heads close to the dolmens, and in the Museums of Algiers and Constantine are various stone celts and flint knives, found in connection with megalithic tombs, amongst which may be cited a diorite celt found by Mr. Dutruge close to an enormous monolith, at Kreuchela, near Constantine. Mr. Papier remarks, with regard to the flint

* Histoire des Monuments Megalithiques de Roknia près d'Hammam-Neskoutin, par J. R. Bourguignat. Paris, 1868.

flakes, that they are not native, as no flint is to be found in those localities, and that they must, therefore, have been imported. These implements sufficiently connect the African dolmens with the stone period of that country.

Mr. Palmer, of St. John's College, Cambridge, has recently given a most interesting account of some ancient remains which he discovered in the large tract of desert country known as the Negeb, or South Country, and the Desert of Et Tíh.

These remains have been attributed to the Israelites during the exodus, or to their enemies, the Amalekites, and aboriginal tribes. They are evidently the remains of large encampments; the hill-sides, for instance, at Erweis el Ebeirig, are covered for more than a mile in every direction with curiously arranged stones; the larger enclosures occupied by the more important personages, the hearths or fire-places, &c., are still distinctly to be traced, whilst there are traces of undoubted tombs outside. The Arabs call them *mahattat* (*i. e.* camping grounds), In connection with them are found *nawámís*, or circular stone huts and circles, associated with cairns and kists. The description of these huts reminds us of the assemblages of tin-washers' bee-hive stone huts and circles on Dartmoor, described by Mr. Spence Bate, and appears to belong to the same type of rude dwellings—such as Picts' houses, *brochs*, *borgs*, or *broughs*—which are found in Shetland, Orkney, Sutherlandshire, and Caithness, and described by Sir H. Dryden and Mr. Anderson. The dimensions of these *nawámís* average 7 feet high by 8 feet in diameter, with an oval top. In the centre of each is a cist, and besides that a smaller hole, both roughly lined with stones, covered with slabs of stone over which earth has accumulated. Human bones have been found in these cists, but never perfect skeletons. In the smaller cist the earth shows signs of having undergone the action of fire, and small pieces of charred wood and bone have been found. Flint arrow-heads have also been discovered in them. Mr. Palmer opened some cairns (the size of the largest of which was 20 feet in diameter, and height about 4 feet) and circles, but found nothing but charcoal and burnt earth. He says, "Whatever the people may have been, whether Amalekites or an older race, it seems nearly certain that they buried in cists, piled great cairns on the top, surrounded the whole with a stone circle in the case of more important personages, and offered sacrifices to the deceased in small open enclosures within the ring. These may probably have been the '*offerings to the dead*,' the eating of which was accounted so great a sin to the Israelites."*

Until within the last few years the only dolmens known were confined exclusively to that area of country inhabited by the Celtic race, and hence all megalithic structures were, with good reason, relegated to an origin wholly Celtic. More lately, however, since the discovery of megalithic tombs in other parts of the world, there has arisen considerable doubt as to the race affinities of the dolmen-builders; and certainly the Celts possess no traditions of the sepulchral character of these monuments, which, according to their folk-lore, were the abodes of witches and fairies, and were, according to the Bretons, the handiwork of the Korils and Teuz (elves and fays).

There are many theories as to the original home of these dolmen-building people, who have been variously named as Proto-Scythians or Proto-Celts, and as to the direction from whence they penetrated Western France and our own islands.

There seems but little doubt that their ancient seat was in Central Asia, and that they were, as M. Bertrand affirms, a conservative and exclusive race, who, resisting absorption by a superior people, were expelled from their aboriginal home, from whence they spread westward; and it is an indubitable fact that the most easterly point in Europe where their sepulchres are found is the Crimean peninsula, and that the megalithic tombs here are the most ancient of their kind known.

Thence, according to M. de Bonstetten, one branch of migration spread towards Greece, Syria, Italy, and Corsica; and another, skirting the borders of the great Hercynian forest (*via* Silesia, where at Oppeln and Liegnitz are found the next megalithic remains), took their route towards the shores of the Baltic, where the cromlechs are considered second only in antiquity to those of the Crimea.

Here there is some difference of opinion as to their line of march. According to M. Bertrand they remained for a lengthened period in Denmark, whence, again expelled, they crossed the water, and reached the Shetland and Orkney Isles, whence they can be traced on either side of the Irish Channel to Brittany, and finally re-crossed the Channel to Brittany. On the other hand, M. de Bonstetten is of opinion that from the Baltic the tide of migration overran Germany,—Friesland, Dreuthe, Schleswig-Holstein,

* *Vide* Palestine Exploration Fund Quarterly Statement, New Series, No. 1, January, 1871.

and Jutland,—and, following the coast line, traversed Belgium, the North of France, Normandy, finally reaching Brittany, where the numerous dolmens attest their prolonged stay. Part are then supposed to have crossed over by the Channel Islands, which are rich in dolmen-mounds, to Cornwall and Devon, gradually reaching the S.E. of Ireland and Wales (the absence of such remains in the West of Ireland and in East England is very marked). Another portion left Brittany, and penetrated southwards along the coast as far as the Gironde, whence, leaving the sea-board to avoid the sandy plains of Gascony, they followed the course of the Dordogne, and traversed France in the direction of the Gulf of Lyons. Small detached bands seem also to have penetrated into Savoy and Switzerland, as shown by a few isolated dolmens in those localities. The mountains seem to have delayed the onward progress of these nomades for some time, in the Departments of Arriège, Upper and Lower Pyrenees, but, at length crossing this obstacle, they leave traces in Portugal, through Spain,—*via* Cordova, Granada, and Malaga,—and finally, crossing the Mediterranean, have left their tombs in the northern coasts of Africa, up to the very frontiers of Egypt.

The African dolmens are probably the most recent, and, indeed, there are some who attribute them to the Roman period, and assert that they are due to the presence of a Roman legion raised in Armorica, who brought with them from Brittany their national customs and Celtic mode of burial; and there is a doubtful account of a dolmen in Algeria, inscribed to the memory of an Armorican centurion.

Another hypothesis, supported by Messrs. Désor and Rougemont, is to the effect that the stream of dolmen-building tribes sprang from Africa originally, and were Euskarian rather than Celtic, and poured in a northerly direction over the Iberic peninsula, and thence into France; in fact, the reverse of M. de Bonstetten's track. The evidence in favour of this last theory is only negative, relying on the absence of tradition in Africa pointing to an invasion from the north, and the Celtic tradition in Ireland that the Irish are of African origin.

For our own part, we believe the European dolmen-builders to be of a Scandinavian origin, and for this reason, that the Scandinavians have followed the ancient custom of erecting chambered tumuli to a very late period; and in their literature we find the records of their funeral ceremonies, which accord with what we should expect to find practised by the dolmen-builders. We are indebted to

Professor Stephens's work for the following translation from the "Elder Edda;"—

"The Elder Edda."—*Sigrdrífumál,* verses 33, 34; Ed. P. A. Munch. Translation (quoted from the *Foreword* of Professor Stephens's "Old Northern Runic Monuments of Scandinavia and England.")

Rede ninth rede I thee:—
rescue the lifeless,
a-field where-er thou find them;
whether sank he on sick bed
or Sea-dead lieth,
or was hewn by hungry weapon.

O'er the breathless body
a Barrow raise thou,
hands and head clean washen;
comb'd and dried eke
in his kist fare he,
and bid him softly slumber.

The fine picture of raising the grave mound over the folk-lord, as found in our noblest English epic, "Beowulf." After his awsome *kamp* (battle) with the fire-drake, which he slays, but at the cost of his own life, the dying Wægmunding's last words are:—

My life-day's now over.
Bid my good barons
to build me a *Low*—
fair after fire-heap—
at the flood-dasht headland.
A *Minne* shall it stand there
to my mates and landsmen,
high-looming
on Hroneness.
so that seafarers
sithance shall call it
Biowulf's * Barrow,
as their beak-carv'd galleys
out of hazy distance
float haughtily by.

Further on, after some fragmentary lines describing Beowulf's (life-brand) *burning of his body* (showing that incremation was prevalent), the lay tells us:—

Gan then to make them—
those Gothic heroes—
A Low on the lithe,
lofty and broad,
by the fearless foam-plougher
seen far and wide,
till on the tenth day
towering stood there
the battle-chief's beacon.

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* Beowulf, near end of Fitte 38.

The brand-scorcht floor
a mound covered
mighty and worshipful,
as found most fitting
their famousest sages.
Within the *Barrow*
laid they beighs and ornaments,
and such driven drink-cups
as in the drake-hoard
the furious warriors
a-fore had taken.
The earth be-gem they
with Earl-sprung jewels,
fling gold on the gravel,
where again it shall lie
to all as useless
as erewhile it was.
Round the *How* rode then
those Hilde champions,
all the troop
of those twelve athelings,
their *Keen* raising,
their King mourning,
word lays chaunting
and of (Walhall) speaking.

Mr. Spence Bate attributes the presence of the numerous megalithic sepulchres and monuments of Dartmoor to the Vikings of the North, who came thither in search of tin, as testified by the enormous extent of ancient stream workings on the West Webber, under Warren Tor; and supports his theory as a philologist by an exhaustive analysis of the etymology of the names of the rivers, hills and places, fortified positions, &c., in the neighbourhood, the majority of which he traces to a Scandinavian origin.

Whether the Scandinavian Norsemen derive their blood from an Asiatic stock is a question for anthropologists to discuss.

II. THE ILLUMINATION OF BEACONS AND BUOYS.

MUCH has been done to reduce the chances of shipwreck upon our coast, always so difficult of navigation, and yet very little is generally known of the progress in the application of science to indicate to the mariner his proximity to danger. There are two ways of assisting seamen in their passage on a coast-line—by the lighthouse, of use by day and night; and by the buoy or beacon, of use by day alone. But often, and indeed in the majority of cases, the rock or projecting spit is too small and not sufficiently important to allow of the erection of a lighthouse with the necessary expensive apparatus and shelter for the keepers; some other expedient must then be found, and recourse is had to the mooring of a buoy, or the erection of a light ironwork beacon. It is evident that the value of these substitutes is but small, as they are visible by day only, and that the advantages to be derived from their illumination are all important; for it must be borne in mind that a vessel when in mid ocean is perfectly safe compared with her position when nearing the coast. The dangers, too, increase with the time of being out of sight of land, because a seaman on a long voyage may, by fogs and cloudy weather, be prevented the verification of his course by solar or lunar observations, thus rendering it difficult for him to determine accurately the situation of a dangerous point. The erection of suitable sea marks is, therefore, a matter affecting equally our foreign commerce and our coast service.

The sources of illumination for beacons and buoys, Mr. Thomas Stevenson, C.E., in his recently published work on "Lighthouse Illumination," considers the following:—

"1st. The adoption of apparent or borrowed lights.

"2nd. The use of dipping lights for indicating the position of shoals, by depressing the lamp and apparatus, so as to cover with the light the ground that is dangerous.

"3rd. The conduction either of voltaic, magnetic, or frictional electricity, or that produced by the efflux of steam, through wires, submarine, or where practicable, suspended in the air, so as to produce a spark either with or without vacuum tubes, or by means of an electro-magnet and the deflagration of mercury.

"4th. The conduction of gas from the shore in submarine pipes.

"5th. Self-acting electrical apparatus, produced by the action of sea-water or otherwise at the beacon itself, so as to require no connection with the shore."

This last method was suggested by Mr. T. Stevenson, in the "Transactions of the Society of Arts for 1866;" and it is worthy of note that Mr. A. Bain, in 1848, produced and maintained a steady light off Brighton by the use of sea-water as the exciting fluid of the galvanic arrangement.

The use of apparent or borrowed light is now, from its simplicity, almost generally known. It consists of a certain combination of prisms contained in a lantern erected on the sunken rock, for producing the divergence of parallel rays emitted by a distant lamp placed on the shore.

Thus the light seen by the seaman is merely a reflection of that on shore, the parallel rays of the shore-light being undistinguishable at the distance from which the apparent or reflected light is seen. One of these apparatus is erected at the Bay of Stornoway, a well-known anchorage in the Island of Lewis, the reflecting beacon being erected on Arnish point, a sunken reef on the south side of the entrance. This beacon is distant 530 feet from the real lighthouse, and consists of a truncated cone of cast-iron bearing the reflectors, which are exactly on a level with the window whence the light is projected. The beacon thus directly marking the spot to be avoided is only accessible at the low water of spring tides, and this is, therefore, the only kind of light available. But there are situations in which a beacon cannot, owing to a great depth of water, be erected, and it becomes necessary to ascertain whether a buoy could be employed. From experiments made on different parts of the coast, Mr. T. Stevenson finds the application would be perfectly practicable. The difficulty to be surmounted is the swinging of the buoy at its moorings; this, however, can be compensated by causing the rays from the shore to subtend a sufficient space to always include the reflectors.

The apparent light is such a marked improvement upon the dipping light, which merely illuminates the sea covering the dangerous spot, at a very small additional expense, that there can be no doubt of its substitution.

The third method of illumination, not as yet fully developed is, however, of paramount importance, as providing light for those situations where the apparent light is inapplicable. Faraday's discovery of the magneto-electric spark was first adapted to lighthouse illumination by Professor Holmes, and employed by the Trinity House in 1858, at the South Foreland Lighthouse. In 1862 this apparatus was placed at Dungeness, and here it has continued to give out its light nightly, having been extinguished on only one occasion, and then but for four or five minutes. Professor Holmes's machine consists of a number of powerful magnets, fixed into an octagonal frame. Bobbins or rods of soft iron, around which are coiled helices of silk-covered copper wire, are caused to rotate before the poles of the magnets, the result of this rotation manifesting itself as powerful electric currents in alternate directions. These currents, brought to one direction by means of a commutator, are conveyed from the machinery-room to two carbon points fixed in the lantern of the lighthouse. The carbons being raised to incandescence are gradually consumed; and this consumption is the weak point of the system, necessitating a delicate automatic arrangement to maintain a constant distance between the points. Of the merit of the light evolved, Mr. Stevenson says:—"Through the kindness of the Elder Brethren of the Trinity House, I had an opportunity of inspecting Dungeness Light after nightfall, at different distances. The weather being very favourable, there was an excellent opportunity of viewing it, and the French revolving light of Grisnez on the opposite coast. The result was upon the whole very satisfactory; the Dungeness sixth-order light, though showing continuously all round the horizon, contrasted well with the revolving, and therefore, more concentrated and first-class (oil) light of Grisnez, when viewed from a distance of about twelve miles. The electric light was generally very effective and striking in its appearance, though it frequently fell off, suffering prodigiously in volume; and once or twice it disappeared altogether for a second or two. This temporary extinction, though no doubt an evil, is not, from its very short continuance, a practical defect of any importance." But there is the important fact to be observed that the electric light appears to possess less power to penetrate the atmosphere than does the oil light. "When viewed from Dover, a distance of about eighteen miles," says Mr. W. Stevenson, "the result was decidedly in favour of Cape Grisnez." Still this loss of penetration could be compensated by increasing the original power; while it should be remembered that the order of the lamps differed greatly. Electric lamps for lighthouse purposes, moreover, are generally of too small dimensions, and hence, as the angle of divergence of the rays within the apparatus must be larger, any variation of the carbon-points from the true focus will give rise to a more extensive displacement of the emergent rays than would be the case with lamps of the size used for the oil light. In furtherance of this view, Messrs. Stevenson, in their Report of November 27th, 1865, to the Northern Lighthouses, recommended the adoption of apparatus of the third and fourth orders as being preferable to that of the sixth. This would entail some alteration in the form of the lenses in order to produce a greater amount of vertical divergence.

The penetrating power of the electric light is not, however, a question that need be raised in its application to the illumination of buoys and similar surface sea-marks; for it is generally sufficient that their light should be seen at a few miles' distance. But there is a corresponding difficulty. Neither Holmes's nor Wilde's machine can be employed, because, as the light is produced by the rapid consumption of the carbon-points, there is involved the requirement of lamp machinery too delicate to withstand the buffets of the waves. Mr. Stevenson therefore decided on the employment of vacuum tubes, or the electric spark without carbons. In order to increase the intensity of the light, Professor Swan suggested the use of a Ruhmkorff's induction coil and condenser. From the experiments instituted between the 2nd and 13th of January, 1866, Mr. T. Stevenson deduced some important results, which were communicated to the Royal Scottish Society of Arts. Platinum electrodes were employed, and the primary current was kept passing for a week without any sensible waste of the metallic points. To put these results into practice was the next step, and a submarine cable was procured by the Commissioners of Northern Lights. But the conditions of working a cable were not so well known as now, and the intense secondary current could not be made to take the desired path. Mr. Siemens was then appealed to, and he submitted a very ingenious device by which the *extra current*, as it is called, from the coils of an electro-magnet, situated at the buoy or beacon, in the primary circuit, was utilised in the production of the spark. The electro-magnet was made to work its own contact-lever, and the luminous effect was increased by forming one electrode of a vessel of mercury, renewed by the action of the electro-magnet on a small pump. This arrangement was placed at Granton Harbour, and although the light was vivid and the current easily managed, insuperable difficulties arose from the deposition of mercurial vapours upon the apparatus. Experiments with the induction-coil were, therefore, resumed, and following some improvements suggested by Professor P. G. Tait, were attended with success. The battery and break were retained on shore at Granton pier-head, while the cable extended a distance of half-a-mile to Trinity Pier, where two induction coils with condensers and the optical apparatus were placed. The coils together contained about eight miles of wire, and the spark was induced by sixteen Bunsen elements, two additional cells being used for working the break. The light was very vivid and striking. With the exception, however, of some enquiries made by the Trinity House, which led to the repetition of the experiments in 1869, to the present nothing has been done, although the recent improvements in electrical apparatus would certainly warrant the endeavour to bring this source of illumination into practice. The greatest obstacle to the adaptation is in the case of floating buoys, where the cable would be subject to abrasion against the sea bottom, in situations where the light would be most useful, always of the worst kind. Yet the wear could be brought, as we know from experience with telegraph cables, within such limits as would be practically reparable. Recent experiments with Wilde's magneto-electric machine without carbons have been made with much better results. From various trials Mr. Stevenson has also found that the most brilliant light is obtained when bismuth points are employed.

The remaining source of illumination is that by gas. In 1853, Admiral Sheringham illuminated a buoy in this manner off Portsmouth. The gas was conveyed as far as possible in iron pipes, the conduction being continued through a gutta-percha tube to the beacon. The gas was lighted by means of a platinum wire rendered incandescent by the passage of a voltaic current from a small battery on shore. The results, however, were not published till quite lately.

A beacon on the Clyde, near Port Glasgow, was, in 1861, lighted with gas, and has since been maintained. This beacon is 300 feet from shore, and the supply of gas is regulated in a very ingenious manner. A float is so arranged in the principal burner that a certain pressure is requisite to admit of the passage of the gas, consequently, when the gas is shut off from the main each morning, the light would be completely extinguished were it not for a small burner that, being fed from a gas-holder of about ten cubic feet capacity, situate in the body of the buoy, remains alight all day. Again, each night as the pressure increases, the float closing the supply-tube to the principal burner rises, and the beacon is fully illuminated. The objection to this system, however, is the liability of the flame to extinction—a risk from which the electric spark is free. The accumulation of water in the pipes is another serious obstacle; while in the case of electricity the risks of faulty insulation would be compensated by the use of duplicate wires.

These numerous experiments, however, go far to prove that the illumination of buoys and beacons from the shore is perfectly feasible, and the time is not perhaps very distant, when, following the suggestion of Messrs. Stevenson, the entrance to the Port of Liverpool, for example, will be lit up from either shore by apparatus easily managed by one or two men. The illumination of Arnish Point by the apparent light for eighteen years, and the conveyance of gas under water at the Clyde for nine years, in both cases with very slight repairs, are facts demanding a more general extension of these principles. It is no longer a question of expense, as in the erection of the Eddystone and Bell Rock Lighthouses, but an easy expedient calling for speedy recognition. While there are many situations where it would be impossible at any expense to construct a lighthouse, these generally admit of the employment of one of the apparatus described, and which would be equally efficacious. The good done by the erection of our lighthouses in the saving of life and property is almost incalculable, and is fully an encouragement to proceed with the illumination of the smaller sea-marks.

III. NATURAL AND ARTIFICIAL FLIGHT.

ARTIFICIAL flight is by no means an idea of mediæval or of modern times. Setting aside its consideration as a poetical and legendary attribute, there are tolerably authentic accounts, if not of the actual flight of man, of the imitation of the movements of birds in well-constructed automata. Archytas had a wooden dove capable of flight, and Regiomontanus made a wooden eagle. These, however, are merely historical records, and there are not many definite plans left to us until 1683, when Wilkins, Bishop of Chester, published his plans of an aërial chariot. From that time to the present hardly a year has passed without the appearance of some proposal, more or less visionary, to solve the problem of aërial navigation. But these proposals have only resulted in ignominious failure, sometimes fatal to the experimenter; and this is hardly a matter of wonder when we consider that, for long, little or nothing was known of the laws of gravitation and of the medium to be controlled. The methodical study of the laws of the natural flight of birds and insects has been neglected even up to the present time. It is, then, hardly just to condemn the student of aëronautics as one needing friendly care, until a complete series of experiments, conducted according to the light of present science, shall have shown the futility of the idea of artificial flight. It was an intention of research that called the Aëronautical Society into life, five years ago, and although the Society has not, as a body, undertaken experimental investigation, individually there has been much done to prevent wasteful work, to point out essential principles, and the causes of failure hitherto. But the thanks of the Society are mainly due to MM. Marey and De Lucy, who, by their able investigations of the laws of natural flight, have done much to elucidate the subject.

Aërostation may be considered under two heads. 1. Ballooning, in which ascent is gained by means of a gas specifically lighter than air. 2. True flight, in which the acts of rising and suspension are due to expended force. There are two obvious reasons why balloons have not been successfully navigated. It is difficult to apply a directive force at the point of suspension of the balloon, while any force applied to the car merely serves to tilt the balloon. Again, a body to be propelled against a current of air, even that created by its own motion, must have a weight in proportion to its surface. This law will become apparent in endeavouring to throw a block of wood and a cube of paper to the same distance.

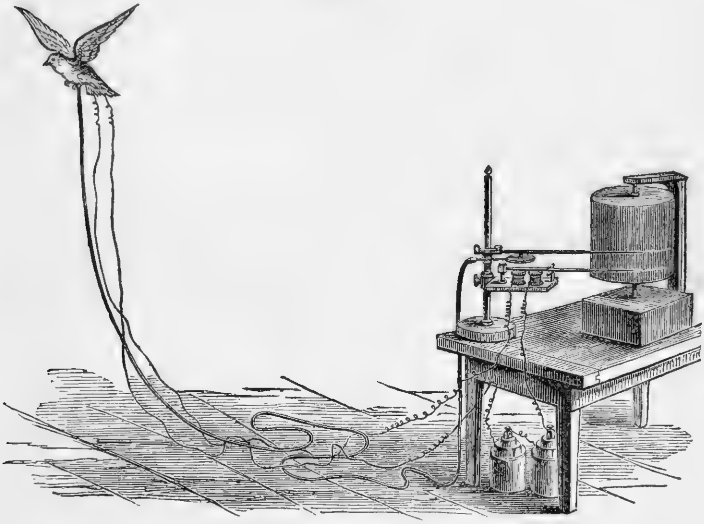
It was for long generally supposed that birds were suspended or balanced by a certain volume of rarefied air confined in the lungs, bones, and feathers. But this explanation will not bear the least reflection, for (the density of the air being 781 times less than that of water) a bird weighing a kilogramme, together with its wings, cubes about 4 decimetres, and can therefore only displace 4 decimetres of air, the weight of which is 5 grammes 20 centigrammes. Hence it follows that a raven weighing 1 kilogramme, to be supported in the air, should have a volume of at least a cubic metre.

In considering the phenomena of flight it is necessary, then, to observe the weight, or gravitating force, the surface or resistance of the air, and the force of projection. The point immediately presenting itself, under this consideration, is the determination of the relation of the extent of wing-surface to the weight of the bird. This question gave rise to much controversy amongst naturalists, until M. de Lucy undertook its settlement by a series of decisive measurements. He sought to establish a common unit between birds and insects in this matter, and, although here not perfectly successful, the experiments clearly proved that birds of large size and great weight are sustained by a much smaller proportional wing-surface than those of smaller size. If the wing is considered as the means of elevation, that is, as an instrument with which to strike the air, and thus derive a power of ascension, there are geometrical reasons why the greater weight of the larger bird should not have a proportional wing-surface. Consider two objects similar in shape,—for instance, two cubes,—one having twice the diameter of the other, then each of the faces of the larger cube is four times the size of each face of the smaller cube, while the larger cube has a weight eight times that of the smaller. This is but a familiar illustration of the fact that all geometrical solids with linear dimensions, bearing a stated relation, have their surfaces proportioned as the square, and their weight as the cube, of their similar linear dimensions. So that two birds of the same form, but one having a width of wing from tip to tip twice that of the other, will have their wing-surfaces proportioned as 1 : 4, and their weight as 1 : 8. Applying these principles, Dr. Hureau de Villeneuve has endeavoured to determine the extent of wing that would enable a bat of the same weight as a man to fly, and he has found that each of its wings would be less than three metres, or a little more than three yards, in length.

The most important points in the consideration of the ultimate capability of man's flight still remain. They are— can man exert sufficient force? and can that force be mechanically employed to raise himself from the ground? The second of these questions is yet to be answered; the first has been solved by the experiments of M. Marey, of the College of France. It is evidently only necessary to consider the power of a bird to raise its own weight, because, when once in the air, by the simple extension of its wings, the bird is converted into a natural parachute, counteracting the direct action of gravity, so that it does not acquire even the descending velocity of nearly 17 feet in the first second, but traverses a much less space,—in fact, a space that does not increase as the square of the time. Therefore it follows that a bird has not, as calculated by Borelli and Navier, to employ force to counteract gravitation, because it finds a counter-balance in the resistance of the air. The force, then, to be expended is only that necessary to prevent the fall for a fraction of a second. Now, M. de Lucy asks, what is the space traversed in one-tenth of a second by a body left to itself? This space is 19·29 inches; but as a bird is supplied with a large surface of suspension the space may be estimated as much less, or at 25 centimetres, in English measure 9·84 inches. The bird, then, has to expend a force sufficient to raise itself, say a weight of 1 kilogramme, or about $2\frac{1}{4}$ lbs., 9 or 10 inches at each flapping of the wing. Now, supposing ten strokes of the wing to be made per second, the total force expended per second amounts to 2 or 3 kilogram-metres. But as between each stroke of the wing there lapses one-tenth of a second, during which the bird does not fall, but elevates itself under the impulse that is given to it, five strokes of the wing are sufficient, and the force required will be only 1 or 1½ kilogrammetres. Now these hypothetical calculations are very closely related in result to the direct anatomical investigations of M. Marey, who found, by exposing the great pectoral muscle, and exciting it electrically to cause motion, that the force of contraction supported a weight of 2 kilogrammes 380 grammes. Admitting that the electrical agent cannot produce so powerful a muscular contraction as that called forth by the will, and doubling or quadrupling the results obtained, there still does not result the force that Koster attributes to the muscle of man. Hypothetically, a bird capable of sustaining itself must expend a force proportional to its weight, and consequently ought to possess a proportional weight of muscle. This has been found experimentally true, and it appears, from the researches of Hastings, published in the "Archives Neërlandaises" for 1869, that the weight of the pectoral muscle is about one-sixth of the total weight of the bird. It would, therefore, be a natural conclusion that we should admit the wings of birds to possess the same velocity, and from this point of view the duration of the stroke will increase with the linear dimensions of the bird,—that is, large birds will make fewer strokes than small ones. M. Marey has invented a novel and most ingenious apparatus for the measurement of the frequency of the strokes of the wings of a bird, and the relative duration of the periods of elevation and depression. By his method the bird is made to record the movements of its wings, by the swelling of its pectoral muscle as it contracts to draw the wings downward. The bird flies in an inclosure 15 metres square and 8 metres high. The registering apparatus (Fig. 1) consists of a revolving blackened cylinder, upon which a style traces a crenulated line corresponding to the motions of the wings. A double connection is established between the bird and the registering apparatus; one by electricity, conveyed through two fine wires to contact points fastened to the wing of the bird; the other by flexible India-rubber tubing, about 12 metres in length. This apparatus is applied to a pigeon by means of a corset. Under this corset, between it and the pectoral muscle, is placed a small, shallow metal basin, containing a spiral spring, and covered by a thin sheet of rubber (Fig. 2).

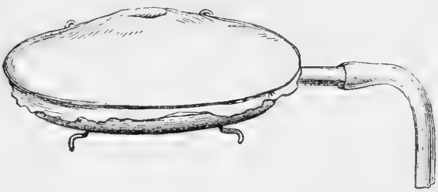
Any pressure applied to the rubber forces the air out of the basin into the tube, and if the pressure ceases the air re-enters the basin, in consequence of the elasticity of the spring. The registering apparatus comprises a similar basin, to the rubber of which is attached a lever carrying a

FIG. 1.



tracing-style. The motion imparted to the first basin is transmitted, by the air confined in the India-rubber tube, to the membrane of the receiving apparatus. In employing India-rubber tubing it is necessary to prevent the elongation of the tube by its own weight as the bird rises in the air, as

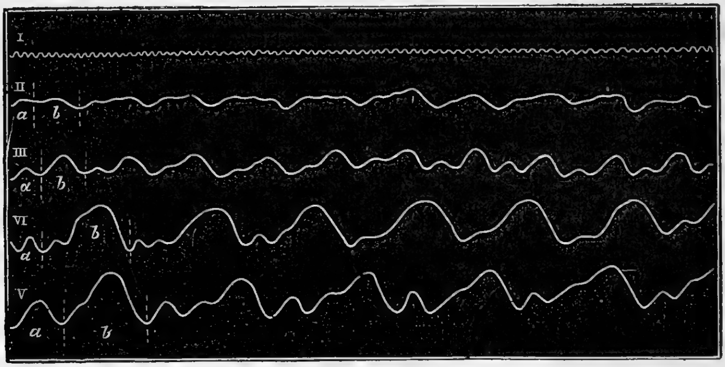
FIG. 2.



the elongation would cause a rarefaction of the air in the interior of the tube, and consequently interfere with the signals recorded. The bird is thrown off at one end of the enclosure,, the dovecote in which it is ordinarily kept being placed at the opposite end. The bird, in endeavouring to reach its cote, causes the traces on the blackened cylinder shown in Fig. 3.

The tracings vary with the kind of bird placed in the corset; but in all may be seen the alternation of two mo-

FIG. 3.

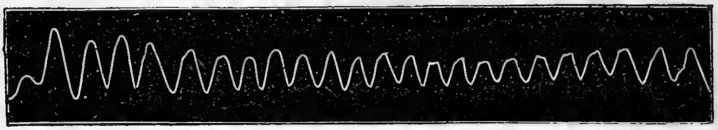


Tracings of the Expansion of the Pectoral Muscle of Various Birds during Flight.

I. Tracings of a Tuning-Fork making 200 vibrations a second. II. From the muscle of a Pigeon. III. From a Wild Duck. IV. From a Hen Hawk. V. From a Harrier.

tions, *a* and *b*, produced during each vibration of the wing, that of *a* corresponding to the action of the muscle elevating the wing, and that of *b* to the muscle depressing the wing, Anatomically, *a* shows the swelling of the median pectoral, and *b* that of the great pectoral. Nothing more should be expected from these tracings than they naturally furnish, that is to say, the number of vibrations of the wing, the

FIG. 4.



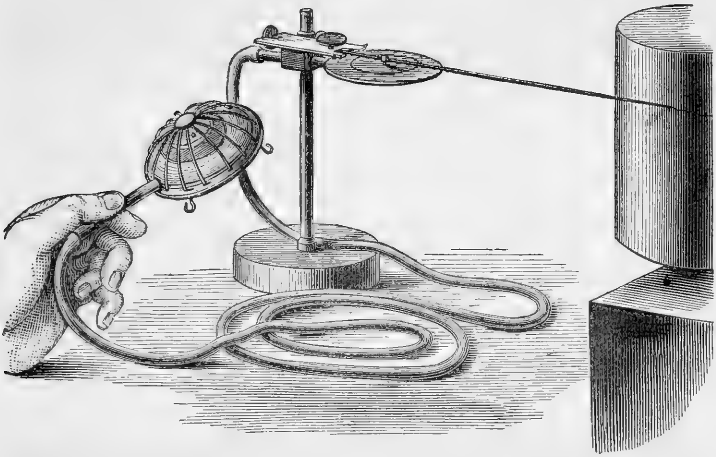
greater or less regularity of its movements, the equality, inequality, and energy of each of them. Confining the investigation to these limits, we obtain the following results :—

	Number of Wing Vibrations per Second.
Sparrow	13·00
Wild duck	9·00
Pigeon	8·00
Buzzard (*Buteo vulgaris*) . .	5·75
Screech owl	5·00
Harrier (*Circus rufus*) . . .	3·00

At the commencement of the flight the strokes of the wings are fewer, but more energetic; in mid-flight they attain a regular rhythm, becoming again irregular at the moment of the descent of the bird. Fig. 4 shows the difference in amplitude and frequency of the wing-strokes of a pigeon during a flight of 15 metres. The extended tracings to the left indicate the movements at the commencement of the flight. A glance at the preceding table will be sufficient to show that Nature has here established a compensation for the diminutiveness of the organs of the smaller birds by rapidity of movement.

To complete the series of investigations M. Marey studied the phenomena of natural flight, not analytical as hitherto,

Fig. 5.

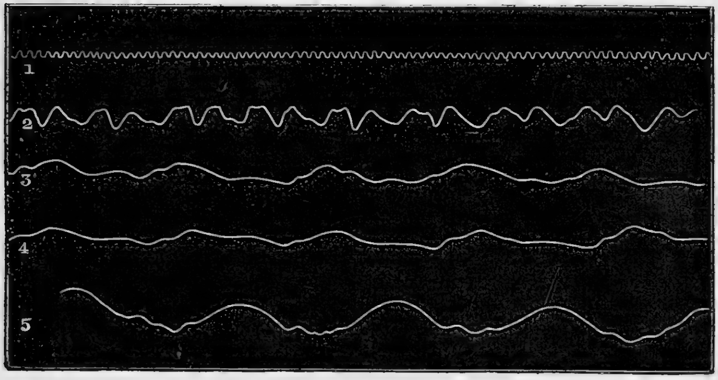


but as a whole. During flight two distinct effects are produced,—the bird is upheld against the force of gravity, and it is propelled horizontally. And now arise the questions:—Is the bird sustained at a constant elevation, or does it oscillate in a vertical plane? Is there a series of altations and depressions, the amplitude and occurrence of which cannot be observed by the eye? Is there any variation of velocity in the horizontal course? Does the action of the wings impart a jerking motion? These questions are of the greatest import, if we observe the flight of birds with the view of applying the laws discovered to the subject of the artificial flight of man; and although some of them might have been answered by deductions from the result of preceding experiments, there can be no doubt of the correctness of

M. Marey's decision to still further investigate, and to depend on experimental results alone. The laws that might be deduced involve in their deduction such complicated calculations that it would be easy for an error to creep in. Besides, possessing the means by which distant motions can be registered, or rather caused to register themselves automatically throughout their whole amplitude, the experiment becomes a matter of no great difficulty. The problem to be solved is that of so arranging the membrane of the apparatus shown in Fig. 1 as to record the bird's rise and fall in a vertical plane,—that is to say, the rise and fall of the bird should produce a varying pressure on the membrane of the metallic drum. Supposing that a flying bird carries on its back a drum of the form described, with the membrane uppermost,—if this membrane follows the motion of the bird there will be no displacement of the air in the apparatus, and the style will remain motionless. But if the membrane is prevented from following the motions of the bird, if there can be given to it a tendency to remain at rest, while the body of the drum attached to the bird is moved, the air in the apparatus will be compressed or rarefied, and motion imparted to the registering style. This tendency can clearly be produced by loading the membrane with an inert body, such as a disc of lead. Fig. 5 illustrates this; the drum is seen attached to the registering apparatus, with the inert mass, the disc of lead, upon the membrane. Practically there are several discs, which may be added or removed as the exigencies of the experiment require. The movements in the horizontal direction are not registered, because elevation and depression can alone affect the inertia of the leaden discs, for when the bird ascends the inertia of the mass resists the upward movement, and causes a record similar to that which would have taken place had the membrane itself been subjected to pressure, and the drum had remained motionless. To protect the apparatus from accidental pressure by the wings of the bird, a wire cage was employed. There is, however, still the objection that an inert mass placed on an elastic membrane may execute vibrations peculiar to itself, and that the apparatus will record these vibrations, as well as the oscillations of the bird. How can this complication be removed? The law of vibrations teaches that the greater the mass, and the feebler the elasticity, the longer will be the period of vibration. Then as the motions we are studying are tolerably frequent, if we arrange matters so that the vibrations of the disc shall be of a longer period than the oscillations of the bird, we get rid of the difficulty.

Experiments with several birds—ducks, harriers, hen-hawks, and owls—have shown that, in relation to the intensity of the oscillations in the vertical plane, very varied types exist. Fig. 6 shows tracings furnished by these birds. The upper crenulated line is produced by a tuning-fork vibrating 100 times per second. Thus the tracings may be estimated absolutely, or in relation to each other. A still clearer view will be obtained of the actions, during the bird's flight, if the oscillations of the bird vertically and the movements of its muscles are recorded at the same time. The tracings represented by Fig. 7 will then be obtained. The duck presents two energetic oscillations at each revolution

FIG 6.



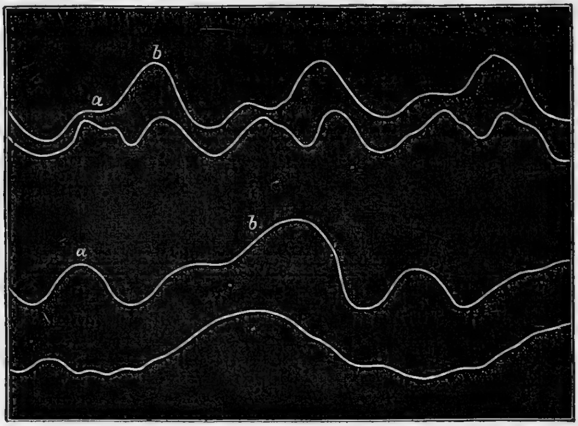
Tracings.

I. From the Tuning-Fork. II. Wild Duck. III. Buzzard. IV. Screech Owl. V. Harrier.

of its wing; the one at *b*, at the moment when the wing relaxes, is easily understood; the other, at *a*, at the moment when the wing rises. To explain the ascension of the bird during the time of the elevation of the wing, it seems to me, says M. Marey, indispensable to call in the action of a boy's kite. The bird, moving forward with acquired velocity, presents its wings to the air in an inclined position, similar to that of the kite, and thus transforms its horizontal force into an ascending one.

The preceding experiment furnishes a very precious lesson in the theory of flight. In fact, if the bird executes a series of ascents and descents, the duration of the descending period will approximately inform us of the amount of positive work which the bird must perform to rise again to the height from which it fell; and we see that the duck, which makes nine vibrations of the wing per second, executes two vertical oscillations during each vibration, or eighteen in a second. Each oscillation is composed of a rise and a fall, so that each descent of the bird cannot last more than 1-36th of a second. Now, if we subtract the effect produced (as in a parachute) by the outspread wings of the bird, we find that a body which falls during 1-36th of a second traverses only 52 millimetres. This fall, repeated eighteen times a second, constitutes a total rise of 9·36 centimetres, necessary to maintain the bird in the same horizontal plane during one second. In the tracings of the harrier the descents are less than in that of the wild duck, probably on account of the large surface of the wings of this bird.

Fig. 7.



The two upper lines are the muscular tracing and the tracing of the oscillations of the duck. The lower lines are tracings from a harrier. Underneath the undulation *a*, indicating the elevation of the wing, is seen a vertical oscillation; and again beneath *b*, indicating the lowering of the wing. The oscillation at *a* in the lower tracings of the harrier, corresponding to the elevation of the wing, is less marked than in the duck.

The remaining question to be solved relates to the determination of the phases of rapidity of flight. The solution can be found by placing the weighted drum vertically upon the bird's back. The tracings furnished are, however, so analogous to the oscillations in the vertical plane, that they show very little more than a proof that during the descent of the wing the speed of the bird is accelerated.

At last, then, we are in possession of the principal facts upon which the study of the mechanical power developed by the bird during flight can be established, and we see that it is during the descent of the wing that the entire motive force which sustains and directs the bird in space is developed. We have also seen that man possesses the requisite strength to raise himself, could the mechanical means be devised. What has been effected by the mechanical means already devised? Almost nothing, for so little has been done upon principle. The "Chalon" machine, drawings of which were exhibited some time since at the Aëronautical Society's meetings, appeared as one of the best perfected schemes,—consisting in elevation primarily by means of gas, wings then giving control to the machine. But even in this case the relative proportion of surface to weight was overlooked; it would be almost impossible to direct such a machine against the wind. It follows, evidently, that if gas is to be employed as a means of elevation, when elevation is attained some other agency must take the place of gas. Perhaps the machine attended with the greatest practical success has been that of Mr. Charles Sinclair, who managed to raise himself some 15 feet in the air without the assistance of a specifically lighter material. His plan consisted in fastening to the body of the aëronaut a series of parallel aëro-planes, somewhat similar to a set of shelves made of light frame-work, covered with canvas, and arranged at about 2 or 3 inches from each other. Running against the wind with these quasi-wings attached to his body, Mr. Sinclair, in his first experiment, found himself elevated a few feet, when one of the planes shifted, and he was violently hurled to the ground. The machine mended, with several improvements in its construction, he again essayed to attain some slight elevation, and, with a preliminary run of 100 feet, rose steadily in the air to a height of 15 feet. This experiment would seem to point to some modification of a boy's kite as a means of elevation. Anyone who has seen a Canadian ice-boat has observed how, at the slightest check, such as that afforded by a small block of ice, the vessel is raised by the force of the wind upon the sails, and carried over the impediment. Similarly the boy runs with his kite to raise it; but we must seek some other means of imparting the required momentum, probably by the inclined plane for that afforded by running. Until some motor is found capable of working in a small compass, and with a moderate weight of fuel, the idea of flying by the aid of extraneous machinery must be given up, and man must trust to his own strength. But, despite failures, there can only be offered to aëronauts an argument similar to that offered to Watts and Stephenson,—"Man never has flown,"—but we must stop short of saying—"and he never will."

IV. THE COAL COMMISSIONER'S REPORT.

THE question which gave rise to the appointment of the Royal Commission, whose Report* we are now about to review—namely, the probable duration of the coal resources of the United Kingdom—was, according to Mr. Jevons's account,† first raised by one John Williams, a mineral surveyor, who, in a work published by him in 1789, entitled "Natural History of the Mineral Kingdom," gave a chapter to the consideration of "The Limited Quantity of Coal of Britain." At that time the coal production of the United Kingdom amounted only to about 7,500,000 tons annually. This subject was also referred to by Sir John Sinclair, in his "Statistical Account of Scotland," and again in 1812, by Robert Bald, "in his "General View of the Coal Trade of Scotland." Later still, Dr. Buckland prominently brought it before the public, both in his evidence before the Parliamentary Committees of 1830 and 1835, in his "Bridgwater Treatise," and again in his address to the Geological Society, on the 19th of February, 1841. Up to this time, however, the public generally did not express any great interest in the matter; and it was not until the publication of Mr. Hull's work‡ on the subject, in 1861, that its real importance began to be better appreciated. This was shortly afterwards followed by Sir William Armstrong's celebrated address as President of the British Association, at Newcastle, on the 26th of August, 1863, in which he stated that "the entire quantity of available coal existing in these islands has been calculated to amount to about 80,000 millions of tons, which, at the present rate of consumption, would be exhausted in 930 years; but, with a continued yearly increase of $2\frac{3}{4}$ millions of tons, would only last 212 years." This announcement, by which the real importance of the question was put before the public in a practical shape, caused at the time considerable excitement throughout the country; and it soon became clear that some confirmation or refutation of the statistics first published by Mr. Hull in his work above referred to, and subsequently promulgated by Sir William

* Report of the Commission to Inquire into the several Matters relative to Coal in the United Kingdom, July 27, 1871.

† The Coal Question: an Inquiry concerning the Progress of the Nation, and the Probable Exhaustion of our Coal Mines. By W. STANLEY JEVONS, M.A., &c. 1866.

‡ The Coal Fields of Great Britain. By EDWARD HULL, B.A. 1861.

Armstrong, was desirable in the public interest. On the 12th June, 1866, the subject was brought before the House of Commons, by Mr. Hussey Vivian, in a very able speech, in which he moved for the appointment of a Royal Commission to inquire into several questions connected with the coal resources of the United Kingdom. This motion was agreed to, and on the 28th idem, the Commission was accordingly appointed, consisting of fifteen members,* with the Duke of Argyll as chairman.

The special instructions to the Royal Commission were as follows:—

"To investigate the probable quantity of coal contained in the coal-fields of the United Kingdom, and to report on the quantity of such coal which may be reasonably expected to be available for use.

"Whether it is probable that coal exists at workable depths under the permian, new red sandstone, and other superincumbent strata.

"To inquire as to the quantity of coal at present consumed in the various branches of manufacture, for steam navigation, and for domestic purposes, as well as the quantity exported, and how far and to what extent such consumption and export may be expected to increase.

"And whether there is reason to believe that coal is wasted by bad working, or by carelessness, or neglect of proper appliances for its economical consumption."

The first act of the Commissioners was to form amongst themselves five committees for the following purposes:—

1. A. Committee on possible depths of working.
2. B. Committee on waste in combustion.
3. C. Committee on waste in working.
4. D. Committee on the probability of finding coal under permian, new red sandstone, and other superincumbent strata.
5. E. Committee on mineral statistics.

Without confining ourselves in any way to the order in which these Committees subdivided the several questions submitted for their investigation, we now proceed to give briefly the results arrived at by them, and which have been embodied in their respective reports, or brought out in the evidence taken by them.

* Sir R. J. Murchison, Sir W. G. Armstrong, Messrs. H. H. Vivian, G. T. Clark, J. Dickinson, G. Elliott, T. E. Forster, J. Geddes, R. Hunt, J. B. Jukes, J. Hartley, J. Percy, J. Prestwich, A. C. Ramsay, and J. T. Woodhouse.

There is but little reason for doubting that British coal was used in small quantities in the days of the Roman occupation of these islands, as it has been found amidst the remains of Roman civilisation in the city of Uriconium and elsewhere. Statements respecting the use of coal before the twelfth century are, however, exceedingly fragmentary; since that period there is tolerably ample information to be obtained respecting the coal trade from the rivers Tyne and Wear, but very little relative to the production at this period of coal in other parts of the kingdom. The first record dates back to the year 1180, when Bishop Pudsey, of Durham, granted some land to a collier, to provide coals for a smith at Coundon, in the county of Durham; similar grants being also made at Sedgefield and Bishop Wearmouth. In 1213, a charter was granted by King John to the men of Newcastle to dig coal. The earliest record of coal being used in the South of England is in 1279, when coal was purchased at Dover for the use of the castle. In the year 1300, coal was used in quantity by the brewers and smiths of London; which was, however, prohibited in 1306, on the ground of its being an intolerable nuisance, but fifteen years afterwards it was used in the royal palace. The first Government tax was laid on coal as early as 1379, amounting to two-pence for every chalder;* but in the reign of Elizabeth it was raised to twelve-pence per chalder, which was regularly enforced to the time of Charles II. Duties were laid on sea-borne coal to assist in building St. Paul's church and fifty parish churches after the great fire of London; and in 1677, Charles II. granted to his natural son, Charles Lennox, Duke of Richmond, and his heirs, a duty of one shilling a chaldron on coals, which continued in the family until it was purchased by Government in 1799, for the sum of £400,000. This impost was known as the "Richmond shilling," and produced soon after it came into the hands of the Government £25,000 a year.

The practice of coal mining had arrived at such a degree of importance at the beginning of the eighteenth century, that we find treatises published as guides to the system of exploration. Many of the collieries were then extensive, and accidents were not unfrequent. An account is given by one author† of a "blast" which occurred in October,

* This was probably the Newcastle chalder, containing 53 cwts. of coal.

† The Compleat Collier: or the whole Art of Mining and Working Coal Mines, &c., as is now used in the Northern Parts, especially about Sunderland and Newcastle. 1708.

1705, when "there was above thirty persons, young and old, slain by a blast, perhaps in less than a minute's time."

In 1771 there was formed a combination among the coal owners who shipped their coal by the three rivers, the Tyne, the Wear, and the Tees, to raise the price of coals to consumers by restricting the quantity supplied. This combination, known as the "limitation of the vend," lasted, with but a few temporary interruptions, until 1845. As this restriction did not apply to coal shipped to foreign parts, it frequently happened that coal was sold to foreign markets at 40 per cent under the prices in the London market. About 1791 a feeling grew up in favour of obtaining coal from the midland and other coal-producing counties for the London market.

Coal was first worked in Cumberland, at Whitehaven, by Sir John Lowther in 1660, but there does not appear to exist any record of the quantity raised until towards the end of the 18th century. Little appears to be now known of the early history of the Lancashire coal-fields. In the Cheshire coal-field, coal is said to have been first opened in Nerse township in 1750. The early history of coal mining in Yorkshire is very obscure; but it is stated in an early number of the "Leeds Intelligencer," a local newspaper, that in 1752 a petition was prepared for Parliament for rendering the Calder navigable, the chief object being the conveyance of coal. The Derbyshire coal-fields are referred to by Pilkington,* who wrote in 1789; and Cambden, in his "Natural History of Warwickshire, A.D. 1730," was perhaps the first to record any facts relative to what is known as the Staffordshire coal district. Shropshire from a very early period appears to have yielded from its stores fuel for the use of man. Shropshire coal has been found in the ruins of Uriconium. The earliest record of any colliery workings in this county however, appear in the "Leeds Intelligencer" for November 23rd, 1756. The first notice of the production of coal in the Forest of Dean appears to be supplied by the records of the Justice Seat held at Gloucester in 1282, where it is stated that sea coal was claimed by six of the ten bailiffs of the Forest of Dean. Coal was sent to London from South Wales as early as 1745, and in North Wales the Mostyn Collieries date from the 23rd year of the reign of Edward I. Amongst the earliest reliable accounts of the Scotch coal-field, we find that in the twelfth century,

* A View of the Present State of Derbyshire, with an Acconnt of its most Remarkable Antiquities, &c. By JAMES PILKINGTON. Derby, 1789.

William de Vetereponte granted to the monks of Holyrood, "totam decimam de carbonaris meo de Carriden." This is the name of a small brook flowing into the Firth of Forth, about three miles north of the ancient palace of Linlithgow. This was in the time of William the Lion, during whose reign the monks of Newbattle worked coal on the margin of the Esk. Arthur Dobbs, author of an "Essay on the Trade of Ireland," writing in 1728, says, "We have of late discovered coal mines in the counties of Cork and Leitrim."

Having thus, in the briefest possible manner, touched upon the early history of the coal-fields of the United Kingdom, we pass on now to notice the amount of coal that has been raised from time to time. The estimated production of the whole kingdom is thus given in the Commissioner's Report:—

"In the three centuries before 1800, it is computed that not less than 850 millions of tons of coals were raised from the coal-fields of the kingdom. During the next fifty years there was a constant and steady increase in the production, and fully two thousand millions of tons of coal were extracted. Up to this time the records of coal produce were most imperfect, and it was not until the year 1854 that reliable returns have been in existence. The average production in 1851, 1852, and 1853, may be taken at 50,875,000 tons per annum. In the years 1854 to 1869, both inclusive, 1,343,793,705 tons were raised, which added to the figures above given, and estimating the return for 1870 at 110,000,000 tons, show that we have already drawn from our original stores of fuel not less than 4,456,000,000 tons* of fuel."

Space will not admit of our entering into any minute detail regarding the consumption of coal, but it may be stated that, of the 107½ millions of tons raised in 1869, 32,446,506 tons were employed in iron manufacture; 25,327,213 tons in manufactures; 3,277,562 for steam navigation, 2,027,500 for railways, locomotives, &c.; 18,481,572 in domestic consumption; and 9,775,470 tons were exported.

Large portions of some of our coal-fields lie at a greater depth than has yet been reached in mining, and it is considered that the increase in temperature which accompanies increase of depth is the only cause which it is necessary to consider as limiting the depths at which it may be practicable to work coal. In this country the temperature of the earth is constant at a depth of about 50 feet, and at that

* The Report says 4300 millions of tons, but in this figure the returns for the three years 1851 to 1853 appear to have been omitted.

depth the temperature is 50° F. The rate of increase of the temperature of the strata in the coal districts of England is in general about 1° of Fahrenheit for every 60 feet of depth. The depth at which the temperature of the earth would amount to blood heat, or 98°, is about 3000 feet. Under the long wall system of working, a difference of about 7° appears to exist between the temperature of the air and that of the strata at the working faces, and this difference represents a further depth of 420 feet; so that the depth at which the temperature of the air would, under present conditions, become equal to the heat of the blood, would be about 3420 feet. Beyond this point the considerations affecting increase of depth and temperature become so speculative as to render it necessary to leave the question in uncertainty; but, looking to possible expedients which the future may elicit for reducing the temperature, it is considered that it may fairly be assumed that a depth of at least 4000 feet might be reached.

Before considering the supplies of coal which still remain for consumption, within a workable depth, it is necessary to refer briefly to the waste that now occurs in working, and through imperfect combustion.

The theoretical value of 1 lb. of coal is 14,000 units of heat, which, if properly applied, should be equal to the power of lifting 10,800,000 lbs. 1 foot high, whilst the highest practical result which has been realised is 1,200,000 lbs., or less than one-eighth of the theoretic value, and this without counting the impurities of ordinary coal, which cannot be taken at less than 10 per cent. *Theoretically*, 1 lb. of pure coal should evaporate about 13 lbs. of water; *practically*, 1 lb. of ordinary coal does not evaporate 4 lbs. The best results are stated to have been obtained in the boilers of Cornish engines, or in boilers constructed upon the model of the "Cornish boiler." The "duty" of the best Cornish engines since 1814 shows that, up to a certain point, there was a gradual increase in the number of pounds lifted 1 foot high by the combustion of 1 bushel (94 lbs.) in the earlier tables, and of 1 cwt. (112 lbs.) in the latter ones; and that, after the maximum had been obtained, there was a steady decline in the effective power obtained; but the highest recorded duty is about 98,300,000, in 1857. Upon the general question of coal consumption, the conclusion arrived at by the Commission is, that "for some time past, in our manufactures, there have been constant and persevering efforts to economise coal, by the application of improved appliances," and there is reason to believe that, "in some branches of manufacture, the limits of a beneficial economy appear to have been nearly reached, and that in other cases a gradual effort would continue to be made for saving fuel." It may be assumed, therefore, that the progress of economy in using coal is not likely to operate in future with greater effort in keeping down the increase of consumption than it has hitherto done. The present consumption of coal for domestic use is generally estimated at 1 ton per head of the whole population, and the future increase under this head may be expected to coincide with the increase of population; whilst, as regards the future exportation of coal, there is reason to doubt whether much further increase will take place in this direction, owing to the steady development of the coal-fields in other countries.

With regard to the available supplies of fuel yet remaining, a not unimportant consideration is the amount of waste incident to mining coal. It is clear, from the evidence adduced on this subject, that, although in many instances waste in working is reduced to a minimum, and although manifest improvement is being made in the working of coal, especially by the extension of the system of "long wall," nevertheless coal is wasted by bad working and by carelessness, and that to a very considerable amount in proportion to what is actually used. Under favourable systems of working the loss is about 10 per cent, while, in a very large number of instances, the ordinary waste and loss amounts to 40 per cent, irrespective of what is sacrificed by the necessity for leaving coal for barriers, for the support of buildings, and for other objects. This is a very considerable evil, and one which requires immediate attention. If it be necessary to husband our coal resources, means should be everywhere encouraged, not only for the introduction of an improved system of getting coal, so as to reduce the waste to a minimum, but for the better utilisation of small coal, none of which should be permitted to be left below that can possibly be raised. One method of utilising small coal, which is briefly referred to by Committee E, in their Report, is by the manufacture of patent fuel, but this is not at present carried on to a sufficient extent to have much effect upon the general question.

In considering the quantity of coal in known coal-fields, 4000 feet has been adopted as the limit of practicable depth in working, and a certain proportion has been allowed for waste and loss incident to working the coal. With these provisions, the estimated quantity of coal in the ascertained coal-fields of the United Kingdom is 90,207 millions of tons, whilst at depths below 4000 feet it is computed that there is a further supply amounting to 7320 millions of tons, which might be obtained if existing obstacles to working at a lower depth than 4000 feet were overcome. Space will not admit of our even touching on the considerations upon which the Commissioners express themselves in favour of finding coal under the permian and newer strata. The supply from this source, within the depth of 4000 feet, is roughly estimated at 56,273 millions of tons. It is also considered probable that coal measures may possibly extend beneath the south-eastern part of England; but Sir Roderick Murchison contends, in opposition to this theory, that "in consequence of the extension of Silurian and Cambrian rocks beneath the secondary strata of the South-east of England, and of the great amount of denudation which the carboniferous rocks had undergone over the area of the South of England previous to the deposition of the secondary formations, little coal could be expected to remain under the cretaceous rocks." As this question is still one of theory, no attempt has been made by the Commission to estimate the quantity of coal lying under the unexplored area of the South of England.

Omitting the probable amount of coal below 4000 feet in depth, there thus appears to be an aggregate quantity of 146,480 millions of tons which may be reasonably expected to be available for use; and it remains now only to see how long that quantity, with an increasing consumption, is likely to last. The bases upon which calculation may be made, as to the probable duration of our coal supplies, are numerous, and varying conclusions have been consequently arrived at by different authorities. The two great principles upon which such a calculation should be based are—the annual increase in population, coupled with the increase in consumption of coal per head of the population. Now, from the year 1811 to 1821 the increase in population was 16 per cent, while in the last decade, from 1861 to 1871, it was 11¾ per cent. These two rates of increase, in conjunction with those of the intervening decades, have been taken as the elements of a curve which, carried forward, shows the extent of the population in future years, supposing no disturbing causes to arise. The rate of consumption of coal per head of the population appears to have been very irregular; but, on an average, the increase in fourteen years amounts to nearly 3 per cent per annum, but it is not thought probable that this rate will be maintained. From statistical tables furnished by the Commissioners, it appears that the absolute increase of the consumption of coal between 1855 and 1859 averaged 0·035 ton per head per annum; that the next six years, 1859 to 1865, averaged 0·145 ton per head per annum; while the last four years, 1865 to 1869, only averaged 0·0463 ton per head per annum. From this it would appear that the annual increase has passed through a point of maximum increase, and that it is now diminishing.

Basing their calculation, however, upon an arithmetical instead of a geometrical increase in the rate of consumption, and simply adding a constant quantity equal to the average annual increase of the last fourteen years, taken at 3,000,000 tons, the Commissioners arrive at the following result, namely, that at the end of a hundred years the consumption would be 415,000,000 tons per annum, and that the now estimated quantity of coal available for use would represent a consumption of 276 years. Taking, however, another view of the case, and supposing that from this time the population of the whole country, and the consumption of coal per head of that population, will remain constant, or merely oscillate without advancing, our available coal would represent a consumption of upwards of 1273 years, at the rate of 115,000,000 per annum.

These two calculations, as to the probable duration of our coal resources, given by the Coal Commission in their Report, may probably be taken to represent two extreme cases, between which the actual truth may probably be found. Whilst, then, we may confidently anticipate the continued existence of coal for some 300 years at least, it by no means follows that its price will remain as at present. With increased depth of workings, and an increased difficulty in raising coal, its price must necessarily rise; and although, therefore, there is no fear that coal will become actually scarce in our time, the same effect will, in a great measure, be secured by its increase in price. The prevention of this evil can, apparently, only be effected by still greater economy in combustion, and the enforcement of a law that no coal, either large or small, shall ever be left in a working beyond what it may be impossible to gain.

V. THE SPECTROSCOPE: ITS IMPERFECTIONS AND THEIR REMEDY.

By Mungo Ponton, F.R.S.E.

THAT the spectroscope, as at present constructed, is an imperfect and fallacious instrument has been already indicated in a previous paper, entitled "Molecules, Ultimates, Atoms, and Waves." It is now proposed to show in what respect the instrument is objectionable, and how it may possibly be improved.

To understand the subject, it is needful to have some acquaintance with the laws of chromatic dispersion. An investigation into these laws will be found in the "Philosophical Magazine" for 1860, pp. 165, 263, 364. When those researches were undertaken, the only existing measurements of the wave-lengths corresponding to the principal fixed lines of the spectrum were those left by Fraünhofer. These wave-lengths, however, have been recently re-measured with more perfect appliances than Fraünhofer could obtain, by M. Angström, of Upsal, who has published the results in his work on the Normal Spectrum, accompanied by an illustrative atlas, in which all the most remarkable lines of the spectrum are laid down on an extended scale according to their wave-lengths.

Since that work was published, M. Angström's measurements have been analysed, and it has been ascertained that the whole of the wave-lengths corresponding to the principal fixed lines are capable of being calculated from one—namely, that corresponding to the more refrangible of the two lines marked E. The formulæ expressing the relations on which these calculations are based will be found in a memorandum contained in a work recently published.* It is there shown, moreover, that there is another curious relation connecting the whole of those wave-lengths together. While the other seven are all capable of being calculated from that of the more refrangible E, the sum of the three equations, by which are thus calculated the wave-lengths corresponding to the three lines A, B, and C, is equal to the sum of the four equations by which are calculated the wave-lengths corresponding to the four lines, D, F, G, and H. This remarkable relation seems to indicate that the wave-

* "The Beginning," &c. Longmans and Co. The memorandum is inserted between the notes and the description of the plates.

lengths of these lines are all interdependent, resembling the strings of a perfectly-tuned instrument; the key-note corresponding to the more refrangible E.

The extreme closeness of the agreement between the wave-lengths as given by M. Angström's observations, and those calculated by the formulæ, combines with this last-mentioned relation to show both the accuracy of the observations and the truth of the relations which the formulæ express. Great confidence may therefore be placed in the correctness of the wave-lengths as calculated from those formulæ.

It became interesting to inquire to what extent the laws of chromatic dispersion, as deduced from Fraünhofer's normals, might be affected by this alteration in the wave-lengths corresponding to the principal fixed lines. Investigation shows that those laws remain unshaken in their principles by this change in the elements of calculation.

The important practical use to which those laws may be applied is to check the accuracy of observations on the indices of refraction corresponding to the principal fixed lines in different media, for the observed indices rarely give results which tally quite exactly with the laws; but the alterations which they must undergo, to render the agreement perfect, are in general so small as to establish the laws, which may accordingly in their turn be employed to correct the indices.

The media, whose indices of refraction for the different fixed lines have been experimentally determined, fall under two categories,—1st, regular; 2nd, irregular. The former embraces by much the larger number of media; and as it is only with such that the spectroscope has any connection, it is to them that attention shall here be confined.

In all media whatever, the relation between the wave-length in the free ether and that within the refracted medium may be expressed by one and the same formula. If U represent the normal undulation, and u its reduced length within the medium, then is

$$\frac{U}{\varepsilon} - a \pm x = u,$$

where ε and a are two quantities constant for the medium and temperature, while x is a small quantity peculiar to each wave, and represents what is termed the irrationality of the medium, or, in other words, the extent to which the fixed lines are extruded or thrust out of their proper places. It is this quantity x, however, that is all-important in the correction of the observed indices of refraction; because it is regulated by peculiar laws, and the indices may be corrected by bringing them into conformity to those laws. If μ represent the refractive index in any medium corresponding to the undulation belonging to any fixed line, we have, of course, in every case,

$$\frac{U}{\mu} = \frac{U}{\varepsilon} - a \pm x = u.$$

But it is only where μ, the index of refraction, is quite correct that the quantities represented by x conform to their proper laws.

These quantities represent additions to or abstractions from the internal wave-lengths, that would arise from the more simple formula,

$$\frac{U}{\mu} = \frac{U}{\varepsilon} - a.$$

They constitute an evidence of a transfer of energy from one set of waves to another within the medium, and by its action. Now the general law of the conservation of energy requires that this transfer should involve an exact compensation,—that what is lost in length by one set of waves should be exactly the same as what is gained in length by another set, or, symbolically, the quantities represented by $+x$ must be exactly equal to those represented by $-x$. In every good set of observations on the indices there is a close approach to this equality, and this constitutes the first law regulating those quantities. It applies to all media, whether regular or irregular.

In all regular media the sign of x is plus for the three waves corresponding to the lines D, E, and F, and minus for the four waves corresponding to the lines B, C, G, and H. In the former the formula is always

$$\left(\frac{U}{\varepsilon} - a\right) + x = u = \frac{U}{\mu};$$

for the latter it is

$$\left(\frac{U}{\varepsilon} - a\right) - x = u = \frac{U}{\mu}.$$

If b_x, c_x, d_x, &c., represent the quantities x, corresponding to each internal wave, we have, in all regular media,

$$3b_x + 2c_x - d_x = 3h_x + 2g_x - f_x.$$

If the difference between b_x and $h_x = \delta$, that between c_x and $g_x = \delta_2$, and that between d_x and $f_x = \delta_3$, then the differences between each pair of δ_1, δ_2, and δ_3, constitute an arithmetical progression.

In all regular media there are two nodes, at which the sign of x changes from plus to minus, the one situated between C and D, the other between F and G. Irregular media differ from regular in the position and number of these nodes, and in the arrangement of the quantities plus x and minus x; but the sums of each of these are always equal.

These laws, and the mode of their application to the correction of the observed indices of refraction, may be illustrated by a single example. For this purpose the specimen of flint glass marked No. 30 by Fraünhofer may be selected. The indices of refraction, as determined by him, are as under:—

B.	C.	D.	E.	F.	G.	H.
1·623570,	1·625477,	1·630585,	1·637356,	1·643466,	1·655406,	1·666072.

The normal wave-lengths corresponding to these seven lines, as observed by Angström, and corrected by the formulæ according to which they are calculated from E, are as follows, E being assumed as unity, and the others stated in reference to that standard:—

B.	C.	D.	E.	F.	G.	H.
1·3033839,	1·245493,	1·1189003,	1,	0·922576,	0·8175183,	0·7464871.

The lines D, E, and H, in the spectrum are double; but it is the less refrangible D, and the more refrangible E and H, whose wave-lengths are here given.

The internal wave-lengths in flint glass, No. 30, are found by dividing each of the above normal wave-lengths by its corresponding index of refraction. This gives the following values:—

b.	*c.*	*d.*	*e.*	*f.*	*g.*	*h.*
0·802789,	0·766232,	0·686196,	0·610741,	0·561360,	0·493848,	0·448052.

The constants, ε and a, for the formula

$$\frac{U}{\varepsilon} - a \pm x = u,$$

are found thus:—

$$\varepsilon = \frac{(3B+2C+D)-(F+2G+3H)}{(3b+2c+d)-(f+2g+3h)},$$

and its value in this case is 1·570504. Then, calling the sum of the seven normal wave-lengths = S, and the sum of the seven corresponding internal wave-length = s, we have

$$a = \frac{(S \div \varepsilon) - s}{7},$$

and its value in this case is 0·026605. From the formula

$$\frac{U}{\varepsilon} - a = u_2$$

we obtain a second set of values of the internal waves, as follows:—

b_2.	c_2.	d_2.	e_2.	f_2.	g_2.	h_2.
0·803309,	0·766448,	0·685842,	0·610133,	0·560834,	0·493948	0·448712.

The differences between these and the former set are the quantities represented by x, and they stand as under:—

x plus.		x minus.	
d_x	0·000354	b_x	0·000520
e_x	0·000608	c_x	0·000216
f_x	0·000526	g_x	0·000100
		h_x	0·000660
Sum	0·001488	Sum	0·001496

The equality here, although near, is not perfect, showing that a slight adjustment is required to bring these quantities under the dominion of the general law of the conservation of energy, Neither do they perfectly agree with the other laws before mentioned, but the difference is small. When these quantities are adjusted according to the laws, neglecting the prefixed cyphers, they stand thus:—

x plus.		x minus.	
d_x	355	b_x	521
e_x	609	c_x	216
f_x	526	g_x	93
		h_x	660
	1490		1490

We have also—

x plus.		x minus.	
$3b_x$	1563	$3h_x$	1980
$2c_x$	432	$2g_x$	186
	1995		2166
Less d_x	355	Less f_x	526
	1640		1640

We have further the following arithmetical progression:—

$$\left.\begin{array}{lr}
h_x - b_x = 139 & \\
c_x - g_x = 123 & \\
\overline{} & 16 \\
f_x - d_x = 171 & \\
h_x - b_x = 139 & \\
\overline{} & 32 \\
f_x - d_x = 171 & \\
c_x - g_x = 123 & \\
\overline{} & 48
\end{array}\right\}\text{Common difference 16,}$$

thus corresponding to the laws. By calculating backwards from these adjusted quantities we obtain the corrected indices of refraction, which will be found to differ but slightly from those derived from observation, as shown in the following table:—

	Observed.	Calculated.	Difference +	Difference −
B.	1·623570	1·623571	0·000001	
C.	1·625477	1·625477		
D.	1·630585	1·630582		0·000003
E.	1·637356	1·637350		0·000006
F.	1·643466	1·643463		0·000003
G.	1·655406	1·655398		0·000008
H.	1·666072	1·666061		0·000011

Also the value of ε becomes 1·570518, and of a 0·026598. These small differences are far within the limits of probable errors of observation.

Reverting to the normals, as wave-length is in the diffracted spectrum inversely equivalent to refrangibility, it follows that the positions assigned to the different lines in M. Angström's Atlas being fixed according to their wave-length, these must be their true positions in the normal spectrum, and that in so far as—in the spectrum produced by any set of prisms composing a spectroscope—the relative positions differ from those assigned to them in M. Angström's Atlas, such differences must be due to the action of the prisms.

To show to how great an extent the lines are displaced in M. Kirchhoff's spectrum, it is needful to compare the relative positions there assigned to the lines with those in M. Angström's Atlas. Fortunately this is not difficult; for the interval between the more and less refrangible lines marked D, which in M. Angström's Atlas occupies six divisions, in M. Kirchhoff's occupies four; and as the relative positions of those two lines cannot differ appreciably in the two spectra, each division of M. Kirchhoff's scale may be reckoned equivalent to a division and a half of M. Angström's. It is only necessary, therefore, to add a half more to each of Kirchhoff's intervals to make them equivalent to Angström's. The following table exhibits the intervals between the lines in the two spectra thus compared:—

	Angström's.	Kirchhoff's.	Differences +	Differences −
A—B.	737	285		452
B—C.	305	153		152
C—D.	667	463		204
D—E.	627	781	154	
E—F.	408	834	426	
F—G.	553	1162	609	
G—H.	374	1542	1168	
	3671	5220	2357	808

A simple inspection of this table suffices to show how much the lines are displaced from their true relative positions by the action of the prisms in M. Kirchhoff's spectroscope.

For the sake of further comparison, suppose a spectroscope to be constructed with prisms having the same refracting angles as those in M. Kirchhoff's instrument, namely, three with the angle 45°, and one with the angle 60°, but composed of flint glass, No. 30 of Fraünhofer. The following are the ultimate differences of deviation in seconds between the different lines, which would be given by such an instrument:

B—C.	C—D.	D—E.	E—F.	F—G.	G—H.	
1826,	4902,	6524,	5926,	11642,	10458.	Sum 41,278

For a short interval like that between the more and less refrangible of the two lines marked D, the differences of the indices of refraction may be assumed to bear to the differences of wave-length the same relation as they do in the case of the adjacent principal lines, which are here the less refrangible D and the more refrangible E. The difference between the relative wave-lengths of these two lines is 0·1189003, and the difference between the wave-lengths of the more and the less refrangible D is 0·0011378—the former difference being nearly 104·5 times that of the latter. The difference between the refractive indices of the less refrangible D and the more refrangible E for flint glass, No. 30, is 0·006768; and on the supposition that this difference is 104·5 times greater than that between the indices of the more and the less refrangible D, the latter difference will be

0·000065. This corresponds to a difference of deviation of about 62″ for the four prisms. As these 62″ of deviation correspond to six degrees of Angström's scale, if we take a tenth of the deviations we shall have the intervals for this flint glass according to M. Angström's scale, sufficiently near for the present purpose. The intervals between the lines resulting from these four flint glass prisms, as compared with those from M. Kirchhoff's four, and with the normals, will accordingly stand thus—

	Normals.	Flint Glass.	Kirchhoff's.	Diff. from Normals.	Diff. from Kirchhoff's.
B—C.	305	183	153	−122	+30
C—D.	667	490	463	−177	+37
D—E.	627	652	781	+ 25	−129
E—F.	408	593	834	+185	−241
F—G.	553	1164	1162	+611	+ 2
G—H.	374	1046	1542	+672	−496

It will be perceived from this table that the intervals of the spectrum, produced by the flint glass, No. 30, of Fraünhofer, approach more nearly in character to those of the normals than do those of M. Kirchhoff's, and that the increased dispersion of this last is in a great measure due to an excessive enlargement of the interval between G and H. The peculiar features of M. Kirchhoff's spectrum are probably traceable to his having given to his prisms curved faces, which, while increasing the dispersion, has augmented the irrationality in a still higher degree.

As all media tend to alter the intervals between the lines to a greater or smaller extent, it is evident that, if prisms are to be retained in the construction of the spectroscope, some device must be adopted in order to give those intervals their true value before the instrument can be regarded as satisfactory. The means that appear available for this purpose are the combining of different media, the giving of one or more of the faces of one or more of the prisms some peculiar curvature, or the introduction of both of those means of correction to render it more perfect. If, as in M. Kirchhoff's instrument, curvature of the faces of the prisms has so greatly increased the irrationality of the intervals, it seems probable that a similar device applied in a different way might be rendered available to restore them to their true values. The aim has hitherto been to obtain a very large spectrum by means of a great amount of dispersion, irrespective of the displacement of the lines; whereas the correct principle of construction should be to obtain an accurate spectrum, corresponding exactly with that obtained by diffraction, trusting to magnification by the telescope for increase of size.

In Mr. Grubb's spectroscope recourse has been had to a combination of flint and crown-glass in the construction of the prisms; there being a central prism of dense flint-glass and large angle, having cemented on each of its two refracting sides, in the reverse position, a crown-glass prism of one-fourth of the angle. Without knowing the exact refractive indices of the two kinds of glass employed for each of the fixed lines, it is impossible to do more than show approximately the effect of this combination on the intervals; but such an approximation may be made by assuming the indices. Suppose, then, that the flint-glass prism has the same indices as Fraünhofer's No. 30, and that the crown-glass has the same indices as Fraünhofer's No. 9, which, when corrected by the laws, are as under:—

B.	C.	D.	E.	F.	G.	H.
1·525832,	1·526849,	1·529587,	1·533000,	1·536052,	1·541659,	1·546566.

The differences between these and the observed indices are still more trifling than in the case of the flint-glass. Suppose the refracting angle of the flint-glass prism to be 90°, and that of each of the crown-glass prisms to be a fourth of this, or 22° 30′, the ultimate differences of deviation arising from such a compound prism would be in seconds as under:—

B—C.	C—D.	D—E.	E—F.	F—G.	G—H.
754,	1862,	2548,	2360,	4728,	4372.

The difference of deviation between the two lines marked D is 32″; consequently, if we take one-fifth of the above deviations, we shall have the intervals, according to M. Angström's scale, sufficiently near for the present purpose. They will then, in comparison with the normal intervals, stand as under:—

	Normals.	Comp. Prisms.	Diff. +	Diff. −
B—C.	305	151		154
C—D.	667	372		295
D—E.	627	510		117
E—F.	408	407		1
F—G.	553	946	393	
G—H.	374	874	500	

It is thus evident that, while the character of the irrationality is much altered by this combination, it is far from being removed. The almost perfect coincidence of the intervals E—F, in the above spectra, is remarkable.

It might nevertheless be possible, by the expenditure of a great amount of time and skill, to find a combination of prisms of diverse media, which, combined with a certain curvature of the faces of one or more of the prisms, might bring the intervals into a more or less perfect agreement with those of the normals.

The true and most effectual remedy, however, appears to be the having recourse at once to the diffracted spectrum in the construction of the spectroscope. There are, doubtless, great practical difficulties in the way of making a handy instrument on that principle; but it is believed that it will be found possible to surmount these more easily than to overcome those attending the obtaining of a refracted spectrum which shall exactly correspond with the diffracted. It is comparatively easy to produce a diffracted spectrum, by means of a system of equidistant fine lines viewed through a telescope at a distance of about 12 feet. The difficulty is to secure a good large spectrum, from such a system, within the compass of an easily portable instrument. Until that be accomplished, it would be well that every spectroscope formed with prisms should have its individual spectrum carefully compared with the normal spectrum formed by a standard instrument, and that a table should be constructed showing how much the positions of the principal fixed lines, and of the borders of the colours, differ in the refracted spectrum from what they are in the normal, such table to be attached to the instrument.

It may not be amiss, however, to throw out a few hints as to the practicability of constructing a compact spectroscope on the diffracting principle. Mr. Lewis Rutherford, of New York, has recently exhibited in London sets of equidistant lines ruled on glass, of which there are 1500 to the inch; and even with these fair diffracted spectra can be obtained. But it is better to have the lines very much closer than these. It is not difficult to obtain copper wire 1-200th of an inch in diameter. Suppose wire of this description to be so wound on a square frame as to leave between each strand an interval exactly equal to the diameter of the wire; this would give a hundred strands and a hundred equal intervals for each inch. A square frame, therefore, of 100 inches free space would contain 10,000 strands of wire, and the like number of equal intervals. It is believed that such a frame might be constructed without much difficulty, and, were it once constructed, any number of photographs might be taken from it, reducing its size to one quarter of an inch. The photograph would thus have 10,000 equidistant lines in the space of a quarter of an inch. These photographs might be taken on thin plates of quartz, cut parallel to the axis of the crystal, and the lines could be covered with a similar plate to prevent access of dust. By using quartz the extreme ultra violet lines could be obtained without absorption, and be rendered visible by means of a fluorescent screen. To prevent the effects of diffraction between the lines, in taking the photographs, the wire frame should have fine, thin, white paper placed behind it, through which the light should be admitted to pass through the frame, and all other light should be excluded from the camera.

Having thus procured the system of fine equidistant lines, the next point is to make such arrangements that a highly magnified spectrum could be obtained from them by means of a telescope placed at a moderate distance. Suppose a box to be formed about 18 inches in length, and that the system of fine lines, with its slit and collimator furnished with quartz lenses, are placed at the top of the box, about the centre, so that the spectrum formed may be thrown down to the centre of the interior of the box, an arrangement being provided for shifting the system of fine lines along a graduated scale, so as to bring successively the different parts of the spectrum into view. Suppose a very small plane reflector, of silver or speculum metal, to be placed in the centre of the interior of the box, at an angle of 45° towards the roof, to receive the image of the spectrum, and let the image formed by this mirror be in the focus of a parabolic reflector placed at one end of the box, so that the rays reflected from the small plane mirror shall be reflected parallel towards the opposite end of the box. Exactly opposite to this parabolic reflector let there be placed a reflecting telescope, the parabolic reflector of which shall receive the parallel rays, and concentrate them in its own focus on another small plane mirror, where they may be viewed by the eye-piece to be composed of quartz lenses. By this arrangement the light would never pass through any other medium than quartz, which does not absorb the ultra-violet rays. The rendering the rays parallel at once would obviate the necessity for viewing the spectrum from a distance, in order to obtain sufficient magnification, which might be secured of any desired amount, by means of the reflecting telescope, without much loss of light. For viewing the ultra-violet rays a fluorescent surface might be substituted for the first of the small plane mirrors.

The chief practical difficulty would be the procuring of two good parabolic reflectors for the instrument; but it is believed that these can now be made of accuracy sufficient for this purpose.

Great care would be required in taking the photograph of the fine equidistant lines, to secure their being accurately in focus; but the true focus once found and fixed, the photographs might be multiplied indefinitely, at a moderate cost.

These hints are merely thrown out for the consideration of practical opticians, and it is quite possible that there may be graver difficulties which have been overlooked. Nevertheless, the devising of some means for constructing a spectroscope on the diffracting principle is well worth the consideration of those engaged in the manufacture of this instrument; for until this principle be adopted, accuracy, certainty, and uniformity of results, cannot be attained.

VI. MODERN CANNON POWDER.

IN the year 1779, nearly a century ago, General Sir William (then Captain) Congreve was sent to Plymouth to examine the gunpowder with which the Fleet was then supplied, on which occasion he reported that there were only four barrels of serviceable powder in the whole of His Majesty's ships. This state of affairs was no doubt due to the fact that the country was then entirely dependent on private manufacturers for its supply of powder, and that the proof to which it was subjected was not such as to ensure its being of good quality. On discovery of the gross frauds which were thus being carried on with impunity, the Government Gunpowder Factory at Waltham Abbey was established, and, under the able superintendence of Sir W. Congreve, the quality of the powder supplied to the army and navy was greatly improved.

From this date until the general introduction of rifled guns in 1860, very little progress was made towards the development of this important manufacture, the only changes being in the direction of improvements in the preparation and purification of the ingredients, the quality of the finished powder being thereby improved, while its character remained unaltered. During the whole of this period the description of powder used with all cannon was what is technically called "L.G.," or "Large Grain," in contradistinction to "F.G.," or "Fine Grain," which was used with small arms and muskets; but this powder was believed to be too violent for use in the rifled breech-loading guns introduced about twelve years ago, and a modified kind was therefore adopted, on the recommendation of Sir William Armstrong. The modification consisted in making the powder much larger* in the grain, and in addition, coating it with a thin film of graphite, so as further to retard its combustion and thus to reduce the strain upon the breech-closing mechanism of the gun.

This new powder was at first called "A_4," but its name was afterwads changed to "R.L.G.," or "Rifle Large Grain" powder, when its use was extended to both muzzle-loading and breech-loading rifled guns. Now at the time of the introduction of this modified powder, the means of testing the action of the charge in the bore of a gun were very imperfect, and the change then made was founded almost entirely upon theoretical considerations. In order to understand these, and also the results of more modern experiments, it will be necessary to say a few words on the subject of the combustion of gunpowder.

In the first place it must be borne in mind that gunpowder, unlike nitro-glycerine, fulminate of mercury, and other detonating substances, is not a chemical compound, but only a mechanical mixture. By the incorporating process during manufacture the three substances of which powder is composed—saltpetre, sulphur, and charcoal—are so intimately mingled that the eye cannot detect the presence of any one of them in a free state. They are notwithstanding only mixed, and the saltpetre can be readily dissolved out by water, or the sulphur sublimed, in the form of vapour, by the application of a moderate heat, leaving in either case the other two ingredients chemically unchanged. The more intimate the mixture, the more nearly does gunpowder approach to a chemical compound, and the more violent is its combustion; but there always must remain a vast difference between the most complete mechanical mixture and the most unstable chemical compound.

For this reason the combustion of gunpowder is only very rapidly progressive, and not instantaneous as is the case with the violent explosives mentioned above. It is this difference that renders gunpowder so valuable as a propelling agent, for, were it not for its comparatively mild action, no gun could be made sufficiently strong to resist its force. The

* The size of grain in "L.G." powder is such that it will pass through a sieve of 8 meshes to the inch and be retained on one with 16 meshes, while the limits of "R.L.G." or "A_4" powder are between a 4-mesh and an 8-mesh sieve.

material of the cannon would be broken before the inertia of the shot could be overcome.

Now supposing one grain or particle alone to be ignited, it will be first inflamed over its whole surface, and the progressive combustion will take place from the exterior to the interior. Its *rate of combustion* will therefore depend upon both its shape and size, leaving out entirely for the present the question of density and hardness. A particle of spherical or cubical form will expose less surface to ignition, in proportion to its volume, than one of an elongated or flat shape, and will consequently require a longer period for the combustion of its entire mass: the larger the particle also, the longer will be the time required for its consumption. Looking, then, at one grain of powder by itself, we may safely say that the larger it is, and the more nearly does its form approach to that of a sphere, the longer will its combustion take, and the slower will be the evolution of the gas. When, however, we come to regard the action of an aggregation of such particles, as in the charge of a gun, the *rate of ignition* of the whole charge is also affected by the size and shape of the grains. The part of the charge first ignited is that near the vent, or touch-hole, and the remainder is inflamed by contact with the heated gas generated by the combustion of this portion, so that the rate of ignition of the whole mass will be regulated by the greater or less facility with which the gas can penetrate throughout the charge, which is itself dependent upon the size and shape of the interstices between the grains. If the grains be spherical and regular in form, the interstices will be comparatively large and uniform, and the gas will penetrate the mass with facility; again, the larger the grains, the larger the interstices between them. If, on the other hand, they be flat or flaky and irregular in shape, the passage of the gas will be more difficult, and the rate of inflammation of the charge reduced.

We see, therefore, that the considerations which affect the more or less rapid combustion of an individual grain of gunpowder also affect the rate of ignition of a charge of such grains, but in an opposite direction; so that a form of grain which will individually burn rapidly may offer an increased resistance to the passage of the heated gas through the charge, and thereby retard its ignition, while a grain which will burn more slowly may allow of the charge being more rapidly ignited. By varying the size and shape of the grain alone, a powder may therefore be obtained a charge of which shall be ignited rapidly throughout but burn comparatively slowly, or one which shall be ignited more slowly, but when once inflamed burn very rapidly. It is necessary to draw a clear distinction between a rapidly igniting and a quickly burning powder: this difference will be more apparent when we come to the discussion of more modern powders.

The grains in both L.G. and R.L.G. powder are very irregular in shape, and the latter is double the size of the former, so that the individual grains will burn more slowly. It was, therefore, believed on theoretical grounds that the larger powder would exert a less violent strain upon a gun, and it was adopted, as we have said, for our rifled guns, the question of *density* being regarded at that time as of minor importance, though it was already attracting some attention. It has since been conclusively proved by experiments that the *density* and *hardness* of the grains of powder are of quite as vital importance as their size and form, in determining the rate of ignition and combustion of a charge.

The density depends on the amount of pressure to which the powder meal has been subjected during manufacture, while the hardness is greatly affected by the amount of moisture present in the meal when pressed; one term applies to the mass, while the other refers more particularly to the surface of the grains. A dense powder may be generally stated to be a slow-burning powder, while a hard one is slow lighting. Density retards the combustion both because there is more matter in the same volume, and consequently more powder to be consumed in proportion to the ignited surface of the grain, and also because the heated gas finds greater difficulty in penetrating the solid mass of the grain. A hard powder need not of necessity be very dense; it is even possible, by pressing it in a moist state, to obtain a very hard powder which shall at the same time be light and porous in the interior of the grains. Such is the Russian prismatic powder (of which more hereafter), and it may be taken as a good specimen of a slow lighting but quick burning powder.

With the improved appliances now used in testing powder, the quality of the large stock of L.G. and R.L.G. in store in this country has been found to be very variable, principally due to variation in the density of different brands. Previous to the year 1868, the proof to which all cannon powders were subjected was very imperfect, and failed utterly in ensuring uniformity in those passed into the service. The density was only roughly ascertained by the process of "cubing," as it was called, while the strength and uniformity of the powder was tested by the "Mortar Eprouvette." "Cubing" consisted in weighing a cubic foot of the powder, a box made to hold that amount being filled by pouring the substance loosely into it; the weight therefore depended, to a great extent, upon the closeness with which the powder packed itself, as well as upon the absolute density of the grains. The shape of the grains, and the amount of glaze the powder has received, affect the closeness with which it packs itself, and would therefore lead to errors in determining the density in this way. At the present time the density is accurately arrived at, by means of a mercury densimeter, in which the weight of a given volume of powder is compared with that of an equal volume of mercury: the density of mercury (corrected according to the readings of a barometer and thermometer at the time) being known, that of the powder is easily calculated.

In the "Mortar Eprouvette" a round shot, weighing 68 lbs. was fired from an eight-inch mortar with a charge of from two to three ozs. of the powder under examination, and the range of the shot from the muzzle of the mortar was measured. The greater the range the better was the powder believed to be, the only limit being a low one. The fallacy of this belief was proved beyond a doubt as early as the year 1864, by comparing the velocity of shot fired with different powders, by means of the accurate instruments then generally in use for that purpose. It was then found that powers which gave the best results in very small charges fired from a mortar were often very inferior when fired in comparatively large charges from guns, and the immediate adoption of a new proof of powder, by measuring the velocity of shot fired under service conditions, was strongly recommended. This recommendation was not, however, carried out until four years later, when Colonel Younghusband, the present Superintendent of the Government Gunpowder Works at Waltham Abbey, introduced the velocity proof which is now in force. The instrument used at Waltham Abbey for measuring the velocity of a shot is an electro-ballistic chronoscope, invented by Captain Le Bouleugè, of the Belgian Artillery,* which surpasses all similar instruments in simplicity and facility of manipulation, though the principle upon which it acts is the same as in others. In it electricity is employed to record the exact instant at which the shot passes two points at a known distance apart, a short space in front of the gun. From this the time occupied by the shot in traversing the distance between these two

* This instrument, and the method of using it, is described in detail in a pamphlet by Lieut. C. Jones, Royal Artillery, Instructor Royal Gun Factories. Printed by order of the Secretary of State for War.

points is known, and the velocity with which it is moving is readily ascertained, and affords a direct indication of the strength and uniformity of the powder. Every kind of powder now passed into the service is subjected to this proof, in addition to being tested by the mercury densimeter.

We have stated that R.L.G. powder was adopted in 1860 for our breech-loading guns, and that its use was afterwards extended to the charges of all rifled guns. When, however, the size of our heavy ordnance was increased more and more, it soon became apparent that even this powder was totally unfit for the large charges then used, and its violent action earned for it abroad the unenviable *soubriquet* of "*poudre brutale*."

In the year 1858 the gunpowder question was referred to a Committee, composed of the Superintendent of the Royal Gunpowder Factory, the Superintendent of the Royal Laboratory, and the Chemist to the War Department;* and it was, in fact, some of the earlier experiments of this Committee that led to the introduction of A_4 or R.L.G. powder. The means at their disposal for determining the manner of combustion, and the pressure exerted upon the gun by different kinds of powder, were very limited. Nevertheless the conclusions they arrived at, as set forth in their Reports of 1859 and 1866, were very correct, and have been entirely corroborated by subsequent researches. As early as 1860 they had satisfactorily proved that the density and hardness of powder exercise an important influence on its character, and in all their subsequent experiments these points were strictly attended to. In their final report (1866) they recommended the adoption of a cylindrical "Pellet" powder of a density between 1·492 and 1·50, but pressed comparatively wet, so that, though light, the powder should be rather hard. The form of this powder is shown at Fig. 1, Plate I., the cavity or indentation having been introduced in order to increase the surface exposed to ignition. This powder was adopted entirely upon experiments carried on with various natures of Armstrong breech-loading guns and smooth-bored mortars, and it is evident that a light, but hard, powder, such as this is, which would be slow lighting but quick burning, would be exactly suited to breech-loading guns, in which the initial resistance of the tight-fitting, lead-coated projectile is very great, as the lead has to be bodily forced forward into the grooves of the rifling.

* Colonel Askwith, R.A.; Captain Boxer, R.A.; and F. Abel, Esq., R.A., F.R.S.

The pellet form was recommended principally as a convenient method of making a large grain powder of considerable uniformity in size and density; but the Committee did not consider that the subject of gunpowder had been exhausted by them, and closed their Report with a recommendation that "systematic artillery experiments should be instituted with this pellet powder, of a sufficiently comprehensive character to test thoroughly the system."

In the meantime, while the labours of this Committee were still progressing, other experiments were being carried on in this country. In 1863-4 a proposal was made to press granulated powder into discs the size of the bore of the gun, and perforated with holes to facilitate the passage of the gas. These discs varied in thickness from 2 to 3 inches, and were made of powder of various-sized grains, the amount of the compressing force differing in different specimens. The results of these trials were not sufficiently satisfactory to lead to the adoption of this form of powder. About the same time a similar description of powder, proposed by Dr. Doremus, an American, was tried unsuccessfully, both in America and in this country; and again, in 1866, discs—made by compressing the powder meal—gave even less satisfactory results.

The Americans, about this period, introduced an irregular large grain powder, which they called "Mammoth," and still use in the large charges fired from their enormous cast-iron smooth-bored guns, to which they obstinately adhered for years after the remainder of the civilised world had been armed with rifled ordnance of wrought-iron or steel. The size of this powder ranges from 0·15 inch to 0·30 inch, and its density is very moderate, being 1·70 to 1·75.

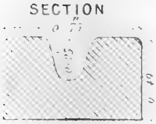
"Prismatic" powder appears, also, to have been tried in America in 1865; it had already been fired with good results from the heavy steel breech-loading guns which the Russians and Prussians have obtained from Messrs. Krupp, of Essen. This powder is shown at Fig. 2, Plate I., being made in the form of regular hexagonal prisms about 1 inch thick and 0·8 inch in the side, perforated with seven holes about 0·1 inch in diameter. In making up charges of this powder, the prisms are built up regularly in the cartridge bags, like honey-comb, which are then tightly tied at the mouth, so that the grains are kept firmly in their place. The perforations thus form long tubes through the charge, by which the gas permeates the whole mass.

The powder meal is pressed into the shape of these prisms in a very moist state, but the pressure is not great, as the

Pl I

Fig. 1

SECTION



PLAN

Pellet Powder
(full size)

Fig 3.

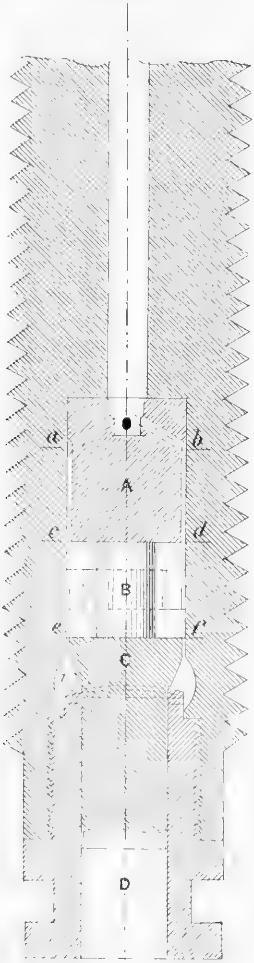
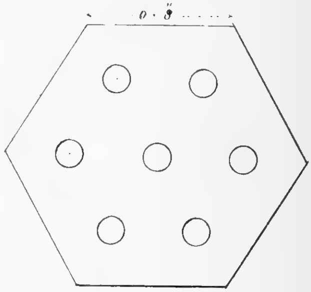
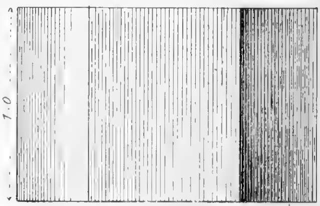


Fig. 2

PLAN



ELEVATION



Russian Prismatic Powder
(full size)

Sectional Elevation
of
CRUSHING INSTRUMENT

Full Size

Vincent Brooks Day & Son Lith.

density of the finished powder is only 1·67. The surface is, however, very hard, and, being to a certain extent covered with a film of saltpetre, which is deposited by the moisture when the powder is dried, it is comparatively difficult to ignite; when once inflamed it burns very rapidly, being light and porous, and in this respect is very like the pellet powder recommended in 1866, being particularly suitable for breech-loading guns. Though this pellet powder was decidedly a step in the right direction, as the strain upon our guns was considerably reduced by its use, it was only nominally adopted into the service in 1867, and was never issued either to our ships or batteries. The reason for this was that there existed no machinery for manufacturing a sufficient supply, and, while the necessary machinery was in preparation, the results of the experiments of the present Committee on Explosives* led to its abandonment in favour of "Pebble" powder.

This Committee was appointed in May, 1869, to enquire generally into the value of various explosive substances,—such as gun-cotton, nitro-glycerine, &c.,—in use, or proposed, for military purposes, and more particularly into the powder question, in which, through mismanagement rather than ignorance, we had fallen behind the rest of Europe. They at once entered on an extended series of experiments, with a view to the "determination of the description of gunpowder whose employment in large charges is attended with the least risk of overstraining the heavy guns" we now employ, and have rendered two Preliminary Reports on this subject.† From these it appears that no less than forty descriptions of British and foreign powders have been fired in large charges from heavy guns, out of which number four varieties were selected for further experiments.

The guns used in these experiments are an 8-inch wrought-iron smooth bore of 6½ tons, and a 10-inch gun of 18 tons: the latter was first used as a smooth bore, and afterwards rifled.

The means employed by the Committee, in the investigation of the action of the large charges fired from these guns, are very ingenious, and may be briefly described as follows:—

* This Committee consists of the following:—President—Colonel Younghusband, R.A., F.R.S. Members—Captain Singer, R.N.; Major Haig, R.A., F.R.S.; Captain V. D. Majendie, R.A.; Captain Stoney, R.A.; Captain A. Noble, late R.A., F.R.S.; F. Abel, Esq., F.R.S. Secretary—Captain W. H. Noble, R.A.

† Preliminary Report of the Committee on Explosive Substances; printed at the War Office, February, 1870; and Progress Report of the same, January, 1871.

1. The determination of the time taken by a projectile in traversing various intervals within the bore of the gun, which was effected by means of a chronoscope invented by Capt. A. Noble, a member of the Committee, and made at the Elswick Ordnance Company's Works. This will be described hereafter.

2. The determination of the pressure directly, by means of Rodman's pressure-gauge fitting on the exterior of the gun, and communicating with the interior of the bore by means of a hollow screw-plug.

3. The determination of the pressure directly, by means of an inner gauge termed a "crusher," which was designed by the Committee to overcome certain defects inherent in the Rodman gauge.

4. The determination of the velocity of the projectile after leaving the gun, by means of Navez-Leur's or Le Boulengé's electro-ballistic apparatus, commonly used for this purpose.

The chronoscope and the method of connecting it with the interior of the gun are shown in Plate II. The principle of action consists in registering, by means of electric currents, upon a recording surface travelling at a uniform and very high speed, the precise instant at which a shot passes certain defined points in the bore. The instrument may be divided into two portions; the one consisting of the mechanical arrangement for obtaining the necessary speed, and keeping that speed uniform; the other forming the electrical recording arrangement.

The first consists of a series of thin metal discs, A A, each 36 inches in circumference, fixed at intervals upon a horizontal shaft, S S, which is driven at a high speed by a heavy weight, B, arranged according to a plan originally proposed by Huyghens, through a train of gearing multiplying 625 times. The driving weight is continually wound up during the experiment by means of the handle H, and the requisite speed is obtained by accelerating the motion by the handle C. The precise rate at which the discs are moving is ascertained by the stop-clock, D, which can, at pleasure, be connected, or disconnected, with the revolving shaft, E, and the time of making any number of revolutions of this shaft can be recorded with accuracy to the 1-10th part of a second.

The speed attained is generally about 1000 inches per second linear velocity at the circumference of the revolving discs, so that each inch represents the 1-1000th part of a second, and, as the inch is subdivided by the vernier, V, into a thousand parts, a linear representation is thus obtained at the circumference of the discs of intervals of time as minute as the *one-millionth part of a second*. As a small variation in speed would affect the relations between the several records obtained, the uniformity of rotation is ascertained, on each occasion of experiment, by three observations,—one immediately before, one during, and one immediately after, the experiment, the mean of the three observations being taken as the average speed. The accuracy of the workmanship in the instrument is shown by the great degree of uniformity at which the speed is maintained. The Report gives the observations in six consecutive rounds; in two of these the speed was absolutely uniform, while the greatest variation in any round is as follows:—

1st observation,	625 revolutions	made in	21·2 seconds.
2nd „	„	„	20·9 „
3rd „	„	„	20·7 „

The arrangement for obtaining the electrical records is as follows:—The edges of the discs are covered with a strip of white paper, and each is connected with one of the secondary wires, G, of an induction coil. The other secondary wire, H, carefully insulated, is brought to a discharger, I, opposite the edge of its corresponding disc, and is fixed so as to be just clear of the latter. The surface of the paper on the discs is coated with lamp-black, so that the passage of a spark from the discharger to the disc burns away the black, and marks the spot perforated by exposure of the white paper beneath.

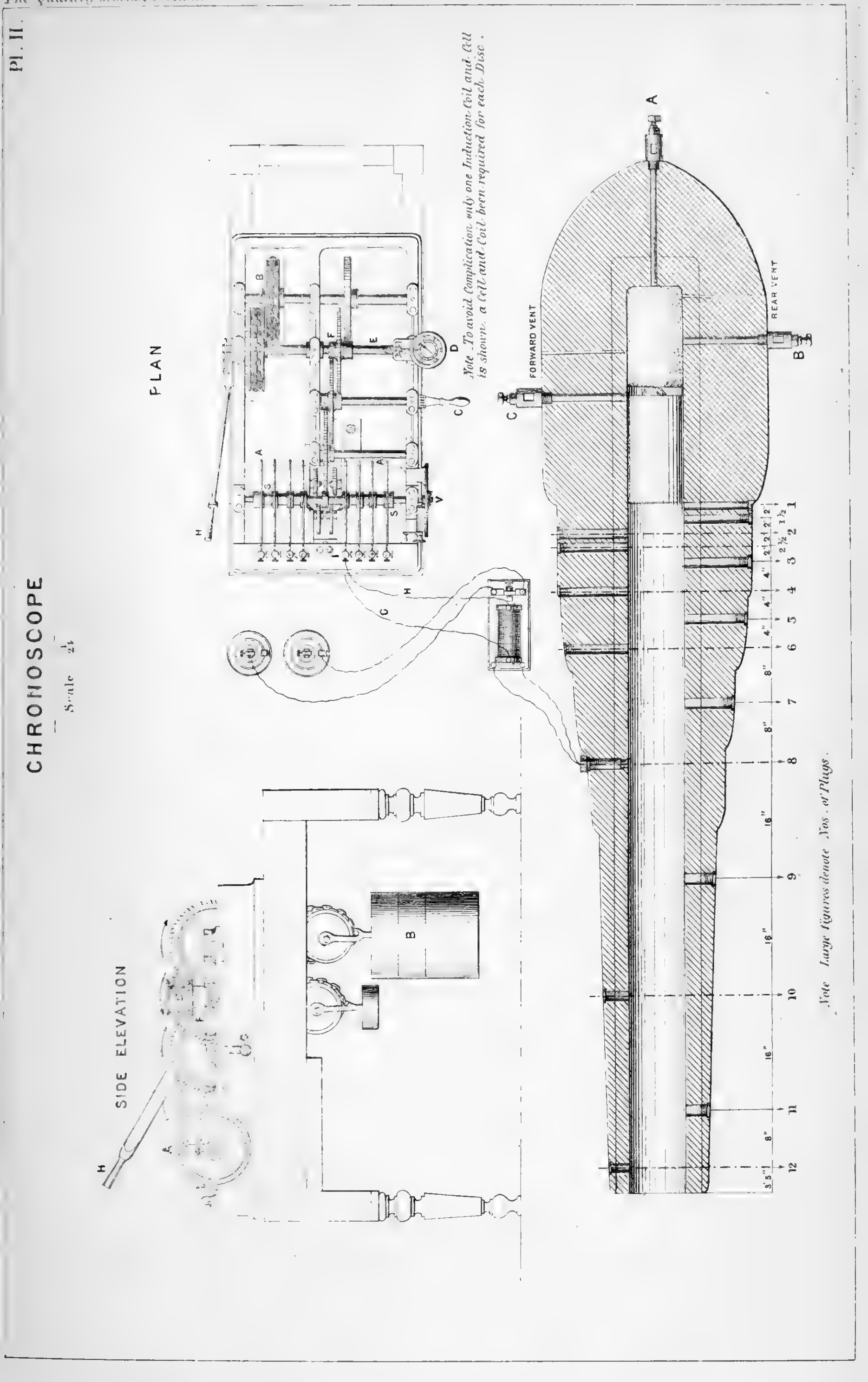
In order to connect the primary wires of the induction coils with the bore of the gun, so that they may be cut by the shot in its passage, the gun has been tapped in a number of places (see Plate II.) for the reception of hollow steel plugs, carrying at the end next the bore a cutter which projects slightly into the bore. This cutter is held in position by the primary wire, which is carefully insulated and passed down the plug, through the cutter, and back out of the plug, the ends being connected to the main wires leading to the induction coils. When the shot reaches the point where a plug is screwed in, it presses the cutter in flush with the bore, and, by so doing, cuts the primary circuit, thereby causing an induced spark to pass from one of the dischargers to the corresponding disc. As each plug is reached, a spark is delivered on the disc in connection with it, and thus the passage of the shot up the bore is recorded at regular intervals. By means of the micrometer, V, the distance between the sparks on the discs is read off, each spot being brought in succession exactly opposite the discharger belonging to the disc it is on: the speed at which the discs are moving being known, the time occupied by the shot in passing from one point to another is readily ascertained, and its velocity of translation calculated.

In order to test the accuracy of the instrument, it is only necessary to cut the whole of the primary wires simultaneously, when the whole of the sparks should be in one straight line, and the deviations from a straight line, that is, from an absolutely simultaneous record, give the instrumental errors.

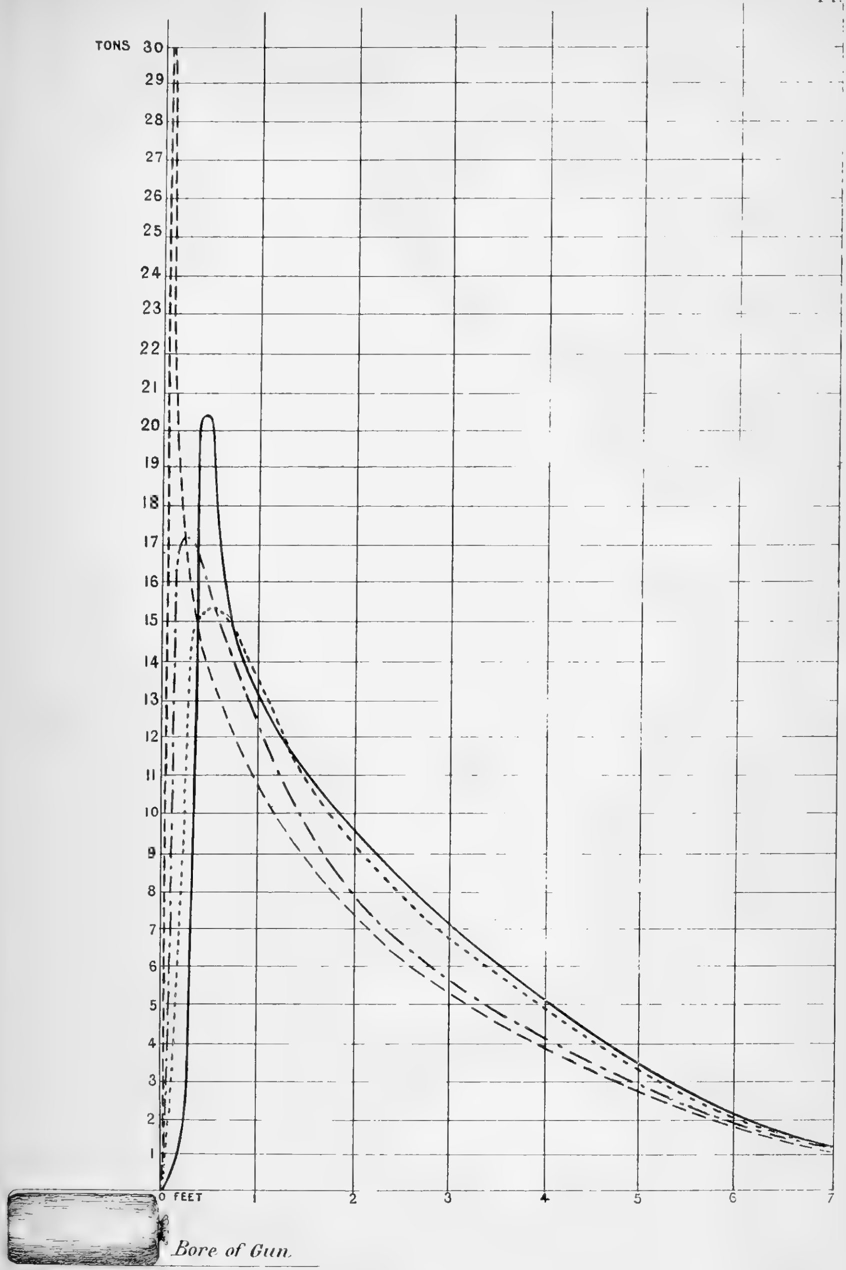
Great difficulties were experienced in securing a simultaneous rupture of the primary wires, and only two methods were found at all satisfactory. One arrangement was to cut the wires by a flat-headed bullet fired from a rifle, across the muzzle of which they were all tightly stretched; in the other they were all wound round a detonating fuze, the explosion of which severed them almost instantaneously. A number of the observations thus obtained are given in the Report, and the errors—including those due to the impossibility of obtaining an absolutely instantaneous rupture of all the wires—seldom exceed 0·000003 second, while the maximum error is only 0·00002 second!

In addition to the holes tapped to receive the cutting-plugs already described, the gun is also bored to take a number of Rodman or "crusher" gauges. When any of these holes, which are twenty-one in number, were not required in the experiments, they were filled with solid steel plugs. The Rodman pressure gauge is shown in position in the gun (Plate II.): it consists of a piston, working in a hollow screw plug open to the bore, the outer end of which carries a pointed knife, against which a piece of copper is placed. When the gun is fired, the gaseous pressure on the base of the piston forces the knife into the copper, and the indent is a measure of the pressure which has acted on the base of the plug. In this instrument the gas has a considerable space to travel between the powder chamber and the piston; thus, before reaching the latter it attains a high *vis viva*, especially in quick burning powders, and acts upon the piston more like a blow than a pressure, and the records are therefore much higher than should be the case.

To remedy this defect the "crusher" gauge was devised by the Committee (see Fig. 3, Plate I.): the reduced dimensions of this instrument allow it to be placed so close to the bore of the gun that the gas has no space to travel before


Pl. II.
CHRONOSCOPE
Scale 1/24
SIDE ELEVATION
PLAN
Note. To avoid Complication only one Induction Coil and Cell is shown, a Cell and Coil being required for each Disc.
FORWARD VENT
REAR VENT
Note. Large figures denote Nos. of Plugs.

Pl. III.



Chronoscope Pressure Curves.

R. L. G. = — — — — — — — — — *Pellet* = — · — · — · — · —

Prismatic = ——————— *Pebble* = · · · · · · · · · · · · · · · ·

Horizontal Scale $\frac{1}{2}$ Inch = 1 Foot

Vertical Scale $\frac{1}{5}$ Inch = 1 Ton

reaching the piston. It consists of a screw-plug of steel, having a movable base which admits of the insertion of a small copper cylinder, B. One end of this cylinder rests against an anvil, A, while the other is acted upon by a movable piston, C, which is kept tight against the cylinder by the spring, *i*. The cylinder is retained in the centre of the chamber, *c, d, e, f*, by a small watch-spring. A gas-check, D, is inserted against the lower extremity of the piston, and should any gas get past this there are passages by which it can escape into the open air. Upon the explosion of the charge the gas, acting on the area of the piston, crushes the copper against the anvil, and, the amount of pressure required to produce a definite amount of compression of the copper having been determined by previous experiments, the pressure on the piston is at once ascertained. The area of the copper cylinders used in the 8-inch gun was 1-12th of a square inch, and that of the piston 1-6th of a square inch.

We have stated that four varieties of powders were chosen out of a large number for further experiment: these were—R.L.G. service powder, pellet service powder, Russian prismatic powder, and "pebble" powder No. 5. When fired from the 8-inch gun, in charges which best suited each kind of powder, the following results were obtained:—

Nature of Powder.	Charge.	Muzzle Velocity.	Maximum Pressure.
	Lbs.	Feet.	Tons.
R.L.G.	30	1324	29·8
Russian prismatic .	32	1366	20·5
Service pellet . .	30	1338	17·4
Pebble No. 5 . .	35	1374	15·4

From this table it is evident that the Service R.L.G. is far inferior to all the others, while the pebble is manifestly the best.

"Pebble" powder, so called from its resemblance to small black pebbles, was first tried in Belgium, but the powder which gave the above satisfactory results is an improvement on the foreign powder, being more uniform in size and density. It consists of irregular cubes, having edges, from 5-8ths to 4-8ths inch in length, made by cutting up the "press-cake" into the required form; the powder is as usual glazed in a revolving barrel, which operation removes the sharp edges. Its manufacture is therefore very simple, little or no new machinery is required for its production, and it is cheaper than any of the other descriptions. Its density is high, about 1·8, but owing to its large size and comparatively uneven surface it is a quick-lighting powder: the whole charge (if of the proper form) is quickly and uniformly lighted, and the maximum pressure in the powder chamber is consequently even throughout its surface, while with powders which are both quick lighting and quick burning, like the old L.G. and R.L.G., intense local pressures, varying in different parts of the chamber, are produced. As an example of this the following results obtained in the 10-inch gun its smooth bore state are given.

Powder and Charge.	Rounds.	Muzzle Velocity. Feet.	Pressure per square inch by Crusher Gauge at A. Axis of Bore. Tons.	B. Centre of Charge. Tons.	C. Front of Charge. Tons.
L.G. 60 lbs.	6	1273	49	28	29
Pellet 64 lbs.	3	1377	$21\frac{1}{2}$	21	21
Pebble 70 lbs.	6	1435	22	22	23

The explanation of these local pressures caused by quick burning powders is very clearly stated by Captain A. Noble, in a paper read at the Royal Institution, on the "Tension of Fired Gunpowder," and may be expressed briefly as follows:—The products of combustion of the first portion of the powder inflamed, in travelling from one end of the chamber to the other, attain a very high velocity before meeting with any resistance, and the re-conversion of the *vis viva* thus acquired into pressure at the base of the shot and the end of the bore, gives rise to the intense local pressures at those points, while the rapidity of combustion of the powder at that part of the charge is probably enormously accelerated by the tension under which it is exploded. "The time during which these abnormal pressures are kept up must be exceedingly minute, even when compared with the infinitesimal times we are considering; for we find the chronoscope pressure, which may be regarded as representing the mean of pressures of a violent oscillatory character, hardly altered at all, even although the local pressures are increased 50 per cent."

At Plate III. a representation is given of the pressure curves calculated from the chronoscope observations, and in the following table the pressures in tons shown by the crusher gauge are compared with those obtained by the chronoscope in the 10-inch gun.

			Pressure per square inch at															
			A.		B.		C.		Mean of B and C		1.		4.		10.		14.	
Charge.	Muzzle Velocity.	Total Work on Shot.	By Gauge.	By Chronoscope.	By Gauge.	By Chronoscope.	By Gauge.	By Chronoscope.	By Gauge.	By Chronoscope.	By Gauge.	By Chronoscope.	By Gauge.	By Chronoscope.	By Gauge.	By Chronoscope.	By Gauge.	By Chronoscope.
R. L. G. —60 lbs. D. 1·742	1327	4824	51·4	—	32·5	—	26·3	—	29·4	24·3	—	10·5	12·4	8·9	4·8	6·5	3·2	4·5
Pellet—64 lbs. D. 1·677 ..	1374	5164	25·0	—	22·9	—	20·1	—	21·5	22·2	—	10·4	11·2	9·0	5·2	6·3	—	4·3
Prismatic —61 lbs.	1344	4947	18·9	—	17·7	—	18·0	—	17·9	19·3	14·7	12·3	12·0	9·9	7·0	7·0	3·7	4·9
W.A. Pebble — 70 lbs. D. 1·782 ..	1442	5384	20·9	—	21·3	—	20·0	—	20·7	21·9	16·0	13·5	12·2	11·4	9·0	8·2	4·0	5·0

The letters and numbers refer to the plugs, reading from breech to muzzle as in the 8-inch gun Plate II., and it is evident that, with the mild kinds of powder the mean maximum pressure in the powder chamber, and also the pressure at different parts of the gun, arrived at by these two perfectly different methods of observation, agree very closely; while the intense local, or wave, action of the violent R.L.G. is also very apparent. The intensity of this local action depends, moreover, in a great measure upon the length of the cartridge ; if this be excessive these objectionable strains at once begin to appear even in the case of pellet or pebble powder, as has been clearly demonstrated in the the 11·6-inch 35-ton gun, firing 120 and 130 pounds of powder, and also in the 10-inch gun when tried with proof-charges of 87·5 pounds.

The specification upon which pebble powder is received from the trade is very strict. Not only must the powder be very uniform in size and density of grain, but it is also further tested by firing battering charges (35 pounds) of every supply from an 8-inch gun, when the pressure in any part of the powder chamber must not exceed 20 tons on the square inch, and the variations in velocity must be comprised within narrow limits. By thus severely testing the whole of our supply of this new powder, the committee are able to ensure that it shall never depart in any important degree from the required standard. The admirable results obtained in the 8-inch and 10-inch guns with pebble powder have been maintained in all the heavier natures, insomuch that the use of this powder, while materially reducing the strain upon the guns below that caused by R.L.G., has at the same time augmented their power to a very considerable extent, as shown in the following table :—

Nature of Gun.	Charge, lbs.		Mean pressure per square inch in powder chamber.		Muzzle velocity.		Total energy of shot.	
	R.L.G. Lbs.	Pebble. Lbs.	R.L.G. Tons.	Pebble. Tons.	R.L.G. Feet.	Pebble. Feet.	R.L.G. Ft.-tons.	Pebble. Ft.-tons.
12 inch, 25 tons	67	85	23	19	1180	1300	5793	7030
10 ,, 18 ,,	60	70	32	23	1298	1364	4693	5160
9 ,, 12 ,,	43	50	21	15	1336	1420	3094	3496
8 ,, 9 ,,	30	35	30	20	1363	1413	2319	2492
7 ,, 6½ ,,	22	30	17	10	1430	1525	1631	1855

With this, the result of the experiments of the last fourteen years, we will leave the subject, though there are many other points of scientific and practical interest to artillerymen which have been set at rest by the committee's researches, while there are others which they are at the present moment investigating.

At one period, as we have already stated, this country was allowed to fall behind some foreign nations in the all important question of powder for heavy guns, and the capabilities of our magnificent Naval Ordnance were sacrificed, and their endurance endangered, by the use of a variable and violent powder. All this has now been set right, and it is satisfactory to feel that our ships and forts are not only armed with the best guns in existence, but that they are also being rapidly supplied with a powder in every respect suitable for their use.

NOTICES OF BOOKS.

Report on Spiritualism by the Committee of the London Dialectical Society; together with the Evidence, Oral and Written, and a Selection from the Correspondence. London: Longmans and Co.

The London Dialectical Society is an association professing perfect liberty of thought and speech. It was designed for the free discussion of every topic—social, political, and religious—and the hearing of all opinions and theories, however strange or heterodox, subjecting all equally to the ordeal of open debate, in confidence that the conflict of minds will promote the advancement of the truth, whatever that may be.

A creed so novel and strange as that of Spiritualism could not expect to escape the ordeal of examination by such a society. But too little was known of it to provide material for debate, and therefore a committee was appointed to inquire into the subject and to report upon it to the Society. The members of that committee were fairly selected, comprising a few votaries of the faith to be investigated, but an immense majority of positive unbelievers, and of those who knew nothing at all about it. But it comprised a great variety of minds and callings—men of science, literary men, lawyers, men of business—the whole being a jury of more than average qualifications to pronounce a fair verdict.

The committee resolved to divide their labours into two parts: the first business was to receive evidence of the experience of witnesses; the second duty was to examine and test the phenomena personally by the aid of their own senses and intelligence.

The General Committee took upon itself the work of examining the witnesses who might tender themselves for or against the new faith, and no difficulty was found in procuring them. Careful notes were taken of the examinations and cross-examinations, and the business was strictly limited to this. In addition, a great number of letters on both sides of the question from persons of distinction, scientific and social, were received, and these, with the reports of the *viva voce* examinations, occupy the greater portion of the volume.

We must say that in our judgment it was a mistake to have printed anything more than narratives of facts: the committee were appointed to inquire into phenomena, not to investigate causes. A considerable part of the Appendix is occupied with mere opinions, many of them ingenious, but some ridiculous and wild, which can interest nobody and are utterly worthless. In any future edition these should be omitted.

After all, the evidence is only the assertion of so many individuals that each had witnessed something which he describes, and carries with it only the weight of one voice. But the Investigation Sub-Committees are of vastly more importance, for they were appointed to witness the alleged phenomena, and to apply to them such tests as ingenuity might devise. Five-sixths of their members were entirely sceptical, and went to the work confident that they should discover a delusion or an imposture. They adopted every practical security against deception: they would not employ a professional medium, but looked for, and fortunately found, one in private life—a lady in high social position—who gave them her assistance through the entire of their protracted enquiry; and this lady had never seen any of the phenomena previously to its being accidentally discovered that they occurred in her presence.

Sub-Committee No. 1 was the most regular and persevering, holding altogether no less than forty meetings, of each one of which a careful note was taken, which is published in the Appendix to the volume before us. The Report states briefly and clearly the results of that careful examination.

Although five-sixths of the members were wholly sceptical when they commenced the inquiry, at its close all who had attended the meetings, so as to witness the phenomena and apply the tests, were completely satisfied that the phenomena were genuine—that it was not imposture nor delusion, as they had expected; and they state their conclusions to be:—

"First: That under certain bodily or mental conditions of one or more of the persons present, a force is exhibited sufficient to set in motion heavy substances, without the employment of any muscular force, without contact or material connection of any kind between such substances and the body of any person present.

"Second: That this force can cause sounds to proceed, distinctly audible to all present, from solid substances not in contact with, nor having any visible or material connection with, the body of any person present, and which sounds are proved to proceed from such substances by the vibrations which are distinctly felt when they are touched.

"Third: That this force is frequently directed by intelligence."

It will thus be seen that the Committee of the Dialectical Society, comprising a large body of sceptics, after cautious examination by a different class of experiments from those detailed in this journal, arrived at the same conclusion as Mr. Crookes,—that there is a psychic force that operates upon inanimate matter beyond the bounds of actual muscular contact or connection; that this force is associated with some special organisation in certain persons; and that it is often directed by some intelligence. But to the conclusions of the sub-committee, the objection was taken that possibly their senses were deceived —that they were themselves in the condition to which the absurd and *non*-scientific name of Electro-Biology has been given, and that they saw and heard only what the medium willed them to see and hear. Improbable as was this explanation, it was not absolutely impossible, and it was to test it beyond doubt that Mr. Crookes devised the mechanism described in former numbers of this journal. Wood and metal at least were not capable of being biologised, whatever that may mean; these materials were not subject to the influence of fear or wonder, but could record only the impression actually and physically made upon them. The reader already knows the result of the experiments made with these passionless agents; they are on all-fours with the results obtained by the forty experimental meetings of the Investigation Sub-Committee. Two or three of the tests employed by the Committee are worth recording, to show the care and ingenuity with which those tests were applied.

So long as there was actual contact with the table, even by a single finger, there was no absolute certainty that muscular force was not the motive power. The attention of the Committee was therefore directed to obtaining, if possible, motion without contact. In this they were entirely successful—not once only, but so frequently as to place the phenomenon beyond doubt. The experiment was contrived thus:—The backs of the chairs were turned to the table, and the party knelt upon the seats, thus precluding possibility of contact with the feet. The arms were laid on the backs of the chairs with the hands extended over the table, in full light, so that the slightest motion of any person would be visible to all the others. The hands were thus held at first at a distance of 6 inches from the table—a heavy and large dining table. It moved several times over a space of from 4 to 7 inches. The hands were withdrawn successively to distances of 9, 12, 18, and 24 inches from the table: still it moved as before, and the sounds came from it. Then the party stood round it at a distance of 3 feet from it, holding hands, and in this position the table moved over a space of 2 feet at one lurch, and frequently over lesser spaces. This did not occur on one evening only, but the experiment was repeated again and again with the same results. It must be observed that the psychic was not a paid or professional medium, but one found in private life whose uprightness was beyond question.

The Committee of the Society limited their investigation to the testing the reality of the phenomena. It was no part of their duty to inquire into the causes, and therefore being completely satisfied, although entirely sceptical when they commenced their labours, they contented themselves with reporting the results, and the minutes of each meeting as they were verified by the signatures of those present are published in the Appendix.

This, however, forms the smallest, though the most important and valuable portion, of the Report. It contains also the evidence taken *viva voce* of the experiences of a great number of distinguished as well as undistinguished persons, who describe a vast variety of phenomena very different from those which the calm and vigilant eyes of the sub-committee were enabled to view. Without questioning the veracity of any of those witnesses, it is sufficient to say that some portion of the marvels they narrate are explicable by certain well-known principles of mental action, whereby ideas formed *in* the mind are by the mind projected as it were outside itself, and appear to itself as if they were impressions conveyed by the senses, when, in fact, they are existing only in the sensorium. It may fairly be anticipated that the scientific examination with which this Force will be subjected everywhere, now that its existence is established, will solve also many of the problems in which the more obscure phenomena of Psychology are at present involved, and we shall obtain a clear insight into the causes of some things which now are inexplicable. At all events, the plain duty of Science is to submit to the most careful and elaborate examination and test all those physical phenomena of Psychic Force that are capable of scientific examination.

Following the *viva voce* evidence are letters received by the committee from a great number of persons eminent in science, literature, and art, who state frankly their opinions—some accepting, others denying, the truth of the phenomena. But it is worthy of remark that all who are convinced have become so after personal examination of these phenomena; while all who deny them, without a single exception, have either never seen them at all or have not bestowed upon them the same patient trial as they would have given to experiments in chemistry or magnetism; and it is a striking fact that there is not a solitary case of any person after a patient investigation coming forward to declare that he had discovered an imposture, and to show by what contrivance the trick was done so that it might be imitated by others.

The Editing Committee have been, we have said, *too* impartial in the introduction of opinions, for they have occupied many pages with mere speculations, such as those of Miss Blackwell, which were certainly not within the proper scope of an inquiry that was designed to collect facts, not opinions. Should a second edition be called for, and we hear that the sale has already become very large, we would recommend to the committee to omit all this portion of the work, and so to reduce its bulk and price. It will thus be introduced to a very wide circle of readers, who now can procure it only from the book club or the circulating library, but who would prefer to possess it for reference as well as for reading. It has made a great stir in the world, and must produce results still more important, for this Report is the testimony of a skilful and intelligent body of men to the reality of phenomena which have been neglected only because they have been erroneously assumed to be delusions or impostures. Now that they are proved to be neither, but facts in Nature, they will be seriously examined by thousands, and by that examination truths of the utmost importance to physiological science cannot fail to be elicited.

The Antiseptic System: a Treatise on Carbolic Acid and its Compounds; with Inquiries into the Germ Theories of Fermentation, Putrefaction, and Infection; the Theory and Practice of Disinfection; and the Practical Applications of Antiseptics, especially in Medicine and Surgery. By ARTHUR ERNEST SANSOM, M.D. Lond., M.R.C.P., &c. London: Henry Gillman. 1871.

DR. SANSOM has succeeded in avoiding any display of that enthusiasm generally attached to the investigation of a single agent; the failures as well as the successes in the application of carbolic acid meet with due recognition. Considering the theories of fermentation, putrefaction, and infection that have been promulgated, Dr. Sansom, after much deliberate independent investigation, concludes that the germ theory, notwithstanding its formidable opponents, still holds its ground, if, indeed, it may not be considered to have taken an established position. He leans to the opinion that the active agents in inducing the changes in fermenting masses are vegetable, not animal, structures. The mobile bacteria and vibriones observed in the early stages of organic decomposition are considered to be non-essential to the processes of decomposition, while the fungoid elements play an essential part. In putrefaction the material affords a more fitting pabulum for forms of animal life, and the complications due to the appearance, vital acts, and mutual decompositions of animalculæ, are superadded to make the process still more complex. But merely theorising is not Dr. Sansom's aim; he places in a light clear to all the numerous practical applications of carbolic acid in the destruction of noxious fungi and insects, and in preventing putrefaction; the action of carbolic acid on inoculable virus; on the poisons of infecting diseases, &c. The action of carbolic acid in surgical cases is treated in the most interesting manner, the details of each case being stated, while the *pros* and *contras* are carefully reasoned out. Dr. Sansom says of the action of carbolic acid on putrefactive decomposition that, "Abundant experimentation has proved that carbolic acid prevents the putrefaction of organic substances. A piece of meat soaked in a 1 per cent solution of carbolic acid for one hour entirely resists putrefaction. Gut, skin, &c., in like manner resist decomposition. Animal size and glue in solution mixed with small quantities of carbolic acid are perfectly preserved in hot weather. Albumen precipitated by carbolic acid does not putrefy. A perfectly fresh egg placed in a sealed bottle, whose interior is coated with a thin lining of carbolic acid, may be preserved perfectly fresh for two months, although the bottle contain plenty of air; nearly the whole of the albuminous material is unchanged by the carbolic acid. Meat treated in like manner may be preserved untainted. A sparrow preserved by Lemaire presented at the end of a month no signs of decomposition, its features being as firmly implanted as just after death. Putrefactive decomposition has also been prevented in the cases of fœcal matters and of blood. As well as preventing decomposition in cases in which the process has not yet commenced, it has also been proved that carbolic acid can arrest putrefaction when once it has set in. Thus meat was hung in the air till the odour of putrefaction was strong. It was divided in two pieces; one was soaked in a 1 per cent solution of carbolic acid; the other in a solution of chloride of lime. In a few weeks that soaked in chloride of lime solution was very offensive, whilst the other presented no bad odour. When vessels were lined with carbolic acid, if by chance air were introduced so that volatilisation of the agent could take place, putrefaction commenced; when, however, the substance experimented upon was soaked in carbolic solution no putrefaction took place." Thus it will be seen that the work is essentially practical, what the agent *might* effect having been left out of the question. The medical applications and the toxic action of carbolic acid, not within our province, are similarly treated. Carbolic acid as a disinfectant receives full attention, the deduction from experimental evidence being that carbolic acid is inferior to metallic salts as an antiseptic when water is freely present; but that when it is a question of the *immediate* disinfection of the semi-solid excreta, then a strong solution of carbolic acid, or emulsion of oil of tar, is highly valuable, or a carbolic powder may be employed. We cannot do more than recommend our readers to peruse Dr. Sansom's work, not merely for its special interest, but as conveying clear knowledge of the nature, the action, and of what is required of antiseptics.

Address delivered at the Spring Meeting of the Royal Institution of Cornwall, on the 23rd of May, 1871. By WILLIAM JORY HENWOOD, F.R.S., F.G.S., &c., President of the Institution, Truro: Netherton. 1871.

THIS address is one which might serve as an example to future presidents of many institutions and associations; each page shows an extensive reading and deep research into the matter in hand—chiefly the mineralogy of Cornwall. Mr. Henwood very clearly explains the technical and local terms in use among the miners, and gives a description of the principal geological characteristics of the lodes and elvans. But the most striking feature of the address is the list of works consulted and referred to. The address concludes with an interesting history of the several steam-engines and their boilers employed in pumping water from the mines.

Organic Philosophy. Vol. III. Outlines of Biology.—Body, Soul, Mind, and Spirit. By Hugh Doherty, M.D. London: Trübner and Co. 1871. 556 pp.

Works on speculative philosophy cannot be noticed in a few words, because there is no standard sufficiently recognised to which they can be at once adjudged. Further, links in the chain of reasoning that may seem accurately forged to some, to others may appear as bearing an undue tension. This is the case in this instance. The views of Aristotle and Anaxagoras, which have been current with many schoolmen, seem to Dr. Doherty unsatisfactory, and their division of the subject illogical. Yet all these views may to a certain extent be correct. As far as they deal with things tangible we can say whether they are correct or incorrect; but the moment the imagination is called into play the standard is removed, and the judgment falls back upon individual reasoning. Then the chances of error that present themselves are innumerable, for of the actual working of the mind we know comparatively nothing; the functions of our intellectual nature may in their complexity possess a paradoxical simplicity, but as yet they are practically entirely unresolved. With this saving clause we can proceed to the consideration of Dr. Doherty's analysis of vital unity. "The Body," he says "is a complete physiological aspect of synthetic unity; the Soul is a complete psychological aspect of synthetic unity; the Spirit is a complete pneumatological aspect of synthetic unity; the Mind is a complete noological aspect of synthetic unity." Hence, according to these definitions, we have the physical functions of the body, the spiritual functions of the spirit, the instinctual functions of the soul, and the mental functions of the mind, as the entire functions of vital unity. How nearly the last three divisions are correct is, as has been said, a matter for individual reasoning. Granted that the subject is correctly so divided, Dr. Doherty certainly follows a logical course of reasoning, and his collateral remarks show a most extensive reading, not only in the branch of philosophy of which he treats, but also in the exact sciences.

The book is well worthy mature deliberation. It is the third of a series of five volumes. The first volume is an outline of Episcosmology (the three kingdoms of nature on our globe, *epicosmos*); the second is a general view of Ontology (eternal forces, laws, and principles); the fourth will be an outline of Systematic

Sociology; the fifth a treatise on Dialegmatics, or Biological Methods, in parallel with mathematics, as a science of method. In treating of Spiritual Genealogy under the principal division of Spiritual Biology, the author speculates—"Palæontology testifies that inferior types of animal organisms have preceded superior types in their first appearance on earth, and therefore the latter must either have started into life at once, without progenitors, or come into this world by the procreative co-operation of inferior types. We suppose the latter to be not impossible, although no instance of such a fact has been recorded in history during the last 6000 years; and natural selection with hereditary transmission mark such very small degrees of variation within known limits that we cannot give it credit for the real origin of species. The possible incarnation of superior types by nearly related inferior progenitors is, then, a rational mode of accounting for the terrestrial origin and metamorphic evolution of individuals and species. Pre-existence may account for both the rise and fall of mankind on earth, as well as for the appearance, development, decline, and final disappearance of any collective realm or species of animal or vegetable organism, and thus ultramundane origin is not less important than mundane genealogy in problems of spiritual evolution. If uncultured and indocile races of spirits were incarnated on a large scale during many generations in the families of a highly cultivated and morally refined nation, such a process would eventually bring the nation down to barbarism and ruin. New revelations and religions may disorganise old nationalities, and re-construct them on new foundations of laws and doctrines, as history shows in all past ages; and in either case ultramundane causes, in the shape of extraneous incarnations or spiritual revelations, produce the mundane effects of social and religious evolutions and revolutions. These are problems of collective biology which we shall have to deal with again, when questions of sociogenesis will suggest those of realmogenesis, still more puzzling and complex; for although we may account for the spiritual conservation and progressive evolution of extinct races of mankind by successive incarnations in superior races living on the earth, we cannot easily conceive the transformation of *lost palæontological* types of animal organism and instinct by successive incarnations, since this would not be heterogenetic evolution in outward form alone, but also in organic constitution; a problem of ultramundane as well as of mundane evolution. The Darwinian theory would account for all kinds of creation by mundane variations transmitted to posterity, but that ignores all worlds of life beyond the natural and the lymbic, and pre-supposes the destruction of souls as well as bodies at the death of individuals. . . . Incarnation means the descent of immortal spirit into a mortal body by the process of embryogenesis. Resurrection means the rising of immortal spirit from the mortal body. This is not the vulgar notion of the resurrection of the mortal body as a mass of dust which had long been scattered to the winds." Such reasoning is beyond the scope of, and cannot be criticised in, a mere notice. The full consideration would absorb many pages of this journal, and in the end would but express the reviewer's opinion.

Miscellanies. By JOHN ADDINGTON SYMONDS, M.D., Selected and Edited, with an Introductory Memoir, by his Son. London: Macmillan and Co. 1871.

BESIDES a memoir these miscellanies comprise articles on general subjects, scientific studies, papers on the social and political aspects of medicine, as well as poems and translations from classical authors, all from the pen of the late Dr. Symonds. Dr. Symonds was one of those men who derive their energy from a strong desire of cultivating the beautiful, and who, working quietly and intensely during their lives, leave their biographer to astonish the reading world with the versatility of their achievements. The chief essay is that on the Principles of Beauty, following up Mr. Hay's idea that the harmony of forms can be explained by the proportion of the component angles; that is, a form is beautiful when the space which it encloses can be analysed into angles which bear proportions to each other analogous to those which subsist between the notes of music. The elaborate manner in which Dr. Symonds has worked out this subject is characteristic of the remaining essays. The translations in verse from the Greek Anthology everywhere present the delicate appreciation of a scholar and of a refined mind. This book should certainly be set on the shelves devoted to the lighter literature of science.

Animal Plagues: their History, Nature, and Prevention. By GEORGE FLEMING, F.R.G.S., President of the Central Veterinary Medical Society, &c.; Author of "Horse-Shoes and Horse-Shoeing," &c. London: Chapman and Hall. 1871.

THIS essentially is a chronological history of animal plagues from B.C. 1490 to A.D. 1800. Mr. Fleming endeavours to show the baneful effects of the maladies, particularly those of a contagious or spreading character, on the agriculture of the country, and how much has been lost by neglecting to study these maladies —a study, says Mr. Fleming, in which the comparative pathologist, physician, general historian, agriculturist, or statesman will find much material for reflection. The author presents a volume bearing on every page evidence of the most patient research. Latin, French, and German accounts of murrains that have from time to time appeared are cited chapter and verse, giving the reader the advantage of making fuller reference for himself should it be required. The arrangement is good: given disease or date, the rest is soon found. But besides the history of each plague there are many valuable conclusions drawn by Mr. Fleming as to the probable cause or causes of the pest. Apart from its special value, the book is very interesting as a general history.

Description of an Electric Telegraph. By Sir FRANCIS RONALDS, F.R.S. Second Edition. London: Williams and Norgate. 1871.

THIS is a reprint of a work published in 1823 when telegraphy was in its extreme infancy. Sir Francis Ronalds describes a telegraph now considered as ranking amongst the historical curiosities of the employment of electricity as a means of communication between distant places, being, in fact, an application of a static charge to cause the diversion of a pith-ball electrometer. The interest of the little work is consequently purely historical; but we cannot wonder that the author should seek to claim attention to his share in the introduction of that connecting link between nations—our present system of telegraphy. The method of testing for faults in the insulation of his line is, in its completeness, quite worthy of the present system.

Insects at Home.—Being a Popular Account of British Insects, their Structure, Habits, and Transformations. By the Rev. J. G. WOOD, M.A., F.L.S., &c.; Author of "Homes Without Hands," "Common Objects of the Sea-Shore and Country," &c. 700 Illustrations. London: Longmans and Co. 1872.

MR. WOOD is so well known by his former works that anything new from his pen will be certain to find favour; but even were he unknown as an author the work now presented to the public would be sufficiently meritorious to establish his fame. "Insects at Home" is a most comprehensive account of the infancy, maturity, and we may say social life of English insects. Moreover, it is an introduction to the study of entomology; and here the beginner will derive great advantage from the clear definition of the many hard names peculiar to this branch of natural science. The task of identifying an insect is by no means an easy one when, as in many entomological works, a very fair knowledge of Greek is required to render the terminology intelligible; but by clear definition and a system of reference by numbers, the author has effectually surmounted this difficulty. The plan of illustration is novel. As the colouring of the woodcuts throughout the work would have greatly increased the cost, the figures are but slightly shaded, and in many cases only outlined. This admits of the reader colouring the woodcuts according to the description given in the text, affording him by these means an illustrated entomological encyclopædia of great fulness at a low price. The filling-in of the colours by the reader has a further effect—it impresses upon his mind the distinctive features of representative class insects. The mounting, modes of preparation and preservation, and all matters relating to the cabinet, are likely to prove extremely useful to the student and to the curator.

Elementary Treatise on Physics, Experimental and Applied. Translated and Edited from Ganot's "Elements de Physique." By E. ATKINSON, Ph.D., F.C.S., Professor of Experimental Science, Staff College, Sandhurst. Fifth Edition. London: Longmans and Co. 1872.

ANOTHER edition of Dr. Atkinson's work has been called for, and he has availed himself of the opportunity by adding many new illustrations, and much new matter selected from those subjects calculated to take a permanent place in elementary instruction. A larger type has been adopted in this edition, rendering the work still more easy of reference. All that need be said is, that this has been long the standard text-book of physical science.

A Manual of Anthropology or Science of Man, based on Modern Research. By CHARLES BRAY, Author of "The Philosophy of Necessity," "Force and its Mental Correlates," &c. London: Longmans and Co. 1871.

MR. BRAY is sufficiently known as the author of some very pertinent works on psychology to render unnecessary any reference to his peculiarly pointed style—he may, indeed, be termed a common-sense writer. This, when the apparent difficulty of taking a common-sense view of the subject is considered, is certainly more than a mere compliment. The book endeavours to show the Unity of Force, and that all Power is Will Power, conscious or automatic, or, as Mr. W. R. Grove has put it, "Causation is the Will, Creation the act of God."

"Physics and Metaphysics, Physiology and Psychology," says Mr. Bray, "thus become united, and the study of man passes from the uncertain light of mere opinion to the region of Science." The habit of quotation, for which perhaps this author is remarkable, here stands him in good stead, and while it gives weight to his reasoning, prevents that straining after the unknowable, so common with writers on that difficult threefold question of the Whence, Why, and Whither. We are glad to see that Mr. Bray has touched upon the phenomena of will or soul force—in fact, a necessary part of his theory. He says:—"The sun has an atmosphere, the world has an atmosphere, and so has man. . . We all know how we are drawn to some people and repelled by others when we are brought within their sphere. But the character of the emanations around us has never been very definitely determined even where its existence has been admitted. Of its existence, however, there can be no doubt, although it is not recognised by physiologists or medical men except in some cases of disease. . . . . Vital force is so strong in others that they possess a curative power by its transmission in cases where healthy vital force is deficient. Old people imbibe vital force when sleeping with young ones, and all persons deficient in vital force draw largely from those with whom they sleep, causing or greatly increasing rheumatic and other pains. In the transmission of force from brain tissue we not only transmit mental states, but these other forces come more or less under the dominion of the Will, accounting for much in electro-biology, mesmerism, and so-called spiritual phenomena. But these are abnormal states; in the natural state the 'released heat taking its character from the tissue,' (Dr. Bird's theory) mixed with more material emanations, forms our personal atmosphere, and people are much more under its influence than they are disposed to credit. Our thoughts and feelings are greatly influenced by those with whom we come in contact, and especially by those with whom we habitually associate, the influence depending upon the particular brain tissue from which the force emanates. . . . . The extent to which we give and take depends upon the constitution. Highly nervous people are very sensitive to the impressions about them. Medwin, writing of Shelley, says: 'So sensitive was he of external impressions, so magnetic, that I have seen him, after threading the crowd in Lung' Arno Corsos, throw himself half fainting into a chair, overpowered by the atmosphere of evil passions, as he used to say, in that sensual and unintellectual crowd.' These phenomena are now illustrated on a very large scale in what are called spiritualist circles. All that is wanted is observation and experiment; but we must look in the right direction for *spirit*, not *spirits*, and for nervous and bodily forces and emanations." Mr. Bray continues, "Professor Owen, writing about what he calls 'thought-force,' says, 'if lines of thought-force were visible, the ghost (of Samuel) would not therefore be more material.' May, then, thought-force, ever become visible? It is evident Professor Owen does not think it impossible. Does what is called by spiritualists 'a medium' supply the conditions?"

Mr. Bray's work should certainly be read. His creed seems to be—know the knowable, *et permitte cætera Deo.*

Rudimentary Treatise on Geology. Part II.—Historical Geology. By RALPH TATE, Assoc. Lin. Soc., F.G.S, Corr. Mem. Acad. Sciences Philad., &c. London: Lockwood and Co. 1871.

THIS little work, partly based on Major-Gen. Portlock's Rudiments of Geology, is one of the admirable Weale's series. This second volume forms a complete epitome of the "History of the British Stratified Rocks." All the principal varieties of fossils are illustrated. The reputation of Mr. Tate as a geologist is a sufficient recommendation that the work will be found accurate in detail.

The Great Pyramid of Gizeh. By A. F. D. WACKERBARTH, F.R.A.S., Professor of Mathematics in the University of Upsala. Translated from the "Tidskrift für Matematik och Fysik." Southampton: Gutch and Cox. 1871.

THE Great Pyramid constitutes a subject which is now being investigated by an increasing number of researchers, and from numerous points of view. The literature of the subject is rapidly growing, and it therefore becomes us, when—as it has already been said in our pages—"theories of such momentous importance are in the balance," not to pass over unnoticed any contribution thereto which may be fairly expected to tend further in the direction of unveiling the still deeper hidden interpretations of realities, which have remained shut up unheeded in perdurable granite and limestone from pre-Abrahamitic ages, down to these latter days, when the light primevally enclosed in thickest masonry of truest form in line and angle seems ready to illumine whole horizons of high antiquity, which have become overlain with multiplied wrappings of misty scales concreted in succeeding darker ages.

Under the principle of founding our acceptance or rejection of theories whenever and by whomsoever propounded, only when they accord or disagree with hard facts, we have examined the latest contribution to the literature of the Great Pyramid, whereof the title appears in the heading of this article. Much pleased, indeed, should we have been if that we could add the work had withstood the test; glad should we have been to sound abroad that its conclusions are founded on a basis as stable as that of the material Pyramid itself; yet in a sentence it must be said, that as the root is planted in fiction, it foreshadows for itself as certain, and like unto that of all half-done work, a precarious and at most but an ephemeral life.

Yet such an assertion on our part is valueless without evidence of being rightly founded. We will, then, endeavour in a few succeeding pages to show forth the true character of the work which has caused us to conclude as aforesaid.

The very opening sentence of the work declares that "the number of Egyptian Pyramids amounts to *several hundreds*," and that the largest and oldest amongst them is the Great Pyramid of Gizeh. How utterly contrary to fact the former part of this assertion is may be gathered by reference to page 189 of the preceding volume of this journal, and to the writings of Howard Vyse, who explored the Pyramids more extensively than anyone else in modern times. We quote his words:*—"The Pyramids of Middle and Lower Egypt are *thirty-nine* in number. They are situated on the western side of the river, and chiefly on the desert hills which form the western boundary of the valley of the Nile."

	1	was in the	Nome,	Latopolitis.
	33	,,	,,	Memphitis.
	2	,,	,,	Heracleopolitis.
	3	,,	,,	Crocodilopolitis.
Total	39			

In order to make up even this small total number there are included many which were originally small and unimportant, and now become mere rounded rubbish heaps of decay-stricken bricks or unwrought stone. Had there been so many as the aforesaid "hundreds," they must necessarily have become a long-maintained Egyptian institution, and it would have occupied the Egyptians all through their history to build them. But quite contrary to that, it is abundantly proven that the idea of a Pyramid was introduced to the land at once perfect, full, and completely developed; it was to some extent, never entirely, never in anyone of its essential characteristics, imitated by a few succeeding generations, in perpetually descending dimensions and order of construction, until finally, and that long before the zenith of ancient Egypt, when both kings and subjects had wealth and power enough to erect any and every kind of building for themselves, Pyramid building had altogether ceased among them. Hence the Pyramids of Egypt occupied but a short period only of the early history of that people, just as they stretch over but a small portion of the earlier settled part of the country—namely, from the apex of the Delta to about fifty miles south thereof.

It is stated in the work before us, that in the lowest chamber of the Pyramid there "is a well formerly supplied with water from the Nile." The fact, however, is that this hole is not a well, but merely a shaft sunk by Howard Vyse and Perring, in 1837, in their search after a lower chamber; and as the bottom of this shaft does not reach down to the level of the highest Nile inundations of the present day, when they are hypsometrically 10 feet above their ancient rise, it is perfectly clear, that whereas the subterranean chamber is near 40 feet higher still, the waters of the Nile could never have reached it by natural flow or

* Pyramids of Gizeh, by Colonel Howard Vyse, vol. iii. p. 1.

infiltration. In the same breath is another mystification, thus—"the chambers are all destitute of hieroglyphs." With somewhat of astonishment would the years'-stricken author of "The Monumental History of Egypt" hear this! And what would the shades of Howard Vyse declare could they address us? And Richard Lepsius too, with the very first plate in his world-famous "Denkmaeler," exhibiting some of the very hieroglyphs themselves as they were found by Vyse, painted in red minium, in some of the "chambers of construction?" Strangely significant is it that in the sectional drawing of the Pyramid forming part of this Swedish work, the essential *five* "chambers of construction" above the "King's Chamber" are omitted. Among the most important testimonies which the entire building contains are these particular hieroglyphs, and notably among them the cartouche of Shufu; they not only confirm the date of the building and name of the king by whom, Herodotus tells us, the Great Pyramid was built, but more than that, they show that the date made out on astronomical grounds is true also. The hieroglyphic, historic, and astronomical dates all closely agree to something near about the year B.C. 2170.

Proceeding onwards we are assured that "this singular theory (*i.e.*, the modern metrological and esoteric theory), was first broached by a *Medical* (*sic*) Professor of Oxford, Dr. Greaves." In his "Pyramidographia," however, published in 1647, the same John Greaves announces himself as "Savilian Professor of Astronomy at Oxford," and this is confirmed, too, by the learned Dr. Hooker: whilst as if to destroy altogether at one blow the labour of modern researchers, it is asserted that "this" (Professor Greaves's) "view has been lately revived by the Astronomer-Royal for Scotland, Professor Piazzi Smyth," which, however, as being so excessively wide of the fact, any candid reader may be satisfied of by a careful examination of the last-named Professor's "Life and Work at the Great Pyramid," wherein doctrines exactly opposite to those of Greaves's are advanced.

As concerning the size of the monument, at page 5 of the dissertation, is to be found a host of erroneous statements, far too numerous to receive attention individually within the limits of a mere review, as to what the Scottish Astronomer-Royal has propounded. We may instance the following:—"He is obliged to make a number of arbitrary assumptions, among which we signalise his assumption 'that the ancient Hebrew standard of length or *sacred* cubit was exactly 25·025 English inches, equal to our 10-millionth part of the earth's polar semi-axis.'" M. Wackerbarth seems lamentably ignorant, if in strict sincerity he so writes. Has he acquainted himself with the directions left to his successors by no less a philosopher than Newton, who some 150 years ago wrote how the true measures of the ancient cubits, both *sacred* and *profane*, would eventually be found by better admeasurements of the stones of the Pyramid? Has he studied the several indications which the stones give, indeed, of two widely distinct cubits having been employed? One, by numerous indications giving a value slightly on the + side of 25 inches, or so close to 25·025 inches, that that particular length is marked out by no less than five different representations! The other by as many, indicating 20·7 as the length of the common or profane cubit; whilst both are doubtless identified in many more representations now gradually being unfolded! M. Wackerbarth, too, may be surprised to learn, that whilst indications of the linear value of these two cubits have been hitherto found scattered at different parts throughout the structure, very lately they are discovered to be laid up together in a specially-set-apart portion of the ante-chamber, with their subdivisions as plainly marked as in an ordinary 2-foot rule; nevertheless the two cubits are so strikingly placed there, that whilst both visible to simultaneous contrast, they are still separated as the *sacred* and *profane* ever must be, even so that they cannot touch each other.

On the same page as that from which the preceding passage is quoted we find it stated, that "Professor Smyth, moreover, assumes, in the teeth of all that is historically known about the Pyramid, that, though built before the age of Abraham, it is not an Egyptian but a Hebrew building," which is simply not true; as anyone may be assured of by turning to "Life and Work," vol. iii., Div. 3, chaps. 2 and 5, on the Egyptian Quarry Marks and Egyptian History of the Times of the Great Pyramid.

Further on it is stated that Professor Smyth, instead of using the mean of the base-length quantities ascertained by him, assisted by Messrs. Aiton and Inglis (which quantities, be it remembered, were from the very nature of the circumstances under which they had been ascertained, at best but uncertain approximations), adopts a length of 9142 inches, and in another place 9166 inches, because to bring out two different results which the theory involves, these two values are called into requisition; all of which in mild expression—is a most ununjustifiable misrepresentation of what the said Professor has set out. It would be well that those who have not seen sufficient as yet to accept the modern theory of the Pyramid, should remember that the several individuals who have worked at its evolution have not attempted to assert anything except in cases where the chain of evidence leads up unbroken to a definite index, where doubts exist, as they certainly do, in regard to the length of the originally perfect base side, the probabilities of certain eventual results have been discussed, and no one has more openly confessed that than Professor Smyth, by publishing to the full *all* the observations of the only accurately marked base-side features, viz., the "socket corners," which he has been able to collect; and finding them to differ from 9102 to 9168 inches, has admitted the difficulty of drawing at present any final conclusion. Professor Smyth has furthermore pointed out on what grounds he at the time of writing deduced 9142 inches as the more probable length, but with an uncertainty about it of ± 25 inches nearly. And who will come forward to *show* (not merely *assert*) that Professor Smyth was not acting aright, in not putting implicit trust in the mean value evolved from the joint measures made by himself with Aiton and Inglis, especially when those made by the Royal Engineers in 1869 gave 9130 inches, or a value immensely closer to his deduced quantity of 9142 than 9110 is. When, however, M. Wackerbarth takes the mean between the Royal Engineer's measure of 9130 inches and the aforesaid 9110, or 9120 inches as being the real length of the base-side, and totally ignores the more carefully measured quantities of the French Academicians—Colonel Howard Vyse and Perring—respectively 9163 and 9168 inches, he himself, on his own showing, tampers with the published measures to confirm a theoretical result of Sir Henry James's long since exploded; according to which it was sought to prove that the base contained 500 Greek cubits of 18·2415 inches—a cubit, indeed, that was never heard of by any ancient Egyptian, nor known in Egypt, until 1500 years after the Pyramid was built; declared, too, in total disregard of what Herodotus told his Greek audience, viz., that the Egyptian cubit was the same as the Samian, Asiatic, or Persian cubit, which so far from being anywhere near the assumed quantity of 18·2415 inches, was 20·7 inches nearly; and this corresponds completely with what Sir Gardner Wilkinson and other modern Egyptologists have ascertained. More than remarkable, too, is it, after what our author has previously said, that he should even be found to testify to the same important truth. For in his table of Egyptian measures (page 15), evidently reprinted from an abstract of a former paper, published in the "Proceedings of the Royal Society of Edinburgh" (vol. vi., p. 235), he distinctly sets forth the cubit of 20·699 inches as a grand culminating quantity. The immediately preceding measure being an alleged equivalent of the Greek Σπιθαμη' = 9·61925 inches, the supposed cubit of 18·2415 inches not being even acknowledged as a possibly distinct standard quantity. It is almost useless to add that such a position is too glaring to need comment.

In paragraph 2, page 6, M. Wackerbarth alleges, that whereas Professor Smyth had only rough stone steps to measure from, he nevertheless "by taking means, and then again means of means, of different measurements of the angles of the remaining stones," made his measurements being out for the side angle of the Pyramid, 51° 51′ 14·3″ = hypothetical π angle. We require to answer such an assertion by merely stating that anyone who will take the trouble to refer to "Life and Work" (vol. ii., pp. 165—176;

vol. iii., p. 28), will find that Professor Smyth never attempted any such unscientific or blundering procedure; but from several sources (all of which gave values of the angle of rise due to the true π angle), found that the residual features of the building afforded evidence of that particular angle in preference to any other. On this same page also it is stated, that "Sir Henry James has completely solved the mystery of the Pyramid's gradient angle, by the simple observation that the corner lines rise 9 units in height for every 10 units of horizontal distance along the angles." It is true, indeed, that in the pages of a contemporary Sir Henry James did try to make out such a case, but it does not appear to have been known in Sweden that that particular attempt had not been recognised as successful in this country.‡

With regard to what indications the Pyramid has been shown to afford concerning the mean value of the solar radius-vector—most notably, too, just at the time when astronomers are divided into two mighty opposing armies on the point, waiting to settle their differences on the occasions of the coming transits of Venus,—M. Wackerbarth seems unaware that that particular feature was evolved by W. Petrie, although he ascribes it to Professor Smyth, and omits to notice how the discussions of Powalky in 1864, and Professor Simon Newcomb in 1867, point out W. Petrie's Pyramid quantity; and that it falls right in the middle of all the values declared by no less than thirteen of the foremost modern astronomers.*

Touching the coffer, a statement purporting to be a description of this vessel is given on page 7, full to overflowing with misstatements and omissions of facts published by various observers, and not by any means free from abuse of the vessel itself for any scientific purpose, by reason of what M. Wackerbarth supposes to be monstrous irregularities of figure. These irregularites are, however, so proportionately small, that they had escaped the notice of all observers prior to Professor Smyth; and although it is sounded in high tones, re-echoed from these observations alone, that no one of the surfaces is "plane," and attempts are made to show them in error by an amount really hideous to an accurate mind, yet the unalterable facts remain. And they assert that the errors in the plane of the east side of the coffer are in height absolutely invisible, and in length over a run of 91 inches, under 0·02 of an inch, or almost certainly within the probable error of the measuring scale, and not the slightest notice is taken of what later observers have shown as qualities—nay, commensurabilities due to the particular irregularities of figure in question. Nor is it even hinted at that certain others—and notably Captain Baker, R.E.,—attach first-rate importance to the concavity of three sides, and the flatness of the fourth, or eastern one.

Pages 8, 9, and 10 contain misrepresentations far too numerous

* See Proc. Phil. Soc., Glasgow, vol. vii.

‡ See "Papers on the Pyramid," by St. John V. Day. 1870.

to mention and refute. The following may therefore serve as an example of the whole. Professor Smyth's measurements of the entrance passage, and the accuracy of the angle thereof, are sought to be invalidated by the large differences among the observations of other travellers. But if the account of observations made by the Scottish Astronomer-Royal be searched, it will be found that these were at least ten times more numerous than any of his predecessors, and made with far more powerful instruments too. Next, the astronomical explanation of the angle of the entrance passage begun thirty-five years ago, by Sir John Herschel, and completed by Smyth, is most unfairly set forth, and treated as of no moment, because shorn—*first*, of the patent fact of the entrance passage being *in the plane of the astronomical meridian*, even more correctly than many modern observatories; *second*, of the consilient meridian data of both the Pleiades-stars and the Vernal Equinox, with the polar star of the passage α Draconis.

With regard to this last star an attempt is made to show that in the star map, "Life and Work," vol. iii., it is marked as of the second magnitude; whilst in Ptolemy's time, it seems to have been of the third magnitude; and that Sir John Herschel says it is now only of the fourth magnitude. Yet what are the facts? In Sir John Herschel's letter* to Howard Vyse, he connects the star not with the fourth but with the third magnitude; and Smyth, in the particular star-map alluded to, has not entered Draconis as of the second, as M. Wackerbarth states, but of the fourth magnitude.

We must, however, conclude our unhappy duty of pointing out the exact value of this most condensed example of misstatement which it has thus far fallen to our lot to examine. We might, indeed, carry our examination much further, but that is needless; and we close this paper with the feeling that if the supporters of the modern theory of the Pyramid have only such opposition to contend with, it may be matter of congratulation to them that their "enemy did write a book."

The Micrographic Dictionary: a Guide to the Examination and Investigation of the Structure and Nature of Microscopic Objects. By J. W. Griffith, M.D., &c., the late Arthur Henfrey, F.R.S., F.L.S., &c. Third Edition. Edited by J. W. Griffith, M.D., &c.; assisted by the Rev. M. J. Berkeley, M.A., F.L.S., and T. Rupert Jones, F.G.S, &c. Parts I. to III. London: J. Van Voorst.

The rapid progress of Microscopical Science since the publication of the last edition of this well-known book of reference in 1860, has rendered the work of revision and addition abso-

* See Vyse's Pyramids of Gizeh, vol. ii., p. 170 (foot-note).

lutely necessary. This has been ably accomplished so far as can be judged by the small portion at present issued. The introduction, treating on the construction of the microscope, its accessory apparatus, and the methods of investigation employed, has been corrected as closely as possible to the date of publication. The assistance of Messrs. Berkeley and Rupert Jones augurs well for the treatment of such very interesting microscopical subjects as Cryptogamic Botany, the *Foraminiferæ*, and Micro-Geology. The text has been added to wherever needed, and the bibliographical notes extended; further additions, however, might have been made in this department with advantage, as one of the student's greatest difficulties is to find references to books. The plates have been corrected; some, however, which have been re-engraved after those in the former editions by Tuffen West have lost, as might be expected, somewhat of their original delicacy; and the microscopist who is familiar with the style of this accomplished artist will miss the work of an old friend whose place can be to him but ill supplied.

The book, saving in this respect, is in no way inferior to the preceding issues, and still forms a most valuable addition to the library of the working microscopist. Considering the great increase in the number of those who now make use of the microscope, principally owing to the formation of societies in and around London and in the country, it seems a question whether the work might not have been profitably issued at a somewhat lower price than formerly; for, although the cost is spread over a large period by the issue in parts, yet the work is still an expensive one, and beyond the means of many earnest students who might be induced to purchase it if obtainable by the expenditure of a smaller sum.

A Systematic Handbook of Volumetric Analysis; or, The Quantitative Estimation of Chemical Substances by Measure, applied to Liquids, Solids, and Gases. By Francis Sutton, F.C.S., Norwich. Second Edition. London: Churchill. 1871.

Every chemist will welcome the second edition of Mr. Sutton's handbook. So much has been done in chemical science since the first edition was published seven years ago, that the revision of even a standard work has been necessary. Volumetric analysis presents nothing very new, but there are many modifications and improvements in the processes. This system of analysis requires, perhaps, a more extended experimental knowledge of the reaction of bodies upon each other than is required in gravimetric analysis; but given this knowledge, there is an immense saving of time and labour. Dr. Frankland and Mr. W. Thorpe, F.C.S., have contributed largely on the analysis of water to this second edition; and Mr. Herbert M'Leod, F.C.S., Professor of Chemistry and Experimental Science at the Indian

Civil Engineering College, furnishes much information on the analysis of gases. The work is too well-known as a technical handbook to need any recommendation. Mr. Sutton must, however, be thanked for keeping pace with the progress of science.

Life Theories: their Influence upon Religious Thought. By LIONEL S. BEALE, M.B., F.R.S., F.R.C.P., &c. London: J. and A. Churchill. 1871.

THE physical theory of life, despite its many advocates and its present vigorous propagation, seems destined to decline at no distant period. It has to meet an almost insuperable difficulty at its outset; for it appeals directly against all the religious prejudices of mankind, while it holds in view no sufficient recompense—except to those enthusiasts and zealots whose recompense is to be known as its propagators. Moreover, its fundamental proposition is founded on assertion, for although influential philosophers have stated that the non-living passes by insensible gradations into the living, no matter in the state of transition has ever been brought to light. This is the point taken up, and ably so, by Dr. Beale in his examination of the present theories of life. He shows how the present hypotheses of spontaneous generation are supported by only the vaguest conjectures, pointing out the true opposition of the living and non-living, of formative agency and formed matter, and that formative agency is not mere force, but force conquered and regulated. Who would say that force was competent to construct a wheel or build a mill? and yet there are men who hold that the sun's force constructs a worm or a plant. Vitality is not the slave of force, but has ever proved its master. Then, says Dr. Beale:—

"If the phenomena of living beings cannot be fully accounted for by physics and chemistry, it is a question still open for discussion whether or not life is due to the working of some agency or power distinct from matter, and the idea of a much higher power capable of influencing all matter may not only be entertained without inconsistency, but an additional argument is gained in its support."

"*Of Power and Force.*—I beg the reader to consider the vast difference between power, force, and property, for these are quite distinct from one another. Power is capable of activity; it may design, arrange, form, construct, build. Property is passive, and belongs to the material particles, and is no more capable of destruction than the particles themselves. Force differs from property in that its form or mode may be changed or conditioned and assume other forms, and be afterwards restored to the original one. Power may cease and vanish, but property is retained, and force in one form or other is persistent."

If vitality be at all acknowedged it must logically and essentially follow that living matter of all kinds and at all periods differs altogether from non-living matter.

"The transcendant difference is not due to chemical composition or to physical constitution or property, but to the presence and activity of a power which cannot under any circumstances be developed from matter that has not been made to live by the influence of that which is already living."

We have, then, in all living beings two distinct sets of phenomena—*vital* and *physico-chemical;* and while the physical properties always remain, the vital may disappear, never to reappear. But can the living exist independently of the non-living? Here we touch upon one of the most difficult and strained questions of the day—a question which has received considerable examination in the immediately preceding numbers of this journal.

"It must be acknowledged that we are not able to adduce scientific evidence in proof that the *living* can exist independently of the *non-living*, because the only evidence obtainable by us is obtained from and through the material. Such a conception, however, may present itself to the mind, and it seems not unreasonable to believe that *vitality* may after all belong to an order of *activities* or *immaterial agents* of which we can really learn nothing directly by the assistance of our senses. Nevertheless, from the effects of the supposed agency upon matter, we can conceive of it as an actual existing power; and by studying accurately the results of its working, why should we not succeed in drawing a correct conclusion concerning its nature and the mode of its action upon matter?"

"After having studied the phenomena of living matter for a length of time and with all the advantages I could obtain, the conviction has been forced upon my mind that vital phenomena must be referred to the influence of an agency distinct from the physical forces of nature. The hypothesis I have been led to adopt is this. I suppose that there is operating upon every particle of every kind of living matter, a *forming, guiding, directing power* or *agency*, which is constantly at work, being transmitted from atom to atom."

"Do not the words 'physiology,' 'biology,' 'pathology,' 'health,' and 'disease,' imply processes that are not simply physical,—imply, in fact, a psychical factor? In spite of all that has been urged to the contrary, there is not one of the actions properly called physiological, biological, pathological, healthy, or diseased, that can be regarded as wholly physical, mechanical, or chemical in its nature. Thoughtful persons have long felt extremely dissatisfied with the material doctrines of life now so prevalent, and though doubtful concerning the precise terms in which the influence of some non-physical power ought to be stated, have acknowledged that the facts rendered imperative the admission of an agency belonging to an order very different from that in which physical and chemical actions are comprised."

Not to speak of what is desirable as truth, there arises the ennobling contemplation of a spiritual agency—of an all-knowing, all-directing, and everywhere-present Creator. While the physical theory of life cramps the mind and unsettles the faith of the thinking man—

"The doctrine of vitality points in an opposite direction. The mind which contemplates vital power will naturally be led to ponder upon the spiritual. The aspirations of the mind will progressively advance, while the intellect increases in strength, encouraged by the hope that it may succeed in forming some conception of the manner in which ever-present, ever-active power designs, guides, and causes to be carried out the never-ceasing changes in living matter."

Why cannot life be re-called? Because "life would never re-appear unless some power able to overcome ordinary tendencies and capable of setting at nought natural laws intervened."

Anyone reading Dr. Beale's convincing arguments must conclude with him that—

"Vitality is as distinct from matter and material properties as is ever-active mind from the inanimate passive substance which it fashions, and upon which it may impress its own fleeting, and perhaps but momentary, conceptions."

And that—

"A theory of vitality (non-material, psychical) will alone enable anyone to account for the facts demonstrated in connection with the life of all living things. Although an immaterial agency cannot be demonstrated to the senses, the evidences of the working of such a power are so distinct and clear to the reason that the mind which remains unfettered by the trammels of dogmatic physics, and is free to exercise judgment, will not deny its existence."

We have before us in these quotations the opinion of a man not only eminent as an author, but well-known to be a shrewd and careful investigator, who has given years to the examination of the functions of the human frame. The account of the researches as to the construction of living tissue is most interesting. Dr. Beale shows that there are two states of matter in living beings; one manifesting truly vital phenomena, nutrition, growth, and multiplication, while the other is the seat of physical and chemical changes only. It appears that of any living being, but a part of the matter of which it is constituted is really living at any moment, and that in the case of adult forms of the higher animals and man, indeed only a very small portion of the total quantity of their body-matter is alive at any period of existence. The living matter or *bioplasm*, in which wonderful changes occur as long as its life lasts (which changes cannot be explained by physics and chemistry), can be examined at any time, and the principal and most remarkable phenomena can be demonstrated with the aid of a $\frac{1}{12}$th of an inch object-glass magnifying 700 diameters. *Bioplasm* exists in all living beings, and upon it their structure, composition, and actions depend. "There is not," says Dr. Beale, "at any period of life, in health, or disease, a portion of any tissue of man's body the size of a pin's head, with perhaps the single exception of the teeth of the adult and in old age, that does not contain some of this living matter or *bioplasm* in which purely vital phenomena take place. Every tissue may be divided anatomically into elementary parts. Each elementary part consists of the *living matter* or *bioplasm*, and the lifeless formed matter (cell-wall, envelope, tissue, intercellular substance, periplastic matter) produced at the moment of the death of the particles of the first. *Formed matter* accumulates in the tissues as age advances, and thus interferes with the free access of nutrient matter to the bioplasm." In examining tissues under the microscope, it is a very advantageous fact that the bioplasts, the germ of each cell, may be artificially and permanently coloured by an ammoniacal solution of carmine, and thus every particle of living matter in a tissue can be clearly distinguished. And what will be seen, say in a small portion of the thick layer of epithelium covering the papillæ of the tongue, is a number of little particles of living matter, often less than the 1-2000th of an inch in diameter, separated from one another at tolerably equal distances by the material they have produced. These living bioplasts attract through the pores of the lifeless matter already formed by them materials suitable for their nutrition. Thus we can see how the new elementary parts gradually grow up from beneath and supply the place of the old ones which are cast off from the free surface. But these phenomena cannot be explained by physics and chemistry, or without calling in the aid of the hypothesis of vital power. "Elements which have the strongest affinity for one another are separated from their combinations, and, perhaps, made to combine with elements with which they have no natural tendency to unite; and all this is effected, not as we see it done in the laboratory by the skilful chemist after prolonged experience and with the aid of complex contrivances, but silently, and, as it were, by a *fiat*, without any apparatus whatever."

It cannot, then, be said that the matter of the world and its material forces necessarily give rise to the development of life; *life* must be regarded as transcending mere matter and its forces—a later gift of an All-Wise Omnipotence.

PROGRESS IN SCIENCE.

MINING.

Among the many difficulties with which the miner has to contend, some of the gravest are those which beset his attempts to carry shafts through water-bearing strata. It unfortunately happens, however, that the Permian beds, which extend over large areas of our coal-measures, are often so highly charged with water that the operation of piercing them in sinking a pit-shaft becomes a task not only ruinously expensive, but fraught with the greatest danger to life and limb. Nevertheless, the future development of our coal-fields must depend in great measure upon the possibility of winning coal from beneath these newer rocks; and, consequently, unusual interest attaches to any improvements which tend to reduce the difficulties of such work to a minimum. For some time past Messrs. Kind and Chaudron have been successfully engaged in boring deep shafts through watery ground; and a valuable account of their system as at present practised in Belgium has been lately given to the North of England Institute of Mining Engineers, by Mr. Warington W. Smyth, F.R.S. The peculiarity of this process consists in sinking the pit *à niveau plein*, that is to say, in carrying on the boring while the water is "at full level" in the shaft. By this means no pumping machinery is needed during the boring, and hence one of the chief items of expense is eliminated, whilst safety to the workmen is ensured by conducting the operation at or near the surface, after the manner of boring an Artesian well. Mr. Smyth's observations were made on a pair of pits now being sunk by this process in the concession of Maurage, on the north rise of the Bassin du Centre, in Belgium. The coal-measures are here overlain by a considerable thickness of cretaceous and tertiary strata, consisting chiefly of sands, marls, and chalk, which in the upper part hold a great amount of water. The foundation of the iron tubbing in these pits is to be fixed at a depth of 636 feet. The four or five men employed in sinking each shaft work upon a platform about 16 feet below the surface. At this working floor the shaft is nearly 19 feet in diameter; but immediately below, it contracts to 15·4 feet, and is carried down of this size to the water-level, which is situated at a depth of about 98 feet. No attempt is made to remove the water until the dangerous ground is pierced through; but the work is carried on by boring, while the shaft—which is nothing more than a gigantic bore-hole—remains filled with water up to its natural level. The boring is effected in two or more stages: during the first, a cylindrical hole is made about 5 feet in diameter, and when this has advanced to a depth of 30 or 40 feet, the upper part is enlarged to the full diameter of the pit. The cutting-tool, or *trépan*, consists of a horizontal bar of wrought-iron, armed upon its lower surface with steel teeth, and attached to thick rods of pine, which are screwed together and fastened at the upper end, by a strong flat chain, to one extremity of a simple lever, the other end of which has direct connection with the piston-rod of a steam-engine at the surface. By admitting steam above the piston, that end of the lever is depressed, whilst the other end carrying the rods is elevated; the fall of the boring-tools is secured by their own weight. The cutter for the large bore weighs about 16 tons, while that for the smaller hole varies according to the nature of the ground; but in piercing hard rock—such as flint—may amount to 8 tons. When the watery measures are pierced, the iron tubbing is let down into its place. This tubbing is made in short lengths, or rings, each with a flange above and below. The bottom flange of the lowest ring is securely seated on a bed cut in water-tight ground, and is surrounded on the outside by a wall of tightly-pressed moss. Upon this moss, as upon a cushion, rests a ring, which slides over the previous piece, and upon this sliding tube the tubbing is built up ring by ring. The successive rings are bolted together, and sheet-lead is inserted between the planed faces of the flanges, while the annular space between the rings and the wall of the pit is filled in with concrete. The water is then pumped out of the pit, and additional security is given to the tubbing by cutting a lower seat, and building up a few lengths of tubbing, tightly wedged under the moss-box. By this ingenious system several pits have already been sunk, safely and successfully, through very dangerous ground.

In spite of the spread of scientific knowledge, the aid which geology is capable of lending to the miner appears still to be too often ignored. One of the most glaring instances of unscientific mining has recently been recorded by Mr. Bristow, F.R.S. During the prosecution of his duties on the Geological Survey, he lately came upon a spot near Easton, in Somersetshire, where a shaft, with steam winding machinery in full operation, was being sunk in the vain hope of reaching coal at a depth of several thousand feet *below* the lowest strata of the true coal measures. Commenced in the lower limestone shales, the shaft had entered the old red sandstone, which it penetrated to the depth of 112 yards—every yard carrying the explorers so much further from the object of their search. The want of scientific knowledge is nowhere more strikingly seen than in such futile experiments, which can only result in the useless expenditure of capital and in keen disappointment to the speculators.

Some remarks on the prospect of finding coal to the south of the Mendips have been contributed to the "Geological Magazine," by Messrs. Bristow and H. B. Woodward; and, in the same journal, Mr. S. Sharp has cited several instances of sinking for coal in Northamptonshire almost as absurd as that at Easton. In Northamptonshire, however, the borings have been made in oolitic rocks, beneath which the coal, if it exist at all, must be hidden at depths almost unattainable. Yet a proposal has been recently made to renew workings in a shaft which was sunk several years ago at Kingsthorpe, near Northampton. This shaft, after passing through the great oolite, inferior oolite, and lias, entered the new red sandstone, and eventually attained a depth of 967 feet from the surface. The project was then abandoned, but £30,000 had already been expended upon the workings. Mr. Sharp now calculates that workable coal cannot be expected to occur at less than 4000 feet from the bottom of the present shaft, thus making a total depth of about 5000 feet from the surface. Still, this undertaking, unpromising as it appears to the geologist, is not without its supporters among unscientific speculators.

As a contrast to these examples of ill-directed energy, we may point to the results which have recently rewarded the spirited enterprise of Mr. J. C. Dawes, who for many years past has been exploring the borders of the South Staffordshire coal-field. It appears that after seven years' search, at a cost of about £20,000, he has now discovered at the Hales Owen workings a portion of the Staffordshire thick coal, about 14 feet in thickness.

During the past quarter, the Colliery Inspectors have issued their Reports for 1870. These reports may be advantageously compared with the corresponding documents for the previous year. Thus, in 1869, there were 108,000,000 tons of coal raised in Great Britain by 345,446 colliers, whilst in 1870 the production rose to 113,000,000 tons, and gave employment to 350,894 miners. Yet the total number of separate accidents in 1869 amounted to 854, and resulted in 1116 deaths; but in 1870, notwithstanding the greater activity, there were only 830 accidents, resulting in the loss of 991 lives. In other words, one life was lost for every 99,777 tons of coal raised in 1869; but in 1870 not less than 113,900 tons were raised for every life sacrificed. During 1870 there were 56 explosions of fire-damp, whereby 185 deaths occurred; whilt in the previous year, with only 48 explosions, not fewer than 257 lives were lost. It would be difficult to carry the analysis of these reports further without introducing tabular statements unsuited to the pages of this journal.

In the iron-stone mines which are under government inspection, there occurred, during the year 1870, 51 accidents, resulting in 55 deaths. In addition to the statistical information which these official reports contain, there will also be found in them, as usual, much information and many suggestions which cannot fail to be highly valuable to the practical miner.

To a recent number of the "Annales des Mines," M. A. Henry contributes a long paper on the different explosive substances employed in mining. In this memoir he discusses at some length the comparative value and the conditions of safety in the manufacture, transport, storing, and the use of the several explosives which have been introduced as substitutes for gunpowder, namely, gun-cotton, nitro-glycerine, dynamite, dualine, and lithofracteur. It appears unnecessary to give an analysis of this paper; for, although only recently published, it was written upwards of a year ago, and its publication delayed by the unhappy state of affairs which interfered with the regular issue of most of the scientific journals in France.

METALLURGY.

Any mechanical process which may be suggested for superseding the laborious work of manual puddling deserves serious consideration. It may be remembered that, some time ago, Mr. Menelaus, of Dowlais, bestowed considerable attention upon mechanical puddling; but after patient and skilful research he failed to secure satisfactory results. Mr. Samuel Danks, of Cincinnati, now claims to have successfully solved the problem by means of his revolving furnace—a furnace which does not, however, appear to materially differ from some of those previously devised for the same purpose. The fire-grate is supplied with a fan-blast below, and with jets of air from above to ensure perfect combustion of the fuel; whilst a valve serves to regulate the blast, and thus keep the temperature of the furnace under complete control during the process. The gases generated by the combustion are conveyed across the fire-bridge through a cylinder into the revolving chamber. This chamber rests upon rollers, and by means of a toothed wheel may be made to rotate freely. The foundation of the lining of the apparatus consists of a mixture of pulverised iron ore and lime, mixed with water to a proper consistence. Upon this "initial lining," the fettling proper is applied. At first a certain quantity of pulverised ore is introduced and allowed to melt; lumps of ore are then thrown into the molten mass; and, when the liquid has set, fresh pulverised ore is introduced; this process being repeated until the fettling is sufficiently thick. In the experiments at Dowlais, much of the difficulty consisted in producing a suitable fettling; but Mr. Danks asserts that any iron ore containing not more than 5 per cent of silica will answer his purpose. The pig-iron may be charged in either a solid or a molten state. When the iron is melted, the furnace is caused to rotate once or twice per minute during the first five or ten minutes of the operation. A stream of water is injected at a certain point, and a portion of the cinder is thus solidified; this is carried down into the molten iron in a continuous stream. After the temperature has been raised, and the cinder run off, the velocity of rotation is increased, the charge becomes violently agitated, the mass acquires a pasty consistence, and the particles gradually cohere into a ball. During the process some of the rich fettling is reduced, so that the puddled product actually exceeds in weight the pig-iron introduced. It is said that furnaces of this construction are at work with excellent results in several parts of the United States. Reliable information respecting the merits of the invention will no doubt be obtained by the deputation sent to America for the purpose of examining the process, at the instance of the Puddling Committee of the Iron and Steel Institute.

During the last ten years important alterations have been made in the dimensions of the blast-furnaces erected in the Cleveland district. This subject has been discussed by Mr. John Gjers, at the Dudley meeting of the Institute. Although the first blast-furnace in Cleveland was built in 1851 by the late Mr. John Vaughan, there are scarcely any furnaces still in existence in that district which were erected prior to 1859. The old furnaces were built of small size, the earliest having a height of only 42 feet, a diameter at the bosh of 15 feet, and a capacity of 4566 cubic feet. Gradually the dimensions have been increased, and in 1871 furnaces were constructed with a height of 95 feet 6 inches, a diameter at the bosh of 24 feet, and a capacity of 28,950 cubic feet. Indeed, one of the furnaces erected in the previous year held not less than 41,149 cubic feet. In the opinion of the author, the useful maximum both in height and in diameter have been already attained, if not, indeed, exceeded. Within certain limits, however, increase of size leads to increase of make, to economy of fuel, and to improvement in the quality of the pig-iron. The author proceeds to give a detailed description of the Ayrsome Iron Works, on the River Tees. Two furnaces are already in blast, and two others will probably be ready for blowing in the spring. The furnaces are closed by a cup-and-cone arrangement, and the waste gases are carried down to an underground culvert. Each furnace has a height of 85 feet, and a maximum diameter of 25 feet.

A subject somewhat akin to this, but more local in its bearing, was brought forward at the same meeting by Mr. T. W. Plum, in his paper "On Increasing the Height of Blast-Furnaces in the Midland Districts." The four Old Park furnaces, built half a century ago, were each 45 feet high; but a new furnace, 60 feet high, has recently been erected in place of one of these old forms, whilst an additional height of 15 feet has been given to another of the furnaces by carrying up a casing outside the former tunnel-head. Hence there are now two 60-feet furnaces and two 45-feet furnaces working side by side. At the time the paper was read, but little experience had been obtained respecting the comparative merits of the two forms of furnace; but even from the results then in possession of the author, he felt justified in concluding that a considerably increased yield had been effected by the increased height, and that in districts where tender cokes were used a height of 60 feet might, under existing conditions, be safely attained, but not perhaps exceeded.

From some recent experiments on the evolution and appropriation of heat in blast-furnaces where raw coal is employed, Mr. I. Lowthian Bell is led to conclude that in the Ferrie self-coking blast-furnace, which has been worked with very economical results, one-half of the saving of fuel may be referred to the increased height of this furnace, and the other half to the combustion of the inflammable gases in the flues constructed in the upper part of this form of furnace.

Mr. Barclay, of Kilmarnock, has proposed certain improvements in the construction of blast-furnaces, whereby a considerable saving of fuel is said to be effected. An annular flue, concentric with the shaft, is constructed in the masonry near the top of the furnace; and a portion of the gases escaping from the upper part of the charge gains access to this flue by a series of radiating passages. A number of vertical pipes arranged around the furnace serve to convey these gases downwards to a lower level, where they enter another annular flue; and, becoming ignited by contact with jets of atmospheric air, again enter the furnace. The heat evolved by the combustion is thus imparted to the materials of the charge, while the admission of air is so regulated that no oxidising action is exerted upon the contents of the furnace.

Some valuable researches on the composition of the gases evolved from the Bessemer converter during the blow have been undertaken by Mr. Snelus, of the Dowlais Iron Works. Analyses were made of the gases given off at different periods of a blow lasting eighteen minutes, and the author thus seeks to gain some insight into the nature of the process which goes on within the converter. The analyses are presented in a tabular form,* and show the relative proportions of carbon and silicon which are successively eliminated at different stages of the operation.

* See Chemical News, Oct. 6, 1871, p. 159.

MINERALOGY.

An extraordinary discovery of native iron, apparently of meteoric origin, was made in 1870 by the Swedish Arctic Expedition when exploring the coast of Greenland. It was not, however, until the autumn of last year (1871) that the largest of these specimens were brought to Europe. So much interest is connected with this discovery that it formed the subject of a recent communication from the Embassy at Copenhagen to the Foreign Office. From some remarks made by Mr. David Forbes upon this communication when submitted to the Geological Society, we learn that the largest of these masses of native iron weighs not less than *twenty-one tons* English, whilst the next in size weighs about 9 tons. The former is now deposited in Stockholm, the latter in Copenhagen. The iron contains nearly 5 per cent of nickel, with from 1 to 2 per cent of carbon, thus agreeing in general composition with many aërosiderites. Moreover, this agreement is strengthened by the development of the well-known figures considered to be characteristic of meteoric iron, when a polished face of the metal in question is etched with acids. The masses of iron were lying on the shore between the ebb and flow of tide, resting immediately upon basaltic rocks probably of meiocene age, in which they appear to have been embedded; and it is curious to note that these neighbouring rocks contain fragments and disseminated particles of similar iron, whilst the so-called meteorites in their turn enclosed fragments of the basaltic rock. Are we, then, to believe that the iron and the basalt were contemporaneous—that, in fact, we are here dealing with *fossil meteorites*, the relics of a meteoric shower in meiocene times? Such appears to be the view held by some mineralogists, including Professor Nordenskjöld. More cautious in his conclusions, Professor Maskelyne believes that the question of their origin, whether meteoric or telluric, can be decided only by examining the basalt at a considerable distance from the objects in question, and thus determining whether the metallic iron is disseminated through the entire mass of rock or is confined to the immediate neighbourhood of the masses of iron. It should be remembered that the Swedish specimens are by no means the first examples of what have been recognised, with more or less probability, as fossil meteorites.

Another species has been added to the short list of minerals already known to contain vanadium. Herr Frenzel, of Freiberg, announces the discovery of a vanadate of bismuth to be named *Pucherite*, after the shaft where it was obtained. The mineral occurs in very small rhombic crystals, of a reddish-brown colour, with a specific gravity of about 5·9. The crystals appear to be disseminated in tolerable abundance through the impure carbonate of bismuth now being raised at the workings at the old Pucher Mine, near Schneeberg, in Saxony.

Professor Church has published in the "Chemical News" the analysis of a fine specimen of *Pitticite* from Redruth, in Cornwall. Excluding the water evolved at 100° C. as accidental, the mineral contained as much as 37·25 per cent of arsenic pentoxide, with only 35·67 of ferric oxide. Phosphorus pentoxide was present to the extent of 1·39 per cent, and the remaining constituents were sulphur trioxide 7·98, and water 17·71 per cent.

A molybdate of molybdenum has been discovered in the lead mine of Bleiberg, in Carinthia, and described by Professor Hanns Höfer under the name of *Ilsemannite*—a name suggested by the late Von Haidinger in honour of J. C. Ilsemann, formerly of Clausthal. The mineral occurs in earthy or crypto-crystalline masses, of a black or blue-black colour, soluble in water. It contains $MoO_2.4MoO_3$; and has probably been produced naturally by the action of sulphuric acid upon the wulfenite, or molybdate of lead, well known to occur abundantly in this mine.

The same mineralogist describes a new fossil resin from the coal of Sonnberg, in Carinthia, to be termed *Rosthornite*.

Some researches on the felspars have been published by Professor Streng. In addition to the theoretical views which he enunciates respecting the chemical constitution of this group of minerals, he describes the result of his microscopic examination of two or three special felspars. He is thus led to regard the *albite* of Harzburg as a mixture containing 96·34 per cent of albite, with 3·66 of anorthite, whilst the *orthoclase* of Harzburg, although presenting the crystalline form of a potash-felspar, really contains almost one-half its weight of albite. The well-known *orthoclase* from St. Piero, in Elba, is also rich in soda, and specimens viewed under the microscope are seen to contain albite to the extent of at least one-sixth or one-eighth of the mass. It is needless to indicate the bearing which such facts have upon the theory of the constitution of the felspars as elaborated by Tschermak.

Further information respecting the conditions under which the diamonds occur in South Africa have been sent to this country, and will be duly published in the "Journal of the Geological Society." Mr. Tobin, who has recently returned to England, has brought with him a most interesting specimen, exhibiting an aggregation of crystals of diamond, apparently associated with a small quantity of foreign matter, which may perhaps represent the matrix. The specimen was found at Du Toit's Pan, which appears to be the present focus of the workings.

A mineral from Arendal, in Norway, hitherto regarded as a variety of garnet, has been found by M. Damour to be really an idocrase. The same chemist publishes the analysis of a garnet from the Rancho de San Juan, in Mexico.

M. Daubrée calls attention to the recent discovery of masses of phosphate of lime in the South of France. These masses, although extremely unpromising in external appearance, are sufficiently rich to prompt active search for similar substances elsewhere.

In Mr. Collins's recently published work on the Mineralogy of Cornwall and Devon,* we are presented with a valuable account of the many minerals found in our two western mining counties. The author's position as lecturer to the Miners' Association is a sufficient guarantee for the general trustworthiness of the book. Of course its especial value lies in the detailed lists of localities, but its interest is by no means purely local. Among the most interesting parts, we may point to the chapter on blowpipe reactions, and to the tabular schemes by which minerals are classified according to their most obvious physical characters. The second part of the work is really a dictionary of mineralogy, and contains lengthened descriptions of the species arranged alphabetically, and illustrated by ten lithographic plates of crystals, remarkable for the accuracy and clearness of their outlines.

ENGINEERING—MILITARY, CIVIL, AND MECHANICAL.

Guns.—In the Engineering Chronicles which appeared in the "Quarterly Journal of Science," some reference was made to experiments carried out by Government between the Prussian and English 9-pounder field-gun. The Prussian gun is known in Prussia as a 4-pounder, that being the weight of the round shot it carries; but as it fires a 9·5 lbs. cylindrical shell, it should really be taken as a gun of the latter capacity. Last November, some further competitive trials were carried out at Shoeburyness between these guns; the practice being made against four rows of targets, each having a frontage of 54 feet wide by 9 feet high, the rows being placed 60 feet apart, one beyond the other, thus giving a depth from front to rear of 180 feet. At a distance of 2500 yards, the Prussian gun (breech-loader) with common shell, made in ten rounds a total of 144 hits, whilst the English gun (muzzle-loader) made only 107 hits. With shrapnell shells the respective performances were, with the Prussian gun 125 hits, and with the English gun 312 hits. At a range of 3000 yards, the effective performances with common shells were 21 hits and 38 hits respectively, and in subsequent experiments with shrapnell shell, the

* A Handbook to the Mineralogy of Cornwall and Devon; with Instructions for their Discrimination, and Copious Tables of Localities. By J. H. Collins, F.G.S., &c. Truro and London, 1871.

English gun gave still higher performances. From these experiments it has been shown that the Prussian gun had reached its maximum range at 3000 yards, as the curve of its trajectory is very high, whilst the trajectory of the English gun being much flatter is much in favour of the latter.

During the last quarter the new 35-ton 700-pounder gun, known as the "Woolwich Infant," has passed through the last stage of its trials at the proof butts, Woolwich Arsenal. At first the diameter of this gun was 11·6 inches, with which bore it gave very singular and uncertain results as regards pressures and velocities, and it was found not to consume the whole of its powder. In order to remedy these defects the bore was enlarged, by which means it was anticipated that the whole of the charge, being shortened, would be consumed, and that better results would be obtained. In its altered state it was tried, last October, with 110 lbs. and 115 lbs. powder charges and a flat-headed 700 lbs. projectile, when the highest initial velocity obtained was 1355 feet per second with 115 lbs. of Belgian S.G. powder, the highest with Waltham L.G. powder being 1300 feet per second; but as the pressures with the Belgian powder were much higher than those given with the Waltham powder, even proportionately to the velocities, that disadvantage may be considered to more than counterbalance the slight increase in velocity. The carriage for this gun was designed by Captain R. A. E. Scott, R.N., upon his compound pivotting principle. The skeleton of the carriage is of cast-iron, plated with wrought-iron, weighing 11 tons, with gear complete, and measuring 9 feet in extreme length at the base. The gun is carried in a saddle-piece, the ends of which work in slides in the cheeks of the carriage, and have a step arrangement for giving the gun three different planes of elevation. The results were, on the whole, satisfactory, notwithstanding some slight defects discovered themselves, which were due chiefly to the manner in which the carriage was mounted. On the 5th of December, when the last rounds were to be fired previously to its removal to Shoeburyness, a defect was discovered in the steel lining of the bore. We shall have more to say on this subject when we give an account of the further experiments at Shoeburyness, which will shortly be carried out.

Mr. Bessemer has recently published an account of his monster gun, by which the inventor anticipates that a weight of metal may be thrown far in excess of anything that has yet been attempted, combined, at the same time, with a lighter form of gun, requiring the employment of less metal in its construction. To achieve this end, he seeks to consume his powder charge in such a manner as to utilise the whole of its effective force, and, at the same time, to avoid throwing any sudden and excessive strain upon the gun. In the present system a given charge of gunpowder may exert at the moment of explosion a force of 60,000 lbs. per square inch on the chase of the gun, and by the time the projectile has traversed a distance of 10 feet, the pressure may be reduced to a mean of 15,000 lbs. per square inch through the entire length. Mr. Bessemer proposes to substitute for this violent and unequal action a continuous force of only some 3000 lbs. to the inch, maintained upon the shot throughout the entire length of its extended travel along the bore of the gun, hoping to obtain an equivalent duty with a vastly reduced strain. The inner tube of the gun may consist of several thick plates of iron, each bent into a tube, and welded, the inner and outer surfaces being bored and turned so as to receive a series of steel hoops placed on hot, and exerting an initial force on the gun. At the ends of the inner tubes are flanged hoops for the purpose of connecting the several lengths together by bolts. The breech may be secured by a movable breech plug screwed into the end of the tube, and made gas-tight by an expanding metal elastic cap, forming a knife edge on the plug, and forced against a ring of copper, or other soft metal, let into a groove formed around the breech for that purpose. In order that a continuous supply of gas under pressure may be generated and made to act on the projectile as it advances, a cartridge or powder chamber is provided, which fits loosely inside the gun; it consists of a cylindrical mass of steel, in which a large number of small holes or chambers have been drilled parallel to the axis of the gun. From 20 to 100 of these chambers are made according to the size of the ordnance, and varying from 2 to 5 inches in diameter, into which the explosive material is placed. By preference, Mr. Bessemer recommends the use in each chamber of a number of separate charges of powder, separated from each other by diaphragms having a fuze for communicating the ignition, or else parted by a thin layer of meal powder. The quantities of powder in these charges increases at every succeeding discharge of the series, and the intervals of time between the discharges diminish, so as to keep time with the increasing velocity of the projectile, and thus keep up the pressure in its rear nearly uniform throughout its entire movement from the breech to the muzzle of the gun. By this means, it is expected that a projectile may be thrown whose weight may be measured by tons, which clearly could not be effected under the present method of gun construction.

Torpedoes.—At page 292 of our last volume, we gave a brief account of torpedoes adopted by the English Government. The Harvey torpedo has since then fully maintained the high opinion we then expressed regarding it, and it would appear to be the one finally adopted for purposes of attack. The Germans, also, have recently introduced a similar offensive weapon in their fleet, and three boats are stated to be now under construction in Devrient's Dockyard at Dantzic, the destination of which is to place torpedoes under, and thus to destroy an enemy's ship. These boats are built almost entirely of iron, and, being about 60 feet long and only 6 or 7 feet broad, they have nearly the form of a fish. The deck is not flat, but round, so as to be but little exposed to damage from an enemy's shot; and, while employed in active operations, no one will be visible on board. These boats will be steered from the bows; and on the deck, above the rudder, there is a slight elevation to allow the steersman to stand on his feet, and a small opening, about an inch wide, to serve him as a look out. As they are intended to operate close to an enemy's vessel, the armour will be as thick as is consistent with high speed. The most curious part of the invention, perhaps, is, that these tiny screw-steamers use petroleum as fuel, which is contained in a number of iron receptacles in the stern, of sufficient thickness to be impervious to projectiles. The chimney is so small that it can scarcely in any case be hit. The hold for the torpedoes is in the middle of the boat, as well as the quarters of the crews.

Pulverised Fuel.—Many devices have from time to time been put forward with the view of utilising small coal. Amongst others, may be noticed that of pulverising it and burning it as a jet, mixed with air. In the year 1831, one J. S. Dawes took out a patent in this country for applying pulverised fuel to the blast-furnace through the tuyeres; it, however, proved unsuccessful, owing, doubtless, to the fact that the agency of fuel in the blast-furnace is chemical as well as physical. In 1846 a patent was taken out by one Desboissiers for pulverising fuel and blowing the dust into the furnace, but though the conception involved some correct ideas, the machinery was totally impracticable. In 1854 Mouchel suggested the injection of powdered fuel and ores, either separately or together, upon a hearth or inclined plane of a cast-iron box, heated by waste heat from other furnaces. Mushet proposed, in 1856, to use pulverised coal, carried by a blast into a reverberatory furnace to produce the oxidation of the iron. More recently still, Crampton has taken up the subject, and achieved tolerable success in the combustion of powdered coal for locomotive and other steam purposes. The plan, however, which appears hitherto to have been attended with the greatest success, is one by Messrs. Whelpley and Storer, of Boston, at whose establishment pulverised coal is applied to metallurgical and other purposes. A description of this method has recently been given by Lieutenant C. E. Dutton, U. S. Ordnance Corps, in a paper read before the Franklin Institute, from which the following particulars have been taken. One great improvement in Messrs. Whelpley and Storer's process is, that the pulverising and blowing of the fine coal into the furnace is effected by one and the same machine. "Conceive an ordinary blowing-fan with the following modifications." We are now quoting from Lieutenant Dutton's paper. "The box is about 18 inches in diameter, and about the same length. Instead of opening at both ends, one end is tight round the journal. The box is divided into two chambers by a diaphragm, so that, really, we have two fans on the same shaft, and their boxes communicate by a hole in the diaphragm around the shaft. The fan at the closed end of the box is in form and function a blowing-fan. The outer fan is the pulveriser. The coal, fed into the open end of the pulverising chamber, is caught by the swiftly revolving paddles, and reduced to powder, and is then sucked by the fan through the diaphragm, whence it is expelled by the ordinary tangential pipe along with the blast. The coal is fed in the form of coarse gravel; it is delivered as fine as flour. The function performed by this machine is a double one. It pulverises the fuel, and delivers it, along with the blast, into the combustion chamber, by a single and indivisible operation." It has been ascertained from practice that the most suitable velocity for the pulveriser is about 10,000 feet per minute for a point on the periphery of the paddle, which, for an 18-inch pulveriser, would be about 2100 or 2200 revolutions per minute. The feed of fuel must, of course, be determined primarily by the requirements of the furnace, and the minimum quantity that will effect the desired temperature should, in each case, be determined experimentally. The amount of air admitted should be sufficient to float readily the pulverised coal, and no more. If excessive, the increased draught through the pulverising chamber will float out much larger particles than can burn effectively. Generally speaking, it is advisable to keep the supply of air quite small. The 18-inch pulveriser, commonly applied to furnaces, reduces 200 lbs. per hour of anthracite coal, the proportions of size being about like the yield of millstones. It requires about 3½ horses' power to effect this. The same power reduces about 300 lbs. of bituminous coal, a large proportion of it being very fine. The entire yield will burn easily, wafted through a hot furnace. A 42-inch pulveriser, requiring about 15 horse-power, will deliver 1000 to 1200 lbs. of anthracite per hour, 2000 lbs. of bituminous coal, 2500 to 3000 lbs. of quartz, 2000 to 2500 lbs. of top cinder, 3500 to 4000 lbs. of limestone, 900 lbs. of unburnt bone, and 50 bushels of wheat.

Patent Gas.—It is some time since any radical change has been introduced in the manufacture of coal gas. Dr. Eveleigh has recently succeeded, not only in manufacturing a gas purer than is generally obtainable, but he likewise converts the residual products into permanent gas in a manner at once practical and economical. The process introduced by Dr. Eveleigh consists of the distillation of coal in iron retorts, at the comparatively low temperature of 900° (Fahrenheit). The hydrocarbon oils go over with the gas, and they are carried together to a condenser, where the oily matters are rapidly condensed, the gas passing on to its own condenser and purifier. The hydrocarbon oils are collected and passed first into a heated pan, where they are re-vapourised, the vapours being conducted to a re-distillation retort charged with charcoal, and heated to a temperature of about 1200° (Fahrenheit). By passing these vapours through charcoal, it is found that they are decomposed and converted into a permanent gas, which, however, is of a lower illuminating power than that produced directly from the coal. This secondary gas is passed through a condenser, from whence it is conducted through a receiver containing the primary gas with which it is mixed, the union of the two giving a gas of very high illuminating power. Dr. Eveleigh's experience during a long course of trials at his works is, that the retorts, the heating pans, and the re-distillation cylinders require only two-thirds of the quantity of fuel employed in the ordinary process, owing to the superior quality of the coke produced. As regards the results of working, he finds that 1 ton of Pelaw Main (Newcastle) coal, without the aid of Cannel, produces 11,000 cubit feet of 18-candle gas, with only 2 grains of sulphur in any form in 100 cubic feet, or about one-twentieth of that usually found. By slightly increasing the distilling heat, 12,000 cubit feet of 17-candle gas can be obtained from the same quantity and description of coal as above, but with a slight increase of sulphur, not, however, exceeding 5 grains. The yield of oil is found to be twenty gallons per ton of coal, and in its re-distillation two valuable products are obtained in addition to the gas. One is the pitch, which has been assessed at a very high market price; and the other is a drying oil, which is of value for varnishing or painting external work, especially for iron work, either exposed to the atmosphere or submerged in water. There appears to be no difference in the quantity of ammonia obtained, but it is stated to be produced in a better, purer, and more marketable form.

Mont Cenis Tunnel.—We have on several former occasions referred to the operations in connection with this *chef d'œuvre* amongst engineering works, and its course of progress has thus been duly noted. On the present occasion, when we have to record the final completion of this gigantic undertaking, it may not be uninteresting at the same time to note a few of the leading particulars regarding it. On the last day of August, 1857, Victor Emmanuel fired the first blast on the Italian side of the tunnel. On the 25th of December, 1870, the headings from either end met under Mont Fréjus; and the official inauguration of the tunnel took place on Saturday, the 16th of September last, when Italian ministers passed through it from Bardonnèche to receive French congratulations at Modane. The length of the tunnel is 13,364 yards, or rather more than 7½ miles, and its sectional area is 71¾ yards, so that about 960,000 cubit yards of rock had to be excavated and carried to spoil with an average lead of 1·875 miles. The weight of the mass of excavation could not have been less than 2 millions of tons, representing a work of 3¾ millions of ton-miles in the carriage to spoil. The ground cut through by the tunnel may be divided into six zones. 1. The anthracite zone that is first followed in leaving Modane, after traversing 420 feet of loose earth, and which is the most elevated in the order of superposition of the beds. It represents an oblique thickness of 6456 ft. 6 in., corresponding to a real thickness of 3732 ft. 6 in. 2. The quartzite zone, 1251 ft. 3 in. thick, following the axis of the tunnel, and of an absolute thickness of 725 ft. 6 in., the thinnest and best defined of all. 3. The gypso-calcareous zone, of an oblique thickness of 2815 ft. and an actual depth of 1627 ft. 3 in. 4. The upper calcareous zone, which has an oblique thickness of 9104 ft. 10 in., and a normal thickness of 5263 ft. 11 in. 5. The middle zone of calcareous schist which the tunnel traverses for 8563 ft., and the thickness of which is 4950 ft. 6 in. 6. The lower zone of calcareous schist, which extends to the Bardonnèche opening of the tunnel. Its oblique thickness is 11,482 ft. 11 in., corresponding to a depth of 6638 ft. 8 in. The total cost of the tunnel amounted to 65 millions of francs, of which Italy will pay something less than 20 millions of francs, and France will have to pay 25,500,000 francs.

Ballooning.—During the late siege of Paris, no less than fifty-four balloons left that city between the 20th of September, 1870, and the 28th of January, 1871, charged with letters and despatches; the letters thus transported being about 2,500,000 in number, and weighing altogether about 10 tons. Besides this freight about a hundred persons were conveyed from Paris by these postal balloons. The principal dimensions of these balloons were as follows:—Diameter, 51 ft. 8 in.; superficies, 8390 square feet; and contents, 72,240 cubic feet. The balloons were of a spherical form, and were made of highly glazed calico varnished, each being composed of forty gores. The gores were cut to shape with perfect regularity, and were strongly sewn together with a coarse double waxed thread. This being done, the exterior received two coats of varnish, and then as soon as the balloon was dry it was ready for inflation, ordinary coal gas being employed for the latter purpose. In the lower aperture of the balloon was fitted a wooden ring, 2 ft. 7½ in. diameter, which was united to the sheet-iron pipe, 5 ft. long, which placed the balloon in communication with the gas pipes. Above, the balloon was fitted with a valve, consisting of a ring of oak, 2 ft. 7½ in. in diameter, provided with a couple of semicircular valves, kept close by india-rubber bands, and arranged so that they could be opened by means of a cord which passed down to the car through the interior of the balloon. The balloon was enveloped and connected to the suspension ring by strong tarred hempen cords, while the suspension ring, which was 3 ft. 3½ in. in diameter, by 3·2 in. high and 2 in. thick, was provided with *gabillots*, to which were attached the eight cords of the car. The latter was made of wicker work and Indian reeds, and was 3 ft. 7½ in. deep, by 4 ft. 7 in. long, and 3 ft. 7½ in.

broad, whilst the distance between it and the suspension ring was 6 ft. 7 in. The total height of each balloon was 68 ft., and its weight 913 lbs.

Sewage Works.—On the 23rd of October last the sewage irrigation works at Leamington, which have been constructed at a cost of £16,000, were formally opened. The sewage has all been taken by the Earl of Warwick, who has undertaken to dispose of it for a term of thirty years, for which purpose he has laid out a farm of 1000 acres on his estate. The population of the district, according to the last census, was 23,429. In order to raise the sewage, two condensing beam engines have been erected at the pumping station, either of which will pump 1,500,000 gallons in twelve hours; but in ordinary weather it is expected that one pair of pumps will be sufficient. At the preliminary trial, one engine pumped 20,000 gallons of sewage in an hour and a-half.

The Sewage Inquiry Commission appointed to inquire into the question of the best means of disposing of the sewage of Birmingham has recently issued its report. The town of Birmingham stands almost on a ridge of high land, and is remote from any large river. Hitherto almost all its sewage has been drained into the small river Tame, polluting its waters to such an extent, that the Right Honourable Sir C. B. Adderley, through whose estate the contaminated waters run, has obtained an injunction from the court of Chancery to restrain the Corporation from continuing to poison this river, and hence the appointment of the Commission. The population of Birmingham is 345,000, and the number of houses in the borough is 73,200, having an average of 59 persons to an acre on the area built upon. The average dry weather flow is 17,000,000 gallons per day, notwithstanding that not more than 20,000 persons are accommodated with water-closets, leaving 325,000 dependent on the open middens with which the town abounds, and which together cover an area of no less than 13½ acres. Analysis of the water in the wells, from which not less than 105,000 of the population are entirely dependent, shows that it consists in reality of filtered sewage. Thus the Birmingham Town Council have to deal with their sewage in such a way, not only to meet the requirements of the court of Chancery, but also to improve the sanitary condition of the town itself, whilst, as there exist no means of disposing of the sewage in an unpurified state, some mode of purification becomes a necessity. From the inquiries made by the Commission from other towns they report as follows:—1. That the land improves greatly under irrigation. 2. That as a rule, no complaints are made of nuisance arising therefrom. In the few instances in which nuisance has arisen, it has been the result of carelessness in conducting the irrigation. 3. The health of the district where irrigation is carried on is not injuriously affected. 4. Cattle thrive on the irrigated land, and no case of their being affected with entozoa has ever been heard of. 5. No other manure has been found necessary for the crops, and the produce, both in quality and quantity, is very satisfactory. 6. The water, after passing through the land, is purified in a satisfactory manner, and in one case cattle drink the effluent water. As at least 10,000 acres would be required to deal with the sewage by irrigation—an extent of land which could not be procured for that purpose—the Commission recommend that 800 acres of land should be purchased, and the purification of the sewage be accomplished by a simple system of downward filtration. The outlay required for this purpose is estimated at £14,160, and the returns at £9,200, thus leaving an annual deficit of £4900, which would have to be made up by local taxation.

Extensive works are now being constructed by the Native Guano Company at Crossness, with a view to utilise a portion of the sewage of the Metropolis by means of the A B C process. The daily outfall of sewage at Crossness is about 50,000,000 gallons, or more than 223,000 tons; at first, however, the Native Guano Company propose to deal with only about 500,000 gallons in the twenty-four hours, which will be drawn from the culvert through which the sewage flows into the great reservoir; and from this point the sewage will flow through a large pipe into the sump of the engine on the Company's works. From this the sewage will be pumped into the mixing room, where there is a cylinder and four A B C mixing pits, all fitted with mechanical stirrers. The cylinder contains one minute's supply of the sewage, which enters at the bottom, and rising with the A B C mixture, whilst being gently stirred overflows by a trough into the settling tanks. The four mixing pits are fed by troughs from a crushing mill, in which the ingredients of the A B C mixture are pounded up with from 82 to 84 per cent of water. The mixture is kept stirred in these pits, and is drawn from them by gravitation into a small pumping well, fitted with duplicate earthenware pumps, each capable of throwing a quantity of mixture equal to from ½ to 1½ per cent of the sewage to a hopper, placed at such a level that it flows thence by gravitation into the rising main containing the sewage at the point where it enters the mixing cylinder. 10,000 grains of raw sewage are to be mixed with from 10 to 18 grains of the A B C ingredients, exclusive of water. The compound comprises from 2 to 3 grains of crude sulphate of alumina, 3 grains of animal charcoal, 10 grains of clay, and a fraction of a grain of blood. As soon as the mixture enters the settling tanks it begins to throw down the precipitate. Each tank when full is allowed to remain for six hours, and at the end of that time the floor will be covered with a deposit of sewage mud, while all above will be clear water. The water will then be run off and passed through filter beds, from which it will flow into the river. The mud will then be run off into covered acidifying tanks, where, after further settlement, and removal of more water, it will be treated with sulphuric acid, in the proportion of 1 pint of acid to a ton of mud, in order to fix the ammonia. The mud is then dried on an iron floor closely roofed in, the products of combustion from the furnace passing over it, and which, together with the steam exhaled by the mud, is passed into a vessel or tank, where it is made to pass through water in order to remove any noxious properties before escaping into the air. It is computed by the company that the 500,000 gallons of sewage to be treated daily at Crossness will result in the production of 4 tons of native guano.

TECHNOLOGY.

Dr. Jeannel has recorded the results of a series of experiments, from which it appears that food, both animal and vegetable, boiled at 95° is more nutritious and of better flavour than when boiled at or above 100°. The author illustrates this point by referring to the experience gained in mountain localities (every 100 metres' rise above sea level make a difference of ⅓° C. less in the boiling-point of water); as, for instance, at Potosi, at 4061 metres above sea level, and an average barometer reading of 454 m.m., the water boils at 86·2°; at Mexico, 2277 metres above sea level, 569 m.m. barometer, water boils at 92·1°; at Briancon, 1321 metres above sea level, 643 m.m. barometer, at 95·4°; these results are also confirmed by the action of the so-called Norwegian cooking apparatus.

The utilisation of crude petroleum for fuel has attracted the attention of many scientific men. The great aim is to discover a process whereby the tendency to carbonisation should be overcome. This difficulty an American inventor has now overcome. From the "Chicago Evening Mail," we learn that his apparatus consists of a cylinder, like a small locomotive boiler set on end, with a smaller cylinder within it, the intervening space being filled with petroleum. The smaller cylinder is filled with six hundred small copper tubes, and through these superheated steam passes, producing vapour from the oil that fills the interstices between the tubes. This vapourised oil rises through a layer of prepared sponge, and just at the point of exit is mixed with superheated steam in any required proportion, thus producing hydrocarbon gas. This gas passes through iron tubes to the point where the fuel is needed, and is there burned, very much like common gas. In the case shown in illustration the kiln was filled with stone, and in a very short time after the fire was lighted the heat was more intense than can be expressed by comparison. All this time the fire was under perfect control, and by a simple turn of a screw the combustion was made more or less intense. The experiment was varied by admitting a greater or less proportion of steam into the pipes, so that in some cases the fire was fed with fifty per cent or more of water, and the remainder of vapourised oil.

A cheap and good process for the utilisation of leather waste has long been a desideratum. This waste represents millions of dollars annually. A process that could reproduce a texture of these cuttings, only half as good as the original leather, would be one of national importance, and would at once establish a new industry. The "Scientific American" describes a process invented by Mr. P. J. McKenzie Oerting, which is said to make uniformly an artificial leather even superior to ordinary tanned sole leather. Examination of these specimens reveals the following facts:—It is much harder than ordinary leather, and does not yield to hammering or compression nearly as much. It is very flexible and elastic. Thin shavings of it possess as great tensile strength as shavings of equal thickness of common oak-tanned leather. It is nearly, if not quite, impervious to water. It cuts smoothly and easily in working. With regard to its durability under wear, it would probably wear longer than sole leather, as it is said not to decompose or change under the ordinary circumstances of wear to which leather is exposed in its various uses. The method under consideration was first brought out in Copenhagen. The ingredients employed and their proportions are as follows:—For first quality, one pound caoutchouc for each three and a quarter pounds leather pulp. For other qualities, the proportion of leather pulp is increased variously up to six pounds for one pound of caoutchouc. The caoutchouc is dissolved in benzol or other solvents, and, when sufficiently dissolved aqua ammonia is added in the same proportion as that of the rubber, and the mass is thoroughly stirred until it assumes a grayish-white colour. The leather pulp is then added, and the whole is kneaded into a plastic homogeneous dough of uniform consistency, which can be pressed or moulded into any required form, or rolled into sheets, as may be required. The ammonia is said to act upon the animal glue in the cuttings, restoring to it its original properties which it had lost to a great degree in the process of tanning. The following are some of the properties and uses of this remarkable substance, as given by Mr. Oerting:—Its waterproof quality makes it especially valuable for pump leather, as well for cold as hot water, and also for harness, as even a continued exposure to all kinds of weather has no effect on it, occasioning neither rot nor crack. It can be made endless, or of any length, width, and thickness required, and of perfect uniformity as to wear, which is generally well known to be impossible with leather belts made of shorter pieces of different hides, and of unequal wearing capacity. It will stand any amount of heat and friction, as well as the most intense cold, will stretch less than any other belting, and can be changed from one pulley to another with ease and rapidity. It is very strong and substantial in the edge, and will stand a great amount of ill use without suffering any injury, and through its combined properties will supply a desideratum much needed. By suitable machinery for moulding, or forming the material in its doughy state into hose, fire buckets, &c., for which purpose it is especially adapted on account of its flexibility, impenetrability by water, and its capacity to withstand any amount of hardship, as well as extreme heat or cold, it will certainly make the best as also the cheapest material yet produced for such purposes. By a different mixture and proportion of the ingredients, a matting for floor covering is made, which on account of its cheapness, its waterproof properties, and its capacity to keep rooms protected from cold and dampness, makes, it is said, an unequalled article for covering offices, passage ways of public buildings, &c., which will withstand an immense amount of wear, and can very easily be cleaned.

At a recent meeting of the Manchester Literary and Philosophical Society, Mr. John Hopkinson, B.A., D.Sc., detailed some experiments on the subject of the rupture of iron by a blow, the results of which are—1st. That if any physical cause increase the tenacity of wire, but increase the product of its elasticity and linear density in a more than duplicate ratio, it will render it more liable to break under a blow. 2nd. That the breaking of wire under a blow depends intimately on the length of the wire, its support, and the method of applying the blow. 3rd. That in cases such as surges on chains, &c., the effect depends more on the velocity than on the momentum or *vis viva* of the surge. 4th. That it is very rash to generalise from observations on the breaking of structures by a blow in one case to others even nearly allied without carefully considering all the details.

In a paper by Mr. H. Wild on an improved method of filling barometer tubes without the necessity of boiling the mercury and without the danger of breaking the tube, the author describes a method of purifying mercury from zinc and other metals which are not easily removed by distillation. Take 1000 grms. of that metal, pour it into a strong flask capable of holding 2000 grms. of water. Take, next, 30 grms. of a solution of chloride of iron (made up of 1 part of the dry salt and 3 parts of water), add this to the mercury, and, after having closed the flask with a cork, shake it vigorously until the metal is so finely divided that with the naked eye no more globules can be seen. Water is next poured into the flask, and the contents, having been well agitated, are left for a moment to settle, and the impure solution poured of; this operation is repeated twice, after which the greyish mass of very finely divided metal is poured into a porcelain basin, which, having been placed on a water-bath, is made dry, and next brought to its normal state of aggregation by rubbing in a glazed porcelain mortar. The metal is next filtered through good tough writing-paper wherein a perforation has been made with a needle.

Dr. Sézille has described a new process of panification. The wheat is first deprived of its outer cover, or husk, by means of properly constructed machinery; the decorticated grain is next several times acted upon by tepid water at about 80° for the first bath and 40° for the subsequent ones, whereby the gummo-resinous cover of the grain is dissolved and removed. This removal is necessary on account of the fact that this substance becomes very deep brown, almost blackish, coloured by fermentation of the dough; the grain at the same time absorbs from 65 to 70 per cent of water, and is then reduced to a paste by means of machinery very similar to that used in chocolate mills. This perfectly white paste is next leavened, and after fermentation is ready for baking. By this process, from the same quantity of grain which by the usual process only yields 108 to 110 kilos. of bread, the yield is increased to 145 kilos. of very superior quality and far greater nutritive power; moreover, a very considerable saving of labour and expenses connected therewith is effected by the application of this new process, which has been thoroughly tested by competent and independent scientific as well as practical men.

A method of coating metallic objects with a very durable black-brown varnish is given by Dr. C. Puscher. On the bottom of a cylindrical cast-iron vessel, 18 inches high, is placed a layer, ½ inch thick, of coal-dust; upon this is placed an iron grating, and thereon are put the iron, steel, or other metallic objects intended to be coated with the varnish. The vessel, having been first closed with a well-fitting lid, is next placed on a bright coke fire, and heated for about a quarter of an hour just to incipient red heat. The vessel is then removed from the fire, and on the lid being removed, after about ten minutes, the metallic objects will be found coated very uniformly with a good and durable varnish, which resists bending, as well as a high temperature, without cracking or coming off. Very small objects, such as hooks-and-eyes, for instance, are better placed along with some coal-dust in a coffee-roasting apparatus, and this turned, as is usual in the roasting of coffee, until the metallic objects have obtained the desired depth of colour and are uniformly coated with the varnish.

By the use of types made of an elastic material, A. Ae. Wilbaux prints on glass by means of fluoride of calcium incorporated in printing ink; the glass thus printed on is next treated at a suitable temperature with sulphuric acid, and after having been washed with water it contains in an indelible engraving the figures of the types.

An elaborate essay on the pigments and dyes known to and used by the ancients has been published by M. E. Rousset. From the contents it appears that the author's primary object is to prove that the ancients were not so ignorant in industrial matters as is commonly supposed to be the case. Among the white pigments they were acquainted with white-lead, which, according to Pliny, was prepared especially at Rhodes. As black pigments, various kinds of charcoal and soot were used by the ancients, the same as nowadays; while they dyed animal skins black with nutgalls and sulphate of iron. By their acquaintance with the use of various kinds of ochre, and by mixing these, either with each other in various proportions, or with black pigments, brown pigments of various shades were obtained. Under the name of Alexandria blue, the ancients (the author includes in that term the ancient Egyptians, as well as Greeks and Romans) largely used a pigment containing oxide of copper, and they also were acquainted with a pigment containing cobalt. Indigo was not unknown to them, but it was not used for dyeing, their blue-dyed fabrics being obtained by the use of pastel-wood, *Isatis tinctoria*. They used the following yellow pigments:—Ochre, massicot, orpiment, and realgar; the latter are poisonous, and do not cover well; the former are devoid of any brilliancy. As to the yellow dyes used by them nothing is positively known, but it seems that woad, saffron, and other native plants were employed. Referring to yellow pigments discovered by modern chemistry, Naples yellow (antimoniate of lead), mineral yellow (oxychloride of lead), and the chromium colours, discovered by Vauquelin (1797), cadmium yellow, discovered by Stromeyer (1817), are mentioned. Among the modern yellow dyes, purrhee and picric acid are enumerated. Vermillion, red ochres, and minium were known from a remote antiquity, though neither Greeks nor Romans were acquainted with the artificial preparation of vermillion, which has been known to the Chinese for a very lengthy series of centuries past. As to red dye-stuffs, there can be no doubt that madder was known and used not only for dyeing fabrics but also in the shape of so-called lake. Kermes, a dye-stuff little known in this country, but yet used in France, was known to and used by the ancients, being undoubtedly used in Egypt in the time of Moses. Among the green paints the ancients were only acquainted with some native green-coloured compounds of copper, and with the acetates of that metal; the number of green pigments discovered in modern times is very large and need not be here alluded to. Of purple dyes known to and used by the ancients, the celebrated Tyrian purple is here at length spoken of. Among the molluscs from which this dye was obtained is the *Janthina prolongata*, yet found in the Mediterranean, and a very common object in that sea near Narbonne (in this very ancient city there were Tyrian purple dye-works at least 600 years B.C.), where in our days some experiments have been made by Dr. Lesson which really prove that the mollusc just named yields, though only in very small quantity, an exceedingly beautiful purple. Dr. Bancroft was also acquainted with this fact, and made several trials for the purpose of ascertaining whether this dye-stuff might be again industrially applied.

A cheap mode of preparing pure dextrine is given by O. Ficinus. 500 parts of potato-starch are mixed with 1500 parts of cold distilled water and 8 parts of pure oxalic acid, and this mixture placed in a suitable vessel on a water-bath, and heated until a small sample tested with iodine solution does not produce the reaction of starch. When this is found to be the case the vessel is immediately removed from the water-bath, and the liquid neutralised with pure carbonate of lime. After having been left standing for a couple of days the liquor is filtered, and the clear filtrate evaporated upon a water-bath until the mass has become a paste; this is removed by a spatula, and, having been made into a thin cake, is placed upon paper and further dried in a warm place; 220 parts of pure dextrine are thus obtained.

Dr. F. Stolba gives the following directions for cleaning glass vessels wherein petroleum has been kept. Thin milk of lime, used for washing glass vessels wherein this liquid has been kept, forms with the petroleum an emulsion, and renders it possible to remove all traces, and, by the addition of a small quantity of chloride of lime to a second washing with milk of lime, even the smell is taken away so completely as to render the vessels fit for pouring and keeping beer therein; if warm milk of lime be used, the operation, which otherwise takes considerable time for completion, is rendered shorter.

MM. Montéfiore-Lévi and Künzel have published a work on the use of divers alloys, and more especially of phosphorus-bronze, for the casting of ordnance and other purposes. In it is given a detailed account of an extensive series of experiments made on the large scale on the preparation of various alloys of copper and tin, and the effect produced thereon by the addition of phosphorus in small quantity. It appears, on the whole, that the addition of this element is of great value in producing alloys possessed of excellent properties, especially for the manufacture of bronze guns. The Academy has appointed a committee of six of its members, among them MM. Dumas and Frémy, to study this important matter, and report thereon.

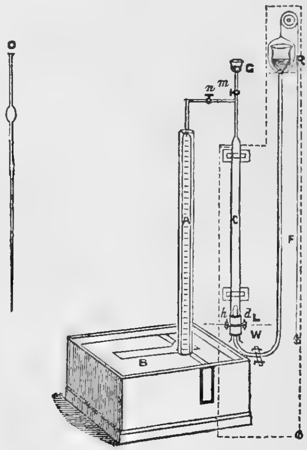
The extraction of animal fats to be used either as food or for cosmetic purposes forms the subject of a memoir by Dr. H. Vohl. The fresh fat is first as much as possible freed from membranes and flesh, next cut up either into small discs or cubes, and then thoroughly washed with cold water, which should contain the least possible quantity of lime, until all blood is entirely removed. The fat is next put into a cylindrical stoneware vessel, 1·25 metres high and 0·5 metre inside diameter, this vessel being placed in a water-bath and provided with a tap at the bottom, so placed that the vessel may be emptied without removing it from the water-bath. The vessel having been three-fourths filled with fat, there is placed on the top of it a stoneware perforated disc, and next very dilute pure hydrochloric acid is poured over it. The stoneware vessel is then closed with a well-fitting cover, and the water-bath heated. From the fat, while melting, the perforated cover carries, by slowly sinking downwards, all the impurities, as far as they are not dissolved by the acid, which at the end of the operation is run off by aid of the tap. The fat is then, while molten, washed several times with warm water, to which, for the last washing, some carbonate of magnesia is added. The fat is next treated with refined petroleum spirit, and the solution separated by decantation from any membranes, &c. which may remain. The solution of the fat is freed by distillation in a water-bath from the petroleum, and the result is the production of a very superior fat, which being absolutely free from water and other nitrogenous organic matter, is not liable to become rancid, and may be preserved for many years.

CHEMICAL SCIENCE.

Dr. J. Lefort has made some important observations on the alteration which well-water undergoes by the proximity of burial-grounds. The contents of this paper bear more especially upon the effect of a decree, dated March 7th, 1808, whereby it was enjoined that no wells should be bored or dug out at a less distance than 100 metres in direction from any burial-ground. The author, having found that not only in many country villages, but also in several towns this regulation was not observed, has made some experiments on the water of a well at Saint Didier (Départment de l'Allier). This locality is situated on an alluvial soil; the water is used for drinking purposes by the parish priest and a portion of the inhabitants, and, on examination, the author found it to contain not only a large proportion of ammoniacal salts, but also, on evaporation, to leave a very large quantity of a dark-coloured organic matter mixed with carbonated salts, which, on being mixed with some hydrochloric acid, gave off an offensive carbonic acid gas, the smell being somewhat akin to a mixture of a concentrated solution of glue and butyric acid. The well is very deep, and the water is quite clear and bright, but exhibits, especially in summer time, a very vapid taste, while, in the warm season of the year, it rapidly becomes putrid. The author comes to the conclusion that in any soil a well dug at the distance of 100 metres from either burial-grounds or battle-fields is certain to be so contaminated with organic and other injurious matter as to make it imperatively necessary to make new and sound regulations on this subject, so as to prevent wells (for obtaining potable water for domestic use) being sunk without a stringent inquiry as to where the water comes from, and through what strata of soil it may have to pass.

Mr. J. Parry has devised and carried out practically a new form of gas apparatus, which possesses advantages over those of Bunsen and Frankland and Ward. The measuring or gas tube, A, is enclosed in a glass cylinder filled with water, and dips into the mercury trough, B. The absorbing and eudiometer-tube, C, is connected with A by a well-fitting elastic tubing of double thickness, firmly wired to the capillary glass tubing shown in the sketch. The use of this apparatus hardly requires to be described. By alternately raising and lowering the mercury reservoir, R, both A and C are filled. The gas for analysis being then bubbled up into A, and measured with the usual precaution. The liquid absorbent required is poured into the cup, G; R is lowered, M opened, thus drawing the test into C, a little being left in the cup to prevent admission of air. The tap, N, is now opened, and the gas drawn into C; both M and N are to be kept closed while the gas is being subjected to the action of the absorbent. The latter must always be run into C as described, or it is drawn into the measuring-tube. To clean C the gas is passed back into A, N is closed, and M opened, B lowered, and the mercury and absorbent allowed to flow out to about the level of L (see dotted line). Unscrewing the nuts H and D, the lower part, W, is taken out, the test, &c., falling into a large basin placed under. While the mercury, &c., is falling down the tube, C, water must be poured into the cup, G, for washing out traces of the absorbent. The eudiometer-tube, C, is well washed with pure water, with the pipette, O (which consists only of an ordinary pipette joined to a long glass tube by a short piece of elastic tubing);

Fig. 8.



it is evident the long tube may thus be conveniently passed up C, and the water blown through and up C, flowing down the sides of the tube into the basin. The part W is made of iron or steel of the form shown; a plug of caoutchouc is firmly cemented into the short iron tube; the platina wires for explosions insulated in glass tubing, also the glass tube (wired to the elastic tubing, F, by which the reservoir, R, is raised and lowered) are passed air-tight through the caoutchouc plug. The flanges, &c., W and L, should be well made with smooth true surfaces; a washer of caoutchouc is placed between the flanges, also the end of C presses down on the plug when the nuts H and D are screwed down. By means of this apparatus analyses are rapidly and conveniently done; explosions are accomplished without difficulty, with far less trouble and risk than with the ordinary eudiometer; the gas for explosion may be expanded to any required extent by lowering R, and any risk of bursting C is thus entirely obviated. Water may be substituted for mercury in the trough, B, but of course, with less accurate results. The eudiometer, C, may be supported by clamps attached to a long iron rod, and R raised or lowered by a pulley.

Dugald Campbell, F.C.S., has recently analysed some ancient Jewish glass obtained from Bessant, secretary to the "Palestine Exploration Fund," and which, that gentlemen writes, "Is all from shafts *round the Temple*, and found at depths varying from 20 to 80 feet," and as its history is truly interesting and its composition is somewhat curious, an analysis may not be out of place in the pages of this journal. The sample of glass weighed altogether about 3 ounces, and consisted of a number of small pieces, many of which in parts had undergone a change both in structure and colour by time and exposure. The portion selected for analysis was from pieces which appeared to have undergone the least if any change, and the results were as follows, in 100 parts:—

Silica	69·30
Lime	8·50
Magnesia	0·55
Soda	13·79
Potash	1·49
Alumina	3·20
Oxide of iron (Fe_2O_3)	2·00
Oxide of antimony	0·29
Oxide of lead	trace
Phosphoric acid	0·80
Loss in analysis	0·08

The specific gravity of the glass is 2·430.

Wagner, in "Versuchs Stationer Organ," xiii., 69 to 75, and 218 to 222, describes the results of his experiments with kreatine as a source of nitrogen for plants. Maize plants grew and developed seeds in a solution in which kreatine was the only nitrogenous substance present. The kreatine was absorbed unchanged, and was detected in the plants. Wagner refers to the fact that Hampe had found that urea is absorbed unaltered by plants. The same observation was made by Dr. C. A. Cameron in 1857, and was reported in the "Transactions of the British Association" for that year, in the "Chemist," for November, 1858, and in the "Repertoire de Chemie, Pur et Appliqué," Paris, December, 1858. Dr. Cameron finds that plants can absorb unchanged, and apparently derive nitrogen from, potassic nitrite, potassic cyanurate, and potassic ferrocyanide.

An examination of the gases occluded in coal forms the subject of a paper by Dr. E. Meyer. Lumps of hard and compact Zwickau coals (Saxony) the size of a walnut were put into a flask, previously partly filled with thoroughly well-boiled and hot distilled water; the flask was next closed with a perforated caoutchouc stopper wherein a glass tube was fitted, which, serving as gas conveying tube, was carried into a vessel filled with water deprived of air. The apparatus thus arranged was kept at boiling heat for some time in order thereby to eliminate the film of air which adheres mechanically to the coal. The boilіng was further continued after the gas-conducting tube had been placed over a graduated glass tube, previously filled with distilled water freed from air and placed on a pneumatic trough. The gas thus collected (constantly evolved from the lumps of coal) was analysed according to Bunsen's method, and found to consist in the first sample operated upon of:—Carbonic acid, 16·9; marsh-gas, 20·4; nitrogen, 53·3; oxygen, 1·7; heavy carburetted hydrogens absorbable by fuming sulphuric acid 7·7 per cent. Second sample—Carbonic acid, 22·4; marsh-gas, 22·3; nitrogen, 48·0; oxygen, 4·1. The large quantity of nitrogen and the small quantity of oxygen deserve special notice. The samples of coal experimented with had been kept in a cellar for a period of several months in contact with air so that it would appear that the oxygen of the air absorbed has served for the oxidation of the coal and formation of carbonic acid.

The presence of manganese has been shown by G. Campani to be a permanent constituent of blood. This subject was taken up by the author in consequence of a paper published in a scientific Italian periodical, wherein Dr. Pollacci asserts that manganese is an integral constituent of blood. It appears from the author's experiments, made with blood of oxen, that the globules as well as the serum contain, along with iron, weighable quantities of manganese.

As bearing somewhat on this subject, we may here record a very delicate test described by Dr. Böttger for the detection of minute traces of manganese. A few grammes of chemically-pure chlorate of potassa are first fused in a test-tube. and, while fused, there is put into it a minute quantity of the substance, mineral or organic, to be tested for manganese. If at the end of the reaction the contents of the thoroughly cooled tube exhibit a peach-blossom red colour, the substance thrown into the fusing chlorate of potassa contained manganese, the presence of which may be thus ascertained in wood, human hair (especially the reddish coloured), coal, and minerals, of all of which only minute quantities are required.

Dr. Roux has published a note on the existence of copper in certain waters. The experiments were chiefly made at the request of the Maire of Saint-Jean-d'Angely (Charente Inférieure), bearing upon the question that some of the spring-water of that town had become impregnated with copper, owing to the waste water of a coppersmith's shop having by percolation through the soil found its way to the spring. The result of very minutely conducted assays of the soil, as well as of the water in the neighbourhood of the works, revealed the presence of copper, but only in very small quantity. Water from Rochefort, pumped up by the aid of copper pumps, contained rather more copper; but in neither case was the quantity of that metal found in the waters alluded to so large as to be capable of giving rise to any injury to health, the less so as the French, by daily using copper cooking-vessels, obtain from these a sufficient quantity of copper in their system to render the detection of that metal in their blood an easy matter.

In the gold mines at the Thames, New Zealand, there are found tolerably large quantities of grey antimony ore, or stibnite, associated with the quartz and other rocks of the older series, from which gold is extracted. Mr. Pattison Muir has examined a sample of this stibnite. It has the appearance of a large mass of steel-grey crystals, radiating chiefly from a central point, some of the crystals being fully an inch in length, and generally very perfectly formed. The crystals are prisms belonging to the trimetric system, soft, and easily cut with the knife in the direction parallel to the principal axis, showing, when cut, a brilliant metallic lustre. Adhering to the crystals is a small amount of gangue, composed seemingly of siliceous matter. For the purpose of analysis a large crystal was broken off perfectly free from any foreign matter. The specific gravity of the crystals = 4·625. On analysis it was found to contain—Antimony, 71·09; iron, 0·24; arsenic, traces; sulphur, 28·47 per cent.

In making some experiments on the action of sulphur on paraffin, Mr. John Galletly found that a mixture of these substances, either in equal parts or with a larger proportion of sulphur, when heated in a flask not greatly above the melting-point of the sulphur, begins to evolve hydrosulphuric acid, and continues to give off this gas steadily, while kept moderately heated for a considerable time. This process is a most convenient one for laboratory use. With a round flask holding about a pound of the materials fitted with a tube bent at right angles about ½-inch bore and 12 to 18 inches long, containing a little loose cotton-wool, and having a smaller tube fitted to the end of this for dipping into the liquid through which it is desired to pass the gas, a convenient stream can be obtained lasting several days. The production of the gas can be stopped and renewed at pleasure by withdrawing or applying the heat. An Argand lamp should be employed, or if a Bunsen is used, the top piece should be on the tube for spreading the flame, so as to avoid heating on one spot. Heavy paraffin oil used for lubricating machinery can be substituted for the solid paraffin, and good results are also obtained with commercial stearic acid, but with the latter the tube conveying the gas soon becomes covered with drops of a milky liquid, which is probably water and finely divided sulphur. With paraffin the tubes remain clear and bright, except for a little sulphur sublimate close to the neck of the flask. Reinsch recommends a laboratory process for obtaining pure hydrosulphuric acid by heating in a glass flask equal parts of sulphur and suet. The recommendation does not seem to have been generally followed, but the advantages resulting from the substitution of paraffin for suet may lead to the more usual adoption of this process.

Drs. K. Kraut and O. Popp have described a series of experiments made with solutions of carbonate of potassa of different strength, into which sodium amalgam was placed and left for a longer or shorter time, the result being the formation of a crystalline potassium amalgam, mixed, however, with a very small quantity of sodium, which the authors consider to have been left in combination with the mercury. The percentage composition of several of the amalgams thus obtained is quoted; as instances, we mention the following:—Hg, 98·370; K, 1·560; Na, 0·036. Hg, 98·420; K, 1·31; Na, 0·119. Hg, 98,044; K, 0·934; Na, 0·641. Potassium amalgam—formula, K_2Hg_{24}; in percentages—Hg, 98·41; K, 1·59. Sodium amalgam, Na_2Hg_{12}, containing 1·88 per cent sodium.

Dr. Berthelot, who has worked long and successfully on the chemistry of the different varieties of carbon, has now treated on the properties of the carbon met with in the Cranbourne (near Melbourne, Australia) meteorite. This carbon must be considered as having been first in state of solution in molten iron, and to have separated therefrom on cooling. Next, the carbon obtained from oxide of carbon, by the decomposition of that gas by iron at a relatively low temperature, is considered. From the reactions of these substances, the author comes to the conclusion that native graphite, about the origin of which very little is known, is certainly not, at least as far as experiments can throw light on this point, derived from either anthracite or from decomposed masses of meteoric iron which contained carbon.

The presence of milk sugar in a juice of vegetable origin has been demonstrated by Dr. G. Bouchardat, who has examined a specimen of sugar obtained (1837) from the juice of the *Achras sapota*, at Martinique, the specimen being preserved among the objects known as belonging to the *Matière Médicale de Mérat*. By treating a portion of the sample of the sugar alluded to with boiling alcohol at 90 per cent, there was left undissolved a substance which, on further investigation, was found to possess all the physical and chemical properties of milk sugar. A careful quantitative analysis of the sample established the fact that it contains 55 per cent of cane sugar and 45 per cent of milk sugar. This latter variety was also found to exist in the ripe fruit of the same tree recently brought from Egypt and analysed by the author.

As a reagent for alcohol, Dr. Berthelot recommends benzoic chloride. When this compound is put into contact with water it is only very slowly decomposed, but if the water contains any alcohol (even as little as 1 in 1000 parts of water) benzoic ether is at once formed; this ether is set free by a single drop of aqueous solution of caustic potassa, the odour of the ether being very peculiar and prominent.

From a lengthy memoir on the nature of the sea-water along the coast of Bohuslän (Sweden) by Professor J. L. Ekman, we quote the most interesting point—viz., that, as regards the quantity of salt contained in the sea-water on the west coast of Sweden, there is greater difference than for any other now known sea. The coast alluded to is, on the one hand, washed by the North Sea, and on the other by the Baltic. In the more northerly part the water at the surface contains rather less than 2 per cent salt, at 60 feet depth 2·5 per cent, at 90 feet depth 3 per cent; in the more southerly portion the discrepancies are greater, but at 600 feet depth 3·5 per cent salt is met with. The average quantity of salt of the oceans is 3·44 per cent; while in the Atlantic, from the equator to from 55° to 60° N. latitude, the quantity of salt in the surface-water is 3·606 per cent, and at depths of from 500 to 10,000 feet 3·578.

LIGHT.

Conversion of cane sugar in the state of solution into glucose under the influence of light has now been proved by E. M. Raoult. The author placed, on May 10 last, a concentrated solution of sugar in water in glass tubes, and sealed the tubes while boiling; these were placed close to each other in the same locality and under identically the same conditions, with the exception that one of the tubes was kept completely in the dark, the other tube being exposed to bright daylight. On October 20 the tubes were opened, and the contents examined. The solutions were perfectly clear, and did not, on being microscopically examined, exhibit the least trace of vegetable matter. The fluid in the tube exposed to light yielded an abundant red-coloured precipitate with the cupro-potassic reagent, thereby indicating the presence of glucose, while the contents of the tube kept in complete darkness did not manifest that reaction at all.

The difficulty of obtaining large specula for telescopes, together with the disadvantages attending the weight, the brittleness, and liability to oxidation, of the speculum metal generally used, has induced Mr. Nasmyth to turn his attention to the employment of silvered plate glass for telescopic purposes, as it possesses perfect truth of surface, is lighter than metal, is not liable to oxidation, and a greater quantity of light is reflected from it than from any metallic surface. To give a concave or convex form to a disc of plate glass, a certain pressure must be made to act equally over the surface. This equal pressure is obtained on Mr. Nasmyth's plan by taking advantage of the weight of the atmosphere. A disk of silvered plate glass, 39 inches in diameter, and 3-16ths of an inch in thickness, is fitted and cemented into a shallow cast-iron dish, turned true on its face so as to render the chamber behind the glass perfectly air-tight; by means of a tube communicating with this chamber, any portion of air can be withdrawn or injected. To produce a concave mirror so slight a power is required, that, on applying the mouth to the tube and exhausting the chamber, the weight of the atmosphere, which amounts in this case to 3558 pounds, acting with equal pressure over a surface of 1186 square inches, causes the glass to assume a concavity of nearly three-quarters of an inch, which, in a diameter of 39 inches, is far beyond what would ever be required for telescopic purposes. On re-admitting the air, the glass immediately recovers its plane surface, and on forcing in air with the power of the lungs, it assumes a degree of convexity nearly equal to its former concavity. The degree of concavity or convexity may be regulated to the greatest nicety, and it is proposed to render the degree of concavity constant by placing in the air-tight chamber a disk of iron turned to the required form, and allowing the pressure of the atmosphere to retain the glass in the form given to it by its close contact with the iron disk. The curve naturally taken by the glass when under the pressure of the atmosphere, is believed by Mr. Nasmyth to be the catenary, inasmuch as its section would be the same as that of a line suspended from each end, and loaded equally throughout its length.

From America we hear of an ingenious application of photography as an aid to locksmiths. Several of the leading railway lines in America have already become bonded carriers, and are engaged in the transportation of imported goods from New York to the interior, under custom-house seal. The peculiar seal used for this purpose shows the practical value of photography to the industrial arts. The photographer for the Treasury Department is now engaged in preparing the seals for the new locks, to be used by that department in the transportation of merchandise in bond, and in such other cases where the protection they afford will be necessary. The lock itself is nothing more than an ordinary padlock, which is provided with an arrangement by which a small piece of glass an inch square is passed over the key-hole and held in place by a small spring, which cannot be reached without breaking the glass itself. By no possible exercise of ingenuity can the lock be picked or opened without breaking this piece of glass. Here comes in the value of photography. A large sheet of glass, red on one side, is prepared in New York, by marking it off into squares of the proper size. On each square is marked a number in figures and irregular spots in red, the rest of the red surface being cut away with hydrofluoric acid. One of these sheets cannot be duplicated. The Government photographer receives them at Washington, and makes three photographs of them, which give perfect *fac similes* of the figures and spots on the glass, and then both glass and photographs are cut into small squares corresponding to each other and packed in boxes, each square of glass having with it three copies on paper. These are forwarded to the officers who will use them. The officer at New York, for instance, whose duty it is, locks the doors of the car containing bonded goods, places the glass square over the key-hole, and forwards the photograph of the same to the officer at Philadelphia or elsewhere whose duty it is to receive the goods. If on the arrival of the car the lock has been disturbed, the inspector is at once aware of it, and the company transporting is liable in bonds required previously. This is an ingenious and practical application of photography to the mechanical arts, and suggests numerous other applications of the art to the safe keeping of valuables, and even the detection of crime, in interference with property, when the progress made shall have rendered automatic photography practical, which is already possible.

Mr. Joseph Sidebotham has given an account of a microscopical examination he had made of dust blown into a railway carriage in which he was travelling near Birmingham. Having collected a quantity of the dust by spreading a newspaper on one of the seats near the open window of the carriage, Mr. Sidebotham brought his microscope to bear on it, and thus describes the result:—"With two-thirds power the dust showed a large proportion of fragments of iron, and on applying a soft iron needle I found that many of them were highly magnetic. They were mostly long, thin, and straight, the largest being about 0·15 of an inch, and under the power used, had the appearance of a quantity of old nails. I then with a magnet separated the iron from the other particles. The weight altogether of the dust collected was 57 grains, and the proportion of those particles composed wholly or in part of iron was 29 grains, or more than one-half. The iron thus separated consisted chiefly of fused particles of dross or burned iron, like 'clinkers;' they were all more or less covered with spikes and excrescences, some having long tails, like the old 'Prince Rupert's drops;' there were also many small angular particles like cast-iron having crystalline structure. The other portion of the dust consisted largely of cinders, some very bright angular fragments of glass or quartz, a few bits of yellow metal, opaque white and spherical bodies, grains of sand, a few bits of coal, &c. I think it probable that the magnetic strips of iron are laminæ from the rails and tyres of the wheels, and the other iron particles portions of fused metal, either from the coal or from the furnace bars."

Some important experiments have been published by Dr. Budde, from which a new theory of the photographic latent image may be deduced. Chlorine gas is passed into a tube closed at one end, and the gas is confined by a column of oil of vitriol saturated with chlorine. This must be done in comparative darkness. A beam of light is then decomposed by means of a prism, and the several coloured rays of the spectrum are allowed to fall in succession on the tube containing the chlorine, an arrangement having been made by which any alteration in volume that might take place in course of the experiment can be detected and carefully registered. When the red rays fell upon the tube, the effect produced was very slight, the increase in the length of the gas column being only the $\frac{1}{25}$th of an inch. According to the degree of refrangibility by the ray to which the chlorine was subjected, so did its expansion increase, until when under the action of the violet, the effect was at its maximum, the expansion being ten times greater than what was caused by the action of the red rays. What is ascertained from this experiment is, that the expansion of the gas is not due to heat, for were that the case the red rays would have exercised the most powerful action, this point having been further ascertained by delicate thermometers. To further establish the fact that the expansion is due solely to the action of light, and not to a decomposition of the sulphuric acid by the chlorine, there was substituted for this acid, saturated with chlorine, the tetrachloride of carbon, the same result being obtained. The result of the experiment appears to warrant the conclusion that the violet rays of the sunbeam act by decomposing the molecule of the chlorine, setting free the two component atoms of which the molecule is supposed to be built up. The two atoms occupy a greater space when separate than when combined, and are also in a favourable condition for entering into combination.

The estimation of the distance of the fixed stars has hitherto baffled the skill of the astronomer. Mr. Fox Talbot has proposed the following manner of effecting this object. Suppose the plane of the orbit of a binary system to pass through the sun, *i.e.*, that the observer is in the plane of the orbit, and that in the spectra of the individual stars there are lines belonging to the same element. The spectra of the two stars, taken through the same slit, should be observed and compared. When the stars appear in the same straight line, it is clear that their velocities relative to the earth are the same, since both are moving perpendicularly to the line of vision; the lines from the two stars will therefore coincide. But when their apparent distance from each other is greatest, the difference of their velocities relative to the observer is equal to the velocity of either star, in its velocity in its orbit about the other. This difference of relative velocity will produce a displacement of the lines, which displacement may be observed and even measured. This gives the value of that velocity; but we know also the periodic time. We have, then, at once the circumference, and thence the diameter of the orbit. We know the greatest angular distance between the stars; we have, then, the distance of the stars from the earth.

M. Papafy has devised a series of sky-rockets adapted for telegraphic service at night for armies when in the field, so arranged that each rocket is, by a variation of coloured light, capable of transmitting six words, visible at a distance of twenty English miles. These signals can be readily kept unintelligible to the enemy, while everything relating to military and strategical matters can be easily expressed. The Prussian War Department has bought the secret of this invention from the author, a Hungarian in service as captain in the United States Army.

W. Müller and Dr. F. Knapp have published a very exhaustive memoir on that kind of glass which owes its beautifully red-purplish colour to gold. The first portion of this monograph contains a review of the literature of the subject alluded to, and of the various theories and opinions held on the condition of the gold while acting as a pigmentary or staining matter. It appears that the quantity of gold required to impart colour to the glass mass is very small indeed, since 1 part of gold in 100,000 of the metal (as the molten glass mass is technically called) distinctly yields a rose-red colour. The authors did not succeed in discovering by experiments what the condition is of the gold in the glass; the chief reason of this failure is that the quantity of gold is almost infinitesimally small.

At last there appears more than a probability that the oxyhydrogen light may be employed with financial success. The experiments recently instituted at the Crystal Palace at Sydenham with M. Tessie du Motay's apparatus are in every way satisfactory—that is, scientifically, for the statements as to economy must at present be based only upon calculation. The apparatus employed is extremely simple and may be described as follows:—A small one-cylinder engine drives a set of three small air-pumps at a rapid rate; these pump air into two retorts built up in a furnace, the external walls of which are about 10 feet long by 6 feet by 6 feet in section. These retorts are charged with manganite of soda, mixed with some oxide of copper. The manganite has a great affinity for oxygen and takes a large percentage out of the air, leaving the nitrogen to escape by small iron pipes. The manganite is thus saturated with the oxygen, which is carried over into a reservoir by a blast of superheated steam, the manganite remaining free to again absorb oxygen. In the reservoir the steam condenses into water, leaving the reservoir filled with oxygen. By means of the alternation afforded by the two retorts the process becomes continuous, and as fast as the air is pumped in so the oxygen is regularly given off. In this way 1 cubic foot of oxygen is the product of 20 cubic feet of air and 15 of steam. The burner is in shape like a double ordinary burner with one tap for the oxygen and one for the hydrogen. The top of the burner is dished out into a hemispherical cavity, in the centre of which is the oxygen hole, and surrounding it some eight or ten smaller holes for the hydrogen. There is thus obtained a solid cone of light. The equivalent of 5 cubic feet of ordinary gas burnt in the usual manner is, in the new light, 1 cubic foot of the same coal gas with about $\frac{5}{8}$ths of a cubic foot of oxygen. At 100 yards from the candelabra of twenty lights in the centre transept of the Palace small handwriting can be easily read.

Mr. John Browning has favoured us with a record of the steps he has taken towards the introduction of compound prisms. In 1864 he made for Mr. Gassiot a very powerful battery of bisulphide of carbon prisms. Instead of the sides of these prisms being made of plane and parallel glass, crown glass prisms were cemented on to them in the manner shown in the diagram (Fig. 9), where A represents the fluid prism and B B the crown-glass prisms added on either side. In a paper read before the Royal Society by Mr. Gassiot, April 7th, 1864, he says:—"In place of giving to the fluid prisms two pairs of parallel sides, advantage has been taken by Mr. Browning of the difference between the refractive and dispersive properties of crown glass, having a refracting angle of 6°, these have been substituted for one of the outer plates of each prism, the bases of these crown glass prisms being brought to correspond with the apex of the fluid prism. By this means the angle of minimum deviation of the prisms is so much altered that eleven prisms can be used instead of eight. An increase of dispersive power, due to refracting angles of 150° of the bisulphide of carbon, is thus gained, minus only the small amount of dispersion counteracted by the dispersive power of the crown glass prisms in the contrary direction." In July, 1869, at the suggestion of Dr. Robinson, of Armagh, Mr. Browning made a large dense glass prism of 60° into a compound prism, the refracting angle of the dense prism being altered to 90°. He has also made several compound prisms of very dense flint glass and

Fig. 9.

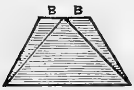
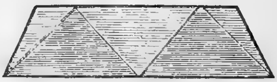


Fig. 10.



light crown glass for the present Earl of Rosse. These compound prisms are more expensive than ordinary prisms, even allowing for the extra dispersive power obtained, and in consequence of the minimum angle of deviation of compound prisms being greater, and their length greater, the size of spectroscopes has also to be increased, and thus will be rendered more cumbersome. A smaller number of compound prisms will produce a given amount of dispersion; but the number of prisms to be made is, under the most favourable circumstances three instead of two; and in the case of a circular battery the smaller number of prisms will occupy a larger circle. The number of plane faces is also greater for a given amount of dispersion, the practical difficulties in securing accuracy not being diminished by the fact that four faces out of the six in a compound prism are connected together. On the score of saving light, such prisms possess undoubtedly some advantage. Mr. Browning has made for Mr. Rutherford, of New York, a compound prism of glass, on a plan which seems to possess some advantages over any hitherto used. The diagram (Fig. 10) represents this prism. In this diagram the two darkly shaded prisms are of dense flint of 90°, while the three other prisms are of crown. Such a prism is very nearly equal in dispersive power to three ordinary flint glass prisms of 60°. There is no loss of light at the two intermediate surfaces, and it is much more compact. Experiment has proved that the angles of the flint glass prisms in this arrangement cannot with advantage be made more than 90°, nor the outside crown glass prisms less than 30°.

Microscopy.—A microscope lamp, combining most of the advantages of existing ones, has been contrived by a committee of the South London Microscopical and Natural History Club, for use at its meetings. It is mounted on a stand supported on a solid ring, after the retort stand model, which experience has proved to be less liable to overturn than any other. The lamp revolves in its socket, so that either the edge or the flat side of the flame can be used at pleasure. Hailes's porcelain shade has been adopted, which has the double advantage of protecting the eyes from the glare of the lamp as effectively as a metal chimney without its disagreeable heat, and affording an easy means of obtaining white cloud illumination. The burner is a small one of the best American manufacture; and all the parts are so simplified that the lamp can be supplied at a lower cost than the majority of microscope lamps, which it quite equals for all practical purposes.

Some microscope lamps have lately been constructed in which benzoline is employed as fuel. This liquid possesses the advantage of being much cleaner in use than paraffine, the leakage of which very penetrating fluid is always a source of annoyance to those microscopists who have to carry a lamp from place to place. The lamps are very small and compact, and are well adapted for illuminating the achromatic condenser directly without using the mirror, a mode of lighting preferred by many of our best observers. There is no doubt that when the burner is properly adapted for the consumption of benzoline, a light approaching in intensity and whiteness that of camphine may be expected. In the few experimental lamps at present made, burners like those for the combustion of paraffine have been used, and consequently the quality of the light is much inferior to that which the fuel is capable of yielding. A lamp giving a very small intense flame and requiring but little attention is a desideratum. The camphine lamp is perfection in every respect, but the care required to keep it in order is an impediment to its very extensive use.

Dr. J. J. Woodward has succeeded in producing some fine photographs of histological preparations by means of sunlight. Hitherto preparations of this kind, stained and injected, have yielded very inferior impressions by daylight, illumination with the lime, magnesium, or electric lights having given better results. The light enters the dark room through an achromatic lens of 2 inches diameter and about 10 inches focus, instead of the usual aperture, and is received upon the achromatic condenser after diverging from the focus. This illumination appears to have remedied the inconvenience caused by diffraction and interference, which in former experiments caused great confusion in the image. The low powers used in photographing these preparations giving in most instances an almost instantaneous picture, Dr. Woodward has contrived a means of regulating the exposure by a simple mechanical contrivance, and for these rapid impressions has managed to dispense with the use of the heliostat. A series of nine photographs, illustrating the capabilities of the process, has been placed in the collection of the Royal Microscopical Society.

Mr. H. J. Slack has continued his observations on deceptive appearances presented by objects under the microscope. Some specimens of fine ruling on glass and steel by Mr. J. F. Stanistreet, F.R.A.S., executed by a simple machine* of his own contrivance, have furnished some singular results, which, as in former cases, point to the necessity of using the greatest caution in interpreting the results of observations on unknown substances. The objects supplied by Mr. Stanistreet consist of stars composed of bands of fine parallel lines, intersecting each other as they approach the centre, and so forming a central star. When examined on a dark ground with powers of 3 inches to $\frac{2}{3}$ inch, the idea of the rays standing up obliquely from the plane of the glass like the rays of a fan is suggested, the apparent slope being varied as the stage with the object is caused to revolve. Under a slight variation of the lighting, the inner star caused by the intersection of the bands can be made to appear in a higher plane than that of the primary star. The circular unengraved space in the centre has the appearance of a deep hole; as in former instances, the eye is perfectly satisfied with the focussing, which gives the appearance of the lines being in different planes. In another star composed of radial lines, those most brilliantly illuminated appear to stand above the rest. Complicated patterns, in which the intersections are more numerous, viewed with a $\frac{2}{3}$ inch and Beck's vertical illuminator, give at the most satisfactory focus the appearance of solid threads to the lines, one under the other at the simple intersections, and a tendency to make the spots of complicated intersection appear higher than the rest. With a one-fifth and Powell and Lealand's modification of Professor H. L. Smith's vertical illuminator, the complex portions of the pattern are resolved, but with a decided suggestion that the cuts are elevations or threads laid upon a semi-transparent surface like white porcelain.

* Mr. Stanistreet's ruling machine is described and figured in the Monthly Microscopical Journal for December, 1871.

Captain F. H. Lang and Mr. Tatem, of Reading, give a mode of re-mounting or selecting diatoms from slides already mounted in balsam. The balsam-mounted slide is placed upon a hot plate, and when sufficiently heated, the cover is removed by means of a needle. The diatoms will either be on the slide or the cover, according to the mode adopted in mounting. Apply at once while upon the hot plate a drop of turpentine, remove the slide to the stage of the dissecting microscope, and add more turpentine. Have ready a clean slip of glass, on which has been placed a drop of turpentine. In the case of large discoid and other forms, having applied plenty of turpentine, they can easily be transferred by means of a fine sable-hair brush from the original slide to the pool of turpentine on the clean one. In the case of the finer forms, it is better to place less turpentine on the original slide, collect the diatoms into a heap, allow the turpentine to dry a little, and then by a twist of the brush to transfer them *en masse* to the new slide. In either case, having got them there, push them together and mop up the superfluous turpentine, and then, still under the dissecting microscope, slant the slide by placing a piece of folded paper under one end, and apply a little benzole by means of a clean brush or glass rod, immediately above them, that is, on the end of the slide that is raised, and allow it to float gradually over them, care being taken that it does not flow with too great a rush and carry away the diatoms with it. Repeat this process some half-dozen times, till the whole of the turpentine and balsam has been washed away, and till the valves are left dry and black after the benzole is evaporated. They can then be transferred in the usual way to any other slide.* If gum has been used to fix the diatoms, it may be found that some of the valves, especially the discoid ones, remain obstinately adherent to the glass after the turpentine has been placed over them. In such a case the process as above detailed must be carried out on the original slide, and then, after the benzole is thoroughly evaporated, water must be applied two or three times in the same way as the benzole, for the purpose of washing away the gum and freeing the diatoms, which can then, when dried, be lifted one by one and transferred in the usual manner. By this simple and easy mothod, selections can be made from balsam-mounted slides of any particular valves required, and spoilt and unsatisfactory mounts re-set.†

Mr. Browning has prepared four series of objects for the micro-spectroscope. The first three consist of fluids in sealed tubes; one set of twelve being devoted to chemical solutions; the second, a similar number of colouring matters from vegetable sources, and the third set of six illustrates blood compounds. The remaining collection consists of various blowpipe beads and crystals. They are accompanied by a catalogue giving brief directions for viewing them, and also a short explanation of the use of the various micro-spectroscope apparatus constructed by Mr. Browning. In the absence of a manual on the use of this important addition to the microscope, the specimens and directions will be of great service to those commencing such studies. It is much to be desired that some competent person would undertake a practical work on the micro-spectroscope, as nearly all the information at present published is contained in various scattered papers.

"The Monthly Microscopical Journal" for September contains a paper by Mr. H. C. Sorby, on "The Examination of Mixed Colouring Matters with the Spectrum-Microscope." It forms a valuable sequel to many other contributions to a knowledge of the spectra of vegetable colours by the same author, and consists chiefly of observations on various *Algæ*. The subject is too elaborate to admit of an abstract. In the same Journal is also "An Account of the Spectra formed by the Passage of Polarised Light through Doubly Refracting Crystals seen with the Microscope," by Francis Deas, M.A., LL.B., F.R.S.E.

Mr. H. G. Bridge communicates to the "Monthly Microscopical Journal" for November some remarks on the mode of mapping spectra with the "Bright Line Micrometer," adapted by Mr. Browning to the micro-spectro-

* Quarterly Journal of Science, vol. i. (N.S.), p. 276.
† Monthly Microscopical Journal, vol. vi., p. 217.

scope. The paper is accompanied with a plate containing twenty-two figures, drawn to scale, of the spectra of various coloured fluids, &c., compared with the lines of the solar spectrum.

Microscopists who are in the habit of making drawings with the aid of a ruled disc in the eye-piece,* will find the "sectional paper" of Messrs. Letts convenient for recording their observations. This paper is ruled in squares of twelve different sizes, varying from $\frac{1}{12}$ inch to 1 inch. The lines are of a very pale-grey colour, so as to be just visible, and scarcely interfere with the finished drawing, while they are sufficiently plain to guide the artist in copying from the ruled field of the microscope.

A simple and efficient erector, for use with the compound microscope in dissection and other manipulations, has been contrived by Mr. E. Richards, F.R.M.S. It consists of a glass mirror platinised on the front surface placed over the eye-piece at an angle of 45°. The microscope is used in a vertical position, which allows vessels of fluid to be placed on the stage, while the observer looks forward at the reflected image. The reflection being from the front surface, the confusion occasioned by the double image of an ordinary mirror is avoided; the platinum surface retains its brilliancy for a longer period than one of silver, and the definition is extremely perfect.

HEAT.

Captain J. Ericsson has constructed an apparatus for measuring the radiant intensity transmitted by flames, to endeavour to prove that the radiant power of flames is not less than that of incandescent solid substances. The measurement of the radiant heat transmitted by flames is of great importance, as it furnishes a means of measuring with precision temperatures which cannot be ascertained by direct contact. The construction of the apparatus is based upon the law that the intensities of circular radiators of different size, imparting equal temperature at equal distance from the radiating surface, are inversely as the squares of the sines of half of the subtended angles, that is, the angles formed by the axes of the circular radiant surface and the heat rays projected from the circumference to the substance receiving the radiant heat, in the prolongation of the axes. It is thus possible to determine the temperature of a circular radiator without knowing its size and distance. A perforated diaphragm of polished silver is so arranged before a thermometer that the circumference of the perforation may form a known angle with the centre of the bulb of the thermometer. The metal cone containing the thermometer and diaphragm is surrounded with a water-jacket. Suppose the thermometer to indicate 282°, and the temperature of the surrounding water to be 73°, then according to the law laid down, if the angle subtended by the centre line of the conical vessel and lines drawn from the circumference of the flame-disc to the bulb of the thermometer be 16° 8′, we know that its temperature must be 12·91 times greater than that of the flame, or 2698° F. ($282 - 73 = 209$; $209 \times 12{\cdot}91 = 2698$). Captain Ericsson promises a full extension of the application.

A simple apparatus for the observance of some beautiful phenomena connected with the vibrations of flames, &c., can be constructed as follows:—A disk of white cardboard, with apertures oblong in radial direction, is set on a spindle, so as to be rotated at any requisite speed. To examine, for instance, the flame of a gas light (in a glass tube, to prevent disturbance by air currents), place the disk in front of the light, so that the eye can see the light through each slit as it comes to a vertical position. If the speed of the disk's rotation is such that the interval of time between two slits passing the eye is just equal to the period of a vibration of the flame, the flame appears to be motionless; but if the velocity be reduced, the flame is seen to go slowly through its changes of form. If the interval be equal to, or one-half of, or one-third of the period of the vibration of the light, the illusory appearance of a disk having as many, or twice, or three times the number of slits really in

* Chemical News, vol. xxi., p. 30.

the disk is seen. This phantom disk will appear to be motionless when the periods coincide; but when otherwise, it revolves in one direction or the other. It is obvious that the vibrations of the flame can be easily counted by this means. The inventor, Mr. Charles J. Watson, counted, with a sixteen-inch tube, 453 vibrations of the flame per second. By this instrument the undulation of the vibrations of a wire can be seen to travel up and down the wire; and if watched by both eyes through the slits, the spiral course of the undulations can be observed.

F. Tommassi has described a series of experiments, illustrated by woodcuts, for the purpose of proving the possibility of converting into dynamical work the dilatation produced in liquids by heat; he proposes to utilise this motive force for the purpose of working hydraulic presses.

A new application of the oxyhydrogen light to the separation of metals has been patented by Tessie du Motay. It is especially applied to the metallurgy of copper. The usual treatment of copper has had until now for its object the extraction of the metal of a certain class of ores, where it is found combined with sulphur, arsenic, antimony, tin, lead, iron, &c. According to the new method the metal is first smelted with a flux of silicates; metallic silicates are formed, in which the sulphur and arsenic are eliminated and replaced by silicic acid. These metallic silicates are then further treated in a blast furnace, and submitted to the reducing property of incandescent charcoal; the metallic oxides are reduced in a metallic state and fused, and thus collected in ingots. The ingot thus obtained is composed of a variety of metals from which the copper has to be separated; this object is attained by smelting these ingots in a reverberatory furnace in the presence of atmospheric air, which oxidises all the metals except the copper; it is by this process of cupellation that M. Tessie du Motay utilises the slowly oxidising property of the oxyhydrogen flame, in order to facilitate the separation of the copper; he directs the flame, obtained by burning a mixture of common street gas and oxygen gas, on to the fused mass. The combustion of this gaseous mixture furnishes a certain amount of carbonic acid and oxide of carbon, as well as a small proportion of water; it is this water, claims the inventor, which, at the high temperature to which it is submitted, has the property of oxidising rapidly all the metals except the copper and lead. The fused metal obtained is then pure copper, if the original ingot contained no lead; and is composed of an alloy of copper and lead, if the ingot contained these two metals.

Dr. A. Vogel records a series of experiments made to prove the well-known fact that sulphuric acid is a constant product of the combustion of coal-gas, even when purified as perfectly as possible from sulphuretted hydrogen; the source whence the sulphuric acid is derived is the sulphide of carbon present in the gas.

The apparent volatilisation of silicium and boron has been observed by L. Troost and P. Hautefeuille. They describe a series of experiments made with pure silicium and boron, each by itself, placed in porcelain tubes, kept at a very high temperature in a slow current of dry and pure hydrogen gas, and the reaction which ensues by the admission into the tube of fluoride of silicium, chloride of silicium, and fluoride of boron. Silicium is under these conditions apparently volatilised, forming a brown-coloured smoke, which, in a cooler part of the tube, is condensed sometimes as amorphous silicium, sometimes deposited in crystalline state. The same obtains with boron, but this apparent volatilisation is due to a simple mechanical effect, conjointly with the existence of compounds of silicium with chlorine and fluorine, which are only formed at a very high temperature, and dissociated at red heat.

We extract the following graphic description of the action of cold on vapours from the "Chemical History of the Creation," by J. Phinn, Editor of the New York "Technologist." The book is full of similar striking illustrations, and the subject is treated in a manner which makes it peculiarly interesting to scientific men:—"It is a generally received opinion that all gases are merely vapours of liquids that boil at a very low temperatures. Thus, while water boils at 212°, common ether boils at 96°, and sulphurous acid at 0°. Consequently, while water is always a solid or a liquid in all parts of the earth, ether would be a permanent gas in any place where the highest tropical temperature prevailed, and sulphurous acid is always a gas except in the cold of the polar regions. Even the dense and brilliant metal mercury, when exposed to a temperature sufficiently high, becomes, in reality, a perfectly colourless and transparent gas, and carbonic acid gas, when exposed to a temperature sufficiently low, becomes first a yellowish, oily-looking liquid, and then a beautiful snow-white solid. The only difference, then, between common snow and carbonic acid snow is, that the one is much colder than the other, while, on the other hand, the only difference between carbonic acid gas and mercury gas is, that the one requires a higher temperature for its existence than the other. There are certain gases, however, which no degree of cold yet reached has reduced to the liquid, far less to the solid form. Prominent among these are oxygen and hydrogen—the two gases that when combined form water. But, after it had been observed that intense cold tended to reduce all gases and vapours to the liquid form, it was supposed that, if hydrogen and oxygen could only be made cold enough, they would become liquid too. So they were cooled with freezing mixtures, but still they remained in the gaseous state. Then a still more powerful freezing agent (solid carbonic acid) was used. But, although mercury became solid, and alcohol, unless very pure, became thick and pasty, instead of clear and limpid, still hydrogen remained unaltered. After a time a still more powerful freezing agent (liquefied laughing-gas) was discovered. Natterer, of Vienna, made a powerful steel pump and pumped laughing-gas into a large iron receiver or bottle until it became liquefied with the pressure. When a little of this liquid was poured into the air it evaporated and produced the greatest degree of cold ever observed—a degree of cold 257° below the zero of Fahrenheit's thermometer. Of such a temperature we have but a very faint idea. Let us begin at the temperature of our own bodies, and gradually descending by well-known stages, see if we can realise the fearful degree of cold that is expressed by 257° below zero. Human existence requires a temperature in the neighbourhood of 100°—a temperature which, under ordinary circumstances, is easily maintained by the chemical and vital actions going on in the system, whenever the external atmosphere does not sink below 60°. When the temperature of the surrounding air falls below 60°, the animal carries on a continual fight for the maintenence of its normal temperature. At 32° the contest becomes more energetic. Warm clothing is called into requisition, and, if the air should be agitated by keen winds, the clothing must be warm and the fuel (food) liberal, or the animal will suffer. When the temperature sinks to zero, even on a clear, still day, there are few persons that do not feel it keenly, and, if any wind should be stirring, ugh! how cold it is! But, keenly though we feel it, we have just begun to descend the scale. Let us take a leap down to −40°. The mercury in our thermometers will now congeal, and, if we find it necessary to expose ourselves, we must encase our persons in triple layers of fur. Let us take another leap of equal magnitude, and descend to −80°. This is lower than any natural temperature ever observed, and it is improbable that human life could be long sustained in an atmosphere cooled to this degree. At six degrees below this point, carbonic acid condenses to a liquid, and the breath of our nostrils would condense as it issued forth, and would fall to the ground in streams. Forty-four degrees below this point carries us to −130°. If such a temperature could be produced over a large area, what strange phenomena would present themselves! The air would be dry—drier than the summer dust—for all moisture would have been precipitated from it long ere this temperature had been reached. No animal could breathe such air, and if plants could live and perform their functions at such a low temperature, they could find no sustenance in an atmosphere as cold as this, for all the carbonic acid would descend to the earth as beautiful white snow. The breath from the nostrils of every animal, provided animals could exist, would yield a shower of these flakes, and the air would be entirely purified from the products of respiration. And yet we are not half way down to the point reached by Natterer.

He went twice as far, and, even then, oxygen and hydrogen did not liquefy, but maintained their condition as clear and beautiful gases."

ELECTRICITY.

M. Ruhmkorff in continuing his many experiments on electro-magnetic induction has met with some results which he considers worthy of publication. When a bundle of iron wires is taken and covered with a very thin copper wire destined to convey an intermittent current from a pile, and when on this is wound another thin wire to obtain the induced current, if the latter is wound on the centre of the bundle where no magnetism is manifested, there is obtained an induced current of an intensity more than double that which the same quantity of fine wire wound on one of the extremities would give. M. Ruhmkorff then constructed an induction coil with an annular core, but only obtained a spark of 2·5 m.m. in length. He then cut the ring, when the spark increased to a length of 5 m.m. Finally, a piece of wood of 5 m.m. in breadth was inserted between the severed ends of the annular core, when the spark increased to 15 m.m. In doubling the thickness of the piece of wood, the result remained the same.

M. Th. du Moncel has lately submitted to the Academy of Sciences of Paris some results of a series of interesting and valuable experiments on the size of the plates of a voltaic couple, relatively to each other, producing the best working current. The experiments and the deductions have a great practical bearing. The experiments were made with two Bunsen elements. In the one the negative electrode was constituted of two plates of carbon plunged into nitric acid in the exterior cup; the positive electrode, a single plate of amalgamated zinc was plunged into the porous cup containing acidulated water. In the other element two plates of zinc, united at the bottom, replaced the two plates of carbon of the first element, while a plate of carbon took the place of the zinc. The liquids were the same, and the vases exactly equal in height. The plates were freshly amalgamated. The plate of zinc of the first element had a submerged surface of 224 square centimetres. The second pile, an active surface of zinc of 544 square centimetres. The surfaces of the carbons were of corresponding size. The intensities of the currents produced were read off from a tangent galvanometer with a ring formed by a copper wire 0·7 m.m. in diameter, and 1·32 metres in length. The pile with the greatest amount of surface of zinc gave a deflection of 78° 5′; the pile with a small surface of zinc, 84° 10′. The temperature of the exciting liquids was—for the first pile, 29·8°; for the second, 30·2°. The weight of zinc dissolved in the first pile was 32 grammes; in the second, 38 grammes. One of the results would appear to be that the consumption of zinc with regard to the amount of work done is nearly the same in both cases. Therefore, in a pile where the current is constantly in circulation, there is the advantage in the point of view of economy of expenditure of zinc in the employment of positive electrodes of small surface. The experiments also prove that the current gains in intensity when the positive electrode is smaller than the negative electrode. These experiments accord with those of M. Delaurier, who found that a plate of zinc of 9 m.m. in size, furnished at an expense of 115 grammes a deposit of copper of 101 grammes in 48 hours, while a surface of zinc ten times as large would only furnish a continued action for one hour with a work represented by a deposit of 5 grammes. The liquids were completely exhausted. A similar observation has also been made by M. Ruhmkorff, who found that if the zinc plate of a bichromate of potassa pile were plunged to one-fifth of its height in the exciting liquid, the couple preserved during six hours an intensity that corresponded with that obtained from the total immersion of the zinc; and in the latter case, when the entire plate was covered, the current could not maintain a platinum wire at a red-heat for longer than ten minutes. These results are strongly in favour of the use of small surfaces of zinc. In practice, where generally commercially pure zinc is employed, oxidation goes on continually all over the surface of the zinc; economy therefore demands the reduction of the surface.

M. W. Beetz has lately made several determinations of the electro-motive force of various hydro-electric elements, employing a compensating battery and a system which seems capable of great exactitude. A Bunsen's element he considers to give an electro-motive force = 1·799, a Grove's element = 1·684, and a Leclanché = 1·167 times that of a Daniell's standard cell.

A printing telegraph for private wires, patented in America about a year ago by Messrs. Pope and Edison, has lately been brought under the notice of the English public. It is well adapted for private wires and for railway telegraphs, being simple and durable in construction, and capable of being understood and operated without difficulty by persons having no special knowledge of telegraphy. The instrument operates upon what is known as the "open circuit" principle, each station transmitting with its own battery—the line at the receiving station being connected directly through the relay to the earth, without the intervention of a second battery. The printing apparatus is placed upon a circular iron base in the centre of the table. In front of it is placed a dial, containing the letters of the alphabet, arranged in a circle, and provided with an index or pointer, mounted upon a horizontal shaft. This shaft also carries a type-wheel and a scape-wheel, with racket-shaped teeth, corresponding in number to the characters upon the type-wheel and dial. An electro-magnet beneath the base is provided with an armature, attached to a vibrating lever, the latter armed with pawls or clicks, so arranged in relation to the scape-wheel that every time the electro-magnet attracts its armature the wheel is made to revolve a distance of one tooth, and the type-wheel and index upon the same shaft a distance of one letter. At the extreme right of the circular base, and partly beneath it, is placed a second electro-magnet, whose armature lever passes in a horizontal direction below the type-wheel. Directly underneath the type-wheel an india-rubber pad is fixed upon the lever, by means of which an impression of the letter opposite it upon the type-wheel may be taken, The lever is also provided with a simple mechanical device for moving the paper forward the proper distance, as each successive character is imprinted upon it. The type-wheel is provided with a suitable inking roller. It will thus be understood that the printing mechanism is operated by two distinct electro-magnets, one of which is so arranged that its successive pulsations may be made to advance the index step by step to any required letter, while the other forces the strip of paper against the inked type upon the wheel, after it has been moved to the proper position by the first magnet. These two electro-magnets are placed in the circuit of a local battery, which is brought into action by a relay placed in the main line circuit, as in the ordinary Morse instrument, and is the same in principle, with the addition of a device termed the "polarised switch," which consists of a permanently magnetised steel bar pivotted between the poles of the relay magnet, the polarity of which depends upon the direction of the electrical current in the main circuit. The polarised switch determines the direction of the local current, causing it to pass through the magnet for moving the type-wheel, or through the impression magnet, as may be required. Two lever finger-keys are connected to the poles of the main battery in such a manner that, by depressing the right-hand key, the positive pole of the battery is connected, through the relay magnet to the line, and the negative to the ground, while the left-hand key, on the contrary, sends a negative current through the relay and line in the same manner. By depressing the right-hand key a sufficient number of times in rapid succession, a series of positive currents is sent through the relays at both ends of the line, which series is repeated upon the local circuits of both instruments. The positive currents deflect the polarised switches to the left, so that the local current is directed into the type-wheel magnet. The index and type-wheel of both instruments, therefore, advance one letter every time the key is depressed, and they may thus be readily brought to any desired letter. When this has been done, the left-hand key is depressed, which sends a negative current, reversing the polarised switch, and throwing the local circuit through the printing magnet, producing the impression of the letter upon the strip of paper. The apparatus, it will be seen, is entirely automatic, while it is also very simple and unlikely to get out of order. A battery of two carbon cells per mile will work the instrument.

M. F. M. Raoult has lately submitted to the Academy of Sciences the result of his researches on the calorific coefficient of the hydro-electric and thermo-electric currents. The law he deduces is—that the heat evolved by an electric current is independent of the nature of the galvanic element, the calorific coefficient, K*e*, being the same for all sources of galvanic (current) electricity.

Count du Moncel has recently published, in the "Bulletin de la Société d'Encouragement pour l'Industrie Nationale," an interesting report on the bichromate of potash battery, in which he states the following conclusions:—Of all the galvanic elements used in industry and the arts, those with bichromate of potash yield the greatest electro-motive force, are the most economical, and give off no irritating vapours, but are, on the other hand, not very constant and become strongly polarised. These effects are less marked in the Delaurier element with two liquids, and are also greatly obviated in the Chutaux element with a constant discharge; the use of the Chutaux element with sand and constant discharge offers peculiar advantages in every respect for electric telegraphy and all similar applications, because this element is for equal force more economical than the Daniell element. The Delaurier element with two liquids may be advantageously used instead of the Bunsen element when strong electric currents are required. In order to obtain a continuous and energetic action with the bichromate of potash battery, the most effectual method is the application of a current of air, as invented by M. Grenet. By the addition of bisulphate of mercury to the bichromate solution in the Chutaux element a very continuous and energetic electric current may be obtained.

Dr. Alvergniat has described some new phenomena of phosphorescence produced by frictional electricity. A vacuum is first made in glass tubes about 45 centimetres in length. After the introduction into these tubes of a small quantity of either chloride or bromide of silicium, the pressure inside these tubes having been reduced to 12 or 15 millims., they are sealed before the blowpipe. When such tubes are rubbed with a piece of silk, there appears inside the tubes a bright luminous flash, which exhibits a rose colour with the chloride, and a yellowish-green colour with the bromide, of silicium. Only when a more perfect vacuum is made in these tubes the induction spark produces a luminous phenomenon in them, but then the phosphorescence by friction entirely disappears.

All draughtsmen are acquainted with the simple device of puncturing holes through a drawing for the purpose of obtaining an outline and afterwards transferring the outline by sifting fine plumbago or other powder through the small holes. The fatigue of making the holes by hand is very great, and M. Cauderay, of Lausanne, proposes to employ the induction coil for this purpose. A table covered with tinfoil is connected with the negative pole; on it may be placed as many sheets of paper as the spark will pass through. The positive pole, consisting of a metal bar, insulated with gutta-percha, can serve as a pencil for copying the tracings. The metal point of the pencil being moved about on the contour and outline of the engraving, electric sparks spring across every time a connection is made, and puncture fine holes through the paper. It is said to require little skill to guide the pencil, as the ink tracings being good conductors, carry the pencil easily along. In the case of valuable engravings it is better to make a copy with the pantagraph and use that for the punching process. The pantagraph is connected with the positive pole of the induction apparatus, and it is placed upon a table, one-half of which is covered with tin-foil. The drawing to be copied lies upon the insulated half, and the sheets of paper to be punctured are laid upon the tin-foil. The pointer of the pantagraph moves around the outlines of the engraving, and between the pen and the foil the sparks pass to pierce the paper upon which the outline is to be made. In this way the engraving or original drawing is in no way injured.

S. H. Lockett, Professor of Engineering at Louisiana University, writing from Niagara Falls, relates the following phenomenon:—"While crossing the upper or new suspension bridge to-day, I had occasion, while conversing with a friend, to point toward the falls with my walking-cane. As soon as I did so, I heard distinctly at the end of my cane a buzzing noise, like that made by electricity passing from a heavily charged battery to a sharp-pointed rod. Repeating the experiment, the same noise was heard. I stopped several passers, and tried their canes with the same result, except in one case where there was no ferule on the cane. I immediately supposed this might be an electrical phenomenon, and set to work to test the correctness of my supposition. I took a key, and held it at arm's length toward the falls, and heard the same sound. Finally, at dark, I returned to the bridge, and pointed my cane in the air, and had the satisfaction of seeing a clear beautiful electric brush on its end. The best point to observe this interesting and beautiful phenomenon is in the middle of the bridge, and the cane must be held at arm's length, so that its end may be at some distance from any part of the bridge. The success of the experiment seems to depend a good deal on the direction of the wind and the amount of vapour blown over the bridge. To-day the wind is strong, and drives the mist directly from the falls to the bridge, but an occasional shifting or lulling of the wind would cause a cessation of the electrical noise or light. My explanation of the phenomenon is this:—As Franklin with his kite and key caught the lightning from the clouds of heaven, so here, from the suspension bridge, surrounded by vapours from the mighty falls, we may stand and gather on our walking-canes the electricity generated by the falling waters and contained in the floating mists. I think suitable arrangements might be made to collect enormous quantities of electricity from these mists, which might be used in producing grand and striking effects, thus adding another attractive feature to the sights at this wonderful place."

The duration of the spark of the Leyden jar has been measured by Professor Rood by an ingenious apparatus, by which intervals of time, measured by *billionths of a second*, are estimated. The wheel, painted black and carrying a distinct white point on its circumference, is provided with some means of giving it a uniform motion of rotation. If the wheel makes one revolution in one-sixth of a second, the white point will appear as a continuous circle; for any impression produced on the eye remains during one-sixth of a second, therefore during one revolution of the wheel all the successive positions on the circumference occupied by the bright point remain impressed on the eye, and hence the circle appears unbroken. Now, if a flash of light in the place of the white point should last one-sixth of a second, the circle would appear complete; but if it lasted one-twelfth or one twenty-fourth of a second, then would the point describe one-half or one-quarter of the whole circle. Thus, by this simple means—remembering that the smaller the arc of the circle, the less the duration of the flash—we can readily measure from the length of this arc very minute portions of time. If, instead of having one white point on the wheel, we have 100 or more radial white bands drawn with the space between them equal to their breadth, then, if the wheel makes ten turns in a second, any radial white band will advance into the position previously occupied by an adjoining black band in one-thousandth of a second, and if the flash of light lasted one-thousandth of a second, all the white bands would, during that interval, have advanced into the position of the black bands, and *vice versa*, and the disk would appear without bands and covered with a uniform grey tint. We can thus readily and accurately measure one-thousandth of a second. With the above apparatus Arago about the year 1835, first showed that a flash of lightning lasted less than one-thousandth of a second, but did not succeed in fixing the minimum limit to its duration. Professor Rood, however, was more fortunate; for during the well-remembered remarkable display of lightning in August, 1869, with an apparatus similar to the above (extemporised from a piece of pasteboard and a shawl pin), he succeeded in measuring one five-hundredth of a second as the duration of those vivid and extensive flashes. It was soon found that the velocity of the revolving disk fell far behind that of the spark of the Leyden jar, for its flash showed the revolving radial bars as absolutely at rest as when the disk was stationary. But Professor Wheatstone, in 1834, substituted for the revolving disk a mirror turning on a horizontal axis, and instead of the white point or bars he used the image of the spark reflected from the turning mirror. If the spark be instantaneous, then will it appear in the rotating mirror just as it is seen when reflected from the mirror at rest; but if the spark last during even an extremely minute fraction of a second, it will appear drawn into a line in the direction in which the mirror turns. Wheatstone thus measured the one million one hundred and fifty thousandth of a second, and ascertained that the electricity from a Leyden jar goes over a copper wire at the rate of 288,000 miles in a second, exceeding light itself in velocity. Professor Rood has now combined the two methods above given by viewing the appearance of stationary parallel and equidistant white and black bands reflected from the revolving mirror while the flash of the Leyden jar illuminated them. The direction of rotation of the mirror being across the length of the bands (which were only sixteen-thousandths of an inch apart), if the flash lasted during the time for the turning mirror to reflect a black band into the adjacent white space, then the bands would entirely disappear, and the plate on which they were drawn would appear of a uniform grey tint. By knowing the number of turns the mirror makes in a second, and the number of bands in the space of one inch, it is easy to calculate the time necessary for the obliteration of the bands. Thus has he produced, by this simple combination, an instrument surpassing in minuteness and accuracy of determination all that has gone before—an accomplishment which cannot but reflect much renown upon American science. He has succeeded (with a mirror making 350 turns in a second) in measuring accurately *forty billionths of a second*, and has shown that this is the duration of the flash of a Leyden jar having only 11 square inches of surface and one twenty-fifth of an inch striking distance—an interval of time just sufficient for a ray of light (going at the rate of 190,000 miles in a second) to travel over 40 feet. The flash from a jar having 114 square inches of surface lasted four times as long as the smaller jar. Thus, for the range of electric flashes we have measures from the five-hundredth to the forty-billionth of a second. To enable the mind to form some idea of the minuteness of the spaces of time measured by Professor Rood, we may mention that the forty-billionth of a second bears nearly the same proportion to a second as a second does to a thousand years.

Mr. George Gore, F.R.S., has, in pursuing his researches in thermo-electricity, constructed a liquid thermo-electric battery. This battery is sufficiently powerful to give a deflection of 40° with a Thomson's reflecting galvanometer, having a resistance of 3040·7 British Association units (=77872·327 miles of copper wire 1-16th of an inch in thickness). It consists of twelve glass tubes three-fourths of an inch in diameter and 10 inches in length, one end of each tube containing a platinum wire hermetically fastened. Each tube is bent into the form of the letter J, the lower limb being the open one. The open end is closed by a cork, through which is passed a second platinum wire. The tubes are fixed vertically in a wooden stand. A tin box, the bottom of which has a long semicircular cavity so arranged as to cover the upper parts of the tubes as with a cap, when it is filled with boiling water forms the upper part of the apparatus and is the source of heat. A lamp placed under a projection from one side of the bath maintains the water at the boiling-point. When it is desired to charge the battery, alternate tubes are filled with a previously boiled and cooled mixture of 1 part of sulphuric acid to 76 parts of water. The remaining six tubes are filled with a similarly prepared solution of 110 grains of hydrate of potassium dissolved in 19 ounces of water. The upper and lower wires are then connected to form an electrical series, the platinum wires in the hot acid being the negative, those in the hot alkali the positive elements of the upper ends; and the platinum wires in the cold acid the positive, in the cold alkali the negative elements at the lower ends of the tubes of the battery. The series is thus connected with acid to alkali and alkali to acid. Mr. Gore believes that the electric currents produced by the direct influence of unequal temperature of copper and platinum electrodes, in conducting liquids which do not act chemically upon those metals, have their origin in the temporary changes of cohesion of the layers of metal and liquid which are in immediate and mutual contact, and that these currents may therefore be considered as very delicate tests of the kind and amount of temporary molecular movements produced by small causes.

If a piece of amalgamated zinc is made to touch a piece of platinum in the presence of acidulated water, the well-known extraction of hydrogen bubbles takes place from the latter metal. Whilst trying a series of such experiments, and also some of a similar nature, Professor Thomas Bloxam observed an unequal evolution of bubbles from some parts of the platinum plate, and accordingly submitted the matter to inquiry. The first series of experiments had for their object to ascertain the amount of gas evolved in a given time from a zinc and platinum plate in contact. Expt. 1. A tube graduated to 1 cubic inch was filled with water acidulated with one-sixth of strong sulphuric acid; a strip of platinum as obtained from the apparatus makers was cut 7 in. long, ¼ in. wide, and introduced into the tube, which was then inverted upon a small plate of amalgamated zinc also standing in the same acid and water. The time at starting and at the termination was accurately recorded, as also the temperature and pressure throughout all the experiments. Time resulting from a mean of many experiments was 22 minutes. Expt. 2. The strip cleaned by heating it in oil of vitriol, and thoroughly rinsing in distilled water, gave as a mean result—time 14 minutes. Expt. 3. The strip merely passed through the finger several times. Time 28 minutes. It will be seen from these results that the platinum as it leaves the instrument makers is absolutely, so to speak, chemically dirty; but the mere contact with the hand is sufficient to materially modify the action. The strip cleaned by oil of vitriol was dipped for a moment into solution of common salt, and again tried, when the time required for collecting the cubic inch of hydrogen was nearly doubled. The strip having been made dirty by touching, was ignited in the flame of a good Bunsen lamp for one minute, and then arranged in the cubic inch tube. The time required was 15 minutes, thus showing that it had been in great measure cleaned, though hardly so satisfactorily as in the heating with oil of vitriol. Expt. 4 was devoted to the action of copper negative plates in similar strips to the platinum; the mean results in these cases were that copper cleaned with nitric acid and washed in distilled water gave the cubic inch of hydrogen in 21 minutes. The strip passed through the fingers as in the platinum experiment. Time required 28½ minutes. A strip of copper oxidised by heating in air furnished the hydrogen in 10 minutes. It would appear from these results, that copper behaves in a similar way as to cleanness of surface as platinum, and that the oxidised surface tends rather to facilitate the liberation of the hydrogen—perhaps from mechanical action, like platinised silver. Expt. 5. Platinised silver, as obtained from the makers, arranged in the same way as the foregoing experiments, furnished the 1 cubic inch hydrogen in 2¼ minutes. A strip cleaned with oil of vitriol as usual, time 2¼ minutes. A strip made dirty by the fingers, time 3 minutes. Hence it appears that the mechanical action of the platinum surface is of great importance, as has been well known, and that as received from the makers it is very considerably cleaner than the new platinum, because, probably, of its method of preparation. It was also found that, comparing smooth with mechanically roughened surfaces of platinum, the latter furnished the hydrogen in two-thirds of the time taken by the former. Expt. 6. A cell of Smee's battery was examined by the galvanometer, using in every case a regular strength of acid, the same wires and all conditions the same, when the following results were obtained:—The negative plate not chemically cleaned, deflection = 52°. Negative plate chemically cleaned, 57°. Negative plate made dirty by fingers, 48°. The platinum taken off the surface gave deflection 50°. Here it appears, then, that the cleaning of the surface of the platinised silver materially diminishes the resistance due to the counter-current and polarisation. The last series of experiments were directed to ascertain the influence of chemically clean surfaces upon electrolysis. A voltameter, the electrodes of which were in their ordinary state, was attached to two cells of Bunsen's battery, and the mixed gases collected with all precaution. The time taken for 1 cubic inch was 2 minutes as a mean of many experiments. The electrodes were then cleaned with hot oil of vitriol, the Bunsens freshly charged, and the 1 cubic inch of gases was furnished in $1\frac{1}{4}$ minutes; thus showing that electrolysis is materially affected by the state of cleanness of the electrodes at the time.

BIOLOGICAL NOTES.

M. Alph. Milne-Edwards has contributed to the Academy of Sciences a very important paper on the Embryology and Zoological Position of the *Lemuridæ*, having obtained from M. Grandidier (in his last Madagascar exploration) specimens of four different genera of distinct groups of these animals in the fœtal state, and submitted them to careful anatomical investigation. His dissections prove that in regard to intra-uterine life there are essential differences between the *Lemuridæ* and the *Simiadæ*. In the latter the placenta is small, discoid, and closely connected to the uterine wall, and the umbilical vesicle is very minute and soon disappears. In the *Lemuridæ* (taking *Propithecus* as the highest type and nearest to the *Simiadiæ*) the chorion is almost entirely covered with thick and serrated villosities, forming a kind of vascular cushion, and constituting a placenta which encaps the amnios, and which he names the *placenta en cloche*, in contrast with the *discoidal placenta* of the human race, and of the *Simiadæ*, the *zonar placenta* of the Carnivora, and the *diffuse placenta* of the Herbivora. The villosities are in large tufts towards the middle and upper parts of the ovum, and gradually diminish towards the cephalic pole, when they disappear. The *caduque utérine* (uterine decidua) is well developed and presents a corresponding arrangement.

Between the chorion and the amnios there is a large membranous sac extending in the direction of the long axis of the ovum, and adhering to the umbilical end by a short thin pedicle. This sac is prolonged at each end into a kind of finger-shaped cornu, is only very slightly attached to the adjacent membranes, and if air is injected into it, under water, we see it expand and its outlines become well marked. It represents the umbilical vesicle much less developed than in most of the *unguiculata*.

The placenta presents the same character in the genera *Lepilemur*, *Hapalemur*, and *Chirogalus*.

From this study of the fœtal membranes of the *Lemuridæ*, it follows that they essentially differ from those of any other mammal. This special type deviates further from that presented by man, the apes, the Cheiroptera, the Insectivora, and the Rodentia, than from that of the Carnivora; for if we suppose for an instant that the caudal pole of the ovum of the dog is invested by the villosities of the placenta, we have an almost exact realisation of the special characters of the Lemurian ovum; and it may be added that the arrangement of the umbilical vesicle is very nearly the same in these two types, while in the apes it is completely different.

These embryological characters are in complete accordance with those furnished by the brain, the cranium, the dental system, and the hands.

The brain of even the highest *Lemuridæ* is not developed posteriorly so as to cover the cerebellum,—a condition which separates them from the apes.

The orbit, which in the apes is completely enclosed outwardly, and separated from the temporal fossa, communicates freely with the latter in all the *Lemuridæ*, and gives their crania a resemblance to those of the Carnivora.

The teeth arming the lower jaw are very different in the two, the distinction between the canines and the incisors being much the more marked in the apes.

The hands, in which the thumb is always well developed and usually opposable to all the other digits, differ from those of the apes. They are admirably adapted for climbing, but are unfitted for the prehension of food, which these animals usually seize with the mouth. The fingers, instead of tapering towards the ends as in the apes, enlarge at their extremities into discoidal pads, imperfectly covered by the nail; and lastly, as is well known, the index finger of the posterior hand terminates in a true claw.

On these and other grounds the author considers that the order *Lemuridæ* must be removed from the *Simiadæ*, and placed in close affinity with the Carnivora.

The embryo of *Macropus major*, the giant kangaroo, has been carefully studied *in situ* by Professor Pagenslecher, and we extract the following remarks from his article in the "Verhand. des Naturh. Vereins zu Heidelberg," as translated in a late number of the "Annals of Natural History:"—

"The left tube contained an embryo, although no yellow body was to be recognised in the ovary. The very vascular decidua separated pretty readily from the walls of the tube, except a few stronger vascular adhesions. The chorion had no connection at all with the decidua, so that it slipped quite easily out of the envelope. The embryo was exactly of the size and maturity of the specimen of which Owen says that it was born thirty-eight days after copulation, and which he has figured. It was enveloped in the amnios. The length from the snout to the extremity of the tail was about 4 centimetres.

"Nothing was to be observed in the way of a preparation of the median sac for the further retention and nourishment of the ovum, nor anything of a preparatory dilatation of the lateral passages.

"In the ventral pouch the left teat was much longer than the right one; but whether from previous sucking or as a preparation I cannot say.

"In comparison with other embryos that of the giant kangaroo is very considerably inferior to an unborn rabbit or a newly born ferret; its size agrees pretty closely with that of an unborn mouse.

"In this comparison the small development of the hinder extremities is remarkable. Whilst on the fore feet the five toes are very distinctly formed even to the claw tips, the hind feet resemble a short-stalked fin, slightly notched into three lobes; the inner lobe is again scarcely perceptibly divided to correspond with the ultimate number of toes. This imperfection of a subsequently most important pair of limbs, in contrast with the perfection of a pair which are afterwards much weaker, is doubtless in accordance with the general law, according to which early completion of form limits growth.

"In the anatomy of the adult animal it may be interesting to mention the existence of a long but fine *ductus Botalli*, showing that even before birth the formation of the partitions of the heart arrives at the same completeness as in placental mammals."

"Professor Cope, of Philadelphia, has recently visited both the Mammoth and the Wyandotte Caves, and has studied the forms of life occurring in each. As the inhabitants of the former have been often described, we shall merely give his list of the latter, with his remarks on their peculiarities and mode of life.

"VERTEBRATA.—*Amblyopsis*, sp. (blind fish.)

"ARTICULATA.—Insects: *Anophthalmus Tellkampfii* (beetle); *Anophthalmus*, No. 2 (beetle); *Staphylinidæ*, sp. 1 (beetle); *Staphylinidæ*, sp. 2 (beetle); *Phalangopsis*, sp. (crickets); Flies: 2 species. Spiders: (beetle); *Aranea*-like, *Opilio*-like. Centipedes: *Pseudotremia*, sp.

"CRUSTACEA.—*Astacus pellucidus* (blind crawfish); aquatic species with egg-pouches external; *Lernæidæ*, species parasitic on blind fish, 14 species.

"The blind fish is very much like that of the Mammoth Cave; and direct comparison will be necessary to determine any difference if it exist. It must have considerable subterranean distribution, as it has undoubtedly been drawn up from four wells in the neighbourhood of the cave. Indeed it was from one of these, which derives its water from the cave, that we procured our specimens; and I am much indebted to my friend N. Bart. Walker, of Boston, for his aid in enabling me to obtain them. We descended a well to the water, some twenty feet below the surface, and found it to communicate by a side opening with a long, low channel, through which flowed a lively stream of very cold water. Wading up the current in a stooping posture, we soon reached a shallow expansion or pool. Here a blind crawfish was detected crawling round the margin, and promptly consigned to the alcohol-bottle. A little further beyond deeper water was reached, and an erect position became possible. We drew the seine in a narrow channel, and after an exploration under the bordering rocks secured two fishes. A second haul secured another. Another was seen, but we failed to catch it, and on emerging from the cave I had a fifth securely in my hand as I thought, but found my fingers too numb to prevent its freeing itself by its active struggles.

"If these *Amblyopses* be not alarmed they come to the surface to feed, and swim in full sight like white aquatic ghosts. They are then easily taken by the hand or net if perfect silence is preserved; for they are unconscious of the presence of an enemy except through the medium of hearing. This sense, however, is evidently very acute, for at any noise they turn suddenly downward and hide beneath stones, &c. on the bottom. They must take much of their food near the surface, as the life of the depths is apparently very sparse. This habit is rendered easy by the structure of the fish; for the mouth is directed upwards, and the head is very flat above, thus allowing the mouth to be at the surface. This structure also probably explains the fact of its being the sole representative of the fishes in subterranean waters. No doubt many other forms were carried into the caverns since the waters first found their way there; but most of them were, like those of our present rivers, deep water or bottom feeders. Such fishes would starve in a cave river, where much of the food is carried to them on the surface of the stream. The *Amblyopsis* belongs, with two other genera of imperfect seers, to the family *Hypsæidæ*, which, with the pike, shore-minnow, and mud-fish families, form the order of *Haplomi*.

"Of the other animals, one beetle (*Anophthalmus*), the cricket (*Phalangopsis*), a fly, the *Opilio*-like spider, the centipede, and the blind crawfish, are probably the same as those found in the Mammoth Cave. Two beetles and two crustaceans are certainly different from those of the latter, and the centipedes are much more numerous. The Gammaroid crustacean which we find in the waters of the Mammoth Cave, and which is no doubt, in part, the food of the blind fish, we did not find; but some such species no doubt exists.

"The mutual relations of this cave-life form an interesting subject. In the first place, two of the beetles, the crickets, the centipede, the Gammaroid crustacean (food of the blind fish), are more or less herbivorous; they furnish food for the spiders, crawfish, *Anophthalmus*, and the fish. The vegetable food supporting them is, in the first place, fungi, which in various small forms grow up in damp places in the cave; they can always be found attached to excrementitious matter dropped from the bats, rats, and other animals which extend their range in the outer air."

The history of the development of the *wing of the butterfly* in the larva and the chrysalis has been carefully studied by Dr. Landois, who, after noticing the labours of Swammerdam and later observers on this subject, proceeds to describe—(1) The development of the wing in the caterpillar; (2) The change which it undergoes in the chrysalis stage; and (3) The processes that go on in the completed wing. The paper, which is too strictly anatomical to admit of a popular analysis, is a very valuable one, and is illustrated by highly magnified figures of the wing-germs of the fore and hind wings after the first moulting of the larva, and likewise after the second and fourth moultings; of a section of the wing in the chrysalis state; of the venation of the perfect wing, &c.—("Zeitsch. f. Wissensch. Zoologie:" Dritter Heft, 1871).

Mr. E. Ray Lankester has just published in the "Quarterly Journal of Microscopical Science," an elaborate paper entitled "Observations and Experiments on the Red Blood-Corpuscle, chiefly with Regard to the Action of Gases and Vapours;" and the following paragraph contains his "general conclusions and summary." The red blood-corpuscle of the vertebrata is a viscid and at the same time elastic disk, oval, or round in outline, its surface being differentiated somewhat from the underlying material, and forming a pellicle or membrane of great tenuity, not distinguishable with the highest powers (whilst the corpuscle is normal and living), and having no pronounced inner limitation. The viscid mass consists of, or rather *yields*, since the state of combination of the components is not known, a variety of albuminoid and other bodies, the most easily separable of which is hæmoglobin; secondly, the matter which segregates to form Roberts's macula; and thirdly, a residuary stroma, apparently homogeneous in the Mammalia (excepting so far as the outer surface or pellicle may be of a different chemical nature), but containing in the other vertebrata a sharply definable nucleus, this nucleus being already differentiated, but not sharply delineated during life, and consisting of (or separable into) at least two components, one (paraglobulin) precipitable by CO_2, and removable by the action of weak NH_3; the other pellucid and not granulated by acids. The chemical differentiation of the outer pellicle is rendered probable by the behaviour of the corpuscles under weak NH_3, which appears to dissolve this pellicle, and so loose the viscid matter from that which restrained it to its oval shape; also from the inability of CO_2 to act on the corpuscle until it has been subjected to the influence of aqueous vapour, which may be supposed to remove or render permeable this pellicle; also from the action of chloroform, oil, and cyanogen, which cause the discharge or diffusion of the hæmoglobin from the corpuscle, perhaps by first removing or rendering permeable—at any rate modifying—this outer pellicle. Steam, chloroform, benzine, bisulphide of carbon, ammonia, and cyanogen, act on the red blood-corpuscle so as to cause the escape of the hæmoglobin. The further action of these reagents causes the elimination of what may be called Roberts's constituent, that which is stained by magenta and set by tannin. A still further action of chloroform, of water, or of ammonia, dissolves first the stroma, lastly the nucleus. The details of these actions are given in the paper. Carbonic oxide and sulphuretted hydrogen produce their respective changes on the hæmoglobin, as demonstrated spectroscopically, without altering the form of the corpuscle, merely effecting the radiation of its body.

In the same number of that journal Dr. Sanderson has discussed the question of the supposed "spontaneous generation of bacteria in certain solutions," which attracted attention in France, and more recently, owing to Dr. Bastian's statements, in this country. Dr. Sanderson shows, First,—That neither bacteria nor fungi ever develope in solutions raised to the boiling-point and placed in carefully cleansed and boiled vessels, which are subsequently closed. Secondly—That if such solutions in such flasks be exposed to atmospheric air, no bacteria ever develope, but yeast-cells and ultimately blue mould do develope (whence it is inferred that the germs of fungi but not of bacteria are carried in the air). Thirdly—That if unboiled water be used, or glass or other surface not duly cleansed be brought into contact with the above-mentioned solutions, bacteria always develope in great quantity (whence it is inferred that water, and surfaces which have been or are more or less damp, are the means of dissemination of bacteria).

Dead or Alive?—A new and very simple method of distinguishing between real and apparent death has been recently discovered by M. Laborde. When a sharp steel needle (not cased only with steel) is driven into the tissues of a living man or animal, in a short time it loses its metallic lustre and becomes dim, or in scientific language—is oxidised; while a similar needle may remain for an hour or more in the tissues of a dead subject without undergoing any apparent change. Hence the oxidation or non-oxidation of the needle affords a decisive proof whether death is real or only apparent.

PSYCHIC FORCE.

THE reception accorded to my previous paper on Psychic Force has been so satisfactory that, had they been in a sufficiently advanced state, I should not hesitate to continue the publication of my investigations. Time which I hoped to spend in research has been occupied in the necessary task of vindicating my honour against adverse criticism. I wish my scientific friends to have an opportunity of seeing my reply to the attacks upon me; but I am reluctant to introduce personalities into the pages of the "Quarterly Journal of Science." I have therefore decided to embody the answers to my objectors in a pamphlet, the publication of which Messrs. Longman have been good enough to undertake. Copies are bound up with the advertisement sheets of this number.

Let me take this opportunity of explaining the exact position which I wish to occupy in respect to the subject of Psychic Force. I have desired to examine the phenomena from a point of view as strictly physical as their nature will permit. I wish to ascertain the laws governing the appearance of very remarkable phenomena which at the present time are occurring to an almost incredible extent. That a new form of Force—whether it be called psychic force or *x* force is of little consequence—is involved in this occurrence, is not with me a matter of opinion, but of absolute knowledge; but the nature of that force, or the cause which immediately excites its activity, forms a subject on which I do not at present feel competent to offer an opinion. I wish, at least for the present, to be considered in the position of an electrician at Valentia, examining with the utmost refinement of instrumental means certain electrical currents and pulsations passing through the Atlantic cable, independently of their causation, and ignoring whether these phenomena are produced by imperfections in the testing instruments themselves—whether by earth currents or by faults in the insulation—or whether they are produced by an intelligent operator at the other end of the line.

W. C.

PSYCHIC FORCE

AND

MODERN SPIRITUALISM:

A REPLY TO THE "QUARTERLY REVIEW"

AND OTHER CRITICS.

BY

WILLIAM CROOKES, F.R.S., &c.

LONDON:
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1871.

PSYCHIC FORCE

AND

MODERN SPIRITUALISM:

A REPLY TO THE "QUARTERLY REVIEW."

THE *Quarterly Review* for October contains an article under the title of "Spiritualism and its Recent Converts," in which my investigations and those of other scientific men are severely handled in the spiteful bad old style which formerly characterised this periodical, and which I thought had happily passed away. It has reverted to the unjustifiable fashion of testing truth by the character of individuals. Had the writer contented himself with fair criticism, however sharply administered, I should have taken no public notice of it, but have submitted with the best grace I could. But with reference to myself he has further mis-stated and distorted the aim and nature of my investigations, and written of me personally as confidently as if he had known me from boyhood and was thoroughly acquainted with every circumstance of my educational and scientific career, so that I feel constrained to protest against his manifest unfairness, prejudice, and incapacity to deal with the subject and my connection with it. Although other investigators, including Dr. Huggins, Serjeant Cox, Mr. Varley, and Lord Lindsay, are included in the indictment and found guilty with extenuating circumstances, for me he can feel no tenderness, which, were it not for my recent sins, he is good enough to observe he "might have otherwise felt for a man who has in his previous career made creditable use of his very limited opportunities." The other offenders who are attacked can well take care of themselves; let me now vindicate myself.

It was my good or evil fortune, as the case may be, to have an hour's conversation, if it may be so termed when the talking was all on one side, with the Quarterly Reviewer in question, when I had an opportunity of observing the curiously dogmatic tone of his mind and of estimating his incapacity to deal with any subject conflicting with his prejudices and prepossessions. At the last meeting of the British Association at Edinburgh we were introduced—He as a physiologist who had enquired into the matter fifteen or twenty years ago; I as a scientific investigator of a certain department of the subject; here is a sketch of our interview, accurate in substance if not identical in language.

"Ah! Mr. Crookes," said he, "I am glad I have an opportunity of speaking to you about this Spiritualism you have been writing about. You are only wasting your time. I devoted a great deal of time many years ago to Mesmerism, Clairvoyance, Electro-biology, Table-turning, Spirit-rapping, and all the rest of it, and I found there was nothing in it. I explained it all in my article I wrote in the *Quarterly Review*. I think it a pity you have written anything on this subject before you made yourself intimately acquainted with my writings and my views on the subject. I have exhausted it."

"But, Sir," interposed I, "you will allow me to say you are mistaken, if-- "

"No, no!" interrupted he, "I am not mistaken. I know what you would say. But it is quite evident from what you have just remarked, that you allowed yourself to be taken in by these people when you knew nothing whatever of the perseverance with which I and other competent men, eminently qualified to deal with the most difficult problems, had investigated these phenomena. You ought to have known that I explain everything you have seen by 'unconscious cerebration' and 'unconscious muscular action;' and if you had only a clear idea in your mind of the exact meaning of these two phrases, you would see that they are sufficient to account for everything."

"But, Sir—"

"Yes, yes; my explanations would clear away all the difficulties you have met with. I saw a great many Mesmerists and Clairvoyants, and it was all done by 'unconscious cerebration.' Whilst as to Table-turning, everyone knows how Faraday put down that. It is a pity you were unacquainted with Faraday's beautiful indicator; but, of course, a person who knew nothing of my writings would not have known how he showed that unconscious muscular action was sufficient to explain all these movements."

"Pardon me," I interrupted, "but Faraday himself showed——" But it was in vain, and on rolled the stream of unconscious egotism.

"Yes, of course; that is what I said. If you had known of Faraday's indicator and used it with Mr. Home, he would not have been able to go through his performance."

"But how," I contrived to ask, "could the indicator have served, seeing that neither Mr. Home nor anyone else touched the—"

"That's just it. You evidently know nothing of the indicator. You have not read my articles and explanations of all you saw, and you know nothing whatever of the previous history of the subject. Don't you think you have compromised the Royal Society? It is a great pity that you should be allowed there to revive subjects I put down ten years ago in my articles, and you ought not to be permitted to send papers in. However, we can deal with them." Here I was fain to keep silence. Meanwhile, my infallible interlocutor continued—

"Well, Mr. Crookes, I am very pleased I have had this opportunity of hearing these explanations from yourself. One learns so much in a conversation like this, and what you say has confirmed me on several points I was doubtful about before. Now, after I have had the benefit of hearing all about it from your own lips, I am more satisfied than ever that I have been always right, and that there is nothing in it but unconscious cerebration and muscular action."

At this juncture some good Samaritan turned the torrent of words on to himself; I thankfully escaped with a sigh of relief, and my memory recalled my first interview with Faraday, when we discussed table-turning and his contrivance to detect the part played by involuntary muscular effort in the production of that phenomenon. How different his courteous, kindly, candid demeanour towards me in similar circumstances compared with that of the Quarterly Reviewer!

Now, let me ask, what authority has the reviewer for designating me a recent convert to spiritualism? Nothing that I have ever written can justify such an unfounded assumption. Indeed the dissatisfaction with which many spiritualists have received my articles clearly proves that they consider me unworthy of joining their fraternity. In my first published article the following sentences occur:—

> "Hitherto I have seen nothing to convince me of the 'spiritual' theory. In such an enquiry the intellect demands that the spiritual proof must be absolutely incapable of being explained away; it must be so strikingly and convincingly true that we cannot, dare not deny it."

> "Accuracy and knowledge of detail stand foremost amongst the great aims of modern scientific men. No observations are of much use to the student of science unless they are truthful and made under test conditions; and here I find the great mass of spiritualistic evidence to fail. In a subject which, perhaps, more than any other lends itself to trickery and deception, the precautions against fraud appear to have been, in most cases, totally insufficient."

> "I confess that the reasoning of some spiritualists would almost seem to justify Faraday's severe statement that many dogs have the power of coming to much more logical conclusions. Their speculations utterly ignore all theories of force being only a form of molecular motion, and they speak of Force, Matter, and Spirit as three distinct entities."

In a subsequent paper, I said that my experiments appeared to establish the existence of a new force connected, in some unknown manner, with the human organisation; but that it would be wrong to hazard the most vague hypothesis respecting the cause of the phenomena, the nature of this force, and the correlation existing between it and the other forces of nature. "Indeed," said I, "it is the duty of the enquirer to abstain altogether from framing theories until he has accumulated a sufficient number of facts to form a substantial basis upon which to reason." New forces must be found, or mankind must remain sadly ignorant of the mysteries of nature. We are unacquainted with a sufficient number of forces to do the work of the universe.

In a third paper, I brought forward many quotations from previous experimentalists, which showed that they did *not* ascribe the phenomena to spiritualism. I then said that the name Psychic had been chosen for the subject "because I was most desirous to avoid the foregone conclusions implied in the title under which it has hitherto been claimed as belonging to a province beyond the range of experiment and argument."

Do these quotations look like spiritualism? Does the train of thought running through them justify the Quarterly Reviewer in saying that "the lesson afforded by the truly scientific method followed by this great master of experimental philosophy (Faraday) . . . . should not have been lost upon those who profess to be his disciples. But it has been entirely disregarded . . . . by men from whom better things might have been expected ?"

I have devoted my enquiry entirely to those physical phenomena in which, owing to the circumstance of the case, unconscious muscular action, self deception, or even wilful fraud, would be rendered inoperative. I have not attempted to investigate except under such conditions of place, person, light, position, and observation, that contact was either physically impossible or could take place only under circumstances in which the unconscious or wilful movement of the hands could not vitiate the experiment. The experiments being tried in my own house, assumption of pre-arranged mechanical contrivances to assist the "medium" was out of the question.

The most curious thing regarding this article in the *Quarterly* is that the writer himself is a believer in a *new force*, and he arrogantly tries to put down any attempt to bring forward another. He refers to various hypotheses—to Sir William Hamilton's "latent thought," Dr. Laycock's "reflex action of the brain," and Carpenter's "ideo-motor principle." The reviewer adopts, without hesitation, Carpenter's hypothesis as the true and universal solvent of the phenomena in question, notwithstanding that this hypothesis is rejected by the physiologists most competent to judge it.

The whole tenor of the article, the numerous references to various "spiritual" phenomena, and the account of some of the reviewer's own experiences, show that he knows little or nothing of any such phenomena as those which I have commenced to investigate. He refers to mesmerism, curative influence, "planchette" writing, table-tilting, table-turning, and to the messages obtained by these means. When he does not impute fraud, he explains the physical movements by the hypothesis of "unconscious muscular action," and the intelligence which sometimes controls these movements, delivers messages, &c., by "unconscious cerebration" or "ideo-motor action."

Now these explanations are possibly sufficient to account for much that has come under the personal cognisance of the reviewer. I will do him the justice to believe that, as he affirms, he did take every opportunity within his reach of witnessing the higher phenomena of "spiritualism," and that on various occasions he met with results which were entirely unsatisfactory. The error into which he falls is this: Because he saw nothing that he thought worth following up, therefore it is impossible anyone else can be more fortunate. Because he and his scientific friends were following out the subject for more than a dozen years, therefore my own friends and myself deserve reprobation for pursuing the inquiry for about as many months.

According to this reasoning science would proceed very slowly. How often do we find instances of an abandoned investigation being taken up by another inquirer, who, more fortunate in his opportunities, carries it to a successful issue.

The reviewer has no grounds whatever for asserting that—

> "He (Mr. Crookes) altogether ignores the painstaking and carefully conducted researches which had led men of the highest scientific eminence to an unquestioning rejection of the whole of those higher phenomena of 'mesmerism' which are now presented under other names as the results of 'spiritual' or 'psychic' agency."

Now I am quite familiar with these researches and with the various explanations of them so elaborately set forth by Dr. Carpenter and others. I made no reference to them, simply because the phenomena which came under their notice are entirely different from the phenomena I have examined. During my experiments I have seen plenty of instances of planchette writing, table-turning, table-tilting, and have received messages innumerable, but I have not attempted their investigation mainly for two reasons; first, because I shrank from the enormous difficulty and the consumption of time necessary to carry out an inquiry more physiological than physical; and, secondly, because little came under my notice in the way of messages or table-tilts which I could not account for.

My reviewer objects to the accordion being tried in a cage under the table. My object is easily explained. I must use my own methods of experiment. I deemed them good under the circumstances, and if the reviewer had seen the experiment before complaining it would have been more like a scientific man. But the cage is by no means essential, although, in a test experiment, it is an additional safeguard. On several subsequent occasions the accordion has played over the table, and in other parts of my room away from a table, the keys moving and the bellows action going on. An accordion was selected because it is absolutely impossible to play tricks with it when held in the manner indicated. I flatly deny that, held by the end away from the keys, the performance on an accordion "*with one hand* is a juggling trick often exhibited at country fairs," unless special mechanism exists for the purpose. Did ever the reviewer or any one else witness this phenomenon at a country fair or elsewhere? The statement is only equalled in absurdity by the argument of a recent writer, who, in order to prove that the accounts of Mr. Home's levitations could not be true, says, "An Indian juggler could sit down in the middle of Trafalgar Square, and then slowly and steadily rise in the air to a height of five or six feet, still sitting, and as slowly come down again." Curious logic this, to argue that a certain phenomenon is impossible to Mr. Home because a country bumpkin or an Indian juggler can produce it.

In the experiment with the board and spring balance the reviewer says that "the whole experiment is vitiated by the absence of any determination of the *actual downward pressure* of Mr. Home's fingers."

I maintain that this determination is as unnecessary as a determination of his "downward pressure" on the chair on which he was sitting or on his boots when standing. In reference to this point I said:—

> "Mr. Home placed the tips of his fingers *lightly on the extreme end* of the mahogany board which was resting on the support."

> "In order to see whether it was possible to produce much effect on the spring balance by pressure at the place where Mr. Home's fingers had been, I stepped upon the table and stood on one foot at the end of the board. Dr. Huggins, who was observing the index of the balance, said that the whole weight of my body (140 lbs.) so applied only sunk the index 1½ lbs., or 2 lbs. when I jerked up and down. Mr. Home had been sitting in a low easy-chair, and could not, therefore, had he tried his utmost, have exerted any material influence on these results. I need scarcely add that his feet as well as his hands were closely guarded by all in the room."

"The wooden foot being 1½ inches wide, and resting flat on the table, it is evident that *no amount of pressure* exerted within this space of 1½ inches could produce any action on the balance."

But as this objection had been made by several persons, I devised certain experiments so as to entirely eliminate mechanical contact, and these experiments were fully described in my last paper.

To show the singular inaccuracy of the reviewer's statements and inferences, I give below in parallel columns, quotations from the *Quarterly Review*, to mark the contrast between its unfair statements and my own actual language as printed in the *Quarterly Journal of Science.*

(*Quarterly Review, Oct.*, 1871).

"He admitted that he had not employed the tests which men of science had a right to demand before giving credence to the genuineness of those phenomena."

"He entered upon the inquiry, of which he now makes public the results, *with an avowed foregone conclusion of his own.*"

"This obviously deprives his 'conviction of their objective reality' of even that small measure of value to which his scientific character might have given it a claim if his testimony had been impartial?"

(*Quarterly Journal of Science, July*, 1870).

"My whole scientific education has been one long lesson in exactness of observation, and I wish it to be distinctly understood that this firm conviction [of the genuineness of certain phenomena] is the *result of most careful investigation.*"

"In the present case I prefer to enter upon the inquiry with no preconceived notions whatever as to what can or cannot be." . . . "At first, I believed that the whole affair was a superstition, or at least an unexplained trick." . . . "I should feel it to be a great satisfaction if I could bring out light in any direction, and I may safely say that *I care not in what direction.*" . . "I cannot, at present, hazard even the most vague hypothesis as to the cause of the phenomena."

"*Views or opinions I cannot be said to possess* on a subject which I do not pretend to understand." . . . . . "The increased employment of scientific methods will promote exact observation and greater love of truth among enquirers, and will produce a race of observers who will drive the worthless residuum of spiritualism hence into the unknown limbo of magic and necromancy."

On page 351 the reviewer insinuates that the early scientific training of myself and fellow-workers has been deficient. Speaking for myself, I may say that my scientific training could not well have commenced earlier than it did. Some time before I was sixteen I had been occupied in experimental work in a private physical laboratory. Then I entered the Royal College of Chemistry, under Dr. Hofmann, where I stayed six years. My first original research, on a complicated and difficult subject, was published when I was nineteen; and from that time to the present, my scientific education has been one continuous lesson in exactness of observation.

The following parallel passages show that my reviewer and myself differ but little in our estimates of the qualities required for scientific investigation.

(*Quarterly Review*, *Oct.*, 1871.)

"Part at least of this predisposition" [towards spiritualism] "depends on *the deficiency of early scientific training.* Such training ought to include—1. The acquirement of habits of correct *observation* of the phenomena daily taking place around us; 2. The cultivation of the power of *reasoning* upon these phenomena, so as to arrive at *general principles* by the *inductive* process; 3. The study of the method of testing the validity of such inductions by *experiment;* and 4. The *deductive* application of principles thus acquired to the *prediction* of phenomena which can be verified by observation."

(*Quarterly Journal of Science*, *July*, 1870.)

"It will be of service if I here illustrate the modes of thought current among those who investigate science, and say what kind of experimental proof science has a right to demand before admitting a new department of knowledge into her ranks. We must not mix up the exact and the inexact. The supremacy of accuracy must be absolute." . . . "The first requisite is to be sure of *facts*; then to ascertain *conditions;* next, *laws.* Accuracy and knowledge of detail stand foremost amongst the great aims of modern scientific men. No observations are of much use to the student of science unless they are truthful and made under test conditions." . . . "In investigations which so completely baffle the ordinary observer, the thorough scientific man has a great advantage. He has followed science from the beginning through a long line of learning; and he knows, therefore, in what direction it is leading; he knows that there are dangers on one side, uncertainties on another, and almost absolute certainty on a third; he sees to a certain extent in advance. But, where every step is towards the marvellous and unexpected, precautions and tests should be multiplied rather than diminished." . . . "Investigators must work; although their work may be very small in quantity if only compensation be made by its intrinsic excellence."

The review is so full of perverse, prejudiced, or unwarranted mis-statements, that it is impossible to take note of them all. Passing over a number I had marked for animadversion, I must restrain myself to exemplifying a few of them.

The reviewer says that in my paper of July, 1870, my conclusion was "based on evidence which I admitted to be scientifically incomplete." Now in that paper I gave no experimental evidence whatever. After testifying emphatically as to the genuineness of two of the phenomena, I gave an outline of certain tests which in my opinion ought to be applied, and, in a foot note, I said that my preliminary tests in this direction had been satisfactory. Is this admitting that I had not employed such tests? Is it fair to say that my results were "based on evidence which I admitted to be scientifically incomplete?"

On p. 346, referring to the results obtained with the board and balance, my reviewer urges that it never seems to have occurred to me "to test whether the same results could not be produced by throwing the board into rhythmical vibration by an *intentional* exertion of muscular action!" Yet will it be believed that at p. 344 he gives in my own words an account of my trying this identical experiment; and if he had taken the trouble to refer to my other paper on p. 486 of the *Quarterly Journal of Science*, he would have seen that I had tested in like manner the special apparatus to which he alludes. Has the reviewer learnt to blow both hot and cold? has his memory faded? or has chagrin at missing the truth in his long investigations spoilt his temper?

The "fact" spoken of on p. 347, that myself and friends attributed to psychic force the rippling of the surface of water in a basin, when it was really produced by the tremor of a passing railway train, is, like many other of the reviewer's "facts," utterly baseless; but as he is careful to tell us that in this particular case the "fact" is *not* one of his own invention, what is to be said of his discretion in believing his "highly intelligent witness?" No such occurrence took place; nor will a passing railway train produce a ripple on the surface of water in the basin in my room. I invite the "highly intelligent witness" to verify the fact.

On p. 348, in speaking of Mr. Varley, the reviewer says that "his scientific attainments are so cheaply estimated by those who are best qualified to judge of them, that he has never been admitted to the Royal Society." It seems natural it should follow that Mr. Varley *is* a Fellow of the Royal Society; he was elected in June last. I seem to be safe in saying exactly the opposite of the reviewer.

Not to weary the reader, I will deal only with three more mis-statements, selecting instances where the reviewer conceives that he is perfectly sure of his facts. In these three instances the reviewer commences his attack upon me with the ominous words "we speak advisedly." If this expression has any meaning, it implies that the writer is more than ordinarily certain of the statement it prefaces—that he speaks with deliberate and careful consideration. Now I also speak "advisedly" when I affirm, with the proof in my hand, that two if not all of these three charges fulminated against me are either heedless or wilful misrepresentations.

The first charge is as follows:—

> "Now we speak advisedly when we say that Mr. Crookes knew nothing whatever of the perseverance with which scientific men with whom he has never had the privilege of associating, qualified by long previous experience in inquiries of the like kind, had investigated these phenomena."

This spiteful statement is utterly false. I should think there are few persons in this country who have examined more carefully into the literature of the subject, or have read a greater number of books on spiritualism, demonology, witchcraft, animal magnetism, spiritual theology, magic, and medical psychology, in English, French, and Latin. In this list I have even included Dr. Carpenter's article on Electro-Biology and Mesmerism in the *Quarterly Review* for October, 1853.

The second well-considered charge runs as follows:—

"We also speak advisedly when we say that Mr. Crookes was entirely ignorant of the previous history of the subject, and had not even acquainted himself with the mode in which Professor Faraday had demonstrated the real nature of table turning."

As to my entire ignorance of the previous history of the subject, that I think is pretty well disposed of in the preceding paragraph.

In 1853 I was intimately acquainted with the late Robert Murray, at that time manager at Mr. Newman's, Philosophical Instrument Maker, Regent Street. I was in his shop several times a week, and in May and June of that year, Murray and I had many conversations on the subject of table turning. I well remember his telling me one day that Professor Faraday had given him the design of a test-apparatus by which he expected to prove that the rotation of the table was due to unconscious muscular action. A day or two after, he showed me the instrument which he was just about to send to Professor Faraday. At that time I was not unfrequently favoured by the late Rev. J. Barlow, Sec. R.I., with invitations to his house in Berkeley Street, and on one of these occasions on entering the room he thus accosted me:—"Mr. Crookes, I am glad you have come, we are doing a little table turning, and have just been trying Faraday's new instrument. He is here, let me introduce you to him." Professor Faraday, in his kindly genial manner, explained to me fully the action of his instrument, and instead of pooh-poohing the remarks of a mere boy—for I was only 21—listened to my objection that his instrument was based upon the assumption that the supposed acting force from the hands would pass through the glass rollers, and replied that he had thought of that, and had got over the difficulty by tying the two boards together so as to render them rigid, when it was found that the table rotated as well with the instrument as without it. Since then I have frequently employed this device of a long delicate indicator to magnify minute movements. Perhaps my reviewer is not aware that this device is one of the commonest in physical laboratories, and was in frequent use long before any of the present generation saw the light. I have adopted it from 1853 up to the present time. In my early experiments I availed myself of Professor Faraday's test-instrument, but recently, when I have frequently made it a *sine qua non* that the operator shall not touch the table or any portion of the instrument, as in Experiments III., IV., and VI.,* it would puzzle even the ingenuity of my reviewer to say how Faraday's instrument is to be applied. In such cases I adopt the well-known and superlatively delicate index, a ray of light.

The *Quarterly* goes on to magnify Faraday's experiment on table turning, utterly forgetting that Faraday did not come to a similar conclusion with the reviewer; at least, it was much more obscurely put if put at all. Faraday, so far as I know, never spoke of a latent power within us, of which we are unconscious, working in our muscles, and leading them to acts which culminate in a form of speech or writing by movements of a table. Faraday would have held this a sufficiently great novelty if put before him as I endeavour to put it before myself after reading the *Quarterly's* article. My belief, however, is that Faraday experimented with questionable phenomena only.

* Quarterly Journal of Science, Oct., 1871, p. 487 *et seq.*

The third charge in which the reviewer speaks "advisedly" runs thus:—

> "For this discovery [Thallium] he was rewarded by the Fellowship of the Royal Society; but we speak advisedly when we say that this distinction was conferred on him with considerable hesitation."

In January, 1863, whilst the interest attaching to the discovery of the element Thallium was fresh in the minds of scientific men, I was both surprised and gratified at receiving the following note from Professor Williamson:—

> "University of London,
> Burlington House, W.,
> 16th Jan., 1863.
>
> "My dear Sir,—I should be glad to see your name on the list of Fellows of the Royal Society, and if you have no objection to my doing so, would do myself the honour of proposing you for election into the Society. Could you spare a quarter of an hour on Monday afternoon to talk the matter over with me at University College, and oblige
>
> "Yours very truly,
>
> "ALEX. W. WILLIAMSON."

This kindness being entirely unsought was the more pleasing to me. At the interview, my certificate was partially filled up and left in Professor Williamson's hands for the purpose of obtaining the necessary signatures. After this meeting with Professor Williamson I took no further steps in the matter, and spoke to no one on the subject; but in due time Professor Williamson wrote that my certificate was duly received at the Royal Society and read at the meeting, adding—

> "There is on the part of the chemists now on the Council a sincere appreciation of your high claims."

Subsequently, the same kind friend wrote—

> "I have much pleasure in congratulating you and ourselves on your being one of the fifteen selected by the Council of the Royal Society for election."

I was formally elected on the 4th of June, 1863.

That discussion ensued when my name was brought before the Council follows as a matter of course. When fifteen only are to be elected from about fifty candidates, it is to be expected that the claims of each should be rigidly scrutinised; but whatever my anonymous reviewer may say "advisedly" on the subject, the *fact* remains that I was elected on the first application, an almost unheard-of honour for so young a man. Considering the large majority of eminent candidates whose election is postponed from year to year (sometimes even to ten years), there is no reason why my election should not have been postponed for at least one year, had there been truth in the statement that "considerable hesitation" was evinced in conferring this distinction upon me.

The grossness of the imputation, that the Royal Society admitted me although my investigations had only a merit purely *technical*, is astounding when the merits of the members generally are considered. I should consider them nearly all as purely technical workers in science, when they have done any work at all; but the curiosity is great when we find that the inquiry in question is purely technical. Professedly, it is a question of apparatus.

In entering upon an enquiry which I have endeavoured to keep within the limits of broad, tangible, and easily demonstrable facts, what qualities would common sense ask for in an investigator? Would an investigation be considered trustworthy were it conducted by a chemical dreamer who could spin off theory by the hour, and cover acres of paper with chemical symbols, but who in a laboratory would be unable to perform the simplest analysis, or build up a piece of chemical apparatus? Let it not, however, be supposed that I am unmindful of the philosophical and fructifying labours of Hofmann, Williamson, and others, in the field of Chemical Philosophy. But with reference to this enquiry, surely it should be conducted by one "who is trustworthy in an enquiry requiring technical knowledge for its successful conduct."

The reviewer assumes that the phenomenon of the suspension of heavy bodies in the air, the up and down movements of a wooden board, and the registration of the varying tension on a spring balance, are *psychical*, not *physical*; and he lays down a dictum that in such matter-of-fact results which I have obtained, one's own eyes must not be trusted, for in such a case "seeing is anything but believing." To show my unfitness for ascertaining the weight of a piece of wood, he accuses me of being ignorant of the knowledge of Chemical Philosophy! He does, however, from his Olympian height, condescendingly admit that my ability is *technical*, that I have made creditable use of my very limited opportunities, and intimates that I am trustworthy as to any inquiry which requires technical knowledge for its successful conduct. Now what does he mean by all this? I always thought that these qualities which are so contemptuously accorded me were just those of the highest value in this country. What has chiefly placed England in the industrial position she now holds but technical science and special researches?

But my greatest crime seems to be that I am a "specialist of specialists." I a specialist of specialists! This is indeed news to me, that I have confined my attention only to one special subject. Will my reviewer kindly say what that subject is? Is it general chemistry, whose chronicler I have been since the commencement of the "Chemical News" in 1859? Is it Thallium, about which the public have probably heard as much as they care for? Is it Chemical Analysis, in which my recently published "Select Methods" is the result of twelve years' work? Is it Disinfection and the Prevention and Cure of Cattle Plague, my published report on which may be said to have popularised Carbolic Acid? Is it Photography, on the theory and practice of which my papers have been very numerous? Is it the Metallurgy of Gold and Silver, in which my discovery of the value of Sodium in the amalgamation process is now largely used in Australia, California, and South America? Is it in Physical Optics, in which department I have space only to refer to papers on some Phenomena of Polarised Light, published before I was twenty-one; to my detailed description of the Spectroscope and labours with this instrument, when it was almost unknown in England; to my papers on the Solar and Terrestrial Spectra; to my examination of the Optical Phenomena of Opals, and construction of the Spectrum Microscope; to my papers on the Measurement of the Luminous Intensity of Light; and my description of my Polarisation Photometer? Or is my speciality Astronomy and Meteorology, inasmuch as I was for twelve months at the

Radcliffe Observatory, Oxford, where, in addition to my principal employment of arranging the meteorological department, I divided my leisure time between Homer and mathematics at Magdalen Hall, planet-hunting and transit taking with Mr. Pogson now Principal of the Madras Observatory, and celestial photography with the magnificent heliometer attached to the Observatory? My photographs of the Moon, taken in 1855, at Mr. Hartnup's Observatory, Liverpool, were for years the best extant, and I was honoured by a money grant from the Royal Society to carry out further work in connexion with them. These facts, together with my trip to Oran last year, as one of the Government Eclipse Expedition, and the invitation recently received to visit Ceylon for the same purpose, would almost seem to show that Astronomy was my speciality. In truth, few scientific men are less open to the charge of being "a specialist of specialists."

Whilst the scepticism of this reviewer in respect to the credibility of eminent witnesses, who give their names and detailed statements of definite facts, exceeds all reasonable bounds, his credulity in believing unattested statements of others, or in expecting his readers to give credit to all the absurd stories of his own experience, is refreshing in its simplicity. He gives five separate accounts of certain *séances*, where he saw something take place, but he condescends to few details; with one exception, no names or tests are given, nor is there a single clue by which the accuracy of his statements can be verified. The only case in which a name and anything like detail is given is an account of a visit to Mr. Foster. Amongst other strange things here recorded, but by no means satisfactorily accounted for, even by our reviewer, is the following:—

> "We were not introduced to him by name, and we do not think that he could have had any opportunity of knowing our person. Nevertheless, he not only answered in a variety of modes the questions we put to him respecting the time and cause of the death of several of our departed friends and relatives whose names we had written down on slips of paper which had been folded up and crumpled into pellets before being placed in his hands; but he brought out names and dates correctly in large red letters on his bare arm, the redness being produced by the turgescence of the minute vessels of the skin, and passing away after a few minutes like a blush."

The accurate answers to the reviewer's questions are supposed to be explained by "unconscious ideo-motor action," which, like "unconscious cerebration," is to explain all phenomena, past, present, and to come. Respecting the latter phenomenon, he says—"The trick by which the red letters were produced was discovered by the enquiries of our medical friends." If the reviewer will not believe my plain statement of facts fortified by eminent witnesses, how does he expect his readers to believe these statements on the simple word of an anonymous writer? His "gullibility," to use his own coarse, but expressive word, is strongly shown in his implicit belief of an obviously exaggerated account given by the well-known Robert Houdin of the way in which he and his son performed some of their tricks.

It is curious to note how Dr. Carpenter is made to pervade the *Quarterly Review* article. The reviewer throughout the article unconsciously manifests his implicit conviction that Dr. Carpenter is to be regarded as the paramount authority in reference to the subtle psychological questions involved in the so-called spiritualistic phenomena. The theories of the profound psychologists of Germany, to say nothing of those of our own countrymen, are made quite subsidiary to the hypotheses of Dr. William Carpenter. An unquestioning and infatuated belief in what Dr. Carpenter says concerning our mental operations has led the reviewer wholly to ignore the fact that these speculations are not accepted by the best minds devoted to psychological inquiries. I mean no disrespect to Dr. Carpenter, who, in certain departments, has done some excellent scientific work, not always perhaps in a simple and undogmatic spirit, when I "speak advisedly" that his mind lacks that acute, generalising, philosophic quality which would fit him to unravel the intricate problems which lie hid in the structure of the human brain.

Here I must bring this enforced vindication to a close. The self-reference to which I have been constrained is exceedingly distasteful to me. I forbear to characterise with fitting terms the spirit of this attack upon a scientific worker; it is enough that I have proved that in ten distinct instances the reviewer has deliberately calumniated me. It is a heavy and a true charge to bring against anyone occupying the reviewer's position amongst scientific men.

I cannot refrain from citing from the *Birmingham Morning News* the following trenchant criticism from the pen of an eminent chemist—himself a disbeliever in "Spiritualism." It will serve, as one instance amongst many, to show the feeling of disgust which the article in the *Quarterly Review* has excited among scientific men, whatever their opinions on this topic may be. After a few prefatory remarks, the writer goes on to say:—

"Either a new and most extraordinary natural force has been discovered, or some very eminent men specially trained in rigid physical investigation have been the victims of a most marvellous, unprecedented, and inexplicable physical delusion. I say unprecedented, because, although we have records of many popular delusions of similar kind and equal magnitude, and speculative delusions among the learned, I can cite no instance of skilful experimental experts being utterly, egregiously, and repeatedly deceived by the mechanical action of experimental test apparatus carefully constructed and used by themselves.

"As the interest in the subject is rapidly growing both wider and deeper, as a very warm discussion is pending, and further and still more extraordinary experimental revelations are in reserve, my readers will probably welcome a somewhat longer gossip on this than I usually devote to a single subject.

"Such an extension is the more demanded as the newspaper and magazine articles which have hitherto appeared, have, for the most part, by following the lead of the *Quarterly Review*, absurdly muddled the whole subject, and ridiculously mis-stated the position of Mr. Crookes and others. In the first place all these writers that follow the *Quarterly* omit any mention or allusion to Mr. Crookes's preliminary paper published in July, 1870, but which has a most important bearing on the whole subject, as it expounds the object of all the subsequent researches.

"Mr. Crookes there states, that 'Some weeks ago the fact that I was engaged in investigating Spiritualism, so-called, was announced in a contemporary (*The Athenæum*), and, in consequence of the many communications I have since received, I think it desirable to say a little concerning the investigations which I have commenced. Views or opinions I cannot be said to possess on a subject which I do not profess to understand. I consider it the duty of scientific men, who have learned exact modes of working, to examine phenomena which attract the attention of the public in order to confirm their genuineness, or to explain, if possible, the delusions of the honest, and to expose the tricks of deceivers.' He then proceeds to state the case of Science *versus* Spiritualism, thus:—'The Spiritualist tells of bodies weighing 50 or 100 lbs. being lifted up into the air without the intervention of any known force; but the scientific chemist is accustomed to use a balance which will render sensible a weight so small that it would take ten thousand of them to weigh one grain; he is, therefore, justified in asking that a power, professing to be guided by intelligence, which will toss a heavy body to the ceiling, shall also cause his delicately-poised balance to move under test conditions.' 'The Spiritualist tells of rooms and houses being shaken, even to injury, by superhuman power. The man of science merely asks for a pendulum to be sent vibrating when it is in a glass case, and supported on solid masonry.' 'The Spiritualist tells of heavy articles of furniture moving from one room to another without human agency. But the man of science has made instruments which will divide an inch into a million parts, and he is justified in doubting the accuracy of the former observations, if the same force is powerless to move the index of his instrument one poor degree.' 'The spiritualist tells of flowers with the fresh dew on them, of fruit, and living objects being carried through closed windows, and even solid brick walls. The scientific investigator naturally asks that an additional weight (if it be only the 1000th part of a grain) be deposited on one pan of his balance when the case is locked. And the chemist asks for the 1000th part of a grain of arsenic to be carried through the sides of a glass tube in which pure water is hermetically sealed.'

"These and other requirements are stated by Mr. Crookes, together with further exposition of the principles of strict inductive investigation, as it should be applied to such an inquiry. A year after this he published an account of the experiments which I described in a former letter, and added to his own testimony that of the eminent physicist and astronomer Dr. Huggins, and Serjeant Cox. Subsequently, that is, in the last number of the *Quarterly Journal of Science*, he has published the particulars of another series of experiments.

"I will not now enter upon the details of these, but merely state that the conclusions of Mr. Crookes are directly opposed to those of the Spiritualists. He utterly, positively, distinctly, and repeatedly repudiates all belief in the operations of the supposed spirits, or of any other supernatural agency whatever, and attributes the phenomena he witnessed to an entirely different origin, viz., to the direct agency of the medium. He supposes that the force analogous to that which the nerves convey from their ganglionic centres to the muscles, in producing muscular contraction, may, by an effort of the will, be transmitted to external inanimate matter, in such a manner as to influence in some degree its gravitating power, and produce vibratory motion. He calls this the *psychic force.*

" Now, this is direct and unequivocal *anti*-spiritualism. It is a theory set up in opposition to the supernatural hypotheses of the Spiritualists, and Mr. Crookes's position in reference to Spiritualism is precisely analogous to that of Faraday in reference to table-turning. For precisely the same reasons as those above quoted, the great master of experimental investigation examined the phenomena called table-turning, and he concluded that they were due to muscular force, just as Mr. Crookes concludes that the more complex phenomena he has examined are due to psychic force.

" Speaking of the theories of the Spiritualists, Mr. Crookes, in his first paper (July, 1870), says:—

" 'The pseudo-scientific Spiritualist professes to know everything. No calculations trouble his serenity; no hard experiments, no laborious readings; no weary attempts to make clear in words that which has rejoiced the heart and elevated the mind. He talks glibly of all sciences and arts, overwhelming the inquirer with terms like 'electro-biologise,' 'psycologise,' 'animal magnetism,' &c., a mere play upon words, showing ignorance rather than understanding."

" And further on he says:—

" ' I confess that the reasoning of some Spiritualists would almost seem to justify Faraday's severe statement—that many dogs have the power of coming to more logical conclusions.'

" I have already referred to the muddled mis-statement of Mr. Crookes's position by the newspaper writers, who almost unanimously describe him and Dr. Huggins as two distinguished scientific men who have recently been converted to Spiritualism. The above quotations, to which, if space permitted, I might add a dozen others from either the first, the second, or third of Mr. Crookes's papers, in which he as positively and decidedly controverts the dreams of the Spiritualists, will show how egregiously these writers have been deceived. They have relied very naturally on the established respectability of the *Quarterly Review*, and have thus deluded both themselves and their readers. Considering the marvellous range of subjects these writers have to treat, and the acres of paper they daily cover, it is not surprising that they should have been thus misled in reference to a subject carrying them considerably out of their usual track; but the offence of the *Quarterly* is not so venial. It assumes, in fact, a very serious complexion when further investigated.

" The title of the article is " Spiritualism and its Recent Converts," and the 'recent converts' most specially and prominently named are Mr. Crookes and Dr. Huggins. Serjeant Cox is also named, but not as a *recent* convert; for the reviewer describes him as an old and hopelessly infatuated Spiritualist.*

* It is due to Mr. Serjeant Cox to state that, so far from being an old Spiritualist, he had seen nothing of Spiritualism until he joined the Investigation Committee of the Dialectical Society, confident that he should thus assist in dissipating a delusion or detecting an imposture; but by that elaborate examination he was satisfied (as he states in his Report) that many of the asserted phenomena are genuine, but that there was no evidence whatever to support the theory of Spiritualism; that he was convinced by what he had seen that the Force was a purely psychical one and in no way produced by spirits of the dead. He is, in fact, a decided

Knowing nothing of Serjeant Cox, I am unable to say whether the reviewer's very strong personal statements respecting him are true or false—whether he really is "one of the most gullible of the gullible," &c., though I must express my detestation of the abominable bad taste which is displayed in the attack which is made upon this gentleman. The head and front of his offending consists in having certified to the accuracy of Mr. Crookes's account of certain experiments; and for having simply done this, the reviewer proceeds, in accordance with the lowest tacties of Old Bailey advocacy, to bully the witness, and to publish disparaging personal details of what he did twenty-five years ago.

"Dr. Huggins, who has had nothing further to do with the subject than simply to state that he witnessed what Mr. Crookes described, and who has not ventured upon one word of explanation of the phenomena, is treated with similar insolence.

"The reviewer goes out of his way to inform the public that Dr. Huggins is, after all, only a brewer, by artfully stating that, 'like Mr. Whitbread, Mr. Lassell, and other brewers we could name, Dr. Huggins attached himself, in the first place, to the study of Astronomy.' He then proceeds to sneer at 'such scientific amateurs,' by informing the public that they 'labour, as a rule, under a grave disadvantage, in the want of that broad basis of scientific culture which alone can keep them from the narrowing and pervertive influence of a limited *specialism*.' The reviewer proceeds to say that he has 'no reason to believe that Dr. Huggins constitutes an exception' to this rule, and further asserts that he is justified in concluding that Dr. Huggins is ignorant of 'every other department of science than *the small subdivision of a branch* to which he has so meritoriously devoted himself.' Mark the words, 'small subdivision of a branch.' Merely a twig of the tree of science is, according to this most unveracious writer, all that Dr. Huggins has ever studied.

"If a personal vindication were the business of this letter, I could easily show that these statements respecting the present avocations, the scientific training, and actual attainments of Dr. Huggins are most gross and atrocious misrepresentations; but Dr. Huggins has no need of my championship,—his high scientific position and the breadth and depth of his general attainments are sufficiently known to all in the scientific world, with the exception of the *Quarterly* Reviewer. My object is not to discuss the personal question whether book-making and dredging afford better or worse training for experimental inquiry than the marvellously exact and exquisitely delicate manipulations of the modern observatory and laboratory, but to protest against this attempt to stop the progress of investigation, to damage the true interests of science and the cause of truth, by thus throwing low libellous mud upon any and every body who steps at all aside from the beaten paths of ordinary investigation. The true business of science is the discovery of truth, to seek it wherever it may be found, to follow the pursuit through bye-ways and high-

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opponent of the theory of the Spiritualists, and has just published a book detailing his experiments, entitled "Spiritualism Answered by Science." The writer of the article in the *Quarterly* must have been quite aware of this fact, for he actually cites a passage from the letter to me in which letter Mr. Serjeant Cox expressly repudiates the theory of Spiritualism.—W. C.

ways, and, having found it, to proclaim it plainly and fearlessly, without regard to authority, fashion, or prejudice. If, however, such influential magazines as the *Quarterly Review* are to be converted into the vehicles of artful and elaborate efforts to undermine the scientific reputation of any man who thus does his scientific duty, the time for plain speaking and vigorous protest has arrived. My readers will be glad to learn that this is the general feeling of the leading scientific men of the metropolis; whatever they may think of the particular investigations of Mr. Crookes, they are unanimous in expressing their denunciations of this article in the *Quarterly*.

"The attack upon Mr. Crookes is still more malignant than that upon Dr. Huggins. Speaking of Mr. Crookes's Fellowship of the Royal Society, the reviewer says, 'We speak advisedly when we say that this distinction *was conferred on him with considerable hesitation*;' and further, that 'We are assured, on the highest authority, that he is regarded among chemists as a specialist of specialists, *being totally destitute of any knowledge of chemical philosophy, and utterly untrustworthy as to any inquiry which requires more than technical knowledge for its successful conduct*.' The italics in these quotations are my own, placed there to mark certain statements to which no milder term than that of falsehood is applicable.

* * * *

"If space permitted, I could go on quoting a long series of mis-statements of matters of fact from this singularly unveracious essay. The writer seems conscious of its general character, for, in the midst of one of his narratives, he breaks out into a foot-note, stating that '*This* is not an invention of our own, but a fact communicated to us by a highly intelligent witness, who was admitted to one of Mr. Crookes's *séances*.' I have taken the liberty to emphasise the proper word in this very explanatory note.

"The full measure of the injustice of prominently thrusting forward Dr. Huggins and Mr. Crookes as 'recent converts' to Spiritualism will be seen by comparing the reviewer's own definition of Spiritualism with Mr. Crookes's remarks above quoted. The reviewer says that 'The fundamental tenet of the Spiritualist is the old doctrine of communication between the spirits of the departed and the souls of the living.' This is the definition of the reviewer, and his logical conclusion is that Mr. Crookes is a Spiritualist because he explicitly denies the fundamental tenet of Spiritualism, and Dr. Huggins is a Spiritualist because he says nothing whatever about it.

"If examining the phenomenon upon which the Spiritualist builds his 'fundamental tenet,' and explaining them in some other manner, constitutes conversion to Spiritualism, then the reviewer is a far more thorough-going convert than Mr. Crookes, who only attempts to explain the mild phenomena of his own experiments."

For six months past false and injurious reports concerning me and my recent investigations have been assiduously circulated in scientific circles. Although aware of their existence and their origin, I forbore to take public notice of them, thinking that their inherent falsehood would weight them too heavily to allow them to float long. The appearance of the *Quarterly* reviewer's attack on me, however, appears to have encouraged my calumniator, and, emboldened by my prolonged silence, a letter was sent to the *Echo* newspaper signed "B.,"* in which the writer put in a definite shape some of these ugly rumours, giving as his authority a certain "Mr. J." Not caring to carry on a paper war with an anonymous slanderer, I demanded that the mask should be dropped, when Mr. John Spiller, F.C.S., came briskly to the front, and in the *Echo* of November 6th accepted the responsibility of "B.'s" calumnies, adducing in corroboration of them a long letter he sent to me six months before—a letter having no relation whatever to the falsehoods related by "B."

A reply to definite accusations, made by a man possessing a certain reputation in the chemical world, is imperatively necessary, and regard for my own reputation makes me decide that my vindication shall be neither halting in language nor doubtful in meaning. And first let me show how little Mr. Spiller knows of the subject on which he speaks so positively. He came to my house unexpectedly one evening in April last, when Mr. Home and some friends had been dining with me. On that occasion nothing worth recording took place: in fact, it was not until some weeks later that my accordion was purchased, and my experimental apparatus devised. Mr. Spiller, however, appeared so struck with the little he did see that he begged me to invite him on similar occasions as often as I could. Mr. Serjeant Cox having given me a general permission to bring to his house any gentleman who took an interest in the subject, in accordance with this permission I invited Mr. Spiller to accompany me on April 25th to a strictly private party, when Mr. Home was expected. Had I thought him capable of committing so gross a breach of the laws of hospitality and good breeding as to publish a garbled and untruthful account of what took place in the privacy of a gentleman's dining-room, I should certainly have considered him not included in that general permission. However, we assembled, and before sitting down it was agreed by the gentlemen present that any objection on the score of suspected trick should be taken at the time, so that it might be subjected to instant proof or disproof. To this condition Mr. Spiller fully agreed.

The meeting at Mr. Serjeant Cox's was not one of my series of "test *séances*," as Mr. Spiller tries to make out, but was purely private, and quite unconnected with the experiments described in the *Quarterly Journal of Science*. It was a preliminary trial, to enable me to judge what class of phenomena could be easiest verified, and what sort of test apparatus I should devise. Mr. Spiller was never present at any test experiments, and saw Mr. Home only on the two occasions I have mentioned.

During the meeting at Mr. Serjeant Cox's many striking phenomena took place, and Mr. Spiller, being a stranger, was specially invited by Mr. Home to examine everything to his heart's content, and move about or get under the table whenever he liked. In accordance with my usual habit of taking notes, I was writing the whole time when I was not scrutinising the occurrences, and it was, therefore, easy not only to take down a description of

* Echo, Oct. 31, 1871.

the phenomena as they occurred, but also to record the actual words or comments used by each person present. From time to time I repeated aloud what I had written, and asked the company if it were correct; when any correction was supplied it was invariably adopted. The narrative of the proceedings was written in full immediately after, and a copy was sent to Mr. Spiller, as well as to others who had been present, for them to approve or alter. Mr. Spiller has dignified this paper by the name of an affidavit, whereas it was purely a private memorandum, never intended to be made public, and only drawn up so that each person might possess a thoroughly truthful account of what was considered at the time to be a very remarkable series of occurrences.

I have before me the paper which Mr. Spiller returned, corrected in pencil, and each correction signed with his initials. Where he has not corrected it is clear that he tacitly assents. His objections are of an utterly insignificant kind, and, comparing what he accepts with what he rejects, it will be seen that he strains at gnats while he swallows camels.

It now appears that Mr. Spiller totally disregarded the agreement assented to by all present—to speak out at the time, and thus to invite and facilitate the most searching inquiry. He arrogates to himself the position of an infallible judge instead of an honest inquirer. Whilst he professed to act openly and above-board, he was really carrying on furtive observations of his own. He recklessly discredits the other witnesses who were present, and expects the world to believe his own unsupported assertion. Brought forward at the time, his observations might have been of service, whilst at this distant date they are valueless. Mr. Spiller seems to imagine that, whilst everything else in nature is to be tested by careful experiment, his own hasty conclusions are to be accepted unchallenged.

The first accusation launched at me by Mr. Spiller is of a suppression of the truth. I am said to have recorded certain phenomena in the *Quarterly Journal of Science*, and to have ascribed their production to the action of a hitherto unknown form of force, notwithstanding that Mr. Spiller had explained to me six months previously the "tricks" by which these things were done.*

From the various forms under which this accusation has been repeated it appears that Mr. Spiller is trying to establish, either that he was present at the test experiments on which my papers in the *Quarterly Journal of Science* were based, or that these papers were but a narrative of what took place in his presence at Mr. Serjeant Cox's. Now I have published no narrative whatever of any experiments at which Mr. Spiller was present, neither have I referred to them in any of my papers. His assertion, therefore, under whichever form it is viewed, is false.

In the *Echo* of November 10th I have gone fully into the analysis of these several accusations, and by placing in parallel columns extracts from Mr. Spiller's printed letters and statements, plainly convicted him in each case of a direct mis-statement of fact.

To show how ignorant I was of his reputed explanations of the few

* Echo, Nov. 6, 1871.

trifling things he thought he found out at Mr. Serjeant Cox's, and how unsuccessfully I begged him to give the information he now says I was aware of, I need only quote from a letter I wrote him on May 24th last. It runs as follows:—

"You have now for the third time given a very mysterious hint that you are in possession of a fact which would make me entirely alter my opinion about Mr. Home. Now I put it to you whether it would not be more consistent with our friendship for you to tell me fairly and candidly what you do know rather than keep me in suspense, week after week. You say it is impossible for you to write about it. That is a word I do not understand. If you will give me a plain statement of facts, and will not insinuate dishonest conduct on the part of myself and family, I promise you that I shall not only be very grateful to you, but will give what you tell me the most serious attention."

Mr. Spiller never came, and to my earnest appeal to put me in possession of his concealed facts I received no answer. And yet he has the audacity to say that I was perfectly aware of his explanation of the phenomena he witnessed!

But it is further reported that Mr. Spiller was my assistant during my test experiments, and found out at my house how the accordion "trick" was done.* Mr. Spiller was not my assistant, nor was he present at my house on any occasion when an accordion or any sort of apparatus was used. I refer to what he said about the only occasion when he ever saw an accordion in the same room with Mr. Home. I quote from a letter he wrote to me on May 3rd:†—"The accordion business [at Mr. Serjeant Cox's] was rather curious, but then I was *not* under the table at the time of 'The Last Rose of Summer' being played." After experience of Mr. Spiller's logical method I am not surprised at the inference that this is the same thing as being under the table and finding out how the trick was done.

It would occupy too much space to re-state the accordion problem, but I will refer all who are interested to my description in the *Quarterly Journal of Science* for July last. If Mr. Spiller has really found out how this "trick" is done, why does he not publish it? for he would then have solved one of the most puzzling problems ever presented to his notice—a problem still unsolved by far wiser heads than his.

Debarred by the editor of the *Echo* from making further use of the columns of that journal, Mr. Spiller retreats to the pages of the *English Mechanic*,‡ where he reiterates accusations the falsity of which I have before exposed by means of his own letters. He complains that his previous perverse mis-statements and personal misrepresentations have brought him under sharp criticism. Of course they have; but this criticism is simply a consequence of his own unwarrantable attack. I cannot argue with my detractor about psychic force, or the explanation of the phenomena recorded at my test *séances*, for the sufficing reason that he was never present at any of these experiments, and he has had no opportunity of knowing anything of the subject except from my published papers. Professing to criticise my investigations, he carefully avoids all reference to any of these papers, and keeps

* English Mechanic, Nov. 3, 1871.

† Published by Mr. Spiller in the Echo for November 6, 1871.

‡ English Mechanic, Dec. 1, 1871.

harping on a weak remark of his own about the size of what he calls a "monster" locket attached to Mr. Home's watch-chain. A stranger to the circumstances would imagine that something very important turned upon the exact dimensions and reflecting power of this trinket. But what are the facts? In his letter to me of May 3rd,* speaking of an accordion which he saw playing at Mr. Serjeant Cox's in Mr. Home's hand, Mr. Spiller says that he "saw a flash of light whilst under the dining-room table"—a reflection from the "*shining surface*" of this locket; and on October 31st† his friend "B." gives (and he endorses) an entirely different tale about this light, which we are now told for the first time "was playing about Mr. Home's fingers as they lay in his lap,"—produced by the reflection from the "*polished reverse side*" of the locket in question. Speaking for myself, I saw nothing of this alleged light, nor did Mr. Home draw attention to it. My part in the transaction was simple. Mr. Spiller was the critical observer under the table on this occasion, and all I did was to write down what he said. In my notes written at the time, and acquiesced in by nine witnesses, I read—"Mr. Spiller declared that the accordion appeared self luminous while it was playing." He subsequently denied this. He is welcome to do so, for it is a matter of no consequence whether he saw a light at all; the real question is, Did the accordion play and how was it played? Whether Mr. Spiller observed any light at all, the source of the light he said he saw, or the size of one of Mr. Home's trinkets, has nothing whatever to do with the subject of my investigations. The locket might be as big as a dinner-plate, and might be polished to the lustre of a speculum; the light it reflected might be as bright as the noon-day sun, and all that it would prove would be my calumniator's incompetency as an observer for not discovering it, or his inaccuracy as a witness for not mentioning it at the time when instant verification or disproof was possible.

Mr. Spiller speaks on one occasion of the "*shining surface*" of this locket; on another of its "*polished reverse side*;" whilst on a third occasion he draws attention to the fact that platinum is "a white metal sometimes used for reflectors." Now to these inconsequential assertions I will oppose facts. The locket in question is now before me. Its obverse and reverse are almost identical, and the whole is so covered with ornamental engraving that there is not a particle of polished platinum about it. Moreover, on each side there are fifteen raised metallic ornaments of different shapes, which still further diminish the amount of light reflected from the surface. I have, moreover, carefully examined the optical properties of this locket. Tested in an accurate photometer, the reflecting power of each side is found to be equal to that of a silvered glass speculum 1·8 millimetres (less than 1-10th of an inch) square! I advise Mr. Spiller to keep silent about this "monster" locket in future, or, like a second Frankenstein, he will find he has conjured up a monster from his own inward consciousness which will devour his reputation.

But, of all the unfounded statements which my disingenuous assailant has circulated, the most outrageous is that he has been threatened with legal proceedings‡

* Echo, Nov. 6, 1871.
† Echo, Oct. 31, 1871.
‡ English Mechanic, Dec. 1, 1871.

because he refused to sign the narrative I sent him of the proceedings at his *séance* at Mr. Serjeant Cox's. Now, although the intrinsic absurdity of such a threat, made under the very eyes of a serjeant learned in the law, must be patent to everyone, it is necessary for me to state, which I do in the most emphatic manner, that this disgraceful accusation is *totally untrue.* I have never threatened Mr. Spiller with legal proceedings; I have never given him the remotest hint of such a thing; never did such a thought enter my mind; and nothing that he has ever said or written in connection with this controversy could induce me for a moment to entertain the idea of legal proceedings.*

I hope I have now finished with the, to me, uncongenial task of combating perverse mis-statements and refuting personal misrepresentations; and that I may be able to devote myself once more to quiet research.

* Since this was written Mr. Spiller has been made to withdraw his accusation (English Mechanic, Dec. 22, 1871). The ungracious manner in which he eats his offensive words "*I was threatened with legal proceedings*" shows that his anxiety to say something spiteful has led him to say the thing that was not.

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I. METEORIC ASTRONOMY.

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Author of "The Sun," "Other Worlds," &c.

BY awarding their Gold Medal to Signor Schiaparelli, in recognition of his researches into Meteoric Astronomy, the Astronomical Society may be said to have definitely sanctioned the conclusions which have been deduced from Schiaparelli's propositions. These conclusions are of such extreme importance, whether as viewed directly, or regarded in relation to the inferences which seem to flow from them, that they may be regarded as affecting our ideas respecting the present constitution as well as the past history and the future fate of all the orbs which people space. I propose to take the opportunity afforded by the recent action of the Astronomical Society to consider the position to which meteoric astronomy has at present been brought, and to point out the connection between the results established by Schiaparelli, and the subject of the solar corona, which has recently occupied so large a share of the attention of astronomers.

We need not consider here the history of the earlier meteoric theories. It would be difficult to show that the more correct ideas of the Greek and some of the Roman writers who have spoken of meteors were less purely speculative than the later view that meteors are mere phenomena of our own air, like lightning or the aurora. And although the theory that meteors are bodies which have been expelled from lunar or planetary volcanoes was discussed by mathematicians of eminence, yet it was not based on exact observation; so that the calculations of Laplace, Obbers, and others, serve rather to illustrate the skill of those mathematicians than to establish any conclusions of value.

We may begin, then, by considering the first important fact tending to prove the extra-terrestrial nature of meteors and shooting-stars,—the circumstance, namely, that meteoric displays occur commonly on certain days of the year.

It had been noticed in very early times that a display of shooting-stars nearly always occurs on the night of August 10. This being known in calendars as St. Lawrence's Day, the meteors which fall on that day have been called the tears of St. Lawrence. Among astronomers, however, they are more commonly called the Perseides, for a reason presently to be cited. Not so early, but still many years before the true theory of meteors began to be recognised, it was known that on or about the 12th and 13th of November, shooting-stars are commonly seen. When Humboldt, after witnessing the remarkable display of 1799, invited special attention to this circumstance, ancient records were examined, and it was found for several centuries this particular part of the year had been characterised by star-showers. "Time out of mind," says Sir John Herschel, "those identical nights more often, but sometimes those immediately adjacent, have been habitually signalised by such exhibitions."

It cannot be too often insisted upon,—since doubts are frequently expressed respecting the truth of the modern theory of meteors,—that this circumstance of periodicity suffices of itself to demonstrate the extra-terrestrial nature of these objects. There are no meteorological phenomena which recur persistently on August 10th and on November 13th; terrestrial volcanoes are not, then, exceptionally active; the moon on those dates may be in any part whatever of her orbit. The one circumstance to which phenomena recurring on a particular date can be held to point is the recurrent passage by the earth of a particular part of her orbit on that date. If we picture the earth circling around the sun in her wide orbit, once in each year, and remember that year after year as she crosses the particular point corresponding to August 10th, and again as she crosses the particular point corresponding to November 13th, her air is alive (as it were) with meteors, we at once see that this is because she comes across the meteors at those stages of her circuit. It is precisely as though a person who travelled continually on a certain road, noticed always at certain stages some peculiarities, such as heat or damp, or the like. He would be certain, after a few experiences of the sort, that the phenomenon was local in its nature,—peculiar, in fact, to the particular part of the road where it was recognised. So we, who voyage with the earth on a wide path round the sun, must conclude—must be absolutely certain—that the two particular regions of circumsolar space which we traverse on August 10th and November 13th are swarming with meteors.

Yet we cannot for a moment imagine that two clouds of meteors are persistently present in these two regions. Each meteor is as surely acted upon by the sun's mighty influence as this earth on which we live; and as surely as this earth, if brought to rest in any way, would be attracted towards the sun and fall upon his globe in about $64\frac{1}{2}$ days, so every member of a meteor-cloud placed where the August and November meteors are encountered would in about the same time fall upon the sun and be destroyed.

It follows that the meteors must be in rapid motion, on a course keeping them clear of the sun's orb; and, moreover, that the place of those which pass away from the region traversed by the earth must be more or less continuously supplied by arriving meteors. In other words, the August and November meteors must form a more or less complete zone or ring. The degree of completeness of either ring must correspond to the regularity of the recurrence of star-falls on the dates corresponding to either system. If it frequently chances that the display is intermitted, either for a few years or for many years in succession, the inference will be that greater or less gaps mar the completeness of the meteor zone; whereas, if no year passes without a display of meteors belonging to a system, we must infer as at least probable that the meteor system forms a complete ring. Thus judged, the November system appears to be very far from forming a continuous zone; since the display is often omitted for more than twenty years in succession, and is seldom repeated during more than four or five successive years. The August system, on the contrary, seldom fails to produce a display of greater or less splendour.

So much may be regarded as demonstrated by the evidence already referred to. But to remove any doubt which may remain as to the justice of the inference that the August and November meteors are bodies travelling in a definite region of interplanetary space, there remains a test of a decisive nature.

Since, according to the hypothesis, all the August meteors are travelling along the same orbital zone, they must cross the earth's track on paths appreciably parallel, moving also with equal velocities. Now, regarding the earth with reference only to her orbital motion, we see that every meteor of the

August system which actually encounters the earth must reach the earth's globe from one and the same direction in space. If the earth were actually at rest, the meteors of this system would seem to fall like a shower coming from a definite quarter. This, indeed, might not appear to be the case to observers occupying a particular point on the earth's surface,—simply because the meteor-shower would be only partially discernible by him, and the circumstances would be such as to cause some illusion as to its origin and nature. But if we regard the earth as a whole,—and for a moment as a sentient being,—it will be obvious that, supposing she were at rest, she would seem to be exposed to a meteoric shower, many meteors falling upon her and yet larger numbers passing by her, but all appearing to come from the same quarter. But taking her orbital motion into account, we obtain a precisely similar result, only the direction of the shower becomes modified. The case may be compared to that of a railway carriage in a steady shower of rain; such a shower will appear steadily to proceed from one quarter of the sky whether the carriage is at rest or moving uniformly in a given direction. *Only* in the latter case, the part of the sky from which the shower seems to proceed will seem to be somewhat nearer to the point of the horizon towards which the train's motion is carrying the carriage. So with the earth passing through a meteor stream; the meteor-shower will seem to fall from one and the same part of the celestial sphere though the earth is travelling rapidly onwards; *only* the part of the heavens from which the shower seems to proceed will seem to be somewhat nearer to the point towards which the earth's orbital motion is carrying her than in the imaginary case of a fixed earth exposed to the continuous downfall of the meteors belonging to the August system.* But in either case the shower will seem to come from a determinate point of the star-sphere enclosing the earth on all sides.

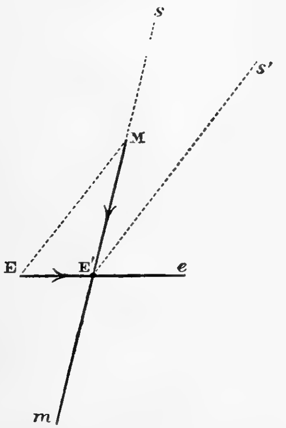
Similar remarks apply to the November shower.

* In considering this matter we need not concern ourselves with questions of perspective, which, though commonly introduced to explain the subject, tend rather to perplex than to enlighten the learner. The case is exceedingly simple in reality:—

Thus, let E *e*, Fig. 1, be a small part of the earth's orbit at the place where any meteor system is encountered, and let M *m* indicate a portion of the meteor-orbit, M E′ being the course traversed by a particular meteor while the earth is moving along E E′, so that this meteor encounters the earth when she has arrived at E′. Then to the observer on earth, unconscious of the motion from E to E′, the course on which the meteor seems to arrive is obviously in the *direction* of a line from M to E, so that a line, *m* E′, parallel to M E is the seeming path of the meteor's arrival; and a point *s*′ on the celestial sphere, in the

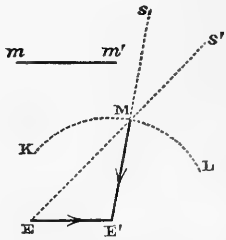
Plate I.

FIG. 1.



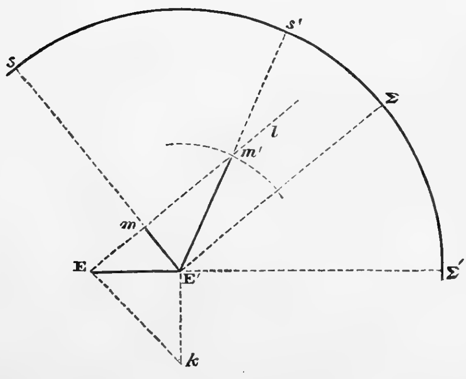
Illustrating the existence of meteoric "radiant points." (See page 140).

FIG. 2.



Illustrating the determination of the real course of meteors when their "radiant point" and velocity are known. (See page 144).

FIG. 3.



Showing how the arc on the heavens on which lies the true radiant of the August meteors can be determined by a simple construction. (See page 145).

Thus a test of the interplanetary nature of meteoric systems is at once afforded, and a test of a remarkably effective nature. For owing to the earth's rotation the star-sphere appears to rotate; and thus the part of the heavens whence a meteor shower seems to proceed is continually changing its place with respect to the horizon. *If*, notwithstanding this continual change of apparent place, a certain region of the stellar heavens continue to be that whence a meteor shower seems to come, the evidence is so much the stronger, because the test is applied under continually varying conditions.

In the case of both the meteor systems referred to, the test confirms the result already deduced. The meteors of each system seem to come from a definite region of the heavens,—or rather from a definite point of the star sphere. In the case of the August meteors this point lies towards the north-eastern part of the constellation Perseus, or, more exactly, the R.A. of the point is about 3 h. 24 m., its north declination about 55°. The November meteors appear to come from a point in the constellation Leo, between the stars ϵ and ζ of that constellation, or, more exactly, the R.A. of the point is about 9 h. 52 m., its north declination about 24°. These points are called the "radiant points" of the meteor systems, and because of the position of those points the August meteors are commonly called the Perseides, while the November meteors are called the Leonides.

It would be necessary to enter at this point on a consideration of the appearances actually presented by the August and November shooting-stars during any considerable display, if it were my present purpose to give a full account of what is known respecting meteors; but I wish at present to restrict the reader's attention as far as possible to the cosmical aspect of the subject.

The test here considered becomes, after it has served this

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direction E′ M′ indefinitely produced, is the point of the heavens from which that meteor seems to proceed. But the directions E′ E′ and M E′ are constant, and the proportion of E E′ to M E′ is constant; therefore the direction E M or E′ M′ is also constant. Hence every meteor of the system must appear to proceed from the point *s*′ on the celestial sphere.

In reality every meteor of the system travels parallel to M *m*, and therefore is proceeding (at the time of crossing the earth's track) as if from a point *s* on the celestial sphere in the direction E′ M indefinitely produced.

Since the visible part of a meteor's course in our atmosphere is a straight line (approximately) directed from one and the same point of the heavens (so far as a given meteor system is concerned), it follows that wherever the observer may be, that course must appear to be such that if produced backwards it would pass through that part of the heavens.

particular turn, the means of proving the existence of other meteor systems which produce no recurrent showers, or no displays so persistently recurrent as to leave us assured of their being due to a true meteor system.

For, if many meteors seen on any night appear to radiate from a particular part of the heavens, we can infer very probably, though not quite certainly, that those meteors form a single group travelling all in the same direction. We can infer this, because we know that the observed event would certainly happen if the meteors form such a group; but we cannot be quite certain that one group only is in question, because two meteor groups crossing the earth's track at the same place might have different directions and velocities so adjusted that the members of each would seem to encounter the moving earth in the same direction. The unlikelihood of the coincidence renders the inference that a single group is in question so much the more probable. Now, if on the same night of other years,—or very nearly at the same date,—more meteors are seen to proceed from the same radiant point, we conclude that not a group but a system is in question, since at the very spot where meteors were really travelling in a definite direction in one year, other meteors are found following in their track in later years. Many millions of miles must separate the first set from those seen later; and the inference is, that along the whole range of those millions of miles there are meteors travelling with equal velocity in one and the same direction,—in other words, traversing a definite orbit region.

Applying this principle, the observers of meteors—Alexander Herschel, in England; Professor Newton, in America; Secchi, in Italy; Heis, in Germany; Quetelet, in Belgium; and Schmidt, in Greece; besides many others—have established the existence of seventy-six distinct meteor systems having radiant points north of the equator; while Neumayer, at Melbourne, and other observers, have noted many others having southern radiant points. It may be concluded safely that upwards of 150 meteor systems cross the earth's orbital track around the sun. Counting,—as resulting probably from the existence of corresponding but more important systems,—those falls of fireballs and of aërolites which have been observed to occur on or near certain definite days, and taking also into account the probability that some meteoric systems have hitherto escaped recognition, we may infer that some 200 systems of bodies traversing the interplanetary spaces pass close to the earth's orbit.

It does not follow, however, be it noted, that any one of these systems centrally crosses the earth's path, whether we regard that path as the elliptic line traversed by the earth's centre or as the elliptic ring traversed by the earth's globe. On the contrary, the chances are that the earth does not pass quite or even nearly through the *core* of any meteor system belonging to the solar domain.

Now when Schiaparelli commenced his inquiries, absolutely nothing was known about the extent of any one of the meteor systems traversed by the earth. To take the August and the November systems, it was known that one part of each lies near the earth's orbit. It was known also that the August system crosses the earth's track at a considerable angle, since even though the earth's motion brings the radiant point down (as it were) nearer to the point towards which the earth is making on August 10th (a point close by the star δ Arietis), the radiant is still some 37° from the ecliptic. It was known further that the November system travels much nearer to the plane of the ecliptic, and in a retrograde course (that is, meeting the earth), because the radiant lies within 10° of the ecliptic, and almost in the latitude of the point towards which the earth is travelling on November 13th (a point close by ψ Leonis). But nothing whatever was known as to the orbit range of either system—the major axis of the mean path of these meteoric families. For anything that had been shown to the contrary (setting aside as too unsatisfactory to be trusted the estimates of the velocities of the meteors while traversing our atmosphere), the part of the systems traversed by the earth might be the portion farthest from the sun, and the perihelion portion might be quite close to his orb; or the part traversed might be the perihelion portion, and the aphelion portion might have any distance whatever,—from a range little exceeding the earth's mean distance to one exceeding many times even the distance of Uranus or Neptune. This ignorance as to the meteoric orbit-ranges was necessarily accompanied by ignorance as to the real velocity with which they cross the earth's track, and this in its turn involved ignorance as to the true inclination of either system. Given the major axis of either meteor orbit, the real velocity of the meteors at the earth's distance would be known; and since the earth's motion is known in velocity and direction, the apparent direction of the meteor's motion being also known, the real direction of the meteoric motions follows at once. That this is so can readily be demonstrated.

Thus, let EE′ be taken to represent the earth's velocity in magnitude and direction, and let *mm′* represent in magnitude the real velocity of any meteors which seem to come in the direction *s′* E; then we have only to describe the circular arc, KLM, around E′, with *mm′* as a radius, to obtain E′M the real direction of the meteor's motion. (Compare the note illustrated by Fig. 1). But this very construction shows us that so long as the real meteoric velocity, *mm′*, is unknown, we cannot tell the real direction, *s′*E, of the meteor's motion. The apparent velocity, ME, would indeed give us what we want (the point M in fact); but a meteor appears too suddenly and vanishes too quickly for exact reliance on any estimates of the velocity with which they traverse our atmosphere.*

Meteoric astronomy had reached this stage when Schiaparelli, who had already directed much attention to the subject, and had speculated somewhat boldly upon it, was led to compare the motion of the great comet of 1862 (No. III.) with that of the August meteors. Observing that this comet passed close to the earth's orbit on a course somewhat resembling that of the August meteors (on the assumption that they are moving with a velocity exceeding the earth's), he inquired whether the agreement would be found yet closer if the actual velocity of the comet where it passes close to the earth's orbit were assumed to be that with which the meteors are travelling when they enter the earth's atmosphere.

As this was the sole assumption made by Schiaparelli, it will be well to consider how far it affects the probability of inferences based on the coincidence actually recognised when this assumption has been made.

The real velocity of the August meteors cannot possibly exceed twenty-seven miles per second; since any greater, where they cross the earth's track, would give them a hyperbolic orbit, whereas it has been shown that they form a closed ring. Again, the real velocity cannot greatly fall short of the earth's, since otherwise the meteors of the system would appear to come from a point lying much nearer to that part of the heavens towards which the earth is travelling on August 11th than is actually the case.

Thus, let EE′ be a part of the earth's course on August

* In any exact investigation of meteoric motions the influence of our earth's attraction, as well as the effects of the earth's rotation,—the former producing a real, the latter an apparent, change in the paths of meteors—must be taken into account. These circumstances do not, however, affect the general reasoning given above, and their effects are unimportant compared with those due to the circumsolar motions of the meteors and of the earth.

10th, Σ' the point on the heavens towards which she is travelling at the moment. Let El be a line directed towards the radiant point of the August meteors, which lies about 40° from Σ'. Then E$'m$ perpendicular to El represents the least possible velocity for the meteors (where EE$'$ represents the earth's velocity). Coming from the direction of a point, s, in the heavens, with this velocity they would seem to radiate from Σ, E$'\Sigma$ being drawn parallel to El, since a meteor which was at m when the earth was at E would meet the earth at E$'$. Again, if we take Em', equal to the diameter of a square of which EE$'$ is a side (that is equal to Ek), this line will represent the greatest velocity a meteor crossing the earth's orbit can possibly have, if it is travelling in a closed orbit. This gives the direction, $s'm'$E$'$, along which an August meteor would actually travel, on the supposition that it has this maximum velocity. We may fairly assume that the meteors are not arriving on a course intermediate to mE$'$ and EE$'$, because then their relative velocity would fall much too far below the velocities estimated by Secchi, Newton, Herschel, and others.

From some point on the arc ss' upon the celestial vault, therefore, the August meteors are certainly travelling; and with some velocity intermediate to the velocities represented by the lines m'E$'$ and mE$'$, where EE$'$ represents a velocity of $18\frac{1}{2}$ miles per second. Moreover, the reader is not to suppose that the figure deals with hypothetical relations, and does not admit of being at once and definitely interpreted. Σ' represents a real point on the heavens, a point on the ecliptic close by the star δ Arietis, towards which, as already mentioned, the earth is actually travelling on August 10th. This point can at once be found on a celestial globe. Σ, again, is a known point, the radiant of the August meteors (already indicated), and about 39° from Σ'. It can, of course, be found on a globe, since its R.A. and declination have been given. Now let the end of a cord be held down on a globe at the point corresponding to Σ', and let the string be stretched over the globe across the point corresponding to Σ, so as to pass beyond Σ. It will indicate the position of the arc, $s's$, on the celestial sphere. It will lie in a circular arc of which $\Sigma\Sigma'$ will represent 39°. And if we take points on it corresponding to

* Because if v be the velocity, it can be shown that a body moving in a circle at a distance (d) from the sun, the velocity in a parabolic orbit will be $\sqrt{2}.v$, at a distance (d) from the sun. The earth's orbit is near enough to a circle to render this relation applicable; moreover, on August 10th, the earth is nearly at her mean distance from the sun.

s and *s*′; in other words, so that *s*′ falls, as shown in Fig. 3, about 26° from Σ, and *s* 90° from Σ, we shall have on the celestial globe the arc *ss*′, along which the true radiant of the August meteors must lie. In other words, the August meteors are really travelling, when they cross the earth's orbit, as if from a point, this point lying somewhere on an arc about 64° long, and having the same place upon the star sphere that the string arc corresponding to *ss*′ has upon the celestial globe.

Thus far there has been no assumption whatever. Moreover, without proceeding a step further, we can indicate a surprising coincidence which Schiaparelli was the first to recognise. The great comet of 1862 did beyond all question cross the earth's orbit from the direction of this very arc, *ss*′, that is, as if travelling from a point lying somewhere on this arc. The elements of the comet as determined by Oppolzer sufficed to demonstrate this. Thus, without any assumption whatever, it was proved that comet and meteors reached the part E′ of the earth's orbit along lines contained within one and the same sector of space, *s*E′*s*′.

But Schiaparelli had already indicated reasons for believing that the velocity of the meteors was nearly equal to the maximum (indicated in Fig. 3 by the line *m*′E′). What he now did was to point out that, granted this assumption only, the point whence the meteors are directing their course as they cross the earth's orbit lies close to *s*′ (on the side away from Σ, of course), and that the point whence the comet was directing its course when it crossed the earth's track had appreciably *the same position*,—the comet's velocity, on this assumption, being identical with the meteor's velocity.

So that to sum up—*Assuming nothing, it was demonstrated that a comet and a meteor system, crossing the earth's track at the same spot, have directions certainly so far agreeing as to lie in the same sector in space—that sector having an angle of only* 64 *degrees*; and, *assuming for the meteors a velocity corresponding to what had already been assigned as the probable value of this element, the comet and meteors cross the earth's track on one and the same course and with identical velocities.*

This in reality indicates the full extent of the coincidence recognised by Schiaparelli. We must not go further, as some have done, and say that the orbit ring of the meteors was further found to coincide perfectly with the orbit of the comets. Still less must we (*as an additional proof of coincidence*) set the elements of the orbits side by side, as though the agreement of the two in respect of eccentricity, inclination, longitude of node and of perihelion, and so on, afforded so many additional evidences of coincidence. The fact is, that setting aside Schiaparelli's assumption as to the meteoric velocities, there is no agreement as respects any of these elements except the longitude of the node. There is a further coincidence not directly expressible in the ordinary orbit elements, and this coincidence, as indicated above, is sufficiently striking. It justifies, in fact, the assumption of equal velocities. Then this assumption being made, and leading to the coincidence of the actual direction of the two orbits where they cross the earth's, the exact coincidence of the two orbits, in all respects, follows inevitably,—since two bodies moving with equal velocities and in the same direction as they cross one and the same point in interplanetary space, must, by the law of gravity, follow identical orbits.

Or the matter may be otherwise expressed thus:—It follows from a discussion of the motions of the comet of 1862, that if the earth had been close by the place where her orbit is crossed by the comet when the comet actually traversed that place the comet's course would have seemed to be directed from the radiant of the August meteors. So much is *certain*. Schiaparelli inferred that the actual course of the comet as respects velocity and direction was identical with the course of the August meteors as they traverse the earth's orbit. We have already had occasion to consider the probability of such an inference, in speaking of the probable association between all the meteors which on any night appear to have the same radiant. The chances are great against the coincidence being accidental.

If I were here dealing with the history of meteoric astronomy, I should have to give a full account of the recognition of a corresponding resemblance between the motions of the November meteors and those of Tempel's comet (No. I., 1866). In particular, it would be desirable to discuss the share which Professor Adams took in the work. I notice with regret, by the way, that in Dr. Schellen's useful work on "Spectrum Analysis," the labours of Adams are left wholly unrecognised, while the comparatively less important researches of Schiaparelli, Oppolzer, Peters, and Leverrier, are pointedly referred to. This is not the place to supply the omission; but I may remark that if we set aside the labours of Adams, the only circumstance in which the discussion of the November meteors differed from that of the August meteors were *these:* there was, first, a reason for assigning a particular period to the November meteors, in the circumstance that great displays of these shooting-stars occur at mean intervals of about $33\frac{1}{4}$ years, and assigning a period amounted to assigning the velocity with which the meteors cross the earth's track; and secondly, the comet which agrees in its motions with the November meteors was dealt with separately, and the agreement only recognised after both orbits had been independently worked out. But the choice of $33\frac{1}{4}$ years as the period of the November meteors was still an assumption; and there were many eminent astronomers who regarded 34-33rds, or 32-33rds of a year as the more probable period. In any case, assuming a period, the task of determining the orbit from the observed position of the radiant was mere mathematical child's-play. But Professor Adams achieved a much nobler work. For he proved from the observed displacement of the node of the November meteors,—that is, of the point where the meteors cross the earth's track,—that the period *must* be $33\frac{1}{4}$ years or thereabouts. This was a task which none but a mathematician of the highest order could possibly have accomplished: and even to such a mathematician the task was a most laborious one. I do not say that Schiaparelli's case was not made out without Adams's assistance. The chances against the observed agreement as a result of accident were as great in the case of the November meteors as in the case of the August meteors, and the chances against both coincidences being accidental were therefore overwhelming. But Adams undoubtedly removed whatever element of doubt still remained; and, furthermore, if Schiaparelli's theory had never been started, Adams's work would of itself have sufficed to establish the true figure of the orbit on which the November meteors travel. It has seemed fitting to say thus much in recognition of the remarkable labours of our great English mathematician; but it need hardly be said that the value of Schiaparelli's ingenious researches and theories is in no way affected by the matter here referred to. The great importance of Schiaparelli's work consists in the light which it throws on cosmical problems of extreme interest and difficulty.

The fact, then, is demonstrated that two of the meteor systems encountered by the earth are so far associated with two comets as to travel on the same orbits. We may not unsafely infer that all the meteor systems encountered by the earth are in like manner associated with other comets. Nor is it very rash to assume that all comets are in like manner associated with meteor systems. Two cases may seem insufficient as a basis for such wide generalisations as these; but it must be remembered that they are the only two cases we have yet been able to deal with satisfactorily. It should be remarked, however, that some other comets have passed close by the earth's orbit, and that in several cases meteoric radiant points have been recognised which correspond fairly with the assumption that those comets are followed by meteor trains. However, as nothing has yet been proved respecting the relative length of meteoric trains following after comets, we cannot expect that the track of every comet should to any great distance be thus followed; and where the distance is relatively small, no evidence of the kind just referred to would be obtainable even a year or two (perhaps) after the comet had crossed the earth's orbit.

And here a word or two may be permitted on the question of the condition of comets freshly arriving on the scene of the solar system. It is assumed sometimes that the train of meteors already exists when the comet first comes within the solar domain. But, however this may be in other cases, there can be no question that in the August and November meteors, the train, at least the train we are acquainted with, must have been formed after the comet had become a member of the solar family. This will be evident if we consider how this last-mentioned result can alone come about. Take the case of the November meteors, the figure of whose orbit shows that the parent comet was forced to become a regular attendant of the sun by the disturbing influence of Uranus. Now, the circumstances under which a comet approaching the sun on a parabolic or hyperbolic orbit can be thus affected must be regarded as exceptional. The planet's influence must in the first place be very energetically exercised; in other words, the arriving comet must pass very close to the planet; for under any other circumstances the sun's influence so enormously outvies the planet's that the figure of the cometic orbit would be very little affected. Moreover, the planet's attraction must produce an important balance of retardation. The planet will inevitably accelerate the comet up to a certain point, and afterwards will retard it; the latter influence must greatly exceed the former. To show how greatly the comet must be retarded, it is only necessary to mention that the actual velocity of the November meteors, when they cross the orbit of Uranus, is less than one-third of the velocity with which Uranus himself travels; but their velocity at the same distance from the sun, when they were approaching him from some distant stellar domain, exceeded the velocity of Uranus in his orbit in the proportion of about 7 to 5. So that, roughly, their velocity at that distance has been reduced in the proportion of more than 21 to 5, or by 7-9ths of its original value. This is a reduction of about 4½ miles per second. Now Uranus could barely impart this velocity to a body approaching him from an infinite distance under his sole influence, and coming as near to him as the nearest of his satellites; but he could not impart so much additional velocity to a body approaching him (more rapidly of course) under solar influence. Now the velocity a body can impart it can also take away. Hence the retarding action of Uranus could under no circumstances account for the velocity of about 4½ miles per second, by which the motion of Tempel's comet must have been reduced, unless that comet passed closer to Uranus than his nearest satellite. Indeed, the maximum velocity which Uranus could impart to a body actually reaching his surface (and exposed to his sole influence in approaching from infinity) amounts only to 13·7 miles per second. Now the distance of Uranus's nearest satellite from the planet's surface is but about 84,000 miles, and Uranus traverses such a distance in less than five hours. So that the first and last meteors so influenced by Uranus as to be forced to part with a velocity of 4½ miles per second out of their actual velocity of about 6 miles per second, were assuredly not separated by a distance equal to a five hours' voyage, or in miles (at 6 miles per second) by so much as 108,000 miles. We may, indeed, safely infer that the actual distance was much less than this. For though all the meteors along a distance of 108,000 miles might have their velocity sufficiently reduced, yet in this case some of the meteors would have their velocity too much reduced, and would thenceforth pursue orbits differing very markedly from the orbits traversed by the remaining members of the system.

It follows, not merely as a probable inference, but I think as a demonstrated conclusion, that if the November meteors came originally into our system as a comet travelling sunward from infinity, then either that comet was very compact, or else Uranus captured only a small portion of the comet, the remaining portions moving thenceforth on orbits wholly different from the path of the November meteors.

There is no escape from this conclusion; for no other planet than Uranus can have brought about the subjection of this comet to solar rule.

One is almost led to doubt the extra-planetary origin of the November meteor system altogether, and to entertain the theory that the comet which produced it was in the first instance expelled from Uranus. We need not conclude, if we accept such a view, that the period named by Leverrier for the introduction of the comet into the solar system (A.D. 126) was necessarily the epoch of the expulsion of the comet by Uranus. The true epoch of this event may have been far more remote.*

This, at any rate, is certain, that if the comets belonging to our system have been introduced, as is commonly supposed, by planetary perturbing influences, then the actual number of arriving comets must have indefinitely exceeded the number captured in this way. For an arriving planet has a million chances of escaping for one of being captured,—so closely must it approach to one of the major planets if its motion is to be sufficiently controlled to cause it to become a permanent member of the solar system, with a perihelion near enough for the inhabitants of earth to recognise the comet, and with an aphelion not greatly beyond the orbit of the disturbing planet.

But be this as it may, the connection between meteors and comets remains an established fact, the existence of many comets in the solar system is a reality, and the fact that the earth encounters more than a hundred meteor systems cannot be disputed.

Now, if we consider what proportion of interplanetary space the earth really traverses, we shall begin to recognise one of the most surprising of the conclusions which are deducible from these demonstrated facts. If the earth were really, as she is sometimes pictured, a globe so large that in an ordinary picture of her orbit around the sun she would be presented, to scale, as a considerable sphere, there would be nothing very remarkable in the circumstance that on her course round the sun she should come into contact with many meteor systems. But when we are reminded that on so large a scale that the earth's orbit would be represented by a circle *ten feet* in diameter, the earth herself would be but about the 190th of an inch in diameter, so that the path her globe actually traverses would be represented by a circle 10 feet in diameter, and marked in with about as stout a stroke as the down strokes of the letters in this page, we see at once how minute a portion of sun-surrounding space this ring-orbit really occupies. Remembering

* It is also possible to account for the present position of the November meteor system by supposing that the encounter took place during that remote period when Uranus (according to the nebular theory) occupied a much larger region of space than at present.

that nearly all the meteor systems encountered by the earth travel on orbits largely inclined to the plane of the ecliptic, and, therefore, cross that plane in two spots (unequally distant in nearly every case from the sun), it will be seen how wonderful the circumstance is, that some 200 such spots should fall on the fine circle which forms the earth's true track in the ecliptic.

Fig. 4 is intended to illustrate the remarkable nature of this circumstance. Here the circle $E_1 E_2 E_3 E_4$ represents the earth's path around the sun, E being the place of the earth at the time of the winter solstice. If we suppose the width of the circular line $E_1 E_2 E_3 E_4$ reduced to about a thirtieth part of its present value, we may regard this circle as picturing the actual shape of the region traversed by the earth. Now we know very little as to the real extent of the seventy-six meteoric rings traversed by the earth; but the cross section of each (regarded as cylindrical where crossing the ecliptic) may average perhaps a million or a million and a half of miles. According to the inclination of each and the actual direction of motion, the cross section with the ecliptic plane will be an ellipse of greater or less eccentricity, and having its longer axis in such and such a direction (which may be any whatever), with respect to the direction of the earth's orbit at the place of transit. These elliptical sections must, therefore, have some such arrangement as is depicted in Fig. 4. And it may be noted that for my present purpose, it is a matter of no importance whether the cross sections of the meteor rings be exaggerated or the reverse: since, on the one hand, if the cross sections are really larger, the importance of the several meteor zones is enhanced, but the circumstance that any given meteor zone is encountered by the earth is rendered *pro tanto* less surprising; while, on the other hand, if the cross sections are really smaller, we must infer that the number of meteoric zones is proportionately greater, to give the earth a reasonable chance of encountering so many systems. As to the shape of the cross section, it matters little what opinion we form; only if the real cross section is circular, the cross section on the ecliptic must have different oval shapes as shown in Fig. 4.

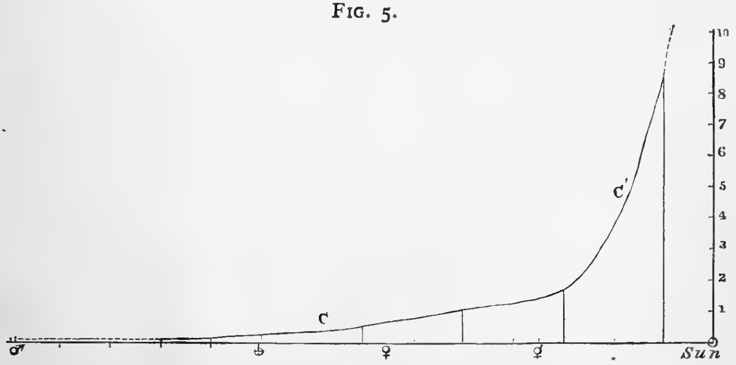
So far, then, as the meteoric systems which cross the ecliptic *descendingly* are concerned, we may fairly assume that their cross sections on the ecliptic form a scheme somewhat resembling what is pictured in the figure. It is known that southern skies are as plentiful in meteors as northern skies, if not more so; hence we must infer that as

Plate II.

FIG. 4.

Ideal view of the ecliptical cross sections of the seventy-six meteor systems having northern radiant points. (See page 152).

FIG. 5.



Illustrating the increase in the richness of cometic distribution with approach towards the sun. (See page 154).

many cross sections as are depicted in Fig. 4 should be added (all overlapping the circle $E_1 E_2 E_3 E_4$) to represent the meteor systems which cross the ecliptic *ascendingly*, or from south to north. Moreover, an addition should be made for meteor systems which have hitherto escaped notice, as well as for a considerable number (perhaps nearly as many again as all yet mentioned) which, because they strike the earth's course on its inner or sun-illumined side, fall on the earth where day is in progress, and so escape recognition altogether. Add also the more sparse systems of "heavier metal" which produce fire-balls, and those others which have supplied the countless myriads of aërolites which are known to have fallen on the earth. I think it will be granted, that if all these circumstances were taken into acccount in Fig. 4, the earth's orbit, as there pictured, would be absolutely encumbered with the oval spots representing the cross sections of meteoric systems.

Now let it be remembered that each of the cross sections corresponds to a long stream of meteors, if not to a complete zone, the meteors travelling around orbits compared with which the orbit $E_1 E_2 E_3 E_4$ has utterly insignificant proportions. Imagine the seventy-six cross sections of Fig. 4 replaced by seventy-six curves carried on various arcs, even only across the circular space $E_1 E_2 E_3 E_4$ (that is, leaving out of account altogether those portions of the systems which lie farther away from the sun). Conceive the like done for about twice as many more systems corresponding to the orders just mentioned. This considered, it will surely begin to appear that the *sphere* of space around the sun, which $E_1 E_2 E_3 E_4$ will represent, is occupied in a remarkable manner by interlacing meteor-comet systems.

But it is certain that the earth's orbit is not clustered round with meteoric cross sections in the peculiar manner depicted in Fig. 4. That all those cross sections are there cannot be questioned, since so many northern meteor systems have been recognised. But outside and inside $E_1 E_2 E_3 E_4$ there must be meteor cross sections which the earth does not traverse. To suppose otherwise is as though a person who had traversed a certain route in a rain storm, should suppose that no rain had fallen to right or to left of his track. There is nothing in the earth's orbit to attract meteors. She herself has not the attractive energy necessary either to compel meteors to approach her track or to retain them against other attractive influences. It is only by chance, as it were, that her track lies thus through those special hundreds of meteor systems,—precisely as it is only by chance that such and such rain drops fall upon our illustrative traveller.

Hence, we have no choice but to suppose that the whole plane space within the circle $E_1 E_2 E_3 E_4$, as well as an immense plane extent of space outside that circle, is as richly bespread by meteor systems as we have seen to be the case with the circular track $E_1 E_2 E_3 E_4$—that is, much more richly than as pictured in Fig. 4. And here, too, we recognise the small importance of the extent we have given to the meteor cross sections in Fig. 4. Since, if the extent of each were smaller, we should have to cover the space within and without with greater numbers of these smaller cross sections, in order to account fairly for the fact that the earth's track lies across so many of them.

But when we remember, further, the connection which Schiaparelli has shown to exist between meteor systems and comets, we find a reason for believing that not merely a uniform degree of meteoric richness continues inwards from the earth's orbit up to or near the very place of the sun, but that a great increase of richness takes place as the sun's place is approached. Mr. Dunkin, in his valuable Appendix to Lardner's "Astronomy," presents the actual densities with which the perihelion points of observed comets are distributed throughout sun-surrounding space. He gives the following table:—

Distance from Sun in Millions of Miles.	Number of Perihelia.	Relative Cubical Space.	Density of Perihelia.
0 to 20	8·65	1	8·65
20 to 40	11·70	7	1·67
40 to 60	20·50	19	1·06
60 to 80	17·20	37	0·47
80 to 100	20·80	61	0·34
100 to 120	8·65	91	0·10

Here it will be observed that the increase of density with approach towards the sun is very rapid indeed in the sun's immediate neighbourhood. It is represented by the ordinates of the curve cc' in Fig. 5, the dotted part of the curve being that inferred from the shape of the part which has been determined by observation. Moreover, it should be noticed that comets having their perihelia between 40 and 100 millions of miles from the sun are, on the whole, more likely to be recognised than those whose perihelia lie nearer to the sun.

We may fairly infer that the law indicated here for comets is that which holds also for meteor systems. Now let the following reasoning be carefully noted:—The members of a meteor system in travelling towards their perihelion open out their ranks, as it were, because travelling there with continually increasing speed, so that a given time difference between the place of two meteors corresponds to a continually increasing distance. We overrate this opening out in taking it as proportional to the square of the distance from the sun. Now the volumes of spherical spaces around the sun vary as the cube of the distance within which they are enclosed. Hence, *cæteris paribus*, the richness of meteoric distribution around the sun being proportional to $\frac{\text{number}}{\text{volume}}$, must vary inversely as the distance. Thus, a set or group of meteors, which at a distance of 90 millions of miles (about equal to the earth's), would spread with a certain degree of richness, would at a distance of 10 millions of miles be spread nine times more richly. Now, the above table shows that at a mean distance of 10 millions of miles (taking this as corresponding to the limits 0 and 20 millions,—an assumption very unfavourable to the argument) we have a density of perihelion distribution represented by 8·65 as against a density of only 0·34 at a distance of 90 millions of miles. Thus the density of perihelion distribution is about $25\frac{1}{2}$ times greater at the former distance; and the actual mean meteoric density is about ($9 \times 25\frac{1}{2}$, or) 230 times greater. Further, the illumination of these meteoric bodies at the lesser distance is 81 times greater, since this illumination varies as the square of the distance. Hence, under equally favourable conditions, the total quantity of light reflected from meteors within a given considerable space, at a distance of 10 millions of miles, exceeds that reflected from a set within an equal space at the earth's distance, in the proportion of 230×81 to 1, or upwards of 2000 times. Nearer to the sun than this still enormous distance the quantity of reflected light must be vastly greater; and if any meteors become incandescent owing to the great heat to which they are exposed, the total amount of light from these sun-surrounding regions must be yet further increased.

It should be noticed that the only assumption which has been made in the above reasoning is so far in accordance with the evidence actually obtained that any other assumption would have a considerable weight of probability against it. For if Schiaparelli's discovery has any cosmical importance at all,—and every one admits that it *has*,—it implies that all comets are followed by meteoric trains.

The reader has only to turn to Fig. 4 and to conceive the meteoric cross sections marked in all over the circular space $E_1 E_2 E_3 E_4$ with that degree of increasing richness *sunwards* which has been indicated above (remembering also, that as at present drawn, the figure shows less than half the real number of cross sections overlapping the earth's orbit), to perceive how richly sun-surrounding space must be crowded with meteoric systems. For not the ecliptic plane alone, but every plane through the sun, must be similarly intersected.

Albeit we must not forget that the meteor-systems severally are almost infinitely tenuous. It has been calculated by Professor Newton, of America, that even on the occasion of the great display of November meteors in America, in 1867, the portion of the system actually traversed by the earth contained only one meteor in 900,000 cubic miles of space: that is, in a cubic space nearly 100 miles long, broad, and deep, so that even taking into account the greatly increased richness close by the sun, we have not to deal with a real crowding of cosmical material; but, on the contrary, with an excessive tenuity, using this word to indicate the relation between the quantity of matter (however distributed) and the volume of the space within which it exists.

The reader will doubtless have surmised already the special purpose which I have had in view in the preceding inquiry. It seems to me that we have, in meteoric phenomena, as well as in the associated phenomena of comets, the explanation of some at least of the features presented by the solar corona. I cannot see how, on the one hand, the irregularities of structure which the corona presents at great apparent distances (up to two or three solar diameters, for instance) can be accounted for except by the theory that during eclipse the complicated network of meteor systems becomes discernible; nor, on the other, how the meteor systems can by any possibility escape recognition when a total solar eclipse is seen under favourable atmospheric conditions.

It has been supposed that, because I have advocated another theory in explanation of other features of the corona, I have abandoned the meteoric theory which I had formerly advocated. It is true that, in general, to advocate a new theory implies that a theory formerly held has been abandoned. But, in the present instance, this is not the case. The solar corona is a complicated phenomenon, and presents features which are severally due to different causes. Its irregularities may reasonably be attributed to one cause, while such features as the straight radial extensions, rifts, and so on may be (or rather must be) ascribed to another. It may be compared, in this sense, with the aurora. In explaining the general nature of this phenomenon, we may call into our aid the meteoric particles continually descending, in an irregular manner, through the upper regions of air; but in accounting for the auroral streamers, we have to consider processes taking place along straight (possibly along radial) lines.

It has been objected to the meteoric explanation, that the parts of the corona near the sun do not present the appearance which we should expect to recognise in meteors close by the sun, and are furthermore much brighter than we could expect even the innermost parts of the meteor region to appear. In this reasoning the circumstance seems to be overlooked that the meteor light which seems to come from regions close by the sun (assuming the meteoric theory to be true) does not come wholly, nor even chiefly, from meteors really so close to the solar orb. We look through a range of meteors two or three hundred millions of miles deep (taking into account space beyond the earth's orbit), and it is the combined effect of the light coming from the whole of this enormous range that we really recognise,—*not* in the corona, but in that proportion of the coronal light which is due to sun-illuminated meteors. The part of the range nearest to the sun may be the part most densely crowded and most brilliantly illuminated; but its extent is limited compared with that of the whole range; moreover, the meteors *there* situated turn but half discs (of reflected light) towards the earth, those beyond showing a much larger proportion of their illuminated halves. It is worthy of notice, indeed, that the farther half of the range supplies much the largest proportion of the light, on account of the greater fulness of illumination,—for in such a case as this, distance *per se* is an element which may be absolutely neglected.*

It need scarcely be pointed out that the spectroscope affords the best means for testing this question. If any considerable proportion of the corona's light is reflected from

* This at first sight may sound paradoxical; but it is strictly true nevertheless. The question is one of the apparent brightness of certain regions of the heavens, not of the total quantity of light received from given groups of meteors. A group of bright objects so far off as to appear like a cloud would preserve its *brightness* absolutely unchanged however far off the observer might remove. Its extent alone would diminish.

meteors, this portion of the light should exhibit a solar spectrum, though of great faintness; or, unless great light-gathering power were employed, a faint continuous spectrum would be seen. The zodiacal light, also, should exhibit a continuous spectrum if it represents the outer portion of the sun-surrounding meteor families. Until within the last few months the coronal light had been known to give a continuous spectrum as well as certain bright lines (or one bright line); and it had been stated that the zodiacal light gives a bright line spectrum. The first evidence was questionable; the second seemed opposed to the meteoric theory. But Janssen has examined the coronal light with the most powerful light-gathering means yet employed, and he recognises the solar dark lines in its spectrum. This evidence is unquestionable. And Liais, in the clear skies of tropical South America, has examined the zodiacal light and gets an infinitely faint continuous spectrum, so that what seemed a strong objection to the meteoric theory is removed. Let us pause, however. Liais has been charged with drawing an ideal picture of the corona during total eclipse, (his drawing by the way singularly countenancing the meteoric theory.) But it was ideal: how, then, shall Liais's evidence be trusted on any other subject? What, however, if it was not ideal at all; but simply *characteristic*, because Liais observed the eclipsed sun under singularly favourable conditions at his southern stations in 1858? This is precisely the inference fairly deducible from (or rather the conclusion forced upon us by) the evidence of the observers of the recent eclipse. Janssen speaks of special forms resembling those seen by Liais; observer after observer speaks of complicated structures within the corona; the photographs tell the same tale; and, lastly, the skilful artist Harrison, specially employed to ban the meteoric theory, has blessed it instead, his drawing as described by his friend Mr. Lockyer (so long the advocate of the atmospheric glare theory) "strongly recalling" the long suspected representation of the corona by Liais.

In conclusion, I believe little question can exist that a large proportion of these phenomena which have seemed most perplexing as well in the solar corona as in the zodiacal light, admit of being very readily explained when studied in the light of these theories of Schiaparelli's, which, after the usual term of doubt, have so recently received the sanction of the highest astronomical tribunal in Great Britain.

II. THE COPPER MINES OF CHILI.

By James Douglas, Quebec.

As the produce of the Chili mines now regulates the price of copper all over the world, and all speculation as to its future price must depend on the probable future yield of these mines, their condition is a subject of prime importance to all interested in the copper trade. I have therefore thought that the following information, derived in great measure from personal observation during a visit made in the latter part of last year to several of the principal mineral districts of Chili, would not be unacceptable to your readers.

All the copper comes, with the exception of but a trifling quantity, from the coast range, and from within 30 miles of the sea, and nearly two-thirds of it from the three great mineral districts of Tomaya, Carrizal, and Chañaral. From the Cordillera of the Andes little is extracted, partly by reason of the drawbacks to mining at high elevations, where for half the year the mines are closed by snow, and where at all times hard work is impeded by difficulty of breathing, and partly by reason of the heavy freight to the coast. But apart from these obstacles, the copper deposits of the Andes have as a general rule been delusive, offering most tempting surface indications of great wealth, which further operations have not realised, while the ore is generally contaminated with other metals, whose separation is often difficult, and which depreciate the value of the copper. A small quantity of copper comes from the Cajon de Maïpu in the Cordillera of Santiago, and the Condes Mines in the Cordillera of the same province produce 200 tons or so of 23 per cent ore annually. The only Cordillera mines which augment notably the production of Chili and the Cerro Blanco—which though situated at the base of the Cordillera, a little south of the Copiopó, prove their relationship to the Cordillera mines by yielding arseniurets of copper and silver and lead ores; and the Esploradora mines of Mr. Sievert, in the Atacama Desert, 120 miles inland to the east of Pau de Azucar.

Proceeding from the south northward I shall briefly describe the several mining regions, and the quantities of copper they severally produce.

South of Santiago very little copper is found. A number of small mines are worked both in the Cordillera and in the coast range; but their total yield falls short of 1000 tons of fine copper annually. Crossing the line of 33° S. latitude there are upon the same parallel, extending from the coast to the Cordillera, a series of important copper deposits—beds and lodes. It includes the mines on both sides the Melon Valley and the Catemo and San Felipe Mines. The metals from the hills bounding the Melon Valley are simple copper ores; those from the Catemo and San Felipe mines yield a little silver. The San Felipe ores are a grey sulphide; but as a rule the lodes are narrow. An exception occurs in the Paral Mine on one of the Coimas group of lodes, where a lode of a yard wide yields on an average a 30 per cent ore; but as the mine is very badly worked, and the stopes are far below the foot of the shaft, twice as many men are employed in pumping as in breaking the ore. The mines throughout this whole district are languishing, and the total amount produced does not exceed 3000 tons a year. In all probablity this yield will not be maintained. Most of the ore is made into regulus and bars at smelting establishments in the Melon and at Catemo. In the San Felipe Valley Urmeneta and Errasuriz have attempted to use the peat—which is here abundant—for smelting, but as yet without advantage.

Travelling northward through the province of Aconcagua and the southern parts of the province of Coquimbo, one crosses chain after chain of hills, running E. and W., divided from one another by fertile, well-watered valleys. The hills are so saturated with copper that a *desmontes* or refuse heap enters as a conspicuous object into almost every bit of mountain scenery, and innumerable slag heaps in many a nook and corner mark the spots where furnaces smelted the ore from neighbouring mines till the hill sides, to the serious detriment of agriculture, had been denuded of timber, when mining and smelting together necessarily came to an end, on account of the heavy cost of transporting on mules the poor metals to the coast. Were the Coquimbo Railroad extended through Combarbola to Illapel, this region would again grow into some importance; but this is an event little likely of accomplishment.

When we reach the river Limari, near Ovalle, we come in sight of the Hill of Tomaya, the most southerly of the great Chili *minerales*. It is an isolated mountain, some 3 to 4 miles long, whose summit is 3000 feet above the plain, and 4200 above the sea. Its steep sides, furrowed with deep ravines, rise to a rugged top; where, viewed from a distance, the rocks seemed heaped pile on pile, as if, a stronghold of the Titans, the hill had been overwhelmed and buried by the missiles of an opposing host of giants—a string of white dots, the houses of the several establishments, about two-thirds of the way up, marks the position of the adits driven on the great lode, and long white streaks reaching far towards its base the enormous piles of *desmontes*, whose total amount probably exceeds 200,000 tons; and as one approaches nearer the hill looks as if a net had been thrown over it, the cords in their regular reticulations being the roads zig-zagging up the almost perpendicular sides.

There is not a spring of water on the hill, and the mines are so dry that they do not supply the needs of the establishments. The Piké mine alone has expended as much as 600 dols. a month, for water-jigging in the English still hutch is all the water concentration that can be effected.

The *minerale* contains three systems of N. and S. lodes:—

(1.) The most easterly lode runs near the eastern base of the hill. It yields very little copper.

(2.) The middle is the great Tomaya lode. It consists of twin veins, with a N. and S. strike and dipping west at an angle of 60°. They vary in distance from each other, being sometimes 5 fathoms apart, at others uniting to form large bunches. The eastern lode is left standing because too poor to work; but the largest stopes are at spots where the E. and W. lodes come together.

(3.) The third system of lodes is on the western slope of the hill. It was in working a mine on this lode that Mr. Urmeneta commenced amassing the fortune which the Piké mine has helped to swell; but the production from here has always been small compared with that derived from the middle lode.

This, the second group referred to above, crops out so near the summit as to form almost the crest of the hill. It runs as a twin lode for from 3 to 4 miles; but when its outcrop descends the N. and S. slopes of the ridge, it breaks up into a number of stringers, which diverge from the N. and S. course. The lode has not been traced as productive beyond the hill on either side.

It is therefore from this isolated hill that a great proportion of all the Chili copper came from the years 1860 to 1865; for Carrizal and Chañaral did not produce then what they do now.

The principal mines on the Maslir lode proceeding from south to north are:—

1. Almagro 2. Pizarro	Of 400 varas each in length along the strike of the lode. They were both rich at surface, but have fallen off in depth.

3. Piké or Pïque of 400 varas. This mine is and always has been the most productive on the hill. It is owned by Don José Tomás de Urmeneta, whose perseverance in prosecuting the work upon it during years of heavy expenditure and disappointment has been rewarded by raising him to the highest rank among successful miners, and by enabling him to confer vast benefits on his country; for Urmeneta was the first man to introduce into Chili first-rate hauling and other mining machinery.

For some fathoms below the outcrop the mine yielded carbonates and other oxidised ores. To these succeeded mixed purple and yellow sulphurets, which gave place to yellow only, about the 80-fathom level. This has become more and more mixed with specular iron and carbonate of lime the deeper the workings have been carried, so that the ore has become steadily poorer at the same time that the cost of extraction has increased.

A banded structure is very distinct in the lower levels. From the floor of the lode there is—

I. A clay selvage.

II. A layer of almost pure specular iron ore.

III. A layer of pure yellow sulphuret.

IV. The rest of the lode consists of yellow sulphuret mixed with quartz, carbonate of lime, and specular iron.

The yield of the lode from wall to wall is from 8 to 10 per cent, and its average size varies from 3 to 6 feet, but in places it bulges to vastly greater size. The great riches of the Piké were derived from some enormous stopes at about 60-fathom level, where the lode expanded to over 20 feet in width, and yielded a purple ore, which, as it came from the mine, averaged 30 to 35 per cent. It is supposed Urmeneta netted in one year at that time from this mine alone 1,100,000 dols.

The underground works consist of an adit level, run from the western face of the hill, which cuts the lode at about 60 fathoms below its outcrop. All above this level is now abandoned to *piqueneros* or tributers. The ore from below is hauled, by means of a Corliss engine and admirable machinery fitted with friction gearing, through three inclined shafts, which attain a depth of 80 fathoms below the end of the adit; but the lowest level is nearly 60 fathoms below this again. The ore is raised this last 60 fathoms on the backs of *Apéres* (ore carriers) and by hand winches. The lowest level of the mine is therefore about 200 fathoms from surface. The ladder shafts and all the galleries are inconveniently low and narrow. The stopes being very full and the ground insecure, the strain upon the timbering is tremendous, and at every yard you meet pieces bent or broken; and as the habit has been to repair the damage by simply adding others without removing the old ones, the passages have been undergoing a gradual diminution in size. At present little is being done to improve the condition of the mine, as the Lecaros Adit is just completed, and henceforth it is intended to draw the metals out by it. This work was commenced as far back as 1840 by Don Ramon de Caros. It enters the hill on its southern flank, at a spot where the lode was supposed to crop out. It was driven but slowly and irregularly till 1864, when Urmeneta bought the work already done and continued it more vigorously. It connects with one of the levels of the Piké at about 180 fathoms from surface. This level has been run under the neighbouring mine, the Chalaca, so that the total length of the adit is about 1000 fathoms, nearly half the length of the hill at that level. It is intended to sink a perpendicular shaft from the level of the adit, as the cost of keeping the inclined shaft in repair is found to counterbalance any advantage it offers. As it is expected that the cost of tramming the metals to grass will be much less than the present cost of raising them, it is intended through it to empty the stopes, many of the older of which are filled with 7 per cent ore, and to break down from the walls large quantities of metal deemed formerly too poor to mine. By drawing on these vast reserves the Piké will be able to keep up its supply for many years to come, even if the percentage of the ore from the workings should fall off. There are now employed in the mine 50 miners, but a larger number of tributers find employment in the upper workings.

Urmeneta is driving another adit from the same slope of the hill, but at a much lower level than the Lecaros. This is, however, being done only to secure certain setts which he owns to the west, on the dip of the lode, in obedience to the Spanish mining law, which requires a certain number of men to be employed on work tending to the development of the claim.

In this mine another hardship of the Spanish mining law in force in Chili is illustrated, though in this instance it redounds to M. Urmeneta's interest. His workings have undermined the neighbouring sett on the same lode. It is a small claim of only 200 varas in length by 100 varas in width. M. Urmeneta owns the claim to the west. Now as the lode dips W. at 60°, and the claims do not follow the lode, but are measured perpendicularly downward, M.

Urmeneta owns and is working the Chalaca mine in depth from the lower levels of the Piké. Even if he did not own the sett to the W. he might undermine his neighbour, for anyone may run into the adjacent mine, if his shaft be the deeper of the two, and break what metal he can, the only reparation to the injured man being one-half of the profits. The reason of these anomalies is that as Government levies an export duty of 5 per cent on all minerals, the law offers every facility for their acquisition. All mines belong to Government, which gives the first comer a title to them without charge, the first condition being that he works them, otherwise his title drops.

4. Chalaca mine of 200 varas, virtually worked out.

5. Rosario 6. San José	Both great mines, and deeper than the Piké; both yielded at the same depth purple sulphuret, and both have suffered the same decline in the produce of the ore as we have described in the Piké.
7. Dichosa 8. Guias 9. Morculago	The Dichosa once produced largely, but north of it the lode breaks up, and though tributers make profits very little regular work is done upon the branches.

The total production of these mines is about 1250 units daily. Of this about half comes from the Piké; and of this half may be said to be extracted from the regular workings below the adit level, and half by tributers from the abandoned stopes, or by ore pickers from the refuse heaps. Half of this yield leaves the hill either hand-picked or jigged to 25 per cent; the other half will stand at about 12 per cent. The scarcity of water is greatly felt and occasions great loss here as at all the other great mines. M. Urmeneta expects to drain from his adit enough to run a concentrating establishment near its mouth, in which beside a pair of Huot and Guyler's beautiful piston hutches there are twenty English hutches to be worked by hand and twenty by steam. He even hopes to run two buddles, but appearances hardly justify the hope.

Every mine has its crushing and concentrating establishment, in which generally a Blake's breaker, and one or more pairs of rolls and sizing sieves prepare the ore for English hutches. At the old Piké establishment Petherick hutches are used, but they do not give satisfaction. At the Rosario, Mr. Lipkin collects the concentrated stuff on the sieve, though the hutch is in other respects like and is worked similarly to the ordinary still hutch. Considering the scarcity of water, I think some form of Rittinger's *Pump-*

Sitze would answer, if slightly modified by the addition of endless chains to discharge the scimpings and concentrated stuff. All attempts at slime concentration have resulted unsatisfactorily. Krupp steel jackets are in general use for the rolls, and are admitted by all to be in every respect economical. A pair will remain in perfect working order for over a twelvemonth, neither pitting nor wearing unequally.

The *desmontes* are enormous. Those of the Piké are the largest and probably the poorest. They originally yielded 6 to 7 per cent, but having been picked over four times probably do not now contain over 4 per cent. Their size may be judged of by the fact that now in their fourth *bouleversement* they are completely overwhelming a good-sized village built on one of the shelves of the hill.

The hands employed in this *minerale* in every capacity number about 4000. As all ages and sexes work, this represents a population of about 8000. Urmeneta employs about 600, and as many more work on his property as tributers.

The rate of wages is for common labour 12 dols. a month, and rations worth 15 cents a day. Miners (native) 18 dols. a month, and rations of 15 cents a day. The same high rates approximately rule throughout all the mining regions of Chili. Cornishmen alone can be trusted with the timbering, and they are even better paid; so that it is evidently a mistake to suppose Chili owes her mining importance to cheapness of labour.

A railroad 36 miles long, connects the mines with the coast at Tongoy. It runs along an almost level plain to Cerillos, at the base of the hill, where, if the shareholders had been wise, they would have made the terminus. Thence it ascends by grades as steep as $5\frac{3}{4}^\circ$, and curves as sharp as 187 feet radius, to the very mouths of the mines.

About 7 miles in a north-easterly direction, following the road, lies the monster lode of Panulcillo, forming also the crest of a ridge, along whose summit it rises like a wall when looked at from the south. The Tomaya people say that Providence placed these great deposits almost side by side that the ores of the one might serve to flux those of the other, but human perversity and English stupidity interfered to frustrate the kind intention.

The Panulcillo lode runs N. and S., and dips at first slightly to the W., and then perpendicularly. It is traceable, though barren, for over a mile to the S., and is visible as a distinct ridge a long way to the N.; but its productive portion is not very extensive. It has been opened—by an adit, driven on the southern extension of the lode, at a point nearly a mile distant from the workings, but the lode proving barren the work was dropped; by another adit, the San Gregorio, likewise to the south of the mine, but much closer, as it commences at the foot of the steep hill on which it exists; it is being pushed forward as the ground through which it cuts is improving; by open workings along the outcrop whence carbonates are still extracted; and by two E. and W. adits, through which the ore is now brought to surface by a steam engine in one case, and a horse whim in the other.

The two excavations, known as the North and South Mines, are separated from one another by a cross course, which does not displace the lode. In depth the lode seems to cut through it. The largest stope is in the North Mine, 400 feet long, 200 feet high, and in places 100 feet wide. The principal stope in the South Mine is almost as enormous. The workings more resemble a narrow rocky defile with steep sides, connected here and there by natural bridges, than the stopes of a mine. Paths are cut into the perpendicular cliffs, along which you climb by the aid of chains or a hand-rail, every now and then crossing by the arches left to support the walls. Into one of these immense caverns the light streams through an opening to surface in the roof, producing a most picturesque effect, which is heightened by the echoes of the miners' blows, and the plaintive chant with which they accompany their work.

The western wall of the lode is well-defined. On it lies a clay selvage; then a layer of galena of varying thickness, and carrying a little silver; then the main mass of the lode, which consists of a solid mixture of common iron pyrites, magnetic pyrites, and garnet, with copper pyrites and a small quantity of black-jack. Not more than 10 per cent of what is broken is thrown away in picking, and there is nothing left behind in the stopes. The yield of copper is now about 4·6 per cent, 1 per cent less than it was three years ago.

The cost of breaking, picking, and delivering the ore at the works is low, otherwise so poor an ore could not be worked. Spalling and picking is done for $1\frac{1}{4}$ cents a quintal metrico = $12\frac{1}{2}$ cents per ton. At this rate good hands make 18 dols. a month. Driving is done by contract at 40 dols. a fathom. In stoping the principal cost is in fracturing the huge masses which are dislocated from the lode by each blast. It is intended to try dynamite for that purpose. In stoping the miner is paid 1 cent per inch for boring. Englishmen superintend the native barretero, indicating the spots where holes are to be made.

The picked ore is heap roasted at the mine, but very rapidly and imperfectly, as the smelting works are constantly ahead of the mine.

The smelting establishment was formerly at the mine's mouth, but has now been transferred to the railroad terminus in the valley, which bounds the hill to the south. The ore descends to it by an inclined tramway: for a certain distance the descent is steep enough to enable the full waggon to haul up the empty one; over the rest the hauling is done by horses. The cost of the ore delivered at the smelting works varies from 2·50 dols. to 3·00 dols. per ton.

There are now sent daily from the mine to the works 100 tons; formerly the daily yield was double that quantity. There has happily been more than a corresponding diminution in the number of hands employed, there being now at the mine and establishment 500 to 1300 formerly.

The smelting establishment consists of ten large reverberatories, and four blast furnaces, erected last year by Charles Lambert, jun. Only three of the reverberatories are running, and these exclusively on the finer ore unsuitable to the blast furnace. Each reverberatory smelts three charges daily of 90 cwts. each, and produces a 45 per cent matt, with the consumption of 1 ton of coal to 3·5 of ore. The 8-tuyere blast-furnaces smelt each on an average 50 tons daily, with the consumption of 1 of coke to 7·5 of ore. So fusible is the ore that the slags do not contain over 1-10th of 1 per cent of copper. The Panulcillo ore is mixed with about an equal part of richer carbonate before being smelted, a necessity that might be partly avoided by a more careful preparatory calcination.

The management in every department of the establishment is admirable; and had the same economy and prudence reigned in days gone by when the price of copper was better and the yield of the ore higher, immense profits, instead of heavy losses, would have been made. As it is, even in the past depressed state of the market, under Mr. Weir's superintendence, the mine has held its own, and now should make most profitable returns. It is the only property in Chili worked by an English Joint Stock Company with an office in England, and though it has suffered from the absence of that close scrutiny which is generally exercised only when a property is under the eye of its owners, now with Mr. Heatley in Valparaiso, and Mr. Weir at the mine, and a good price for copper, the enterprise ought to take a new lease of life.

A branch of the Coquimbo and Ovalle Railroad terminates near the smelting works. This railroad, in addition to the large yield from Panulcillo, used to carry considerable quantities of ore from Las Cardas, Cerillos, Tambillos, Andacolla, and other stations, all of which are now producing so little that the profits of the road have dwindled to nothing.

Mineralogically one of the most curious copper deposits in Chili is at Andacolla. A large bed of indurated magnesian clay is more or less permeated with a black sulphuret of copper. In drying the clay evidently cracked, and gave rise to innumerable narrow fissures, which became filled with the copper constituents of the bed. This has since undergone a very beautiful transformation in these veinlets near surface into silicates and carbonates; at greater depths into black oxide, red oxide, and metallic copper. In one of these miniature lodes one sees sometimes a central thread of undecomposed sulphuret, bordered by successive bands of black and red oxides and metal.

So soft is the ground that powder is rarely used in mining it. But the saving in this item is almost compensated for by the cost of timber. Stoping is out of the question. All the ore is extracted from galleries, which are timbered as fast as they are driven, and remain open for a fortnight or so, when the ground crushes in the timber tunnel, and the miner may commence driving again in the same spot.

The mines are situated in an arid plateau, famous from of old for its gold mines. The people, therefore, are accustomed to the use of the *batea*, or conical wooden dish of the gold washer, and therefore adopted it for the concentration of these ores.

The ore as it comes from the mine is sorted, the rich vein stuff separated from the bed stuff and the latter crushed to one-fourth of an inch. Women squatted round long tanks, wash the crushed ore in the *batea*, extracting less than one-half of its total contents, which, however, they raise to 25 per cent. A woman will wash 2 tons in twelve hours, extracting about 2 cwts. of concentrated stuff. When copper commanded a good price, even this wasteful method of washing (the only one practicable from water) gave most profitable returns; latterly, however, the pecuniary results have been less favourable, and the yield of the *minerale* has become insignificant.

The next mine worthy of notice is the Brillador, belonging to Charles Lambert. It is the nearest to the sea of all the Chili copper mines, being not over three miles in a straight line from the northern sweep of the Bay of Coquimbo.

This may be the reason of its having been more extensively worked than any other mine in the Indian and Spanish periods. Indian burrows—for they were nothing else—18 inches square, were often met in the older workings of the Farellon mine. From these narrow passages alone they extracted the ore, as they were never found to terminate in a larger excavation. Stone and copper hammers are still turned up in the refuse heaps, identical in shape and mode of attachment of the head to the handle with those from the Indian workings in the Lake Superior mines. There are three mines on an east and west lode, with a dip to the south, which can be traced for many miles, but is productive only at three points, where three huge dykes form the north wall of the lode. Adjacent to these dykes occur the chimnies of ore which have made the mines so wealthy.

The lode, like the great Tomaya lode, is a double vein, the richer being the southerly, the poorer the northerly. They appear to bulge alternately, but where they run together there such bunches occur as that from which Mr. Lambert, in 1847, is reported to have made 1,000,000 dols. profit. The northerly lode is left standing.

Of the three mines, work has always been most vigorously prosecuted on the first and third—the Farellon and Panteon mines; the intermediate mine, the Bronze, never having been very rich.

The Farellon is worked to a great depth. At surface it yielded carbonates, which gave place at once to yellow sulphuret without the interposition of the vitreous ore, which yielded the great riches of Tomaya. All these old workings are quite abandoned, and the ore is now extracted by an adit level, which strikes the lode at over 100 fathoms from its outcrop. From the end of this adit a shaft is sunk for 80 fathoms on the inclination of the lode, which carries in the lower levels from 1 to 2 yards of solid yellow ore. Although the ore is cut off to both the east and west by cross courses, there are so many fathoms of solid metal in view, that one cannot doubt but that if the Brillador were again actively worked, it would again assume its old rank among Chili mines. Neither Tomaya nor Carrizal can show anything to compare with the face of ore in the deepest workings of the Farellon. The realisation of Mr. Lambert's confident anticipation of a rise in the price of copper may induce him to work his famous mine with the force it deserves.

The Panteon Mine, at about a mile distant, once yielded handsomely; but the lode was lost long ago during Mr.

Lambert's residence in Chili, and all his skill and the expenditure of a couple of hundred thousand dollars failed to re-discover it. The Panteon is a huge quarry, from which from prehistoric times till to-day carbonated ores have been mined. The ores were, before Mr. Lambert came into possession, smelted principally in a neighbouring valley. The slag heaps were virtually heaps of regulus, as the smelter of those days utilised only the bath of metallic copper, which resulted from a single fusion of the rich carbonates, and was as ignorant of the mode of treating the rich slag as he was of calcining and smelting sulphuretted ores. To this day not only do the old *desmontes* of the Panteon supply the furnaces of the Compañia (Mr. Lambert's Works, near Sirena) with carbonates, but the old Spanish slag heaps are being still overhauled.

Mr. Lambert built the first reverberatory furnace in Chili, and first smelted sulphuretted ores, which previously had been thrown aside as unserviceable. The Farellon Mine also was the first mine worked by stopes.

The dykes which, as before said, form the north walls of the productive portions of the lode, are themselves filled with little veins, which decrease in produce and size as they recede from the lode.

Fourteen leagues north of Sirena is the *minerale* of Higuera, several lodes of yellow sulphuret in a clayey gangue, of very intermittent yield. The ores are smelted at the mines and on the coast at the port of Totoralillos.

This is the last extensive *minerale* in the Province of Coquimbo. Soon after crossing the line of the Province of Atacama, and before reaching the river of Huasco, lies the *minerale* of San Juan, consisting like most other *minerales,* of a group of copper lodes and a number of mines which have been worked from time immemorial, but have never taken rank with the great Chili mines. The most important mines are those now worked by Messrs. Harker and Dickson, at Lebrar.

If the traveller crosses the valley of Huasco, at Vallenar, he enters the southern extension of the desert of Atacama, known as the plain of Algaroval. A range of hills separates this plain from the sea much as the coast range of Central Chili confines the great valley of San Fernando. In these hills at a distance of about six leagues in a straight line from the coast, has always been known to exist, but has been worked vigorously only within the last fifteen years, the great *minerale* of Carrizal, which sends more ore to market than even the hill of Tomaya.

There are, with a general direction of north-east and south-west, six lodes, only one of which, however, gives importance to the *minerale*. This is the *Veta Principal*. It forms the very crest of the hill of Carrizal, and then extends across the valley to the south-west, after which its direction is somewhat altered. On the hill it is wide, though broken, and there occur the richest mines. In the valley to the south-west it is narrower and more compact, yet yields well; but to the north-east, after the highest crest of the hill is passed, the mines are not producing profitably. On this lode there are 16 setts, yielding monthly about 4000 tons of 13 per cent ore; but most of this comes from six mines, the Mondaca, Remolinos, Portazuela or Bazanillo Alto, Toro, Cantado, and Santa Rita, all adjoining and situated on the crest of the hill or on its slope towards the valley.

To the north are two other lodes—the Veta del Agua and the Veta Malakoff, on both of which setts have been taken, though the lodes are unproductive, and shafts are being sunk by owners of mines on the Veta Principal in order to secure that lode in depth; another instance, in addition to that afforded by the Urmenita adit at Tomaya, of the detriment the law does to mining by involving the miner in useless expense in order to secure the permanency of his mine. The shafts being sunk to the north of the Mondaca mine should strike the Veta Principal—the nearer, the Margerita shaft, at 130 fathoms; the further, the Sebastopol shaft, at 165 fathoms. Unless these shafts strike good ore, showing that the run of poor ground now existing in the lower levels of the principal mines on the Veta Principal is local and partial, the *minerale* will rapidly decline, for none of the other veins show signs of great productiveness.

To the south of the Veta Principal and near at hand is a small lode, the Veta Santa Rosa, probably only a branch of the Veta Principal.

Then at a considerable distance on the southern slope of the hill are the Veta Greusa, and at its base the Veta Lachos. On the former ten setts have been secured, which yield perhaps 100 tons a month of 13 per cent ore, but only one is covering expenses; on the latter the same number of mines are being worked, and two only are returning profits.

The Veta de Verdiones, furthest to the south, is like the Veta Malakoff, large but unproductive.

The deepest workings are in the Portazuela Mine, whose shaft has reached a depth of 270 fathoms on the inclination of the lode, but the most productive mine is the Mondaca.

Its shaft attains a depth of 220 fathoms. An aneroid barometer indicated a vertical depth of 1000 feet. The total length of the shafts, wings, and levels is about 7 miles. There are employed on Company's account fifty *barreteros* (miners); and 150 tributers work on their own account in the abandoned upper workings.

The mine is supplied with good hauling machinery, worked by a 15 horse-power engine. In all the mines in this *minerale* great care is taken to condense the steam. So perfectly is this done, that at the Mondaca 5000 gallons monthly replace the loss in making steam and supply the wants of the establishment.

The mine is admirably worked. The levels are straighter, the shafts loftier, the timbering is better, and the exploratory work is further ahead of the stopes than in any of the Tomaya mines, thanks to the wisdom of the principal owner, Don Ramon Ovalle, and the skill of the manager, Mr. McAuliff. But the same is generally true of all the large mines in this region; due probably to the fact that they have been systematically worked only since the introduction of European methods of mining.

At surface and for some 60 fathoms below the lode is narrow; but here a branch, supposed to be the Veta Santa Rosa, falls into it, and it bulges suddenly to tremendous size. So at intervals it alternately contracts and expands: here diminishing to a yard in width; there bulging to six or seven times that size. The largest excavation in the Mondaca mine is 260 feet deep, 180 feet long, 45 feet wide. Twenty-one miners have worked abreast upon a solid face of ore. At about 120 fathoms from surface magnetic pyrites comes to preponderate so largely that for many fathoms the lode is left standing; but good yellow sulphuret of copper has reappeared in the central chimney of ore, and it is hoped the mine will resume its former richness. No banded structure is observable. The copper pyrites, mixed with quartz, magnetic pyrites, common pyrites, and a trace of blende, being mixed irregularly through the lode. A clay selvage occurs on the floor of the lode, and on the hanging wall a mineral resembling compact asbestos.

Although the lode is not producing as well as in the upper levels, the yield has not fallen off, so great are the reserves of good ore left to draw upon. The following table of the expense and production from 1862 to 1870, kindly furnished me by Mr. McAuliff, gives a good idea of the immense amount of work done and copper produced from a single large Chili mine :—

Date.	Annual Outlay.		Annual Production in Quintals Metrico.		Percentage.	Cost of Extraction per Quintal.
Year.	Dols.	Cents.	Dols.	Cents.		Cents.
1862	113,981	06	224,789	70	15½	51
1863	118,150	75	248,999	43	15½	48
1864	158,661	20	352,453	16	15½	45
1865	157,986	31	240,317	18	15	66
1866	145,449	13	261,590	44	15	56
1867	140,409	76	245,473	08	$13\frac{4}{10}$	57
1868	117,952	43	178,018	39	15	66
1869	119,329	37	203,000	13	13½	59
1870	159,702	00	266,000	00	13	60
	1,231,622	01	2,220,641	51		

The metrical quintal is equal to 1·971 English cwts.

About 4000 quintals monthly of rich oxidised ores are still extracted from the outcrop of the lode by tributers, consisting of carbonates and small quantities of black and red oxides, and oxychloride.

The Portazuela and Bazanillo Alto, owned by Messrs. Gonzales and Templeman, yields about as much. The same deterioration in depth has, however, there taken place as in the Mondaca.

Desmontes heaps cover the whole north-west face of the hill. They contain probably 300,000 tons of 3 to 4 per cent ore. The metal as broken from the lode probably carries about 6 per cent of copper, for it takes 3 tons to yield 1 ton of 13 per cent stuff, and the 2 tons thrown away contain about 3 per cent. It is found that the loss in picking when the ore is broken by a Blake, exceeds that incurred when hand labour is employed; hence the tributers refused to use it, but all the large mines find economy in treating the mine ores by it.

Some of the ore is sold to the smelting works on the coast, but most of it is heap-roasted and run into regulus at two establishments in Carrizal Alto, near the mines, and at two others at Canta del Agua, 5 miles from the mines, on the road to Carrizal Bajo.

The scarcity of water entirely precludes water-dressing at the mines. For a time a dressing establishment was run with success by Mr. Ovalle at the Canta del Agua, the water being obtained by cutting across the valley, and arresting the brackish water which filters through it from the plain of Algaroval towards the sea. The water flows in good sized streams into the eastern edge of that plain from the Andes, but is soon lost in the sand. In its long passage through miles of soil it becomes saturated with soluble mineral matters, and among others with salts of lime, which in the Canta del Agua form thin beds of limestone of great purity. The strata of soil in the Canta del Agua are :—

1 yard of	loose sand, pebbles, and salt.
½ yard of	sand, alternating with thin layers of carbonate of lime, which on removal rapidly reform.
	Sand and clay.
	Old river bed—a compact mass of shale and boulders of undetermined thickness, impermeable to water, and on which it flows.

Following the Canta del Agua into the Plain of Algaroval, and across it in a south-east direction, the railroad runs to Yirba Buena, 71 miles from Canta del Agua, and 99 from Carrizal Bajo on the Pacific. This station is at the foot of the spur of the Andes, where occurs the argentiferous copper lode of Corro Blanco. The lodes yielded at surface antimoniate of copper rich in silver; this changed at about 30 fathoms to argentiferous copper glance and galena, and the grey copper was replaced by copper pyrites, which is now the prominent product, though small quantities of the other ores are still exploited. The principal mines have been yielding largely and profitably for some years. Beside the principal lodes at Carrizal and Cerro Blanco, there are innumerable others worked on a small scale by two or more poor miners, the yield from all of which combined is considerable.

The next copper deposits worthy of note lie between Copiapó and Nantoko, in the hills bordering the river of Copiapó. They are exceedingly numerous and yield rich oxidised ores; but none of the mines are worked in the same systematic manner as those of Tomaya and Carrizal, nor do any of them promise to rise into great importance. On the contrary, partly from the unskilful way in which they have been worked, and partly from the high percentage required by the smelter (18 per cent), which no mine can continue to give after the rich outcrop has been removed, unless water dressing is possible, the yield of the Copiapó copper mines must continue to fall off.

Not so, however, that of the great mines back of the port of Chañaral, in the desert of Atacama. From Chañaral de las Animas, on the Pacific, the valley of the Rio Salado, in which water flows only after such exceptionally heavy rains as may fall twice or thrice in a century, runs almost due east and west for about 10 leagues to the mines of La Salado, and at about half that distance a valley enters from the south-east, in which is the *minerale* of Las Animas. From the Calita de las Animas, the former port of the *minerale*, the first shipment of copper from Chili is said by Dr. Philippi to have been made to Europe, in a whaler, in the year 1820. It came from the mines of Las Animas, shortly before discovered by Don Diego de Almeida. The lodes of La Salado were not discovered till about 1840. Since then both districts have been worked with very varying fortune, and perhaps more money has been lost than in any other Chili mines, owing to the cost of mining with high wages, dear provisions, total lack of water, and long carriage. A railroad, however, has just been opened from Chañaral to La Salado, so that the yield of both *minerales*, large as it is at present, will probably be notably increased. The metals are smelted in part at the port, in part at Mr. Sievert's establishment at Pan de Azucar, and partly sent south. Very little can be picked to a high enough grade for shipment to England.

A ride of five hours across the hills to the north brings one to the Mina Descubridora de Carrizalillo, owned by the estate of Mr. Watters, much of whose ore is rich enough to bear exportation from the port of Pan de Azucar to England. Watters's curious and rich San Pedro mine lies about 18 leagues in the interior, a little north of Tres Puntas, and at a still greater distance in the interior, some 40 leagues back of Pan de Azucar, is Sievert's Esploradora Mine, the last of a chain of mines between the coast and the Cordillera, worked by the same indomitable owner, who, in making such out-of-the-way mines pay without the aid of a railroad, has performed one of the greatest mining feats in South America.

All these mines together produce about 7000 tons fine annually.

Still further north, along the coast of Chili and Bolivia, as far as Tocopillo, are innumerable copper deposits, as at Paposa, El Cobre, and Cobijæ, which yield in abundance 5 to 10 per cent carbonates, oxychlorides, and other oxidised ore; but the distance from fuel, and the difficulty, in most instances, of picking the metal to a high percentage, have prevented their being profitably worked; though considerable quantities of metal are smelted at Taltal or sent south to Lota.

The *barilla* of Bolivia is native copper, which occurs disseminated in fine grains through a bed of sandstone, which extends probably from Corocoro, in Bolivia, to San Bartolo, in the northern part of the Desert of Atacama, in Chili, through an extension of $5\frac{1}{2}°$ of longitude. It was worked at San Bartolo by the Indians before the Spanish conquest; and since then one or two unsuccessful attempts have been made to re-open the mines, but the difficulty of transport through the desert has effectually frustrated them. From Corocoro, however, though the metal has to be brought on mule-back for 70 leagues to Tagna, and thence by rail to Arica, the port of shipment, some 2000 tons (fine) are annually exported, which is yet only half of what was formerly produced.

The foregoing are the main sources of supply of the copper, the news of whose fortnightly shipments are looked forward to by all copper miners and speculators so anxiously, as affecting so sensitively the price of the metal.

In round figures, the several mineral districts above enumerated may be said to yield as follows:—

	Tons.
Southern Chili, south of the Maipocho and Maipu rivers	1000
Catemo, and the districts of Petorea, Illapel, and Combarbola	3000
Tomaya	9000
Panulcillo, Cœrillos, Tambillos, and Andacollo	3000
Brillador Mine, Higuera, and small mines in the northern part of the Province of Coquimbo	3000
Peña Blanca, including San Juan and Lebrar.	1000
Carrizal	8000
Cerro Blanco	2000
Copiapó	6000
Chañaral, and mines of Mr. Watters and Mr. Sievert	7000
Coast mines of Bolivia and Barilla	5000
	48,000

Are these several districts going to maintain their yield?

Wherever any quantity of copper is produced by poor miners, working on their own small mines, there will doubtless be a notable falling off. Wages are daily rising. The Chili *peon* can get 1 dollar a day on the Peruvian railroads, and will therefore no longer work at home for 25 cents. Numbers of small mines, yielding each a few tons of picked ore annually, and which paid their workers—who were often at the same time owners—small wages, are being abandoned. Though the yield from each may be insignificant, their total production is by no means trifling. A great deal of the copper smelted at Guayacan and the Copiapó establishments is bought in small parcels of a few cwts. each: the Catemo district especially will suffer from this cause. On the other hand, improved methods of treatment and means of transport are leading to certain ores being treated which would formerly have been deemed valueless. An experiment is now being made on some very extensive beds of 4 to 5 per cent purple sulphuret near Tiltil, on the eastern slope of the coast range. They occur at an elevation of 5700 feet above the sea, and so perpendicularly above the nearest spot suitable for a concentrating establishment, that an aërial wire-road, which carries the ores from the mines, descends 2250 feet to the mill, with an average grade of 33°. There is during most of the year water enough for buddling, which is effected by eleven concave buddles, for which the ore is crushed by a Blake, four sets of steel-jacketed rolls, and a battery of twelve stamps. The Hunt and Douglas method is used for reducing the ore to metal in the wet way. It is a bold attempt, surrounded with many difficulties; but being under the management of Mr. Waring, one of the best mechanical and mining engineers in Chili, it bids fair to be remunerative. If the experiment succeed, other deposits of a like nature will be worked, and may supply the deficiency certain to arise from the causes just referred to.

Tomaya will doubtless produce less in the course of a few years; for the *desmontes*, which have for some years been yielding a considerable portion of its quotum, will hardly bear re-picking: the old workings, abandoned to tributers, are of course not inexhaustible, and the mines have invariably and steadily grown poorer in the deeper levels. Increased cost of production going on simultaneously with diminution in the percentage of the ore, must result sooner or later in a serious falling off. The facility of extracting poor ores, which the Lecaros adit may afford, may ward off the evil day for some years; but within the next decennial period the supply from this quarter must decrease, and that considerably.

Panulcillo sails so close to the wind that if copper falls it will inevitably fall; if copper stands at even 15s., there seems to be no reason why it should not live and prosper; for although the mine has been worked improvidently, and the necessities of the company have prevented any unproductive work being done, in the course of a twelvemonth or so of good prices the evil might be rectified and the supply of ore brought up to the old figure. No large Chili mine depends so entirely on a slight variation in the price of copper as Panulcillo. As at the late low price it managed to hold its own, a rise of 2s., or even 1s. per unit, would make the enterprise profitable.

No doubt the Brillador could yield more than it does. Higuera and the mines back of Peña Blanca taken all in all are as likely to fall off as to increase.

Carrizal, if the Veta Principal continues to grow poorer, has seen its best days; if, however, the new shafts now being sunk strike good ore, the *minerale* will produce more than ever. Judging from the experience of Tomaya, Carrizal, Catemo, and other mines, the Chili lodes steadily grow poorer in depth, the ore becomes more and more mixed with iron either as sulphuret or oxide; but as the deepest mine has not reached 300 fathoms in the inclination of the lode, it is unsafe to conclude that the deterioration will be progressive. Should it be so, Chili, even with the advantage of better prices, will not again bring up her production to that of 1869.

Cerro Blanco may continue producing as much as at present for some years to come. The Copiapó, suffering as it does from the increase in wages and the emigration of its population to the new silver *minerale* of Caracoles, will probably decline; but Chañaral, now that it has a railroad, may be expected to increase its yield. Bolivia is rapidly falling off in her production.

There is very little chance of any new mines of consequence being opened. A copper lode in a desert country cannot escape detection, more especially in Chili, where all the inhabitants are directly or indirectly interested in mines, and where professional mine hunters are constantly searching even the most arid and elevated regions for mineral. All the great lodes now worked, except, perhaps, those of the Salado and others in the Atacama desert, have been known and worked from time immemorial. There is, therefore, no probability of an increase in the production of Chili, but every probability of a steady decline. But that decline will not be sudden nor speedy. All the large mines have ore enough in sight, or reserves, to keep up the supply to nearly its present proportions for years to come. The great vacillations in the quantities shipped from month to month do not indicate a like vacillation in the production, which the railroad returns of ore or regulus carried to the coast from the different mines show to be comparatively constant. Mine owners and smelters are able in Chili as well as in England to hold large stocks in anticipation of a favourable turn in the market. Larger shipments than usual may be looked forward to as a result of the present favourable price,—since some large stocks are held, especially in the north. But advanced prices are not likely to increase the production to any great extent; for even if they rose sufficiently to justify ore being broken which has heretofore been left standing on account of its poverty, miners to do the work could not be found.

It is pretended that the new railroads penetrating the Cordillera, in Peru, will bring to market vast quantities of ore heretofore shut out by heavy freights; but it remains to be ascertained whether Cordillera copper mining in Peru will reverse the universal ill success which has attended it in Chili.

Copper Smelting in Chili.

As is well known, the condition in which the copper comes to England is not that of ore. Twenty-five years ago very little copper was smelted in Chili; whereas, in 1870, only 3·16 per cent was exported as ore, while 55·35 per cent was exported as bars and ingots, and 41·48 per cent as regulus. The previous review of the mines has shown how little ore of high produce is or can be obtained. It is therefore imperative that it be smelted as near the mine as possible. But the high price of fuel—the average cost of Chili coal delivered at any of the northern ports being 8 dollars a ton—renders smelting so expensive that only by the exercise of the greatest skill can it be profitably conducted.

Mr. Lambert, as already stated, erected the first reverberatory furnace in Chili about the year 1837, and by him were built the first extensive smelting works in the port of Coquimbo. But smelting received its greatest impulse from the operations of the Mexican and South American Smelting Company, whose large establishment at Herradura, near Coquimbo, was run from 1848 to 1857 with persistent loss. It, however, benefitted Chili by introducing Napier's method, which with certain modifications has continued to be that used in making bars.

There are throughout Chili about ninety furnaces making regulus, and about sixty calciners and furnaces making bars and ingots.

The two largest establishments are at Lota and Guayacan. The former is owned by a company, which likewise owns and works some coal beds in the neighbourhood. The steamers carrying coal north to the smelting works at the mines return laden with ore; hence the Lota Company, with coal of their own at hand, and being their own carriers of ore, can afford to smelt even poorer mineral than can the furnaces at the mines. The Guayacan works are owned by Messrs. Urmeneta and Errasuriz, and are among the largest in the world, running ordinarily seventeen triple hearth calcining furnaces, thirteen smelting reverberatories, and two refining furnaces. When in full blast the works can turn out monthly from 15 per cent ore, as regulus, bars, and ingots, equal to 1000 tons fine. The same proprietors have furnaces at Cerillos, at the foot of the Tomaya hill, where the poorer Tomaya ores are run into regulus, and other works at Tongoi, the port of Tomaya, where the rest of the Piké and some other Tomaya ores are run into regulus, and where also some bar copper is made; but most of the regulus of Cerillos and Tongoi is sent for further treatment to Guayacan.

On the other side of the neck of land which divides the Bays of Herradura and Coquimbo lies the town of Coquimbo, with the abandoned smelting works of Charles Lambert and of Don Ramon Ovalle and Co., and the active works of Edwards and Co., where such care is taken in the selection and smelting that their bars and ingots bring a better price in the English market than those of either Lota or Guayacan. They run into bars all the calcined regulus produced at the Compañia establishment of Mr. Lambert, situated on the Elqui river, just beyond the town of Sorena, and on the opposite side of the bay to Coquimbo. In the days of old activity there were here seven reverberatories running, each with its three-storied calciner attached; now only two are are running. At the Compañia are the only sulphuric acid works on the west coast. The acid is consumed in the manufacture of blue vitriol from the carbonate ores of the Panteon mine. The sulphate of copper finds a ready and profitable sale to the amalgamating works of the Copiapó.

But there are many other furnaces scattered throughout central and northern Chili, either at the mines themselves or at the nearest ports. The following are approximately the number of furnaces running regulus in the several districts:—

Lota	8
Catemo	6
Cerillos and Tongoi	6
Guayacan	13
Edwards	4
Brillador	2
Panulcillo	6
Higuera	4
Copiapó	5
Peña Blanca	5
Carrizal	19
Chañaral	5
Pan de Azucar	3
Tocopillo	4
	90

This number tallies with that obtained by estimating the number of furnaces from the work done. There were produced from January to July, 1871, in bars regulus 410,679 quintals of fine copper, which would represent about 876,111 quintals of 50 per cent regulus. As a furnace smelts about 300 quintals of 10 per cent ore into, say, 55 quintals of 50 per cent regulus daily, it would require eighty-seven furnaces running constantly to furnish the quantity above stated.

The proportion of coal consumed to ore smelted in making regulus is, at Panulcillo, in the reverberatories as 1 to 3·5; at Guayacan as 1 to 2·8; at the Compañia as 1 to 2·6; and at Carrizal as 1 to 2·6. The coal principally used is the Chili coal of Lota and Coronel, whose smelting value is about one-seventh less than that of English smelting coal.

At Caldera, the port of Copiapó, there is a smelting establishment in operation, and two up the river, one at Punta del Cobre and another at Nantoko, at both of which oxidised ores of 18 per cent are run into 62 per cent regulus; and argentiferous and auriferous regulus are made by smelting copper ores with the poorer class of silver and gold ores, and even gold tailings.

At Guayacan the operations are as follows:—A mixture is made of 15 per cent ore. Sufficient carbonates can generally be had to avoid the necessity of calcining any of the sulphuret. This mixture is run into a 50 per cent regulus.

In making bars the regulus is calcined dead, in accordance with Napier's recommendation; but at Guayacan it is, before entering the calciners, crushed between Cornish rolls to 1-8th of an inch, instead of being disintegrated in water, as is done at the Compañia.

The result of the fusion of the calcined regulus is a bath of 96 per cent metal, which is run into bars, and a rich matt of 70 per cent. This 70 per cent matt is then smelted into blister copper and a rich slag. The blister copper is refined in charges of 15 to 20 tons. Willow rods are used in polling, and in addition to anthracite dried aloe stems are thrown upon the bath of metal.

Mr. Francis, the smelter, to whose intelligent superintendence Guayacan owes much of its prosperity, says he can make a ton of refined ingots out of 13 per cent ore with 5 tons of coal.

III. NATURAL AND ARTIFICIAL FLIGHT.

IF ever the important problem of Artificial Flight is to be solved, it is reasonable to conclude that the same laws and forces which produce Natural Flight must be discovered and applied. Imbued with this belief, Dr. J. Bell Pettigrew, of the Royal College of Surgeons, Edinburgh, has made a series of elaborate inquiries into the structure and function of natural wings, and the peculiar properties requisite in artificial wings to produce artificial flight. Dr. Pettigrew has been engaged in these researches since 1865, and has carefully analysed, figured, and described, not only the movements of the wings of insects, bats, and birds, but he has also examined in detail the movements of a large number of animals fitted for swimming, such as the otter, seal, sea bear, walrus, penguin, turtle, crocodile, porpoise, fish, &c. By comparing the flippers of the seal, sea bear, and walrus with the fin and tail of the fish; and the wing of the penguin (a bird which is incapable of flight, and can only swim and dive) with the wing of the insect, bat, and bird, he has been able to show that a close analogy exists between the flippers, fins, and tails of sea mammals and fishes on the one hand, and the wings of insects, bats, and birds on the other; in fact, that theoretically and practically these organs, one and all, form flexible helices or screws, which, in virtue of their rapid reciprocating action, operate upon the water and air after the manner of double inclined planes.

Guided by these indications, he has especially directed his attention to the twisting flail-like movements of the wings of insects, of the flippers and tails of sea mammals, and of the fins and tails of fishes. These he finds all act upon the air and water by curved surfaces, the curved surfaces reversing, recriprocating, and engendering a wave pressure, which can be continued indefinitely at the will of the animal.

In order to prove that sea mammals and fishes swim, and insects, bats, and birds fly, by the aid of curved figure-of-8 surfaces, which exert an intermittent wave pressure, Dr. Pettigrew constructed artificial fins, flippers, and wings, which curved and tapered in every direction, and which were flexible and elastic, particularly towards the tips and posterior margins. These fins, flippers, and wings were slightly twisted upon themselves, and when applied to the water and air by a sculling or figure-of-8 motion, curiously enough not only reproduced the curved surfaces referred to, but all the other movements peculiar to the fins and tail of the fish when swimming, and to the wings of the insect, bat, and bird when flying.

To Dr. Pettigrew is due the discovery of the celebrated figure-of-8 or wave theory of flight which has been exciting so much attention on the Continent and in America. As early as 1867 Dr. Pettigrew gave his novel theory to the world in an evening lecture, delivered at the Royal Institution of Great Britain. On that occasion (*vide* Proc. Roy. Inst. of Great Britain, March 22, 1867) he pointed out the interesting fact that the wing was a screw *structurally* and *functionally*; in other words, that the wing when at rest was *twisted upon itself*, and that when it was made to vibrate or reciprocate it *twisted and untwisted figure-of-*8 *fashion*. The wing was shown to be as effective in water as in air, and the tail of the fish was represented as lashing from side to side, after the manner of an oar in sculling. In June of the same year (1867) he read a memoir on the subject to the Linnæan Society of London (Trans. Linn. Soc., vol. xxvi.), in which he described, figured, and compared the movements made by the fins and tail of the fish and the wing of the bird in flying and diving. These movements he showed were *reciprocating movements*, produced by helicoidal surfaces, which were mobile and plastic, and acted at a great variety of angles, so as to obtain a maximum result with a minimum of power, and, what is not less important, with a minimum of slip. The fish was represented as throwing its body into figure-of-8 curves in swimming, and the wing of the bird into similar curves in flying and diving—the figure of 8, when the animals were progressing at a high speed, being opened out or unravelled to form *first a looped, and then a waved track.** The following quotation from his memoir will explain the relation :—" The water and air are acted upon by curve or wave pressure, emanating in the one instance from the body of the fish, and in the other from the wing of the bird —*the reciprocating and opposite curves* into which the trunk and wing are thrown in swimming and flying, constituting, in reality, *a mobile helix or screw*, which, during its action, produces the precise kind and degree of pressure adapted to fluid media, and to which they respond with the greatest readiness." He also contrasted the screw formed by the wing with that at present employed in navigation; and showed that the latter, which is *rigid*, cannot be compared in point of efficiency with the former, which is *flexible and elastic*. The rigid screw of the ship is made to revolve, the one blade following the other in rapid succession, and all striking at a given angle which never varies. One blade, as a consequence, virtually performs all the work. From the fact that the one blade, which may be taken to represent the whole, moves in only one direction (it revolves round a given axis), it cuts out as it were the portion of water which corresponds with its area of revolution—a circumstance which greatly increases the slip, while it correspondingly diminishes the actual propelling power of the screw. It is otherwise with the screws formed by the tail of the fish and the wings of flying animals. These are *flexible and elastic*, and, when they are made to vibrate, they are also made to reverse the direction of the stroke, and reciprocate in such a manner that the stroke from above downwards, or from right to left, as the case may be, is made to produce a current, which being met by the wing or tail when it makes a counter stroke from below upwards, or from left to right, greatly augments the propelling power. This holds true of every successive stroke made by the wing or tail. This power is further augmented by the elasticity and flexibility which contribute to the continued play of the natural screw, and by the fact that the wing of the bird and the tail of the fish strike at a great variety of angles—this peculiarity enabling them to diminish the slip to a minimum and to increase the propelling power to a maximum. Dr. Pettigrew

* Nearly two years after Dr. Pettigrew wrote, Professor Marey, of Paris, obtained similar results by the aid of the sphygmograph; and since then M. Senecal, M. de Fastes, M. Ciotti, and others have been labouring in the same field. These investigators have confirmed Dr. Pettigrew's original hypothesis, but, so far as we are aware, have added no new facts.

arrived at these results from a careful study of the extremities and travelling surfaces of a large number of animals fitted by nature for moving in water and air, and from numerous experiments with artificial fish tails, fins, and wings, which he made to vibrate with steam by a direct piston action. Continuing his researches, Dr. Pettigrew presented a second memoir on the subject to the Royal Society of Edinburgh, on the 2nd of August, 1870. This is published in vol. xxvi. of the "Transactions" of that Society, and in it he gives the details on which the conclusions arrived at in his first memoir were based.* Dr. Pettigrew shows that the wing acts as a kite both during the down and up strokes, and that it elevates and propels in either case—the rising and falling movements gliding by insensible degrees into each other to form one pulsation; that when the wing rises the body falls, and *vice versa;* that the wing, when the body of the flying animal is advancing in space, describes *a waved track*, the body describing a similar but smaller wave; that the wing is twisted upon itself when at rest and when in motion; that the blur or impression produced on the eye by it when made to vibrate rapidly is concavo-convex and twisted; that the under or concave surface of the wing, in virtue of its being carried obliquely forward against the air by the body, is effective during both the down and up strokes; that the wing rotates in the direction of its length and breadth as it rises and falls; that it reverses its planes more or less completely at every stroke; that it produces during the one stroke the currents by which it is elevated during the succeeding stroke—the wing literally rising on a whirlwind of its own forming; that the wing is movable and flexible as well as elastic, and capable of change of form in all its parts; that it is forced into waves during its action, and impinges upon the air as an ordinary sound does; that it produces a cross pulsation, the pulsatile waves running in the direction of the length of the wing and across it; that during its vibration it moves on the surface of an imaginary sphere; that the natural wing when elevated and depressed must move forwards; that the movements of the wing are comparatively slow at its root, but very rapid at its tip; that balancing is in a great measure effected by purely mechanical arrangements which operate independently of the will of the animal; that weight is necessary to horizontal flight; that the wing acts upon yielding fulcra; that a regulating

* These memoirs extend to some 220 pages quarto, and are illustrated by nearly 200 original engravings and woodcuts.

power is necessary in flight, the wing being at all times thoroughly under control; that the wing in the bird descends as a long lever and ascends as a short one, the tip of the wing describing an ellipse as it does so; that the wing forms a parachute from which the body is suspended both during the down and up strokes; that the wing of the bird opens and closes as it rises and falls, and has a valvular action; that all wings are drawn towards the body and partly elevated by the action of elastic ligaments, &c.

Dr. Pettigrew's researches are dual in character. He first carefully describes and figures what is found in nature, after which he proceeds to demonstrate that the structures and movements which he has described and figured may be reproduced artificially. He takes, *e.g.*, a fish's fin or tail, and shows that during its action it lashes from side to side figure-of-8 fashion—the margins of the fin or tail throwing themselves into double or figure-of-8 curves as it does so. He then takes the wing of an insect, bat, or bird, and by placing the creature in certain positions the spectator can clearly trace the figure of 8 made by the tip and margins of the pinion. He, however, goes further. He points out how the tail of the fish and the wing of the bird may be imitated both as regards its structure and function. He, in fact, proves experimentally that the fish-tail and the wing have many features in common, and that propellers formed on the fish-tail and wing model are the most effective that can be devised, whether for navigating the water or the air. To operate efficiently on fluid, yielding media, the propeller itself must yield. Dr. Pettigrew has made this point very clear; and in this, we think, he has made a valuable discovery, for there can be little doubt that the propeller at present employed in navigation is faulty both in principle and application.

In the concluding part of his second memoir, Dr. Pettigrew explains that the inclined planes hitherto employed for water and air are *rigid*, whereas they ought to be *flexible* and *elastic;* that the old rigid inclined planes are made to attack the water and air at one angle and at a uniform speed, whereas they ought to strike at a great variety of angles and at a variable speed; that the inclined planes at present in vogue either advance in a straight line or revolve in one direction, whereas they ought to reverse and reciprocate to form vibrating *laminæ;* that the inclined planes at present employed draw a current after them, which, being never met, is consequently never utilised; that the artificial fish-tail and wing create the currents on which they mainly depend for support and progress by a peculiar reciprocating figure-of-8 action. Dr. Pettigrew's spiral elastic wings and flexible elastic screws will be hailed with satisfaction by all interested in the navigation of balloons. They possess advantages for this purpose which will necessitate their being universally adopted.

Dr. Pettigrew shows how an artificial insect, bat, or bird's wing may be made to vibrate with a wavy, continuous, reciprocating motion, devoid of dead points, the wing literally floating on the air. He points out how a properly-constructed artificial wing will, when elevated and depressed, inevitably dart forwards in a series of opposite curves, and that by altering the angle of inclination of the wing with the horizon, it may be made to fly upwards and forwards, horizontally, or downwards and forwards—flight, as he explains, being essentially not a vertical but a progressive and almost horizontal movement. He likewise gives directions as to the nature of the forces to be employed in driving artificial wings and propellers. The artificial wings and propellers, he states, are made to resemble the wing of the insect, bat, and bird, or the caudal fin of the fish. They are composed of flexible and elastic material, which varies in thickness, the thicker portions, which are consequently the more rigid parts, corresponding to the root and anterior margin of the wing and the root and lateral margins of the tail of the fish. When made to vibrate or reciprocate, the margins of the propeller formed on the wing and fish-tail model are thrown into double or figure-of-8 curves, from the fact that the propeller twists and untwists during its action. This twisting movement enables the propeller to evade and seize the water and air alternately with wonderful rapidity and power—the efficiency of the propeller increasing in a direct ratio to the velocity with which it is made to vibrate. By adding springs which antagonise each other, the propeller is lashed about with greater facility and with a more continuous play—a similar result being obtained by working the steam expansively.

The subjoined passages and illustrations from Dr. Pettigrew's memoirs will serve to elucidate the figure-of-8 or wave theory of flying, and cannot fail to be interesting to the reader, the more especially as they are strikingly original.

"*The Wing a Twisted Lever or Helix.*—The wings of insects and birds are, as a rule, more or less triangular in shape, the base of the triangle being directed towards the body, the sides anteriorly and posteriorly. They are also conical on section from within outwards and from before backwards; this shape converting the pinion into a delicately graduated instrument, balanced with the utmost nicety to satisfy the requirements of the muscular system on the one hand and the resistance and resiliency of the air on the other.

The neuræ or nervures in the insect's wing are arranged at the axis or root of the pinion, after the manner of a fan or spiral stair; the anterior one occupying a higher position than that farther back, and so of the others. As this arrangement extends also to the margins, the wings are more or less twisted upon themselves, and present a certain degree of convexity on their superior or upper surface, and a corresponding concavity on their inferior or under surface; their free edges supplying those fine curves which act with such efficacy upon the air, in obtaining the maximum of resistance and the minimum of displacement; or what is the same thing, the maximum of support with the minimum of slip. (*Vide a b* of Fig. 1, and *r g*, *s a*, of Fig. 1A.) . . . .

Fig. 1.

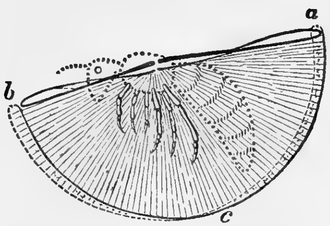
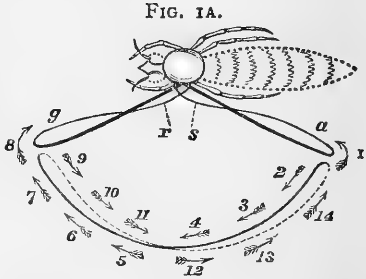


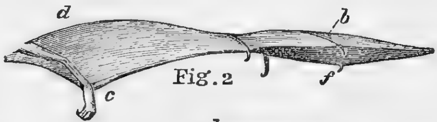
Fig. 1 repesents the oblique direction of the stroke of the wing in the flight of the insect (wasp)—how the wing is twisted upon itself at the end of the up (*a*) and down (*b*) strokes, and how the tip of the wing, during its vibration, describes a figure-of-8 track in space (*a*, *c*, *b*).

Fig. 1A.



Wasp seen from above and laterally. Shows how the wing twists upon itself during its action, the posterior or thin margin being inclined alternately forwards (*g*) and backwards (*r*) at the end of the down stroke, and backwards (*a*) and forwards (*s*) at the end of the up stroke. It also shows how the margins of the wing form figure-of-8 curves, and how the margins and tip of the wing form figure-of-8 tracks in space (1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14).

All wings obtain their leverage by presenting oblique surfaces to the air, the degree of obliquity gradually increasing in a direction from behind forwards and downwards during extension, when the sudden or effective stroke is being given, and gradually decreasing in an opposite direction during flexion, or when the wing is being more slowly recovered preparatory to making a second stroke. The down stroke in insects—and this holds true also of birds—is therefore delivered *downwards and forwards*, and not as previous investigators have stated, vertically, or even slightly backwards.* . . . To confer on the wing the multiplicity of movement which it requires, it is supplied at its root with a double hinge or compound joint, which enables it to move not only in an upward, downward, forward, and backward direction, but also at various intermediate degrees of obliquity. . . . The wing of the bat (Fig. 2) and bird (Figs. 3 and 4), like that of the insect, is concavo-convex, and more or less twisted upon itself. The twisting is in a great measure owing to the manner in which the bones of the wing are twisted upon themselves, and the spiral nature of their articular surfaces, the long axes of the joints always intersecting each other at nearly right angles. As a result of this disposition of the articular surfaces, the wing may be



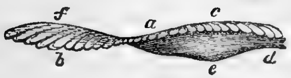


FIG. 3.

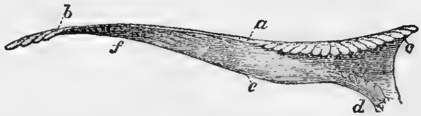


FIG. 4.

Fig. 2. Right wing of the Bat as seen from behind and from beneath. When so regarded, the anterior or thick margin (*d f*) of the wing displays different curves from those met with on the posterior (*b c*) or thin margin, the anterior and posterior margins crossing each other, as in the blades of a screw propeller.

Fig. 3. Left wing of Heron, partially extended, seen from beneath and from behind,—shows the spiral configuration and crossing of the anterior (*d e f*) and posterior (*c a b*) margins of the pinion; *e*, anterior axillary curve pointing downwards; *f*, posterior axillary curve pointing upwards; *c a*, posterior axillary curve pointing upwards; *b*, posterior distal curve pointing downwards. The posterior axillary and distal curve are reversed in complete extension (compare *b a c* of the present Fig. with *b a c* of Fig. 4.

shot out or extended, and retracted or flexed in nearly the same plane, the bones of the wing rotating in the direction of their length during either movement. This secondary action, or the revolving of the component bones upon their own axes, is of the greatest importance in the movements of the wing, as it communicates to the hand and forearm, and consequently to the volant membrane, or to the primary

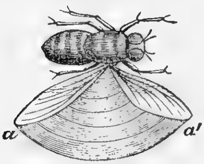
* The up stroke when the body is progressing at a high horizontal speed is delivered *upwards and forwards*, so that the wing invariably acts obliquely after the manner of a kite. Whether the wing is made to vibrate vertically or horizontally, it, practically speaking, in *progressive flight*, strikes *downwards and forwards* during the down stroke, and *upwards and forwards* during the up stroke.

and secondary feathers, the precise angles necessary for flight. It, in fact, insures that the wing, and the curtain or fringe of the wing, shall be screwed into and down upon the air in extension, and unscrewed or withdrawn from the air during flexion. The wing of the bat and bird may therefore be compared to a huge gimlet or auger, the axis of the gimlet representing the bones of the wing; the flanges or spiral thread of the gimlet, the volant membrane or rowing feathers."

"*The Wing Twists and Untwists during its Action.*—That the wing twists upon itself structurally, not only in the insect, but also in the bat and bird, any one may readily satisfy himself by a careful examination, and that it twists upon itself during its action I have had the most convincing and repeated proofs. The twisting in question is most marked in the posterior or thin margin of the wing, the anterior and thicker margin performing more the part of an axis. As a result of this arrangement, the anterior or thick margin cuts into the air quietly, and as it were by stealth, the posterior one producing on all occasions a violent commotion, especially perceptible if a flame be exposed behind the insect. Indeed, it is a matter for surprise that the spiral conformation of the pinion, and its spiral mode of action, should have eluded observation so long; and I shall be pardoned for dilating upon the subject when I state my conviction that it forms the fundamental and distinguishing feature in flight, and must be taken into account by all those who seek to solve this most involved and interesting problem by artificial means. The importance of the twisted configuration or screw-like form of the wing cannot be over-estimated. That this shape is intimately associated with flight is apparent from the fact that the rowing feathers of the wing of the bird are every one of them distinctly spiral in their nature; in fact, one entire rowing feather is equivalent—morphologically and physiologically—to one entire insect wing. In the wing of the martin, where the bones of the pinion are short and in some respects rudimentary, the primary and secondary feathers are greatly developed, and banked up in such a manner that the wing as a whole presents the same curves as those displayed by the insect's wing, or by the wing of the eagle, where the bones, muscles, and feathers have attained a maximum development. The conformation of the wing is such that it presents a waved appearance in every direction—the waves running longitudinally, transversely, and obliquely. The greater portion of the pinion may consequently be removed without materially impairing either its form or its functions. This is proved by making sections in various directions, and by finding that in some instances as much as two-thirds of the wing may be lopped off without destroying the power of flight. Thus, in the summer of 1866, I removed the posterior two-thirds from either wing of a blow-fly, and still the insect flew, and flew well. The only difference I could perceive amounted to this, that the fly, while it could elevate itself perfectly, flew in circles, and had less of a forward motion than before the mutilation. It had, in fact, lost propelling or driving power, the elevating or buoying power remaining the same. I took another blow-fly and removed the tip or outer third of either wing, and found that the driving power was the same as before the mutilation, while the elevating or buoying power was slightly diminished. These experiments prove that the posterior or thin elastic margin of the wing is more especially engaged in propelling, the tip in elevating. The spiral nature of the pinion is most readily recognised when the wing is seen from behind and from beneath, and when it is foreshortened. It is also well marked in some of the long-winged oceanic birds when viewed from before, and cannot escape detection under any circumstances, if sought for,—the wing being essentially composed of a congeries of curves, remarkable alike for their apparent simplicity and the subtlety of their detail."

"*The Wing during its Action Reverses its Planes, and describes a Figure-of-*8 *track in space.*—The twisting or rotating of the wing on its long axis is particularly evident during extension and flexion in the bat and bird, and likewise in the insect, especially the beetles, cockroaches, and others which fold their wings during repose. In these in extreme flexion the anterior or thick margin of the wing is directed downwards, and the posterior or thin one upwards. In the act of extension, however, the margins, in virtue of the wing rotating upon its long axis, reverse their positions, the anterior or thick margins describing a spiral course from below upwards, the posterior or thin margin describing a similar but opposite course from above downwards. These conditions, I need scarcely observe, are reversed during flexion. The movements of the margins during flexion and extension may be represented with a considerable degree of accuracy by a figure of 8 laid horizontally. . . . It may likewise happen, though more rarely, that the anterior or thick margin of the pinion may be directed upwards and backwards during the return or up stroke. I infer this from having observed that the anterior margin of the wing of the wasp (when the insect is fixed and the wings are being driven briskly) is not unfrequently directed upwards and forwards at the beginning of the down stroke, and upwards and backwards at the commencement of the up or return stroke. A figure of 8, compressed laterally and placed obliquely with its long axis running from left to right of the spectator, represents the movement in question. The down and up strokes, as will be seen from this account, cross each other, the wing smiting the air during its descent from above, as in the bird and bat, and during its ascent from below, as in the flying fish and boys' kite. The pinion thus acts as a helix or screw in a more or less horizontal direction from behind forwards, and from before backwards; but it has a third function—it likewise acts as a screw in a nearly vertical direction from below upwards. . . . If the wing (of the larger domestic fly) be viewed during its vibrations from above, it will be found that the blur or impression produced on the eye by its action is more or less concave (Fig. 5, *a a'*).

FIG. 5.



Blur or impression produced on the eye by the rapidly oscillating wing of the Blow-Fly when the insect is progressing at a high speed. Seen from above and from the side *a a'* represents the waved track made by the wing in progressive flight.

This is due to the fact that the wing is spiral in its nature, and because during its action it twists upon itself in such a manner as to describe a double curve,—the one curve being directed upwards, the other downwards. The double curve referred to is particularly evident in the flight of birds from the greater size of their wings (Fig. 4, *b a*, *a c*). The wing, both when at rest and in motion, may not inaptly be compared to the blade of an ordinary screw propeller as employed in navigation. Thus the general outline of the wing corresponds closely with the outline of the propeller, and the track described by the wing in space is twisted upon itself propeller fashion. The great velocity with which the wing is driven converts the impression or blur into what is equivalent to a solid for the time being, in the same way that the spokes of a wheel in violent motion, as is well understood, completely occupy the space contained within the rim or circumference of the wheel. . . . From these remarks it will appear that not only the margins, but also the direction of the planes of the wing, are more or less completely reversed at each complete flexion and extension; and it is this reversing, or screwing and unscrewing, which enables the wing to lay hold of the air with such avidity during extension, and to disentangle itself with such facility during flexion,—to present, in fact, a more or less concave, oblique, and strongly resisting surface the one instant, and a comparatively narrow, non-resisting cutting edge the next. The figure-of-8 action of the wing explains how an insect or bird may fix itself in the air, the backward and forward reciprocating action of the pinion being so regulated as to afford support, but no propulsion."

"*The Wing, when Advancing with the Body, describes a Waved Track.*—Although the figure of 8 represents with considerable fidelity the twisting of the wing upon its axis during extension and flexion, when the insect is playing its wings before an object, or still better, when it is artificially

FIG. 6.

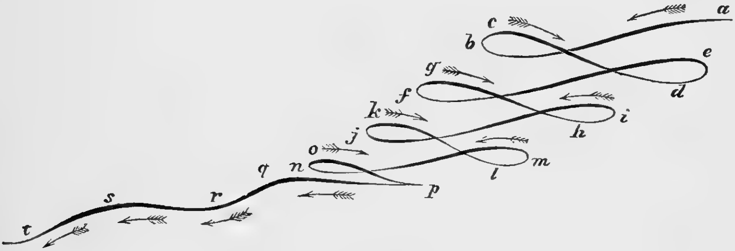
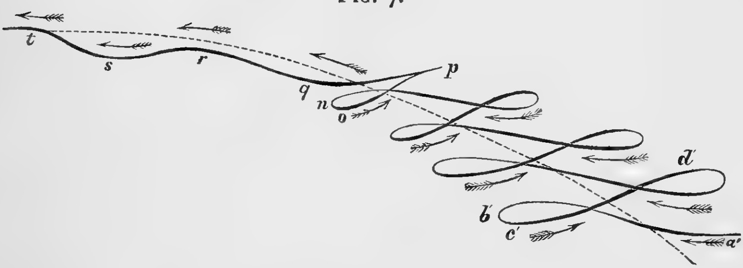


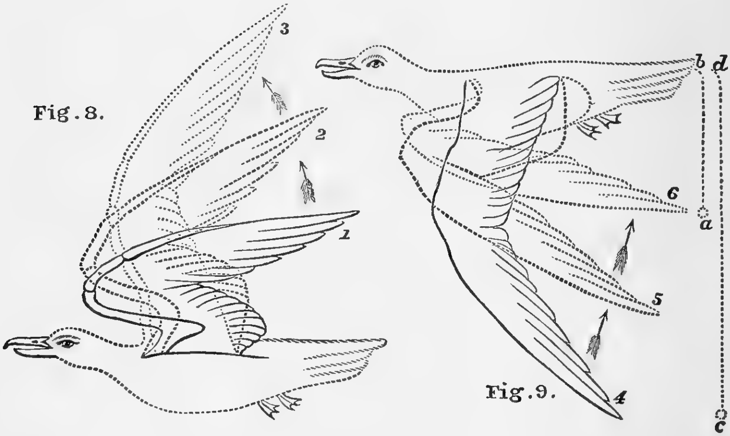
FIG. 7.



fixed, it is otherwise when the insect is fairly on the wing, and progressing rapidly. In this case the wing, in virtue of its being carried forwards by the body in motion, describes an undulating course."

"*How the Figure of* 8 *is Unravelled, and becomes a Waved Track.*—When the insect flies in a horizontal direction, and the speed attained increases with the duration of flight, the wing reciprocates less and less perfectly, because the figure of 8 sweeps described by it are converted into a looped and then a waved track, as represented at *a b c d e f g h i j k l m n o p q r s t* of Fig. 6; the corresponding looped waved track due to recoil being shown at similar letters of Fig. 7. The body of the insect is carried along the curve represented by the dotted line of Fig. 7.

The waved track formed by the ascent and descent of the wing in the progressive flight of the bat and bird is originated in a similar manner, but in this case the figure-of-8



Figs. 8 and 9 show the more or less perpendicular direction of the stroke of the wing in the flight of the bird (gull)—how the wing is gradually extended as it is elevated, 1, 2, 3 of Fig. 8—how it descends as a long lever until it assumes the position indicated by 4 of Fig. 9—how it is flexed towards the termination of the down stroke, as shown at 4, 5, 6 of Fig. 9, to convert it into a short lever, *a b*, and prepare it for making the up stroke. The difference in the length of the wing during flexion and extension is indicated by the short and long levers *a b* and *c d* of Fig. 9. The sudden conversion of the wing from a long into a short lever at the end of the down stroke is of great importance, as it robs the wing of its momentum, and prepares it for reversing its movements.

loops are disposed more vertically, because of the more vertical direction of the stroke as represented at 1 2 3, 4 5 6 of Figs. 8 and 9.

The unravelling process is shown *a b c d e f g h i j k l m n o p* of Fig. 10; the completed wave-track being seen at *s t u v w* of the same figure."

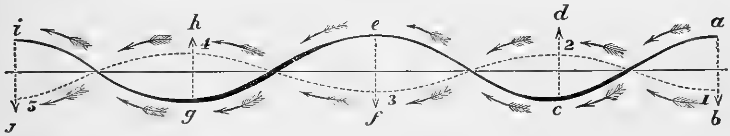
"*The Natural Wing, when Elevated and Depressed, must move forwards.*—It is a condition of natural wings, and of artificial wings constructed on the principle of living wings, that when forcibly elevated or depressed, even in a strictly vertical direction, they inevitably dart forward. This is well shown in Fig. 11."

If, for example, the wing is suddenly depressed in *a vertical direction*, as represented at *a b*, it at once darts downwards and forwards in a curve to *c*, thus connecting the vertical down

FIG. 10.

stroke into *a down oblique forward stroke*. If, again, the wing be suddenly elevated in a strictly vertical direction, as at *c d*, the wing as certainly darts upwards and forwards in a curve to *e*, thus connecting the vertical up stroke into an *upward oblique forward stroke*. The same thing happens when the wing is depressed from *e* to *f*, and elevated from *g* to *h*. In both cases the wing describes a waved track, as shown at *e g*, *g i*, which clearly shows that the wing strikes *downwards*

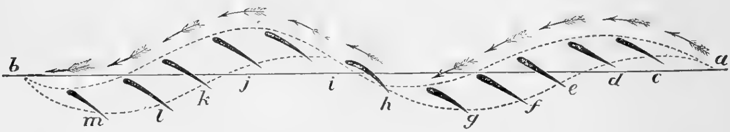
FIG. 11.



and forwards during the down stroke, and *upwards and forwards* during the up stroke. The wing, in fact, is always advancing, its under surface attacking the air like a boy's kite. If, on the other hand, the wing be forcibly depressed, as indicated by the heavy wave line *a c*, and left to itself, it will as surely rise again, and describe a waved track, as shown at *c e*. This it does in virtue of its flexibility and elasticity, aided by the recoil obtained from the air. In other words, it is not necessary to elevate the wing forcibly in the direction *c d* to obtain the upward and forward movement *c e*. One impulse communicated at *a* causes the wing to travel to *e*, and a second impulse communicated at *e* causes it to travel to *i*. It follows from this that a series of vigorous down impulses would, *if a certain interval were allowed to elapse between them*, beget a corresponding series of up impulses, in accordance with the law of action and reaction, the wing and the air under these circumstances being alternately active and passive: I say if a certain interval were allowed to elapse between every two down strokes; but this is practically impossible, as the wing is driven with such energy that there is positively no time to waste in waiting for the purely mechanical ascent of the wing. That the ascent of the pinion is not, and ought not to be, entirely due to the reaction of the air, is proved by the fact that in flying creatures (certainly in the bat and bird) there are distinct elevator muscles and elastic ligaments, delegated to the performance of this function. The reaction of the air is therefore only one of the forces employed in elevating the wing; the others are vito-mechanical in their nature."

"*The Body and Wings move in Opposite Curves.*—I have stated that the wing advances in a waved line, as shown at *a c e g i* of Fig. 11; and the same remark holds true, within certain limits, of the body, as indicated at 1, 2, 3, 4, and 5 of the same figure. Thus, when the wing descends in the curve

Fig. 12.



a e, it elevates the body in a corresponding but minor curve, as shown at 1, 2; when, on the other hand, the wing ascends in the curve *c e*, the body falls or descends in a corresponding but smaller curve (2, 3), and so on *ad infinitum*. The undulations made by the body are so trifling when compared with those made by the wing that they are apt to be overlooked. They are, however, deserving of attention, as they exercise an important influence on the undulations made by the wing, the body and wing swinging forward in space alternately, the one rising when the other is falling, and *vice versa*. Flight may be regarded as the resultant of three forces:—the *muscular and elastic force*, residing in the wing, which causes the pinion to act as a true kite, both during the down and up strokes; the *weight of the body*, which becomes a force the instant the trunk is lifted from the ground, from its tendency to fall downwards and forwards; and *the recoil obtained from the air* by the rapid action of the wing. These three forces may be said to be active and passive by turns."

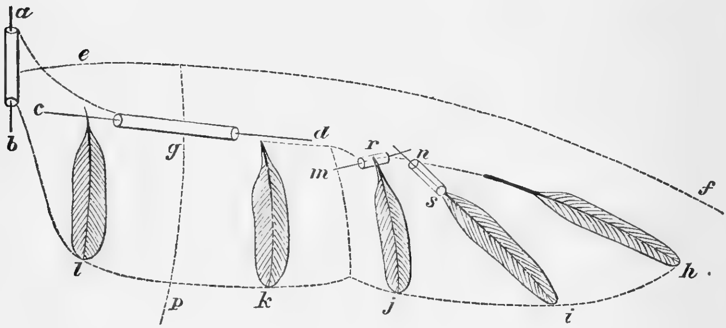
"*The Wing acts as a true Kite both during the Down and Up Strokes.*—If, as I have endeavoured to explain, the wing, even when elevated and depressed in a strictly vertical direction, inevitably and invariably darts forward (Fig. 11), it follows as a consequence that the wing, as already partly explained, flies forwards as a true kite, both during the down and up strokes, as shown at *c d e f g h i j k l m* of Fig. 12, and that its under concave or biting surface, in virtue of the forward travel communicated to it by the body in motion, is closely applied to the air, both during its ascent and descent, a fact hitherto overlooked, but one of considerable importance, as showing how the wing furnishes a persistent buoyancy, alike when it rises and falls."

"*Where the Kite Formed by the Wing Differs from the Boy's Kite.*—The natural kite formed by the wing differs from the artificial kite only in this, that the former is movable in all its parts, and more or less flexible and elastic, the latter being comparatively rigid. The flexibility and elasticity of the kite formed by the natural wing is rendered necessary by the fact that the wing is articulated or hinged at its root; its different parts travelling at various degrees of speed in proportion as they are removed from the axis of rotation. Thus the tip of the wing travels through a much greater space in a given time than a portion nearer the root. If the wing was not flexible and elastic, it would be impossible to reverse it at the end of the up and down strokes, so as to produce a continuous vibration. The wing is also practically hinged along its anterior margin, so that the posterior margin of the wing travels through a much greater space in a given time than a portion nearer the anterior margin. The compound rotation of the wing is greatly facilitated by the flexible and elastic properties of the pinion."

"*Compound Rotation of the Wing.*—The wing during its vibration rotates upon two separate centres, the tip rotating upon the root of the wing as an axis (short axis of wing), the posterior margin rotating upon the anterior margin (long axis of wing). *Vide a b* and *c d* of Fig. 13. This compound rotation goes on throughout the entire down and up strokes, and is intimately associated with the power which the wing possesses of alternately seizing and evading the air. It is this arrangement which enables the wing to present an infinity of inclined surfaces to the air—the angle of inclination being increased or diminished at any stage of the down and up stroke."

"*The Wing of the Bird Cranked slightly Forwards—the Compound Rotation of the Rowing Feathers.*—It will be observed from Fig. 13 that the wing is cranked somewhat forwards (compare position of axis *a b* with that of axis *c d*), a very slight movement of rotation at *c d* being accompanied by a considerable rotation of the posterior margin (*h i j k l*). This figure also shows that the individual primary, secondary, and tertiary feathers of the bird's wing have each what is equivalent to a long and a short axis. Thus the primary and secondary feathers marked *h i j k l* are capable of rotating on their long axes (*r s*) and upon their short axes (*m n*). The feathers rotate upon their long axes in a direction from below upwards during the down stroke, to make the wing impervious to air; and from above downwards during the up

FIG. 13.



stroke, to enable the air to pass through. The primary, secondary, and tertiary feathers have thus a distinctly valvular action. They rotate upon their short axes (*m n*) during the descent and ascent of the wing, the tip of the feathers rising slightly during the descent of the pinion and falling during its ascent."

"*The Wing during its Vibration produces a Cross Pulsation.*—The oscillation of the wing on two separate axes—the one running parallel with the body of the bird, the other at right angles to it—is well worthy of attention, as showing that the wing attacks the air on which it operates in every direction, and at almost the same moment, viz., from within outwards, and from above downwards, during the down or effective stroke; and from without inwards, and from below upwards, during the up or return stroke. As a corollary to the foregoing, the wing may be said to agitate the air in two principal directions, viz., from within outwards, or the reverse, and from behind forwards, or the reverse, the agitation in question producing two powerful pulsations—a longitudinal and a lateral; the longitudinal running in the direction of the *length* of the wing, the lateral in the direction of its *breadth*. As, however, the curves of the wing glide into each other when the wing is in motion, so the one pulsation merges into the other by a series of intermediate and lesser pulsations.

The longitudinal and lateral pulsations occasioned by the wing in action may be fitly represented by waved tracks running at right angles to each other, the longitudinal waved track being the more distinct."

"*Analogy between the Wing in Motion and the Sounding of Sonorous Bodies.* — It is a remarkable circumstance that the undulation or wave made by the wing when the insect and bird are fixed or hovering before an object, and when they are progressing, corresponds in a marked manner with the track described by the stationary and progressive waves in fluids,* and likewise with the waves of sound.† This coincidence would seem to argue an intimate relation between the instrument and the medium on which it is destined to operate—the wing acting in those very curves into which the atmosphere is naturally thrown in the transmission of sound, in order, as appears to me, to secure the maximum of progression with the minimum of slip. Can it be that the animate and inanimate world reciprocate, and that animal bodies are made to impress the inanimate in precisely the same manner as the inanimate impress each other? This much seems certain:—The wind communicates to the water similar impulses to those communicated to it by the fish in swimming; and the wing in its vibrations impinges upon the air as an ordinary sound would. The extremities of quadrupeds, moreover, describe spiral tracks on the land when walking and running; so that one great law would seem to determine the course of the insect in the air, the fish in the water, and the quadruped on the land."

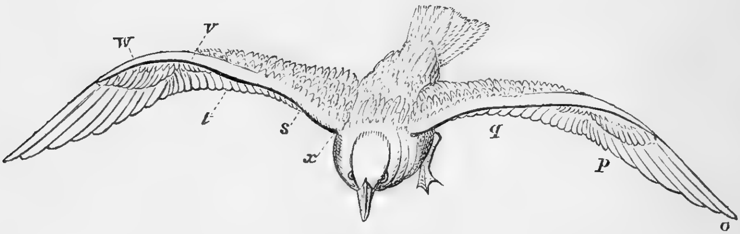
"*Weight contributes to Horizontal Flight.*—That the weight of the body plays an important part in the production of flight may be proved by a very simple experiment. If two primary feathers are fixed into an ordinary cork, and the apparatus is allowed to drop from a height, the cork does not fall vertically downwards, but *downwards* and *forwards*, and for the following reasons. The feathers are twisted flexible inclined planes, which arch in an upward direction. They are, in fact, true wings in the sense that an insect wing in one piece is a true wing. When dragged downwards by the cork, which would, if left to itself, fall vertically, they

* Handbook of Natural Philosophy. Vol. on Electricity, Magnetism, and Acoustics, pp. 366-7. By Dr. LARDNER. London, 1863.

† *Op. cit.*, pp. 378, 379, 380.

have what is virtually a down stroke added to them. Under these circumstances they inevitably dart forward, a struggle ensuing between the cork tending to fall vertically and the feathers tending to travel in a horizontal direction. As a consequence, the apparatus describes a curve before reaching the earth. This is due to the action and reaction of the feathers and air upon each other, and to the influence which gravity exerts upon the cork. The forward travel of the cork and feathers, as compared with the space through which they fall, is very great. Thus, in some instances, they advanced as much as a yard and a half in a descent of three yards. If the body of the sea gull depicted in Fig. 14 be taken to represent the cork, and the flexible mobile helices

FIG. 14.



Sea Gull suspending its body from its wings as from a parachute, and converting (by the aid of its wings) the vertical fall of the body into horizontal travel. This figure shows the twisted configuration of the wing of the bird, and how the anterior (*x s t v w*) and posterior (*q p o*) margins are disposed in different planes and appear to cross each other.

formed by the wings of the bird the primary or quill feathers, the conditions of the above experiment are reproduced with the utmost exactitude. A bird cannot be said to be flying until the trunk is swinging forward in space and taking part in the movement.

When the gannet throws itself from a cliff the inertia of the trunk at once comes into play, and relieves the bird from those herculean exertions required to raise it from the water when it is once fairly settled thereon. A swallow dropping from the eaves of a house, or a bat from a tower, afford illustrations of the same principle. However flying creatures acquire momentum, whether by throwing themselves from elevations or by the vigorous flapping of their wings, there can be no doubt, that the weight of their bodies operating upon the inclined planes formed by the pinions, contribute largely to the production of flight. This circumstance alone can explain why the albatross is able to sail about for an hour or more at a time, without once flapping its wings. Once launched into space, its motionless outspread pinions, which act as true kites, cause it even in a calm to descend *not vertically*, but in a *downward and forward curve;* and if a breeze be blowing, the air plays on the under surface of the wings in such a manner as enables it to pursue a horizontal course, or to ascend, descend, or wheel in any direction whatever. Weight is therefore a principal factor in flight. By its aid a flying animal is actually enabled to rest on the wing. But for this arrangement, the protracted migratory journeys of birds would be impossible."

"*The Wing at all times thoroughly under control.*—The advantage which the wing derives from being movable in all its parts consists in this, that it can be intelligently wielded even to its extremity. This enables the insect, bat, and bird, to tread and rise upon the air as a master—to subjugate it in fact. The wing, no doubt, abstracts an upward and onward recoil from the air, but in doing this it exercises a selective and controlling power: it seizes one current, evades another, and creates a third; it feels and paws the air as a quadruped would feel and paw a treacherous yielding surface. It is not difficult to comprehend why this should be so. If the flying creature is living, endowed with volition, and capable of directing its own course, it is surely more reasonable to suppose that it transmits to its travelling surfaces the peculiar movements necessary to progression, than that those movements should be the result of impact from fortuitous currents which it has no means of regulating. That the bird requires to control the wing, and that the wing requires to be in a condition to obey the behests of the will of the bird, is pretty evident from the fact that most of our domestic fowls can fly for considerable distances when they are young and when their wings are flexible; whereas when they are old and the wings stiff, they either do not fly at all or only for short distances, and with great difficulty. This is particularly the case with tame swans. This remark also holds true of the steamer or race-horse duck (*Anas brachyptera*), the younger specimens of which only are volant. In the older birds the wings become too rigid and the bodies too heavy for flight. Who that has watched a sea-mew struggling bravely with the storm, could doubt for a moment that the wings and every individual feather of the wings are perfectly under control? The whole bird is an embodiment of animation and power. The intelligent active eye, the easy graceful oscillation of the head and neck, the folding or partial folding of one or both wings,—nay more, the slight tremor or quiver of the individual feathers of part of the wings so rapid that only an experienced eye can detect it,—all confirm the belief that the living wing has not only the power of directing, controlling, and utilising natural currents, but of creating and utilising artificial ones, which is not less important. But for this power, what would enable the bat and bird to rise and fly in a calm or steer their course in a gale? It is erroneous to suppose that anything is left to chance where living organisms are concerned, or that animals endowed with volition and travelling surfaces should be denied the privilege of controlling the movements of those surfaces quite independently of the medium on or in which they are destined to operate. What would we say of that quadruped or that fish which depended for the major portion of its movements on the ground it trod, or the water it navigated? I will never forget the gratification afforded me on one occasion at Carlow (Ireland) by the flight of a pair of magnificent swans. The birds flew towards and past me, and I had my attention directed to their presence by a peculiarly loud whistling noise made by their wings. They flew about fifteen yards from the ground, and as their pinions were urged not much faster than those of the heron,* I had abundant leisure for studying their movements. The sight was very imposing, and as novel as it was grand. I had never seen anything before, and certainly have seen nothing since, that could in any way convey a more adequate idea of the prowess and guiding power which a bird may exert. What particularly struck me was the perfect mastery which they seemed to possess over everything. They had their wings and bodies visibly under control, and the air

* I have frequently timed the beats of the wings of the common heron (*Ardea cinerea*) at Warren Point (Ireland). In March, 1869, I was placed under unusually favourable circumstances for obtaining reliable results. I timed one bird high up over a lake for fifty seconds, and found that in that period it made fifty down and fifty up strokes; *i.e.*, one down and one up stroke per second. I timed another one in a heronry belonging to Major Hall. It was snowing at the time (March, 1869), but the birds, notwithstanding the inclemency of the weather and the early time of the year, were actively engaged in hatching, and required to be driven from their nests on the top of the larch trees by knocking against the trunks thereof with large sticks. One unusually anxious mother refused to leave the immediate neighbourhood of the tree containing her tender charge, and circled round and round it right overhead. I timed this bird for ten seconds, and found that she made ten down and ten up strokes; *i.e.*, one down and one up stroke per second precisely as before. I have therefore no hesitation in affirming that the heron, in ordinary flight, makes exactly sixty down and sixty up strokes per minute. The heron, however, like all other birds when pursued or agitated, has the power of greatly augmenting the number of its beats.

was attacked in a manner and with an energy which left little doubt in my mind that it played quite a subordinate part in the great problem before me. The necks of the birds were stretched out, and their bodies to a great extent rigid. They advanced with a steady stately motion, and swept past with a vigour and force which greatly impressed, and to a certain extent overawed, me at the time.* Their flight was what one could imagine that of a flying machine constructed in accordance with natural laws would be."

So much for Natural Flight; and now for a few words on Artificial Flight.

"*How to Construct an Artificial Wing on the Insect Type.*—The following appear to me to be essential features in the construction of an artificial wing:—

The wing should be of a generally triangular shape.

It should taper from the root towards the tip, and from the anterior margin in the direction of the posterior margin.

It should be convex above and concave below, and slightly twisted upon itself.

It should be flexible and elastic throughout, and should twist and untwist during its vibration, to produce figure-of-8 curves along its margins and throughout its substance.

Such a wing is represented at Fig. 15, and as it is forced into undulations when it is made to vibrate, I propose to designate it the *wave wing*.

If the wing is in more than one piece, joints and springs require to be added to the body of the pinion.

In making a wing in one piece on the model of the insect wing, such as that shown at Fig. 15, I employ one or more tapering elastic reeds, which arch from above downwards (*a b*) for the anterior margin. To this I add tapering elastic reeds, which radiate towards the tip of the wing, and which also arch from above downwards (*g*, *h*, *i*). These are so arranged with reference to the anterior margin of the wing, that they confer *a certain amount of spirality* upon the wing as a whole. The anterior (*a b*) and posterior (*c d*) margins are disposed in different planes, and appear to cross each other. I then add the covering of the wing, which may consist of india-rubber, silk, tracing cloth, linen, or any similar substance.

The artificial wave wing just described is endowed with the very remarkable property that it will fly in any direction, demonstrating more or less clearly that flight is essentially

* The above observation was made at Carlow-on-the-Barrow, in October, 1867, and the account of it is abstracted from my note-book.

a progressive movement, *i.e.*, a horizontal rather than a vertical movement. Thus, if the anterior or thick margin of the wing be directed upwards, and the angle which the under surface of the wing makes with the horizon be something like 45°, the wing will, when made to vibrate by the hand, fly with an undulating motion *in an upward direction*, like a pigeon to its dovecot. If the under surface of the wing makes no angle, or a very small angle, with the horizon, it will dart forward in a series of curves, in a *horizontal direction*, like a crow in rapid horizontal flight. If the angle made by the under surface of the wing be reversed, so that the thick margin of the wing be directed downwards, the wing will describe a waved track, and fly downwards, as a sparrow from a house-top or from a tree. In all those movements progression is a necessity. The movements are

FIG. 15.

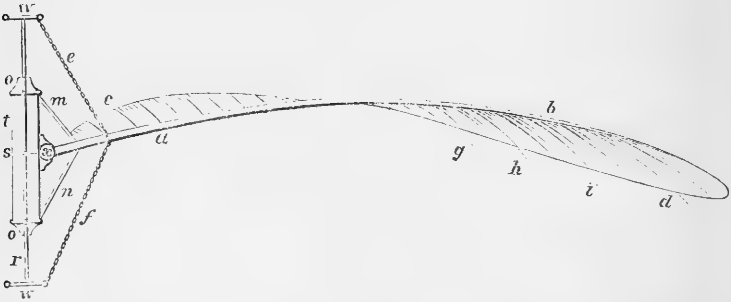


Fig. 15. Elastic spiral wing, which twists and untwists figure-of-8 fashion during its action to form a *mobile helix or screw*. This wing is vibrated by a direct piston action, and by a slight adjustment may be made to act vertically or horizontally, or at any degree of obliquity.

a, b, Anterior margin of wing, to which the neuræ or ribs are affixed. *c, d*, Posterior margin of wing crossing anterior one. *x*, Ball-and-socket joint at root of wing, the wing being attached to the side of the cylinder by the socket. *t*, Cylinder. *r*, Piston, with cross heads (*w w*) and piston head (*s*). *o, o*, Stuffing boxes. *e, f*, Driving chains. *m*, Superior elastic band, which assists in elevating the wing. *n*, Inferior elastic band, which antagonises *m*. The superior and inferior elastic bands assist in securing continuity of vibration, by removing the dead points at the end of the down and up strokes. To the superior and inferior elastic bands an anterior and posterior horizontal band is added. These run at right angles to the former and limit the action of the wing in an anterior and posterior direction.

continuous gliding *forward movements*. There is no halt or pause between the strokes, and if the angle which the under surface of the wing makes with the horizon be properly regulated, the amount of steady tractile and buoying power developed is truly astonishing. This form of wing, which may be regarded as the realisation of the figure-of-8 or wave theory of flight, elevates and propels both during the down and up strokes, and its working is accompanied with almost no slip.

It seems literally to float upon the air. No wing that is rigid in the anterior margin can twist and untwist during its action, and produce the figure-of-8 curves generated by the living wing. To produce the curves in question, the wing must be flexible, elastic, and capable of change of form in all its parts. The curves made by the artificial wave wing are largest when the vibration is slow, and least when the vibration is quick. In like manner, the air is thrown into large waves by the slow movement of a large wing, and into small waves by the rapid movement of a smaller wing. The size of the *wing curves* and *air waves* bear a fixed relation to each other, and both are dependent on the rapidity with which the wing is made to vibrate. This is proved by the fact that insects, in order to fly, require, as a rule, to drive their small wings with immense velocity. It is further proved by the fact that the small humming bird, in order to keep itself stationary before a flower, requires to oscillate its tiny wings with great rapidity, whereas the large humming bird (*Patagona gigas*), as was pointed out by Darwin, can attain the same object by flapping its large wings with a very slow and powerful movement. In the larger birds the movements are slowed in proportion to the size, and more especially in proportion to the length of the wing, the cranes and vultures moving the wings very leisurely, and the large oceanic birds dispensing in a great measure with the flapping of the wings, and trusting for progression and support to the wings in the expanded position. This leads me to conclude that very large wings may be driven with a comparatively slow motion, a matter of very considerable importance in artificial flight secured by the flapping of wings."

"*The Artificial Wave Wing can be driven at any speed—it can make its own currents, or utilise existing ones.*—One of the distinguishing features in the artificial wave wing is its adaptability. It can be driven slowly, or with astonishing rapidity. It has no dead points. It reverses instantly, and in such a manner as to dissipate neither time nor power. It alternately seizes and evades the air so as to extract a maximum amount of support with a minimum of slip, and a minimum expenditure of power. It supplies a degree of buoying and propelling power which is truly remarkable. Its buoying area is nearly equal to half a circle. It can act upon still air, and it can create and utilise its own currents. I proved this in the following manner:—I caused the wing to make a horizontal sweep from right to left over a candle; the wing rose steadily as a kite would, and after a brief interval, the flame of the candle was persistently blown from right to left. I then waited until the flame of the candle assumed its normal perpendicular position, after which I caused the wing to make another and opposite sweep from left to right. The wing again rose kite fashion, and the flame was a second time affected, being blown in this case from left to right. I now caused the wing to vibrate steadily and rapidly above the candle, with this curious result, that the flame did not incline alternately from right to left and from left to right; on the contrary, it was blown steadily away from me, *i.e.*, in the direction of the tip of the wing, thus showing that the artificial currents produced met and neutralised each other always at mid stroke. I also found that under these circumstances the buoying power of the wing was remarkably increased."

"*How the Wave Wing creates Currents, and rises upon them, and how the Air assists in elevating the Wing.*—In order to ascertain in what way the air contributes to the elevation of the wing, I made a series of experiments with natural and artificial wings. On concluding these experiments, I felt convinced that when the wing descends it compresses and pushes before it, in a downward and forward direction, a column of air corresponding to its area. The air rushes in from all sides to replace the displaced air, and so produces a circle of motion. The wing rises upon the outside of the circle referred to, so that it is not difficult to comprehend how the air comes indirectly to assist in elevating the wing. The artificial currents produced by the wing during its descent may be readily seen by partially filling a chamber with steam, smoke, or some impalpable white powder, and causing the wing to descend in its midst."

"*The Artificial Wave Wing as a Propeller.*—The wave wing makes an admirable propeller if its tip be directed *vertically downwards*, and the wing be lashed from side to side by a sculling figure-of-8 motion, similar to that executed by the tail of the fish. Three wave wings may be made to act in concert and with a very good result; two of them being made to vibrate in a more or less horizontal direction with a view to elevating, the third being turned in a downward direction, and acting at right angles to the others for the purpose of propelling."

"*A New Form of Aërial Screw.*—If two of the wave wings represented at Fig. 15 be placed end to end, and united to a vertical portion of tube to form a two-bladed screw, similar to that employed in navigation, a most powerful elastic aërial screw is at once produced, as seen at Fig. 16.

This screw, which for the sake of uniformity I denominate *the aërial wave screw*, possesses advantages for aërial purposes to which no form of *rigid screw* yet devised can lay claim. The way in which it clings to the air during its revolution and the degree of buoying power it possesses are quite astonishing. It is a self-ajusting, self-regulating screw, and as its component parts are flexible and elastic, it accommodates itself to the speed at which it is driven, and gives a uniform buoyancy. The slip I may add is nominal in amount. This screw is exceedingly light, and owes its efficiency to its shape and the graduated nature of its blades, the anterior margin of each blade being comparatively rigid, the posterior margin

Fig. 16.

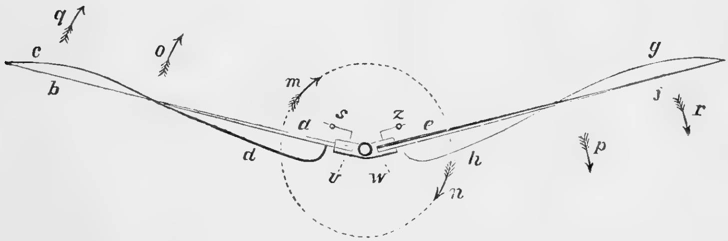


Fig. 16. Aërial wave screw whose blades are slightly twisted upon themselves (*a b*, *c d*; *e f*, *g h*), so that those portions nearest the root (*d h*) make a greater angle with the horizon than those parts nearer the tip (*b f*). The angle is thus adjusted to the speed attained by the different portions of the screw. The angle admits of further adjustment by means of the steel springs, *z*, *s*, these exercising a restraining, and to a certain extent a regulating, influence which effectually prevents shock.

It will be at once perceived from this figure that the portions of the screw marked *m* and *n* travel at a much lower speed than those portions marked *o* and *p*, and these again more slowly than those marked *q* and *r*. As, however, the angle which a wing or a portion of a wing, as I have pointed out, varies to accommodate itself to the speed attained by the wing, or a portion thereof, it follows, that to make the wave screw mechanically perfect, the angles made by its several portions must be accurately adapted to the travel of its several parts as indicated above.

x, Vertical tube for receiving driving shaft. *v*, *w*, Sockets in which the roots of the blades of the screw rotate, the degree of rotation being limited by steel springs, *z*, *s*. *a b*, *e f*, Tapering elastic reeds forming anterior or thick margins of blades of screw. *d c*, *h g*, Posterior or thin elastic margin of blades of screw. *m n*, *o p*, *q r*, Radii formed by the different portions of the blades of the screw when in operation. The arrows indicate the direction of travel.

being comparatively flexible and more or less elastic. The blades are kites in the same sense that natural wings are kites, and are flown as such when the screw revolves. I find the aërial wave screw flies best and elevates most when its blades are inclined at a certain upward angle, as indicated in Fig. 16. The aërial wave screw may have the numbers of its blades increased by placing the one above the other, and two or more screws may be combined and made to revolve in opposite directions so as to make them reciprocate, the one screw producing the current on which the other rises, as happens in natural wings."

"*The Aërial Wave Screw operates also upon Water.*—The form of screw just described is adapted in a marked manner for water, if the blades be made of carefully tempered finely graduated steel plates and reduced in size. It bears the same relation to, and produces the same results upon, water as the tail and fin of the fish. It throws its blades during its action into double figure-of-8 curves, similar in all respects to those produced on the anterior and posterior margins of the natural and artificial flying wing. As the speed attained by the several portions of each blade varies, so the angle at which each part of the blade strikes varies; the angles being always greatest towards the root of the blade, and least towards the tip. The angles made by the different portions of the blade are diminished in proportion as the speed with which the screw is driven is increased. The screw in this manner is self-adjusting, and extracts a large percentage of propelling power with very little force and surprisingly little slip. A similar result is obtained if two finely graduated angular-shaped steel plates be placed end to end (vertically or horizontally matters little), and applied to the water by a slight sculling figure-of-8 motion, analogous to that executed by the tail of the fish, porpoise, or whale. If the thick margins of the plates be directed forwards, and the thin ones backwards, an unusually effective propeller is produced. This form of propeller is likewise very effective in air."

IV. THE GEOLOGY OF THE STRAITS OF DOVER.*

By William Topley, F.G.S.,
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ENGINEERS have for long busied themselves with discussions upon the feasibility of connecting England and France by railway, and during the last few years the question has come much before the general public. The late war for a time laid all such discussions on one side, but recently they have been revived, and once more have come prominently forward.

* The authorities especially referred to in the preparation of this paper are as follows:—the Admiralty Charts; the Maps of the Geological Survey of England, Sheets 3 and 4; the Memoir on Sheet 4, by Mr. F. Drew; the Papers by Mr. Phillips and Dr. Fitton, in the "Geological Transactions,"

The various schemes proposed are of three classes *:—a *Bridge* over the sea, a *Tube* on the bed of the sea, and a *Tunnel* beneath the bed of the sea. With the second of these, or the tube scheme, Geology has nothing to do. Concerning the first, there is the important question of foundation, upon which a geologist's opinion might be sought; but whether the answer were favourable or otherwise would but little affect the project. It is a question which can easily be settled by actual experiment, at comparatively little cost; and even if a good foundation were hard to find (an improbable supposition), engineers would be at no loss to make one: the question is solely one of expense.

With the third scheme, that of tunnelling, the case is very different. Here nearly all the questions which arise at the outset are such as can only be fully appreciated by one having some geological knowledge; they are, chiefly, the depth and thickness of the various rocks beneath the Channel, their extent, physical characters, permeability, &c. Perhaps in the present state of the question we shall be doing some service by (1) briefly narrating what is actually known of the geological structure of the coasts bordering the Straits of Dover; (2) by enquiring what help this knowledge affords us towards ascertaining the actual condition of the sea-bed; and (3) by testing the various tunnel schemes by the data thus obtained.

The physical geography and geological structure of the opposite coasts of the Channel have so many characters in common, that a description of one side will in great part serve for that of the other. There are, however, some points of difference which are of great importance in this question.

The most striking features of the Straits of Dover are the Chalk cliffs, rising steeply from the shore, of great height, and of dazzling whiteness. The similarity in the appearance of these cliffs led many old writers to speculate on their

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vol. v., 1st series and vol. iv., 2nd series; the Statement of the Executive Committee of the Channel Tunnel, with Engineer's Report and Diagram (1869). The thicknesses of the oolitic rocks in the Bas-Boulonnais are taken from a Paper by M. Rigaux, in Bull. Soc. Acad. de Boulogne (1865); the geology of the Bas-Boulonnais is partly from a Map by M. du Souich, and partly from original observations. A detailed account of the strata passed through in the Calais Well is given by Mr. Mackie in the Geol. and Nat. Hist. Repertory, vol. i., p. 122: this is taken from a table and specimens preserved in the Museum at Calais.

* The proposal to construct large ferry-boats which will convey trains of carriages over the Channel does not concern us here.

former extension across the straits, uniting England and France. We shall presently see that the strata immediately underlying the Chalk present similar resemblances, a fact which the old writers overlooked.

Richard Verstegan was one of the oldest writers who gave any good and sound reasons for this opinion.* Others had suggested it before, but, says Verstegan, "These authors, following the opinion the one of the other, are rather content to think it sometimes so to have been, than to labour to finde out by sundry pregnant reasons that so it was indeed." "These cliffs on either side the sea, lying just opposite the one unto the other, both of one substance; that is of chalke, and flint, the sides of both towards the sea, plainly appearing to be broken off from some more of the same stuffe or matter, that it hath sometimes by nature been fastened unto, the length of the said cliffs along the sea shore being on the one side answerable in effect, to the length of the very like on the other side, and the distance between both, as some skilfull saylers report, not exceeding 24 English miles; are all great arguments to prove a conjunction in time long past, to have been between these two Countries; whereby men did pass on dry land from the one unto the other." Verstegan supposed that the separation has occurred since the Deluge, because all beasts were then destroyed but such as were taken into the ark. "But long after it could not be before the ravenous Woolf had made his kind nature known unto man, and therefore no man, unless he were mad, would ever transport of that race for the goodness of that breed, out of the continent into any Isles. . . . . But our Isle, as is aforesaid, continuing since the flood fastened by nature to the great continent these wicked beasts did of themselves pass over."

In following the chalk cliffs of England from Dover towards Folkestone, we find that near the latter place they terminate as sea-cliffs, but strike away inland in a fine cliff-like range of hills, which is known as the Chalk escarpment. The outline of this at Folkestone Hill is shown in Plate III., the height here is 575 feet. Standing on the edge of this escarpment and looking westward, we see the steep face sink rapidly down into the lower ground to the south; the beautifully rounded forms which chalk hills assume can nowhere be better studied than here. One of the most promi-

* Restitution of Decayed Intelligence, in Antiquities concerning the most noble and renowned English Nation, &c. This work was published in Antwerp in 1628. The quotations here given are from the edition of 1653, chap. 4.

nent points of the range is a projecting spur crowned by a fine camp; it is known as Cæsar's Camp, or Castle Hill.

Looking northward from the crest of the escarpment, we see that the land falls away with an almost imperceptible slope. This plateau is furrowed by long valleys, which, commencing near the escarpment, run northward into the larger valley of the Dour, which enters the sea at Dover. Beyond the valley of the Dour the ground rises in a second escarpment, the high land of which again falls away northward into the low-lying lands of Deal and Minster.

Returning now to the sea-cliff at Dover, and examining the rock of which it is composed, we shall find that, although all of chalk, it is of various kinds of chalk. The hill on which Dover Castle stands, and all to the north of it, is "Chalk-with-flints," or "Upper Chalk."* The hard flints, generally white outside but black when fractured, lying in regular rows, each row gradually inclining to the north, and thus showing the *dip* of the beds. Passing again towards Folkestone, we find, soon after leaving Dover, that the chalk of the lower part of the cliff loses its flints, whilst they still continue on above, but get higher and higher above the sea-level, until at Folkestone Hill they only cap the cliff and escarpment. The lower division of the chalk is known as the "Chalk-without-flints," or "Lower Chalk." Most of it resembles the Upper Chalk, save that flints are absent; but the lower part of it is different in character. This is less pure in composition, contains some clayey matter, and is darker in colour than the overlying beds. This is the "Grey chalk," or "Chalk marl," the latter name being perhaps more properly employed for only the lowest part. The Grey Chalk rises from the shore a little east of Lydden Spout, and forms the base of the cliff, until we reach the undercliff, where it is in great part hidden by masses of fallen rock. It reappears at the base of Folkestone Hill, and can be traced from thence all along the lower slope of the escarpment. It is through this Grey Chalk that one proposed tunnel is intended to be driven.

Emerging from beneath the Chalk, we find a thin band of green clayey sand (Upper Greensand). On the shore below the undercliff it is 20 feet or more in thickness, but inland it thins away, until, at the foot of Castle Hill, it is only 18 inches thick; still further east it apparently disappears altogether for many miles. It is an important formation in

* The chalk of the Isle of Thanet is higher in the series than this. See a Paper by Mr. WHITAKER, Quart. Journ. Geol. Soc., vol. xxi., p. 395; and DOWKER, Geol. Mag., vol. vii., p. 466.

Surrey, Hants, and Sussex, containing there some hard beds of "firestone" and "Malm rock," which form a small escarpment. Compared with the overlying Grey Chalk the Upper Greensand is a permeable bed, or one through which water passes with tolerable freedom.

At the base of the chalk slope is a broad tract of clay, known as the Gault. This may be distinguished all round the country, at the foot of the Chalk escarpment, by most of it being in pasture. In Kent, Surrey, and Sussex, it averages about 100 feet in thickness. This bed is very constant in character; it is a stiff bluish and blackish clay, wholly impervious to water, and makes a strong dividing line between the partially porous beds above and the alternations of highly porous and partially impervious beds below.

The series of beds underlying the Gault is known as the Lower Greensand: the characters which they present are important, because it has been proposed that a submarine tunnel shall start from the English coast near Folkestone, in which case the tunnel would first be driven through these beds.

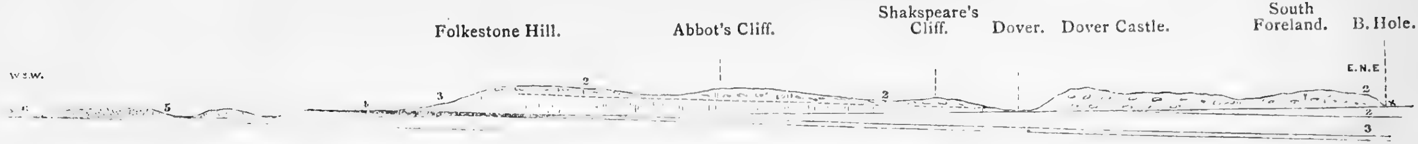
The Lower Greensand in Kent presents four well-marked divisions. Underlying the Gault is a bed of sand, known as the Folkestone Beds. It makes a very light soil, and is thus strikingly distinguished from the neighbouring Gault. It is highly porous, water passing through it with great rapidity. The division next below (Sandgate Beds) consists of clay and sandy clay; compared with the Folkestone Beds above, it is impervious to water, but it is much less so than the Gault. Its outcrop is frequently marked by a line of springs, and the water of wells sunk through the Folkestone Beds is held up by this stratum.

The third division of the Lower Greensand is known as the Hythe Beds: it consists of alternate beds of limestone and calcareous sand, the former being locally known as "Kentish Rag," and the latter as "Hassock." This division, like the Folkestone Beds, is highly porous. Below this is a band of clay, known as the Atherfield Clay, which geologists class with the Lower Greensand, because the fossils which it contains are, like those of that formation, of marine origin: for our present purpose it might be classed with the great mass of Weald Clay which underlies it, the fossils of which are of fresh-water origin.

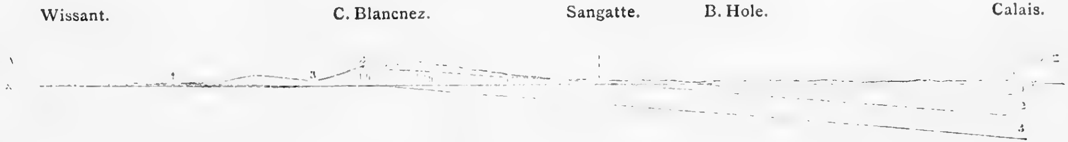
The Wealden Beds extend along the inland border of Romney Marsh, and form the high cliffs of eastern Sussex. They consist of alternations of clay and sand, the former largely predominating in the upper part, and the latter in

Plate III.

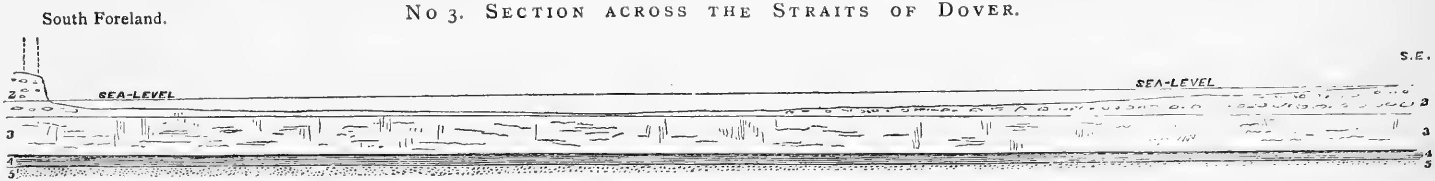
No. 1. Section along the English Coast.



No. 2. Section along the French Coast.



No 3. Section across the Straits of Dover.



Scale for Nos. 1 and 2 { Horizontal. 0 1 2 3 4 *Miles.* / Vertical. 500 1000 *feet.*

Scale for No. 3 ...... { Horizontal. 0 1 2 3 4 5 6 *Miles.* / Vertical. 500 1000 *feet.*

1. Drift and Tertiary Beds.
2. Chalk-with-flints.
3. Chalk-without-flints.
4. Upper Greensand and Gault.
5. Lower Greensand.
6. Weald Clay.

** Sea Level.

the lower part. The Weald Clay, as the highest division of these beds is called, can hardly be more than 500 feet thick on the Kentish coast,—probably it is less than that; but here there is no evidence by which we can determine its exact thickness. As a whole it is a stiff impervious clay, but it contains some thin beds of sand and limestone, which yield water.

The more sandy division below is the Hastings Beds: here are thick beds of porous sand and sandstone, divided by the Wadhurst Clay, which closely resembles Weald Clay in character. The Ashburnham Beds form the base of the Hastings Beds. These, as exposed on the coast, differ from the overlying Wealden division chiefly in the larger quantities of coloured clay which they contain. As a whole, this division is impervious, but there are many bands of sandstone which hold water.

All the beds here described dip to the north-east, and successively disappear as we pass in that direction. A well sunk through any one of the strata described will certainly meet with that which has been described as underlying it; but it is by no means certain that a well sunk, say, through the Chalk, would necessarily pass through *the whole* of those described. The reason of this we will immediately see.

Passing now to the French coast, we will suppose ourselves at Calais. This town stands on a wide flat of Tertiary beds covered up by sand and gravel. These beds underlie the whole of the flat country which stretches in a south-easterly direction past Guines. To the south of this there is rising and undulating ground, formed by the Chalk, which ends, as in Kent, with a fine escarpment. The various divisions of the Chalk are well seen along the cliffs between Sangatte and Wissant; these resemble those seen on the English coast, differing only in thickness, the Chalk-with-flints being much thinner on the French side of the Channel than on the English side. The lower divisions of the Chalk are more nearly alike.

Cropping out from beneath the Chalk we find, at first, the same beds as on the English coast; there is a thin band of Upper Greensand, a thicker band of Gault clay, and below these the top sandy division of the Lower Greensand (Folkestone Beds). These beds, as seen near Wissant, very closely resemble those of the English coast, differing only in thickness, they being each much thinner on the French than on the English coast. Perhaps the Folkestone Beds are the only division of the Lower Greensand which is here represented, for below them come some mottled clays, with sand, ironstone, and pebbles, which represent the thick Wealden formation of the English coast. The thickness of these is uncertain, but probably it nowhere exceeds 100 feet. Below them come the Oolitic rocks, which are not exposed on the coast of Kent or Sussex. We need not minutely describe these rocks. Only the higher divisions (Portland and Kimeridge Beds) are seen along the coast, the lower divisions cropping out inland. On the north-east of Marquise there are still lower beds, of the Carboniferous, Devonian, and Silurian formations, which occupy only a few square miles of the surface, but are known to underlie the higher rocks, at no great depth, over a great extent of country.

The Chalk escarpment which is cut by the coast near Wissant passes in a curved line round the country, and meets the coast again near Neufchatel, on the south of Boulogne. In a similar manner, the Chalk escarpment which is cut by the English coast at Folkestone passes round the country to the west, and reaches the Channel again at Beachy Head. The large district thus enclosed by the Chalk escarpment, on the English side, is known as the Weald. The similar district enclosed by the Chalk escarpment on the French side of the Channel is known as the Bas-Boulonnais. Although these two districts are now separated by the English Channel and the Straits of Dover, geologically considered they are one and the same.

Such being the geological structure of the coasts, we have now to consider what is the probable structure of the bed of the Channel. Here, of course, we can only be guided by analogy. We are entitled to assume that beds which have a constant character wherever we can study them, will probably maintain that character throughout the short space which intervenes between the French and English coasts. The beds of Chalk, which resemble each other on both sides, will, without doubt, have the same character beneath the bed of the Channel. When the beds are thicker on the one side than on the other, as is the case with the Chalk-with-flints, the intervening area will be occupied by that formation having its proper general character, but gradually diminishing in thickness as we pass from England to France. So, again, with the Gault; as there is no place all round the Weald and the Bas-Boulonnais where this division is absent, we may be perfectly sure that it is present in its proper position in the bed of the Channel, but with diminishing thickness as we pass from England to France. So, also, with the Folkestone Beds, which in the south-east of

England is the most constant division of the Lower Greensand, and in the Bas-Boulonnais is probably the only division of that formation which is represented. The Folkestone Beds at Wissant differ only in thickness from those at Folkestone; and there is no reason to doubt that this division also passes right across the Channel.

But here our certainty ends. We have seen that the Wealden formation is of very great thickness on the English coast; it is there not less than 1000 feet thick, and is perhaps considerably more. In the Bas-Boulonnais the Wealden Beds are probably not more than 100 feet in thickness, and it is quite uncertain which part of our English series they represent. Somewhere in the bed of the Channel the well-marked division of the English Wealden Beds must disappear, and they must gradually put on the characters which we find them to possess in the Bas-Boulonnais; but where this change takes place, and whether it is gradual or comparatively sudden, we have no means whatever of knowing. In the map which accompanies this paper I have shown the Weald Clay and the Hastings Beds striking out from the English shore in the direction in which, for some few miles, they will probably range. On the French side of the Channel I have also shown the Wealden Beds ranging for a short distance parallel with the Folkestone Beds; but I have not ventured to prolong the line far into the Channel. And, notwithstanding the fact that in some maps and sections which have appeared on this subject the lines have been projected with apparent confidence, there is not the slightest evidence to show in what direction the outcrops run.

In mid-channel there are two shoals, known as the Varne and the Ridge. It is a matter of great importance to know of what rocks these are composed; they will probably, however, be Portland or Wealden. From some experiments undertaken by M. Thomé de Gamond it seems probable that they are the former. The Portland Beds form the cliffs at Cap Grisnez, and under them there are the Kimeridge Beds. These rocks dip to the north-east; and, supposing M. de Gamond's observations to be correct, somewhere between Cap Grisnez and the Varne the Kimeridge Clay would wholly disappear. Somewhere to the west of the Varne and nearer to the English coast the Portland Beds would also disappear, and would be overlain by the Wealden series. Probably the Hastings Beds would immediately overlie them, and above these the Weald Clay would come on; but where the outcrop may be, or what the thickness of the various beds, we do not know.*

A similar doubt hangs over the submarine range of the Lower Greensand divisions, other than the Folkestone Beds. We have seen that the clayey Sandgate Beds and the calcareous Hythe Beds are well developed, and have their normal characters, on the Kentish coast; whilst on the French coast they are wholly absent. Somewhere, then, in the bed of the Channel these also must disappear; but where this change takes place we cannot tell.

There are two principal schemes proposed for connecting England and France by a submarine tunnel. There is that with which the names of Low and Hawkshaw are associated, which is proposed to run from near the South Foreland to a point between Sangatte and Calais. This tunnel it is supposed will be made wholly through the Chalk. The other is that of M. Thomé de Gamond, which he proposes to take from Eastwear Bay, near Folkestone, to Cap Grisnez.† This would pass through a number of different beds; how many we cannot tell, for it crosses the lines along which the changes above indicated must somewhere occur. There is no doubt that it would pass through all the English divisions of the Lower Greensand, for these it would intersect very near the coast; and we cannot suppose that the change which these must undergo takes place suddenly. Farther out in the Channel it would probably go through Weald Clay, possibly it might touch the Hastings Beds, beyond this it might intersect the Portland, and, finally, it would cut through the Kimeridge. But as we have no certain knowledge, nor even probable conjecture, where the outcrops of these various divisions occur, we cannot tell where the tunnel would intersect them. The various beds intersected by this tunnel are of very different characters; some are highly porous, some are wholly impervious. They all crop up somewhere in the bed of the Channel, and the porous strata among them are no doubt fully charged with water. There are two tunnels on the South Eastern Railway which pass through the Lower Greensand. There is that on the main line to Dover at Saltwood, which is driven partly through the porous Folkestone Beds and partly through the underlying Sand-

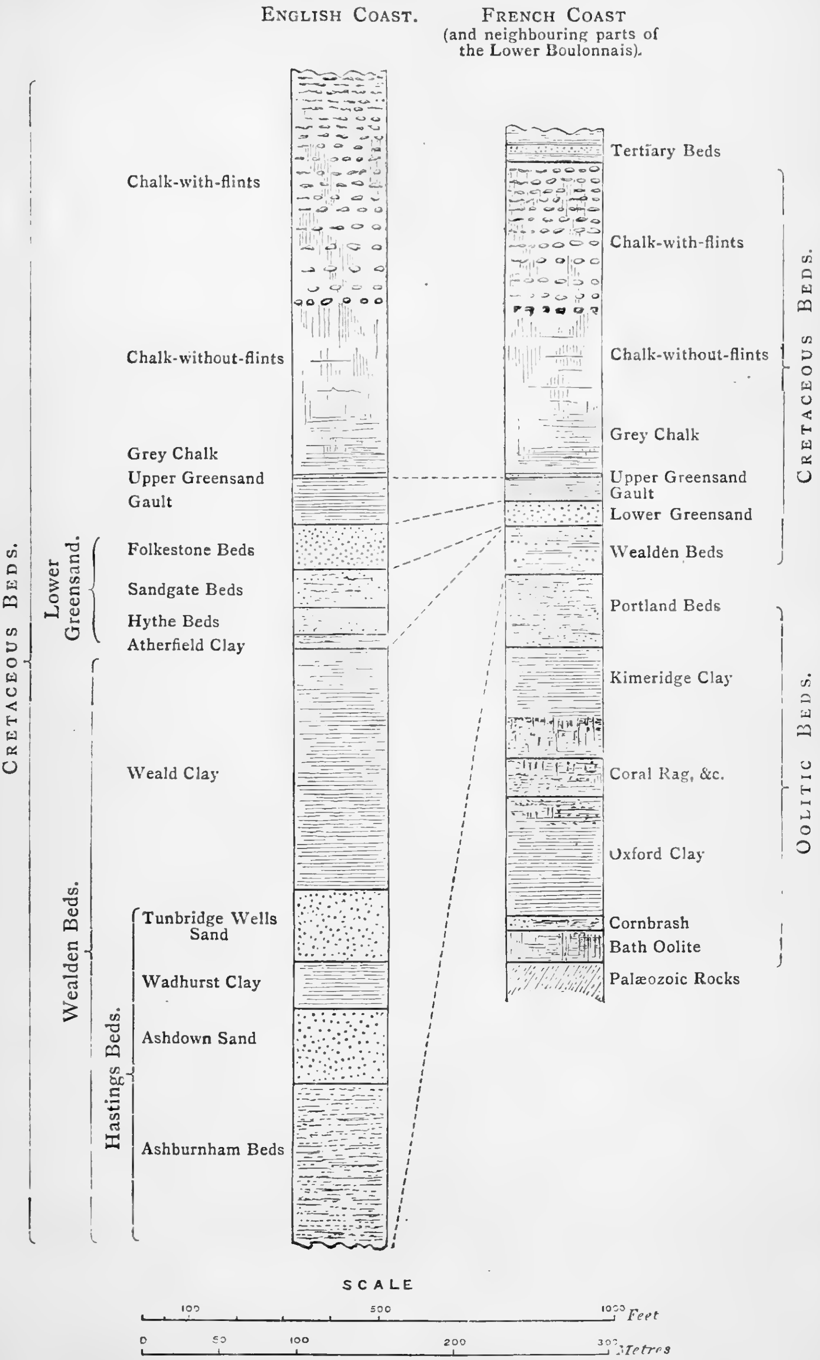
* M. Thomé de Gamond dived to the bottom between the Varne and the English shore, and brought up specimens of "Weald Clay;" but the exact spots at which they were obtained is not stated.

† M. de Gamond has apparently given in his adhesion to the schemes of Messrs. Low and Hawkshaw, so that there is really only one proposal now before the public.

Plate IV.

VERTICAL SECTIONS

ILLUSTRATING THE GEOLOGY OF THE STRAITS OF DOVER.



gate Beds. An account of the great difficulties which had to be surmounted here is given by Mr. Simms.* The Sevenoaks tunnel, on the new line to Tunbridge Wells, was driven through the Hythe Beds, and partly through the underlying Atherfield Clay. The former in their lower parts were largely charged with water, which pours out in a fully-formed stream from the tunnel's mouth.

The tunnel which it is proposed to take from the South Foreland to near Sangatte will, it is supposed, go entirely through the Chalk-without-flints.

The one great danger to be dreaded in driving a tunnel under the Channel is *water*, in quantities so great that it cannot be mastered. It is only in view of this danger that engineers need be much concerned with the geological structure of the Straits of Dover; for, supposing there were no great influx of water to be anticipated, it is a matter of *comparatively* little consequence whether the tunnel be driven through clay, sand, or chalk.

Water may occur chiefly from two causes:—the natural porosity of the rocks allowing water to pass freely down from the sea along the bedding into the tunnel; or faults and fissures cutting through the beds, and thus letting in water. We will consider the former first.

We have seen that two divisions of the Lower Greensand in East Kent are exceedingly porous (the Folkestone Beds and the Hythe Beds); these are *naturally* porous, and water passes freely along the bedding whether fissures be present or not. Any tunnel driven through them would, without much doubt, meet with very large quantities of water. Again, as regards the Wealden Beds; a large proportion of the Weald Clay is a stiff impervious clay, very well adapted for tunnelling where *clay only* occurs; but at various horizons within it we meet with beds of sand, which would most probably be charged with water. Still lower, and further out to sea, there would be the Hastings Beds, the sands of which are finer in grain, and perhaps less pervious to water than those of the Folkestone Beds, but still sufficiently so to make tunnelling through them a hazardous operation. The Portland Beds are mainly porous, whilst the Kimeridge Beds are mainly impervious; but in the latter series there are beds of sandstone which would probably yield much water.

If the tunnel could be driven wholly through the Gault there need not be much fear of water. But this cannot be

* Practical Tunnelling as exemplified in the particulars of Bletchingley and Saltwood Tunnels. 1844. 2nd edition, 1859, by W. D. Haskell.

done; the bed is too thin for this on the French coast. There is no other bed of clay which can be chosen, for no other goes right across the Channel. The Weald Clay of the English coast disappears, or else vastly changes its character before reaching the French coast. The Kimeridge Clay of the French coast may not pass beneath the English coast, and if it does so it would be at too great a depth to be of use. Moreover, these clays contain beds of sand, which if tapped by the tunnel would be, to say the least, exceedingly troublesome.

The Chalk now remains to be considered. This, there is no reason to doubt, passes evenly beneath the Channel from shore to shore, and throughout it may be expected to maintain its usual characters, subject only to the decrease in thickness already noticed, The Chalk is well known to be a water-bearing bed; all the deep wells near London get their water from it. But in this respect it is not all alike.* The Upper Chalk is more pervious to water than the Lower Chalk, and in that the water does not pass at all readily through the *mass* of the chalk, but runs along joints and fissures, and especially along the lines of flint. In sinking wells through the Chalk it is frequently found that water is got at some of the bands of flint, whilst the intermediate bands of Chalk are either without such water, or else allow it to pass much more slowly.

In the drainage works at Norwich it was found that water percolates slowly through the re-arranged chalk which occurs there, but "in the hard and undisturbed chalk the water sooner disappears, although in larger quantities, through the various joints and fissures, and along the horizontal beds of flint, until it reaches the level of saturation."

The following paragraph is of sufficient importance to be reprinted here: †—

"The most important point in the sewerage works, as bearing indirectly upon the excavation of the proposed Channel Tunnel, is the fact that the chief difficulties met with in engineering operations occurred beneath the river level. Here the chalk and marl (re-arranged chalk) are thoroughly saturated, and, at only 20 feet below the level of the water in the river, it was found necessary to close-timber the top and sides of the tunnel before the brickwork could be put in, on account of its giving way by the pressure of water. The springs under the head of only 20 feet also

* These and similar questions are fully discussed by Mr. PRESTWICH in his "Water-Bearing Strata of London," 1851; pp. 57 to 74, and 134 to 142.

† J. E. TAYLOR and A. W. MORANT: "The Water-Bearing Strata in the Neighbourhood of Norwich. "Geological Magazine," vol. vii., p. 121, 1870.

boiled up with great force. On the contrary, above the water level, driftways were driven hundreds of feet in length, and left for months together without any timber shoring being required. It requires little imagination to perceive how great will be the pressure of water, and how immense the difficulties arising from it in the proposed Channel Tunnel excavated beneath 20 or 30 fathoms of water, and through a bed of hard chalk, doubtless abounding in joints and fissures. It has been the low level, 20 feet beneath that of the river, which has created the chief engineering difficulty and expense attending the sewerage works at Norwich."

If the foregoing description of the works at Norwich may be taken as a fair example of what would occur beneath the Channel, of course it is idle to think of driving a tunnel more than 20 miles long through such rocks, the whole of the water met with having probably to be raised to the surface by pumping. But there is no reason to think that the Chalk beneath the Channel will resemble this; rather is there every reason to think that it will be very different from it.

In the first place we must remember that these Norwich works were in the Chalk-with-flints, wherein water is sometimes abundant along certain lines. They were carried on very little below the river, and consequently any small joints or fissures would serve to convey the river-water down; whereas with a larger mass of rock above the works the joints and fissures might die out, or give place to others along different lines, and thus the water-channels would not be continuous.

But there has been no proposal to carry a tunnel through the Chalk-with-flints. It is proposed to make it mainly, if not entirely, through the lower part of the Chalk-without-flints, which is not itself a truly permeable bed. Indeed, even if it were fully charged with water, as probably it would be, it would yield only a small quantity of that water from its own mass; almost the whole of the water met with in the tunnel would enter by joints and fissures.

Joints and fissures in rocks are not formed as *open cracks* in the rocks; they become so by the incessant passage of water along them. Wherever water is passing through a jointed or fissured rock, especially if that rock be calcareous, it is gradually enlarging the cracks, and is rendering the passage of water more easy. But if the rock be beneath the sea, although it may be fully charged with water, this water may not, and probably would not, pass through it; hence there would be no tendency for the joints to increase in width. They would probably be closed cracks, not always easily detected, even by a tunnel intersecting them. In a similar manner we would not expect to find openings or small caverns in the Chalk, such as well-sinkers sometimes meet with; for these also are caused by the incessant passage of water wearing away the chalk more along certain lines than others.

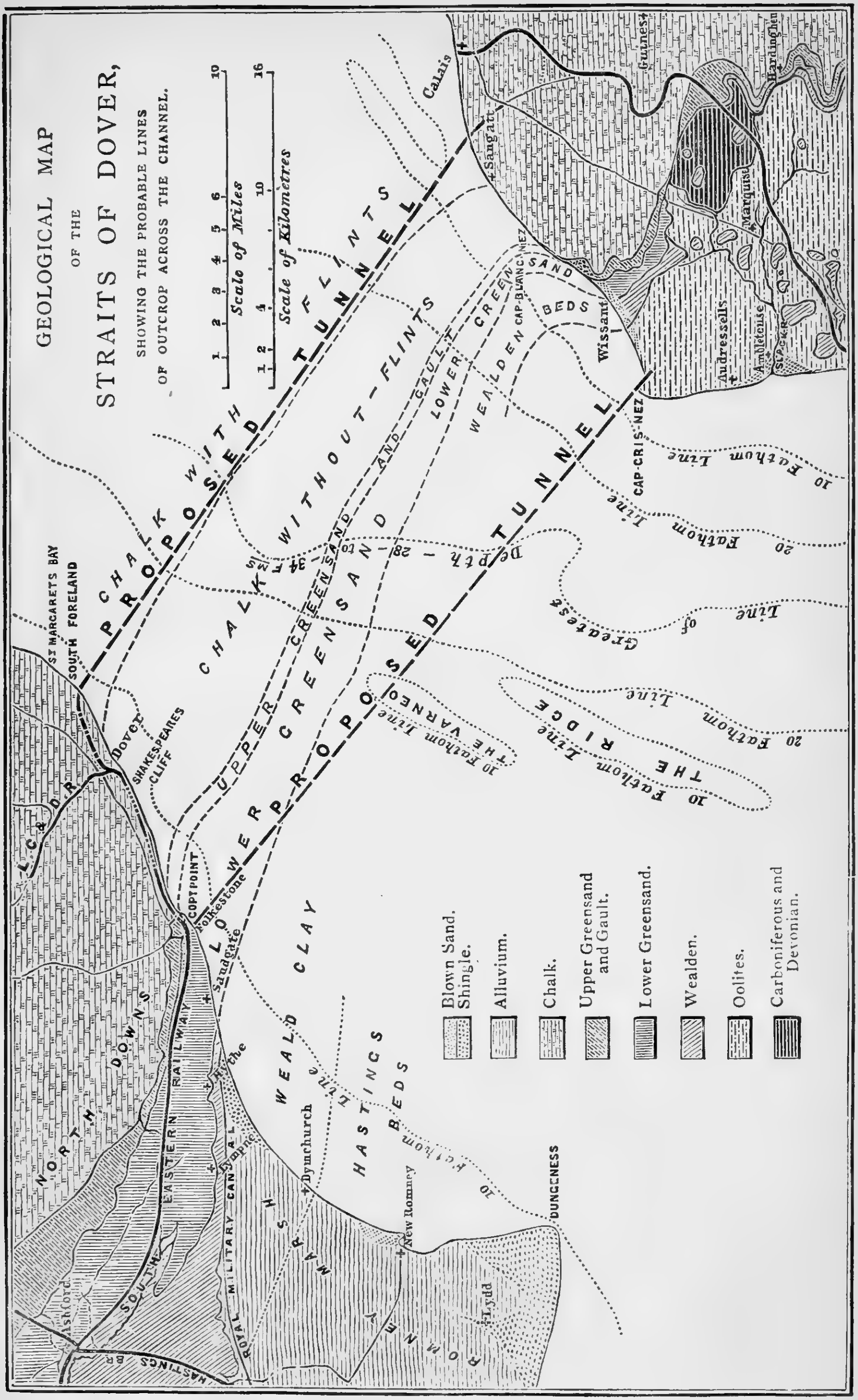
There is one other point connected with this subject which may be noticed here. It was said above that a deep well sunk through the Chalk would certainly meet with the bed next below it, but might not pass through the whole series of rocks if continued to a greater depth. The reason of this is, that under the north of France and the south-east of England there is a ridge of Palæozoic rocks, a part of which is exposed in the Bas-Boulonnais. It has been found in the deep well at Calais, and in wells at Harwich and Kentish Town. So far as is known the Chalk series is always perfect above it; some of the Gault is also found, but below that we may come directly on to the older rocks; or there may be a thickness, great or small, of the Lower Greensand and underlying beds.

As this ridge of old rocks is always, as far as is known, well below the Chalk, it is almost certain that a tunnel driven through the Chalk would not meet with it. But a tunnel driven through the underlying beds might do so. It is not at all likely that such would be the case, for on the French coast there is a good thickness of Oolitic strata above the old rocks. Apparently these old rocks are more deeply covered up by secondary strata as we pass westwards.

But if the Palæozoic rocks were met with in the tunnel it would probably not cause any inconvenience in the work. Perhaps it might turn out to be a very great advantage; for where these rocks have been reached in wells *no water has been obtained*. It may be that these rocks are destitute of water, certainly the water that they contain, if any, does not rise in the wells; and hence the wells and borings referred to have failed as sources of water-supply, although the information they give is of great interest and importance.

It is not likely that any tunnel crossing the Straits of Dover would meet with these old rocks; yet, as they are known to underlie the Chalk at no great depth, they may prove of great service; for if the shafts, which will be sunk on both coasts in starting the tunnel, were carried down well into the old rocks, the water might pass from the tunnel, down the shafts, into these old rocks, and there disappear.

Plate V.


GEOLOGICAL MAP
OF THE
STRAITS OF DOVER,
SHOWING THE PROBABLE LINES
OF OUTCROP ACROSS THE CHANNEL.
Scale of Miles
Scale of Kilometres
Blown Sand. Shingle.
Alluvium.
Chalk.
Upper Greensand and Gault.
Lower Greensand.
Wealden.
Oolites.
Carboniferous and Devonian.
PROPOSED TUNNEL
PROPOSED TUNNEL
CHALK WITH FLINTS
CHALK WITHOUT FLINTS
UPPER GREENSAND AND GAULT
LOWER GREENSAND
LOWER GREENSAND
WEALDEN BEDS
WEALD CLAY
HASTINGS BEDS
ROMNEY MARSH
NORTH DOWNS
SOUTH EASTERN RAILWAY
ROYAL MILITARY CANAL
HASTINGS BR
L.C.&D.R.
Greatest Depth 28 to 34 Fms
THE VARNE
THE RIDGE
10 Fathom Line
20 Fathom Line
Calais
Guines
Hardinghen
Sangatt
Marquise
Wissant
Audressells
Ambleteuse
CAP GRIS NEZ
CAP BLANC NEZ
ST MARGARETS BAY
SOUTH FORELAND
Dover
SHAKESPEARES CLIFF
COPT POINT
Folkestone
Sandgate
Hythe
Lympne
Dymchurch
New Romney
Lydd
DUNGENESS
Ashford

What is here suggested is often done on a small scale, where a dry mass of rock is known to underlie the surface. In quarries the water which collects in the bottom, being perhaps held up by an impervious bed of clay, immediately beneath the stone that is worked, is often got rid of by boring a hole down to a lower porous bed which contains no water. Of course this fact must first be ascertained, or the boring may bring up more water into the quarry. All well-sinkers are, however, familiar with the fact that water found in a well at one stratum is often lost if the well be continued further down; and therefore they often rest content with a comparatively small supply, rather than continue the well further down, and thus risk losing the whole.

Wet lands, overlying chalk or other limestones, are often drained by sinking a pit to such rocks, and conducting the surface drains into the pit. Town sewage has been disposed of in a similar manner, perhaps to be again pumped up by neighbouring deep wells.

Whether or not the Palæozoic rocks will carry off water in the way here suggested can easily be ascertained by experiment. If they fail to do so, then the water must be raised to the surface by pumping. If they do thus carry off water, there will not only be an immense saving in the cost of pumping, but, what is of far greater importance, there will be no danger of a great influx of water drowning out the tunnel.*

One great danger anticipated by some people in driving a tunnel beneath the Straits of Dover is, that of meeting with some great fault, or enormous dislocation of the strata, to which they suppose the formation of the Straits to be due. There is, however, not the slightest evidence to warrant any such anticipation. So far as we can judge from a study of the shores the chalk seems to pass evenly across the channel-bed, and such examinations of that bed as have been undertaken tend to confirm this opinion. The Straits of Dover have been worn through by the slow and long-continued action of the waves. It is, however, very likely that at one time, when the land stood at a higher level, and before the sea had eaten out the Straits, that a river ran from south to north through the chalk escarpment, which then stretched across from Folkestone to Wissant. The higher streams of this old river are the Rother on the

* I do not wish much importance to be attached to these suggestions; for the rocks may be full of water and yet yield no rising water to wells; or they may be comparatively dry and yet so compact in structure as to prevent water from passing readily through them.

English side, the Wimereux and the Slack on the French side. Perhaps at that time, or at a still earlier period, the South Downs also stretched across the Channel, to join the chalk escarpment on the south of Boulogne. Between the North and South Downs there would be a mass of high land, ranging nearly east and west, joining the high cliffs of Fairlight with those of La Crèche.

The North Downs are now cut through by several rivers which drain the Weald; they are from east to west,—the Stour, the Medway, the Darent, the Mole, and the Wey. Whatever causes may have formed their valleys will also have formed the old valley in the Straits of Dover, which we may call that of the Rother. But there is no evidence to prove that any one of these valleys owes its formation to disruption of the strata; they have all been wholly excavated through the solid rocks by the rivers which run in them. We have, therefore, no reason to suppose that the Straits of Dover has been caused by faults or dislocations. On the contrary, we have every reason to suppose that the beds range uninterruptedly across from shore to shore.

There is probably no geological formation in England through which more tunnels or deep cuttings have been made than through the Chalk. All the railways, in approaching London, from the north, west, or south, must pass the Chalk escarpment; and although advantage has usually been taken of valleys which break through the escarpment, yet in many cases tunnels have been necessary. These tunnels and deep cuttings are generally driven through the lower beds of the Chalk,* the same as those through which it is proposed to take the Channel Tunnel. There are hundreds of wells in the London district which penetrate more or less deeply into the Chalk.

There is, therefore, abundance of information available as to the kind of work which must be undertaken. All these tunnels and cuttings are, however, driven above the sea-level, and similar works undertaken below the sea *may* meet with far greater difficulties. But this is not at all sure to be the case. The lower beds of chalk beneath the sea may contain no more water than those beneath the neighbouring shores.

There are numerous cases in the mining districts, both of metal and coal, in which the workings are carried far out

* Some of these were described in a Paper by Mr. S. Hughes, "On Chalk Excavations, and on the Means adopted under Different Circumstances of Intersecting the Great Chalk Ridges of England, for the Purposes of Railway and Canal Communication." Civ. Eng. and Arch. Journ., vol. ii., p. 207, June, 1839.

below the bottom of the sea; yet it is only in exceptional cases that an extra quantity of water is met with in such workings; often, indeed, the workings are drier beneath the sea than beneath the land. Faults, too, are cut in the coal-workings beneath the sea, as beneath the land; but these give no more trouble and yield no more water in the one case than in the other. The same fault is frequently cut several times in parallel lines of workings.

To sum up the main geological points of this paper:—We have seen that the Chalk, the Gault, and the Folkestone Beds, have the same characters on both shores of the Channel, and that they may fairly be assumed to pass evenly across the bed of the Channel. The beds below the Folkestone Beds differ on opposite sides of the Channel, and we have no means of telling how these various beds run beneath the Channel, nor their probable thickness at different points.

Any tunnel driven through beds lower than the Folkestone Beds would therefore be undertaken with this disadvantage, that there would nowhere be any certainty as to the rocks which would have to be pierced. Of this, however, we may be quite sure,—that the water-bearing beds of these rocks would be fully charged with water, and that they are of such a nature that water would be discharged in large quantities into the tunnel.

A tunnel driven through the Chalk would have this advantage,—that it could certainly be taken through the Lower Chalk for most, if not all, the way; and though the Chalk itself may be fully charged with water, yet it would not readily discharge that water into the tunnel, except through fissures; and we have every reason to suppose that such fissures would be far less common and of far less importance than those met with in Chalk above the sea-level.

Lastly, there is no reason to suppose that any great fault will be met with during the progress of the work.

V. THE GOLD COINAGE.

THE right of coining in England, as in other countries, has always been the privilege of the Crown, and a national gold coinage begun with Henry III., but before that time gold pieces of money known as "byzants," of the value of about ten shillings, were coined at Constantinople and circulated freely in England. At a subsequent period "florences" were circulated in the same way, their name being derived from the fact that they had been struck at Florence. English gold coins have borne various names at different times. The "noble" of the reign of Edward III. was followed by the "angel" and "rose noble," or "rial," of Edward IV.; then came the "double rial," or "sovereign," of Henry VII., and the "laurel" of James I.; the latter coin was adopted by Charles II. at the Restoration, and afterwards became known as the "guinea." The present "sovereign" was issued by a proclamation, dated July 1st, 1817, under the authority of the latter part of section ii. of the Act of 56 of George III., cap. 68, passed in 1816. Its standard of fineness is 22 parts fine gold and 2 parts alloy; as it is impossible to obtain an exact admixture of metals in coining, a "margin" has at all times been allowed to Mint masters; this margin of departure from the standard for gold coins is at present fixed by law at 2 parts per 1000. There is also a "margin" permitted by law of departure from the standard weight, but modern improvements in machinery have permitted this departure to be confined within very narrow limits, although in former days it seems to have been used to some extent in defraying the expenses of coining. The present standard weight of a sovereign is 123·27447 grains, and the variation from the standard weight permitted by law is 1·6 in each 1000 parts.

Any person may take gold of not less than standard fineness to the Mint, and the Mint is bound to return for it the same weight in coin. The gold so "imported," as it is technically termed, must be accompanied by a "trade assay report," stating its degree of purity, and the importer or his agent must be present to verify, with the Mint officers, the weight of the importation. The bullion is then assayed at the Mint, and if the importer does not object to the degree of fineness thereby determined, it is melted with the proper proportion of alloy, and coined. The "alloy" is copper. Practically, however, the Bank of England is the only importer of gold bullion, because the owners of ingots can readily realise their value by selling them at the Bank, which is compelled by the Bank Charter Act of 1844 to purchase them at £3 17s. 9d. per oz. If they took the gold to the Mint they would get three-halfpence per ounce more for it, but then there would be the delay of melting and coining, and the disadvantage of receiving the money in sovereigns instead of bank-notes: the latter might be carried away in a purse, and the former might have to be carried away in a sack. The Bank of England accepts gold in ingots, weighing as a rule 200 ozs., from Rothschilds, Raphael and Sons, Browne and Wingrove, and other large houses, and they bear the "brands" of the said houses. The Bank of England makes a profit of about £2000 on each million by this three-halfpence per ounce.

As regards light gold coins, the law is that any person who is tendered such, "shall, by himself or others, cut, break, or otherwise deface, any such coin tendered to him in payment." Light gold is received by the Mint at the same rate as ingots. As only the Bank of England and a few other bodies comply with the requirements of the Act, it is to be feared there is much light gold in circulation, whereby the public will be the losers. Formerly coins were "sweated" by attrition or by being shaken together in large quantities in a bag; but comparatively recently the aid of electricity has been invoked, and precious metal is removed from coins by electro-dissolution. An interesting case of this kind was tried at the Central Criminal Court, January 31st, 1870.

The operations of the Mint may be divided into two classes—the metallurgical and the mechanical.

After the ingots reach the Mint from the Bank of England, the mechanical methods of turning them into sovereigns are very simple, although the machinery employed is necessarily made to do its work with the greatest precision and accuracy. The gold is first melted in a crucible about as big as a man's hat, then it is poured into moulds, whereby it is made into bars 24 inches long and half an inch deep. These bars are then rolled cold, by different sets of rollers, till they each form a band. These bands are then forcibly dragged between two motionless surfaces of steel to bring the band to a standard thickness, from which "blanks" for sovereigns can be punched. After the little blank discs are punched, they are passed through a machine which raises but does not mill their edges. In the next machine the sovereign is finished by stamping, and the one blow both impresses the effigies and dates and mills the edges. Next they are weighed by automatic machinery, which separates those which are of wrong weight from those which come within the limit of error permitted by law, and the former are sent back to the melting-pots to go all through the processes again. At various stages in these operations the gold has to be assayed by the Chemist of the Mint. The melting and rolling processes are of much the same character as making rails for railways, only the process with regard to gold is more delicate and exact; there are also special precautions to prevent loss of metal.

The ingots are assayed when they first reach the Mint, and then the amount of fine gold or of copper to be added to them, to bring them to the standard required by law, is calculated by a rule-of-three sum. Copper is used as the alloy. About 1200 ozs. of gold and alloy are sent to each melting-pot, and this operation is called "potting the ingots." It is advisable to consider the various processes in greater detail, and commencing with the operation of melting, it may be observed that there are seven furnaces at the Mint which are used for the fusion of the gold, each 1 foot square and 2 feet deep to the top of the bars. The melting-pot is made of a mixture of Stourbridge clay and plumbago; it is $9\frac{1}{4}$ inches deep and 7 inches in inside diameter at the top. The pot is placed in the furnace on a bottom which rests on two bars, then it is covered by its muffle and lid, and surrounded by fuel, which gradually heats the pots to redness; the ingots are then placed in it, and the alloy added through a funnel. When the whole mass is melted, the foreman stirs it with a rod made of plumbago and clay, and can tell by the feel when the metal reaches that particular state of viscosity which causes it, when moulded, to form a workable bar. The firing is then poked out, the muffle and lid of the pot removed, the pot lifted out with a tongs, and by suitable means supported by a loop of iron and a cord hanging from the ceiling. Before the melting, pieces of charcoal are placed in the bottom of the pot, for the purpose of reducing any oxide which may be present in the alloy, for copper will dissolve oxide of copper, and oxide of copper often makes the gold very brittle. This charcoal afterwards floats on the top of the melted gold, and a piece of stick held at the mouth of the pot whilst pouring into the moulds, stops the charcoal but not the gold. Each pot of gold forms seven bars. The moulds are made of cast-iron, and a large number, about twenty, of these moulds are mounted on a tram, which can be moved from one part of the building to another on rails. Each bar is marked to show from what pot it was poured; it is also marked with a letter which indicates the date of moulding. Bars for sovereigns are 2 feet long, 1·375 inches broad, ½ inch thick, and have an average weight of 180 ozs. Troy. Bars for half-sovereigns are 2 feet long, 1·125 inches broad, ½ inch thick, and weigh 170 ozs. Troy. Assay pieces are cut from each bar, and so labled as to show from which bar they come. The assay piece is flattened out, and from a bar a piece is cut, which is sent to the Assayer of the Mint.

The bars next have to be rolled. They are passed between steel rollers about seven times, the distance between the two rollers being reduced each time, and the mechanical arrangements are such that this rolling greatly lengthens the bar and reduces it to a band, but does not materially increase its breadth. Owing to the wear of the moulds in which the casting is done, the bars are never of exactly the same size. After the process of rolling, the resulting bands are very hard. Their ends are sheared off, and then they are 20 inches long.

The sheared bands, if for half-sovereigns, have next to be annealed. They are placed in copper tubes made without solder, and the ends of the tubes are luted on with clay. These tubes are then placed on an iron carriage, and run into the furnace, in which they are left for twenty minutes. Then the hot tubes are plunged into cold water as quickly as possible, with the bands inside. This rapid cooling prevents the oxidation of the copper; also, if they were cooled slowly, the gold would become so pasty and soft as to stick to the machinery. The bands are then rolled a little more, and passed through the last or "gauging" mill. Now and then the workman at this mill will punch a blank sovereign out of the fillet, and weigh it, to see that he is giving the fillets the right thickness, and he has the power of accurately adjusting small differences in distance between the rollers.

The fillets after being weighed are taken to the "drag" room, for they are not made of uniform thickness by simple rolling. The ends of the fillet are then passed to the extent of 2 inches through the rolls of a flatting mill. Then the fillet is passed to the draw-bench, where it is dragged between two pieces of steel, so as to become of the exact gauge. The flattened end is first put between the pieces of steel; an iron "dog" then bites hold of the end, and drags the rest of the fillet through the opening. The rolling mills and the dragging machinery are, of course, all driven by a steam-engine. After the fillets have passed the draw-bench, they are cut into lengths of 18 inches, and sent to the "trier," who punches out one or two blanks from each length, and weighs it or them very carefully, and he allows for a slight loss which will afterwards arise from annealing. The fillets are next taken into the cutting-out room, where the blanks for sovereigns are punched from them; the rest of the fillet then looks like a ribbon full of round holes, and these perforated bands or "scissel" are returned to the melting-pots.

The blanks are frequently examined, to see that their edges are smooth, and they are frequently weighed: this part of the operations is called "pounding." They are then put into bags in batches of about 720 ounces. Afterwards they are "rung" by boys, to sort out those which may be dumb or cracked. Cracked blanks are caused by bubbles of air enclosed in the metal at the time it is cast into bars. Next the blanks are taken to the edge-compressing machine, which rolls each blank with slight pressure, so as to give a "collar" to it. This machine rolls 700 blanks per minute. The blanks are afterwards annealed in copper tubes, 2804 being placed in each tube; the lids of the tubes are luted with clay, and then they are placed in a reverberatory furnace for about twenty minutes. They are then taken out, and when they are at a very low red heat, or at a temperature lower than that at which copper combines readily with oxygen, the tubes are plunged into a cistern of cold water. When the tubes are cold the blanks are removed, and after drying in sawdust they are ready for transmission to the coining-press.

The old atmospheric presses of Watt and Boulton are still in use at the Mint, and the obverse and reverse of the coin, as well as the milling of the edge, is completed at one operation. These old presses, which have done their work for so long a time, certainly are efficient, and there is no doubt that—looking to the nature of the coins produced by them—they leave but little to be desired; but a report of a Commission which visited the various Mints of Europe states that in all the European Mints visited, with the exception of Constantinople, the coining is performed by lever presses of German or French construction. The noise of the old atmospheric press is deafening, and doubtless the advantages and disadvantages of substituting the lever press for it will be duly weighed. And here we might venture to suggest that the designs on the coinage do at least admit of a somewhat more artistic treatment than that shown on the coins now in circulation. It should be remembered, however, that within the last two years Pistrucci's beautiful design of St. George and the Dragon has again been restored to the sovereign.

The last mechanical operation to which the finished coins are subjected is the weighing, which is performed by automatic balances devised by the late Mr. Cotton. It is impossible to describe these beautiful machines without the aid of diagrams; it will be sufficient to state that the coins are not only weighed, but are divided into three classes, the light, the heavy, and those which are either of standard weight or are within the "remedy" or allowance permitted by law.

Formerly the law directed that the "remedy" should be on a pound weight of the coins, but the Coinage Act of 1870 specified the actual legal weight of each individual coin, and rendered it necessary that each separate piece when issued, instead of each pound weight of pieces, should be within the limits of weight and fineness assigned to it: for instance, the weight of the sovereign is, as has been already stated, 123·2747 grs., and the variation permitted is 2-10ths of a grain above or below this weight.

It will be evident that in actual working a considerably smaller "remedy" must be used in order to render it impossible for a coin, the weight of which exceeds the prescribed limits, to pass into circulation. It will be evident to all who are familiar with the small dimensions of a weight which represents the 1-10th of a grain, that the attainment of accuracy in coinage must be attended with many difficulties. It is nevertheless the fact that the aggregate weight of the coins representing many millions in value does not exhibit a greater variation than 1 oz. from the exact standard.

It must be remembered that the law also directs coins to be produced of an alloy the true composition of which is rigorously guarded; therefore the most important chemical operations of the Mint is the process of assaying. A piece of metal is first brought to the exact weight by cutting or filing: each weighed portion is then added to molten admixtures of lead and silver contained in porous cups or "cupels" of phosphate of lime, which are arranged in rows in a muffle or small oven. The proportions of the latter metals are calculated so as to bear a definite relation to the supposed amount of gold and base metals present in the alloy. The lead oxidises and is absorbed by the porous "cupel," together with the copper and other oxidisable metals, and the silver and gold remain in the form of a button, which may also contain platinum, iridium, or metals possessing similar properties. The button is reduced by rolling to a thin strip, which is annealed and bent into a loose coil or "cornet." The cornet is placed in nitric acid of the specific gravity of 1·25, and the acid is maintained at incipient ebullition for fifteen minutes; the coil is then treated in a similar manner with nitric acid of the specific gravity of 1·4, the result being that the silver is removed by the action of the acid, and the gold remains in a spongy state. The sponge of the gold retains the original form of the coil, but it is necessary to impart a certain degree of coherence to the metal by annealing it at a dull red heat. A small quantity of silver is invariably retained with the gold. It is necessary therefore to make check assays on the pure gold, or on standards of known composition, upon which the accuracy of the result will in a great measure depend. The concluding process consists in weighing the gold cornet. The weights employed bear a decimal relation to the original weight of the assay piece operated upon, and the amount of gold therefore present in the alloy is at once indicated without further calculation.

Analysis of assay reports,* extracted from an official memorandum by the Chemist of the Mint, shows that coins taken from half a million of gold pieces, issued by the Mint in the year 1870, had the following composition:—

				Per Mille of Gold.
3	per cent	of the coins	contained	916·3
10	,,	,,	,, ...	916·4
25	,,	,,	,, ...	916·5
29	,,	,,	were of exact standard	916·6
14	,,	,,	contained	916·7
9	,,	,,	,, ...	916·8
8	,,	,,	,, ...	916·9
2	,,	,,	,, ...	917·0
100				

Mean composition of the coins . { 916·617 gold.
83·383 copper.

1,000,000

That this result exhibits the attainment of a very high degree of accuracy will be evident when it is remembered that the allowance prescribed by law would have permitted a variation, above or below the exact standard, of two parts in the thousand.

* Appendix to First Annual Report of the Deputy Master of the Mint (1870).

It is well to allude to a difficulty incidental to coining, arising from the occasional brittleness of the gold alloy, which is generally due to the presence of minute quantities of lead, antimony, arsenic, or bismuth. Occasionally the use of impure copper to alloy the gold introduced these other metals, so very great care is taken in the selection of pure samples of copper. Imperfect annealing will sometimes make the gold brittle. The brittleness, however, is more frequently due to the admixture of baser metals. There is a remedy, however, for the evil, which is found to be the most simple and efficacious. It consists in sending a stream of chlorine gas through the molten alloy; the gas rapidly acts upon the baser metals, combines with them, and the chlorides are then driven off as vapour. In order to test the process, an experiment was made on gold made brittle by the addition of 0·05 per cent antimony and 0·05 per cent of arsenic, making a total base alloy of 0·1 per cent, while 0·05 per cent of either metal would have produced very brittle gold. By passing a stream of chlorine through this gold, for $3\frac{1}{2}$ to 4 minutes, it was converted into gold of excellent quality.

Formerly, when bars of gold were found to be unfit for coinage, the impure metal was returned by the Mint to the importer, and the original melters, who were held responsible, not unnaturally objected to bear the loss. Now, in consequence of the successful result of the above-mentioned experiments, the Mint no longer rejects bars which during the process of coining are proved to be brittle: thus all vexatious disputes have been avoided.

Of late years there have been many important changes in the establishment of the Mint: of these probably the most important was that effected in 1848, namely, the abolition of the contract system, by which for many centuries the Mint had been worked. The old "Masters of the Mint" were Contractors under the Crown, and their operations were controlled by officers, the principal of whom were the Warden and Controller, and the Assay Master, each exercising independent functions, the Assay Master being responsible to the Crown and to the people for the "standard fineness" of the coin.

After the abolition of the office of the "Moneyers," eminent scientific men have held the office of Master of the Mint. Now the office devolves upon the Chancellor of the Exchequer, and the appointment of an officer with the title Chemist of the Mint proves that the scientific requirements of the Department have been duly provided for; indeed, the Reports which lately have been presented to Parliament, while they abundantly show that the Department has undergone a thorough re-arrangement, also attest the fidelity and accuracy with which the operations of coining are conducted.

VI. BRIEF NOTES ON RECENT CHANGES IN BRITISH ARTILLERY MATÉRIEL.

By Capt. S. P. OLIVER, F.R.G.S.

IN looking through the Extracts of the Quarterly Report of Proceedings of the Department of the Director-General of Ordnance (from vol. viii., part 1, to vol. ix., part 3), it will be observed that the labours of the various committees have not been light or unsuccessful. The results of various experiments and proposals are given by at least a dozen of these said committees, of whom the principal are the following, viz., committees on mitrailleurs, on torpedoes, on range-finders, on gunpowder and other explosives, on ship's deck and turret targets, on mountain artillery equipment, on rifled shell guns for field service and heavy rifled howitzers and mortars for vertical fire, on Palliser's method of strengthening cast-iron guns with internal tubes, on Palliser's projectiles, on traversing arrangements, on wads for preventing erosion of bore, on Moncrieff carriages, on shells and percussion fuzes for field service, on improvements of bronze, on proportions of ammunition for ordnance and small arms to be maintained in fortresses at home and abroad, besides Noble's important electro-ballistic experiments, and Majendie's experiments relative to the distance of cartridge-filling sheds, &c.

From an inspection of the reports of the above, it would appear that the following results are noticeable as affecting future warfare and modern requirements:—

I. The immediate introduction into the service of the small Gatling gun of 3 cwts., with musket-proof shields, and to be drawn by two horses. This gun is found to be a most effective weapon in defending villages, field intrenchments, caponnières, for covering the approach to bridges or *têtes-du-pont*, for defending a breach, and for employment in advanced trenches and field-works where economy of space is important. For naval purposes these machine guns are well adapted for use in the tops of vessels of war, for boat operations, service up close rivers, &c.

II. Gunpowder Service P.* (as the pebble-powder described in the last number of the "Quarterly Journal of Science" is officially designated) is to be introduced for battering charges of all rifled guns of 7-inch calibre and upwards, and for all service-charges of 40 lbs. and upwards, so soon as a sufficient quantity has been manufactured.

III. Additional experimental proof of the special value of compressed gun-cotton for demolition of stockades, bridges, &c., and for submarine mines. Capt. Noble also proposes to apply gun-cotton yarn as a fuze priming, it possessing the advantage of consuming almost instantaneously, thus not interfering with the burning of the fuse, whilst its sensitiveness makes ignition certain, even by the lowest charge.

IV. "Picric powder" is not unlikely to be used in future. It is an invention of the War Department chemist, Mr. Abel, who has been designing this explosive agent specially provided for charging shells. This mixture is neither so violent in its action as gun-cotton, nitroglycerine, or picrate of potash powder, but at the same time is a much more powerful explosive than gunpowder, and has other properties which appear to render it peculiarly adapted for employment in shells; it is readily and expeditiously prepared, can be pressed and granulated without difficulty, and at the same time is particularly remarkable for its *safety* as compared with all other explosive agents; it is, in fact, somewhat less sensitive to ignition by percussion than gunpowder. By recent experiments made it appears that picric powder, when used as a bursting charge for Palliser shells, will sustain the action of battering charges of R.L.G. powder in guns up to and including the 9-inch of 12 tons: the safety of this substance is being further tested from 10-inch and 12-inch guns.

V. The last explosive compound to be noticed, as likely to take no small part in future warfare, is Kreb's patent "lithofracteur," which is in use in the Prussian service. From Mr. Abel's analysis it appears to be composed as follows :—

Nitroglycerine	42	parts
Nitrate of soda	25	,,
Sulphur	4	,,
Sand, siliceous earth, sawdust, and coarsely-powdered coal	29	,,
	100	,,

* See "Modern Cannon Powders," Quart. Journ. of Science for January, 1872, p. 58.

The "lithofracteur" differs from the nitroglycerine preparation called "*dynamite*" chiefly in containing a proportion of gunpowder constituents, viz., saltpetre, carbon, and sulphur, mixed up with the other ingredients. From the samples analysed it appears that this substance is crudely mixed, varies considerably in composition, and possesses no improvement over dynamite. On the 20th February, 1872, some exceedingly instructive trials of this compound were made—by Professor Engels, of M. Kreb's Cologne factory—at Mr. France's Breidden Quarry, on the banks of the Severn, near Shrewsbury, before the W. O. Committee, when it appears to have most favourably endured the various tests, both chemical (to ascertain the fuming and explosive points) and mechanical (to test its safety under heavy blows and concussion); for instance, 5 lbs. of lithofracteur was exposed to a fierce flame and consumed without explosion. Cartridges of it were subjected to heavy blows, such as the fall of round shot upon them, when the plastic substance, although squeezed out, neither inflamed nor exploded. Similarly, it was ignited by a Bickford fuse in confinement, and inflamed without explosion. Naked cartridges fixed to buffers of a railway truck, allowed to run on an incline into violent collision with a stationary one, also failed to explode. On the other hand, the same substance when fired with a Bickford fuze and detonator, in thin zinc tubes $4\frac{1}{4}$ inches in diameter, either against stockades, in mines, or under water, exploded with inconceivable violence, the explosion in each case being perfect. It was stated that M. Rietschoten has conceived the bold proposition of cutting through a sandbank of 1500 yards breadth, off Rotterdam, and so forming a deep channel through it by sinking tubing of that length charged with 10 tons of lithofracteur. For the defence of channels and entrances to our harbours nothing can be imagined more effective. The serious objection against it seems to be that, after keeping, the folds of thin paper of the cartridge become saturated with nitro-glycerine, absorbed from the material within, so that, according to Mr. Abel, in the application of it the hands of the operators must become soiled with that poisonous substance. No doubt this will be obviated.

VI. For the better prevention of the erosion in the bores of rifled guns, various descriptions of wads have been proposed and experimented upon. The Royal Laboratory paper wads appear to be objectionable on account of pieces of them flying back in a state of ignition, which has, in the case of the Bolton's wads, been already supposed to account for a dangerous accident on board H.M.S. *Minotaur*, when a 14-lb. cartridge exploded in the hands of No. 3 of a detachment, by which two men were severely burnt and eleven others scorched and singed. A coating of Szerelemy's zopissa cement applied to Bolton's wads has also been found impracticable, as it is found not to prevent them catching fire and burning to a very dangerous extent. Col. Reilly proposes a hempen wad with tin or copper edges, similar to that used in the Prussian breech-loading rifled guns, which has been found to effectually check the rush of gas over the seat of the projectile. Capt. Noble has experimented on several natures of wads, consisting either of tin cups with short deep or split flanges, or of cowhide cups with similar flanges, or a combination of cowhide and tin cup; the conclusion arrived at being against the tin cups, in consequence of their tendency to fly about in front of the guns, whilst the cowhide cups with deep flanges appeared to answer best as a gas check. Although Major Bolton has succeeded in making wads of an uninflammable material, it is found that the scoring of the bore is not entirely prevented, and at present the question of wads in any known form remains in an unsatisfactory state.

VII. Although the traversing arrangements for naval guns are such that heavy muzzle-loading rifled guns, at close intervals, are worked with ease and rapidity between decks, still hitherto on shore batteries—especially in casemates, where there is more room than on board ship—the old luff tackle traversing gear has been adhered to; and Sir Collingwood Dickson, the Inspector-General of Artillery, considers the present land traversing gear as utterly ineffective, the guns moving so slowly that it is impossible to obtain that rapidity of fire so absolutely necessary against ships moving under steam. Mr. Cunningham's ingenious training gear, fitted outside the rear racer, has been found to interfere with the convenient service of the gun; but a new system of coupling two fore or two hind trucks together, and driving by friction, the power being communicated by means of bevelled and toothed gear, is now recommended for general adoption: this plan possesses the great advantage of having the gear wholly contained in the platform; the matter is, however, of so much importance that the various details must be well considered before patterns are sealed.

VIII. The facilitation of loading heavy guns is also a question of importance, especially in bringing up the heavy shell and placing it in the bore; consequently various alterations have been made to shot-bearers and barrows, slings, muzzle-derricks, &c. With the same view it has been decided to ease off the loading side of the grooves at the muzzle, to facilitate the entry of the projectile into the bore. This is done by cutting away the lands, and thus widening the grooves to the extent of 1 inch, tapering down to the original width of the groove in a length of 2 inches, measured parallel to the axis of the bore, the corners being well rounded off.

IX. Amongst other changes, necessitated by the introduction of pebble powder, have been the regraduation and assimilation of Land-service and Sea-service tangent sights; and advantage has been taken of this opportunity to secure, as fully as the various classes of projectiles will allow, the introduction of an uniform system of applying the information which is engraved on the sight bars; and as the slow-motion elevating screw in tangent scales for land service is abolished in sights for guns of 64-pounder calibre and upwards, the land and sea service sights for guns of these natures will be interchangeable. Take the 7-inch M. L., for instance, the following information is to be found on the hexagonal bar of its centre hind sight:—On one side is given for double shell full range in yards, next common shell full fuze, common shell full yards, Palliser shot or shell and common shell battering yards, common shell battering fuze, and double shell full fuze.

The above are, of course, only a very few of the numerous alterations which are gradually changing the character of our Artillery *matériel;* but these few desultory notes, in which our heavy ordnance manufacture and trials between targets and shot and shell are necessarily excluded, will serve to show that our factories are not at a standstill. It is reported that Mr. Scott Russell has in hand a gun on an entirely new principle, with which he is going to astonish us.

NOTICES OF BOOKS.

The Debatable Land between This World and the Next. With Illustrative Narrations. By ROBERT DALE OWEN. London: Trübner and Co.

SIXTEEN years ago the author of this book, then American Minister at Naples, spent the evening of the 25th of March at the house of the Russian Minister, Mons. K—, in the company of several visitors from different parts of the world, among whom were the Chevalier de F— (the Tuscan Minister) and his lady. Madame K— introduced the subject of automatic writing; and declared her conviction that some persons had the power of thus replying correctly to questions, the true answers to which were entirely unknown to them. It was proposed to try the experiment; and each person present accordingly took pencil and paper, and waited the result. After a few minutes one lady's hand began to move, making irregular figures on the paper. Mr. Owen proposed that questions should be asked; whereupon Madame de F— said "Who gave me these pins?" pointing to three gold-headed pins that fastened her dress; adding "If Mrs. M— can answer that I shall believe." After a short time the lady's pencil slowly wrote out—(the last two words being written backwards)—"The one that gives you a Maid and a Cook. E." Madame de F— turned pale, and cried "Magic, if there be such a thing;" and then told the company that the pins had been given her by her cousin Elizabeth, who lived at Florence, and who at her request had sent her, a few days before, a lady's maid and a cook. Mr. Owen pondered over this strange occurrence, and determined to get to the bottom of it. Mrs. M— was not a Spiritualist. Madame de F— had only been a few weeks in Naples, had not mentioned even her cousin's name to any one, and had the slightest possible acquaintance with Mrs. M—, having only just exchanged cards with her. She expressed the strongest conviction that the three or four facts, accurately stated in the few words written, could not possibly have become known out of her own family. Mr. Owen was then a complete sceptic; but this circumstance induced a course of study which has been continued for fifteen years, and which eventually changed the whole feelings and tenour of his life. He is now a confirmed Spiritualist; that is, he not only believes the phenomena to be real, but he has satisfied himself that they furnish a sufficient proof of a future existence for man. Yet, it may surprise some of our readers to hear, he is fully imbued with the spirit and teachings of modern science; and his book is one continued protest against the miraculous. He maintains that all these phenomena happen under law, just as much as do the various phenomena (many of them still inexplicable by science) presented by plants, animals, or man. He treats this question seriously and dispassionately, as the great question of the age; which he may well do, since he claims that it furnishes an experimental proof of immortality. He writes with the earnestness suited to such a theme, and with the sense of responsibility of one who, by long and patient study, has arrived at important truths of the highest value to his fellow men. Rationalism, he tells us, cannot object to this belief, that it contravenes the doctrine of law; for its phenomena occur strictly under law: nor yet that it assumes the existence, in spiritual matters, of that direct agency of God which the naturalist finds nowhere in the physical universe; for its revealings come to man mediately only: nor yet that it is dogmatic, exclusive, or intolerant, as Infallibility is; for its adherents adduce experimental evidence, open to all men, and gleaned after the inductive method, for the faith that is in them. He shows us how important it was for the welfare of man that the belief in such phenomena should die out when it did, and leave us free to develope the doctrine of law, and to overthrow the very idea of infallible or absolute truth in matters of religion. All the horrors of witchcraft, and all the persecutions of priests, arose from the dogma of infallibility; for if that dogma had been true, persecution would not have been a crime, but a duty. The world could not reach the fundamental truths of these phenomena, or understand their real import, as long as they believed in the devil and in their own infallibility. Now, they are able to investigate the phenomena calmly, and reason upon them logically; and it is a suggestive fact that a large proportion of investigators are persons untrammelled by dogmatic creeds, and fully imbued with the teachings of modern science and philosophy. Mr. Owen thinks that the belief in modern spiritualism is spreading as fast as can be wished, and even faster than can be expected, considering that almost every educated man is prejudiced against the very attempt to investigate it. He well remarks, that the growth of any new-born hypothesis so startling in character, resembles that of a human being. During its infancy its suggestions carry small weight. It is listened to with a smile, and set aside with little ceremony. Throughout its years of nonage it may be said to have no rights of property, no privilege of appropriation. Proofs in its favour may present themselves from time to time, but they are not deemed entitled to a judgment, by the rules of evidence; they are listened to as fresh and amusing, but they have no legal virtue; they obtain no official record; they are not placed to the credit of the minor. An adolescent hypothesis is held to be outside the limits of human justice.

One of the best features of the book, as a literary work, is the distinctness with which each piece of evidence is presented, and the fulness and logical force with which its teachings are discussed. This is so different from what is usual when ghost stories are narrated (the authors appearing afraid to contemplate the logical consequences of a story they yet maintain to be true) that it will be well to give a few of the cases in outline, with the author's summing up at length, in order to see what a well-educated and highly-intelligent man can say in favour of what is generally considered to be an exploded superstition. Let us first take an old but well-authenticated story. Lord Erskine related to Lady Morgan (herself a perfect sceptic) the following personal narrative. On arriving at Edinburgh one morning, after a considerable absence from Scotland, he met, in the street, his father's old butler, looking very pale and wan. He asked him what brought him to Edinburgh. The butler replied, "To meet your honour, and solicit your interference with my Lord, to recover a sum due to me, which the steward at the last settlement did not pay." Lord Erskine then told the butler to step with him into a bookseller's shop close by, but on turning round again he was not to be seen. Puzzled at this he found out the man's wife, who lived in Edinburgh, when he learnt for the first time that the butler was dead, and that he had told his wife on his death-bed that the steward had wronged him of some money, and that when Master Tom returned he would see her righted. This Lord Erskine promised to do, and shortly afterwards kept his promise. Lady Morgan then says, "Either Lord Erskine did or did not believe this strange story: if he did, what a strange aberration of intellect! if he did not, what a stranger aberration from truth! My opinion is that he *did* believe it." Probably hundreds of readers of this narrative by Lady Morgan have said with her, "What a strange aberration of intellect!" and have thought no more about the matter. Mr. Owen is not satisfied with this careless mode of getting over a difficulty. His remarks are as follows: "What sort of mode to deal with alleged facts is this? A gentleman distinguished in a profession of which the eminent members are the best judges of evidence in the world—a gentleman whom the hearer believes to be truthful—relates what, on a certain day, and in a certain place, both specified, he saw and heard. What he saw was the appearance of one, in life well known to him, who had been some months dead. What he heard, from the same source, was a statement in regard to matters of which previously he had known nothing whatever; which statement, on after enquiry, he learns to be strictly true; a statement, too, which had occupied and interested the mind of the deceased just before his decease. The natural inference from these facts, if they are admitted, is that, under certain circumstances which as yet we may be unable to define, those over whom the death change has passed, still interested in the concerns of earth, may, for a time at least, retain the power of occasional interference in these concerns; for example, in an effort to right an injustice done. But rather than admit such an inference—rather than accept disinterested evidence coming from a witness acknowledged to be sincere, and known to the world as eminently capable—a lady of the world assumes to explain it away by summarily referring the whole to the 'dog-ears and folds of early impression!' What human testimony cannot be set aside on the same vague and idle assumption? It is time we should learn that the hypothesis of spiritual intervention is entitled to a fair trial, and that, in conducting that trial, we have no right to disregard the ordinary rules of evidence. Either Lord Erskine, one morning in Edinburgh, issuing from a bookseller's shop, met what wore the appearance of an old family servant who had been some months dead—or else Lord Erskine lied. Either Lord Erskine heard words spoken, as if that appearance had spoken them, which words contained a certain allegation touching business which that servant, dying, had left unsettled—or else Lord Erskine lied. Either Lord Erskine ascertained, by immediate personal interrogation of the widow, that her husband, on his death-bed, had made the self-same allegation to her which the apparition made to Lord Erskine—or else Lord Erskine lied. Finally, either, as the result of this appearance and its speech, a debt found due to the person whose counterpart it was, was actually paid to his widow—or else Lord Erskine lied. But Lady Morgan expresses her conviction that Lord Erskine did not lie."

"In itself, the thing was a trifle. Thousands on thousands of such cases of petty injustice occur, and pass away unnoticed and unredressed. To the widow it was, undoubtedly, of serious moment; but I think no sensible man will imagine it a matter to justify the direct interference of God. If so, and if Lord Erskine spoke truth, *an apparition is a natural phenomenon.*"

How is such evidence as this refuted or explained away? Scores, and even hundreds, of equally well attested facts are on record, but no attempt is ever made to explain them. They are simply ignored, and, in many cases admitted to be inexplicable. Yet this is not quite satisfactory, as any reader of Mr. Owen's book will be inclined to admit. "Punch" once made a Yankee debtor say—

> "This debt I have repudiated long ago;
> 'Tis therefore settled. Yet this Britisher
> Keeps for repayment worriting me still!"

So our philosophers declare that they have long ago decided these ghost stories to be all delusion; *therefore* they need only be ignored; and they feel much "worrited" that fresh evidence should be adduced and fresh converts made, some of whom are so unreasonable as to ask for a new trial on the ground that the former verdict was contrary to the evidence Let us, however, consider another case, the parties to which are intimately known to our author, and whose character is vouched for as above suspicion.

A young lady, Miss V., while at her aunt's country mansion, was, owing to press of visitors, asked to occupy a room believed to be haunted. Miss V. accepted it willingly, being quite fearless. Awaking in the night, she saw in her room a woman in old-fashioned dress, who, after a little while, came towards her, and seemed to try in vain to speak. Miss V. became frightened, drew the clothes over her face, and when she looked again the figure had disappeared. She then jumped up, and found the door of her room locked on the inside. With the light of day the impression somewhat faded; she began to think she must have imagined or dreamt it, and in a short time thought no more of the ghost. Some time afterwards Miss V. met with a friend interested in spiritualism, and had with her several *séances.* At one of them an alleged spirit announced herself as Sarah Clarke, a name unknown to both ladies. A communication was then received to the effect that she had many years ago been housekeeper in Miss V.'s family, and had vainly endeavoured to communicate with the young lady while she was staying in the old mansion; that her object was to confess a crime of which she had been guilty, and to ask her old mistress's pardon for it. She had stolen some family plate, and begged Miss V. to tell her aunt, and beg for her forgiveness. Next time Miss V. visited her aunt, she ascertained that Sarah Clarke *had* been housekeeper in the family thirty or forty years before,—that some plate *had* mysteriously disappeared, but that Sarah was much trusted, and was never suspected. The aunt declared that if Sarah Clarke had taken it she freely forgave her. From that time the haunted chamber was free from all disturbance. Mr. Owen comments on this, as follows: "Knowing the standing of the parties I am able to vouch for the truth of this story. Let us consider what it discloses as to the next world. There is repentance there as here. There is restless regret and sorrow for grave sin committed while here. There is anxious desire for pardon from those whom the spirit wronged during earth-life. In other words, the natural effects of evil doing follow us to our next phase of life; and, in that phase of life as in the present, we amend, and attain to better things by virtue of repentance. . . . . Another corollary is, that when such spiritual phenomena present themselves, an endeavour to establish communication with the manifesting spirit may result in benefit, alike to a denizen of the other world and a disturbed inhabitant of this. In this way Mrs. Propert (see p. 224), getting rid of the midnight foot-falls, might have been in quiet possession of her villa at this day. I invite attention also to the strong proof of identity furnished by Miss V.'s story. The name of the housekeeper was unknown to both ladies when her (alleged) spirit gave the message. There was nothing to suggest such a name or such a confession as was made. Yet on enquiry both name and confession were found to correspond with facts that had taken place thirty or forty years ago; to say nothing of a new fact, tallying with all the rest—the cessation of the spiritual visits, as soon as the visitor had no longer any motive to show herself."

"How extraordinary," many readers will exclaim, "that a man of Mr. Owen's ability should waste his time in discussing ghost stories!" It is indeed extraordinary; for do we not know all about possible and impossible spirits? Our men of science and our philosophers are not quite sure that a spirit is possible; but *if* possible, they are all quite clear that *spirits* would never behave in the ridiculously human way in which reputed ghosts invariably act. Let us, therefore, refuse to listen to these ghost stories told by people we know nothing of, and hear what Mr. Owen has to tell us of the wonders he has himself witnessed.

He spent an immense deal of time in trying to discover that gross imposture, the spirit rap, but in vain! For this purpose he once lived for a week in a medium's house, with full power to investigate. He walked all over the house with the medium, but the raps came everywhere. They sounded on the floor, walls, or ceiling of every room, on every article of furniture, on doors and windows, on the marble mantel-piece, and the steel grate. With the same medium they occurred on board a steamer, on the stool he sat on, on the keel of a small boat in the water, on the ground out of doors, on trees, and on rocks by the sea-shore. With every test that he could apply, he could find no physical cause for these sounds. Sometimes they occurred as delicate tickings, at others like blows of a sledge hammer—so tremendous that it seemed impossible any article of furniture could resist them: yet the table on which they resounded showed not a scratch! On almost all these occasions the rooms were searched, the doors were locked, and the mediums were held fast; yet Mr. Owen could never find out the trick! How strange, when the thing is said to be so simple that our men of science will not even take the trouble to refute it!

In the matter of table-moving he had no more success. When Faraday exposed table-turning, he remarked that experimenters who thought tables even rose in the air should suspend them in a balance, and see if the weight was affected by this supposed force. Mr. Owen, at the suggestion of the late Dr. Robert Chambers, did this. Together, they suspended a table, weighing exactly 121 lbs., about 8 inches from the floor, by a powerful steelyard: two mediums were present, whose feet and hands were attended to; yet, without any contact whatever, the table, when requested, became lighter, coming down to 60 lbs., having thus lost half its weight: when requested to be made heavier it weighed 144 lbs. What are we to make of this? Two thoroughly reliable witnesses and a balance tell us one thing; but men of science say it can't be true: which are we to trust?

Continuing his researches, Mr. Owen had sittings alone with a medium. He examined the room, he locked and *sealed* the doors, and took with him privately marked slips of paper. He held the medium's hands; yet writing was somehow effected on the paper placed under the table, both in pencil and ink. Yet more; on one occasion he saw part of the writing done, by a small luminous hand on the floor, holding the pencil. On this experiment Mr. Owen remarks as follows: "Were these spiritual autographs? What else? Had I not *seen* one of them written? Had I not seen one of these slips rise higher than the table, and sink back again? Had I not felt Kate's two hands under mine at the very time when that hand wrote and that paper rose and fell? Did Kate write eight or ten lines with both her hands clasped? Did I write them with my left hand without knowing it? Or had Kate brought the slips ready written? I picked them up, and examined them critically, one by one. My private mark on one corner of each—letters of the German alphabet, written in the German character—still there! What way out? Are the senses of seeing, hearing, and touch, in sane healthy persons, unworthy to be trusted? For me, common sense bars that way out. I see nothing unlikely—not to say incredible—in the theory that God may vouchsafe to man sensible proof of his immortality. For others, to whom spiritual intercourse seems an absurdity,—for those more especially to whom the hypothesis of another life wears the aspect of a baseless dream,—let them select their own path out of the difficulty. I think that, on any path they may take, they will have to accept theories infinitely less tenable than those they decide to reject."

Mr. Owen also saw much of Mr. Foster, the medium who has names written on his hands and arms. On one occasion Mr. Foster extended his hand upon the table; it was perfectly free from any mark whatever. Gradually a faint red mark appeared on the wrist, which increased till it formed the letter F, remained visible two or three minutes, and then faded away. This was the initial letter of a name Mr. Owen had secretly written on a piece of paper, and folded up tightly, and which was mixed with about twenty others on the table. Dr. Carpenter tells us (in a letter published in "The Spiritualist" of March 15, p. 21) that this is done by first tracing the writing on the tense skin with a hard point, and then rubbing the place to bring out the red blush. But unless we are to believe that Mr. Owen and the late Dr. Robert Chambers, as well as many other careful observers who have narrated their experiences with Mr. Foster, all make grossly false or imperfect statements, this explanation by no means covers the facts; as will be admitted by all who read Mr. Owen's narrative or the evidence of Mr. E. L. Blanchard given at page 135 of the "Report of the Committee of the Dialectical Society."

Having seen so many inexplicable things himself, Mr. Owen is quite ready to believe others, when they narrate their experiences; yet he often takes an immense deal of trouble to test and confirm them, as is well shown in the marvellous story of M. Bach and the old Spinet. To be properly understood this must be read in the full detail given by Mr. Owen: in outline it is as follows: Mons. Leon Bach purchased, at an old curiosity shop in Paris, a very ancient but beautiful *spinet*, as a present to his father, who is a great-grandson of *the* Bach, and is a composer and musical amateur. The next night the elder Bach dreamt that he saw a handsome young man, dressed in old court costume, and who told him that the spinet had been given to him by his master King Henry. He then said he would play on it an air, with words composed by the King, in memory of a lady he had greatly loved: he did so, and M. Bach woke in tears, touched by the pathos of the song. He went to sleep again; and on waking in the morning was amazed to find on his bed a sheet of paper, on which was written, in very old characters, both words and music of the song he had heard in his dream. It was said to be by Henry III., and the date inscribed on the spinet was a few years earlier. M. Bach, completely puzzled, showed the music to his friends, and among them were some spiritualists, from whom he heard, for the first time, their interpretation of the phenomena. Now comes the most wonderful part of the history. M. Bach became himself a writing medium; and through his hand was written, involuntarily, a statement that inside the spinet, in a secret niche near the key-board, was a parchment, nailed to the case, containing the lines written by King Henry when he gave the instrument to his musician. The four-line stanza, which it was said would be found on the parchment, was also given, and was followed by the signature—Baldazzarini. Father and son then set to work to search for this hidden scroll; and after two hours' close examination found, in a narrow slit, a piece of old parchment about eleven inches by three, containing, in very old writing, nearly the same words which M. Bach had written, and signed—Henry. This parchment was taken to the Bibliothèque Impériale, and submitted to experienced antiquarians, and was pronounced to be an undoubtedly genuine autograph of Henry III.

This is the story; but Mr. Owen is not content with ascertaining these facts at first hand, and obtaining photographs of the spinet and the parchment, of both of which he gives good representations. He also sets himself to hunt up historical confirmation of the story, and after much research and many failures, he finds that Baltasarini was an Italian musician, who came to France in 1577, and was in great favour with Henry III.; that the King was passionately attached to Marie de Cleves, who became wife of the Prince de Condé; and that several of the allusions to her in the verses corresponded to what was known of her history. Other minute details were also found to be historically accurate.

Mr. Owen then carefully discusses the nature of the evidence, the character of the persons concerned, and the possibility of deception. M. Bach is an old man of high character; and to suppose that he, suddenly and without conceivable motive, planned and carried out a most elaborate and complicated imposture, is to suppose what is wholly incredible; but Mr. Owen shows further, that the circumstances are such that M. Bach could not have been an impostor, even had he been so inclined, and concludes by remarking: "I do not think dispassionate readers will accept such violent improbabilities. But if not, what interesting suggestions touching spirit-intercourse and spirit-identity connect themselves with this simple narrative of M. Bach's spinet!"

Recurring to Mr. Owen's own experiences, perhaps the most astounding is his account of the gradual formation of an apparition, distinctly visible to several spectators. Every precaution was taken to render trick or imposture impossible; yet if so, what marvel of modern science is equal to this? What natural phenomenon so worthy of investigation! Our author's remarks on this case will sufficiently indicate its nature. He says: "My faith in the reality of this appearance is not at all shaken by reflecting that a Signor Blitz, or a Robert Houdin, having a theatre at command, arranged with ready entrances and exits, with practical trap-doors, with dark lanterns in the wings, with the means of producing dissolving views, could probably reproduce all I witnessed. But here were a few ladies, in private life and in moderate circumstances, quietly meeting in two apartments which were daily used as school-rooms by one of their number, containing not even a recess where a chair could be hidden away. They meet to satisfy a laudable curiosity, admitting visitors now and then by courtesy only. No remuneration is demanded, nor, very surely, would any have been accepted. They meet, on this occasion, at my request, after having discontinued their researches for months, vexed with unjust suspicions. They allow us to lock every exit, after a close examination of the rooms. Here is neither motive nor opportunity—to say nothing of qualification—for deception. The coin of the realm may be counterfeited, but the coiners must have professional skill, an appropriate location, and expensive machinery. Nor do counterfeiters ply their unholy calling except with the prospect of large gains. Certain it is that I beheld the gradual formation of the figure; that I witnessed its movements; that I received from its hand an actual flower; that I saw the figure disappear. Add to this, that the place of its disappearance was illuminated by invisible agency, in answer to an unexpressed thought of mine."

We may particularly commend to the sceptical reader's attention the very full account of the bell-ringings at Major Moor's, at Greenwich Hospital, and other places, continuing for months, and baffling all attempts to find a cause for them; to the disturbances at Lydersterne Parsonage, continued for sixty years; and to many others, none of which have ever been explained.

Mr. Owen is not content to let these matters rest (with the sceptical), or contemptuously to ignore them (with the scientific); but actually imputes them to spirits, whose agency he believes is proved by other evidence, of the nature of which we have already given some examples. This evidence, taken as a whole, proves, he thinks, that there is not habitual intercourse between the two worlds; that we seem, probably, something like apparitions to those spirits who visit us; that they often seek communion, from affection or from other motives; that they have difficulties in reaching us,—difficulties wisely interposed, because, if spiritual intercourse were as common as earthly communion, we should many of us be dissatisfied with our lot, and neglect our earthly duties. "They seek from time to time to visit us. But coming from their world of spirits, invisible to ordinary sight, inaudible by ordinary speech, how are they to make their presence known? How are they to attract our attention? In what manner does a traveller, arriving under cloud of night before a fast-closed mansion, seek to reach the in-dwellers—seek to announce his presence? Is it not by KNOCKING OR RINGING?" This is our author's reply to sneers at "rapping" and "bell-ringing" phenomena.

We have devoted so much space to a sketch of Mr. Owen's book, because, in the first place, it merits notice as a literary work of a high class; and in the second, it brings prominently before us what is either the most gigantic and mysterious of delusions or the most important of truths. In either case it deserves a full and fair discussion. Neither is such a subject out of place in a scientific journal, for, in whatever light we view it, it is really a scientific question. If a fallacy or a delusion, it is of so wide-spread a nature, and influences such numbers of well-educated and even scientific men, that we have a right to demand of science a full and satisfactory exposure of it. If a truth, then it is certainly, as Mr. Owen maintains, a science of itself; a new science, and one of the most overwhelming importance in its bearings upon philosophy, history, and religion. It is now becoming almost a common thing to acknowledge that there is a certain amount of truth in the facts; with a proviso, always, of the writer's repudiation of the spiritual theory. For my own part, the only thing that makes the facts credible on evidence *is* the spiritual theory. Mr. A., or Prof. B., or Dr. C., may state that *they know* certain of the facts are true, but that all these facts can be explained without calling in the aid of spirits. Perhaps they can. But why should I, or any other reader, accept A., B., or C.'s facts, and reject Mr. Owen's, when the former are not one whit more intrinsically probable, or supported by one iota better testimony, than the latter? Yet these latter actually *force* upon us the spiritual theory, just as the facts of Geology *force* upon us the belief in long series of ancient living forms, different from those now upon the earth. I must accept all the equally well-attested facts, of equal intrinsic probability, or reject all. I cannot believe in Cretaceous fossils as realities, and reject Silurian as freaks of nature; neither can I accept the facts B. may have witnessed, and reject those of the rest of the alphabet. Yet if all the main classes of facts are admitted, the spiritual theory appears as clearly a deduction from them as the theory of extinct animals follows from the facts presented by their fossil remains. The position of the Quarterly Reviewer is, that there are no facts worth speaking of, and, therefore, no true spiritual theory can be founded on them. This is safe ground, as long as all the evidence for the facts is carefully denied, misrepresented, or ignored. But when there are ten thousand witnesses to these facts, of whom say nine thousand are as good and competent as A. or B., it is not safe ground for A. or B. to admit just so much of the facts as they have witnessed themselves, and reject the rest. The problem we have now to solve is—how much of the facts are true. Till this is done by some better test than individual experience, it is premature to discuss what theories may or may not explain them. In the meantime, let no one prejudge the question till they have studied Mr. Owen's facts and carefully weighed his arguments.

ALFRED R. WALLACE.

Spectrum Analysis in its Application to Terrestrial Substances and the Physical Constitution of the Heavenly Bodies. Familiarly explained by Dr. H. SCHELLEN. Translated from the Second Edition by JANE and CAROLINE LASSELL. Edited, with Notes, by W. HUGGINS, LL.D., D.C.L., F.R.S.

THE first edition of this work, though a valuable contribution to the literature of Spectroscopy, was not free from defects, and, although published so late as 1870, it failed to give as complete an account as could be wished of some of the more remarkable applications of spectroscopic analysis. The present edition (we refer for the moment to the German editions) has been carefully revised, and several of the sections have been enlarged and enriched, more particularly those which relate to observations of the sun. The work thus forms a sufficiently complete and tolerably exact synopsis, as well of the phenomena of spectrum analysis as of the methods by which they have been applied to the interpretation of some of the most recondite problems of nature.

Such a work well merited translation into English, and those students of spectroscopic analysis in England and America who do not read German owe thanks to the Misses Lassell for undertaking the labour of translation, and to Dr. Huggins for editing and annotating this excellent treatise.

The work is divided into three parts. The first deals with the artificial sources of high degrees of heat and light; the second, with spectrum analysis in its application to terrestrial substances; and the third, with the application of the analysis to the heavenly bodies.

We need say little about the first part of the work. It is obvious that the book would have been incomplete without: an account of the chief artificial sources of light and heat; and the fifty pages devoted to the description of the Bunsen burner, the magnesium light, the oxyhydrogen flame, the lime-light, and the electric light, cannot be regarded as in excess of the requirements of the practical student of spectroscopic analysis.

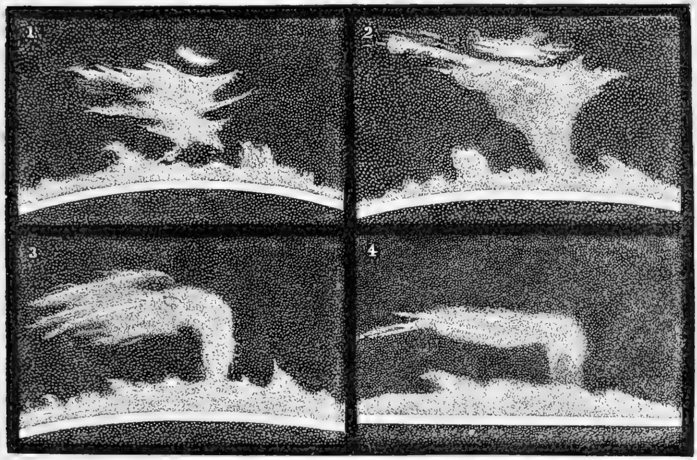
With the second part we cannot express ourselves wholly satisfied. The explanation of the fundamental principles of prismatic analysis is insufficient, and even in part unsound. Indeed we cannot but notice that where Dr. Schellen approaches a subject involving mathematical considerations, his conceptions are wanting in clearness and exactness. His explanation of the "indivisibility of the pure colours of the spectrum" may be cited as an instance, and in some respects a very remarkable instance. The subject matter is obsolete, to begin with, since it relates in reality only to Newton's recognition of the fact that light from a given part of the spectrum cannot be resolved into light of other colours,—a fact of no importance in the present position of the science. But Dr. Schellen, in attempting to explain this, falls into the mistake of asserting what is untrue, though it would be unquestionably most important if true. He says, "If a small round hole be made in the screen, in any portion of the image of the spectrum,—the extreme red, for instance,*—a red ray passes through it, and appears on the opposite wall as a round spot of red light. If a second prism be interposed in the path of the ray that has passed through the screen, the ray will suffer a second refraction, and the image be thrown upon another place on the wall: this new image, however, is simply red like the incident ray, *and by a careful adjustment of the prism shows no elongation, but appears perfectly round.*" This, of course, is utterly erroneous; were it true, our spectroscopists would gain nothing by employing more than a single prism in any of their researches. In extending the reasoning, Dr. Schellen introduces a figure representing the action of a second prism on the central colour of the spectrum; but in *this* figure the second prism is so placed as to reverse the action of the first, the contrary being the case in the figure illustrating the former part of his reasoning. In the case illustrated by the second figure a round spot of light would naturally be obtained, the second prism restoring to parallelism the rays which the first prism had caused to diverge.

But although in dealing with theoretical considerations Schellen is thus not unfrequently inexact, the description of the practical

* The reference in the text is to Fig. 28. It should be Fig. 32, as will appear by a reference to the first German edition.

details of spectroscopic analysis is in nearly every instance trustworthy; while in the few instances where there is room for questioning Dr. Schellen's accuracy, the editor supplies, in a foot-note, the necessary emendation. The latter portion of Part II., in which the various orders of spectra are described, and the application of the analysis to the investigation of terrestrial substances is considered, becomes thus exceptionally valuable; for to Schellen's own familiarity with the subject, and his careful study of the labours of Bunsen, Kirchhoff, Thalén, and other continental spectroscopists, is superadded Dr. Huggins's independent mastery of this department of the new science. And we cannot but notice here how little acquaintance with the history of spectroscopic analysis is displayed by those who suppose that Dr. Huggins's application of the analysis to the heavenly bodies includes the whole of his labours as a spectroscopist. We would invite those who entertain this opinion to the study of

FIG. 17.

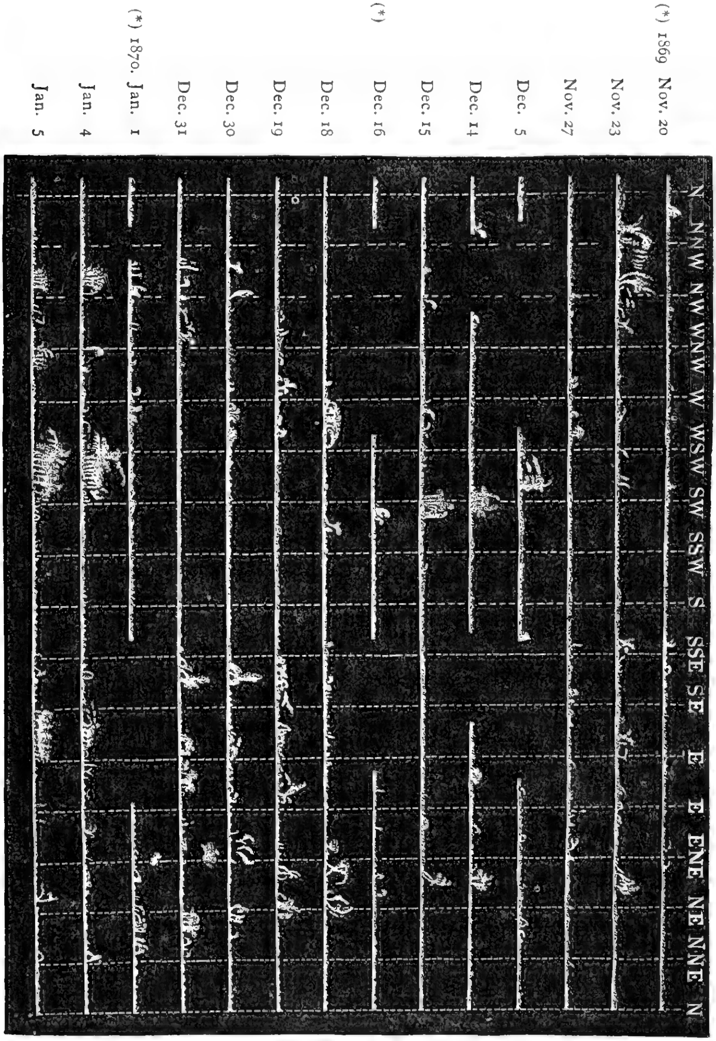


Changes in the form of a Prominence.

Dr. Watts's useful treatise, the "Index of Spectra," noting that all the work by Huggins recorded and tabulated in that work preceded those astronomical labours which have been regarded as justly entitling him to be called the Herschel of the Spectroscope.

Before passing to the consideration of the last part of Schellen's work, we would invite attention to the extreme interest of those spectroscopic researches whose difficulty depends on the minuteness of the quantity of matter placed under analysis. If our wonder is excited when we see a Huggins or Secchi obtaining, from beyond the immeasurable distances which separate us from the stars, information respecting the structure of those orbs, we have equal reason to contemplate with interest the re-

Fig. 18.



Respighi's observations of the Prominences round the entire limb of the Sun.

cognition of the existence of the 10,000th part of a grain of some element in solution, or of yet minuter portions, by the use of the induction spark,* or the discovery, by this potent means of analysis, of elements which had previously escaped recognition.

The third part of the work occupies two-thirds of the volume. The sections treating of the solar spectrum and its lines are well written, and being illustrated by reduced copies of Kirchhoff's maps and scale, and of Angström's and Thalén's maps with wave-length scales, are of great value to the student of practical spectroscopy. A sufficient account is also given of the atmospheric lines which make their appearance in the solar spectrum when the sun is near the horizon.

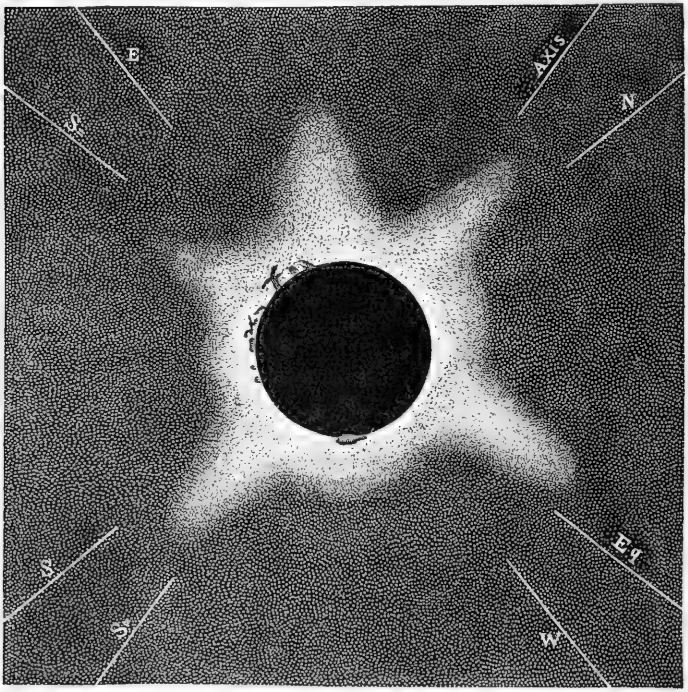
Schellen passes thence to the consideration of the solar spots, and, having remarked that it would lead him too far from his subject to dwell on their phenomena, naturally proceeds to supply about a score of pages of descriptive matter, for which the reader might very well have been referred to astronomical treatises. The discussion of the spectra of spots and faculæ, which follows, is full and satisfactory, save that Schellen places rather more reliance on Secchi's observations, and especially on his asserted recognition of the presence of aqueous vapour in the solar spots, than we believe to be justified by the evidence. Secchi, on one occasion, could recognise the bands of aqueous vapour, due to the passage of a cloud over the sun, more clearly in the vicinity of spots than elsewhere on the solar disc, and therefore somewhat hastily concluded "that the absorptive power in the sun producing these bands is intensified by the absorptive action of the aqueous vapour contained in the earth's atmosphere; and that the cause of the absorption in the sun in the neighbourhood of the solar spots is therefore the same as that which is present in a fog, namely, aqueous vapour." It does not seem to have occurred to him that the general absorption in the spot spectrum would render the atmospheric bands apparently more distinct in that spectrum, without any increase in the real darkness of those particular bands. The editor remarks, very justly, that Secchi's result appears to him to need confirmation; and we could even have wished that he had more pointedly indicated the fallacy in Secchi's reasoning.

The phenomena of solar eclipses are treated very fully, and in a very interesting manner. The sections relating to this part of the subject are also abundantly illustrated. We speak of the phenomena of eclipses as including the prominences and sierra, though in reality the most valuable observations of the last-named phenomena have been made when the sun has not been eclipsed. In dealing with the history of the method by which this has been accomplished, Dr. Schellen falls into some mistakes which are corrected by the editor. In particular, it is to be noticed that Schellen fails to ascribe the open slit method, by which the actual

* In this way the 100,000,000th of a milligramme of strontium, or the 80,000,000th of a grain of Crookes's new element, thallium, can be made to reveal its presence.

figures of the prominences can be studied whenever the sun is visible, to Zöllner and Huggins, who independently enunciated the method—Huggins accompanying his paper with an account of the first successful observation of a prominence by this method. The coloured illustrations of this part of the subject are very beautiful, and add largely to the value of the treatise. They include views by Zöllner, Respighi, and Young, the coloured views by the two last-named observers being now published in

Fig. 19.

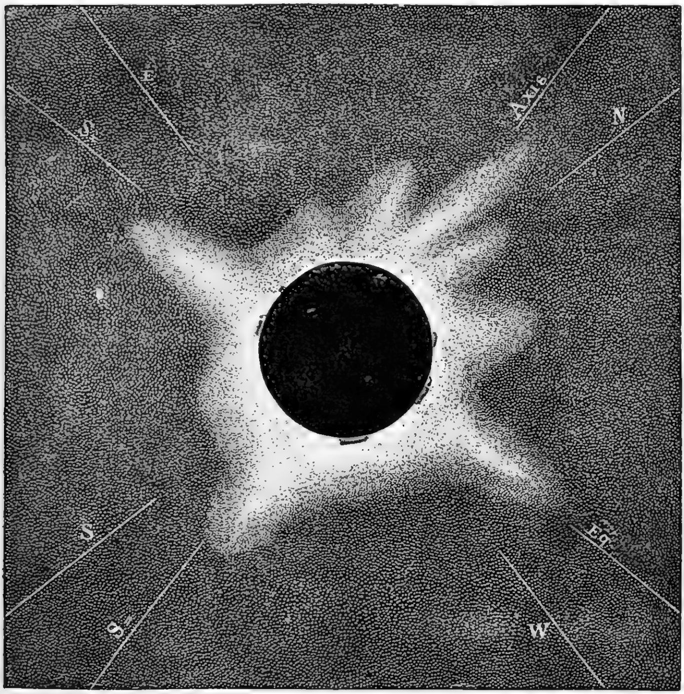


Gould's Drawing of the Corona, August 7, 1869. (4h. 58m.)

England for the first time we believe. The accompanying figures (17 and 18), though instructive, afford but a faint idea of the weird beauty of the enlarged coloured drawings of particular prominences.

In dealing with the corona (a subject which he somewhat unhappily intermixes with the treatment of the prominences) Schellen merely sums up without submitting to analysis the evidence which had reached him. Accordingly he arrives at no definite conclusion on the question whether the corona is a solar or terrestrial phenomenon. The extract given by the editor from the Report of the Council of the Astronomical Society appears to us, we must admit, to be scarcely more satisfactory, since it speaks of theories as admissible which could not at the time be possibly entertained in the presence of evidence already available. Of course after the results of the recent eclipse all the doubts thus expressed must be regarded as definitely disposed of; the reality of the corona, even to its utmost visible limits, as

FIG. 20.



Gould's Drawing of the Corona, August 7, 1869. (5h.)

a solar appendage of wonderful extent and complexity, being now admitted by all. But it is worthy of notice in this place how feeble had been the evidence on account of which the true theory of the corona was so long disputed. The difficulty of delineating an object so delicate, and the consequent differences between pictures taken by different observers at different places, and under different atmospheric conditions,—changes supposed to have been recognised by one and the same observer,—and imagined differences between photographic pictures really resembling each other very closely,—such was the evidence urged against the long array of facts pointing the other way.

As respects the second point, we may illustrate the nature of the evidence, by showing the two drawings, Figs. 19 and 20, by Dr. Gould on which chief stress was laid by the advocates of the atmospheric glare theory. It will scarcely be believed that the differences between these drawings (differences which are so readily explained by the changes of condition to which the air was subjected *through which* a solar corona must necessarily be seen, as well as by the changing power of the eye to appreciate the outer faint coronal light) were urged as demonstrative of the fact that the corona is a phenomenon of our own atmosphere!

As respects the third point above referred to, it is necessary to point out that the argument relating to the photographs by Lord Lindsay and Mr. Brothers (pp. 372, 373 of the present work) is vitiated by the circumstance that in some unexplained way Lord Lindsay's photograph was inverted. The perfect agreement between the prominences shown in all the photographs when Lord Lindsay's picture is re-inverted abundantly proves this, and thus the supposed discrepancy between a photograph taken at the beginning and one taken at the end of totality has no real existence.

The sections of the work relating to the fixed stars, nebulæ, and comets, are extremely interesting, and may be accepted as fairly presenting the present position of the subject,—with one exception only, that undue prominence is given to Father Secchi's study of star spectra. Of the section on meteors we find ourselves unable to speak altogether with approval. Dr. Schellen does not sufficiently distinguish between the points which have been demonstrated and those which remain matters of speculation. Nor is the history of the subject quite accurately rendered.

However, such blemishes as these do not importantly detract from the value of Schellen's excellent treatise. It could, indeed, scarcely be expected that so large a volume should be free from some defects; and, on the whole, the work before us is as sound and trustworthy as it is wide in range and exhaustive in treatment. It is a treatise without which no scientific library can be regarded as complete. The work of translation has been accomplished carefully, and all things considered, satisfactorily. Here and there occur passages which appear to us somewhat cumbersome, a fault chiefly due, we believe, to the anxiety of the translators to follow their author as closely as possible. But such passages are few and far between; and, apart from its scientific value, the book is very readable. The editorial notes greatly enhance its value.

Elementary Treatise on Natural Philosophy. By A. PRIVAT DESCHANEL, Inspector of the Academy of Paris. Translated and Edited by J. D. EVERETT, M.A., D.C.L., &c., Professor of Natural Philosophy in Queen's College, Belfast. In four Parts: Part III. Electricity and Magnetism. London: Blackie and Son. 1872.

THIS part of the translation of M. Deschanel's "Traité Elementaire de Physique" may be considered as the masterpiece of

FIG. 21.

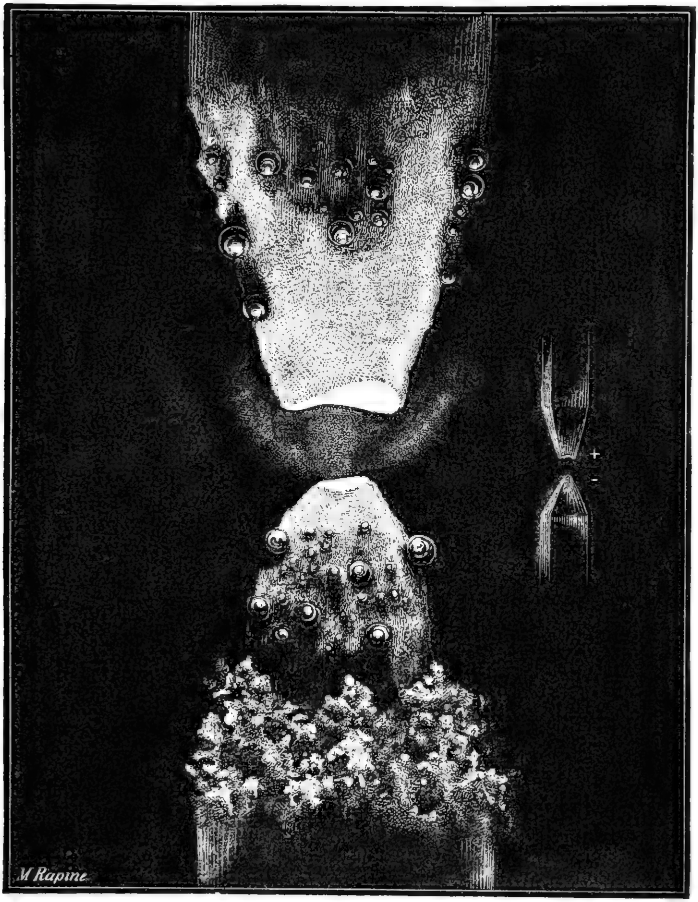


Image of the Carbon Points.

Professor Everett's series of three volumes now before the public. French writers appear to be but very slightly acquainted with the researches of Faraday in electro-magnetism, and of Sir William Thomson in general electric principles. There has therefore been much to add, and Dr. Everett is to be congratulated on his happy method of collating the experiments necessary to an English work. The description of the various forms of electrometers, for instance, leaves nothing to be desired.

We must again commend the illustrations, and express our indebtedness to Messrs. Blackie and Sons for being able to lay before our readers a specimen of the exquisite woodcuts. The one selected exhibits the image of the carbon points of the electric lamp as viewed through a lens, the natural size of the carbons being indicated by the sketch at the right hand. The book is well bound in limp cloth covers, a style of binding every student will appreciate.

Experimental Mechanics. By ROBERT STAWELL BALL, A.M, Professor of Applied Mathematics and Mechanism in the Royal College of Science for Ireland (Science and Art Department). London and New York: Macmillan and Co. 1871.

THIS volume contains the substance of a series of twenty evening lectures upon "Experimental Mechanics" delivered by the Author at the Royal College of Science for Ireland. The aim has been to create in the mind of the student physical ideas corresponding to theoretical laws, and thus to produce a work which may be regarded either as a supplement or an introduction to manuals of theoretical mechanics. We cannot praise the attempt too highly, nor congratulate Professor Ball too sincerely upon his success. The laws of mechanics are essentially simple, and in this work they meet with an appropriate exposition. No complicated formulæ are introduced into the calculations, numerical or geometrical illustration being chiefly employed. The method of treatment of the laws relating to friction, to the mechanical powers, to the strength of timber and structures, to the pendulum and to the laws of motion, is original and extremely explicit. But it must not be misunderstood that the work is too simple for the advanced student,—far from it. Mr. Ball has well distinguished between the class of students who know mathematics and those who do not. Therefore we can safely recommend the book to the notice of all.

PROGRESS IN SCIENCE.

MINING.

One of the most important branches of enquiry undertaken by the Royal Coal Commission related to the statistics of the production, consumption, and export, of our mineral fuel. The labour of this enquiry—which is represented by the third volume of the Report, consisting of 500 pages—devolved almost exclusively upon Mr. Robert Hunt, F.R.S., the share taken by the late Sir R. Murchison being merely nominal. In consequence of this additional work, followed as it unhappily was by a return of illness, the Keeper of Mining Records was unable to issue his annual volume of "Mineral Statistics" last year. During the past quarter, however, the official returns have been published,* and we hence learn the amount and value of the minerals raised in the United Kingdom in 1870. According to our usual custom, we here present a general summary of these statistics:—

	Tons.	£
Coal	110,431,192	27,607,798
Iron ore	14,370,654	4,951,220
Copper ore	106,698	437,851
Tin ore	15,234	1,002,357
Lead ore	98,176	1,200,209
Zinc ore	13,586	41,058
Iron pyrites (sulphur ore) ..	58,428	36,026
Arsenic †	4,050	17,739
Gossans, ochres, &c.	4,844	4,261
Wolfram and tungstate of soda	51	653
Manganese	4,838	19,499
Nickel	½	27
Barytes	6,515	3,771
Clays, fine and fire (estimated)	1,200,000	450,000
Earthy minerals, various (do.)	—	575,000
Salt	1,489,450	744,725
Coprolites (estimated) ..	35,000	50,000
Total value		£37,142,194

In discussing these figures, Mr. Hunt calls attention to the increase in the production of coal, the produce of 1870 having exceeded that of the previous year by upwards of *three millions* of tons. The great activity in the iron trade caused the consumption of a large proportion of this additional coal, but at the same time our exports were increased.

With the statistics of our own coal production it is interesting to compare that of the United States. From the American official returns for 1870 we learn that during that year the total production of coal of all kinds amounted to 34,600,461 tons. Of this quantity 15,368,437 tons consisted of anthracite and semi-anthracite which had been sent to market, and 3,842,876 tons of similar fuels retained for home consumption. The bituminous coal sold amounted to only 4,589,148 tons, and even out of this quantity 420,683 tons had been imported; but, in addition to what found its way into the market, it is estimated that 10,800,000 tons of bituminous coal were mined and consumed in the United States, without being included in the tabular returns. The statistics further show that, in the great coal-producing counties of

* Mineral Statistics of the United Kingdom of Great Britain and Ireland, for the year 1870. With an Appendix. By Robert Hunt, F.R.S., Keeper of Mining Records. London: Longmans, and Stanford. 1872.

† Nearly 2000 tons of this arsenic will have been obtained from the burning-houses on the tin mines. A considerable quantity is produced at Swansea, of which no return has been obtainable.

Schuylkill, Northumberland, Columbia, and Dauphin, 129 fatal accidents occurred during the year, but it is notable that only *six* of these deaths resulted from explosions of fire-damp.

At a recent meeting of the Wigan Field Naturalists' Society, Mr. Perrins read a paper on the probable duration of the important coal-field of Wigan. Coming from one of the leading mining surveyors of the town, Mr. Perrins's estimate deserves attention. For the purpose of his enquiry he considers that the coal-field occupies an area embraced within a radius of two miles from the parish church. In this district there are about twelve workable coal-seams, and the author describes the geological features of each of these in succession. He divides the field into a number of belts, each of which is separately examined, and its yield of coal estimated. We pass over these details, however, and arrive at once at the aggregate result. The whole of the Wigan coal-field originally contained an amount of mineral computed at 231,810,000 tons. Of this quantity 96,285,000 tons have already been removed, thus leaving 135,525,000 tons still underground. At the present rate of working about 2,000,000 tons are removed annually; and assuming this rate to continue, without increase or diminution, the Wigan coal-field will be exhausted in little more than fifty years. Seeing that these collieries employ, directly and indirectly, about 7000 men, the gradual diminution of the coal must have a serious social influence upon the locality. It is cheering, however, to remember that the iron trade around Wigan has of late received a great impetus, and it is therefore to be hoped that the development of this industry may compensate for the decline of the coal trade.

A successful trial of Mr. G. L. Scott's recently-patented steam-ventilator has been made at the Lower Moor Colliery, at Oldham, where a 4-feet seam of coal, faulted in all directions, is worked by intricate galleries extending for a distance of a mile from the shaft, the shaft being 888 feet in depth. A brick chamber, in communication with the upcast shaft, is covered by perforated iron plates carrying 72 vertical cast-iron pipes, each about 5 feet in length and 7 inches in internal diameter. Inside each of these pipes is a small vertical steam pipe, which terminates by an orifice, $\frac{3}{16}$ths of an inch in diameter, at about 2 feet below the top of the larger pipe. The system of pipes forms a rectangular arrangement occupying a space about 10 feet square, and is supplied with steam by a 4-inch pipe from the boilers of the winding and pumping engine. The results obtained by the use of this simple steam-ventilator compare favourably with those of the old furnace system, but it was not considered necessary, at the trial in question, to test the full power of the new apparatus. It is almost needless, however, to state that any ventilator which—like this steam apparatus—can be placed at the surface, so as to be under complete control, must possess great advantages over the ordinary system of ventilation, in which the apparatus is lodged in the pit itself.

To prevent accidents from over-winding in mine-shafts, an ingenious apparatus has been patented by Mr. W. Walker. The load to be raised is carried by chains or ropes attached to the lower ends of two curved metal levers, hinged together in the middle, and connected above with the winding-rope or chain. These levers are so constructed that when the load is being raised their lower ends diverge, whilst the upper extremities remain closed together, and hold the winding-rope between them. On the other hand, if the levers turn upon their common hinge, the lower parts are brought together, and the upper ends open out so as to release the rope. At that point which forms a limit to the raising of the load, and beyond which it would be dangerous to wind, there is a fixed beam, with an aperture sufficiently large to admit the winding-rope and the head of the safety apparatus. In the event of over-winding, the upper parts of the levers pass through this aperture, but the rest of the apparatus is arrested by a guard-plate, or collar, which is placed over the levers, and comes in contact with the under side of the beam. As the load now tends to rise, the distended ends of the levers are brought together by pressure against the sides of a slot in the collar, and as the lower ends of the levers come together, the upper parts forming the head fall apart, and rest upon the top of the beam so as to support the load, whilst the released rope is carried upwards by the continued winding.

Through the Geological Society of London, Mr. G. Milner Stephen, of Sydney, announces the recent discovery of a very valuable deposit of tin ore in New South Wales. The ore occurs in crystals and rolled fragments, in beds of conglomerate and in granite. It appears that the deposit extends over an area of several miles in the district of New England.

From the same authority we learn that a party of four men, induced by the offer of a reward by the French Government, have discovered gold on the banks of the River Bondé, on the N.E. coast of New Caledonia. Hitherto only auriferous drift has been found, but it is expected that quartz reefs will soon be discovered.

In a paper "On Gossans," by Mr. W. Argall, of Great Huel Vor, recently read before the Miners' Association of Cornwall and Devon, the author suggested that a collection of these products might advantageously be preserved for reference in some central museum, the collection being accompanied by analyses of the specimens and records of the conditions under which they occurred. Remembering the great value set by miners upon the occurrence of gossan as superficial indications of mineral deposits beneath, the suggestion seems to be worthy of notice. Unfortunately, however, these gossans are rather unattractive to the eye, consisting as they generally do of ochreous oxide of iron mixed with cellular quartz; the result, in fact, of the alteration of the upper part of the ore by atmospheric and other influences. Some of the richest deposits, especially copper lodes, bear masses of this unsightly gossan on the outcrop, or "back," of the vein; and Continental miners, not less than Cornishmen, attach great importance to the occurrence of this *Chapeau de Fer*, or *Eiserne Hut*, as likely to betoken the existence of a productive vein of ore. A collection of gossans, chiefly Cornish, has for many years past been exhibited in the Museum of Practical Geology.

With the opening of the new year there appeared a new monthly magazine devoted to the mining interest of this country. Under the editorship of Mr. Nelson Boyd, F.G.S., the "Mining Magazine and Review" promises to become a technical journal of much value to those engaged in mining, quarrying, metallurgy, and kindred pursuits. The three numbers already issued contain many interesting articles bearing upon these branches of industry.

METALLURGY.

Our chronicles last quarter contained a brief account of Danks's rotary furnace for puddling iron by machinery. Soon after the publication of that number, Messrs. Jones, Snelus, and Lester, the three Commissioners appointed by the Puddling Committee of the Iron and Steel Institute, having returned from their visit to the United States, issued their Report. This Report has been published in the Journal of the Institute, and contains an elaborate description of the furnace, details of the mode of fettling, the consumption of fuel, and the quality of the iron produced. The visit of the Commissioners has convinced them that the process can be successfully carried out, and that even in its present state it offers commercial advantages over hand-puddling. At the same time they believe that the process is only yet in its infancy, and is capable of rapid development. Materials were taken out from this country to be experimented upon in the presence of the Commissioners, and samples of the fettling materials and of the iron produced in the furnace have been brought home. It has been arranged with the inventor that the royalty to be paid in Great Britain shall not exceed two shillings per ton on machine-made puddled iron.

We learn from the "Mining Journal" that an improved furnace for puddling iron by machinery, the invention of Mr. A. Spencer, has recently been at work, with the most encouraging results, at the West Hartlepool Iron Works. Spencer's revolving furnace is a box-like vessel, of square or rhomboidal transverse section, furnished with circular ends having apertures through which a communication is established with the fire-grate on the one hand and the chimney on the other. The sides are built up of a number of flanged plates, bolted together into a polygonal form, and cast with honeycombed or cellular sides so as to form cavities for receiving and securely holding the fettling with which the converter is lined. This fettling consists chiefly of mill-furnace cinder, or ball-furnace tap; and having been melted in a cupola furnace, is either run directly into the converter or moulded into masses, which when cold fit the cavities in the sides. When the molten charge of cast-iron has been introduced into the converter, the charging-holes are closed, and the vessel is caused to revolve by means of large spur-wheels attached to the end-plates: the liquid metal becomes rapidly agitated against the flat sides, and in about half an hour the balls of wrought-iron may be removed, at least 6 cwts. of metal being turned out at each heat.

Some improvements in mechanical puddling-furnaces have also been lately suggested by Mr. W. Ferrie, of the Monklands Iron Works. He causes the movable bed or hearth of the furnace to revolve by fixing to it a cylindrical flange which projects downwards into a circular gutter filled with water, sand, or ground fire-brick, by which means a connection is sealed between the fixed and the moving parts of the furnace, without interfering with the freedom of motion.

An elaborate essay on the Metallurgy of Silver in Mexico, by M. P. Laur, has been published in two recent numbers of the "Annales des Mines." After noticing the mode of occurrence of the ores, the author describes in detail the several metallurgical processes used at the different works,—namely, amalgamation at ordinary temperatures by the *patio* process; amalgamation in the *cazo*, or vessel containing a boiling solution of common salt; and the old Saxon process of amalgamation with iron in rotating barrels. M. Laur raises many objections to the generally-received theory of amalgamation by the *patio* process, and advances an original view of the reactions which occur in the *patio*. It may be well to state that the *torta*, or heap of materials used in the reduction of the silver ores by this method, consists essentially of a mixture of common salt, "magistral" or roasted copper pyrites, mercury, and silver ores usually in the state of sulphides. According to most metallurgical chemists, the chloride of sodium and the sulphate of copper in the magistral react upon each other, and thus produce sulphate of soda and chloride of copper: this chloride then converts the sulphide of silver into a chloride, which dissolves in the salt water present in the *torta;* the mercury then rapidly attacks the solution of chloride of silver, reducing the metal and becoming in turn converted into calomel, whilst the free silver forms, with some of the unaltered mercury, an amalgam, which is retorted, and the mercury distilled off while the silver remains behind. Modifications of this theory have been proposed, but the essential points remain—that the silver is formed into a chloride, and this is decomposed by the mercury. M. Laur's Mexican experience leads him to believe, on the contrary, that the sulphide of silver is reduced *directly* to the metallic state, and does not pass through the intermediate condition of a chloride. By the reaction of the magistral with the salt, sulphate of soda and protochloride of copper are formed:—

$$NaCl + CuO.SO_3 = NaO.SO_3 + CuCl.$$

This chloride is then reduced to the state of a sub-salt, which combines with the sodium chloride to form a double salt—

$$2CuCl + NaCl + Hg = NaCl.Cu_2Cl + HgCl.$$

The double salt reacts directly upon the silver sulphide, and effects its reduction:—

$$NaCl.Cu_2Cl + AgS = NaCl.CuCl + CuS + Ag.$$

The protochloride of copper passes again to the state of subchloride, while the sulphide of copper is oxidised into sulphate, and the cycle of change thus commences afresh.

Messrs. Hargreaves and Robinson, of Widnes, have patented certain improvements in the method of treating metallic sulphides, especially pyrites, by which they are made to yield the greatest amount of sulphur, and are then converted into chlorides. The pyrites is first roasted in a series of tall vertical retorts, so arranged that each becomes in turn the first, intermediate, and last vessel of the set, with respect to the passage of air or gas through the series. A current of chlorine or of hydrochloric acid gas is passed through the spent pyrites contained in these retorts and maintained at a high temperature.

Other improvements in roasting pyrites have been patented by Messrs. Perret Brothers and Olivier, of Paris. The heat generated in the furnace in which the crude pyrites is roasted is conducted into another furnace, where it is utilised in further roasting the ore, so as to drive off the sulphur more completely. The operations may be conducted in a single furnace, built with several floors, through which the heat circulates.

From Mr. Hunt's volume of "Mineral Statistics" for 1870 we are able to extract the following table, showing the quantities and value of the metals obtained from the ores raised in the United Kingdom during that year:—

		£
Pig-iron	5,963,515 tons.	14,908,787
Tin..	10,200 ,,	1,299,505
Copper	7,175 ,,	551,309
Lead	73,420 ,,	1,452,715
Zinc	3,936 ,,	74,096
Silver	784,562 ozs.	196,140
Gold	191 ,,	750
Other metals (estimated) ..	—	3,500
		£18,486,802

MINERALOGY.

Among a number of "Mineralogical Communications" recently made by Prof. Vom Rath to "Poggendorff's Annalen," perhaps the most interesting are those which relate to the constitution of the felspars. Several lime-soda felspars have been carefully examined, including specimens of andesine from Vesuvius, Frejus, and Predazzo; of oligoclase from Nieder-mendig and Veltlin; and of labradorite from Iceland. These studies confirm Tschermak's views regarding the composition of the lime-soda series, and tend to show that these so-called species may all be represented as isomorphous mixtures of albite, or pure soda felspar, and anorthite, or pure lime felspar. Hence we can never expect to find an oligoclase quite free from lime, or a labradorite free from soda. It has indeed been supposed that the mineral called *Ersbyite* represents a labradorite without soda, but Vom Rath's investigations show that this species cannot be referred to labradorite, but should rather be placed with scapolite.

In a former number of this Journal allusion was made to the discovery of crystals of *boracite* in the salt-mines of Stassfurt, in Prussian Saxony. These crystals have since been described by Dr. Schultze, from whom we learn that they were found among the residual matters obtained from the treatment of salts which had been raised from the upper beds of these great deposits, and had been lixiviated in the chloride of potassium works at Stassfurt. The author offers some theoretical views on the probable origin of this boracite, and the formation of certain nodules containing alternating layers of boracite and other minerals. The double sulphate of soda and magnesia, termed *Simonyite*, has been found in the Anhalt portion of these salt-mines. Simonyite contains $NaO.SO_3+MgO.SO_3+4HO$; and the sulphate of soda present in the salt may render it of economic value in the manufacture of carbonate of soda.

Julianite is the name proposed by Prof. Websky for a new species somewhat resembling fahlerz, but possessing isomorphous relations with buntkupfererz. It occurs in small groups of cubic crystals, presenting a dark grey colour, and containing $As_2Cu_3S_6$, part of the arsenic being replaced by antimony and iron, and part of the copper by silver. The ore was formerly found in the Friederike-Juliane Mine, at Rudelstadt, in Silesia.

Under the name of *Beyrichite*, Prof. Liebe describes a new species from the Westerwald. It is found in groups of macled prisms, of a lead-gray colour, with a faint metallic lustre. Its composition may be formulated as $3NiS.2NiS_2$; but the mineral contains a variable proportion of iron, which, in this calculation, is supposed to replace the nickel: if the iron, however, be due to the presence of iron pyrites, the formula of Beyrichite becomes $2NiS.NiS_2$.

A native silicate, hitherto undescribed, has received from Herr Frenzel the name of *Bismuthoferrite*. It occurs, with a variety of hypochlorite, at Schneeberg, in Saxony, and contains $Bi_2O_3.2Fe_2O_3.4SiO_2$.

Trögerite and *Walpurgine* are two new species, of which preliminary notices have been lately published by Weisbach, of Freiberg. Both are arseniates of uranium from Schneeberg, but the composition of the former may be represented as $5Ur_2O_3.2AsO_5+20HO$, whilst that of the latter coincides with the following formula:—$(5Bi_2O_3.3Ur_2O_3)2AsO_5+10HO$.

Some little time back a new fluo-phosphate of alumina was described by MM. Des Cloizeaux and Moissenet. As the mineral came from the tin-mines of Montebras, in Central France, it received the distinctive name of *Montebrasite*. The substance occurs abundantly in parts of the gangue of these veins, and is accompanied by fluor-spar, apatite, chalcolite, wavellite, and turquois. Its analysis led to the following formula:—$2(Al_2F_3.3MF)+4Al_2O_3.3PO_5$. M. Pisani, not satisfied with its determination as a distinct species, has lately examined the mineral afresh, and from his analysis declares it to be identical with the mineral known as Amblygonite. It appears, then, that the mineral of Montebras does not possess characters sufficiently diagnostic to entitle it to take rank as a distinct species; but, nevertheless, the discovery of so rare a mineral as amblygonite in large quantity in these mines is a matter of much mineralogical interest.

Dr. Streng announces the discovery of *Tridymite* (Vom Rath's species of silica) in an orthoclase porphyry near Waldbökelheim, where it occurs in crystals of similar form to those found in trachytic rocks.

Some zeolitic minerals from the dolerites of the Bergonne limestone have been described by M. Gonnard. The dolerite is strongly magnetic, and certain specimens exhibit marked polarity. Where the rock becomes amygdaloidal the cavities contain zeolites, of which three are here specialised—namely, *mesole*, in globules of dull white colour; *christianite*, not the felspar so named by Montecelli; and the variety of chabazite known as *phacolite*.

The Greenland meteorites noticed in the last number of this Journal have been made the subject of a further communication to the Geological Society, in which Prof. Nordenskjöld describes carefully the conditions under which the masses of iron occur at Ofivak, and also gives analyses of the metal. A portion of one of the largest blocks yielded—Iron, 84·49; nickel, 2·48; cobalt, 0·07; copper, 0·27; magnesia, 0·04; sulphur, 1·52; phosphorus, 0·20; chlorine, 0·72; organic matter, water, and loss, 10·16. Nordenskjöld adheres to his opinion that these remarkable masses of metal are genuine meteorites.

Success continues to attend the diamond workings in South Africa. At present the most active operations are going on at the "New Rush," on the Colesberg Koppie. Mr. Gilfillan has just returned to this country with some remarkably fine diamonds, accompanied by samples of the associated rocks and detrital matter. We have had an opportunity of examining these specimens, and believe that the results of Mr. Gilfillan's observations on the conditions under which the diamonds are found will shortly be communicated to the Geological Society.

ENGINEERING—CIVIL AND MECHANICAL.

Guns.—In our last quarter's chronicles reference was made to a crack which had been discovered in the lining of the new 35-ton gun known as the "Woolwich Infant." Three theories have been advanced to account for this internal injury, which were recently stated by Capt. Dawson, R.N., in a lecture on Guns, at Plymouth. The first theory is, that the bottom stud, flattened by the blow above the shot caused by the escaping gas, overrode the groove, causing a squeeze which delayed its exit, and led to an accumulation of gas in the powder-chamber. 2nd. That the wabble caused by balancing the shot on two studs, and the irregular action of the powder above it, due to the non-centreing of the shot, wrenched out or sheered off the stud, and set up a motion of the shot across the bore, which enhanced the difficulty of its escape. 3rd. That pebble powder developed some new quality, when ignited in 120-lb. charges, which it did not possess when fired in quantities of 100 lbs. and under. Captain Dawson is evidently of opinion that the Woolwich system of rifling is one of the primary defects in this new gun, and that the injuries were attributable to the vicious system of grooving on an increasing twist, which necessitated the concentration of rotary effort on a single row of studs incapable of giving adequate rotation with the present amount of spiral, and this angle of spiral could not be increased, because the studs would not endure the extra effort, but would be sheered off, and cease to act at all. Contrasting other long iron bearing systems which had undergone official trial with the present short bearings, Capt. Dawson showed that, whilst the whole effort of rotating a 700-lb. shell was now thrown upon a total of 5½ inches of stud bearing, it would under the Scott iron flange system be diffused over 13 feet of bearing; and that this latter system had narrower shallower grooves, which took only one-fourth the quantity of metal out of the gun, and therefore made less space for escaping gases to erode the bore; whilst, instead of the lower groove being spiked by its own stud, Scott's iron flange would receive the shock on a rib 22¼ inches long. In the trial between two 7½-ton guns on this system, the Woolwich rifled gun was declared incapable of further firing, except "under precautions" against bursting, whilst the grooves and lands of Scott's gun were perfectly uninjured. Yet Scott's guns gave the greatest hitting power at the muzzle, and projected its shot 1500 yards with 2 degrees of elevation, using 20 lbs. of powder, whilst the Woolwich one required 25 lbs. to reach the same distance. Comparing the work done by the respective projectiles, the Scott iron bearing 110-lb. shot escaped from its charge so much more easily than its studded rival, that—though the conditions were exactly alike in all else—the Scott hit a muzzle blow 133 foot-tons heavier than the French projectile, and no less than 260 foot-tons more powerful than that of the present 7-inch gun, the charges of which have to be reduced to make amends for its lack of endurance.

Navigable Balloon.—The experimental balloon recently constructed by the French Government, upon M. Dupuy de Lôme's system, rose from the court-yard of the Fort Neuf, at Vincennes, on the 2nd of February last, having on board MM. Dupuy de Lôme, Lédè (Engineer in the Marine), Yon, and Dartois (aërostats), and ten other persons, forming altogether a total of fourteen men. The construction of this aërial machine starts with the principle that, in order to obtain a navigable balloon, its permanence of form must be maintained without any sensible undulation of surface, and a horizontal axis of least resistance must be obtained in a direction parallel to the propelling force. The permanence of form is assured by a fan carried in the car, and put in communication by a tube, with a small balloon placed within the large one at its lowest part. The volume of this small balloon is one-tenth of that of the large one. It is furnished with a valve opening both within and without, and regulated by springs. The large balloon is provided with two hanging tubes open to the air, and falling for a distance of 25 feet from the lower part of the balloon. The inflation of the little balloon causes the hydrogen to fall more or less in the hanging tubes, but never sufficiently to force it out of their open ends. To obtain a horizontal plane of least resistance, the form given to the balloon was that developed by the arc of a circle turning round its chord, and in which the versed sine was nearly one-fifth of the length of the chord. The extreme total length of the balloon is 118 feet 6 inches; its greatest diameter 48 feet 9 inches; its extreme height 95 feet 6 inches; and its cubic contents 122,000 feet. The screw is 29 feet 6 inches in diameter, and the total ascensional force of the balloon 3·799 tons. The rudder for steering the balloon consists of a triangular sail placed beneath it, and near its rear, which is kept in position at the bottom by a horizontal yard 19 feet 18 inches long, turning round a pivot on its forward extremity. The height of this sail is 16 feet 4 inches, and its surface 161 square feet, Two ropes for working the rudder extend forward to the seat of the steerer, who has before him a compass fixed to the car, the central part of which is large enough to carry a crew of fourteen men. The screw is carried by the car, and is driven by four men, or by eight men working at a capstan. The gas-escape valves, of which there are two, are placed at the top of the balloon, immediately over the pendant tubes, and through which the cords for working the valves pass into the car. The conclusions arrived at by M. Dupuy de Lôme, from the results of the trial trip, were,—that the stability of the balloon was perfect, that it manifested no signs of oscillation under the action of the eight men working the screw, and that the shifting of the weight in the car produced no sensible movement. The vertical axis was only shifted, under the most trying conditions, a small part of a degree, and longitudinally there was no change. In comparing the direction of the balloon drifting freely before the wind with the direction given to it when the screw was in operation, it was found that the resultant made with the normal direction an angle of 12°. It is stated, also, that the speed given to the balloon with 27½ revolutions of the screw was 6½ miles an hour, whilst the rate due to the wind alone was from 26 to 37 miles an hour.

New Railway Projects.—The number of plans deposited at the Private Bill Office of the House of Commons, in connection with new railway projects, amounts to 144. There are also 28 tramway Bills, and 53 Bills of the miscellaneous class, including docks, harbours, local improvements, gas, water, and irrigation works. Amongst the railway schemes there is a proposition to connect the Great Eastern, the North London, and the East London Railways, with the Metropolitan and Metropolitan District Railways. The London and South Western Railway Company propose to effect a junction between their line and that of the London, Chatham, and Dover Company, by means of a railway from their Waterloo Station to the Blackfriars Station of the latter Company. The Metropolitan District Company propose an extension of their line to that of the South Western Company at Barnes. The Metropolitan and St. John's Wood Railway Company propose three new extensions,—to the Hampstead Junction Railway near the Edgeware Road, to join the Midland Railway at the Finchley New Road, and to Green Street, Grosvenor Square, respectively. The Mid-London Railway consists of a series of lines starting from the West end of Oxford Street, and running to the London, Chatham, and Dover Railway near Farringdon Street, and to the East London Railway at St. George's in the East, with junctions to the authorised lines of the Central London and of the East London Railways. Another Mid-London scheme consists in the construction of a railway from the London and North Western line at Willesden to the West end of Oxford Street. Turning to provincial plans, the first proposition of magnitude is the old Brighton, Eastbourne, and London Railway scheme: commencing by a junction with the London, Chatham, and Dover Railway at Penge, and the South Eastern Railway at Beckenham, it runs thence to Brighton and Eastbourne, having branches to Westerham and to the Surrey and Sussex Junction Railway at Oxhead, and a goods branch at Lewes. This scheme involves terms and conditions to be entered into with no less than fifteen existing companies. Most of the main lines have suggestions for extensions in different directions, amongst which we notice one by the London and North Western Railway Company for a line from their Bettws-y-Coed Station to the Festiniog Railway, to be constructed on a gauge of 1 foot 11½ inches, the same as that of the Festiniog Railway. There are four sets of plans deposited for crossing the Severn by bridges, and one for a line to pass under it by a tunnel.

Railway Brakes.—Numerous railway accidents might have been easily prevented had there existed sufficient brake power on the train to enable it to be pulled up more quickly than can be done with brakes on the ordinary system. Naylor's continuous brakes, which have been designed to enable brake power to be applied to each carriage throughout the train, were tried not long since on the Sevenoaks branch of the London, Chatham, and Dover Railway. For a long time it has been feared, in many quarters, that if a set of brakes were applied nearly simultaneously, on a train travelling at a high speed, it might create an unpleasant sensation to the passengers. The above-named trials showed, however, that if the application of the brakes is duly regulated no such effect is produced. Thus, with the experimental train travelling at 57 miles an hour, down an incline of 1 in 101, the driver on the engine applied the set of continuous brakes, shut off his steam, but did not reverse his engine, while his fireman applied the tender brake, yet, even with this suddenly applied retarding force, no unpleasant sensation was experienced by any one in any part of the train. The following are some of the results obtained at the experiments above referred to:—

No.	Gradients.	Speed in Miles per hour.	Distance travelled after applying Brakes and Shutting off Steam.	Time required to pull up.
1	1 in 101 down.	53	Distances not measured.	55 seconds.
2	Do.	43	460 yards.	38 "
3	1 in 101, up not quite all the way, partly down an incline.	35	212 "	20 "
4	1 in 101 down.	57	750 "	60 "
5	Level.	50	700 "	

Mr. Naylor has four different methods of letting his brakes on and of taking them off again. In every case the brakes are let on by slackening out the continuous chain, and the variations in the plans consist in the different methods adopted for tightening this chain, and thus taking the brakes off. Mr. Naylor, in describing the application of his brakes, remarks:—"Everything connected with my brakes is fixed below the under frame of the carriage, and is attached by longitudinal angle irons; between these angle bars two plates are suspended, that carry two pulleys in a fixed position; between these two pulleys there is a third one, which is moved up and down, in connexion with the brake lever, by the continuous chain that runs from carriage to carriage. The force upon the brake lever is obtained by angular links, one attached to the frame, and the other to the end of the brake lever. To the knee joints of these links is applied the tension of the spring. It is obvious that the force pressing upon the brake lever depends upon the strength of the spring, and therefore anything that is required can be accomplished, even to the extent of skidding all the wheels; consequently this brake, when once applied, must be as powerful as any other."

An arrangement for retarding locomotives, by the admission of steam against the pistons, is being introduced by Mr. William Bouch, at the North-Eastern Railway Works at Darlington. A pipe leads from the steam space of the boiler to a longitudinal pipe, which conveys the steam to branch pipes, through which it is admitted into each end of the cylinders at the same time, this steam being admitted after the main supply of steam has been shut off from the ordinary valve boxes of the cylinders, and when it is necessary to retard the progress of the engine or to stop it entirely. The supplementary portion of steam fills both ends of the cylinders, and presses against each of the pistons alternately, so as to act as a cushion in the to-and-fro movement. This mode of retarding the progress of the engine may be used in conjunction with the brakes already fitted to act against the wheels, or it may be used alone, in which case the entire stoppage of the engine would be under the control of the steam cushion.

The Sand Blast.—Tilghman's sand blast has, during the past year, created considerable sensation in the United States. Its object is to drill, cut, or grind, hard substances, such as granite, metal, or glass, and its action depends upon the expulsion, at a considerable velocity, of quartz sand, by a steam or air jet passing through a tube, and striking the material operated upon. In some early experiments made with this apparatus, a hole, 1½ inches in diameter and 1½ inches deep, was drilled through a block of corundum in 25 minutes, with a pressure of steam of 300 lbs. With 100 lbs. pressure a hole, 1 inch by ¼ inch, and ¼ inch thick, was cut through a hard steel file in 10 minutes. At the fair of the American Institute, at New York, last year, a diamond was sensibly reduced in weight in one minute, and a topaz was entirely destroyed. It is a curious feature of this invention that, whilst hard substances are thus rapidly affected, soft and delicate materials are left untouched when exposed to the same influence. Thus, if a thin stencil sheet of india-rubber be laid over a block of granite or marble, and the blast turned upon it, the stone is cut or drilled, while the rubber remains untouched. If a photographic film of bichromatised gelatine be placed on a sheet of glass, and the jet applied, a picture may be engraved, and in the same manner flowers and fern-leaves may be reproduced with the utmost delicacy. For grinding glass a very slight pressure is sufficient, that produced by air under 4 inches of water being ample for the purpose.

Engineering Societies.

Institution of Civil Engineers.—At the fifty-fourth Annual General Meeting of this Institution it was announced that there were on the books, on the 30th of November last, 14 honorary members, 724 members, 1048 associates, and 203 students,—together 1989, as against 945 ten years ago. At the first meeting in January, Mr. Hawksley, who was elected President at the Annual Meeting, took his seat, and delivered an excellent Address, the leading topics dwelt on being—the use of the Engineering profession to Government in times of war, railway construction, and hydraulic engineering. On the 23rd of January a paper was read on "The Somerset Dock at Malta," by Mr. Charles Andrews. This dock is situated in the French Creek. The works were commenced in 1865, from designs by Col. Clarke, and under the superintendence of Mr. Andrews. The length on the floor is 428 feet; the length over all, 468 feet; the width of the floor, 42 feet 6 inches; the width between the copings, 104 feet; and the width of the entrance, 83 feet. The depth of the invert, floor, and entrance, below the average sea level, is 33 feet 6 inches. The cost of the Dock, inclusive of the caisson, has been about £120,000. The entrance cost £30,000, exclusive of clearing the rock from the site.

An important paper, "On the Value of Water, and its Storage and Distribution in Southern India," was read by Mr. George Gordon, on the 30th of January last. From this paper we learn that there are two general systems employed in India, namely, Tank and Channel Irrigation. The existing tank irrigation is chiefly of native origin, and may be divided into three classes:—1. Tanks formed by the closing of the passage of a considerable river through a narrow gorge, in a range of hills, by means of a high dam or "bund." 2. Those formed in the plains by embankments carried across the drainage of the country, and impounding the water of one or more streams, these tanks being of great superficial area, but shallow. 3. Tanks which might be considered intermediate between the other two, having in general a greater length of dam than the first and a greater depth of water than the second. Few examples of the first kind remain entire. Under the head of Channel Irrigation, it was stated that only rivers of the larger class, which had a continuous flow for several months, were available for extensive irrigation projects. The smaller rivers were merely torrents, which quickly carried off heavy falls of rain and then became dry again. The water, however, is in many cases intercepted by chains of tanks, of the second or third class, built across these torrents. The details of irrigation channels were then dwelt upon, and other particulars,—such as their cost of construction, the net profit per acre upon various crops, loss by evaporation, &c., which it would occupy too much space to follow out in further detail now.

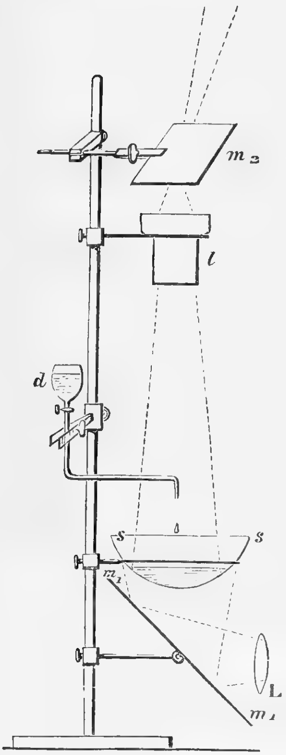
Institution of Mechanical Engineers.—The Members of this Institution met at Birmingham on the 25th of January last. Amongst the papers then read were two to which we shall here refer, viz., "On the Strength and Proportions of Rivetted Joints," by Mr. Walker Brown, of Bristol, and "A Description of the Disintegrating Flour Mill, and Machine for Pulverising Minerals, &c., without Grinding, Crushing, or Stamping," by Mr. Thomas Carr, of Bristol. In the former paper, four different modes of fracture possible in rivetted joints were described, consisting in—shearing of the rivet; crippling of the plate, or elongation of the rivet hole in the line of strain; fracture of the metal between the rivet hole and the edge of the plate in the line of strain; or tearing of the plate along the line of the rivet holes, at right angles to the line of strain. From the consideration that a perfect joint would be one offering equal resistance to each of these modes of fracture, the proper proportions were deduced for the various descriptions of rivetted joints, with the aid of data furnished by different experiments previously recorded, and by a series of experiments recently made for the purpose by the writer.

In the process of disintegration by Carr's Disintegrator, the particles of the material operated upon are shattered in mid-air by a succession of blows delivered with extreme rapidity in opposite directions, and are thus pulverised by the force of the blows alone, without being subjected to the compression or friction which accompanies the ordinary processes of grinding, crushing, or stamping the material between two surfaces. The disintegrator consists of a pair of circular discs, rotating in contrary directions upon two shafts situated in the same line; the opposing faces of the discs are studded with a series of short projecting bars, or beaters, arranged in successive concentric rings or cages; and the rings of beaters fixed in one disc intervene alternately between those fixed in the other disc, and revolve in the opposite direction. The material to be pulverised is supplied through an opening in the centre of one of the discs, and receives from the innermost rings of beaters a centrifugal motion, propelling it towards the circumference of the discs: in its course through the machine it encounters successively the several rings of beaters revolving alternately in opposite directions at a high speed, and the particles are thus dashed violently by each beater against the beaters in the next outer ring running in the contrary direction, whereby the material is effectually broken up and reduced to powder.

MECHANICS.

At the Meeting of the Royal Scottish Society of Arts on the 11th of March, an apparatus was exhibited by Dr. R. M. Ferguson, for showing the motion of waves on the surface of a liquid, and for illustrating more particularly the propagation, reflection, and interference of the waves of sound. It is similar in principle to that exhibited by Professor Morton, in Philadelphia, and described in our last October number. It has the advantage, however, of greater simplicity, and is merely a combination on an ordinary chemical stand of things generally in the hands of a teacher of science, and used for the most part for other purposes. The accompanying cut shows the arrangement. *l* is the illuminating lens of a magic lantern. *m* is a plate-glass mirror placed at 45° to turn the horizontal light into a vertical direction; *ss* is a glass saucer made by cutting the end from an ordinary glass shade; *l* a photographic lens; m_2 a second mirror to turn the light again horizontal but at right angles to its first horizontal direction upon a screen. The glass dish is partially filled with methylated spirits, and the dropping apparatus, *d*, intended to excite the waves, is filled with the same liquid. This last consists of a Wolff's bottle funnel, furnished with a glass stopcock (of the ordinary German make), bent into the form shown, and having its lower extremity where the drop issues drawn out into a pipette point. The whole can be fitted to a retort stand with the aid of the usual rings and clamps that form the complement of its "fixings." The great difficulty in making a successful arrangement for exhibiting what must necessarily lie horizontal on a vertical screen, is the full illumination of the object to be exhibited. The diverging cone of rays that falls on the surface of the liquid is in no way altered in its divergence when it passes through a flat transparent vessel. The surface of the liquid is perfectly transparent and does not catch the light. The cone of rays therefore continuing to expand as it approaches the defining lens *l*, passes for the most part outside of it, and hence the imperfect illumination. The glass shade and its liquid contents form a converging lens, which throws the whole light incident on it through the lens *l*, and thus no light is lost. The course of the light is shown by the dotted lines. In Professor Morton's apparatus, a horizontal lens is placed immediately under the flat-bottomed vessel containing the liquid to effect the necessary convergence. In Dr. Ferguson's arrangement the lens and containing vessel are one and the same thing. Such composite lenses were stated to be singularly accurate and effective, and give considerable latitude in regard to size. The glass dish used was 8 inches in diameter, and was filled to form a lens 6 inches in diameter. It was cut from a shade whose cylindrical part was 10 inches across. The surface of the liquid was in focus with the lens used at about 9 inches, but the waves were best seen when the lens was 15 inches distant. The dropping apparatus can be brought over any part of the surface, and the rapidity of the drop regulated by the stopcock. When the glass dish is full, different figures with upright sides of metal plate can be placed in it so as to alter the shape of vessel at will, and illustrate the conformation of the wave systems of plane figures of all kinds. The sides of these reflect the waves, and the outer edges can be darkened with paper so that only the inside is seen. The waves were exhibited by the aid of the lime-light, on a screen 15 feet in diameter, with singular beauty and clearness. The same apparatus was used to illustrate a course of lectures on Sound, in the Edinburgh Museum of Science and Art. The screen employed in this case was a square sheet of ground glass, 3 feet in size, when the demonstrations could be distinctly seen by the audience in the full blaze of the gas of a hall well lit by sunlights. Spirits were used instead of water, as under the conditions they give a slower wave and a clearer definition.

FIG. 22.



Lockwood and Co. will publish on April 1st the first number of a new Quarterly, under the title of "Naval Science," a magazine intended to promote the improvement of Naval Architecture, Marine Engineering, Steam Navigation, and Seamanship. Its appearance is looked for with considerable interest in naval and scientific circles, as it is to be edited by E. J. Reed, C.B., late Chief Constructor of the Navy, and the contributors will include the most eminent authorities in the several branches of the above subjects.

LIGHT.

In the oxyhydrogen light the use of cylinders of burnt dolomite in place of ordinary lime is recommended as increasing the regularity of the light.

An interesting observation upon the influence which the temperature of the prism exercises upon the position of the lines has been made by M. Blaserna. It has already been noticed by Verdet, that where liquids are used for prisms, the displacement of the lines is quite noticeable, the index of refraction altering with the temperature. For solid prisms, however, it was assumed that the influence of change of temperature upon their refractive power was quite insignificant. M. Blaserna has found, however, upon observation, that this is not so unimportant as is generally believed; but that, while much less sensitive than liquid prisms, the displacement of lines could readily be observed with one of flint glass. The prism used in the experiment was one of Duboscq's, and was exposed for some time to the direct rays of the sun. It was then quickly placed in a spectroscope in the shade and some prominent line observed, when it was found that as the glass cooled the refractive power increased. (An opposite result is obtained for bisulphide of carbon). He was able to observe in this manner a displacement of the D line, amounting to 3″ for a difference of temperature equal to 1° C. In the instrument used the interval between D and D′ is 12″, so that a change of 4° C. would suffice to place D in the position of D′. M. Blaserna remarks, as the result of his interesting observation, that an error might very readily be made in spectroscopic work, by comparing together observations made in sunlight and in shade, or those made in the morning with those in the night.

In "Poggendorff's Annalen," M. L. Schonn contributes a paper on the employment of cylindrical lenses in spectroscopic observations. As in observations of the spectrum we have to do with lines of light, and these, moreover, straight ones; and as, in addition, it is not necessary that the rays falling on the prisms should all be parallel to each other, but only that they should all be in parallel planes, M. Schonn thinks it would be far better to make use of lenses suited to this especial purpose, and to employ throughout none but cylindrical lenses also for illuminating the slit. From a preliminary examination he has ascertained that one thus obtains well-defined spectra, in which the full extent of the prism is rendered available. Perhaps, also, another disposition of the spectrum might at the same time be recommended—namely, one wherein the image of the slit should be horizontal, inasmuch as lines that lie in a horizontal position are seen with greater precision.

Just before going to press we received, through the Royal Astronomical Society, an announcement of the discovery of a minor planet (118), Peitho, at Bilk, by Dr. R. Luther.

1872.	M.T. at Bilk. h. m. s.		h. m. s.		° ′ ″
March 15	14 18 59·6	R.A. =	12 7 26·73	N.P.D. =	79 42 33·5

From an observation made by Dr. Tietjen, at Berlin:—

1872.	M.T. at Berlin. h. m. s.		h. m. s.		° ′ ″
March 21	9 33 23	R.A. =	12 1 36·36	N.P.D. =	79 20 46·1

The daily motion obtained from these observations is in R.A. – 60·6″, and in N.P.D.—3′ 45″. The planet is of the 11th magnitude.

Microscopy.—An improved erector for use with binocular microscopes has been contrived by R. H. Ward, M.A., M.D., Professor of Botany and Microscopy in Rensselaer Polytechnic Institute, U.S. The lenses of a 1½ or 2-inch objective may be packed or screwed into the upper end of an adapter, which when screwed into the nose-piece of the microscope, carries them up close to the binocular prism, and into the lower end of which the object-glass may be screwed. A more convenient arrangement is an adapter with a sliding tube adjustment, which varies to the extent of an inch or more the distance between the erector and objective. Different powers and distances will of course be used according to the wants of different observers. The combination considered most convenient by the inventor consists of a 2-inch erecting lens close to the binocular prism, and a ⅝-inch objective at a distance measured to its lower end of from 3 to 4½ inches below the erector, giving powers of from 10 to 50 diameters, and requiring a working distance between the stage and the binocular prism of $4\frac{1}{2}$ to 5 inches, which is quite practicable with large stands. Where sufficient working distance cannot be obtained, the object may be placed upon the sub-stage, or more conveniently, the sub-stage removed, and the body racked down, so as to focus through the empty stage upon the table, where the object may be supported by any convenient method. A modification of the above has been arranged by Mr. F. Oxley, of the Quekett Microscopical Club. Into the nose-piece of the microscope he screws a 3-inch or 4-inch objective, and has a sliding fitting made with the universal screw to carry an objective placed beneath the stage. The object to be viewed is placed on the table, where it can be very conveniently manipulated; a $1\frac{1}{2}$-inch objective beneath the stage answers very well in combination with a 4-inch (the single combination of Ross) on the microscope. This arrangement has the advantage of being inexpensive; it can be adapted to any microscope at a cost of a few shillings. This erector is free from many of the objections to the usual erector placed within the body. The view given of the object is extremely good, and considerable variation of magnifying power may be obtained by altering the distance between the two objectives. The microscope is really converted into a telescope of short focus, the inverted image being formed by the combination below the stage, and again inverted and consequently erected by being viewed with the binocular microscope. The contrivance is especially valuable to those who have dissections or other manipulations to conduct under the microscope: all such operations can be carried on in the most comfortable manner, with the hands resting on the table, and the body free from the constrained stooping position involved in the use of single microscopes; and all these advantages combined with the large field, fine definition, and capability of distinguishing vertical distances which are so peculiarly the properties of a good binocular instrument.

"Silliman's Journal" contains an account of a new eye-piece micrometer and goniometer, designed by Dr. H. T. Porter and Mr. J. P. Southworth. The scales in both instruments are made by photography instead of the usual process of engraving with a ruling machine and diamond. The original scale being drawn upon paper and consisting of 100 heavy black lines about $\frac{1}{6}$th of an inch apart, the lines marking every ten divisions are 6 inches long, and extend 1 inch on each side of the scale; those marking each five divisions are 5 inches long and extend half an inch on each side; the remaining lines are 4 inches long. A negative is taken in which the scale is reduced to 2 inches in length. From this a transparent positive is taken reducing the scale to about half an inch in length. In this the lines are $\frac{1}{200}$th of an inch apart. A thin glass cover is cemented on with Canada balsam for the protection of the lines; it is then mounted in a similar manner to Jackson's micrometer. For the goniometer, a circle about 18 inches in diameter is drawn with India ink, and divided into degrees. The centre is indicated by a dot, and one diameter is drawn. Every five and ten degrees are indicated by longer lines than those indicating single degrees; every ten degrees of each quadrant are numbered from 0° to 90°. A negative of this diagram is taken as before, and a reduced positive of a size to fit the tube of the microscope; this disc is mounted to fit the tube at the focal point of a positive eye-piece. A cobweb is drawn across the diameter of the lower lens. When a crystal is to be measured, the stage is moved until the apex of the angle coincides with the centre of the goniometer, and the diameter with one side. The eye-piece is now turned till the cobweb crossing the diameter at the centre coincides with the other side of the angle. The number of degrees can be read off at the circumference without the trouble of looking outside as in ordinary contrivances or reading the verniers of a rotating stage. The inventors claim for these photographic scales the advantages of cheapness and greater legibility than that of the usual ruled ones. The transparent spaces are quite clear enough not to seriously impair the definition of the instrument.

In the course of some remarks on the structure of the Arborescent Lycopodiaceæ of the Coal Measures, delivered before the Royal Microscopical Society, by Mr. W. Carruthers, F.R.S, it was mentioned that the late W. Nicol of Edinburgh, well known as the inventor of the single image prism, was the discoverer of the mode now in use of making sections of fossils for microscopical examination. His large collection is now deposited in the British Museum.

A good substitute for a cement which is a favourite with many microscopists, although far from trustworthy, viz. asphalte, will be found in a varnish employed by Messrs. Horne and Thornthwaite in the process of etching thermometer tubes and other glass instruments. Ordinary varnishes were found to fail for this purpose, as they permitted the hydrofluoric acid to creep under the ground and so cause a rough imperfect line to be etched; the new varnish resists the acid perfectly. Its good qualities for the microscopist are—that it adheres well to glass, and when hard does not become brittle, but comes away in shreds when scraped, like gold size, the most reliable of varnishes for fluid mounting; its advantage over gold size is that it possesses more body, owing to an admixture of a suitable quantity of copal. This property renders it very suitable for making cells on the turntable; these are, however, much improved by baking at a moderate temperature, like japanned wares, without which process all cells formed of oil varnish are liable to be softened by subsequent coatings: neglect of this precaution is a frequent cause of objects mounted in such cells being spoiled by the running in of the cement.

HEAT.

M. Laborde has made some experiments on calefaction. He let a thin thread of water pass through the jet from the blowpipe, and he found on examination that the water which had thus passed through a heat capable of melting almost any metal was but slightly warmed; in fact, the difference was but 3°. If a jet is passed through an ordinary flame the increase in temperature is considerably higher, probably owing to the incandescent particles carried away by the liquid from the smoke. A sheet of water presents similar evidence. If the jet from the blowpipe is directed against it it is not pierced, nor is there any sensible heating effect; the finger can be brought to within a few millimetres of the flame, and yet there is no sensation to indicate the near proximity of an otherwise so potent source of heat.

"The event of the *séance*," says the Abbé Moigno, in "Les Mondes," speaking of a recent meeting of the Academy of Sciences at Paris, "was a truly singular and noteworthy incident connected with the subject of the sun's temperature. On the one hand, Father Secchi maintained, and attempted to establish, his figure of ten million degrees of temperature (centigrade) against MM. Ericsson, Zöllner, and Faye; on the other hand, M. Vaulle, Professor of the School of Mines, and formerly a Laplace prizeman, established by means of a very learned and novel theory, a maximum temperature of 10,000 degrees. Thereupon, M. Henry Sainte-Claire Deville announced that he is engaged in determining the temperature and pressure at the sun's surface, and asserted (as the result of his first researches) that the sun's temperature exceeds some three or four times the temperature at which platinum melts, amounting, therefore, to from 6000 to 8000 degrees. Next, M. Fizeau stated, that having compared (like M. Leon Foucault) the solar light with that of the glowing carbon points of the electric light, he had estimated that the former is about three times as intense as the latter; hence, inferring the relative calorific intensities from the observed relative luminosities, he also had deduced a solar temperature of 8000 degrees. So that, according to French science, the solar heat does not exceed 10,000 degrees; and, in fact, it is emitted by ignited and dissociated terrestrial elements.

MM. E. and M. Becquerel have made some observations on the differences of temperature at divers depths between turfed and denuded soil, when both were covered with snow. They were made in the Jardin des Plantes, Paris, two contiguous pieces of ground being chosen for the purpose, one of which was covered with short vegetation, the other being bare. The thermo-electric pile was used to detect the differences of temperature. Snow commenced to fall at Paris about two o'clock in the afternoon on the 7th of last December;

the next day the earth was covered with a layer having a mean thickness of between 7 and 8 centimetres. The temperature of the air rapidly fell on the 9th, the minimum observed at the Jardin des Plantes was 20·7° C., and at the Observatoire 21·5° C. The table of observations of the temperatures observed at different depths beneath the snow-covered soil, show, however, that at no time was it lower than the zero of the centigrade scale in the case of the turfed ground, at depths of 0·05 m. and 0·03 m. At the depth of 0·05 m., the temperature, starting about 0·05° C., rose to 0·7° C.; at 0·1 m. and 0·3 m. the temperatures were 1 and 2 degrees, and the variations 1 and $\frac{2}{10}$ths of a degree, indicating a tolerably constant temperature at each station. At these depths, therefore, in ground of this kind, roots, seeds, and other organic bodies are preserved from the ill effects of a frost even of 20° below zero. In the case of denuded soil, it was otherwise from the 2nd of December to the 6th, at a depth of 0·05 m.; the temperature was always below zero, and on the 15th at zero. On the 7th it was 1°, after which it continued to fall till the 10th, from which it again rose a few tenths daily up to the 15th. From the preceding observations, it therefore appears, that a bed of snow 7 or 8 centimetres in thickness, completely protects objects at a depth of 0·05 m. from even such a severe frost as 20° C., if the earth is turfed; whilst when it is bare, the temperature at the same depth is 1·5° below zero. Seeds and roots will therefore be kept with perfect security in turfed ground, which, if the ground had been ploughed or otherwise tilled, would undoubtedly have perished.

MM. Leygue and Champion have invented an apparatus for measuring the temperature of the detonation of explosive compounds. The apparatus is founded on the known distribution of the temperature in a metallic bar heated at one of its extremities, and consists simply of a copper bar of 0·025 m. diameter and of 0·60 m. length. Small cavities are drilled in the bar at distances 0·10 m., these cavities being filled with oil in which the bulbs of small thermometers are immersed. The following are some of the temperatures of the detonation of the chief explosive compounds; the degrees are centigrade. Chassepot powder, 191°; fulminating mercury, 200°; Abel's protoxide, 205°; sporting powder, 288°; picrate of potash, 336°; nitro-glycerine, 256° to 257°; artillery powder, 380°.

M. Fosselli has succeeded in producing an amount of cold just below the zero of the Fahrenheit scale by simple mechanical action, creating rapid evaporation. He employs a wheel formed of a spiral tube, both ends of which are open, set vertically and half immersed in the fluid to be cooled, so that the latter passes constantly through the whole length of the tube, half of which is constantly above the liquid, and being wet, gives rise to active evaporation and consequent refrigeration within it.

M. Ch. Tellier describes an arrangement and apparatus whereby the vapourisation of ether is employed for the production of intense cold, provision being made to recover the ether in a very ingenious way, by causing it to be absorbed by sulphovinic acid, from which it is afterwards again separated by a simple distillation. An apparatus is now being made according to the author's instructions with which it will be possible to manufacture a ton of solid ice per hour, while the apparatus, of great simplicity and without any complicated fittings, will admit of constant action, and thereby making the ice at a very low price.

Mr. Fletcher has brought out an improved gas furnace and hot-blast blowpipe. The speciality of this furnace is the burner. It is as simple as an ordinary Bunsen's burner, but the flame is solid to the centre. Copper will fuse in any part of the flame; and to make a crucible furnace simply requires a support for the crucible, and a fire-clay jacket to prevent radiation. The lower part is a chamber 6 in. by 3 in., open at the bottom, in which the gas is partially mixed with air. This mixture is conducted to the top of the burner through a mass of fine tubes, with an arrangement to supply between each exactly the amount of air necessary to consume it instantly. A flame produced by this means, consuming 20 feet of gas per hour, is about 2 inches high and almost colourless. The whole of the available heat is generated below the object to be heated, which, therefore, is not also cooled by the passage of unburnt gas and air. The point of greatest heat commences, as with a blowpipe, at the point of the blue cones, about ½ in. or ¾ in. above the tubes; and if the flame is protected with a ring of fire-clay, continues uniform for some inches above. The great heating power of the hot-blast blowpipe is obtained by an arrangement which enables both gas and air to be supplied to the jet at an exceedingly high temperature. The jet will fuse a strand of 6 or 8 fine platina wires into a bead with a small point of flame, and will give a small light with a cylinder of lime. With the gas fully turned on, it will melt 3 ozs. of 18-carat gold on pumice-stone. Steel wire burns readily, with brilliant scintillations, and wrought-iron wire is readily fused. The additional heating power may be used or not at will, and the blowpipe can be used either with the mouth or a foot-blower; when used with the mouth the head is not confined in one position, as in case of the old form, and both hands are at liberty. The extreme power is exerted to the best advantage on as small objects as possible, and when the gas is turned down to a blue cone of about 1 or 1½ inches long. The point of this blue cone will fuse a few grains of platinum, supported on lime, in five or six seconds, if a foot-blower is used. If, however, a smaller quantity is taken—say half a grain—it is fused into a bead instantly, either with the mouth or a foot-blower. If a platinum wire is used, it should be held with the point exactly in front of the point of the blue cone. With care, a bead may be fused on the end of a platinum wire almost as thick as ordinary copper bell wire. If a very fine wire is used, it will melt almost as quickly as it can be passed along the flame: in these experiments the eyes should be protected from the blinding glare of light, more especially in fusions on lime. For soldering and heating platinum crucibles, the gas should be turned full on, so as to produce a large rough flame, the heating power of which is about double that of the ordinary blowpipe. The lower burners need never be turned on more than is necessary to allow the flame just to reach the top of the coil.

ELECTRICITY.

The electric currents obtained by the bending of metals have been examined by P. Volpicelli. After relating at length his method of experimenting, he summarises the results of his experiments in the following points:—All metals when being bent or twisted give rise to the development of an electric current, but copper exhibits this phenomenon in the highest degree. Lead, although not an elastic metal, also gives rise to the generation of an electric current, thus exhibiting an instance of the conversion of mechanical force into electricity; the electric currents thus produced are not perceptibly due to the development of heat due to the action of bending or twisting. When the bending is accompanied by the tearing asunder, or, rather, the distending, of the two ends of the metallic wire, a current is produced in an opposite direction from that obtained by putting the two ends nearer together; by increasing or decreasing the velocity of the bending, the intensity of the electric current is also increased or decreased. A metallic wire made of various metals soldered together produces, other conditions being the same, a less intense current by bending than when the wire is made of one and the same metal.

M. Eugenio de Zuccator has invented a novel method of rapidly copying manuscripts or designs, whether produced by hand or photography. An ordinary letter copying press is used for printing from the design, which is formed upon a varnished metal plate. This plate, which is of iron, is either coated with a shellac varnish, and the writing or design to be copied then traced thereon with a metal point, or it may be coated with gelatine and bichromate, and the design produced by means of photography with a transparent positive. In any case the lines are formed of bare metal upon a surface of varnish. One wire of an electric battery is connected to the bed of the copying press, and the other to the upper plate of the instrument, so that when the press is screwed down and the top and bottom plates come in contact, an electric current passes. The varnished metal plate, upon which a memorandum has been scratched, or otherwise produced, is covered with a few sheets of copying paper wetted with an acid solution of prussiate of potash, and then screwed into the press. The characters or design upon the varnished plate are formed of bare metal, and in these parts an electric current is set up; this action permits of the union of the iron with the potash, and the consequence is, Prussian blue is formed in lines corresponding to those upon the varnished plate. Copies thus produced in blue ink may be printed at the rate of 100 per hour. The patent is the property of Messrs. Waterlow and Sons.

At a recent meeting of the Royal Society, a paper was read by Professor J. Clerk Maxwell, F.R.S., "On the Induction of Electric Currents in an Infinite Plane Conducting Sheet." The paper contained a scientific demonstration of what takes place in a conductor of infinite extent, acted upon by a system of magnets or electro-magnets. It is of singular importance in giving an insight into the action of dampers on magnetic needles, such as Harris's copper ring surrounding a ship's compass; in fact, into those causes which check the vibration of needles, and which seem to exercise a kind of unseen friction on suspended magnets. The author states that former writers do not take into account the inductive action of the currents on each other, though the existence of such an action was recognised. In his investigation a system of magnets or electro-magnets is supposed to exist on the positive side of the sheet; and to vary in every way by changing its position or its intensity. The nature of the currents induced in the sheet, and their magnetic effect at any point, and in particular their reaction on the electro-magnetic system which gave rise to them, are determined. The result is presented in a simple form by the aid of the principle of images which was first applied to problems in electricity and hydrokinetics by Sir W. Thomson. The essential part of this principle is that we conceive the state of things on the positive side of a certain closed or infinite surface (which is really caused by actions having their seat on the surface) to be due to an imaginary system on the negative side of the surface, which, if it existed, and if the action on the surface were abolished, would give rise to the actual state of things in the space on the positive side of the surface. The state of things on the positive side of the surface is expressed by a mathematical function which is different in form from that which expresses the state of things on the negative side, but which is identical with that which would be due to the existence, on the negative side, of a certain system which is called the image. The image, therefore, is what we should arrive at by producing (stretching), as it were, the mathematical function, as far as it will go; just as in optics the virtual image is found by producing the rays, in straight lines, backward from the place where their direction has been altered by reflection or refraction. In the case of a plane conducting-sheet there is a moving train of images. If positive and negative images, according to the sides of the plane, are formed and started at given intervals of time, a train or trail will begin with a single positive image, followed by an endless succession of pairs of images. This trail when once formed continues unchangeable in form and intensity, and moves as a whole away from the conducting-sheet with a constant velocity. If the conductivity of the sheet were infinite, or its resistance zero, that velocity would be zero. The images, once formed, would remain stationary, and all except the last formed positive image would be neutralised. Hence the trail would be reduced to a single positive image, and the sheet would exert a repulsive force on the pole, whether the pole be in motion or at rest. This case does not occur in nature as we know it. Something of the kind is supposed to exist in the interior of molecules in Weber's theory of diamagnetism.

With the first day of this year the electric light was introduced at the South Foreland Lighthouse, which is situated between Dover and Deal. The position is of great value owing to its situation with regard to the opposite coast of France and the approaches to the North Sea and the mouthof the Thames. The completion of the works at the Foreland will form a triangle of electric lights described by those of Dungeness, Cape Griznez, and the South Foreland. The South Foreland towers are 449 yards apart, and their lights are respectively 372 feet and 275 feet above high water of spring tides. Buildings for the requisite machinery and apparatus for the production of the electric light have been erected midway between the two lighthouses. The electricity is generated by one of Professor Holmes's magneto-electric machines, worked by a small horizontal condensing engine. There are four of these engines, two being for the service of each lighthouse—one used ordinarily, but in times of fog both. They make 400 revolutions per minute, effecting with this speed an alternation in the direction of the currents of 6400 times in the same period. The electrical currents are sent from the machines by underground wires to the lantern of each lighthouse. The steam-engine, boiler, and magneto-electric machines are all duplicated in case of accident or want of repair to any part. The supply of water for the steam-engines is obtained from a well sunk through the chalk a depth of 280 feet to the high water level of the sea. This well, although the water is remarkably pure and free from salt, is curiously affected by the action of the tide. During each flood-tide the well is quite dry, but throughout each ebb-tide there is an abundant supply of water. The optical apparatus in each lantern is of the dimensions of a third order for fixed light, but has been especially designed and manufactured for the purpose of the electric light. From the high lighthouse 246 degrees of the arc surface is illuminated, and from the low lighthouse the arc illuminated is 106 degrees. The landward arc of the light, instead of being waste light to the mariner, is also utilised, and is in each case carefully gathered up, and by reflecting prisms arranged on each side of the main apparatus, equally distributed over the portion of the surface illuminated by the latter, thus adding very considerably to the power. Each apparatus is provided with an efficient oil-lamp as a further precaution in case of accident.

M. Trouvé has found a new application of electricity to medical purposes in his electric probe for bullet wounds. This probe consists of three distinct parts, the battery, the probe, and the indicator; and the adaptation is founded on the conductibility of metals. The battery is an ordinary zinc-carbon element, the exciting liquid being bisulphide of mercury. The probe is flexible or rigid, and is a conductor. The indicator has in its interior a very small electro-magnet with a vibrator and two small rods of steel, very sharp and insulated from each other; and as soon as these points, which are in connection with the battery, touch any metallic substance, the vibrator begins to move. The surgeon with this apparatus can even distinguish the different metals from one another. If the metal is lead, the trembler vibrates regularly; if, however, it is iron or copper, the trembler has a jerky movement. Iron may be distinguished from copper by its action upon the needle of a galvanometer.

Dr. Joule has given a note of some experiments on the polarisation by frictional electricity of platina plates, either immersed in water or in alternate series with wet silk. The charge was only diminished one-half after an interval of an hour-and-a-quarter. It was ascertained both in quality and quantity by transmitting it through a delicate galvanometer. He suggested that a condenser on this principle might be useful for the observation of atmospheric electricity.

Some investigations of a novel kind relating to the directive power of large steel magnets, bars of magnetised soft iron, and of galvanic coils in their action on external small magnets, have been brought forward by Dr. Airey, C.B., P.R.S., Astronomer Royal, and James Stuart, M.A. The experiments show the distribution of force in the sphere of magnets, and a marked difference was observed between the behaviour of an ordinary steel magnet and that of a galvanic coil without a core; for whereas in the instance of the steel magnet the focus lies within, and some distance from, the end of the bar, the focus of the coil was found to be at the centre of the end of the coil. It would be interesting to know if this be so, for it might be supposed that the focus is a little distant from the flush end, perhaps little more than one-twelfth part of the diameter of the coil's wire from it. The presumption would be that the rings of the coil have, unless clasping a core, each a magnetic action of their own, besides their combined action, and that it is the last ring which determines the focus alluded to. The apparatus used for the experiments is a bar magnet, 14 inches in length, placed, in one series, with its edge towards the small compass on which its directive power is estimated, and another series with its flat side towards the small compass; also a galvanic coil, 13·4 inches in length, animated by a battery of three cells, and in the same coil, with the insertion of a soft iron coil. In the field of experiment the earth's magnetism was sensibly neutralised by external large magnets. The observations with the small compass were taken in thirty marked stations in one oval ring surrounding the magnet, and in thirty-eight stations of another oval ring outside the first one. The following specific points are remarked:—At a constant distance from the steel the greatest force exerted by a *magnet* is not the longitudinal force at the end, but the transverse force near the end. In going round the magnet there are six maxima and six minima of force. The law of attraction of the *core* of a galvanic coil is not very different from that of a magnet. The force produced by the core within the coil is very much greater than that produced by the coil alone. In some positions of the small compass it is about forty times as great, and in some about *one hundred and seventy* times as great. The law of force at different parts of the *coil* differs greatly from that at corresponding parts of the magnet or core. In the coil it is proportionally far greater at the end, and its direction is different. Near the end of the magnet or core the directions of force converge to a point within it distant from the end by about one-twelfth of its length. Near the end of the coil the directions of force converge to a point as exactly as possible at the centre of the end of the coil.

M. Becquerel, in a paper "On the Chemical Effects of Caloric in the Action of Powerful Electric Discharges," states that the reduction of oxides of silver, lead, tin, and copper, can be effected by mixing these substances with charcoal dust in U-shaped tubes, and exposing them to the heat derived from the discharge of a powerful induction apparatus; the oxides of nickel, cobalt, iron, and chromium, mixed with charcoal powder and powdered sugar, and put in a platinum crucible, are also reduced. Silica and alum are fused, and small crystals are sometimes found in the fused mass.

In spite of the labours of Faraday and of L. Foucault, and in spite of the excellent arguments of these two great physicists in favour of the conductibility of liquids without electrolysis, this conductibility is not generally admitted. M. Favre has endeavoured to elucidate the subject, and has sought to show that, under some circumstances, the electrolysis of a liquid traversed by an electric current is impossible. He placed in circuit with a single Daniell's cell an electrolytic cell of sulphate of copper: after a quarter of an hour there was a deposit of copper in the pile, but on the negative plate of the voltameter there was found neither the metal nor its oxide. On the porous cup of the element there was also copper deposited. With the voltameter out of circuit and the circuit of the element open, copper was deposited on the porous cup only. And by several carefully considered experiments M. Favre shows that the volume of gas disengaged in the voltameter is by no means equivalent to the reduction and oxidation of the materials of the pile, thus proving that a portion of the current must have traversed the liquid without electrolytic effect. He finds, also, that three Smee's cells are needed to decompose sulphuric acid; it requires, it would seem, a force corresponding to 45,000 calories in order to electrolyse the acid.

Sir W. Thomson gives the following law for rendering galvanometers sensitive with a minimum of wire. The curve forming the transverse section of the coil is expressed by—

$$x^2 = \left(\frac{y}{a}\right)^{\frac{2}{3}} - y^2,$$

x being the abscissæ from the zero point, passing through the magnet; y, the ordinates; and a, a constant.

Prof. Osborne Reynolds, from some experiments on the electro-dynamic effect the induction of statical electricity causes in a moving body, has been led to the consideration of the induction of the sun as a probable cause of terrestrial magnetism. If an electrified body be placed near a moving conductor so as to induce an opposite charge in the moving body, this charge will move on the surface of the conductor so as to remain opposite the electrified body, whatever the motion might be. Suppose the moving conductor to be an endless metal band running past a body negatively charged, the positive charge would be on the surface of the band opposite to the negative body; and here it would remain, whatever might be the velocity of the band. The effect of the motion of this negative electricity on the conductor would be the same as that of an electric current in the opposite direction to the motion of the band. If the moving body consisted of a steel or iron top spinning near the charged body, the effect of the electricity on the top would be the same as that of a current round it in the opposite direction to that in which it was spinning. It might be that the electricity in the inducing body would produce an opposite magnetic effect on the top; but even if this were so, its effect, owing to its distance, would be much less than that of the electricity on the very surface of the top. If no account were taken of the effect of the inducing body, the current round the top would be of such strength that it would carry all the electricity induced in the top once round every revolution. If the top were spinning from west to east by south it would be rendered magnetic with the positive pole uppermost, that is, the pole corresponding to the north pole in the earth or the south pole of the needle. In one of the experiments, to show that such a current might be produced, a glass cylinder, 12 inches long and 4 inches across, was covered with strips of tin-foil, parallel to the axis, with very small intervals between them. These strips were about 6 inches long and ½ an inch wide, and the intervals between them $\frac{1}{200}$th of an inch. In one place there was a wider interval, and from the strips adjacent to this wires were connected, by means of a commutator, with the wires of a very delicate galvanometer. This cylinder was mounted so that it could be turned 1200 revolutions in a minute, and brought near the conductor of an electrical machine. This apparatus, after it had been thoroughly tested, was found to give very decided results. As much as 200° deflection was obtained in the needle, and the direction of this deflection depended on the direction in which the cylinder was turned, and on the nature of the charge in the conductor. When this was negative, the current was in the opposite direction to that of rotation. It may be taken as experimental proof of the fact previously stated, that, if a steel top were spinning under the inductive influence of a body charged with negative electricity, the effect would be that of a current round the top such as would render it magnetic. The cause of terrestrial magnetism has not been the subject for so much speculation as many other more unimportant phenomena, being regarded as a cause from which other phenomena might result, but not, as itself, the result of other causes, and yet when two phenomena have a relation to each other there is good reason for believing them connected in some way, either one being derived from the other, or else both springing from the same cause. The direction of the earth's magnetism bears a marked relation to the earth's figure, and yet it can have had no hand in giving the earth its shape, which is explained as the result of other causes. Assuming that the figure of the earth has something to do with its magnetism, its rotation, which keeps it in shape, causes it to be magnetic. We are led to believe the cause of this magnetism to be associated with the sun, from the influence it exerts on this magnetism, although the cause itself cannot be the result of either the sun's heat, light, or attraction. The analogy between the magnetism produced by a spinning top by the inductive action of a distant body charged with electricity, and the magnetism in the rotating earth, probably caused by the influence of the sun, which influence is not its *mass* or heat, seems to suggest what its influence really is. If the sun were charged with negative electricity, it seems to follow, from Prof. Reynolds's experiments, that the inductive effect upon the earth would be to render it magnetic, the poles being as they are.

TECHNOLOGY.

Ordinary brick-dust, made from hard-burnt, finely pulverised bricks, and mixed with common lime and sand, is universally and successfully employed as a substitute for hydraulic cement in the Spanish American dominions. It is regularly known in Cuba as an article of commerce, and sold in barrels, by all dealers in such articles, at the same price as cement. The proportions used in general practice are one of brick-dust and one of lime to two of sand, mixed together dry, and tempered with water in the usual way.

For curiosity's sake we quote the following account of a series of experiments made on January 27th last at Fort de Montrouge, in the presence of His Majesty Don Pedro II., Emperor of Brazil, for the purpose of testing dynamite:—"An iron-hooped, well-made, oaken cask, of 2 hectolitres (rather more than 44 gallons) cubic capacity, was placed in a vertical position, then filled with water, and a square hole left in the lid, the size of the hole being sufficient to admit of throwing through it a parcel of four cartridges, each containing 20 grms. of dynamite, care being taken to light the fuses previous to casting the cartridges into the water. After the explosion not a trace even of the cask was to be seen, and on the spot where it had been standing a funnel-shaped hole was formed, 4 decimetres (15·748 inches) deep."

According to Dr. Sézille's new process of panification, which is spoken very highly of on the Continent, the wheat is first deprived of its outer cover, or husk, by means of properly constructed machinery; the decorticated grain is next acted upon several times by tepid water (about 80° for the first bath and 40° for the subsequent ones), whereby the gummo-resinous cover of the grain is dissolved and removed. This removal is necessary on account of the fact that this substance becomes very deep brown, almost blackish, coloured by fermentation of the dough; the grain at the same time absorbs from 65 to 70 per cent of water, and is then reduced to a paste by means of machinery very similar to that used in chocolate mills. This perfectly white paste is next leavened, and after fermentation is ready for baking. By this process, from the same quantity of grain which by the usual process only yields 108 to 110 kilos. of bread, the yield is increased to 145 kilos. of very superior quality and far greater nutritive power; moreover, a very considerable saving of labour and expenses connected therewith is effected by the application of this new process, which has been thoroughly tested by competent and independent scientific as well as practical men.

M. Deligny's process for the preparation of pyrophosphate of lime for agricultural purposes is as follows:—The coprolite, or other native tribasic phosphate, or bone-ash, is first treated with hydrochloric acid, and the mixture thus obtained dried in a reverberatory furnace, leaving a pyrophosphate of lime, which, according to the author, contains 55 per cent of phosphoric acid, while the hydrochloric acid used is—at least in a great measure—recovered. The reaction is based upon the fact observed by the author, that from a mixture of acid phosphate of lime and chloride of calcium, when heated to 100°, the hydrochloric acid is driven off by the acid phosphate, leaving a bibasic phosphate, which is very readily assimilated by plants.

Dr. Louvel has experimented on the large scale on a process of preserving grain, ship-biscuits, and flour, and especially preventing these articles being damaged by insects, rats, and mice, by placing the same into strongly-made iron (boiler-plate) vessels of sufficient capacity to contain 10 cubic metres (27·512 bushels) of grain or flour, and next, after having hermetically closed the man-hole of this vessel, producing a vacuum in it by the aid of an air-pump, it being sufficient for practical purposes that the vacuum be from 10 to 12 centimetres of mercury, that is, a reduction of about from one-sixth to one-seventh of the ordinary atmospheric pressure. The results of these experiments (sufficient time having been amply given to test their real value) is satisfactory in every respect. The cost of the apparatus, including air-pump, gauges, tubing, and fittings, is about £64, but one air-pump can be used for a number of these vessels. It should be observed that the quantity of grain lost or rendered unfit for human food by the ravages of insects, rats, mice, worms, &c., amounts on an average to fully 13 per cent of a crop, while, moreover, by this mode of preserving, much labour required for shovelling grain about in the granaries is rendered unnecessary.

CHEMISTRY.

Dr. S. Meunier states that when a piece of meteoric iron is fixed to the positive pole of a Bunsen battery, the other pole being a piece of silver, and the electrodes are placed in an aqueous solution of bisulphate of potassa, and contact made, there will appear on the iron an iridescent colouration similar to the colouration brought about by heating the polished iron; if no contact takes place, there is produced upon the iron a figure similar to that which acids produce, but by this method it is more readily and more neatly brought out.

Professor J. Lawrence Smith has given a detailed description and analyses of the large masses of meteoric iron in North Mexico, with the description of a new mass—the San Gregorio Meteorite. This immense mass of meteoric iron is situated on the western border of the Mexican desert. Its shape bears some likeness to that of a blacksmith's anvil. Its greatest length is 6 feet 6 inches; it is 5 feet 6 inches high, and 4 feet thick at its base. On one part of its surface "1821" is cut with a chisel, and above this date is the following inscription:—"Solo dios con un poder este fierro destruerâ, por que el en mondo non habra quien lo puedo deschacer" (Only gods can have the power to break up this mass of iron, for there is no body on earth who can break it to pieces). The weight of this mass is nearly 5 tons; it lies near to the spot where it has fallen, but nothing is further known of its history. A small detached specimen of this stone, which is of the softer meteoric iron (sp. gr. = 7·84) gives the following chemical results:—Iron, 95·01; nickel, 4·22; cobalt, 0·51; copper, minute traces; phosphorus, 0·08. The Mexican desert has an extension of 400 miles from east to west, and 500 miles from north to south, equal to an area of 200,000 square miles. The United Kingdom has an area of 121,518 square miles.

In works on chemistry, indigotine (pure indigo blue) is described as a body insoluble in water, alcohol, ether, fatty and essential oils, and dilute acids and alkalies; yet strong and boiling alcohol, and even somewhat more so methylic alcohol (not methylated spirit), dissolve enough indigotine to become blue-coloured, but, on cooling, the greater part of the dissolved substance is thrown down again. By experimenting with phenic acid, M. C. Méhu has found that this substance is an excellent solvent for indigotine, which may be extracted readily and in pure state from indigo, care being taken to wash this latter substance first with water, then with dilute hydrochloric acid, and next several times with boiling alcohol: in order to prevent the solidification of phenic acid on cooling (heat has to be applied to dissolve the indigotine), some camphor may be added to it. With 500 grms. of phenic acid, 2 grms. of indigotine may be readily obtained in one operation, and in very pure well-defined crystals. Indigotine is also soluble, to some extent, in phenic acid when cold, this solution exhibiting a very deep purple colour.

Mr. C. Bullock has published the results of a few experiments to solve approximatively the question, "What amount of acids or alkalies is necessary to give a distinct change of colour to the test-paper?" Blue litmus-paper should be distinctly blue, but not a deep shade, in colour; the directions given by Dr. Fresenius in his "Qualitative Analysis" will afford a sensitive paper. When carefully made it affords the reaction with one drop of acetic acid at 30 per cent in the following quantities of water:—In 4 ozs., it turns red immediately; in 6 ozs., completely red in half a minute; in 10 ozs., changes on the edge in one-fourth minute, and is completely reddened in 1 minute; in 13 ozs., it is completely red in 1½ minutes, and remains red when dry; in 16 ozs. of water the limit of distinct reaction is found. Reddened litmus should have a purple-red colour, and the paper when dry a distinct red colour free from blue. With 1 grain of anhydrous carbonate of soda in 32 ozs. of water the paper turns blue in 1 minute; in 56 ozs., in 3 minutes; in 64 ozs., in 4 minutes; in 80 ozs., in 7 minutes; in 160 ozs. of water the limit of distinct reaction is found; the blue shade can be seen before the colour is dissolved from the paper. In these experiments the paper was submerged in the liquid.

QUARTERLY LIST OF PUBLICATIONS RECEIVED FOR REVIEW.

Index of Spectra. By W. Marshall Watts, D.Sc. With a Preface by H. E. Roscoe, B.A., Ph.D., F.R.S. *Henry Gillman.*

A Synonymic Catalogue of Diurnal Lepidoptera. By W. F. Kirby. *J. Van Voorst.*

The Laws of the Winds prevailing in Western Europe. Part I. By W. Clement Ley. *Edward Stanford.*

Geology of Oxford and the Valley of the Thames. By John Phillips, M.A., F.R.S., F.G.S. *Oxford: Clarendon Press.*

Theory of Heat. By J. Clerk Maxwell, M.A., LL.D., F.R.SS. L. & E. *Longmans & Co.*

A Manual of Zoology for the Use of Students. By Henry Alleyne Nicholson, M.D., &c. Second Edition. Revised and Enlarged. *William Blackwood & Sons.*

Technical Arithmetic and Mensuration. By Charles W. Merrifield, F.R.S. *Longmans & Co.*

A Dictionary of Chemistry. By Henry Watts, B.A., F.R.S. Supplement. *Longmans & Co.*

The Origin of Species by Means of Natural Selection; or, the Preservation of Favoured Races in the Struggle for Life. By Charles Darwin, M.A., F.R.S. *John Murray.*

A Chart of the Northern Hemisphere of an Equal-surface Projection, with a Key-map on the same Projection. By R. A. Proctor, B.A., F.R.A.S.

A New Star Atlas. By R. A. Proctor, B.A., F.R.A.S. *Longmans & Co.*

Astronomy and Geology Compared. By Lord Ormathwaite. *John Murray.*

Dr. Pereira's Elements of Materia Medica and Therapeutics. Edited by Robert Bentley, M.R.C.S., F.L.S., and Theophilus Redwood, Ph.D., F.C.S. *Longmans & Co.*

A Treatise on the Theory of Friction. By John H. Jellett, B.D. *Macmillan & Co.*

Elementary Natural Philosophy. By J. Clifton Ward, F.G.S. *Trubner & Co.*

Experimental Steam Boiler Explosions. By Professor R. H. Thurston. *Journal of the Franklin Institute.*

The Natural History of the British Diatomaceæ. By Arthur Scott Donkin, M.D. *John Van Voorst.*

The Year-Book of Facts in Science and Art. By John Timbs. *Lockwood & Co.*

An Elementary Treatise on Curve Tracing. By Percival Frost, M.A. *Macmillan & Co.*

Science Primers—Physics. By Balfour Stewart. *Macmillan & Co.*

Science Primers—Chemistry. By H. E. Roscoe. *Macmillan & Co.*

Monograph of the British Graptolitidæ. By H. Alleyne Nicholson, M.D., &c. *William Blackwood & Sons.*

New Theory of the Figure of the Earth considered as a Solid of Revolution, founded on the Direct Employment of the Centrifugal Force instead of the Common Principles of Attraction and Variable Density. By William Ogilby, M.A. *Longmans & Co.*

Corso di Geologia del Professore Antonio Stoppani. Vol, i., Dinamica Terrestre. *Milano: G. Bernardoni.*

Erratum.—In No. 33, page 9, line 26, for "*quartzose cells*," read "*quartzose celts*."

THE QUARTERLY
JOURNAL OF SCIENCE.
JULY, 1872.

I. THE MUSIC OF SPEECH.

By the Rev. R. William Higgs, Oxford Scholar.

OF all the gifts of God to man, the most godlike, that of rational speech, is universally the most neglected. The neglect is without extenuation, and particularly in the matter of our native tongue. A sonorous and yet easily modulated language, English offers many advantages to the elocutionist. The language as a language has been studied most thoroughly. So much might be said of the languages of all civilised countries. But can we go a step further and say that speech has been so far studied that anyone of moderate attainment in science can explain how sentiment, logical continuity, or logical conclusion are expressed in elliptical sentences or phrases,—can tell how a "yes" may imply negation or a "no" affirmation? Certainly not. Yet we send our sons to school or college to imbibe what is generally understood as the "*je ne sais quoi*" of intonation without thought as to whether or no that peculiar and admired style is imparted upon principle or only by imitation. Need it be said how often imitation is too late discovered to have been the routine pursued. And what are the consequences? The boy completing his education enters a profession or the counting-house, and in addition to his own mannerism falls into those most prominent among his fellows. Thus he obtains a peculiar intonation, pleasing or the reverse—thus is there one set tone pervading certain cliques of society, too hackneyed a subject for re-consideration here.

We Englishmen, or those of us who cry so loudly for "progress," should be proud that the discovery that speech has a peculiar music of its own is due to a member of an English-speaking nation. Dr. James Rush, of Philadelphia, in his "Philosophy of the Human Voice," has shown that the sentiment and the logic of our speech have a distinct mode of expression apart from the subject-matter—has, in fact, discovered the true meaning of "correct intonation." His views and researches have not been published to-day; in America the work has reached the sixth edition; here it is comparatively unknown. In the following pages, then, will be attempted an epitome of Dr. Rush's investigations, showing that a standard of correct elocution is no longer a matter of arbitrary taste, but has passed into the defined and law-ruled realms of science.

The constituents of the human voice may be referred to the five following modes:—

Quality,
Force,
Time,
Abruptness,
Pitch.

The quality of a voice is sufficiently distinguished by the metaphorical terms—rough, smooth, harsh, full, thin, musical. This mode does not require any elucidation. "Even in simple conversation," says M. Garcia, "if the intention be to represent anything extensive, hollow, or slender, the voice produces, by a moulding movement, sounds of a corresponding descriptive character."

There is a peculiar quality pertaining to the voice of an actor or orator of long practice, ordinarily described as "roundness of tone," the *ore rotundo* of Horace. This orotund quality may be acquired without that long and wearying toil to which the professional speaker must submit, as, indeed, may be acquired any vocal mode by diligent analysis. First we notice in this quality of voice a resonance similar to that possessed by a musical instrument, the tone of which has become mellow by age. Again we notice that in coughing a certain fullness of tone can be sometimes recognised in the short percussive vocalities. The resonance of the musical instrument arises from a definite relation of the vibrating body of air within the instrument to the number of vibrations made by the reed or string per second. In speaking into a vase there are some notes of the voice which find, as it were, an answer, and are produced with greater sonority. When in coughing we contract portions of the pharynx, this resonant cavity of the mouth then contains a body of air capable of being set in vibration by the tone uttered; the result is a peculiar fullness or breadth amounting occasionally to hoarseness. The vocality of the cough has the duration only of the passing breath; keeping the organs in their assumed position, and prolonging the breath, we obtain the desired orotund quality of voice. We have artificially placed our vocal organs in the position assumed by those of an experienced speaker. We have, too, found that there are two distinct forms of respiration—the one continuous, in which the orotund is first produced; in the other the breath issues in interrupted jets. In continuous breaths we sigh, groan, and sneeze. The interrupted jets of breath are employed in speech, in laughing and crying. Having acquired a continuous orotund vocality—a child first cries in long wails—the student must learn to interrupt this continuity and to speak in the orotund voice, soon as easy to him as his colloquial speech. Thus a thoughtful investigation gives immediately that which in long years even assiduous practice may fail to accomplish by mere imitation.

The terms usually employed to denote variation in degrees of force are also self-descriptive.

Mr. Millard,* who with Professor Bell, of University College (Lond.), has devoted considerable attention to the scientific study of vocality, very clearly subdivides the time or duration of the voice into—

1. Quantity or sound heard;
2. Rate;
3. Pause or rest.

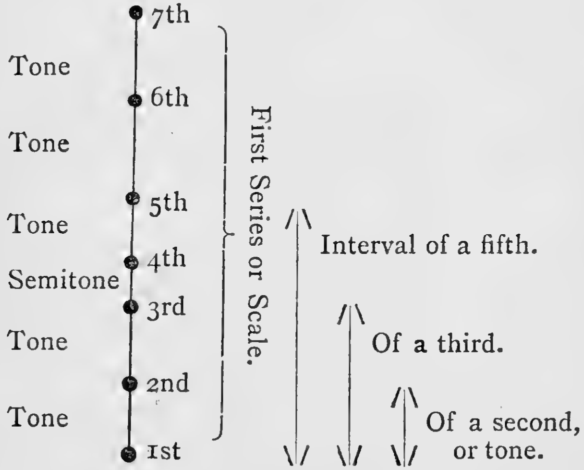
"By quantity is meant the length of sounds heard separately; by rate the pace of sounds heard in succession; and by pause the measure of silence or rest." Mr. Steele, in his "Prosodia Rationalis" (1779), gives examples of an application of the symbols of music to his idea of the time of discourse. But these are constituents of speech each worthy a separate volume to consider and exemplify.

Exception might, perhaps, be taken as to the correctness of giving a specific division to abruptness; but Dr. Rush thinks this a mode of the voice quite distinct from that of force. It is perhaps a delicate point. Abruptness is analogous to the *staccato* of the musician. We come now to the most subtle and the least understood constituent of speech, variation in pitch or the variations of the voice between gravity and acuteness. And here we must assume that the reader has a slight acquaintance with the intervals of the notes of music. "As music and speech," says Dr. Rush, "when regarded under the mode of pitch, are subdivisions of the general science of tunable sound, the reader will perceive the necessity of designating and explaining

* A Grammar of Elocution. Longmans.

those terms which belong alike to both, or are restrictively appropriated to each." The different degrees of pitch in music are denoted by what is termed the *scale*, the relation of which to speech may be thus illustrated. When the bow is drawn across the string of a violin, and the finger at the same time gradually moved, with continued pressure on the string, from its lowest attachment to any distance upwards, a *mewing* sound, if it may be so termed, is heard. This mewing is caused by the gradual change from gravity to acuteness, through the gradual shortening of the string: and as it thus rises by a succession of uninterrupted momentary changes, each continuous or concreted, as it were, in its increments of time and motion, let it be called a *concrete* sound. This movement of pitch on the violin is termed a *slide*.

The reader may himself exemplify this concrete sound by uttering the single syllable *aye*, as if he were asking a question with the expression of earnest surprise, yet rather deliberately, beginning at the lowest and ending at the highest limit of his voice. The gradual rising-movement in this case is concrete; yet as the voice, and any other tunable sound may be continued in one interrupted movement upon the *same line* of pitch, without rising or falling, it is proper to remark that the term concrete is in this paper applied only to an uninterrupted movement in a *rising* and in a *falling* direction. Now, the sounds of what is called the *scale* in music do *not* rise by connected or concrete movements; but are made by drawing the bow only while the finger is held stationary at certain successive places on the string, thus showing an interruption of the continuous upward slide. These places are seven in number; their distances from each other being determined by a natural law, and rendered precisely measurable by a scientific rule for subdividing the string, which we need not consider here. Other sounds, still ascending on the string above these seven, may be made by a similar interrupted progression. But since the second series of seven sounds, though of higher pitch, yet adjusted by the same rule, do each to each in order so nearly accord with the first seven that they may be considered as a kind of repetition of them, and as the same is true of all the series of seven that may be formed between the lowest and the highest limit of sound, the whole extent of variation in acuteness and gravity is regarded as consisting of the simple scale of seven sounds, repeated in different series or ranges of pitch. Thus, while we take the line in the annexed diagram to represent the concrete progress of the voice, the positions of the notes, represented by the black discs, will distinguish the discrete progress in singing or on the pianoforte. While the progress between two notes of music may be represented by two discs,  the progress of the voice in speaking may be well represented by  or more elegantly thus— , denoting the full, rotund opening and the delicate attenuation of the inflection. This explanation of the manner of concrete and discrete progressions in an upward direction is to be understood of the downward course, under a reverse movement of the gradual slide on



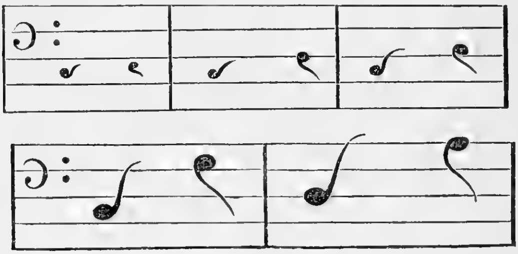
the string. The variations of pitch on most musical instruments are discrete. The violin and its varieties derive much of their expressive power from being susceptible of the concrete movement, this movement being one of the great sources of expression in the human voice. Having thus pointed out the distinction between the discrete movements of the voice in singing and the concreted movement, also termed the slide or inflection in speaking, we may at once proceed to analyse these movements of the speaking voice, and to ascertain their corresponding mental conditions. We shall see that each mental state—enquiry, surprise, assertion, command, remonstrance, threatening, scorn, and sarcasm—has a corresponding mode of inflection, these modes collectively forming a natural language intuitive alike to all. It is the perversion or disconnection of the tone of voice from its corresponding mental condition that makes ordinary reading aloud "like sweet bells jangled, out of tune and harsh." "A *natural* expressiveness," says Professor Bell, "may and should be given even to the A B C."

Inflections are either—

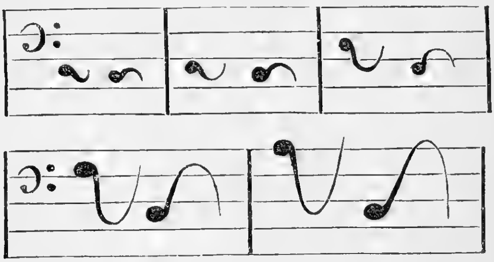
1. Simple; or,
2. Compound,

both forms being either rising or falling. A simple inflection consists of a single slide of the voice in either an upward or downward direction; thus, / or \. The compound inflection is obtained by uniting two or more simple inflections; thus, /\ or \/. When the last interval is an upward inflection the compounded inflection is said to be rising; and when the reverse is the case the compound inflection is said to fall. The expressiveness is in all cases limited by the interval through which the voice passes.

Simple Inflections.



Compound Inflections.



It is clear that the limbs or constituents of a compound inflection may be equal or unequal. Of the multiform permutations of the several inflections we cannot here give an account, nor is it necessary; for let once the principle be grasped, and the deduction of specific rules is an easy matter. Mr. Millard says:—"The fundamental law of inflection, under which, indeed, nearly all the rules written by elocutionists may be included, is as follows:—The rising inflection is indicative of doubt and incompleteness of expression; the falling, of certainty and completeness of expression; the *length* of the slide varying in accordance with the speaker's intensity.

Thus, if I put the question, "Do your know your lesson?" to a boy confident in his knowledge, he would give his affirmative with one of the falling intervals:

\
Yes.

And here the verbal and vocal sign would justly and expressively coincide. If, on the other hand, he were doubtful, though affirming by the *word*, the rising inflection, which by the natural law he would employ, would overrule the verbal sign and betray his doubtful state of mind:

/
Yes.

From this general law all specific rules for the management of the voice may be deduced. Thus, with regard to questions which, in their purely logical form apart from sentimentality, may be considered as conveying neither affirmation nor denial, the meaning being evolved from the answer, the inflection is naturally a rising one, the lowest limit of the interval being a major third. This moderate degree of inflection is, however, rarely to be met with in ordinary conversation, sentiment or a false habit of intonation extending the interval employed to a fifth or even more. And here arises one of the causes of failure of an untrained speaker on a public platform—he having at most only an octave and a third or fifth at his command, and seldom even this, in using the extended intervals to convey ordinary meaning, leaves literally no room for effect; and in rendering impassioned passages requiring relatively longer inflections, degenerates into a scream or a monotonous growl. Perhaps the day may not be far distant when from all professorial chairs of elocution students shall be taught to restrict their mere assertion within proper vocal limits, leaving the remaining notes of their voices to the cause of expression.

Questions are sometimes assertions in disguise, and may be regarded as, or even more than, assertions; for instance, "*Who said so?*" may be fully rendered (with the downward inflection), "*Tell me who said so.* Finally, the belief implied in a question may be overruled by the passion with which it is asked. Thus, where Byron says, "*Shall he expire, and* *unavenged?*" the belief of the speaker is accompanied with anger, indignation, and other states of mental excitement.

Imperatives will of course take a downward direction, as also will exclamations. In the latter case how often do we hear on the amateur platform the exclamation of Macbeth, *What hands are here!*" rendered with the rising inflection as a question. We cannot here dwell upon the numerous rhetorical forms of speech—a little thought on the reader's part will soon enable him to class them as being subject to a continuative rising or a decisive falling inflection, bearing in mind the direction of one of our oldest poets—

> "The wise Plato sayth, as ye now rede,
> The word must need accorden with the dede;
> If men shall tellen proprely a thing,
> The word must cosin be to the werking."

The simple musical intervals and their correlated mental states may be recounted as follows:—

The Tone.—This interval should be considered the interval of unimpassioned speech; for, as we judge of all musical intervals relatively, it matters little what we adopt as a unit. However, from the peculiar nature of the musical scale, the tone only is the unit that can be adopted in rational speech. If this be done the rising tone will be indicative of continuity, and the falling tone of the approaching completion of the sense. Finally, three successive falling tones indicate the completion of a merely assertive sentence.

The Third.—Two tones, or a third, is the musical interval correlated to that state of mind which may be termed moderately assertive or appellatory. As has been said, the interval is naturally employed (either in three successive syllabic tones or otherwise) in the completion of an assertion.

The Fifth.—Falling; this interval denotes command, exceeding confidence, wilfulness, or petulance; rising, it appeals in surprise or passion. While the *octave* is expressive of still more passionate feelings.

The reader may exemplify to himself these intervals by the utterance of the following formulated phrases:—

Rising:

3rd.	5th.
Is it gone?	Can it be gone?

Falling:

3rd.	5th.
It is gone.	It must be gone!

There is yet another interval, *the semitone*, which, as can be most easily exemplified by repairing to the pianoforte, is expressed of plaintiveness or melancholy. "Pity the sorrows of a poor old man" is a sentence that to be read with effect should be read entirely in semitones.

We must now pass to the consideration of the complex or compound inflections correlated to complex states of mind. Perhaps there is no better proof of the truth to nature of the system of musical inflection (if we can call that a system which is really natural) than is afforded by an analysis of these complex intervals. They will be found to consist of united simple inflections, being thus antithetic in their nature. A complex interval is employed only in reference to some matter previously understood. For formulated expressions conveying an idea of the movements of the voice the student of speech is indebted to Professor Bell, whose endeavours to render speech graphically were long ago recognised by the Society of Arts. He exemplifies the compound tones as follows:—

Rising:

3rd.	5th, or Octave.
(Remonstrance)	(Threatening)
Not—	Certainly not—

Falling:

3rd.	5th, or Octave.
(Scorn)	(Sarcasm)
But—	Or rather—

the dash being supplied by the accented word. Let the reader make a few trials with the word "so" or "thus;" his ear will soon be enabled to measure the intervals.

This measurement of the intervals is the great difficulty of the system; a certain but essentially slight musical training is necessary to enable the student to cultivate his own voice or judge of the intonation of others; yet the difficulty is so small, and the consequent rewards so great, that one would not imagine it to impede the progress of the analytic study of the voice. This would, however, appear to be the case, since teachers of elocution delight more in the enunciation of arbitrary rules than in the enquiry how Nature provides for the utterance of the thoughts of man.

Science has advanced so far that it may be stated almost generally that that which is evident to one sense may be also made evident to another. In Leon Scott's phonautograph, and in König's manometric flames we have two beautiful illustrations, where speech, as sound evident to the ear alone, is stopped in its vibratory path and made to record itself to the eye.

These instruments are now too well known to need description here; it may perhaps suffice to say that the author has with these instruments analysed visibly many of the complex modulations of speech, reducing them to the principles first evolved by the fertile brain of Dr. Rush.

Of the remaining constituents of expressive speech, such as emphasis, full or median stress, &c., it is here needless to speak—they are sufficiently well known to students of music in their occurrence in the singing voice. But there is an almost distinct character—it cannot be termed a constituent—of beautiful speech upon which we must dwell. It is *rhythm*. And again the English reader meets with assistance from the character of his language. With the French language the case is different. It has, indeed, a perceptible variation in the force of its accents, and in the duration of its quantities; but not sufficiently marked, nor of such a systematic character as to make an available prosodial meter. The French epic and dramatic lines, for they cannot be called prosodial measures, properly consist each of twelve syllables, though they have sometimes ten or eleven. Among them is occasionally found a succession of accent and quantity resembling the various structures of English verse. There is an example of anapæstic measure in the first canto and second line of Voltaire's *Henriade*, "*Et par droit de conquête et par droit de naissance.*"

Allowing for the manner of the French in prolonging their syllables many like correspondences to the usual English measures may be gathered from what they call their heroic rhyme. But all such cases are accidental in French versification, and do not accord with the general character of its irregular succession—a succession shocking to the English ear, and utterly without a flowing rhythmus either as poetry or prose. Speech would not be convenient for the interchange of thought and passion if *every* syllable of every word were successively accented. For by this uniform accentuation it would want that vocal light and shade, and that pronounced relief required for a distinct picture of ideas —words, and consequently ideas, would not be easily distinguished from each other, and speech would be inconveniently slow. Whether this slowness would result from the hiatus in passing from one syallable to another, each with a full radical stress upon it, we need not here inquire. Thus, to the alternation of strong and weak accent, with the variations of long and short quantity, is ascribable much of the power and beauty of speech. This being the character of the accentual function, Mr. Steele, by an original view of the relations between accent, quantity, and pause, made a division of the line of speech, analogous to that of the bars of musical notation. These may be called accentual sections. We will attempt to explain part of the system of Mr. Steele by the following sentence, using italics in place of his symbol for the accented syllable, and numbering the sections merely for reference:—

(1) ⌉ In the | (2) *sec*—ond | (3) *cent*—u-ry | (4) ⌉ of the | (5) *christ*—ian | (6) *e*—ra |
(7) ⌉ the | (8) *em*—pire of | (9) *Rome* | (10) ⌉ compre- | (11) *hend*—ed the | (12) *fair*—est |
(13) *part* of the | (14) *earth* ⌉ | (15) ⌉ and the | (16) *most* ⌉ | (17) *civ*—i-lised | (18) *por*—tion |
(19) ⌉ of man | (20) *kind.* |

Mr. Steele first assumes the time of the several bars to be equal, like that of the bars in music, the term bar meaning not the vertical lines but the space between them. He next subdivides a sentence into bars, each of equal time, that time consisting either altogether of verbal sound or of a verbal sound and of a silent time or pause. Supposing, then, a bar or accentual section to contain in its verbal time, one, and never more than one, accented syllable, or heavy *Poize,* as he calls it, and one or more unaccented, which he calls the light *Poize;* the beginning of the bar is always occupied by the heavy accent, and the end by the light, or in their absence by a respectively equivalent silent time or pause (⌉). In the first bar of the above example there is no heavy accent, for the sentence begins with two light syllables, but its time is indicated by the symbol of a silent pause; while the two light are set at the end of the accentual section. The word *second,* in the next bar, has a heavy syllable followed by a light one, and thus makes a full and audible time. In the third bar, the word *century* has a heavy, followed by two light syllables. The fourth has the same time in syllable and pause as the first. And so on. It is worthy of remark, that if this sentence is read without its linear divisions, the voice of a good reader is disposed to make its pauses in those very places, and of that duration, visibly indicated by the vertical lines placed before the accented syllable and by the symbols of pauses both in the light and heavy part of the bar. It will, perhaps, be asked here—What is the meaning of these divisions? And what useful purpose they serve in instruction? All works on elocution, before the time of Mr. Steele, recommended the accurate accentuation of words, and a strict attention to their separation at the proper places for pausing. And although Mr. Sheridan has given particular cases of notation for rhetorical emphasis and for pause, he has proposed no broad rule to direct a pupil on these points, as Mr. Steele has done, in his simple divisions by bars placed before the heavy accent. The importance of the subject in our early schools may be learned from the manner in which children begin to read; for their hesitating utterance, and their close attention to the single word, lead them to lay an equal stress on every syllable, or at least on every word. This habit continues a long time after the eye has acquired a facility in following up discourse, and in some cases infects pronunciation throughout subsequent life; as it is not till the tongue goes tripping, or rather halting, with its firm and tender step on words, that the ear becomes sensible of the use and beauty of accent.

Dionysius, of Halicarnasus, in a summary of the constituents of an elegant elocution, describes rhythmus, as supporting or "sustaining the voice;" and it must be admitted that a well-marked arrangement of the varying stress and quantity of syllables does sustain the voice, by keeping it from that careless staggering of speech, that running of words against each other, which by crossing and arresting the easy step of language, confuses and thwarts the expectation of both the ear and the mind. "From the pen of a person of fine rhythmic perception," continues Dr. Rush, "even a letter of business, with its enumeration of particulars, may flow with graceful variety, and terminate with decisive satisfaction to the ear; for the Greek idea of rhythmus sustaining the voice in discourse, applies not more to maintaining a rhetorical dignity, than to preserving common language from a loose and unmeasured rudeness."

Of the power of rhythm to confer freedom of speech none are perhaps more aware than those who undertake to cure stammering and stuttering, it being the corner-stone of their systems: the defined beat of a well-marked line can be made to coincide with the pulsation of the stutterer. When talking to a stutterer one will find occasionally very difficult combinations of letters uttered with perfect freedom, simply because the rhythm has not been interrupted. Very few who stutter in speech do so when singing.

The further details of the subject of rhythmus must be left to the rhetorician. We have said enough to show that, in the words of the great Vandenhoff, "The human voice is to be considered as a musical instrument—an organ constructed by the hand of the Great Master of all harmony. It has its bellows, its pipes, its mouth-piece; and when we know the 'stops,' it will discourse most eloquent music. It has its gamuts, or scales of ascent or descent; it has its keys or pitch, its tones, its semi-tones, its bass, its tenor, its alto, its melody, its cadence. It can speak as gently as the lute—'like the soft south upon a bed of violets'—or as shrilly as the trumpet; it can tune 'the silver sweet note of love,' and the iron throat of war. . . . The art that wins this music is elocution."

II. A FEW THOUGHTS ON THE DECIMAL SYSTEM.

THE decimal system of notation is the system in vogue not only in England but also in most, if not in all, countries. The fact that all nations count by tens would seem to point to some one and the same cause influencing all nations alike. This cause is probably found in the fact that man has ten fingers. It is very common to indicate any number by holding up the same number of fingers. This is observed in our own country, and many a man in a strange land, with the language of which he was not familiar, has noticed the same method of explaining what number has been mentioned.

Now man having *only* ten fingers, when any larger number is to be denoted by this method all the fingers are held up, the hand is then closed, then all the fingers are again held up, and so on until the requisite number of tens has thus been indicated, when as many fingers are displayed as may be required to denote the units.

Thus the simple fact that man has ten fingers seems to have settled what is really a question of some importance, viz., the scale of notation to be employed by the whole human race. It seems questionable whether the decimal system is really the best; indeed, many who are considered the highest authorities on the subject seem to incline in favour of the duodecimal scale, or counting by twelves. One argument in favour of this scale over the decimal is that the radix 12 is exactly measured by the numbers 2, 3, 4, and 6, besides itself and unity, whereas 10 is only thus measured by 2 and 5.

It does not, however, seem at all probable that we shall change our present system of notation, at all events for some very considerable time to come. The use of another system, however, presents no very great, and certainly no *insurmountable*, difficulties, as any one may easily convince himself. Let us take, for example, the duodecimal system. Here it will be necessary to introduce two new names, for we are now about to count by *twelves* instead of by *tens*. The first ten numbers can retain their old names, but the names of the other two, *eleven* and *twelve*, which belong essentially to the old system, must be changed. Instead of these let us take *maun* and *bar* respectively, and let us call the system of counting by twelves the "*bar*" system.* In writing in figures we should have to introduce two new symbols, viz, one for ten and one for eleven, or maun as we will call it. Under the bar system, twelve or bar will be represented by 10 in the same manner as ten under the decimal system. Let the symbol *ν* represent ten and *ε* stand for eleven.

Our system would then be as follows :—

One	1	Seven	7	Bar-one	11	Bar-seven	17
Two	2	Eight	8	Bar-two	12	Bar-eight	18
Three	3	Nine	9	Bar-three	13	Bar-nine	19
Four	4	Ten	*ν*	Bar-four	14	Bar-ten	1*ν*
Five	5	Maun	*ε*	Bar-five	15	Bar-maun	1*ε*
Six	6	Bar	10	Bar-six	16	Two-bar	20

and so on, two-bar-one 21, &c., up to three-bar 30, four-bar 40, &c., ten-bar *ν*0, maun-bar *ε*0, and one "*gross*" 100, or bar times bar. The word "*gross*," being convenient and already used in English to denote a dozen dozen, has been retained.

Then one gross and one 101, &c., one gross one-bar 110, one gross two-bar 120, &c., one gross ten-bar 1*ν*0, one gross maun-bar 1*ε*0, and so on, two gross 200, three gross 300, &c., up to bar gross 1000, which we will call "*one range*." A range of ranges might be called a bi-range, and so on a tri-range, &c. Anyone can frame a multiplication table, using the bar system in the same manner as is usually done in the decimal system, and anyone so doing will find that he will have no difficulty at all *so long as he confines himself*

* The word "*bar*," first suggested by the Hindoostanee word *barah*, meaning twelve, has been retained in consequence of its English signification, being used here in much the same sense as in the phrase "a bar of music." The word "*maun*" was selected merely in order to have a short word, and one which should sound differently from the name of any other unit.

to the bar system; but if he should wish to translate into the decimal system some slight difficulty would present itself, though of course the conversion from one scale of notation to another is a simple mathematical operation. What is to be noticed is, that there is no inherent difficulty in the bar system any more than in the decimal system of notation.

We will now examine the decimal system in reference to Weights, Measures, and Coinage.

Starting from the fact that the decimal scale of notation is in general use, and, *by reason of this fact,* the decimal system is the one best suited for employment in our weights, measures, and coinage; for in this manner all the so-called compound rules of arithmetic, and those depending on our present system of tables, such as reduction, are done away with, and other rules, such as interest, discount, &c., are much simplified.

Seeing, then, that there is such a manifest advantage in favour of a decimal system of weights, measures, and coinage, how comes it that we have not that system? Who originated the system, or rather the medley or absence of all system which is in use in England at present? Topsy's answer, when asked who made her, would be appropriate here—"I s'pect I grow'd. Don't think nobody never made me." This was certainly the case. The foot, like the *pied* in France, was the length of the foot of the reigning king; and it seems to be a well-ascertained fact that this length, *the standard length of the kingdom,* changed, in some cases at all events, from reign to reign, according to the actual length of the king's foot. Then, too, in measures and weights there were local usages in different parts of the country, and from these at last there grew up one standard system for the whole country; but many local usages had to be subjects of severe legislation before they were finally abandoned.

It now becomes a question whether the heterogeneous system of weights, measures, and coinage now in use ought to be continued, or whether we ought not to abolish it, and replace it by a decimal system.

We will first consider the question of the weights and measures separately. Just let us take a glance at what it is necessary for an Englishman to know if he would be thoroughly acquainted with the weights and measures of his own country.

We have three distinct tables of weights, viz., Avoirdupois, Troy, and Apothecaries' Weight. We have, it may be said, one table for the measure of Length, but in reality there are more, for the link, the chain, and the pole are never used except by surveyors, and the hand is only used in measuring horses, &c. Then, too, the Admiralty knot differs from the geographical mile, which differs again from the ordinary mile. Next we have square measure, which, introducing the squares of the numbers used in long measure, introduces greater confusion. This is still more the case in cubic measure, for here the cubes of those numbers come into use. Again, wine measure differs from ale and beer measure, and, lastly, we have dry measure. This leaves out of account such things as the weight of a sack of flour, the size of a square of flooring, the different kinds of tons or loads, &c.

We will now give an example to point out the absurdity of the present system :—You are asked, "Which weighs the most, a pound of gold or a pound of feathers?" At first you feel indignant at such an insult. "Why, of course, they weigh the same." You are told "No;" and on thinking the matter over you remember that gold is weighed by troy weight and feathers by avoirdupois weight; and then if you have a good memory you recollect, or if not, you find on reference to the tables, that the pound avoirdupois weighs 7000 grains, while the pound troy weighs only 5760 grains, so that the pound of feathers weighs more than the pound of gold. Your questioner proceeds further, and says, "Which weighs most, an ounce of gold or an ounce of feathers?" Remembering the last, you say, "Why! the ounce of feathers." But no, the pound avoirdupois is heavier than the pound troy, but the ounce avoirdupois is lighter than the ounce troy. Is it not ridiculous? Is it not disgraceful for the greatest commercial nation on the face of the globe?

Again, a hogshead or a puncheon of beer contains less than a hogshead or a puncheon of wine. Such differences as these have no use whatever, and are extremely inconvenient.

In speaking of the weights and measures at present in use in England, we have several times made use of the word "system." This has been convenient, but it should not be understood to imply that there is any uniformity in any particular table of weights or measures. With a small exception, to be noticed presently, no such uniformity exists. Thus in avoirdupois weight we have—

16 drams	make	1 ounce.
16 ounces	,,	1 pound.
14 pounds	,,	1 stone.
2 stones, or 28 pounds .	,,	1 quarter.
4 quarters, or 112 pounds	,,	1 hundredweight.
20 hundredweights . .	,,	1 ton.

There is certainly very little uniformity or rule about this, but we might have chosen worse examples.

The exception spoken of just now is a testimony borne to the worth of the decimal system by engineers, surveyors, &c. They used a chain of 22 yards or 66 feet, and this they divided into 100 links for their own convenience. It will be seen that a link is no exact number of inches. Its length is 7·92 inches. Taking the chain as the unit of what may be called "surveyors' measure," we have—

100 links make 1 chain.
10 chains ,, 1 furlong.

And in square or superficial measure—

10 square chains or 100,000 square links make 1 acre.

It is a case of rebellion from the existing system to the most convenient system for working with, viz., the decimal system. There are other cases of rebellion. Thus, the inch is not unfrequently divided into tenths and hundredths; and the metrical system has been adopted in some scientific works, notably in books on chemistry.

But on the subject of the inconvenience of our present system of weights and measures there is no need for long argument. We have all been schoolboys; and who is there who does not remember either in his own person or in the person of the majority of his schoolfellows the immense amount of trouble caused by the "Tables?" How often does the schoolmaster (supposing that he does not allow reference to the tables) hear the excuse, "Please, sir, I know how to do it, but I don't know my tables." Further, we very much suspect that it is scarcely necessary to go back to school-boy days. How many Englishmen are there who are perfect in all their tables? We appeal to every reader to judge for himself. It is to be feared that the proportion is very small indeed. If this state of things were inevitable there would be nothing to say against it: but it is far from being so.

There is a system already in use, not only by the nation which first employed it, but since then more or less completely adopted by several others. It is scarcely necessary to say that allusion is made to the Metric System introduced by the French.

The first idea of the metric system was to obtain one standard, that of length, which could always be reproduced by calculation, and so to avoid any difficulty as to the necessity of having a material standard which would be liable to be destroyed. In order to get this the length of the circumference of the earth was obtained from the measurement of an arc of a meridian, and this length was divided into 40,000,000 equal parts to obtain the unit of length, which received the name of "*mètre*." But as other measures of the circumference of the earth have given somewhat different results, it is plain that this first object of the metric system has failed. Not so the other objects. Having obtained the mètre, the standard of length, all other standards are derived from it, so that it is only necessary to keep *one* material standard, viz., the mètre.

For square or superficial measure we have the square mètre, but for the measure of land another unit is employed. Surveyors in France make use of a chain, a decamètre, or 10 mètres in length (about half our English chain), and the surface of a square decamètre is taken as the unit for measuring land, and is called an "*are*."

In solid measure the cubic mètre is used, or in measuring fire-wood, &c., the amount of wood which can be piled in cubic mètre is called a "*stère*." It will be seen that there is a difference. There is not a cubic mètre of wood, but the pile of wood, on account of the interstices, occupies a cubic mètre.

For dry and fluid measure the litre is the standard. It is the volume contained in a hollow cubic decimètre; or, in other words, it is the volume contained in a cube whose side is a decimètre or the tenth part of a mètre.

The gramme is the standard of weight, and is the weight of a cubic centimètre of distilled water at its maximum density.

The standard coin is the franc, a piece of silver weighing 5 grammes.

In the metric system the learning of tables is a very simple matter. It consists in acquiring the names of the different standards, and remembering that the Greek words, *deca*, *hecto*, and *kilo* must be prefixed to indicate respectively ten times, one hundred times, and one thousand times the standard; and that the Latin words, *deci*, *centi*, and *milli* prefixed to any standard signify the tenth, hundredth, and thousandth part respectively of the standard. This needs no comment; the facts speak for themselves.

A friend in England happened a short time ago to send me in India the following cutting from a local newspaper:—

"*The Metric System.*—The "Times," in discussing the proposed adoption of the metric system, says:—The essential inconvenience would be the utter subversion of all existing conceptions of length, magnitude, and proportion. At least nine-tenths of our fellow subjects are tied and bound to a multitude of common English facts, inextricably entangled with the soil, the village, and the town. They have a farm or garden of so many acres or roods; they dwell so many miles from the village, railway, or county town; they live by selling quarters of corn, bushels of potatoes, gallons of milk, or pounds of meat; their wives buy clothing for themselves and their children by the yard; they purchase the food of the household in pounds, and its luxuries in ounces; they measure their drink by pints, and their bread is bought in quartern or half quartern loaves. All these things are no more than names to people who never have occasion to consider any but large quantities; but to the middle and lower classes they are the elements of daily existence; they are the measures, not of mere pecuniary transactions, but of their resources, possessions, wants, and enjoyments. A quartern loaf, for instance, is not so much a thing of a given value; for its price may vary; but it is a known quantity of bread, good for so much food, and likely to last for such and such a time. The cost of a dress is a matter of choice, but not so its size. The rent of an acre of land is a variable quantity, but in the mind of every farmer an acre represents a score of associated realities. It will take a known time to plough, it will feed so many sheep, it will take so much manure, and so on. What, then, we are asked to do by Mr. J. B. Smith and his friends is to take all these people out of a world they know, in which all the dimensions, relations, and necessities of existence are familiar to them, and plunge them into a world in which everything will be strange to them, in which all their old bearings will be rendered obsolete, in which they will be puzzled every day to know where they are standing, how much they are eating and drinking, what they are selling, and what they are buying. How is a countrywoman to understand how many mètres she wants for her petticoat, or how many decagrammes of milk she must buy for her baby? If the measure could be enforced, the mass of the population would be placed, as it were, in a new country, with all the daily conditions of life uncertain."

I must first premise that I know no more of the argument of the writer in the "Times" than what I have learnt from this quotation, and any remarks that may be made in this article will necessarily refer *only* to this quotation.

The writer begins thus:—"The esssential inconveniences." From this we are, I think, justified in concluding that he goes on to state what he considers to be the strongest arguments against the adoption of the metric system.

In his next sentence it appears to me that there is at least one word too many, viz., the word "*inextricably.*" For, in other countries with measures and weights as much unlike the metric system as our own, with a people not a whit more enlightened, and as much tied and bound to a multitude of common facts, just as inextricably entanged with the soil, the village, and the town as is the case in England, the change to the metric system has been already made and with the greatest success.

In aid of his argument the writer brings forward instances, *all*, or at least *most*, of which are such as are constantly recurring, and which are just those things which will be most quickly learnt. Allowing that a man accustomed to buy his pound of tea *may* be somewhat confused when he goes for the first time after the introduction of the new system to make his customary purchase, can we suppose that this confusion will last long? Will he be slow in forming his estimate of the weight of a kilogramme or hectogramme, when he constantly has to buy a kilogramme or so many hectogrammes?

The quartern loaf forms a great part of the writer's bill of fare. It seems that "it is a known quantity of bread, good for so much food, and likely to last for such and such a time." Precisely, and so is a loaf which weighs a kilogramme "good for so much food, and likely to last for such and such a time;" and it will be hard to persuade most people that a housekeeper would be more than a few days in finding out exactly *how much* food the new style of loaf is good for, and exactly *how long* it is likely to last. It would then become *in only a few days* "a known quantity of bread." The same would apply to all other instances, which occur frequently, and the very rarity of the occurrence of others diminishes their importance. The summing up of this truly alarming quotation is appalling. There would evidently be a famine, or some dire calamity. "All the daily conditions of life uncertain:" only think of it!

It does not seem to have occurred to the writer that precautions might be taken to mitigate the evils which he foresees; that it might be made generally known beforehand, for instance, by means of printed bills, &c., that a mètre is about $39\frac{3}{8}$ inches; that a kilomètre is about 1100 yards, or 5 furlongs, or $\frac{5}{8}$ths of a mile; that the hectare is about $2\frac{1}{2}$ acres; the kilogramme about $2\frac{1}{5}$ pounds; that 50 kilogrammes are about 1 hundredweight; and 1000 kilogrammes about a ton; whilst the litre is about $1\frac{3}{4}$ imperial pints, so that $4\frac{1}{2}$ litres are about 1 gallon. Yet even such a simple precaution as this would do away with most of the inconvenience of which he so loudly complains.

Further, it must be remembered that while the *advantages* of the change will extend to all ages, the *inconveniences* will be confined to the generation existing during the transition stage, and to most even of them for a year or so at the furthest. *The shorter the transition stage the less will be the inconvenience.*

We will now turn to the question of the system of coinage. Here another consideration complicates matters, viz., what metal shall be employed as the standard. In England we have gold, whereas the French standard is silver. As it is to be expected that gold and silver will vary in their relative value, it is necessary to choose one of the two as our standard, so long as we employ them for the purposes of the currency. Opinion in England seems decidedly in favour of keeping gold as the standard; and, moreover, the franc, the standard coin of the French, is very small for such a purpose.

It has been proposed to coin a piece of 25 francs, and to divide this into tenths, hundredths, and thousandths. This would differ but little from our present sovereign, and if France would adopt gold as her standard, this system would seem the most feasible that could be devised.

It would be a great advantage to introduce the decimal system for coinage at the same time as the decimal system for weights and measures. There would thus be only one change. The metric system of weights and measures has already been largely adopted, and therefore it seems desirable for us to adopt *that* system in preference to any other decimal system. The franc, too, has been adopted to some extent, as, for instance, by Belgium, Switzerland, and Italy. It would be a step gained, therefore, if we could adopt a decimal system of coinage which would chime in as much as possible with the system already in use in several countries.

The afore-mentioned proposal would effect this. A coin might be struck exactly equal to 25 francs, and approximately to our present sovereign. This might be called the "standard," and would have the advantage of giving as its tenth part a piece equal to 2 francs 50 centimes exactly, *and approximately equal to our florin, and also (what is of no mean importance) to the rupee*, which may be called the universal coin of India. The hundredth part of the standard would be exactly 25 centimes, or approximately $2\frac{1}{2}$ pence, and the thousandth part exactly $2\frac{1}{2}$ centimes, or about our farthing. The tenth, hundredth, and thousandth parts might, if it were thought desirable, receive names, and for this purpose the words "dimes," "cents," and "mills" have been proposed.

Each coin should bear its name in words, and also its value as a decimal part of the standard stamped on it. It would be an improvement to place the abbreviation St. *after* the number indicating the standards, and *before* the numbers indicating the decimal parts. The St. would always then indicate the position of the decimal point.

The following might be the system of coinage :—

Gold Coins.

		Approximate English value.	Exact value in Francs, &c. Frs.	Cents.
One standard .	1 St.	£1	25	0
Five dimes . .	St. 0·5	10*s.*	12	50
Three dimes. .	St. 0·3	6*s.*	7	50

Silver Coins.

One dime. . .	St. 0·1	2*s.*	2	50
Five cents . .	St. 0·05	1*s.*	1	25
Three cents . .	St. 0·03	7½*d.*	0	75
Two cents . .	St. 0·02	5*d.*	0	50
One cent . . .	St. 0·01	2½*d.*	0	25

Copper Coins.

Four mills . .	St. 0·004	1*d.*	0	10
Two mills . .	St. 0·002	½*d.*	0	5
One mill . . .	St. 0·001	¼*d.*	0	2½

In this table there are seven coins which may be said to be in existence now, viz., the first two gold coins, the first two silver coins, and all the copper coins; for the difference between the coins now in existence and the coins spoken of above would be very slight.

It will be noticed that a new gold coin has been placed in the list, viz., a piece of 3 dimes, of the value of 7½ francs. As will be remembered, the French have a gold coin of the value of 5 francs. This is of a suitable circumference, but is rather thin; whereas the coin here proposed, being worth exactly half as much again, would be better in this respect, and would be found very convenient.

The new silver coins advocated here are those of 1, 2, and 3 dimes, of the value of 25, 50, and 75 centimes respectively. From their likeness to our twopenny, fourpenny, and sixpenny pieces, it is reasonable to conclude that they would be useful, and would cause no confusion.

The copper pieces being to all intents and purposes the same as those now in use call for no remark.

Supposing that England were to introduce this system for *herself and for her colonies and dependencies*, it is almost certain that all those nations which at present use the franc, having silver as their standard, would make gold their standard, and come into the system so as to obtain uniformity, for the change would be slight and insignificant for them. Moreover, let us think what is meant by the phrase Great Britain and her colonies and dependencies. There is only need to mention the United Kingdom of Great Britain and Ireland, India, Australia, New Zealand, and Canada to show the importance of the phrase. Then, consider the amount of commerce which is carried on by this assemblage of nations, both among themselves and with foreign nations, and it becomes a matter of extreme probability, almost amounting to certainty, that if Great Britain, her colonies, and dependencies were to adopt the metric system of weights and measures (already largely adopted by other nations), together with the decimal system of coinage here advocated, these systems would become universal over the whole civilised globe.

Whether such were the case or not the gain to Great Britain and her dependencies would be enormous. Let us compare the account books of the present system with those which would be used under the system advocated here. In this latter, besides the columns for the number of standards, there would be one column each for dimes, cents, and mills. This contrasts favourably with the present system; for, besides the column for pounds, there are now in reality four columns, viz., a *double* column for shillings, and a column each for pence and farthings. Further, under the decimal system it is merely a case of simple addition, whereas under the present system there are three different reductions to be made, viz., the division by 4 to reduce the farthings to pence, by 12 to reduce pence to shillings, and by 20 to reduce shillings to pounds. One result of the change would then be that bankers', merchants', tradesmen's, and, in fact, *all* account books, would be more easily and more correctly kept. The gain *to any one individual in any one day* might be small, but when we consider the gains of *all the people concerned*, the total gain becomes considerable *even in one day*; if, further, we consider *all the gains of all the people concerned for a year, or still more for all future time*, the total becomes simply overwhelming.

The advantage, too, to the commerce between England

and India alone would be very great in the uniformity that would thus be established in all accounts.

To India, too, considered alone, the benefit derived from the system advocated would be very great. The substitution of a rational system of weights and measures for that now in use would be highly beneficial, and the introduction of gold coins would be a great relief. It is hardly realised in England, because not experienced, what a nuisance in carriage and loss of time in counting is occasioned by the payment of any large sum of money in silver.

I do not at present recollect the exact proportion between the weight of the sovereign and that of the florin or rupee, but it will be near enough for our present purpose to use the following equation :—

The weight of 5 florins or rupees = the weight of 8 sovereigns = 2 ounces troy.

In order, then, to pay a sum of £40 we, in India, have to pay 400 rupees, that is to say, a weight of about 160 ounces troy as against a weight of about 10 ounces, which would be necessary to pay the same sum in England. In other words, we have now to use in India 16 times the weight of a coin that would be necessary to pay any sum were a gold coinage in circulation here. And since any weight of silver occupies more space than the same weight of gold, it follows that the comparison between the *bulks* of gold and silver coin necessary to pay any sum is still more in favour of the gold.

It would seem, then, that there is a large amount of argument in favour of England's making a change to a decimal system of weights, measures, and coinage, and that for the weights and measures the metric system should be adopted; whilst for coinage it should be a decimal system *in connection* with the system used in France, Belgium, Switzerland, and Italy, and which is a part of the metric system. It must here be noted that it is of importance to adopt a system in connection with one in use by *several* nations, rather than with one in use by *one nation only, although that one nation may be of the combined importance of the several nations;* for we thus have the adhesion of *several nations instead* of *one only*, and are to this extent nearer to the realisation of a universal system of weights, measures, and coinage; and the *universality* of the system is only second in importance to the fact of its being a *decimal* system.

The proposal to introduce a decimal system of weights, measures, and coinage seems to have set people to work to endeavour to find out all the difficulties of the scheme. This looking out for the difficulties of any new scheme is but reasonable, but it should be done in the spirit of a brave man, who, being convinced of the desirability of some object, looks first to see what difficulties lie in the way of its attainment, in order that he may be prepared beforehand, and be the more ready to meet and overcome them. Instead of this, we as a nation have been content to let the difficulties which look big and threatening prevent our adoption of a system which would be a great blessing to the land. We ought rather, having found the difficulties which oppose us, work heart and soul to remove them, and they would soon be found to be much less formidable than they appear. We all know that possession is said to be "nine points of the law;" and it is this and this only which has enabled the old system to hold out so long as it has done.

Even the opponents of the decimal system admit that there is no more, and perhaps, in fact, much less, difficulty in its working than in that of the system now in vogue. What they object to is the *change*, and not the *system* itself. Are we, then, for the sake of some *temporary inconvenience* to decline a *permanent advantage?* Are we, in fact, to suffer a *permanent* inconvenience because we are afraid of incurring a *temporary* trouble? Should we not rather constantly aim at attaining the permanent advantage, and at the same time endeavour to find out how we can make the temporary inconvenience as small as possible? In doing otherwise we merely add one more to the already long list of illustrations of the old saying, that "It is the lazy man who gives himself the most work."

Once understood, the new system would be easier than, and far preferable to, the old, and it is not *in itself* difficult to understand. The only difference lies in the fact that we are familiar with the old system, and consequently refer all questions of weights and measures to it; but once our conceptions of weights and measures formed in the new system, we should think in kilogrammes, mètres, litres, &c., and it would no longer be necessary to translate into the weights and measures of the old system. It is exactly the same as with languages. We, being English, think French difficult, and *vice versa*. In learning a foreign language the beginner generally translates every phrase mentally into his own tongue, but after he has properly acquired the other language he no longer makes any mental translation but seizes the meaning direct. So much is this the case that frequently an individual, although knowing thoroughly well the meaning of some passage of foreign language, may yet be totally unable to render the same exactly in his mother-tongue. The best way of learning a foreign language is undoubtedly to do without the aid of one's own as soon as possible, and the same is true with regard to the change to the decimal system of weights, measures, and coinage. The less comparison there is between the old system and the new the better, and the more we learn the new system, so to speak, from itself, by its use and without reference to the old, the quicker and the better shall we be able to understand it.

The great argument of our opponents seems to be that "the common people would not understand the system, and would not be able to use it." Now on what is this belief based? Is there any reasonable ground for supposing that this would be the case? The change has been successfully made in several other nations under as great difficulties as we should have to encounter. Do our opponents wish us to suppose that our own common people are below the common people of other nations in intelligence? For my own part, I cannot but think that, having the experience of other nations before our eyes, there is no reason to believe that we should fail on this score, and surely the common people of the United Kingdom are not willing to lie under the imputation thus thrust upon them. No; let us take all needful precautions; let us make the change as easy as possible; let us give all reasonable means to every man, woman, and child of learning the use of the decimal system, and then we need not fear to make the change. Moreover, I submit that, in that case, we should not be justified in delaying the change. It is neither reasonable nor desirable to make the lower classes the measure of the nation. We are bound to give them facilities for acquiring the use of the system, but, having done that, it is not our fault if they do not choose to avail themselves of those facilities; and the rest of the nation should not be deprived of an advantageous reform on their account.

Let us now see how the change might best be effected; by what precautions we can best overcome the difficulties that must inevitably arise.

In the first place, as we have seen, *the difficulty lies in the change itself, and only exists during the transition stage.* What is the conclusion? It is, *that the transition stage should be made as short as possible.* How may this best be done? Let it be agreed that the present system shall remain in force until some certain date, say, for instance, until the 1st of January, 1874, and that, from that date, its use shall be *illegal,* and the use of the decimal systems here proposed *compulsory*. Meanwhile, from the present date, numbers of specimens of the new weights, measures, and coinage might be made by the Government. These should not be for actual use in business, but merely for exhibition, in order to render the public familiar with them. We have already said that it is desirable to avoid translation as much as possible, and learn the new system from itself alone, and by its use.

Let the Government supply each shopkeeper with a set of such weights or measures, on the new system, as he would use in his business on the condition of his displaying them in his window. On the 1st of January, 1874, these sample weights and measures would become the property of the shopkeeper, and the Government would take up his old weights or measures and destroy them as such, utilising them in some other manner.

Further, every school should be supplied with a complete set of weights and measures according to the new system, and the scholars should be instructed in their use, and made familiar with their appearance and size.

Tables, of course, would be prepared showing the exact relations of the weights and measures of the new system to those of the old; but at the same time the comparison between the two systems should not be encouraged beyond what was absolutely necessary for the equitable settlement of claims.

If we do not adopt the decimal system for our weights, measures, and coinage we shall deserve even greater reproaches than have been heaped upon the workmen who destroyed the machinery of Arkwright and his brother inventors. For here we have a machine which is beautifully simple, with the working of which we are already familiar, and which would save labour to every man, woman, and child in the kingdom, and we refuse even to apply it to a new use.

There is no nation which has such a commerce as our own, and, consequently, there is no nation which would benefit so much by the adoption of a decimal system. Let us, therefore, no longer delay; let us recollect that the longer we delay the greater will the change be, and let us boldly make the change at once.

In conclusion, it is scarely necessary to state that the present article has not been meant as a dissertation on the decimal system. There are already many and excellent publications on the subject. The writer of this article is convinced that it can only be the fact that the great mass of the nation are content not to think on the subject, or has its time too fully occupied to do so, which has thus long delayed the introduction of the system into England. In this belief the present article has been written, in the hope that, by putting the matter in a plain, and, so to speak, everyday point of view, the thoughts of the public generally might be turned to the subject, which, in the writer's opinion, would be sufficient to seal the fate of our present system for ever.

III. THE CONSTRUCTION OF THE HEAVENS.

By Richard A. Proctor, B.A., Cambridge;
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Author of "The Sun," "Other Worlds," &c.

"A knowledge of the construction of the heavens has always been the ultimate object of my observations."—Sir W. Herschel (*Phil. Trans. for* 1811).

It may appear strange, but is nevertheless strictly true, that though the name astronomy implies the science which deals with the laws of stellar distribution, yet astronomers as a body have given less attention to the discussion of these laws, or, in other words, to the solution of the problem of the construction of the heavens, than to any other department of their science. The observation of the stars has indeed occupied a large share of the labours of astronomers; but such observation has been directed to the exact determination of the position of individual orbs upon the vault of heaven. At the great public observatories the prosecution of such work—the importance of which, be it understood, I am very far from questioning—has been the object of many years of patient labour by many hundreds of skilful astronomers: catalogues in which star places are recorded by hundreds of thousands are now in existence, and the magnitude, colour, proper motion, annual parallax, and other relations of multitudes of stars have been carefully recorded. But these labours have not been carried out for the purpose of obtaining information respecting the structure of the sidereal universe. They have all related to the recognition and accurate placing of individual stars; and indeed it may be said, without exaggeration, that they have all been intended to subserve terrestrial purposes rather than to extend our knowledge of celestial relations.

And it is particularly remarkable how few of the astronomers whose names stand highest in the roll of scientific fame, have taken any considerable degree of interest in the problems presented by the stellar heavens. Hipparchus and Ptolemy studied the stars in order to obtain information respecting the earth. Copernicus neither formed nor attempted to form any theory of the sidereal system. He expressed, indeed, the opinion that the universe of stars is spherical, but he based this opinion on considerations far removed from the actual study of the stars. Kepler, in like manner, did not direct his attention to the distribution of the stars, and the evidence which that distribution may afford respecting the constitution of the heavens; he, like Copernicus, expressed an opinion respecting the sphere of stars, but the opinion was based on fanciful analogies, not on observed facts.* Galileo limited himself to the observation of the stars with his telescope, enunciating only the theory that the Milky Way consists of a multitude of stars, as demonstrated by his telescopic researches. Hevelius did

* His reasoning is so remarkable that I venture to quote it in this place, for it affords a very suggestive insight into the nature of Kepler's mental organisation, especially when we consider that the work in which these ideas are put forward—the "Epitome," was published partly in 1618, and partly in 1620, while the three laws of Kepler were published in 1609 and 1619; in other words, that he was bringing out a farrago of fanciful and baseless speculations during the very period which saw him force from nature the key to one of the noblest of her secrets. After explaining that the stars are necessarily enclosed within the substance of a spherical shell having the sun at its centre, he proceeds to reason thus:—The radius of the concavity enclosing the sun is determined by a simple proportion. Saturn, the outermost planet, travels at a distance from the sun equal to 2000 times the sun's radius; therefore by the harmony of relations the distance of the sphere of the fixed stars is equal to 2000 times the distance of Saturn from the sun. But the thickness of the crystalline (the spherical shell enclosing the stars) can also be determined. All the matter of which the universe is formed is divided into three equal parts. One third is included in the body of the sun; another third forms the substance of the planets and of the celestial ether which fills up the space within the sphere of the fixed stars; the remaining third forms the crystalline. Now since the ether fills a space exceeding the sun's volume 64 trillions of times $[(4{,}000{,}000)^3]$, its density must be proportionately small compared with his. And the density of the crystalline must be a mean between the sun's density and that of the ether. Thus the crystalline has a density 8000 million times $[(4{,}000{,}000)^{\frac{3}{2}}]$ less than the sun's; and as its mass is equal to the sun's, its volume is 8000 million times greater. Hence, since its inner diameter is known, it is easy to calculate its thickness, which is found to be equal to the 6000th part of the sun's radius—or, according to nineteenth century measurements, to about seventy miles.

It would appear, as Struve points out, that Kepler in thus reasoning was chiefly striving to accommodate the Copernican theory with the ideas entertained in his day respecting the waters above the firmament spoken of in the Pentateuch.

not discuss in any way the relations presented by the stars. Huyghens, however, in his "Cosmotheoros," made many judicious reflections respecting the stellar system, and advanced opinions which were carefully based on observed facts. We owe to him the definite enunciation of the theory that the stars are suns like our own, probably, like it, the centre of planetary schemes. Huyghens, like his predecessors, failed to discuss attentively the configuration of the star groups, whether those seen by the naked eye or those revealed in the telescope, with the purpose of thence ascertaining the laws according to which the stellar universe is constituted. Newton, Halley, Flamsteed, and their contemporaries, paid in like manner very little attention, or none at all, to that stupendous problem, the solution of which Sir W. Herschel afterwards set before him as "the ultimate object of his observations." And passing at a step over the interval separating Newton's day from our own times, it may be said that the Herschels and the elder Struve have been the only astronomers who have attempted to form broad and connected views respecting the structure of the universe, while only half-a-dozen names need be added, if we would include those who have given attention to the subordinate problems associated with the great one of determining the laws of the sidereal system.

But even more remarkable than the carelessness with which nearly all the great astronomers of the last three centuries have viewed this noble problem, is the fact that attention was first fairly called to the problem, and worthy attempts were first made towards its solution, by men who were not astronomers* in the true sense of the term, and whose names, with a single exception, remain in undeserved obscurity. How seldom do we hear the names of

* Lest this remark should be misunderstood, I may as well explain in what sense I use the word astronomer. I was somewhat roundly taken to task when in the first edition of my "Other Worlds" I spoke of Whewell in one place, and of Humboldt in another, as not being astronomers. There cannot be any question that Whewell had a clearer insight into many astronomical subjects than many who must be described as astronomers; and a like remark applies to Humboldt, Brewster, and others, as well as to those of whom I speak above. Yet it is impossible for any astronomer to read many consecutive pages of matter written on astronomical subjects by Whewell, Humboldt, Brewster, Kant, and others, without feeling that they were emphatically *not* astronomers. What then, it may be asked, is my definition of an astronomer? It is one which immediately removes anything that might seem invidious in the distinction I draw between the above-mentioned eminent men, and others—not more eminent, some far less eminent—whom I should emphatically call astronomers. An astronomer is one who devotes the main portion of his scientific life and labour to the study of astronomy, either generally or in some special department. This definition excludes, and, as I take it, properly excludes,

Wright, Lambert, and Michell associated with those of the Herschels and Struve; how seldom, indeed, are they mentioned at all; and if the name of Kant is held in high honour, how little is this due to its association with astronomical theories! Yet I do not hesitate to say, that Wright, Kant, Lambert, and Michell did more to advance men's ideas respecting the constitution of the sidereal universe than all the astronomers who lived before the time of Sir W. Herschel.

We owe to Wright, of Durham, the enunciation of that theory of the universe which is so commonly presented in our text-books of astronomy, as representing the "outcome," so to speak, of the labours of the elder Herschel. Nor did Wright simply enunciate that theory; he based it on observation. All that was hypothetical in his reasoning was the idea with which he started that the stars are arranged with a certain general uniformity throughout the sidereal system. "It seems inconsistent," he said, "with the harmony observed in all the other arrangements of nature, that one scheme of stars should be arranged with perfect symmetry, while another is scattered irregularly." It is far safer—so Wright reasoned—to conclude that the seeming incongruity between the aspect of the Milky Way, which is unquestionably a zone of stars, is due only to the imperfect nature of our survey, both as respects extent of space and duration in time. "When we reflect," he proceeds, "upon the various configurations of the planets, and the changes which they perpetually undergo, we may be assured that nothing but a like eccentric position of the stars could occasion such confusion among bodies otherwise so regular; in like manner we may conclude, that as the planetary system if viewed from the sun would appear

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those who turn for awhile from their special branches of research to the study of some special astronomical subject, however skilfully they may treat that subject. It also excludes those (and again, as I take it, the exclusion is right and proper) who have made all science their subject. But unlike other definitions which I have heard advanced, it does not exclude any class of astronomical workers or thinkers. If we take a familiarity with any special branch of astronomical lore, whether observational or mathematical, or theoretical or historical, as essential to the character of the true astronomer, we must in every case exclude many who have given nearly the whole of their scientific life and labour to the advancement of the science of astronomy. We might thus adopt a definition excluding Adams and Leverrier, or another excluding Airy or Challis, or another excluding Huggins and De la Rue, or another excluding Dawes and Webb, and Knott and Browning. Newton could be excluded because he was not an observer of the heavens; Sir W. Herschel because he was not an adept in manipulating the formulæ of Laplace and Lagrange. In fine, we might successively exclude every student of astronomy that has ever lived, with perhaps the single exception of the younger Herschel.

perfectly symmetrical, so there may be some place in the universe where the arrangement and motions of the stars may appear most beautiful. If we suppose the sun to be plunged in a vast stratum of stars of inconsiderable thickness compared with its dimensions in other respects, it is not difficult to see that the actual appearance of the heavens may be reconciled with a harmonious arrangement of the constituent bodies of such a system with respect to some common centre, provided it be admitted at the same time that the stars have all a proper motion. In such a system it is manifest that the distribution of the stars would appear more irregular the farther the place of the spectator was removed from the centre of the stratum towards either of the sides. It is also evident that the stars would appear to be distributed in least abundance in the opposite directions of the thickness of the stratum, the visual line being shortest in those directions; and that the number of visible stars would increase as the stratum was viewed through a greater depth, until at length, from the continual crowding of the stars behind each other, it would ultimately assume the appearance of a zone of light."*

It will be obvious that we have here a complete enunciation of what has been called the Grindstone theory of the stellar system. The theory is based by Wright on observed facts, precisely as it was afterwards based by Sir W. Herschel on other observed facts. Assuredly whatever credit appertains to the invention of the theory must be in justice ascribed to Wright, who thus in 1750 reasoned out and clearly described the views to which Herschel was led in 1784. Wright, indeed, did not convince his contemporaries. Either they were unwilling to examine the reasoning he advanced, or they could not recognise its force; but neither remissness nor slowness of apprehension on their part can afford just ground for depriving Wright of the credit due to his ingenious analysis of known facts.

Kant's speculations, so far as they relate to the present constitution of the universe, must be regarded as simply an extension of Wright's theories. Kant had read Wright's work, the "Theory of the Universe," which had been reprinted in a Hamburg journal of the year 1751, and he admits that his views respecting the universe of stars were suggested by Wright's theories; but he found himself

* I have not followed here Wright's actual text, not having present access to his work. The above passage is taken from the abstract of Wright's theory in Professor Grant's excellent "History of Physical Astronomy."

unable to indicate "to what extent his system is a reproduction or amplification of Wright's." There can be no question that unconscious memory had a much larger share in the production of Kant's ideas as published in 1755, than he himself (in the absence of Wright's work to refer to) could probably have imagined.* The most important point to be noticed in Kant's work is his extension of Wright's speculations to other orders of systems than observation had yet revealed. Wright had considered that the nebulæ indicate the existence of other systems, not necessarily like our own star system, but of the same order in the scheme of creation. Kant considered that these star systems are members of a new system of a higher order. "We trace here," he added, "the first terms of a series of worlds and systems, and these first terms of an infinite series enable us to infer the nature of the rest of the series."†

Strangely enough while Kant borrowed his views almost wholly, though unconsciously, from Wright, he seems to have been disposed to regard Lambert's views—which in reality were unlike his own—as borrowed from him. In his correspondence with Lambert, Kant remarked in 1763, that "the accordance between their ideas extended even to the most minute details." It will be seen, however, that in forming this opinion Kant misinterpreted the theses of Lambert.

Lambert, like Huyghens, Wright, and Kant, regarded the stars as suns resembling our own sun in importance, and like it surrounded by planetary systems. Each sun with its family of planets formed in Lambert's theory *a system of the first order.* He considered that our sun belongs to a vast globular group or cluster of stars, forming *a*

* Professor Huxley, in his fine essay on "Geological Reform," remarks that Grant, in his "History of Physical Astronomy," makes but the briefest reference to Kant. The reason for this may probably be found in the circumstance that, in describing Wright's ideas, Grant had already presented all those of Kant's views which could properly find a place in a history of physical astronomy. The student who is anxious to inquire into those more speculative ideas of Kant which are not to be found in Wright's "Theory of the Universe," should refer to Kant's original work, "Allgemeine Naturgesichte und Theorie des Himmels; oder Versuch von der Verfassung und dem mechanischen ursprunge des ganzen Weltgebündes nach Newton'schen Grundsatzen Abgehandelt."—Kant's Sämmtliche Werke, Bd i., p. 207.

† I omit all reference to the ideas of Wright, Kant, Lambert, and others, as to the existence of central suns, simply because those ideas were purely speculative. I may add here, however, that analogy now no longer requires that we should consider every system as necessarily governed by a central orb. We now know from observation that many systems of orbs exist in space which have have no dominating centre.

system of the second order, including all those stars, spread over all parts of the heavens, which do not belong to the milky way. He further held that there are many systems of the second order, and that these are not distributed throughout all space, but are all found near a certain principal plane or mean level, and being ranged one behind the other to a great depth, form by their concourse the Milky Way. This is *a system of the third order*. Analogy suggests, he added, that there are in the universe many Milky Ways; "perhaps, for instance, the nebula in Orion may be a Milky Way, nearer to us than the rest. Should this be the case, telescopic research will reveal many others, forming together *a system of the fourth order*." And he proceeded thence to the inference that there may be systems of yet higher orders.

It will be observed that in regarding the Milky Way as formed, not *directly* of the concourse of many stars, but of the concourse of many clusters of stars, Lambert adopted a totally different conception of its nature than Kant had formed. Nor was this view of Lambert's, any more than his other opinions, merely speculative. He based it on the observed irregularity of the Milky Way, on the different richness of its various parts, and on its branching figure. In passing, it may be observed that Lambert here anticipated the conclusions to which Sir William Herschel was led after he had abandoned the principle of star gauging.

Lambert thus describes the grandeur of the universe of systems as pictured in his theory:—"How far soever we may extend the scale," he says, "we must necessarily stop at last. And where? At the centre of centres, at the centre of creation, which I should be inclined to term the capital of the universe, inasmuch as thence originates motion of every kind, and there stands the great wheel in which work the teeth of all the rest. From thence the laws are issued which govern and uphold the universe; or rather, there they resolve themselves into one law of all others the most simple. But who would be competent to measure the space and time which all the globes, all the worlds, all the worlds of worlds employ in revolving around that immense body—the Throne of Nature and the Footstool of the Divinity! What painter, what poet, what imagination is sufficiently exalted to describe the beauty, the magnificence, the grandeur, of this source of all that is beautiful, great, and magnificent; and from whence order and harmony flow in eternal streams through the whole bounds of the universe."

Michell limited his attention to the lucid stars, not enunciating any complete theory respecting the galaxy. The most important of his theoretical views, or rather the most important of the facts which he established,* is that which relates to the existence of laws of arrangement (even among the stars visible to the naked eye), according to which the stars form systems, separated from each other by relatively vacant spaces. "We may conclude," he says, "that the stars are really collected together in clusters in some places, where they form a kind of systems; whilst in others there are either few or none of them, to whatever cause this may be owing, whether to their mutual gravitation or to some other law or appointment of the Creator. He then proceeds to inquire whether the sun "likewise makes one of some system." He considers that this is probably the case, and he endeavours to separate those stars which probably belong to the same system as the sun from those which do not. "There are some marks," he says, "by which we may with great probability include some and exclude others,—while the rest remain more doubtful. Those stars which are found in clusters and surrounded with many others at a small distance from them, belong probably to other systems and not to ours. And those stars which are surrounded with nebulæ are probably only very great stars, which upon account of their superior magnitude are singly visible, whilst the others which compose the remaining parts of the same system are so small as to escape our sight. And those nebulæ in which we can

* Amongst the facts which Michell established must be included the existence of binary star systems. Nothing can be more complete or demonstrative than Michell's reasoning on this subject; and I can conceive no reason why the credit of the discovery should be refused to the man who deduced it by exerting the noblest of human faculties—the power of abstract reasoning, in order that it should be handed over to another (however eminent) who deduced it from direct observation. It is true Michell failed to convince his contemporaries, and that his reasoning if it had not been subsequently confirmed by Sir W. Herschel's observations might have remained to this day unappreciated, save by the few. But this does not do away with the fact that Michell's demonstration was complete; and his credit ought not to be diminished because the many could not grasp the full value of the proof. A very slight extension of such a method of judging might deprive the Herschels in their turn of the credit now (as I think unjustly) assigned to them for the discovery, to make it over to the first person who (say) by obtaining photographic records of double stars, should enable every one to see for himself that one component circles around the other. Surely the reputation of the Herschels does not require that any credit should be assigned to them which is justly due to another. Michell has indeed been particularly unfortunate in such matters, since, to mention no other instances, the so-called Cavendish experiment for weighing the earth was not only devised by him, but the very instrument with which Cavendish conducted the experiment was constructed under Michell's superintendence.

discern only a few stars even with the assistance of the best telescopes, are probably systems which are still more distant than the rest." On the other hand, he inferred that those stars "which being placed at a greater distance from each other compose the larger constellations, and such as have few or no smaller stars near them when examined with telescopes, belong probably to our own system. Variable stars he also regarded as in all probability members of the system of stars to which our sun belongs; though the reasoning on which he based this opinion is not such as can now be admitted; for he considered their variations of brilliancy to be due to variations of distance (a cause which would necessarily be more effective in the case of stars relatively near to us); but we now know that this explanation is not the true one. He also judged red stars to be much larger and nearer than their apparent brightness would suggest, and hence inferred that they also belong to the system of stars which includes our sun.

In passing to the work of Sir W. Herschel as a student of the sidereal heavens, I cannot but express some degree of surprise that so little has been done to bring the records of his labours properly before the student of astronomy. His papers merely collected into a volume would form a most important accession to astronomical literature. But if suitably edited and illustrated by the work of his son, and of others who have succeeded in the same field of research, the volume would do more to advance the study of sidereal astronomy than any work which has been published during the last century. It must be added that nearly all that has hitherto been done in making Herschel's words and work public, has been rather an injustice to his memory than otherwise. It seems to have been supposed that his own account of his work might be treated as we should treat such a work as Sir John Herschel's "Outlines of Astronomy," and that extracts might be made from any part of any paper, without reference to the position which that paper chanced to occupy in the complete series. It does not seem to have been noticed that not only was there a progression in the ideas as well as in the work of the great astronomer, but that there was a complete change in his opinions during the progress of his labours. Hence views expressed by him in his earlier papers are not uncommonly in strong contrast with those which he advocated in later years; opinions which he regarded as certainly just at one time were rejected as most probably incorrect after a few years of fresh labour; and whereas in 1785 he enunciated with some definiteness a theory respecting the constitution of the universe, he not only abandoned this theory (implicitly) in 1811, not only gave up the very principles on which it had been based, but he did not consider himself in a position during any subsequent part of his career to state with the same degree of definiteness any theory whatever respecting the constitution of the heavens.

It is to be premised, however, that even the theory of 1785* is not properly described in our text-books of astronomy, and that some of the accounts which purport to be descriptions are the merest travesties of the noble conceptions of Sir W. Herschel.

The sole point in the remarkable paper of 1785 which seems in any degree to correspond with the account usually given, is the hypothesis on which the principle of star-gauging is based. Herschel adopts a general uniformity of stellar distribution within the sidereal system as a means of forming some idea of the shape of that system. If there were actual uniformity, it would be sufficient to turn a telescope successively to different parts of the heavens, to obtain a sufficiently accurate idea of the extension of the stellar system in those different directions; for, obviously, the farther away the boundary of the system might lie in any direction, the more stars would be seen in that direction. But though Herschel thus adopts a principle resembling that usually associated with the method of star-gauging, and though he arrives at a conclusion in some sense resembling that which has received the name of the Grindstone Theory (though it would more properly be called the Cloven Disc Theory), yet it is only on the most cursory reading that either the principle of generally equable distribution, or the conclusion that our sidereal system forms a cloven stratum of stars, can be regarded as according with Herschel's real views. He fully recognised the existence of subordinate systems within our sidereal system. This is shown by his remark that it is "a very extensive, branching, compound *congeries* of many millions of stars, which most probably owes its origin to many

* It will be observed that I pass over the paper of the year 1784, in which Herschel first began to discuss the constitution of the heavens. That paper is so obviously a mere introduction of his subject, so obviously preliminary, that one cannot but be amazed to find how much stress is laid upon it in many works on astronomy, and especially among French writers. We doubtless owe this to Arago's misapprehension of the purport of the paper. Struve has pointed out, also, very justly, that Arago has misconstrued the details of Herschel's theories. There scarcely exists, indeed, a less trustworthy account of Herschel's works and theories than Arago's, whom yet many English writers accept as an authority on the subject.

remarkably large as well as pretty closely scattered small stars that may have drawn together the rest."

Again, let the reader carefully study the following extract from the paper of 1785, and he will find that whereas it is perplexing in the extreme if the sidereal system be regarded as a mere cloven stratum of stars, pretty uniformly distributed, it becomes perfectly clear (and wonderfully striking) when we remember that Herschel considered the milky way to be compound in structure and branching in figure :—

"If it were possible to distinguish between the parts of an indefinitely extended whole, the nebula we inhabit might be said to be one that has fewer marks of profound antiquity upon it than the rest. To explain this idea, perhaps, more clearly, we should recollect that the condensation of clusters of stars has been ascribed to a gradual approach; and whoever reflects upon the numbers of ages that must have passed before some of the clusters could be so far condensed as we find them at present, will not wonder if I ascribe a certain air of youth and vigour to many very regularly scattered regions of our sidereal stratum."

Yet more strikingly opposed to the common conceptions of Herschel's earlier theory, is the following passage from the paper of 1785 :—"Our system after numbers of ages may very possibly become divided so as to give rise to a stratum of two or three hundred nebulæ; for it would not be difficult to point to so many beginning or gathering clusters in it. This view of the subject throws a considerable light upon the appearance of that remarkable collection of many hundreds of nebulæ which are to be seen in what I have called the nebulous stratum of Coma Berenices. It appears from the extended and branching figure of our nebula, that there is room for the decomposed small nebulæ of a large, reduced, former great one to approach nearer to us in the sides than in other parts. Nay, possibly, there might originally be another large joining branch, which in time became separated by the condensation of the stars; and this may be the reason of the little remaining breadth of our system in that very place; for the nebulæ of the stratum of Coma are brightest and most crowded just opposite our situation, or in the pole of our system. As soon as this idea was suggested," he adds, "I tried the opposite pole, where accordingly I have met with a great number of nebulæ, though under a much more scattered form."

It will be quite obvious, I think, that Herschel entertained at this time ideas altogether different from those usually ascribed to him. In particular, it is to be noticed that he regarded the nebulæ in two aspects at this time. There were some which he held to be parts of a much larger nebula, possibly parts of our own Milky Way. Others he held to be external Milky Ways, and therefore themselves made up of subordinate nebulæ. At this stage of his career, however, he seems to have entertained no doubt as to the stellar character of any nebulæ; and it is to the ideas he thus early promulgated respecting the nebulæ, that we must ascribe the common but altogether erroneous opinion, that Herschel regarded all the nebulæ as external galaxies of suns. He never entertained so general a theory as this; but even the theory he did entertain in 1785 respecting some of the nebulæ was modified ere long as respects a very large proportion of those objects.

He had not changed his views, however, in 1789, when he compared the nebulæ of various orders to plants in various stages of growth. The following passage does not relate, as has been asserted, to his hypothesis of the development of nebulæ into stars, but relates to various orders of stellar systems. "This method of viewing the heavens," he says, referring to the view that differences of antiquity may account for the different appearances of nebulæ, "seems to throw them into a new kind of light. They now are seen to resemble a luxuriant garden, which contains the greatest variety of productions in different flowering beds; and one advantage we may at least reap from it is, that we can, as it were, extend the range of our experience to an immense duration. For to continue the simile I have borrowed from the vegetable kingdom, is it not almost the same thing whether we live to witness successively the germination, blooming, foliage, fecundity, fading, withering, and corruption of a plant, or whether a vast number of specimens, selected from every stage through which the whole plant passes in the course of its existence be brought at once to our view.

But the time was approaching when Herschel was to form entirely new views respecting the constitution of the heavens. I pass over the paper of 1796—though if space permitted I might quote passages from it, suggesting in a very interesting manner the progression of Herschel's ideas—and turn to the paper of 1802. There can be no mistaking the import of the following passages from this important paper:—"The stars we consider as insulated," says Herschel, "are also surrounded by a magnificent collection of innumerable stars, called the Milky Way, which must occasion a very powerful balance of opposite attractions, to hold the intermediate stars in a state of rest. For though our sun, and all the stars we see, may truly be said to be in the plane of the Milky Way, yet I am now convinced by a long inspection and continued examination of it that the Milky Way itself consists of stars very differently scattered from those which are immediately about us." . . . . "On a very slight examination it will appear that this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments." . . . . "The milky appearances deserve the name of clustering collections, as they are certainly much brighter about the middle and fainter near their undefined borders. For in my sweeps of the heavens it has been fully ascertained that the brightness of the Milky Way arises only from stars; and that their compression increases in proportion to the brightness of the Milky Way." . . . . "We may indeed partly ascribe the increase both of brightness and compression," in these clustering regions," to a greater depth of the space which contains these stars; but this will equally tend to show their clustering condition; for, since the increase of brightness is gradual, the space containing the clustering stars must tend to a spherical form if the gradual increase of brightness is to be explained by the situation of the stars."

It was in this paper that Herschel first put forward the hypothesis that many nebulæ are formed of some substance possessing "the quality of a self-luminous milky luminosity," and "possibly at no great distance from us." But the requirements of space render it advisable that I should leave untouched the whole of those considerations which Herschel adduced, in this paper and elsewhere, in favour of his nebular hypothesis.

That portion of Herschel's observing career which we have now reached may fairly be regarded as including the most important of his researches into the constitution of the heavens. His mental powers were now at their prime. He had acquired great experience. His circumstances were such that he could give his whole attention to scientific work. Moreover, he had seen the importance of being solely guided by known facts, since the theory which he had adopted more than a quarter of a century before (and almost in the beginning of his observations on the stars) had had to be abandoned.

Nine years of earnest labour passed before he again spoke at any length on the subject of the star-depths. It will be admitted that we should attach especial value to the announcements he then made. Yet the striking words with which he prefaces the paper of 1811 seem almost to have escaped notice:—"I must freely confess," he says, "that by continuing my sweeps of the heavens my opinion of the arrangement of the stars and their magnitudes, and of some other particulars, has undergone a gradual change; and, indeed, when the novelty of the subject is considered, we cannot be surprised that many things formerly taken for granted should, on examination, prove to be different from what they were generally but incautiously supposed to be. For instance, an equal scattering of the stars may be admitted in certain calculations; but when we examine the Milky Way, or the closely compressed clusters of stars, of which my catalogues have recorded so many instances, this supposed equality of scattering must be given up. We may also have surmised nebulæ to be no other than clusters of stars disguised by their very great distance; but a longer experience and a better acquaintance with the nature of nebulæ will not allow a general admission of such a principle, although undoubtedly a cluster of stars may assume a nebulous appearance when it is too remote for us to discern the stars of which it is composed."

In the paper of 1811, however, Herschel does not (save in these prefatory remarks) consider the structure of the sidereal heavens. The paper is devoted exclusively to the discussion of the nebular hypothesis as to the formation of stars. It was not until 1814 that he endeavoured to extend his reasoning so as to include the subject of stellar grouping. "The observations contained in this paper," he says in 1814, "are intended to display the sidereal part of the heavens, and also to show the intimate connection between the two opposite extremes, one of which is the immensity of the widely diffused and seemingly chaotic nebulous matter; and the other, the highly complicated and most artificially constructed globular clusters of compressed stars." In this paper he enters also into a discussion of ambiguous objects,—that is, objects "of such a construction or at such a distance from us, that the highest power of penetration which hitherto has been applied to them, leaves it undecided whether they belong to the class of nebulæ or stars."

But the chief interest of the paper of 1814 resides (in my judgment) in the remarks which Herschel makes respecting the clustering condition of parts of the heavens. He explains that his expression "forming clusters," was used "to denote that some peculiar arrangement of stars in lines making different angles, directed to a certain aggregation of a few central stars, suggested the idea that they '(the former)' might be in a state of progressive approach to them '(the latter).' This tendency to clustering seems chiefly to be visible in places extremely rich in stars. In order, therefore, to investigate the existence of a clustering power, we may expect its effects to be most visible in and near the Milky Way." I invite the reader's special attention to the circumstance that the Milky Way is here pointedly referred to as a stellar region distinct in its characteristics from the region of stars forming our constellations. In studying Herschel's papers we have to be continually on the watch for indications of the sort, since he does not always judge it necessary to make definite assertions of his opinions on such points.

He then describes the irregular clusterings of stars, noting in particular, that "though they are in general very promiscuously scattered, they are yet sufficiently drawn together to show that they form separate groups," while in many places a falling off in the number of stars surrounding the clusters "indicates a tendency to future insulation." "Those which are in and very near the Milky Way," he says, "may be looked upon as so many portions of the great mass drawn together by the action of a clustering power, of which they tend to prove the existence. In describing the various orders of irregular clusters, Herschel is particular to notice *rows* and *streams*, *ridges* and *shelvings*, of stars, as indications of a preponderating clustering power.

It is most important to notice here that Herschel had not yet replaced the theory of 1785 by any complete new theory.* He says at the close of the paper of 1814—"The extended views I have taken in this and my former papers of the various parts that enter into the construction of the

* The careful Struve did not fail to recognise the distinction between the theory of 1785 and Herschel's subsequent labours. "Ou peut demander," he says, "pourquoi les astronomes ont-ils maintenu généralement l'ancien système sur la Voie Lactée, énoncé en 1785, quoiqu'il eût été entierement abandonné par l'auteur luimême comme nous l'avons démontré. Je crois qu'il faut en chercher l'explication dans deux circonstances. C'était un système entier, imposant par la hardiesse et la précision géométrique de sa construction, et que l'auteur n'avait jamais révoqué dans sa totalité. Dans ses traités publiés depuis 1802, on ne rencontre que des vues partielles, mais qui suffisent, en les comparant entre elles, à comprendre l'idée finale du grand astronome." I should point out, however, that Struve somewhat over-estimates the precision of the theory of 1785, and that he also fails to notice the circumstance that the papers of 1817 and 1818 are distinct in all respects from Herschel's former work.

heavens, have prepared the way for a final investigation of the universal arrangement of all these celestial bodies in space." He was now in his *seventy-sixth* year, however, and though still in the full possession of his wonderful powers, the time that remained to him for investigating the universal arrangement of the heavens was short indeed, when the wideness of the subject is considered. But he was not even yet fully prepared to enter on the work. For he proceeds to mention as a reason for not doing so, that he is "still engaged in a series of observations for ascertaining a scale whereby the extent of the universe, as far as it is possible for us to penetrate into space, may be fathomed."

In the papers of 1817 and 1818, the last of the series, and written, be it remembered, when Herschel was in his seventy-eighth and seventy-ninth years, he describes the principle here referred to, and its application to estimate the profundities of various parts of the stellar heavens. The principle was essentially this—that the telescopic powers necessary to reveal and to resolve star groups, or particular star orders within groups, afford an indication of the distance at which those groups or orders lie. I conceive that no question can exist that the principle is unsound, and that Herschel would himself have abandoned it, had he tested it earlier in his observing career, and when *not* his mental power, *but* his mental elasticity, was greater than at the advanced age which he had now reached. I have not here space to enter into all the objections which present themselves against it. Presently, in considering Sir John Herschel's theoretical views, I shall have to adduce a conclusive instance of the unsoundness of the principle. Let it here suffice to remark, that repeatedly in applying it, Sir W. Herschel found regions of the heavens very limited in extent, where the brighter stars (clustered like the fainter) were easily resolved with low powers, but where his largest telescopes could not resolve the faintest. These regions, if the principle were true, must be long spike-shaped star groups, whose length is directed exactly towards the astronomer on earth,—an utterly incredible arrangement, even if we could believe in the dynamical possibility of such grouping.

Herschel did not live to carry out that final investigation of the universal arrangement of all the celestial bodies in space which he had looked forward to in 1814. In 1820 he read, as first President of the Astronomical Society, a paper on the double stars, and soon after his health broke down, insomuch that, though he lived till 1824, he was incapable of great or prolonged mental exertion.

Sir John Herschel's labours among stars and nebulæ were directed almost wholly to the completion of the survey of the heavens. I do not know that there is a single passage in his works in which he touches on the subject of the constitution of the heavens otherwise than as it were incidentally. He nowhere definitely enunciates a theory of the universe.

Two circumstances, however, in Sir John Herschel's work require to be attended to.

The first relates to the distribution of the brighter orders of stars over the heavens. In his "Outlines of Astronomy," he says that the stars of these orders, including those down to and those somewhat below the range of the naked eye), increase largely in number as the Milky Way is approached. In his "Observations at the South Cape," he asserts the reverse. "On a general view it appears," he says, "that the tendency to greater frequency, or the increase of density in respect of statistical distribution in approaching the Milky Way, is quite imperceptible among stars of a higher magnitude than the 8th, and except on the very verge of the Milky Way itself, stars of the 8th magnitude can hardly be said to participate in the general law of increase. For the 9th and 10th the increase, though unequivocally indicated over a zone extending at least 30° on either side of the Milky Way, is by no means striking. It is with the 11th magnitude that it first becomes conspicuous, though still of small amount when compared with that which prevails among the mass of stars of magnitudes inferior to the 11th, which constitute 16-17ths of the totality of stars within 30° on either side of the galactic circle." He then adds as a conclusion, "following inevitably from this" (and this conclusion is to be carefully noted, since, if just, it is of the utmost importance), "that the larger stars are really nearer to us taken *en masse*, and without denying individual exceptions, than the smaller ones. Were this not the case, were there really among the infinite multitude of stars constituting the remoter portions of the galaxy numerous individuals of extravagant size and brightness, as compared with the generality of those around them, so as to overcome the effect of distance and appear to us as large stars, the probability of their occurrence in any given region would increase with the total apparent density of stars in that region, and would result in a preponderance of considerable stars in the Milky Way, beyond what the heavens really present, over its whole circumference."

The discrepancy between the statements in the "Outlines" and in the "Observations" is so complete that I ventured to communicate with Sir John Herschel respecting it, though I was not without a tolerably definite idea as to its origin. He replied as follows:—"I thank you for calling attention to that section in my "Outlines." Undoubtedly there *is* a discordance of statements which requires correction. But the appeal there" (that is, in the "Outlines") "is rather to a vague naked eye impression than to the statistical result of actual enumeration; and assuredly on a cursory view of the heavens on a clear night, stars down to the 7th and 8th magnitude do affect the eye, though we cannot fix them by reason of that strange law which curtails a star *directly* looked at of a very large aliquot part of its photometric effectiveness." It will be seen that Sir John Herschel here maintains at once the reality of the aggregation of the brighter stars on the *visible* Milky Way, and the justice of the statistics which related to the Milky Way regarded as a *zone*. This is undoubtedly the true explanation of the matter, as my equal surface chartings have since abundantly demonstrated. The brighter orders of stars, down to the 11th, do unquestionably crowd upon the real Milky Way,—that altogether irregular region of star streams and star clusterings which has been aptly compared to a band of broken *cirrus* clouds; but if we lose sight of these irregularities, if we conceive of the Milky Way as of a uniform zone, and take the average distribution of the brighter stars over that zone, we no longer find that exceptional degree of richness, for the gaps and *lacunæ* of the Milky Way are as poverty stricken, notwithstanding their position in that imagined *zone* of richness, as the brighter parts are rich in respect of these higher orders of stars. But this explanation shows that the conclusion based by Sir John Herschel on the zone-statistics should be *not merely abandoned but replaced by its converse.* Let the reader carefully re-study it in the light of the above considerations, and he will see at once that this is the case.

The second point to which I would invite attention in Sir John Herschel's work is his discussion of the phenomena presented by the two Magellanic clouds. His reasoning cannot be too carefully considered. It runs thus:—"Taking the apparent semi-diameter of the Nubecula Major as three degrees, and regarding its solid form as, roughly speaking, spherical, its nearest and most remote parts differ in their distance from us by little more than a tenth part of our distance from its centre. The brightness of objects situated in its nearer portions, therefore, cannot be *much* exaggerated, nor that of its remoter *much* enfeebled, by their difference of distance; yet within this globular space we have collected upwards of 600 stars of the 7th, 8th, 9th, and 10th magnitudes, nearly 300 nebulæ, and globular and other clusters *of all degrees of resolvability*, and smaller scattered stars innumerable of every inferior magnitude, from the 10th, to such as by their multitude and minuteness constitute irresolvable nebulosity, extending over tracts of many square degrees. Were there but one such object, it might be maintained without utter improbability that its apparent sphericity is only an effect of foreshortening, and that in reality a much greater proportional difference of distance between its nearer and more remote parts exists. But such an adjustment, improbable enough in one case, must be rejected as too much so for fair argument in two cases. *It must, therefore, be taken as a demonstrated fact, that stars of the 7th and 8th magnitude and irresolvable nebulæ may co-exist within limits of distance, not differing more in proportion than as nine to ten.*"

I pass to the work of the elder Struve, but shall only deal with that portion which relates to the distribution of the stars.

Having found reason to believe that stars of the brighter orders are more crowded on the Milky Way zone than elsewhere, Struve tested the matter by comparing the numbers of stars in different hours of right ascension in Weisse's Catalogue of 31,085 stars, down to the 9th magnitude, included between 15° north and 15° south of the equator. After estimating the numbers of stars which might be supposed to have escaped recognition in the different "hours," and so raising the total number of stars to 52,199 (of which 21,114 were hypothetical), he found a marked excess of richness in the Milky Way "hours." Thus, taking the four hours, 5, 6, 7, and 8 (crossed centrally by the Milky Way near Orion), and the four hours, 17, 18, 19, and 20 (crossed centrally by the Milky Way near Aquila), he found an average of 3031 stars for each hour; while the average for the remaining sixteen hours was but 1747 stars.

So far Struve's procedure was legitimate enough. But he now extended his reasoning somewhat too daringly. For he first regarded all the stars in each hour division as gathered on the equator, though the divisions extended *thirty* degrees in declination, and having thus a fine equatorial ring of 52,199 stars unequally rich in different parts, he conceived this ring converted into a flat disc by a radial spreading of the stars towards the several parts of the ring, this spreading being uniform between the distances corresponding (according to Struve's estimate) to the position of stars of magnitudes 1 to 6, of 7th magnitude, of 8th, and lastly of 9th magnitude. It seems impossible to attach the slightest weight to the evidence afforded by the resulting disc, since the construction of the disc is based on suppositions (as to the uniform scattering of stars radially towards the equator), which are directly contrary to Struve's own clear recognition of a want of uniformity circularly round the equator.

Struve deduced from this imperfect method of reasoning, combined with the study of Sir W. Herschel's star-gauges and a theory on the extinction of light, the conclusion that the Milky Way is unfathomable in its own level. He considered this view to be in accordance with the results obtained by Sir W. Herschel in his papers of 1817 and 1818. Herschel did not, however, find the Milky Way unfathomable all round the central line of the zone, but only in certain definite directions—towards the heart, in fact, of certain clustering aggregations. Struve misunderstood Herschel's remark that "when his gauges would no longer resolve the milky way into stars, it was not because its nature is ambiguous, but because it is fathomless," for he thus renders Herschel's words into French, "nous pouvons faire la conclusion que *si* nos jauges cessent de résondre la Voi Lactée en étoiles, ce n'est point parce que la nature en est douteuse, mais parce qu'elle est insondable." Doubtless he read Herschel's "when" as equivalent to the German "wenn."*

As Sir John Herschel has remarked, disposing thereby at once of Struve's infinite extension theory and of his reasoning respecting the extinction of light, "We are not at liberty to argue that at one part of the galaxy's circumference our view is limited as by a sort of cosmical veil, which extinguishes the smaller magnitudes, cuts off the nebulous light of distant masses, and closes our view in impenetrable darkness; while at another we are compelled by the clearest evidence telescopes can afford to believe that star-strewn vistas *lie open*, exhausting their powers and stretching out beyond their utmost reach, as is proved by that very phenomenon which the existence of such a veil would render impossible, viz., infinite increase of number and diminution of magnitude, terminating in complete

* Thus is explained what Sir John Herschel and Professor Nichol clearly found perplexing, viz., Struve's conviction that the elder Herschel's results confirmed his own remarkable theory.

irresolvable nebulosity." This reasoning is complete against Struve's inferences as based on Struve's assumptions. It is not, however, the less certain that irresolvable nebulosity does *not* necessarily imply that the objects producing it "stretch out beyond the utmost reach of the telescope," as is proved by the irresolvable nebulosity discovered by Sir John Herschel himself within the bounds of the Magellanic clouds.

I shall treat very briefly of the results to which I have been led by my own researches into the subject of the constitution of the heavens, because they have been fully presented elsewhere.*

The purposes I have had in view throughout my inquiries have been two—First, to proceed in perfect independence of all preconceived theories—to inquire how the stars and nebulæ are spread in space, how they differ in magnitude or constitution, and what laws govern their changes and movements, instead of adopting any assumptions on these points as bases for reasoning; and, secondly, to endeavour in every case to render palpable to the eye the relations which have hitherto been presented only in elaborate catalogues or in tables of statistics. It is to subserve the latter purpose, which is obviously associated most intimately with the former, that I have adopted the method of equal-surface charting. In dealing with the proper motions of the stars I have drawn from each star an arrow whose length and direction shall indicate the rate and direction of the star's proper motion. I venture to believe that by these and other contrivances I have been enabled to exhibit the above-mentioned relations altogether more intelligibly than has hitherto been the case, and that laws hitherto unrecognised have thus been brought to light.

In the first place, I have been able to show that the stars, down even to the sixth magnitude only, are markedly crowded upon the milky way, and that, moreover, there are two rich stellar regions (as respects these lucid stars), nearly opposite each other, one covering the constellations Cepheus, Cygnus, Cassiopeia, and Draco, the other occupying the space included between the heel of Argo, Crux, and the Greater Dog. The following table (shortened from one in my "Essays") indicates the numerical relations involved, the numbers in the second column representing the total number which would be visible if the whole

* I may be permitted to point out that the researches themselves, as originally submitted to the Royal Astronomical Society, are contained in my "Essays on Astronomy."

heavens were as richly spread as the several regions indicated in the first column :—

	Richness.
Northern Milky Way	9,940
,, rich region	9,050
,, poor region (equally large) .	2,567
Gaps in Milky Way	1,240
Southern poor region	2,361
,, rich region (equally large) .	13,126
,, Milky Way	13,596

It will be observed that the richness of the whole Milky Way would be represented by 11,768, *exceeding more than nine times* the number representing the distribution of stars over the gaps and lacunæ of the Milky Way.

By extending the process of charting to include all the 324,198 stars in Argelander's series of forty charts, even more decisive evidence is obtained of the absolute intermixture in space—*i.e.*, within certain definite clustering aggregations—of stars of the brighter orders and the telescopic stars studied by the Herschels. For in the very regions where the Herschelian gauges showed the minutest telescopic stars to be most crowded, my chart of 324,198 stars shows the stars of the higher orders (down only to the 11th magnitude) to be so crowded that by their mere aggregation in the mass they show the Milky Way with all its streams and clusterings. This evidence, I venture to affirm, is altogether decisive as to the main question, whether large and small stars are really intermixed in many regions of space, or whether the small stars are excessively remote. It is utterly impossible that excessively remote stars could seem to be clustered exactly where relatively near stars are richly spread. This might happen, no doubt, in a single instance, but that it could be repeated over and over again, so as to account for all the complicated features seen in my chart of 324,198 stars I maintain to be utterly incredible.

By applying the process of equal-surface charting to the nebulæ, I find that they are so arranged over the heavens as to leave no room for doubting their association with the sidereal system. Moreover, they are found to run into streams or branches, intimately associated with streams of stars. And strangely enough, as the great star-streams seen in Eridanus and Aquarius are prolonged so as to enclose the Magellanic Clouds, so the great southern nebular streams extend themselves to the same groups. The association of these streams of nebulæ and stars could scarcely be regarded as accidental; but the fact that both sets of streams converge upon those very regions where stars and nebulæ are so strangely intermingled removes all doubt from the conclusion that in the nebulæ (for these streams include all the known southern nebulæ) we have not to deal with external galaxies, but with objects belonging to the sidereal system.

The charting of the stellar proper motions has further led me to the discovery of the fact that the stars in many parts of the heavens are travelling in systems—or as it were drifting—through space. Thus the five stars, β, γ, δ, ϵ, and ζ, Ursæ Majoris, are found to be all moving together, as one grand scheme.* In Gemini there is a much more remarkable instance of drift, nearly all the stars of that constellation (Pollux is one of the exceptions) travelling in one general direction. In Taurus there is also well-marked drift. These instances of drift suffice to afford independent testimony in favour of the existence of systems subordinate to the great sidereal system.

The general conclusions to which I have been led by these and other methods of research, which space will not permit me to particularise further, are chiefly these:—The sidereal system is altogether more complicated, altogether more varied in structure, than has hitherto been supposed. Within one and the same region co-exist stars of many orders of real magnitude, the greatest being thousands of times larger than the least. All the nebulæ hitherto discovered, whether gaseous or stellar, irregular, planetary, ring-formed, or elliptic, exist within the limits of the sidereal system. They all form part and parcel of that wonderful system whose nearer and brighter parts constitute the glories of our nocturnal heavens.

It has been supposed that the new views to which I have been led would, if accepted, tend to reduce our estimate of the scale on which the universe is constructed. This, however, is not the case. It is true that I cannot recognise in the clustering regions of the Milky Way the profundities commonly believed in; but I believe in profundities far vaster. It is true that I believe the varieties of structure

* More than two years ago, speaking of this discovery, which I had then but recently made, I expressed my conviction that if ever Dr. Huggins was enabled to determine the proper motions of these stars in the line of sight, he would find them to be all travelling one way, either receding or approaching. Quite recently he has tested the matter, and he finds that not only have these five stars similar spectra unlike the spectra of α and η, but that whereas α is approaching, and η receding more slowly, the five are all receding at the rate of thirty miles per second.

brought into view by higher and higher telescopic powers to be real and not apparent; but if I thus recognise the existence of stars much smaller than neighbouring orbs, it is because I recognise in the larger orbs suns which surpass our own thousands of times in volume; while this very variety enhances out conceptions of the wonders of the sidereal system. It is true, again, that I cannot recognise in the star-cloudlets the external galaxies spoken of in our text-books of astronomy, that I recognise parts of our sidereal system where hitherto the common opinion has been that outlying universes are in question; but I reason thus, because I have been led to the conclusion that our sidereal system is much more extensive than has hitherto been supposed. I do not draw the nebulæ inwards to the star-depths, but I extend the star-depths outwards until they include the nebulæ. According to my views, the range of our telescopes is neither greater nor less than has been hitherto supposed: if, then, I conceive that the sidereal system reaches to unknown depths beyond that range, it is not because I would reduce our estimate of the scale on which the sidereal system is constructed, but because I would enlarge that estimate, and that not by a little but indefinitely.

A few last words on the wonderful scene presented by the star depths. Let us reflect on what we know about our own earth and its wonders, what seems to our conceptions its vast extent, and the activity that pervades its every portion, whether we consider life upon its surface or the physical forces at work in all parts of its gigantic globe. Regarding this orb on which we live as one among many which attend upon the sun, all speeding with inconceivable velocity around that glorious star, how wonderful becomes the thought that the dimensions of all the members of the solar system—nay of the sun himself, are reduced to utter insignificance, compared with the range of that system. Next let us dwell upon the thought that that whole system is travelling bodily through space with utterly inconceivable velocity; and that the region of space thus traversed by the solar family is so vast that the dimensions of the solar system are in turn dwarfed to utter insignificance. Then let us picture the scheme of suns of which our sun is a member; not the sidereal system, scarcely even an appreciable fraction of the sidereal system, but the particular family of suns to which the sun belongs; and let us consider how the domain of the sun, the region of space over which he holds sway,—a region so vast that millions of years would be required for him to gather in from its outermost parts the material over which his sway extends, is in *its* turn reduced to mere nothingness by comparison with the scheme of suns of which our sun is a member. Then lastly—so far as human researches are concerned, but by no means lastly in real truth—let us picture to ourselves that the scheme of stars to which our sun belongs is but one of the atoms, so to speak, of which the frame of the sidereal system is built. We can speak of these things, but who shall understand them: who shall form adequate conceptions respecting them? The astronomer can spread out the figures which represent these wonders, but there his power ceases; he can neither conceive them himself nor render them conceivable by others. I know not, then, how I can more fitly draw my subject to a conclusion than by quoting that wonderful dream in which the German poet presents the feebleness of human conceptions in the presence of the infinite wonders of the universe. It may, indeed, truly be said, that on the one hand astronomical discoveries have made this dream a reality, while on the other, if we accept as well what the poet's words suggest as what they actually present, they exhibit the truest picture which words can convey of that universe which nevertheless is unpicturable:—

"God called up from dreams a man into the vestibule of heaven, saying, 'Come thou hither, and see the glory of my house.' And to the servants that stood around his throne he said, 'Take him and remove from him his robes of flesh, cleanse his vision, and put a new breath into his nostrils: only touch not with any change his human heart, the heart that weeps and trembles.' It was done: and with a mighty angel for his guide, the man stood ready for his infinite voyage; and from the terraces of heaven, without sound or farewell, at once they sailed away into endless space. Sometimes with the solemn flight of angel wings they fled through Zaharas of darkness, through wildernesses of death, that divided the worlds of life; sometimes they swept over frontiers that were quickening under prophetic motions from God. Then, from a distance that is counted only in heaven, light dawned for a time through a shapeless film; by unutterable pace the light swept to them, they by unutterable pace to the light. In a moment the rushing of planets was upon them; in a moment the blazing of suns was around them.

"Then came eternities of twilight that revealed but were not revealed. On the right hand and on the left towered mighty constellations, that by self-repetitions and answers from afar, that by counterpositions built up triumphal gates, whose architraves, whose archways, horizontal, upright, rested, rose, at altitude, by spans, that seemed ghostly from infinitude. Without measure were the architraves, past number were the archways, beyond memory the gates. Within were stairs that scaled the eternities around; above was below, and below was above, to the man stripped of gravitating body; depth was swallowed up in height insurmountable, height was swallowed up in depth unfathomable. Suddenly, as thus they rode from infinite to infinite, suddenly as thus they tilted over abysmal worlds, a mighty cry arose, that systems more mysterious, that worlds more billowy, other heights and other depths, were coming, were nearing, were at hand.

"Then the man sighed and stopped, shuddered and wept. His overladen heart uttered itself in tears; and he said, 'Angel, I will go no farther; for the spirit of man acheth with this infinity. Insufferable is the glory of God. Let me lie down in the grave, and hide me from the persecution of the infinite, for end I see there is none. And from all the listening stars that shone around issued a choral voice, 'The man speaks truly; end there is none that ever yet we heard of!' 'End is there none?' the angel solemnly demanded: 'Is there indeed no end? And is this the sorrow that kills you?' But no voice answered, that he might answer himself. Then the angel threw up his glorious hands to the heaven of heavens saying, 'End is there none to the universe of God? Lo! also there is no beginning.'"

IV. MEDIÆVAL AND MODERN ORDNANCE AND PROJECTILES COMPARED.*

By Capt. S. P. OLIVER, Royal Artillery.

THANKS to the deep researches of Major-General Lefroy (now Governor of Bermuda), Captain H. Brackenbury, and others, the history of European and foreign ordnance, from its hazy origin in the middle ages down to modern times, is tolerably familiar to all military

* I. "On Two Large English Cannon of the Fifteenth Century, preserved at Mont S. Michel, in Normandy." By Brigadier-General LEFROY, R.A., F.R.S. —"Proc. Roy. Art. Institution, Woolwich," vol. iv., No. 1, 1864.

II. "Mons Meg, the Ancient Bombard, preserved at Edinburgh Castle." By J. HEWITT, Esq., War Office.—"Journal of Archæological Institute," No. 37.

and naval artillerists through the medium more especially of the periodical publications of the Royal Artillery Institution at Woolwich.

On the contrary, however well up (thanks to the daily press) in the size, weight, and power of contemporary cannon, and in the velocity and penetration of elongated projectiles, &c., the public are too apt to regard with contempt what they consider the puny prototypes of our present magnificent guns; and perhaps would be surprised to learn that the cannon and missiles, both European and Oriental, whilst the art of their construction was yet rude and still in its infancy, attained a size and weight equal to, if not exceeding, our largest and latest pieces and projectiles.

The Orientals were far in advance of the Europeans in their acquaintance with metallurgy, and there is good authority for supposing that bronze was known to them at a remote age, and, according to Fergusson, "the Indians were as familiar with the use of iron in the fourth century B.C., as the Greeks themselves were, and for anything we know to the contrary, may have understood the art of extracting it from the ore and using it for arms and cutting tools before these arts were practised in Europe" ("Rude Stone Monuments," p. 480). Indeed, it is not impossible that bullets were used in Asia when our ancestors were not beyond flint arrow-heads; at all events it seems certain that gunpowder was used in India and China before the Christian era. We are apt to take a certain pride in our forgings when we read of iron bars 270 feet* in length being coiled on a mandril to form the coil of a gun in the nineteenth century A.D.; but Mr. Fergusson rather takes the conceit out of us when he informs us of the existence of an iron pillar in the courtyard of a mosque near Delhi, consisting of a solid shaft of wrought-iron standing 22 feet 6 inches out of the ground, and 26 feet deep in the ground, that is nearly 50 feet in total length, and 5 feet 6 inches in circumference. Mr. Fergusson adds that such a single

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III. "Ancient Cannon in Europe." By Lieut. H. BRACKENBURY, R.A.—"Proc. Roy. Art. Institution, Woolwich," vol. iv., No. 10, 1865, *et. seq.*

IV. "An Account of the Great Cannon of Muhammad II., recently presented to Her Majesty by the Sultan, with notices of other great Oriental Cannon." By Brigadier-General LEFROY, R.A., F.R.S.—"Proc. Roy. Art. Institution," vol vi., No. 6, 1868.

V. "Notice of a Stone Cannon Ball Dredged up in Falmouth Harbour, and of the use of Stone Shot." By Sir Charles LEMON, Bart., M.P., F.R.S.—"Twenty-sixth Annual Report of the Royal Institution of Cornwall." Truro, 1845.

* The bar of iron to form coil for new 36-ton gun will be 1200 feet in length.

forging "could not have been made in Europe before the introduction of steam machinery, nor, indeed, before the invention of the Nasmyth hammer." There is an inscription on the pillar which leaves no doubt that the pillar must have have been erected in the third or fourth century of our era. Mr. Fergusson leaves it "to those practically skilled in the working of metals to explain how any human being could work in close proximity to such a mass heated to a welded heat, or how it was possible without steam machinery to manipulate so enormous a bar of iron." If Mr. Fergusson considers that it was necessary to heat at one time the *whole* pillar to a *white* heat it was doubtless a difficult task to manipulate it, although a man need not be a human salamander to approach it closely; and when cooled to the *cherry-red* heat requisite for forging it could easily be approached without inconvenience. But in all probability this pillar was built up, being formed in lengths placed from opposite sides in the furnace, and welded under a huge stone or metal hammer, which was probably lifted and dropped after the style of a pile-driving monkey. Indeed among the Chinese there is in use (and among the Chinese that means also has been in use for the last thousand years) a hammer or stamper not dissimilar to the "*Schwartz hammer*" or "belly-helve" of modern days. A colossal hammer of this description could readily be erected and used by artificers unacquainted with steam machinery.*

The earliest authentic record of the existence of cannon in Europe is found in a document dated 11th February, 1326 A.D., and fifty years afterwards we read of cannon throwing projectiles of 200 lbs. weight at the seige of Odruik, and a contemporary chronicle also gives an account of a 450-pounder gun being used by the Duke of Burgundy in 1377, the diameter of whose bore must have been at least 21 inches. In Italy, however, the manufacture of cannon developed most rapidly. Capt. H. Brackenbury gives an interesting account of the performances of two huge Venetian bombards in 1380 at Brondolo. One named the "*Trivisana*" was

* Capt. C. B. Brackenbury, R.A., describes the endurance of the men at Mr. Krupp's Essen establishment as being severely tested when large steel castings are made. He says, "It has occurred that some of them have shaken their heads about undergoing so terrible an "*ordeal by fire*" during hot weather, but the energy of Mr. Krupp has carried all before him, and by extra pay for heavy work, and exciting their undoubted *esprit de corps*, they have been brought to face anything, although many of them have fainted under the trial when large castings have necessarily been made in hot weather."—*Vide* Report of a Professional Tour of Officers of the Royal Artillery under Col. A. C. Pigou, R.A., 1865, p. 116.

a 295-pounder, and the other, the "*Veneziana*," was a 145-pounder. "Pisani established these guns in battery on the bank, and proceeded to bombard Brondolo. On the 22nd January the great bombard was fired, and it struck the Campanile of Brondolo, knocking down a great piece of the wall, the stones of which struck and killed the Genoese general and his nephew. On the 23rd the same bombard knocked down a great piece of the wall of the same Campanile and killed twenty-two men. Capt. Brackenbury, after tracing the history of cannon through the second half of the fourteenth century, states in the following few pertinent words the conclusions to which he is led:—"Cannon of iron and bronze under the various names of *guns*, *bombards*, *cannon*, *sclopi* or *schioppi*, are found bound down to large heavy wooden beds, and employed in sieges both for attack and defence throughout the whole period. Projectiles of lead, stone, and, in Italy, iron, and even bronze were thrown by them; also arrows and Greek fire. But it appears from the length of time which sieges lasted that the art of opening a practicable breach by means of cannon had not yet been invented. Indeed it is very doubtful whether with such powder sufficient force could have been obtained for that purpose. This powder was still a comparatively feeble agent; the ingredients pounded by hand in a mortar were themselves but imperfectly purified, and when reduced to a state of fine powder the gas must have passed very slowly through the mixture, and an immense quantity of the charge must have been blown out without being ignited. To prevent excessive windage the leaden shot were driven forcibly home into the bore of the piece by means of a mallet and drift, and the soft nature of the metal allowed them completely to fill the bore. With iron and stone shot fired from the large guns no drift was used, but the shot was inserted from the muzzle and the powder by a scoop from the breech, which was then closed by a wooden tompion; the hot iron was still used to fire the charge through a vent, which was often covered to keep the powder dry. But rough as these appliances were *we must not despise too much the cannon of the fourteenth century*. They were suited to the age. To knock down such a piece of wall as to kill twenty-two men at once is a feat which is not easy even in these days."

Fortunately for military archæologists, there are in existence good examples of early ordnance whose date is well established; the oldest known being two English guns abandoned at the siege of St. Michel, Normandy, at the end of October, 1423, and which now remain in front of the

Plate I.

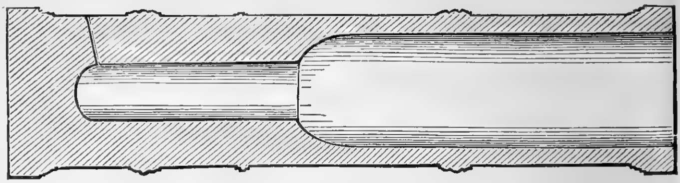
I.

TZAR-POOSCHKA.

THE GREAT GUN OF MOSCOW.

Cast A.D. 1586.

Diameter of bore, 36 in.; diameter of chamber, 19 in.; length of bore, 122 in.; length of chamber, 70 in.; total exterior length, 210 in.; weight, 38·5 tons.

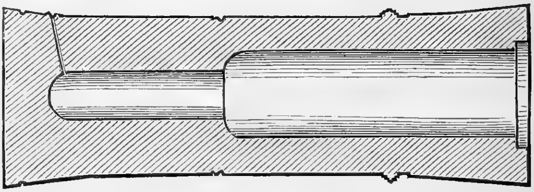


II.

GREAT GUN OF BEEJAPOOR.

THE MALIK-I-MYDAN, OR MASTER OF THE FIELD.

Cast A.D. 1548.

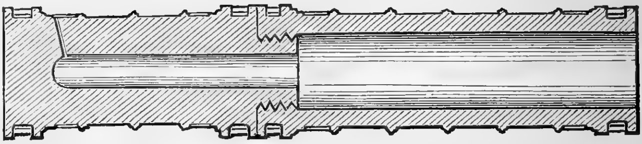


III.

CANNON OF MUHAMMAD II.

(*A.D.* 1464).

Length, 17 ft.; diameter of powder chamber, 10 in.; diameter of bore, 25 in.; weight, 18 tons 14 cwts. 3 qrs. The stone shot weigh 670 lbs.



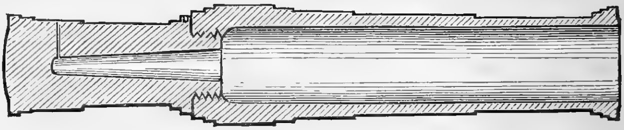
IV.

THE DULLE GRIETE.

WROUGHT-IRON GUN AT GHENT.

Probable Date A.D. 1430.

From measurements by Professor W. Pole, F.R.S., September, 1864.



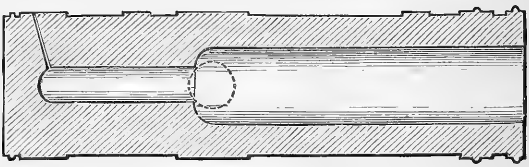
V.

GREAT GUN OF AGRA.

DHOOL DHANEE.

Cast A.D. 1628. *Broken up and sold for the metal*, 1832.

From a drawing by Captain J. F. Boileau, Bengal Engineers, April 28, 1832.

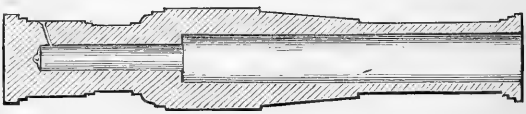


VI.

MONS MEG.

A.D. 1460.

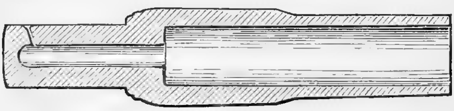
EDINBURGH.



VII.

MICHELETTE.

LA GRANDE.



VIII.

MICHELETTE.

LA PETITE.

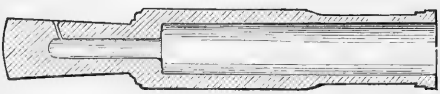
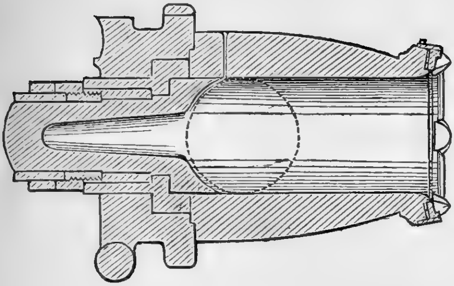


Plate II.

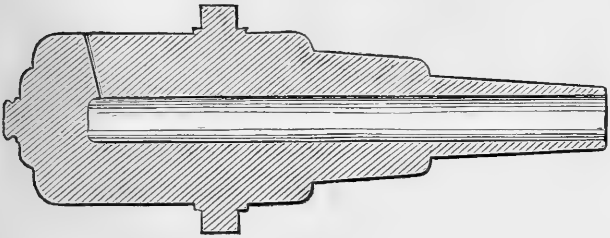


I.

MALLET'S 36-INCH MORTAR,

OF 42 TONS.

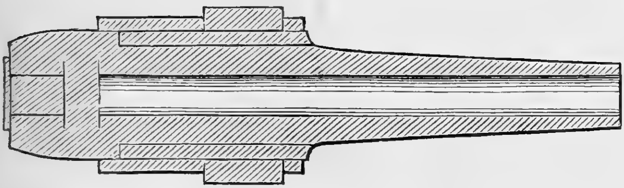
Tried at Woolwich, 1857-8.



II.

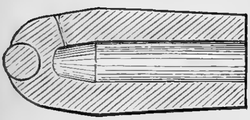
12-INCH WROUGHT-IRON MUZZLE-LOADING GUN, 35 TONS.

Rifling:—No. of grooves, 9; depth of groove, 0·2″; width, 1·5 (*i.e.*, same as 10-inch); twist, from 0 to 1 in 35 cals.



III.

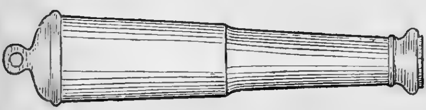
KRUPP'S 11-INCH PRUSSIAN BREECH-LOADING GUN.



IV.

13-INCH SEA-SERVICE MORTAR.

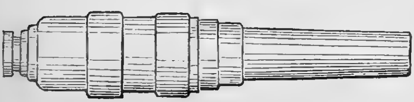
Weight, 100 cwts.



V.

68-POUNDER.

95 cwts.



VI.

ARMSTRONG.

7-INCH BREECH LOADER.

second gateway of the fortress on Mont St. Michel. Professor Pole took great trouble in measuring these guns and the granite cannon balls he found within them in 1863. He estimates the weight of the larger gun at about 5⅓ tons, and that of the smaller at 3¼ tons. He says the largest gun is 19 inches in calibre, and 12 feet in total length, of which the chamber composes more than one-fourth; the granite balls are about 18 inches in diameter; the smaller gun is 15 inches in calibre, and 11 feet 9 inches in total length. The construction of the barrels is clearly visible; they are formed of wrought-iron, being made up of longitudinal bars, each about 2¾ inches wide by 1 inch thick, and round the outside are seen the lines of hoops, each about 2¾ inches wide placed quite close to each other. It is not possible to discover whether the hooping is single or in several layers.

The exterior of the breech or powder chamber consists not of hoops but of *longitudinal bars,* their flat surfaces giving to it the section of a polygon. The next gun in point of age is the far-famed "Dulle Griete," the great bombard of Ghent, which dates about 1430. This enormous wrought-iron piece is calculated to weigh 13 tons, and has a bore of 25 inches in diameter. Professor Pole estimates the weight of the granite ball, 24 inches in diameter, to be about 700 lbs. The next on General Lefroy's list is that "*mickle-mouthed murtherer,*" Mons Meg,* the ancient bombard preserved at Edinburgh Castle, which is supposed to have been built previous to 1460. She weighs 5 tons, and the diameter of her bore, according to Lieut. Bingham's† drawing, is 20 inches. Mr. Hewitt mentions the mode of construction of this Scottish gun as being plainly shown at the point where it has been "*riven.*" Longitudinal strips of iron are ranged like the staves of a cask and welded together; and then a number of rings or hoops, also of wrought-iron, are driven tightly over them. The thickness of the bars is 2½ inches; that of the hoops 3½ inches. There is no core beneath the strips, as in some of the early bar and hoop guns; but the welded staves themselves form the cylinder. The magnitude of this engine, the contrivance of its parts, and the nice proportions of its outline, show that it is by no means one of the earliest efforts of the gunsmith's art." "The name of Mons borne by this bombard is generally attributed to its

* Mons Meg was brought into action at the siege of Dumbarton, 1489, and at Norham, 1497, in the reign of James IV. of Scotland, and Henry VII. of England.

† The late Col. Charles Bingham, R.A., Deputy Adjutant-General, Royal Artillery.

having been fabricated at the town of that name in Flanders; and the probability seems to gather strength from the circumstance of the great gun of Ghent resembling it so closely in model and construction. These mediæval wrought-iron bar and hoop guns were manufactured up to the middle of the sixteenth century; and there are yet in existence in the Tower and in the Repository at Woolwich some very perfect specimens, which, together with brass cannon, were recovered by Mr. John Deane in the summer of 1840 from the wreck of the "Mary Rose" at Spithead, which "goodlie shippe of England was by too much folly drowned in the middest of the haven; for she was laden with much ordinance, and the portes left open, which were very lowe, and the great ordinance unbreached; so that when the shippe should turne the waters entered, and so dainly she sank." This occurred during the reign of Henry VIII., in 1545, the very year in which cast-iron cannon were first made in England. One of the guns is in an excellent state of preservation, considering it to have been immersed for 300 years: it is composed of a tube of iron, whose joint or overlap is as its length; upon this is a succession of iron hoops composed of iron 3 inches square; these appear to have been driven on whilst red-hot. It has a movable breech chamber, and is let into a solid block of elm, hollowed out to allow of the breech-piece being drawn back for loading, a wooden chock and iron wedge being used to keep the chamber in position when forced home. Both the brass cannon (then a new invention according to Grose, who dates the first founding of brass cannon in England as 1537), and these hoop and bar guns were found loaded, the brass guns with iron shot, and the iron guns with stone shot. Stone shot appear to have been common up to this period, as we learn in Rymer's *Fœdera* of two hundred stone shot being made in the Folkestone limestone quarries as early as the 17th year of Edward III., 1334; and in the same author is an order of Henry V. to a mason of Maidstone to provide 7000 stone shot from the quarries there, which, like the Folkestone beds, consists also of the lower greensand limestone formation, and, besides, several ancient cannon shot have been found there in the *débris* of the old workings. We cannot leave this portion of the subject without regretting that Captain Brackenbury has not yet been able to carry on his history of the ancient cannon in Europe beyond the date of 1400, since February, 1866. The Artillery Institution has looked for Part III. on the ordnance of the fifteenth century, which is not yet forthcoming. We must hope that the

Professor of Military History at the Royal Military Academy will find leisure to publish a continuation of his interesting history.

We will now turn the attention of the reader to the Oriental cannon, and it should be noticed that General Lefroy points out how these Indian and Russian ordnance were in all probability copied from Flemish originals. Comparing the gun of Muhammad II., presented by the Sultan to the Queen in 1868, and now at Woolwich, with the Ghent Bombard, the General finds the dimensions, allowing for the necessary difference between wrought-iron and bronze, to correspond more closely than can be attributable to accident, and this correspondence extends to the method of construction in both cases, the powder-chamber being a separate forging or casting, and screwed to the body.

In proof of the family resemblance of all the great bombards which is so apparent on comparing drawings made to the same scale, General Lefroy subjoins the following table of their principal dimensions:—

List of Great Bombards Now or Lately Extant.

Nature.	Date.	Weight in tons.	Diameter. Bore.	Diameter. Chamber.	Diameter. Exterior.	Length. Bore.	Length. Chamber.	Length. Over All.
Wrought-Iron:—			"	"	"	"	"	"
English guns now at Mont S. Michel .. (A)	Before	5·3	19·0	5·75	30·0	94·0	48·0	197·0
English guns now at Mont S. Michel .. (B)	1423	3·3	15·0	5·01	22·0	80·0	36·0	156·0
Dulle Griete, Bombard of Ghent	After 1430	13·0	25·0	10·00 / 6·00	39·0	1272·0	56·0	197·0
Mons Meg, Edinburgh ..	Before 1460	5·7	20·0	10·00	29·5	106·0	45·0	159·0
Bronze:—								
Cannon of Muhammad II., Woolwich	1464	18·7	25·0	10·00	41·5	110·2	75·8	204·7
Malik-I-Mydan, great Gun of Beejapore	1548	40·0	28·5	16·00	57·0	93·0	60·5	170·6
Tzar Pooschka, great Gun of Moscow	1586	38·5	36·0	19·00	63·0	122·0	70·0	213·0
Dhool Dhanee, great Gun of Agra	1628	30·2	23·2	10·05	45·5	108·0	50·0	170·2

The hugest piece of ordnance ever known to have existed was a celebrated cast-bronze piece at Agra, named the *Z*ufr Bukh, Giver of Victory, which weighed fifty-two tons, according to the account extracted by Major-General Boileau, R.E., from a pamphlet in the Hindoostanee language, by which the date of its fabrication appears to be anterior to A.H. 1037, *i.e.* A.D. 1627. What has become of this gun does not appear. Next in importance is the renowned great gun of Beejapore, Malik-i-Mydan, or Lord of the Plain, forty tons in weight, cast at Ahmednugger in the reign of Boorhan Nizan Shah I., A.H. 956 or A.D. 1548, under the superintendence of the General of Artillery, Muhammad Vin Hassan Roumi, that is, of Constantinople. Notwithstanding its great weight, Colonel Meadows Taylor considers that this gigantic cannon could be brought to Bombay, *viâ* Sholapore, whence it could easily be transported to the Repository at Woolwich. General Lefroy says:—

"The superstition of the Hindoos long ago converted this gigantic cannon into an object of worship, and they might be seen placing offerings of flowers and copper coins within the muzzle. It is believed to have been last fired on the occasion of a visit of the Rajah of Sattara to Beejapore in the last century, and the people gravely assert that it caused all the pregnant women within hearing to miscarry. The charge was 80 lbs. of powder. The shot, of which several remain, are made of fine hard basalt, and weigh about 1000 lbs. It is mentioned in the "Journal of the Royal Asiatic Society of Bengal" (Vol. I), that an Italian who served in the Mogul armies under the title of Rumi Khan, had this gun in his park of artillery, and used it in several battles, occasionally firing sacks of copper coins out of it."

The Dhool Dhanee, "The Disperser or Scatterer," or great gun of Agra, cast in 1628, was broken up and the material sold for over £3000 in 1832; fortunately Major-General Boileau took careful measurements of it before its economical and ignoble destruction. That these huge pieces were by no means uncommon appears from the fact, that the Malik-i-Mydan was one of 701 cannon abandoned in the disastrous retreat from Kulliani, A.D. 1562. Many other fine specimens of Indian guns, such as the large Bhurtpoor cannon at Woolwich, cast as late as 1677, might be quoted to show the remarkable skill in metallurgical art possessed by the Asiatic races, in which the Chinese and Japanese were not behindhand. Perhaps the most remarkable gun, being the largest in calibre, viz. 36 inches, ever cast, is the work of the gunmaker Andrea Ichowhow, of Moscow, A.D. 1586, called the Tzar Pooshka, weight 38 tons. The outline of this gun (Plate 1) is reduced from Plate 5, copied from a drawing obtained by General Lefroy from Major-General de Novitzky, Aide-de-camp to the Emperor of Russia.

We now come to the great cannon of the Dardanelles, which "have been a subject of wonder to travellers and of interest to artillerymen from the earliest period. There are no other examples of guns which have remained in use for four centuries, and are still in a very real sense effective pieces of ordnance. They testify to the former energy and power of the Ottoman race as no other military monument does, and remind us of an event which has had a greater influence on the politics of Europe than almost any other within the same period—the fall of Constantinople. Monuments of the military genius of Muhammad II., they remind us also of "the splendour and the havoc of the East," by their prodigious size and cost and power. They form a class apart, and although there is reason to think that they are referable to a Flemish original, they bear the stamp of a national character and of an epoch of conquest of which European history presents scarce any other example." These guns were formerly very numerous, there being between forty to fifty of them at least. According to Mr. Wrench, H.M. Vice Consul, there were extant—twenty-one in 1868, three of which were since broken up, and two others sentenced to the same end; so excluding the one presented to Her Majesty there now remain fifteen: ten of these, comprising the largest (29·5 inches calibre), are in the Fort Kilit Bahar, on the European side of the Dardanelles, and the remaining five in the Sultanieh Fort of Chanak Ckalessi, on the Asiatic side of the straits. The Kilit Bahar guns average 25·25 inches calibre, the smallest bore being 19·5, and the Chanak guns 23·6 inches; the date of their construction extends from A.D. 1458 to 1521. General Lefroy surmises that those guns dated 1521 may possibly have been cast on the island of Rhodes for the subjection of that fortress, which fell December 22nd, 1522; and there is all the more reason for supposing this to have been the case, when we consider the Turkish habit of casting great ordnance on the spot where they were wanted with extraordinary energy and readiness. Thus, "in the first siege of Rhodes, 1480, Muhammad caused sixteen great pieces to be cast, called basilisks or double cannon, 18 feet long, and carrying a ball of 2 or 3 feet diameter; and here also we are told that their mortars "threw stones of a prodigious size, which flying through the air by the force of powder fell into the city, and lighting upon houses broke through the roofs, made their way through several stories, and crushed to pieces all that they fell upon; nobody was safe from them, and it was this kind of attack that gave the greatest terror to the Rhodians.

General Lefroy refers to the singularity of these guns three or four centuries old taking part in modern engagements, as in the memorable instance afforded in the passage of the Dardanelles by Sir John Duckworth's squadron, on March, 1807.*

The Dardanelles proper, it must be remembered, is where the channel is narrowed to little more than three quarters of a mile. On each side of this narrow strait stands a castle mounted with these heavy cannon. They are called the inner castles of Europe and Asia, or the castles of Sestos and Abydos; so it will be noticed that these guns could easily fire across the strait as Bishop Pococke, writing about 1740 quaintly observes, "they fire likewise with ball in answer to any ship that salutes the castle, as this does much damage where they fall, so the lands directly opposite pay no rent. James† in his naval history gives the following account:—"It was at 7 a.m. on the 19th February, 1807, that Sir John Duckworth's squadron weighed and steered for the entrance of the Dardanelles. The British ships then formed themselves in line of battle in the following order:—*Canopus, Repulse, Royal George, Windsor Castle, Standard*, having in tow the *Meteor, Pompée, Thunderer*, having in tow the *Lucifer, Endymion, Active.* At 8 a.m. the *Canopus* arrived abreast of the outer castle, both of which opened fire upon her. At 9.30 a.m. the leading ship of the British squadron arrived abreast of the inner pair of castles which also opened fire within point-blank shot; this fire was returned by the ships of the squadron in succession as they passed, and doubtless with some effect. The damage sustained by the British ships in passing the Dardanelles, for that object had now been attained, was comparatively trifling." Not a mast or yard had been shot away, but only a few spars injured. The total—six killed and twenty-one wounded." Then follows the account of the destruction of the Turkish squadron, which was burnt and blown up as it lay beyond Abydos, whilst Lieutenant Nichols, R.N., with Lieutenant Finmore of the Marines, and party, landed and took by assault a redoubt, from which the Turks fled at their approach; he then set fire to the gabions and spiked the guns, eight of

* Baron de Tott relates how he saw one of these pieces loaded with 330 lbs. of powder and discharged. He observed the shot break into three pieces at 600 yards from the gun, and these pieces crossed the Dardanelles, leaving the surface in a foam where they struck, and went bounding up the opposite shore.

† "Naval History of Great Britain, from the Declaration of War by France in 1793, to the Accession of George IV." By WILLIAM JAMES. New Edition. Bentley, 1860. Vol. iv., p. 209 *et seq.*

which were of brass, and carried *immensely large marble balls.*

It was in repassing or rather escaping through the Dardanelles on the 3rd of March following, that Sir John Duckworth's vessels suffered such discomfiture from the stone shot. The ships then proceeded down the channel in nearly the same order in which they had sailed up. Hoping to propitiate the Turks Sir John fired a salute of thirteen guns, this produced an immediate return of shot and shell from the two castles, and from the battery on Point Pesquies. The other batteries on both sides, successively as the ships arrived abreast of them, opened their fire, and received a fire in return. The mutual cannonade was kept up until nearly noon, when the British squadron anchored off Cape Janizary, out of the reach of further molestation.

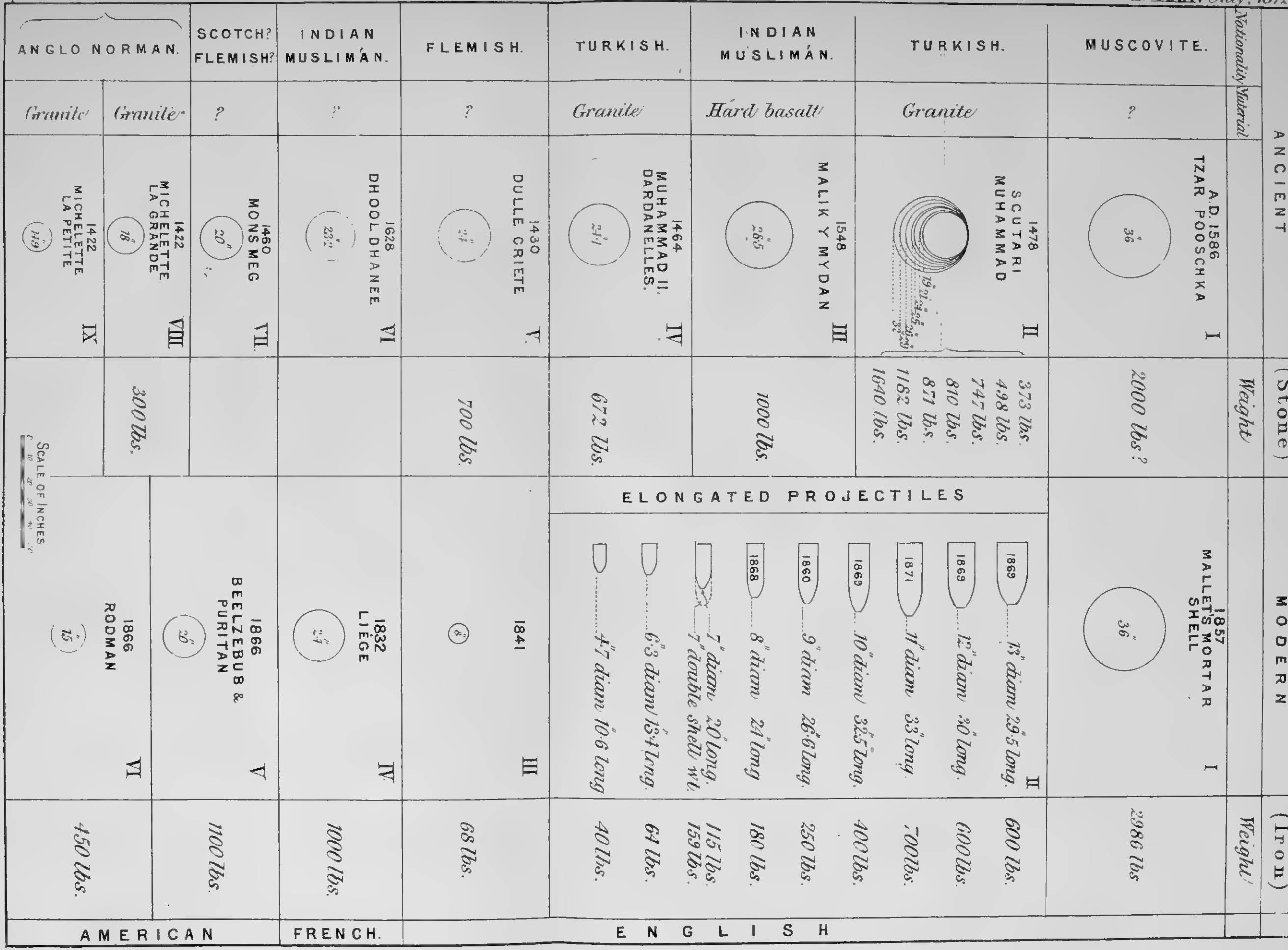
The following is the detail showing the damage caused by the stone shot:—"The *Canopus* had her wheel carried away and her hull much damaged by the stone shot, but escaped with the loss of only three seamen wounded. On board the *Repulse,* a stone shot from the castle on the Asiatic side came through between the poop and the quarter-deck, and killed two quartermasters, five seamen, and three marines, and wounded one lieutenant of marines, two corporals, and five privates, also two quartermasters and a boatswain's mate: total, ten killed and ten wounded, the only loss which the *Repulse* on this occasion sustained. The same shot badly wounded the mizen mast, broke and carried away the wheel, and did other serious damage. The *Royal George* had several lower shrouds cut away, her masts slightly wounded; a large stone shot also stuck fast in her cutwater. Her loss amounted to two seamen and one marine killed, two officers, one petty officer, twenty-two seamen, and two marines wounded: total, three killed and twenty-seven wounded.

"A stone shot of 800 lbs. weight struck the mainmast of the *Windsor Castle* and cut it more than three quarters through; her loss amounted to three seamen killed, one petty officer and twelve seamen wounded. On board the *Standard,* a stone shot from the castle of Sestos, weighing 770 lbs., and measuring 6 feet 8 inches in circumference, and 2 feet 2 inches in diameter, entered the lower deck, killed four seamen, and having set fire to the salt boxes which were on the deck for immediate use, caused an explosion, that badly wounded one lieutenant, three petty officers, thirty-seven seamen, and six marines; the alarm of fire that followed the explosion caused four seamen to leap overboard, all of whom were drowned, making the *Standard's* total loss by this single shot (and which was all she sustained) amount to eight killed and drowned, and forty-seven wounded. The *Pompée* had the good fortune to escape without being struck by a shot in hull, masts, rigging, or sails. The *Thunderer*, on the other hand, was a good deal damaged, and had two seamen killed, and one lieutenant, one midshipman, ten seamen, and two marines wounded. The *Lucifer* had no one hurt. The *Active* received a granite shot weighing 800 lbs.,* and measuring 6 feet 6 inches in circumference, which passed through her side 2 feet above the water, and lodged on the arlop deck close to the magazine scuttle, without injuring a man. The aperture made by it was so wide that Captain Mowbray, on looking over the side to ascertain what damage it had done, saw two of his crew thrusting their heads through at the same moment. Had there been a necessity for hauling to the wind on the opposite tack she must have gone down."

Such was the last appearance, in European waters at least, of huge stone cannon-shot during action, and if they did as much execution in proportion during the siege of Scutari, from June 22nd to the 21st July, 1478, inclusive, a period of thirty days, they must have caused tremendous destruction. At first there were two guns only placed in battery against the place, which were gradually increased to eleven towards the end of the siege; these guns varied in calibre from 19·8 inches to 32·4 inches, and their weight from 373 lbs. to 1640 lbs. Each gun fired on an average 16 shots a day, giving a total of 2534 rounds. The weight of such a number of stone shot, which must have weighed at least 1000 tons, leads General Lefroy to suppose that these were cut on the spot, being dressed into spherical form from blocks in neighbouring quarries by slave labour. The relative sizes of these shot are shown in the diagram. We will hope that our ironclad fleet of the present day would not have to retreat after the fashion of Sir John Duckworth's squadron, although by this time 10-inch elongated projectiles have probably replaced the stone shot in the Dardanelles.

* General Lefroy considers the weight of the last shot exaggerated, and suggests that the boatswain put his foot into the scale, as a ball 78 inches in circumference will be under 25 inches in diameter, and not weigh more than 760 pounds. Some of these stone shot are at the Tower, others in the gun wharf at Portsmouth, at Sir John Duckworth's seat in Devonshire, and one at Admiral Tucker's, Trematon Castle, Cornwall.

RELATIVE SIZE OF ANCIENT & MODERN PROJECTILES



Vincent Brooks Day & Son Lith.

Having thus briefly glanced at the ordnance of Mediæval times, and followed its characteristics as far as the time of the Tudors, we may here notice that ever since that period the architecture of artillery has been almost at a stand still; and the accompanying table of the time of Charles I. will give a good idea of the size of British Service Ordnance and Ammunition in use during the seventeenth century; and we know that those of the eighteenth century were not far different until 1779-80, the period when systematic improvements commenced in the manufacture of carronades, followed by General Blomefield's long naval pattern guns, Congreves, Dickson's, Millar's, Monk's, and Dundas's, the latest guns cast in the service.

Extracted by Colonel Clark, R.A., F.R.S., from "The Complete Souldier." Auctore Thomas Smith, 1628.

The Names of the Great Ordnance now used.	Height of diameter of each piece, in inches and parts.	Height of the bullet in inches and parts.	Weight of the shot in lbs. and parts.	Compass of the shot in inches and parts.	Weight of corne powder to charge each piece in lbs. and parts.	Weight of the piece in lbs. about.	Length of the piece in feet about.
Cannon	8	$7\frac{1}{4}$	64	$25\frac{1}{7}$	32	8000	12
Cannon Serpentine ..	$7\frac{1}{2}$	$7\frac{1}{4}$	52	$23\frac{4}{7}$	26	7000	$11\frac{1}{2}$
French Cannon	$7\frac{1}{4}$	7	$46\frac{3}{4}$	$22\frac{11}{14}$	$23\frac{1}{4}$	6500	12
Demi-Cannon eldest ..	$6\frac{3}{4}$	$6\frac{1}{2}$	$36\frac{5}{8}$	$21\frac{3}{14}$	20	6000	$11\frac{1}{4}$
Demi-Cannon ordinary	$6\frac{1}{2}$	$6\frac{1}{4}$	32	$20\frac{3}{7}$	18	5600	$10\frac{1}{2}$
Demi-Cannon	6	$5\frac{3}{4}$	$24\frac{1}{2}$	$18\frac{6}{7}$	16	5000	11
Culvering	$5\frac{1}{2}$	$5\frac{1}{4}$	19	$17\frac{2}{7}$	15	4600	$13\frac{1}{4}$
Ordinary Culvering ..	$5\frac{1}{4}$	5	$16\frac{1}{4}$	16[illegible]	$12\frac{1}{2}$	4300	12
Demi-Culvering ..	$4\frac{1}{2}$	$4\frac{1}{4}$	$11\frac{3}{4}$	$14\frac{1}{7}$	9	3000	11
,, something less .	$4\frac{1}{4}$	4	9	$13\frac{5}{14}$	$7\frac{2}{3}$	2300	10
Saker ordinary	$3\frac{3}{4}$	$3\frac{1}{2}$	6	$11\frac{11}{14}$	5	1900	$9\frac{1}{2}$
Sakeret or Minion ..	$3\frac{1}{4}$	3	$4\frac{3}{4}$	$10\frac{3}{14}$	$3\frac{1}{2}$	1100	8
Falcon	$2\frac{3}{4}$	$2\frac{1}{2}$	$2\frac{1}{3}$	$8\frac{9}{14}$	$2\frac{1}{4}$	750	7
Falconet	$2\frac{1}{4}$	2	$1\frac{1}{8}$	$7\frac{1}{14}$	$1\frac{1}{2}$	400	6

Notwithstanding, the science of the theory of gunnery regarding the flight of projectiles through the air were not thought unworthy the careful notice of such illustrious philosophers as Leonardo da Vinci in 1452, followed by Tartaglia (time of Henry VIII.), and Galileo, in his dialogues on motion, published 1646. Later, the names Sir Isaac Newton, Robins, 1742, Hutton, and Sir J. Herschel, bring us down to our own times. But although modifications had been occasionally made in the manufacture of ordnance, the general principles of construction remained unaltered. Captain Stoney tells us, that "Anyone who examines the old guns in the Tower of London, or in the Museum of Artillery, at Woolwich, may see that they are of the same genus as modern smooth-bores, and even notice some specimens quite as soundly and artistically cast as any of those of the present century; nay more, he may infer that our modern cast guns can scarcely be superior to their prototypes in range, power, or susceptibility to rifling.* Captain Stoney attributes this stagnation in the construction of rifled ordnance to the backward state of metallurgical science and mechanism, and not to the ignorance of theory. Thus the manufacture of cast-steel in large masses has only recently been accomplished, whilst rifling machines now work accurately and true to within $\frac{1}{1000}$ of an inch.

But, after all, it is not with rifling that we have to do in this paper, which is intended more to compare the size and weight of the pieces of ordnance ancient with modern, and to let alone all questions of their comparative energy, velocity, and penetrative power. Having premised this, we now proceed to consider our modern monster ordnance. As first examples, although not illustrated, should be quoted the French mortars, with shells of 13-inch diameter, which they threw into Cadiz from a long range at its siege in 1810. These notable pieces of ordnance (their weight is not given, but one of them may still be seen raised as a trophy in St. James's Park, London), were invented by Colonel Villantroys, and called cannon-mortars; they were cast at Seville, and being placed in slings, threw projectiles over Cadiz, a distance of more than 5000 yards.† To obtain this flight the shells were partly filled with lead, and the reduced bursting charge was too small for an effective explosion. Towards the latter end of the siege of Antwerp, in 1832, a mortar cast at Liège was placed in position against the citadel. It weighed 7 tons, and threw shells of 23·6226 inches diameter, which did but little actual execution, but produced a great moral (or rather demoralising) effect. This mortar was afterwards burst at practice with a charge of 9 kilos. (about 20 lbs.). Although the French adopted the Paixhan shell-guns as early as 1822, and General Millar introduced 10-inch and 8-inch shell-guns

* "Text-Book of the Construction and Manufacture of Rifled Ordnance in the British Service." By Capt. F. S. Stoney, R.A., and Lieut. Charles Jones, R.A. Royal Gun Factories, 1872.

† At the siege of Cadiz cast-iron shells filled with lead, forming projectiles of great strength and density, were thrown from mortars to a distance of 3¾ miles. *Vide* "American Artillery Course," p. 69.

in 1833, the effect of horizontal shell fire from guns of *very large bore* was only fully appreciated by the British authorities in 1855, in the early part of which year two experimental malleable-iron guns of 13-inch bore were ordered of Mr. Nasmyth, but the idea was given up and the guns counter-ordered. In the following year Messrs. Horsfall, of the Mersey Ironworks, Liverpool, completed the first wrought-iron 13-inch gun of 22 tons weight, length 15 feet 10 inches, which projected with great success solid shot each weighing 280 lbs. to a range of 6000 yards, with a charge of 50 lbs. of powder at a point-blank elevation. This famous piece, the first of its kind, was presented to the nation, and is now mounted at Tilbury Fort. But slightly subsequent in date to the last-mentioned piece, come Mr. Mallet's monster 36-inch mortar, which if not the heaviest pieces of ordnance ever built, may certainly claim a pre-eminent position among all pieces either ancient or modern (the great gun of Moscow not excepted) as regards the weight of their projectiles, which when filled weigh nearly 3000 lbs. a-piece. These mortars, only two of which were forged and built together, owe their origin to the energy developed by Great Britain during the Crimean War. Mr. Mallet's original design was completed prior to October, 1854, and the manufacture, due solely to the personal responsibility of Lord Palmerston, commenced in 1855 by Messrs. Mare and Co., of the Thames Ironworks, Blackwall, who contracted to supply them at the rate of £140 per ton within ten weeks; they each weighed about 40 tons, and consequently cost £5600 a-piece. Mr. Mallet thus revived the old principle of constructing built-up guns, and next to Professor Treadwell, who first demonstrated the superiority of coiled wrought-iron over steel barrels in 1842, may be looked upon as the modern inventor of the "*ringed structure*" in guns. The fact that an enormous accession of strength could be attained by external rings with initial tension being suggested to him by Mr. Clarke's work on the Britannia Bridge in 1850.

The mortar may be shortly described as composed of a massive cast-iron base about 7½ tons, in which is fitted a wrought-iron breech piece in which is bored the chamber proper, strengthened externally by two layers of wrought-iron hoops and one heavy ring. On this rests the chase in

* The Story of the Thirty-Six Inch Mortars of 1855 to 1858. By Major-General Lefroy, C.B., F.R.S., R.A. "Proceedings of Royal Artillery Institution, Woolwich," vol. vii., No. 4, 1871.

three lengths, each of two thicknesses, tied down to the base with longitudinal bars or ties connecting the breech and muzzle rings. In consequence of the failure of the contractors the mortars were not actually finished until a year and ten months after the order, long after the termination of the war, being finished by Messrs. Fawcett, Preston, and Co., from forgings supplied by Horsfall and Co. The author had the pleasure of being present when some of the experiments were made with the first of these mortars, one of which was mounted for this purpose in the Woolwich Marshes. The shells of nearly 3000 lbs. weight had a capacity for nearly 500 lbs. of powder, and were thrown to the maximum observed range of 2759 yards with a charge of 80 lbs. of powder, the charge being arranged in bags by a gunner standing in the bore of the mortar.

The shells, which cost by contract about £16 a-piece, after describing their trajectories through the air with a slow and noble motion,* penetrated into the moist soil to a depth varying from 20 to 30 feet. It is to be regretted that only nineteen rounds in all were fired, the great expenses incident on the repairs of defective rings, wedges, &c., led to the then Secretary of State for War, General Peel, putting an end to further experiments. Mr. Robert Mallet attributes the comparative failure of his mortars to the rapid combustion and *brisante* quality of the brutal large-grain cannon-powder employed; and, doubtless, had pebble- or prismatic-powder been then invented and employed, the results might have been far more satisfactory. It is not too late to repeat these experiments yet, and the realisation of Mr. Mallet's cherished plans may not be beyond the bounds of possibility. The second of the two mortars, which has never been fired, is mounted in the Arsenal at Woolwich, where it forms a conspicuous object; whilst the first, having been dismounted from its crumbling bed by the agency of gun-cotton, still lies, perhaps not uninjured, in the Woolwich Marshes.

Superior in weight to the Mallet mortar, the American Rodman guns do not throw such heavy projectiles; but still as smooth bores, the 20-inch guns, "*Beelzebub*" and "*Puritan*," the 1100-pounders, are formidable weapons of

* The hemispheres of these fine shell being painted black and white, the gyrations were distinctly visible to the naked eye. Attempts were made without success to trace the trajectory of these great globes by means of photography, also by following with the point of a fine pencil the apparent foreshortened path of the shell on the ground-glass plate of the camera previously divided into squares.

over 50 tons weight. Unfortunately a diagram with detailed measurements of these guns could not be obtained in time to be inserted in the accompanying illustration; but their projectiles are shown for the sake of comparison. These cannon, shaped somewhat like a huge soda-water bottle, are cast-iron shell guns, cooled from the interior; at present, however, they may be looked upon as merely experimental pieces, and they would give no signs of injury before bursting. Lieut.-Colonel Owen rightly points out that the bursting of one of these guns would probably completely paralyse a ship's crew and destroy all confidence in them. Our wrought-iron gun, on the other hand, always gives timely notice by the tell-tale gas-escape channel if any injury happens to the interior steel tube, and an explosive burst is all but impossible with proper powder if not with other explosives. The great 1000-pounder of 50 tons, which was exhibited at the Paris Exhibition by Herr Krupp, of Essen, in 1867, must be remembered by all who saw it on that occasion. The manufacture of this steel gun continued during night and day for sixteen months at a cost of £15,750, and resulted in this unique specimen. It has a forged inner tube strengthened with three layers of rings over the powder-chamber and two layers over the muzzle portion; the rings having been forged from ingots without welding. All these last-named pieces of large ordnance may be looked upon as experimental weapons, and therefore hardly come within the scope of comparison with the mediæval ordnance which were in actual use, and served in action, sieges, &c.; and therefore the first service gun which we have to compare in weight and size of piece, bore, and projectile, is the lately adopted English 35-ton gun. This has been so lately described in the "Quarterly Journal of Science" as to need no further detailed description, which is familiarly known to all interested in heavy artillery. The first of these which was built, "The Woolwich Infant," notwithstanding the injuries it sustained during the crucial tests to which it was subjected at proof, promises well for the future;* and others have been and are being manufactured. Already nine others have passed the proof, and four more are all but ready for the proof-butts. But however suitable for naval purposes (for instance, the four guns of the *Devastation's* turrets will be able to hurl simultaneously a concentrated broadside of 1½ tons weight of iron projectiles on any given

* The defective inner steel tube of the "Woolwich Infant" is to be replaced by a new one, when it will be as good as ever it was.

object), every artillerist agrees that they are not long enough for our sea defences on land. As it is, we have found to our cost that the muzzles of our 9-inch and 10-inch guns do not project far enough beyond Colonel Inglis's iron shields in the embrasures of our lately constructed forts at Portsmouth and Plymouth; and from late experiments it has been found that when the guns in these new-fangled embrasures are traversed extreme right or left with extreme elevation (about 11°), the blast of the explosion at the muzzle is felt with such violence within the casemate that it would be impossible (in spite of the massive cable mantlets erected inside to avert its effect) for a gun detachment to fight the gun, whilst the shields themselves after many rounds would be severely racked: the projectile when it leaves the muzzle passing within an inch (if not less) of the outer edge of the shield-plates.

In addition, a considerable portion of energy is lost in so short a gun, for when large charges are fired a certain amount of the pebble-powder is thrown out of the muzzle unconsumed, and there is no doubt that a greater velocity and penetrative force will be given to the projectile by lengthening the gun. Consequently a new gun is to supersede the S.S. 35-ton gun, and we may hope shortly to see a L.S. 36-ton "Infant" at least 3 feet longer than its elder brother, and its performances at Shoeburyness will be well worth looking forward to with interest. Preparations have already been commenced at Woolwich for the manufacture of the huge forgings which will be required, such as a stupendous new steam-hammer of 30 tons, capable of striking a blow of many hundreds of tons, and other appliances. Taking the 35-ton 12-inch gun as the type, we need do no more than allude to the 12-inch of 25 tons, the 11-inch of the same weight, and the 10-inch of 18 tons. Another 18-ton gun of the *Hercules* it appears has lately (30th May) been permanently disabled when at target-practice off Portland by the premature bursting of a shell in the bore near the muzzle of the piece. As usual, it would seem that the shell broke through its stud-holes where the rifling offers the maximum resistance to their escape, both the steel lining and outer chase coil being split. This is the third gun of its kind which has been thus injured on board the same ship during the past three years. The 12, 9, 7, and 6½-ton guns, together with the 64-pounder, finish the tale of our present wrought-iron guns, whose projectiles are shown on the plate. And now for our cast-iron and bronze pieces, which are not yet obsolete, for although rifled guns have come into fashion, smooth-bore must constitute for some time, if not always, a considerable portion of our armaments. The Woolwich gun-foundry, established 1717, was virtually superseded by the forge in 1859,* the date of the last bronze gun being cast there. The few cast-iron guns which are occasionally required to keep the stock in store up to the specified number are obtained by contract.

The two heaviest pieces are the well-known Dundas 68-pounder of 112 cwts., 1841, and the new pattern 13-inch sea-service mortar of 5 tons, 1857. These are shown for the sake of comparison as illustrating our heaviest pieces during the first half of the nineteenth century, since the end of which such tremendous strides in the art of gunnery have been made. The old 110-pounder or 7-inch Armstrong breech-loader is also given, and illustrates the transitional epoch from the heavy smooth-bores to the heavier rifled pieces with elongated projectiles. General Lefroy, in illustrations of the guns left at Mont St. Michel, contrasts the 68-pounder and the 110-pounder Armstrong with Michelette and Mons Meg, which suggested the present paper. Dr. Déthier also draws a comparison between the American 20-inch Rodman and the cannons of Muhammad II.

It should be remarked that there are still in our own service pieces of ordnance, not yet obsolete, in whose construction can be easily traced the ancient traditional proportions descended from the earliest cannon, the "*bombards*" and "*crakys*" of war in olden days; these will readily be detected by anyone narrowly observing their sections; and, properly speaking, a diagram showing a section should have appeared in the plate for the sake of comparison. I refer to the brass coehorn howitzers of $4\frac{2}{5}$ inch howitzers cast at Woolwich in 1738, and still retained in the service, together with the royal and coehorn mortars of about the same date. These apparently insignificant pieces, although small, are very useful for colonial and mountain service or in the advanced trenches of a siege attack, whilst the royal mortar is fitted for firing Manby's life-saving apparatus.

Whilst comparing the size of the guns and projectiles of modern days with those of the middle ages, it cannot have failed to strike the observant reader that there is much in common with both; but beyond this all comparison ceases; for instance, it would be absurd to compare the rough hollowed elm block in which the gun from the *Mary Rose*

* Notes on Cast-Iron and Bronze Ordnance. By Captain F. S. STONEY, R.A., Royal Gun Factories, Woolwich. 1867.

man-of-war is mounted with either Scott's naval gun-carriage and slide or Moncrieff's elaborate elevating gear. In fact, we have only to look at the numerous crafty mechanical details involved in a simple double-plate wrought-iron carriage and platform with their fittings, whether American, Elswick connected screw-shaft, or hydraulic or other compressors, not to see that it is more in the delicate appliances, such as fuses, and minutiæ of adjustments, such as sighting, &c., that our real superiority obtains over the noble and massive, yet rugged ordnance, of our ancestors.

NOTICES OF BOOKS.

Remarks on Recent Oceanic Explorations by the British Government, and the Supposed Discovery of the Law of Oceanic Circulation, by Dr. W. B. Carpenter, F.R.S. By WILLIAM LEIGHTON JORDAN. Buenos Ayres: Imprenta Inglesa, 55, Calle de Cuyo. 1871. 56 pp.

THERE have been promulgated several theories attempting to explain the phenomena of oceanic circulation; and among these that best known to the scientific public, because most sedulously advanced, originates with Dr. Carpenter. This author seems to possess a peculiar and often very convenient disregard for the objections of his *confreres* in science, more especially when these objections are unanswerable, finding his reward in the applause of a too credulous public. Despite the publicity given to the facts advanced by Mr. Jordan in 1868, Dr. Carpenter, we believe, still continues to ignore them. An adage says, "Facts are stubborn things," consequently there cannot be much surprise that they should again occur to Dr. Carpenter's notice. Yet it seems strange that in such a tangible science as geography these facts should not have arisen dominant once and for all. To this there appears to be only one answer, namely, that geography is taught ordinarily in generalities; the exceptions or anomalies to these generalities being omitted by or unknown to the authors of our books of reference. In dealing with the facts brought forward by Mr. Jordan as opposed to Dr. Carpenter's theory, we shall quote extensively from the pamphlet sent us to review.

In the first place Mr. Jordan says, speaking of the explorations, "My object is not to descant on the unquestionable merits of these researches as regards the discovery of facts, but to deal with deductions made by Dr. Carpenter from them, which I consider erroneous." He then proceeds to the consideration of Dr. Carpenter's arguments regarding the Mediterranean and Baltic, and assuming them to be irrefutable, passes to the unsound assertion that "a vertical circulation must, on the same principle, be maintained between polar and equatorial waters by the difference of their temperatures.

"As regards this temperature theory," continues Mr. Jordan, "two incontrovertible facts exist, which combined are sufficient to annihilate it. The one, the fact that both in the Atlantic and Pacific Oceans the cold under-currents flowing towards the equatorial regions tend to the eastern parts of these regions, where they rise to the surface to flow westwards, becoming gradually heated in their course until, on the west of each ocean, they branch off northwards and southwards as warm currents. The other fact is, that if water were set in motion between the Equator and the Poles by differences of temperature, the rotation of the earth would immediately act upon it, tending to carry that moving towards the Equator westwards, and that moving towards the Poles eastwards; so that one of the most striking features of the circulation would be the tendency of the cold water to flow towards the Equator on the west of the ocean, eastwards through the equatorial regions, and to branch off northwards and southwards as warm currents, on the east of the ocean. These general features are exactly the reverse of the general features of the circulation which actually exists; and therefore nothing more is requisite to explode the theory; for what *must* be the result when fact and theory clash like this?

"Besides this broad discordance with actual facts, the details of Dr. Carpenter's theory are open to criticism. Is not the assertion, that a difference of level in the ocean will cause a surface current, rather reckless? Has it been certified by experience? or can any recorded phenomena be adduced in support of it?"

The system of upper and under currents through the Strait of Gibraltar and the Sound, Mr. Jordan goes on to show to be the "result of differences in specific gravity, as explained by Captain Maury's theory, which had better be left in its original simplicity, for this modification, with which Dr. Carpenter has attempted to encumber it, is not an improvement, but an erroneous complication."

"Captain Maury says wherever there is a 'difference of specific gravity between sea-water at one place and sea-water at another,' 'whether it be owing to difference of temperature or to difference of saltness, &c., it is a difference that disturbs equilibrium, and currents are the consequence. The heavier water goes towards the lighter, and the lighter whence the heavier comes; for two fluids differing in specific gravity, and standing at the same level, can no more balance each other than unequal weights in opposite scales of a true balance. It is immaterial whether this difference of specific gravity be caused by temperature, by the matter held in solution, or by any other thing; the effect is the same, namely, a current.'* This is sufficient to account for the Gibraltar current without the necessity of a surface current caused by difference of level, to set the circulation in motion. The Atlantic water which supplies the waste caused by evaporation in the Mediterranean flows in as a surface current because it is lighter than that of the latter sea, which the action of specific gravity consequently carries out below it.

"The currents in the Experimental Illustration of the General Oceanic Circulation in which water in a long narrow trough, as exhibited before the Royal Geographical Society, is heated at the surface of one end by a lamp and chilled at the other by ice,

* Physical Geography of the Sea, par. 406.

can be explained by the action of difference in specific gravity alone; the difference in temperature causes a difference in specific gravity, and for the restoration of the equilibrium a surface current of the lighter flows towards the denser water, whilst at the same time an under-current of the latter flows towards the former. The expansion caused by heat tends to raise the level at one end, and the contraction caused by cold tends to depress it at the other end; but if this difference of level were produced without any difference in the specific gravity, it would, in an open ocean, as already shown, be restored not by currents, but by a tidal movement of the ocean, the higher level sinking and the lower rising simultaneously.

"It appears that in the Mediterranean the surface heat, resulting from the action of the sun's rays, causes evaporation, which *lowers* the level and *increases* the density of water; and therefore when, in treating of the general oceanic circulation, Dr. Carpenter says that the level of Polar water is reduced and its density increased by surface cold, it might be expected that he would admit that the same effects are caused in the Equatorial regions by surface heat, or at least give some explanation as to why it should be otherwise. But instead of this Dr. Carpenter, without explanation, deliberately asserts that in the Equatorial regions the level of the water is *raised*, and its density *diminished* by the surface heat to which it is exposed: that is to say, that surface heat in the Equatorial regions causes exactly the opposite effects to those which it causes in the Mediterranean. If the observed effects in these two regions are really so diametrically opposite, can they be the result of the same cause? or may not the difference in the effects rather be the result of a difference in the ratio which the evaporating action of the sun's rays in the two places bears to the respective amounts of fresh water supplied by rivers and rain? But has it ever been shown that, whatever be the cause, there actually are such different effects in the two regions, or is this another reckless assumption to suit the exigencies of a preconceived theory?"

But by no means do Dr. Carpenter's anomalous deductions end here. Besides the vertical circulation resulting from differences in temperature, Dr. Carpenter says that "the Gulf Stream forms part of a *horizontal* or *superficial* circulation in the North Atlantic, of which the Trade Wind constitutes the *primum mobile*." Thus the general features of the theory seem to be that the difference of temperature in Polar and Equatorial regions causes a vertical, and the Trade Winds a horizontal circulation throughout the ocean. This is brought forward not merely as a theory by which a system of circulation might be caused, but as an efficient cause of the circulation which actually exists in the ocean.

Three years ago Mr. Jordan, making use of his own and Captain Maury's arguments, endeavoured to show the inefficiency of the winds as a cause for the existing currents of the ocean. He said, "'Theoretically considered, it appears plausible enough to assume that the winds must tend, to some extent, to cause a system of currents, by driving the surface water before them; which, wherever it accumulates against obstructions, must tend to run off in streams. But, practically considered—that is to say, considering what are the actual winds which blow, and what the actual currents of the ocean—it appears to us incomprehensible how any one who studies these systems of aërial and oceanic circulation can reconcile them as cause and effect. It appears to us surprising how, considering the enormous volume and weight of water borne along in the oceanic currents, any one can help doubting the power of the comparatively light atmosphere to keep such a mass in motion, even if it were shown that the *course* of the oceanic currents corresponded with that which would naturally result from the action of the winds which exist. But when it is found that the winds tend to a great extent to neutralise each other, and that, even in the region of the Trade Winds, where the power of the winds is greatest, ocean currents, even on the surface of the ocean, run across and against those winds, whilst in the lower strata immense under-currents run their course regardless of the winds which blow above; it then seems surprising how any one can consider that the position and direction of the ocean currents which exist are in accordance with the current-creating action of the winds, even if it be assumed that the latter are sufficiently powerful to control the enormous volume of water which is carried along in those currents.'"*

He also endeavoured to show the inefficiency of the Temperature theory. And after making use of Sir John Herschel's arguments, said: "'Besides the objections urged by Sir John Herschel against the theory which makes differences in specific gravity the prime cause of the currents of the ocean, we may here observe that, if differences in specific gravity, resulting from the difference of temperature and other conditions in Polar and Equatorial regions, were the principal cause of ocean currents—in consequence of the tendency of the heated and cold water to exchange positions in order to re-establish their equilibrium in specific gravity—then the heated water flowing from the Equator would be under that influence of change of latitude which tends to carry it eastwards, and the cold water from the Polar regions would be under that influence of change of latitude which tends to carry it westwards; so that, therefore, the warm water would naturally flow from the Equator on the east side of the ocean, and the cold water as naturally flow towards the Equator on the west side of the ocean; whereas, in fact, with the actually existing currents of the ocean, the very

* Vis-Inertiæ, p. 62. London: Longmans, 1868.

reverse is the case: the cold water is brought to the Equator partly by currents running towards the Equator on the eastern side of each ocean, and partly by under-currents rising to the surface chiefly in the eastern parts of the ocean; then it flows westwards through the Equatorial regions, and flows from the Equator on the west of the ocean as warm currents, having been heated during its course through the Equatorial regions.'"*

Afterwards alluding to this in connection with the winds he remarks that: "'The idea of the Trade Winds being considered a sufficient cause to account for this complete reversal of the course which the currents would naturally take under that influence of the axial rotation of the earth is one which I do not suppose any one will seriously maintain.'†

"I suggested the action of the winds and specific gravity as a means by which a system of oceanic circulation might be caused, but at the same time rejected it as too preposterously at variance with the actually existing circulation to be entertained by any one conversant with the subject. In this I was mistaken; for Dr. Carpenter now brings it forward in an elaborate article as the true theory of the existing oceanic circulation.

"But how has Dr. Carpenter succeeded in making this theory appear to explain the existing currents of the ocean?

"First: by utterly ignoring that influence of the earth's rotation to which I have alluded. This is the force which, according to Halley's theory of the Trade Winds, gives those winds an easterly direction, making them N.E. and S.E. winds, instead of N. and S. It is the force indicated by Captain Maury as the cause of the Gulf Stream tending eastwards after it leaves the Gulf of Mexico. It tends to carry the Gulf Stream eastwards, because that current is running from the Equator, and at the same time it tends to carry the cold Labrador current westwards, because the latter is running towards the Equator. Thus, by its action, those currents running in opposite directions are made to flow side by side without intermixing.

"If warm water at the Equator and cold water at the Poles rush towards each other to re-establish their equilibrium, why do not the warm waters of the Gulf Stream and the cold waters of the Labrador current, when brought into juxtaposition, intermingle and at once restore their equilibrium? It is simply because they are held in the grasp of the gigantic power created by the earth's rotation, and the puny powers created by differences of temperature or specific gravity are impotent in its presence. These latter appear in Dr. Carpenter's theory to be the paramount forces, to which the Winds are secondary. If, then, the force of the above action of the earth's rotation mocks the forces created by differences of temperature where the latter do not

* Vis-Inertiæ, p. 59. London: Longmans, 1868.
† *Ibid.*, p. 66.

act in concert with it, how can the lesser force of the winds be supposed to control it?

"Secondly: without a word of argument, or a shadow of proof, Dr. Carpenter assumes that differences of level in the ocean must cause surface currents from the higher to the lower levels; and such currents, flowing into the Polar regions, he makes the first impetus of the circulation; so that the fact that differences of level in the open ocean are naturally restored by tidal movements, and not by currents of any sort, cuts away the tap-root of his theory.

"And thirdly: without any explanation, Dr. Carpenter treats the action of surface heat as causing exactly the opposite effects in the Equatorial regions to those which it causes in the Mediterranean. With most astounding and glaring inconsistency he makes (not by argument, but by mere assertion) the same cause produce different effects, just as the exigencies of his theory in different parts of the ocean require. Besides these things, considering also minor details to which I have not alluded, I could scarcely have believed it possible that such loose and reckless theorising could have been perpetrated by a Fellow of the Royal Society, and still less by a man selected by the British Admiralty as their scientific representive, and this too in treating of a subject with which the acceptance of the latter post makes it his special duty to be well acquainted."

Considering that, according to Dr. Carpenter's theory, the great predominant current-creating force is in the Polar regions, it might be expected that some facts would be brought forward in its support; but to this Dr. Carpenter does not condescend; for after the assertion that "the surface flow of Equatorial water towards the North Polar area is a fact universally admitted," and an argument regarding the course from the Equatorial regions of the water which "passes north and north-east between Iceland and Norway towards Spitzbergen," he branches off to draw distinctions between "the *mere* Physical Geographer" and "one who takes a *scientific* view of the matter"—rather an unhappy distinction for the author himself it would appear. Certainly the fact referred to is admitted; but will Dr. Carpenter contend that in the latitude of the north of Norway the volume of the surface-flow of warm water northwards on the east of the ocean is anything like equivalent to that of the surface flow of cold water southwards on the west of the ocean? And it should also be observed that in those high latitudes, according to the theory of the current-creating action of the earth's rotation, there exists, west of the surface flow of warm water, an under current of the same flowing northwards with a surface current of cold water immediately above it flowing southwards. A part only of the warm current is tilted up to the surface on the east of the ocean, whilst the remainder under-runs the cold current flowing in the opposite direction. "Be this, however, as it may," continues Mr. Jordan, "why is it that no more facts from that region, so important to the theory, are brought forward in its support? Is it in consequence of a consciousness that nearly all genuine facts from thence would be death blows to the theory? It seems to me that it would be scarcely possible to invent any theory by which, in the absence of other causes, a system of oceanic circulation might be caused that could, in those regions, be more completely at variance with the results of actual researches than this temperature theory of Dr. Carpenter's."

Much as we should like to introduce an account of the arguments diffidently advanced by Mr. Jordan, their length precludes such a course in a notice of his pamphlet. But his views may be thus recapitulated:—

"First: the very basis of Dr. Carpenter's theory requires that the force of gravitation should re-adjust the level of the ocean, by bringing down the level in the Equatorial regions and raising it in the Polar regions, in consequence of the existence of the reverse effects resulting from differences of temperature. In reply to this, practical researches tend to show that the action of differences of temperature and other conditions between Equatorial and Polar regions has the reverse effect from that which Dr. Carpenter assumes them to have—that they tend to lower the level in the Equatorial and raise it in the Polar regions; so that the re-adjustment to be made by the force of gravitation is in the opposite direction to that required by Dr. Carpenter's theory.

"Secondly: Dr. Carpenter assumes that the action of gravitation would cause a surface current through the ocean from the higher to the lower level: and to such currents flowing into the Polar regions he attributes the first impetus which sets the circulation in motion. In reply to this it has been pointed out that the action of gravitation would, in the open ocean, tend to re-adjust such differences of level by tidal movements, the higher levels falling and the lower rising simultaneously without causing any current. So that therefore, even if Dr. Carpenter were right in assuming the level of the Polar to be lower than that of the Equatorial regions, no such surface currents would be created by that difference of level as those to which he attributes the first impetus which sets the circulation in motion.

"Thirdly: according to Dr. Carpenter's theory, in the Polar regions the surface currents flow towards and the under currents from the Poles. In reply to this, practical researches tend to show that the very reverse is the case:—that the cold waters pour away from the Poles chiefly in the upper strata, whilst warm currents flow beneath them towards the Poles. So that the vertical circulation in the Polar regions—the great paramount force-creating region, according to Dr. Carpenter—appears to be the reverse of that necessitated by his theory.

"Fourthly: if a circulation were caused by differences of specific gravity, or any other effects resulting from differences of temperature between the Equatorial and Polar regions, it has been shown that the currents would flow towards the Equator on the west of the ocean, eastwards through the Equatorial regions, and from the Equator on the east of the ocean, and this course, which would certainly result from Dr. Carpenter's theory, is exactly the reverse of that of the currents which form the horizontal circulation in the Equatorial regions.

"Fifthly: Dr. Carpenter adapts the Mediterranean theory to what is known of the vertical circulation of the temperate and equatorial regions by reversing the action of surface-heat from that admitted in the former regions; this strategic movement, however, seems to be a descent from bad to worse, for by it the theory appears to be placed in direct antagonism with the source of its own creation; namely, the vertical circulation in the Polar regions. And—to follow the parallel suggested in Sir Roderick Murchison's contingent eulogy—this gross deviation from consistency has led Dr. Carpenter into such confusion that where veins and arteries interlace he has taken up veins for arteries and treated arteries for veins, even supposing his location of the heart to be exact; but in this he has made a still more grievous mistake in imagining that source of circulation to lie divided in the cold extremities of the earth instead of in the vast expanse of the Equatorial regions.

"Dr. Carpenter's theory of the action of surface-cold may prove most valuable in the consideration of minor details of oceanic circulation. But as a cause of the main features of the existing circulation it is so absurdly incongruous and inadequate that in this Essay Dr. Carpenter must be said to have lowered himself to the level of a '*mere*' theorist. The negative reply to the question whether the '*so-called*' explanation of oceanic circulation is or is not the '*real*' explanation, is so glaringly obvious that it is surprising how any one acquainted with the subject could be so infatuated as to take the false course in which Dr. Carpenter has involved himself. It is a degradation of the term '*scientific*' to apply it to such theorising. How much more really scientific—how much nobler is the spirit of the true 'Physical Geographer' who completely subordinates theory to fact. This is the spirit which breathes through Maury's writings, and enables the reader to turn over the pages with a refreshing confidence that he need fear no delusion. A recently increasing departure from it in too many English scientific writers has culminated in this audacious effusion of Dr. Carpenter's. If such essays are to be brought forward by leaders of English science, history will record a degradation of English intellect in the present generation; and the laurel which the civilised world by acclamation placed on England's brow, in honour of Newton's discoveries, will soon be torn away to be worn elsewhere."

Apart from the valuable theory which Mr. Jordan has advanced in his work, we think he has performed a duty in thus clearly proving the errors of a theory too assiduously expounded. But it must be said that we lay down Mr. Jordan's pamphlet with feelings of the most intense surprise—surprise that such a theory should meet with the slightest favour, and that Dr. Carpenter should have evolved it in any moment but those of "unconscious cerebration."

Coal Economy. By FRED. CHAS. DANVERS, Assoc. Inst. C.E., of the Public Works Department, India Office, &c. London: W. H. Allen and Co. 1872.

WHILE public attention has of late years been directed again and again to the serious waste of coal consequent upon our present systems of coal mining, and to the recklessness with which the fuel is too often consumed, it is curious that little or nothing has been written on the injury and waste which the coal suffers after it emerges from the pit's mouth, and during the successive stages through which it passes before reaching its final destination.

Yet this loss is by no means insignificant; for it is obvious that the coal in the course of being screened, and during its transport, whether by land or water, must be continually subjected to movements which tend to cause disintegration, and inevitably result in the formation of more or less small and comparatively useless coal. It is pleasing to find that this neglected phase of coal economy has received the careful attention of Mr. F. C. Danvers, who in the work before us discusses the several sources of waste, and points out the best means of utilising the small coal, whilst incidentally he offers much useful information on the machinery employed in connection with the mineral traffic.

It is not with the waste in underground working that our author has to deal. That question was fully examined by the Coal Commission, and Mr. Danvers's work begins exactly where that of the Commissioners ended. Suffice it, then, to say that in the steam collieries at least 40 per cent is usually lost in working by the "pillar and stall" system, and about 15 per cent by the "long wall" system. If the small coal be raised to the surface with the large coal, the pit is said to be worked on the "altogether" principle; if the small coal is left underground it is worked on the "separation" principle. In the former case as much as one-half of the output may consist of "small," whilst in the latter case only from 5 to 10 per cent passes through the screens. The author describes the construction of several forms of screen used in the South Wales coal-field, and in the neighbourhood of Newcastle—the two localities selected as offering the best examples of modern colliery appliances.

After leaving the screens at the pit's mouth, the coal suffers more or less breakage during railway transit. Mr. Danvers does not, however, think that the amount of disintegration in a waggon increases in proportion to the length of the journey; for after the first few miles the large lumps get firmly seated on a bed of small coal formed by the mutual abrasion of sharp corners. Admitting this, one may question the propriety of screening at the pit, since the small coal thus removed would evidently be useful in filling up spaces between the larger blocks.

It must not be supposed that the injury which coal suffers stops at the end of its journey. If the fuel be stored in open air it gradually disintegrates, and at the same time loses much of its heating power: indeed, the value of steam coal may actually be reduced one-half by exposure to the air for six months. It is consequently important that coal should be protected in the depôts in which it is stored; and of late years much attention has been paid to the construction of such buildings.

After the employment of even the most approved machinery in connection with coal traffic, there must needs be a considerable amount of almost valueless small coal—*duff*, *slack*, or *waste*, as it is usually called—and it remains to see how this can be profitably utilised. Without following the author into his long discussion of the principles of combustion and the economic use of fuel, we may cite his opinion that the method at present most convenient and most generally applicable for utilising small coal is that of manufacturing it into artificial or patent fuel. Several methods devised for this purpose are here described: different patentees have suggested the use of pitch, tar, starch, and alkaline silicates as the cementing material by which the slack is formed into blocks. The author recommends that the coal be washed to remove as far as possible the shale and other associated impurities, and then be moulded into blocks by means of a mixture of starch and bituminous matter.

Before closing the present work it is right to state that the information which it contains was originally collected by Mr. Danvers with the view of forming a Report for presentation to the Indian Government.

Coal mining is at present in its infancy in India; but the rapid extension of the railway system—not to speak of other sources of demand—creates a growing increase in the want of fuel—a want which the forests, in spite of all conservancy, cannot continue to supply, and which must ultimately necessitate the development of the coal-fields of India. But in a country where the coal resources are but limited, and where the coal is for the most part extremely tender and liable to suffer spontaneous disintegration from the large proportion of associated iron pyrites, it is evident that the greatest care and economy should be exercised in the transport of the fuel. Hence Mr. Danvers's subject is invested with peculiar interest in our Indian possessions. Nevertheless his book contains so much that is valuable, that it may be studied with almost equal profit at home by all who are practically interested in the welfare of the British coal trade.

A Text-Book of the Construction and Manufacture of the Rifled Ordnance in the British Service. By Captain F. S. STONEY, R.A., Assistant Superintendent Royal Gun Factories; and Lieutenant C. JONES, R.A., Instructor Royal Gun Factories. Corrected up to January, 16th, 1872. Printed under the Superintendence of Her Majesty's Stationery Office.

CAPTAIN STONEY and Mr. Jones, in their carefully compiled Text-Book, supply a want which has long been felt by students in the Royal Gun Factories at Woolwich Arsenal. Both Naval and Military as well as civilian Artillerists have all experienced the great difficulty of keeping pace with the constant and rapid change both in modern ordnance and its *matériel*, more especially since the introduction within the last ten or twelve years of various systems of rifling, followed by their inevitable train of new inventions, in the shape of fuzes, explosives, &c.

This difficulty has hitherto been enhanced from the fact of there having been no authorised text-book to start from as a recognised stand-point of departure.

It is with great satisfaction, then, that we hail the publication of a work compiled by two such competent authorities as the late Assistant-Superintendent and the present Instructor at the Woolwich Gun Factories, to which department of the Arsenal the volume is creditable.

In a technical work of this description there is naturally no room for originality, but a practical gunner can alone fully appreciate the vast amount of nice research required, and the laborious care necessary to collect and arrange the formidable array of minute details which, however apparently trivial in themselves, are in reality of the most practical importance as a whole when brought together within manageable compass.

Such condensation can only result from sound knowledge of the subject and ability on the part of the compilers; and, judging from the result, the authors of the present volume under notice appear to possess both.

The text is well illustrated with eleven lithographed detailed coloured plates, comprising all the wrought-iron muzzle-loading guns, as well as the small steel and bronze Abyssinian and Indian guns, besides numerous woodcuts of details, fittings, &c.

The work commences with a brief historical sketch of our rifled ordnance, from Lancaster's gun in the Crimea, and Colonel de Beaulieu's field pieces at Solferino, down to the present Prussian system of Krainer, and our own Fraser cheap construction Woolwich guns; then follows the consideration of the various rival systems of rifling up to the latest date; and our authors, in conclusion of Chapter I. remark, "that the tests and trials bearing on this question (that of construction), while exemplifying the pains taken to obtain the best war *matériel*, cannot fail to satisfy the most sceptical, that the present construction of our guns is sound and durable, and also that we have made marked progress of late years in heavy ordnance. In Chapter II. we have Hart's, Armstrong's, Whitworth's, Palliser's, Rodman's, and Fraser's systems of construction discussed, and our British ordnance is compared with foreign to the manifest advantage of the former. "For supposing the American cast-iron guns are as durable and as little liable to burst as our sinewy guns of wrought-iron, and that their *apocryphal* charges are actually used, our guns possess the great advantage of being able to *pierce* armour-plates with shot, nay even with *shell*, which the American guns could only crush or '*rack*' with solid shot. Various examples of Prussian guns bursting explosively are also given as a proof that the uncertain character of steel renders it a dangerous material, and that we must not trust it until greater improvements take place in its manufacture. In Chapter III. the materials for ordnance, such as bronze, cast-iron, steel, and wrought-iron are compared, and their physical properties described. Its deficiency in hardness, however tough and tenacious the metal, points out bronze to be unsuitable, except under restricted circumstances, for ordnance. However valuable the hardness of cast-iron, yet its brittleness renders it unsafe, and only useful for converted guns with a wrought-iron lining. The defects of steel, also, may be stated to be brittleness, uncertainty, and deficiency in extensibility, when strained beyond its elastic limits, which render it unsuitable for the exterior portions of a gun; at the same time, from its hardness, high tensile strength, and freedom from flaws and defects, it is well suited for the inner barrel. Lastly, wrought-iron is valuable as a gun material, on account of its comparatively high tenacity, combined with its malleability and ductility. The next chapter is taken up with an account of the principal operations in the manufacture of wrought-iron ordnance, the machinery employed, the methods of forging, welding, and coiling on a large scale, the lathes, rifling, and other machines. Further on we have the manufacture of the breech-loading Armstrong guns, of which, although suspended latterly, a knowledge is required by officers who have to deal with this class of guns in the service. We will now proceed to watch the progress of a 7-inch Fraser cheap-construction gun through its rough process of manufacture, many details necessarily being omitted. First, the gun consists of the following parts:—

I. An inner barrel or tube of steel, called *the A tube.*
II. Two single and slightly taper coils united together, called *the B Tube.*
III. A triple coil, a trunnion ring, and a double coil, all welded together, called *the breech-coil or jacket.*
IV. A cascable.

I. The steel for the A tube is received from the contractors in the form of a solid ingot and severely tested. It is next roughly bored and turned and becomes a tube, which is next toughened by being thoroughly heated to a certain temperature, and then plunged in a bath of oil until cool. This process frequently warps the steel tube, and not unfrequently causes the surface to crack; the barrel is therefore slightly turned and bored again to make it straight inside and out, as well as to remove any slight flaws; and for fear some of these flaws should escape notice, the tube is now subjected to the water-test. By certain mechanical contrivances the tube is filled with water, which is subjected to a pressure of $3\frac{1}{2}$ tons per square inch; if no moisture exudes on the exterior of the barrel under this pressure the tube is passed as sound.

II. The B tube is composed of two coils made and welded together in the usual way, and rough- and fine-bored to the degree of smoothness requisite for close contact with the steel A tube on which it is to be shrunk; and here it may be remarked, that it is found easier to turn an inner tube to fit an outer one than *vice versa.*

III. The triple coil is formed by coiling three bars one over the other and welded under a powerful steam-hammer and turned down, being fitted with a shoulder for the reception of the trunnion ring.

The double coil is similarly prepared also to fit the trunnion ring, the trunnion ring being also ready; all three parts are heated to redness, the trunnion ring is dropped upon the shoulder of the triple coil, and the double coil dropped through the upper part of the trunnion ring on the triple coil, and the whole mass placed bodily in a furnace and raised to welding heat; being next placed under the most powerful hammer in the department, six or seven blows suffice to amalgamate the three parts together. The breech coil thus formed is next turned in a very powerful lathe, the trunnion ring slotted smooth in a self-acting vertical machine, and the trunnions themselves turned down to shape in a break lathe. The jacket is next rough- and fine-bored, and its muzzle end recessed on the inside so as to overlap the breech end of the B tube.

IV. The cascable is first forged from the best scrap iron into an oblong block, then turned cylindrical and a bevel thread cut on it; the button is turned on it, and a hole is drilled through one end for the purpose of screwing it into the gun.

All the parts are now ready for building up.

First the B tube is shrunk over the muzzle of the A tube, and the A and B tubes shrunk up are placed together in a lathe, and they are fine-turned to receive the jacket.

The half-formed gun being placed standing on its muzzle in the shrinking pit, the jacket is heated and shrunk on, and allowed to cool naturally, a jet of water playing up the bore to keep the interior cool.

The cascable is next screwed in, an operation which it may be imagined requires great care, for the front of it must bear evenly against the end of the steel barrel. One round of thread is turned off the end of the cascable, so that there may be an annular space there, which in connection with a channel now cut along the cascable and across the thread, will form a gas-escape or tell-tale hole in case the steel barrel should split at the end. Various finishing processes are next proceeded with previous to proof, which we need only enumerate, viz., fine-boring, second rough cutting of chamber, finished boring, broaching of bore and finishing chamber, lapping, rifling, and temporary venting. The manufacture of the higher natures of guns are on the same principle, and the only difference is in having extra coils. All guns are minutely examined before proof, and gutta-percha impressions are taken of the whole length of the bore in four quarters. The bore of all guns of 9-inch calibre and upwards is also accurately gauged every 3 inches. The proof is based on the highest charge which the gun will fire on service, viz., two rounds of $1\frac{1}{4}$, the highest battering charge and service projectiles; this will not improbably be altered to the highest service charge with heavier projectiles. After proof the guns are again tested by the water being pumped at high pressure into the bore, and gutta-percha impressions again taken and the bores gauged. If there should be any doubt about a slight defect the gun is again subjected to five more rounds, and if after that the defect does not appear to increase the gun is passed.

The last chapter treats of the important duties of examining, testing, and preserving guns, &c., as to their defects and repairs, &c.

Finally, in a valuable Appendix, are given range tables and fuze scales and other useful information.

We can only conclude by remarking that nothing could be more opportune at the present time than the appearance of this compendious and satisfactory text-book.

The Law of the Winds Prevailing in Western Europe. By W. Clement Ley. With Charts, Diagrams, &c. Part I. London: Edward Stanford. 1872.

This work is the first instalment of what promises to be a very useful record of the main lines pursued by storms in their courses about our own islands, and some of the neighbouring countries. Whilst recording with care observations actually made, both of barometric change and of direction of wind, Mr. Ley attempts to classify the results, and we think with great success; still he himself points out that much more observation is requisite, and the combined efforts of many interested observers are required before much accurate scientific knowledge can be said to have been collected. We wish him and his fellow labourers all success, and we are quite sure that the carefully-prepared diagrams and the systematic character of the records in this work will greatly facilitate the labours of future observers. The description of the origin of circular motion in winds is most clear and luminous, and is capable of being understood by a mere child.

Observations of Comets, from B.C. 611 *to* A.D. 1640, *extracted from the Chinese Annals.* Translated with introductory remarks and an Appendix comprising the Tables necessary for reducing Chinese time to Euoropean reckoning; and a Celestial Atlas. By John Williams, F.S.A., Assistant-Secretary of the Royal Astronomical Society, &c., &c. London: printed for the Author. 1871.

Mr. Williams has done useful work which required a rather peculiar combination of acquirements. It is not many persons who unite a competent knowledge of the Chinese language with an acquaintance with the latest discoveries in astronomy. The work before us is the result, however, of such a combination, and we must thank the author for having undertaken a work requiring so much labour, and necessarily leading neither to profit nor renown. To the few persons who will make use of the lists here given, and they will naturally be few, this work is invaluable, and they will no doubt feel doubly grateful and assured of the accuracy of what is thus prepared for them, as the author furnishes them with the means of testing his accuracy and of going over the ground after him. The Chinese mode of reckoning time from the earliest epoch of the present day is described, and the divisions of the heavens among the same people are fully explained. The labour of explaining all this in a manner intelligible to those previously unacquainted with sinology, of copying, translating, and explaining a great quantity of Chinese records, must have been immense, and the reward is that the present work may become a work of reference for those who may be investigating the paths of comets as in future time they may appear to us.

Geology of Oxford and the Valley of the Thames. By JOHN PHILLIPS, M.A., F.R.S., &c. Oxford: Clarendon Press. 1871.

THE work before us, the excellency of which is attested by the name on the title-page, is calculated from its extremely practical nature to do more for the study of geology than any amount of scientific manuals. To a young man whose mind is opening as minds do open when they are first thrown amidst the new intellectual arena to be found in a University town, a book like this may be of inestimable value. We cannot imagine a present more likely to influence for good the future career of a young man for the first time going up to Oxford than one which should give a purpose to his leisure hours, lead him to notice the world around him, and supply a purpose to the time devoted to recreation and to exercise. Such is Professor Phillips's book. From the Malvern and Gloucestershire hills to the Reculver; from gneiss and granite to the alluvium of the Essex marshes; from Cambrian fossils to bronze celts, the river which more than any other contributes to the commercial superiority of England lays open to the willing student pages upon which are written the history of the great world of nature in a style less difficult to master than the involved sentences of Thucydides, and in a continuous history in which there is no fear of finding more of myth than of fact. We shall hope to see the student, who in the morning plods through the difficulties of the Greek historian, the legends of the Latin chronicler, and the arguments of Greek philosophers, obeying the impulses he receives from our English Bacon, going forth to gain air and exercise, not to a mere bodily athleticism, but, under the guidance of another professor, studying the stone quarries of the neighbouring country, watching the flowing of the water, and collecting day by day evidences of the changes in what now is a river-basin, was more than once a mighty estuary, and has even formed the bed of a glacial sea. The work of Professor Phillips is exhaustive. He traces the history of those who have made the valley of the Thames their special study; and when we remember that this begins with Tradescant of the "Physic Garden," and includes Smith and Buckland, we see that this is almost a history of English geologists. The physical geography of the whole valley and its surrounding hills is carefully treated with diagrams of the appearance of the country submerged to various depths. The tributary rivers receive a due share of attention, and are all traced to their source; and then each geological period has a chapter to itself, in which its various strata are described and localised, the fossils it contains catalogued and delineated, and the theories derivable therefrom remarked and discussed. In one chapter the changes of the earth's surface from upward and downward movement, from the wearing away of rivers, and from the deposit of floods, is exemplified from this one typical river-basin. Finally, a single chapter discusses the economical questions of the amount and quality of the supply of coal, iron, brick and pottery earth, ochre, Fuller's earth, glass, sand, and last, not least, water.

In this way does the Oxford Professor both inculcate the theory and show by practice that observation is necessary to the foundation of this science; that geology is a philosophical study branching off into many kindred sciences, and that it has a practical value for chemist, agriculturist, miner, metallurgist, and engineer; and like every other branch of natural science, it leads from phenomena which our senses can observe to discoveries useful to man in every career.

Volcanos, the Character of their Phenomena, their share in the Structure and Composition of the Surface of the Globe, and their Relation to its Internal Forces. With a Descriptive Catalogue of all known Volcanos and Volcanic Formations. By G. Poulett Scrope, F.R.S., &c. Second Edition, with Prefatory Remarks, and a List of recent Earthquakes and Eruptions, &c. London: Longmans. 1872.

We notice the second edition of this exhaustive work on Volcanos, both because it is a long time, thirty-seven years, since the first edition was published, and also because some considerable additions have been made in the present issue. The list of recent eruptions extends from 1860 to the end of August, 1871, and includes those disturbances which are not mentioned in the body of the work nor in the catalogue of existing volcanos. This makes the information rather scattered, and any one who wishes to become possessed of the whole history of a particular mountain, after he has read the account in the first Appendix, has to search through the list in the second for the records of subsequent eruptions. The other new objects discussed are the igneous fluidity of the interior of the globe, the true character of lavas, the coincidence of volcanic and atmospheric disturbance and tidal action, the foliation of the (so-called) metamorphic crystalline rocks, and the ratio of development of subterranean forces. In the first case our author decides against internal fluidity, and in favour of the origin of disturbance at comparatively small depths. He is inclined to think that lavas in their most molten condition are not homogeneous masses, but include most of the crystals to be afterwards found in them in a state of mechanical mixture in their more fluid materials, and that their fluidity is attributable to a certain extent to the presence of interstitial water. The difference of barometric pressure is adduced as one cause among others of the variation of eruptive activity, since the boiling-point of water, which takes such a large share in these phenomena, is in this way changed. There appears to be no evidence in favour of greater activity of volcanos in former ages than at present, though their action at no time has been regular, rhythmical, or capable of being reduced to an average. Our author therefore sides rather with the Uniformitarians than with the Evolutionists, and he even goes so far as to see no geological reason that would point to a gradual cooling down of the planet.

Reports on Observations of the Total Solar Eclipse of December 22, 1870. Conducted under the Direction of Rear-Admiral B. F. Sands, U.S.N., &c. Washington: Government Printing Office. 1871.

We may without offence hope that the United States Navy may never be worse employed than it was in the case of the expedition of which the Report is now before us. The claims of hospitality to the scientific men from the other side of the Atlantic, for which several of the writers in this volume make grateful acknowledgments, we shall ever be willing to liquidate: these claims can never be so great that we shall not be anxious to repay them with interest.

In the present volume we have the reports of the observations of Professor Simon Newcombe, at Gibraltar; and Professors Asaph Hall, Wiliam Harkness, and J. R. Eastman, at Syracuse; and a letter from Capt. Tupman, of the Royal Marine Artillery, accompanied by sketches. Professor Newcombe made his observations from Buena Vista, a point about midway between Europa Point and the town of Gibraltar, and about twenty miles from the line of central eclipse. He enjoyed a fairly clear sky during the period of totality, but was disappointed in the brilliancy of the corona.

Professor Hall was on one of the bastians of the town of Syracuse. He paid most attention to the corona, and though, as in the former case, previously the clouds had impeded the view during totality, he was able to make the required observations. An adjoining spot was occupied by Professor Harkness, whose observations were principally with the polariscope, and with the assistance of Capt. Tupman he also used the spectroscope to the corona. Professor Eastman was about forty yards from the former party. With the assistance of his wife he intended to make observations with a clinometer polariscope (on moon, sky, and corona) and photometer; he was, however, unsuccessful with the latter.

The whole of the reports are exceedingly interesting and well worth reading, though in one or two places they betray by their language their American origin.

A Vision of Creation. A Poem. By CUTHBERT COLLINGWOOD, M.A. and M.B., Oxon, &c. With an Introduction, Geological and Critical. London: Longmans. 1872.

IT is not our custom to review poetry, and even in this case we shall have but little to say about the literary value of Mr. Collingwood's work; it is the geological introduction which induces us to notice the poem. The key to the title will occur to everyone who has read Hugh Miller's "Testimony of the Rocks." A seer such as existed among the Jews before the Prophets of later date displaced them—a seer of visions in a patriarchal age—whether Moses himself or some earlier worthy, is left undecided, is visited by a heavenly messenger, who is commissioned to communicate to him the mysteries of the origin of the universe, but who first draws his attention to the physical phenomena surrounding him, and describes how man shall in after ages learn for himself from the records around him how the world came into existence. This angelic being then conveys the seer to a desolate region, and presents to his eyes visions of the early condition of the world when emerging from its primeval chaos into the full perfection of its present condition, and these visions are accompanied with an explanation of the events as they take place. The words of the Book of Genesis are used as an outline, which is filled in by the light of modern science. Thus the beginning is made to answer to the period anterior to all geological record and to be suggestive of the Nebular Hypothesis. The earth, "without form and void," corresponds to the Azoic age of igneous rocks. This state of chaos the seer describes as "a world, yet not a world," for which the Greek is given, and a reference is added to Genesis i., 2., leading one to suppose that this is the Septuagint version of that verse, which is misleading. The creation of light is made simultaneous with the so-called metamorphic rocks of the Laurentian and Cambrian series. The first appearance of a firmament or atmosphere is the period of the Silurian rocks formed beneath a shallow sea of almost universal extent. The appearance of dry land characterises the Devonian period, in which the old red sandstone produces mosses, &c., whilst vegetation of marked type, both "herb" and "fruit-tree," have left their record in the carboniferous strata. The appearance of sun and moon, already created, but until now hidden by dense mists, denote the introduction of a new era, and the new red sandstone accordingly marks the beginning of the secondary rocks, and lies between the vegetation and the reptiles of the next age.

These are the "sea monsters," "the moving creature that hath life," the huge Saurians of the Lias and Oolite. The "fowl" in the expanse of heaven are first found in the Upper Oolite, but it is scarcely so clear as the author would make out that the birds were numerous in the period of the chalk. Perhaps we should scarcely look for the remains of birds at the bottom of a deep sea, and we hardly know what may have been the contemporaneous land. The sixth day describes in the Meiocene times the creation the huge mammals of the pachydermatous genus, whilst man follows all in a period still antecedent to the present "seventh day," in which the present order of things continues mostly unchanged under a reign of law. All this is told pleasantly in flowing blank verse, which rather weakly challenges comparison with Milton.

Dr. Pereira's Elements of Materia Medica and Therapeutics. Abridged and Adapted for the Use of Medical and Pharmaceutical Practitioners and Students, and Comprising all the Medicines of the British Pharmacopœia, with such others as are Frequently Ordered in Prescriptions or Required by the Physician. Edited by ROBERT BENTLEY, M.R.C.S., F.L.S., and THEOPHILUS REDWOOD, Ph.D., F.C.S. London: Longmans, Green, and Co.

THIS abridgment of Dr. Pereira's great work will be welcomed both by medical and pharmaceutical students. The original work, valuable as it was, contained much matter which was not required by the student, and its great bulk rendered it too expensive for many to possess it. In 1865, Dr. Farre, Professor Bentley, and the late Robert Warington, prepared an abridgment which came within the reach of all, and which was certainly a most useful book, the appearance of the "British Pharmacopœia," however, and the rapid progress of the science of Materia Medica, rendered another edition necessary, and we are glad that its preparation was entrusted to such competent men as Professors Bentley and Redwood. The medicines described comprise in addition to those of the "British Pharmacopœia," many remedies frequently ordered by medical practitioners, and more space might with advantage have been allotted to some of these remedies, the information in some instances being, we think, insufficient for the student. The first part of the work is devoted to the new system of chemical notation (the symbols and atomic weights being given according to both the old and new systems); a description of medicines derived from the mineral kingdom and definite chemical compounds, organic as well as inorganic, which are obtained as products of decomposition. The second part embraces medicines derived from the vegetable kingdom, including bodies of definite chemical composition obtained as educts from vegetable substances, and here the latest discoveries are introduced. In the third division a description is given of medicines derived from the animal kingdom, including bodies of definite chemical composition obtained as educts from animal substances.

In studying Materia Medica much assistance is gained from good illustrations. We remember well the good we derived in our own studies from Stephenson and Churchill's valuable work on "Medical Botany;" but as this work is far too scarce for many students to possess a copy, we shall be glad, in a future edition of the work before us, to see engravings of all the more important plants, &c., instead of so many references to plates in scarce or expensive works on Medical Botany, which, in most cases can only be consulted at the Pharmaceutical or Medical Libraries.

This abridgment of Dr. Pereira's excellent work reflects the greatest credit on its Editors, and we commend it to the student of medicine and pharmacy as a necessary companion when preparing for examination.

Official Handbook to the Marine Aquarium of the Crystal Palace Aquarium Company. By W. A. Lloyd, Superintendent of the Aquarium. Second Edition. 1872. Pp. 64.

It is not often that the guide book of a public exhibition comes under the notice of the reviewer, such publications in the majority of instances being mere catalogues, and but rarely possessing any scientific interest. The present little book is, however, an agreeable exception.

After explaining the arrangement and general contents of the thirty-eight tanks comprised in the public portion of the aquarium, the author devotes twenty-nine pages to the history of the Marine Aquarium. Among the earliest notices is the following quaint extract from Pepys's Diary:—"Thence to see my Lady Pen, where my wife and I were shown a fine rarity; of fishes kept in a glass of water, that will live so for ever—and finely marked they are being foreign." The experiments of Madame Power, the late Dr. N. B. Ward, Dr. George Johnston, and Sir John Graham Dalyell, are described. These are followed by an account of the first entirely successful marine aquaria, the simultaneous results of the trials of the late Robert Warington and Mr. P. H. Gosse, F.R.S. Then follows an account of the various public aquaria, commencing with that of the Zoological Society, and describing most of those at home and on the Continent. The principles necessary to the maintenance of unchanged sea-water in a condition fit for the support of animal life are discussed at some length, and a detailed account given of the contrivances employed at the

Crystal Palace. Mr. Lloyd justly places the greatest reliance on constant circulation of the sea-water in the tanks; this is effected by a steam engine and suitable pump, both in duplicate to provide for accidents, capable of forcing from 5000 to 7000 gallons per hour through the range of tanks.

Nearly the whole of the pipes, strainers, and other apparatus, including the pumps, are made of vulcanite, which has been found effectually to resist the action of sea-water, the only exception being some of the larger pipes, which are of stone-ware.

Pages 38 to 64 are devoted to a brief and popular, but by no means unscientific, account of the various animals kept in the tanks. These are arranged according to their classes. Many interesting facts relating to the habits of these creatures are here given, much of which is derived from the author's personal experience in aquaria both at home and abroad.

The publication of this little work is a step in the right direction, and Mr. Lloyd is to be congratulated on having produced a guide quite equal in its own department to the admirable ones of the Royal Gardens and Museums at Kew.

Astronomy and Geology Compared. By LORD ORMATHWAITE. London: John Murray. 1872.

The Origin of Species by Means of Natural Selection. By CHARLES DARWIN, M.A., F.R.S., &c. Sixth Edition, with Additions and Corrections. Eleventh Thousand. London: John Murray. 1872.

Man in the Past, Present, and Future. From the German of Dr. L. Büchner. By W. S. DALLAS, F.L.S. London: Asher and Co. 1872.

THE title of Lord Ormathwaite's work is that of the first Essay, in which the evidence afforded by Astronomy and Geology are compared with the view to ascertain the value of that of the latter science. The third Essay treats of progress and civilisation. The author's thoughts are throughout clearly expressed; and there is little need to tell the reader that under the affliction of extreme dimness of sight an amanuensis has been employed. The second Essay entitled, "Remarks on the Theories of Mr. Darwin and Mr. Buckle," gives the points on which Lord Ormathwaite is at issue with Mr. Darwin, Mr. Buckle receiving but little consideration. Strangely, one of the chief objections to Mr. Darwin's theory is answered by an addition to "The Origin of Species" in the present edition, both works being published at the same time. We quote the reply, because the argument is one often advanced against the theory of the mutability of species. "But as my conclusions have lately been much misrepresented," says Mr. Darwin, "and it has been stated that I attribute the modification of species exclusively to natural selection, I may be permitted to remark that in the first edition of this work, and subsequently, I placed in a most conspicuous position—namely, at the close of the Introduction—the following words:—'I am convinced that natural selection has been the main but not the exclusive means of modification.' This has been of no avail. Great is the power of steady misrepresentation; but the history of science shows that fortunately this power does not long endure. It can hardly be supposed that a false theory would explain in so satisfactory a manner as does the theory of natural selection the several large classes of facts above specified. It has recently been suggested that this is an unsafe method of arguing; but it is a method used in judging of the common events of life, and has often been used by the greatest natural philosophers. The undulatory theory of light has thus been arrived at; and the belief in the revolution of the earth on its own axis was until lately supported by hardly any direct evidence. It is no valid objection that science as yet throws no light on the far higher problem of the essence or origin of life. Who can explain what is the essence of the attraction of gravity? No one now objects to following out the results consequent on this unknown element of attraction; notwithstanding that Liebnitz formerly accused Newton of introducing 'occult qualities and miracles into philosophy.'"

I see no good reason why the views given in this volume should shock the religious feelings of any one. It is satisfactory, as showing how transient such impressions are, to remember that the greatest discovery ever made by man, namely, the law of the attraction of gravity, was also attacked by Leibnitz, "as subversive of natural, and inferentially of revealed religion." A celebrated author and divine has written to me that "he has gradually learnt to see that it is just as noble a conception of the Deity as to believe that He required a fresh act of creation to supply the voids caused by the action of His laws." This quotation not only answers Lord Ormathwaite's objection, but it also removes the obstacle to the reception of the Darwinian theory by the most timorous in acknowledging the advancement of scientific inquiry. It may be said to be the chief addition, as it is that embodying the widest principle; the remaining corrigenda relate to natural science particularly, and are further evidence in support of the theory.

Dr. Büchner divides his work under three heads: "Our Origin;" "What are We?" "Whither are we Going?" In considering the origin of man, the author very carefully brings to the surface all the geological proofs of man's antiquity, buried to the general reader in almost inaccessible works. The method of the arrangement is admirable. "What are we?" cannot strictly be said to follow the Darwinian theory of evolution, because Dr. Büchner's lectures were delivered before Mr. Darwin's "Descent of Man" was known in Germany; but the following obtains in the superior evidence brought forward by Mr. Darwin. Dr. Büchner has answered the preceding questions; but the next he proposes, "Whither are we going?"—we must agree with his translator, Mr. W. S. Dallas, F.L.S., in saying that we do not arrive at the same conclusions. Man, Dr. Büchner, who is a thorough materialist, considers to be immortal, to the extent that the living principle which animates the material form is transmutable, and, following the doctrine of the conservation of energy, is never removed from this world, being absorbed by other, on the decay of the present material; thus it is form only that suffers change. From this basis it would appear that man is to go on improving until the struggle for the means of existence becomes a struggle for existence itself, guided by a fully developed intellect. Then the author proceeds to depict this mundane paradise, governed by a republic, possessing no restrictions as to private property, the restoration periodically of capital to the community. In spite of this philosophical Hades to which we are to be consigned, it is certainly comforting to know that we are not an animal system to be developed by-and-bye into a yet more perfect organisation; but that the advance we make in this present life will benefit us when we become somebody or something else, or a portion of several somebodies or somethings else. Seriously, while admiring the logical continuity of the first sections of the work, it must be said that the last portion is liable to cause the condemnation by many of the entire work. So far from being the opinion that the theory of evolution logically results in utter materialism, Professor Huxley has just said in the "Fortnightly Review," that "personally he was not a materialist, but, on the contrary, believed that materialism contained grave philosophic error." And Professor Huxley is not only a naturalist second to none, but an enthusiastic advocate of Darwinism. We recommend the books collectively and individually to our readers.

A Manual of Zoology for the Use of Students. With a General Introduction on the Principles of Zoology. By H. Alleyne Nicholson, M.D., D.Sc., &c., Professor of Natural History and Botany in University College, Toronto, &c. Second Edition. Edinburgh and London: W. Blackwood and Sons. 1871.

Only six months ago we noticed the first edition of this work as fulfilling a want in our series of text-books. That there were many to whom this work has proved acceptable is evidenced by the early issue of this second edition. The author has taken advantage of the opportunity to make many additions, the plan of classification still being based essentially upon the views put forth by Professor Huxley. Every care has been taken to render the work self-explanatory. An admirable glossary is appended; the book being, indeed, the best hand-book on the subject.

Introduction to the Study of Biology. By H. ALLEYNE NICHOLSON, M.D., D.Sc., M.A., Ph.D., F.R.S.E., F.G.S., &c., Professor of Natural History and Botany in University College, Toronto. Edinburgh and London: William Blackwood and Sons. 1872.

THIS little work is something more than a school-book. It is based chiefly upon the Introduction to Dr. Nicholson's "Manual of Zoology." We commend it for the concise description of the several biological theories, their errors and agreements. The book is well enough bound to form a useful and a handsome present.

The Earth's Crust. A Handy Outline of Geology. By DAVID PAGE, LL.D., F.G.S., Professor of Geology in the College of Physical Science, Newcastle-on-Tyne—University of Durham. Sixth Edition. Edinburgh and London: William Blackwood and Sons. 1872.

THIS is another edition of Dr. Page's Outline-sketch of Geology, now sometime out of print. It forms one of Messrs. Blackwood's Natural Science Series; and is admirably suited to the student attending elementary lectures on geology.

Science Primers. II. *Chemistry.* By H. E. ROSCOE, Professor of Chemistry in Owen's College, Manchester; Author of "The Spectrum Analysis," "Lessons in Elementary Chemistry," &c. III. *Physics.* By BALFOUR STEWART, Professor of Natural Philosophy, Owen's College, Manchester; Author of "Elementary Lessons in Physics." London and New York: Macmillan and Co. 1872.

IN publishing these elementary works on Chemistry and Physics, "the object of the authors has been to state the fundamental principles of their respective sciences in a manner suited to pupils of an early age." Notwithstanding the difficulty of describing in simple language experiments really scientific in their nature, we need scarcely say Professors Roscoe and Stewart have been eminently successful. They have accomplished their object in a manner new to elementary treatises, that of teaching by direct experiment. Nothing is advanced in either of these little works that is not proved by a simple experiment. It ought to be said that the teaching follows the experiment, the young pupils accompanying the teacher as assistants in the exploration of the truths concealed by Nature. Chemistry and Physics in the Science Primers are taught, certainly as they ought to be, not as mathematical demonstrations, but as experimental sciences. We do not hesitate to say that a Science Primer will soon be found in every satchel. But these little books can be viewed in another light: they show that the method of instruction, scientific more particularly, pursued by a Continental nation celebrated for its technical training, is being followed and improved upon by our own schools. We may thus regard them as signs of prolonged national success.

An Elementary Treatise on Curve Tracing. By PERCIVAL FROST, M.A., Formerly Fellow of St. John's College, Cambridge; Mathematical Lecturer of King's College. London: Macmillan and Co. 1872.

THIS treatise requires, for its successful reading, only a slight familiarity with the higher branches of algebra, and is, indeed, an intellectual pleasure-ground for the student of moderate mathematical attainments. The arrangement is in well-ordered sequence.

A Treatise on the Theory of Friction. By JOHN H. JELLETT, B.D., Senior Fellow of Trinity College, Dublin; President of the Royal Irish Academy. London: Macmillan and Co. Dublin: Hodges and Co. 1872.

MR. JELLETT has been the first to remove the theory of friction from the trammels to which it was subject as a department of applied mechanics, and to give it a new standing purely mathematical. Such an endeavour is worthy of the highest encomium, because we derive not only a subject rich in material for mental labour, but are likely to arrive at improved construction of mechanical appliances. It might be said to be an ascent from rule to principle. To the mathematician, as well as to the engineer, this work will be of the highest interest.

A New Star Atlas for the Library, the School, and the Observatory. By RICHARD A. PROCTOR, B.A. Camb., F.R.A.S.; Author of "The Sun," "Other Worlds than Ours," &c. London: Longmans and Co. 1872.

MR. PROCTOR quotes on his title-page the exclamation of Carlyle, "Why did not somebody teach me the constellations, and make me at home in the starry heavens, which are always overhead, and which I don't half know to this day?" How many before and since these words were penned have had occur to them the self-same thought, have desired a familiar acquaintance individually, as it were, with the members of that celestial system which, as far as we can tell, suffers no decay, and which is the most approximate symbol of eternity. Had the exclamation been uttered now, it is only due to Mr. Proctor to say that it would have lost its point, for most worthily has he filled the post of the "somebody" so pathetically lamented by Carlyle. Anyone who can understand a map can use Mr. Proctor's little Atlas. There are twelve maps, each exhibiting a tenth part of the heavens. No pains have been spared to clear the maps of all which could cause confusion to the beginner; but this has been done in such a way that the more advanced student will find nothing wanting. We cannot enter into detail here; but those accustomed to the use of astronomical maps will admit the clearness of this Atlas when we say that α and β Andromedæ are separate 1·25 inches. Not the least interesting portion of the book to the general reader will be that devoted to the cursory consideration of the errors some even of our most popular writers have fallen into when describing a scene under a starry sky. The work is intended as a companion to Webb's "Celestial Objects for Common Telescopes," and is, in fact, an epitome of Mr. Proctor's Large Star Atlas, so well known to telescopists. We hope we may soon have to notice a second edition.

Theory of Heat. By J. CLERK MAXWELL, M.A., LL.D. Edin., F.R.SS. L. & E., Professor of Experimental Physics in the University of Cambridge. London: Longmans and Co., 1871. 312 pp., 8vo.

Technical Arithmetic and Mensuration. By CHARLES W. MERRIFIELD, F.R.S., Barrister-at-Law, Principal of the Royal School of Naval Architecture and Marine Engineering, late an Examiner in the Department of Public Education, &c. London: Longmans and Co., 1872. 308 pp., 8vo.

THESE volumes are two of the admirable series of text-books of science, edited by Professor Goodeve, and published by Messrs. Longmans. Perhaps no other series of elementary works can show so many names of eminent men. We take them in the order of publication, and have to notice first Professor Clerk Maxwell's treatise on the theory of heat. The opening chapters are devoted to thermometry, calorimetry, and elementary dynamical principles. Then, upon this basis, Dr. Maxwell proceeds to lay before the student a perfectly systematised and simple explanation of the difficulties likely to be encountered in pursuing the study of the present theory of heat. There are necessarily some mathematical formulæ introduced into the work, but these are of the simplest kind, and present not the slightest obstacle even to an ordinary reader. The work is turned out with the neatness characteristic of extreme care and of a knowledge of the subject intimate in the highest degree.

The next volume we have the pleasure to notice is that by Mr. Merrifield, the subject being the eminently useful one of technical arithmetic. Technical arithmetic has been, till within the last few years, a very misty affair, the operations being carried on mostly by rule of thumb. This has decidedly not been for the want of published works on the subject, but arises simply from continually treading the same path—rules not principles have been inculcated. Mr. Merrifield reverses the order of things, and teaches principles, leaving the deduction of rules to follow in course. Nowhere is this more evident than in the extraction of roots, a simple formula for the nth root being given, and which can be easily remembered. Be it in the treatment of contracted methods of working the ordinary rules, or in elucidating the principles of solids of revolution, the arithmetic is the best we have seen.

Natural Philosophy for General Readers and Young Persons. Translated and Edited from Ganot's "Cours Elémentaire de Physique." By E. Atkinson, Ph.D., F.C.S., Professor of Experimental Science in the Staff College. London: Longmans and Co. 1872.

Dr. Atkinson's translation of Ganot's "Eléments de Physique," the fifth edition of which we recently noticed, is better suited to more advanced pupils, who are capable of mastering the mathematical formulæ it contains. The present work is elementary and abridged of all matter likely to prove too difficult for the junior student. It must be understood that it is not a smaller edition of the "Elementary Treatise on Physics," but a translation of Ganot's "Cours Elémentaire de Physique," a text-book very extensively circulated in France. Dr. Atkinson has made many additions, and at all times very happily. The illustrations are highly praiseworthy; they are far from resembling the linear sketches with which many of our school-books are disfigured, and, indeed, in some instances, are pretty little views. The letterpress is clear. The work is well calculated to take its stand as a text-book of physics for the middle and upper classes of boys' and of girls' schools, and as a familiar account of physical phenomena and laws for the general reader. It will form an admirable hand-book for students studying to matriculate in the London University.

A Manual of Chemical Physiology, including its Points of Contact with Pathology. By J. L. W. Thudichum, M.D. London: Longmans and Co. 1872.

In Dr. Thudichum's experience as a teacher, the printed notes to his "Researches Intended to Promote an Improved Chemical

Identification of Diseases," published in several numbers of the annual "Report of the Medical Officer of the Privy Council," have proved so useful that he has been tempted to submit them to the public. The manual is a complete epitome of physiological or animal chemistry, revised to the latest date; and is well adapted to the requirements of the medical student preparing to meet the examining board. A second part of the work is an analytical guide, devoted to the assistance of those who desire to make themselves practically acquainted with the phenomena and constituents of animal bodies. The work is systematically arranged throughout, and is well illustrated with engravings of the various absorption spectra.

Elements of Chemistry: Theoretical and Practical. By WILLIAM ALLEN MILLER, M.D., D.C.L., LL.D., late Professor of Chemistry in King's College, London. Revised by HERBERT MCLEOD, F.C.S., Professor of Experimental Science, Indian Civil Engineering College, Cooper's Hill. Part I. Chemical Physics. Fifth Edition, with Additions. London: Longmans and Co. 1872.

THE call for a new edition of Dr. Miller's "Elements of Chemistry" so soon after the lamented death of the illustrious author is a proof, if any were needed, of the extensive circulation of the work. The additions made by Professor McLeod are chiefly from the Transactions of the learned Societies, and are most carefully selected. In gas-analysis, and many other departments of experimental science, Mr. McLeod is himself a high authority, so that numerous little difficulties are smoothed away. This edition is exceedingly complete.

The Discovery of a New World of Being. By GEORGE THOMSON. London: Longmans and Co. 1871.

THIS is a most surprising work. One is surprised at the first few lines, and afterwards at one-self for being surprised. With the familiarity of an old acquaintance—for there is no preface or introduction—Mr. Thomson commences to address the reader thus:—"In the year 1862 the author commenced to write a series of essays on the Human Mind. Being then unacquainted with the systems either of ancient or of modern philosophy . . ." Just what many are so fond of doing—sitting down to write about a matter of which they know, and have taken the trouble to ascertain, nothing. But then the reader and Mr. Thomson are such old acquaintances—sufficient reason that the reader should be button-holed until the author says, in the last page but one of the book, "We must explain what, and how much, we mean, by entitling this essay—The Discovery of a New World of Being." Surely very little more need be said. If the author could explain his discovery in a page, why does he talk for nearly three hundred? But this is but an additional proof how much is needed an essay on the limits of the use of the imagination. "All the faculties of the mind," says Mr. Thomson, "are only a series of flashes of the imagination." What will next be foisted upon the imagination, that convenient vantage-ground for all that is inexplicable. If the use of the imagination is to preclude good logic, and the ordinary courtesies due to the reader, there will not long be many in its favour. There is, however, some recommendation due to the work; Mr. Thomson has a delicate appreciation of the beauties of our religion.

New Theory of the Figure of the Earth. By William Ogilby, M.A., Trin. Coll. Camb., F.G.S., F.Z.S., &c. London: Longmans and Co. 1872.

The author of this *brochure* presents a new theory of the figure of the earth, considered as a solid of revolution, founded upon the direct employment of the centrifugal force, instead of the common principles of attraction and variable density. Mr. Ogilby thinks mathematicians have not yet succeeded in solving the problem of the figure of the earth, and that they depend too much upon algebraic analysis when reasoning upon physical subjects. He says—"It is high time that mathematicians should moderate these unwarrantable pretensions (the omnipotence of analysis to the exclusion of all other modes of philosophical reasoning), understand that the laws of Nature do not depend on the integration of a differential coefficient, or the solution of a transcendental equation, and learn that there are more secrets in Nature than are comprised in the analytical shibboleth $\frac{d^2v}{dt^2}$."

The purpose is to deprecate the *abuse*, not the *use*, of the science of mathematics, but "to employ it, according to the recommendation of Bacon, and in imitation of Newton, not as the mistress, but as the handmaid, of physics,—not as the maker of the laws of Nature, but as their humble interpreter." With this view Mr. Ogilby sets out, making no hypothesis of any description, nor requiring other data than those which are furnished by observation and experience. Starting from the admitted phenomenon that the earth is a heterogeneous solid, whose mean density, magnitude, and periodic rotation are known quantities, the author proceeds to examine the action of centrifugal force in producing its present figure, determining the law of gravity at its surface, the variation of curvature, the length of the terrestrial axis, and the change of local ellipticity at every point of the surface. The results obtained are definite, and we commend them to the notice of our mathematical readers.

Index of Spectra. By WILLIAM MARSHALL WATTS, D.Sc., Senior Physical Science Master in the Manchester Grammar School. With a Preface by H. E. ROSCOE, B.A., Ph.D., F.R.S., Professor of Chemistry in Owen's College, Manchester. London: Henry Gillman. 1872.

DR. ROSCOE, in the preface, alludes to the inconvenience arising from the employment of different scales in mapping spectra. Spectroscopists generally will agree with him. Nothing is more annoying than to find, after collecting the measurements scattered through many volumes, that before the measures are comparable they must be reduced to a common standard. This difficulty will no longer exist. Dr. Watts has much facilitated spectroscopic research, by collecting all existing measurements of the spectra of the elements, and presenting them on a uniform scale of wave-lengths, upon the basis of Angström's measurements of the wave-lengths of the principal Fraünhofer lines. This basis is without doubt the most accurate, for Angström's measurements are so very exact that it is unlikely that any corrections which may be rendered necessary by new and more exact measurements will affect them, except in the decimal place. Besides Angström's numbers, however, Dr. Watts gives those obtained by Fraünhofer, Ditscheiner, Bernard, Van de Willigen, Mascart, and Esselbach. Of course we have the mapping of the spectra of the elements by Huggins, Thalén, and Kirchhoff; and where these three *savants* have not endorsed the examination of the spectrum, in each case reference is made to the original memoir. Mascart, Ketteler, and Müller's numbers were obtained by direct observation of the diffraction spectra. The measurements for chlorine, bromine, iodine, phosphorus, sulphur, selenium, nitrogen, and oxygen, are those by Plücker, and are reduced to wave-lengths by means of an interpolation curve drawn from the lines of oxygen and nitrogen. The measurements and calculations have been verified, as far as possible, by Dr. Watts, upon his own spectroscope, while twenty-eight of the results are checked by comparison with those of Dr. Gibbs. The numerical results are well assisted by the graphic illustrations, in which, on Bunsen's plan, the intensity of the line is indicated by the height. A complete set of chromolithographs, of the double spectra of nitrogen, sulphur, and carbon, is appended. Besides the method of graphic interpolation, Dr. Gibbs's interpolation and extrapolation formulæ have been given, with simple and clearly-explained examples. We are sure that every spectroscopist will agree with us when we say, sincerely is Dr. Watts to be thanked for the laborious task he has undertaken. His work needs no recommendation; it will be, if it has not already been, placed on the shelves of every scientific library. It should be known that the scale of wave-lengths adopted is applicable to spectroscopes of all sizes; there is consequently no difficulty in its universal application.

Index to Gmelin's Handbook of Chemistry. By HENRY WATTS, B.A., F.R.S., F.C.S., Editor of the "Journal of the Chemical Society." London: Harrison. 1872.

THE last volume of this important chemical work has now been offered to the public. To chemists, and to all interested in chemical manufactures, the value of Gmelin's "Handbook" is incomparable; and it is due to us to make known to those intending to complete the set of these volumes that the earlier volumes are becoming scarce, and that early application will be needed to possess those remaining. Subscribers who have paid their subscription for the year 1864 will have forwarded to them the Index and the concluding (eighteenth) volume. The Index is very copious, reference being made to volume and page. Every subject is included under as many heads as possible. It is entirely unnecessary to recommend the work.

The Year-Book of Facts in Science and Art. By JOHN TIMBS, Author of "Curiosities of Science," &c. London: Lockwood and Co. 1872.

THIS is a well-arranged digest of the most important discoveries and improvements of the past year. The title-page is faced with a clear engraving from a photograph of Sir W. Thomson, F.R.S.

Elementary Natural Philosophy. Being a Course of Nine Lectures, specially adapted for the use of Schools and Junior Students. By J. CLIFTON WARD, F.G.S., Associate of the Royal School of Mines; of Her Majesty's Geological Survey. London: Trübner and Co. 1871. 215 pp., 8vo.

THIS little book is evidently intended to serve a double purpose, as a handbook for the very young student, and as an aid to the schoolmaster who may wish to introduce the study of elementary physics into his curriculum. It contains the latest experiments likely to answer the end in view. The work is altogether very praiseworthy.

PROGRESS IN SCIENCE.

MINING.

It has often been remarked that the occurrence of colliery explosions is closely connected with certain states of the weather. A valuable paper on this subject has been read before the Royal Society by Mr. R. H. Scott, F.R.S., the Director of the Meteorological Office, and Mr. W. Galloway. After reviewing the evidence bearing upon this subject, the authors conclude that we are quite justified in maintaining that certain meteorological changes are proximately the cause of most of the accidents in our coal mines. The continuous records kept at Stonyhurst Observatory furnished the meteorological data used in this enquiry. Curves for the barometer and thermometer were plotted for the three years extending from 1868 to 1870, while records of the colliery explosions during the same period were obtained from the Government Inspectors, either directly or from their published reports. On comparing the actual dates of the explosions with the corresponding meteorological records, it was found that out of 550 explosions 266 might be attributed to the state of the barometer, and 123 to that of the thermometer, while the remaining 161 remained unaccounted for on meteorological grounds. In other words, exactly 70 per cent of the explosions were directly related to meteorological influences. Some of the authors' conclusions are of much practical value. Thus, they show that we must not always expect an accident to follow immediately after a fall of the barometer, but that in many cases explosions have not occurred until two or three days after the barometric column had reached its minimum. It appears that during violent oscillations of the barometer the number of explosions does not correspond to the number of minima, but the greatest number of accidents occur when a serious break follows a long period of fair weather. Elevation of temperature must, of course, greatly interfere with the natural ventilation of a colliery; and hence, if a warm day occur in a cold season, when natural ventilation is relied upon, it is very likely to be followed by an explosion. For a like reason, the first hot days of spring are too often marked by colliery accidents.

A paper on increased safety in working coal was read by Mr. S. Firth, M.A., at a recent meeting of the Midland Institute of Mining Engineers. By carefully comparing the dangers incident to the present systems of working by "bord and pillar" and by "long wall," the author concludes that the latter is by far the safer method. But although advantageous in many respects, the long-wall system exposes the collier to considerable danger from falls of the roof, from falls of coal, and from the use of gunpowder. Now, Mr. Firth argues that these sources of peril may be almost entirely removed if coal-cutting machinery be employed in connection with long-wall work: the miner is then no longer exposed to danger by falls of coal, the roof is much safer, and no powder is required. Hence the author advocates the adoption of the long-wall system coupled with the use of coal-cutting machinery, and believes that the danger of colliery work will then be limited to such items as could be readily held in check by strict attention to the Government rules. To use the author's words, it may thus become "possible for miners to work and win coal without the operation being a perpetual fight for life."

To encourage inventors in improving coal-cutting machinery, Mr. Firth has liberally offered a prize of £500 for the machine which shall be best adapted for general use in coal and ironstone mines, and shall reduce manual labour to a minimum. The machines entered for competition are to work with compressed air supplied at a pressure of 50 lbs. to the square inch. It is hoped that this stimulus will lead to great improvements in coal-cutting machines, and that the miner will thus eventually be relieved of one of the most fatiguing and dangerous branches of his work.

An improved form of safety-lamp, which appears to offer considerable advantages, has been constructed by Mr. H. D. Plimsoll. At the base of the lamp is an air-chamber in form of a truncated cone, the upper rim of which closely surrounds the wick of the lamp. The bulk of this cone is occupied by the oil-reservoir, and the space between the outside of this vessel and the inner wall of the conical cavity is reduced to a width of about one-eighth of an inch. Air is admitted to this chamber through very minute orifices in a metal plate, and passes through the narrow space to the wick. The brilliant light thus obtained is not dimmed by any cage of wire-gauze, for the flame is merely surrounded by a cylinder of thick glass. Should any fire-damp enter the lamp, the quantity is said to be too small to break the glass by its explosion. The flame cannot be communicated to the exterior, as it is cooled down by the metal walls of the narrow air-chamber, while the explosion inside the lamp at once extinguishes the flame, and thus prevents the lamp becoming red-hot. If the air be very dangerous, Mr. Plimsoll cuts off all communication with the outside, and connects the air-chamber of his lamp with a reservoir of pure air, and as soon as this supply is exhausted the lamp is necessarily extinguished. As the light is brilliant there is not much temptation for the miner to unscrew the top; but should he do so, a self-extinguishing arrangement comes into play and effectually thwarts his object.

Air-compressing machinery has of late been very successfully applied to underground haulage and ventilation at the Ryhope Colliery, in Durham; and a valuable description of the machinery, by Mr. W. N. Taylor, has been published, with ample illustrations, in a recent number of the "Transactions of the North of England Institute of Mining Engineers." The moving power is a 150 horse-power steam-engine, with two horizontal cylinders, each 32 inches in diameter, and having a 5-foot stroke. This engine is placed at the surface, and works two air-compressing cylinders, each 33 inches in diameter, with a stroke of 5 feet, directly connected with the piston-rods of the steam-cylinders. The air, after passing through four receivers, is finally conveyed to a double hauling engine with two cylinders, 14 inches in diameter, and working with a stroke of 18 inches. This engine is placed underground, and effects the haulage by means of a rope-drum with spur-gear. A large and economical power is thus available underground, and can be readily directed to any part of the workings; manual labour in hewing and putting the coals may in this way be readily superseded. The compressed air is also a valuable ventilating agent, freeing any working-place from gas, and lowering the temperature of the workings so that our deep mines may be wrought with much more convenience to the men and profit to the owners.

It has long been held by Mr. Godwin-Austen, Mr. Prestwich, and some other distinguished geologists—although staunchly opposed by the late Sir R. I. Murchison—that coal-bearing rocks may exist at comparatively moderate depths beneath the newer rocks in the south-east of England. To decide this point an experimental boring is about to be undertaken in the Wealden area. The spot selected for the experiment is at Archer's Wood, near Battle, the property of Lord Ashburnham, who has nobly given the site, and has further aided the undertaking by a liberal subscription. This boring will prove by actual experiment what are the characters and thickness of the strata immediately beneath the Ashburnham beds, or the lowest series of the Wealden formation, in Kent and Sussex. At the same time it will decide whether palæozoic rocks can be reached, at this locality, within a depth of 2000 feet; and, finally, whether the carboniferous strata of Belgium and North-Eastern France extend across the Channel, as has been suggested, and are continuous with some of our western coal-fields. The care of this scientific undertaking, which is one of great national importance, is entrusted to a committee of some of our most eminent geologists, and the work will be commenced at once, so that something may be done before the British Association visits Brighton. A subscription-list has been opened, and large sums have already been subscribed. Should productive coal-beds be discovered at moderate depths, it seems likely that the social aspect of south-eastern England would be completely changed, and agriculture give way to mining.

At a recent meeting of the Institution of Civil Engineers, Mr. E. Bainbridge read a paper explaining the ingenious method of sinking shafts through water-bearing ground without the use of pumping machinery, as brought into practice by Messrs. Kind and Chaudron. As the method was fully explained in our chronicles last January, when noticing a paper read in the North by Mr. W. W. Smyth, it seems unnecessary to refer again to the subject in this place.

All who are interested in the welfare of our West of England metal-miners must take deep interest in the progress of the Miners' Association of Cornwall and Devon. Each year this association issues a neat little report; and last year's report, which is now before us, is not less interesting than its predecessors. In addition to several short papers of local interest, Mr. J. H. Collins offers some remarks on the successive Mining Schools of Cornwall, in which he traces the history of the several schemes which have from time to time been projected for offering technical instruction to the Cornish miner. The last and only enduring scheme is that which set afoot the present Miners' Association—an institution which was founded in 1859 through the praiseworthy exertions of Mr. Robert Hunt, F.R.S.

METALLURGY.

Although it seems likely that the success of Mr. Danks's rotary furnace will eventually lead to a complete revolution in our system of puddling iron, it is obvious that the general introduction of his furnaces must be a work of much time and great expense. In the meanwhile our ironmasters will no doubt gladly avail themselves of any means of improving our present laborious method of manual puddling without involving any material alterations in their existing plant. Such a method has been lately brought prominently forward by Mr. F. A. Paget, who at the last meeting of the Iron and Steel Institute, and subsequently at the Society of Arts, directed public attention to M. Dormoy's process of puddling with a rotating rabble. The rabble is thrown into rapid revolution by steam-power, while its direction is readily controlled by the hand of the puddler. A revolving shaft above the furnace carries a pulley which is connected by a belt with another pulley below. This lower pulley is secured to the outer end of the rabble, which is thus caused to rotate somewhat in the fashion of the well-known revolving brush used by the hair-dresser. By means of a handle jointed to the lower pulley, the puddler readily directs the movements of the rabble. From 300 to 500 revolutions per minute are made by the tool when white pig-iron is treated, and from 800 to 1000 for grey pig. The point of the rabble in the furnace carries a disc which agitates the molten metal, and effectually renews the surface exposed to the air. When the iron "comes to nature," another rabble is used, having, in place of the disc, a short twisted point. Dormoy's method has been successfully employed in Austria and France. It is said that the yield of wrought-iron is increased by at least 30 per cent, while the consumption of fuel is proportionately diminished; that the puddler is relieved of much of his fatigue, although the number of heats in a given time is considerably increased; and, finally, that the phosphorus and sulphur are removed to such an extent from the metal that excellent iron may be obtained from inferior brands of pig. It is right to mention that the rotating rabble, although first brought successfully into actual practice by M. Dormoy, was independently invented and experimentally employed some time ago by Mr. Hutchinson, of the firm of Messrs. Pease, Hutchinson, and Co., of Darlington; but we believe that the results of his experiments, although satisfactory, were never made public.

A rotary puddling machine, recently invented by Messrs. Howson and Thomas, was described by Mr. Howson at the late meeting of the Iron and Steel Institute. This invention is said to be not so much competitive with the Danks furnace as subsidiary thereto. Many of the details of the ordinary puddling furnace are retained, so that the improvements may be adapted, at moderate cost, to the working plant at present in use. The ordinary hearth is replaced by a revolving chamber, of which the best form is that of two cones placed base to base; it is constructed of wrought-iron, with hollow cast-iron trunnions mounted on rollers, which in their turn rest upon a carriage running on wheels in a direction across the axis of the furnace. Motion is imparted by an ordinary engine, and the machine makes from three to eight revolutions per minute. The chamber is protected internally by a lining of bricks of oxide of iron. In order to remove the ball of iron when ready, the chamber is moved from its normal position, and being slightly tilted the ball is readily pushed out through the open trunnions. To avoid cooling the chamber by influx of air through the space formed at the joint of the trunnions—for a certain amount of play must be left to allow for expansion—the opening against which the trunnion works is formed by two cast-iron rings enclosing an annular space, which communicates by a flue or pipe with the space immediately above the fire-grate. By the draught thus produced in the annular space the air which leaks in at the joint is carried off by the most direct route, and consequently little or no air gains admission to the working chamber. This method of checking the ingress of the air is found to be of great importance in the practical working of the machine.

Mr. J. Head, of Middlesbro', has described at some length the Newport puddling furnace, of which a model is exhibited in the International Exhibition. The chief feature of this furnace consists in its economy of fuel. Records of the working show that a given weight of puddled iron may be produced by little more than half the quantity of coal required in the ordinary furnace, while there is also a saving of about 10 per cent of iron. Thus the average consumption per ton of puddled bar was 12 cwts. 3 qrs. 6½ lbs. of coal, and 20 cwts. 2 qrs. 24 lbs. of refined iron; in other words, the puddled bar weighed 96½ per cent of the iron charged. The fettling employed was made in a tap furnace from various cinders and scraps, and contained no hæmatite or other expensive ores.

It is almost needless to mention at this date that the detailed reports on the now celebrated Danks furnace, by Messrs. J. Jones, Snelus, and Lester, were submitted to the Iron and Steel Institute at their last meeting. These reports of the individual commissions supplement the general joint report dispatched from America, and referred to in our Chronicles last quarter. It is gratifying to learn that the high opinion expressed by the Commissioners has been fully borne out by the results which have already attended the working of the furnaces erected on this principle in England. The first of the new furnaces was put up by Messrs. Hopkins, Gilkes, and Co., of Middlesbro', and has worked with great success. In a purely scientific journal we may perhaps be spared the pain of referring at any length to the technical objections which were raised in certain quarters against the validity of Mr. Danks's patent-rights in this country.

Few points in the metallurgy of iron are more important than the elimination of phosphorus from pig-iron containing this prejudicial element. The extent to which it may be removed by the Henderson process is well seen in some determinations recently made by Mr. E. Riley, and published in the "Chemical News" (May 17, p. 237). At a trial made at the Blockhairn Iron Works, in Glasgow, on the 23rd of last December, the charge consisted of 360 lbs. of No. 4 Dalmellington pig-iron, 100 lbs. of ilmenite, 10 lbs. of manganese, and 42 lbs. of fluor-spar. The pig-iron contained 1·14 of phosphorus, but this was eliminated at such a rate that samples of the refined iron taken in succession at thirty, forty, and fifty minutes after fusion contained respectively 0·23, 0·15, and 0·12 of phosphorus. Finally, the phosphorus was so far removed that the wrought-iron contained only 0·07 per cent. On the other hand, the cinder yielded 0·52 per cent. As it was found that all the phosphorus had not passed into the cinder, some of it must have been volatilised during the refining. Samples of bar-iron and plate rolled from iron puddled under

Henderson's treatment, have been tested by Mr. Kirkaldy, and found to stand a very high tensile strain.

At a recent meeting of the Institution of Civil Engineers, Mr. I. Lowthian Bell read an interesting paper on the conditions which favour and those which limit the economy of fuel in the blast-furnace. As iron-smelting consumes nearly one-sixth of all the coal raised in the country, it is important that the highest degree of economy should be practised. Having determined the quantity of heat absorbed in the process of smelting, and the calorific power of the fuel employed, Mr. Bell proceeds to determine the weight of combustible necessary to produce the required effect. Assuming that the ore yields 40 per cent of pig-iron, and requires 15 cwts. of limestone as flux, it will need theoretically about 25½ cwts. of ordinary Durham coke to produce a ton of iron. Mr. Bell discusses the theory of the hot-blast, and believes that its values lies in this principle—that the rate of reduction must not proceed less rapidly than that of fusion. By constructing the furnace sufficiently large the use of hot-blast may be dispensed with, and yet no fuel sacrificed. Beyond a certain limit, however, no advantage is gained by increasing the capacity of the furnace.

Mr. E. F. Mondy has called attention to the magnetic properties of certain copper-slags. Several specimens of ore-furnace slag exhibited strong polarity; those which presented a porphyritic appearance from included fragments of quartz, being more strongly magnetic than a vitreous specimen. Polarity was also evident, though to a less extent, in crystallised metal-slag and in roaster-slag. Feeble magnetism was also exhibited in refinery-slags.

In the April number of the "Journal of the Royal Institution of Cornwall," Mr. J. H. Collins publishes a note on a portion of the incrusted surface of a block of jew's tin found on Tremathack Moor, in the parish of Madron, and recently acquired by the Institution. All ancient blocks of tin pass in the West of England under the name of "Jew's tin," whatever may have been their origin. The block in question was partially coated with a hard, brittle, brown incrustation, which was found to contain 90·62 per cent of binoxide of tin, 1·66 of chloride of tin, 1·04 of peroxide of iron, 0·43 of metallic tin, 0·41 of silica, and the rest of moisture evolved at 120° C.

MINERALOGY.

For a long time mineralogists were acquainted with only a single species of crystallised silica. This was the well-known and ubiquitous mineral—common *Quartz*—a mineral which, as every one knows, crystallises in the hexagonal system, and has a sp. gr, of 2·6. Not long ago Professor Vom Rath showed that silica was dimorphous, and described under the name of *Tridymite* a new species which had a sp. gr. of only 2·3, and yet crystallised in the hexagonal system, but with different parameters from those of quartz. We now learn that silica is trimorphous, for a third form of crystallised silica has been discovered by Professor Nevil Story-Maskelyne, F.R.S. For this new species the name of *Asmanite* is proposed—a name fancifully derived from Asman, the Sanscrit term for the thunderbolt of Indra. Indeed, Asmanite is a meteoric mineral, and was detected among the constituents of the meteorite which was found in 1861 at Breitenbach, in Bohemia, and is now deposited in the British Museum. Asmanite has a low sp. gr. (2·245), and in this point resembles Tridymite, from which it totally differs, however, in crystalline form. Optical examination shows that it is a biaxial mineral, and crystallographic measurements prove that it belongs to the orthorhombic or prismatic system. It is difficult, however, to measure the specimens, for the mineral occurs only in minute grains, more or less rounded, and exhibiting but few crystalline faces. Professor Maskelyne has nevertheless determined the ratios of the parameters of the crystals, and the inclination of the optic axes. Its hardness is 5·5. Two analyses show that it consists essentially of silica, and contains but a small percentage of foreign matter. The Asmanite is associated in the Breitenbach meteorite with enstatite, chromite, troilite or meteoric pyrites, and nickeliferous iron.

Professor Viktor Von Lang has published an account of the crystalline form of *Guarinite*, a mineral resembling sphene. This mineral was described by Guiscardi, who referred it to the tetragonal or pyramidal system; but the author shows that, from the optical characters of specimens in the British Museum, it must be transferred to the rhombic system, although the angles of the crystals and their general habit closely resemble those of a tetragonal mineral. The same distinguished physicist publishes his crystallographic determinations of the form of the rare mineral *Leucophane*.

In examining some of the fine crystals of *cuprite*, or red oxide of copper, from near Liskeard, Professor Schrauf, of Vienna, has detected an icositetrahedron of the value 322, and therefore new to this species.

A note on the occurrence of cobalt in connection with the tin ores of Cornwall has been published by Mr. R. Pearce in the "Journal of the Royal Institution of Cornwall." The author has found cobalt in a sample of tin-stone from Dolcoath Mine, where it probably occurs as an arsenide of cobalt in the mispickel, or white arsenical mundic, associated with the tin. He also finds that the furnace-product known to tin-smelters as "Hard-Head," contains cobalt derived from the tin ore which may have come from different localities. One specimen of hard-head contained 4·4 per cent of cobalt, and Mr. Pearce suggests that its extraction may be profitable, especially if the tin can at the same time be obtained, as the specimen in question yielded as much as 16·25 per cent of tin.

Two new instances of pseudomorphism are recorded in "Poggendorff's Annalen," by M. Helland, of Christiania." One of these is a pseudomorph of mica after garnet from Röstöl, near Arendal, in Norway. The garnet is an iron-alumina variety, rich in manganese, and occurs in small icositetrahedral crystals of a reddish brown colour in a veinstone of pegmatite composed of orthoclase, oligoclase, and mica. Some of the crystals, while retaining their form, have become converted into a green potash-mica. A nucleus of unchanged garnet remains in the centre of some of the partially altered crystals. The second pseudomorph consists of steatite in the form of augite, and occurs at Snarum, not far from the locality which yields the celebrated pseudomorphs of serpentine after olivine.

It has been recently found by Professor Zirkel that the felspathic mineral called *Bytownite*, usually grouped with the lime-soda felspars, so far from being a pure and homogeneous mineral, is made up of at least four distinct constituents. Microscopic sections show that it is really a crypto-crystalline rock containing a triclinic felspar, a green hornblende, a mineral taken to be quartz, and black granules of magnetic iron. Henceforth, then, Bytownite must be expunged from our text-books on mineralogy.

Some experiments on the alteration which felspars suffer by the action of saline solutions and other agents have been made by M. H. Birker and R. Ulbricht, and are of interest as bearing upon the natural weathering and alteration of felspathic minerals. The felspar used contained 8·51 per cent of potash, 3·37 soda, 1·3 baryta, 16·03 alumina, and 65·52 of silica. The action of the several agents was allowed to go on for 2½ years. Distilled water had practically the same solvent action, whether it contained air or not. Carbonate of lime, nitrate of lime, and several other salts exerted but little more action than water alone. Carbonic acid, and carbonate of lime with carbonic acid, increased the action on the alkalies and the silica. Lime-water dissolved more of the felspar by acting on the silica; sulphate of ammonia had an energetic action; but of all the agents used magnesia was found to be the most active.

A set of useful Mineralogical Tables, arranged by Dr. J. Emerson Reynolds, has been published in a recent number of the "Journal of the Royal Dublin Society." Although compiled primarily with a view to facilitate the examination of the fine mineral collection in the possession of the society, these tables admit of much wider use, inasmuch as they contain a considerable amount of general information, and form indeed a concise synopsis of the mineral kingdom. The crystalline system, chemical formula, lustre, colour, hardness, and specific gravity of each species are registered in these tables. In calculating the formulæ, the new atomic weights have been employed, and the quantivalence of the elements marked in most cases. Many of the formulæ are constructed, however, upon an original plan proposed by Dr. Reynolds some time ago, and described in the "Philosophical Magazine." The tables are preceded by an explanatory introduction, indicating the system adopted in the arrangement of the collection, and containing notes on the more interesting species in each group.

The third part of Dr. Schrauf's great work—the "Atlas der Krystall-Formen des Mineralreiches"—has lately been published. This part contains ten plates comprising 160 finely-executed figures of crystals. The species are arranged in alphabetical order, and the crystals figured in the present part extend from "Apophyllite" to "Barytes."

ENGINEERING—CIVIL AND MECHANICAL.

Guns.—We have, on former occasions, recorded the history of the celebrated "Woolwich Infant," as the first 35-ton gun was named. This gun has, since the completion of its proof, remained inactive in front of the butts, not having fired a shot since the trial was made which proved the non-importance of the injury done to its steel lining. Nine other similar guns, completed at the Royal Gun Factories, have since been tested, their proof consisting of three rounds only, viz., two with the ordinary service charge of 110 lbs., and one with the proof charge of 115 lbs. of gunpowder—a more severe strain than they are ever likely to have to endure again.

On the 23rd of March last a paper was read before the Institution of Civil Engineers by Mr. Bashley Britten, "On the Construction of Heavy Artillery, with Reference to Economy of the Mechanical Forces Engaged," wherein it was remarked that so long as nothing definite was known as to the power exerted by gunpowder, the strongest guns were deemed the best. But a good gun was a scientific instrument, which like any other engine, ought to produce the greatest results with the highest economy of means. When the principle of the rifle was applied to cannon, and elongated projectiles were introduced, the skill of engineers soon conquered the difficulty of producing ordnance of ample power to resist high bursting pressure; but this success in the manufacture of strong guns led to the disregard of other considerations of even greater importance, chief amongst which was the use of the strongest powder, which after from 500 to 600 rounds destroyed the interior of the guns by the intense heat and the pressure of the gases. This led to the appointment, in May, 1869, of the Committee on Explosives. From their report, printed in 1870, the following argument was founded. The Woolwich heavy guns were designed for R.L.G. powder, and with it were found to be strong enough to bear all the marginal proofs. The 8-inch guns had to bear a pressure of from 25 to 30 tons per square inch, and the 10-inch guns from 45 to 50 tons per square inch, from the service charges; but henceforth, with pebble powder, the pressure would never exceed about 15 tons in the former and 20 tons in the latter case, as no benefit would be derived from a higher pressure. The principles represented in the designs of the Woolwich system of ordnance were examined under the following heads, viz.:—(1) the quality of the material used; and (2) the power of the charge; and it was argued that with the use of pebble-powder cast-iron might be used with perfect propriety for the exterior of ordnance, at a saving of at least 50 per cent of the cost of the new guns even of the present type. A comparison was then entered into with reference to the proportion which the bore should bear to the length of the gun, small bores and long chases being preferred on account of the greater amount of expansion that would be utilised from the explosion of the powder charge under such conditions.

On the 17th of May last Commander W. Dawson, R.N., read a paper before a meeting of the officers of the two services, at Portsmouth Dockyard, upon the "Seven-Inch Gun." This paper related exclusively to the system of rifling adopted in the manufacture of ordnance at the present day. After tracing the history of the introduction of rifling cannon in this country, the author then gave the results of certain competitive trials which took place in 1863, and after describing the simplicity of the Scott centreing iron-bearing projectile, and the peculiarities of its construction, it was observed that the distribution of strain and the perfect centreing in the bore fully accounted for the grooves of the Scott gun having been as perfect at the end of the competition as when the gun left the factory. It also explained why the centreing shot of the same weight, fired with similar charges, escaped out of its gun so much more readily than the French or Woolwich one, striking a far more heavy blow. The relative merits of the uniform and increasing spiral were then dwelt upon, and the superiority of the former system most conclusively proved by reference to the results of actual experiment. It was shown that the stud system of centreing was ill-adapted for any rapid twist of spiral, and it was stated that the question of long-bearing centreing projectiles *versus* the short-bearing non-centreing system was now under consideration.

Torpedos.—A very interesting paper was recently read by Mr. C. W. Merrifield, F.R.S., before the Institute of Naval Architects, on "Torpedo Warfare," the main consideration being as to how a vessel may best be constructed with a view to defending it from the action of torpedos: the one great principle advocated by Mr. Merrifield being that of sub-division. Extending armour plates over the bottom of a vessel, whilst it would add considerably to its displacement, would not afford any sure means of defence; for approaching a coast known to be protected by torpedos, a number of small ironclads were preferable to one large one, as in the event of a serious accident the loss in life as well as in material would be less. Again, these vessels should be absolutely subdivided into compartments, with air-tight coverings, so that in the event of an accident to one compartment, the water might be driven out by means of pumping air into it. In order to neutralise the effect of the blow given by the explosion of a torpedo, the author proposes that the bottoms of vessels should be constructed with double cells, the lower one being filled with water, which, acting as a buffer, would distribute the blow of concussion and render it less injurious to the fabric of the vessel; the upper cells should be connected by means of pipes and stop-cocks with an air-pump, so that in the event of the middle skin becoming broken the upper cell would be cleared of water by means of a "plenum."

Sewage of Paris.—Our neighbours across the Channel, no less than ourselves, are engaged in the consideration of that most important of all sanitary questions, viz., how best to dispose of town sewage. The Paris sewers at present discharge themselves into the Seine at Clichy and St. Denis, by two principal outfalls, giving a daily discharge of 8,850,000 cubic feet, which affects the water of that river in three different ways. The sand and mud are deposited near the outfall of the two collectors, and the volume of the deposits reach the amount of 3½ millions of cubic feet a year; they produce fermentation in the warm weather, and it is necessary to dredge constantly at a yearly cost of about £8000. The light mud and organic matters rest on the surface of the river and pollute the water for a considerable distance, and the soluble matters produce serious impurities. Since 1867 experiments have been going on, first at Clichy, and subsequently at Grenvilliers, for the purification of the sewage waters with sulphate of alumina, and for utilising them for fertilising purposes. The Conseil des Ponts et Chaussées now, after recommending a complete dredging of the Seine, state that the experiments made at Grenvilliers having given satisfactory results as to the disinfecting the sewage water by their application to irrigation, and by their purification with sulphate of alumina, and these results being such that, if they were known to have a durable efficacy, they would prevent the discharge into the Seine of the impure sewage water, they counsel the Municipal Administration to continue and to develop these experiments. The Prefect has now submitted for the approbation of the Municipal Council a project which will, however, only deal with less than a third of the Paris sewers, but will form a part of the complete scheme. Should the work be undertaken we hope to be able to give further particulars regarding it it in a future number.

Clifton Tunnel.—This tunnel, which will be about one mile in length, is being constructed with the view of affording railway communication between Bristol and the new docks now in course of construction at Avonmouth. The tunnel will be straight, and on an incline of 1 in 64 throughout its length. The tunnel is being bored from each end, and, no water having been met with, the working down hill is just as easy as working up hill. A top heading is being constructed about 10 feet wide and 8 feet high, and the widening out of the tunnel is being carried on at some distance behind, the face of the full-sized tunnel being generally about 30 yards behind the face of the heading. One of Captain Beaument's diamond drilling-machines is being used for boring holes in the face of the heading, the rock being removed by blasting. The enlargement of the tunnel to the full size necessary is done by hand labour in the usual manner. The holes which are bored by the drills are about 2¼ inches in diameter, having a core of 1⅛ inches in diameter, and from 3 feet to 3 feet 6 inches deep. The core is easily removed, and the hole is then left beautifully straight and circular. Some difficulty is experienced in getting out the central piece of the heading; but after this is done the remainder comes down easily. The explosive employed is dynamite, which is found to answer much better than powder. The material through which the tunnel is constructed is very hard. The air-engine which drives the drill works at about 130 revolutions per minute with the air at 40 lbs. The air is compressed by a steam-engine at the top of the shaft, whilst the exhaust-air from the boring machine ventilates the tunnel perfectly.

Subaqueous Foundations.—The method of sinking cylinders for foundations by the use of compressed air was first suggested by Mr. John Wright, of Rochester, about twenty years ago, when Resident Engineer upon the Rochester bridge. Recently Mr. Wright has made certain modifications in his previous invention, in order to provide a means by which tunnels can be constructed on the beds of rivers or in other waters, to obviate the necessity of having any thickness of earth between the top of the tunnel and the bottom of the river. For this purpose, caissons or bells open at the bottom, and from which water is kept out by air under pressure, are employed, and a continuous foundation can be formed. The caisson having been lowered into position where a section of a foundation is to be constructed, the water is forced out in the usual manner, and the work of the foundation proceeded with, the section of the work being made somewhat shorter than the diameter of the caisson, in order that the latter may be lifted and moved a distance after one section of the work has been completed. Upon a continuous substructure thus formed, the tunnel is constructed in horizontal courses in a similar manner, except that instead of the sections being made solid, they are now formed of a sufficient width for the purpose, a bell of suitable size being employed. While the upper part of the first course of blocks is being built separately within the bell, a portion of the invert, or so much of the tunnel as is to be contained in the height of the lower block is formed, the hollow of the invert thus made being filled in with puddle to the full height of the block. The sides of the tunnel may also be formed in suitably shaped blocks, care being taken that the space contained within the sides is filled with clay as the work proceeds, until the tunnel is completed by arched blocks forming the roof; and when the joints are all made good, the puddle can be removed, leaving the work finished. The caisson or bell can readily be raised and shifted from place to place by a traveller, running upon rails supported on piles, the traveller being fitted with a suitable engine and travelling gear. Inside the bell a tank or hopper is fixed, and from this a shaft rises through the top of the bell to above the surface of the water, this shaft being open at each end, and dipping a short distance below the mouth of the hopper. The shaft and hopper are always kept filled with water, by means of which the escape of air from the bell will be prevented. Within this shaft is conducted a set of revolving buckets, by which all earth excavated from the bottom of the bell and thrown into the hopper can be drawn up.

Hydraulic Boring Machine.—A novel invention by Mr. Theodore Allen, of the American Society of Civil Engineers, has recently been adopted by the Department of Docks of the City of New York, with a view to obtain a survey of the hard bottom of the Hudson and East rivers, adjacent to New York City. By Mr. Allen's plan the boring rod is forced down by means of hydraulic pressure. As a basis it was considered that a pressure of 1000 lbs. to the square inch upon the area of the tube would be ample for all purposes,—that is, when a resisting of density sufficient to withstand that force was met with, it would more than suffice to sustain the weight upon the piles. The tube itself had to be of considerable area in order to bear this pressure when unsupported for some distance in the water or soft mud. A tube of 2⅞ inches exterior diameter, and 1⅝ inches interior diameter was deemed the smallest that could be used with safety. The length of sections of the tube was fixed at 8 feet, the weight of a section of this length being as great as could be conveniently handled. This tube was then secured or clamped to a cross head by a contrivance which, while securely holding the tube, permitted it to be turned round. At each end of the cross head a piston rod was attached, the pistons moving vertically within a cast-iron cylinder of 6 inches diameter, and having sufficient length to admit of a stroke or movement slightly greater than the length of each section of the tube. The pistons are packed with cup leather packing, and the cylinders are supplied by means of a fly-wheel pump, the proportions of which enable 50 lbs. pressure per square inch in the steam cylinder to exert a force of 500 lbs. pressure per square inch in the pump barrel. In order to regulate the pressure upon the tubes an ordinary weighted lever valve is used, the raising of the lever informing the attendant that the requisite resistance has been obtained. During the test, before the acceptance of the apparatus, the safety-valve was set at 450 lbs. to the square inch, causing a total pressure on the tube of 23,850 lbs., or 3674 lbs. to the square inch.

Engineering Societies.

Institution of Civil Engineers.—Mr. Emerson Bainbridge read a paper on the 5th of March last, "On the Kind-Chaudron System of Sinking Shafts through Water-bearing Strata without the use of Pumping Machinery." The author pointed out that the plan of sinking pits hitherto practised in this country consisted in dealing with the water by means of large pumping-engines, in leaving the bottoms of the pits dry enough to allow the sinkers to block the well, and in keeping back the water in the upper strata by metal rings, cast in segments about 4 feet long, and connected by wooden joints, which were wedged tight, when all the tubbing was fixed. The evils of this system were:—1. The heavy first cost of the plant when special pumping machinery was used. 2. The expense of the wedging tubs, and the cost of fixing them. 3. The delay caused by the sinkers being compelled to work always in water. 4. The first high cost of the tubbing and of fixing it in the shaft, and the liability of the tubbing leaking in consequence of the numerous joints. In the application of the Kind-Chaudron system these evils were to a great extent avoided. This system consisted of a combination of Mr. Kind's well-known apparatus for boring wells, with an ingenious device, invented by M. Chaudron, for fixing cylindrical tubbing under water in such a manner as to make it quite secure and water-tight. A comparison between the two systems showed, that whilst with the system of sinking by the aid of pumping machinery, the average cost per foot had amounted to £114 7s., and the rate of sinking to 8·9 feet per month, with the Chaudron system the average of all the pits was equal to £22·9 per foot, and the speed of sinking to 15·8 feet per month.

On the 12th of March last Mr. J. H. Latham read a paper "On the Soonkasala Canal of the Madras Irrigation and Canal Company." This canal has recently been opened for its entire length from Soonkasala to Cuddapah, a distance of 190 miles. The objects of this canal are twofold; first, the continuous irrigation throughout the year of large areas of country in the Bellary, Koonder, and Nellore Districts; and the second purpose, apparently necessary to the success of the first, was a means of communication by canals, by which the produce of those areas could be sent to a market more cheaply than by cart or railway—more expeditiously than by cart, and more securely than by railways. After giving a brief history of the canal, the author gave detailed particulars of some of the most important works undertaken in its construction; its capabilities for irrigation and navigation; and finally concluded by a comparison between it and the Grand Junction Canal between Brentford on the Thames and Braunston.

A paper on "Explosive Agents applied to Industrial Purposes," was read by Mr. F. A. Abel, F.R.S., on the 14th of May. The nature and properties of gunpowder, and its special advantages and defects as an explosive agent for industrial purposes, was first briefly reviewed. The application of other compounds, including the use of chlorate of potash, including "poudre picrate," and those produced by the action of nitric acid upon organic substances, such as nitroglycerine and gun-cotton, were referred to, and the two latter particularly dwelt upon and explained, and their advantages for blasting purposes were enumerated. In conclusion, after referring to some recent interesting experiments of Dr. Sprengel, on the application of readily oxidisable and other powerfully oxidising liquids in the production of violently-detonating mixtures, the author showed that, even in the application of gunpowder to industrial purposes, some decided advance had lately been made, for its violent explosion could be developed, like that of all other explosive mixtures and compounds, through the agency of a detonation, whereby its action might be considerably intensified, and its application to some important classes of work, such as in submarine operations, greatly facilitated.

Institution of Mechanical Engineers.—At a meeting of this Society on the 2nd of May last at Birmingham, after the adjourned discussion upon a paper read at the last meeting, "On the Strength and Proportions of Rivetted Joints, &c.," a paper was read, "On a Steam Jet for Exhausting Air, &c., and the Results of its Application," by the President, C. W. Siemens, Esq., F.R.S., D.C.L. The form and application of the steam jet having remained hitherto essentially the same as in the original steam blast of the locomotive, it occurred to the author that much might be done to improve its effect by a judicious arrangement of the parts, so as to avoid eddies in the combined current of steam and air, and to utilise more completely the initial momentum of steam. These objects have now been effectually accomplished by the employment of a very thin annular jet of steam, in the form of a hollow cylindrical column, discharged from an annular nozzle. The air to be propelled by the steam jet is admitted through an exterior annular orifice surrounding the jet, and also through the centre of the hollow jet; and the area of the air passages is gradually contracted on approaching the jet, whereby the velocity of motion of the entering air is so much accelerated, before it is brought in contact with the steam, as to avoid the great difference in the velocity of the two currents at the point where they come together, which caused the eddies that previously impaired the efficiency of the steam jet. By the annular form of the steam jet the extent of surface contact between the steam and air is greatly increased, and the quantity of air delivered is by this means very much augmented in proportion to the quantity of steam employed. The combined jet of steam and air is discharged through an expanding delivery pipe of considerable length, in which its velocity is gradually reduced and its momentum accordingly utilised by being converted into pressure. This improved steam jet has been applied for exhausting one of the pneumatic despatch tubes employed at the Central Telegraph Station in London, for conveying the carriers containing telegraphic despatches from one station to another. Another application is to the lifting of water from a moderate depth; for exhausting the vacuum pans employed in sugar-boiling; and as a blower for accelerating the distillation of fuel in gas-producers for heating purposes.

LIGHT.

Mr. Harcourt has found that by heating to redness a mixture of hydrogen and bisulphide of carbon vapour, the latter is decomposed into sulphuretted hydrogen. The removal of bisulphide of carbon from coal-gas is thus rendered possible, for by heating the gas to redness the sulphur combines with hydrogen to form sulphuretted hydrogen, which can be easily removed by passing through a purifier containing oxide of iron. By passing coal-gas, containing 30 grains of sulphur in 100 cubic feet, through a red-hot tube, and then through an iron purifier, the sulphur was reduced to about 5 or 6 grains in 100 cubic feet. It might be imagined that the passage of the coal-gas through a red-hot tube would deteriorate its quality; but Mr. Harcourt found the contrary to be the case. By passing gas of 14·91 candles rapidly through a tube heated to dull redness, the illuminating power was found to be 15·1 candles, and by passing the gas through a tube heated to bright redness the illuminating power was increased to 16·66 candles. A parallel case to this occurs when marsh-gas is decomposed eudiometrically into hydrogen and carbon; the resultant gas occupies almost twice the original volume of the gas, but possesses a far greater illuminating power than that of the original marsh-gas, owing to the presence of acetylene or some such body. It seems easy to deduce from Mr. Harcourt's results a process for the desulphuration of coal-gas.

In reference to the very simple and ingenious arrangement for a vertical lantern made by Mr. Ferguson, and described in our last issue, we would add a further suggestion made by Prof. Morton, President of the Stevens Institute of Technology, whose form of the same apparatus we published in our last October number. This is to carry yet a step further the simplicity of the instrument by substituting a watch-glass full of water for the photographic or other objective lens. Dr. Morton also tells us that he finds this method can be developed into a very striking illustration of refraction and the action of lenses in the following manner:—A well-defined photograph on glass is placed as an object in the vertical lantern, the place of the objective being supplied by an empty watch-glass previously adjusted. There will then appear on the screen no image, but only a nebulous mass of light. On pouring water into the watch-glass, however, a sharply-defined image will come out as soon as the water ceases to move in ripples. By removing the water and substituting alcohol or some more highly refracting liquid, such as a solution of muriate of tin, a lens of shorter focus and higher magnifying power will be obtained.

Dr. Morton sends us also the following explanation of the general relation between the condenser and objective in the magic lantern, on which was founded the plan first adopted by him of separating the ordinary lantern condenser into two elements, one of which was placed before and the other after the mirror of the vertical lantern, by which the efficiency of that instrument was so largely increased. It is curious to note that this point seems to have escaped the able constructor, M. Duboscq, who publishes in *Les Mondes*, vol. xxiv., p. 649, a vertical lantern in which this very important element is absent. "If a system of condensers are well arranged, the front surface of the outside lens will be evenly illuminated, as may be seen by placing a sheet of thin paper against it, but from this surface the rays will travel outwards with all the irregularities of distribution caused by spherical and chromatic abberration, and by the action of the condensers as image-producer of the source of light." "If now the object-glass while in focus with the object, is also in focus with the front of the condenser, it will produce on the screen an image of the equally illuminated surface of the condenser, the irregularities in direction of the rays as they leave that surface having, as we know from the simple theory of image formation by lenses, no effect inimical to such a result. But if the object is moved forward on the cone of rays issuing from the condenser, then, when the object-glass is in focus with this, it will tend to produce an image of a section of the light cone made in the same plane." Here, however, the irregularities in distribution of the light before noticed have developed to the extent of rendering the area of such a cross section very irregular in illumination and colour, and this irregularly-illuminated surface the object-glass will reproduce in the image which it throws upon the screen. "If in the vertical lantern we place the condensers all together before the first mirror, then the object above the mirror will be at a considerable distance from the front of the condensers, and the ill results already named will follow. If, however, we place, first, only so much of the condenser as will bring to parallelism the rays diverging from the source of light, they will then pass on to the remaining lens after reflection, exactly as they would have done had the condenser occupied its usual position, and will evenly illuminate its surface in contact with which the 'object' finds itself, and thus they will produce a field evenly illuminated. It is this set of conditions which has defeated the attempt frequently made to use large condensers with small pictures, by moving the latter forward on the cone of rays coming from the former. For use with the vertical lantern I find the following combination of lenses makes a very good condenser:—Nearest to the light a plano-convex lens of 4½ inches diameter and focus of 9 inches; next to this, with its flat side also towards the light, a plano-convex lens of 5 inches diameter and 7 inches focus; then beyond the mirror a plano-convex lens 5 inches diameter and 8 inches focus, with its flat face upwards or away from the light. One might naturally suppose that with so rude a lens as a watch-glass full of water a very ill-defined and unsatisfactory image would be formed; but this is not the case, because the rays of light which pass through any point of the object do not spread over the whole of the object-glass (as is the case with the rays from an object which enter an ordinary portrait camera or telescope), but fall upon a small element of the lens only. They therefore acquire only the spherical and chromatic error proper to such a small element, and not that due to the entire lens. Any one familiar with the action of lenses for other purposes will be astonished by the accuracy of the image obtained in the lantern with a common plano-convex lens whose curved face is turned towards the object." "It is, of course, the previous concentration and focalising of the rays by the condenser which accomplishes this good result in the manner named."

The "Journal of the Franklin Institute" reports a lecture by Professor A. M. Mayer, at the Sheffield Scientific School, which included a very beautiful acoustic illustration of the method of determining stellar motions by the spectroscope. The lecturer dwelt upon the analogy between sound waves and those of light. Whenever one of the coloured waves composing white light is lengthened, its colour is modified in the direction of the red end of the spectrum, and *vice versa.* The motions of a star when at right angles to the view is susceptible of telescopic measurement; but, the lecturer continued, the motion of a star when in the direction of the line of sight can only be measured on the foregoing principle, the standard being the displacement from their normal position of certain lines in its spectrum. This principle is capable of illustration by means of waves of sound. With the lantern an image of a tuning-fork beating 256 times a second is thrown upon the screen. By the side of one of the prongs, and just touching it, a cork-ball is suspended by a filament of silk. On sounding a second fork tuned in accurate unison with the first, anywhere in the room, even at a distance of 30 feet, the first is thrown into vibration, and the image of the cork-ball projected a foot or two away from the prong. When, however, the second fork is carried rapidly to or from the first, no motion of the cork is perceived, the wave of sound being shortened or lengthened by an amount sufficient to throw it out of unison with those produced by the first fork. If a fork vibrating 254 times a second be now rapidly approached to the first, the waves are so much shortened as to be in unison with it and to throw the ball into motion. A fork vibrating 258 times a second carried rapidly from the first, exhibits by the lengthening of the waves the same phenomenon.

Microscopy.—Dr. James Murie, F.L.S., &c., continues his paper* on the classification and arrangement of microscopic objects in a communication to the Royal Microscopical Society. The principles of classification are dwelt upon at some length, a matter of especial value in the case of large and varied collections. Several instances are given of the mode of settling the proper place of objects which might be entitled to be located in any one of several departments. For example, of egg-shell the author says, "Although composed of the salts of lime, it is virtually an animal product, and therefore to

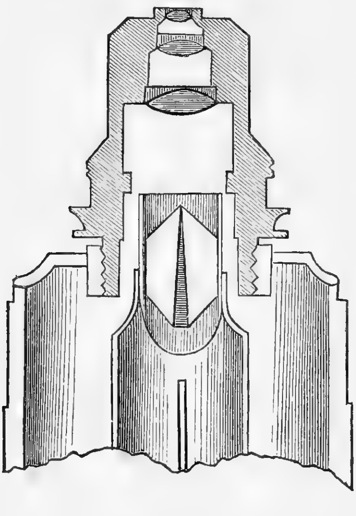
* Monthly Micro. Journ., vol. i., p. 69; vol. vii., p. 201.

be excluded from the inorganic division. As to its physiological place, it may take rank among component elementary bodies, supposing there is such a group, and either form part of an embryological section, or come under a division accessory to the generative organs of birds. Its most natural position is illustrating the textural character and peculiarities of the enveloping masses of the young of vertebrates, and therefore subsidiary to development near the ovum." Again, "insects in amber are kept not as typical of the structure of the amber itself, but as exemplifying a peculiar nidus wherein an insect may become imbedded. I would incline, therefore, to place such an object among the groups of insects: here either close to the genus which the insect represented, or in a separate and subsidiary heading devoted to "*Insects imbedded in foreign substances*" at the end of *Insect structure*.

"Ought pearls to go along with *shells*, or as *examples of disease?* This again depends somewhat upon the nature of the collection. Strictly speaking, pearls are but a morbid condition of the nacreous material of shells, and hence are true examples of disease. They therefore, according to principle, should be ranged among *Pathological textures*. In a miscellaneous series, however, where pathology forms either an unimportant factor, or is not meant to be exemplified at all, then pearls necessarily come under the heading *Mollusca*, or *shell;* and a sub-series either together or after the representative genus to which the shell belongs.

Respecting cabinets, after discussing the merits of the various contrivances for storing objects, Dr. Murie advocates for large and increasing collections a system of small cabinets of similar dimensions, each devoted to a group or subsidiary division, and numbered and labelled accordingly, and so arranged that to all intents and purposes they represent but one vast cabinet. In support of this opinion, Dr. Murie calls attention to the great convenience of such a system as exemplified in the Botanical Department and Insect Room of the British Museum. He says, "such is my *beau ideal* of a microscopic cabinet, compound yet harmoniously single; adapted to meet the wants of a limited, a moderate, or a numerous series; expansion being in the ratio of increment of slides. But, furthermore, the same principle is applicable to very modest microscopical collection; such, indeed, as even the amateur, or those of limited means may aspire to. As a closing sentence to this clause, I may even make bold to say that, like other fashions and hobbies, that of cabinets is an infectious one; a piece of handsome furniture is attractive. Would that the zest for a thorough mastery of the contents was as powerful a stimulant. The whole paper, as well as the preceding one, is worthy the attention of those interested in the arrangement of microscopic or other natural history collections.

Fig. 1.



Mr. J.W. Stephenson, F.R.A.S., brings before the Royal Microscopical Society an improvement on his erecting binocular. The prisms are very much reduced in size, and are placed in a small brass tube, which is attached to the nozzle of the microscope in such a manner that it projects and enters into the interior of the object-glass mounting, and is thus brought very close to the back combination of the objective; this change of position, and also the reduction of the amount of glass traversed by the divided pencils, materially improves the definition of the instrument, which can now be used satisfactorily with a power of $\frac{1}{8}$th of an inch. The dimensions of the prisms are 0·68 of an inch in length, 0·412 of an inch in width, and 0·2 of an inch in thickness, and are inclined to each other at an angle of $4\frac{3}{4}°$; this makes the angle between the bodies $9\frac{1}{2}°$, and the imaginary point to which they converge nearly 15 inches. Provision is made for the employment of polarised light by substituting for the upper prism a plate of black glass so placed that the light is reflected from its surface at the polarising angle of $56\frac{3}{4}°$; a suitable alteration in the inclination of the bodies is made with this view without in any way affecting the use of the microscope for ordinary observations. This arrangement is found to give much better definition than a Nicol prism placed within the body.

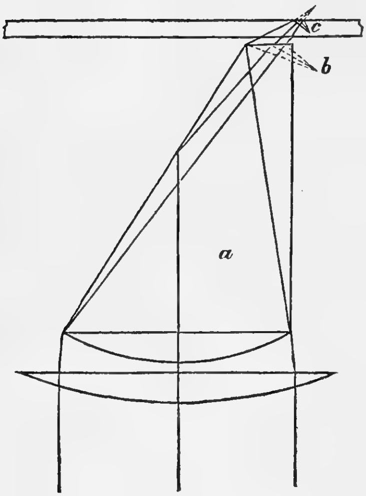
Captain Hutton gives, in the "Monthly Microscopical Journal" for May, an account of his examination of three kinds of vegetable fibres: those of *Phormium tenax*, "New Zealand Flax," and "Sizal" and "Manilla Hemp." He remarks, as most persons do who have studied the subject of vegetable fibres, that there is no difficulty in distinguishing these substances when in quantity; but that character is lost when only a small amount is available for examination. The following results of microscopical observations are given:—*Manilla.* Fibrous bundles oval, nearly opaque, and surrounded by a considerable quantity of dried up cellular tissue, composed of rectangular cells. The bundles are smooth; very few partly-detached ultimate fibres are seen, and no spiral tissue. *Sizal.* Fibrous bundles oval and surrounded by cellular tissue. Smooth and very few ultimate fibres projecting from the bundles. More translucent than Manilla, and can always be recognised by the large quantity of spiral fibres mixed up in the bundles. *New Zealand Flax* (*Phormium tenax*). In machine-dressed *Phormium* the bundles are translucent and irregularly covered with tissue. Spiral fibres can be detected among the bundles, but not in the same quantity that is seen in Sizal. Many more ultimate fibres stick out from the bundles, which are flat instead of oval. In those places where the bundles are entirely freed from tissue they are generally divided longitudinally into two or more smaller bundles or fasciculi, and in these places the number of half-detached ultimate fibres is greatly increased; these are, however, rarely broken, most of them having the end perfect. Spiral fibres are here absent. In Maori-prepared *Phormium* the bundles are almost entirely free from tissue, and quite so from spiral fibres. They are always broken up into many fasciculi, which average, perhaps, some twelve or fifteen ultimate fibres in each fasciculus. Many ultimate fibres are semi-detached, and they are much more broken than in machine-prepared fibre. For examination of the ultimate fibre Captain Hutton recommends boiling for two or three hours in a weak solution of potash, and separation of the fibres by dissection under the microscope in water. The ultimate fibres of Sizal will be found to separate easily, those of *Phormium* with more difficulty, while it will require great care to prevent breaking those of Manilla; this latter fibre will require a much longer boiling to render separation easy. According to Captain Hutton's table of measurements, the average length of the fibre of *Phormium* will be found to be nearly twice that of the others; while the average diameter is not more than half that of Manilla, which is again much less than that of Sizal. The cell wall of *Phormium* is also much thinner than that of either of the other two. These researches are a valuable contribution to a subject the histology of which is at present very little known. Good and characteristic distinctions among vegetable fibres are much to be desired, and the present paper is a step towards grappling with the great difficulties into which these researches are attended.

Dr. John Matthews about two years back produced a simple and ingenious self-centreing turn-table; this proved very useful in varnishing slides, which were originally truly centred; its convenience for this purpose, however, rendered it quite useless in the case of slides which were, as in the majority of instances, eccentric. This defect has been remedied by adding a second plate capable of allowing the slide to be thrown out of the centre to the extent required. This new turn-table combines all the advantages of the old construction with those peculiar to its original self-centreing form, and is also provided with a convenient hand-rest, which will be found a great assistance in forming cells with varnish, as it enables the brush to be held with very great steadiness.

Mr. F. H. Wenham has produced a $\frac{1}{8}$th objective capable of working, both as a dry and immersion lens, with the same front. Its performance under both conditions leaves nothing to be desired.

An account of "An Improved Reflex Illuminator for the Highest Powers of the Microscope" is communicated to the Royal Microscopical Society by Mr. F. H. Wenham.* It consists of a cylinder of glass (*a*) $\frac{1}{2}$ an inch long and $\frac{4}{10}$ths of an inch diameter, the lower surface of which is worked convex to a radius of $\frac{4}{10}$ths of an inch. Starting from the bottom edge, the cylinder is worked off to a polished face at an angle of 64°: close beneath the cylinder is set a plano-convex lens of $1\frac{1}{4}$ focus. Parallel rays sent through the lens, after leaving the lower convex surface of the cylinder, would be refracted to the point shown by the dotted lines if continued in solid glass, but by impinging on the inclined polished surface (which is far within the angle of total reflection) they are thrown on the flat segmental top; here they would be totally reflected and beaten down again to the point (*b*) outside the cylinder; but if an object slide (*c*) be laid over the flat top with an intervening film of water, the rays proceed on till the focal point reaches the upper surface, or is slightly beyond it; here total reflection now takes place; all the light is concentrated to a minute spot in the centre of the field of view of the microscope, and most of the rays are available for any object brought there by traversing the slides over the water top of the illuminator, which must be kept full without allowing any to run down the reflecting surface. This illuminator is fitted into the ordinary sub-stage, and has an independent rotatory movement of its own like that of Nachet's prism. The cylinder is to be brought up nearly level with the stage. The centre of rotation is set true by a dot on the fitting seen with a $1\frac{1}{2}$ objective. A drop of water is then placed on the top, upon which the slide is laid. The required objects on the slide are found with a low power, and may be distinguished by their brilliant appearance, while those on the cover are nearly invisible. The light is thrown up either by the plane or concave mirror. The former is generally the best and most controllable; the lamp should not be placed much beyond the stage, else its direct rays will get underneath and mar the blackness of the field. Having got the best effect by tilting the mirror, the illuminator is to be rotated, by which means various effects are brought out. The paper reviews the author's past contrivances for the same purpose, and anticipates the objections that may be raised against

FIG. 2.



* Monthly Micro. Journ., vol. vii., p. 237.

the new arrangement. It also contains some valuable remarks on total reflection and the transmitting power of various media, which are too long for quotation, and cannot be profitably given in abstract.

HEAT.

M. F. Tommasi has endeavoured to show the possibility of converting into dynamic work the expansion produced in liquids by heat; and the easy application of this motive force to the hydraulic press. He endeavours to show that by the expansion of oil instead of water, the pressure being caused to act directly upon the piston of the press, five-tenths of the quantity of heat employed in the ordinary steam engine produces the same degree of tension. The apparatus is very simple, consisting merely of a boiler in communication with a piston-box, the boiler being filled with colza or other oil, and heated by means of gas. It would appear that there is effected over other machines a saving of about 75 per cent.

The "Berichte der Deutschen Chemischen Gesellschaft zu Berlin," has an important paper on the specific heat of carbon. Dulong and Petit showed by experiments with twelve of the metals, that the product of the atomic weight and specific heat—in other words, the so-called atomic heat—had the same value, about 6·5, for all elements. Experimentalists have, from time to time, observed many striking departures from that law, and a comparison made by the author of the paper, H. F. Weber, of the numbers obtained by Regnault, De la Rive, Kopp, and Wüllner, as representing the specific heat of carbon, clearly shows that the different allotropic modifications of this element have very different specific heats, no one of which obeys Dulong and Petit's law; while the values assigned by these physicists to the specific heat of any one modification vary within wide limits. This is probably due to the fact that the specific heat of carbon in all its allotropic conditions varies with the temperature. Experiments with two large diamonds, conducted in the physical laboratory of Professor Helmholtz, in Berlin, have shown that the specific heat of carbon increases with the temperature to a degree surpassing any other substance, the specific heat of diamond being trebled by an increase of temperature from 0° to 200° C.

The following experiment has the advantages of certainty in its performance and of novelty. Take a wide glass tube open at both ends, and some little distance into the interior push a piece of fine wire gauze. If the gauze is now heated to redness over an ordinary Bunsen burner and then removed, it will shortly emit a shrill note for five to ten seconds.

The Real Evaporative Efficiency of Steam-Boilers Determined.—The American Institute of the State of New York have made public the result of an important series of trials of steam-boilers which was made at their last annual exhibition. The committee appointed to decide upon the merits of all steam apparatus and machinery entered at the "Fair," consisted of R. H. Thurston, Professor of Mechanical Engineering at the Stevens Institute of Technology, Chairman; and T. J. Sloan and Robert Weir, well-known engineers, Members. To secure an accurate determination of the amount of water passing out of the boilers, both evaporated and unevaporated, it was led into a large surface condenser. The weight of the water of condensation and of the condensing water was exactly measured, and the temperature of the water of condensation and the condensing water, of the steam in the boiler and of the entering feed and injection water, was obtained by delicate thermometers. The greatest care was taken to insure such accurate determination as should make the results of these trials valuable as *standards* with which to compare those of future similar trials. The log was kept by students selected from advanced classes of the Stevens Institute of Technology, and under the supervision of the chairman, himself a professional and experienced engineer. The committee thus obtained results that are extremely valuable to the engineer, as exhibiting the true efficiency of what they considered good boilers of quite dissimilar forms, and hardly less useful to the physicist, as giving the modulus of efficiency of large evaporating apparatus. The arrangement described enabled a determination of the quantity of heat transferred to the condenser from each boiler, during its twelve hour's trial, to be readily made. The weights of coal and wood were also recorded as is usual. To ascertain the weight of water actually evaporated and that carried over unevaporated, the committee use the formula, $Hx+h(W-x)=U$, where H is the number of thermal units transferred by each pound of steam, h the thermal units carried over to the water of condensation by a pound of water, x the weight of steam, and $W-x$ the weight of water, by which the total quantity of heat U is transferred from boiler to condenser. Introducing other values from the log, and solving for x, the results exhibited in the fifth column of our table are easily deduced. At the end of the elaborate report of the committee is a very complete table of results, from which we extract the following:—

Name.	Style.	Area of Heat Surface.	Square feet required to evaporate one cubic foot per hour.	Ratio H. S. to grate area.	Percentage of water primed.	Temperature of flues.	Evaporation per lb. combustible feed 212° F.	Modulus of Efficiency.
Root ..	"Sectional*"	876⅓	23·59	32·5	0·00	416·6° F.	10·64	0·709
Allen ..	"Sectional*"	920	17·41	28·5	0·00	345·8° F.	10·60	0·707
Phleger ..	"Sectional*"	600	22·74	26·1	3·26	503·7° F.	10·49	0·699
Lowe ..	"Tubular†"	913	21·63	24·2	6·90	389·6° F.	10·40	0·693
Blanchard	"Tubular‡"	440	33·48	51·8	3·00	221·6° F.	11·34	0·756

ELECTRICITY.

M. Bouman, of Holland, has effected a considerable improvement in the arrangement of the Léclanché cell, rendering the pile still more constant. In the flat-bottomed glass jar a plate of gas carbon and a rod of amalgamated zinc are placed vertically, a short distance apart. The intervening space and the surrounding parts of the vessel are then two-thirds filled with the usual mixture of coarsely-pulverised coke and black oxide of manganese. In order to prevent the zinc poles from coming into contact with the black mass, it is protected by a cover of woollen cloth. The porous cup is thus dispensed with. Léclanché made the connection with the carbon plate by means of a covering of sheet lead; but as this was liable to oxidation, complete contact was often destroyed. Bouman remedies this evil by cutting a slit in the gas carbon pole, and inserting a platinum wire, on the ends of which are clamps in connection with the outer circuit. Thus modified, the elements were found to run without interruption for a year sufficiently strong to drive an electric clock, and to sustain an alarm clock for two years. Mr. Higgs, by arranging a series of small-sized carbons and cylinders of zinc in alternate series in a similar manner to the series of the old iron-zinc deflagrator, has been enabled to dispense with additional cells, at the same time evolving the full electro-motive force. He has succeeded in introducing into one small vessel twelve couples of zinc and carbon (adopting M. Bouman's method of surrounding the zinc with felt), thus deriving the electro-motive force of about ten Daniell's cells from a very diminutive battery. Batteries exposing a larger surface are being made on the same principle.

An improved method of nickel plating has been devised by Mr. Keith, by which he obtains a flexible and tenacious deposit. The deposits at present obtained are so brittle that the articles will not suffer the least bending. The invention consists in adding to the various solutions of nickel, whether formed of single or double salts, materials which by their presence prevent the decomposition of the solution of the plating bath, and the deposition of oxide of nickel and

* Wrought-iron tubes. † With special provision for perfect combustion.
‡ Feed-water heater and mechanical draught.

other impurities upon the articles receiving the coating of nickel. There is added to the solution of nickel one or more salts, either single or double, acid or neutral, or associate, formed by the union of organic acids, acetic, citric, and tartaric, with the alkalies and alkaline earths, ammonia, soda, potash, magnesia, or alumina. These additions will, it is asserted, counteract the tendency to decomposition of the solution by action of the electric current. These various organic acid salts may be added interchangeably and collectively, though the inventor prefers to use, in case of the double salts of nickel and alkalies and alkaline earths, the organic acid salts which have for their bases the alkali or alkaline earth which is associated with the nickel in its double salt. Thus, when using a solution of nickel and ammonia, an organic acid salt of ammonia is preferred, though the similar salts of soda and potash will answer very well. In case of using a solution of a double salt of nickel and potash, or a double salt of nickel and soda, an organic acid salt of soda and potash is selected. Of the salts which can be used to accomplish the effect, the tartrates are preferable. A comparatively small quantity of the organic salts is necessary to be added, though more will not change the character of the deposit. The following bath is said to work well:—To 20 gallons of a solution in water of the double sulphate of nickel and ammonia of 7° Baumé, add 1 gallon of a solution, of an equal gravity, of neutral tartrate of ammonia in water. Mix well, and the bath will be ready after standing a few hours.

Two new electric detonators for mining purposes have been introduced at the Royal Arsenal, Woolwich, one for land, the other for marine service. The first consists of a tin tube filled with fulminating mercury, surmounted by a spherical head of beech-wood. Through this head, and embedded in gutta-percha to insulate them, run the electric wires, separated at their extremities. In this separation, and between the fulminating mercury and the wires, a small quantity of loose gun-cotton is placed. The passage of an electric current generates sufficient heat to ignite the gun-cotton. The second detonator is a modification of the old platinum wire fuse, and consists, like the other, of a tin tube filled with fulminating mercury and with a beech-wood head. To the ends of the insulated wires is attached about $\frac{3}{10}$ths of an inch of platinum wire 0·003 inch thick. Some loose gun-cotton is placed around the wire, and is ignited when the wire is raised to incandescence by the passage of the current. The ignition of the gun-cotton explodes the fulminating mercury, which in its turn explodes the powder.

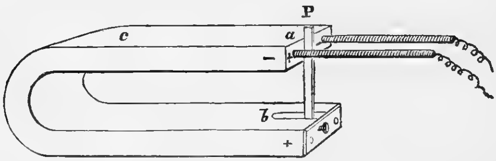
M. Ruhmkorff has invented a new or rather modified apparatus for the generation of ozone. The modes of preparation of this body have been:—1. By the decomposition of water by the pile; 2. By means of phosphorus; 3. By the action of concentrated sulphuric acid upon barytic oxide; 4. Finally, by passing a series of sparks from an electrical machine or from an induction coil through oxygen gas. The apparatus of M. Ruhmkorff is founded on this last method. It consists of a rectangular wooden case containing several horizontal ranges of metal plates, the alternate plates being connected, the one set to one terminal and the other set to another terminal, in connection with the poles of the battery or machine. These plates being thus arranged as a condenser serve to increase the intensity and number of the sparks. Tubulures connected with a reservoir of oxygen keep the case supplied, the ozone being drawn off by an aspirator. With this apparatus many new results have been obtained shortly to be brought to the notice of the scientific world.

The metal cadmium has been found by Dr. Schonn capable of being rendered, under certain circumstances, indifferent to the action of acids. It has long been known that iron, if plunged into nitric acid of a certain degree of concentration, acquires a peculiar surface condition, rendering it indifferent to the action of the strongest acid. Iron which has undergone this surface change has been termed passive. It appears, too, that such iron has acquired some peculiar physical qualities, since it will form a galvanic circuit with ordinary iron; the changed metal behaving electrically negative to the other.

That such iron has really been decidedly altered in character is evinced again by the fact that it refuses to reduce copper from solutions of its salts. It appears from Dr. Schonn's observation, that if cadmium is wrapped with some platinum wire, it may be placed, without being in the least acted upon, in strong nitric acid, though if the wire surrounding is removed, or if the acid is diluted, the cadmium is instantly attacked, thus showing that the passivity of the cadmium is due entirely to its contact with the platinum. The same author has shown that tin will give a similar phenomenon.

M. D'Arlincourt has invented a relay said to be very easily adjusted and effective in its working. At the negative extremity, *a*, of a powerful horse-shoe magnet, *c*, are fixed the two poles of an electro-magnet. A vertical bar of iron, P, centred at *b* in the positive pole of the horse-shoe permanent magnet,

Fig. 3.



and therefore permanently positively magnetised, vibrates between the two poles of the electro-magnet on the other extremity of the horse-shoe magnet. When a series of currents are passed through the wires of the electro-magnet, the bar P necessarily executes a corresponding series of vibratory movements, being displaced from the positive pole of the electro-magnet, to which it returns on an interruption or change in the direction of the current. In this relay it is said that the difficulty of residual magnetism arising from the coercive force of the iron of the electro-magnet, is removed, and that the degree of rapidity of the oscillations of the bar consequently depends upon the rate at which the current can be made and interrupted in the line. With the relay disposed as a translating apparatus at Paris, messages were transmitted from Marseilles to London, traversing the cable at Dieppe. The trial showed the relay capable of being worked at a high speed and with great precision.

M. Lenz has lately submitted a valuable paper on the properties of iron deposited by the electric current. He arrives at the following conclusions:—That iron and copper deposited by the galvanic current retain a gas, principally hydrogen. The volume of the gas absorbed by the iron varies between very wide limits, being sometimes as much as 185 times its own volume. The absorption of the gas takes place principally in the first layers of metal deposited. The absorbed gas may be disengaged by heating the iron to a temperature of 100° C. These researches are likely to throw great light upon the electrolytic deposition of metals, and the obtaining of a reguline deposit.

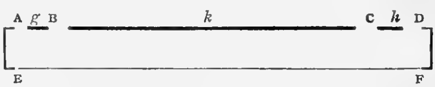
M. Volpicelli finds that the least flexure produced in a strip of metal gives rise to an electric current when the strip forms part of a conductive circuit. This was shown for the first time by Peltier, and the results of his experiments were confirmed by M. A. de la Rive. Peltier formed a large circle with a copper wire, which he put in communication with a galvanometer of small resistance, and found that by bending the copper wire he produced a current in the galvanometer that could not be attributed to the magnetic influence of the earth. M. Volpicelli has repeated these experiments with a reflecting galvanometer, and has arrived at numerous new results. He finds that the electric current due to flexure may be obtained not only with iron, but with all metals; but that copper, under the same circumstances, produces on an astatic needle a greater deflexion than any other metal. These currents present the remarkable fact of an entire transformation of force into electricity. The currents do not sensibly depend upon the heat produced by flexure. Flexures in contrary directions in the length of the wire produce currents in opposite directions. A diminution or augmentation in the rapidity of flexure produces a diminution or augmentation of the intensity of the electric current. A strip of different metals soldered together gives a current of less intensity than a strip of a single metal.

It is interesting and not uninstructive to occasionally take retrospective views of progress in science, and to note the previsions of the more striking inventions. Whether Shakespeare when he made Puck say, "I'll draw a girdle round the earth in forty minutes," had dreams of the then future electric telegraph, cannot be supposed; but we have more definite fore-shadowings in Strada's "Prolusiones Academicæ," 1617, proposing a dial with alphabet and magnetic needle. Addison alludes to this invention in the "Spectator," No. 244, 1712. Glanvill, in his "Vanity of Dogmatising," 1662, says:—"To confer at the distance of the Indies by sympathetic conveyance may be as useful to future times as to us in a literary correspondence." Evidently he is here thinking of the magnetic needle, for he continues afterwards to discourse of "conference at a distance by impregnated needles." Bailey's "Dictionary," 1730, article "Loadstone," says:—"Some authors write that, by the help of the magnet or loadstone, persons may communicate their minds to a friend at a great distance; as suppose one to be at London and the other at Paris, if each of them have a circular alphabet, like the dial-plate of a clock, and a needle touched with one magnet, then at the same time that the needle at London was moved, that at Paris would move in like manner, provided each party had secret notes for dividing words, and the observation was made at a set hour, either of the day or of the night." Sir Francis Ronalds, in 1816, constructed a tension telegraph at Hammersmith, the signals being read off pith balls. In 1816 also Andrew Crosse said:—"I prophesy that by means of the electric agency we shall be enabled to communicate our thoughts instantaneously with the uttermost parts of the earth." This remark, then a wild chimera, has been quickly achieved; not only can we communicate with the most remote continent, but we could, undoubtedly, had we the wires laid, telegraph a signal eight times round the world in one second.

In Pflüger's "Archive," Professor L. Hermann has described some experiments as to the conductibility of living muscle. He finds that living muscle offers very much greater assistance to an electric current passing across the fibres than along them, in the ratio of 7 : 1. Muscles in the condition of *rigor mortis* do not show this difference. The specific resistance of living muscle in the longitudinal direction, taking the length of mercury as unity, is 2,330,000, and in transverse direction 15,134,000. A similar difference occurs in the case of the nerves, the ratio being then 5 : 1. The specific resistance of nerve in the longitudinal direction is 2,554,000, and in the transverse direction 12,586,000, the resistance of mercury being again taken equal to 1. The longitudinal resistance of nerve is augmented by heating it to 50° C. (122° F.), the transverse resistance simultaneously diminishing. Professor Hermann thinks these differences of the resistance of the longitudinal and transverse sections of muscle and nerve due to the different polarisation of the sheath and nucleus of the fibres, and elucidates this by mathematical formulæ.

Professor J. T. Bottomley, M.A., F.C.S., describes the improved form of gravity battery, as devised by Sir William Thomson, and in use in the Physical Science Laboratory of Glasgow University, as follows:—The cell is of glass, a pan made by glass-blowers for containing milk. The diameter is 21 inches. A disc of thin sheet copper is laid on the bottom; and a thick copper wire covered with gutta-percha is soldered to the copper disc, and rises to the top of the cell as an electrode. In the upper part of the cell is a heavy mass of zinc, cast into the form of a circular gridiron, with three ears or projections, which rest on the edges of the glass. The gridiron shape is adopted in order to permit the hydrogen to escape. The distance between the zinc and copper plates is about 2½ inches. A large sheet of parchment paper covers the under surface of the zinc, and the corners and edges of the paper are brought up round the vertical sides of the zinc, so as to form a kind of bag round it. The parchment paper is thus a separator between the mass of liquid in the cell and that immediately surrounding the zinc. There is a circular hole in the middle of the zinc, and the tube of a glass or earthenware funnel passes through this and through a hole in the parchment paper, the edges of which are tied round the tube, down to the bottom of the cell. The cell is then filled up with a saturated solution of sulphate of zinc till the level of the liquid is higher than that of the top of the zinc; and on the top of this a layer of pure water 2 or 3 inches deep is poured carefully, so as to avoid mixing. The pure water forms an atmosphere into which the sulphate of zinc formed during the action of the battery may diffuse, and thus crystallisation is avoided. To set the cell in action crystals of sulphate of copper are put into the funnel. The dense solution flows down over the copper plate at the bottom, and in less than five minutes the cell is in full work. The average resistance of each cell is 0·19 of a B.H. unit.

Professor G. Carey Foster, F.R.S., has recently brought forward some valuable observations and improvements on the Wheatstone bridge. The fault of this instrument in its usual form is, that the resistance of the copper bands at the two angles is not taken into account. When large resistances have to be measured the importance of this omission is very small; but when resistances are to be measured smaller than the resistance of the apparatus, it is at once apparent that the mistakes incurred become serious. The essence of the fault lies in this, that the same amount will be added for resistance of the band whether the measurement be one or a thousand; and to make it quite flagrant, the nature of the fault is of the kind that would make $(1+a)$ a thousand times less than $(1000+a)$. Suppose a to be 1 only; then 2 would be made to answer for the thousandth part of 1001. For the purpose of making accurate measurements of very small resistances, such as those of short lengths of wires, it is essential that we should either have a standard of comparison, the value of which can be changed continuously, or, in other words, by indefinitely small amounts; or, that the method adopted for comparing the unknown resistance to be measured with an invariable standard, should be such as to enable us to vary continually, or by indefinitely small amounts, the indicated ratio between the unknown resistance and the standard. The former of these alternative means of measurement, namely a continuously adjustable standard, is afforded by the rheostat of Wheatstone and Jacobi; and the second, or a continuously adjustable method of comparison, is afforded by those forms of Wheatstone's "Differential Resistance Measurer" in which the ratio between the resistances to be compared is established by moving one of the galvanometer contacts backwards and forwards along a graduated wire, until a point is found which causes the current in the galvanometer to vanish. But neither the indications of the rheostat nor those of any of the adjustable forms of Wheatstone's bridge can be accepted immediately as trustworthy, where anything like minute accuracy is required. The direct indications of both instruments are liable to require corrections, the most important of which are those required on account of the resistance of connections not represented in the resistance of the scale, and those required on account of irregularities in the diameter or conducting power of the wire by which the adjustments are made. Professor Foster proposes to employ a graduated wire as an adjustable standard for the measurement of any resistance less than its own, in such a way as to eliminate entirely from the result the resistance of the connections by which it is united with the rest of the circuit. A German silver wire, E F, one metre in length,



graduated in 1000 millimetres, is connected at the ends with a copper band, which passes at right angles to each end of the wire, and runs parallel to it on the opposite side, where it is interrupted at A, B, C, and D, for the introduction of conductors of known and unknown resistances. In the ordinary use of the apparatus, the gaps A and D are closed, either with thick copper latches of insensible resistance, or by wires, the resistances of which are known in comparison with that of the standard wire, EF. The resistances to be compared are inserted at B and C. The battery wires are connected with binding screws at *g* and *h*, one terminal of the galvanometer being connected to the screw, *k*, the other galvanometer contact being in connection with a sliding contact, which is moved backwards and forwards on EF, until the position is found which causes no current to pass through the galvanometer. It is then clear that the scale reading corresponding to this position would in general indicate the true ratio of the resistances at B and C, only when the resistance of the copper band is an insensible fraction of these resistances as well as of the resistance of the wire, EF. But if the resistance to be measured is less than that of the whole metre wire, a measurement of it in terms of the latter, which is quite unaffected by the resistance of the connections, can be made by obtaining the balance by the sliding contact on EF; first, when the unknown resistance is inserted at D, and a connector of insensible resistance at A; and secondly, when the two have been interchanged, the gaps B and C being occupied by any convenient conductors which do not differ more in relative resistance than the wire to be measured, and the whole of the wire, EF. By inserting two resistances at A and D, and getting the balance upon EF, and then interchanging them, and getting the balance again, the difference of their resistances is obtained in terms of the resistance of the standard wire. For resistances inserted at A and D are equivalent to ungraduated prolongations of EF, and, therefore, when the balance has been obtained by adjusting the sliding contact, in order to maintain it after interchanging the conductor at A and D, the contact must be shifted a length of EF, the resistance of which is equal to the difference of the resistances at the two ends. When the resistance of one of the conductors A or D is negligible, that of the other is given by the shift of the sliding contact required to maintain the balance when this conductor is inserted; first at one of the end gaps and then at the other. Thus the wire, EF, becomes an adjustable standard of resistance, the indications of which are unaffected by the resistance of the end connections.

M. Böttger states that copper and brass may be coated with metallic zinc in the following way:—Finely divided zinc, in a non-metallic vessel, is covered with a concentrated solution of sal-ammoniac; this is heated to boiling, and the articles of copper or brass, properly cleansed, are introduced. A few minutes then suffice to produce a firm and brilliant coating. The requisite fineness of the zinc is produced by pouring the molten metal into a mortar, and triturating it until it solidifies.

Not an unimportant feature in electrical science is its application to military telegraphy. At a recent meeting of the Society of Telegraph Engineers, Capt. E. D. Malcolm, R.E., read an interesting paper, detailing the progress made in our military field telegraphs, as calculated to supply the communicating link between the outposts and the army. It appears that we have at present one troop of the Royal Engineer train devoted to this service, this troop being divided into three sections, each carrying twelve miles of wire in half-mile lengths. These pieces can be conveniently joined together by means of an ebonite jointer, which makes a practically water-tight joint in less than half a minute, and which, in the case of searching for faults, can be divided in a shorter time. The cable is a strand of seven No. 22 B.W.G. copper wires, insulated by Hooper's compound. It is 3 in. thick and weighs 300 lbs. per mile. It is rolled on wooden drums, placed on waggons drawn by six horses. The poles are of wrought-iron tubing in two lengths, the butt 10 feet long and $1\frac{1}{4}$ inches in diameter, the top 9 feet long and 1 inch in diameter, fitted inside the lower half and fixed by a bayonet catch. The wire is carried and insulated in a wooden plug fitting into the top of the pole. The earth plates are of wrought-iron, 18 inches long, $4\frac{1}{2}$ inches wide, and $\frac{1}{2}$ inch thick, and strong enough to be driven home in any soil; while they are accompanied by a six-gallon cask of water to insure moist earth. The office-van contains a pair of Morse recording instruments, fitted with Siemens's polarised relay, and with Digny's ink-roller. The battery is a form of Daniell's, arranged by Sergeant Mathison, R.E.

TECHNOLOGY.

According to Dr. J. M. Maisch, an excellent liquid glue about the consistency of molasses is made by dissolving glue in nitric ether; this fluid will only dissolve a certain amount of glue, consequently the solution cannot be made too thick. It is much more tenacious than that made with hot water. By adding a few pieces of caoutchouc, cut into scraps the size of buckshot, and allowing the solution to stand a few days, frequently stirring, it will resist dampness twice as well as glue made with water.

In a long interesting paper on the alloys now frequently employed for making imitation jewellery, known as Abyssinian gold, Talmi gold, &c., Dr. C. Winkler points out that the alloy is not galvanically gilt, but is plated; that is to say, a very thin sheet of gold is made to adhere to a yellow metal by rolling them together, and afterwards shaping, moulding, and chiselling by means of steel tools, the amount of gold varying from 1·03 to 0·03 per cent. As regards wear and tear, the author admits that, by careful plating, these articles really answer well.

The use of inks of a similar composition to that now generally employed appears to be of more ancient date than is generally supposed, for on examining a manuscript of the year 910, and belonging to the papers of the Cluny Abbey (Paris), Dr. Balard found that the ink it was written with was similar in composition to that now in use: it appears that MM. Coupier and Collin have succeeded in preparing an ink which is not acted upon by nitric and hydrochloric acids, chlorine, or bromine; it is not, however, quite proof against alkalies.

The very common use, especially in England, of soda for washing linen is very injurious to the tissues, and, moreover, has eventually the effect of yellowing it. Dr. Quesneville states that in Germany and Belgium the following mixture is now extensively used:—2 lbs. of soap are dissolved in 25 litres of water as hot as the hand can bear it; three large-sized tablespoonfuls of liquid ammonia and one spoonful of best oil of turpentine are then added. These fluids are incorporated rapidly by means of beating the soap-suds and other fluids with a small birch-broom. The linen, &c., is then put into the liquid and soaked for three hours, care being taken to cover the washing-tub with a closely-fitting wooden lid; by this means the linen is readily cleansed, requires very little rubbing, and there is also a saving of time and fuel. Ammonia affects neither linen nor woollen goods, and is largely used as washing-liquor in the North of England.

The following method is recommended by M. Mène for dyeing veneer wood. The wood is first steeped for twenty-four hours in a solution of caustic soda, and boiled with it for half an hour. It is then washed, to remove all the alkali, and having become as soft as leather and equally elastic, as well as capable of absorbing dye-stuffs, it is immersed for twenty-four hours, first in a decoction of logwood, and then, after having been superficially dried, into a boiling solution of sulphate of iron. When required to be yellow-dyed, it is immersed in a solution of picric acid to which ammonia is added. Wood, after steeping in soda, may also be dyed with coralline. The dyes are fast, stand varnishing, and thoroughly penetrate through the whole of the wood, which, after drying, may be sawn and veneered.

C. Daniel has described a method of painting with oil paints upon tin-foil stretched uniformly on sheets of plate glass until the painting and varnishing are finished. The tin-foil thus prepared is used instead of paper-hangings, and for decorative purposes; gilding can also be applied.

A rapid dryer for oil-paints and varnishes is formed by dissolving in 100 parts of water by the aid of heat, 12 parts of best shellac and 4 parts of borax; pouring the solution, after cooling, into bottles, which should be well corked. According to M. Mène, this solution is mixed with oil of turpentine, and added to oil-paints; the liquid may also be employed as a varnish.

A very good wood varnish for furniture and other wooden objects is made by adding to 1 kilo. of fluid copal varnish 15·62 grms. of best boiled linseed oil, and heating the mixture. The wood to be varnished is first coated with a solution of gelatine, to which either some precipitated chalk or some red ochre is added, according to the nature of the wood. When the coat of varnish is dry the article is rubbed with a solution of wax in ether.

It is well known that it requires some tact to bend a glass tube with an even curb and without collapsing its sides, and many chemists never do succeed in bending them skilfully. Prof. J. Lawrence Smith bends them satisfactorily by using the flame given by the Bunsen burner described in his article on alkali determination in silicates in vol. xxiii. of the "Chemical News." The extremity of the burner is flattened out so as to give a short and thin but broad flame, something like the flame of an ordinary gas-burner. The tube is placed in this flame and turned round and round, until a good heat is given to the tube; it is then withdrawn from the flame and can be bent with a perfect curve and without collapse of the sides of the tube. A tube of 1 centimetre and more can be thus bent very readily.

Sulphur is recommended by M. J. Ménard as a lubricating material. To 100 kilos. of good colza oil add 5 kilos. of sulphur, and heat this mixture to from 130° to 140° until all the sulphur is dissolved. It is said that, being a bad conductor of heat, sulphur prevents the heat caused by the friction of machinery being carried over upon the oil, which can therefore serve for a longer period than usual as a lubricating medium.

Glycerine has been found by experience to be useful as a means of increasing the elasticity and strength of leather. C. Mène states that hides which have been partly tanned by the usual process, may be greatly improved —especially if required for machine-belts—by being soaked for some time in glycerine.

CHEMICAL SCIENCE.

It will be remembered that in 1869 a Lectureship was founded by the Chemical Society in honour of the illustrious Faraday, to be held by some eminent foreign *savant*, who during the term of his tenure, was to deliver a discourse before the Society. The second "Faraday Lecture" was delivered on May 30th by Professor Cannizzaro, of Palermo, at the Lecture Theatre of the Royal Institution. The learned Professor's discourse was entitled "Considérations sur quelques Points de l'Enseignement Théorique de la Chemie." On Friday a dinner was given to the Professor, at which about 150 were present, including the Italian Ambassador and the Right Hon. the Chancellor of the Exchequer.

At the meeting of the chemical section of the German Association for the Advancement of Science, at Rostock, Professor Schulze read a paper on the direct oxidation of carbon by means of permanganate of potash in an alkaline solution, which excited lively debate, and was justly regarded as one of the most important chemical discoveries of the year. In addition to copious quantities of oxalic acid and of other products not yet determined, the author obtained an acid to which he has given the name of anthraconic, and which he found to closely resemble mellitic acid in its properties. The experiment was repeated with charcoal purified in a stream of chlorine gas, also by calcining cream of tartar, by the reduction of carbonic acid with phosphorus, and from graphite. All of these varieties of carbon yielded analogous results. So great was the interest manifested in the announcement, that the leading chemists adjourned to the Professor's laboratory, there to repeat the tests and to examine into the nature of the incidental products. They soon came to the conclusion that the new body was identical with mellitic acid. By treating the anthraconic acid with caustic soda, benzol was produced, which was converted into nitrobenzol in the usual manner, and from this product aniline was manufactured.

According to a decree in France, dated March 7, 1808, it has been enjoined that no wells shall be bored or dug out at a less distance than 100 metres in all directions from any burial-ground. Dr. Lefort having found that not only in many country villages, but also in several towns, this regulation has not been observed, has made some experiments on the water of a well at Saint Didier (Département de l'Allier), which locality is situated on an alluvial soil. This water is used for drinking purposes by the parish priest and a portion of the inhabitants, and, on examination, the author found it to contain not only a large proportion of ammoniacal salts, but also, on evaporation, to leave a very large quantity of a dark-coloured organic matter mixed with carbonated salts, which, on being mixed with some hydrochloric acid, gave off an offensive carbonic acid gas, the smell being somewhat akin to a mixture of a concentrated solution of glue and butyric acid. The well is very deep, and the water is quite clear and bright, but exhibits, especially in summer time, a very vapid taste, while, in the warm season of the year it rapidly becomes putrid. The author comes to the conclusion that, in any soil, a well dug at the distance of 100 metres from either burial-grounds or battle-fields is quite useless to protect the water of even deep wells from becoming so contaminated with organic and other injurious matter as to make them very dangerous to health.

A Detroit druggist, assisted by two gentlemen, resolved to make a number of experiments on spontaneous combustion. They first took a piece of cotton cloth, which had once formed part of a sheet, and which had been used until quite threadbare, and smeared it with boiled linseed oil. An old chest was placed in the loft of a store-room back of the drug store, a piece of zinc over it, and another piece under it, and then the chest was filled with paper and rags, and this particular piece of cloth placed in the centre. Although the room was not a light one, and the weather cold, in eight days there was such a smell of fire about the trunk, and the chances were so good for a conflagration within it, that the contents were emptied. An examination showed that the fibre of the oil-cloth had untwisted and shrivelled up, and that the rag looked as if it had been held too near a hot blaze. In April, when the rays of the sun were stronger, a pair of painter's overalls, literally covered with paint and oil, were rolled up, a handful of pine shavings placed inside, and these were crowded in next to the roof boards of the loft. The experiment was not a week old when, during one warm afternoon, a smell of smoke alarmed a workman in the next room, and he found the overalls burning, and so tinder-like was the cloth that it had to be crowded into a pail of water to prevent total destruction. During the hot weather of August, a handful of old cotton rags, in which two matches were placed, but which were not smeared with oil or other matter, were shut up in a tin box, and hung up in the loft, a rear window allowing the afternoon sun to shine directly on the box for several hours. Toward the close of the fourth day the druggist took down the box to see how the experiment was progressing, and found the contents to consist of nothing but a puff of black cinders, which flew all over him as the lid was lifted. Having a vacant corner in his brick wood-house at home, the druggist took the trunk up there, where there was no danger of burning a building. He filled the trunk with the contents of the paper rag-bag, and then smeared one with benzine and threw it in last of all. The trunk was shut tight, everything cleared away from its vicinity, and he commenced watching. One day the family came home to find a few ashes marking the place where the trunk stood, while the bricks above and around were badly stained with smoke.

At the last meeting of the Chemico-Agricultural Society at Belfast, under the presidency of Dr. Knox, late Poor Law Inspector, the subject of whisky adulteration was brought under consideration by Dr. Hodges, who exhibited a specimen of that liquid brought to him by two men who had been physically incapacitated by drinking a small portion of it in a public-house. He found, on analysis, that it contained a large amount of naphtha. He had also discovered that ingredients of even a more deleterious character were used in the process of adulteration,—mixtures containing sulphate of copper (blue-stone), cayenne pepper, sulphuric acid (vitriol), and a little spirits of wine. One specimen submitted to Dr. Hodges by a number of provision cutters and curers, was composed of naphtha and a slight colouring of whisky. The men who had imbibed a small quantity of it were affected with serious symptoms; and this, said Dr. Hodges, was a fair specimen of the drink sold in low-class public-houses. The trade in this noxious compound is carried on with impunity, no local authority in Belfast or in the Province of Ulster caring to exercise the powers with which the legislature has invested them for the suppression of the traffic.

Mr. J. T. King recommends the following process for detecting sulphuric acid in vinegar. It will detect the 500th part of free sulphuric acid, and is accurate for all practical purposes. An ounce of the vinegar to be examined is, by evaporation upon a water-bath, reduced to about half a drachm, or the consistency of a thin extract; when quite cold half a fluid ounce of strong alcohol is thoroughly incorporated; the free sulphuric acid will be taken up by the alcohol, to the exclusion of any sulphates; the alcoholic liquid solution should stand for several hours, and then be filtered; add to the filtrate 1 fluid ounce of pure distilled water, and evaporate off the alcohol by the application of a gentle heat; the remaining liquid is again left standing for several hours, and again filtered; to the filtrate, previously acidulated with a few drops of pure hydrochloric acid, a solution of chloride of barium is added, which, if sulphuric acid be present, will yield a white precipitate.

Dr. Personne accidentally obtained a small piece of a chocolate-brown substance, which originally was apparently a paste, but is now hard. On further examination it was found to consist of a lime-soap, mixed with myrrh, olibanum, benzoin, and probably some essential oil. The author states that at the present day there is sold in Egypt as a perfume a substance of similar composition, and locally known as *Bouh-Kourre-bare*, which means perfume from the Arabian frontier.

M. A. Muntz has instituted, with great care and on a practically large scale, a series of experiments, with the view of ascertaining the quantity of materials consumed by the hop-plant during its growth and development from spring to autumn (hop-picking time), for a number of 6316 plants placed on a hectare (= 2·47 acres). The quantities alluded to, expressed in kilos., are—Water, 11270·270 kilos.; carbon, 2624·361; hydrogen, 315·547; oxygen, 2011·393; nitrogen, 91·141; phosphoric acid, 22·699; magnesia, 24·352; potassa, 41·812; soda, 0·455; non-specifically determined mineral matters, 133·278.

According to G. Salet, when a crystal of iodine is put into a hard glass tube, the tube sealed, and then strongly heated to redness at some distance from the iodine, the latter substance will be seen, after the heating of the tube has been discontinued, to become volatilised, and to exhibit when entering the still hot portion of the tube a brilliant red light. This experiment may be also made by means of a glass tube provided with a spirally-wound platinum wire, which is made red-hot by means of an electric current.

A new product of decomposition of commercial aniline has been found by R. Braun and Ph. Grieff. When a large quantity of commercial aniline is distilled along with lime, the last portion of the distillate is not quite freely soluble in hydrochloric acid; further investigation led to the conclusion that the properties of that substance agree with those of carbazol, as described by Graebe and Glaser. Carbazol is present in crude tar oil, but the authors surmise that this substance might be formed by the drying of the mixture of aniline and lime in contact with the very hot metal of the iron still.

Based upon experiments very successfully made with animals, Drs. Eulenberg and Wohl propose the use of animal charcoal, made up into pills as an efficient antidote against the bad effects of phosphorus in the lucifer match manufactories; they prefer this substance to oil of turpentine, which, though an effectual antidote against phosphorus, causes, in many instances, severe headaches when internally taken.

The magnetic sand, which exists in immense quantities on all sides of Mount Etna, has a sp. gr. of 2·813, and is scarcely acted on by acids. The results of its analysis by J. B. Hannay gave—Silica, 52·71; magnetic oxide of iron, 19·44; alumina, 19·09; lime, 6·61; and magnesia, 1·85.

THE PROGRESS IN SCIENCE AT THE INTERNATIONAL EXHIBITION OF 1872.

ENTERING the South Gallery we are in the department apportioned to recent scientific inventions and new discoveries. This year the department presents no striking feature, so that we must proceed to notice the exhibits as they occur to us. At the entrance of the South Gallery stands the Electric Motor Clock of Messrs. Cooke and Son, of York; this clock, actuated by a not very powerful battery, controls and drives the many clocks required in the Exhibition buildings. The instrument has now stood a very severe test, this being the second year of its working successfully. The construction is exceedingly simple. There are several excellent electric clocks exhibited by Messrs. Moseley and Co., in the French *annexé*, of which we are sorry no description has been given. Continuing, however, our walk through the South Gallery, we pass a bowser, for tightening and holding ropes, exhibited by A. Paget and Co., several hoists and pulley-blocks, by Tangye Brothers and Holman, and by Head, Wrightson, and Co., and arrive before a model of a house, constructed entirely of waterproof paper, and over which a plentiful rain of water is continually flowing. The paper is rendered waterproof by the cupro-ammonium process, now the property of the Waterproof Paper and Corrugated Fibre Company, and can be seen in the roll, single or double. There are also combinations of cotton or linen fabrics with waterproof paper, in slabs, flat or corrugated, of various thicknesses, for roofing or building purposes, waterproof tubing, panels, &c. Cupro-ammonium is stated to be prepared by the immersion of copper scraps in concentrated liquid ammonia. The fluid becomes deep blue in colour, and possesses remarkable solvent and agglutinising properties with regard to paper, linen, silk, and bone. Fibrous material treated with this fluid is rendered waterpoof. It is stated that oxide of copper precipitated and re-dissolved by ammonia does not possess the solvent power of the solution obtained by the immersion of copper-turnings in ammonia. Any paper sent to the company is made waterproof and corrugated at an expenditure of 40 per cent on the original cost. Near to this exhibit are two non-conducting cements for covering boilers and steam-pipes by Messrs. Fox, Head, and Co., and by Messrs. Leroy and Co. Mr. Barlow, F.R.S., exhibits a very clever instrument for calculating the cubical contents of earthwork. Messrs. T. A. Skelton show a Catoptric Street Lamp now in use on Waterloo Bridge, devised to prevent the waste of light from the higher parts of the lamp. The invention consists in arranging strips of silvered glass in such a way that the maximum amount of light is thrown upon the pathway. Messrs. Lynch and Co. exhibit a knobbed blue-glass poison-bottle to prevent the use of poison by mistake in the dark. Captain E. Sawyer contributes three drawings of a pivot-revolving gun-carriage on rails. This year there are exhibited many very useful little inventions, all of which it is impossible that we can enumerate. Among the most prominent is a contrivance for the prevention of guttering in candles, the contribution of the Rev. J. Langton Clarke, a small glass socket fitting on the candle. This socket is studded with small points of coloured glass, forming a very pretty as well as useful ornament. Another of these minor inventions, and one that will be appreciated by artists, is a reservoir palette for flat tints, preventing evaporation, by Messrs. Ackermann. Returning to what must really be the true object of an exhibition of this nature, the development of manufactures, we have a model of a patent Puddling Furnace, by Mr. Danks; a model of a kiln for burning bricks, by Mr. Batchelar; and a cutting and crushing sugar machine from Belgium, contributed by Léon Goffin. Captain W. H. Noble, R.A., contributes some specimens of gunpowder (R.L.G., L.G., pebble, pellet, and prismatic) recently tried by the Committee on Explosives; also a crusher-gauge for directly determining the pressure of fired gunpowder. Messrs. Bessemer have a very complete series of sectional models illustrating the Bessemer principle of constructing Continuous Low Pressure Ordnance. From Sir Joseph Whitworth and Co. there are excellent models of a Six-pounder Breech-loading Rifled Gun and Carriage, made of Sir Joseph Whitworth's patent compressed metal; and models of a breech-loading field-gun and a breech-loading naval gun and carriage. Major W. Palliser, C.B., shows a Service Muzzle-loading 64-pounder Rifled Gun, converted on the Palliser principle from a smooth bore 32-pounder gun, and having fired 2300 rounds. These are in the West Arcade. Returning to the South Gallery we have, completing the list of furnaces and heating apparatus, the Newport Patent Puddling Furnace and Boiler invented by Jones, Howson, Gjers, and J. Head; also a sectional model of Whitwell's Patent Hot-Blast Fire-brick Stove, erected at the moderate cost of £850 to £1000. Near at hand is a model of an admirable Boat Lowering Apparatus invented and exhibited by Mr. E. J. Hill. This apparatus appears to be extremely simple; by a peculiar form of eye it is contrived that should either end of the boat alone touch the water, the eyes are not liberated; should the boat touch the surface of the water in the complete plane, the eyes are immediately disengaged and the boat set free. The direct application of recently discovered scientific principles meets with but slight representation in the present Exhibition. Dr. Siemens, F.R.S., exhibits his Deep Sea Photometer, an apparatus for comparing the intensities of light at different depths of water. This photometer was used on board H.M.S. *Shearwater*, in August, 1871, in the Straits of Gibraltar. Dr. Siemens also contributes an Apparatus for Working Pneumatic Despatch Tubes on the continuous current system, as established for the transmission of postal telegrams between the West Strand and Central Telegraph Stations, together with an exhauster or steam-jet for drawing the air through the tubes. Colonel H. Stuart Wortley exhibits some results of his New Dry Photographic Process, for working, by the use of collodio-bromide of silver, without a silver bath. Passing from the gallery containing the preceding exhibits, the visitor interested in the useful application of science to every day purposes, will pause before the table at which an attendant of Messrs. D. Nicoll and Co. explains the use of a Fire-proof Starch. This starch renders fabrics uninflammable without affecting their colour, strength, or appearance, and requires only ordinary treatment by the laundress; it can be obtained in small quantities at a moderate price.

Science, as represented in its own division, may be said to have now been exhausted by our imaginary visitor; but the ground upon which Science and Art marshal their combined forces is where the several kinds of musical instruments are exhibited. The instrument of general interest, the pianoforte, appears either in the gorgeous case-work of Messrs. Hopkinson and of Messrs. Erard, whose instruments are marvels of marqueterie work, or in the full-toned bijou pianofortes of the former firm, certainly the most powerful of the middle-class instruments, and in this respect not inferior to some of the grands exhibited. The Iron-Bijou-Grand Pianoforte, as well as the Full Concert-Grand contributed by this firm, are worthy of notice in a scientific point of view, from the substitution of iron for wood in those portions of the instrument subject to strain. When we consider that the strings of a seven-octave pianoforte tuned to concert-pitch exert a tension equal to nearly a half-score of tons, the employment of some material occupying less space and of more regular structure than wood is evidently desirable.

The applications of electricity are yearly becoming more numerous, and since its first employment in organ construction by Mr. Barker, of Paris, it has received considerable attention from Messrs. Bryceson Brothers and Co., who built their first electric organ for Her Majesty's Opera in 1868. The organ exhibited in the present Exhibition consists of one manual and eight stops. A voltaic battery, electro-magnets, and attenuated air as a motive power, are the chief agents employed. Each key is connected to a separate electro-magnet in the organ, the armature of which is attached to the lever valve of a pneumatic pallet of peculiar construction. On pressing a key an electric current is completed around its corresponding magnet, and the armature being attracted causes the pallet to open and admit wind to the pipes; immediately the key is released the current ceases, the armature returns, and the pallet is closed. Powerful west gallery organs may thus be played from the east end of the church entirely through a cable of insulated wire an inch in diameter, or attached to additional manuals provided for that purpose at a mechanical organ placed close to the choir. The electric current is supplied from four cells of a single fluid battery, the electrodes of which are automatically withdrawn from the exciting fluid when the wind is out of the organ.

The processes of envelope manufacture are among the most interesting exhibits, chiefly by Messrs. Goodall and Son, and by Messrs. John Dickinson and Co. The process commences at the northern end of the gallery, where the paper, as it arrives in endless rolls from the mill, is fixed to the cutting-machine to be cut into sheets of the required size. The paper has next to be "glazed." This is done by interleaving it sheet by sheet with plates of zinc or brass, and passing it in small quantities between rolls under enormous pressure varying from 20 to 40 tons. Out of this paper the "blanks" of the required size are punched, and then have to be gummed on the "nose," that portion which has to be wetted when fastening the finished envelope, 4000 envelopes being gummed in one hour. The blanks have now to be folded, an operation effected by means of machinery. 300,000 envelopes can be completed weekly.

About three o'clock daily the centre of interest is the Marinoni machine, printing the "Echo" newspaper at 12,000 copies per hour. The printing-press is the consummation of scientific principle, and we regret not being able to afford space for the description of the many admirable inventions. We must, however, record the endeavours of Mr. Walter, M.P., to attain the best form of "perfecting" machine, or one that prints both sides of the sheet at a single operation, in order to dispense with the manual skill required in other machines to lay on sheet after sheet with the requisite accuracy. The Walter press requires but two attendants, and the paper passes continuously through the machine. At one end of the machine is placed a reel of tightly-rolled paper, in the form in which it leaves the paper-mill, nearly four miles in length. The paper is led from the reel into a series of damping cylinders, and thence between the first and second of four cylinders raised perpendicularly above each other. The top cylinder is encircled by the stereotype casts of four pages of type, whilst round the lowest of the four cylinders are the stereotypes of the remaining four pages of the newspaper. The paper is thus printed first on one side and then on the other. Passing to other cylinders, the continuous sheet is cut into lengths, each forming a complete newspaper. 12,000 copies of the complete paper can be printed in one hour. It should be said that it is to Mr. Walter that we owe much of the progress in printing.

We understand upon authority that no "Official Report upon Recent Scientific Inventions and New Discoveries" will be published this year.

QUARTERLY LIST OF PUBLICATIONS RECEIVED FOR REVIEW.

Elements of Chemistry: Theoretical and Practical. By William Allen Miller, M.D., D.C.L., LL.D. Revised by Herbert McLeod, F.C.S. Part I.—Chemical Physics. Fifth Edition. *Longmans and Co.*

Introduction to the Study of Biology. By H. Alleyne Nicholson, M.D., &c. *William Blackwood and Sons.*

On Mankind: their Origin and Destiny. By an M.A. of Balliol College, Oxford. *Longmans and Co.*

Natural Philosophy for General Readers and Young Persons. Translated and Edited from Ganot's "Cours Elémentaire de Physique." By E. Atkinson, Ph.D., F.C.S. *Longmans and Co.*

Essays on Astronomy. By Richard A. Proctor, B.A., Cambridge. *Longmans and Co.*

Geological Survey of Ohio. Report of Progress in 1870. By J. S. Newbury. *Columbus: Nevins and Myers.*

The Earth's Crust. A Handy Outline of Geology. By David Page, LL.D., F.G.S. Sixth Edition. *William Blackwood and Sons.*

A Manual of Chemical Physiology, including its Points of Contact with Pathology. By J. L. W. Thudichum, M.D. *Longmans and Co.*

Man in the Past, Present, and Future. From the German of Dr. L. Büchner. By W. S. Dallas, F.L.S. *London: Asher and Co.*

Conversations on Natural Philosophy. By Mrs. Marcet. Revised and Edited by Francis Marcet, F.R.S. Fourteenth Edition. *Longmans and Co.*

An Exposition of Fallacies in the Hypothesis of Mr. Darwin. By C. R. Bree, M.D., F.Z.S. *Longmans and Co.*

Contributions to Molecular Physics in the Domain of Radiant Heat. By John Tyndall, LL.D., F.R.S. *Longmans and Co.*

New Formulæ of the Loads and Deflections of Solid Beams and Girders. By William Donaldson, M.A., A.I.C.E. *E. and F. N. Spon.*

Patterns for Turning; comprising Elliptical and other Figures cut on the Lathe without the Use of any Ornamental Chuck. By H. W. Elphinstone. *John Murray.*

PAMPHLETS AND PERIODICALS.

Report of the Miners' Association of Cornwall and Devonshire. *Falmouth: W. Tregaskis.*

Lectures Delivered in the Lecture-Room of the Industrial and Technological Museum, Melbourne. *Melbourne: S. Mullen.*

On Experimental Stations. By E. Packard, jun., F.C.S. Read before the Framlingham Farmers' Club.

On Pressure Logs for Measuring the Speed of Ships. By James R. Napier, F.R.S.

On the Method of Creation of Organic Types. By Edward D. Cope, A.M.

Catalogue of the Pythonomorphia found in the Cretaceous Strata of Kansas. By E. D. Cope, A.M.

Sur une Combinaision de Bioxyde de Chrome et de Dichromate Potassique dichromate Kalichromeque. Par M. D. Tommasi.

Sur un Nouveau Dissolvant de L'Iodure Plombique et de son Application a la Pharmacie. Par D. Tommasi.
Hypotheses. By F. J. Finois.
The New Patent relating to the Sewage of Towns. By J. Brough Pow, Esq.
Naval Science.
The Popular Science Review.
The Geological Magazine.
The American Chemist.
The Westminster Review.
Macmillan's Magazine.
Blackwood's Magazine.
The Civil Service Gazette.
Revue Bibliographique Universelle.

PROCEEDINGS OF LEARNED SOCIETIES, &c.

Monthly Notices of the Royal Astronomical Society.
The Journal of the Royal Historical and Archæological Association of Ireland. No. 8. *Dublin: McGlashan and Gill.*
Proceedings of the Bristol Naturalists' Society.
Proceedings of the Newcastle Chemical Society.
Monthly Microscopical Journal. *Robert Hardwick.*
Proceedings of the Royal Society.
Ofversight af Kongl. Vetenskaps-Akademiens Förhandlingar. *Stockholm: Norstedt and Söner.*

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THE QUARTERLY

JOURNAL OF SCIENCE.

OCTOBER, 1872.

I. THE ORIGIN OF THE GREAT CYCLONES.

By Professor THOMPSON B. MAURY,
United States Naval Observatory.

IN his sketch of the early triumphs of science in England, Macaulay tells us, "One after another, phantoms which had haunted the world through ages of darkness fled before the light." It was no wonder that men, taught by Bacon to investigate instead of to imagine, should have learned more in a day than all their fathers for a thousand years before them. It was not strange that unscientific prelates and courtiers and literary gentlemen should have made brilliant discoveries in chemistry and physics, where, for centuries, professional alchemists and schoolmen had floundered in their own muddled dreams. But it seems passing strange that the phenomenon of storms and tempests should have escaped the eye of true science till quite recently, and remained so long one of those "phantoms" of which the historian spoke. The time has certainly arrived for a vigorous invasion of the vast and splendid, but yet unsubdued, domain of meteorology. The key to success in this aggressive movement, I conceive, lies in the determination of the cause of cyclones and the generation of the storms which perpetually travel over their ordained and fire-sprinkled paths. As Professor Buys Ballot, the eminent chief of the Royal Dutch Meteorological Institute, in announcing the scheme of inquiry for the General Congress of Meteorologists to be held this year, has so well expressed it, "*the origin of depression systems is the great problem to be solved.*" This solution I have attempted here. I am not unconscious of its importance and difficulty. But no scientific inquiry conducted, as I shall strictly conduct this, upon the exclusive principle of a sufficient induction of observed facts, can be justly regarded as presumptuous. It is the glory of such a method of inquiry that while it risks nothing it has the best opportunities for solid and splendid acquisitions, and may not inaptly be compared, as some one has suggested, to

Wellington's strategy in forming the famous lines of Torres Vedras.

The only theory for the generation of cyclonic storms which has engaged more than an ephemeral existence is that of M. Dové, the distinguished meteorologist of Germany. Taking the West Indian cyclone as the type of all others, beginning in the intra-tropical regions and extending into high latitudes, M. Dové is the well-known advocate of the doctrine that these storms "Owe their origin to the intrusion of the upper counter trade-wind into the lower trade-wind current."* Without now entering into a discussion of this hypothesis, it will be proper for me to mention a different one advanced some time ago by myself, as a substitute for the older one of Dové, and as affording a satisfactory solution of all tropical storms and cyclones. This hypothesis, which will here be fully stated, was also based on the agency of the trade-winds, but in a manner totally distinct from that assumed by the German meteorologist; and in the original statement of my own views it is asserted that "the origin of cyclones is found in the tendency of the South-East trade-winds to invade the territory of the North-East trades, by sweeping over the Equator into our hemisphere, the lateral conflict of currents giving an initial impulse to bodies of air, by which they begin to rotate." This explanation will take us aside for a while to notice the law of storms and the region of the trade-winds, upon which, according to the two hypotheses, the tropical storm has its birth.

It can hardly be necessary in stating the law of storms, as first announced and established by Mr. Redfield, to do more than call attention to its fundamental condition. In Europe, meteorology has made rapid strides. Since the institution of the Weather Bureau in the United States, the eye of every American has become familiar with the daily isobaric and other isometeoric lines on the weather maps of the War Department, displaying with the fidelity of an automatic instrument a tri-daily photograph of the never quiet aërial ocean. He has learned to watch its mighty pulsations. He traces from day to day the exactly delineated area of every passing storm, and follows its cloud-led march from shore to shore of the Continent. Nothing of importance in the meteoric world in his own country can escape him, for he is in telegraphic communication with every part and corner of its vast territory.

* Dove's Law of Storms, p. 264.

While philosophers and meteorologists, in many countries less favoured with a system of weather telegrams, have been discussing and disputing the reality of the law of storms, he sees it daily verified and is himself a storm warner.

That "law," first elaborated by Mr. Redfield and Col. Reid, simply stated, is this:—"All gales or hurricanes are great whirlwinds, in which the winds blow in circuits around an axis vertical or inclined; that the winds do not blow in horizontal circles, but in sinuous spirals towards the axis, a descending spiral movement externally, and an ascending one internally, at the cyclonal centre; that the direction of apparent revolution is always uniform, being against the motion of the sun or against the hands of a watch turned face upwards, or from right to left in the northern hemisphere, and just the contrary in the southern hemisphere. The moment a barometric fall occurs, or, in any way, a sinking or depression of the atmospheric sea takes place, that moment, from all sides, the air begins to press inwards in radial lines upon the vortex. If the earth was not revolving on its axis, and the surface was smooth and polished so as to offer no friction, the air currents would flow in the direction of the spokes of a well-made wheel; on reaching the centre they would ascend as in a hollow and somewhat erect cylinder of air until they had restored the equilibrium of the agitated mass. The earth's diurnal motion diverts them, however, to the right. Still they go forward to the centre of agitation, and, on reaching it, ascend. In ascending they grow cooler and cooler; their interstices grow smaller and smaller; their moisture is wrung from them at the altitude of a few thousand feet; and the evolution of the latent heat stored away in the vesicles of their aqueous vapour now begins, and the tempest begins to rage in earnest. This process goes on as long as the storm cylinder or centre is fed with the vapour of water, and, as we shall presently see, it ceases when that supply fails, as quickly and necessarily as the wheel or screw of the steamship ceases to revolve when her engineer cuts off the steam from her cylinders.

Thus we behold the cyclonic meteor set in motion internally from the circumference to the centre, simply by the formation of an atmospheric depression, and we see that internal motion—the air blowing in sinuous spirals upon the centre—prolonged, sustained, or intensified by the liberation of the latent caloric of aqueous vapour condensed into rain from the ascending air-current.

From this philosophy of the cyclone—which, as far as it relates to the agency of aqueous vapour, is entirely inductive—it would be inferred that great cyclones, hurricanes, and typhoons ought to occur in regions of great evaporation, where the intro-moving winds would take up large quantities of moisture for the central area of the storm, and keep its furnace well supplied with appropriate fuel. I need hardly say that such an influence is sustained by actual observation all over the globe. The tropical hurricanes of the West Indies are violent indeed. But they do not attain the violence of the East Indian typhoons; in the former the barometer does not fall so low, and for a very obvious reason. The water of the equatorial current in the Atlantic, which leaves the Antilles, has been but a short time beneath the tropical sun in its passage from the coast of Africa. The waters of the Bay of Bengal, the China Seas, the South Indian Ocean, have been more than three hundred days under a vertical sun, and are all aglow with its heat, and the evaporation from their superheated billows exceeds that of all other waters on the globe. The moment a storm centre is formed in the Indian Ocean, and the intro-moving air currents reach it, the quicksilver suddenly drops in the barometric tube, and before the mariner can reef his sails, they are often torn into a thousand ribbons, and his vessel thrown on her beam ends.

It is a remarkable fact that the hurricane season in the West Indies closes with *heavy rains* in the early or middle part of November, when the sun has been nearly two months below the line, when solar evaporation in the Caribbean Sea has been partially arrested, and the reservoirs of the air exhausted of their vapour or storm-fuel by the drenching rains of which we speak.

From what has been said of the immense amount of condensation due to an ascensional movement of air in the storm centre, it must not be supposed that the chilling arises solely from the elevation of the air to colder regions of the atmosphere. As the air ascends into the upper regions the pressure of superincumbent atmosphere is taken off from it on all sides, and it expands and loses heat, the force required for the expansion being withdrawn from the store of heat. The glass receiver of an air-pump full of humid air, when exhausted partially, shows a frost of vapour and clouds within, proving that the process of rarefaction has chilled the contents of the receiver. Air was compressed by Professor Tyndall, by means of a column of water 260 feet high, to an eighth of its original volume, and then allowed to escape by a stop-cock. As it rushed out it expanded so violently and caused such an intense cold that the moisture in the room was congealed "in a shower of snow, while the pipe from which the air issued became bearded with icicles."

We are now prepared to understand the assumed agency of the trade-winds in the production of cyclones.

The South-East trade-wind is in many respects like its Northern counterpart, but decidedly fresher and stronger. In the vicinity of the famous island of St. Helena, and from that rock to the island of Ascension, it is perhaps the most constant wind on the globe. Here its direction is very nearly fixed due S.E., and from the last-named latitude it blows steadily and strongly right up to the Equator, and crosses the line in the winter of the Southern hemisphere with a marked velocity.

The South-East trades occupy a much larger area of the earth's surface, and sweep a wider zone than do the North-East trades. The celebrated navigator, La Perouse, writing in the month of September, 1785, while on his way to the South Atlantic, says:—"We crossed the Equator on the 29th of September in $15\frac{1}{2}^\circ$ west longitude. Those sailors are quite in error who are afraid of finding calms near the line at this season;" and the reason for this observation is that the belt usually known as the equatorial calm belt is actually and constantly penetrated in July, August, and September by the South-East trades, which have come up from below the Equator. Coleridge, availing himself of the popular notions of his day, delineated this region of calms as a terrible one for the mariner, in which his bark was calm-bound under a hot and copper sky—

"Day after day, day after day,
We stuck; nor breath, nor motion;
As idle as a painted ship
Upon a painted ocean."

But, unless we except certain seasons of the year, this once-dreaded region is not more tranquil than certain other spots and districts of the Atlantic to which the name of Horse Latitudes has in our time been given, but which was called by the old Spanish sailors *El golfo de las yeguas,* "the mare's sea," to denote its roughness. Certain it is the South-East trade does cross the Equator in summer, and make itself felt, at least as far as the parallel of 10° N. Mr. J. K. Laughton tells us "The S.E. trade in the hurricane months blows across the line far to the northward." Dampier, Horsburgh, Dové, Capt. M. F. Maury, and I believe all writers without an exception, have fixed 10° or 12° N. as the limit reached by the S.E. trade in summer—a fact which is in remarkable contrast with the winter southern limit of the N.E. trade, which, as we have seen, does not attain a lower latitude than 3° N., or, as many observations give, 5° N.

Besides this fact, we have in the S.E. trade a powerful and not a gentle wind. If we compare the average velocity of the South-East trades with that of the North-East, we shall find, according to the researches of the U.S. National Observatory, that "velocity of the North-East trades is from 14 to 18 miles per hour, that of the South-East from 25 to 30 miles an hour." Add to this the yet more significant fact, from the same authority, that "The force of the trade-winds (S.E.) as determined by the average speed of 2235 vessels sailing through them, is greater between 5° and 10° south than it is between 25° and 30° south," and we have what amounts well nigh to a demonstration, that the South-East trades do violently overleap the Equator. Experiments also show that while the average force of the freshest trade-wind in the North Atlantic taken just abaft the beam of vessels is sufficient to propel them 6 knots an hour, that of the S.E. trade-wind is sufficient to propel them 8 knots an hour.*

With the facts now before us, we are in a position to weigh and measure the wind-forces which meet and conflict in the torrid zone, and, as it is claimed, give the initial impulse to so many hurricanes or cyclones annually; and especially to those which are generated in the West Indies, and which subsequently come home to every American citizen whose habitation lies east of the Rocky Mountains, from the Gulf of Mexico to Lake Superior and Maine, and, I may also add, to every European.

It is a marked trait in nature's workings, that the most feeble and insignificant force may, and often does, work gigantic results.

Scientific travellers tell us that, in their ascents of the Alps, far up amid its sublime and dreadful acclivities, the cautious guide is often so situated that he dares not open his lips to speak lest the faintest agitation of the air may dislodge immense masses of ice, and bury the party in an instant beneath the avalanche or storms of snow. It can be demonstrated by the laws of mechanics that the upward discharge of a single cannon will produce a cylindrical vacuum in the upright path of the projectile, and that, under a calm and still atmosphere, such a discharge will

* MAURY's Sailing Directions, p. 853.

generate a cyclone. The spark from a rushing locomotive may fire a prairie or a vast sea of forest, as has recently been instanced in the majestic and expansive conflagrations of the North-West; and, we must remember, that a sound generalisation does not demand of the physicist that he should assign a cause commensurate with every effect he sees; but only such a cause as is equal to the origination of such an effect, or one that is sufficient to initiate it. The engineer of the *Great Eastern* moves with perfect ease the small lever which opens his steam valve, and, instantly, the mammoth steam-ship responds; her wheels begin at first slowly to revolve, condensation in her engines commences, and soon the vessel is pushing back the mighty waters and cleaving her way through the ocean. The touch of the engineer's hand has only opened a way for the pent-up energy of her boilers to come into play; all the manual force exerted in occasioning this result, if directly applied to propelling the ship, would not have moved her an inch. To revert to the former illustration, it has not done as much to overcome the inertia of the floating leviathan as is exerted by the tender foot-fall of the chamois on the glacier of Mont Blanc, which sends the ice mass, whirling and leaping with terrific ruin, thousands of feet below. It must now be evident to the reader that there is a remarkable coincidence between the occurrence of West Indian hurricanes and the conflict of the two great trade-winds.

(1). This coincidence is true in regard to PLACE. The West Indian cyclones are generated just where the hypothesis I have advanced would lead us to look for them. "The West Indian hurricanes arise," says Dové, "at the inner edge of the zone of trade-winds N.E. in the so-called region of calms, where the air ascends and flows away in the upper strata in the direction opposite to that of the trade-wind. This renders it probable that the primary cause of the cyclone is the intrusion of a portion of this upper current into that which lies underneath it.* The latter part of this sentence is conjectural, but the former the geographical fact with which we have to do. Loomis says, "The West Indian hurricanes generally originate near the equatorial limit of the trade-winds, where these winds are irregular between the latitude 10° and 20° N., and longitude 50° and 60° W. This is just the region of trade-wind interference, just the territory on which the over-leaping S.E. trade, deflected a little by the land-mass of

* Dove's Law of Storms, p. 135.

South America, comes in conflict with the lighter and feebler N.E. trade. I may further point out the equally striking coincidence, due to analogous causes, that it is in the very same region, the northern equatorial current of the North Atlantic and the southern equatorial respectively, under the propelling influence of the N.E. and S.E. trade-winds, rush together off the archipelago of the windward Antilles, and penetrating the meshes of this insular network move westward to form the Gulf Stream.

(2). But striking and suggestive as are the coincidences between the origin of cyclones, and the conflict of S.E. and N.E. trade with regard to place, they are equally or more striking with regard to the TIME at which cyclones occur.

The West Indians occur most frequently from July to October. From Poey's "Chronological Table of 365 West Indian and North Atlantic Hurricanes, from 1493 to 1855," it appears that 42 occurred in July, 96 in August, 80 in September, 69 in October. The most violent hurricanes occur between the 15th of August and the 15th of October; but that of August 10, 1831, and that of October 29, 1867, may be mentioned, with others, as noted exceptions to the remark. But, as Professor Eastman well states, "Storms of this class seldom, if ever, occur in the West Indies later than the 20th or 25th October; in fact so well defined is this limit supposed to be by the residents in that portion of the world, that the end of the "hurricane season is believed to be more nicely determined* than even the change of seasons."

(3). In point of *intensity* also the coincidence appears to be marked, not merely at the time when the irruption of the S.E. trade is most impetuous, but in August and October—the extremes of the "hurricane season"—which are the two months of the most violent cyclones, as we may easily see, the beginning and close of the battle of the trades are the periods of most desperate conflict in the West Indies. This conflict, it must be understood, is never settled, but the contest is fought on another line, or rather upon a line which ever sways backward and forward, at right angles to the meridian, and nearly under the vertical sun.

(4.) The theory by which I have sought to explain the generation of cyclones is fully sustained by the laws of mechanics, as Professor Eastman, speaking of Dové's theory, and has Dové himself has shown, "a mass of air

* See an able discussion of this hurricane by Professor EASTMAN, of the U.S. Naval Observatory; prepared by order of Admiral B. F. Sands.

moving towards the N.N.W. will, on coming in contact with the trade-wind moving towards the S.W., have a general direction towards the W. by N. (the regular path of the West India hurricane), and will at the same time fulfil all the conditions necessary to generate a cyclonical motion in the direction E., N., W., and S.," *i.e.*, against the hands of a watch. According to the old Redfield Law of Storms, Mr. Buchan, discussing the moving dust whirlwinds and pillars of sand so frequent in the East, says, "The direction of the eddy of the whirlwind, especially when of small diameter, differs from the rotation of the winds in a storm, in that it may take place either way according to the direction of the stronger of the two winds which gave rise to it. Thus, suppose a whirlwind produced by the brushing of a north against a south wind, then if the north wind be the stronger, and on the west, the whirl will be in the direction of the hands of a watch; but, if the south wind be the stronger, the eddy will turn in the opposite direction* (cyclonical). In the theory, the south wind (the S.E. trade), as we have seen, is the stronger. Dové himself, though advocating a different view, admits the mechanical correctness of this; and, indeed, were this the place, it were easy to demonstrate its truth by a direct appeal to mechanical law.

(5). The TYPHOONS of the East Indies and of the Southern Indian Ocean are explained by the theory now advanced, as happily as are the hurricanes of the West Indies.

According to the testimony of all writers, which may be stated in the words of Professor A. Keith Johnston, "Cyclones originate in the space between the Equator and the tropics near the equatorial limit of the trade-wind during winter, when these winds are irregular, and *during the changes of the monsoons*."† Out of all the hurricanes and typhoons remembered and chronicled since the year 1600, only three or four have occurred near the winter or summer solstice.

It is, of course, impossible here to discuss the exact limits of the monsoons in the East Indies. It is only necessary to examine any monsoon-chart to see that the region of vernal and autumnal monsoon changes and the typhoon regions perfectly coincide. When the South-West monsoon begins in the spring to blow over India it speedily draws upon the belt of N.E. trade-winds in the North

* Handy Book of Meteorology, pp. 305, 306.

† Physical Atlas, p. 62.

Indian Ocean. Sailors have long known this influence, and called it "the backing of the monsoon." This backing continually enfeebles the N.E. trade, finally absorbs and turns it right about to the opposite point of the compass from which it was blowing. Observation shows that the S.W. monsoon then works its way backwards through the northern trade-wind belt to its extreme equatorial limit. When this process is complete, the S.E. trade, on reaching the Equator, finds no adversary to resist its progress, and sweeps on in the general draught of the S.W. monsoon to swell its force. From May to August all goes smoothly enough. The monsoon has everything its own way, and the seaports of India have their harbours flooded with the sea, and the mountains of India are drenched with torrential rainfalls. In August the monsoon begins to change, and then begins the same conflict, but on a somewhat more terrific scale than we have witnessed in the Atlantic off the Windward Islands in the same month between the aspiring and impetuous S.E. trade and its northern congener.

In my first paper on this subject, published more than a year ago, I said, "In the Bay of Bengal the typhoon would follow in August, from the conflict of the two lateral currents of the South-East trade, and the North-East trade enfeebled by the counter monsoon influence. It is doubtless this enfeebling of the North-East trade that renders it an easier prey to the impact of the South-East trade; and this explains the violence of typhoons, which is greater than that of West India hurricanes."

Within a few weeks my attention has been called to the following brief letter in "Nature" from the distinguished geographer, Mr. Joseph John Murphy, F.G.S., which, as an able argument in point, is here partially quoted:—

"*Origin of Cyclones*—In "Nature" of the 23rd June, 1871, there is an account of a paper, by Mr. Meldrum, on the origin of storms in the Bay of Bengal, showing reason to believe that the cyclones of the Bay of Bengal and the Southern Indian Ocean originate in the meeting of the trade-winds of the northern and southern hemispheres at some distance north or south of the Equator. I do not know of any equally complete evidence on the subject for the cyclones of other parts of the world, but there is very strong reason for thinking that they always so originate. The two trade-winds meet in the Atlantic a little to the north of the Equator; for this reason cyclones are frequent in the West Indies, but unknown over the South

Atlantic, and they are most numerous at the end of summer.—JOSEPH JOHN MURPHY, Old Forge, Dunmurry, Co. Antrim."

On referring to Mr. Meldrum's paper, which I had never before seen or heard of, I discovered with great pleasure that his long-continued and accurate researches beautifully sustain the theory.* Mr. Meldrum gave at length full accounts of the particular storms which verified his belief, but which our space does not permit us to quote.

The writer commenced by observing that in various papers published during the last ten years he had stated as the result of an examination of a large body of observations, that the tropical cyclones of the Indian Ocean south of the Equator originated between two contrary streams of air, viz., the N.W. monsoon and the S.E. trade-wind; and, in a paper read on the 10th of November last, he remarked that what had been found to hold good in that part of the ocean might be found to do so generally.

As, then, observation had shown that the tropical cyclones of the Indian Ocean, south of the Equator, were formed in the belt of the calms between the N.W. monsoon, a continuation of the N.E. trade and the S.E. trade-wind, and nowhere else, there was at least a presumption that the cyclones of the Bay of Bengal were also formed in that belt, at those seasons when it stretched across the Bay, and separated the N.E. trade-wind from the S.W. monsoon; and this presumption was strengthened by the fact that most, if not all, of the cyclones that occurred there did so at the change of the monsoons; that is, when two contrary winds prevailed in the Bay, and were more or less in conflict. These general considerations rendered it possible, if not probable, that the cyclones of the Bay of Bengal were formed between two contrary and pre-existing winds. But that was not sufficient. It was necessary to bring the matter to the test of facts; and this could only be done by examining the observations taken in particular storms.

The storms and cyclones reported and traced by the U. S. Signal Service, as published from day to day in the press telegrams, also seem to establish the theory which was at first put forth with great diffidence. These reports show, in general, that the cyclones which come to us from the tropics and Gulf of Mexico move from the tropics westward,

* Mr. MELDRUM on the Origin of Storms in the Bay of Bengal.

curve to the north and east, and travel upon a parabolic curve, which oscillates with the sun in declination. This curve in summer strikes into the Florida coast, and the storm which moves along its path strikes on the Georgia and Carolina coasts, as did those of the 18th and 26th of last August. Their cyclonic intensity is of course reduced or nullified when they spread themselves on the land, as was noticed in the latter of the two storms just mentioned.

In summer this curve does not run further westward than Alabama and the Alleghanies, and hence the dry and arid season in the north-west, whose plains and prairies are then so parched that the cattle have often to be driven hundreds of miles to the water-course, and kept there till late in October. Dové himself admits that the origin of cyclones is explained "if we assume that a portion of the south-east trade-wind travels far over the Equator into the northern hemisphere."* This I have assumed and proved. My reasons for declining his explanation—viz., that "the primary cause of the cyclones is the intrusion of the upper current (from the coast of Africa) into the (N.E.) trade-wind which lies underneath it"—are quickly stated.

This view is based upon the existence of an abnormal upper current of air which has "ascended over Africa, and flows away laterally in the higher strata of the atmosphere as an easterly wind," and has, to use Dové's words, "some connection with the fall of dust in the North Atlantic,—an occurrence which is frequently observed."—(P. 186).

To sustain this he cites the fact, noticed by Piazzi Smyth on the Peak of Teneriffe, 10,700 feet above the sea, that this dust obscured the sun; and he also cites the remarkable occurrence of the erupted ashes of the volcano of Coseguina, in Central America, being "carried by the upper counter trade-wind, not only as far as Kingston, in Jamaica, a distance of 800 English miles against the direct (or surface) trade-wind, but also 700 miles to the westward, where they fell on the ship 'Conway,' in the Pacific Ocean." From this well-known fact he infers that "In the higher regions of the atmosphere the air does not always move regularly from S.W. to N.E., but that the regularity of this movement is interrupted by currents flowing from E. to W." (See "Law of Storms," p. 186).

It must be allowed that these are very slender and meagre inductions of fact to bear the weight of his hypothesis. I do not deny that his upper current colliding with the surface

* Law of Storms, p. 264.

trade would be able to generate a cyclone; but the existence of such an upper current in the cyclone region is not established by safe *observations.* The Peak of Teneriffe is near the African coast, and under the influence of the North-African monsoon. It is near the parallel of 28° N. lat. and 15° W. long., while the West-Indian hurricane region is in 10° to 15° N. lat. and 60° W. long. The sea-dust observed off the coast of Africa, when examined by the great German microscopist, Ehrenberg, was distinctly pronounced by him full of "*South-American Infusoria;*"* the same was true of all the blood-rains and sea-dust of the Cape Verde Islands, Lyons, Genoa, and other places, showing a southerly or S.W. current in the high atmosphere, not from Africa, but from South America, and, as Prof. Eastman says in his little publication (p. 15, 5th clause), "If a mass of air from the return trade-wind, moving from the S.W., be precipitated into the N.E. trade-wind, it will meet with equal opposition along its whole front, and *there will be no tendency to a rotatory or cyclonical motion.*"

On the 18th of July, when Prof. Dové's easterly current from over Africa should have been blowing, the astronomer on the Peak writes:—"Long we gazed at the novel upper clouds sailing over our heads from the south-west" (!) "and having moved his station to Alta Vista, 10,700 feet high, on the 25th of August," he went down to Orotar on business, and "leaving the station with a south-west breeze, changed it, at an altitude of 6,700 feet, to a north-east. Returning again on the 30th, he experienced a similar change at about the same height, the strength of the wind increasing as he ascended. Arrived at the station, the south-west wind was found to be blowing with great violence, and it so continued to blow for more than a fortnight, when, as the winter seemed to have set in, the party quitted the mountain."†

Here we see, by the very testimony Prof. Dové depends upon, in the very midst of the "hurricane season" in the tropics, when his fancied easterly wind should have been in full blast, Piazzi Smyth found only a south-west wind. There is *no evidence of an easterly wind* except the long-disputed dust of Ehrenberg.

If the stratum of the regular N.E. trade-wind is, in Central America (what Piazzi Smyth, as we have just seen, found it to be at Teneriffe), 6,700 feet deep (and it is probably three or four times as deep), it is easy enough to see how

* See EHRENBERG's Passat Stanb und Blut Regen.

† See PIAZZI SMYTH's Teneriffe, p. 113.

the ashes which fell on the ship "Conway" while in the Pacific, S.W. of Coseguina, were borne thither to her deck, in the superior part of the surface trade-wind; and such is the generally-accepted interpretation of the incident among geographers.

Lastly, if the ingenious theory of Prof. Dové explains the phenomena in the West Indies, it is totally inapplicable in the East Indies. The inference forced upon the mind by what has been proven of the mechanical and lateral interference of the great bodies of trade-winds, from both hemispheres, fully explains the *origin* of the rotatory motion, and the further development of the cyclone will follow from what has been already laid down. If the rotation, at the *moment* of interference, be anticyclonic, *i. e.*, with the hands of a watch, the intro-moving winds will, before they reach the depressed centre of the nascent cyclone, feel and obey the mechanical or diurnal motion of the earth, and they will quickly control the rotation of the atmosphere, and force it to become cyclonic. Once formed, however small, and though no larger than "*a man's hand*," the meteor grows, and, being embedded in large air-currents, begins its progressive movement. It has often seemed unaccountable to me that Mr. Redfield did not see and use this solution of cyclones. He appears to have at one time entertained it, but dismissed it, because, to use his own words, "it cannot always explain the uniformity of the direction of rotation, nor the continued activity of the storms in their progress to other regions and in higher latitudes, where their greatest violence is sometimes developed." But, as we have demonstrated, "the uniform direction of rotation" is fully explained by the earth's rotation, and the "continued activity of the storms" by the continued or increased supply of aqueous vapour to their centrical cylinders and the ensuing condensation, acting and reacting upon each other. It is not a fact that the storm continues its activity from the momentum of the first rotation, as the fly-wheel of a machine set revolving by a single impulse, but the rotatory intensity rises or falls with the rise and fall of the barometer in the centre of the meteor.

It is all-important, in the practical application of this whole subject, to bear in mind the movement of the sun in declination.

The solar forces, marshalled around the Equator at the time of the Equinox, may be viewed as the extended lines of some immense army in position, and girdling the entire globe on this great circle. They must be daily conceived of as in motion—now, with slow, but steady and unfaltering tread, marching upon meridian lines to the northern tropic; and now, having faced about at our summer solstice, with unchanging step countermarching toward the southern tropic. The day's march of this imaginary but real host, if measured in a line running due north and south, is the invariable distance of about 15 geographical miles. Before its forward and gleaming movement, either north or south of the Equator, and in front of its ever-outstretched lines, all opposing climatic forces waver, and finally give way. The tropical belts of calms, the zones of the perennial trade-winds, the bands of the fierce anti-trades, the monsoon influences, barometric areas, the cyclone-breeding districts, ocean currents, gulf streams, nay, the very ice barriers that have intrenched themselves in strong cordon within the mysterious periphery of the polar circles, swaying back and forth, up and down on the earth's surface, in ceaseless vibration, ever and everywhere follow the lordly sun in his declination, and strictly adjust their movements to his.

In nothing that has been said has it been at all my design to assert that cyclonic storms originate nowhere but in the intra-tropical regions and under the causal influence of the conflicting trade-winds. I have discussed only the great storms of the parent-zone of aërial disturbance. The celebrated Royal Charter Storm, charted by Admiral Fitzroy, and so memorable in meteorologic annals, is a type of a large number that originate in high latitudes. No practical meteorologist, accustomed to study every day the atmospheric movements, and to chart the undulations of the aërial ocean—

"Varium et mutabile semper"—

can have failed to notice that the nuclei of storms are formed without regard to latitude. The interference of strong air currents of different temperatures, the upward deflection of surface currents by mountain barriers, producing condensation of aqueous vapour, the flames of a burning city, as Chicago, the widespread conflagration of a prairie, will often produce the physical circumstances which give birth to depressions. In the present state of meteorologic science it may be rash to go further. But, whatever theories meteorologists may devise capable of explaining how rotative or retrograde storms *may be* originated, I have not much hesitation in venturing the opinion that future researches will show, as a matter of *fact*, they are formed chiefly, if not exclusively, along the edges of the great atmospheric currents—the surface currents and the upper currents alike—the polar streams which descend into our valleys with their blasting frosts, and the "aërial gulf streams" which move invisibly, but with benign influence, above our heads. The great problem, therefore, in this view for the modern meteorologist is to produce a chart of the upper atmosphere. If the mechanics of the circulation of the air were half so clearly unfolded as those of oceanic circulation, the science of storm-warning would receive an immense impulse. We earnestly, but respectfully, commend the subject to the congress of meteorologists soon to convene.

II. WEATHER PROPHECIES.

THE science of the weather may be said to have sprung up within the last half century, and we must not therefore wonder that, until very recently, meteorological science has rather been concerned with the weather as it has been, than in prophecying what kind of weather may be expected. Indeed, this is almost the case at the present day; for were it not for the telegraph, storm-signals would be of little avail. Much was gained when, from the conclusions drawn from a large number of observations, a storm could be telegraphed from any place as *coming*, instead of as happened. This stage of the science is perhaps as far as can be usually attained in the present day; in some future time, from the careful study of the laws, it may be possible to predict, with average certainty, the state of the weather from day to day, or even for several days to come. It remains to be seen how far this power has been attained; and it may not be uninteresting to notice, in passing, the very unstable ground upon which weather predictions were founded before meteorology included this second division.

Whether we take as type the old dame's faith in the gambols of her cat, the high flight of some birds and the low flight of others, the "camel" in the clouds, the chirruping of grasshoppers, or Marshal Beaugaud's rule—

> "Primus, secundus, tertius, nullus,
> Quartus, aliquis,
> Quintus, sextus, qualis;
> Tota Luna talis"—

we have much the same arbitrary system, or rather want of system, although these signs may not be without some definite cause, more or less remotely connected with coming changes in the weather. In many country places it is common to hear it remarked that "the rain will soon clear up, for the birds are singing;" the coming change is perhaps already sensible to their more delicate organisation. There is also the appearance of the clouds; and to this indication even the lamented Sir John Herschel attached somewhat of a reliance, in that "anvil-shaped clouds" portended a gale of wind. But as a rule, the moon may be considered to hold the first place of influence upon weather predictions. Halos round the moon are the phenomena most commonly observed, and are readily explained by the laws of the reflection of light from the particles of aqueous vapour suspended in the atmosphere. When these halos are coloured, we may infer the presence of watery particles in the higher regions of the atmosphere; when the halos are white, we may conclude that the particles are frozen, and expect cold weather. Crossed halos, mock moons, or highly developed phenomena, indicate larger crystals of ice, and probably frost, hail, snow, or heavy rain after three or four days, according to the season of the year. Similarly the laws of reflection of light indicate that the cause of a deep purple morning or evening sky is the large amount of moisture present in the atmosphere. Another effect of the moon, when at the full, is to clear the sky of cloud, traceable, says Sir John Herschel, to a distinct physical cause, the warmth radiated from its highly heated surface; though why the effect should not continue for several nights after the full remains, in the opinion of the same accurate observer, problematic. Other lunar prognostics, founded on arbitrary rules, as to the time of the day or night at which the changes or quarterings take place, are worse than useless, for they are calculated to mislead, and are generally included in almanacs or note-books intended for sale only, being in some cases attributed to an eminent meteorologist or astronomer—Sir W. Herschel or others.

It is of course far from our purpose to enter here into a disquisition on the theory of the trade and anti-trade winds, and their barometrical indications—subjects that can be usefully discussed only in a treatise on meteorology: we limit ourselves to the present position of weather prognostics, although it must be admitted that any advance yet made or likely to be made in prognosticating the weather arises from the study of such recurrent phenomena, the investigation being much aided by the highly developed character of the laws of the expansion of gases, upon which laws the theory of the wind is founded. Thus we know that a rise in the barometer, together with a fall in temperature, as shown by the thermometer, indicates the approach of a cold northerly current of air; while a fall of the mercury in the barometer, with a rise of that in the thermometer, indicates that a southerly or warm air current is on its way. Northerly currents may include winds from the north-west and north-east, as well as from the north; similarly, southerly currents may include winds from the south, south-east, or south-west. When the barometer rises whilst a north-east wind is blowing, with prevalent hail, rain, or snow, there may be no change. Of barometrical indications alone, it is generally known that a rapid rise portends changeable weather; a slow rise, the contrary; a rapid fall, heavy wind, rain, and snow; while a fluctuating height of the column of mercury indicates unsteady weather. With a heavy gale of wind in the east or south-east, changing south, the barometrical column may fall until the wind shifts its quarter. Upon such observations did Admiral Fitzroy base his code of instructions, now to be found by the side of every barometer, his forecasts depending on the indications of the barometer and thermometer, with observations as to the direction and force of the wind with regard to time and place, and its previous course taken altogether. These indications are thus not absolute, but relative to the preceding state of the weather. But also these indications are valid for only a short interval before the actual advent of the storm; and in some instances, as in the Hyperborean storm of 2nd and 3rd October, 1860, the interval is too short for any advantage to be taken of the notice. The particulars of this storm, which presents in true character the difficulties which the meteorologist must encounter, are too interesting to be omitted, and we shortly recount them from the complete and admirably-conducted investigation published by Professor C. Piazzi Smyth, in the Annals of Scottish Meteorology for 1856 to 1871. The term *Hyperborean* has been employed to prevent confusion with tropical hurricanes; it has also been called, from its essential locality, the Edinburgh storm. We have to consider only the practical lessons to be deduced from the observations of this storm; the account of the actual observations must be read from the before-mentioned report of Professor Piazzi Smyth. First, then, the barometric notice was insufficient and too local to be of service, while the storm was too quick in its movements. St. Hilda is the most westerly station; and even if the storm could have been telegraphed thence, the message would have allowed only two hours for preparation, and would have arrived while the eastern men were sound asleep. If a message could have been sent from Iceland the day previous to the arrival of the storm, many wrecks would have been prevented. So that we see the present system of meteorology necessitates not only diligent but earnest watching of the signals that should be afforded by a network of cables and overland wires, for it is by a series of connected observations, extended over a large area, that the usefulness of this branch of meteorology is alone likely to be advanced.

But, it may be asked, what definitive knowledge can be gained, say not of storms, but of average weather for some future period? Here we must again refer to Professor Piazzi Smyth's report on the rock-thermometers at the Royal Observatory, Edinburgh, and to the Proceedings of our own Royal Society for the 2nd of March, 1870, in which predictions of the weather during the winters of 1871-72 are attempted. The rock-thermometers have by their readings shown some well-marked supra-annual cycles, the relation of which to the sun-spot cycles will be known to our readers. And on this point it may be stated that the Radcliffe astronomer announces in his report for 1871, that the *mean* azimuthal direction of the wind at Oxford, rigorously computed from automatic records during the last eight years, varies year by year through a range of 58° on the whole, between maximum and minimum of visible sun-spots; the tendency of the wind to a westward direction increasing with the number of spots; and with such west wind, it is to be presumed, the amount of rain also. "The most striking and positive feature of the whole series of observations," continues Professor Piazzi Smyth, "is the great heat-wave which occurs every eleven years and a fraction, and nearly coincidently with the beginning of the *increase* of each sun-spot cycle of the same eleven-year duration. The last observed occurrences of such heat-wave, which is very short-lived, and of a totally different shape from the sun-spot curve, were in 1834·8, 1846·4, 1857·8, and 1868·8; whence, allowing for the greater uncertainty of the earlier observation, we may expect the next occurrence of the phenomenon in or about 1880·0. The next largest feature is the extreme cold close on either side of the great heat-wave: this phenomenon is not quite so certain as the heat-wave, partly on account of the excessive depth and duration of the particular cold wave which followed the hot season of 1834·8. That exceedingly cold period, lasting as it did through the several successive years 1836, 1837, and 1838, was, however, apparently a rare consequence of an eleven-year minimum, occurring simultaneously with the minimum of a much longer cycle of some forty or more years, and which has not returned within itself since our observations began. Depending, therefore, chiefly on our later observed eleven-year periods, or from 1846·4 to 1857·8, and from the latter up to 1868·8, we may perhaps be justified in concluding that the minimum temperature of the present cold wave was reached in 1871·1; and the next similar cold wave will occur in 1878·8." Between the dates of these two cold waves there are located, according to all the cycles observed, even including that earlier one otherwise exceptional, three moderate and nearly equidistant heat-waves, with their two intervening and very moderate cold waves, but their characters are quite unimportant. With regard to all the waves, it may be just to state that there has been in observation more uniformity, and will be therefore in prediction more certainty, for their dates than for their intensities.

We have thus very briefly surveyed the position of meteorology, and little remains to be said beyond that the results are highly in favour of the hopes of physicists to render meteorology an exact science.

III. THE AMORPHOLITHIC MONUMENTS OF BRITTANY,

COMPRISING THE CIRCLES, AVENUES, CARNEILLOUX, AND OTHER ASSEMBLAGES OF SHAPELESS STONES.

By S. P. Oliver, Capt. R.A., F.R.G.S.

Part II.

We have hitherto assumed that all the megalithic monuments have been constructed for sepulchral purposes, but an important question has been raised as to whether the dolmen-mounds are in any way associated with the circles, avenues, alignments, and other assemblages of upright stones found in their immediate neighbourhood. Is their origin coeval? and, were the lines constructed by the dolmen-builders, or not? Now, as to their contemporaneity; the Rev. E. L. Barnwell* states

* Rev. E. L. Barnwell, Archæologia Cambrensis, 3rd series, vol. x., p. 57.

that "of all the monuments usually called Celtic, whether of pillar-stones, chambers, circles, avenues, &c., the simple chamber, cromlech or dolmen, with or without its covered gallery, is now generally acknowledged to be the earliest." But to us there seems every reason to suppose that the stone avenues hereafter described belong to a period far anterior to that when the menhirs and dolmens were erected, when we compare their shape and position. These assemblages of stones are conspicuous for their want of uniformity, absence of elegance, and, in fact, general shapelessness—some globular, others rhomboidal, &c.—all sorts of irregular, fantastic, rough, and bizarre forms. The finest appear to have been chosen for their majesty of size and bulk alone, and but little heed taken as to how or in what position they should stand as long as they were in their proper alignment; they are erected any side uppermost, as often as not with their heavier portion in the air, being well nigh balanced on their smaller end; so much so, as would lead one to suppose sometimes that they had been designedly so placed. Although it is possible that they were originally thus placed in equilibrium, still it is more probable that they were left in the readiest position which came to hand. There is no trace of their having been fashioned artificially. At all events they present a striking contrast to the isolated pillar-like menhirs of smooth elegant exterior, placed generally with an especial view to their stability, and exhibiting a practical knowledge of the position of the centre of gravity on the part of those who erected them. Many of the menhirs present the appearance of having been fashioned artificially, as at Locmariaquer, where that magnificent specimen, *Le Grand Menhir*, now lying in four pieces, exhibits the artificial handiwork of man; whilst on the south side of the menhir, *La Boulaie* (*Moustoir-ac*), are two sculptured figures in relief, whilst others exhibit fluting, and other signs of ornamentation belonging to an advanced period of art. Again, we know that the custom of erecting monoliths has descended down to historic times; thus we have records of the erection of memorial pillar-stones (the *Bauta*-stones and *Minne*-stones of Scandinavia, for instance, and the *Gullaunes* of Ireland)—

> Blissful a Son is
> tho' born but lately,
> his father already fallen;
> seldom *Bauta-stones*
> bound the folk-path
> save raised by kin to kindred.
>
> The "Elder Edda," Hávamál, v. 71.
> *Professor Stephens's Trans.*

whilst a modern Danish poet, M. A. G. Ochlenslaeger, writes—

> "Rush to arms with ready tread;
> Raise a *Rune-stone* o'er mine head;
> *Rune-stone* rist as Ettin strong,
> Ringing my fame time's waves along!"

Now, we have no historical accounts of the circles, avenues, and alignments being erected. In like manner, the dolmens are still more remarkable for the admirable smoothness, flatness, and clever adaptation of the huge slabs composing them, whilst they, too, are found in many instances elaborately ornamented.

Under the general head, therefore, of megalithic monuments, which includes all the menhirs, avenues of pulvens, and dolmens, &c., we may not be wrong in applying the distinguishing term *Amorpholithic* to the lines and circles in Brittany, or to any other assemblages of stones of a similar shapeless character.

On this characteristic difference we venture to suppose that the stone avenues are attributable to an age considerably anterior to that of the dolmen-mound builders.

Mr. Fergusson infers, as far as speculation is possible, that the French examples of rude stone monuments are, as a rule, of earlier date than the British or Scandinavian, and he denies that the semicircular or quadrangular enclosures can be classed with stone circles proper, such as those of Wiltshire and Cumberland, Moytura and Stennis, so far as almost to deny the existence of circles in France. We are almost inclined to agree with him.

If the isolated menhirs were erected by the same peoples or their ancestors, it is not likely that they would be placed so as to interfere with the evident design of the alignments; an example of which interference we find at Kermario, where a single intruding menhir rises abruptly amidst the smaller stones of the avenues, of which it is wholly independent, and therefore is probably a subsequent erection.

In spite of their propinquity (for we find many instances of menhirs and dolmens on either side of the lines, likened by some French writers to the outposts of the main army; but which M. de Freminville calls "*pierres d'avertissement*," which were placed to warn the approaching intruder lest he should profane the neighbouring Druidical sanctuary), it seems not unlikely that there was no connection between the lines and assemblages of amorpholiths and the other megalithic structures, or between the races of men who constructed them: on the other hand, it is not improbable that the most suitable stones of the more ancient lines were often taken to construct the neighbouring cromlechs. In opposition to this we cannot help admitting that there seems to be some analogy between the narrow entrance gallery leading to the chamber, sometimes circular, of the dolmen-mounds, and the narrow avenue, which widens as the circle is neared.

The French *savans* appear to have confined their labours to the contents of the interiors of the dolmens, and, with the exception of a few vague speculations, have altogether neglected to examine thoroughly the gigantic stone circles and avenues, which are amongst the most conspicuous and renowned objects in Brittany. The celebrated stone avenues of Carnac are a series of alignments in the Commune of that name extending from the borders of the Commune of Trinité to a spot between Carnac and Plouharnel. It has been taken for granted that these alignments were continuous; and it is to the careful measurements and plans made by two English gentlemen, Sir H. Dryden and the Rev. W. Lukis, that we are indebted for their discovery that these lines in reality consist of three totally distinct groups.*

These groups are named for convenience after the villages or homesteads nearest to them, viz.—(1) *Menec;* (2) *Kervario*; and (3) *Kerlescant.* It is not improbable that the names of these localities were acquired from the circumstance of the stones standing upon them; for, according to Lepelletier's Celto-Breton dictionary, the word *Menec* signifies a memorial or souvenir, and the word *Kervarv* the place of death. The words *Carnac* and *Carneilloux* are similarly identified with Celtic words signifying a charnel house or ossuary: so in the Cymri we find *Carnedd* as the Welsh for cairn, tumulus, or tomb.

(I). Commencing with the group nearest Carnac, we find a circle of stones, or rather the remains of one, on a slight elevation enclosing several of the farm buildings and dwellings of the village of Menec, after which the group is named. This enclosure is somewhat elliptical in plan, its largest diameter being 92 yards, and its shortest 80.

These stones, which are of uncouth form, are of considerable size and bulk, having an average height and breadth of about 9 feet, whilst they are rather flat and thin. The circle forms the south-western limit of a series

* The author was unaware of the existence of Mr. Vicars's map, since re-published by Mr. Fergusson, when the above was written.

of eleven alignments, which stretch from this eminence in a north-easterly direction over undulating ground gradually sloping to a lower level, for a distance of 1025 yards. These lines, the southernmost of which alone meet the circle, are about 11 yards apart at their south-west extremity, and converge to within 5 yards of one another at their further extremity; the size of the stones composing them also diminishing by degrees to an insignificant size. There now occurs an interval of 600 yards before we reach the next group of amorpholiths, the largest of which are on a plateau near the farm of Kervario. Some of these stones are the most gigantic in the Carnac series.

(II). At Kervario there is no terminal circle, although some scattered blocks of no inconsiderable size may perhaps indicate vestiges of one, but they are more probably blocks left in transit towards their destined situation.

This group consists of only ten lines, 12 yards apart, converging gradually for a distance of about 1250 yards, until the traces of their termination are lost in the plantations of the Chateau de Kercado.

It must be borne in mind that these lines are not absolutely straight, but that each crest of hill or course of brook there is generally observable some slight deviation in direction of the alignments, which has probably arisen from error in the original laying out of the lines, and not from any intentional serpentine plan on the part of the architects, as some writers would have us to believe. These slight deviations go far to prove that the country presented the same features and outlines of contour when these alignments were planned as at present; at all events that its character cannot have materially altered since the days of the construction of the said lines.

(III). The third group to be described, viz., the *Kerlescant* lines, is 400 yards beyond the last-named series, and differs considerably both from the *Kervario* and *Menec* monuments. Here there is a terminating enclosure (somewhat of an horse-shoe form rather than that of an incomplete circle), having a diameter of 96 yards, and associated with thirteen lines of stones extending, however, in the same direction, *i.e.*, to the north-east, but only for a distance of 286 yards, about one-quarter the length of the other groups, and yet the breadth of the whole group is as broad as the Menec series, viz., 110 yards at the western end, giving to each avenue an average breadth of 9 yards, which rapidly diminishes, however, to 5 yards at the north-east extremity.

The eight southernmost lines alone are opposite the horse-shoe enclosure, whilst in a corresponding position opposite the five northern lines is a long barrow, a significant fact. Again, the avenue between the fifth and sixth lines is of a greater width throughout its entire length than the others, the lines composing it being nearly parallel.

There is also a certain symmetry observable, taking the whole thirteen lines together, there being a centre group of three lines, on either sides of which is a broader avenue tolerably parallel, beyond again on either side is a group of five lines converging, the breadths between which are largest on the outermost side. Such are the three groups which, together, constitute the celebrated lines of Carnac, and which have been hitherto erroneously described as one monument. There is another misconception of the site of these circles and avenues, to which I may here allude in order to correct. Some writers speak of "the great plain of peulvens at Carnac* on the solitary peninsula of Quiberon," and "the rugged wastes of Carnac." Now the neighbourhood of Carnac is not the desert that would be inferred from the above-quoted descriptions; there are fields and gardens, farms and villages, woods, and some almost pretty scenery. It is certainly not *on* the peninsula of Quiberon, celebrated for the ill-fated expedition of the Royalists in 1795. The peninsula itself is certainly solitary, and its sand-dunes, by degrees being planted, are not inviting, and there are some remains of lines and circles of stone upon it, which will presently be noticed. Mr. Lukis, in speaking of the view from Mt. S. Michel, talks of vast heaths, undulating country, fir plantations, and small villages, nestling among elm trees, and, further on, of threshing yards and gardens—that is a true description of the landscape, and does not suggest "the rugged wastes of Carnac."

Before considering in detail the characteristics of these curious alignments, we will proceed to describe the other assemblages of amorpholiths in the departments of the Morbihan and Finistère. Our poet laureate, Alfred Tennyson, must surely have had these parts of Brittany in his view, rather than Cornwall, when he wrote in "The Holy Grail"—

> "And then, with small adventure met, Sir Bors
> Rode to the lonest tract of all the realm,
> And found a people there among their crags,
> Our race and blood, a remnant that were left
> Paynim amid their circles, and the stones
> They pitch up straight to heaven: and their wise men
> Were strong in that old magic which can trace
> The wondering of the stars, and scoffed at him
> And the high Quest as at a simple thing."

The most extensive of all the lines is not far from

Erdeven, stretching from the road south of the village towards the south-east for the distance of over a mile and a half. There are, as well as can be made out (for there is sometimes considerable difficulty in tracing these lines), ten lines of stones nearly parallel; the convergence is very slight, the breadth of the lines at the western end being 220, and at the eastern extremity 190 feet. But it must be remembered that these alignments are not actually traceable over the whole extent of this long tract of land; about half way is an eminence, upon which are four dolmens and a long barrow, and here traces of the lines are lost for some distance; but the same number of lines occur again further east, and continue to a spot not far from the huge dolmen of Courconneau, and it is presumed that they formed a portion of the same series. There is a slight deviation between the direction of the eastern and western portions, but only of 3°, the western portion being 117°, and the eastern 120° to the east of the meridian.

From near the head of these lines is an alignment consisting of 25 stones extending in a N.E. direction (42° E. of N.) for a distance of 354 feet. The stones are at present all prostrate, excepting erect menhirs at either end. They are all large stones; in fact the largest masses to be found throughout all the series of alignments in Brittany, the largest measures 21 feet 6 inches long by 10 feet broad and 5 feet thick; two others are nearly as bulky within a few inches. What the meaning of this diagonal line outlying the main body of the Erdeven lines can be is a mere matter of conjecture. It is certain that the blocks lie close together as those composing the circles, at Menec and Kerlescant. If this, therefore, was portion of a circle, it is the sole example of one north of accompanying lines. At the eastern extremity of the Erdeven lines, but at some small distance, is an enclosure of stones nearly oblong, measuring 150 feet long by 85 broad.

Whilst on this subject, it would be wrong to omit any mention of the Red Carnac pebbles—although they are not mentioned in Mr. Lukis's paper, nor even alluded to by the Vannes Society—which occur in multitudes, wherever there are these lines of menhirs. Whether the sites of the alignments have been chosen from the presence of these pebbles, or whether these pebbles are the concomitants or deposed on the sites of the alignments, or whether they form part of a regular geological deposit wholly distinct from any connection with the menhirs, is at present undetermined; and these stones have proved a puzzle to some of the best geologists, and remain as hard nuts to crack for future speculators. I have been permitted to extract the following from the MSS. of Mr. Lukis, sen.:—"Over this district (*i.e.* about Carnac) the surface of the ground produces red pebbles, or worn stones, which at first sight resemble red jasper, but they are evidently belonging to the alluvial of 'the neighbourhood, and have eroded angles and surfaces of a peculiar nature, resembling that of artificial rubbing by the hand of man.' In a note written to me by Dr. John McCulloch, he says, 'I can make nothing of these, unless they are of barbaric art.' These stone pebbles of Carnac and its neighbourhood had also occupied the attention of Dr. Buckland, who likewise considered them as the work of man.

"Mrs. Buckland, his widow, gave me several of them, which had been the subject of much conjecture, and which she said the learned doctor had shown to Chantrey, who endeavoured to persuade him that these might have been used for rude sculpture, in like manner as the bits of white sandstone grit were used by himself when he wished to rub the folds of garments during his employment.

"As these stones are found strewed about the neighbourhood of the menhirs of Carnac, they are given to travellers by the native guides as relics left by devotees at the base of the alignments. These remarkable pebbles differ from those on the neighbouring beach, and are derived from a different source; they are polished usually on all their faces, and have a strong inclination to wear down in a rhomb, rather than, like rolled pebbles on the beach, into round or nodular form. Some appear to flatten on both sides, and become pitted on both surfaces, as stones of silex do when acted on by Eolian force mixed with sand."

The St. Barbe lines are not a very remarkable group; here traces of three lines exist for a distance of three hundred yards. A few of the stones at the head of these lines are of great bulk, the two largest measuring 14 ft. × 12 ft. by 7 ft., and 14 ft. × 11 ft. × 4 ft. 7 in. respectively.

A short distance to the west of these alignments are four stones, which may indicate vestiges of a circle, but they are so doubtful as not to be included in the plan.

Not far from the St. Barbe lines are some conspicuous stones near the mill at Plouharnel; they are called "*Les trois pierres du vieux moulin;*" the largest is prostrate, and measures 16 ft. long × 11 ft. × 5 ft. The two others standing are respectively 12 and 8 feet in height; they seem to have once formed the head of a series of alignments.

At Kerzine, near Plouharnec, are eight lines of stones in a dilapidated state, extending one hundred and fifty yards, whilst in the same neighbourhood, at Kerœnthue, is a line of four large stones, with a detached one at some distance; the larger of these measures 16 ft. × 9 ft. × 5 ft.

The lines at St. Pierre, which lie on the sandy pit of Quiberon, south of Fort Penthievre, are remarkable for the fact that they are detached from the neighbouring circle, which stands ninety-five yards distance to the south-west. Traces of only five lines remain, some of which extend a distance of two hundred yards to the coast line, and possibly before the encroachment of the sea extended still farther.

Such is the description of the most remarkable lines in the Morbihan, and we will now proceed to consider certain noticeable alignments of Crozon, near Brest. The alignments on the promontory of Crozon, near Brest, are altogether on a smaller scale than those at Carnac, and have accordingly attracted much less attention than the latter gigantic remains; but the whole locality is extremely interesting, and literally teems with menhirs, alignments, &c. De Fréminville mentions them, and gives accurate illustrations of the most important lines at Landaoudec and Toulinguet in his volume on the Antiquities of Finistère.

We visited the three most extensive of these alignments, viz. Landaoudec, Leuré, and Logatjar.

The most important of these is on the Lande by the mill of Landaoudec, about half way between Lanvéoc and Crozon villages. The stones are in rather an unintelligible position, and most of them are now prostrate, whilst many of them have evidently been removed into the banks on either side of the road; but still there is the trace of an enclosure associated with orientated lines, which also seem to have converged. Near the eastern extremity of the longest line, which extends some three hundred and fifty yards, are the remains of a kist, and near the mill are some isolated menhirs. The largest stones measure 11 feet 3 inches by 6 feet 6 inches, and 9 feet 9 inches by 4 feet 6 inches. The largest stone standing is 6 feet 9 inches high by 5 feet 6 broad. According to De Fréminville there exist some scattered stones to the north of the mill, but we did not examine them.

The lines of Leuré are about two miles to the westward of the last-mentioned group, and are not far from the little port of Fret, where the steamer from Brest touches. These lines exhibit the features usually distinctive of the Crozon alignments, viz., lines at right angles to one another. The longest line consists of eleven stones, arranged east and west, and extending over one hundred and seventy yards, and a shorter alignment (thirty-three yards in length) at right angles, composed of four upright and three prostrate blocks. In this last line are the largest stones. The upright ones measure 7 feet 6 inches high by 4 feet thick, and 4 feet broad; and 6 feet high by 6 feet 6 inches broad, and 5 feet 6 inches thick.

The menhirs of Gatjar or Logatjar are situated on the down above the village and port of Camaret.

They are by far the most conspicuous and clearly defined of all the alignments on the promontory, as they are not overgrown with furze bushes like the two last-mentioned lines. Here, again, we find the characteristic features of two short lines about fifty yards long at right angles to a longer line two hundred and sixty yards long, lying north-east and south-west. The short lines, one of fourteen, the other of twelve stones, are on the north-west side of the longer line. The conspicuous erect stone is 11 feet 4 inches high; the largest prostrate stone is 13 feet long by 4 broad.

Near all these alignments are outlying menhirs, which M. de Fréminville terms "*les menhirs d'avertissement,*" which, according to him, announced to the approaching visitor the vicinity of a sacred enclosure.*

An outlying menhir at Gatjar, close by some stones which may have formed a dolmen, probably stood at the foot of the tumulus which formerly covered the sepulchral chamber. One suggestion made as to the disposition of these alignments at Logatjar is that they commemorate a naval victory, and that the arrangement of the stones indicates the position of the fleets engaged. Admiral Thévenard, true to his profession, is the originator of this idea, in his "Recueil de "Mêmoíres relatifs à la Marine," and supposes from the position of these lines, erected on a lofty promontory overlooking the sea, that they represent the order of battle of the Armorican fleets.

There are several other assemblages of stones in this neighbourhood. Amongst others there are some, probably portions of alignments, near the village of Goulien, in the bay of Dinan, and a Carneillou, perhaps a circle, with two parallel alignments stretching eastwards from it, near the cliffs overlooking the Anse de la Pallue, north of the Vec de la Chèvre.

Between the Pte. de St. Hernot and Pte. de Morgatte are

* Antiquités de Finistère, (2nde parte, p. 20 *et seq.*), par M. LE CHEV. DE FREMINVILLE. Brest. 1835.

also two enclosures, one with an avenue, the other with double lines of stones, bearing the local name of "*Maison du Curé*." On the banks of the River Laber, also, close to the farm Raguénez, is an alignment, and there is a Carneillou at the manor of Tréběron, some distance inland from the coast. This last is curious, from the fact that the tumulus, which appears to be associated with these lines, is named *Le Tombeau d'Artus*.* These last we did not examine, and they are therefore merely mentioned as a record of their existence for the benefit of those who may intend to visit these localities. The accompanying Table (p. 447) gives the principal alignments in Brittany. And the following list shows the angles of orientation, or bearings, of the principal lines of stones; the variation being taken to be 23 deg. W.

E. of mag. N.		
57 deg.	0 min.	Camaret.
83 ,,	45 ,,	Menec tail.
86 ,,	30 ,,	Kervario.
87 ,,	30 ,,	Kerdouadec.
91 ,,	15 ,,	Menec head.
93 ,,	15 ,,	Kerdouadec.
93 ,,	30 ,,	Leuré, W. part.
99 ,,	0 ,,	Leuré, E. part.
106 ,,	0 ,,	Erdeven head.
113 ,,	0 ,,	True east.
117 ,,	0 ,,	Kerlescant.
118 ,,	0 ,,	Cojou.
122 ,,	0 ,,	St. Pierre.
135 ,,	0 ,,	Erdeven tail.
136 ,,	0 ,,	St. Barbe.
145 ,,	0 ,,	Kerzine.

It is well worthy of remark that like everything else in Brittany the origin of these avenues, menhirs, &c., is ascribed to the "fairies," a superstition which is common to most if not all countries where anything unusual exists, and where any striking natural or artificial wonder is universally attributed by the local traditions and folk-lore of the native peasantry to pigmies and giants, and later, as priestly

* *Artus* or *Artur*. "Le roi *Artus* fut enterré dans l'île *d'Aval* ou *d'Avalon*, sur les côtes qui avoisinent Lannion, et à peu de distance de son séjour favori, ce château de Carduel or Kerduel, si célébré par les chroniques de la table ronde, et appartenant aux enfants de M. de la Fruglaye. Les Anglais ont voulu, mais à sort, s'approprier ces localités."—(De Freminville.) From this it would appear that the Arthurian legends are as much Armorican as British. *Vide* "Traces of Affinity between the Bretons and the Cornish," "British Quarterly Review," No. 104, October 1, 1870.

Name of department.	No.	Local name.	Description.	Length in yards.	Breadth in feet. West.	Breadth in feet. East.	Diameter of circle.	Remarks.
MORBIHAN.	1.	MENEC.	Eleven lines of stones with circle at W.	1000	330	195	290	Ends apparently certain.
	2.	KERVARIO.	Ten lines.	1250	323	180	—	E. end undefined.
	3.	KERLESCANT.	Thirteen lines extremely convergent, and Horseshoe enclosure at W.	285	450	205	300	Ends well defined.
	4.	ST. BARBE.	Three lines remaining, original number uncertain.	300	125	—	—	E. end undefined.
	5.	ERDEVEN.	Ten lines nearly parallel, with a diagonal line at N.W.	2680	220	190	—	Ends certain. Gap for some distance about centre of lines.
	6.	ST. PIERRE.	Five lines, the circle distant 290 feet to the S.W. of the head.	210	110	—	180	W. end certain, E. end terminated by sea coast.
FINISTERE.	7.	PLOUHINEC.	Eight lines.	150	240	—	—	Ends uncertain, much scattered.
	8.	CRUCUNY.	— — —	—	—	—	—	Not measured.
	9.	KERDOUADEC.	Two lines, with nondescript enclosure and divisions.	350	—	—	—	Ends not defined.
	10.	LEURE.	One long line and a short one at right angles to it.	172	—	—	—	Ends not defined.
	11.	LOGATJAR.	One long line and two short ones at right angles to it.	267	—	—	—	Ends apparently certain.
ILE ET VILAIN.	12.	COJOU.	Two parallel lines.	140	80	—	—	A great part destroyed.

influence was in the ascendant, to saints and demons. We have classical authority for the popular belief that "there were giants in those days," the Greeks and Romans giving to their god-like heroes supernatural strength. For instance, take the boulder which Turnus in his last despair hurls at Æneas—

"He said no more, but looking round
A mighty stone espied,
A mighty stone, time-worn and grey,
Which haply on the champaign lay,
Set there erewhile the land to bound,
And strifes of law decide:
Scarce twelve strong men of later mould
That weight would on their necks uphold
To-day's degenerate sons:
He caught it up, and at his foe
Discharged it, rising to the throw,
And straining as he runs."

"Nec plura effatus, saxum circumspicit ingens
Saxum antiquum, ingens, campo quod forte jacebat,
Limes agro positus, litem ut discerneret arvis;
Vix illud lecti bis sex cervice subirent
Qualia nunc hominum producit corpora tellus;
Ille manu raptum trepidâ torquebat in hostem,
Altior insurgens et cursu concitus heros."
VIRGIL, "Æneidos," lib. xii., 896 to 902.

In like manner the monkish legends represent the devil throwing stones at some saint, and the saint replying with interest, causing the demon to become lame by some menhir dropped on his toe, &c.

It is not long since the Wiltshire shepherd would scarcely pass near Stonehenge at eventide for fear of surprising the unholy orgies of the giants, and believed in the possibility of becoming the victim of an enchanted pixy-ring. Indeed, the tradition is still prevalent that the stones of Stonehenge were transported by miraculous agency from afar. Geraldus Cambrensis, A.D. 1187, gravely affirms that Stonehenge was removed from a mountain in Ireland to Salisbury Plain by the magician Merlin. In Brittany accordingly we find the neighbourhood of the dolmen-mounds, the peulvans, and avenues to be haunted either by the *dur* or dwarf, and the Karrigwen (Korrigan or Korils), the mischievous elves or the Teuz, the benevolent fairies. Mr. John Lukis mentions that one day he lately visited a fine cromlech on Mr. Bruce Pryce's property, near Cardiff, in Wales; on asking some children who were playing about the name of the spot, they at once replied "*Castle Korrig*," and was particularly struck with the Celtic name of the Breton fairy as applied to a cromlech in Wales. Similarly in Ireland, the mighty *Raths* and *Cathairs* are frequented by "the good people," either in the forms of the lovely and tiny *Sheogues*, the fair and perfect *Lianhaun-shee*, or the quaint cobbler *Leprechawn*. The banshee's wail is heard by the solitary *gullaune* pillar-stone; whilst if we wish to find the mischievous *cluricaune*, we would search the sites of the *carnail* or *carns*, the *murs*, *miams* or *cumots*, and for warlocks on the *Slieve na Calliagh* or witches' hill. As may well be supposed, archæologists and travellers, both native and foreign, have advanced from time to time innumerable theories more or less ingenious or absurd, and mostly paradoxical as to the origin and destination of the circles and alignments of amorpholiths. Indeed, as many, if not more, strange and opposite conclusions have been drawn by various writers in respect to these monuments in Brittany as has been the case with Stonehenge in our own country.* Innumerable purposes have been proposed for their construction, such as covenants, altars, boundaries, deliverances, sepulchres, and victories, &c., whilst others are in favour of the Epicurean hypothesis of παρέγκλισις or arbitrary deflection. The peasantry of the neighbourhood firmly believe that these ranks of stones are the effect of a miracle; the tradition is as follows:—St. Cornely, Pope, and patron saint of Cattle, being pursued by an army of pagan soldiers, and unable to escape, exercised his saintly power, and converted the army into stone; hence these stones are called in Breton *Saint Cornili soudared*. It is evident that this tradition has only originated since the arrival of Christian missionaries in Armorica, where semi-pagan stone worship has yet some hold among the most ignorant folk in the whole of France. The parish church of Carnac is dedicated to Saint Cornely, and a figure of him stands outside the tower, with some rather comical sheep and oxen at his feet. Unfortunately, when this church was built (A.D. 1639), some of the pagan soldiers were broken up in order to supply building material, and there are many gaps in the ranks in consequence. M. de la Sauvagère, *officier du génie*, appears to have had curious notions of Roman castrametation; he supposes that these stones were erected "*pour servir d'appuis aux tentes et le mettre*

* "Mr. Samms, in his "Britannia," will have the structure to have been *Phœnician*; Mr. Jones and Mr. Webb believe it to be *Roman*; Mr. Aubrey thinks it was *British*; and Dr. Charleton derives it from the *Danes*. And yet if the true old writing of the name be STAN-HENGEST, as the Monasticon seems to tell us, I cannot see, says Bishop Nicholson, why the Saxons may not have as just a title as any to the honour of it. There is a manuscript treatise said to have been written upon this subject by one Mr. John Gibbons, and 'tis possible this gentlemen may have a different notion from all the rest." —(Preface to "The Most Notable Antiquity of Great Britain, vulgarly called Stone-heng on Salisbury Plain.")

à l'abri du vent;" and the Comte de Caylus takes the trouble to produce a quantity of evidence that the Romans were not in the habit of encamping in such order. M. Mahé attributes all the megalithic monuments to the Hebrews and Greeks; whilst M. de Penhouet finds an identity between the name of Carnac in Brittany and Karnac at Thebes in Egypt; the same theorist also finds the Bas-Breton language as now spoken to be of a pure Phœnician origin. Dr. John Bathurst Deane saw here a temple in the form of an enormous serpent. This not very satisfactory hypothesis at one time found a supporter in Dr. Thurnam, who has, however, now given up this ophite or dracontium theory as untenable. At one time, indeed, such a reliable antiquary as Canon Jackson,* of Salisbury, believed in this ingenious invention of the highly-imaginative Stukeley. M. de Cambry would have the lines of Carnac to form an enormous astronomical calender ("un thême céleste, un zodiaque"), each line representing a sign, of which the ancient Gauls only (according to this theorist) recognised eleven; whilst others would have these and similar structures to be connected with astronomical observations; take, for instance, the following work published at Salisbury in 1771, entitled "Choir Gaur; the Grand Orrery of the Ancient Druids commonly called Stonehenge, astronomically explained and mathematically proved to be a Temple erected in the earliest ages for observing the motions of the Heavenly Bodies." Another astronomer (?), *anonymous*, by abstruse calculations backwards, ascertained that about 2000 years ago an occultation of one of the planets (also anonymous), must have taken place at such a point in the heavens as would have enabled

* "In England of course attempts to solve the riddle of Carnac have not been lacking. One which has attracted much attention and support is, that it was a temple in the form of a serpent—a kind of building which (so the propounders of this doctrine told us) "the serpent worshippers or 'Ophites' used to construct, and to which they gave the name of 'Dracontium.'" A great deal of ingenuity and learning has been brought to bear upon this theory. I myself, "*faute de mieux*," used rather to acquiesce in it, depending wholly and entirely, as I did, upon the deliberate statements of its champions, that such structures *were made*, and that "*the ancients gave to them the name of Dracontium.*" Having never met in the course of my own limited classical reading with any thing or name of the kind, and beginning to wonder where any notice of it was to be found, I consulted one of the first Greek scholars of our day. He shook his head, and added that a Greek word with that meaning was to him unknown. I ransacked lexicon after lexicon, but no "serpent-temple," called by the ancients a Dracontium, was to be found. On further investigation it came to light that the word "Dracontium" was actually coined by an ingenious but rather extravagant antiquary, Dr. Stukely, as a name very suitable and convenient for a thing, which thing was also a creation of his own brain. Upon making this discovery I took leave of the Ophites.—Notes and Queries, 4th Series, Vol. iv., p. 3.

an observer to view it through the celebrated cross stones of Stonehenge; his theory being that those cross stones were purposely so placed to fix the point of observation permanently, so that astronomers in after ages might be able to compare notes in their observations. Still later, Dr. Thurnam watched the rising of the sun from the altar stone where he stood, when it was seen to rise precisely over the top of the isolated stone, which is 10 feet high and about 200 feet distant from the entrance to the temple, apparently intended to direct the observation at the summer solstice to the point of the rising sun.

Latour d'Auvergne (le Premier Grenadier de France), in his "Origines Gauloises," was one of the first to refer these stones to a Celtic origin, in which supposition he is supported by Le Comte de Caylus, as well as by MM. Pelloutier, Mahé, and Déric. M. de Freminville, in company with Messrs. de Robien Borlase and Pallas, is inclined to the belief that these amorpholiths are the tombs of warriors slain in some memorable victory of exceptional consequence, perhaps erected by the Veneti (whose capital, Dariorig, is identical with Locmariaquer across the Crach river), and the isolated menhirs are supposed to indicate the graves of those slain at the outposts. M. L. Galles, in a memoir of the Departmental Archæological Society, quotes old Alaus Magnus (Archbishop of Upsal in 1546), who gives examples of the five different arrangements of these stone alignments; the translation of the passage is as follows:—"As to the obelisks or huge stones erected by the hands of giants and athletes, nowhere are they to be found more frequently than among the Ostrogoths, Westrogoths, and northern Suconi, where two or three roads meet, and as well in vast solitudes, which have long since been depopulated by plague, famine, and war, and which have not yet been restored to their former state of cultivation through the negligence or apathy of the inhabitants. These stones, erected in many places, have a length of 10, 20, or 30 feet, and a breadth of 4 or 6 feet. Their position is wonderful, their order still more wondrous, and their character most prodigious. In lines straight and long, they represent the contests of athletes; arranged in square, the *mêlée* of battle; in circle, the sepulchre of families; whilst if the lines form an angle, they represent an army of horse and foot, which either there or close by has triumphed over its enemies." So also there are other inadmissible opinions, which would make these stones to be memorials of the defeat of the Veneti* by Cæsar, or a

* Mr. Fergusson's latest theory will be discussed in Part III.

cemetery of the same people, or a military trophy in honour of Hercules, or a grove of sacred oaks, and these great stones placed in lines like rows of trees. Mr. Yates has written to prove that these lines of stones are merely the result of agricultural operations by the clearance of natural boulders off the fields.

There are not wanting, either, some who will not believe that these stones were placed by the hand of man, but think that all such remains as these in Brittany, and others, as Avebury in Wiltshire, are so many freaks of nature, which, by glacial or other means, has transported these masses from a distance, and scattered them as moraines or erratic boulders.

One of the most ingenious theories is that suggested by Canon Jackson, of Salisbury, in "Notes and Queries," July, 1869. He supposes the original number of the stones at Carnac to have been eleven thousand, and intended as a great national memorial of the eleven thousand British ladies who were wrecked, and perished on the coast of Armorica, in the year of our Lord 381. Mr. Lukis disposes of this presumed key to the rusty old lock, by showing that the number eleven has been assigned to the rows of stones at Carnac by careless observation, and that there are three groups of stones—one of ten, another of eleven, and one of thirteen rows.

Mr. Lukis, who (with his father, brother, and in company with Sir Henry Dryden) has made the amorpholiths of Finistère and the Morbihan his especial study for many years, has established the principal characteristic feature of these systems of avenues and enclosures as follows :—(*Vide* "The Stone Avenues of Carnac," a paper read at the Blackmore Museum, Salisbury, at the meeting of the Wiltshire Archæological Society, 1870, by the Rev. W. C. Lukis, M.A., F.S.A., Wath Rectory, Ripon.)

1. "The lines do not lie strictly east and west, but vary a little to the north and south."

2. "The narrow end is invariably eastward, and the head or wide part is toward the west end, and on elevated ground."

Referring to these features further on, Mr. Lukis remarks—"There is a feature which is common both to groups of rows of stones and to the sepulchres, which may help to throw some light on the subject, viz., their orientation. By far the larger number of the sepulchral monuments—those, I mean, which are usually termed dolmens—have their entrances between the east and south points of the compass, *i.e.*, nearly ninety per cent are so turned, which, it must be admitted, cannot be an accidental circumstance. So, too, the avenues are similarly orientated. If, therefore, the builders of the tombs had a religious reason for this arrangement, the same motive must have been dominant in the minds of the constructors of the avenues; and the inference is not without force, that the same people erected both. This arrangement may be a token of their religious reverence for the deified orbs of heaven, the sun and moon." —(P. 13 of Mr. Lukis's paper). To this we would add that this *orientation*, which is attributed to all these monuments, as regards the dolmens and tumuli, is well substantiated; but on looking at Mr. Vicars's map of the Carnac district in "Rude Stone Monuments," and the position of the stone avenues, we cannot fail to remark that the lines of Erdeven, St. Barbe, and St. Pierre, are nearly at right angles to those of Menec, Kervario, and Kerlescant; and again, that whilst the prevailing point of the entrances of the dolmens is south-east, the general direction of the three Carnac groups of alignments is considerably north of east. (Compare the points of compass given in table of the alignments.)

3. "The stones are always largest at the western termination, and of small size in the other direction. In the Menec and Erdeven groups, however, the stones slightly increase in size towards their commencement."

4. "Where there are circles connected with the lines, they are always at the large 'end.'"

5. "The circles are composed of stones differing in form from those of the lines. They are thin and wide, and not so tall as the tallest of the lines, averaging about 5 feet above ground."

6. "The stones of the circles nearly touch each other, whereas those of the lines have spaces of from 7 to 20 feet between them."

7. "The average distance between the lines at the west end is 30 feet, at the east end 18 feet."

8. "In no case is there, strictly speaking, an attachment of the circle to the lines."

Further, it appears probable to Mr. Lukis, "that the number of the lines in each series was determined at first, and the whole number begun at once." The size of the stones indicates this. Again, he presumes "that they were begun at the west end. Probably in all cases the circles were added last, at least after the wider or west portion of the series had been erected; because at St. Pierre, Quiberon, the circle is 77 yards on the south side of the lines, at Menec the centre of the circle is south of the direction of the central avenue, and at Kerlescant it is a large segment, and not a complete circle."

We do not quite follow Mr. Lukis in the last observations; for does not the separation of the circle from the head of the lines at St. Pierre rather intimate a certain independence of the two monuments? or again, perhaps, indicate a bi-fold arrangement, similar somewhat to that previously noticed in this paper as occurring, either accidentally or intentionally, at Kerlescant? Supposing that eight additional lines ran easterly from the St. Pierre circle, we should have an almost parallel example to those at Kerlescant and Menec.

The plan of the Kerlescant monument is evidently the most complete example remaining to us, exhibiting, besides its remarkable symmetry of design, an intelligible ending or finish, viz., a series of avenues terminating in a circle, close alongside of a smaller but similar series leading up to a sepulchral barrow. Nowhere else do we find complete circles, tumuli, and lines associated together. We meet with lines without circles (see the table, p. cxxv.), although traces of circles may yet be discovered in connection with them, but seldom circles without lines. Two instances alone of these latter are given by Mr. Lukis, one on the Ile aux Moines, and the other on the Ile El-Lanic; but as the sea has encroached on the south-east side of this latter island, so as to have washed away a considerable portion of the circle itself, some of the stones composing it being yet visible below low-water mark, so, probably, there formerly existed avenues leading to it.

IV. THE PUBLIC HEALTH ACT, 1872.

THE Act of Parliament bearing the above title, and dated the 10th August, 1872, is mainly an Act for the division of the county into sanitary districts, for the constitution of certain sanitary authorities, and for the transference to those authorities of all existing powers and obligations of a sanitary nature.

Clause 51 of this Act enacts that "every sanitary authority shall have power to direct the destruction of any bedding, clothing, or other articles which have been exposed to infection from any infectious disorder, and to give compensation for the same." With this solitary exception, the Act gives no explicit direction as to how the sanitary authorities are to conserve the health of the nation. In this respect it differs from the original Bill as first brought into Parliament. That, as will be remembered, contained a multitude of very minute, and in many respects arbitrary, clauses relative to the pollution of rivers. These clauses, which embodied the recommendations of the Royal Commission on the Pollution of Rivers, were abandoned by the Government as being altogether impracticable, and it is understood that the Government is about to make an end to the investigations of the Royal Commission, which have proved of so little utility to the state.

We will endeavour briefly to describe the provisions of the Public Health Act, which has just become the law of the land.

First, it does not apply to Scotland, nor to Ireland, nor to London. It applies exclusively to England outside the Metropolis.

All England, with the exception of the capital, is divided into sanitary districts, and these sanitary districts are of two kinds, viz., urban and rural. This Act constitutes *local sanitary authorities* in each of these districts, bearing the official title, "Urban Sanitary Authorities," or "Rural Sanitary Authorities." The Mayor, Aldermen, and Burgesses of a borough, or the Improvement Commissioners of a district, or the Local Board of a district, are constituted the urban sanitary authorities. In like manner the guardians of a rural union are constituted the rural sanitary authorities in rural sanitary districts.

It will thus be seen that this Act creates no new official body. It takes existing official bodies—the town councils of towns, and the poor law guardians of country places, and erects them into urban or rural sanitary authorities. The department of the central government of the country which is to preside over and regulate the action of the *local sanitary authorities* is the Local Government Board. Having fixed the sanitary authorities in the different districts, the Act proceeds to clothe them with authority, and to destroy that which was previously in existence, and which clashes with them. To these sanitary authorities revert all powers and duties exercised under the Local Government Acts, the Sewage Utilisation Acts, the Nuisances Removal Acts, the Common Lodging Houses Acts, the Artizans' and Labourers' Dwellings Act, the Diseases Prevention Act, and the Bakehouse Regulation Act.

The fourth section of the Artizans' and Labourers'

Dwellings Act, 1868, and the fourth section of the Sanitary Act, 1866, are repealed. The working of the "Alkali Act, 1863, and of the Metropolis Water Acts, 1852 and 1871, are confided to the Local Government Board (which, as we have said, presides over the sanitary authorities) by Clause 35 of the Public Health Act.

There are various enactments respecting the holding and acquisition of property by sanitary authorities, and respecting alterations of the areas occupied by sanitary authorities. These enactments, however, need not occupy our attention.

Clause 10 is very important. It enjoins on every sanitary authority the appointment of a legally qualified medical practitioner as medical officer of health in its district. It is further enacted that such appointment shall be made for a period not exceeding five years.

Clause 34 enacts that "the approval of the Local Government Board, and not that of one of Her Majesty's Principal Secretaries of State, shall be required for the appointment and removal of analysts under an act of the session, holden in the twenty-third and twenty-fourth years of the reign of Her Majesty, intituled 'An Act for preventing the Adulteration of Articles of Food or Drink.'"

The former of these enactments would seem to place the officer of health too much under local influence. It is most desirable, however, that this should not be so. If the medical officer of health or the chemist does his duty efficiently, he will often incur the opposition of wealthy persons who are local sanitary authorities. He ought by all means to be independent of them.

Intimately connected with the independence of these officers is the amount of salary which they are to receive. The duties of the medical officer of health are hardly compatible with his continuing to practise; and his salary ought, therefore, to compensate him for the abandonment of practice. It would, indeed, not be unwise, if the abandonment of active medical practice were made a condition of acceptance of office as a health officer under the Public Health Act.

It appears that nothing of this sort is in contemplation, but that the health officer is to be underpaid, and left to eke out his income as best he may. Under these circumstances, something like the following state of affairs may be expected to arise. A few high-minded men, who happen to be selected as health officers, will prefer what they deem to be the public interest, and sacrifice their own private advantage to it. These will suffer a species of martyrdom.

Others, again, of an opposite cast of mind, will manipulate their office so as to extend and develope their practice, sacrificing, if need be, every public consideration to that end. The vast majority, however, will simply recognise the fact, that to discharge the duties which are thrust upon them is impracticable, and will make no effort to do so.

This aspect of the case has, naturally enough, been discussed in medical circles. In particular, the British Medical Association has directed attention to it, and has pleaded for an allowance being made by the Chancellor of the Exchequer, in order to admit of adequate payment of the medical officers of health.

The Public Health Act, 1872, is admittedly only the prelude to an adequate Public Health Act. Let us hope that its successor will, at any rate, provide for the efficiency of the health officers and analysts.

V. ARTIFICIAL FLIGHT: AN AËRIAL SHIP.

IN continuing the record of what has been done, at home and abroad, to subjugate to man the dominions of the air, we must give attention to the most perfect of aërial machines yet constructed—the aërostat of M. Dupuy de Lôme. We have first to consider the extended study of the subject, in order to ascertain what had been done by others, and the causes of their failure, partially or in totality; this, too, with the necessity of collating the data from which conclusions might be drawn. The conception may be viewed as perfected, as a means to an end, during the siege of Paris, when many schemes of communication between the besieged and those interested in their trials were attempted. Out of many, the machine we are about to describe is that only in which the difficulties presented by an insufficiency of principle in the science of aërostation have been met. It is now our object to show in what manner these difficulties have been overcome.

Hydrogen gas was employed, and was obtained by the action of sulphuric acid and water on iron-turnings, the gas being afterwards washed and dried. The gas so obtained was calculated to exert an ascensional force of 735 to 1120 grms. per cubic metre, under an atmospheric pressure of 760 m.m. at the ordinary temperature. M. Troost, Professor at the Ecole Normale Supérieure, invented a varnish for the taffetas covering of the balloon which was found to be quite impervious to water. This varnish has gelatine for its base, and is prepared from two solutions, termed A and B respectively. A consists of 1 part of pure gelatine, 1 part of glycerine, 6 parts of pyroligneous acid. B consists of 1 part of tannin, and pyroligneous acid 6 parts. The glycerine is dissolved in the pyroligneous acid by the aid of gentle heat, and while warm A is poured into B, the mixture being stirred with a wooden spatula. The mixture is re-heated in an open vessel for some time, acid being added as that in the mixture is evaporated. The balloon consists of white silk taffeta, lined with india-rubber, and again with nanzouk; and to the nanzouk lining the composition is applied.

M. de Lôme's attempt, although so eminently successful, cannot be characterised as ambitious, because, instead of depending upon speed for the hold of the machine upon the air, the object was the very modest one of giving to a balloon, of the best shape and dimensions, a velocity of about 8 kilometres with regard to the surrounding air. This being attained the same aërostat would, of course, when impelled by the wind, move at a more rapid rate; thus, with a fresh breeze, at the rate of 4 metres per second, the aërostat would be perfectly under command within an angle of 33° on each side of the wind's plane, or within a sector of 66°. And if we double the rate of movement of the wind, we must inversely halve the angles within which the aërostat may be considered safely under control. That the plane of movement shall be directly under the control of the aëronaut requires that much less resistance should be presented to the air than is the case with the ordinary balloon, and careful calculation led to the following dimensions:—Length, 36·12 metres (118 ft. 6 in.); diameter at centre, 14·84 metres (48 ft. 8 in.); the area of section through the centre being 172·96 square metres (1862 square feet), and the volume 3454 cubic metres (121,983 cubic feet). M. de Lôme, in his original paper, shows that to obtain a speed of 8 kilos. (about 5 miles per hour) it is necessary to maintain a motive power equivalent to 30 kilogrammetres (or 217 foot-pounds) per second, this motive power being preferably obtained from manual labour utilised in rotating a shaft to which is attached a two-bladed screw. In this case, if we take R to represent the resistance of the atmosphere at a speed, s, of the balloon in seconds, d the diameter of the screw, p the pitch of the screw, and n the number of revolutions per minute, then

$$\frac{pn}{60} = (1+a)s;$$

and the work of the screw per second will be—

$$R\frac{pn}{60} = (1+a)RS.$$

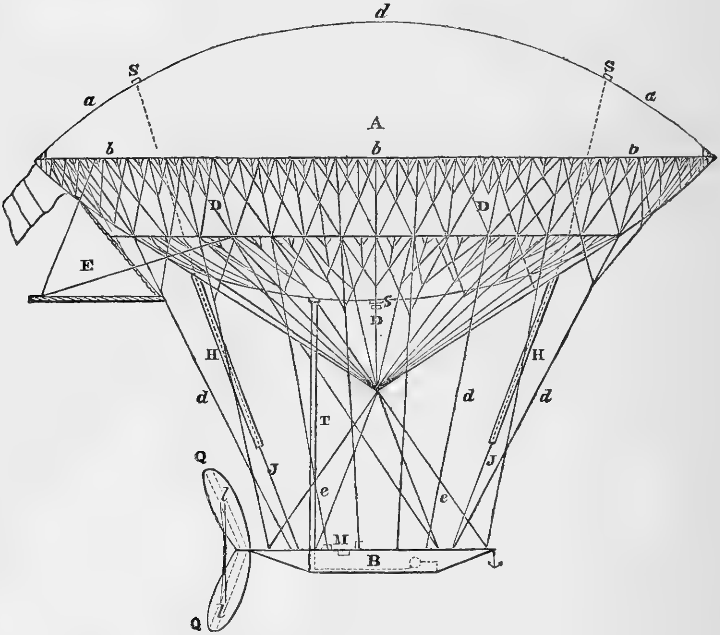
Consequently the amount of work required to move the aërostat is the last quantity plus the amount lost as friction. The numerical examples quoted by M. Emile Leclert, Architect of the French Navy, in a recent admirable article contributed to "Naval Science," will here suit our purpose. He shows that the resistance of a thin plane being taken equal to 0·665 kilogramme per square metre (14 lbs. per square foot) for a speed of 2·22 metres (7·25 feet), the resistance of the balloon alone may be estimated at 6 kilogrammes (1·23 lbs. per square foot), and of the entire machine at 11 kilos. (2·25 lbs. per square foot). With the values $d = 9$ metres ($29\frac{1}{2}$ feet), $p = 8$ metres ($26\frac{1}{4}$ feet), fraction of speed of each blade at the extremity 1-16th, and at the centre of action 1-10th, the theory of the screw leads to the conclusion that $a = 0{\cdot}26$. From this conclusion values are obtained for n and T, giving as a result that for a speed of 2·22 metres per second, or 8 kilometres (5 English miles) per hour, the labour of 4 men in turning the winch is necessary, and 8 men for a speed of 2·22 metres $+ \sqrt[3]{2} = 2{\cdot}80$ metres per second, or 10 kilometres or $6\frac{1}{4}$ miles per hour. These numerical results are quite opposed to the conditions required by Navier's theory,* being a much smaller number of men.

M. Dupuy de Lôme has found that a balloon to be successfully navigated must always be maintained at an equal degree of inflation, in order that the resistance to which the balloon is exposed in its passage through the air should remain constant, and capable at any moment of being defined. The balloon, at starting, being inflated fully with hydrogen, the constant degree of inflation is preserved by means of the hanging-tubes, H H, Fig. 2. These tubes have the ends open, and are pendant about 25 feet below the balloon. As the gas expands it forces itself down these tubes, while its own pressure in the tube reacts upon the body of gas in the balloon, preserving such an excess of interior pressure as prevents the shape of the outer covering being altered by the force of the wind. Still further to maintain a constant surface there is provided a small internal balloon (termed a *ballonnet*), which, as the gas escapes, through diminution of

* Memoires de l'Institut de France, vol. xi.

pressure, from the primary balloon, can be filled with air. As the gas expands in the larger balloon it would be forced out of the pendant tubes, were it not that a valve, opening at a low pressure, is attached to the ballonnet. The

FIG. 1.



FIGS. 1, 2, 3. A, the balloon; B, the car, with D, the net-work; *a a*, taffetas covering; *b b*, collar attaching the upper netting to the covering of balloon; *d d*, silken ropes suspending the car; *e e*, balance ropes for the car; *f*, small internal balloon; *g g*, line of intersection of the surface of the balloon with that of small internal balloon; E, gaff-sail, or rudder; H, pendant tubes, the length of which regulate height of the column of hydrogen; J, the cords regulating the valves S; T, tube for filling small balloon with air; M, crank for working the screw, Q; *l*, stays, strengthening the screw.

ultimate proportions of the aërostat, as given by M. Dupuy de Lôme, are—

Height from top of balloon to keel of car = 29·12 metres (95½ feet).

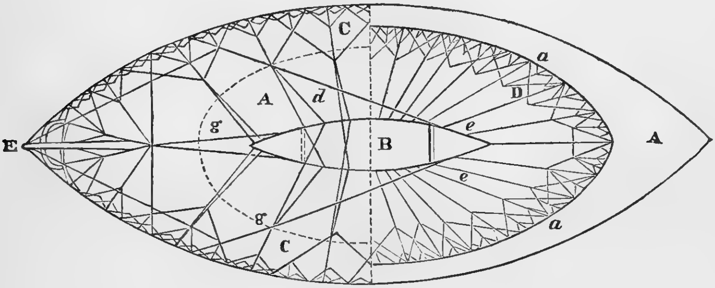
Distance between screw shaft and major axis of balloon = 20·45 metres (67·1 feet).

Distance of major axis from centre of gravity of complete machine (without ballast) = 15·54 metres (51 feet).

The rudder is a triangular sail of 15 square metres (161½ square feet) area, manipulated by cords from the car.

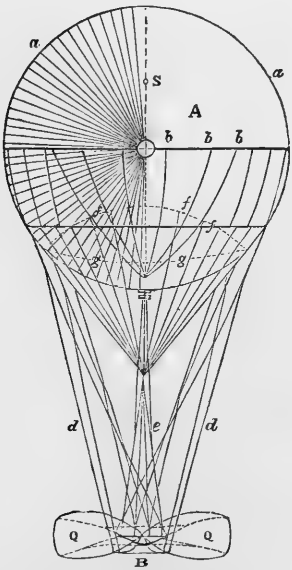
We come now to the description of the journey actually undertaken in this machine, premising that instances of so complete a fulfilment of calculation are very rarely occurrent.

FIG. 2.



The ascent took place from the Fort Neuf of Vincennes. The crew consisted of 14 men, with baggage and provisions weighing 1·13 tons. There were on board MM. Dupuy de

FIG. 3.



Lôme, Zédé (Ingénieur de la Marine), Yon, and Dartois, aëronauts. The instruments weighed 1·75 tons, and there were 0·27 ton of packages to be carried to the destination of the balloon. The total weight, with 0·59 ton of available ballast, amounted to 3·74 tons, and the balloon, when thus ballasted, had an ascensional force sufficient to keep it in equilibrium close to the ground. In the first ascent 3 cwts. of ballast were thrown out, the balloon rising from the earth on February 2, 1872, at 1 o'clock. From 1 o'clock to about a quarter past but little more was done than to admire the graceful evolutions of the machine, and the readiness with which it answered to both helm and screw. At 1 h. 15 m. observations were commenced, and showed the car to be 560 metres (1837 feet) above the departure station, and moving in a north-easterly direction with a speed of 12 metres (39 feet) per second. The course was then altered to the south-east, at an angle of 83°, with the former direction. The number of men working the screw, at 25 revolutions per minute, was eight, the aërostat moving with regard to the earth at a speed of 16 metres (or 52·5 feet) per second. Afterwards this speed increased, with 27·5 revolutions of the screw per minute, to 17 metres (55·8 feet) per second. The speeds given by the form of anemometer employed, as due to the balloon, or rather to the screw, were 2·35 metres (7·7 feet) to 2·82 metres (9·3 feet). The descent was commenced at 2 h. 35 m., and was effected at the destination, Mondecourt, near Noyon, without any shock or the slightest accident.

We should now consider the results that have been attained in the experiments with the aërostat. They are—the maintenance of a constant exterior surface by means of the ballonnet; freedom from rocking motion, even while two or three persons are walking in the car; and a perfect control, the head of the aërostat being shifted to or kept in any direction, with a maximum force of 60 kilos. from the manual labour of the eight men.

These are the mechanical improvements that have been achieved, but the most important result is that an impetus will be given to the study of aërial navigation now that the science has found a theory seldom paralleled in its application. The remark of one of our greatest men, "Impossible; I don't know the word," has indeed been practically shown to be an admitted principle by M. de Lôme.

M. de Lôme further proposes to remove seven of the eight men employed to work the screw, and substitute an engine of eight-horse power, with one man as engineer. The ballast would then consist of the fuel and water, while the aërostat could be impelled at the rate of 14 miles per hour, at a much larger angle, with the plane of direction of the wind. There has thus now been opened to us a new path in the science of aërostation, and it is difficult to limit the imagination to those new wonders we may expect within even a few more years.

VI. PAPER AT THE INTERNATIONAL EXHIBITION.

By F. C. Danvers, Assoc. Inst. C.E.

THE International Exhibition of 1872 is the second of the series of annual International Exhibitions held at South Kensington. These Exhibitions, which will henceforth take the place of their larger prototypes, so far at least as this country is concerned, shall, it is intended, be opened each year for the exhibition of special industries, each in their turn. Last year, the classes to which the Exhibition was confined were mainly three—the fine arts, pottery, and woollen and worsted fabrics. This year, again, it also consists of three main divisions—fine arts, manufactures of cotton and cotton fabrics, articles of jewellery worn as personal ornaments, paper and stationery. Printing also occupies a prominent position, whilst musical instruments of all kinds and acoustic apparatus are also represented. The third division consists of scientific inventions and new discoveries.

A notice of the principal exhibits classed under the third division appeared in the last number of the "Quarterly Journal of Science," and we shall not, therefore, refer further to them now. The fine arts division we shall also upon the present occasion leave unnoticed; and as within the limits of a single article it would be impossible to do justice to all the manufactures classified, we purpose to confine ourselves to those which occupy the most prominent part of the Exhibition, namely, cotton and cotton fabrics (so far as they may be said to be connected with paper manufacture), paper, and stationery.

In respect of the exhibits which form the subject of our present article, it cannot be said that there is a very satisfactory show, foreign countries especially being hardly, if at all, represented by specimens of paper manufacture. This subject has been very much neglected on former occasions, there having been only eleven exhibitors from Great Britain in both the Exhibitions of 1851 and 1862. Nevertheless, it cannot be denied that paper forms a most important industry, especially in this country, and is capable of being applied to a number of purposes with great advantage, where other and more expensive materials are now principally used. In the present article we shall not, therefore, strictly confine ourselves to a notice of what is presented to the public eye at this year's International Exhibition, but we propose to extend our review so far as to notice other uses of paper which are not shown there, and, as a fitting introduction to our subject, a brief history of paper manufacture may well precede our further remarks on the subject.

Without going back so far as to the earliest periods, when the only means of recording events were by engraving in clay, stone, and metals, which were used by the ancients for all matters of public notoriety; and passing by the subsequent use of thin slices of wood, and skins of animals for similar purposes, we shall come at once to consider the use of those materials whence we may trace the origin of paper manufacture from vegetable substances. Thus, then, it would appear that the use of boards was in some measure superseded by that of the bark and leaves of certain trees, and it is from the latter that the first paper was manufactured. The plant whose leaves were mostly used for writing on was the *Papyrus*, a kind of large rush, which grew upon the banks of the Nile. At a very early period the Egyptians appear to have manufactured a species of paper from its leaves, or, as some suppose, from the stock of the plant. It is uncertain when the use of the *Papyrus* leaf was superseded by paper made from it; but Egypt enjoys the honour of having invented the process, and Isidore even fixes the locality of its first manufacture at Memphis. Varo, the Roman, ascribes the date to the time of Alexander the Great, after the founding of Alexandria; but Pliny quotes a passage from the writings of Cassius Hemina, an ancient annalist, in which he speaks of some books found in the tomb of Numa, when it was opened 535 years after his decease, and asserts that these books were of *paper*, and had been interred with him. As Numa preceded Alexander by 300 years, this circumstance, if admitted, would carry back the date of the invention anterior to that time. Dr. Gill, in his Commentary, says, "On the banks of the Nile grew a reed, or rush, called by the Greeks *papyrus*, or *byblus*, from whence come the words *paper* and *bible*, or book, of which paper was anciently made, even as early as the time of Isaiah." This, then, if correct, would take the manufacture back to a still earlier date.

Egypt long had the monopoly of the manufacture of papyrus paper, and at the time of Augustus the trade was considerable, not only at Rome, but throughout the world. In the ninth century, the use of papyrus made in Egypt ceased in Europe, and it was replaced, almost universally, by paper made of cotton from the East.

It is a curious circumstance, that there is at the present day no plant known to botanists which corresponds exactly with the papyrus of ancient times. In the prophecy of Isaiah a very remarkable prediction occurs with reference to this plant, in the following words:—"The paper reeds by the brooks, by the mouth of the brooks, and everything sown by the brooks, shall wither, be driven away, and be no more." So far as the papyrus plant is concerned, that prophecy appears to have received an actual fulfilment.

The art of making paper from vegetable fibre, reduced in water to a pulp, is said to have originated in China. The oldest known records in that country are inscribed on very thin slips of bamboo, carefully dried by artificial heat, and many of these are preserved in the pagodas. At a later period the Chinese made use of a peculiar kind of silk, called silk paper; and finally, both were replaced by an inventor named Tsaï-lun, who made use of the bark of certain trees, hemp, rags, old nets, &c., which he boiled down to a pulp, and thus produced paper proper. According to different authorities, this invention dates from between A.D. 89 and 105, whilst by another it is stated that the results of this invention were presented to the Emperor of China about the year A.D. 153.

The manufacture of paper from the paper mulberry (*Broussonetia papyrifera*) was introduced into Japan about A.D. 610. Up to the year A.D. 280, silk, with a facing of linen, was used for writing upon, and thin wood shavings were also employed. In that year, however, paper was imported from the Corea; and this appears to have been the only paper known to the Japanese until the year 610, when two priests named Douchô and Hâjô were sent over to Japan by the King of the Corea. Douchô is said to have been a clever man, learned in the Chinese classics, and, moreover, a skilful artist. Besides the manufacture of paper, he also introduced that of writing-ink and millstones into the country. Although the paper made by Douchô was very good of its kind, it did not take ink well, and had, besides, other drawbacks. In consequence of this, Shôtoki Taishi, a son of the reigning Mikado, who had learned paper-making of Douchô, introduced the manufacture of paper out of the paper mulberry, which plant he caused to be extensively planted all over the country, and the mode of paper manufacture to be largely promulgated among the people.

Some time in the seventh century the art of paper-making became also known to the Persians, and about the year 700 it passed into Arabia. The Arabs carried the knowledge into Europe in the earlier half of the twelfth century, and established a paper manufactory in Spain. Upon the authority of Edresi, who wrote in 1150, a paper was then made at Xativa, an ancient city of Valencia, equalled by none produced elsewhere, and which was extensively exported to the East and West. From Spain the art extended into France, in which country, as well as in Italy, there existed paper factories at the commencement of the fourteenth century. In Germany, too, a paper factory was established at Nuremberg, in 1390, by Ulman Stromer, who also wrote the first work ever published on the art of paper-making.

Paper made from cotton became general at the close of the twelfth and the beginning of the thirteenth centuries, but in the fourteenth century it was almost entirely superseded by paper made of hemp and linen rags. English manuscripts on linen paper date as early as 1340, but it is believed that the manufacture did not exist here until the end of the fifteenth century: indeed, the earliest trace of the manufacture in this country occurs in the *Bartolomæus* of Wynkyn de Worde, printed in 1496 by Caxton, in which it is said of John Tate, jun., whose mills were at Stevenage, Hertfordshire,—

> "Which late hathe in England doo make thys paper thynne
> That now in our Englyssh thys booke is prynted inne."

In 1858 Sir John Spielman, a German, established a paper mill at Dartford, for which the honour of knighthood was conferred upon him by Queen Elizabeth, who was also pleased to grant him a license "for the sole gathering for ten years of all rags, &c., necessary for the making of such paper."

It is recorded in the "Craftsman," that William the Third granted the Huguenots, who brought with them many of the improvements which France had introduced into the manufacture, a patent for establishing paper manufactories, and that Parliament likewise granted them other privileges.

For a long time England imported almost all its paper from France. Although the manufacture was introduced at the end of the fifteenth century, scarcely any but wrapping-paper was made in this country before the Revolution, but the manufacture afterwards advanced so rapidly, that by 1760 Britain was almost wholly independent of foreign supply. In that year James Whatman, who had learned the art of paper-making whilst travelling in the suite of the British ambassador to Holland, where the best papers were then made, established a paper factory at Maidstone. Not long before this wove moulds had been invented by Baskerville, to obviate the usual roughness of laid paper.

Paper was made entirely by hand until about the year 1803, when M. Didot, a Frenchman, brought over to this country a model of a continuous machine, which was taken up by Messrs. Fourdrinier, at that time the principal stationers and paper manufacturers in Great Britain, and it was perfected and manufactured by Mr. Bryan Donkin, whose firm are, at the present day, the principal manufacturers of paper machinery in the United Kingdom. In 1809 Mr. Dickinson, a paper maker, invented another method of making endless paper; and in 1826 M. Canson, of Annonay, first applied suction-pumps to the Didot and Fourdrinier machines.

The principle of paper-making machinery is simply this: instead of employing moulds and felts of limited dimensions, as was originally the practice, the peculiar merit of the invention consists in the adaptation of an endless wire gauze to receive the paper pulp, and again an endless felt, to which in progress the paper is transferred, and thus, by a marvellously delicate adjustment, while the wire at one end receives a constant flow of liquid pulp, in the course of two or three minutes there comes out at the other end of the machine a continuous length of paper carefully wound upon a roller.

Having thus briefly recorded the history of paper manufacture, we pass on now to a consideration of the different materials from which it is made; and, first of all, we will refer to the raw products, as shown in the International Exhibition. First of all, then, we must notice the cotton exhibits, as, so far as we at present know, it was from the raw cotton that paper was first manufactured, and, as has been already stated, this manufacture had its origin in China.

Various specimens of the cotton plant are exhibited, growing in pots, with their cotton pods fully developed, in a separate building or conservatory, called the "Cotton-growing House," in the west grounds of the Exhibition building, where many varieties of Indian, Egyptian, American, Chinese, and other cottons may be studied in their natural state. In Room I. in the west galleries, close by, the further development of cotton through the various stages of ginning, blowing, making into laps, carding, breaking, finishing, drawing, slubbing, roving, the mule, and weaving-looms, are all successively shown, most of the machines being exhibited in actual work, whilst cases along the side walls contain specimens of almost all the known species of cotton in their raw state, accompanied, in some instances, with specimens of the manufactured article, in all the various stages through which it passes before arriving in the perfect state in what it is known as "cotton goods." The seeds of some species of cotton, after ginning, have a certain quantity of short cotton fibre adhering to them. This is carefully cleaned, and the "cotton waste" so obtained used for paper manufacture. The other principal materials employed in the production of paper in the various provinces of China are hemp, the young shoots of the bamboo, the mulberry-tree, the rattan, sea-weed, rice and wheat straw, silk cocoons, the bark of the *Broussonetia papyrifera*, and the pith of the *Aralia papyrifera*, from which the celebrated Chinese rice-paper is made. In Japan, the principal plant from which paper is manufactured is the *Broussonetia papyrifera*, or paper mulberry. Another plant, known as the *Toroto*, which grows not unlike the cotton plant, yields a paper fibre from its root, but neither the flower nor the seed are of any use for the purpose. There are also the *Makoso*, or paper plant, the *Kajiso*, and the *takaso*, used for paper of an inferior quality.

The following is a list of the Indian fibres used for paper-making, samples of which are all exhibited together with, in most cases, samples of paper made from them.

Punjab Flax, *Linum usitatissimum.*
Rheea, *Bœhmeria nivea.*
Puya Bark, *Bœhmeria puya.*
Puya Fibre, *Bœhmeria puya.*
Nilgiri Nettle, *Urtica heterophylla.*
Mudar, *Calotropsis gigantea.*
Bedolee Sutta, *Pæderia fœtida.*
Jute, *Corchorus olitorius.*
Sufet Bariala, *Sida rhomboidea.*
Ambaree, *Hibiscus cannabinus.*
Roselle, *Hibiscus sabdariffa.*
Indian Mallow, *Abutilon indicum.*
Bun Okra, *Urena lobata.*
Himalayan Hemp, *Cannabis sativa.*
Sunn Hemp, *Crotalaria juncea.*
Jubbulpore Hemp, *Crotalaria tenuifolia.*
Dunchee, *Sesbania aculeata.*
Pine Apple Fibre, *Ananassa sativa.*
Marool, *Sanseviera zeylamca.*
Agave, *Agave americana.*
Great Aloe, *Fourcroya gigantea.*
Adam's nettle, *Yucca gloriosa.*
Plantain, *Musa paradisiaca.*
Screw Pine, *Pandanus odoratissimus.*
Red Bast, from Pegu.
Shaw Nee, Shaw Young, Shaw Laybway, and That Pootnet Shaw, all from Burmah.
Nepal Paper Shrub, *Daphne cannabina.*

Besides the above, there are also exhibited samples of Indian paper made from gunny bags, refuse gunny, fishing nets, old records, and bamboo.

From the Report of the New York Industrial Exhibition of 1853, it appears that the materials principally used in America for paper-making are raw cotton and cotton waste. Linen rags are imported from Europe, but the principal consumption would appear to be cotton, either as above named or in rags. Sir William Hooker, in his Report on Vegetable Products at the Paris Exhibition of 1855, gives an exhaustive list of fibres produced in different countries, many of which are used for paper-making. It would occupy too much space to insert their names here, and we have therefore contented ourselves with pointing out where such information may be obtained.

The Indian, Chinese, and Japanese are by far the most perfect collections of paper-making materials in the Exhibition, and we have therefore given greater prominence to the products of those countries. Queensland shows a specimen of the *Sida retusa*, an indigenous weed, the fibre of which is used as a substitute for flax, and the refuse makes pulp for paper. Many of the exhibitors show specimens of the Esparto grass, both Spanish and Algerian, from which paper is now largely made, also of straw and straw pulps, as well as of pine wood. One case exhibited by F. B. Houghton shows all the different stages through which pine wood passes from the rough block to paper pulp; whilst another exhibitor, P. L. Simmonds, shows the various processes for reducing the waste from cotton seeds, and rough pine wood and bamboo, to the consistency of paper pulp.

In passing now from a consideration of the different raw materials used in paper-making, the next step is to review briefly the various processes employed in paper manufacture. Up to a certain point these processes necessarily vary with different materials beyond which the same kind of machinery may be employed. According to the nature of the raw materials different treatments must be observed in reducing them to a pulp, but after that stage has been reached the remainder of the manufacture may be generally said to be the same. We shall, therefore, first briefly notice the machine for manufacturing the pulp into paper, and then give a short account of how some of the raw materials are first reduced to a state of pulp.

There are two excellent models of paper-making machines respectively on scales of $\frac{1}{8}$th and $\frac{1}{4}$th full size, exhibited by

Messrs. B. Donkin and Co., of London, and by Messrs. G. Bertram and Co., of Edinburgh.

The pulp, when sufficiently ground, descends into a reservoir, in which it is kept constantly agitated in water. From this reservoir the pulp passes into a trough, where it is strained by means of a sieve, or "knotter," as it is called, the under part of which communicates with an exhaust pump in order to facilitate the passage of the pulp through the fine meshes of the "knotter." Passing from the strainer, the pulp is next distributed equally throughout the entire width of the machine, and is afterwards allowed to flow over a lip or ledge in a regular and even stream, whence it is received by the upper surface of an endless woven wire band, upon which the first process of manufacture takes place. This wire band, as it travels forward, has also a gentle vibratory motion given to it in order to assist the pulp to spread evenly over its surface, and to facilitate the separation of the water, which latter is also further aided by the action of a suction pump beneath, and thus the pulp solidifies as it advances. The width of the paper is regulated by deckle or boundary straps, which travel at the same rate as the wire gauze, and so limit the spread of the pulp.

The partially solidified pulp now passes under the "dandy" roller, which is employed to give any impression to the paper that may be required, and in forming what is called the "watermark," to which we shall presently refer more fully. The paper then passes under two "couching" rollers, which are simply wooden rollers covered with felt. Merging from these the paper is received from the wire gauze by a continuous felt, which conducts it through two pairs of pressing rollers, and afterwards to the drying cylinders, which are heated internally with steam. The paper, after passing over these cylinders, is finally wound upon a reel. For the finer sorts of paper, and especially for writing paper, a subsequent process of sizing must be undergone, which in most cases is effected continuously with the manufacture of the paper, and before it is finally wound.

To convey some idea of the number of substances which have been really tried for paper making, it may be stated that in the library of the British Museum may be seen a book, printed in low Dutch, containing upwards of sixty specimens of paper, made of different materials, the result of one man's experiments alone, so far back as the year 1772. In fact, almost every species of tough fibrous vegetable, and even animal substance, has at one time or another been tried. Cotton in its raw state requires far less preparation than a strong hempen fabric, and thus, to meet the requirements of the paper-maker, rags are classed under different denominations, as, for instance, *Fines, Seconds, Thirds* (composed of fustians, corduroy, and similar fabrics), and *Stamps* on *Prints,* as they are termed by the paper-maker, which are coloured rags, distinguished by certain well-known marks, indicating their various peculiarities. Samples of these may all be seen in cases exhibited by Messrs. John Dickinson and Co., and Messrs. T. H. Saunders, together with specimens of the different chemicals used in paper manufacture, and of the various colouring matters employed.

The first process necessary is to examine the rags, open all seams, remove dirt, pins, needles, buttons, &c., which would be liable to injure the machinery or to damage the paper. The rags are then cut in small pieces, mostly by hand, and sorted according to their quality. A machine for cutting rags, jute, hemp, &c., is exhibited by Messrs. B. Donkin and Co., and when this is used, sorting the rags necessarily precedes the operation of cutting.

After cutting the rags are removed to a dusting machine, in which all the dust and dirt contained in them is knocked out. They are then boiled in an alkaline ley or solution, made more or less strong as the rags are more or less coloured, the object being to get rid of the remaining dirt and some of the colouring matter. The mode of performing this is by placing the rags in large cylinders, which are constantly, though slowly, revolving, thus causing the rags to be as frequently turned over, and into which a jet of steam is cast. A large spherical boiler used for this purpose is shown by Messrs. B. Donkin and Co. The rags now pass into the comminuting machine, in which they are ground into pulp, and afterwards the mass is conducted into another engine, where, if necessary, it is bleached by an admixture of chloride of lime. The pulp is then let down into large cisterns to steep, prior to being reduced to a suitable consistency by a heating engine, after which it is conducted to the paper-making machine, as has been already described.

Before passing on to notice various uses to which paper is applied, a short space may be advantageously devoted to giving an account of some special manufactures. For this purpose we shall confine ourselves to a few only of the most interesting.

The well-known rice-paper of the Chinese is obtained from the pith of the *Aralia papyrifera,* a plant allied to the

Artocarpus, or bread-fruit tree. The pith is carefully taken out, cut into sheets, and smoothed with an iron preparatory to use.

The most important paper made in China is that from the bamboo. In the month of June the bamboos, which are on the point of producing new shoots, are cut down and divided into lengths of 6 or 7 feet each. These pieces are placed in a pit dug in the earth, and kept full of water, and remain there for one hundred days or more; they are then beaten with a mallet to remove the green bark which covers their surface. The bamboo, thus prepared, is boiled in a large wooden vessel full of water containing slaked lime, and placed within a metal boiler. The fire is generally kept up for eight days, and at the expiration of that time the fibres are taken out and carefully washed; they are afterwards plunged in a ley made from wood ashes, and then laid in a boiler covered an inch thick with ashes from burnt rice-straw, when water is added, and the whole is boiled; these last operations are repeated in rotation during ten days. By this time the fibres begin to rot, and they are then pounded in large mortars, the stampers or pestles being generally moved by water-power in a very simple manner. When reduced to pulp, it is placed in vats, and a liquid added, which is supposed to contain chlorine to whiten the mass. The sheets of paper are formed in the same manner as hand-made paper in Europe, but the frame is composed of woven fibres of the bamboo instead of wire; when formed the sheets are laid one upon another upon a table, till a heap of about a thousand is produced, when a plank is laid on the top, and pressed down with great force by means of cords passed round the table, and the paper is left to drain. The drying is achieved by placing the sheets, one by one, by means of a brush, on the outer surfaces of a hot stove built of brick.

Some of the Chinese, and especially the Corean papers, are smooth on both sides; this dress is produced by first polishing the surface with dried leaves, and afterwards pressing it by means of heavy rollers moved by hand.

The manufacture of paper from the *Broussonetia papyrifera*, or paper mulberry, is thus conducted by the Japanese:— The mulberry stalks are cut into lengths of 2½ to 3 feet, which are then steamed in a vessel made of straw until the skin begins to separate at the cut ends. The skins are then stripped off by hand and dried, the stripped sticks being used as firewood. The drying is effected by hanging them over transverse poles in the open air. After drying they are weighed into portions of about 32 lbs. each, and tied up in bundles. They are then washed in running water, in which they are left for a day and a night, after which they are taken in, and the inner fibre separated from the outer skin. The outer dark skin is then scraped off with a knife, and is used for making an inferior kind of paper; it is called "Saru-Rawa," and after being thoroughly washed in running water, which causes it to open out flat, it is boiled. It is then allowed to rot, and is well beaten, after which paper is made of it.

It usually takes two or three days to make paper from the inner bark. Tied into bundles of about 32 lbs. weight each, it is taken to the river and thoroughly washed, and afterwards steeped in buckets of water; the water is then run off, and heavy stones placed on the fibre to express the remaining liquid; they are then boiled in water infused with the ashes of burnt buckwheat husks. It is afterwards placed in a basket, and boiled a second time to get rid of the ash infusion. After a third washing it is placed in a wooden vessel and pounded into pulp. It is then mixed with a paste made from boiling the roots of the *Tororo* plant, and formed into sheets by hand moulds, and then dried in the open air, except in wet weather, when it is sometimes dried by the heat of a fire.

Paper manufacture from pine wood is extensively carried out in Sweden, and the use of that fibre is, it is believed, largely on the increase in this country. After the particulars already given of the means employed for reducing fibres to pulp, all that is necessary, in referring to the use of pine wood on the present occasion, is to point out the principal difficulties experienced in its use, and the manner in which those difficulties have now been overcome. The almost inexhaustible supply of this material would naturally commend itself to paper-makers; but the obstacles to its extended employment arose from the large quantity of alkali necessary for disintegrating the fibre, and the necessity for very strong vessels in which to perform the operation, because it is only by boiling at a high temperature with a solution of caustic soda that this can be performed. These two difficulties have now been successfully overcome; that with regard to safe boilers by means of a process invented by Mr. F. B. Houghton, which dispenses with the use of fire under the boiler, and the expensiveness of caustic soda is avoided by employing the process discovered by M. Tessie du Motay, by which the tedious and expensive process of evaporation and calcination, which had only for its object the destruction of the resinous matters taken from the wood, is now performed without evaporation or calcination, by simply passing a current of gas through the liquor which has been employed for boiling the wood; this separates the resin from the liquor, leaving it floating in it. It is then coagulated and falls to the bottom, and the liquor can then be treated as if it were new soda-ash. Finally, the resin, instead of proving an obstacle, becomes a source of revenue more than sufficient to pay all the expenses.

We must now briefly notice some of the manufactures of which paper forms the basis. The one most completely illustrated in the Exhibition is the manufacture of envelopes, by Messrs. John Dickinson and Co. The paper, as it arrives from the mill, each of about three-quarters of a mile in length, in rolls, is fixed to the cutting-machine. The sheet is first cut longitudinally by two circular cutters, working, like scissors, on each other. It is then cut transversely by a revolving knife at the end of the machine, and which is so arranged that sheets of any required size can be cut by it. The paper, on leaving this machine, is too rough for writing purposes, and has next to be "glazed." This is done by interleaving it, sheet by sheet, with plates of zinc or brass, and passing it in small quantities between rolls, under a pressure varying from 20 to 40 tons. The paper is then put in bundles of sheets under a punching-machine, which cuts out the "blanks." These have to be gummed on "the nose," that is, on that portion which has to be wetted when fastening the finished envelope. The girls who are employed on this work can gum some 4000 per hour. The blanks are then put into a chamber of racks, heated by steam, in order to dry the gum. The next process is the stamping, which is done by hand under a small ball-press; and at this point the process of black-bordering, when required, is carried out. This is also done by hand, the blanks being arranged in a row overlapping each other, so as to leave a width of each uncovered equal only to the width of the desired border. The blacking is then done by hand, by means of a brush; two edges only can be blacked at once, and when dry the other two edges are done in a similar manner.

The blanks thus finished are placed by parcels into a folding-machine—of which there are several different kinds shown at work in the Exhibition—which folds and gums up the envelopes entirely automatically. The finished envelopes are then banded in packets of twenty-five, the defective ones being thrown out. Ten of these packets are then sealed up together and labelled. The black-bordered envelopes are packed in boxes, each box containing one gross.

The Waterproof-Paper and Corrugated Fibre Company exhibit specimens of their manufactures, paper and other fibrous materials being waterproofed by the solvent action of cupro-ammonia. By the agglutinising property of the same fluid, combinations of cotton or linen fabrics are made with waterproof-paper. This material is manufactured in slabs, flat or corrugated, of various thicknesses, for roofing and building purposes, waterproof-tubing, panels, and so forth.

Messrs. Pavy, Pretto, and Co. exhibit, all over the building, elegant curtains made from paper, of very elegant appearance, and which can be sold at a price so moderate as to place them within the reach of all.

The material employed is called "felted fabric." It is not a real tissue, but rather a species of Japanese paper, remarkable chiefly for its texture, being firm and tough, yet pliable. In point of durability it must compare favourably with most other materials used for similar purposes, inasmuch as the colours with which it is printed are indelible, and do not fade under the effects of sunshine, damp, or dust. It is impermeable, light and warm for quilts and curtains, and needs no washing. The fibres used in this manufacture are subjected, under the influence of heat and pressure, to very perfect operations of chemical disaggregation, and comminution by mechanical treatment; it is then washed in alkaline and antiseptic baths, crushed under ponderous cylinders, and then desiccated and bleached by the sulphurous gas; it is then further washed, and the fibres disintegrated by grooved cylinders, pulped in poachers, and finally felted in a paper-machine modified for the special requirements of the manufacture.

From Sweden there is a case exhibited, showing several different uses to which pasteboard is applied in that country. It is made from a mixture of straw and rags:—

Yellow pasteboard, as it is called, is applied on the inner part of the walls of a house to protect the hangings.

Grey rag pasteboard, which apparently contains a mixture of grey ragstone, is used for floorings.

Raw pasteboard, of which is formed asphalte and roofing.

Tarred pasteboard, employed to keep out draught and moisture in buildings.

Asphalte roofing pasteboard.

In China paper is often used in the place of glass for windows. This is mostly made in the Corea, and is often of large size, and as strong as a thick fabric. It is generally rough, but sometimes well-glazed, in which case it is worth sixpence or more per square yard. This paper is submitted to the action of steam, and then dressed with a mixture formed of oils of *Sterculia tomentosa* and of hemp-seed, mixed with white-lead and castor-oil seeds. The paper used for covering umbrellas is prepared much in the same way, and resists the effects of rain and sun for a long time.

It is, however, to Japan we must look for the most instructive information as to the different uses to which paper may be applied. The Japanese are wonderfully proficient in giving to paper the hardness and weight of heavy wood, and manipulating it in all sorts of shapes. Some of the common paper made is so tough that it can only be torn with difficulty. Coats, hats, shoes, umbrellas, boxes of all kinds, ornaments of every description, pocket-handkerchiefs, fans, &c., are made from paper; in fact a Japanese will turn paper into a hundred useful forms. The imitation-leather paper is made by mixing oil with the pulp; in the same manner all waterproof-paper is manufactured. The juice of persimmons is sometimes also used in making paper intended to resist dampness. From that country we have directions* for making paper-cloth *warranted to wash*, which is known by the name of "Shifu," and which are as follows:—

"Take some of the paper called 'hôshô' (used for letters, books, &c.), or some of the best 'senka' (paper used for making rain-coats), and dye it of the colour required. Boil some of the roots called 'Ron-niaku-no-dama,' with the skins on; try them with the inner portion of a rice-stalk; when it penetrates easily they are sufficiently boiled. Peel them and let the water run off, and then pound them into a paste. Spread this paste on either side of the paper, and let it dry in the sun till quite stiff. Then sprinkle water on it until it is thoroughly damped, and leave it in that state for a night. The next morning roll it upon a bamboo of the thickness of the shaft of an arrow, and force it with the hands from either end into a crumple in the centre; unroll it, and repeat this process two or three times, rolling it from each side and corner of the paper. Then crumple it well in the hands by rubbing it together until it becomes quite soft, and then sprinkle water on it again to damp it. Pull it out straight and smooth, fold it up, and pound it with a wooden mallet. It may then be put into water, as much and as often as is liked, without sustaining injury, having become

* *Vide* "Reports on the Manufacture of Paper in Japan." Printed Parliamentary Paper, No. 400 of 1871.

a strong and lasting material. This cloth is made principally in the Daimiate of Seudai. Boxes, trays, and even saucepans, may be made of this cloth; and saucepans thus manufactured sustain no injury over a strong charcoal heat. Bags may be made of it, in which wine may be put, and heated by insertion in boiling water. Paper thus prepared may be used for papering windows, and will withstand the rain without being oiled."

For the manufacture of oil-paper for rain-coats, &c., the paper should be that locally known as "senka" or "tosa-senka." The glue used for joining the paper is made of young fern shoots, ground and boiled into a paste, and thinned by admixture with the juice expressed from unripe persimmons. The dye is usually green, yellow, red, or black. Whichever colour is used, the colouring matter—generally a powder—is boiled with bean paste, and the paper is then painted with it. The preparation of the paper consists principally in softening it by rubbing it in the hands. The oil used is a seed oil called "Ye-no-abura."

Amongst the specimens of Japanese manufactures at the International Exhibition are the following:—A net coat, worn next to the skin in warm weather by the better classes. This is manufactured by rolling strips of strong paper of equal size into a sort of string, and then working them by hand into a neat net-pattern. It takes some days to complete a garment of this kind, and it will bear washing.

Hats worn by the higher class of Yakunins are made by working paper into a very heavy substance by placing many layers over each other until it attains a very hard and wood-like material. It is then varnished to render it waterproof.

Paper hats are also worked so as to resemble straw hats by the paper being twisted, and then plaited, shaped, and varnished.

Paper made to resemble leather is shown in the shapes of a box and cover for sandals. It is much used by the natives, and is well adapted for binding books, covering boxes, &c.

The above notices will suffice to show the variety of purposes to which paper may be applied besides those which are too well known to all to need any special notice on the present occasion. We shall conclude the present article by a brief allusion to the art of the so-called "water-marking" of paper, which is now carried to a very high state of excellency, so as almost to deserve being classed amongst the fine arts.

The ordinary mode of effecting these paper-marks is that of affixing a stout wire, in the form of any object to be represented, to the surface of the fine wire gauze, of which the hand mould, or machine dandy roller, is constructed. To produce a line water-mark of any autograph or crest, the pattern or device must first be engraved on some yielding surface, from which an electrotype in copper is afterwards taken in the usual manner, thus producing the device in relief. This is then affixed to the surface of the wire gauze of the mould, and produces a corresponding impression on the paper. Supposing perfect identity to be essential, as in the case of a bank note, the device is first engraved on a steel die, those parts which are intended to give greater effect in the paper having to be cut deepest. The die is then hardened and properly prepared in the usual manner, and an impression transferred from it on to a plate of sheet-brass by being placed under a steam hammer or other stamping apparatus. This being done, the die, with the mould plate in it, is next taken to a perforating or cutting machine, where the portion of the mould plate projecting above the face of the die is removed, whilst the portion embedded in the engraved design is left untouched. The latter is subsequently taken from the interstices of the die and placed in a frame upon a backing of fine wire cloth, and so forms a mould for the manufacture of paper of any pattern that may be desired.

Light and shade are occasioned by a very similar process, but one which requires more care, and is consequently somewhat more tedious. In the former case the pulp is distributed equally throughout the entire surface of the wire forming the mould, whereas, to produce the effects of light and shade, means have to be adopted for increasing, to a very great nicety, the thickness or distribution of the pulp, and at the same time to make provision for the water to drain away. This has been accomplished by first taking an electrotype of the raised surface of any model or design; and again, from that, forming in a similar manner a matrix or mould, both of which are subsequently mounted upon lead or gutta-percha, in order that they may withstand the pressure which is required to be put upon them in giving impression to a sheet of very fine copper wire gauze, which, in the form of a mould, and in the hands of the vat-man, suffices ultimately to produce those beautiful transparent effects in paper pulp.

At the Exhibition there is a large display of water-marked papers, consisting principally, so far as England is concerned, of specimens of English and Indian bank-notes; whilst the Imperial State Paper Establishment of St. Petersburg exhibits a large collection, including landscapes, portraits, and other designs. These collections of water-marked papers are exhibited as transparencies, being arranged in glass frames in front of windows, in Room 23, in the south galleries of the Exhibition building.

VII. THE PHYSIOLOGICAL POSITION OF TOBACCO.

By WILLIAM E. A. AXON, M.R.S.L., F.S.S.

Recherches Physiologiques et Cliniques sur la Nicotine et le Tabac. Par le Docteur A. BLATIN. Paris, 1870. 8vo.

Ueber Tabak in toxikologischer Beziehung, mit besonderer Berücksichtigung der im Tabaksrauch enthaltenen chemischen Verbindungen. Vom Dr. HERM. VOHL und Dr. HERM. EULENBERG (Vierteljahrschrift f. ger. Med., N.F., xiv., 2). 8vo.

Académie de Medicine L'Absinthe et le Tabac. Par M. JOLLY. Paris, 1871. 8vo.

Tabak ist Gift! Physischer und psychischer Einfluss des Tabaks auf den menschlichen Organismus. Von B. LUNDAHL. Berlin. 8vo.

Du Tabac. Son influence sur la Santé et sur les facultés intellectuales et morales. Hygiène des Fumeurs par le Docteur DRUHEN ainé. Deuxieme Edition. Besançon, 1867. 8vo.

Die Rauchhexe. Von J. V. STREBEL. Zweite Auflage. Stuttgart, 1869. 8vo.

IS Tobacco good for the health of man? Does it add to his strength, make him readier for work, more capable of endurance, add to his length of life and happiness?

The question may for all practical purposes be confined to humanity, for, with one or two exceptions, the brutes avoid the tobacco-plant, and we are not aware that any of them are in the habit of burning its leaves and inhaling the fumes in the manner adopted by man. We may conclude, then, that if tobacco has any uses, to man is due the credit of having discovered them.

In speaking of the physiological position of tobacco, we have to deal with the action of the essential principles of that plant upon the human system. The peculiar effects of tobacco are due to the action of the essential oil of tobacco in the case of chewing and snuffing, and to that combined with the empyreumatic oil in smoking. Nicotine, as this essential principle is called, is so deadly an alkaloid, that the amount of it contained in one cigar, if extracted and administered in a pure state, would suffice to kill two men. According to the experiments of Vohl and Eulenberg, the nicotine is decomposed in the process of smoking into pyridine, picoline, and other poisonous alkaloids, which can also be obtained in varying quantities by the destructive distillation of other vegetable substances.

Nicotine, as for convenience we may continue to call the poisonous principles of tobacco, can enter the body through various channels—by the stomach, by the lungs, by subcutaneous injection, and by the skin itself. But in whatever manner it enters the human system, its effects are, in the main, uniform.

The most immediately noticeable symptom following smoking is the undue acceleration of the labouring forces of the heart. Under the stimulus of tobacco the heart beats more quickly, as is evidenced by the rising pulse. We have not the mass of detailed evidence as to this fact which exists in relation to alcohol, but the experiments made by Dr. Edward Smith, and related to the British Association in 1864, are full of interest. "The experiments were made at 10 p.m., when the rate of pulsation naturally declines (as he had proved by hourly experiments published in his work on the "Cyclical Changes of the Human System"), and at least four hours after any fluid or solid food had been taken. They were made in the sitting posture, after it had been maintained fifteen minutes, and with the most absolute quietude of body and mind; and thus all influences were eliminated but those due to the tobacco. The rate of the pulsation was taken every minute for a period beginning two or three minutes before the smoking began, and continuing during twenty minutes or until the pipe was exhausted.

The following are the chief results obtained :—

Experiment 1.

Pulsation before smoking was 74½ per minute.

Smoking 6 minutes—79, 77, 80, 78, 78, 77 per minute = 78·1 average.

Smoking 7 minutes—83, 87, 88, 94, 98, 102, 102 per minute = 93·4 average.

Smoking 8 minutes—105, 105, 104, 105, 105, 107, 107, 110 per minute = 106 average.

After smoking 11 minutes—112, 108, 107, 101, 101, 100, 100, 100, 100, 98, and 91.

There was thus a maximum increase of 37½ pulsations per minute.

Experiment 2.

(Smoking through camphor julep in a hookah).

Pulsation before smoking, 79½ per minute.

Smoking 6 minutes—81, 81, 81, 83, 82, 82 per minute = 81·6 average.

Smoking 17 minutes—85, 89, 89, 93, 96, 90, 94, 94, 93, 92, 95, 95. 95, 96, 94, 97, 93 = 93.

The maximum increase was 17½ pulsations per minute.

Experiment 3.

(Smoking an empty pipe).

Pulsation before smoking, 78 pulsations per minute.

Smoking 11 minutes—76, 78, 77, 76, 79, 79, 80, 80, 79, 78, and 79.

There was no increase in the rate of pulsations from the effort of smoking, or from its interference with the respiration.

Experiment 4.

(To ascertain if, after smoking 6 minutes, during which the effect is very small, and then ceasing smoking, any increase in the effect would follow).

Pulsation before smoking, 75 pulsations per minute.

Smoking 6 minutes—76, 75, 79, 79, 76, 78.
Smoking 1 minute—82. Cease smoking.
Smoking 10 minutes—81, 88, 83, 82, 84, 83, 83, 80, 82.

The rate of pulsations was maintained, but was not materially increased.

Experiment 5.

(To prove if the rapidity of smoking causes a variation in increase of pulsation).

a. Greater volume of smoke.—

Pulsation before smoking, 70½ per minute.

Smoking 6 minutes—68, 70, 71, 70, 72, 74 = 70·8 average.
Smoking 6 minutes—76, 77, 86, 89, 91, 94 = 85·5 average.
Smoking 4 minutes—98, 95, 96, 95 = 96·0 average.

The maximum effect was thus 27½ pulsations per minute.

b. Smoking faster.—

Pulsation of the last minute in the previous part of this experiment, viz. 95 per minute—smoking 3 minutes, 94, 94, 96.

c. The pipe recharged.—

Smoking 5 minutes—87, 93, 96, 96, 96.

There was, therefore, a large effect upon the pulsation, but probably not more than would have occurred with ordinary smoking.

Numerous other experiments were made with tobaccos of different reputed strengths and upon different persons, and the author gave minute directions as to the proper method of making such inquiries."

The heart, then, during the act of smoking, was doing extra work; in some of the experiments this additional labour amounting to more than 50 per cent.

The effect upon the heart is not caused by direct action upon that organ, but by paralysing the minute vessels which form the batteries of the nervous system. Thus paralysed, they can no longer offer effectual resistance, and the heart, freed from their control, increases the rapidity of its strokes, expanding the vessels, with an apparent accession, but real waste, of force.

Its effect in lowering the animal temperature is very striking. When the walls of the blood-vessels are distended with that fluid, the increase in volume decreases the rapidity of the circulation and augments the local warmth. When the walls partially collapse, the circulation becomes quicker, but the heat diminishes. The heat, in fact, is transformed into motion.

Blatin illustrates this by an experiment upon a dog. He took a spaniel of medium size, and noted the arterial tension in the carotid, and the rate of pulsation before and after the subcutaneous injection of 0·004 m.grm. of nicotine into the abdomen. The tension increased from between 0·141 m.grm. and 0·144 m.grm. to between 0·148 m.grm. and 0·155 m.grm.; the pulse rose from 115 to 328 beats per minute.

Again, he introduced the hæmadynamometer into the abdomen of a dog four or five months old, and found the pressure to be 0·082 m.grm. On injecting 0·002 m.grm. of nicotine, the pressure increased to 0·090 m.grm.

The spaniel named as the subject of the first experiment was selected fifteen days after for another operation. Its pulse was at 120. Section of the pneumogastric nerve increased the beats to 210, but the injection of 0·004 m.grm. of nicotine into the abdomen, whilst producing the usual symptoms of poisoning, had no influence upon the circulation. In a terrier dog, poisoned with 0·003 m.grm. of nicotine, the pulse rose from 104 to 190 beats, and the exposure of the pneumogastric nerve to the action of galvanism did not diminish them. Thus the increase of the heart's action, caused by tobacco, results from its paralysing effect upon the pneumogastric nerve. The increase in pressure he considers due, first, to the quickened heart-beat, and secondly, to the paralysing influence of tobacco upon the splanchnic nerve, which is to the vascular system what the par vagum is to the heart. In small doses it increases the excitability of these nerves; in large doses it diminishes it, and that in proportion to their extent. The secondary effect of this is to augment the arterial pressure and heart-beats, and to contract the muscles of the vessels.

The vertigo and trembling noticed in animals poisoned by tobacco are owing to the smaller calibre of the blood-vessels, consequent upon the contraction of their walls produced by nicotine.

Blatin also endeavoured to ascertain the effects of tobacco upon respiration. A small dog, making 16 respirations per minute, was pricked ten times in the abdomen with a needle dipped in an aqueous solution of nicotine; the effect was, in five minutes, to increase the breathings to 38. Three days after, a drop of pure nicotine was introduced into a wound made on the inside of the leg. In an instant the respirations rose from 16 to 25, another moment saw them rise to 38; they then began to decrease, and in five minutes had fallen to 12. Two more drops were now placed on the wound; the breathings descended from 11 to 10, then to 9, stood five minutes at 8, and then another drop of nicotine reduced them to 4. The respiration was now quite irregular. Section of the pneumogastric caused no change, and in a quarter of an hour the animal died. From this it is clear that a small dose of nicotine accelerates, whilst a large one progressively diminishes them. Section of the pneumogastric produces the same effect as a strong dose of nicotine. A small dose accelerates the respiration, even after the section of the par vagum. This will be caused by its action upon the spinal cord. Strong doses cause the same paralysing action we have already noticed acting upon the circulation.

Blatin was struck with the diminution or destruction of the excitability of the nervo-motors when the doses were feeble. Sensibility is only affected by very large doses. When a strong solution of nicotine is injected under the skin of a frog, galvanism has little or no effect upon the nervo-motors. The effect is most noticeable on the nerves nearest the wound.

From this he concludes that the paralysis is caused less by the circulation than by absorption across the tissues. This he tested further, by tying with ligatures one of the posterior members of a frog, leaving only the blood-vessels and nerves free, so that the poison could only reach the nerves by the circulation. Some nicotine was then injected subcutaneously into another member. The poisoned limb did not respond to electrical excitement, but the one which bore the ligatures was evidently sensible to it, though not to the normal degree.

The action of nicotine upon the iris is well known, yet whilst some consider it to produce dilatation, others affirm its effect to be contraction. The iris is composed of two orders of muscular tissue. The circular fibres influenced by the *motor oculi*, and the radiating fibres obeying the great sympathetic, perform the two functions of the iris, dilatation and contraction. The stimulation of the third pair of nerves causes a contraction of the pupil; a larger dose of nicotine destroys its susceptibility and dilatation follows, the upper lid falls, strabismus ensues, the eyeball becomes fixed—in short, the motor power of the eye is paralysed. M. Blatin considers that the muscular fibre of the eye is not at all affected by the poison.

To determine the influence of tobacco upon the secretions, he made some experiments upon a dog, to which small doses of nicotine were daily administered. An increased dryness of the mucous system and a large secretion of urine were the first result. A wound made on its leg had not cicatrised in eight days, in spite of the well-known rapidity with which wounds usually heal in dogs. The mouth became dry, the throat inflamed, the animal, although constantly drinking, was unable to quench its thirst. Some drops of water placed upon the wound moistened it only a few moments. As the pressure of the blood is increased by this poison, in small doses it is a diuretic.

From all these experiments we may conclude that nicotine acts both on the heart and vessels, and is a vasculo-cardiac poison.

Blatin proposes to divide tobacco poisoning into two classes, acute and chronic. The first is the result of a large or unaccustomed dose; the second, the accumulative consequences of doses, perhaps small, but continually repeated.

The unpleasant experiences of the first pipe will enable most smokers to understand the nature of this acute poisoning. Children have even been made ill by sucking at pipes, empty, but already coated with tobacco juice. Sometimes a very slight dose exercises a fatal effect upon systems in which tolerance has not been established. Thus a youth of 14, having smoked 15c. worth of tobacco as a remedy for toothache, fell down senseless and died the same evening.* Blatin also tells us of a medical student, aged 22, who, after smoking a single pipe, fell into a frightful state—the heart became nearly motionless, the chest constricted, his breathing was extremely painful, the limbs contracted, the pupils insensible to light, one dilated, the other contracted. These symptoms gradually lessened, but did not disappear until four days after.†

But it is chronic nicotism which has the greatest interest for us. The poisonous effects of tobacco in larger doses are too evident for denial, and need scarcely be insisted upon. Far more important is it to learn whether tobacco, in the quantities daily consumed by its habitual users, has a permanently injurious effect upon the human system.

It is often only after a number of years that nicotic symptoms appear, as though the poison acted by a process of accumulation, until the system was charged to satiety. And thus anything which disturbs the equilibrium of the functions, and so diminishes the elimination of the poison, may give rise to morbid phenomena.

There is a theory not unknown, even amongst medical men, that the toxic influences of tobacco are only transitory, and that all the poison is ultimately expelled from the system. But it is certain, from an experiment of M. Morin,‡ that the nicotine can be detected in the tissues of the lungs and liver after death.

So little is the theory true which would have us believe that the tobacco poison is immediately excreted, that the very cannibals turn up their noses at the nicotised flesh of smokers! §

Blatin made experiments upon three dogs to determine the effects of chronic poisoning. From 15 to 30 c.grms. of tobacco were mixed with their food, and given twice or

* Druhen, p. 44.
† Blatin, p. 76.
‡ Year Book of Medicine (New Sydenham Society), 1861, p. 447, and Blatin, p. 93.
§ Strebel, p. 30.

thrice daily. The vomitings which were noticed at first soon ceased, the action of the heart became extremely irregular, the circulation grew daily feebler, digestion became difficult, appetite diminished, they were subject alternately to diarrhœa and constipation, the mucous membrane of mouth and pharynx soon became so dry that deglutition was very difficult, the gums swelled, the teeth loosened, and some of them fell out. These and other symptoms preceded paralysis of the posterior extremities, blindness, deafness, and death from sheer exhaustion. Their autopsy showed the heart to be pale, soft, slightly atrophied, the blood poor in the red globules, fluid, and deprived of fibrine.

A closely parallel case in a human subject is given. Brigitte V., a married woman, of 46 years, having lost one of her children, took to tobacco as a consoler. She snuffed, smoked, and chewed, spending about 2 francs weekly for tobacco. When Dr. Le Briert was called, her voice was rough, not a word could be distinguished, respiration was difficult, pulse feeble and intermittent, the heart beat with difficulty, the pupil dilated and insensible to the light, hearing defective, but not absolutely lost, swallowing difficult, &c., &c. Next day she died, all her organs being in a manner paralysed by the influence of tobacco.

The rough voice of snuff-takers, and the "smoker's sore throat," are also due to the influence of tobacco. Some smokers occasionally spit blood, often immediately after going to bed, and this affection may be confounded with true hæmoptysis.

M. Blatin regards all these local affections as trifling, when compared with the gradual saturation of the system with nicotine, which, accumulating in the tissues, waits for the opportunity, varying, according to individual habits and constitution, of declaring its poisonous nature.

The trembling, which is one of the usual symptoms of acute, is also a common result of chronic, nicotism. A very distinguished Parisian physician had hands which shook so much that he could not write. Whenever he remained without tobacco for any length of time these tremblings disappeared. Another case mentioned by Blatin is noteworthy. A man of 45 years consulted him respecting violent and numerous attacks of vertigo. When he felt one of them approaching he was obliged to lie down wherever he might be in order to avoid falling. In the country, where he had plenty of exercise, they were less frequent than in the town, where his occupation was sedentary. Cessation from tobacco and a tonic regimen quickly restored him.

A physician of 52 was afflicted with similar disagreeable symptoms, and was also cured by abstinence. Habit had become so strong that he could not resist at times the temptation to slight indulgence. Finding that these returns to tobacco were immediately followed by his old painful attacks he renounced it for ever.

The circulatory system presents in chronic nicotism similar symptoms to those found in acute poisoning. The most noticeable of these is the intermittent pulse, of which many cases have been collected by Decaisne and others.

Decaisne speaks of narcotism of the heart, but Blatin does not consider the action to be directly upon that organ, but considers the effects described to result from an irregular relaxation of the ganglia of the great sympathetic nerve.

When a person suffering from intermittent pulse was carefully examined, Blatin found the stoppage in the heart's beat followed a series of apparently normal movements. The systole and diastole succeeded in due regularity, and nothing in the play of the central organ indicated trouble, when the heart suddenly stopped in diastole, sometimes for the space of three arterial pulsations. When it awakens from this syncope its action is abnormally quick, as if it wished to make up for the lost time, and force the mass of blood across the organs at one stroke. But, with force insufficient for this purpose, it is exhausted in fruitless efforts, hesitates, wavers, acquires fresh power, commences again, now violent, now feeble, and fulfils very imperfectly the duties which it should perform. Gradually it calms; a foreign element seems to appease the tumult, the heart again becomes regular. The explanation appears to be that the irritation of the sympathetic nerve stops short the movements of the heart, and thus causes the intermittence; then the susceptibility of the nerve is lessened or paralysed, and the cardiac functions are left to the sole direction of the auto-motor ganglia; hence the disordered beats, which decrease as the nervous force coming afresh from the pneumogastric moderates and regularises it.

From intermittent pulse to angina pectoris the distance is not far. That tobacco may produce all the usual symptoms of that painful disease has been abundantly shown by Beau. To the cases which he has cited may be added an epidemic of this nature noted by M. Gelineau, with which a great part of the crew of the *Embuscade* were struck. The patients were all great smokers. It is worthy of notice that this disease is much more common amongst men than women.

Difficulty of breathing approaching asthma has also been recorded. Blatin gives a case of a young officer whose asthma could be attributed to no other cause, and who was cured by simple abstinence and tonic medicines.*

Tobacco, acting upon the cardiac and pulmonary branches of the pneumogastric, is not likely to leave untouched its gastric terminations. In an animal under the influence of small doses of nicotine the gastric juice is secreted with increased rapidity, and the action of the walls of the stomach is more noticeable. With strong doses or long-continued usage this secretion is very considerably diminished, and the peristaltic motion enfeebled. That is to say, the tobacco acts upon the pneumogastric, excites it in small, and paralyses it in large, doses. The smoker takes his after-dinner pipe or cigar to aid digestion. Undoubtedly, it excites the par vagum, increases the gastric secretion, and accelerates the peristaltic motion. Undoubtedly, also, this daily stimulation enfeebles the nerve, and digestion becomes more difficult. The swing back from the excitement causes a reaction, which only an increase in the doses can overcome. The nerve is partially paralysed. The appetite fails, nutrition is impeded, dyspepsia reigns conqueror.

A military man of 37 years fell into a consumption without any other affection antecedent or concomitant than distaste for food, and salivation. Dr. Roques, after various essays, learned that he was a great user of tobacco, which had led to a sort of chronic fluxion of the salivary glands, and an almost total cessation of the digestive functions, and consequently caused the feeble and consumptive state into which he had fallen. Gradual diminution and ultimate abandonment of tobacco led to a cure in about three months.†

The influence of tobacco upon vision is well known. One of the symptoms produced in acute nicotism is blindness, and chronic nicotism gives rise to similar affections. Thus Mackenzie found that patients afflicted with amaurosis were mostly lovers of tobacco in some form. Sichel found cases of complete amaurosis, which, incurable by other means, were easily conquered by cessation from the weed. Hutchinson found, out of 37 patients, 23 were inveterate smokers. The observations of Wordsworth and others have so clearly established the fact that the continued excitement of the optic nerve by tobacco sometimes produces amaurosis, that it is now generally cited in text-books as one of the causes of that disease.

* BLATIN, p. 159, from l'Abeille Méd., t. iii., 1846.

† *Ibid.*, p. 165, from Mémoire de Med., et de Chir. Prat., t. v.

We have completed our brief examination of the physiological action of tobacco, but in concluding it may be well to point to some portions of the evidence which are especially noteworthy.

The fact that tobacco reduces the animal temperature is an important one. It shows the fallacy of those who smoke to keep the cold out, and proves conclusively that tobacco is neither a generator nor conserver of vital heat, but, on the contrary, a wasteful destroyer of it.

The influence of tobacco in liberating the heart from those restraints which regulate its healthy action, naturally leads to the conclusion that in frequent doses that organ must, sooner or later, undergo a structural transformation. Although when thus excited it has less pressure to overcome than when in a normal condition, yet the extra exertion cannot but be evil in its results, since it causes an irregularity in the supply of blood, and thus degrades tissue.

Tobacco belongs to the class of narcotic and exciting substances, and has no food value. Stimulation means abstracted, not added, force. It involves the narcotic *paralysis* of a portion of the functions, the activity of which is essential to healthy life.

It will be said that tobacco soothes and cheers the weary toiler, and solaces the over-worked brain. Such may be its momentary effects, but the *sequelæ* cannot be ignored. All such expedients are fallacious. When a certain amount of brain-work or hand-work has been performed, Nature must have space in which to recuperate, and all devices for escaping from this necessity will fail. It is bad policy to set the house on fire to warm our hands by the blaze. Let it, then, be clearly understood that the temporary excitement produced by tobacco is gained by the destruction of vital force, and that it contains absolutely nothing which can be of use to the tissues of the body.

Tobacco adds no potential strength to the human frame. It may spur a weary brain or feeble arm to undue exertion for a short time, but its work is destructive, not constructive. It cannot add one molecule to the plasm out of which our bodies are daily built up. On the contrary, it exerts upon it a most deleterious influence. It does not supply, but diminishes, vital force.

It has been denied that tobacco leads to organic disease, but the evidence is very strong the other way, and it would be very remarkable if continued functional derangement did not ultimately lead to chronic derangement of the organs: that it causes functional disturbance no one dreams of denying; indeed it has been remarked that no habitual smoker can be truly said to have a day's perfect health.

It is scarcely requisite that we should add that tobacco is in no sense a necessary of life.

Even in our days, notwithstanding the vast consumption of tobacco, it is a habit of the minority only. The female sex, to their honour be it said, with very rare exceptions, abstain from this indulgence. If the claims of the apologists of tobacco are correct, why is it that an entire sex avoids it? The frailer body and more mobile mind of woman seem to stand in greater need of "soothing" and "refreshing" than the coarser frame of man.

It is not necessary; for all men do not smoke, and the abstainers are not subject to any inconvenience or disadvantage, but the reverse.

Homer sang his deathless song, Raphael painted his glorious Madonnas, Luther preached, Guttenberg printed, Columbus discovered a New World before tobacco was heard of. No rations of tobacco were served out to the heroes of Thermopylæ, no cigar strung up the nerves of Socrates. Empires rose and fell, men lived and loved and died during long ages, without tobacco. History was for the most part written before its appearance. "It is the solace, the aider, the familiar spirit of the thinker," cries the apologist; yet Plato the Divine thought without its aid. Augustine described the glories of God's city, Dante sang his majestic melancholy song, Savonarola reasoned and died, Alfred ruled well and wisely, without it. Tyrtæus sang his patriotic song, Roger Bacon dived deep into Nature's secrets, the wise Stagirite sounded the depths of human wisdom, equally unaided by it. Harmodius and Aristogeiton twined the myrtle round their swords, and slew the tyrant of their fatherland, without its inspiration. In a word, kings ruled, poets sung, artists painted, patriots bled, martyrs suffered, thinkers reasoned, before it was known or dreamed of. Who of us can realise Moses with a "churchwarden" in his mouth, or St. Paul smoking a prime Havannah?

Think of ancient Greece, of her glory in arts and arms and song, of her poets, sculptors, architects, after whom the moderns toil in vain. We do but follow in their tracks with halting steps and slow, and yet they lived their lives, and thought their deathless thoughts, and gave immortal beauty to the silent stone, without tobacco.

What shall we say, then, to this habit? It is in no case necessary or beneficial; it is a social nuisance; it is devoid of all æsthetic beauty; it is an unmanly leaning on a solace to care and labour neither sought nor needed by the weaker sex; it is an enormous and yearly increasing source of national improvidence. Above all, it is the foe to youthful development, the bane of youthful blood and brain. The subject may seem to some too trivial for serious attention; but when we consider the extent of juvenile smoking, we see that the national life and stamina are seriously threatened by this ignoble habit. So a noble tree, heaven-aspiring, with wide-spreading branches, whose leaves are a refuge for the singers of God, may be attacked by some insignificant parasitical plant, which winds round and round it in serpent-folds, and sucks away its sap and vigour, till the green leaves are blasted and the singers flee away, till the glory is departed, and Death and Ruin alone remain.

NOTICES OF BOOKS.

The Beginnings of Life; being some Account of the Nature, Modes of Origin, and Transformations of Lower Organisms. By H. Charlton Bastian, M.A., M.D., F.R.S., Fellow of the Royal College of Physicians, Professor of Pathological Anatomy in University College, London, Physician to University College Hospital, Assistant-Physician to the National Hospital for the Paralysed and Epileptic. In Two Vols. London: Macmillan and Co. 1872.

Questions concerning the nature and origin of life have for many years exercised the highest powers of the greatest minds. It has always been admitted that at a certain point of the investigation our ability of realising the great mystery of vitality ceases, and we are constrained to admit that—

"*All that we know is—nothing can be known.*"

To attempt to bridge over the gap which divides the living from the dead by patient and profound research is very laudable and honourable. Experimental evidence has, in late years, made an appreciable advance in this direction; but yet the gap is very wide, and there is much danger that a too hasty generalisation from as yet imperfect data may do harm, even to those who have made the subject their special study—much more to those who belong to the large class of scientific *dilettanti*. The author has presented us with a work of laborious reasoning, and records of many instructive and valuable experiments. He tells us that rather more than three years ago he was content—

"Stare super vias antiquas;"

but his microscopic investigations led him to renounce many of his old prepossessions for a new doctrine, which now he advocates with the ardour of a convert.

Of the subject opened out, it is obvious that a small portion only can be adequately discussed. The doctrine which the author especially urges is the possibility of the origination of living beings from the elements of dead organic matter. Until the present time this view has been denominated the Theory of Heterogenesis, or Spontaneous Generation. The latter term, however, certainly seems inapplicable when the idea of volition is excluded. Dr. Bastian has therefore employed the less objectionable coinage *Archebiosis*, which indicates the process of passage of the non-living into the living, owing to the occurrence of certain new molecular combinations.

The author commences with a Prolegomenon concerning the nature of vitality, designed to show that philosophically there can be no abrupt line of demarcation between dead matter and living matter. Nevertheless, in our opinion, even with all the light which the doctrines of the correlations of forces and the inter-relations of force and matter give us, we are constrained to admit a vast difference in kind, if not in degree, between the force-attributes of a particle of matter living and those of a particle of matter dead. We may show that the modes of manifestation of force by a living being are purely physical, but that is not sufficient to demonstrate that the force which controls or liberates, changes or co-ordinates these is physical also. And even if we are to renounce the idea of a special vital force directing the living machine, the difficulties are scarcely less, for the relations of force to matter, in reference to degree alone, would, in the case of a living organism, be entirely inexplicable. We must imagine matter supersaturated, as it were, with force, without there being any appreciable affection of its surroundings, and the resultant combination over-riding the laws which govern both force and matter under ordinary conditions. We cannot see that the author's metaphysical speculations at all smooth the path for the easy run of his theory, and we rejoice to find that he admits that of an absolute commencement of life we know nothing whatever. "The gradual transition from the not-living to the living is still hidden from our view, and so, perhaps, it may ever remain."—(P. 128.)

Let us consider the present position of the question that the author discusses. Our common experience of visible Nature teaches us that living things originate from pre-existing living things, and in no other way whatever. The modes of derivation of progeny from parent are sometimes complex and elaborate, at others direct, simple, and demonstrable. The progress of research has tended to show that—even in parasites of parasites of animals, the lesser fleas that bite the greater fleas—the parentage can be traced and demonstrated. And when the highest powers of the microscope are brought to bear upon the subject, the same law is found to hold good. The lowliest visible speck, a mere assemblage of apparently structureless material, can be seen to become detached from the parental mass, and in its turn to propagate other individual buds of protoplasm in like manner. The law, therefore, "Omne vivum e vivo," is universal, in so far as the direct interrogation of visible Nature declares. If there is an exception to this law, in cases of any organisms, it is for those who assert such exception to demonstrate it by evidence. This is what Dr. Bastian attempts to do.

The logical methods which might be adopted for such demonstration are the following:—First, the synthetic method. The fundamental ingredients of organisable matter are carbon, oxygen, hydrogen, and nitrogen, with frequently traces of sulphur and phosphorus. If by taking these inorganic materials, and placing them in juxtaposition, it could be shown that a material possessing the attributes of vitality resulted, the proof would be complete; but this has never been done. The analytical method is equally powerless for the elucidation of the problem, for, as we have just said, in Dr. Bastian's own words, the passage of dead molecules into living matter can never be demonstrated. The method therefore adopted is that of concomitant variations, and here it must be recollected that the several conclusions present, at the best, *alternatives* only.

Direct microscopical examination of a liquid in which the lowest forms of life are prone to develope is considered to afford many data for the elucidation of the problem, though it is admitted that the experience is not crucial. Dr. Bastian says, "When a fluid containing an organic substance in solution is allowed to remain in contact with air, during moderately warm weather, it soon undergoes changes of a putrefactive or fermentative character." It must be admitted, however, that this statement is too general. *All* solutions of organic matter are not susceptible of these changes. In those in which they do occur it is acknowledged that an invariable accompaniment of the changes is the appearance of specks of material, which in the course of a few hours are evidenced as minute, rapidly-moving bodies, known, for the most part, as *Bacteria*. It is urged that, as these have no demonstrable origin from visible parents in the fluid, they must, in all probability, have been formed out of the non-living ingredients. The current views as to the nature of these lowly organisms are very conflicting, and it must be admitted that their life-history has yet to be written. It would appear, however, from morphological considerations, that they are not the simplest form which living matter assumes. The simplest organism with which we are acquainted is the Protamœba: this consists of a mere mass of plasma, apparently perfectly homogeneous and structureless, which multiplies by the mere separation of a portion of its substance from the parent mass: it is simply growing, moving, multiplying protoplasm. In a slightly higher form of Amœba the constituent protoplasm, instead of being perfectly homogeneous, is condensed in granular masses at various points, which become discharged from the parent as germs or ovules. It would seem that a bacterium presents still a higher differentiation: it is of a more definite, cylindrical shape; it moves with great rapidity in certain directions, forward or backward, and multiplies in some cases by fission, in others, as figured by Dr. Beale, who has examined these organisms with the highest powers yet employed—by ovulation. According to the view of the gradual evolution of the living from the dead, it would seem, *prima facie*, most probable that the simplest form—like the protamœba—should be first formed, and the bacterium differentiated therefrom. No observer, however, has substantiated this view.

In the description of the first appearance of living specks in an organic infusion Dr. Bastian's observations closely follow those of Mantegazza. He insists, with a great probability, that if these were derived from germs pre-existing in the fluid, such germs must have been invisible under a power magnifying 1000 diameters. The dilemma, as regards the origin of bacteria, therefore remains—"Either they have been developed from a multitude of pretty evenly disseminated *invisible* germs, or they have been produced in the fluid by a process of Archebiosis."—(P. 297). Considering the extreme minuteness of bacteria themselves, it is surely not contrary to reason to suppose that their germs are so minute as to be diffused through a fluid without betraying their presence to the vision. Is it not as probable that they should be undetected, just as the particles of a salt in solution are undetectable by the microscope? But Dr. Bastian asks—What is the medium whereby such possible germs could possibly be transmitted? He does not discern, with Shelley, that—

> "Those viewless beings,
> Whose mansion is the smallest particle
> Of the impassive atmosphere,
> Live like man."

It is clear that the germs of fungi, as well as other vegetable seeds, are transmitted by the air. As regards bacteria, however, it has been clearly proved, by Prof. Burdon Sanderson, that the great vehicle for their diffusion is *water*—that even ordinary distilled water may contain their germs "in such profusion that even so small a quantity as is introduced into a glass in rinsing is sufficient to render a relatively enormous volume of liquid fruitful."—(Thirteenth Report of Medical Officer of Privy Council.) Dr. Sanderson recognises the particles of the germinal substance of bacteria as of such minuteness as to be not only invisible under the microscope, but insufficient to affect, so far as it was possible to ascertain, the optical purity of the water through which the electric beam was passed. We therefore do not recognise Dr. Bastian's objections as fatal to the view that the lowest organisms observed in a putrefying infusion can be derived from pre-existing living particles. Such particles may exist undetected, and may be transmitted by air, by water, or by both.

The next argument, one that has always played a very important part in the controversy, is that derived from the action of heat, which, it is urged, if of sufficient intensity, must destroy the life of any possibly pre-existent protoplasm in any given medium. Dr. Bastian has subjected organic infusions and saline solutions, in flasks from which air has been expelled, to temperatures varying from that of boiling water in the first series of experiments to upwards of 307° F. in other series. After the flasks had been put aside for certain periods the contents were microscopically examined, with the result of the discovery, in many cases, of various fungoid organisms and bacteria particles. In considering these results, we must at first not fail to make due allowance for the very difficult nature of the experiments, and the liability to error in the conduct of them. All experimenters have not attained the same success in the discovery of organisms, after like experimentation, as Dr. Bastian. Dr. Burdon Sanderson in no case obtained evidence of organisms in fluids heated from the ordinary temperature of boiling water, prolonged for an hour or two in some cases to 200° C. in others. He is very careful, however, to avoid generalisation, and to say that these results only occurred (but this uniformly) in the particular putrescible solutions he employed : these fluids, however, included serum of blood and Pasteur's solution. Prof. Frankland also repeated Dr. Bastian's experiments, with the result of obtaining "not the slightest evidence of life." This is sufficient, we think, to show that so difficult a mode of experimentation is not free from sources of error. But fully admitting that Dr. Bastian met with various fungoid organisms and bacteria, which he has carefully figured, let us consider the possible signification of these results, setting aside, for the moment, the hypothesis of Archebiosis. In the first place, it is necessary to make a division between those fungoid bodies, which it would be impossible to assert to be actually living at the time of examination, and the bacteria, &c., which positively manifested vital movements. It is well known that fungi will occur in saline solutions if they contain nitrogen; and it has been shown that they will freely develope in salts, which might, *prima facie*, appear to afford very unlikely pabula. Unless, therefore, every drop of the solutions employed by Dr. Bastian had, before experimentation, been microscopically searched—a course obviously impossible—the objection must be held valid that the fungi which he took out of his solutions might be precisely those which he placed into them originally. This, however, would not obtain in the case of the actively moving bacteria which were met with in a few instances. It is urged that direct observation has shown that a temperature far short of that of boiling water, 127·5° F., is sufficient to destroy the life of all bacteria. *A fortiori*, therefore, in case of the higher temperatures, life must have been impossible. Without entering upon the much-debated question of the limits of vital resistance to heat, we may yet pause to enquire whether it can be proved, with complete satisfaction, that every single bacterium-particle in a solution, or in a glass vessel in which a solution is contained, is of necessity elevated to the temperature indicated by the external means of heating? It is an old conjuring trick to plunge the hand, previously moistened with a solution of soap, into molten lead, and even iron, and to withdraw it completely unharmed. May we not conceive bacterium-particles in a fluid to be in some cases protected, in like manner, by a film? It must be recollected that if a single particle escape the destructive influence, it is capable of begetting myriads in the solution. Davaine calculated that one bacterium-particle would, in the course of 62 hours, become the parent of

71,000,000,000,000 similar particles. Moreover, Dr. Sanderson has shown, with great probability, that the germinal matter of bacteria may be imported simply by contact of a glass surface which has not been superheated. We cannot but conclude that the investigation on this particular point is so beset with difficulties and sources of error that careful repetition and reconsideration of the experiments are necessary. And then remains the dilemma—either the doctrine of Archebiosis is true, or else living matter is occasionally more able to resist destructive influences than we should, from our own experience, think probable.

A comparatively small portion of Dr. Bastian's book is devoted to the consideration of the phenomena of fermentation. There is, however, a very vehement opposition to the views of Pasteur. The changes which the author characterises as fermentations would, in very many instances, be excluded from such a definition by workers in this field of enquiry. Is it admitted that the change of cyanogen into oxamide by hydrochloric acid, and the decomposition of tartaric acid by heat into carbonic acid, water, and pyrogallic acid, come under the category of fermentations? We believe it wrong to ascribe to Liebig the exclusive view that dead organic matter is the determining cause of fermentations—of course understanding the so-called catalytic changes. Certainly, in case of alcoholic fermentation, Liebig recognises the determinative agency of the living yeast-plant. The latest exponent of this physical theory, M. Fremy, has been constrained to adopt a hypothesis which gives a sort of half-life to the determining agencies of fermentations. M. Fremy classes the organic materials which induce them as "hémi-organisés." We rather think, however, that a perusal of the recent "Comptes Rendus" will tend to the conclusion that the triumph of the controversy remains with M. Pasteur.

Taking all the evidence adduced by the author, we cannot accept the dogma of the origination of living beings from dead organic matter as proved. Still more cogent reasons must be advanced before we can conclude that the same lower organisms which we see multiplying, by conversion of surrounding materials into their own substance, and transmission out of their own protoplasm of a countless progeny, are also formed out of lifeless matter. We cannot yet discard the axiom "Nihil frustra."

The greater part of the second volume of Dr. Bastian's work is devoted to another phase of the enquiry, and the observations contain many points of interest and value to naturalists. The author, having satisfied himself of the truth of Archebiosis, proceeds to consider the process of Heterogenesis. This has usually been confounded with the former hypothesis, but the author defines it as the process whereby "the matter of already existing living things gives birth to other living units wholly different from themselves, and having no tendency to assume or revert to the parental type." Dr. Bastian has supplemented the observations of Pouchet and others, which were said to demonstrate the differentiation of the pellicle formed by the coherence of bacteria, at the surface of a decomposing infusion, into monads, paramecia, &c., by new investigations, tending to show that areas of the same pellicle become converted into spores of fungi. It is impossible to prove whether there is this conglomeration and conversion of bacteria, or whether the appearances are due to developing ova or fungi seen below the film of bacteria. In these observations the hypotheses of Archebiosis and Heterogenesis seem inseparably connected; but not so in the next and extremely interesting enquiry that Dr. Bastian takes up. The mutability of living forms is acknowledged by all observers, the divergence in their views being only a question of degree. Those who legitimately doubt that a milk-globule can become actually transformed into a penicillium (see p. 311, vol. ii., *et seq.*) will find much less difficulty in believing that living matter, under various conditions, is able, Proteus-like, to assume an endless diversity of forms. The morphological views developed by Dr. Bastian are worthy of very attentive study. The work concludes with five appendices, containing details of experiments and an enquiry concerning "The Germ Theory in relation to Epidemic and 'Specific' Contagious Diseases."

Although we have had occasion to differ from many of the author's conclusions, we are glad to acknowledge that Dr. Bastian has made an important contribution to scientific literature, evidencing wide research and laborious reasoning, and has spared no pains to illustrate his meaning by well-executed wood-engravings.

Air and Rain. The Beginnings of a Chemical Climatology. By ROBERT ANGUS SMITH, Ph.D., F.R.S., F.C.S., (General) Inspector of Alkali Works for the Government. London: Longmans, Green, and Co. 1872.

"WHEN we are children, air is to us nothing." With this pithy little sentence Dr. Smith commences his exhaustive treatise upon the constituents of the atmosphere and the results of his investigation; and while he shows how especially erroneous is the idea of the nothingness of air we entertain as children (and not unfrequently by our actions as children of a larger growth do we give credit to this idea), he tells us how far we may be alarmed, in invading with microscope and chemical test the regions around us, by the army of aërial corpuscles and microcosmic organic matter which our fears may endow with undue power. "We have," he says, "many people so afraid of this organic matter of the air, and of all its floating particles, that they would like to filter it all out, and breathe the gases pure. We must not allow our fear to go too far. We have no reason to be sure that air free from floating particles would be wholesome; we have only the proof that, if there is an excess of some kind, it is unwholesome. Apparently everyone can breathe air tainted with any disease without being hurt, if the taint is small enough. Inconceivably small particles injure; but we must learn to divide even the inconceivably small. We can bear a larger amount of taint if it is diluted enough. Dilute sufficiently the air of a hospital, and infection ceases. One short time of the infected air produces disease; a long period of the diluted air produces none, although the number of particles that must pass over a certain spot must be much greater in the long time than when the stronger mixture passes in the short time. We learn from this that the amount that does injury is not infinitesimal; there must be a certain quantity. I do not doubt that we shall measure that readily: we can readily measure the amount of ammonia and organic matter in the infectious and non-infectious atmospheres. The really practical work must begin after this. It was my desire to have done so in one of the hospitals, but having begun I was interrupted, and it must not be done in a hurry. I do not feel hopeless of being able to say that in a scarlet-fever atmosphere there must not only be so much nitrogenous organic matter and so many germs, otherwise infection will be certain."

For the actual chemical instruction relative to the matter before us we must refer the reader to the work itself; but to the general subject of ventilation we may here give some attention.

"The demands of ventilation would be best explained," says Dr. Smith, "if we could reply to these questions:—What is the smallest amount of carbonic acid which may be called injurious? and what is the smallest amount of organic matter?" The answer to the former question Dr. Smith has determined very accurately by means of a hermetically-sealed leaden chamber, and the effects on his own person of remaining in different states of the atmosphere. The amount for a healthy place is below 0·4 per cent, while about five times that amount affects a candle sensibly. But the practical question—How often must we renew the air of a room in order to maintain a given degree of purity?—is that most nearly affecting the general reader, and which we shall endeavour to answer from the work before us. Suppose that a man brings 100 cubic feet of air to contain 0·4 per cent of carbonic acid in an hour from zero, he will bring 1000 cubic feet to contain 0·04 in the same time, being at the end of the hour ready for another 1000 cubic feet, in order not to exceed this limit. But in actuality the air supplied also contains 0·04 per cent of carbonic acid, and the space in which the man is situate cannot be maintained at the degree of purity. The limit usually assigned is 0·06: this being 0·02 higher than that which he receives as fresh, a man would require a constant supply of 2000 feet per hour. But then the air-supply of the majority is not so nearly pure as represented by 0·04, and it may be necessary to increase the supply to 3000 cubic feet per hour. Now that we know the quantity to be supplied per hour, we must ascertain the rate of change at which the air-supply becomes dangerous as a draught: this has been put down at a velocity less than ·5 feet per second, with an entry of 48 square inches and a similar outlet, independently of the fire-place in the room; or, in other terms, the 3000 cubic feet of air per head must be delivered so that the whole air in the space inhabited is changed not oftener than six times an hour. It is so difficult, or indeed impossible, to effect so many changes per hour that separate ventilation must be provided. Of the nature of this ventilation it is difficult to form an opinion, since it varies with every case. Thus we see that in the case of a small room, with a large number of inhabitants, the supply of air to prevent accumulation of carbonic acid must amount to a draught that is in itself dangerous, and that consequently the larger the number of persons the narrower are the limits between the two dangers. But a third element comes now to light, for with increase of numbers there is an increase of heat, and a percentage of carbonic acid that could be easily withstood under the influence of cold becomes exceedingly injurious at a high temperature. These reasons against overcrowding are plain to any average intelligence; and in Dr. Smith's work so full are the illustrations that he must be dull indeed who is not incited to a regard for healthful ventilation. Most cordially do we recommend Dr. Smith's book to our readers, and we think it would in many cases be an admirable present to those who could, if they knew how, render help to their fellow-creatures by supplying information as to the extremes of draught and slow-poisoning by bad air.

Essays on Astronomy: A Series of Papers on Planets and Meteors, the Sun and Sun-surrounding Space, Stars and Star-Cloudlets; and a Dissertation on the Approaching Transits of Venus. Preceded by a Sketch of the Life and Work of Sir John Herschel. By Richard A. Proctor, B.A. Camb., Honorary Secretary of the Royal Astronomical Society; Author of "Other Worlds than Ours," &c. London: Longmans and Co. 1872.

These are the collected essays, by Mr. Proctor, referred to in his other works, particularly in "The Sun" and "Other Worlds than Ours," and arranged in one volume, for the convenience of readers who have no means of reference to essays published in the "Proceedings" of scientific societies. The first three essays relate to the life and work of Sir John Herschel; the remaining papers are devoted to dissertations on the planets Mars and Saturn, meteoric astronomy, the zodiacal light and the solar corona, star and star-cloudlets—their nature, movements, arrangement in space, and aggregation into systems. Further, there are ample appendices on the rotation of Mars and the proper motion of the Sun, as well as three essays on the transit of Venus. This breathless list conveys but an inadequate idea of the labour which Mr. Proctor continually undertakes for the benefit of the public; and, what is more, the popular descriptions are given *after* the facts have been presented to and received by some eminent Society. Thus Mr. Proctor has a twofold claim upon the public, for hard work and for accuracy. We can recommend this volume as being one of the most solid Mr. Proctor has yet written.

An Exposition of Fallacies in the Hypothesis of Mr. Darwin. By C. R. BREE, M.D., F.Z.S., Senior Physician to the Exeter and Colchester Hospital; Author of "Species not Transmutable, nor the Result of Natural Selection," &c. London: Longmans and Co. 1872.

WE cannot here enter into the discussion invited by Dr. Bree, because we should be led beyond the limits of our space; but we can recommend the work as a clear and earnest protest against the exaggerated claims of the Darwinian theorists. But the work remains, for all the care bestowed upon it by its author, still only a protest. Dr. Bree has attempted too much. Had he devoted his energies to single combat with Dr. Darwin some sore wounds might have resulted to the latter, but when he encounters, also, Mr. Spencer and Professor Huxley, he should not expect to make much mark against such redoubtable champions. There are, in Dr. Bree's present work, many valuable arguments against the Darwinian theory, which arguments may be considered to fail solely for lack of illustration. In saying so much we have almost taken the field against Dr. Bree; this we do for the reason that we think his high talent as an entomologist and ornithologist would be serviceably employed if concentrated to the answering of one point of the theory of evolution, for in the sciences named Dr. Bree has corrected not a few errors made by Dr. Darwin. Thus his work is calculated to lead to the ultimate discovery of the truth; and we recommend to all our readers, who like to know something of both sides of a question, the perusal of Dr. Bree's exposition.

Magnetism and Deviation of the Compass. For the Use of Students in Navigation and Science Schools. By JOHN MERRIFIELD, LL.D., F.R.A.S., Head Master of the Plymouth Navigation School. London: Longmans and Co. 1872.

THIS is a work well adapted to the candidate in navigation. The explanations are clear and well arranged, embracing every useful information on magnetism and the deviation of the compass. The want at present felt by students, on account of the alteration in the examinations conducted by the Board of Trade, is well supplied in this manual, which will prove of great benefit to those preparing for a nautical life.

As Regards Protoplasm. By JAMES HUTCHINSON STIRLING, F.R.C.S., and LL.D., Edin. New and Improved Edition. London: Longmans and Co. 1872.

IN the present edition of Dr. Stirling's controversial pamphlet there is appended a second part, in reference to Prof. Huxley's second issue, and a preface in reply to Prof. Huxley's "Yeast." The subject-matter is too well known to the public to need again noticing in this place: we may say that the additions are too personal to be either quoted or condensed with advantage. The pamphlet should be read in its present form.

Patterns for Turning: Comprising Elliptical and other Figures Cut on the Lathe without the Use of any Ornamental Chuck. By H. W. ELPHINSTONE. With Seventy Illustrations. London: John Murray. 1872.

MR. ELPHINSTONE, in this elaborately illustrated work, provides directions for the cutting of elliptical and other figures, on a lathe furnished with a division-plate, an ornamental slide-rest, and eccentric cutting-frame, and an overhead motion. The book is intended for perusal on the bench, and is admirably arranged for the use of the practical mechanic. The formulæ are exceedingly simple, and require but a very elementary knowledge of mathematics. Every amateur turner should obtain a copy of Mr. Elphinstone's work.

A Handbook of Chemical Technology. By RUDOLPH WAGNER, Ph.D., Professor of Chemical Technology at the University of Wurtzburg. Translated and Edited from the Eighth German Edition, with Extensive Additions, by WILLIAM CROOKES, F.R.S. With 336 Illustrations. London: J. and A. Churchill. 1872.

WE present to our readers a notice of this work, although criticism is of course not within our province. We give a synopsis of the contents, in the hope that our readers will find a practical answer to many technical questions of daily occurrence. Under the head of Metallurgical Chemistry the latest methods of preparing iron, cobalt, nickel, copper, copper salts, lead and tin and their salts, bismuth, zinc, zinc salts, cadmium, antimony, arsenic, mercury, platinum, silver, gold, manganates, aluminum, and magnesium are described. The various applications of the voltaic current to Electro-Metallurgy follow under this division. The preparation of potash- and soda-salts, the manufacture of sulphuric acid, and the recovery of sulphur from soda-waste, of course occupy prominent places in the consideration of chemical manufactures. Soap manufacture will be found to include much detail. The technology of glass, stone-ware, limes, and mortars, will be interesting to the builder and engineer. The technology of vegetable fibres has been considered to include the preparation of flax, hemp, cotton, as well as paper-making; while the application of vegetable products will be found to include sugar-boiling, wine and beer brewing, the distillation of spirits, the baking of bread, the preparation of vinegar, the preservation of wood, &c. Information is given in reference to the production of potash from sugar residues, the use of baryta salts, the preparation of sugar from beet-root, tanning, the preservation of meat, milk, &c. The preparation of phosphorus and animal charcoal are included in the technology of animal products. The preparation of the materials for dyeing have required much space. The final sections of the book are devoted to the technology of heating and illumination, the production of gas, coke, and tar from coals; the extraction from the tar of benzol, carbolic acid, aniline, anthracen, asphalte, naphthaline; the preparation of tar colours, as rosaniline, aniline blue, Manchester yellow, Magdala red, alizarine, iodine green, picric acid, &c.

The work is illustrated with 336 engravings. In presenting it to our readers we offer the latest improvements in chemistry, as applied to arts and industry, with the endeavour to merit the confidence of the manufacturer and the student.

Health and Comfort in House Building. By J. Drysdale, M.D., and J. W. Hayward, M.D. London and New York: E. and F. N. Spon. 1872.

In this work we have most valuable practical suggestions as to the construction of houses with regard to warmth, comfort, and ventilation. Drs. Drysdale and Hayward, after devoting much time and thought to the subject, recommend that—in order to obviate any ill effects resulting from the free ingress of cold air into our rooms—a system of warming and ventilation be employed, utilising the heat of the kitchen chimney by means of a syphon-shaft and foul-air chamber. The latter consists of an air-tight zinc drum, about 6 feet in diameter by 5 feet high, placed under the roof of the house, each room having a separate escape-tube to the drum. The expansion of the water is provided for by a safety tube at the top of the circuit. This is the high-pressure system. The low-pressure system consists of a boiler, with pipes from 1 to 4 inches diameter, and a supply-cistern; coils of tubing, about 2 inches diameter, are placed where warmth is required, smaller pipes being used to convey the water to the coils. The size and length of the apparatus cause it to be rather unsightly, and the heat cannot be so equally distributed, although it is preferable in places removed from large towns, from the fact that an ordinary mechanic can attend to any repairs that may be necessary. The high-pressure system is better adapted to large houses, where several lobbies require warming. That described in the foregoing work is an admirable system, peculiarly adaptable to our climate, where the direct admission of cold air to our rooms is endurable only a few months in the year with due regard to health; and we commend the work to our readers as well deserving their consideration.

New Formulas for the Loads and Deflections of Solid Beams and Girders. By WILLIAM DONALDSON, M.A., A.I.C.E.; Author of "Switches and Crossings," and a "Treatise on Oblique Arches." London and New York: E. and F. N. Spon. 1872.

IT has been well said, by an American writer, that "The least deviation from the assumed standard (of pronunciation) converts the listener into a critic." And the same may be said of spelling. In the present work we have *formulas*, the assumed standard being *formulæ*. Now Mr. Donaldson may be more studious of the formation of the plural in the English language than is perhaps agreeable; for certainly this deviation from the assumed standard not only causes the thoughtful reader to pause, but its reiteration interferes with the grasping of the sense, and is most damaging to the ready perception of the course of reasoning. Where argument has to be so closely followed as in a mathematical work, every consideration should be given that there occurs nothing to distract the attention. This variation we advise Mr. Donaldson to alter in future editions, at least in the body of his work. We say advisedly in future editions, for the practical worth of the work will be at once recognised by the engineer. The formulæ are clearly deduced and plainly stated, while the illustration of the applications is of a high order.

Contributions to Molecular Physics in the Domain of Radiant Heat. A Series of Memoirs published in the "Philosophical Transactions" and "Philosophical Magazine," with Additions. By JOHN TYNDALL, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution. London: Longmans and Co. 1872.

SCIENTIFIC readers will recognise these memoirs which Dr. Tyndall has now produced in a book-form; to the general public the work will not be of interest, being a more technical investigation than those elucidated in Dr. Tyndall's well-known volumes. To recommend the work is unnecessary, but we may say that the additions conduce much to clearness.

On the Mechanism of Accommodation for Near and Distant Vision. By R. E. DUDGEON, M.D.

THIS is an exhaustive pamphlet on the mechanism of accommodation for near and distant vision, and evidences much careful and experimental research, the more praiseworthy and valuable as it is the endeavour of a professional man, of much daily experience, to throw light upon a difficult subject.

Hypotheses. By F. J. FINOIS. New York: Voytits, Ann Street. 1872.

MR. FINOIS's pamphlet of 33 pages contains as much matter for thought as many volumes. Each hypothesis is included in a short sentence, and deals with a certain phase of the subjects generally classed under the heads of sidereal, sidero-terrestrial, terrestrial, intellectual, mental, and social phenomena. We quote an illustration from the division of intellectual phenomena.

"§ Objects have been accepted as self-existent, whilst space has been declared to be 'naught,' or an abstract notion, which, however, is measured by a yard stick.

"Space is therefore to be accepted as just so self-existent as any object.

"§ The Intellect never has succeeded in analysing an Atom, a Force, and a point of Space, into any other elements.

"Matter, Forces, and Space are therefore to be accepted as the ultimate constituent elements of the Universe, which itself constitutes a sphere with diameters of infinite length.

"Forces occupy Atoms, and atoms occupy points of space. Forces cause the atoms to change their places in space. Their inseparable union and their mutual relation, to be called primordial affinity, manifest themselves as motion, of which synthesis, development, decadence, and analysis of compound forms is the result.

"§ There are three general senses for receiving impressions made by the phenomena:—

"1. The sense of sight pre-eminently for Space.

"2. The sense of touch pre-eminently for Matter.

"3. The sense of hearing pre-eminently for Force.

"The individual senses of smell and taste are auxiliaries of the sense of touch, for Matter.

"The senses assist each other, so that their combined action alone furnishes complete sensations.

"§ There are three general measures:—

"1. Space is measured by the straight line, and its compounds represented by the respective measuring instruments.

"2. Matter is measured by weight.

"3. Force, or its intensity of action, manifesting itself as velocity, is measured by the diurnal rotation of the earth, the day and its divisions, represented by time-pieces.

"§ Every language contains words which designate space, matter, and force.

"The mathematical formulæ of physics, pre-eminently the science of motion, contain the signs of these elements."

The book cannot be merely read; it must be studied line by line, word by word, and is well worthy the trouble.

Treatise on the Manner of Testing Water-Wheels and Machinery. By JAMES EMERSON, of Lowell, Mass. Lowell, Mass.: Stone and Huse. 1872.

WHILE we cannot here enter into a description of the methods of testing water-wheels and machinery employed by Mr. Emerson, his eminence as an American engineer will recommend his work to the notice of all concerned in this branch of engineering construction. The book is fully illustrated with engravings of the various wheels and turbines in use in American streams; and contains tables of quantities of water, in cubic feet per minute, flowing over weirs of different lengths, with varying depths of water, as well as tables of the velocity due to head of water. The work contains much in little space.

Corso di Geologia del Professore Antonio Stoppani. Volume I. Dinamica Terrestre. Milano: G. Bernardoni e G. Brigola. 1871.

THIS course of geological study is virtually an enlarged edition of the "Note ad un Corso di Geologia," published by Professor Stoppani in 1865. The present work will be comprised in three large octavo volumes, of which the first is now offered to the public. The subject of terrestrial dynamics is considered under two heads—the dynamics of the exterior of the earth, atmospheric and oceanic circulation, &c., and the investigation of the action and results of subterranean forces. Under the latter head we have a full but concise account of volcanic eruptions, particularly of the history of Vesuvius. The work is well illustrated, and will form a valuable addition to the library of the geologist, as being a record of geological facts founded upon the authority of Dr. Stoppani.

Life of Richard Trevithick, with an Account of his Inventions. By FRANCIS TREVITHICK, C.E. Illustrated with Engravings on Wood, by W. J. WELCH. Volume I. London: E. and F. N. Spon. 1872.

VERY little has been said or recorded of Trevithick that is now known to the general reader; yet the history of his inventions is almost one with an account of the steam-engine, especially with the earlier forms employed in mining operations. That the present biography is written with a loving hand may at once be gathered; and the book is remarkable from the fact that, though the pen has been taken up by a friend, the faults and peculiar temperament of the subject of the biography are by no means glossed over. The true character of Trevithick is caused to appear through the letters and documents of which the biography, as written by Mr. Francis Trevithick, may be considered the setting. The work might also be called a history of the steam-engine, and in its twofold character will be doubly welcome to the student of this branch of engineering science. The illustrations are capitally arranged, being at the same time ornamental and descriptive. The book will be read with advantage by all, while, as a record of the details of the progress of steam, it will occupy a prominent place in the front ranks of scientific literature.

Modern Examples of Road and Railway Bridges; illustrating the most Recent Practice of Leading Engineers in Europe and America. By WILLIAM H. MAW and JAMES DREDGE. London: Offices of "Engineering," 37, Bedford Street, Strand. Berlin: A. Asher and Co., 11, Unter den Linden. 1872.

WITHOUT in any way desiring to deny the value of sound theoretical knowledge as to the principles of bridge-building, it must be admitted that, in the construction of such works, many circumstances may be expected to arise wholly beyond the reach of any previous calculations. The treatment of such occurrences successfully requires often a master mind, possessed of good practical skill rather than of theoretical refinement; and this can only be obtained by experience, accompanied by a knowledge of what has previously been accomplished. For this reason any publication which deals in a scientific manner with the best known achievements in any branch of modern science cannot fail to prove a valuable contribution to scientific literature. Of such a character is the volume now before us, which contains accounts of upwards of seventy bridges, of a variety of types of construction, selected—as stated by the joint authors in their preface—so as to illustrate the most recent practice of the leading engineers of this country, of the Continent, and of the United States; but while the chief subjects for illustration and description consist of the principal bridges erected during the last ten years, the work includes also accounts of special structures not generally classed under the generic term "Bridge," but which, strictly speaking, belong to it; such as the sewer-crossings and other special structures on the Metropolitan Railway, the arrangements for carrying gas mains in connection with the Beckton Gas Works, railway station roofs, &c.

This book is well got up, and printed upon a rich thick paper, and the subject matter is illustrated with numerous well-executed woodcuts, whilst there is, also, that great desideratum for all works of reference, a good index. The description of each bridge is given in a clear and concise manner, accompanied by such statistics of weights, strains, &c., as are necessary for a complete comprehension of the particulars in each case.

Of course iron bridges and works occupy the greater portion of the book; but there are also given specimens of bridges of timber, masonry, and artificial stone, or the *béton aggloméré* of M. Coignet. Bridges on the truss, girder, cast-iron, and suspension principles are fairly represented, whilst swing-bridges, piers, and methods adopted in the reconstruction of bridges in France, are not omitted.

Whilst this work must be invaluable to professional men, it will prove, we think, by no means uninteresting to the intelligent general reader.

ABSTRACT OF THE PROCEEDINGS OF THE BRITISH ASSOCIATION.

Brighton Meeting.—1872.

Retiring President.—Sir William Thomson, LL.D., F.R.S.
President.—Dr. William B. Carpenter, LL.D., F.R.S., F.L.S., F.G.S.

THE present Meeting of the British Association for the Advancement of Science, held at Brighton, has been attended with the greatest success. The arrangements of the Local Committee were admirable, and the many places of interest near to the London of the Sea were freely opened to visitors. It is unnecessary to enter into the detail of these arrangements, which could interest but few, and which will be well known to many: it is our purpose to present the reader with an Abstract of the Papers read before each Section. With this view we present a consideration of the chief arguments advanced in the *Inaugural Address of the President.*

"ON MAN AS THE INTERPRETER OF NATURE."

It is recorded in earliest history, and remarkable at the present day, that man, whilst declaring the faults of his fellows, regards his own with a very partial vision. Of this inherent weakness we have a most flagrant example in the Address of the President of the British Association to the ladies and gentlemen assembled at Brighton to express their interest in the scientific discoveries of the last year. Dr. Carpenter has taken upon himself to lead them "to the consideration of the mental processes by which are formed those fundamental conceptions of matter and force, of cause and effect, of law and order, which furnish the basis of all scientific reasoning, and constitute the *Philosophia prima* of Bacon." Now, by Dr. Carpenter's own showing, this means *his* individual consideration of the mental processes; in fact, a close adherence to the sophistry of judging others by the standard of self; for he afterwards says, "Nature is what he (each man of science) individually believes her to be." Thus, that variation in conception which Dr. Carpenter asks for in every other branch of science, he denies to mental philosophy. It is not enough in his consideration of scientific exactness that the high probability that the spectrum of a hydrogen flame means hydrogen when seen by Mr. Lockyer in the spectrum of the sun's chromosphere, or that Dr. Huggins should deduce from the different relative positions of certain lines in the spectra of different stars, that those stars are moving from or towards us in space—a probability that has been accepted by every scientific man as a certainty, upon the assumption that the same lines represent the same elements in every luminary (we use Dr. Carpenter's own propositions throughout)—but Dr. Carpenter must point out that these conclusions *may* or *may not be* correct. We then have a direct aspersion cast upon all scientific men and all scientific methods. Scientific men are told slightingly what they knew before, viz., that they are as human beings fallible and individually liable to form erroneous conclusions. Every scientific man shows his appreciation of the fallibility of his judgment by carefully guarding against error, and by acknowledging error when it occurs to him. But, on the other hand, a philosopher has hitherto been considered justified when the same event has happened or happens always under the same conditions, in deducing a conclusion which he may submit to his fellow-inquirers to stand or fall as it may be true. Yet this tacit understanding of human fallibility does not satisfy Dr. Carpenter, who requires that we should always qualify our deduction with "*may or may not be.*" Notwithstanding, that in no one of Dr. Carpenter's own works do we find that he adheres to his own formula—and he should remember that even law-makers are held bound to keep the laws they make—we cannot, and we think scientific men generally cannot, but express the opinion that Dr. Carpenter has exceeded all bounds of decorum. He has been guilty of proclaiming either that scientific investigation is conducted loosely, or that scientific men assert that which is not positively true, for in no other light will his address be intelligible. In either case the assertion is erroneous, and calculated to lower other men in popular esteem, and exalt Dr. Carpenter as the discoverer of an imposition. Such conduct is unworthy a man holding the position of President of the British Association.

We will now examine the assumption upon which the whole of Dr. Carpenter's address turns. He says, "I hope to satisfy you that those who set up their own conceptions of the orderly sequence which they discern in the phenomena of nature, as fixed and determinate laws, by which those phenomena not only are within all human experience, but always have been and always must be invariably governed, are really guilty of the intellectual arrogance they condemn in the systems of the ancients, and place themselves in diametrical antagonism to those real philosophers, by whose comprehensive grasp and penetrating insight that order has been so far disclosed." Who do set up their own conceptions as fixed and determinate laws? Certainly not accredited scientific men, and yet upon the assumption that such men, and those who so strongly adhere to their own conceptions are the same, does Dr. Carpenter assail scientific men not only collectively, but individually. Dr. Carpenter's remarks would be calculated to form a serious obstacle to the advancement of science, were it not that, to use his own words, judgment "is eminently a personal act, the value of its results depending in each case upon the qualifications of the individual for arriving at a correct decision."

There is one convincing argument against the liability of error from individual interpretation which Dr. Carpenter has overlooked. It is that philosophic principles are really not isolated, neither are they individual, for if each subject be taken up by one man there remains sufficient diversity to prevent error in considering the conclusions collectively; but it happens that many men with many theories take up the same subject with concordant results. So that unless Dr. Carpenter flies directly in the face of all received reasoning on probability of error, which being of error includes probability of correctness, his remarks are childish.

Dr. Carpenter further places great stress upon the invidious distinction between "seems" and "really is," as regards the scientific interpretation of Nature. Has science been proved sufficiently inaccurate to warrant such a distinction? We think not, and that the distinction has been made by Dr. Carpenter on his own authority alone. Again, he tells us that "the philosopher's interpretation of Nature seems less individual than that of the artist or the poet;" and proceeds to reason upon this ground,—a veritable quicksand, for whereas the artist and poet are allowed liberties on account of the machinery they employ, the philosopher is allowed none, even while his machinery is of a more restricted order. Even generalisation Dr. Carpenter denies to the scientific enquirer, who employs generalisation as the artist does his rules of perspective or the poet the rules of prosody and grammar—that is, as a method of order. But the simile of artistic and poetic interpretation is in itself faulty, for the artist and poet are not required to represent Nature accurately, but only so far approximately as may affect the feelings, whilst the philosopher must most correctly trace Nature as she presents herself, if his interpretation is intended for the eyes of common-sense men or of men co-equal in scientific attainments, according to the speciality of the character of the phenomenon to be interpreted: the one is an appeal to the feelings, the other to the reason. The one calls not for accuracy, the other demands it.

Dr. Carpenter is far from understanding the difference between laws made for men and those made by them: in the first there is not, and has never been, a cessation of the directing force, whilst in the latter the law is not always enforced. In the one case, given the same conditions the same results are inevitable, because the origin is constant; but in the other the results vary with the origin. But this consideration brings us within the territory of Theology, with which, in common with Dr. Carpenter, we, as professed scientific men, publicly have nothing to do; and as we do not recognise Dr. Carpenter in any way as an authority on this new point which he appears anxious to claim, we shall merely endorse the general belief that the boundaries of theology and science are, where they touch, perfectly defined, while we shall certainly resent the endeavour he has made to place theology in supremacy to science in the investigation of the Laws of Nature, whatever we may do in investigation as to the Cause of those laws.

The very attributes of science have been attacked by Dr. Carpenter; and whatever may be the issue of the matter, there remains the fact that he has placed himself under the necessity of more clearly stating his reasons for the attack.

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

President.—Warren de la Rue, D.C.L., Ph.D., F.R.S., V.-P.R.A.S., V.-P.C.S.

Vice-Presidents.—Prof. G. C. Foster, F.R.S.; F. Fuller; James Glaisher, F.R.S.; J. Norman Lockyer, F.R.S.; J. Phillips, F.R.S.; H. J. S. Smith, F.R.S.; W. Spottiswoode, F.R.S.; Sir W. Thomson, LL.D., F.R.S.; Sir Charles Wheatstone, D.C.L., F.R.S.

Secretaries.—Prof. W. R. Clifford, M.A.; J. W. L. Glaisher, B.A., F.R.A.S.; Prof. A. S. Herschel, B.A., F.R.A.S.; G. F. Rodwell, F.R.A.S., F.C.S.

The President opened the proceedings by stating that he would confine his attention to astronomical photography. He showed that, at the very outset, astronomical differs from ordinary photography, inasmuch as the image of the object which falls upon the collodion film is always in motion. Strata of air are ever in motion between the plate and the object, and each of these strata causing refraction the result is a "flickering" image to be impressed upon the sensitive film, being a similar effect, from the same cause, of the appearance of the "twinkling" of stars. Dr. de la Rue has observed that this flickering, at certain times, chances to be at a minimum, and it has been at such times that he has obtained his celebrated photographs of the moon. He then detailed the difficulties arising from the non-coincidence of the foci of the chemical and visual rays of light, from which it accrues that, if the prepared plate be placed where a visually well-defined object falls upon the film, the focus not being that of the chemical rays the best result is not obtained. Dr. de la Rue, to remedy this defect, employs a reflecting instead of a refracting telescope. Mr. Rutherford, of New York, on the contrary, employs a refracting telescope, but one in which special adjustments have been made. The rays of light from the heavenly bodies are not only refracted during their passage through the atmosphere, but are absorbed by floating particles, solid and liquid, this absorption amounting to, perhaps, two-thirds of the chemical rays. For these and similar reasons, it would appear advantageous that the British Association or the Royal Society should possess a permanent observatory, situated at a great elevation, in a climate as free as possible from cloud. The President of the Section detailed the successful experiments of Lord Lindsay and Mr. Rannyard, as to the question whether reflections from the back surface of the photographic plate played any part in the production of the fringes observed in some astronomical photographs. In these experiments plates of ebonite and non-actinic yellow glass were prepared, and it was soon found that the outer haze completely disappeared in the photographs taken on ebonite, while on the yellow glass plates it was much fainter than on ordinary white glass plates. A wetted black paper at the back of an unground plate greatly diminished the outer haze; and a yellow glass plate ground on both sides, the back coated with a black varnish, caused the outer haze to entirely disappear, showing the greatest part of photographic irradiation to be due to reflection from the second surface.

The failure of photography to present truthful pictures of nebulæ and comets was passed under consideration, and thought to be due to the variation in atmospheric currents and the interposition of aqueous and solid particles. In following up the consideration Prof. Zollner's theory of the self-luminosity and train of comets was commented upon, the President inclining to the view that electricity may be demonstrated to be sufficient cause, provided it be granted that electricity may be developed by the action of solar heat,—if not in the process of evaporation, at least in the mechanical and molecular disturbances resulting from it. In conclusion, Dr. de la Rue expressed a hope of seeing the photographic method, as applied to sun-observations, joined to the work of Greenwich Observatory, and alluded to the importance of the subject now before the Royal Commission, presided over by the Duke of Devonshire.

Colonel Strange proposed the vote of thanks to the President, and the proposition was seconded by Sir William Thomson.

The Eclipse Committee.—The Report of this Committee, by Mr. J. Norman Lockyer and Dr. J. Thomson, was read by the Secretary, and detailed the results of the Melbourne and Indian Expeditions. The Indian Expedition made known to us that the luminous hydrogen exists at the enormous distance of seven or eight minutes from the sun's limb, instead of only about ten seconds, as previously calculated; and that there was strong radial polarisation of the corona.

Luminous Meteors.—The Report of the Committee, read by Mr. James Glaisher, F.R.S., included the notice of the August and November meteors. The work done in determining the radiant points of the meteors occupied much of the Report. Mr. Birt, F.R.A.S., then read a paper on "Lunar Objects Suspected of Change."

The Spectrum of Hydrogen.—Prof. A. Herschel read Mr. Schuster's paper on the "Spectrum of Hydrogen," stating that the author thought the second spectrum to be due to some hydrocarbon taken up from the india-rubber tubing, from grease, or from impurities on the surface of the glass; and describing experiments, with chemically-clean vacuum-tubes, in which an alteration of the spectrum occurred.

Spectral Rays.—Prof. Clifford read the Report of the Committee on "Spectral Rays," arranged upon a scale of wave-numbers.

Astronomical Refraction.—Mr. Forbes, in a paper on "Astronomical Refraction," pointed out the sources of error, in the observation of stars, due to the moisture of the atmosphere, and variation of barometric pressure in two separate places of observation. As an instance of the latter cause of error, he cited measurements taken during one month at Greenwich and Chislehurst, only five miles apart, giving a range of barometric difference of $\frac{18}{1000}$ths of an inch. Thus differences of atmospheric refraction would, perhaps, explain discrepancies in observations of the polar distances of the stars.

A series of mathematical papers also were read, of which obviously we cannot here give extracts.

On a subsequent day Sir Wm. Thomson, F.R.S., gave a summary of the Report of the Tidal Committee, as well as a Report on the Gaussian Constants of Terrestrial Magnetism; and later strongly supported the views of Dr. Carpenter on Oceanic Circulation.

SECTION B.—CHEMICAL SCIENCE.

President.—Dr. J. Hall Gladstone, F.R.S., F.C.S.

Vice-Presidents.—Prof. F. A. Abel, F.R.S., F.C.S.; Prof. Williamson, F.R.S.; J. H. Gilbert, Ph.D., F.R.S.; Sir Benjamin Brodie, Bart., F.R.S.; Prof. G. C. Foster, F.R.S.

Secretaries.—Dr. Mills; W. Chandler Roberts, F.C.S.; Dr. W. J. Russell, F.R.S.; T. Wood, Ph.D.

The Inaugural Address of the President opened with an admirable estimate of the position of Chemical Science as a science and as a means of education. He thought that chemistry was rapidly approaching the status of an exact science,—that the chemist stood hourly more in need of the mathematical training necessary to the physical inquirer. The relation of many branches of physics to chemistry was very close, as instanced by spectroscopy and electro-chemistry. The President then stated that he considered chemistry to be an essential branch of the education of every lady and gentleman. Yet he thought the so-called educated classes in England were supremely ignorant of science, and had not arrived even at the first stage of improvement—the knowledge of their own ignorance. The cry of *cui bono* was unluckily too prevalent, and the assistance offered to young and aspiring experimentalists, though great, was ineffectual. Dr. Gladstone concluded his Address by alluding to the magnificent liberality of Mr. J. B. Lawes, who has made over his laboratory of Agricultural Chemistry to trustees, and endowed it, for the benefit of science, with the sum of £100,000.

Professor Williamson proposed and Dr. Longstaff seconded the vote of thanks to the President.

Essential Oils.—Dr. Gladstone, Dr. Wright, and Mr. W. C. Roberts, of the Mint, read a paper on the results of their experiments on the composition of some essential oils used in perfumes, in which it appeared that the choice of the sample greatly affected the value of the result.

The Fusion of Arsenic.—Prof. Mallet reported the successful fusion of arsenic in narrow glass tubing (barometer tubing) enclosed in an outer tube of iron, and heated in a charcoal fire. Arsenic thus treated appears, on cooling, as a fused, compact, crystalline mass, of steel-grey colour, and has a sp. gr. of 5·709 at 15° C. The fusing-point is between that of antimony and silver.

Prof. Mallet exhibited also three specimens of meteoric iron from Augusta Co., Virginia.

British Gold Coinage.—Mr. W. Chandler Roberts, the Chemist of the Mint, detailed the precautions taken to ensure accuracy in the preparation of the gold-copper alloy, an account of which has appeared already in these pages.

Decomposition of Water.—On a subsequent day Dr. Gladstone read a paper on the "Mutual Helpfulness of Chemical Affinity, Heat, and Electricity, in producing the Decomposition of Water." Only certain of the metals are capable of displacing the hydrogen of pure water. Pure zinc does not do so, but does so when the chemical tension is aided by the electrical tension set up when the zinc is the positive plate of a voltaic couple. The effect of varying the distance between the plates of zinc and copper was commented upon, and it was stated that the chemical action increased slowly till the plates are within an inch or so of each other; nearer the action increases at a rapidly accelerating ratio. Heat assists the action. Magnesium alone decomposes water, but more vigorously when in contact with copper, upon which some of the hydrogen gas makes its appearance. If a more negative metal than magnesium be employed in contact with an again more negative metal, there is still a deflection of the galvanometer employed to measure the amount of action. The order for pure water seems to be—platinum, silver, copper, iron, tin, lead, zinc, magnesium.

Manufacture of Chlorine.—Mr. Weldon read a paper on his process of manufacturing chlorine by means of manganite of magnesium. The inventor employs a still to yield an absolutely neutral still-liquor, consisting of a mixed solution of chloride of magnesium and chloride of manganese. The liquor is conveyed from the still to an iron pot, whence it is pumped or syphoned into evaporating-pans, and thence again into a blind furnace, where the evaporation is continued to dryness. The residue is then gently heated with access of air, the chlorine of the two chlorides being driven off, partly free and partly as hydrochloric acid, and manganite of magnesium produced. The loss during the process is merely mechanical, the chemical loss being absolutely *nil.*

After this paper Dr. Crace Calvert read an interesting report on bleaching-powders, with the idea of promoting useful discussion.

Ignition of Cellulose by Saturation with Fatty Oils.—Prof. Dewar read a paper, by Mr. John Galletly, on the "Danger of Ignition of Rags, Paper, &c., Saturated with Oil," and it was shown that cotton-waste, saturated with either boiled linseed oil, raw linseed oil, rape oil, Galipoli oil, castor oil, lard oil, or seal oil, and confined in a closed chamber, became, after from 10 hours to 100 minutes, ignited to ash completely, or more or less charred. Sperm oil refused to char the waste. The cause assigned is rapid oxidation, and the author submits that the term "spontaneous combustion" may be objected to for the same reason that Gerhardt objects to spontaneous decomposition produced by oxidation. Heavy oils from coal and shale, chiefly the olefines, prevent this oxidation, even when mixed with a large proportion of fatty oils. Twenty per cent rape oil gave no indication of heat at 17° F.

Precipitation of Silver by Copper.—Mr. Alfred Tribe contributed a paper of great interest on this subject, stating that silver precipitated from its nitrate solution by copper always contained the latter metal. The presence of the copper he considered due to the dissolved oxygen in the silver solutions, or to the absorption of that gas from the air by the produced copper-nitrate during or subsequent to the precipitation.

Dust from Vesuvius.—Mr. George Gladstone, F.C.S., stated that analysis of the dust thrown up by Vesuvius during the recent eruption proved it to consist of aggregations of crystallised quartz, dotted over with the magnetic oxide of iron. By boiling the sand in hydrochloric acid the whole of the iron is removed. None of the samples contained titanium.

Chemical Nomenclature.—Dr. A. Crum Brown read an interesting paper on chemical nomenclature, to which in an abstract we could not do full justice.

SECTION C.—GEOLOGY.

President.—R. A. C. Godwin-Austen, F.R.S., F.G.S.
Vice-Presidents.—Thomas Davidson, F.R.S., F.G.S.; Prof. P. Martin Duncan, M.D., F.R.S., F.G.S.; Rev. Thomas Wiltshire, M.A., F.G.S.; Prof. J. Phillips, M.A., LL.D., F.R.S.; J. Prestwich, F.R S.
Secretaries.—Henry Woodward, F.G.S.; Louis C. Miall; George Scott; William Topley, F.G.S.

The President, in his Address, dwelt upon the richness in interest of the county of Sussex to the geological observer, upon the place of the fresh- and brackish-water formations on the geological scale, and upon the Wealden formation, especially the Wealden formation of the European surface.

Prof. Phillips proposed and Prof. Hull seconded a vote of thanks to the President.

The principal papers read in this Section were as follows:—"The North-East of Ireland," by Prof. Hull, F.R.S.; "The Geology of the Neighbourhood of Brighton," by Mr. James Howell; "The Sub-Wealden Exploration," by Mr. W. Topley, F.G.S.; "On the Distribution of Goitre in England," by Mr. G. A. Lebour, F.G.S.; "On the Discovery of a Zeuglodon in the Beds of the Eocene Formation," by Mr. H. G. Seeley; "On the Investigation of Plates of Coral from the Mountain Limestone," by Mr. J. Thomson, F.G.S.; "On Brachiapoda," by Mr. Thomas Davidson; "Report on Fossil Crustacea," by Mr. H. Woodward, F.G.S.; and a paper "On the Prospect of Finding Productive Coal-Measures in Norfolk and Suffolk," by Mr. John Gunn. All these paper contain too much detail to admit of useful abstraction.

SECTION D.—BIOLOGY.

President.—Sir John Lubbock, Bart., M.P., F.R.S.
Vice-Presidents.—Prof. Balfour, M.D., F.R.S.; John Ball, F.R.S.; John Beddo, M.D.; George Bentham, F.R.S.: J. Cordy Burrows, M.D.; T. Spencer Cobbold, M.D., F.R.S.; Prof. Flower, F.R.S.; Col. A. Lane Fox, F.G.S.; Dr. Hooker, C.B., F.R.S.; J. Gwyn Jeffreys, F.R.S.; J. Burdon Sanderson, M.D., F.R.S.; Prof. Wyville Thomson, M.D., F.R.S.
Secretaries.—Prof. Thiselton-Dyer; H. T. Stainton, F.R.S.; F. W. Rudler, F.G.S.; J. H. Lamprey; Dr. Gamgee, F.R.S.; E. Ray Lankester; Dr. Pye Smith, F.R.S.

The President opened the proceedings by reading the usual Address. He pointed out the place that natural science should occupy in the curricula of the schools, and then went on to say that the Darwinian theory of the Origin of Species was much misunderstood. Mr. Darwin had called attention to a *vera causa*, had pointed out the true explanation of certain phenomena; it was quite another thing to assume, as was too often done, that all animals were descended from one primordial source. Passing on to consider the valuable work done at Kew, the President said that any change would be calculated to affect its completeness or impair its scientific character.

Dr. Carpenter proposed the vote of thanks to the President.

Mr. Spence Bate then read the Fourth Report of "The Marine Fauna of the South Coast of Devon;" Prof. Newton contributed a paper on "The Extinct Birds of Mascareen Islands;" Mr. J. Robertson a paper on "The Pholades;" and Dr. Dhorn a Report on Zoological Stations.

On subsequent days papers on Zoology and Botany, Anatomy and Physiology, and Anthropology, were read, but are of too technical a nature to be abstracted.

SECTION E.—GEOGRAPHY.

This Section was under the presidency of Mr. Francis Galton, F.R.S., F.G.S., F.R.G.S.; and as the main subject was the discovery of Dr. Livingstone, now perfectly familiar to our readers, we pass on to the consideration of the proceedings of the next Section.

SECTION F.—ECONOMIC SCIENCE AND STATISTICS.

President.—Prof. Henry Fawcett, M.A., M.P.

Vice-Presidents.—Sir John Bowring; Grant Duff, M.P.; Sir James K. Alexander; Right Hon. J. G. Dodson, M.P.; James White, Esq., M.P.; R. Dudley Baxter, M.A.; William Newmarch, F.R.S.

Secretaries.—J. G. Fitch, M.A.; Barclay Phillips.

Professor Fawcett, in his Inaugural Address to the Section, considered the present rise in prices, and the incompatibility of this rise with the possession of a fixed income, the caution with which a man should leave only a fixed income to his widow and children, when he has other means of disposing of his money, and the time that must necessarily elapse before the incomes of the clergy and others at fixed rates can be advanced to meet essential requirements. The danger of limiting the liberty of the subject by over-legislation, with the view to the amelioration of the condition of the poorer classes, was the subject finally discussed.

Sir John Bowring, who proposed a vote of thanks to the President, then read the Report of the Committee on Uniformity of Weights, Measures, and Coins; and Mr. Burgess a paper on International Coinage.

On subsequent days papers were read upon—"Our National Accounts," by Mr. Frank P. Fellowes, F.S.S.; "The Education of Women," by Miss Emily A. Shirreff; "On Preserved Meats," by Dr. Edward Smith; and "The Pollution of Rivers," by Major-General Sir James E. Alexander, F.R.S.E.

SECTION G.—MECHANICAL SCIENCE.

President.—F. J. Bramwell, C.E.

Vice-Presidents.—John Hawkshaw, F.R.S.; C. W. Merrifield, F.R.S.; Charles B. Vignoles, F.R.S.; W. J. Macquorn Rankine, LL.D., F.R.S.; James Nasmyth, F.R.S.; W. Froude, F.R.S.

Secretaries.—H. M. Brunel; P. Le Neve Foster, M.A.; John G. Gamble, B.A.; James N. Shoolbred.

The President, Mr. F. J. Bramwell, in his Address, dwelt chiefly upon the subject of "Coal," as being that, at the present time, most prominent in the minds of the public at large,—if for no other reason than this, that the steam-engine is still the very crowning glory of mechanical engineering, and that coal is the staff of life, and, so to speak, the breath of the nostrils of the steam-engine. It was pointed out that the supply of coal is a finite quantity; that, unlike the fuel wood, which grows year by year to replace the annual consumption, the fuel coal is given to us once for all; that we are therefore dealing with a store that knows no renewal; that if we waste it the sin of that waste will be visited on our children; and that it becomes us to look upon coal as a most precious, valuable, and limited deposit, of which we are the stewards and guardians. Mr. Bramwell then reviewed the other sources of power within our reach, namely, the wind, the power of our streams, and the power derivable from the rise and fall of the tide.

The most important paper read before this Section was one by Mr. F. Ransome, on "Artificial Stone," to which we shall refer more fully on a future occasion.

Breech-Loading Small Arms.—Mr. Merrifield read a paper, presented by Mr. A. Wylie, of Birmingham, on the "Progress of Invention in Breech-Loading Small Arms during the past Twenty Years," with comments on the Reports of the Small-Arms' Committee.

Aërial Navigation.—A paper on this subject was read by Mr. C. A. Bowdler, who wished to see experiments with aërostats attempted with a view to the introduction of these machines into military tactics. The author believed in the practicability of aërial navigation by simple mechanical means. Manual propulsion having been secured, the power of direction was the next desideratum, and was accomplished by rotating the balloon to any required position, by a rudder, consisting of a vertical disc fixed in a line with the axis of the propeller. By turning the plane of the disc, the current of air forced from the fan on the rudder caused the whole machine to rotate right or left, as in the case of a ship.

Deep-Sea Soundings.—Sir Wm. Thomson, F.R.S., gave the results of some experiments on deep-sea sounding, the weight or load being sustained by a steel-wire of No. 22 gauge. The experiments had been made at a depth of 2700 fathoms, and had been attended with perfect success.

Lights at Sea.—Sir W. Thomson contributed a second paper on "The Identification of Lights at Sea," recommending a system by which lighthouses could be recognised by means of signals in accordance with the Morse alphabet.

Measurement of Waves.—A novel paper was contributed on this subject by Mr. C. W. Merrifield, F.R.S., Principal of the Royal School of Naval Architecture, who considered it desirable to confine the measurement to the two points of ascertaining the aggregate height of the waves and their number during measured intervals of time. The machinery could consist of a float sliding up and down strained wires, at the head of a pier or in a standard of framework. A line from this float could pass over a pulley, the motion of which, transmitted through its shaft, would give all the required measurements. The measurement of the aggregate height of the waves would be effected by simply connecting a ratchet-wheel, pawled so as only to turn one way, with the float pulley. To count the waves it would be necessary to record the number of times the float reversed its motion, the record being effected with a pencil on a slip of Morse paper.

Other papers of interest were read, but their application is too technical for abstraction.

Next year the British Association will meet at Bradford, under the presidency of James Prescott Joule, L.LD., F.R.S.

PROGRESS IN SCIENCE.

MINING.

It was stated in our "Chronicles" last quarter, that Archer's Wood was the site selected by the Sub-Wealden Exploration Committee for their experimental boring. But, as it was found that the geological features of that locality would seriously interfere with the work, the operations have necessarily been transferred to a new site. A beautiful spot in Councillor's Wood, in the parish of Netherfield—the property of J. C. Mappin, Esq., who has most liberally given the site—has finally been pitched upon: here the plant has been erected, and the boring commenced under the direction of Mr. Bosworth, an experienced boremaster from Leicestershire. The neighbouring rocks are the shales and limestones of the Ashburnham beds, or lowest members of the Wealden series. It is proposed that the bore-hole shall be 9 inches in diameter, and shall be carried, if necessary, to a depth of 2000 feet. As soon as a sufficient depth is attained, and the rods therefore become heavy enough, it may be expected that good cores of rock may be drilled out; and these 9-inch cylinders will, of course, yield much information to the geologist. Soon after the commencement of the boring, a large party from the British Association visited the works, but the hole was then only a few inches in depth, and the rock was churned up into a kind of mud. Scientific addresses on the object of the exploration were delivered on the spot by Mr. Prestwich, F.R.S., Mr. W. Topley, and Mr. H. Willett, whilst Mr. Bosworth, the engineer, explained the mechanical details of boring. It cannot be too widely known that this undertaking is a purely scientific experiment, for the purpose, mainly, of ascertaining what older rocks occur beneath the Wealden beds in this locality. Reasoning from well-known geological data, it is expected that some of the secondary formations will be either entirely absent or but feebly represented, and that a ridge of palæozoic rocks will be reached at a moderate depth. Those palæozoic rocks, when found, may or may not contain coal; if productive coal-measures be struck, so much the better; but, in either case, the great object of the boring will be accomplished. There seems, however, to be a popular notion afloat, that this is really a trial-sinking for coal in Sussex—in fact, a kind of commercial mining enterprise. No doubt many of the subscribers to the Exploration Fund take this view of the undertaking, but the geologists, who have set the work afoot, lend little or no support to such an inference. It is nevertheless true, that though coal-measures may not be found at this spot, the evidence there obtained may lead to a successful search for coal elsewhere in the south-east of England. Anyhow, the Netherfield boring is a work of great scientific interest, and reflects the highest credit on Mr. Willett, the spirited projector of this enterprise.

Although we may never be able to burn Sussex coal, it seems likely that Sutherlandshire coal may soon find its way into the market. Since the days of Elizabeth, it has been known that deposits of coal exist in the neighbourhood of Brora, on the east coast of Sutherlandshire—not the true fuel of the palæozoic coal-measures it is true, but a very fair coal of oolitic age. These deposits have not been worked for many years, but the present high price of coal has again turned attention to the Brora fuel, and Mr. Jones, of Shropshire, has been employed by the Duke of Sutherland to report upon the Sutherlandshire coal-field. Acting on this report, the Duke is about to renew workings; an engine is to be immediately erected for pumping water from the old pits, and a new experimental shaft is being driven at a place called Strath Steven, on the sea-shore, about one mile east of the old works.

Anyone interested in the development of the mineral resources of Queensland should read the excellent paper by Mr. R. Daintree, published in the August number of the "Journal of the Geological Society"—a paper which may be advantageously studied in connection with the fine display of mineral products exhibited in the Queensland annexe to the International Exhibition. In the south of the colony, extensive beds of coal occur in lacustrine strata, apparently of mesozoic age; and in addition to these secondary coals there is a northern coal-field of even greater extent, which is supposed to belong to the true coal-measures of palæozoic age. The coal of each period is characterised by a distinct flora, *Tæniopteris* being the characteristic fossil of the mesozoic and *Glossopteris* that of the palæozoic coal-measures. Auriferous veinstones are extensively worked in Queensland, both in Devonian strata and in metamorphic rocks. It is interesting to learn, that throughout the whole extent of the great Devonian area in the colony, no rocks have yet been found to include auriferous reefs which will pay to work, except where the strata have been disturbed by intrusion of certain trap-rocks, either diorite, diabase, or porphyrite. The gold from these veins is always alloyed with a large percentage of silver. Thus, a specimen of so-called "spider-leg gold"—that is, native gold with a filiform structure and a semi-crystalline surface—yielded 89·92 per cent of gold, and 9·69 of silver. It seems clear, from the structure of the veinstones, that they have been formed by hydrothermal action, but it is difficult to conjecture how the gold and silver could have been simultaneously precipitated from a state of solution so as to form a homogeneous alloy. In the Canoona Diggings, gold has been found in serpentine. Some rich copper lodes have been worked in the metamorphic rocks, especially at Peak Downs and Mount Perry, while stanniferous granites of extraordinary richness have been discovered in the neighbourhood of the Severn River. One of the most valuable features of Mr. Daintree's memoir is the large number of analyses of local rocks which it contains. These are not only of great service to the geologist, but, in many cases, may also be of much use to the miner; for every practical man believes that the character of the "country" is closely related with that of the mineral veins by which it is traversed.

Attention has been lately directed to the mineral resources of Newfoundland, and the best means of effecting their development. Valuable deposits of lead ore occur on the shores of Placentia Bay, and have been worked at the La Manche mine. Copper ore is found in association with serpentine rocks, and workings have been carried on at Tilt Cove mine, south of Cape John. The copper ore is said to be here intersected by a vein of nickel. Coal appears to be abundant in the neighbourhood of St. George's Bay, and fine marbles are common in many parts of the island.

The results of Mr. Mark Fryar's mineral explorations in the Mergui district of Tenasserim have been published in India. One of the most remarkable features of interest to the miner is the wide distribution of tin ore, in the shape of detrital matter from stanniferous rocks. It appears, indeed, to be almost universally distributed through the river gravels of the country, and Mr. Fryar believes that a thorough examination of the hills through which the rivers flow might be rewarded by the discovery of rich veins of tin-stone.

About twelve months ago, a thermal saline spring was noticed by some miners working in the 160-fathom level of Wheal Seton, in Cornwall. As operations were suspended at that part of the mine, no further notice was taken at the time; but on recently resuming the workings, it was found that the temperature of the spring had increased and the flow of water had augmented. Between 40 and 50 gallons of water were discharged per minute, at a temperature of nearly 100° F., while the air in the closed end was heated to more than 90°. On evaporation, the water leaves a white saline residue. A sample examined by Mr. S. T. Rose, chemist of the Truro Agricultural Association, contained more than 1000 grains of saline matter in the imperial gallon. The spring occurs in a great cross-course, at a distance of between three and four miles from the sea. It is supposed by local miners that the heat of the spring betokens the presence of a strong lode somewhere in the neighbourhood.

Singing-flames, instead of being merely used for pleasing acoustical experiments, promise to become of value to the collier in warning him of danger in the pit. An ingenious application of such flames to the construction of a new safety-lamp has been made by Dr. A. R. Irvine, of Glasgow. When a mixture of an inflammable gas and air, in such proportions as to be explosive, is ignited on the surface of wire-gauze, and a suitable tube or chimney is placed over the flame close to the gauze, a musical sound is distinctly heard, varying in pitch with the magnitude of the flame and proportions of the chimney. Applying this principle, Dr. Irvine has constructed an improved safety-lamp. While the atmosphere is not vitiated by fire-damp, the lamp burns quietly; but when the quantity of inflammable gas rises to explosive proportions the flame emits a musical sound, and thus the danger appeals to the ear, just as in Davy's ordinary lamp the combustion of the explosive mixture within the gauze cage appeals to the eye. Several forms of the singing-lamp have been devised. In one form, adapted to the use of the viewer, the air gains admission near the top of the lamp, while in another form, adapted to the purposes of the working miner, a brilliant light is obtained by the use of paraffin oil.

From an official report recently issued by a special committee appointed by the War-Office to enquire into the subject of the transport and storage of Lithofracteur, it appears that the committee, whilst fully admitting the safety of the explosive under ordinary conditions, find that, on exposure to a high temperature, part of the nitroglycerine separates from the solid substance by which it was absorbed, and exudes from the cartridge. The explosion of a single drop of the pure nitroglycerine might determine the explosion of the entire charge. It appears, then, that lithofracteur cannot be said to be a perfectly safe mining explosive.

It is said that no fewer than twenty-two distinct forms of machine for rock-boring have been constructed by Messrs. McKean and Co. The "McKean Rock Drill"—the final result of their labours in modifying the Haupt Drill—has lately been in successful operation on hard Scotch granite, in which it has been drilling holes 2 inches in diameter, at the rate of about 6½ inches per minute. For use in the mine, the drill is mounted on a vertical column fixed rigidly by a telescopic and screw adjustment. The boring-tool is securely fixed to a solid piston moved by steam or compressed air, and kept working in suspension, the valve-ports being opened and closed so as to leave a cushion at each end of the piston in its reciprocating movement. During the back stroke of the piston, a rotatory motion is imparted to the boring-tool.

That coal-cutting machinery has not been more extensively introduced into our collieries seems to be due, in great measure, to the enormous cost of laying down pipes from the surface to convey the air into the pit, and thence to the part of the workings where the machine happens to be in use. This difficulty may be to a great extent overcome by a recent invention patented by Mr. E. T. Simpson and Mr. F. Hurd. These gentlemen propose to use a portable air-compresser, which may be worked in any part of the mine by manual or animal power.

A correspondent of the "Mining Journal" offers for public competition a prize of £20 for an essay containing the best description of the principal machines and tools used in general mining and quarrying operations, with the results of the writer's experience in their use.

At the recent Cornwall Polytechnic Exhibition, some experiments were made with the Burleigh rock-drill, and with a model illustrating Scott's plan for ventilating mines by injecting steam into one of the shafts. Among the other exhibits were a model of Willoughby's Spring Stamps, one of Borlase's patent ore-dressing machines, and an improved pulveriser by Mr. Sara, of Redruth.

METALLURGY.

From an analysis recently published in the "Chemical News," it appears that a pure wrought-iron has lately been made on a large scale at the Bowling Iron Works, at Bradford, by the Henderson process. The wrought-iron contained 99·5 per cent of iron (by direct determination), with 0·272 per cent of carbon, and the barest traces of sulphur, whilst phosphorus, silicon, and manganese were entirely absent.

The manufacture of charcoal-iron has just been resumed in South Staffordshire. On the same site as that originally occupied by one of the earliest charcoal furnaces in the county, Mr. Light has recently erected a furnace and blowing-engine for the make of charcoal pig-iron, and other furnaces are in progress. The charge is raised in a cage moved by a water balance-lift. The furnaces are blown by an engine having a blowing cylinder above the steam cylinder, and the links connected to the beam working between the two cylinders. By this means the engine works smoothly without a parallel motion. The charcoal-iron run from these furnaces will bear the brand, "Bradley Bridge Charcoal."

Within the last few months the "Barron Steel Process" is said to have been successfully at work in the United States. The cast-iron tools to be converted into steel are first placed in revolving drums, and the rough surface which the castings present as they leave the mould is thus worn smooth by attrition. They are then packed in layers in iron boxes, closely covered with clay, and subjected to the action of oxide of iron or some other decarburising agent. After being annealed in these boxes for a period of from three to six days, the tools are brought into the state of malleable iron, and it only remains to convert them into steel. This conversion is effected by exposing them in a large retort, in the centre of an oven, to the action of certain gaseous compounds of carbon. By this means about a ton of iron tools in the retort may be converted into steel in from eight to ten minutes.

Some improvements in the mode of removing slag from the blast-furnace have been patented by Mr. D. Joy, of Middlesbro'. The slag, as it is run from the furnace, is received into recesses formed on the rim of a cylinder or wheel, and, having been carried through part of a revolution, is discharged from the cylinder. During its passage through the machine, the slag is cooled by a blast of cold air, or by both air and water, the water being the waste liquid from the hydraulic machine by which the cylinder is caused to rotate.

At the recent meeting of the Iron and Steel Institute at Glasgow, Mr. John Mayer read a paper on the rise and progress of the Scotch iron manufacture. The author traced the history of the trade from 1760, and, of course, dwelt especially on the discovery of the black-band ironstone by Mushet, and the introduction of the hot blast by Neilson—the two great events which gave such powerful impulses to the development of the iron industry in Scotland.

Two papers on Reversing Rolling Mills were brought forward at the same meeting—one by Mr. J. D. Napier, of Glasgow, and the other by Mr. G. Stevenson, of Airdrie. In the discussion on the comparative merits of the two systems introduced by these gentlemen, opinion seemed to favour Mr. Napier's differential gear, an arrangement which has been successfully employed at the Codnor Park Iron Works in Derbyshire, the property of the Butterley Company.

MINERALOGY.

A new mineral species, apparently of much interest, has been described in the "Chemical News"* by Mr. Hugo Tamm. It presents the form of a dense, crystalline, dark steel-grey powder, consisting of distinct crystals strikingly resembling those of silicon, and having a specific gravity of 12·5. Unfortunately but a very small quantity of the substance has been at the disposal of the analyst, but it has been satisfactorily determined that the mineral contains 88·05 per cent of tungsten, 5·6 of iron, and 0·15 of manganese. There yet remains 6·2 per cent of some undetermined substance, and it is difficult to conjecture what this missing constituent is likely to be. Mr. Tamm is prepared, however, to say that it is not oxygen, or sulphur, or arsenic; but suggests that it may be phosphorus, nitrogen, hydrogen, or even a new element. Should it be condensed hydrogen, the mineral will be of great interest, as the first example of a native alloy containing hydrogenium.

* Vol. xxv., No. 659; July 12, 1872, p. 13.

After all, the author admits that the deficiency may be due to an analytical error. As tungsten and iron are evidently the main constituents of this mineral, the name *Ferro-tungstine* has been provisionally adopted. Mr. Tamm had indeed proposed that it should be termed *Crookesite*, but it will be remembered by our mineralogical readers that this name has already been appropriated to a Swedish species unusually rich in thallium, described a few years ago by Prof. Nordenskjöld.* Under these circumstances Mr. Crookes suggests that, as soon as the missing constituent be identified, the new mineral may appropriately be designated *Tammite*.

In compliment to the Keeper of Minerals at the British Museum, the name of *Maskelynite* has been bestowed by Dr. Tschermak on a colourless cubic silicate, having the composition of labradorite, from a meteorite which fell at Shergotty in India, on the 25th August, 1865. The maskelynite is accompanied by augite and magnetite. Tschermak has also described a meteorite which fell near Gopalpoor on May 23rd, 1865, and which consists of nickel-iron, magnetic pyrites, chromite, chrysolite, bronzite, and a mineral resembling felspar.

During the Swedish Expedition to Greenland, in 1870, it was observed that many of the holes in the ice were covered at the bottom with a peculiar deposit in the form of a grey powder, often agglomerated into small globular masses. This glacial sand has been examined both microscopically and chemically. It does not possess the characters of a clay, but the main ingredient seems to be a sandy trachytic mineral, for which Prof. Nordenskjöld suggests the name *Kryokonite*. The origin of this ice-dust appears enigmatical: whether it comes from the basalt region near the coast, or from the supposed volcanic tracts in the interior of Greenland, or whether it is of meteoric origin, are questions which the Professor at present hesitates to answer.

Under the name of *Manganophyll*, Herr Igelström has described a new species of mica from the iron and manganese mine of Pajsberg, near Filipstadt, in Sweden. It possesses a colour varying from bronze to copper-red, and appears to crystallise in the hexagonal system. It contains 21·4 per cent of protoxide of manganese, and is hence the richest manganese-mica hitherto known. Manganophyll appears to stand very close to Breithaupt's Alurgite, from the manganese mines of St. Marcel, in Piedmont; and it is likely enough that the two species may be united.

A preliminary notice of a new lead-ore has been published, in Leonhard and Geinitz's *Jahrbuch*, by Dr. Laspeyres. It seems to be a hydrated sulphato-carbonate of lead, a compound entirely novel in the mineral kingdom. The new species was brought from Sardinia by Herr Max Braun, the well-known mining engineer at the Vieille Montagne Company's zinc mines near Aix-la-Chapelle. Unfortunately the name *Braunite* has been appropriated, as every mineralogist knows, to an oxide of manganese; and the Sardinian lead-ore is, therefore, to be called *Maxite*.

Some confusion is likely to arise in the application of the name *Roepperite*, a name which has been lately applied by Dr. Kenngott to denote a manganesian dolomite, described by Roepper, from Stirling Hill, New Jersey; whilst Prof. Brush has independently used the same name to designate a chrysolite containing zinc, described also by Roepper from the same locality, but distinguished by Kenngott as *Stirlingite*.

Another new species has been detected by Prof. Wiesbach, of Freiberg, among the minerals recently raised at Schneeberg, in Saxony. The mineral is an arsenate of uranium and copper, and has been christened *Zeunerite*. It strikingly resembles copper-uranite in its grass-green colour and its crystalline form, but of course differs in being an arsenate instead of a phosphate.

For upwards of thirty years corundum has been recognised at several of the gold-washings in the mountainous countries of North Carolina and Georgia, in the United States. But within the last two or three years, the search for

* See Quarterly Journal of Science, October, 1867, p. 542.

an improved kind of emery has led to the discovery of many new localities of corundum in this region. Some of these localities have been described by Prof. C. U. Shepard. The corundum seems to be found across a stretch of country at least 170 miles long and about 10 miles broad; but it is likely that this zone will be yet much extended. Throughout this region gneiss is the prevailing rock, but the corundum occurs directly in a chrysolitic rock, resembling serpentine.

Some crystallographic studies of the beautiful specimens of *Datolite*, well known from the Bergen Hill Tunnel, in New Jersey, have been described in "Silliman's Journal" by Mr. E. S. Dana. Upwards of 200 specimens have been carefully examined; four distinct types are recognised, and many new forms are described.

Mr. T. D. Rand has described some pseudomorphs of serpentine in form of staurolite, from the neighbourhood of the soapstone quarry, on the north-east bank of the Schuylkill, between Philadelphia and Montgomery counties. Six-rayed stellate forms of serpentine have been found, identical in shape with crystals of staurolite. Mr. Rand also describes the species Hisingerite from the Gap Mine, Lancaster Co., Pennsylvania; and an alkaline incrustation, consisting chiefly of sulphate of soda, from some decomposed mica-schists cut through on the new line of the Philadelphia, Wilmington, and Baltimore Railroad.

An analysis of the mica from the granite of the Adamello group, south of the Ortler, in Tyrol, has been published by M. A. Baltzer. From this analysis it appears to be an iron-magnesia mica allied to Soltmann's lepidomelane and to Scheerer's mica, from the Norwegian zircon-syenite. The Adamello granite was regarded by Zirkel as a diorite, and by Vom Rath as a distinct species of rock, whilst according to Baltzer, it is merely a variety of granite.
The same authority has examined the Blegi oolite from Glärnisch, in the Canton Glarns. This is an oolitic iron-ore, evidently deposited from the old Jura sea. Treated with water, it yields a solution containing many of the constituents of sea-water. The saline matter must have been derived from the sea-water included in the mass at the time of its deposition.

Dr. Carl Klein, of Heidelberg, has published in Leonard and Geinitz's *Neues Jahrbuch*, the results of his crystallographic studies on the epidote and apatite of the Sulzbachthal in the Pinzgau.

Extensive deposits of *Cryptomorphite* are said to have been recently found in Nevada. The mineral occurs in large nodules, and is described as having, in many cases, the appearance of French chalk.

ENGINEERING—CIVIL AND MECHANICAL.

Guns and Armour.—On the 5th of last July an important experiment was made with our present system of guns and armour for our ironclads by an attack upon the turret of the armour-clad steamer *Glatton* by one of the 25-ton guns fired from the *Hotspur*. The result of this contest proved that up to the present the art of defence is in advance of that of attack, inasmuch as the vessel attacked came out of the contest free from any material injury. In this respect the present experiment corresponds in its results very much with those obtained by the trials which took place in 1866, when the armour of the *Royal Sovereign* was attacked by the 9-inch 12½-ton gun of the *Bellerophon*, and with the results of the experiment made in 1861 against the cupola gun shield of Captain Coles on board the *Trusty*. In each case the heaviest available artillery was brought to bear against the shield, and in each case the defence proved superior to the attacking force. So far, then, it may be inferred that our present system of armour-plating is sufficient protection against the heaviest artillery that can be brought against it; to what extent it would prove effective against the enormous masses of iron which low pressure guns upon Mr. Bessemer's process might hurl upon it is a question which may perhaps be safely deferred until that system has been adopted.

Railways.—The most important question in connection with railways that has been brought prominently forward during the past quarter is that of the proposed construction of a line along the valley of the Euphrates river, as a means of obtaining more expeditious access to our Indian possessions. This subject has recently been under investigation before a Parliamentary Select Committee, whose report has now been published. This document contains very extensive information regarding all that has hitherto been done with the view of testing the practicability of constructing a Euphrates Valley Railway, and regarding the various alternative lines that have been proposed for connecting the Mediterranean Sea with the Persian Gulf by means of an iron road. The recommendations of the committee are in favour of adopting Alexandretta as the terminus of the proposed line at its Mediterranean end, and the port of Grane for the Persian Gulf terminus; with regard to the latter, however, they think that a local inquiry conducted by competent scientific authorities is needed before finally deciding. With regard to the route, that by the Euphrates is preferred to the Tigris Valley, as being considerably the shorter and the cheaper to make, although the latter route might attract the larger amount of traffic, and would connect itself better with the projected Turkish system. In the absence of any actual surveys it has been impossible to fix any sum as the probable cost of either route, but it is thought probable that the sum of ten millions sterling would be ample to cover the expense of the shorter of the two routes.

From Mr. J. Danvers's annual report on Indian Railways, which has just been published, we learn that there are now in existence in India nine guaranteed railway lines, four of which have now been completed, viz., the East Indian, the Great Indian Peninsula, the Scinde, Punjab, and Delhi, and the Eastern Bengal. The total length of the open lines is 5136 miles, and further lengths aggregating 940 miles are under construction. There are also twelve state railways sanctioned, having a total combined length of 157½ miles. Of these latter four are completed, namely the Nulhattee branch to the East Indian Railway, the Calcutta and South Eastern, the Khangaon, and the Oomrawattee lines. The total length of state lines open is 68½ miles, and 1503 miles therefore remain to be completed at present. Most of the guaranteed lines have a 5-feet 6-inch gauge, whilst the gauge adopted for the state railways is 1 metre, or 3 feet 3⅜ inches; exceptions have, however, been made in the case of some of those lines. The total estimated cost of all lines sanctioned or completed in India amounts to little less than 110 millions sterling, of which about 92 millions have already been expended.

The St. Gothard Railway Company has just concluded a contract with a Swiss contractor, M. L. Favre, of Geneva, for the construction of a tunnel under the Alps between Goeschenon and Airolo. This will be a more gigantic operation than the Mont Cenis tunnel, inasmuch as its length will be 14·8 kilometres, or rather more than 9 English miles. The estimated cost for excavating this great tunnel is 50 millions of francs, or, say, 2 millions of pounds sterling, and the probable time that it will take in execution is estimated at about 8 years.

Tunnels, however, cannot in all cases be adopted by railways requiring to pass mountainous obstructions, and lines laid on steep inclines must then be constructed. The Cantagallo Railway, in Brazil, meets with such an obstacle in the Organ mountains, which intervene between the important coffee-producing district of Cantagallo, situated on a high level, and Caxoeira in the plains, whence a railway already exists to Rio Janeiro. This railway, from Caxoeira across the mountain passes to Nova Friburgo and Cantagallo, is being constructed with the same gauge as the Mont Cenis Railway, namely 1·1 metres, or 3 feet $7\frac{5}{16}$ inches, and that part of it which traverses the coast side of the mountains will be laid with the central rail. In a length of 7¾ miles, this mountain portion of the line rises about 3000 feet, the gradient varying from 1 in 20 to 1 in 12, and being for the greater part of the length 1 in 13, while the curves are of 40 metres, or about 140 feet radius. In descending from the summit level on the Cantagallo side, the mid-rail is dispensed with, the gradients over the 12 miles extending to Nova Friburgo being easy.

The value of light railways, especially for thinly populated districts, has recently formed the subject of a paper, read by Mr. Henry S. Ellis before the Exeter Chamber of Commerce. Light railways are of three kinds, namely—(1.) Those which follow as nearly as possible the surface of the ground, avoiding the hills and the necessities for expensive over or under bridges, and, by travelling at low speeds, avoiding the necessity for expensive signal and other apparatus usually required by the Board of Trade for through lines. The gauge is usually the national one, viz., 4 feet 8½ inches. (2.) The patent narrow gauge railway, invented by Mr. Fell, which, by being constructed on timber supports of different heights, to suit the undulations of the country, avoids, to a great extent, severance of land and expensive cuttings and banks. The gauge of such lines varies from 8 inches to 2 feet. A railway of this description, about a mile in length, has been recently constructed at Aldershot, in 45 days, for the purpose of conveying stores to the Victualling Office. The gauge of this line is only 18 inches, but it is capable of conveying siege guns 7 tons in weight. The cost is about £2000 per mile. (3.) The wire tramway, which, like the Fell system, is mounted on iron or timber supports, to suit the undulations of the country; but it is only adapted for the conveyance of loads not exceeding 10 cwts., and is generally employed for short distances only—250 yards to 3 or 4 miles. One has been constructed as long as 15 miles.

Mr. Ellis then gave some very interesting statements regarding a light railway recently constructed by the Duke of Buckingham, from which the following particulars have been taken:—The cost of the Wotton Railway, including sidings and two goods' sheds (one 25 feet by 25 feet, and one 60 feet by 25 feet), is rather less than £1400 per mile, exclusive of the value of the land. The main line is very nearly 7 miles long. The gradients from Quainton to Wotton are favourable,—two short inclines of about 1 in 78, in opposite directions, being the worst,—and the general inclination being downwards to Wotton, which is 50 feet below Quainton. The gradients from Wotton to Brill are heavy, the ascent being—with one exception, not quite a quarter of a mile—continuous for 2¾ miles, varying from 1 in 100 to 1 in 51. The latter is a quarter of a mile; but an incline of 1 in 64, on a curve of 12 chains radius, for a quarter of a mile, is quite equal to the steep gradient. The total ascent is about 130 feet in this portion of 2¾ miles. The line was commenced on the 8th of September, 1870. The first portion to Wotton was in use on the 1st of April, 1871, and the greater portion of the remainder was brought into use for minerals, agricultural produce, &c., in November: the last quarter of a mile and Brill Station were brought into use last April, and another branch of 1¼ miles in length was in course of construction last May.

Continuous Brakes.—With the extensive development of railway traffic, and the extension of train services on existing lines, the necessity for some more powerful control over the train, when going at high speeds, makes itself more and more felt, especially on heavily worked lines. We have, on more than one occasion, recently noticed the different designs which have from time to time been put forward, with the view of providing an efficient continuous brake throughout the entire train, so that it might be placed under more effective control, by enabling it to be suddenly brought to a stand-still, and so avoid collision or other accidents which are now too often experienced. The best brake of this kind that has hitherto been introduced is that known as the Westinghouse Air-brake, which, although only just introduced into this country, is already in use on some 20,000 miles of railway in America, where it has been applied to 1200 locomotives and 4000 cars. It has already been applied in this country to trains on the London and North-Western, the Caledonian, and the Charing Cross Railways. Mr. Westinghouse's invention consists of three distinct parts:—First, the arrangements for compressing the air; secondly, the appliances by which the air so compressed is conducted along the train, and made available for applying the brakes; and thirdly, the construction of the signalling apparatus. The latter, however, can only be considered as an adjunct to the brake, as it is perfectly complete without it. The first part of the apparatus consists of an air-compressing pump, fixed to the side of the fire-box of the locomotive, having a steam-cylinder and an air-cylinder, by means of which the air is compressed into a reservoir, placed beneath the foot-plate, and having a capacity of about 12 cubic feet. From this reservoir the air is led by a pipe to a three-way cock, placed so as to be conveniently under command by the engine-driver, the two other branches of the cock communicating the one with the atmosphere, and the other with a pipe leading to a duplicate line of pipes running under the carriages the whole length of the train, by which the compressed air is led to the brake machinery. These latter pipes are connected together between the vehicles by means of lengths of india-rubber tubing. Under the tender, and under each carriage to which the brake is applied, is placed an air-cylinder with a piston working inside, the piston-rod from which communicated with a thrust-rod, and through it the pressure of the air upon the piston is applied to the brake gear. By the adoption of a special mode of connection the brakes are left free for application by the ordinary hand apparatus, if necessary. The application of the brakes is effected simply by turning the three-way cock above referred to, so as to admit compressed air from the reservoir into the lines of pipes extending through the train. The air thus admitted into the pipes passes into the cylinders, forces out the pistons, and thus, through the intervention of the thrust-rods and brake gear, exerts the requisite pressure on the brake-blocks. During a recent experimental trip on the South-Eastern Railway—the train consisting of an engine, tender, and six carriages—the brake was first put on at a time when the speed was 30 miles an hour, and the train was stopped in 18 seconds, and in a distance of 149 yards, up a gradient of 1 in 142. The second stop was made on a rising gradient of 1 in 120, the speed being the same as before. In this case the time taken in arresting movement was 16 seconds, and the distance 107 yards. The speed of the train was 55 miles an hour when the third stop was made, on a falling gradient of 1 in 120, the time required from the moment of applying the brake to stopping the train being 32 seconds. On the fourth and last occasion the speed of the engine had been increased to about 60 miles an hour at the moment of putting on the brakes, and the train was brought up in a distance of 400 yards, the road in this case being tolerably level.

Portland Breakwater.—The completion of this magnificent work was inaugurated by His Royal Highness the Prince of Wales, on Saturday, the 10th day of August last. So far back as 1794 the first conception appears to have been entertained of the advantages that would be derived from the construction of a breakwater at Portland, but it was not until fifty years afterwards that any active steps were taken towards the accomplishment of that object. In 1847 an Act was passed authorising the construction of the breakwater, and the preliminary works were commenced in the same year. The work of constructing the breakwater was practically commenced in 1849, from designs by the late Mr. Rendel, Mr.—now Sir John—Coode being the resident engineer. On the 25th of July, in the same year, the Prince Consort laid the foundation-stone, and ten years later the works were so far advanced as to afford a safe anchorage during a storm. The Portland breakwater works commence with a pier, which starts from the island of Portland near the point where it is connected with the mainland by the Chesil Bank. This pier runs due east for a length of about 1900 feet, at which point there occurs an opening 400 feet wide, and having a minimum depth of 45 feet, to admit of the entrance of ships of war of the largest class from the southward. On the other side of this opening the breakwater proper commences, and is carried out to sea for a distance of 6000 feet, from its starting-point.

The pier, or inner breakwater, consists of a rubble mound, composed of stones of all sizes. After the mound had been consolidated, a trench was excavated within it to the level of low water at spring tides, and a wall of masonry was erected therein. The face-courses of this wall, up to 6 feet above high-water level, are hewn granite. The sea-wall is strengthened by counterforts, 20 feet apart, and connected by arches, so that a platform is obtained 15 feet in width, exclusive of the footway and the parapet. The breakwater proper is simply a rubble bank, the material of which it is formed being for the most part the "cap-stone," which covers the valuable Portland stone, but which of itself is valueless, except for this purpose.

The total length of the whole work is about $1\frac{5}{8}$ miles, the width of the structure at the base being 300 feet, at low-water level from 90 to 100 feet, and at the top 60 feet. The average height from the bottom is 70 feet. The sheltered area afforded by these works is about 2100 acres in extent up to low-water line. Connected with the works are two forts, an inner and an outer one, placed one at the end of the pier and the other at the end of the breakwater proper. The inner fort is 100 feet in diameter, mounts 8 guns, and stands in $9\frac{1}{2}$ fathoms of water. The outer fort, on the North Head, has a rubble base, 45 feet high, and containing 140,000 tons of stone. The diameter of the fort is 200 feet, and it stands in 10 fathoms of water.

Vienna Exhibition.—On the 1st of May, in next year, an Universal Exhibition is to be held at Vienna, which, from the dimensions proposed to be given to the building, bids fair entirely to eclipse all former Exhibition buildings, the space devoted for it being no less than 2,783,809 square yards. These proportions will be better appreciated by a comparison with the areas covered by former Exhibitions, which were as follows:—

London (Hyde Park), 1851	94,000 square yards.
Paris (Champs Elysées), 1855	123,214 " "
London (Brompton), 1862	222,316 " "
Paris (Champ de Mars), 1867	527,645 " "

The design for the Vienna Exhibition building is due, we understand, to Mr. Scott Russell. The central part of the building consists of an imposing iron rotunda, or dome, with an outside diameter of 353·68 feet, and a height of 275·5 feet. A conical iron roof, rising to a height of 158·09 feet, is supported by 31 iron columns, each 79·99 feet in height. This conical roof carries an iron lantern, having an outside diameter of 106·27 feet, and a height of 32·8 feet up to the roof, which latter rises 24·27 feet. Upon this is placed another structure, 26·2 feet in diameter and 60·6 feet high. The whole space covered by the rotunda has an outside circumference of 1111·20 feet, whilst the inside circumference is 1038·28 feet, having an available space of 9722·28 square yards. The rotunda communicates with the main gallery, or nave, which has a width of 82 feet and a total length of 2968·4 feet: this nave is crossed, at equal distances, by 16 cross naves, 49·2 feet wide and 672·4 feet long, creating thus, at both sides of the main gallery, 24 courts, which are enclosed at three sides, and which have the same length as the cross nave and a width of 114·8 feet.

Meetings of Societies.

Mechanical Engineers.—The Institution of Mechanical Engineers held their Annual Meeting at Liverpool, under their President, Mr. C. W. Siemens, on the 30th of July last and following days. In his Address the President referred principally to the question of economy of fuel, which, in the presence of ever-increasing demand and of failing supply, is rapidly rising into a question of national importance; and he remarked that it would not be difficult to prove that—in almost all the uses of fuel, whether to the production of force, to the smelting and re-heating of iron, steel, copper, and other metals, or to domestic purposes—fully one-half of the present enormous consumption might be saved by the general adoption of improved appliances which are within the range of our actual knowledge.

The papers read at this meeting were as follows, but space will not admit of any more full reference regarding them:—

1. "On the Progress effected in Economy of Fuel in Steam Navigation, considered in Relation to Compound Cylinder Engines and High-Pressure Steam," by Mr. F. J. Bramwell.
2. "On the Application of Water Pressure to Shop Tools and other Engineering Works," by Mr. Ralph Hart Tweddell.
3. "Description of Coal-Cutting Machinery with Rotary Cutter, worked by Compressed Air," by Mr. Robert Winstanley.

4. "On Buckholz's System of Decorticating Grain, and making Semolina and Flour by means of Fluted Metal Rollers," by Mr. W. Proctor Baker.

Iron and Steel Institute.—The fourth Provincial Meeting of the Iron and Steel Institute was held this year in Glasgow, at the beginning of August last, under the presidency of Mr. Henry Bessemer. In his Address the President dwelt upon the various centres of iron manufacture which the Institute had successively visited, and the special branches of the manufacture for which each had become famous. The following papers were then read and discussed:—

1. "On the Geological Survey, and the Geological Position of the Coal and Ironstone Strata on the West of Scotland," by Mr. James Geikie, F.R.S.E.
2. "On the Rise and Progress of the Iron Manufacture in Scotland," by Mr. John Mayer.
3 and 4. "On Reversing Arrangements for Rolling-Mills," by Mr. Graham Stevenson and Mr. R. D. Napier.
5. "A new Miners' Safety-Lamp, for Indicating by Sound the Presence of Explosive Mixtures of Gas and Air, based on a new form of Singing Flame," by Dr. A. K. Irvine.
6. "On the Rise and Progress of Iron Steam Ship-Building on the Clyde," by Mr. D. Rowan.
7. "On Lauth's System of Rolling Iron by Three-High Rolls," by Mr. Lauth, of Pittsburg, United States.
8. "On Further Improvements in Spencer's Rotary Puddling Furnace," by Mr. Adam Spencer.

LIGHT.

An alcoholic solution of fuchsine introduced into a hollow prism produces a highly anomalous spectrum, which, instead of proceeding regularly from the red to the violet, like the ordinary solar spectrum, stops at a certain point, returns backward, then stops again, and resumes a direct course to the end.

Drs. Zöllner and Vogel have succeeded in measuring the velocity of the sun's rotation by means of the spectroscope. The value obtained for the motion of a point on the sun's equator is 0·42 German mile per second from one series of observations, and 0·35 mile from a second. This new application of the spectroscope visibly recording the sun's rotation will develope great interest, and we are sorry that the method of observation cannot be given shortly.

Prof. Young has observed some peculiar phenomena relating to vision whilst pursuing a series of experiments. He writes as follows:—"In the course of some experiments with a new double-plate Holtz machine, belonging to the college, I have come upon a very curious phenomenon, which I do not remember ever to have seen noticed. The machine gives easily intense Leyden-jar sparks, from 7 inches to 9 inches in length, and of dazzling brilliance. When, in a darkened room, the eye is screened from the direct light of the spark, the illumination produced is sufficient to render everything in the apartment perfectly visible; and what is remarkable, every conspicuous object is seen twice at least, with an interval of a trifle less than one quarter of a second—the first time vividly, the second time faintly; often it is seen a third, and sometimes, but only with great difficulty, a fourth time. The appearance is precisely as if the object had been suddenly illuminated by a light, at first bright, but rapidly fading to extinction, and as if, while the illumination lasted, the observer were winking as fast as possible. I see it best by setting up, in front of the machine, at a distance of 8 or 10 feet, a white screen having upon it a black cross, with arms about 3 feet long and 1 foot wide, made of strips of cambric. That the phenomenon is really subjective, and not due to a succession of sparks, is easily shown by swinging the screen from side to side. The black cross, at all the periods of visibility, occupies the same place, and is apparently stationary. The same is true of a stroboscopic disc in rapid revolution; it is seen several times by each spark, but each time in the same position. There is no apparent multiplication of a moving object of any sort. The interval between the successive instants of visibility was measured roughly as follows:—A tuning-fork, making 92½ vibrations per second, was adjusted so as to record its motion upon the smoked surface of a revolving cylinder, and an electro-magnet was so arranged as to record any motion of its armature upon the trace of the fork; a key connected with this magnet was in the hands of the observer. An assistant turned the machine slowly, so as to produce a spark once in two or three seconds, while the observer manipulated the key. In my own case, the mean of a dozen experiments gave 0·22″ as the interval between the first and second seeing of the cross upon the screen, separate results varying from 0·17″ to 0·39″. Another observer found 0·24″ as the result of a similar series. Whatever the true explanation may turn out to be, the phenomenon at least suggests the idea of a reflection of the nervous impulse at the nerve extremities, as if the intense impression upon the retina, after being the first time propagated to the brain, was there reflected, returned to the retina, and from the retina—travelling again to the brain—renewed the sensation. I have ventured to call the phenomenon 'recurrent vision.'"

Investigations to test the influence of pressure on the spectra of gases have been carried on by M. Cailletet. He fixed two platinum wires in the end of a thick glass tube, into which the gases were passed. The spark from an induction coil connected with three Bunsen elements passed between the wires. At ordinary pressure the bright lines of the spectra of the gases appeared on a slightly illuminated ground, and as pressure was increased they grew brighter; but they by-and-bye became merged in a continuous spectrum, whose brightness also increased with the pressure. At a certain pressure (between forty and fifty atmospheres) the discharge suddenly ceased; and though the battery power was increased, and the distance between the platinum wires reduced to one-half millimetre, it was not possible to obtain the spark beyond this point. It is thus seen that a spark which passes easily in the rarefied gas of Geissler tubes, or the electric egg, meets with considerable resistance in compressed gas. The brightness of the spark at the point beyond which the discharge is unobtainable is 200 times greater than at ordinary pressure.

Dr. Burt's investigations upon the subject of the influence of variously coloured light upon vegetation shows that green light is almost as fatal to vegetation as darkness; that red light is very detrimental to plants, although in a less degree than green light; that although yellow light is far less detrimental than the preceding, it is more injurious than blue light; and that all the colours taken singly are injurious to plants, and that their union in the proportion to form white light is necessary for healthy growth.

M. P. Thomas, acting under instructions from the Paris Society of Civil Engineers, has reported upon the process of the oxygen light of Tessie du Motay. The report simply treats of technical advantages and disadvantages, leaving out of sight the economical question, which is somewhat to be regretted, in view of the indistinct statement of the causes which have led to its removal from some of the streets of Paris where it had been introduced. The conclusions arrived at are the following:—1. Theoretically, the combustion of oxygen does not increase the illuminating power of a given volume of gas. 2. Practically, however, it enables a burner to consume four times the quantity of gas that can be burned in air, without detriment to the utilisation of the light which may be developed. In particular, it utilises the entire luminous capacity of the gases, however rich, and in almost any quantity. Consequently it would be disadvantageous to employ it for ordinary street-lighting, on account of the limited quantity of gas consumed by the burners, the only advantage gained being the beauty of the light, provided the gas is very rich. Here, unquestionably, would arise the objection of expense, from the complication of the apparatus. But it is very advantageous, and the more so in direct proportion to the richness of the gases employed, for great centres of light (sun-burners, &c.), where a large volume of gas is to be consumed without loss.

Prof. Morton writes to say that, from information received from Dr. Schellen, it would appear that M. Dubosçq had previously been the maker of an apparatus for projecting horizontal objects, which involved all the essential features of the vertical lantern described by Prof. Morton in an earlier number of this Journal (p. 396, July, 1872), the apparatus constructed by M. Duboscq being, however, entirely unknown to Prof. Morton.

Microscopy.—A new instrument for making microscopical drawings is described by Mr. Isaac Roberts, F.G.S., in the "Monthly Microscopical Journal," vol. viii., p. 2. It consists of an adaptation of the pantograph used in copying drawings. The smaller ends of the levers carry a thin glass disc, with two cross lines ruled upon it: this is inserted in the eye-piece through a slit in its side, and acts as a pointer; the pencil is attached to the opposite extremity of the levers. When in use, the paper is supported on a desk level with the slit of the eye-piece. The pencil is guided with the right hand, the centre of the cross lines being moved over the object to be delineated while looking through the microscope. This contrivance, although very ingenious, has most probably the defects of the ordinary pantograph, which can only be used with advantage for the reduction of drawings, its powers of execution being defective when used to enlarge, or even to copy, on the same scale.

Mr. Walter White, in "Science Gossip" for August, describes an ingenious machine for cutting vegetable sections. The great novelty is the use of a wedge for raising the substance to be cut, instead of the usual screw with micrometer head. The author claims for his invention the advantages of cheap construction and efficiency. As a cutting instrument, Mr. White recommends a good ordinary razor, fixed in a bradawl handle and carefully sharpened. For cutting transverse sections, the stems—previously prepared by soaking--are fixed in a cork just fitting the tube of the section-instrument; herbaceous stems are best packed in strips of the same stem. Leaves should be placed between the two halves of a cork, cut lengthwise. Seeds, and other small objects, are best cut when bedded in a mixture of wax and spermaceti, formed into a plug by casting in the tube of the section-instrument. Wood for cutting longitudinal sections can be fixed in the same manner. The razor, while cutting, should be kept constantly wetted with methylated spirit, in which stems for cutting are most conveniently preserved until wanted for sections.

Dr. R. H. Ward, in a paper read before the Medical Society of New York, gives a valuable analytical catalogue of the objectives and medical and students' microscopes procurable in England, on the Continent, and in America. The table of objectives comprises those of twenty-seven makers, giving the nominal focal length, angular aperture, and price, with numerous explanatory remarks. This is followed by a synopsis of students' microscopes, in which the advantages and disadvantages of the several patterns of stand, the material used in construction, and the various appendages of the instrument, are all clearly stated. A list of accessory apparatus is given, distinguishing between that absolutely needed and that which may for some special purpose be desirable. The paper concludes with a list of nineteen students' microscopes of American construction, giving name of maker, model of stand, height in inches, weight, diameter of tube, material, adjustments furnished, details of stage, mirror, oculars, objectives, range of magnifying power, accessory apparatus, and price in dollars. It would have been well had this carefully classified table included the instruments of English and Continental opticians, as in the author's list of objectives. The paper is, however, valuable as giving in a convenient form the substance of the catalogues of a large number of microscope makers. The paper is reprinted in the "Monthly Microscopical Journal" for August.

M. Mouchet, in advocating the use of coloured glasses in Microscopy, for the purpose of saving the eye-sight, reviews carefully the various positions in which they may be placed: these are four in number,—before the mirror, under the object, between the objective and eye-piece, and, lastly, above the eye-piece itself. The position before the mirror necessitates the employment of glasses of a certain size, but gives good results, and, when the coloured glass is placed in contact with the flat side of a plano-convex condenser, causes perhaps the minimum loss of light. The tinted glass placed under the object increases the thickness of the mounting, and prevents very oblique light passing through, and also interferes with the use of illuminators, such as the achromatic condenser, &c.; glasses within the body cause trouble and loss of time in fixing and removing them. The most convenient position seems to be above the eye-piece. For this purpose M. Mouchet has constructed a revolving diaphragm pierced with nine openings, each 1 centimetre in diameter. Seven of the openings are fitted with coloured glasses corresponding to the tints of the solar spectrum; another with a silvered glass, according to Foucault's process; and the ninth with a very finely roughed glass, which may be removed at will, and thus leave one opening free. This revolving diaphragm is attached to a ring placed on the tube which receives the eye-piece. Although M. Mouchet considers the apparatus useful in observations by direct sunlight, it seems to have been forgotten that in the employment of condensed sunlight the object needs protection from heat as much as the eye of the observer, and this is effectually done by the cell of ammoniacal sulphate of copper used by Dr. Woodward in modifying the illumination of objects to be photographed by sunlight.

HEAT.

M. Toselli has recently invented a cooling machine, which he terms a dynamic refrigerator. It consists of a revolving disc, formed of a metallic tube bent into a complete spiral, having one end open, and with the other end communicating by a hollow shaft or axis of rotation with an external tube, this communicating with a worm contained in a separate vessel. This worm terminates in a discharge-pipe, having outlet into another vessel containing the revolving disc. The disc is half immersed in cold water, and as the surface of the disc above the water is continually wet, there is exposed considerable evaporating surface. At the same time a stream of water is forced through the hollow spiral, parting with some of its heat under the influence of the evaporation and by radiation, assisted by a current of air from a fan worked by the same motive power. The stream of water lowered in temperature passing through the worm refrigerates the liquid contained in the worm-tub. The decrease in temperature is, to a certain extent, dependent upon the hygrometric condition of the atmosphere. The least effect is a diminution of 4° or 5° F., while the maximum effect in strong sunlight amounts to 32° to 33° F. The invention, if found practicable, will be of great service to the brewer, and to manufacturers, where large quantities of fluid have to be cooled quickly.

Experiments on the joint effect of pressure and heat upon paraffin have been made by T. E. Thorpe and J. Young on the larger scale. 3½ kilos. of paraffin (fusing at 46°, sp. gr. at 13° = 0·906) yielded about 4 litres of fluid hydrocarbons, of which 0·3 litre boiled below 100°, 1·0 litre at from 100° to 200°, and 2·7 litres at from 200° to 300°; there remained a solid residue in the retort, which, on being treated with ether, was found to have as constant melting-point 41·5, and to consist, in 100 parts, of—C, 85·19; H, 15·34. By the action of bromine, this body was found to belong to the C_nH_{2n+2} series. The authors next describe the results of the fractional distillation of these fluid hydrocarbons. After treating the liquids thus obtained with bromine, these substances were found to be—Pentan, boiling-point 35° to 37°; hexan, 67° to 68°, sp. gr. at 18° = 0·6631; heptan, 97° to 99°, sp. gr. at 18·5° = 0·6913; octan, 122° to 125°, sp. gr. at 15·6° = 0·7165; nonan, 147° to 148°, sp. gr. at 13·5° = 0·7279. These hydrocarbons are stated to belong to the series—

$$CH,\text{— - —}CH_2 \ldots (nCH_2)\text{— - —}CH_2.$$

To demonstrate the action of water and heat, or of heat only, upon sugar, 200 grms. of sugar and 1000 grms. of water have been kept for some twenty-eight hours at the temperature of boiling water; the optical rotatory power of the sugar has then quite disappeared. Sugar and water have been enclosed in sealed tubes and kept for about thirty days at the temperature of boiling water, with the result that the sugar had become entirely converted into an uncrystallisable compound, probably identical with that which Berzelius and Gelis have obtained by rapidly heating sugar to 160°. By heating sugar for eight days consecutively in sealed tubes in steam of 5 atmospheres pressure (75 lbs. to the square inch, and 153° C.), the sugar is converted chiefly into carameline, $C_{12}H_4O_4$. M. Maumené states that sugar, while being refined or extracted on the large scale, should never be submitted to a higher temperature than 75°, because below that degree of heat sugar is not perceptibly altered.

It is generally admitted that water freezes at 0°, and can only be cooled down below its freezing-point by being kept perfectly tranquil. This holds good for a cold of −10° or −12°, but is, according to C. Tellier, not true for temperatures of −3° or −4°; because water may be cooled down to these degrees of temperature, and will not freeze, even when the vessel containing it is very violently shaken or stirred. Only a sudden and very violent shock to the vessel will cause the water to become frozen, the temperature rising to 0°; but if, in water cooled down to −3° or −4°, the slightest particle of ice or snow is put, congelation briskly sets in and crystallisation ensues, just as in a supersaturated solution of sulphate of soda. Practically this observation is important, as it proves the necessity of water containing ice if it is desired to cool it down to 0° precisely.

Asbestos resists the joint action of friction, humidity, and high temperature, and has therefore been proposed by M. Day as an excellent and durable substitute for hemp or flax in the stuffing-boxes of steam-engines.

The inconveniences attending the application of chloride of zinc and so-called water-glass solution for protection of wood from fire are well known; according to F. Sieburger, it is preferable to first coat or wash the wood twice with a hot and saturated solution of 3 parts of alum and 1 part of sulphate of iron, and next with a more dilute solution of sulphate of iron, to which sufficient pipe-clay has been added to render it as thick as ordinary oil paint. Another excellent method is to repeatedly paint the wood with a hot solution of glue, until a thin coat of glue remains on the surface; then the wood is painted with a thicker solution of glue; a mixture of 1 part of sulphur, 1 part of ochre or pipe-clay, and 6 parts of sulphate of iron, is applied with a dredger, the ingredients having been first separately pulverised and thoroughly mixed.

M. Ch. Mène states that some officers of the Austrian navy have made experiments for causing the smoke arising from the fuel consumed in steamships to be discharged under water by the aid of a blowing machine; at the same time a more active and regular combustion is obtained, and the chances of fire on board greatly lessened, while the funnel or iron chimney, one of the most vulnerable parts above deck, is rendered unnecessary. There is reason to believe that the experiments were in every respect successful.

In order to test whether any bearings or parts of machinery become heated by friction, Dr. Mayer proposes to cover the parts with a thin layer of iodide of mercury, as this red-coloured salt becomes black when exposed to a temperature of about 70°.

We are indebted to M. Collet for an account of the spontaneous combustion of a wooden beam in a building at Ribemont (Aisne). The oak beam was found to be on fire during one of the hot days in the summer; it was directly exposed in an open yard to the concentrated heat of the sun's rays; the combustion, although proceeding slowly, was quite distinct, but was not attended with flame; the smoke, however, had a peculiar appearance, and on blowing the wood it burst into flame. The author asserts that the fire was entirely due to the heat of the sun.

ELECTRICITY.

Captain Hans Busk has designed an electric bell aneroid, which indicates, by ringing one or other of a series of ten different bells, any change in the atmospheric pressure, amounting to 1-10th of an inch on the barometric scale. The mechanism is put in action by a constant battery of twelve Leclanché cells. The bells are arranged in the back of the aneroid case, which is constructed similarly to that of a bracket clock, and to each bell is attached an electro-magnet, with vibrating armature and conducting wires. The dial of the aneroid is formed of a disc of ebonite, 3-16ths of an inch in thickness. In this disc are insulated the steel index of the instrument and sixteen platinum points, forming terminals to the wires from the electro-magnets. To the index-hand is fastened one end of a thin strip of platinum foil. As the hand passes from 1-10th of a degree to another, it brings the platinum foil in contact with one of the platinum points; and, as the hand is one terminal of the battery, the bell corresponding to the platinum point brought into contact is rung, and continues ringing until the hand passes into contact with another point, or until the circuit is broken by a disconnecting switch. In the instrument constructed for Captain Busk by Mr. Browning there are sixteen points, but only ten bells have been found necessary. The object of the inventor was to devise a simple and efficient apparatus which should indicate unerringly, on board ship, or to a person in charge of a lifeboat station, any important approaching alteration in the state of the atmosphere; and it is interesting to note the rapidity with which these changes occasionally suceeed each other.

Prof. Edwin J. Houston contributes the following to the "Journal of the Franklin Institute:"—"Having occasion recently to set up a large battery for experimental illustration of the properties of the voltaic arch, I noticed a fact which I believe has hitherto escaped observation. The battery consisted of about eighty half-gallon cells; fifty-five were Browning's modification of the nitric acid battery of Grove. The negative element consists of sawed strips of very dense coke, the positive element of zinc, so arranged as to use both surfaces of the coke. The remaining cells were of the iron battery. When first set up, the arch between the carbon points measured fully 2 inches, while the flame frequently reached an equal distance above the upper carbon. The quantity of the current was very good—much better, in fact, than the size of the plates would have led me to expect. The phenomenon to which I would call attention is as follows:—"Wishing to show the well-known experiment of the rotation of the light by a magnet, I approached a compound bar magnet to the light, holding it with one end pointing directly to the arch, in a horizontal plane, equidistant between the carbon electrodes. When the nearest end of the magnet was 4 inches from the electrodes, the light was instantly extinguished. The regulator of the light which was employed is a form recently patented by Browning, of London. The carbon points are kept at a constant distance from each other by the action of a small magnet, worked by the battery current. Though inapplicable to small batteries, with the current I employed it gave a light admirable for its steadiness. Thinking that the extinguishing of the light was produced by some cause other than the approach of the magnet, the experiment was repeatedly tried in a number of ways, until it was clearly shown that the cause could not be attributed to accident, but to the approach of the magnet. Though I have failed to find any published notice of this phenomenon, it seems probable that it may already have been observed, as the conditions of the experiment would be almost exactly reproduced whenever the rotation of the light of the voltaic arch by the magnet was tried. Still it may be conceived that, though the necessary conditions for success in this experiment have often been *nearly* produced, they have seldom, if ever, been exactly produced; for it was noticed that in no case was the light extinguished unless the length of the arch was nearly as great as the tension of the electrode admitted; that is, unless the electrodes were separated by nearly their maximum distance, consistent with the passage of the current. Were this condition not observed, in all cases, the approach of the magnet produced no other effect than the rotation of the light, until it assumed a position in a vertical plane 90° from a similar plane passing through the magnetic axis of the bar. Then, again, another necessary condition is that both the tension and the quantity of the current be of a strength greater than that of the current on which the experiment of rotation is generally tried. I have experimented with flames when these latter conditions were absent, and, although the rotation was observed, the extinguishing of the light was in no instance produced. The compound bar magnet employed is formed of three bars, held together by brass screws. It is 1 foot long, 1 inch broad, and $\frac{3}{4}$ inch thick, and is not at all remarkable for the strength of its magnetism. As to the cause of the phenomenon, I think it may be attributed to the tendency of the flame to rotate on the approach of the magnet. This might cause the extinguishing of the light in two ways:—either by the irregularities on the surface of the carbon electrodes offering greater resistance to the passage of the current from some points than from others, or by the current being unable to pass through the greater distance of the arched path, which is always assumed by the light on the approach of a magnet. Another assumption, which, though perhaps not so simple as those already mentioned, at least as probable, is that on the approach of the magnet there is a slight increase in the non-conducting power of the medium between the electrodes, produced by their polarisation, and which, though always acting, can only manifest itself in a striking manner when the distance between the electrodes is near a maximum and the tension of the current is exerted to the utmost in passing through the non-conducting medium. This assumption of the polarisation of the medium between the electrodes and its consequently diminished power of conducting the current seems to be somewhat sustained by the fact that a powerful electro-magnet, in the form of a horse-shoe, when approached, did not extinguish the light, although it produced rotation of the current; for we may conceive that the two poles acting simultaneously on the medium would neutralise each other's effects. I noticed, on several occasions, that the south pole of the magnet would not extinguish the light until it was approached 1 inch nearer than the north pole, namely, to within 3 inches of the electrodes. This, however, may have been accidental."

Sir William Thomson desires to make known the following case, among many constantly occurring under his notice, of the employment of inferior copper wire in the construction of electrical apparatus. He received lately, from a Glasgow bell-hanger, a large quantity of cotton-covered copper wire, which was being largely used for the coils of electric bells; and upon having it tested very accurately, by means of his new multiple arc conductivity-box, its resistance per metrogramme was found to be no less than 0·439 of a B. A. unit, that of ordinary good copper wire for such purposes being about 0·16 of a B. A. unit.

M. J. Morin has arranged a new sulphate of copper voltaic element, with the view of the application of continuous currents to therapeutic purposes. It has for its object the complete avoidance of the inconvenience which results, in the ordinary sulphate of copper element, from the deposit of zinc either on the copper or the porous cell. It consists of a cylinder of copper, the interior of which is concentric with the cylinder of zinc; the annular space comprised between these two metallic surfaces is divided into two equal parts by a cylinder of filter-paper. Ordinary sandstone is put between the interior surface of the copper and the paper diaphragm, and sublimated sulphur on the side of the zinc; the whole is plunged into a solution of sulphate of copper, which penetrates through the mass, by means of the numerous minute orifices, freely to the copper. Some hundred elements prepared in this manner, and frequently in use, have been set up for more than twenty months, and the alteration which they have undergone shows that this is but half the time for which they may be worked: they have been perfectly closed during this time, and have neither been subjected to repair nor inspection.

Professor Forster, in a recent number of "Poggendorff's Annalen," gives an account of a curious effect observed in a gold-leaf electroscope. He was one day showing to an audience that if the leaves were in a state of divergence through negative electricity, the divergence would be increased on approach of a *negatively* electrified body, and that it would be diminished in the opposite case. To prove this, he had a magnified image of the leaves projected on the screen. He rubbed a rod of caoutchouc with cat-skin, and touched with it the ball of the electroscope. On removing the rod the leaves showed about 70° divergence. He again rubbed the rod, and brought from above towards the electroscope, the axis of the former being at right angles to the vertical axis of the latter. Expecting the divergence to increase, he was surprised to find that it diminished, and on further approach became zero, while it increased on slightly removing the rod. If the rod was withdrawn very slowly the divergence remained zero, but rose again to the former amount as the rod was further withdrawn. Frequent repetition of the experiment gave similar results, only, he remarks, the electrical source must be a pretty powerful one. After various futile attempts to explain the matter, there arose the doubt whether the divergence which appears after making a rubbed caoutchouc rod touch the ball of the electroscope was really due to negative electricity (−E). He became convinced that while the rod is, of course, negatively electrified, yet the leaves diverge with positive electricity (+E). He thus describes his experiments:—"Let a caoutchouc rod be rubbed with cat-skin and brought near the knob of a Fechner electroscope" (in which, it may be explained, there is only one leaf suspended between the opposite poles of a pile). "The leaf then moves to the + pole. The rod is therefore − electrified. Now rub the rod again, and touch with it the knob of the gold-leaf electroscope" (with two leaves). "Then remove the rod. Next, let the knob of this charged electroscope be brought near the knob of a Fechner electroscope (uncharged). The leaf of the latter moves to the − pole; the leaves of the former, therefore, diverged with +E; and thus the electroscope became + electrified through contact with the electrified rod. It remained to ascertain the reason of this fact, which, once ascertained as a fact, removed the difficulty in the first experiment. When the strongly − electrified rod is brought near the knob, there takes place decomposition of the electricity in the electroscope. The +E flows into the knob, in which it is retained by the rod; the −E flows into the leaves, which diverge in consequence. While thus influenced by the rod −E escapes, and there is a progressive collection and retention of +E in the knob. At the moment of contact between knob and rod, the rod communicates the −E present at its point of contact to the knob, and this neutralises in the latter a corresponding quantity of +E. As, however, the excited parts of the rod not in immediate contact with the knob do not give up their −E, this surplus of −E still carries on the decomposition and retention of electricity in the electroscope; so that there comes to be much more retained +E in the knob than −E in the leaves (for a constant waste of −E takes place in the electroscope). If the rod is now slowly removed its retentive influence on the knob decreases, and a certain quantity of −E flows into the leaves, neutralising there a certain quantity of −E. When the rod is just so far removed that as much +E can flow from the knob into the leaves as there is −E in these, the leaves become unelectrified and the divergence zero. On further withdrawal, still more of the hitherto retained +E flows from the knob to the leaves, and now commences a divergence with +E. When the rod is entirely removed, the whole of the hitherto retained +E is free, and produces strong divergence. As the rod is approached again, these phenomena are witnessed in reverse order. This theory rests on the supposition that the +E which has been separated and retained by the rod preponderates over the −E communicated to the electroscope by contact (which is easily understood, as an electrified non-conductor gives up its electricity only at the point of contact). For this preponderance to take place it is necessary, as has been said, that the electric source should be a very active one. It can, further, be easily shown that in such circumstances the −E flows away from the electroscope (without conduction) if the electrified rod be brought near the knob without touching it. In this case no electricity can flow directly from the rod to the knob, and yet, if after a few seconds the rod be withdrawn, the leaves diverge considerably with +E. The explanation is simple. The fact, however, that it is possible, by contact of the electroscope with a negatively electrified body, to obtain + divergence, appears to me one of considerable importance. Thus, supposing, in the absence of a pile electroscope, it were required to determine whether a body rubbed with a certain kind of material became + or − electrified, it would be a very likely method to first cause divergence in the electroscope leaves with a rubbed caoutchouc rod, and then test the electric state of the body in question by its influence on the divergent leaves, and call it + or − accordingly. On the former supposition, that the leaves diverged with electricity of the same sign as the caoutchouc rod, any such judgment would be absolutely false. A + electrified body would be taken for a —, and *vice versa.* Such error might be guarded against if, instead of bringing the caoutchouc rod near the electroscope, a small charge of −E were brought from the former, by means of a proof plane, to the knob of the latter, as the proof plane would not collect sufficient electricity to produce the disturbance observed in the other case."

Mr. Planté has recently described a new form of secondary battery. Two plates of lead (20 inches long by 8 inches wide), are rolled up in spiral, being separated from each other by a few strips of india-rubber. This spiral is placed in a jar containing acidulated water, and having a gutta-percha cover, on which are fitted bending screws connected with the plates. Twenty such elements are placed in two rows of ten each, and charged from the primary battery, which consist of two Bunsen's couples. By means of a commutator of peculiar construction these secondary elements may be connected either for quantity or for intensity. When the elements are joined in series an electro-motive force equal to thirty Bunsens is obtained, giving a current by means of which platinum wire may be fused. In the secondary couples the chemical action generating the current is the reaction of hydrogen on peroxide of lead, the current from the primary battery having caused decomposition of the water oxidising one of the plates, and developing hydrogen on the surface of the other. By the above arrangement the quantity of electric work from the direct action of the primary is transformed by condensation. The case is somewhat similar to that of a hydraulic press or crane. In a pile driver, *i.e.*, a heavy body raised by degrees to a great height, by a series of successive efforts, is then left to itself, and gives back at once the greater part of the work thus expended on it. So, when, after charging, the secondary circuit is closed, the sum of the accumulated chemical actions caused by the primary current is given out in the form of a very intense current of short duration. The effect, when the couples are joined for quantity, corresponds to the fall of a very heavy mass raised a small height; when joined for intensity, to the fall of a small mass raised to a great height.

Mr. W. H. Preece, of Southampton, gives the following information about lightning conductors:—Ordinary galvanised iron wire, known as No. 4, which is ¼-inch in diameter, tipped with a gilded brass point or cone, is amply sufficient for any dwelling-house. It costs about a penny per yard, and the brass cones would cost about sixpence each. Thirty shillings would pay for all the materials required for an ordinary house. The reasons for recommending this wire are these:—When telegraph poles were first erected in this country they were protected with lightning conductors. This practice was subsequently found to be too expensive, and was abandoned. Such "earth-wires" or conductors, were, however, found to effect another and very important object, and their use was continued on all main lines. Mr. Preece says, I have never known a case of a pole so protected being damaged through a thunderstorm, whereas scarcely a thunderstorm occurs without some unprotected poles being injured. I remember, near Romsey, twenty unprotected poles being shattered by one discharge, and upon the Basingstoke and Andover line 15 per cent were found to have been struck. The line was renewed and earth-wired, and not a single case of damage has occurred since, though some years have elapsed. A pole was very recently found in South Wales with 8 inches of its top shattered—the earth-wire only went so far; the charge from that point went harmlessly to earth through the wire. The cross arms are frequently found damaged as far as the earth-wire, never beyond. Instances could be multipled *ad infinitum*, and as the wire used is generally No. 8 (170 inches in diameter), and sometimes even smaller, I think I am fully justified in saying that No. 4 wire, which is twice as thick, and offers half the resistance, is amply sufficient for the protection of our houses. The precautions necessary in fixing conductors are these:—(1) The conductor must be solid and continuous from its gilded point to the ground; (2) Its connection with the ground must be sound and good. It may be connected with the iron, gas, or water mains, or be buried several feet deep in a bed of coke, or be attached to a mass of metal in moist earth, or be carried down a well; (3) Each conductor, if there be more than one, should make a separate earth if possible, and they should all be connected together below the surface. The lead roofing and all external masses of metal should be connected with them; (4) All joints and connections should be soldered. It is better that each chimney stack should have its own conductor, and they should be periodically examined to see that their points remain intact, and that their metallic continuity is perfect. The custom is to fix them, and then to leave them to chance. The precautions that are unnecessary are these:—(1) It need not be copper; (2) It need not be insulated; (3) It need not be carried externally to their disfigurement in the cases of church spires, columns, and chimneys. I never pass Trafalgar Square without regretting the disfigurement of Nelson's statue. It is, however, better to carry the wire externally in the case of dwelling-houses, lest it pass too near the lead gas-pipes, which, being good conductors and soft metal, might be fused. The wire can go round a corner as well as through it, but acute angles are best avoided. The more direct the course to the earth the better. The area protected by a conductor is said to be that whose radius is equal to twice the height of the conductor from the ground; but it is safer to take the radius as equal to the height of the conductor. Thus, for small houses one conductor is enough, but it is safer to attach one to each stack. If it project a yard above the stack it is sufficient, and it is within reach for inspection. The stack pipes down the sides of a house are convenient conduits for the wire, and there is no reason why it should not be carried down to the ground inside them, so as to be out of sight. If there be no convenient stack pipe the wire can be run up and stapled to the brickwork or stone. With thirty shillings for materials and ten shillings for labour any intelligent man can, with these directions and precautions, safely protect his house from the destructive effects of thunderstorms.

Mr. J. Baynes Thompson has shown that common electro-plating is not applicable to steel or iron, as these metals cannot be got perfectly clean, that is, chemically clean; therefore no adhering coating can be obtained. In fact, for all manner of plating or soldering, the first requisite is, that the two metals that are to be applied to each other must be chemically clean, or no proper adhesion can be obtained. This cleanness is obtained in various ways: in soldering by various fluxes; in electro-plating, to such metals as that method is applicable, by dipping the article in an acid which will readily dissolve the metal of which it is made, and not only so, but the salt formed by this solution of the metal in the acid used must be readily soluble in water, or no clean surface can be obtained. There is still another condition to be considered; that is, when the surface of the metal has been made thoroughly clean, it must be protected from contact with the air in its transit from the cleansing-baths to the solution wherein it is to be coated. This condition, not being recognised in the first attempts at electro-plating, caused many failures and much trouble, until it was discovered that a film of mercury prevented the contact of the air with the cleaned metal. Moreover, mercury has the advantage that it amalgamates with the metal to be coated, and with the coating, though this amalgamation is not absolutely necessary; yet it facilitates the coating of metals with other metals by electro-deposition, when the two metals will readily amalgamate. There are cases where amalgamation is not possible; for example, where one of the metals will not amalgamate, as with steel or iron coated with aluminium or nickel; not that it is impossible to form an amalgam with these metals, for even steel can be amalgamated with the intervention of sodium, but it is not possible for plating purposes, as a diluted solution of a mercuric salt must be used. For all such cases as these, where the amalgamation process cannot be used, pyro-plating is employed, this name being given to distinguish the process from the electro-plating process, and because the coating is driven into the surface of the metal on which it is put by means of heat and pneumatic pressure. It is not confined to coating with silver, as its name might indicate, but it is at present applied to coating with gold, platinum, silver, nickel, aluminium, copper, brass, or bronze and aluminium-bronze. The process is very simple, but the details require much care and attention. The end to be obtained is simply this: that the metal to be coated shall be "chemically clean" when immersed in the solution in which it is to be coated. There are several ways in which the attainment of this end may be prevented—by inadequate means for cleansing; by a passage through the air of two or three feet after being cleansed; by the metal being positive in the coating solution—in this case the metal is fouled on contact. This refers to cyanide solutions, to sulphur and chloride solutions, to double sulphates and chlorides, as of nickel and ammonia, and of platinum and potash or soda. All of these may be used in certain cases for pyro-plating, but they are *not* used. There is a special solution used for pyro-plating in all cases, because most of these solutions leave matters on the metal that is being coated if it be in the slightest degree porous or "roaky," as is the case with steel that has been badly faggoted, and on the article passing through the furnace these matters volatilise and cause an irruption in the coating. The amount of metal put on is ascertained by having a test-surface put in with the articles, and the exact time of plating and the exact weight of the test noted. This test is carefully weighed from hour to hour until the amount desired is put on. After being dried, the articles are put into the furnace. The firing-furnace, as it is technically termed, is of simple construction. A bright red heat is required in the chamber where the articles are placed. In firing knife-blades and other cutting instruments, care has to be taken that the heat is not carried higher than 450° and 500° F. This is ascertained by trials on a pad of prepared test-paper; a blade is taken out from time to time and tried upon the pad, and the colour noted—whether the blade scorches it straw-colour, yellow, pale brown, deep brown, or black. After the desired heat is attained, the blade is instantly immersed point downward in cold water, and all that were in the firing-chamber with it. The theory of the process, which is technically termed "burning in," is this:—On the instantaneous immersion in cold water, the coating is seized and retained by the suddenly contracting under metal. This is seen to be the case on filing or grinding the coating from the under metal; for though the coating may be filed or ground off until both coating and under metal are filed or ground off together, yet the under metal remains spotted all over with an infinity of little points, the pores filled by the coating metal.

TECHNOLOGY.

An American engineer recommends the following composition as a cement for leather, wood, &c., resisting the action of water, both hot and cold, and most of the acids and alkalies. Three parts, by weight, of shellac, and one part of caoutchouc are to be dissolved, in separate vessels, in ether free from alcohol, applying a gentle heat. When thoroughly dissolved, the two solutions are to be mixed. If the glue be thinned by the admixture of ether, and applied as a varnish to leather, it renders a joint or seam water-tight.

M. B. Renault has submitted to the Academy of Sciences a new method for obtaining the reproduction of designs. The design is traced with gummed ink upon stiff glazed paper, over which fine brass or bronze powder is dusted. By this means a kind of plate is obtained, admitting of the most varied designs being taken off upon prepared paper. By softening the ink with vapour of alcohol, the metallic powder can be removed when spent by use.

A method of testing petroleum and other inflammable fluids, and also of determining their specific gravity, has been patented in the United States. The apparatus consists of an upright glass cylinder supported in the top of a chamber formed in the upper part of a base or stand. A lamp is placed in the base, the heat from which is transmitted through the chamber to the lower part of the glass cylinder, and the chamber may be made to contain air, water, &c., as required to regulate its intensity. The glass cylinder contains a thermometer, which is fixed therein, and is closed at the top with a brass cover. The fluid to be tested is made to completely fill the glass cylinder, so that the thermometer is entirely submerged and cannot be affected by the surrounding atmosphere. An orifice in the brass cover is opened, to allow the escape of vapour from the fluid under test, and when necessary the lamp is lighted.

A flame is held over the orifice, and at the moment the evolved vapour is ignited the temperature of the fluid is correctly indicated by the thermometer. In ascertaining specific gravities by this instrument, a hydrometer is also placed within the glass cylinder in such a manner that its scale-tube is free to move up or down through a hole in the brass cover. The surface of the fluid tested is plainly visible through the glass cylinder, and the scale may be accurately read.

A so-called rubber-graphite paint has recently been patented, said to be waterproof and to present other advantages of reducing the corrosive influence of exposure to the atmosphere, &c. It is a solution of pure india-rubber in linseed oil, which is ground with graphite into a thick, elastic, smoothly-flowing paint. Compositions of which india-rubber forms a part possess, in a very high degree, the quality of resisting the action of moisture and of corrosive gases. The graphite is a pure form of carbon; and it is well known that paints containing carbon in form last longer than other kinds, holding their body and colour when other paints are totally destroyed. So that the combination may, as suggested, form a paint of great durability and highly-protective qualities. Cream colour or drab paints can be obtained by this method.

An admirable paper on the preservation of wood has been written by M. Hermann Haupt, C.E. in an American contemporary. The experiments show—That so long as the celts of wood are occupied by air and moisture, no preservative solution can be introduced, and the expulsion of air and water must be the first step in any effective process for preserving timber from decay. That water can be expelled by a long-continued application of heat, but air only by expansion in a vacuum, and the combination of heat and vacuum will secure the most rapid expansion both of water and air. The preservative fluid must be introduced while the cells are empty, consequently the process must be carried on *in vacuo*. That no pressure, however great, applied externally to the surface of timber, can force fluid into the interior so long as air or water is contained in the cells. When air alone is present, there may be penetration to a limited extent, superficially, but water is practically incompressible. If, however, the pressure is applied at one end only of a log, as in the Boucherie process, a fluid may be forced through and exude from the other end. An apparatus to fulfil the conditions which, from the preceding discussion, appear to be essential to success, must be founded on a process similar to distillation *in vacuo*. It must consist of at least two vessels—one a receiver corresponding to a retort, in which the material can be placed and subjected to the action of heat; the other a condenser, in which all escaping vapours can be condensed, and the vacuum maintained during the process in both vessels. The condenser may be of much smaller capacity than the receiver; they should communicate by pipes furnished with stop-cocks, and both be supplied with thermometers, vacuum gauges, and pumps. As an illustration, suppose wood is to be impregnated with dead oil or ony other fluid. The receiver must be filled with the wood to be operated on, the door closed air-tight, and the air expelled from both the receiver and condenser. The expulsion of the air may be effected in various ways. Steam may be admitted at one end to drive out the air at the other end; the subsequent condensation of the steam should leave a vacuum, but in the experiments of the writer this plan has been only partially successful. The air may be exhausted by an air-pump, but a perfect vacuum cannot in any way be secured. The vessels may be filled with water, and the water removed by a pump below the level of the bottom into which the water flows. This should remove all the air excepting that which escapes from the cells. As the atmosphere supports a column of water 33 feet high, pipes may lead to a tank at a level about 40 feet lower, where the location is favourable, and thus by filling the vessels with water and opening cocks to allow the water to flow by gravity into the tank, a very perfect vacuum could be produced. This arrangement would be particularly favourable for maintaining a vacuum in the condenser; a pipe in the condenser could throw jets of water in spray from numerous fine perforations, and the water would consequently flow in the tank 40 feet lower, maintaining a constant vacuum without the aid of pumps. This object can be accomplished in almost any locality by placing the condenser at the top of a building or on trestle work. Assuming that a vacuum has been created and provision made for maintaining it during the whole process, the next step will consist in the application of heat, which may be done most conveniently by steam-pipes introduced in the receiver. The length of time during which the timber must be subjected to the baking process will depend upon the dimensions of the logs, and can only be determined by experiment. It is obvious, however, that the circumstances are favourable to the most rapid evaporation possible; the temperature can be regulated at pleasure, and the removal of pressure by vacuum will give a very low boiling-point. As the vapours pass over they will be immediately condensed. Should the vacuum become vitiated by the escape of air from the cells, it may be improved by the use of an air-pump. The condition of the vacuum will be indicated by the gauges. When sufficient time has been allowed for the wood to dry thoroughly, cocks must be opened connecting the bottom of the receiver with a tank of dead oil at a lower level. As a vacuum exists in the receiver, the atmospheric pressure will force up the oil and the timber will be immersed in the fluid. When the immersion has continued a sufficient length of time, which also must be determined by careful experiment, cocks may be opened at the top of the receiver to admit air. The oil not absorbed will immediately flow back to the tank from which it was taken; the air pressing upon the exterior of the cells, which are partially filled with oil, while a vacuum exists in the interior, will force the oil before it, and thus coat in its progress the interior of the cells. It is probable that in this way a sufficient amount of dead oil may be introduced into the cells to prevent fermentation and decomposition while still far below the point of saturation, and the process may prove rapid and economical. Instead of admitting air in the manner proposed to expel the oil from the receiver, it is possible that better results may be obtained by allowing the oil to remain until it becomes heated by the steam coils, and the vapour collecting at the top expels the oil and penetrates the pores. Too much oil might be introduced by this mode of treatment, and it is probable that the introduction of air, followed, perhaps, by a second bath of oil to close the cells superficially and exclude moisture, would give the best results. All these and other questions that may arise can only be settled by experiment.

In China and Japan the rice-paper plant is cultivated upon the hills and high-lying ground. In the autumn of each year, before the leaves fall, the Japanese cut off the young shoots and cut them into slips, which are tied up into bundles and boiled in large copper kettles or cauldrons closely shut down. The boiling is continued until the bark has peeled off the wood, when the former is carefully dried and stored away for future use. When it is required for paper-making it is thoroughly soaked in water for three or four hours, after which the brown skin is scraped off. At the same time the bark which covered the younger shoots is separated from the older and tougher sorts, from which an inferior kind of paper is made. Bark which has been kept for some years is only fit to make the commonest packing paper, and is manufactured with less care. When the bark is well cleaned and arranged in order according to its quality, it is again boiled until the matter separates into a filamentous substance. This boiling is succeeded by another operation, called washing, which is of great importance in the manufacture of paper. If it is not continued long enough the paper will be of coarse quality; and if, on the other hand, the substance does not receive enough boiling, the paper will be very white, but too soft and greasy to write upon. The pulp is placed in a basket which will admit the water on all sides, and this is plunged into a ewer and stirred about with violence for some time. Then the substance is placed upon a smooth table and beaten with wooden rollers. After the beating an infusion of rice is poured upon it, and the mixture is suffered to stand until dry, when the substance is raised leaf by leaf in the form of paper. These leaves are placed between boards, and the remaining moisture gradually pressed out. According to another account the stem is cut into lengths of 10 or 12 inches, and the pith forced out and placed in hollow bamboos, where it swells out to its natural bulk, and dries into a compact mass. This pith is cleverly cut by a workman, who holds a sharp knife against the side of the cylinder, which is then turned round, so that the pith is cut into a broad strip about 4 feet long. This is cut into small squares, and sold in packets for different purposes. It is supposed that the paper made from the pith is the rice-paper which is imported into this country. It cannot be made until the tree has attained a considerable bulk, and is too old to produce many shoots such as are used in the first process. The tree from which this paper is made is particularly abundant in the island of Formosa. It is at first a small shrub, but after flowering it throws out several branches, and grows to a height of about 25 feet. It is generally cut down before it attains its full maturity, because the pith and bark degenerate in the older parts. Several large palmate leaves crown the stem. It has been supposed by many botanists that there are two or three different species of plants from which the Chinese make their paper, and there are apparently several ways of manufacturing it.

An enamel for copper cooking utensils is made by fusing together 12 parts of white fluor-spar, 12 of gypsum, and 1 part borax, dissolving the mass in water to a thick paste, which is applied as a paint to copper vessels, and when dry, this is rendered adhesive by being thoroughly baked.

An alloy, composed of 3 parts of tin, 39·5 copper, and 7·5 parts of zinc, is very well adapted for joining brass or copper to iron and steel.

CHEMICAL SCIENCE.

Mr. A. P. S. Stuart, an American chemist, remarks that everyone who has prepared hydrofluoric acid knows that sulphuric acid and fluor-spar form an exceedingly hard compound, which it is very difficult to remove from a platinum retort. The formation of this hard mass may be prevented by mixing with the fluor-spar about an equal weight of gypsum and the proper quantity of sulphuric acid. After the expulsion by heat of the hydrofluoric acid, the residuum in the retort is found in a pasty condition, easily removed by water.

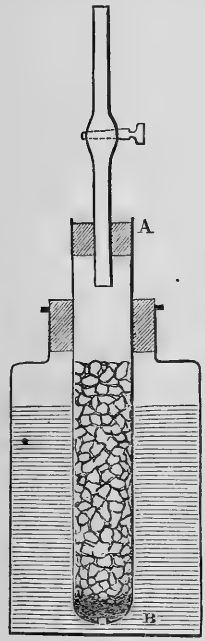
M. Puscher recommends that, to coat zinc with iron, the objects should first be plunged into a hot solution of 160 grms. of ferrous sulphate and 90 grms. of sal-ammoniac in 2500 c.c. of boiling water. After two minutes' exposure they should be removed and brushed off in water. This has for its object simply the cleansing of the surface. They are again placed in the bath and heated, without brushing or washing, until the sal-ammoniac fumes have disappeared, then washed, and this operation repeated three or four times. A coating of iron will be found formed on the zinc, which takes a fine polish when scratch-brushed.

Prof. Bischof has found that the analysis of a clay gives a distinct indication as to its power of resisting extreme heats, on the ground that the value of a refractory clay varies with the proportion of the alumina to the fusible matter, and again with that of the alumina to the silica. The more alumina a clay contains in proportion to the fusible matter (iron, alkalies, &c.), the more refractory is it. Silica, on the contrary, augments its fusibility. Except in certain determinate cases, the clays containing alumina, silica, and fusible matter in equal proportions, have an equal power of resisting fire. If there is taken for clays the general formula—$mAl_2O_3+nSiO_2+RO$, the degree of resistance to fire is measured by the ratio of $\frac{m}{n}$. The higher the value of this fraction, the more refractory the clay.

In cases where tubes cannot be connected by india-rubber, corks, &c., Mr. Karsten makes a gaslight joint by bending the two tubes to be joined vertically, the lower one capable of sliding within the other. When in this position, the open end of the upper or outer tube is surrounded by mercury retained in a cup, through the bottom of which the smaller tube passes. The whole apparatus is most easily constructed, and can readily be taken to pieces and put together again.

M. J. Boussingault has made some experiments to investigate the condition in which carbon exists in meteoric iron. In the meteoric iron of Caille, 0·12 per cent of combined carbon was detected, while the celebrated Lenarto meteorite was found to contain neither graphite nor combined carbon.

New forms and new modifications of apparatus are constantly being proposed and described for the convenient generation of H_2S, CO_2, and H in laboratory work. Many of these are somewhat complicated and expensive, more or less difficult to fill, and liable to breakage. Mr. William Hutchings advocates a trial of the following application of an old principle:— A large wide-necked jar is provided with a well-fitting stopper of cork or caoutchouc. Owing to the difficulty of boring a large hole in caoutchouc, cork will generally be preferred. In this a hole is bored wide enough to take a piece of large-bore glass tube, A B. This tube is of such a length that it reaches almost to the bottom of the jar, and projects 2 or 3 inches above the stopper. One end of it, B, is held in the gas-blast, and turned round and round till it softens and partially runs together, leaving only a small opening. The other end is fitted with a caoutchouc stopper, through which passes a glass tube of suitable bore with a glass stop-cock, all fitting perfectly tight. The tube reaches a little way below the stopper, and 3 or 4 inches above it, and is connected by india-rubber tube with wash-bottle, &c. The opening at B is covered with lead-shavings, a plug of lead wire, or pieces of glass, to allow the acid to rise into the tube, at the same time preventing any of the bits of marble, zinc, or sulphide of iron falling out. The tube, A B, being filled, is closed by its stopper and stop-cock tube, and inserted through the cork into the jar, which is a little over two-thirds full of the dilute acid. According as the stop-cock is opened, a stream of gas of any required amount is given off, and on closing the cock the acid is all driven into the jar. It is evident that the tube A B must not fit air-tight into the stopper of the jar. The best way of arranging is, after boring the hole in the cork for the tube A B, to cut a little channel with the triangular file, just sufficient to allow of the rise and fall of the liquid in the jar. A jar about 10 inches high, and appropriately wide, with a tube of 1 to 1¼ inches bore, gives a very suitable apparatus for ordinary use. By using a very large jar, or a large cylinder, and larger and much longer tube, a constant stream may be had for many days.



Professor Dewar recently exhibited, before the Royal Society of Edinburgh, two modifications of Sprengel's pump adapted to lecture illustration. In both instruments the mercury receptacle is made of iron, and, instead of the india-rubber joint of the original, a well-ground stop-cock, terminating in a Y-shaped piece bored out of the solid. In the one form the drop-tube is of glass, attached by means of marine glue; in the other, of carefully made india-rubber tube, 4 or 5 m.m. in thickness, of a very small uniform bore, made expressly for the purpose by the Edinburgh Rubber Company. The iron funnel-shaped receptacles are ground at the inner apex, so as to fit perfectly finely-ground iron tubes. By means of these tubes the preliminary exhaustions are made by a hand-pump, and then they are withdrawn. This device saves a separate joint. The barometer tubes are attached to solid T-shaped pieces of iron tube, and between these pieces and the main tubes each has a small glass bulb. Both forms work, for all practical purposes, as well as glass, and suit admirably for Frankland's water analyses, Graham's experiments, &c.

It would seem that the amount of ammoniacal gas absorbed by charcoal continually decreases as the temperature rises from 0° to 55°, but at that point a sudden change occurs, and the amount of gas given off becomes considerably diminished. In the case of cyanogen the absorption takes place very rapidly, being confined almost entirely to the first ten minutes, and the curve representing the absorption between 0° and 80° is continuous: the results obtained may be given in tables, and also represented by absorption curves. Hydrogen and nitrogen are very slightly absorbed by the charcoal.

M. Daubree gives the following result of his examination of the rocks of native iron which were discovered in Greenland, in 1870, by M. Nordenskiold:—

Metallic iron	40·094	71·000
Iron combined with oxygen, sulphur, and phosphorus	30·015	
Carbon, combined	3·000	4·064
„ free	1·064	
Nickel		2·065
Cobalt		0·091
Sulphur		2·070
Arsenic		0·041
Phosphorus		0·021
Silicium		0·075
Nitrogen		0·004
Oxygen		1·210
Combined water		1·095
Hygrometric water		0·091
Soluble substances:— Sulphate of lime	1·288	1·354
Chloride of calcium	0·039	
„ of iron	0·027	
Chromium, copper and loss		1·001

A process of printing on cotton or silk, which promises to be of practical utility, has been devised by M. Vial, of Paris. The cloth is first washed over with a solution of a salt of silver; then a coin, a medal, or a metal design —the metal being more electro-positive than silver—is placed upon it, and the design is produced by a deposit of silver in black lines in the fabric.

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