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THE QUARTERLY

JOURNAL OF SCIENCE.

JANUARY, 1875.

I. THE ILLUMINATED DISC OF THE MOON.

By Lieut.-Colonel A. W. Drayson, R.A., F.R.A.S.

IN the examples which we have given relative to the pole star and the pointers, the reader's attention has been directed towards the northern portions of the heavens, and to those curves and straight lines affecting certain phenomena connected with the apparent motion of the celestial bodies. We will now call attention to the southern portion of the heavens, and refer to those laws which affect the apparent position of celestial objects near the Southern Meridian.

In order to comprehend the relative position of celestial objects as regards the equator of the earth, it is necessary to trace out on the sphere of the heavens an imaginary arch or curve which represents exactly in the heavens that curve which the equinoctial (that is the equator produced to the sphere of the heavens) actually occupies in the heavens.

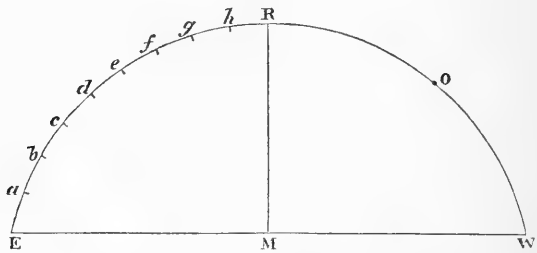
To an observer situated at the North Pole the equinoctial would be a great circle in the heavens, exactly coincident with his horizon. To an observer in 45° north latitude the equinoctial would appear an arch in the heavens, 45° above his horizon directly south, and coincident with his horizon at the east and west points. To an observer at the equator the equinoctial would appear a straight line, cutting the east and west points of the horizon, and passing through the zenith.

The apparent curve or arch in the heavens formed by the equinoctial, as seen from latitude 45° north, can be traced out with great accuracy. The altitude of the equinoctial on the meridian is always equal to the co-latitude; thus the meridian altitude of the equinoctial for 45° north latitude will be 45°. Taking arcs of the equinoctial of 10°, we should have the following as the altitude of the points 10° distant from each other, viz.:—

7° 3′
14° 0′
20° 42′
27° 2′
32° 48′
37° 46′
41° 38′
44° 8′
45° 0′

Thus, in the following figure, let M R represent a portion of the meridian, E W the horizon, M the south point of the horizon, E the east and W the west points. The arc, E, *a*, *b*, *c*, R, is the equinoctial, being that quadrant between the east point on the horizon, and the meridian, *a*, *b*, *c*, &c., represent arcs of 10°.

FIG. 1.



The point *a* will be 7° 3′ above the horizon, *b* 14° above the horizon, *c* 20° 42′, and so on. In the same manner the curve from R to W may be traced out during the whole quadrant.

Let us suppose that there are three stars, *a*, *b*, *c*, situated on the equinoctial, and in the positions indicated by *a*, *b*, and *c*. Also that another star was situated at O, also on the equinoctial.

To an observer at the North Pole these four stars would appear on the horizon, and a straight line joining *a* and *c* would pass through *b*, and if produced would pass through the star O. To an observer at the equator, on the meridian, of which R M is a part, and so situated that R was in the zenith, the three stars, *a*, *b*, *c*, would also appear in the same straight line, and this straight line if produced would pass through O.

To an observer at 45° north latitude the line passing through *a*, *b*, and *c* would not be a straight line, or would not appear as a straight line. If the stars, *a* and *c*, be joined by a straight line, and this straight line were produced in the direction of and beyond the meridian, R M, this line would not pass through O. In order to pass through O this line must be curved and not straight, and no straight line joining *a* and *c* and produced would pass through the star O, if the observer were in 45° north latitude. Having examined these facts, we will now note how the same laws apply to the illuminated disc of the moon.

The moon shines and is visible to us in consequence of the light of the sun and because of the moon reflecting this light. The moon being a sphere, it follows that half the moon must at all times be illuminated by the sun. When the illuminated portion of the moon is turned directly towards the earth the moon is of course full; when this is the case the moon and the sun as seen from the earth are 180° apart.

When the moon and the sun as seen from the earth are 90° apart the moon will appear half illuminated, the illuminated hemisphere visible to us being turned exactly towards the sun.

Let us take for illustration the following conditions:—The moon and the sun both on the equinoctial, the moon apparently half illuminated, the sun therefore 90° from the moon.

Under the above conditions, we will suppose an observer situated at the North Pole of the earth, and we will examine the appearance of the moon and sun as seen by him.

To an observer at the North Pole the equinoctial would coincide with the horizon; consequently, the moon and the sun would both appear on the horizon, and 90° apart. The line separating the dark part from the light part of the moon would appear at right angles to the horizon; consequently a line drawn at right angles to the line separating

Fig. 2.

the light part from the dark part of the moon, and produced, would pass through the sun's centre. The appearance of the half moon and the sun to an observer at the North Pole would be, as shown in the sketch above (Fig. 2), where M represents the moon, S the sun, and the line H R the horizon.

The point, however, to which particular attention is directed is that the line drawn at right angles to the line of light and shade on the moon, and produced towards the sun, will pass exactly through the sun's centre. Such a line is H R.

The moon and the sun, as here shown, are 90° distant from each other, and the moon consequently is half illuminated.

We will now suppose exactly the same conditions to prevail as regards the sun and the moon's position in the heavens. They will still be supposed 90° distant from each other, and both situated on the equinoctial, but we will now examine the appearance of these two objects as seen by an observer on the equator.

We will suppose an observer to be so situated on the equator that his zenith is equidistant from the sun and the moon. The sun and the moon being 90° distant from each other, it follows that to this individual the moon would have an altitude of 45° above one part of the horizon (say the east) where the equinoctial cuts it; the sun therefore would be 45° above the west horizon. The line drawn at right angles to the line of light and shade on the moon would be directed past the zenith and towards the sun's centre.

From an examination of this and of the preceding demonstration, in which the appearance of the sun and moon is described as they would appear to an observer at the pole, it will be evident that the line drawn from the moon to the sun must coincide with the equinoctial, and that under the conditions named, viz., both the moon and the sun being on the equinoctial, the line of light and shade on the moon will always be at right angles to the equinoctial.

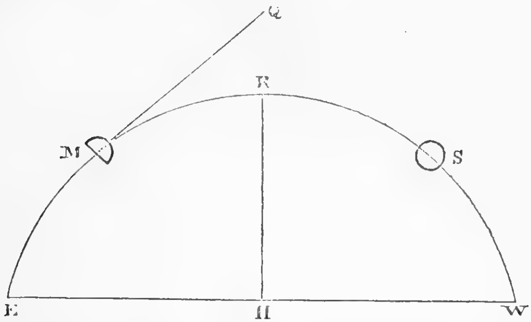
We will now consider the appearances presented by the sun and moon when seen by an observer in 45° north latitude, and under the conditions of these two celestial bodies being 90° apart, both on the equinoctial, and both equally distant from the meridian.

From the demonstration already given as regards the position of the equinoctial on the sphere of the heavens, we can at once place the sun and the moon in the positions they would occupy under the above conditions.

Thus, let E H W (Fig. 3) represent the horizon, H R that part of the meridian intercepted between the horizon and the equinoctial, E M R S W the position of the equinoctial on the sphere of the heavens, M the position of the moon

45° east of the meridian, S the position of the sun 45° west of the meridian.

Fig. 3.

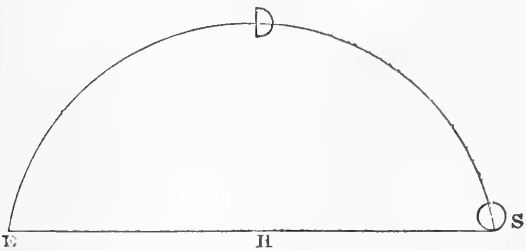


Under the above conditions the straight line of the moon will be at right angles to that part of the curved line formed by the equinoctial, on which the moon at the time is located. Thus the moon will appear tilted sideways as it were, as shown in this diagram, and the line of light and darkness on the moon will not appear at right angles to the horizon.

If we draw a line at right angles to the line of light and shade on the moon, and produce this line as a straight line past the meridian, H R, this line will not now pass through the sun's centre, but will be traced in the direction of M Q, and will pass considerably above the sun.

When the moon reaches the meridian, the sun, 90° from it, will be on the horizon and in the west. The moon will then appear in the position as regards the line of light and shade vertical, as shown in the following diagram (Fig. 4),

Fig. 4.



where, as before, H M is a part of the meridian, E H S the horizon, M the moon, S the sun. A line drawn at right angles to the line of light and shade on the moon will now, if produced, be a line parallel to the horizon, and will not be a line coincident with a straight line joining the sun and the moon.

In these two examples the right hand side of the moon has been given as the illuminated side; exactly the same laws will hold good when the left side of the moon is illuminated, the most marked effects being visible when the moon is 90° distant from the sun, and when the sun and moon are equally distant from the horizon, and the observer is at or near 45° latitude.

It may be difficult for those individuals who are not gifted with great powers of observation to perceive that the pointers sometimes point more directly towards the pole star than they do at other times. We believe, however, that this fact is one which merely requires attention to be directed to it, after which it will be perceived.

The phenomenon connected with the illuminated disc of the moon is one which does not require such careful observation as does that relative to the pole star and the pointers: very average observation will enable a person to note the facts to which we have called attention relative to the illuminated portion of the moon's disc that is turned towards the earth. Thus, if the reader either does not understand the demonstrations here given, or doubts their accuracy, he can examine the fact for himself, and if he select the suitable conditions, he can twice a month observe the phenomenon in the heavens. The best time to note the fact is when the moon is in either the first or the last quarter, and when consequently she is half illuminated. If the moon be illuminated on the west side, that is during the first quarter, the best time to observe the effect is at 3 P.M. If the moon be illuminated on the left or east side the best time will be 9 A.M. The winter time is better than the summer, because during winter the moon's light enables her to be better seen than during the summer.

The same geometrical laws of the sphere which cause the pointers to alter the direction in which they point relative to a fixed star, also cause the line of light and shade on the moon to appear sometimes not at right angles to the line joining the moon and the sun. It is a singular law, but it is one not without interesting results.

Singular as it may appear to some readers, yet it is a fact that not one individual out of one thousand to whom we have spoken on the subject has ever remarked that the pointers did not always appear to point to the pole star, and scarcely one person in a hundred has ever remarked that appearance of the moon when half illuminated which we have here called attention to, and of which we have given the geometrical cause.

Thus it may be said that, under certain conditions, straight lines in the heavens may appear like arcs of circles or ellipses, whilst arcs of circles appear like straight lines, and these changes are due to the relative position on earth of the observer, in spite of the fact that, compared to the alteration in position on earth of an observer, the distance of the fixed stars are infinite, and at a superficial glance it would seem that no change of position on earth could alter their apparent relative position as regards each other.

II. RAILWAY ACCIDENTS.

By Fred. Chas. Danvers, Assoc. Inst. C.E.

SO much attention has of late been given to the subject of railway accidents, and the best means of preventing them, and so important is it in the interest of the public generally, that a few pages of the "Quarterly Journal of Science" may, with advantage, be devoted to a consideration of how far all known and practicable means for the mitigation of the dangers of railway travelling have been adopted. In investigating this question we must refer briefly, in the first instance, to the early history of railway legislation, with a view to trace what steps have been taken by the Government for the protection of travellers, prior to enquiry as to what action has been taken by the railway companies themselves with the same object.

The earliest railway or tramway Act was passed in 1801, for the construction of a railway from Wandsworth to Croydon, for "the advantage of conveying coals, corn, and all goods and merchandise to and from the metropolis and other places." From this period new tramways or railways were sanctioned in almost every session. The Acts by which the earlier railway companies were established followed very closely, in their general scope, the provisions which had been applied to canal companies. The promoters of the project were constituted a corporation, and were authorised to raise such money, either by shares or by borrowing, as they required for completing their undertaking; and they were empowered in their corporate capacity to take lands compulsorily, and to charge tolls at their discretion for the use of their railway, within the limits of certain prescribed rates, for various classes of goods. In the Act for the Liverpool and Manchester Railway, passed in the year 1825, and in other subsequent similar Acts, a further provision was introduced, that if the dividends should exceed 10 per cent an abatement should be made from the maximum tonnage rates of 5 per cent on the amount thereof for each 1 per cent, which the Company might divide over and above a dividend of 10 per cent on its capital. In their capacity as owners of a road, railway companies were not intended by Parliament to have any monopoly or preferential use of the means of communication on their lines of railway; on the contrary, provision was made, in all or most of the Acts of Incorporation, to enable all persons to use the road on payment of certain tolls to the company, under such regulations as the company might make to secure the proper and convenient use of the railway. But no sooner were railways worked on a large scale with locomotive power than it was found impracticable for the public in general to use the lines, either with carriages or locomotive engines; and the railway companies, in order to make their undertakings remunerative, were compelled, with the assistance of the persons who had been previously engaged in the carrying trade of the country, to embark in the business of common carriers on their lines of railway, and conduct the whole operations themselves.

In consequence of the increasing number of Railway Bills annually coming before Parliament, and the necessity for securing consistency in private bill legislation, the House of Commons, in 1837, appointed a select committee, to which were referred all petitions for private bills, and it was the duty of this committee to decide how far the standing orders had been complied with in each case.

In 1840, another Select Committee of the House of Commons, appointed to report on the railway system, came to the conclusion that the right secured to the public by the Railway Acts, of running their engines and carriages on the railways, was practically a dead letter. In consequence of their recommendation that the executive government should be entrusted with the duty of inspecting new lines of railway, and of exercising a general supervision over the manner in which the railway companies used their powers, an Act was passed by which it was provided that no new railway for the conveyance of goods or passengers should be opened without previous notice to the Board of Trade, and the Board were empowered to appoint officers to inspect all new railways. The Board was also empowered to require, under a penalty, that every railway company should deliver to them returns, in whatever form they might prescribe, of the traffic in passengers and goods, as well as of accidents attended with personal injury, and a table of tolls and rates from time to time levied on passengers and goods. All bye-laws already made by companies were to be certified to the Board, and no new ones were to be made without its sanction. The Board was also constituted the guardian of the public interests, being empowered at its discretion to certify to the law officers of the Crown any infraction of the law, and the law officers of the Crown were thereupon required to take the requisite legal proceedings. The power which had been conferred upon proprietors of land adjoining railways by their private Acts of Parliament, for making junctions, was placed under the control of the Board of Trade, with a discretion to regulate the manner in which it should be exercised.

In 1842, the returns of the accidents required to be made to the Board of Trade were extended to all cases, whether or not they were attended with personal injury; and in 1844 parliamentary trains were established by law, and the powers of the Board of Trade to compel railway companies to comply with the law were extended to all unauthorised proceedings on the part of the railway companies. In 1846 an Act was passed establishing a Board of Commissioners of Railways, to whom the powers possessed by the Board of Trade were transferred; but in 1851 the Board of Commissioners was abolished, and its powers and duties were re-transferred to the Board of Trade.

In 1857 a Select Committee on Accidents on Railways was appointed, who in their Report of the 25th June, 1858, classified the causes of accidents under the three following heads:—Inattention of Servants; Defective Material, either in the works or rolling stock; and Excessive Speed. Much stress was laid by the Committee on the necessity for punctuality in the departure and arrival of trains; they considered that it should be imperative on every railway company to establish a means of communication between guards and engine-drivers, and that a system of telegraphic communication on the lines should be enforced, in order that they might be worked on the block system; and they concluded by recommending that, with respect to signals, breaks, and other precautions, such details should be left to the management of the railway boards, but that the Boaro of Trade should be invested with further powers to enabd them the more effectually to control the working of railways with a view to diminishing the number of railway accidents.

Bills have at various times been introduced with the object of compelling railway companies to adopt some precise system of working, but they were not passed; and in 1866, in a bill of this nature, it was for the first time proposed to compel railway companies to adopt a means of communication between passengers and guards, and between guards and engine-drivers of all trains. This Bill, however, did not, at the time, become law, but was withdrawn.

In 1865 a Royal Commission was appointed to enquire generally into the subject of railways, and to report, amongst other matters, whether, with a due regard to the progressive extension of the railway system, it would be practicable, by means of any changes in the laws relating to railways, "more effectually to provide for securing the safe, expeditious, punctual, and cheap transit of passengers and merchandise upon the said railways."

Up to this date the legislation upon railways directed that no line should be opened until it had first been approved and passed by the Board of Trade Inspectors, but after it had been once opened for traffic the manner of working was left entirely in the hands of the railway company, power being, however, reserved to the Board of Trade to cause the railways, the engines, and the carriages to be inspected by their officers whenever they might think fit, and they might, when applied to, make regulations for the safe working of the traffic at the junction of the lines of two companies. The railway company, in undertaking the duty of carriers, became liable under the common law to compensate persons injured, and under Lord Campbell's Act to compensate the relatives of persons killed by the company's negligence or by that of their servants. Thus Parliament relied upon the principle of leaving the responsibility of the safe working of railways with the companies rather than upon giving the Board of Trade the power and duty of interfering in the details of management.

The Royal Commission of 1865, in their Report, expressed the opinion that the plan of relying for the safe working of railways upon the efficiency of the common law and of Lord Campbell's Act, had been more conducive to the protection of the public than if the Board of Trade had been empowered to interfere in the detailed arrangements for working the traffic. They recommended, however, that, on the one hand, railway companies should be absolutely responsible for all injuries arising in the conveyance of passengers, except those due to their own negligence; and that, on the other hand, the liability of the railway companies be limited within a maximum amount of compensation for each class of fares; but that any passenger should be entitled to require from the company any additional amount of insurance he might desire, on paying for it according to a fixed tariff. They also recommended that claims for compensation should not be admitted unless made within a certain period, and that the railway companies should have the right of medical examination of the claimant; and, further, that to the power already possessed by the Board of Trade of appointing officers to inspect railways and rolling stock, should be added a power for the inspecting officer to require the attendance of the officers and servants of the company as witnesses, and the production of books and documents bearing on enquiries directed by the Board of Trade; and that the reports of the inspecting officers on accidents should be made public.

In 1870 a Select Committee of the House of Commons was appointed to enquire into the law and the administration of the law of compensation for accidents as applied to railway companies, and also to inquire whether any, and what, precautions ought to be adopted by railway companies with a view to prevent accidents. On the second point the Committee pointed out that on those lines where the block system had been adopted it had materially conduced to the safety of the public, and they recommended the evidence collected by them on this subject, as well as that in favour of the principle of the interlocking of signals and points, and concerning continuous breaks, to the careful consideration of railway boards of directors.

Last year (1873) a Bill was introduced into Parliament for the "Regulation of Railways," with a view to the prevention of accidents. This Bill had for its object the enforcing upon all railway companies the obligation of securing an interval of space between trains following each other on the same line of rails, which is now generally effected by what is known as the block system, and it further proposed to enforce the interlocking system. A Select Committee was appointed by the House of Lords to consider this Bill, but whilst strongly recommending the adoption of both the block system and the interlocking system on all important lines of railway, yet, relying on the great exertions recently and very generally made by different railway companies to extend both systems, and other great improvements now in progress, the Committee recommended that the Bill should not then be proceeded with. They recommended, however, that the Board of Trade should call for such information as might enable the inspectors, in their annual reports, to state specially the progress made in their adoption on all passenger lines, after which, it was considered, Parliament would be in a condition to decide whether or not it would be right to require the further and more prompt extension of these systems on those lines where they might still be necessary.

Another Commission is at the present time occupied in considering how railway accidents may best be prevented, and what legislation, if any, is desirable on the subject in the interests of the public at large. It will be observed that, hitherto, the action of Parliament has been rather to recommend and advise than to pass coercive measures to compel railway companies to adopt improved means for the protection of their passengers. At the same time, additional powers have been vested in the Board of Trade from time to time for the more efficient inspection of lines open to the public, and there can be no doubt that the duties devolving upon that branch of the public service have hitherto been conducted satisfactorily in the general interests, but it is hardly to be supposed that its action should meet with universal approbation, or, indeed, that it should be always free from blame. It is very obvious that the officers of the Board of Trade are not in good odour with the present President of the Institution of Civil Engineers; and, as his observations may probably be taken to represent the feelings of railway officials generally towards them, we quote the following remarks made by him in his inaugural address on the 13th of January last:—

"There is also a 'popular delusion' which I think ought to be corrected. The public believe that the various recommendations made to the railway companies from time to time by the officers of the Board of Trade, such as the block system, interlocking of points, &c., are really inventions of those officers, whereas the fact is that not one of these systems or inventions, or any new idea in connection with the workings of railways, has ever really been suggested by them.

"The railway companies also are at a great disadvantage with the public in respect to the reports which are from time to time made by the Government inspecting officers—their dictum is never questioned by the public; and although railway officers of great experience constantly differ from those officials in the conclusions at which they arrive, the railway companies feel that any appeal against these reports is useless, and practically judgment is allowed to go by default.

"In making their reports, the officers of the Board of Trade are in the position of *ex post facto* judges, and I need hardly point out that there is a great difference, to use an expression of our late President, Mr. Hawksley, between looking into the events of the week that is past, and looking into the middle of next week; and should the country at any time become the purchasers of the railways, these officers will soon find the difference in their position when the responsibility of working the lines devolves upon them.

"Captain Tyler, in his valuable Report on Railway Accidents in 1872, says: 'Whatever be the amount of care taken, the item of human fallibility will still remain, and will always be the cause of a certain number of accidents.' And he states that in 180 cases of accidents out of 238, 'negligence, want of care, or mistakes of officers, were apparent.'

"This is a subject to which for years past I have devoted a great deal of attention and anxious thought, and I attach much more importance to the item of 'human fallibility' than Captain Tyler appears to do."

To these remarks Captain Tyler replied as follows, in a paper read by him before the Society of Arts in May last:—

"When Mr. Harrison attributes to the author that he does not sufficiently appreciate the element of human frailty as contributing to accidents on railways, and leaves it to be understood that improved arrangements will not materially lessen the number of accidents and their serious results, the author would venture to reply that he estimates that cause of accident at no more and no less than has actually been found by experience of many years to attach to it."

This subject also was referred to by the Select Committee of the House of Lords, who, in their report of last year, remarked:—

"It may be confidently stated that the general safety of railway travelling would be increased by the more extensive employment of the block and of the interlocking systems. Some witnesses stated that these precautionary arrangements and mechanical appliances tend to lessen the sense of responsibility in the engine drivers. Such an effect may have been produced, but, nevertheless, the advantages resulting from the introduction of these systems are practically admitted by all the witnesses, and, in the judgment of the Committee, decidedly preponderate."

We do not propose to follow up this subject further at present, beyond remarking that, whilst fully admitting the element of human frailty, which must exist wherever the hand of man is engaged, we entirely concur in the conclusion arrived at by the Select Committee of the House of Lords, that the introduction of improved mechanical contrivances for the more efficient and safe working of railways is likely to overbalance in its advantages the evils likely to arise from the element of "human frailty," which must be, at all times, inseparable from their introduction.

The next subjects for consideration are the extent to which railway passengers are liable to accidents, and how far former risks are increased or diminished in proportion to the number of travellers, and to the adoption of means with a view to their prevention. A general review of the number of fatal accidents to passengers from all causes beyond their own control, between the years 1847 and 1873 inclusive, is contained in Captain Tyler's General Report to the Board of Trade on the accidents which have occurred on the railways of the United Kingdom during the year 1873, from which the following extract is taken:—

"The total number of persons recorded at the Board of Trade as having been killed on railways during the year was 1372, and the number of injured was 3110. Of these, 160 persons killed, and 1750 persons injured, were passengers; and the remainder, 1212 killed and 1360 injured, were officials or servants of the railway companies, or trespassers, or others who met with accidents at level crossings, or from miscellaneous causes. Of the passengers, 40 were killed, and 1522 were injured, from causes beyond their own control. The total number of passenger-journeys having been 455,272,000, it follows that the proportion of passengers killed was, in round numbers, 1 to 2,845,450, and of passengers injured 1 to 260,155; and that the proportions of passengers killed and injured from causes beyond their own control were respectively, 1 in 11,381,800, and 1 in 299,127. This was a decrease on the average of the number killed, and an increase of the number injured, from causes beyond their own control, in the previous three years, in which the proportions were 1 to 11,123,931 killed, and 1 to 357,000 injured. Of the officers and servants of railway companies there have, during the past year, in proportion to the total number employed, as nearly as they can be estimated (say 250,000), been killed from all causes 1 out of 323, and injured 1 out of 213; but accidents to servants do not appear, in many cases, even now to have been reported by certain of the railway companies, and their numbers would, if the whole truth could be ascertained, be considerably increased.

"The following statement shows the proportion of passengers killed to passenger-journeys for the three years ending 1849, the four years ending 1859, the four years ending 1869, and the four years 1870, 1871, 1872, and 1873, respectively:—

Year.	Number of passengers killed from all causes beyond their own control.	Number of passenger-journeys exclusive of journeys by season-ticket holders.	Proportion killed to number carried.	
1847, 1848, 1849	36	173,158,772	1 in 4,782,188	
1856, 1867, 1858, 1859	64	557,338,326	1 in 8,708,411	
1866, 1867, 1868, 1869	91	1,177,646,573	1 in 12,941,170	
1870	66	336,545,399	1 in 5,099,172	Average of these 3 years, 1 in 11,123,931
1871	12	375,220,754	1 in 31,268,396	
1872	24	422,874,822	1 in 17,619,784	
1873	40*	455,272,000	1 in 11,381,800	

From these figures it appears that the average of fatal accidents for the last four years was higher than in the similar cycle immediately preceding; and the conclusion that would naturally be formed at first thought is, that a maximum of safety in railway travelling has been arrived at. On a closer examination, however, it does not in any way seem that this is the case. No doubt traffic has increased on many lines in a more rapid ratio than the development of increased accommodation for such traffic. But the accidents in 1870 were considerably in excess of the proportion given in the above table since 1856; but if we omit that bad year, and' take only the average of the last three years, it will be seen that the number of passengers killed from all causes beyond their own control was only 1 in 20,089,993, which shows a considerable improvement

* The deaths of two of this number were not the results of train accidents

upon any of the earlier periods referred to. The year 1871 was, it appears, exceptionally free from fatal accidents; but Captain Tyler shows that it is not desirable to lay too much stress on the results of working in the case of any particular year, either as to the number of sufferers or as to the number of accidents. More returns of accidents than formerly have been rendered by the companies within the last two years. Inquiries have also been instituted during those two years into a greater proportion of cases, and there is, humanly speaking, much of chance in both. A dangerous or defective mode of working is frequently carried on for a great length of time without bad results, while there are accidents and loss of life where greater precautions have been adopted, or less risk is apparently incurred. A comparatively trifling defect may in one case lead to much loss of life, whilst important defects may, in another case, be unattended with accident.

Setting aside considerations of humanity, the railway companies have a positive and direct pecuniary interest in the avoidance of accidents, and capital laid out with that object in view is not likely to be wholly unproductive. Under Lord Campbell's Act the railway companies are pecuniarily liable to those to whom any injury is caused by accidents, &c., on their lines, and, during the ten years from 1848 to 1857 inclusive, there was paid as compensation on account of passengers and goods injured on fourteen lines of railway, no less a sum than £414,440, or at the rate of over £40,000 a year. For the five years ending with the year 1871, there was similarly paid £2,348,568, of which £1,622,370 was as compensation for personal injury, and £726,198 as compensation for damage to goods. These sums do not, however, include anything on account of injury to the servants of the railway companies, to whom the latter are not liable by law in the same way that they are towards their passengers or goods traffic.

The following table shows the number of train accidents that have formed the subject of inquiry, and have been reported on, by officers of the Board of Trade, during the past four years. The number of cases inquired into during the preceding five years averaged 83 per annum, upon which those for the year 1870 show an increase of 57 per cent:—

1870.	1871.	1872.	1873.	Cause of Accident.
9	19	21	24	From engines or vehicles meeting with, or leaving the rails in consequence of obstructions, or from defects in connection with the permanent way or works.
10	22	17	23	From boiler explosions, failures of axles, wheels, tyres, or from other defects in the rolling stock.
61	9	22	18	From collisions between engines and trains following one another on the same line of rails, excepting at junctions, stations, or sidings.
	63	91	98	From collisions within fixed signals at stations or sidings, &c.
18	19	32	20	From collisions at junctions.
3	2	5	3	From collisions between trains, &c., meeting in opposite directions.
1	—	—	3	From collisions at level crossings of two railways.
14	12	34	36	From passenger-trains being wrongly turned or run into sidings, or otherwise through facing points.
—	2	7	5	From trains entering stations at too great speed.
6	11	9	11	On inclines.
9	12	8	6	Miscellaneous.
131	171	246	247	

An examination of this table will show that the more serious classes of accidents are evidently upon the increase, more particularly from collisions within fixed signals at stations or sidings, and from passenger trains being wrongly turned, or run into sidings, or otherwise through facing points. But it must be observed that the accidents are in no respect proportionate to either the length of, or the amount of traffic on, any particular line of railway, some lines being particularly unfortunate in this respect, while others enjoy comparative immunity from accidents. Increase of traffic, high speed, and variations of speed, tend materially to increased risk, to greater numbers of accidents, and to more severe accidents when there is insufficient accommodation in lines and sidings, when signal and point arrangements are defective, when the means of securing intervals between the trains are defective, without sufficient break-power, without good construction and high maintenance, and when the appliances and apparatus are not adapted to the exigencies of the traffic. But when, on the other hand, the accommodation is sufficient to enable the traffic to be worked under safe conditions, when high speed is employed only over a good permanent way in suitable portions of railway, and under proper circumstances, and when good arrangements are made to preserve intervals between the trains, of whatever class, then such extra risk may be in a great measure obviated. Some of the great railway companies have made, and others are making, great progress in providing the necessary remedies. It was stated by Mr. T. B. Farrer, in his evidence before the Select Committee of the House of Lords last year, that the railway companies had then already spent upon the introduction of the block system and the system of interlocking signals, between £700,000 and £800,000, and that they were proposing to spend a great deal more; on a previous occasion, however, it had been stated before the same Committee, by Mr. J. S. Farmer, that, in his opinion, a great deal of expense had been thrown away in tinkering at the signals, in trying to do as little as possible, instead of grasping the thing comprehensively in the first place.

However much has already been accomplished, a good deal yet remains to be done, especially on certain railway systems; and Captain Tyler expresses it as his opinion that it is partly on account of sufficient attention not having been paid in previous years to the various means of safety that some of the great railway companies now appear so unfavourably at the head of the accident list, and partly also because they have found it difficult, with constantly increasing traffic, simultaneously to make up for past omissions and to keep up with present requirements.

In a circular letter addressed by the President of the Board of Trade to the several railway companies in November, 1873, on the subject of the great increase in the number of railway accidents during 1872, Mr. Chichester Fortescue remarked that a large proportion of these casualties appeared to have been due to causes within the control of the railway companies. "If it may be contended," the circular goes on to state, "that the traffic on many lines has very greatly increased, and with it the risks of railway travelling, it is no less true that it is within the power of the companies to take care that the permanent way, the rolling stock, and the station and siding accommodation, are kept up to the requirements of the traffic; that the officers and servants are sufficient in number and quality for the work to be done, and that proper regulations for their guidance are not only made, but enforced; that pains are taken to test every reasonable invention and expedient devised for the purpose of preventing danger; and that such of those expedients as experience proves to be effective are adopted without undue delay.

"In the face of the facts collected and analysed by Captain Tyler, and of the numerous accidents of the present year (many of them the subject of Board of Trade enquiries) it is difficult to suppose that such is the case.

"There can indeed be no doubt that methods of working and mechanical contrivances, the value of which has been thoroughly ascertained, have been too slowly introduced, and that there is great reason to believe that sufficient provision has not been made for the safe working of the increased traffic by the enlargement or re-arrangement of stations and sidings, and the laying down of additional lines of rail.

"But whatever may be thought of these and othes causes as contributing to the result, the present insecurity of railway travelling imposes upon the railway companies the grave responsibility of finding appropriate remedies for so great an evil."

On the subject of the frequent unpunctuality of trains it was remarked, "The inconvenience, vexation, and loss caused to passengers by this breach of the conditions upon which the companies profess to carry them, constitute in themselves a serious subject of complaint. But the evil arising from unpunctuality does not end here. The service of the line is disarranged; the chances of accident are multiplied; the trains are forced, in order to make up for lost time, to travel at excessive speed through complicated stations, or under other circumstances where such travelling may be equally dangerous."

It is further remarked that the returns of accidents to railway servants show a lamentable number of casualties, often fatal, in proportion to the numbers employed; and, finally, a hope is expressed that the railway companies themselves "will make every effort to meet the reasonable demands of the public and of Parliament."

The Board of Trade, as the branch of the Government which has to look after the interest of the public in respect to railway travelling, for which purpose it has been invested with special powers, could not with any degree of propriety have passed over, without some special notice, the alarming increase in the number of railway accidents recorded in 1872, which had increased nearly 44 per cent over 1871, 88 per cent over 1870, and 196 per cent over 1869. It is not proposed to consider, separately, the replies to this circular which were sent to the Board of Trade, as the remarks which they contained with reference to the principal causes of accident prevailing on railways, will be noticed further on under the different headings to which they respectively belong.

The means of safety which the accidents occurring last year show to be required, are thus given in the last General Report to the Board of Trade :—

1. The judicious selection, training, and supervision of officers and servants, and the preservation of good discipline.

2. Maintenance in high condition of the permanent way.

3. Good design, construction, and material of axles.

4. The application of tyre fastenings which will prevent the tyres from flying off the wheels in the event of fracture.

5. Improved coupling of vehicles in trains.

6. Signal and point arrangements with modern improvements, including concentration and interlocking of the signal and point levers, and locking-bolts and locking-bars for facing points.

7. Safety points to goods or siding connections with passenger lines.

8. Increased use of the telegraph, with block-telegraph systems for securing intervals of space instead of illusory intervals of time only between trains.

9. Sufficient siding accommodation for the collection, distribution, and working of goods traffic, so that goods trains may be shunted and marshalled independently, and kept out of the way of passenger trains, and may not encumber and endanger the traffic on the main lines.

10. Continuous breaks, to be worked by the engine-drivers as well as the guards, as occasion may require.

We propose to consider these several means for providing increased security to railway traffic under the following headings, viz.—1. Efficiency of Staff. 2. Maintenance of Permanent Way. 3. Maintenance of Rolling Stock. 4. Signals and Points. 5. Telegraph, and the Block System. 6. Siding Accommodation. 7. Break Power.

1. *Efficiency of Staff.*—It will be readily understood that, all mechanical appliances for ensuring safety being perfect, the efficiency, both as regards strength of establishment and individual intelligence, on the part of the railway staff is yet necessary in order to secure freedom from accident and danger. Even under the most perfect organisation, however, the fallibility of human nature must ever be a bar to the attainment of absolute security, but the risk may be lessened to the last practical limit by the maintenance of a fully efficient staff, and the strict enforcement of all regulations laid down for their guidance. In a paper on "Railway Accidents" read before the Institution of Civil Engineers as far back as April, 1862, Mr. James Brunlees, the author,

observed that the negligence of servants, their payment, and their hours of working, were matters of the greatest importance, and he remarked that most of the accidents caused by negligence might be traced to ignorance or to inefficiency. The wages usually given by railway companies were too low to command the services of men of intelligence, steadiness, and self-reliance, and, in consequence, inferior men were employed, who were incapable of appreciating the importance and necessity of executing their duties with promptness and exactitude. In the official report to the Board of Trade on railway accidents for the year 1870, Captain Tyler remarked, after enumerating the accidents of the year under their respective headings: "Accidents from all the above causes are more or less preventible, except in so far as it will never be possible, under the best arrangements, altogether to avoid accidents from negligence or mistakes on the part of employés, although it is practicable, under good arrangements and systems, and with good discipline, very much to reduce their number."

In the year 1871, out of 171 investigated accidents, there had been in 121 cases of negligence, want of care, or neglect of servants; in 1872, out of 238 cases, 180 were due to negligence or mistakes of officers or servants, and in 1873, out of 241 accidents, a similar negligence was apparent in 182 cases.

Whatever be the means and appliances provided, or the amount of care taken, the item of human fallibility will always be the cause of a certain number of accidents. But the number of accidents from this cause, as was remarked by Captain Tyler in his report for 1873, may be very much reduced by "improvements in regulations and discipline, by greater care in the selection, training, payment, and employment of competent men in sufficient numbers and for reasonable hours, and by providing them with the requisite siding and other accommodation, with proper signal and point apparatus, with the best means of securing intervals between trains, with sufficient break-power, and with other necessary appliances." It has been argued that railway servants are apt to become more careless in the use of these improvements, in consequence of the extra security which they are believed to afford; but, whilst Captain Tyler remarks that by the results of more extended experience this argument has received further confutation, Mr. Harrison, the President of the Institution of Civil Engineers, and no mean authority on railway matters, stated, in his inaugural address, that there was an undoubted tendency on the part of engine-men and other railway servants to believe that all these arrangements of the block system and additional signals do, in fact, provide for their safety, and that consequently they do not keep the same look out, or use the same care that they would do on a line apparently less protected, "and that this is the case," he remarked, "observation and inquiry have clearly demonstrated."

Here, then, we find two leading authorities at issue in regard to a statement of fact, and it is, of course, very difficult to draw a fair conclusion between the two. The result of Mr. Harrison's experience seems to prove that, at present, railway servants have not become sufficiently experienced in respect to the true value of signals, and other means of safety on railways, but there is surely reason to hope that, as a body, they possess sufficient intelligence to enable them in time to appreciate more fully the extent to which these safeguards are valuable, and how much also depends upon their individual discretion.

In respect to enforcing discipline, Mr. Harrison observes that the difficulty is becoming constantly greater, as dismissal is no longer a punishment, when employment can at once be had elsewhere; and a reprimand is constantly met with the reply, "Oh! very well, I'll go." This gentleman has found that nothing attaches men more to the service of a railway company than giving them comfortable cottages, with gardens to cultivate.

The efficiency of the staff on a railway depends mainly upon three circumstances: First, the selection of none but respectable and tolerably educated men; secondly, the establishment of a fixed code of rules for their guidance, and seeing that those rules are strictly enforced; and, thirdly, the maintenance of an efficient number of men to do the required work; the payment of liberal wages, so as to keep them in the service; the holding out of prospects of promotion to the most efficient; and the proper treatment of them whilst in the service.

No doubt all modern improvements on railway working tend to increase the expense to the railway companies, but this is a matter for which there is apparently no remedy. "The question of the effect of the labour market on railways, both in their construction and working," says Mr. Harrison, "has come forcibly home to every one connected with them. It is not too much to say that all new works are now costing from 30 to 40 per cent more than they did a few years ago, and nearly double the time is required to complete them."

As will be shown further on, the adoption of the block system on all lines will necessitate a considerable increase of staff for working it, and with these additional elements of "human frailty" there will evidently exist an increase in the numbers of those to whom the safety of the travelling public will be entrusted, and increased safety can therefore only be expected to result if the rules laid down for the guidance of the companies' servants are, in the first instance, judiciously framed, and afterwards rigidly enforced.

2. *Maintenance of Permanent Way.*—The accidents caused by defects in permanent way are, happily, not nearly so numerous as they were in former years. The art of constructing railways, in the first instance, and of properly maintaining them afterwards, is so much better understood now than formerly, that accidents arising from defects in its observance would be a great slur upon the professional officers of any company. In the year 1854, thirteen accidents occurred from the defective condition or neglect of the permanent way. In the following year thirty-one cases arose from the same causes, but in the year 1856 there were fewer accidents of this description, which fact may be attributed to the greater attention given by engineers to the permanent way, and to the introduction of the fished joint, and of other improved methods of connecting rails. In the year 1857 twenty accidents were caused by the neglect, or imperfect condition, of the permanent way; in four of these the permanent way had been neglected, and in five it had been constructed in a defective manner. In 1858, twenty-nine accidents, and in 1859 fourteen accidents, were due to the state of the permanent way.

In commenting on this class of railway accidents, due to permanent way defects, which occurred during 1870, Captain Tyler stated that only nine were attributable to the conditions of the way and works, or to obstructions on the permanent way, &c. "This," he observed, "is a great improvement upon former years, when, say ten years ago, 16 per cent of railway accidents were caused principally by defects of permanent way; and the improvement is due, partly to the increased strength in some cases of rails and chairs, partly to placing the sleepers in some cases nearer together, and especially to the disuse of wooden trenails for attaching the chairs to the sleepers, and to the now almost universal employment of fish-joints for fastening the ends of the rails together." As to the remedy suggested for this class of accidents, it is remarked that next in importance to proper maintenance, and even as part of it, is the question of discipline amongst those employed in repairs, with a view to ensure, as far as possible, that due warning shall be given to the engine-drivers when a rail has to be taken out, while the road is being lifted, or whenever the line is not in a fit condition to be run over at speed.

Twenty-six accidents occurred in 1871 owing to defects of construction. These defects, it was then pointed out, were not as promptly corrected as they ought to have been, as new materials were supplied, on many lines of railway; each company, or each individual officer, waiting too often to buy his own experience, and profiting too little by the experience of other companies. Defects of maintenance, which appeared in nineteen cases, occurred partly from the over-work of materials, and partly from the want of more careful supervision, and of more careful record and comparison, from which much valuable information might be obtained. The number of accidents due to defective construction of road or works was four in 1872, and six in 1873, and to defective maintenance of the same, sixteen in 1872, and twenty-four in 1873.

It may perhaps be considered that forty accidents in one year, upon all the railways in the United Kingdom, due to defective construction or maintenance, is hardly above the number that might be expected to occur from such causes, considering the vast amount of traffic which now takes place in the neighbourhood, more particularly, of large towns and cities, but it must be remembered that these constitute a class of accident which is preventible by the exercise of due care on the part of the permanent way staff, and proper supervision during construction. It is, therefore, one which should not be seen in the official returns, unless accompanied by some such causes as exceptional floods, or other reasons to show that they were not occasioned by any laxity of duty or neglect of ordinary precautions on the part of the railway company or their officials.

3. *Maintenance of Rolling Stock.*—With regard to locomotives, instances do rarely occur—and they were more common in former than in recent years—of boiler explosions, due in some instances to want of proper care in the selection of water for their use, and in others, to a faulty mode of staying the boiler. These causes of accident are to be avoided by frequent inspection, by which the earliest intimation of any deterioration may be obtained, and the employment of weakened or worn-out boilers be discontinued. During the seven years from 1854 to 1860 twenty-one locomotives exploded, but in the annual returns

to the Board of Trade only two accidents from this cause are stated to have taken place in 1870, and two in 1873; no record of a similar accident appearing in the two intervening years.

The most common accidents to rolling stock are the breaking of the axles and wheel tyres. These cases may be traced generally to one or other of the following causes: sometimes they occur in the winter months, owing possibly, in some degree, to the rigid state of the permanent way in frosty weather; some are due to the use of bad iron or steel, and others to defects either in the welding of, or in the mode of attaching, the tyres of wheels. The existence of flaws in either axles or tyres may completely escape detection until they are discovered upon the occurrence of an accident, and such cases must be included amongst the risks which cannot be foreseen or avoided. The high speed at which trains travel as a general rule must subject both tyres and axles to very severe blows and jerks, especially when passing over points, or portions of line that are out of repair, and uneven, and it is in such cases that flaws or cracks are most likely to result in a complete fracture. "There is no satisfactory test," said Captain Tyler, in his report for 1870, "to which axles can be subjected from time to time in the course of running, as far as is known, by which flaws can be detected." With regard to fracture of tyres, it was stated in the same report that in two cases the tyre was attached to the wheel by means of rivets through holes bored in the tyre, and it was remarked that the "old system of boring holes through the tyres is essentially a vicious one, and is particularly undesirable in the case of steel tyres. It affords no security in the event of fracture, and even leads to increased risk of fracture, in consequence of the weakening of the tyre at the sides of the rivet holes."

In 1871 there were twenty-two accidents of this class, in which three persons were killed and thirty-four were injured; in 1872 there were seventeen accidents, occasioning the death of two passengers and five servants of companies, and injury to forty passengers and eight servants of companies, whilst in 1873 there were twenty-three accidents owing to the same causes, killing ten passengers and two servants of companies, and injuring fifty-four passengers and seventeen servants of companies. The chief methods recommended for adoption with a view to avoiding accidents from the breaking of tyres, consist in the use of improved modes of fastening them to the rims, so as to prevent them from flying off the wheel. They may fail from the brittle nature of the material, or from defects of manufacture, or from being too tightly shrunk on the wheel, and they have frequently failed from one of these causes, or from a combination of them. The danger consists, not in the fracture, or in the tyre becoming divided, whilst running, into two or more parts, but in the probability of the tyre, which is, or ought to be, in a state of tension on the wheel, flying suddenly and violently from it when fracture occurs, and this danger is greater with steel than with iron tyres.

3. *Signals and Points.*—During the seven years from 1854 to 1860 inclusive, as many as eighty-eight accidents happened from the use of improper or inefficient signals. Accidents have been caused by the total want of signals, especially at sidings, others have arisen from their defective form, or from their bad position. Many accidents have occurred in connection with distance signals; in some cases they have been placed so near to the station that the engine-driver has been unable to stop within the space allowed. It was observed by Captain Tyler in 1870, that out of sixty-one collisions, independent of the collisions at junctions or level crossings, thirty-one, or more than half of them, were due to defective arrangements with regard to signals or points, but that in twenty-eight cases out of these negligence was combined with the defects, and that the latter contributed more or less to the negligence; and out of eighteen collisions at junctions there were ten cases in which defective signal and point arrangements were the cause. In 1871 there were fifty-three accidents caused by defective signal and point arrangements, or want of locking apparatus; in 1872 the number of accidents due to similar causes was seventy-one, and last year it was seventy-eight, so that this cause of accident would appear to be growing rapidly in importance.

When trains were few, and the speed at which they travelled was moderate, a comparatively crude method of signalling sufficiently answered every purpose; with the increase of trains, the complications of junctions, and the greater difficulty that consequently existed in controlling a number of signals at any one point, it became necessary to place all the signal and point levers in or around the signal cabins; and, in order to afford a better view to the signalman, the cabins were raised to a greater or less height above the ground, and placed in convenient situations, according to local circumstances. But even then, when the control was more conveniently placed in the hands of one man, there was still, as the levers in or near a cabin became more numerous, a liability to mistake, from the signalman pulling over a wrong lever; or the levers were fastened over by blocks of wood, which the signalman forgot to remove; and to prevent such mistakes, and serious accidents resulting from them, it became further necessary to interlock the levers with one another. By 1860 many improvements had been introduced upon the interlocking system, and the inspecting officers of the Board of Trade began to insist on the use of locking apparatus at the junctions of new branches with existing lines.

By the application of locking and other apparatus it is possible to prevent nearly all accidents from collision occurring, in the ordinary way of working, in consequence of any mistake of the signalman. Conflicts between signals, and conflicts between points and signals, may alike be avoided; and a good combination of locking-bar and bolt may be made to insure that the facing points are completely over before the proper signal is lowered, and may also prevent them from being moved during the passage of a train. It is, of course, impossible to provide against all the contingencies which may arise—such as, in certain cases, against the absolute neglect of drivers to pay attention to the signals made to them; or such as a signalman, when two trains are running towards a junction at one time, setting his points and lowering his signals first for one of them, and then altering them and preparing for the second train, without allowing time for the first train to stop short of the junction. But provision may be made, and is made to some extent, even for the contingency of an engine-driver neglecting to obey signals.

In a paper recently read before the Institution of Civil Engineers, by Mr. R. C. Rapier, a detailed description of signals and points was given, besides an account of different methods of interlocking the two, so as to avoid accidents which might occur in the event of wrong signalling. It would be impossible to follow out that paper in detail here, but we may briefly state that it was there shown that the mere connection of switches and signals was not sufficient, but that effective interlocking required the movement of the switches to be completed before the alteration of the signals could be made, and *vice versa;* whilst, as regards facing-points, it was stated that, although it was desirable to avoid them as much as possible on a line of light traffic, the use of facing-points, properly controlled, might be made one of the greatest safeguards where trains were frequent, and travelled at different rates of speed.

5. *Telegraph and the Block System.*—In two papers on "Railway Accidents," by Mr. Brunlees and by Captain Galton, read at the Institution of Civil Engineers in 1862, it was deduced from statistical tables that the great majority of accidents were attributable to preventible causes, and that, of these, 27 per cent were due to the absence of the electric telegraph. The advantages of the telegraph in connection with the working of railways were dealt with in an able paper by Mr. W. H. Preece, which was read at the Institution of Civil Engineers so far back as January, 1863, and although all the views expressed by him on the subject at that time have not met everywhere with approval or adoption, the system generally has come to be recognised as absolutely necessary for the safe working of any line of railway, and it forms a most important element in the now universally adopted block system.

The first attempt of a block system introduced on railways was by maintaining a presumed time interval between trains; this plan, however, failed, because those intervals could not in practice be observed; and the permissive system for reducing the time intervals by the aid of the telegraph, and sending trains timed to travel, and capable of travelling, at various speeds, one after another, into the sections, with a caution to each, may also be considered to have failed, because it does not afford sufficient protection to the traffic. Under these time systems collisions have occurred from engine-drivers slackening their speed to avoid collision with trains in front of them, and being run into by trains behind them. The greater the variety of speed between the trains, the more does the weakness of such systems become apparent.

The proposal to divide the line of railway into telegraphic sections, and thus to preserve space intervals between trains, was made by Mr. (now Sir William) Cooke, as far back as 1842, and was first practised, it is believed, on a portion of what is now the Great Eastern Railway, in 1844; and, subsequently, a train telegraph system was established on portions of the London and North Western Railway. This latter, however, was not a block system, or a space system, but a time system worked with the aid of telegraph instruments, and it is now known as the permissive system. As regards the block system, there are many descriptions of instruments for working it, and various rules and regulations applicable to it on different lines of railway. The main principle involved is simply by the division of a line into block sections, and allowing no engine or train to enter a block section until the previous engine has quitted it, to preserve an absolute interval of space between engines and trains. This may be done mechanically or electrically. Any means of communication with which the signalmen may be provided will enable them to inform one another of the approach of a train, of its entrance into a block section at one end, and of its exit from that block section at the other end.

Mr. Harrison, the President of the Institution of Civil Engineers, has stated that the block system will, as soon as it is possible to complete the necessary works, be introduced throughout the whole of the railways in England. It was stated by Mr. Farrar, before the Select Committee of the House of Lords last year, that the railway companies had already spent upon introducing the block system, and the system of interlocking signals, between £700,000 and £800,000, and they were proposing to spend a great deal more. Besides this expense there is a considerable annual cost to be incurred in working those systems; the increased cost of the staff alone is estimated for the Great Eastern Railway at £13,860, and on the Midland at £130,000 per annum. In the case of the North Eastern Railway it is calculated that on the completion of the block system, the number of signalmen will be increased from 500 to 2000. Mr. Rapier, in his paper to which we have already referred, shows that the probable cost of the interlocking and block system on fourteen of the principal railways would be about ½ per cent on the whole cost of the lines, and that then their carrying power might be so increased that three times as many trains could be run on the block system as without it, and with greater safety. The probable cost of maintaining the block system was stated to be about 2½ per cent on the traffic receipts, and this comparative percentage was less on the lines which had a great number of points to protect than on some of the light traffic railways.

6. *Siding Accommodation.*—It was pointed out in the Report to the Board of Trade on Accidents that occurred during 1871, that collisions at stations often occurred from the want of accommodation at the stations or sidings, passenger lines being unduly obstructed from the want of sidings in which to place slow or stopping trains, or in which shunting may be performed. The same deficiency of accommodation may also be the indirect cause of collisions on the line between stations, when, for instance, from the want of siding accommodation, a slower train is despatched in advance of a faster one, without a sufficient

interval between them to allow of its proceeding forward to the next place of refuge before it is overtaken, and it is stated that the want of improvement in, and addition to, the siding accommodation, combined with the want of modern appliances for working the points and signals from suitable cabins, and interlocking the levers with one another, and of telegraph-working for assisting in protecting an obstructed station, have principally to answer not only for the accidents themselves, but also for the negligence of the servants by which those accidents were more or less directly occasioned.

7. *Break Power.*—The subject of break power is one of especial importance, many lives and much property being hourly dependent, in a greater or less degree, on the power and efficient state of the breaks. It has been found that most of the collisions which have occurred might have been prevented had those in charge of the trains possessed the power of stopping within a few hundred yards. This is more particularly necessary on account of the high speeds and heavy trains now adopted on all lines. It is therefore essential that there should be ample break power to each train, and, whatever system may be adopted, it should be powerful, simple, and capable of being applied in the shortest possible time. On certain railways, where the necessities or convenience of the companies have been the means of inducing more rapid improvements in this respect, systems of continuous breaks have for many years been in successful operation; and the experience of these lines has left no doubt of the value of such systems of breaks. Amongst the simpler means of providing extra break power are—increasing the numbers of guards and of break-vehicles; enabling a guard or breaksman to apply the breaks of two adjacent vehicles; allowing the guards and breaksmen to walk through the trains, and to apply the breaks of the various vehicles provided with them; or by such a system as may enable a guard from his own van to apply the breaks of several vehicles, in which may be combined an economy in guards with efficiency in break power. In the use of any good system of this description, it becomes unnecessary to skid the wheels of break-vehicles, and flat places in the wheel tyres are thus avoided. Perhaps the most perfect system of continuous breaks yet introduced is that which enables the engine-driver to control the train, and by means of compressed air to apply all the breaks at once without the development of any manual exertion.

The limit of space to which we are necessarily confined for a single article has prevented any detailed account of the various methods of intercommunication in trains, which, by the Regulation of Railways Act of 1868, is directed to be provided in every train carrying passengers and travelling more than twenty miles without stopping, or of the several other minor arrangements suggested, or introduced, with the view of more effectually securing the safety of passengers.

With the adoption of the improved methods of interlocking signals and points, and of the block system, no doubt very considerable addition is made to the safety of travellers, but the companies are thereby put to great additional expense, both in first cost and for subsequent maintenance, for which the only return they can look to is an increased immunity from accidents. To ensure absolute security is not, however, possible, by the adoption of any means hitherto suggested. The introduction of the block system necessitates the maintenance of a considerably increased staff of signallers, and at the same time it introduces so many additional elements of human fallibility, whose liability to err can only to a limited extent be guarded against by the employment only of competent men, and the strict enforcement of such rules and regulations as it may, in each case, be considered advisable to frame for their guidance.

III. HUMAN LEVITATION;

ILLUSTRATING CERTAIN HISTORICAL MIRACLES.

ACCORDING to Archbishop Trench, a physical definition of man might be given as "the animal that weighs less when alive and awake than dead or asleep" ("Notes on the Miracles," ed. vii., p. 289). This he calls "a fact which every nurse who has carried a child would be able to attest;" and refers to Pliny ("Natural History," vii., 18). He concludes, "that the human consciousness, as an inner centre, works as an opposing force to the attraction of the earth, and the centripetal force of gravity, however unable now [*i.e.*, since Adam's fall] to overbear it." Unluckily, Pliny's words do not make this form of psychic force a human prerogative. They are, "Mares præstare pondere; et defuncta viventibus corpora *omnium animantium*, et dormientia vigilantibus." Perhaps, even in the absence of any accurate experiments, we might pretty safely say this very broad statement is unproved—that some animals might be found in whose weight, awake and asleep, or alive and dead, our most delicate balances would not detect a difference. It is said to have been so with the fish, alive and dead, about which our " merry monarch " suggested to the founders of the Royal Society one of their first subjects of inquiry. But it was not pretended to be settled without experiment; nor does it now appear how the Archbishop and his authorities, the nurses, could be contradicted as yet by anything short of direct and special experiments, and those not very easy, nor likely to be made with much accuracy to-day or even to-morrow. The monarch's joke (if such it were) was said to impress a lesson on the scientific men; and it really seems as if the archbishop's bit of traditional physics might unwittingly teach one greatly needed by those of the present day, who repeat, *ad nauseam*, that in their studies experiment has superseded dogma, whatever may be the case in other subjects. Le Play names the following authors as pledged to this position, " Les sciences physiques, disent les nouveaux docteurs, n'assignent à l'homme aucune place exceptionelle dans la nature; car il se confond par des transitions insensibles avec les autres animaux.* MM. Baumgartner, Büchner, Burmeister, Cotta, Czolbe, Feuerbach, Giebel, Huschke, Löwenthal, Lotze, Moleschott, Muller, Orges, Rossmässler, Strauss, C. Vogt, R. Wagner, Zimmermann." He invites any of them to correct him if they are misrepresented by this sentence from Büchner's " Force et Matière," 1865, p. 234. " The best authorities in physiology are now sufficiently agreed that the soul of animals differs not from the human soul in quality, but only in quantity." Now, without implying that Trench's dictum above is anywise better founded, or that a physical difference between human and brute soul can or ever will be detected, we would humbly ask, supposing two physical propositions like the following were made—which are not so absurd as plenty that have had to be met, and very laboriously demolished—could any or all of those eighteen physicists, or all European science, lay hands to-morrow on disproof thereof? Supposed proposition :—" (1). That between the weight of every adult man (or, say, male white) with and without his soul, *i.e*, alive and dead, there was a difference of a drachm and a quarter; but (2) that in the case of no brute animal (or, say, no woman or no negro)

* L'Organisation du Travail, pp. 231-2.

was there a thousandth of this difference; or, if you please, always a difference in the contrary direction," so that the human and brute soul (or the male and female human, or the European and the negro) should be so widely different physically as for the one to possess gravity and the other levity: would there, in any of these cases, be any record of experiment to this hour that could negative the statement? If not, then who, the theologian or the scientists, are the rasher dogmatists, or the more unphilosophical?

Surely Faraday had done all his teaching before that wonderfully unlucky dictum, that investigators ought to approach an inquiry with "preliminary notions of the naturally possible and impossible." It amounts to this, that before investigating whether nature contains x, you ought to know what nature does and does not contain. A tolerable attainment truly. The only man we have known, and perhaps the first, to claim this sublimity of knowledge, was Daubeny, who, in the stir raised by Colenso's denial of Noah's story, or, rather, the saying of Christ about "the day that Noë entered the ark, and the flood came and took them all away," deigned to assure the clergy (in their paper "The Guardian"), and, apparently, to their satisfaction, that "nature evidently" [to botanical professors] "contains no forces competent to produce" the described catastrophe. Faraday's requirement then is even exceeded; here is knowledge not only of what the universe contains, and does not contain, but of what it did or did not many centuries ago! But we are wrong in calling his knowledge unparalleled. Here is a perfect parallel elicited by the very same subject, "The Noaic Deluge," by the Rev. S. Lucas, F.G.S., a writer just as certain (from Geology) that such deluge happened, as Colenso and Daubeny that it did not. At page 2 of this little book, Mr. Lucas says, "We run no risk of contradiction when we affirm that nothing at the command of mere natural law, or of mere material force, could produce it." He knows, then, what of law and of material force universal nature commands—nay, what it did and did not command at a certain past epoch; and, more, knows this so well as to "run no risk of contradiction" in affirming it! He proceeds, "No mere natural forces [mere !], however gigantic and powerful, could break up all the fountains of the great deep, and open the windows of heaven: consequently, these events must be deemed miraculous," &c. Why is it that no Englishman of this generation seems ever to touch either of these questions, what is natural, and what is miraculous, without deeming that "consequently" the other is called up? What is their connection? We never could see more than between the questions whether a writing is true and whether the ink is good. Mr. Lucas goes on, "It is not the gentle whisper of quiet, nor the louder utterance of agitated, nature, but the thunder-voice of Almighty God which speaks in it." (Elijah, then, greatly erred, it seems, in his estimates of what was in the wind, earthquake, and still small voice). But if some things are done by "agitated nature," and others are beyond her power, and yet done, how many Creators or Lords have we? Evidently two, according to Mr. Lucas, a scientist who, like Daubeny, is up to the Faradayan standard, having apprehension of "the naturally possible and impossible."

This ditheism, and the said knowledge or faculty, though doubtless extant long ago, our reading traces no further back than Dr. Newman's "Essay on Miracles," published about 1830, or fifteen years before his change of religion. He makes it one "criterion" of a miracle to have no physical cause. The second of his classes of doubtful ones is "(2.) *Those which from suspicious circumstances attending them may not unfairly be referred to an unknown physical cause.*" Now, granting for the sake of argument, that it is *de fide* that some events happen with no physical cause, how are we to distinguish these? A most important task, because they are claimed as the miraculous credentials of various opposed creeds; and any physical cause, known or unknown, according to Dr. Newman, vitiates their claim. Whoever is to apply this criterion plainly needs, no less than the very requirement of Faraday, the very attainment of the above authors—an exhaustive knowledge of the universe and its contents. This is only possible by inspiration, as Dr. Newman would doubtless allow; and as high a degree thereof as ever was ascribed to any; for, surely, to know what is in nature would be a gift nowise inferior to His who, we are told, "knew what was in man." But no less a faculty does Dr. Newman make necessary to any and every learner who will know whether to regard a given fact as miraculous; though the sole use of miracle is to attest to him another's occasional and limited inspiration. What it is to prove is the inspiration of certain words; but to find whether it is any proof, you must first have inspiration unlimited.

Locke said, "To discourse of miracles, without defining what one means by the word" (which is all I find present writers doing) "is to make a show, but in effect to talk of nothing." As his definition does not quite satisfy us, we would define a miracle to be "a predicted occurrence, so improbable when predicted, that when accomplished it convinces men of a superhuman* presence." The time between prediction and verification may be half a minute, or fifty centuries; it may be "Peace, be still," or "They shall afflict them 400 years,"† or "Be thou clean," or "Go and wash in Jordan seven times;" but prediction there must be. Voltaire alone, that we are aware, points this out, that prediction is the prime essential. Without it there is no proper miracle, for all value lies in the event's relation to some antecedent utterance—not quite always in its verification, but, occasionally, when it is a king's, in its falsification: as when Nebuchadnezzar's prediction that he would burn the three Jews was falsified; or that of the Arian powers that they would instal Arius; or that of Julian that he would restore the Jews their temple.

Now to take a notorious case or two: If Peter made the prediction ascribed to him in Acts, v., 9, and Sapphira fell as

* "Superhuman," or superkingly, does not of course imply "divine;" but Locke showed that when a miracle is set forth as divine, nothing can be simpler than the test of this claim. It will be divine if carrying "the marks of a greater power than appears in opposition to it." Professor Tyndall says he is asked to infer, from Aaron's rod or snake being larger than those of Jannes and Jambres, that these men were "not good." On the contrary, it is nowhere implied that they were less good than Moses. As far as appears, they faultlessly performed their duty, a most important one, similar to that on which the late emperor sent Houdin to Algeria, and for which we have here daily more pressing need. Houdin, by outdoing the Arab jugglers, arrested their mischievous authority. Pharaoh's magicians, tried their thaums (not juggling, apparently, but spirit sorceries), and made to their master an honest report, that, if credited, would have saved him and his army. From the snake experiments, as well as later, they logically inferred, "This is the finger of a god," or, rather, of the Supreme. If Aaron's serpent swallows ours, and we cannot produce one to swallow Aaron's, it is safe to infer that he is right. There can be no power greater than Aaron's God; because the Supreme (if there be any) would not allow an advantage over himself to be claimed and shown. This would be abdicating, and he would no longer be supreme. That "as Jannes and Jambres withstood Moses," so did certain bad men resist truth, nowise implies the former were bad. What they did, we maintain, ought always to be done in like cases.

† Longer periods might be instanced by travelling out of the legends in every child's hands. *E.g.*, Sir Isaac Newton deduced from Rev. xii., 6, that the corporate holders, in his day, of the ruins of ancient Rome, would hold possession thereof altogether 1260 years. He did not undertake to ascertain or state exactly when this possession had begun; but it is easily found, in Baronius, &c., that their first holding any of those buildings (namely, the only one in Europe that, "prepared" in antiquity, continues unruined and in use) dated from bequest of Phocas, who was deposed and killed in Sept.-Oct., 610. Moreover, whenever this class of facts come to be examined, it will be found that every date-prediction in the Old Testament has been verified, and, if observed, would have ended religious strife; and that the very longest, extending to 2300 years, had its consummation in this, our generation, not merely to the year, but the month and day.

stated (for we would no more rank her husband's death among miracles than the liar's recorded in Devizes market-place, since it was not predicted); or if the prediction in Matt., xvii., 27, was made and verified as the story implies;* either of these, we may presume, would be held as thorough miracles as any on record or tradition. But where, in either of them, is there room to insert anything without physical cause, or to make a breach of continuity? Would Dr. Newman say the act of the fish picking up a shining coin was unnatural; or less natural at one time, the moment before being caught, than at any other time? Would Mr. Lucas "run no risk of contradiction in affirming that nothing at the command of mere natural law could," at that time, cause apoplexy in Sapphira? and, "consequently, these events must be deemed miraculous." What have the question of miraculousness and that of naturalness to do with each other?

Dr. Newman repeated, in his "Apologia," this paradoxical distinction between the "miraculous" and the "providential," and, on further inquiry, assured the present writer that "special providences are one kind of Divine works, and miracles are another." But on being asked for some notion of the difference, he was silent; and we believe that any number of divines or philosophers may be challenged in vain ever to make out a more real or objective distinction here than if he had said, "Stars near enough to have a detected parallax are one kind of Divine works, and those beyond present parallax-measuring are another kind." Whether instruments exist that have detected a star's parallax or not, is precisely and merely such a difference as

* The passage is one of those most needing re-translation. "The takers of [temple]-shillings came to Peter, and said, Does not your master pay the [temple]-shillings? He saith, Yes. And as he entered the house, Jesus anticipated him, saying, What thinkest thou, Simon? From which do the kings of the earth take tax or toll? From their own sons, or from strangers? And when he said [*Vat.* and *Sin.* MSS.] From strangers, Jesus said to him, In that case the sons are free. But lest we make these men stumble, go thou to the lake, cast hook, and the first fish rising do thou take, and having opened the mouth thereof, thou wilt find a florin, which take and give to them for me and for thee." Definite coins ought by no means to be rendered "a piece" or "pieces," and the monetary terms used in translating the New Testament are most important, as influencing future language. The *δηναριος*, being a day's wage, ought to be rendered a *dollar*, as long as our coinage has no term better than the ambiguous and most barbarous "crown" and "half-crown." Lacking also any term like *talents* (for want of which we have such slang as "ponies" and "monkeys") this has to go untranslated. But *μνας* might be *purses*; *στατηρ* and *δραχμή* should be *florin*; and *δίδραχμα*, *shillings*; *ασσάριον*, a *groat*; *κοδράντης*, a *penny*; and *λεπτά δύο ὁ εστι κοδράντης*, "two *obols*, which make a penny," giving this decent word a chance of superseding our wretched "halfpenny."

whether the physical modus of an occurrence, say, of the fish getting the coin into its mouth, or of the great deep being broken up, and mankind swept off "in the day Noë entered the ark," is or is not satisfactorily clear to Mr. A. or B., to Daubeny or Lucas. The difference, then, between miracle and non-miracle would be purely subjective, and different for any two men existing, or that ever will exist. Thus, to Mr. Lucas, who finds from Geology that the deluge happened, but "knows" the universe to contain no "mere natural law" that could effect it, this event would be a miracle; while to us, who can find in no part of either its legend or relics the smallest departure from the laws even now known of the visible part of nature, and are driven to credit such legend by its minute correspondence in detail with all the latest discoveries in nature, it would be *no* miracle!

In the stifling dust raised around all these matters since Dr. Newman's paradox, Professor Tyndall has made the true remark that there can be no prayer-granting without miracle, or, rather, the miraculous element. If unseasonable weather changes, after prayer for "seasonable" weather (the only prayer thereon ever sanctioned by the church), into seasonable, this implies the miraculous, but no more of the unphysical than Peter's fish. Dr. Tyndall, however, first ignoring the church's important adjective, compared this to a prayer that an eclipse might be delayed; as if such delay would be a thing "seasonable." He then lays down as "science" the gratuitous paradox that winds and clouds of to-morrow may be, like the planetary motions, pre-determined by only brute cosmic forces; which, if as true as it is demonstrably false, would not even then give the fixity he wants, as the planetary system itself is invaded at any moment by unknowable comets and meteors, and solar radiation hourly altered by storms of the photosphere. He requires, at the outset of his attack, all the present century's discoveries to be ignored. But let us grant him a solar system as simple as mediæval ignorance ever fancied; this would not help him. Yonder is a gardener, who may dig twenty more spadefuls before dinner, or perhaps only nineteen. Is Dr. Tyndall prepared to prove that whether they shall be twenty or nineteen is already as determined, by laws of brute matter, as the next transit of Venus? If not, he should have warned readers that the whole prayer argument was a mere *jeu d'esprit*, hanging on the assumption of this extreme necessarianism. Relax one stitch thereof, and the whole fabric falls, thus:—If there be any uncertainty about that twentieth spadeful, on this may depend whether a slug is turned up or not; on the slug may depend a young swallow's dinner who is feeble, and on this may depend whether he shall follow his colony, and reach Africa; but on this fledgling's arrival or non-arrival may depend whether a certain insect shall serve him for supper, or be left to lay a million eggs, which, in that case, will next month be each a locust laying a million more; and on this billion of locusts and their progeny may depend whether at Christmas all Ashantee and three Senegambias of forest shall be green as Eden or a leafless wilderness, and its mean temperature 100° or only 70°; and on whether such an area be the hottest or coolest portion of the planet's intertropical lands, may well depend, by Dr. Tyndall's own showing, the winds and drought or wet of a season, over half Europe or the whole. It behoved him, then, to be quite sure about that gardener's last spadeful, and all such causes, which yet he wholly leaves out of account! The weather of large districts may as plainly be still more quickly affected by events that acts of man or beast unconsciously bring about—as forest fires; avalanches that a goat may set rolling; dykes burst, and Zuyder Zees refilled for ages by the burrowing of a rat; shoals of herrings or of whales that by turning right or left may make a month's difference in the break-up and drifting to us of half a year's polar ice. Here we confine ourselves to visible nature and known forces. Let the insane assumption be granted that there is no invisible nature, nor aught unknown, and even so, He that owns and actuates the cattle on a thousand hills, might thus plainly, by only one of their hoofs, make the winds his ministers, and flames of fire his messengers.

The flood of paradox thus accumulating from the crudities of teachers of the most different schools makes all we have said, and more definition, necessary for protection, ere one can touch matters even distantly bearing on these. The writer hopes, therefore, he will be understood to have no knowledge as to the limits of the "naturally possible and impossible;" and lacking this, or any ground for the conceit of two such differentiated powers as "nature and God," he can only understand by "nature" the course of whatever has happened; and thus can make no distinction between saying a thing happened, or that God did it, and saying it is natural. "If the dead rise not, then is Christ not risen," and he who thus wrote, we suppose, would have added, if relevant, "and if the living are never born of virgins, then was Christ not so." Before a man can know this to be never natural, he must know that neither was Christ so, nor Adam, nor any pre-adamite man, mammal, or ascidian; that no virgin ever bore the head or progenitor of a new race, to be superior to her ancestral one. Now, since Aristotle's eternity of generations is by general consent now exploded, it seems, on the whole, more probable that this thing may have sometimes happened; even as many times as the earth contains distinct organic species. But if only once; or if but one planet has once been overtaken by steam enough to deluge it totally in one day; or if a bodily levitation, like those of Mr. Home, testified by the editor of this journal, by Lord Lindsay, and many others, has only once occurred to any man; then each of these occurrences is sometimes natural. Under what conditions, we may never be able the least to define, but whatever happens we must call natural, whether the naturalness be clear to few or many, to all or none of us. We can only hold it to be within the regular course or evolution of the Eternal Evolver, "with whom is no variableness nor shadow of turning," and whose machinery more than one ancient termed "His wheels" (Ezek., i., 16; Dan., vii., 9); the continuity of which, through miracle and non-miracle alike, the former seer surely not obscurely intimates. "Whithersoever the spirit was to go, they went, and turned not." No reversal or instant of interruption in the Evolver's laws. "When the living creatures went, the wheels went by them." "When those stood, these stood;"—"for the spirit of the living creature was in the wheels,"—not away, or distinct from the mechanism. And though wheel be within wheel, and "as for their circles, they were so high that they were dreadful," yea, worshipped; and in these days, no less than Ezekiel's, it has been "cried unto them in my hearing, O Wheel;" yet is this twice repeated that they are "full of eyes," and that the living spirit is not distinct from, but is "in the wheels."

Neither spiritualists nor admitters of historical miracle, then, believe, as they are taxed with doing, any interruption of natural law. We as fully hold the continuity and eternity of evolution and its laws as Sir William Grove does. But to the false charge of our holding this superstition, there is added the boast that modern practical science has abandoned the same, and insists on absolute continuity. This is no truer than the above, as an example from those making most practical claim to science will show. While the Sydenham Crystal Palace was building, in August, 1853, a scaffolding fell and killed some men, so that an inquest had to be held. The witnesses and experts, including three first-rate engineers, named Vignoles, Crampton, and Fox, agreed that the fall was "one of those events that cannot be accounted for;" that neither the materials nor plan of construction could be better (*Builder*, 1853, p. 546); the two former that both were such as they would repeat identically, and Crampton advised that this should be done, "believing it to be perfectly safe,"—safe by physical law, though in fact it had fallen. Neither our engineers nor coroners' courts, then, object to miracles in Dr. Newman's sense—events without physical cause, or breaches of physical law. They will on occasion, on what they consider "dignus vindice nodus," admit as readily as any mediæval monk, that "Deus intersit" in this capricious manner. But as Paul said that if the Sadducees were right, he and his fellows must be "false witnesses of God," in testifying that he raised up a man from death, "whom he raised not up if so be that the dead rise not;" we must admit that, similarly, if the view of continuity common to St. Paul, Grove, Tyndall, and ourselves be sound, these engineers are just such witnesses, in swearing that, to overthrow the scaffolding, He interrupted natural law, which He did not interrupt if (as we hold) it is continuous.

Events that we hold to be, like all events, natural and "in the wheels," but which are not explicable without the volition of unseen beings, and have so been taken to attest the presence of an invisible population, require some distinctive name. Any that were clearly predicted would of course, by my definition, be miracles; but when they are not predicted, this definition excludes them, and I would suggest the non-scriptural but classic and patristic term *thaums*. In every historical age of the most civilised countries, these have been as well attested as any terrestrial facts, not reproducible at will, can be attested; and during the centuries, before and since the Reformation, that the frightful superstitions as to the crime of witchcraft held sway, plenty of such facts were always sufficiently testified to induce English judges and juries, and afterwards American ones, to consign hundreds of unfortunate, harmless women to death. One phenomenon always then held to fix this crime—and which, if proved in court, would cost the subject his or her life—was bodily levitation; in which some force was seen to work, in Archbishop Trench's words, "as an opposing force to the attraction of the earth," and also "to overbear it." The thing is testified now, of Mr. Home, a Scotchman, the American Davenports, when children, the eleven cases already recorded, as witnessed by the Editor of this Journal, and probably of more individuals now living, and by a greater mass of respectable testimony than it ever was in any single past age; though there were examples in every age of which civilised records remain. Now the most striking point is the close correspondence of minor details in the old accounts with those noted by modern witnesses, who evidently never saw nor received any tradition of those accounts, and, indeed, are generally under the error that the whole is as new a discovery of this age as galvanism or photography.

As Newton is held to have proved that gravitation and inertia in every mass are proportional, we might expect that whatever overbears the former would be equally capable of neutralising the latter; and, in fact, the elder records hardly speak of visible suspensions like those of Mr. Home, but mainly of sudden unseen transfers of the person to a distance; like that alleged of Dr. Monck last year, from his own residence at Bristol to the garden of his friend, Mr. Young, at Swindon; or the earlier but better attested one of Mrs. Guppy, from her house at Holloway to a circle of her friends assembled at No. 61, Lamb's Conduit Street; or, a few months ago, that of Mr. Henderson, a well-known photographer of London, for a smaller distance, but attested by eighteen persons besides himself—the nine assembled with him at Mr. Guppy's, and the whole Stokes family, at Highbury, where he was unexpectedly found. It is easy to see that two or three such transfers occurring to one man, as Abaris the Scythian, in the time of Pythagoras, could not fail to procure him the surname of "*Æthrobat*," or air-walker; and in the next age the story that Apollo, of whom he was a priest, had bestowed on him a golden arrow, whereby to be conveyed wherever he desired. But this most natural error, that the adepts can be levitated at will, and in what direction they please, does not tinge the older, yet more sober, record of our earliest historic æthrobat, him of the Old Testament. "And behold," expostulates the courtier to whom Elijah (vainly sought for three years) had first reappeared, "thou sayest, Go, tell thy lord, Behold, Elijah is here; and it shall come to pass, as soon as I am gone from thee, that the spirit of Yahveh shall carry thee whither I know not; and so when I come and tell Ahab, and he cannot find thee, he shall slay me." Then he recounts his piety (1 Kings, xviii., 11), "And now thou sayest, Go, tell thy lord, Behold, Elijah is here; and he shall slay me." He dares not go till the prophet has sworn, "I will surely show myself to him this day." Like all our modern æthrobats, though he cannot will nor direct his levitations, he can prevent them. The allusive and matter-of-course way that their general fact here comes in, so that, but for this and one mention after his final disappearance, we should not guess the phenomenon to have occurred in all Hebrew history, is inimitable, and makes it far stronger than if particular cases had been described. Not even was it introduced by any such note as that respecting young Samson, "And the spirit of the Lord began to move him at times in the camp." It seems assumed that readers of this brief abstract from the annals will no more need telling that Elijah was frequently air-borne, than to be told what country the Pharaoh ruled; or than the sons of the prophets needed to explain when, after his ascension, they said to his successor (2 Kings, xii., 16), "Behold now, there be with us fifty strong men; let them go, we pray thee, and seek thy master; lest, peradventure, the spirit of Yahveh hath taken him up, and cast him upon some mountain, or into some valley. And he said, Ye shall not send. And when they urged him till he was ashamed, he said, Send. They sent therefore fifty men; and they sought three days, but found him not. And . . . he said unto them, Did I not say unto you, Go not?"

About three centuries after this, but in Europe, and not yet in quite such broad historic daylight as in Ahab's Israel, we find the pair of levitants, Abaris and Pythagoras. If it were still mythic twilight, all or the main share of this and any other marvellous feature, would be heaped upon the crown of the great seer and martyr, the greatest European of his age, or perhaps of any age, the founder of the most civilised religion of the next thousand years, whose votaries ceased to quote him by name, but only as "*He*," and maintained that "Three kinds of being are biped—birds, men, and our master." But the habitual air-walking is ascribed only to his humble friend Abaris, of whom nothing else is known; and but one single levitation to the great sage. According to all his three biographers, it was universally held that once he had on the same day addressed a class of his disciples in the city of Metapont (near the modern Taranto), and another circle in Taormina, at the foot of Etna. As in every modern case, we observe, it is only into the company of friends, either recorded to be at that moment speaking of him, or very presumably having him in mind, that the levitant is carried. That Pythagoras was a born thaumaturge, or first-rate "medium," as it is now called, appears, apart from all legend, by the most remembered of his peculiar dicta, namely, that "All things whatever are to be hoped for, because all are possible to the Gods." This is evidently identical with Christ's most enigmatical doctrine of receiving "whatsoever ye ask believing;" and is a sentiment that, true or false, could never enter the head of a person not familiar with thaums, and these of manifold kinds and sorts. Moreover, the followings gathered by this old man, as well as by the short public career of Christ, could not be accounted for by mere eloquence, however great. And surely it needed a prophet, in the fullest sense, to teach, 2000 years before any other European would believe, that the earth is a planet, and turns on its axis; as well as to develop that psychologic and religious creed, of plural incarnations of the same soul at long intervals, and suspension of memory during each earthly life, but restoration after it, which, though the creed for the last twenty centuries of a majority of our race, is barely now penetrating Europe: and of which, by the way, we may remark that if Christians oppose it, they have to make their own Master ("whose goings forth have been from of old, from everlasting"), a mendacious witness, alike in saying Abraham had seen his day and rejoiced to see it; or that Nicodemus and similar men, must be born again before they could even see his kingdom; or repeatedly affirming his young cousin John to be Elias, when it is plain that the son of Elizabeth could not be Elias if there be no "resurrection of (or in) the flesh."

Passing over apocryphal stories, chronology brings us next to the Author of Christianity and his opponent; for all the accounts we have of the most public prodigy—unless that of feeding the 5000 were more so—and apparently the first quite public one wherein Jesus figured, represent this wonder as not his spontaneous act, if his at all, but provoked, or rather wrought, by his adversary, "the devil" of the first and third gospels; who, according to the Jewish legends, was the same Judas who afterwards compassed his capture and death. Among the six legends extant, four coming to us by tradition of his church, and two by that of his enemies, only one, the gospel of John—a treatise professedly supplementary to others—omits this event; and in that very document, out of three times that Jesus mentions a Διαβολος, in the first he refers simply to the said man; as the writer himself informs us after recording the speech, "Have not I chosen you twelve? and one of you is Διαβολος." In the other gospels it is used but thrice by Him, always in parables; and nine times by the narrators, solely in this story, where it is an alternative term for "tempter," though Mark, who gives no detail thereof, uses the Hebrew "Satan," and Διαβολος does not occur in his gospel. It is a word, then, used by no narrator except in this story; in this by only two of them; and elsewhere by Jesus alone, either in mystical doctrine and parable, or, once out of three, as a name for Judas. From the gospels alone, the only natural conclusion respecting this "tempter," whether seen conveying Jesus about, or with him on "the pinnacle of the temple" (necessarily visible to multitudes), would seem to be, that he was some man well-known to the multitude; and this has been the view of those critics in the present age who, like Strauss and Rénan, have chosen to ignore the existence of any other ancient materials on this history than are in every child's hands. They hold that the priestly party deputed some subtle Pharisee to test and expose the new Prophet's claims, and the contests to which he drew him, first in the Jordan wilderness, and afterwards at the capital, are what the forty days' temptation denote. Now, according to the legend translated for us by Luther and Wagenseil, and which was certainly current among the Jews of the fifth century,* as soon as news of the movement excited by the Galilean wonder-worker reached the Jerusalem Sanhedrin, they consulted, and agreed that the miracles proved their author to have obtained access by magical arts to the adytum of the temple, and stolen from thence the knowledge of the right utterance of the mightiest divine name, which was well-known to be recorded on a certain stone, and the pronouncer of which might effect any prodigy whatever. Hence the precautions taken always for its concealment, since David had found this stone† at his first preparing the foundations; and there was now danger of the whole nation, or even world, being perverted, by the abuse

* I follow Wagenseil's translation in vol. ii. of his "Tela Ignea Satanæ," Altdorf: 1681. That this document has undergone no essential change since the fifth century appears from its ending with the story of Simeon Stylites, whom it confounds with Simon Peter, and regards as the highest authority then among Christians. But the reverence of Christendom for this fakeer could not continue such as to convey this notion, even to outsiders like the Jews, much longer than poor Simeon's own life, which ended in 459. This legend must rank then, for date, with our gospels, no MS. of which can be traced with certainty above that century.

† The stone, it is insisted, was untouched by tools, yet having "insculptum nomen Dei ineffabile." The tradition evidently grew from certain prophetic texts, especially Zech., iii., 9; having some relation also to Isa., xxviii., 16, and Ps., cxviii., 22, the two verses most quoted (ten times) in the New Testament. The idea of healings and other wonders to be wrought by the utterance of a name or spell, was also plainly prevalent in New Testament times:—Acts, iii., 16; iv., 10, 29; v., 40; and, especially, xix., 13.

of this tremendous spell. The only chance of remedy for the awful calamity, they agreed, was if some orthodox rabbi could, by similar trespass, obtain the same power, and then display it before the increasing multitudes now following the Nazarene, to convince them that such marvels were no credentials for the divinity he claimed. At this point, Judas, the hero of these legends, and whom they place in a widely different social rank from what the evangelists seem to imply, for he is himself a member of the Sanhedrin, comes forward. He is an expert magician (or what would now be called a highly-developed "physical medium"), and he would undertake this patriotic task, were it not for the sin involved. They immediately all offer to relieve him of the sin; for the idea pervades these Hebrew tales, that any guilt whatever can be transferred to a voluntary substitute. "Upon us be all the guilt!" cry the whole council, if Judas will but take the trouble, dangers, and glory of the enterprise. The means are then described by which he penetrates the holy-of-holies, and gets out again, with the magically guarded secret. He proceeds to the Galilean crowd, and repeats all the miracles of their Master, decidedly more extravagant as here told than in any of the gospels; for each of the contending thaumaturgi invites the people to bring him cripples or invalids of any kind, and he will heal them; lepers, and he will cleanse; blind, and he will give them sight; or corpses, and he will revive them. And to try the last, they in each case open an unknown tomb, and bring nothing but dry bones; which have to be endowed first with flesh and then with life, as in Ezekiel's vision. The contest being a drawn one, the rivals now repair to Jerusalem, where, almost in the words of the fourth gospel, Jesus is made to say: "Nunc autem ascendam ad Patrem meum cælestem, et sedebo ad dextram ejus; idque oculis vestris intuebimini. Tu verò, Juda, illuc non pertinges. Continuò enuntiat Jesus Nomen immensum, venitque ventus et eum stitit inter cœlum et terram. Nihil moratus Juda et ipse Nomen eloquitur, atque hunc quoque ventus inter cœlum et terram suspendit. Sicque ambo per aëreas plagas circumvolitabant. Stupebant ista summoperè omnes intuentes. Juda autem, prolato rursùs Nomine, Jesum invadit, in terram illum deturbaturus: atque sic invicem colluctabantur." Now have we not here, and in the gospel accounts of the temptation on the summit (το πτερυγιον) of the Temple, a degree of coincidence in two opposite parties' traditions, inexplicable unless both recorded a real event? In each, a preeminent adversary provokes a contest with

Jesus, first near the upper Jordan (where both make His movement to have commenced) and afterward at Jerusalem. In each, this results in levitations and aërial journeys of both together, and they are simultaneously seen over the Temple. In each, the adversary's aim is a precipitation of Jesus from a great height into the crowds of the holy city. In each, he fails ;* and we may add that, in the only account naming and identifying him, he is the same man who, in the speech ascribed to Peter (Acts, i., 18) is stated to have incurred, three years later, the exact fate he here plans for his victim ; and, further, that he is the only man to whom is anywhere appropriated the term Διαβολος, which, in narratives, is peculiar to the opponent who " taketh " and " setteth " Jesus in these strange positions. Surely, on the whole, we must regard this devil as a man—the tool employed (doubtless by the great real Satan) to play a similar part towards this " last man-Adam " to that borne toward his and our greatest ancestor, the Messiah of Eden, by the villain "*Ha-nachash*," remembered only by the name his punishment earned ; " the crawler," doomed on his belly to go, and dust to eat with all his food, all the remaining days of that life. And as the other application made of this term Διαβολος is to that father of Pharisees and " murderer from the beginning," may we not conclude that Jesus, whose view of resurrection was so plainly Pythagorean, implied these two to be incarnations of the very same " wicked one ;" and probably Doeg, against whom the

* The above story, whose end is untranslatable, makes this, like all the other miracle contests, a drawn one ; neither magician displaying any advantage, like Aaron's, over the other ; and any moral aspect, such as the gospels give to the struggle, being absent, as there is no moral element in these legends. In the ancient "Toldoth Jeshu" quoted, he merely works wonders, chiefly of healing, and maintains that he is not of illegitimate birth, as the Sanhedrin insist, but is the Creator of heaven and earth, " Deus, et quidem Filius Dei," and requires all to obey him as such ; the crowd acknowledging him purely on the strength of his miracles. Hence a reader is more reminded of the fourth gospel than of the others, wherein such claims are subordinate to moral parables and precepts. In this document, however, his conduct has no more spot than in any of them,—Mr. Voysey would say it has less. It is far otherwise with the mediæval "Toldoth Jeshu Natzri," published by Huldrich, in which he is made atrocious, and murders his father Joseph. Both documents agree with the gospels, against present " criticism," in placing his birth at Bethlehem. They also startlingly dissolve some of the seemingly most incompatible differences in Matthew and Luke. Thus, they both make Mary a Bethlehemite, but, one of them, Joseph a Nazarene, which would most naturally reconcile the annunciation stories, if Mary was at that time on a visit to her future husband's town. Again, if Judas (whom they agree with the fourth gospel in making a spy deputed by the rulers from the first, always feigning discipleship, merely to effect the capture intended throughout) was, as they both make him, a member of the Sanhedrin, the purchase of the field Aceldama might be indifferently called his act or that body's.

maledictory Psalms were uttered, an intermediate birth of the same? And if so, may not the natural wishes for such a wretched soul's amendment, expressed by many before Burns, have long ago received some realisation, at least if Matthew's story of his remorse and suicide be held probable; and an amelioration have commenced, if not earlier, at any rate with those results of his three years' companionship with Him who is declared to be "the Saviour of all men, especially of them that believe?"

The Hebrew accounts also present, immediately after this, an equally close coincidence with the Christian ones on the next most public display of miracles, and the only ones, before his death, related so as to involve levitation. Those of one day and night are thus concentrated. Returning to the Jordan, they say, he caused two millstones to float, and standing or sitting thereon, he caught fish, and distributed to his crowd of followers on the banks. The later "Toldoth" has but one millstone, and "the sea" instead of the river. Now the evangelists all place the feeding of the five thousand with fish, on a bank overlooking the Lake of Galilee; and the miraculous walking, on the following night, upon the same lake; which is a mere expansion of the Jordan, but is most frequently called by them "the sea." Moreover, to avoid the crowd's taking him "by force, to make him a king," they tell us, immediately before one miraculous fishing (Luke, v., 3) he addressed from one of two boats the multitude on the shore. The "millstone" has evidently come from a figure used in his own prediction of the remorse of his betrayer; and thus there is not one feature or word of this grotesque medley, not traceable to a parallel in the most prominent incidents of the gospels.

Soon after this, and the Baptist's death, we read of His holding a very private meeting, with only the three most developed of his eleven chosen disciples, and while he was "transfigured" before them (a phenomenon lately paralleled, we are told, by those who observe modern media), two human forms conversed with him, "which were Moses and Elias." In the similar accounts now given us, of the re-embodiments at New York or in London, a slight atmospheric change causes the forms to announce that they must disappear, and one has complained of the "density of the air" making her embodiment difficult. Now Jesus chose for the above transfiguration "a high mountain." Has any reason for this choice been ever suggested; or for his habit of ascending mountains to pray?

For a few weeks after his death, his own form was again repeatedly materialised; most notably, according to Matthew, on "a mountain" (which may probably be the occasion when Paul says five hundred were present) but otherwise, as we read, in no wider circle than "the eleven;" exactly as the visitors to the Eddy family in Vermont now describe their (the visitor's) dead and buried relatives to reappear and converse. Like these He invited handling, and pointed out that "a spirit hath not flesh and bones as ye perceive me to have;" like these He ate and drank; and like these vanished. Two of these memorable visits were terminated by visible ascensions into heaven*—one from Bethany, in the evening of the first Easter Day (Luke, xxiv., 50), and another from Mount Olivet some weeks later (Acts, i., 9-12). Disappearances of such tangible forms by ascension, though within doors, are recently testified by Mr. Robert Dale Owen and others.

As the gospels represent spirit possession to have been a most prevalent affliction at that time, so do they indicate the levitation of the possessed; as in English and American witch trials, two centuries ago and later.—Mark, ix., 17-26 —A demon that "whithersoever he *took*" (καταλαβη) his victim, tore and rent him, often also "*cast* him into the fire or into the water." Luke, ix., 39—"Lo, a spirit *taketh* him;" 42, "The demon *threw* him down." The phrases are as distinct from any used of a lunatic throwing *himself* down, or injuring himself (Mark, v., 5) as in the English witch levitations. The phenomenon was more associated with bad than good spirits, being only once related of Christ between his temptation and death; and only on Peter's request does he grant to him also to come unto him on the water. We never again read of it among the wonders attending any apostle; but one of their first seven deacons, Philip, seems to have been congenitally a psychic excelling them all, "for unclean spirits, crying with loud voice, came out of many that were possessed" (viii., 7), and, as an instance of hereditary mediumship, "the same man had four daughters, virgins, which did prophesy" (xxi. 8). He accordingly affords the last Scriptural case of an æthrobat; for after his baptism of the destined founder of African

* In an appendix to the "Boyle Lectures" for 1708, Whiston proved that the scriptural heaven of angels, and of the just, can be no other than the upper strata of our atmosphere; and that such is the abode the writers assign to Christ ever since his first posthumous appearance to Mary Magdalen, saying "Touch me not, for I have not yet ascended to my Father." As he appeared at least twice more during that day, and allowed himself to be touched, we must infer that he then *had* ascended; and, in short, ascended as often as he vanished.

Christianity (viii., 33), "the spirit of the Lord caught away Philip, that the eunuch saw him no more, and he went on his way rejoicing: but Philip *was found* at Azotus;" this phrase, instead of "found himself," seeming to imply that he alighted among friends, as in most other recorded cases of the kind.

Whether we read Christian, Jewish, or Pagan accounts, the first Christian century abounds in thaums beyond any other. False Christs were to arise, and to "show great signs and wonders." The most typical instance of these doubtless was Philip's original rival, Simon Magus, the mere beginning of whose career the Acts do but touch. For a whole generation he travelled and proclaimed himself both the Hebrew Messiah, and an incarnation of each people's chief deity; basing all his claims on a series of prodigies which no contemporary, friend or foe, seems to have ever denied. In the "Recognitions," a work soon after ascribed to Clement, and certainly current in the next century, his translations through the air figure among these; and another Clementine (or, as now held, pseudo-Clementine) book, the "Apostolic Canons or Constitutions," contains the professedly earliest account of his end at Rome, by a public display of this faculty, in defiance of one or two Christian apostles; at whose prayer that he might fall—but not fatally—he is related to have so fallen as to break both legs, and then, from shame, to have committed suicide. If one of his opponents was Paul, and the other unnamed, nothing was more natural than for a dramatic instinct to fix on his first rebuker, Peter, as having thus re-encountered him; and this may have originated the whole momentous legend that brings Peter to Rome, the first traces of which appear in the Patristic repetitions of this adventure.

An equally attested æthrobat of that century, whose long life was held indeed nearly to fill it, was Apollonius of Tyana, the most famous and closest of all imitators of Pythagoras. His life, by Philostratus, a work of some bulk, and written, Dr. Newman says, with elegance, has the rare advantages of being certainly drawn up within a century of his death, and from all the materials that a literary empress, the wife of Severus, could collect;—the philosopher's own writings, a diary of his favourite and constant companion, Damis, memoirs by his chief earlier acquaintance, and the archives of the numerous cities that had received and honoured him. A century later, this book was made the basis of an attack on Christianity, answered by Eusebius, and now lost; but there is no evidence of Philostratus himself having written with any view, as Dr. Newman says, "of rivalling" the Christian marvels. None of his translators (including Berwick, a clergyman) have believed they detected any such aim, and it seems clear that this courtly professional bookmaker could have seen no documents of the despised sect, or some trace of allusion would be found. All his marvels imitate, on the contrary, tales current of Pythagoras; and most are either childish, objectless, or such as elude any real test—witch-finding, communicating by whispers with birds and animals; when imprisoned with Damis, drawing his leg out of the fetter, and then putting it in again, &c. But there are two that Dr. Newman thinks resemble Scripture miracles in forcing themselves into the history "as a component part of the narrative"—the first being the alleged cause of his acquittal when on trial before Nero ,for the crime the latter had invented, of philosophising in Rome. His accuser, Tigellinus, coming to unroll the bill of indictment, found only a blank paper (which may have been a miracle or may not). The other is the latest and most detailed point of his whole public career. He surrendered similarly before Domitian, who had revived the edict banishing philosophers (among whom the apostle John seems to have been reckoned) not only from Rome, but from the Continent. The trial attracted great notice, the grandest tribunal being used, and decorated as for a festival; but it ended sooner than was expected, by the emperor acquitting him, only adding that he must be detained for a private interview that he desired after the day's business. The aged prisoner, with thanks, briefly declined the honour, unless the emperor could detain both his soul and body. The former no human power could; no, nor, unless the gods willed it, even his body. He added a line of Homer, wherein Apollo says, "you cannot put me to death, for I am not liable thereto;" and on these words, vanished from the court; on the same afternoon as suddenly surprising his friends Damis and Demetrius, while talking of him in a grotto at Puteoli. One other such levitation occurs many years earlier, when at Smyrna he was crowded by sick persons, and by deputations inviting him to various cities. The Ephesians sent begging him to stay a pestilence; whereon, thinks his biographer, he designed to imitate his great master's passage from Italy into Sicily, for on replying "Yes; let us go at once," immediately he was at Ephesus. Dr. Newman, who does not mention this prodigy, thinks its sequel, the staying of the plague, "the best authenticated of his professed miracles, being attested by the erecting of a statue to him in consequence." It was also a count in his indictment for magic before Domitian. The second and last levitation, from Rome to Puteoli, and its effects in court, are described with much apparent truth to nature, but Philostratus goes into the supposed reasons, and a panegyric of his hero's wisdom, shown in the manner and timing of the marvel. Now he has to embody, as the chief piece extant from the hand of Apollonius, a very elaborate defence he had prepared for this occasion, and intended to have spoken by the waterclock; but not a word of which he really delivered, except the above line of Homer, wherewith it was to have ended. This glaring inconsistency strikes of course all commentators, and poor Philostratus, at his wit's end, seemingly despaired of glossing it over. It is created, however, solely by his gratuitous assumption that such thaums are the work of their subject. By all parallel accounts, from the case of Elijah to that of Mr. Home, they are so independent of his will, that we can no more suppose Apollonius to have known he was to be caught away, than do psychics of the present day. The inconsistency, then, disappears—nay, comes to tell strongly in the story's favour. On the whole, is it not better attested than any other marvellous one of its age? And will any Christian consider it less called for, less opportune, or less worthily ascribable to the Supreme Providence than most of those in his Testament? Can any say it would have occurred to better purpose at a trial of St. Paul or St. John? Supposing the account accurate, whatever intelligence directed the event so timed it as to force on a vast assembly the dogma that a man is no more liable to extinction of being than Homer's Apollo. And this in a world's capital where, for well-nigh two centuries, all belief in an after-life had, among the most educated, been extinct. In the senate of Cæsar's and Cicero's time, it was treated as a dream beneath serious notice. What could Christianity or any of its teachers do without this basis, which they rather assumed in their hearers than professed to prove? Paul could but appeal to his own veracity and apparent interests:—"If the dead rise not," we are false witnesses, or we are "of all men most miserable." A Sadducee or a modern Comtist would reply, "So you are." And in a society where such negation was held as a "positivism," and no facts overthrew it, what change could aught that we find in the New Testament have effected?

Iamblichus, in the next century (De Myst., Lib. III., c. 5), declared that one of the marks of obsession by spirits was, for the body "to appear elongated or thicker, or be borne aloft in the air."

In the century of the Church's triumph, at least one Christian and one heathen case of levitated persons are recorded. Sozomen relates after Hilarion, the founder of monachism in Palestine, that as four of his monks, whom he names, were returning to their convent of Bethelea, in the desert of Gaza, the youngest, but most esteemed, one Malchio, who soon afterwards left this life, suddenly vanished from their midst, and later in the journey reappeared ("Eccl. Hist.," Lib. VI., c. 32). The other case is an Egyptian prostitute, who came to Zosimas, an abbot, to beg his prayers and instruction in Christianity. As she was kneeling at his feet, he told her to turn and pray for herself and others. This he described her doing like Hannah, silently moving her lips:—"Juravit autem, sermonis sui testem appellans Deum, quod animadvertens longiùs protrahi orationem, oculos aliquantum à terrâ sustulit, viditque ipsam orare in altum sublatam, et in aëre suspensam, velut ad cubitum unum; quod cùm vidit, majori correptus metu, multùmque anxius, et omninò nihil proloqui audens, solùm intra se dicebat identidem, Domine miserere. Sic autem in terrâ jacens, scandalizari cœpit senex cogitando, ne fortè spiritus esset atque orationem simularet." Plainly, in the days of the British Solomon and the *Novum Organon*, this poor woman would, on any British ground, have made acquaintance with the halter or the stake. But Zosimas, after due probation, baptised her; and after the life of an exemplary nun, she became revered to this day as St. Mary Ægyptiaca; though nothing approaching miracle seems to have been ascribed to her as a Christian, or after this first interview ("Acta Sanctorum Aprilis," Vol. I., p. 79). Ecclesiastical miracles in general follow a distribution quite opposite to that of these phenomena. The darker and less historical the age, the more miracles, but the fewer of these phenomena. The testimonies to these, absent so far as we see in the ages from the fourth century to the ninth, increase in number, respectability, and accuracy, from the latter to the present day. Till the last two centuries, indeed, all persons known in Christendom to be subjects of levitation were probably either burnt or canonised, according to the ruling clerical view of their orthodoxy or the reverse. The following is an attempt to collect some of the chief examples not condemned, with the volume and page of the Bollandists' "Acta" where particulars may be found:—

Forty Levitated Persons, Canonised or Beatified.

Name, Country, and Condition.	Date of Life.	Acta Sanct.	Vol.	Pages.
Andrew Salus, Scythian Slave ..	880— 946	May	VI.	16*
Luke of Soterium, Greek Monk ..	890— 946	Feb.	II.	85
Stephen I., King of Hungary ..	978—1038	Sept.	I.	541
Ladislaus I., Ditto (his grandson)	1041—1096	June	V.	318
Christina, Flemish Nun	1150—1220	July	V.	656
St. Dominic, Italian Preacher	1170—1221	Aug.	I.	405, 573
Lutgard, Belgian Nun	1182—1246	June	III.	238
Agnes of Bohemia, Princess ..	1205—1281	March	I.	522
Humiliana of Florence, Widow ..	1219—1246	May	IV.	396
Jutta, Prussian Widow Hermit ..	1215—1264	May	VII.	606
St. Bonaventure, Italian Cardinal	1221—1274	July	III.	827
St. Thomas Aquinas, Italian Friar	1227—1274	March	I.	670-1
Ambrose Sansedonius, Itln. Priest	1220—1287	March	III.	192
Peter Armengol, Spanish Priest..	1238—1304	Sept.	I.	334
St. Albert, Sicilian Priest	1240—1306	Aug.	II.	236
Princess Margaret of Hungary ..	1242—1270	Jan.	II.	904
Robert of Salentum, Italian Abbot	1273—1341	July	IV.	503
Agnes of Mt. Politian, Itln. Abbess	1274—1317	April	II.	794
Bartholus of Vado, Italian Hermit	1300	June	II.	1007
Princess Elizabeth of Hungary ..	1297—1338	May	II.	126
Catharine Columbina, Sp. Abbess	1387	July	VII.	352
St. Vincent Ferrer, Sp. Missionary	1359—1419	April	I.	497
Coleta of Ghent, Flemish Abbess	1381—1447	March	I.	559, 576
Jeremy of Panormo, Sicilian Friar	1381—1452	March	I.	297
St. Antonine, Archbp. of Florence	1389—1459	May	I.	335
St. Francis of Paola, Missionary..	1440—1507	April	I.	117
Osanna of Mantua, Italian Nun..	1450—1505	June	III.	703, 705
Bartholomew of Anghiera, Friar..	1510	March	II.	665
Columba of Rieti, Italian Nun ..	1468—1501	May	V.	332*-4*, 360*
Thomas, Archbishop of Valencia..	1487—1555	Sept.	V.	832, 969
St. Ignatius Loyola, Sp. Soldier	1491—1556	July	VII.	432
Peter of Alcantara, Spanish Friar	1499—1562	Oct.	VIII.	672, 673, 678
St. Philip Neri, Italian Friar ..	1515—1595	May	VI.	590
Salvator de Hortâ, Spanish Friar	1520—1567	March	II.	679-80
St. Luis Bertrand, Sp. Missionary	1526—1581	Oct.	V.	407, 483
St. Theresa, Spanish Abbess ..	1515—1582	Oct.	VII.	399
John à Cruce, Spanish Priest ..	1542—1591	Oct.	VII.	239
J. B. Piscator, Roman Professor..	1586	June	IV.	976
Joseph of Cupertino, Italian Friar	1603—1663	Sept.	V.	1020-2
Bonaventure of Potenza, Itln. Friar	1651—1711	Oct.	XII.	154, 157-9

As the lives of all these are pretty fully recorded, we have the means of drawing several generalisations. It is plain that all displayed the qualities most distinctive of the present "spirit-mediums," and many were accompanied from childhood by some of the same phenomena, though I find nothing resembling the "raps." The hereditary nature of their gifts is shown by the Hungarian royal family producing five examples; and it is also notable, on this head, that out of forty there should not be one of British or French birth, although some of the most remarkable spent much of their lives in France, and all other Christian races seem represented. A feature absolutely common to the whole forty is great asceticism. Only four married, and all were in the habit of extreme fasting, "macerating" their bodies either with hair shirts or various irons under their clothes, and many of submitting to bloody flagellations. Again, all, without exception, were ghost-seers, or second-sighted; and all subject to trances, either with loss of consciousness only, or of motion and flexibility too, in which case they were often supposed dead; and the last in our list, after lying in state three days, and being barbarously mutilated by his worshippers, for relics, was unquestionably finally buried alive.* Many were levitated only in these unconscious states; others, as Joseph of Cupertino (the greatest æthrobat in all history), both in the trance and ordinary state, and (like Mr. Home) most frequently in the latter; while a very few, as Theresa, seem to have been always conscious when in the air. Several were, in certain states, fire-handlers, like Mr. Home. The Princess Margaret was so from the age of ten. Many had what was called the "gift of tongues," that is, were caused (doubtless in an obsessed state) to address audiences of whose language they were ignorant. Thus the Spaniard, Vincent Ferrer, is said to have learnt no language but his own, though he gathered great audiences in France, Germany, England, and Ireland. Connected with this, we should note how general a quality of these persons was eloquence. All the men (unless the two kings), and most of the women, were great preachers, though few wrote anything, except Bonaventure and Thomas in the thirteenth century, and Theresa in the sixteenth, who were the greatest Catholic writers of their ages. It is also very notable that the list contains the founders of six religious orders—the first special preaching order, Dominicans, the Jesuate Nuns, Minim Friars, Jesuits, Carmelite Nuns, and Oratorians; and all of these, except the second, great and durable.

The great majority of them, though often seen suspended, were at heights from the ground described only as "a palm" half a cubit, a cubit, and thence up to five or six cubits, or, in a few cases, ells. But the Princess Agnes and the Abbess Coleta were, like Elijah, carried out of sight, or into the clouds; and Peter of Alcantara and Joseph of Cupertino to the ceilings of lofty buildings. The times that these and others were watched off the ground often exceeded an hour; and the Archbishop of Valencia (1555) was suspended in a trance twelve hours, so that not only all the inmates of his

* This appalling story of insane superstition, to be paralleled probably among no non-Catholic people on earth, will be found in "Acta Sanctorum Octobris," Vol. XII., p. 158-60.

palace, and clergy, but "innumerable" lay citizens, went to see the marvel. On recovery, with the missal he had been reading in his hand, he merely remarked he had lost the place.* In this and all cases the subjects were either praying at the time, or speaking or listening to a particular religious topic that, in each case, is recorded to have generally affected that person either with trance or levitation. We have seen that Apollonius vanished on declaiming his favourite verse of Homer. So the topic of the Incarnation would cause Peter of Alcantara to utter a frightful cry, and shoot through the air "ut sclopeto emissus videretur;" that of Mary's birth would have a like effect on Joseph of Cupertino; and Theresa, after obtaining by prayer the cessation of her early levitations, was yet obliged to avoid hearing John à Cruce on the Trinity, finding that this topic would cause both him and her to be raised with their chairs from the floor. A contemporary painting of them in this position, beside the grating where it occurred, has been engraved in the volume above cited. Joseph of Cupertino, on entering any church having a Madonna or his patron, St. Francis, as an altarpiece, would be borne straight thereto, crying, "My dear mother!" or "My father!" and remain with his arms and robe so among the candles as to alarm all with the danger of his catching fire; but always flying back to the spot whence he had risen. Others were raised up to images or pictures, as the Abbess Agnes in early girlhood, often before a crucifix, "in tantum eam arripuit amor Sponsi sui, quòd relictâ terrâ tam altè fuit corpus suum purissimum sublevatum in aëre, quòd ipsi imagini, supra altare in eminenti loco positæ, se pari situ conjunxit; ubi osculans et amplexans, visa est super Dilectum suum innixa."

Of invisible transfers to a distance, the only subjects seem to have been Columba of Rieti, said to have been carried from her mother's house in that town to the nunnery that afterwards received her, at Spoleto, twenty miles distant; and the river transits of Peter of Alcantara. The lives of Joseph of Cupertino, indeed, allege that the rare miracle of "geminatio corporis," or bodily presence in two distant places the same day, was twice vouchsafed to him while dwelling at

* This prelate, the annual income of whose see was 18,000 ducats, had no sooner settled in his palace than he got rid of all luxurious furniture, and made it a hospital or poor-house; himself often sleeping on straw, if beds ran short for the paupers. Charles V. had named another person for this see, but the secretary to whom he was dictating mistook the name, and, taking another paper, said, "I imagined your Majesty to have said Thomas of Villanova, but the error will soon be rectified." The emperor said, "By no means: the mistake was providential; let it stand."

Rome—once to assist at the death-bed of a named old man of his native village, whom he had promised to attend if possible; and again at the death of his mother. It is also related of the great Spanish æthrobat that, while the business of a jubilee detained him at Madrid (1556-9), a lady, Elvira de Caravajal, in Estremadura, declared her resolve to have no other confessor till Father Peter might be within reach; and the same day he presented himself in her castle, announcing that he had been brought expressly from Madrid, and that she ought not to choose confessors so distant. There is doubtless plenty of exaggeration, and many stories of this kind must be apocryphal, but the notable fact is that they are told only of the same persons as the fully-attested levitations and other phenomena parallel to the modern so-called spiritism.

The river transits of Peter of Alcantara have all the testimony we could expect for events of a past century, in that of the witnesses examined at his canonisation. Coming, during a flood, to a ferry on the Tagus, called Alconete, where it receives the tributary Almonte, he could not persuade the ferryman to risk the passage; as night came on, and he was due and urgently expected on the other side, he prayed for some means of transit; when he suddenly found himself at the door of another ferry-house on that bank for which he was bound. Under perfectly similar circumstances, he was similarly helped at a ferry on the Douro called Buycillo. Again having to cross the Tagus, from Portezuelo, where he left a companion exhausted, to reach a convent that expected him, at Garrovillas, he could by no cries arouse the sleeping boatman, whose house was beyond the river. While praying, he found himself before a house unknown to him, with the river beyond it; and, seeing lights, he besought the inmates to tell him if he could anywise be put across, as he was expected at Garrovillas. They asked what the reverend Father meant: was he not just come from thence? No, he had come from Portezuelo. They assured him he must be dreaming: that nobody could come thence without crossing the river. The miracle was at length perceived, and thanks rendered. After this, we find him arriving with a companion, at a place where the swollen Guadiana had to be crossed, and the ferryman would not venture. The above experiences had given him faith to do what? Pray for a repetition of them? No; such a prayer is at no time ascribed to him, but conduct both more humble and more scriptural. Ordering his companion to tuck up his robe, and follow his footsteps, he leads on, like his namesake of old upon the lake of Galilee, or the two prophets, or Joshua's priests long before; and in the sight of the ferryman, and many others on each bank, both walk across, wetted "hardly above the feet,"* though all knew the river to be, when lowest, never fordable at this place, and it was now in a high flood.

As bearing on the *dark séance* question, it may be remarked that each of these four passages was after sunset, though none, like that in the gospels, after midnight. Again, Christians who may be inclined to suppose their Master's works of any kind necessarily unequalled, must be reminded that if so, they make him to have prophesied falsely in John, xiv., 12.

A singular feature alleged of Mr. Home, and of levitated inanimate objects, occasionally has been their rapid gyratory movement. Precisely the same is recorded of an early mediæval case, the Flemish nun, Christina, who was liable, when conversing on the Lord, to be caught from her sisters and spun like a top;† and the same occurred in some of the trances into which similar conversations would throw Peter of Alcantara. Instead of rising to the level of the tops of trees, or being shot into the church as from a bow, he was sometimes hurried with a speed "as that of the wind" (though old and infirm), and without touching the ground, through various passages, to either his cell or the church, where he would then remain for hours insensible. Once, on hearing the words *In principio erat Verbum*, &c., chanted, he thus rolled away like a wheel, but raised a cubit from the ground.‡

* The expression "dry shod" in the Old Testament, may very probably, even in the Hebrew, and still more so in the lost tongue whence Samuel translated it, have meant only with dry body garments.

† "Cum ipsis aliquando sedendo loqueretur de Christo, subito et inopinatè rapiebatur à spiritu, corpusque ejus velut trochum ludentum puerorum in vertiginem rotabatur, ita quod ex nimiâ vehementiâ vertiginis nulla in corpore ejus membrorum forma discerni posset."

‡ "Accensus spiritu genua et caput inflectens, implexusque tanquam rota volatu celerrimo ad portam conventus, indeque per alias portas sine ullâ læsione ferebatur, ultra cubitum à terrâ elevatus; usquedum coram SSS. Sacramento in ecclesiâ, in altissimâ, extasi raptus substitisset." It should be remarked that the grace of "maceration bestowed on this saint, and the physical toughness implied, were perhaps unequalled. His nightly sleep was less than two hours, with the forehead leaning against a wooden rest, his cell being, to prevent any posture but kneeling, planned two feet by four, and too low to admit of rising. For forty-six years he went in all weathers bareheaded and without sandals; and after washing his clothes, would wear them dripping wet. Before his daily flogging, he thought it necessary in winter to make the skin more sensitive by exposure to the frosty air. He ate usually but once in three days, and sometimes fasted for eight; and so admirable, said St. Theresa (who preserved these particulars), was the attenuation of his body and limbs, that they resembled a bundle of roots.

Another modern phenomenon often remarked is the rising of a group of bodies, animate or not, preserving their mutual contact and relations though loose. This was equally noted centuries ago, not only of Juan de la Cruz and Theresa with their chairs, but of all who were levitated in a kneeling posture, the loose robes adhering as if still pressed by the floor. It was especially so described in the case of Philip Neri, whose levitations, sometimes to three or four ells, were chiefly witnessed as he knelt at the rails of the great altar in St. Peter's (the present building, though then unfinished). Paintings constantly represent this phenomenon then with good reason.

Though in all the cases since 1500 the acts of canonisation record plenty of named witnesses who swore to these occurrences, none are so superabundantly attested as the innumerable flights of Joseph Desa, known, like most Italian ecclesiastics, only by the name of his native place, Copertino. Nor have the miracles of any other, that we know, effected anything so important as the Romanising of a Protestant sovereign. This friar was of very low birth, a tailor's son, and driven, apparently by harsh treatment from his mother, to seek the most menial positions in various convents from the age of seventeen. He was all his life liable to periods of extreme religious melancholy, and, when not in these, to trances and fits of various kinds. After monastic vows, he became a priest and preacher, and more and more gifted with miracles of healing, till, at the age of thirty-three, these brought him into trouble with the Inquisition, and he was cited first to Naples and thence to Rome, to clear himself from the imputation of starting some new heresy.* His orthodoxy, however, was established, and the general of his order introduced him to several cardinals, and to the Pope, Urban VIII., on approaching whom, to kiss his toe, Joseph's reverence to Christ, whose majesty he held the pontiff to represent, caused him not only to fall into a trance, but to be raised from the floor, and suspended, to the astonishment of all, until his superior ordered him otherwise. The Pope, highly astonished, observed that if Joseph died in his pontificate, he would himself be able to give testimony of this marvel. He was sent to the monastery of his order's founder, Francis of Assisi; and from thence the fame of similar wonders spread, till, in 1650, occurred their most important result. Prince John-Frederick of Brunswick, aged

* "Accusationis hæc summa fuit; Discurrere per eas provincias hominem triginta trium annorum, et hunc alterum Messiam totos vicos post se trahere cum prodigiis ad singulos passus, celebratis à plebe, quæ omnia credit."

25, who was heir to either that Protestant state or Hanover, whichever might first fall vacant, and who afterward succeeded to both, was now on an European tour: Having heard of the wonderful monk of Assisi, his curiosity led him from Rome to visit that place. With two counts, a Catholic and a Lutheran, he arrived there on a Saturday, begging one interview with Friar Joseph, and intending to depart the same day. The superior, who had been warned of the arrival, and well instructed how to act, lodged them, and prevented any sight of Joseph till the next day, when they were introduced by a secret door into the chapel where he, uninformed that any stranger was present, had to perform mass. As had been expected, an impressive part of the service overcame the speaker, he became unconscious, and, as frequently happened in these trances, rose and floated some time in the air. Questioned afterwards by the superior, but still unaware that strangers were listening, he could only tell that he had fainted; that before the swoon he had been trying in vain to break the holy wafer; that afterwards he broke it, but with difficulty; and that this preternatural hardness, he had no doubt, indicated some hard-hearted heretic to have been present, for whose conquest let them all pray. The prince's curiosity, growing with what he had seen, kept him there another day. On Monday, Joseph, while elevating the host, again swooned, and was seen to rise following it, and remained suspended with his knees and feet one palm (or by another account a foot) from the floor; while the clear face of the wafer, visible throughout so small a chapel, became marked with a cross of jet-black —in short, as clear a case of what is now called "direct spirit-drawing" as Mr. Dale Owen has ever described. The friar, in an insensible state, yet holding up the monstrance over his head, hung immovable in the air an eighth part of an hour. The Lutheran count said, "It was a cursed day that I came into Italy. At home I always enjoyed a quiet mind; but in this country, puzzles about faith and conscience keep pursuing me." The prince sought more interviews with Joseph, and before a third day had elapsed, he had solemnly promised to believe "all that the Catholic Church believes," and as soon as he could satisfactorily dispose matters in his states to that end, to return and be formally received; which accordingly he fulfilled in about a year, at the same place, on his knees, before two cardinals and Friar Joseph.

After this the friar had to be removed successively to more and more secluded mountain convents, to avoid the excessive crowding of people to Assisi, and wherever he performed mass, which at length he was forbidden to do publicly. His aërial flights, and more frequent trances, to which he had been subject from the age of eight, continued, the former to his last year, and the latter to his last day, in 1663, at Osima. Four biographers soon published memoirs of him, one at least during the life and with the approbation of Prince John's widow,* a Bavarian princess whom he married in 1668, after becoming Duke of Hanover.

With the unfortunate Bonaventure of Potenza, buried alive by his devotees in 1711, seems to have ended the series of saints (and such they undoubtedly were) in whom this phenomenon of levity was inseparable from their devotions; and about the same time we disposed of our last witch—Protestantism, though finding no saints to canonise, having, through its first two centuries, thousands of witches to burn. And this must never be left out of account as bearing on the present rarity of these phenomena. They are rarer than in any century before the last, because hereditary psychics are rarer; necessarily after so persistent an attempt at their extermination. The mother of the Eddy family is said to be great-granddaughter of one of those condemned in girlhood at Salem, in Massachusetts, but who broke prison. We may conceive the artificial clearing of our race from psychics to be as possible as was the clearing the vertebrate kingdom of the dodo; and if—as might have happened by a few Biblical words being slightly better translated—our ancestors, two centuries back, had killed fewer of them by 30,000, would there not naturally be now several times 30,000 more of them in the world than at present?

It should be noted, respecting the Prince of Brunswick, that whatever power or intelligence superintended those miracles, that power showed great ignorance of the future. At the time, indeed, no conversion could seem more important to the interests of Catholicism than that of this young German sovereign, but eventually it appeared that any other ruler would have had more permanent influence. In fact, all public effect of his pious dispositions absolutely ended with his life in twenty-nine years. Though he lived till

* "The biographers were Nutius, Agellus, Pastrovicchi, and Bernino. Late editions of the two latter are in the British Museum, and have frontispieces in each of which Joseph hovers in the air, in one before the top of a tall calvary; in the other borne forward fifty paces at sight of the church of Loretto. Pastrovicchi says, in page 87 of this edition:—"Attesta la Serenissima di Brunswik ancor viva mentre questo Libro scriviamo, qualmente in quella Corte a tempo del Duca Gio: Frederico suo Marito, *Non ci faceva oltro, che parlare del servo di Dio, Padre Fra Guiseppe da Copertino, al quale egli conservava una tenerissima divozione, e ne aveva l'Effigie,*" &c.

1679, and had four daughters, all his states of Brunswick, Hanover, Calemberg, and Grubenhagen, passed, for want of a son, to his Protestant brother, Ernest Augustus, father of George I. Plainly, then, if the power directing the Assisi miracles could have looked forward but a few years, not this heir-apparent, but his younger brother, also on his travels, and of similar education and disposition, would have been chosen. His conversion, though not then looking so important, would, in all present probability, have recovered to Rome by this time great part of Germany. What dynasty England might have chosen is of course impossible to guess; but his posterity would not, by being Catholics, have been excluded from any of the continental thrones they have actually filled.

The conclusion we draw is, that the very common notion of our having, or philosophers having, divided all describable events into the "naturally possible" and "naturally impossible," and assuming to have fixed this limit, can lead to nothing but priestcraft and superstition. Unless our calling things "impossible" could prevent their happening, it only gives them prestige whenever and wherever they may happen. Prince John of Brunswick was probably brought up to hold very nearly these most falsely-called "positivist" ideas—and we see the natural result. The more impossible or preternatural a Faraday or Comte can persuade us to consider any feat, the more helplessly will its occurrence hand us over to whatever body of men or other beings can at all manipulate that feat.

IV. THE BOUNDARY BETWEEN MAN AND THE LOWER ANIMALS.

TAKE any man of fair average understanding, whether educated or not; set him to arrange a crowd of things whatsoever, capable of classification; the probability is that, except specially trained in the study of the natural sciences, he will group them under two heads. These two classes he will take to be not alone mutually exclusive, but utterly antagonistic. He will assume them to be separated by a "hard and fast" landmark. We may tell him, if we think proper, that Nature—pardon the personification—does little *per saltum.* We may take his two classes, A and B, and show him how the one fades into the other, not by steps, but continuously and insensibly. We may point out that class B contains forms differing respectively from each other quite as much as some of them do from A. We may prove that facts would warrant a division into three, or ten, or twenty groups, as well as, or better than, into two. Or we may show that the classification is, in its principles, arbitrary, unnatural, and *ex parte hominis*, resting not on any difference in the matters to be arranged, but merely on the relation they bear to the convenience or prejudices of man, or of some particular section of men. All our explanations and objections will have little weight; man will have his two classes, and no more. This inbred tendency to dualistic arrangements, or to dichotomy, as it is called by some writers, is probably founded upon a bodily fact. Man has two hands. Place before a child a quantity of flowers, shells, or pebbles, and he will generally scrape them into two heaps, to the right and to the left. This mode of proceeding follows man from childhood to maturity, from savagery to civilisation, and from objects materially under his hands to those presented to his mind only. Had he been a three- or a four-handed animal, his philosophies and his creeds would have been greatly modified. Our binary or dualistic classifications, like our decimal arithmetic, have a morphological basis.

Instances of these binary divisions are plentiful, from the categories of Aristotles, or even from an earlier date, down to the present day. Every science, every branch of erudition, has its antithetical couples strung together, like braces of birds in a poulterer's shop. We speak of Ormuzd and Ahriman, of earth and heaven, of matter and spirit, of matter and force, of nature and man, of good and evil, of soul and body, of light and darkness, of heat and cold, of conductors and non-conductors, of metals and metalloids, of acids and bases, of combustibles and supporters of combustion, of organic and inorganic, of animals and vegetables, of man and beast.

The same love for dualism shows itself in the affairs of daily life, and in common conversation. Thus, the raw youth and—oddly enough—the statesman divide mankind into friends and enemies; our divines have their saints and sinners, their church and world, and other antitheses not a few; our moralists enlarge on vice and virtue; our political and social orators have much to say concerning order and progress, liberalism and conservatism, capital and labour. We speak of white men and men of colour, forgetting that

Celts, Teuton, and Slav are as remote from each other respectively as any of them are from the swarthy Aryan of India, and that the latter approaches no nearer to the negro than do our worshipful and self-worshipping selves. We are prone to contrast Englishmen with foreigners. We do not, indeed, conceive that the latter class is perfectly homogeneous: we admit that there is some little difference between a Frenchman or a German on the one hand and an Ashantee or a Papuan on the other; still we hold that all foreigners differ so widely and fundamentally from Englishmen that such nice shades of variation may well be overlooked.

Here also belongs that tendency to "successive halving" upon which the English opponents of the metric system of weights and measures—such as Sir J. Herschel—depend as one of their chief arguments. They tell us that it is "an instinct of human nature" to divide everything into two portions—that the hundredweight is thus halved and halved again down to the $3\frac{1}{2}$ lb., and that the authorities have even been memorialised to sanction the use of a $1\frac{3}{4}$lb. weight. All this is a further illustration of man's natural proneness to dichotomy.

There is a singular feature in certain cases of binary arrangement, which may be most readily understood by a reference to the views prevailing in chemical science during the first quarter of the present century. Oxygen was, tacitly at least, singled out and placed in one class by itself, the remaining elementary bodies forming the second. It was the great element, as much superior to the rest as "man" is to "beast," as "Englishman" is to "foreigner"—we might almost say as "respectable Englishman" is to "pauper." Every newly-discovered substance was primarily, if not exclusively, studied with reference to oxygen. Wheresoever it entered it was supposed to play the active part, other substances present in the reaction being merely passive. Thus it was the "supporter"—the cause—of combustion, whilst the substances with which it united—compounds of carbon, hydrogen, and the like—were merely combustibles, bodies capable of suffering or undergoing the process.

This is a typical and an instructive case. In his binary arrangements man is very apt to take the one class in his right hand, if the phrase may be allowed, and to view it with especial interest and favour. Nay, sometimes this exceptional favour is the only base upon which such classifications rest.

We do not mean to lay down the law that every binary arrangement must needs be rejected. There may be cases where a division into two groups is quite natural and justifiable. Such groups may even be exclusive, and to some extent antithetical. Thus we may feel bound to refer all substantive entities to the two classes, "matter" and "spirit." We may neither be able to break down the boundary between these two groups, nor to point out a third co-existing group. Still, the possibility of matter and spirit being both phases of a something as yet hidden is gradually dawning upon some of us.

Looking, again, upon material objects, we may feel forced to regard them as either "organic" or "inorganic." The distinction between these two groups may grow less and less striking as our knowledge extends, but it does not entirely vanish. Still less does a third group make its appearance.

Again, the division of the higher forms of animal life into males and females—obnoxious as it is to the champions of the Woman's Rights Movement, and inconvenient as it proves to a certain class of world-betterers—can neither be abrogated nor explained away. There is, to be sure, a time in the life of hen pheasants, and other female gallinaceous birds, when they—in the magniloquent language of a weekly literary organ of epicœnes and garotters—"rise up and look their tyrant in the face," in the hope that, "ever after, he will sit uneasily on his" roost. In plainer words, hens who reach a "certain age" assume, to some extent, male plumage; attempt, not very successfully, to crow; and make themselves generally ridiculous, by attempting to be what they are not. Does such a change come over not merely individuals, but whole species, and is the human race approaching the time for this change? If so, there is an opening for some of "our poor relations." If differentiation means development, what conclusion must be drawn from the effacement of differences?

In spite, however, of the above-mentioned exceptions, and of a few others, real or apparent, I maintain that every case of binary classification is suspicious, as being probably *ex parte hominis*. The history of Science fully justifies this caution. The successive overthrow of such arrangements is certainly not the least striking feature in the career of modern discovery. This assertion I shall briefly endeavour to make good.

In the dark ages—or ages of faith, as they are called by men of the De Maistre school, who wish their revival—no antithesis was more common and more firmly established than that of "earth and heaven." The planets were not then known to be bodies composed of the same ultimate elements as our earth, governed by the same physical forces, and consisting, in the main, of a nucleus partly solid and partly liquid, surrounded by an atmosphere of gases and vapours. They were supposed to be semi-spiritual in their nature, formed of no one knew what, perfectly spherical in form, and free from such blemishes and imperfections as hills and valleys. The moon alone, being "lowest" and nearest to the earth, had in consequence—as generally happens to those who indulge in "questionable" company—contracted certain stains and impurities. Readers not conversant with the history of astronomy will find difficulty in believing that such was the interpretation put upon those spots in the moon which the telescope resolves into mountains, chasms, and ravines. As for our earth, it was regarded as a place utterly vile and "impure," the "sink of the universe,"—differing from the heavenly bodies in its nature, and contrasting with them in every respect. Those very features which conduce most to its beauty, and which alone render it habitable, were considered as proofs of its vast inferiority. These absurdities were recognised as philosophic truths; they were consecrated as articles of the true faith, and reigned for centuries unchallenged. Between earth and heaven there was a "great gulf." But the progress of astronomy showed finally that the earth and the remaining planets were sister orbs, between which no such antithetical distinction could be maintained. The "great gulf" was found to be merely the fog and darkness of ignorance, and, as it has been beautifully expressed, "earth was restored to her place in the heavens."

Turning from astronomy to chemistry, no antithesis in science was more firmly established a hundred years ago than that of acid and alkali, or, as we should now say, of acid and base. But in the progress of research it was found that one and the same body could fulfil either of these functions according to circumstances. Thus alumina in contact with a body more basic than itself—such as potassa or soda—plays the part of an acid; but if brought together with a substance of a more decidedly acid character—such as the sulphuric or nitric—it acts as a base. Thus the old absolute view of acid and alkali as two classes, mutually exclusive and essentially antithetical, has passed away, and the terms have acquired a mere relative significance. In this case the change of view was fortunately not retarded by any extraneous complications, such as social or theological prepossessions.

Very similar has been the case with the classification of the simple or elementary bodies. The science of the past had here, also, its dichotomous arrangement; metals and non-metallic bodies, or, as the French and Germans call them, metalloids. To apply the term metal*loid* to a body which is all the time proclaimed eminently *unlike* a metal is one of the curiosities of scientific nomenclature. To fence in these two classes with strict definitions, and to assign to every element its place in one or the other, were tasks on which learning and research were freely spent. And what is the result? That now there are several elements which form a debateable land between the two classes; that the love of paradox has caused one of the very bodies from which the idea of metal was originally obtained to be proclaimed non-metallic; and that the most truly philosophic chemists are beginning to regard the whole question as comparatively unimportant.

Turning to Physics, we may be reminded that "heat and cold" were once viewed as distinct and antithetical forces. The zero of our ordinary English thermometrical scale is a fossil remnant of this supposition. At 32 degrees below the freezing-point of water it was held that there was an absolute privation of heat, and that below this only greater or less degrees of cold could be experienced. Now every man of science, every person of decent education,—always excepting the author of "Trinology,"—conceives of cold merely as a degree of heat which is low in relation to his feelings, or to his general experience.

Let us look next at the antithesis "vegetable and animal," somewhile accepted without demur. We have still no difficulty in distinguishing the higher forms of "kingdom," as they are inaptly designated. We do not confound the eagle with the oak, or the donkey with the thistle. But on the confines there is here, again, a debateable land across which man has hitherto been unable—and finds himself from day to day less able—to erect one of those sharp, glaring, property-like fences in which his soul delights. An appeal was made to chemistry to find some absolute distinction between animals and plants. For a time the appeal seemed likely to be successful. We were told that animal matter contained nitrogen, which was wanting in vegetables. But soon nitrogenous bodies analogous to albumen and caseine were found in the seeds and in the juices of plants. It had been said that animal matter exposed to destructive distillation yielded ammonia, whilst by treating vegetable substances in a similar manner acetic acid was obtained; yet 99-100ths of the ammonia of commerce is derived from the destructive distillation of a vegetal matter,—namely, coal. Next certain substances were pronounced exclusively peculiar to the vegetable world—such as starch, alkaloids like quinine, and colours like indigo. One by one these, or their representatives, have been traced in the animal world. As a last resource the advocates of a chemical distinction selected protagon—a substance found in the nerves of animals. But alas! even this compound has been distinctly recognised in the grain of Indian corn. Again, it was asserted that plants were entirely dependent in temperature upon the surrounding medium, whilst animals, to a greater or less extent, possess a temperature of their own. This view is also abandoned. Several flowers are distinctly and measurably warmer than the surrounding atmosphere. Thus, according to Garreau, the flower of *Arum cordifolium* has been found to have the temperature of 121° F., whilst that of the air was merely 66° F. On the other hand, the milk of a freshly-gathered cocoa-nut is several degrees colder than the atmosphere.

There are, of course, certain anatomical features upon which those who worship their own ignorance—under the imposing title of "common sense"—may rely as furnishing a criterion. Bones and nerves cannot be distinguished in plants; but neither can they be traced in all forms of animal life. Thus the old antithetical distinction between plant and animal becomes less tenable, less striking, the more closely it is examined.

So we might proceed with a large majority of the contrasted couples, either of science or of common life. We should find, almost invariably, that where a dualistic arrangement has been adopted, that it is either radically false in principle, that the two classes are not mutually exclusive, or that the presumed antagonistic groups are merely the extreme members of one connected series. We should find more and more evidence that Nature repudiates our pigeon-boxes, and that her groups are referrible not to any outside boundary or definition, but to an internal type.

But whilst Science is successively repudiating these antithetical couples of the past, and regarding them as mere "alms for oblivion," one case of dualism remains almost untouched. We still speak of "man and beast" as did our forefathers in the eleventh century. Man deems himself, not the first and highest member of a series—or group of series—including all forms of animal life, but a creature removed from them far more widely than is the ape from the coral-worm. He differs from them, according to his own view, not in degree but in kind. Swainson excludes him altogether from the zoological circle, and views him as the "aberrant" form of the spiritual world, having with animals relations not of affinity, but merely of analogy. Every attempt to point out his true place in the scale of being is rejected as insulting, cynical, even blasphemous. Curiously enough those who intellectually and morally approach nearest the apes, and in some respects fall below them, are most exasperated at being approximated to "mere brutes." Even some of the advocates of what is conventionally—though illogically—known as "Darwinism" stop short in their train of reasoning when they have reached the anthropoid apes, and discover grounds for not pushing the enquiry further. Whether their grounds for thus advising us to "rest and be thankful" are scientific or sentimental we shall not now examine. Those, however, who value consistency will find that man is at any rate much nearer to the brutes than his self-love is willing to own, and that the points upon which he relies to establish a "great gulf" between himself and the rest of the animal world—if not baseless assumptions—are, at the best, sadly inadequate.

Does language constitute the boundary line? So says a large amount of vague popular opinion, represented by such phrases as "dumb animals." So say also enquirers not a few, among whom a prominent place belongs to Professor Max Müller. Of a different opinion is Quatrefages.* We will therefore examine what element of truth is in this view. In how far can man claim the exclusive privilege of the "spoken word," commonly deemed co-extensive with reason? Have animals no means—whether by sounds or gestures—of communicating their notions and their sentiments to other individuals of their own species, or of nearly allied groups? Have the varied sounds they utter no meaning beyond the expenditure of a certain amount of superfluous vital energy? Domestic animals may possibly have learned from man the bad habit of not keeping silence when they have nothing to say. But a little observation will convince us that the sounds made by birds and beasts are regularly repeated under certain recurring circumstances, and are evidently understood and acted upon by others of the same species. This may readily be noted in the case of domestic fowls, whose vocabulary is rather extensive, and may to some extent be understood. It may be objected that

* Unity of the Human Species. The unity, by the way, of the "human," or of any "species," will doubtless be conceded, subject always to the question—What is included in such species?

these sounds consist chiefly of danger-signals, calls to food, or calls relative to sexual and parental instincts, and that—few in number, vague and indefinite in character—they make but a very faint approach to the character of a language. This may be here the case; but we have positive evidence that brutes are capable of connected thought—of trains of reasoning: this is generally allowed to involve, of necessity, the use of language, or of symbols of some kind answering the same purpose. Man, at all events, cannot think or reflect on any subject without the internal use of words. Coleridge, indeed, was of opinion that if language had never arisen, man might have come upon some method of conducting the reasoning process without its aid. But this view meets with little acceptance. This intimate connection of speech and reason is acknowledged in the idioms of several languages; but that language depends solely upon sound, and must be addressed to the ear alone, is a false assumption. The senses of sight and touch may each be the medium through which symbols of determinate meaning can be laid before the mind of an intelligent being. The language of ants, conveyed by varied touches with the antennæ, has been observed by many naturalists,—we need only refer to Mr. Belt,—and is clearly adequate to the communication and transmission not of mere danger-signals and calls to food, but of precise and definite information concerning duties to be performed. A messenger ant can send a number of his companions to some precise spot, and can inform them beforehand what task they are to undertake, just as well as a human messenger in a manufactory or an aide-de-camp on a battle-field. Nor is the ant who has brought intelligence obliged to go first to the place where help is wanted; those whom he has first touched rush off in the proper direction, whilst he goes on collecting more forces. The case, then, stands thus:—Man is no longer able to deny that brutes think. If he will not admit that they think in words, or by means of equivalent symbols, he is, I submit, bound to show that they think in some different manner, or by the aid of some other instrument.

There are on record authenticated instances proving that animals are capable of conveying or transmitting to each other, not mere general and vague intimations of good or evil, but pieces of distinct and specific information. Turning to beings which—morphologically at least—approach man much more nearly than the ants to which I have been referring, we find the following:—" A little Blenheim spaniel of hers once accompanied her to the house of a relative, where it was taken to the kitchen to be fed, on which occasion two large favourite cats flew at it several times, and scratched it severely. The spaniel was in the habit of following its mistress in her walks in the garden, and by degrees it formed a friendship with a young cat of the gardener's, which it tempted into the house,—first into the hall, and then into the kitchen,—where, on finding one of the large cats, the spaniel and its ally fell on it together, and beat it well. They then waited for the other, which they served in the same manner, and finally drove them both from the kitchen. The two friends continued afterwards to eat off the same plate as long as the spaniel remained with its mistress in the house."* How, without some kind of language capable of entering into special details did the spaniel persuade its friend to enter the house, and join in hostilities against two beings of its own species? Yet there are on record cases, not a few, of dogs and of cats which have brought allies from a distance to aid them in revenging an injury. To give all the well-authenticated and current instances where one animal has thus been known to convey to another some definite piece of information would be foreign to my plan, but there are many works on Natural History that will fully supply the deficiency.

I have next to call attention to the fact that domestic animals frequently understand what is said by man. I do not refer to words of command to which they have been trained, nor to other cases where the tone of the voice and the accompanying looks and gestures might reasonably be expected to throw some light upon the meaning of the speaker. Take the following instance:—A woman was often annoyed by the depredations which the poultry, and especially a certain young cock, committed in the garden. One day, after driving out the poultry, she said, in the heat of the moment, 'I wish that cock were dead!' A little favourite dog, which had been present as she spoke, ran out, and shortly afterwards returned, dragging in, to her surprise and horror, the lifeless body of the offending cock." We may grant that the dog had seen its mistress drive out the poultry, and knew thus that they were the object of her anger; but how, except from understanding her words, could he learn that the cock was, in her opinion, the main offender, and that she desired his death? Take another case:—"The Rev. James Simpson, of Liberton, had a very intelligent dog. He remarked one day to a friend, in its

* JENYN's Observations on Natural History, p. 71.

hearing, that he should be compelled to get rid of it, being about to change his residence. The dog forthwith disappeared, and never returned."*

Further, instances in abundance could be brought to show that a dog very readily discovers whether his master is on a hostile footing towards any persons he meets, even where no threatening gestures or loud speaking may have occurred. If you talk about a cat in her hearing she will generally put on an air of affected unconcern, or rather unconsciousness, similar to that she assumes if a scrap is thrown to her when not very hungry. Now it is inconceivable that a being totally devoid of a language, and therefore unaccustomed and unqualified to receive communications through any such channel, could understand the language of man.

But if domestic animals have languages of their own, and do, to some extent, understand that of man, how is it that he is so little able to understand theirs? Perhaps his motives are much less urgent. Place two beings, A and B, in close daily contact, and give A unchallenged and unlimited power over B. It will then always be found that B will have a far clearer insight into A's principles, passions, prejudices, foibles, and character in all respects, than A, in turn, has into those of B. In the old times of negro slavery —say in Jamaica or Carolina—how little did "massa" really know of Quashie and Sambo, and how very much, on the other hand, they knew of him! Paradoxical as it may seem I feel compelled to assert that, within a certain sphere, the lower mind sees into—not comprehends, that is another matter—the higher mind better than the higher can see into the lower. The planet sees the sun, whilst the sun is all unconscious of the planet. Place yourself on the summit of a hill, and a spectator down amidst the fields or houses of the plain may watch your every movement, and yet escape your notice, despite—or perhaps rather because of—the wide extent of view you enjoy. Of course only minds similar in nature and equal in power can fully appreciate each other. The fool is unable to comprehend the aims and the motives of the genius, but he sees into the character of the latter, detects any apparent shortcomings, and draws his private advantage therefrom. "The children of this world"—*i.e.*, the commonplace, routine characters—"are wiser in their generation than the children of light."

Hence—if a brief digression be allowable—the little soul has, in dealing with his fellow men, a decided advantage, as

* Shirley Hibberd, Clever Dogs, &c.

shown in the following incident, which is no fable or apologue, but the literal record of a fact:—A certain man in the North of England had two sons, not palpably idiots, but what are locally known as "softies." One of these was suddenly missing. He was traced to a wood near the town, but there the clue was lost, and all search proved vain. In the afternoon the other "softy" begged that he might go in quest of his lost brother. The proposal was accepted, and the boy—followed at some little distance by his father—entered the wood. He sauntered carelessly up and down, with his eyes fixed on the ground, constantly shouting—"Aw see thee, Johnnie!" Suddenly a voice from the midst of a dense thicket replied—"Nay, thou duzzant." What wise man could thus understand the workings of a fool's mind? The real "*Encomium Moriæ*" has yet to be written. These considerations will go far to explain the frequent failure of men of genius to secure the ordinary material prizes of life, —a failure by no means invariably due, as sometimes asserted, to extravagance, indolence, or intemperance. This opinion will certainly be voted flat blasphemy against that god of the modern world, the "self-made man." His worshippers, literary or illiterate, may not relish the imputation that the peculiar and profitable gift of their idol for over-reaching inventors, customers, workmen, and all sorts and conditions of people, is nothing divine, but merely a slavish—and even a currish—faculty.

To return:—What wonder that man fails to understand the lower animals, just as the master is unable to understand the slave, or the sane man an idiot?

It may, perhaps, be argued that domestic animals have been so profoundly modified by contact with man, that they may thus have to some extent acquired the gift of language. I reply that, making full allowance for such modification, these animals must either have possessed the rudiments of a faculty for language or not. If they had when in a state of nature even the slightest germ of such a faculty, then absence of language cannot serve to distinguish the lower animals from man. If they had no such germ, and if the faculty for language could be created by mere association with mankind, then assuredly it is a characteristic far too unimportant to serve as the basis of a classification.

It is not, however, among domestic animals that the faculty of language appears most fully developed. Save among mankind, it appears in the greatest perfection among the social birds and insects—species which unite not merely in families, but in tribes and communities. To the ants I have already referred. Among rooks, some slight, but unmistakable traces of law, and of formal procedure against criminals, have been observed. This surely is inconceivable in the absence of language. In Guayana a great number of monkeys have been observed assembling upon a huge tree. A large old male, perched near the summit, delivered a long chatter, after which the whole assembly burst into a tumultuous jabbering and screeching. When this ceased, the orator resumed his harangue, and was again after a time interrupted by the whole chorus. Ludicrous as this sounds to us, can we consider it as utterly unmeaning? On the other hand, can we conceive of any meaning except the assemblage had some faint outline of a language?

In many instances it would be impossible for animals to alter their habits in conformity with changed circumstances, unless they were able to communicate information to their contemporaries and to their posterity. There are many circumstances proving that the lower animals possess the power of tradition. Look, for instance, at the dread of man shown by birds and mammalia in peopled regions. This fear is not innate, as we learn from voyagers who have visited uninhabited islands,* such as the Gallapagos or the Falklands. In such lonely parts the feathered tribes fear a man no more than they do a goat or a sheep. But wherever he sets his foot, his habits of promiscuous and wanton destruction become known with wonderful speed, and he is soon regarded as dangerous, and shunned accordingly, long before every individual bird can have personally observed the effects of a fowling-piece. Here, then, is a new fact, observed, remembered, and circulated. But the vague dread with which birds regard man when first his real nature becomes known is not permanent. They observe him, and note how far and under what circumstances he is dangerous, and also when and where he is powerless. This knowledge, also, is communicated. A town sparrow knows exactly how near it is safe to let a boy approach before taking flight. Its conduct, which may be regarded as cautious impudence, is a due medium between the unsuspecting confidence of the birds of some untrodden isle and the panic terror of those in rural districts. These and similar facts, which the field naturalist and the sportsman will call to mind in quantity, prove that the lower animals can tell each other what they observe and remember.

Some objectors may urge that the languages of brutes, be they vocal or signal, are not "articulate," and consequently

* Darwin, Naturalist's Voyage, p. 398.

bear no resemblance to those of man. So long as they answer a corresponding end, I consider this distinction immaterial. But every language seems inarticulate to those to whom it is quite unintelligible. The English peasant hearing a conversation between two Frenchmen proclaims it mere chatter, such as a monkey might utter, and in which he can recognise nothing like words. The ancient Greek called all barbarians "tongueless." The modern Pole, Wend, or other Slavic man, terms his German neighbours "mute." If, then, men fail to recognise the articulate character of human languages which they do not understand, can we wonder if sounds, produced by vocal organs differing more or less from their own, seem to them mere aimless growling, shrieking, and chattering? I am far from supposing that the languages of brutes are remarkable for regularity, complication, and richness of expression. But how wide is the diversity in this respect among the human race! The Veddahs of Ceylon "communicate with each other by signs, grimaces, and guttural sounds which bear little or no relation to distinct words."* If we assume that the languages of the chimpanzee and the pongo are as much inferior to the tongue of the Veddahs as that is, in turn, to Greek or Sanskrit, need we wonder, if to human ears the ape-dialects seem utterly inarticulate?

Finally, there are animals really dumb—devoid of vocal organs, and therefore incapable of mutual communication by sound; devoid also of antennæ or corresponding parts requisite for a language of signs; solitary, and therefore needing no language. The gulf between such animals on the one side, and the vertebrata and articulata on the other, is incomparably wider than that existing between the latter and man. Man has a very complicated and highly-developed speech; the ape, the rook, and the ant, a comparatively rudimentary, but the oyster none. If, then, the animal world is to be divided according to the presence or absence of this faculty, the line will fall, not between man and the anthropoid apes, but between the vertebrates and articulates on the one hand, and the molluscs and still lower forms on the other. Hence all attempts to establish a "great gulf" between "man and beast" by means of language are altogether illusive.

* Sir E. TENNANT, vol. ii.

V. SCIENCE, HER CLAIMS, POSITION, AND DUTIES.*

PROBABLY no literary production of the day has occasioned so much excitement as the world-famed Address of Dr. Tyndall. Although bringing forward no new facts, no novel generalisations, it has—in virtue of the occasion, and of the high standing and official position of its author—called forth a deluge of criticism, as unique in quantity as (we would fain hope at least) in quality. In clubs and in taverns, in public conveyances and at tea-tables, in pulpits and on platforms, Dr. Tyndall has been the denounced of all denouncers. From Cardinal Cullen to Dr. Cumming, from Professor Blackie to Dean Cowie, clericals of all creeds, "friars white, black, and grey," are leagued against the bold speaker. Dr. Magee has treated the world to a "Tale told by a bishop, full of sound and fury." M. l'Abbé Moigno has scarcely room left in his redoubtable "*tete bretonne*" for the great pyramid, and, not to speak of stray controversial paragraphs, has reprinted the speech with Ultramontane annotations, and addressed to Dr. Tyndall a serious and impertinent letter of remonstrance. As for the lighter attacks,—the cavils penned by fast *litterateurs*, or uttered by the self-styled "respectable and intelligent classes,"—they are absolutely beyond enumeration. Nothing more is wanting in this candid and appreciative style of criticism save that the "Address" should be solemnly burned at Oxford, and formally condemned by his Holiness the Pope. If, as it has been suggested, one at least of Dr. Tyndall's motives was to learn, by actual experience, how such a speech would be received, his curiosity has been fully gratified. There can be no doubt that, had the power been equal to the wish, he would, before this date, have shared the fate of Protagoras and Anaxagoras and Hypatia, of Roger Bacon and Virgilius of Salzburg, of Giordano Bruno and Vanini, of Telesio and Campanella, and many more who by some strange accident are *not* mentioned in Sir D. Brewster's "Martyrs of Science."

If, as we hold, there is much in the "Address" on which issue may fairly be joined, much that is incapable of demonstration, and more, perhaps, that is inopportune, not the less do we feel bound to protest against the bulk of its critics.

* Address Delivered before the British Association assembled at Belfast, with Additions, by JOHN TYNDALL, F.R.S., President. London: Longmans, Green, and Co.

Their spirit and their point of view are equally faulty. To accuse a man of "having permitted the cheers of his audience to stimulate him to the utterance of words which no right-minded man could employ," when all the time he was delivering a speech previously drawn up and actually printed; to say, with Dean Cowie, that the Professor ended his speech "by terming himself a material atheist;" to speak of Professors Tyndall and Huxley as "ignoring the existence of God, and advocating pure and simple materialism;" or to represent our author as "patting religion on the back,"—is a style of criticism not recognised among gentlemen, and requiring more malice than subtlety, profundity, or learning.

Professor Tyndall, in some parts of his speech, had apparently forgotten that it is the duty of the British Association to confine itself to facts, and to inductions carefully drawn and rigidly verified, or at least capable of verification,—that Science deals not with noumena but with phenomena, and that the ground on which Lucretius and Bishop Butler wage their endless and fruitless warfare lies beyond her jurisdiction. Consciousness and sensibility are for us ultimate facts, about which we may speculate,—and quarrel, if foolish enough,—but which we can never explain. Instead, however, of pointing out where the orator had ceased to be scientific, they, with sublime inconsistency, denounced Science with an eagerness as if the matter had been prepared long beforehand, and merely an occasion had been sought for its delivery. The ostensible *casus belli* was the Belfast speech; the real quarrel seems to be with the scientific spirit in its legitimate manifestations, and with its growing influence in the world. Of this Dr. Magee's late sermon on the "Gospel of Science" is a striking example. There are minds who seek in everything what—for want of a better name—we term unvarying law, and to whom the arbitrary is essentially painful. There are other minds who have a no less decided craving for will and for personality. To this latter class some of Dr. Tyndall's critics seem to belong by virtue of race.* But has this class any right to forbid the former from even stating its views?

To follow the "Address" from proposition to proposition,

* It is a curious fact, not hitherto recognised, that all or nearly all Professor Tyndall's denouncers appear to belong to the Celtic race, as far at least as we may judge from their names. The Abbé Moigno declares himself a Breton, consequently a Celt of the Celts. Such names as Cullen, Magee, Fraser, Cowie, and Blackie speak for themselves. We believe that this rule will be found to hold good in other cases where men of science or scientific researches are attacked from the same point of view.

weighing each along with the objections raised against it and with the fervid eulogies of Dr. Guardia,* would be simply a life-task. This will readily be granted when we remind ourselves that, in addition to the vexed question of "materialism,"—more correctly called *apneumatism,*—the transformation of species is one of the points upon which judgment must be given. Indeed, passing over the merely historical portion and the "drawn game" between Lucretius and Bishop Butler, the main part of the "Address" is devoted to an exposition of the speculations of Darwin, Huxley, and Wallace, on the origin of species, and to the researches of Herbert Spencer on the development of intelligence. To this part of the subject we are led by the remark that Bishop Butler "boldly embraced the whole animal world in his scheme of immortality," like the Rev. J. G. Wood in our day,—thus overthrowing one of the most formidable barriers erected between man and the lower animals,—and by a reference to modern geology and palæontology. We do not know whether due weight has been laid upon the circumstance that the infant chimpanzee and the human infant have a very considerable resemblance to each other, which, as the two beings respectively approach maturity, becomes smaller and smaller. What does this signify? If, standing on an eminence overlooking a wide tract of country, we see two streams gradually and continuously diverging from each other as they flow,—say, eastwards,—we very naturally conclude that if we could follow them in the opposite direction we should find them gradually converge. In the same manner the increasing divergence of animals, as they approach perfection, seems strongly to support the view of an ultimately common origin of species now apparently distinct. It is a ludicrous error to suppose that the doctrine of evolution is necessarily connected with the atheistic hypothesis of existence. Just the same charges were in former days brought against the heliocentric theory of the solar system. Should "transformism"—as the Abbé Moigno inelegantly calls it—become better understood, divines will cease to dread it, and we shall see once more the Church and the World yoke themselves side by side to the chariot of success. The recognition of the development theory is, however, retarded by an abuse to which one of its doctrines is put by a certain social school, and which gives a certain plausibility to such attacks as those of the eloquent and subtle, though sorely mistaken, Bishop of Peterborough.

* Moniteur Scientifique, November, 1874.

When Darwin, Wallace, Bates, Belt, and other practical naturalists, speak of the "struggle for existence" and of the "survival of the fittest," they mean by the term "fittest" simply that which is in the closest harmony with surrounding conditions. But outsiders—such as divines, lawyers, politicians, economists, and literary men—very generally go away with the impression that by this word the advocates of development imply whatsoever is in the abstract best and worthiest, or whatsoever is most useful to man and most beautiful in his eyes. Under the influence of this mistake they are led astray, in one of two opposite directions, according to their prepossessions. Those, on the one hand, who know that without man's active intervention worthless weeds would soon choke the precious grain, believe that they have here found the *reductio ad absurdum* of "Darwinism." On the other hand, certain economists who hold that success is the sole test of merit, and who believe the inventor who starves in a garret less "worthy" than the "financier" who robs him of his invention, fancy they see in the theory of development the scientific consecration of their private creed. Their consequent adoption of a part of its language brings "Darwinism" into contempt.

Scientific men should, above all things, insist that every scientific theory must stand or fall on its own merits, and not in virtue of its assumed "tendencies." We can acknowledge no extraneous jurisdiction. Time was when the Church claimed to be the depository not merely of spiritual but of physical truth; this usurpation is at an end. Science, no longer a "handmaid," sits crowned and armed in her own sphere. But here, as elsewhere, rights imply duties. Firmly and consistently as she must claim and hold the undivided right to the interpretation of the physical universe, no less firmly must she abstain from every attempt to extend her jurisdiction over the emotional phase of man's being.

Professor Tyndall, desirous, doubtless, to lay before the public some of the most recent and advanced results of modern speculation, was not happy in certain parts of his speech. If he did not actually cross the border, he appeared so to do. A teacher, further, should beware of needlessly exciting against himself and his views the passions of his pupils. It is rarely prudent to attack a prejudice in pitched battle. All that there is in this "Address" really valuable, truthful, and to the point, might, we believe, have been said without wounding the feelings of many estimable men, and without raising a storm, all whose results will scarcely be favourable to the cause of Science. One of the best remarks on the "Address" was made by the Abbé Moigno, on learning that it had been drawn up among the Alps. A speech dealing with such important subjects, and so liable to be misunderstood, if not wilfully misinterpreted, should, the Abbé declares, have been composed not amidst the incidents and distractions of travel, but at home, in the quiet of a well-stocked library. Can Professor Tyndall suppose that this speech and its echoes will make the present Administration and its supporters at all more disposed to give national education a scientific basis, or to liberate the Universities from ecclesiastical control? We fear that, though not foolish himself, he will be the cause that folly is in others. Little as he may be hurt by the attacks of his censurers, we imagine that he cannot help feeling compunction for having brought down upon the public the "weak, washy, never-ending flood" of speeches, sermons, resolutions, and leading articles, from which we have suffered for the last two months.

VI. THE SPECTROSCOPE IN ITS APPLICATION TO MINT ASSAYING.

By Alexander E. Outerbridge, Jun.

THE invention of the simple instrument called the spectroscope has led, within a brief period of years, to such astounding revelations, that it is not unnatural to imagine that untold possibilities may still lie concealed in its future. Those who are at all familiar with the subject of spectrum analysis, do not require to be told that the spectroscope has increased tenfold the range of human knowledge within the domain to which it is applicable; and has also reduced much of what has heretofore been little better than matter of speculation, to a certainty as convincing, to a scientific mind, as a mathematical demonstration. Applicable alike to the analysis of tangible substances and of the celestial fires, its mysteries have been wrested from it, as it were, from an invisible world, by its devoted students.

All the classes of observations hitherto accomplished, fall under the head of *qualitative* analysis, in which perfection appears to have been already attained. Should a like perfection be attainable *quantitatively*, little more would appear desirable. With the view of probing for new facts in this direction, a limited but promising field of experiment has lately been adopted simultaneously, but independently, in the assay departments of the Royal Mint in England, and of the U. S. Mint at Philadelphia, and the attempt has been made to insert the wedge of future investigation by obtaining from the spectroscope a quantitative analysis of the composition of metallic alloys.

In a late annual report of the Royal Mint, Mr. Wm. Chandler Roberts, the chemist of the Mint (who with Mr. J. Norman Lockyer, the pioneer in spectroscopic research, has been conducting a series of experiments), states he is satisfied that by means of the spectroscope very minute differences in composition of gold-copper alloys can be astained. He, however, "refrains from describing the process, as the exact method of manipulation had not been determined upon. In the following paper, which also appears in No. xcviii. of the "Journal of the Franklin Institute," the attempt is made to narrate, not technically, the process adopted, and the conclusion arrived at, in the experiments made in Philadelphia, the details of which are contained in a report by the writer to the chief assayer of the Mint, dated May, 1874, and published in the "Proceedings of the American Philosophical Society" of the 15th of that month, vol. xiv., page 162.

The beautiful parti-coloured band of light, resembling a section of a miniature rainbow, resulting from the passage of a ray of white light through a prism, is familiar to every one; this simple experiment forms an appropriate introduction to the fascinating study of spectrum analysis.

Every kind of light not strictly mono-chromatic may, by means of the prism, be resolved into its component colours. The spectroscope is a simple combination of prisms and lenses for the scientific examination of these different colours or spectra.

The numerous terrestrial elements, when in the state of incandescent vapour, give their own distinctive colours, which appear in the spectroscope as *lines of light* arranged in definite positions, whereby each element may be easily recognised.

The passage of powerful electric sparks (from an induction coil) between two terminal points of the metal to be ex-examined, vaporises a small portion of the metal, and this incandescent vapour transmits to the eye of the spectroscopic observer its *luminous autograph* which nature never counterfeits. Should either or both of the metallic points, or *elec-*

trodes, consist of an alloy of two or more metals, the autograph of each may be clearly read.

Mr. Lockyer noticed, while studying these luminous autographs, that when he separated the metallic electrodes, causing the spark to leap a greater distance through the air, the spectral lines no longer continued to cross the entire field of vision, but certain of them *broke in the middle*, and upon further increasing the distance between the electrodes, the hiatuses in the spectral lines increased proportionately, *but unequally with different alloys*. As the proportion of either metal of an alloy is increased, *its lines lengthen*, and conversely with the lines of the other metal. Upon this discovery, Mr. Lockyer based the theory of a possible method of quantitative analysis.

The spectroscope was known to be marvellously sensitive to the impression of these autographs, and it therefore appeared plain that could such a method of analysis be reduced to a practical basis, its value would be immense in assaying metals used in coinage. For although the present modes of assaying precious metals have been brought to great perfection, yet the process is slow and tedious, requiring many chemical operations and great delicacy of manipulation; and "there is something captivating in the idea of a determination, as it were, by a flash of lightning, or in the twinkling of an eye, what proportion of gold or silver is present in any bar or coin."

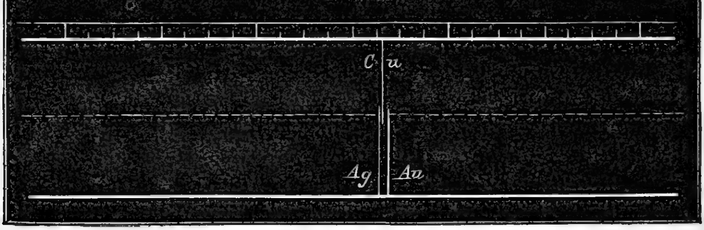
It was with the hope of reducing this beautiful theory of Mr. Lockyer to practice that these experiments were undertaken. The investigation extended over a period of several months, the principal part of the work being conducted at the University of Pennsylvania, with the benefit of the excellent apparatus and appliances afforded in the new college building—a privilege kindly extended by Prof. Barker of that institution.

A powerful induction coil, reinforced by Leyden jars, in connection with a two-prism Browning spectroscope, was employed, and it was found possible, after repeated comparisons of the spectra of different known alloys of gold and copper, to map the difference of fineness between specimens having respectively 500 and 750 parts of gold in 1000 of the alloy, and even to recognise the variation between coin-ingots 895 and 902 fine. This variation, within 7000ths, was by no means marked, although it seemed probable that a more delicate adjustment of apparatus, and further experience, would render the distinction more decided.

The spark, in passing through the air, also vaporises its constituents, viz., oxygen, nitrogen, &c.; these, of course, write *their* signatures in the spectroscope, and it is necessary to eliminate the numerous bright air lines which thus appear in all the spectra. Some of the lines of different metals appear in close proximity, and might readily be misinterpreted. Thus a bright blue line of bismuth is almost identical in position with one of zinc. A green line of iron is nearly coincident with a bright gold line. The difficulty which presented itself in the exact comparison of these proximate lines was overcome by using a pure metal as one electrode and another pure metal as the other electrode. The effect thereby produced was very curious. With pure gold and pure copper as the electrodes, the gold lines extend across only one half the field of the spectrum, and the copper lines extend only across the other half, the medial termini of both sets of lines being perfectly sharp and bright. By this means a double spectrum of copper and gold is obtained, or, rather, a section of a complete gold spectrum and a section of a complete copper spectrum are visible in immediate juxtaposition, thereby enabling a most accurate comparison of lines, which in reality are not identical in position, but which by the previous method were apparently so.

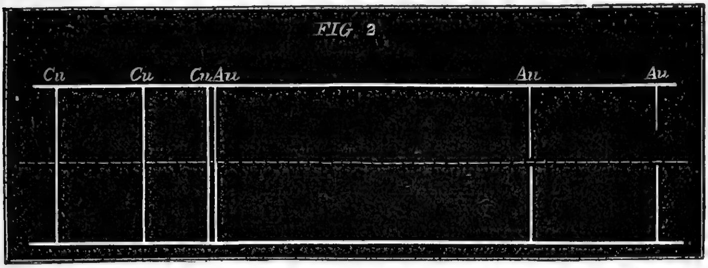
By a slight modification of the experiment, substituting pure copper as one electrode and an alloy of silver and gold as the other, the proximate lines of these three metals are presented mapped, as it were, on a natural scale. (Fig. 1.)

FIG. 1.

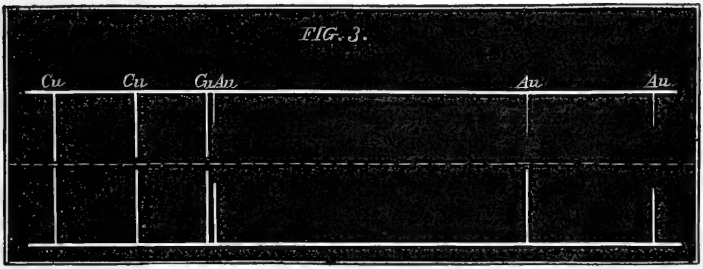


Further modifications of this principle suggested themselves, and were tried with indications of valuable results.

By using as one electrode an alloy of gold and copper of comparative fineness, and a baser alloy of the same metals as the other electrode, a result not before observed presented itself. The lines of both copper and gold crossed the entire field of vision, but in the section representing the fine alloy, the gold lines were strong and bright, while in the section representing the base alloy the gold lines were very faint. (Fig. 2.)



By now gradually increasing the distance between the electrodes, the faint gold lines of the base alloy cease to join their bright counterparts of the fine metal at the central line. (Fig. 3.)



The intervening space is at first minute, but as the electrodes are further separated, the ends of the faint lines gradually recede towards the outer edge of the spectrum, until they finally disappear altogether.

The general principle was thus satisfactorily proved, that where two alloys of different grades are subjected to this treatment, the gold lines of the baser compound are noticeably the fainter of the two, and, what is more important, they may be reduced in length by separating the poles, until they disappear.

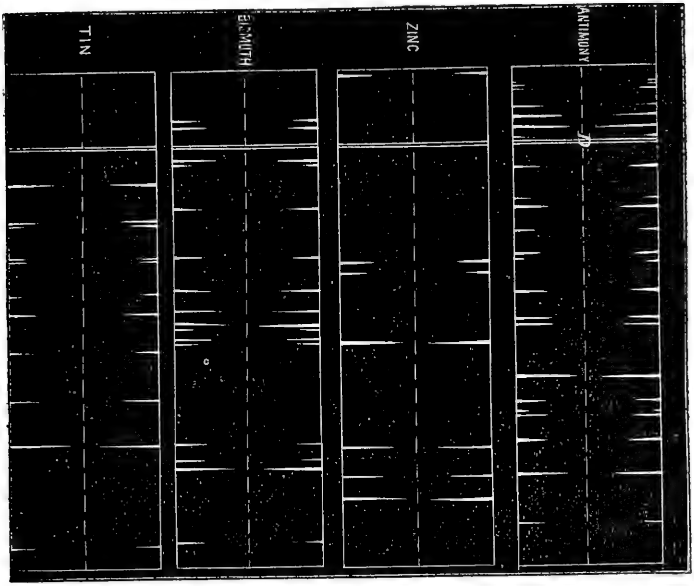
This pointed to the possibility of the future application of spectrum analysis to assaying, at least, as a test method. For, if an alloy of absolute known fineness were adopted as one electrode, and an ingot-slip assayed by the old process to an equal grade of fineness were inserted as the opposite electrode, in case the assay were correct, the gold lines in both sections of the spectrum should appear of equal brightness, and, more especially, should begin to recede from the central line of the spectrum at the same moment, and should disappear at the same moment.

The spectra being inevitable natural effects of physical causes, a variation between two specimens of supposed equal fineness would, *in theory*, be necessarily indicated by the respective lines failing to correspond in their reciprocal action.

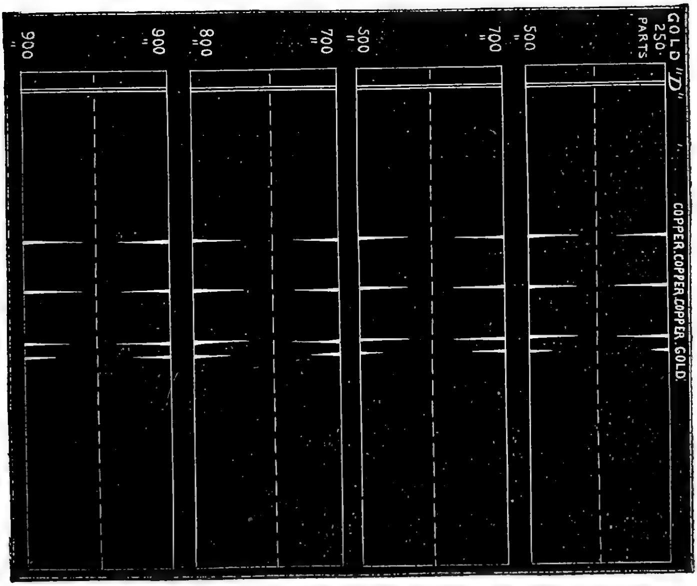
A serious source of error in these comparisons was soon discovered, viz., that if one electrode was nearer the centre of the slit of the spectroscope than the other, its spectral lines would appear proportionately longer than those of its *vis-a-vis*, even though both electrodes were of the same pure metal. It then became necessary to devise a special apparatus for manipulating the electrodes when under examination. This was constructed by Mr. Samuel James, the machinist in the Mint, and admirably fulfilled its object. A woodcut of it is appended hereto. Its peculiarity consisted in an automatic combination of accurately proportioned screws, acting in opposite directions, by which a single motion of the hand sufficed to cause the upper and lower electrodes to approach or recede from the central line of contact in an equal degree. The electrodes, which consisted of small strips of metal cut to a point, were held by a suitable arrangement on the outer circumference of two metallic rings insulated from each other, the upper one slotted to receive a series of twelve electrodes of varying *known* fineness, and revolving horizontally, so that each electrode might in turn be adjusted to face a single electrode of *unknown* fineness fixed on the lower ring. Its object was to admit of the electrodes being separated to any desired extent, while preserving the line of vision through the spectroscope, directed to the centre of the spark.

In regulating the height of the instrument, the apparatus was always adjusted by passing the spark between two electrodes of pure gold, so that, on separating the points, the respective spectral lines corresponded exactly in their reciprocal action.

A systematic series of experiments was now commenced, in which the behaviour of the more volatile metals was at first studied, viz., lead, zinc, bismuth, tin, antimony, cadmium, mercury, aluminum, &c. All these give more decided spectra than the less volatile precious metals, and some interesting results were noticed. Approximate illustrations of some of these spectra are appended.



SPECTRA OF VOLATILE METALS



QUANTITATIVE SPECTRA OF GOLD-COPPER ALLOY.

A very curious fact is apparent in all these spectra, viz., the *unequal lengths* of the spectral lines. Some of the lines of bismuth, for example, are seen to extend nearly across the field of vision, while others appear as mere points upon the edge. Mr. Lockyer has published some most interesting investigations upon the subject of these "long and short lines."

Proceeding to the examination of gold alloys, and starting with base poles,—making the lower pole 250 fine and the upper pole 500 fine,—the gold lines from the upper half were both longer and brighter. Now substituting in place of the 250 pole one 700 fine, the lower half showed the brighter gold lines. Then, changing the 500 pole for one 800, the brightness of the gold line was again reversed. This alternating effect may be continued, decreasing in degree as the fineness of the poles approach more nearly together, until both poles are of the same fineness, when the lines will be equal in length and intensity.

These experiments proved satisfactorily that comparatively wide variations in the composition of gold alloys were discernible. A series of graduated alloys, of more approximate fineness, was now prepared at the Mint, viz.—

Gold and Copper.	Gold, Silver, and Copper.
938·0	940·1
917·0	918·7
906·0	866·8
888·3	888·0
883·5	884·1
876·5	883·0

These alloys were carefully prepared, and assayed closely.

With one electrode pure gold and the other 938 fine, the difference between the respective spectra was of course very marked, the copper lines appearing in the one and not in the other. Substituting for the pure gold the alloy 876·5, the difference was still very marked, for, although both gold and copper appeared in each, the copper lines were much brighter and somewhat longer in the baser alloy, while the gold lines were brighter and longer in the finer. But on comparing the alloys 876·5 and 883·5 (reducing the variation to seven-thousandths), it was both a surprise and disappointment to find the visible difference of result but slightly appreciable. And the same with regard to the alloys of 883·5 and 888·3, and the same with other alloys with equal or less comparative variation of fineness. A variation of one-thousandth required an effort of the imagination, as well as of the eye, to detect any difference whatever. And, although the attempt was made to map an apparent difference between alloys varying two-thousandths, it would certainly not have been a safe test on which to base an assay. Frequent repetitions with changes of adjustment were tried, the battery power varying from one to six Bunsen cells, in connection with Leyden jars varying from one very small jar (improvised out of a test-tube) to fifty large jars, representing a metallic superficies of many square feet, with variations of the distance of the electrodes apart, and with and without the use of a condensing lens; but all these failed to give closer results.

It is true that these changes of conditions produced certain variations in the effects observed,—as, for instance, it was noticed that an increase in the Leyden jar surface always lengthened the lines, the distance between the electrodes and all other conditions remaining the same,—while a decrease in the condensing surface had an opposite effect. Thus, to take the extreme cases, with the single small Leyden jar above referred to, and one cell of battery, the lines broke when the electrodes were not more than one-sixteenth of an inch apart, and disappeared entirely on separating the points one-eighth of an inch.

With fifty Leyden jars and six cells of battery it was found impossible to break the lines at all, even by removing the electrodes to the extreme limit of the spark, and in this case new lines also appeared.

Other variations occurred—such as a momentary irregularity in the length and brightness of the lines, under a strong battery power, owing to the unequal action of the spark; a difference in the action of the gold lines dependent upon the nature of the alloy, silver tending to lengthen them more than equal admixture of copper; the length of the lines is also dependent upon the distance between the spark and the slit (when the latter is used without the intervening condensing lens); moreover, the eye itself is liable to become confused by continued comparisons of very slight differences. The above, and other modifications, so far from solving the problem of close work, rather indicated possible sources of error.

Another element of the process suggested itself as likely to render the results uncertain for the practical purpose of assaying, viz., whether the quantity of metal vaporised, and giving the spectrum, is not too infinitesimal to give safe results for a large melt. This would be affected by the least want of homogeneity in the metal. This is a serious

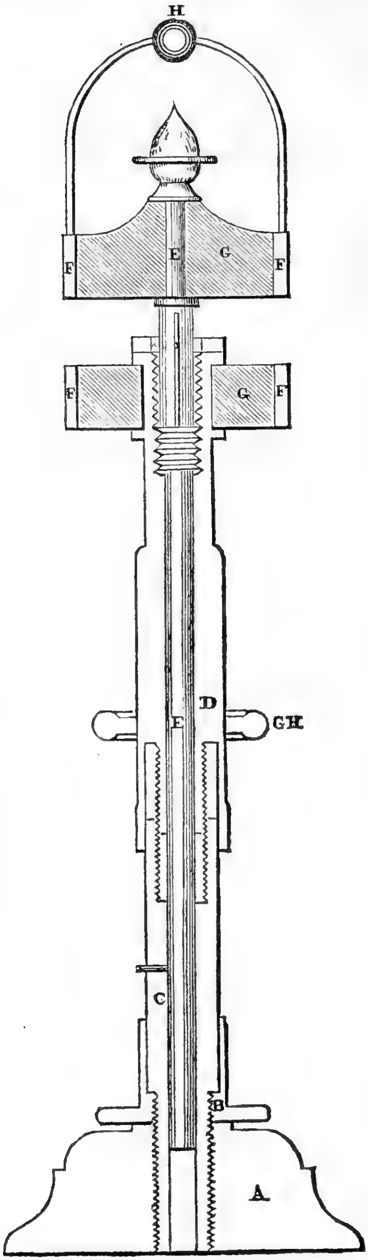


FIG. 4.—ADJUSTABLE ROTARY STAND FOR SPECTROSCOPIC ASSAY.

consideration, and with the view partly to search for unknown sources of error and partly to ascertain generally the quantity of metal operated on in a spectroscopic assay (should that ever be possible) the following experiment was tried:—Having weighed small electrodes, averaging eighteen milligrammes each, with the greatest possible accuracy, on the gold assay balance of the Mint (which is sensitive to a twentieth of a milligramme, or even less), and having arranged a spark register, it was found that 1000 sparks might be passed between these poles, each spark showing the spectrum of the metal distinctly, and yet the loss in weight was too small to be made the base of calculation. Thus, a gold pole lost in weight, after passing 1000 sparks, 1-1000th of a grain; this gives for each spark 1-1,000,000th of a grain of gold, producing a bright spectrum. The number was then increased to 3000 sparks as a test. The loss of weight depends of course upon the electric volume, and in the experiments tabulated an endeavour was made to keep the latter constant. A slight deposit of the vaporised metal from the opposite pole takes place in fine division, but this is easily removed—in the case of copper and gold poles by dipping the gold for a moment in weak acid or by gentle rubbing. The annexed tables, marked A and B, show that the loss in weight is marvellously small, averaging less than seven-tenths of a milligramme of gold for 3000 sparks. To give the amount for each spark, this must be divided by the number of sparks; thus, in round numbers, an electrode loses 1-1000th of a grain after passing 3000 sparks; or for 1000 sparks 1-3000th of a grain, or for each spark 1-1,000,000th of a grain. The exceedingly small quantity of metal thus assayed renders this process, in the writer's opinion, inapplicable to the operations in the Mint; for it is necessary to determine gold assays to the 1-10,000th part of the normal assay weight, and it is hardly conceivable that a discrimination to the 1-10,000th part of the spark assay weight, or the 1-10,000,000,000th of a grain, is practically possible. Even if it were, it would not be proper to assume that a test on such an atomic scale would correctly represent the value of a large deposit, or even of gold ingots. It would certainly not be in the case of silver, which segregates.

The experiments made by Cappel to determine the *minimum* amount of each element that will show a spectrum have been published in tabular form. His method was to volatilise "solutions of the metallic salts between the poles of a small induction coil in Mitscherlich's glass tubes with platinum wicks. A series of solutions, each one half the strength of the preceding one, were prepared from a number of metallic chlorides. The spectrum in connection with the positive pole was continually observed while increasingly concentrated solutions were brought in succession into the action of the spark, until the lines of the substance were clearly visible." If a sceptical mind refuses to believe the results of Cappel, who tells us that 1-600th of a milligramme (1-38,800th of a grain) of nickel will *just* write the signature of that metal, what will he say when, glancing at table B appended hereto, he finds the statement that 1-60,000th of a milligramme (1-3,880,000th of a grain) of nickel will sign its name in *brilliant characters?* And yet the author does not hesitate to say that even a *smaller* amount of this metal will show a spectrum, for it must be remembered that in these experiments a much stronger spark was used than was necessary to show a visible spectrum. When reduced to a minimum, as was done in the case of the miniature Leyden jar, which still gave a distinct spectrum, the loss in weight after 3000 sparks, for silver, copper, and tin, was *absolutely inappreciable* on the balance.

The table of loss shows another curious and unexpected result, viz., that the loss in weight of the volatile metals very slightly exceeds, and in some cases does not equal, the loss of the less volatile metals. Thus, in three different experiments of 3000 sparks each, copper loses but 0·1 milligramme, while gold loses 0·5 milligramme.

An unexplained anomaly was also noticed in relation to the sensitiveness of the spectroscope to the metals present in small proportion. Although Mr. Cappel has shown that 1-4000th of a milligramme of gold will show a spectrum (it is even less than 1-6000th of a milligramme, according to an experiment performed by the method described above) yet a comparatively large proportion of gold may be present in an alloy, the presence of which will not be indicated at all by the spectroscope.

In a slip composed thus—Silver 708 parts, copper 254 parts, gold 38 parts—the spectra of *silver* and *copper* are alone visible.

In fact, in a base alloy of gold and copper containing from 20 to 25 per cent of gold, the gold spectrum is barely visible; while in a fine alloy of gold and copper it was found that 1 per cent of the latter suffices to show the copper spectrum. Also in an alloy of nickel and copper containing 25 per cent of nickel, its spectrum is scarcely visible. Its seems evident, therefore, that the spark selects the more volatile metal as its vehicle.

If the spectroscope fails to reveal the presence of anything less than 200 parts of gold in a base alloy, even a theorist must admit that one could scarcely expect to be able to discriminate with certainty a variation of 1-10,000th in a fine alloy.

For the foregoing reasons, the conclusion seems inevitable, that in the state of spectroscopic science as it now exists, assaying by means of spectrum analysis is, for the present, impracticable for the purpose of Mint operations.

Although these experiments have resulted negatively from the utilitarian standpoint from which they were undertaken, it is hoped that they may prove not altogether without value in a more general point of view. The fact that quantitative proportions of composite substances may be recognised at all, even to a rough degree, cannot but be regarded as a first step. All observations bearing upon the action of the spectral lines in indicating such proportions are at least worthy of being recorded. Not the least curious of these incidental observations is the fact that while the spectroscope is sensitive to the minutest fraction of a grain of gold in the pure state or in solution, it fails to reveal the presence of a much larger proportion in a base alloy. Another is the fact that while the spark appears to select for its vehicle of transmission the more volatile metal in an alloy, and would thus seem to vaporise a greater quantity of the volatile than of the non-volatile component, yet, in point of fact, the loss of weight by such volatilisation is in some instances much less in the former case than in the latter.

The *rationale* of these apparent paradoxes is not at present evident, but if we may judge by former experiences in which problems even more mysterious have been resolved by study, we are warranted in anticipating that when a large number of observations, to be made perhaps by many experimenters groping in the dark, shall be collated, the true scent may of a sudden be struck which shall discover the desideratum of quantitative spectrum analysis.

Tables.

The *first* column shows the weight of the metallic electrodes in milligrammes before passing the sparks. *Second* column shows the weight after passing 3000 sparks. *Third* column shows the *total* weight of metal volatilised (in fractions of a milligramme). *Fourth* column shows the amount of metal volatilised by *each* spark (in fractions of a milligramme). *Fifth* column shows the amount of metal volatilised by each spark in fractions of a grain troy.

Table A.

			1.	2.	3.	4.	5.
*Upper Pole	..	Gold	16·6	15·9	0·7	1-4286	1-277000
Lower ,,	..	,,	16·7	16·0	0·7	,,	,,
Upper ,,	..	Copper	18·5	18·4	0·1	1-30000	1-1940000
Lower ,,	..	,,	,,	,,	0·1	,,	,,
Upper ,,	..	Gold Ingot	24·0	23·4	0·6	1-5000	1-324000
Lower ,,	..	,, ,,	,,	,,	0·6	,,	,,
Upper ,,	..	Tin	20·0	19·6	0·4	1-7500	1-486000
Lower ,,	..	,,	,,	19·4	0·6	1-5000	1-324000
Upper ,,	..	Silver	24·8	24·6	0·2	1-15000	1-976000
Lower ,,	..	,,	25·1	25·0	0·1	1-30000	1-1940000
†Average ..	..	Lead	91·6	90·0	1·6	1-1870	1-121000

Table B.

Upper Pole	..	Gold	20·5	20·0	0·5	1-6000	1-388000
Lower ,,	..	Copper	10·0	9·9	0·1	1-30000	1-1940000
Upper ,,	..	Gold Ingot	21·0	20·4	0·6	1-5000	1-324000
Lower ,,	..	Copper	20·2	20·0	0·2	1-15000	1-976000
Upper ,,	..	Silver	6·0	5·8	0·2	1-15000	,,
Lower ,,	..	Tin	20·0	19·4	0·6	1-5000	1-324000
‡Upper ,,	..	Nickel	12·0	11·95	0·05	1-60000	1-3880000
Lower ,,	..	,,	12·0	11·9	0·1	1-30000	1-1940000

* The upper pole usually formed the positive electrode.

† The average of lead is given because the results varied in different experiments.

‡ The *minimum* of metallic nickel producing a spectrum according to Cappel's tables is one six-hundredth of a milligramme.

NOTICES OF BOOKS.

Outlines of Cosmic Philosophy, based on the Doctrine of Evolution, with Criticisms on the Positive Philosophy. By John Fiske, M.A., LL.B., Assistant Librarian and formerly Lecturer on Philosophy at Harvard University. London: Macmillan and Co. 1874.

The groundwork of the present volumes appeared in 1869 and 1871, in the form of a Course of Lectures delivered at Harvard University, and afterwards in London, Boston, and New York. The work is a detailed treatment of Mr. Herbert Spencer's "New Philosophy," and it contains further criticisms of the Positive Philosophy, and some new matter relating to the evolution of society, and the conditions of human progress. The author commences by showing the relativity of all knowledge; we cannot know the absolute, only the relative, for "we cannot know things as they exist independently of our intelligence, but only as they exist in relation to our intelligence;" and, secondly (and this assertion we commend to all free-thinkers and materialists), "the possibilities of thought are not identical or co-extensive with the possibilities of things." If this were recognised more fully, how much less should we hear of those unfortunate conflicts between Religion and Science, of which we have heard a great deal too much of late. Led a step further, we have to realise that *all knowing is classifying;* that when we note any natural phenomenon, for example, and believe it to be explained, we have simply correlated it with other phenomena which it resembles. The elements of likeness, difference, and relation, enter into and limit all our cognition. The second chapter (on "The Scope of Philosophy") may be summed up in the following words:—"Common knowledge expresses in a single formula a particular truth respecting a particular group of phenomena; Science expresses in a single formula a general truth respecting an entire order of phenomena; Philosophy expresses in a single formula a universal truth respecting the whole world of phenomena. Philosophy therefore remains, as of old, the study of the Cosmos,—save that is the study of phenomena not of noumena, of evolution not of creation, of laws not of purposes, of the How? not of the Why?" The third chapter discusses the "Test of Truth;" and at the outset we meet with a very concise definition of truth, although described as only "provisionally defined:"—"Truth may be provisionally defined as the exact correspondence between the subjective order of our conceptions and the objective order of the relations among things." Concerning the methods of Philosophy, the author distinguishes between the subjective and the objective methods; the former belonging to metaphysical philosophy, which vainly attempts to frame plausible hypotheses concerning objectives by purely subjective means; the latter belonging to the Cosmic philosophy, which attempts to collect into a universal body of truth the generalisations obtained by Science, and thus adopts the method of Science, the objective method. Metaphysics is well distinguished from Physics in the fifth chapter:—" A scientific explanation is a hypothesis which admits of verification,—it can be either proved or disproved; while a metaphysical explanation is a hypothesis which does not admit of verification,—it can neither be proved or disproved." Thus our author considers Newton's hypothesis of gravitation, and Descartes's hypothesis of vortices, as strictly scientific; for the first admitted of proof, and the latter of disproof; while Stahl's hypothesis of a vital principle was purely metaphysical, for it admitted of neither.

In the chapter on "Anthropomorphism and Cosmism" Dr. Fiske has some very pertinent remarks concerning the apparent antagonism between Science and Religion, "the abiding terror of timid or superficial minds:"—" While Atheism scoffed at religion, and denied that the religious sentiment needed satisfaction; while Positivism, leaving no place in its scheme for religion to occupy, was compelled by an afterthought to proclaim that the religious sentiment finds its legitimate satisfaction in the service of an idealised Humanity; Cosmism, on the contrary, assigns to religion the same place which it has always occupied, and affirms that the religious sentiment must find satisfaction in the future as in the past, in the recognition of a Power which is beyond Humanity, and upon which Humanity depends. The existence of God—denied by Atheism and ignored by Positivism—is the fundamental postulate upon which Cosmism bases its synthesis of scientific truths. The infinite and absolute power which Anthropomorphism has in countless ways sought to define and limit by metaphysical formulæ, thereby rendering it finite and relative, is the power which Cosmism refrains from defining and limiting by metaphysical formulæ, thereby acknowledging—so far as the exigencies of human speaking and thinking will allow—that it is infinite and absolute. Thus, in the progress from Anthropomorphism to Cosmism, the religious attitude remains unchanged from the beginning to the end." We do not remember to have seen this view put in more forcible and just language.

In the chapter on the "Classification of the Sciences" we are told that Astronomy was a science in the days of Hipparchos; Physics became a science when Galileo discovered the laws of falling bodies; Chemistry, when Lavoisier disproved the theory of phlogiston and explained the true principles of combustion;

Biology, when Bichat indicated the relations existing between the functions of organs and the properties of tissues; while Sociology has at present become a science. The following complete classification of the sciences is finally adopted:—

ABSTRACT SCIENCES, Dealing with *relations*, that are—	Qualitative	LOGIC.
	Quantitative	MATHEMATICS.
ABSTRACT-CONCRETE SCIENCES, Dealing with *properties*, that are manifested—	In movements of masses ..	MOLAR PHYSICS.
	In movements of molecules, and in aggregations of molecules that are homogeneous	MOLECULAR PHYSICS.
	In aggregations of molecules that are heterogeneous ..	CHEMISTRY.
CONCRETE SCIENCES, Dealing with *aggregates* (with their properties and relations), as actually exemplified—	In stellar and planetary systems	ASTRONOMY.
	In the earth	GEOLOGY.
	In living organisms	BIOLOGY.
	In the functions which adjust organic actions to specific relations in the environment	PSYCHOLOGY.
	In the mutual relations of living organisms grouped into communities	SOCIOLOGY.

Dr. Fiske concludes the first part of his book by asserting that "the law of evolution is the first generalisation concerning the concrete universe as a whole, which has been framed in conscious conformity to the rigorous requirements of the objective method, and which has therefore served to realise the prophetic dream of Bacon, by presenting Philosophy as an organism of which the various sciences are members." And, finally, he fuses into one sentence the two thoughts (of Plato and of D'Alembert) which he places on the title-page of the work:—"To a thinker capable of comprehending it from a single point of view, the universe would present but a single fact, but one all comprehensive truth; and it is for this reason that we call it Cosmos, and not Chaos."

Part 1 (Prolegomena) is followed by Part 2 (Synthesis). At the basis of the physical sciences lie two universal propositions, or, as we should prefer to call them, axioms:—*Matter is indestructible* is the one, *Motion is continuous* is the other. "The fundamental elements of our conception of matter are its force-element and its space-element, namely, resistance and extension. The fundamental elements of our conception of motion are its force-element and its space- and time-element, namely, energy and velocity. That in each case the force-element is primordial is shown by the facts that what we cannot conceive as diminished by the compression of matter is not its extension, but its power of resistance; what we cannot conceive as diminished by the retardation of motion is not its velocity, but its energy." In the fourth chapter (on the "Law of Evolution") we find some curious and most interesting remarks on the relative condition of the molecules of carbon, hydrogen, oxygen, nitrogen, in organic bodies. It is shown how the molecules of the first-named element are far less mobile than those of the others, since solid carbon has never been expanded into the liquid form by the application of the most intense heat, while the gases hydrogen, oxygen, nitrogen, have never been condensed into the liquid form by the application of the most intense cold, accompanied by extremely high pressures. The fifth chapter contains a very graphic and comprehensive account of planetary evolution and the nebular hypothesis. The next chapter treats of the "Evolution of the Earth." In discussing the conditions of thought and consciousness, the author mentions that consciousness cannot continue for an instant unless oxygen is in contact with the grey tissue of the cerebrum; that mental exertion bears a "marked ratio" to the weight of the brain, and to the amount of phosphorus contained in it; and, finally, that the amount of heat evolved by the cerebrum during the act of thinking varies with the amount of mental activity which is taking place.

The doctrine of Natural Selection is very fully discussed, and the chances in favour of its being a true explanation are declared to be as many thousand millions to one. When this theory is analysed it is found to consist of eleven propositions, which may be enumerated as follows:—

"1. More organisms perish than survive.
2. No two individuals are exactly alike.
3. Individual peculiarities are transmissible to offspring.
4. Individuals whose peculiarities bring them into closest adaptation with their environment are those which survive and transmit their peculiar organisations.
5. The survival of the fittest thus tends to maintain an equilibrium between organisms and their environments.
6. But the environment of every group of organisms is steadily, though slowly, changing.
7. Every group of organisms must accordingly change in average character, under penalty of extinction.
8. Changes due to individual variation are complicated by the law that a change set up in any one part of a highly complex and coherent aggregate, like an organism, initiates changes in other parts.
9. They are further complicated by the law that structures are nourished in proportion to their use.
10. From the foregoing nine propositions, each one of which is indisputably true, it is an inevitable corollary that changes thus set up and complicated must eventually alter the specific character of any given group of organisms.

11. It is postulated that, since the first appearance of life upon the earth's surface, sufficient time has elapsed to have enabled such causes to produce all the specific heterogeneity now witnessed."

In a long chapter devoted to "The Composition of Mind" the author arrives at the conclusion that the mind has grown in strength and complexity as the nervous system has grown in definiteness and coherence. Next he traces the evolution of mind, and in discussing this he clearly points out that the operations of thought profoundly modify the structure of the brain, even in the course of a few years. The brain sometimes increases in structural complexity till the end of life, and often for many years after the age of 25. The brains of five very eminent men, examined by Wagner, were found to possess extraordinary complexities of structure,—ridgings, and furrowings, and deep irregular fissures. We are told, further, that the cerebrum and cerebellum are the organs in which ideal feelings and thoughts are generated, and that they are made up of tissues which undergo chemical changes with great rapidity. Exception is taken to Locke's comparison of an infant's mind to a blank sheet of paper, upon which experience writes knowledge: our author prefers to regard the infant's mind as a sheet written over with invisible ink, which is made visible by the developer of experience. The remaining chapters of the second volume—on the Evolution of Society, the Conditions of Progress, the Intellectual and Moral Genesis of Man, Matter and Spirit, &c.,—indicate a vast amount of reading, and a judicial examination of the more important now before the world.

The whole work is intensely suggestive, both to the man of science and to the metaphysician. The author has worked up into a homogeneous whole the more prominent views of Herbert Spencer, Freeman, Buckle, Bain, and has added somewhat of his own. To those who wish to be *au courant* with advanced modern thought, from Darwinism to Sociology, from the mechanical theory of heat to the nebular hypothesis, the book will be a great boon. No man of science can read it without profit. The facts are grouped together in a clear logical manner, and although some of the definitions appear to us to be quite unnecessarily complex, the book cannot be said to be overloaded by metaphysical technicalities.

A Treatise on Magnetism, General and Terrestrial. By HUMPHREY LLOYD, D.D., D.C.L., Provost of Trinity College, Dublin, formerly Professor of Natural Philosophy in the University. London: Longmans, Green, and Co. 1874.

THE author begins his treatise by giving an account of the general phenomena of magnetism, the processes of magnetisation, and the measurement of magnetic force. He then passes on to the magnetism of all bodies, discovered by Faraday in 1845, and extended by Knoblâud, Tyndall, and others. It is curious, however, that the fact that a piece of bismuth repels a magnetic needle was remarked no less than a century ago by Brugmans, while Lebaillif made the same observation with regard to antimony in 1829. From the experiments of Tyndall and Weber we may conclude—

"1. That all bodies are *polarised* under the action of a magnetic force.

2. That in soft iron, and other *para-magnetic* bodies, the pole induced in the side next the inducing pole is of a *contrary* kind, and that, consequently, the body is *attracted*.

3. That in bismuth, and other *dia-magnetic* bodies, the pole induced on the near side is of the same nature as the inducing pole, and that, therefore, the body is *repelled*."

Becquerel has found that the specific magnetism of manganese is such as would result from the presence of 1-1000th part of its weight of iron, while that of gold would result from the presence of 1-114000th part of its weight of iron; but this latter statement is not very clear, since we find gold given in the list of dia-magnetic bodies. The table of results given on p. 63 is of special interest. It shows the quantitative relationship between various bodies chiefly dia-magnetic (−), with a few magnetic gases (+), in regard to absolute force acting on equal volumes and reduced to a vacuum. From this we learn that while oxygen is +17·5, nitrogen is almost neutral on the side of attraction +0·3, carbonic acid is quite neutral, and hydrogen almost neutral on the side of repulsion, −0·1. Water rises to −96·6, sulphur to −118·0, and bismuth—the most dia-magnetic body of all—to −1967·6.

The positions of the four magnetic poles, the existence of which was inferred by Halley, are given as follows, the determinations being those of Prof. Hausteen:—

		Latitude.	Longitude.
Stronger North Pole	...	70° 5′ N.	99° 6′ W.
Weaker North Pole	...	85°·21′ N.	118° 39′ E.
Stronger South Pole	...	69° 26′ S.	138° 35′ E.
Weaker South Pole	...	77° 17′ S.	120° 57′ W.

A detailed account is given of the various elements of the earth's magnetism, and the methods of observing them. The book would be improved by good drawings of the various instruments now employed in magnetic observations. Some useful Curve Tables are given. The whole subject is treated mathematically, and the book is a valuable contribution to our magnetic literature. It will be especially useful to observers and workers in the numerous magnetic observatories which are now to be found in every part of the world.

The Common Frog. By St. George Mivart, F.R.S. &c. London: Macmillan and Co.

We love the frog, not merely when daintily dressed and served up at table,* but when enjoying life in his native pool. It is pleasant to watch him on a calm sunny day, as he rests embowered amidst reeds and water lilies, with his eyes just above the surface, contemplating the world with full approval, and to hear him now and again give vent to his feelings in a gentle murmur, all unlike his nocturnal chorus. Our friend's habit of rising to the surface of the water in fine weather, and of remaining at the bottom in gloom and storm, was known and noticed by Aristophanes, who represents him as saying—

> "When the sun rides in glory, and makes a bright day,
> 'Midst lilies and plants of the water I stray;
> But when the sky darkens with tempest and rain,
> I sink, like a pearl, in my watery domain."

The barometric propensity of the frog is often utilized on the Continent. A tall glass jar is filled with water, and fitted up with a miniature flight of stairs, and in it is imprisoned a frog—very often the pretty green tree-frog. Those who have carefully observed the movements of the little captive, can thence deduce weather forecasts quite as trustworthy as those furnished by the barometer. Mr. Mivart, indeed, awards to the frog the title "the martyr of science." "The frog," says he, "is the never-failing resource for the physiological experimenter. It would take long indeed to tell the sufferings of much-enduring frogs in the cause of science. What! frogs can do without their heads? What! their legs can do without their bodies? What! their arms can do without either head or trunk? What is the effect of the removal of their brains? How can they manage without their eyes, and without their ears? What effects result from all kinds of local imitations, from chokings, from poisonings, from mutilations the most varied? These are the questions again and again addressed to the little animal."

But, after all, the frog has less to fear from science than from brutish ignorance.

> "Whene'er we take our walks abroad,
> How many frogs we see"—

not to speak of toads and efts—which have evidently been wantonly tortured to death. Yet, like his nearest allies, the frog, far from being hurtful, is the friend of man. "It feeds," says Mr. Mivart, "exclusively upon living animals, such as insects and slugs, which it catches by suddenly throwing forwards beyond the mouth the free *hinder* part of the tongue (furnished

* We can vouch for the fact that the common frog, if a shade less delicate than the "Cambridgeshire nightingale," is yet, when properly prepared "choicely good."

with an adhesive secretion), and then retracting it, with its prey, in a most rapid manner." This style of hunting is carried on by the common frog upon the ground, and by the tree-frog up among the branches; the only misfortune being that not merely vermin, but rare and lovely species, which to capture gladdens the heart of the entomologist, thus come to an inglorious end. It is, by the way, a remarkable fact that the tree-frog, spread over all parts of Europe, and far from uncommon in the north of France and in the Channel Islands, is not found in Britain. Our climate certainly opposes no obstacle to his existence and multiplication, and suitable food he could find in plenty. His natural enemies are not more numerous in England than in France or Germany. What cause, then, can have led to his extinction since Britain was severed from the Continent? Or has he only appeared in Western Europe since that event. A similar fact, noticed by the author, is the absence of the land eft, or salamander. We are compelled, however, to differ from Mr. Mivart when he pronounces this animal common throughout Europe. In the more eastern parts of the Continent it is strictly local, being found in the greatest plenty in the Valley of the Neisse, between Ostritz and Hirschfelde. Yet we have in Devonshire, Wales, and Cumberland, localities so similar in character that we may wonder why this harmless, though dreaded,* creature is a stranger to our island.

The following passage will, perhaps, lead some persons to salutary reflection:—"Many persons are accustomed to make much of the distinctive peculiarities of the human frame. In fact, however, man's bodily structure is far less exceptional in the animal series, is far less peculiar and isolated, than that which is common to frogs and toads."

To minds of a higher degree of culture, the following sentences will prove even more suggestive:—"For some years, individuals of this species (the axolotl of Mexico) have been preserved in the Jardin des Plantes, at Paris, and a few years ago one individual amongst others there kept, was observed, to the astonishment of its guardians, to have transformed itself into a creature of quite another genus—the genus *Amblystoma*,—one rich in American species. Since then several other individuals have transformed themselves, but without affording any clue as to the conditions which determine this change—a change remarkable indeed, resulting, as it does, not merely in the loss of gills and the closing up of the gill openings, but in great changes with respect to the skull, the dentition, and other important structures.

"There is, moreover, another and very singular fact connected with this transformation. It is that no one of the individuals

* An officious gensd'arm on the frontier of Bohemia who insisted on overhauling our collecting boxes, fled in terror, calling upon St. Nepomuc to protect him when he saw a cargo of salamanders.

transformed (although we must suppose that by such transformation it has attained its highest development and perfection) has ever yet reproduced its kind, and this in spite of every effort made to promote reproduction by diet, and as to putting together males and females both transformed, also transformed males with females untransformed, and males untransformed with females transformed. Indeed, the sexual organs seem even to become atrophied in these transformed individuals. Moreover, all this time the untransformed individuals have gone on bringing forth young with the utmost fecundity."

We do not know of any modern observation more calculated to stagger a naturalist of the old school. The power of reproduction in every species was heretofore regarded as necessarily correlated with the highest development and full maturity. Here, on the contrary, we see such power left behind as a species advances to perfection.

The author suggests the possibility that the genus *Menobranchus* may be a "persistent larval form, and which now never attains its adult state." These considerations have an important bearing both upon the genesis and the extinction of species. The reader will not fail to remark that a current of what some will call anti-Darwinism pervades the whole work. In how far either the author or Mr. Darwin is in the right, and whether either of them has done more than apprehend some phases of a truth not yet revealed to us in its entirety, we shall not presume to determine. But we must point out that Mr. Mivart's polemic is strictly scientific, and, therefore, legitimate, based as it is on a searching morphological and physiological analysis of organic forms, and on a review of their mutual relations. It differs *toto cœlo* from the rhetorical and sentimental anti-Darwinian diatribes which pass current in the pulpit, on the platforms of young mens' institutes, and in commercial rooms, and which consists in little more than appeals to vulgar prejudice by a speaker whose tongue runs the more freely the less he knows of his subject.

Mr. Mivart has produced a work which may be read with pleasure and instruction by any person of decent education, and which at the same time will prove suggestive to the few who make the philosophy of organic life their special study.

It is an able monograph of the frog, and something more. It throws valuable cross-lights over wide portions of animated nature. Would that such works were more plentiful.

A Glossary of Fossil Mammalia. By J. E. Gore, Assoc. Inst. C.E. Roorkee: Thomason College Press.

In this tiny volume of fifty-one pages, the author gives a "list of fossil mammals, compiled for the use of students." The classification adopted is neither geological—according to the formations in which the remains have been found—nor zoological—according to their structural affinities—but alphabetical. The descriptions are necessarily of the briefest, and the localities are sometimes extremely vague, as, *e.g.*, "South America," "North America," &c. Controversial points are, in general, avoided, though the author thinks it "impossible to imagine how all the different races of mankind could have descended from one pair of ancestors in the comparatively short period of 6000 years." The extreme conciseness of the book has certainly one advantage: circumstances which might have been lost to the reader amidst copious descriptions, stand out here in startling relief. Among these we may mention the very small number of mammalian forms whose remains have hitherto been discovered. Where so much has evidently disappeared, no valid argument can be built upon the non-occurrence of any particular type, *e.g.*, an anthropoid intervening between the lowest man now known and the highest ape. In apes our fossil fauna is remarkably poor. Yet among the comparatively few forms which have reached us, how many are evidently "connecting links" between groups now existing in isolation.

We cannot help asking whether the work would not have been more useful to the student had the author extended his plan so as to include all the vertebrates? Even then it would have been far from bulky. For the purposes mentioned in the preface the work will doubtless be useful.

Sulphur in Iceland. By C. Carter Blake, D. Sc. London: E. and F. N. Spon.

The importance of sulphur in industrial chemistry can scarcely be conceived by the outside public. Perhaps if we say it plays a part in the chemical arts something analogous to that of iron in mechanical operations, we may give some idea of its value. In its uncombined, elementary state, it is largely used in the manufacture of gunpowder. Burnt in the air, and thus converted into sulphurous acid, it serves to bleach woollen goods and straw. But it is in the form of sulphuric acid, when combined with the largest possible amount of oxygen, that its applications are widest and most important. It enables us to decompose common salt, and thus to obtain a series of products, of which soda-ash, caustic soda, soda crystals, bicarbonate of soda, hydrochloric acid, bleaching-powder, soap, and glass, are the most prominent. By its aid we act upon certain insoluble minerals, such as coprolite and apatite, and convert them into valuable manures, or obtain from them phosphorus, essential to the match-trade. In numerous chemical processes we require, it is true, other mineral acids, such as the nitric and hydrofluoric; but before we can obtain these in a separate state we must first have sulphuric acid, which, as the great master-key to Nature's treasure-houses, sets them free from inert combinations. If we wish to obtain alum, which has many honest uses besides the questionable part it is made to play in the hands of the baker, we have again recourse to sulphuric acid. The very vegetable acids, such as the oxalic, generally require its aid before they can appear in a state of purity. In refining fats and oils it plays a prominent part. If we turn to the tinctorial arts, we find the dyer using this same acid at almost every stage. In short, we may say that, from the vastest operations of the manufacturer to the most delicate and minute procedures of the analyst, sulphur, directly or indirectly, is everywhere present.

The supply of sulphur becomes, in consequence, a question of national importance. Hitherto we have obtained it from Sicily, from Spain, and even from Mexico. The Mexican deposits labour, naturally, under the drawback of inconvenient distance. The sources in the Mediterranean countries, if not actually approaching exhaustion, have been very largely drawn upon, and the prices are hence high, with the probability of an increase. Thus, according to authentic information, Sicilian sulphur costs on the spot, £3 16s. 10d. per ton, and by the time it reaches England, its price has risen to £5 17s. 4d. At the same time there exist few richer deposits of sulphur capable of being worked at a less cost and much nearer to our own coasts. Iceland possesses sulphur-beds, compared to which those of Sicily are insignificant. The mineral lies on the surface, and requires merely digging, whilst in Sicily mining to the depth of 40 to 60 feet is required. At an outside estimate, Iceland sulphur can be delivered in any English port at £3 per ton. It appears that there are in Iceland two principal sulphur regions—that of Krisuvik, in the south-west, and that of Lake Myvatn, in the north-east of the island. A dispute appears to have arisen as to the comparative advantages offered by these two localities. Mr. Vincent, in a paper read before the Society of Arts, on the 15th of January, 1873, appears to have come forward as the advocate of the Krisuvik deposits, and to have indulged in some geological speculations which have at least the merit of singularity. Dr. Blake, in the pamphlet before us, proves, on the most indubitable testimony, the superiority of Lake Myvatn and its district as a source of sulphur, and exposes the geological dreams and geographical errors of his opponent in a very outspoken manner.

To us it seems important mainly that the resources of that wonderful island should be developed, alike to its own benefit and to that of our chemical manufacturers. If Mr. Vincent and his friends think Krisuvik the best locality, there is no reason why they should not go to work accordingly. Dr. Blake's treatise will do good service in drawing the attention of English capitalists to a region where such vast stores of wealth have been hitherto lying useless.

Elements of Physical Manipulation. By Edward C. Pickering. Part I. London: Macmillan and Co.

Treatises on chemical manipulation have been given to the world by Faraday and Mr. G. Williams, not to speak of the instructions on the subject appended to works on chemical analysis. But a systematic manual of physical manipulation has hitherto been a desideratum. The boundaries of the two subjects are not, indeed, very easily drawn. As the author remarks, "the object of all physical investigation is to determine the effects of certain natural forces, such as gravity, cohesion, heat, light, and electricity." Now, among the effects of heat, light, and electricity, chemical changes in the bodies acted upon form a very important part. Thus chemical and physical manipulation must be to a certain extent identical. The chemist is obliged to take note of many of the physical properties of the substances which come under his hands, and to account for their physical changes. The study of prolonged light is an intimate part of physics, but its action upon certain organic acids, and upon sugars, affords the chemist valuable means for their qualitative detection and quantitative determination. This, as a general truth, was pointed out by Comte, who shows that every science supplies methods, or means of research, for those succeeding it in the scale of increasing complication.

Mr. Pickering begins his work with an exposition of the two methods—the analytical and the graphical—of mathematically representing and examining the results of an experiment. In the former method each quantity is represented by a letter, and the conclusions are then drawn by algebraic methods and the calculus. The graphical method represents quantities by lines or distances, with a view to their geometrical treatment. The former method, as the author justly remarks, is the more accurate, and would be generally preferable were it not for accidental errors, and were every physical law capable of representation by a simple equation. The graphic method, on the other hand, has the advantage of speed, and enables the accuracy of results to be seen at a glance.

The author next proceeds to give instructions for physical measurements—the determination of time, weight, and distance. Of these considerations, the first and last, though playing hitherto no appreciable part in chemistry, are in physics of the highest importance.

Under the head "general experiments," we find valuable remarks on the estimation of tenths, on verniers, on the insertion of cross-hairs; suspension by silk fibres; temperature curve; testing thermometers; eccentricity of graduated circles; contour lines; cleaning mercury; calibration by mercury; calibration by water; the cathetomer; the hook gauge; the spherometer; estimation of tenths of a second; rating chronometers; making weights: method of weighing; decanting gases, and their reduction to standard temperature and pressure; standards of volume; reading microscopes; the dividing engine, and ruling scales.

Successive sections then treat of the mechanics of solids; the mechanics of liquids and gases; sound, and light.

We strongly recommend this work to all physical students; and we will even say, that now so much attention is directed to subjects where physics and chemistry inosculate, its careful study will be useful to chemists. This is especially the case with the section on light. A second volume is announced, dealing with light and electricity.

The Chemistry of Fermentation in the Process of Bread Making. By T. KARR CALLARD. London: Elliott Stock.

THE author of this pamphlet does not accept the "fungoid theory" of yeast. He holds that the "yeast cells are simply vesicles of carbonic acid gas entangled in the elastic gluten;" and the growth of yeast he considers as "nothing more than the conversion of fresh gluten into its own condition." What, then, effects the conversion? The constituents of bread, we are told, are "flour, yeast, potatoes, salt, and water." The mention of potatoes would certainly have astonished our forefathers, and at the present day will astonish those numerous families in the north of England who bake for their own consumption, and who, be it remarked, produce bread less white and "fluffy" than that of the London bakers, but sweeter, more digestible, and free from the tendency to turn sour. The use of potatoes in baking is certainly legalised by Act of Parliament, and cannot, therefore, be technically called an adulteration. That it is necessary the experience of generations has abundantly disproved. We are also told that flour containing little gluten is superior to samples in which this constituent is more plentiful! As a rule, we know that the wheats of cold climates are poor in nitrogenous matter, whilst those of Algeria, Hungary, Italy, and the Black Sea districts are rich. African wheat may contain twice as much gluten as Scotch. We grant that the last-mentioned may yield whiter bread, but we protest against colour being accepted as the standard of merit in any article of food, in place of flavour and nutritive power. A certain amount of starch is doubtlessly needed to dilute the gluten, and facilitate the process of bread making, but the latter is the blood-forming constituent.

The author's condemnation of the Kuhlman process for the detection of alum in bread is perfectly just. But if it ever has been "in the highest esteem" among chemists, it certainly is not so at present. To turn to alumina and sulphuric acid possibly present in salt as a means of explaining the occurrence of alum in bread, seems to us rather far-fetched.

We share the author's regrets that any baker should have been—as it is intimated—convicted wrongfully; but still more do we regret that hundreds should have used alum and other sophistications all their lives, and should have escaped.

Transactions of the Royal Society of Victoria. Vol. X., February, 1874. Melbourne: Stilwell and Knight.

It is satisfactory to see that the cultivation of science is not entirely overlooked by the "Greater Britains" of the southern ocean. The present volume makes its appearance under peculiar auspices. A certain Government grant had been withdrawn in 1868, and since that time there had been no funds to issue any transactions, or even to pay for the printing of the volume which had last appeared.

We are not quite satisfied with this explanation. We certainly hold that it is good policy on the part of Governments—as those of Germany are, and have long been, doing—to give every facility for the cultivation of science. We believe that museums, libraries, observatories, laboratories, founded and upheld at the national expense, will prove an excellent investment. But, in default of Government aid, great things may be accomplished by private munificence, as we see in the United States. It is scarcely creditable that so flourishing, energetic, and wealthy a community as Victoria should allow the transactions of its chief learned society to lie unpublished for want of funds!

The local administration has, it seems, repented at last of its mistaken parsimony, and the present volume has been prepared —under difficulties. Some of the papers, we are told, are altogether wanting; several have been returned to their authors: and some are not recoverable. We are happy to learn that there is "a prospect of greater regularity in the issue of the transactions for the future."

Sanitary regulations, or rather their necessity, appear to be engaging much attention at Melbourne. In addition to a paper on the "Decay of Gaspipes in Certain Soils," one on "Street

Odours, and Neglect of Ventilation (which appears to have been lost), and one on the "Yan-Yean Water Supply" (which has shared the same fate), we meet with an interesting essay by S. W. Gibbons, F.C.S., on "Air and Water Poisoning in Melbourne." The sanitary state of this rising city is not to be rejoiced over. In the gutters flows a liquid which is "simply sewage, differing only in age from that which flows in London sewers. Our sewer is on the roadway instead of under it, and the contents are carried off more rapidly. Moreover, the gases it evolves, instead of being confined in covered drains, and smelt only through occasional gratings, are here diffused in the atmosphere, to be breathed by the residents and passers-by, and only observed when they are more than usually noisome, as in Bourke and Swanston Streets, at night. Then it is ready-made pestilence." Mr. Gibbons has made careful microscopical examinations of the water from the street-gutters, and the results quite confirm the foregoing general description. The people of Melbourne, it appears, have the pleasant habit of flushing their cesspools *into the street!* These same cesspools appear to be constructed in a more offensive and dangerous manner even than we have them in England. "In a very illustrative case that lately came under my notice," says Mr. Gibbons, "the floor of the closet which joined the house was *two feet above that of the front parlour.* It was only emptied when the contents neared the floor. The fermented urine had eaten its way through the cesspit walls, and had saturated the ground under the house." To appreciate fully this state of things, we must remember that the average temperature is about 10° F. higher than that of England, and the supply of water decidedly less.

Passing to other subjects we cannot help noticing with regret that chemistry finds apparently few votaries at the antipodes. Of the fifty-five papers given in the index, not one can rank under the head of chemical research, and even technological chemistry and metallurgy are very meagrely represented. Not less do we deplore the absence of geological, zoological, and botanical research. In a country so imperfectly explored as is Australia, we should think that men of an observant turn of mind would feel themselves irresistibly drawn to such studies, and that a rich harvest of recorded facts would be the result. The quantity of work which has to be done before the organic geography of Australia is even tolerably complete, and before even a superficial oversight of its palæontology is acquired, is perfectly immense. How comes it that a body like the Royal Society of Victoria takes so little interest in what may be called its natural task?

"The Classificatory System of Kinship," by the Rev. Lorimer Fison, is a most interesting investigation into the social arrangements of savage man in pre-historic times. It would be impossible within our limits to give any just idea of its contents, but we hope that so valuable a contribution to ethnology will appear in some form accessible to the English public.

A paper on "Matter, a Mode of Motion," ranks, unfortunately, among the missing.

La Pluie et le Beau Temps. Par PAUL LAURENCIN. Paris: J. ROTHSCHILD.

THE appearance of this book might be taken as a sign that an interest in the state of the weather, sometimes considered as an English national peculiarity, has spread to our neighbours across the Channel. M. Laurencin has given the public a popular and handy manual of meteorology. He treats of the atmosphere, atmospheric heat, atmospheric currents, atmospheric moisture; rain, with its good and evil effects; storms, cyclones; the rainbow; fine weather; climates; the seasons; prevision of the weather; the moon; observatories; and the sanitary effects of rain and fine weather.

On all these subjects he gives in a pleasant, chatty manner, a great amount of information in which even the educated classes are often found wanting. A curious feature of the work is the quaint old proverbs in which the French peasantry have expressed their traditional weather wisdom. Some of these are preferable to their English representatives. We say in this country—

> "A rainbow in the morning
> Is the shepherds' warning;
> A rainbow at night
> Is the shepherds' delight."

Jacques Bon-homme, it appears, is less confident:—

> "Arc-en-ciel du matin
> Pluye sans fin;
> Arc-en-ciel du soir,
> Il faut voir."

A very wise conclusion! The work is abundantly illustrated.

Many of the engravings are essential to a right understanding of the text. But the views of sunrise and sunset, and the cuts of a swallow, a cart-horse, and a cat—the latter evidently in a very bad humour—can scarcely be pronounced either useful or ornamental.

Taken as a whole, we may pronounce this manual no unworthy member of that valuable and interesting series of popular scientific works which during the last few years have issued from the French press.

Les Rochers; Description de leurs Elements; Methode de Determination. Par Edouard Jannetaz. Paris: J. Rothschild.

Elementary works on geology are plentiful in the languages of all civilised nations, or cultur-völker, as the German phrase goes. These manuals give a description of the various rocks; treat of their occurrence and localities, their chemical components and organic remains; their supposed origin and age, and the changes they are inferred to have undergone from volcanic or from glacial action. But one thing is wanting. The tyro, who does not wish to limit his studies to books, but to become himself a reader of the great stone documents, asks "How am I to distinguish the various species of which you speak?" To this question ordinary geological text-books return no definite answer. The student may go to the localities indicated for any particular formation, and may there study its characteristic features, so as to recognise it on any future occasion. M. Jannetaz, however, endeavours to supply a direct answer to the question, and to give instructions for the determination of rocks.

In the first part of the work we find a description of the "mineral species most important from a lithological point of view," with their chemical, optical, and physical characters, the crystalline form being in most cases shown in a diagram. The second part treats of the rocks themselves, their essential and their secondary or accessory constituents. The section on granite gives a characteristic instance of the manner in which this department is worked out.

Lastly, we come to the "method to be followed in the practical determination of rocks," a portion of which we transcribe, to give the reader an idea of its nature:—

Sect. 1.—*Globular rocks* (p. 226).

I. Globules harder than the point of a graver, p. 226.	A. Vitreous globules. B. Crystalline ditto. C. Irregular grains, without intervening matter. D. Globules forming mamillary masses.
II. Globules softer, p. 228.	1. Effervescing in HCl, and not becoming magnetic on charcoal. 2. Effervescing in HCl, and becoming magnetic. 3. Not effervescing, but becoming magnetic. 4. Not effervescing, and not becoming magnetic.

Each of these heads is then further extended, until the clue followed leads the enquirer to the species sought.

We are perfectly aware that in no department of natural science is there a royal road to identification of species. When systematists have done their best, much will still be left to the patience and the tact of the student. We know that the very boundaries of species, and consequently their number, are points upon which eminent authorities differ. Still, making all needful allowance for such considerations, we consider that M. Jannetaz has, by the production of this little work, done much to smooth the path of the student, and we expect that it will, by facilitating observation, give a new impulse to geological science.

Causeries Scientifiques. Par HENRI DE PARVILLE. 1873. Paris: J. Rothschild.

THIS yearly volume has for its object to give an account of discoveries and inventions—of the progress of science and industrial art. But it does not consist of mere extracts from scientific and technological journals, and from the transactions of learned societies. The scientific novelties and facts are, of course, obtained from a variety of sources, but the description is original. In this manner is produced a whole much more readable than the English "Year-Book of Facts," and justly entitled to the name it bears—"Science-Gossip."

Like all annual works of its kind, it is, as far as the strictly scientific reader is concerned, open to the charge of giving what is called in Scotland "pipers' news." But for educated though not professional readers it furnishes a survey of the discoveries and inventions of the past twelvemonths, alike fascinating and instructive. The author wanders on from one subject to another in an easy, natural, and conversational manner. In France it is not considered necessary to employ a heavy and ungainly style when writing on scientific topics.

The Vienna Exhibition naturally plays a prominent part, an entire chapter being devoted to the improved artillery there displayed, into which is introduced an account of the far-famed establishment of Krupp, at Essen. Another interesting chapter is occupied with the researches of M. Pasteur on fermentation, and the manufacture of beer, naturally leading to the consideration of atmospheric germs, and of the conditions and products of decomposition. The effects of alcohol, dietetically and medicinally, are next examined, and the dreadful effects of "absinthe," over and above those capable of being traced to the alcohol it contains, are fully expounded. It is surprising and disgraceful that this diabolical beverage is not totally proscribed by law in all civilised countries. It is painful to see that its sale is extending in England, no one—not even the temperance party—uttering a protest.

The sanitary influence of leaden water-pipes (a subject which the French engineers had much better have left to be dealt with by chemists) is discussed at some length.

We find an interesting account of certain human monstrosities which have lately attracted public attention, such as the "dog-man, Yeftichjew, and his son. It is remarkable that in the human subject an excessive development of hair is accompanied by a defective growth of the teeth.

The celebrated Guadeloupe skeleton has been assigned by the researches of M. Hamy to an age between the first appearance of the Caribs in the lesser Antilles, and the dawn of the historical epoch. When the Caribs first showed themselves in those islands is still, however, a very doubtful point.

It will be perceived that one subject follows upon another in this work in a somewhat promiscuous manner, without any regard to scientific method. To meet this difficulty we find at the end a "methodic table," in which the subject-matter is classified under the heads of astronomy, physics, mechanics, &c.

PROGRESS IN SCIENCE.

MINING.

About a month ago, Mr. Robert Hunt's annual volume of "Mineral Statistics" was issued from the Mining Record Office, being thus earlier by nearly a couple of months than the corresponding volume for the preceding year. This advance has been effected by the zeal of the Keeper of Mining Records, coupled with a readiness on the part of most of our mine-holders to respond to his solicitations. Indeed, so far as the collection of the colliery-statistics goes, matters stand precisely as they did last year; that is to say, the returns demanded by the Coal Mines' Regulation Act never come within sight of the Keeper of Mining Records, and, consequently, a special application has to be made to each coal-owner for the purpose of eliciting the needful information bearing upon the position of our coal-production. Even as regards the metalliferous mines, the compulsory returns, furnished in compliance with the Act, contain merely the quantity of ore raised; hence the percentage of metal, and the market value of the ore, have to be sought in other directions. From one source and another, however, Mr. Hunt manages to bring together a vast body of valuable information in his Statistical Annual, and this with the best possible guarantee that the information may be taken to strictly represent the true position of our mineral industries. The results of his labours during the past year may be epitomised in the following conspectus, which shows at a glance the amount and value of the mineral produce of the United Kingdom in 1873:—

	Tons.	Cwts.	Value.
Coal	127,016,747	0	£47,631,280
Iron ore	15,577,499	0	7,573,676
Copper ore	80,188	10	342,708
Tin ore	14,884	17	1,056,835
Lead ore	73,500	10	1,131,907
Zinc ore..	15,969	0	61,166
Iron pyrites	58,924	3	35,485
Arsenic	5,448	17	22,854
Bismuth	1	4	68
Cobalt	0	6	12
Manganese	8,671	6	57,766
Ochre and Umber..	6,368	8	5,410
Wolfram	49	19	526
Clays, fine and fire, and shale (estimated)	1,750,000	0	656,300
Salt..	1,780,000	0	892,500
Barytes	10,269	11	7,993
Other earthy minerals (estimated)	—		3,000
Total value			£59,479,486

At a recent meeting of the Royal Geological Society of Cornwall, Dr. C. Le Neve Foster, Inspector of Metalliferous Mines for the West of England, read a paper on the Lode at Wheel Mary Ann, near Liskeard. This lode offers peculiarities of structure which deserve careful study by the mining geologist, for the sake of the light which they seem to throw upon the succession of changes that must have occurred in some of our mineral deposits. From Dr. Foster's observations underground, and on specimens taken from various parts of the vein, he has been enabled to deduce the following history of its formation.

fissure was first formed, accompanied probably by a shifting of the strata, so that a number of open spaces were left, and into some of these cavities fragments of the walls would eventually fall. These fragments were cemented into a breccia by the subsequent deposition of the cab, a sort of hornstone, which more or less completely filled up the fissure. At a later date the fissure was reopened, and lined with successive layers of quartz, galena, carbonate of iron, and calc-spar, together with fragments of the walls and of the prior-formed vein-stuff.

As it is of first importance to encourage habits of accurate observation among the young men engaged in our mining industries, we gladly call attention to a very creditable paper by Mr. A. K. Barnett, of Penzance, in which he offers some observations on the mineralogical and physical characters of the Elvan courses, the greenstones, and the sandstones of Cornwall, with remarks on their associated minerals. This paper, accompanied by a map of Cornwall, showing the Elvans and other intrusive rocks, is published in the last "Report of the Royal Cornwall Polytechnic Society." It is well known that the Elvan courses are intimately connected with the copper and tin lodes, on which they are generally believed to exert a beneficial influence, bunches of ore being frequently found at their intersection with the veins. A large number of the Elvans were examined by Mr. Barnett, who obtained a sufficiently fine collection of specimens to receive the Polytechnic Society's medal.

Whilst cross-sections of individual mines are frequently made, one does not often have an opportunity of seeing general transverse sections showing the connection of one mine with another, such as those recently prepared by Captain Maynard, of East Pool Mine, and described by him in the "Report of the Cornwall Polytechnic Society." By studying these sections, some of which are carried over several miles, and comprise a number of mines in their course, many facts are brought out which would probably be lost sight of in the study of sections which represent only individual mines.

In a paper on the "Geology of North West Lincolnshire," recently read before the Geological Society by the Rev. J. E. Cross, the position of the Lincolnshire iron ore, which of late years has been largely worked near Frodingham and Scunthorpe, was carefully examined, and its geological horizon definitely determined by palæontological and stratigraphical evidence. It appears that this iron ore occurs in the lower part of the Lower Lias, in the zone of *Ammonite semicostatus.* It is therefore much lower in the geological series than the celebrated Cleveland ironstone, which occurs in the Marlstone or Middle Lias. Possibly the ironstone worked at Caythorpe, near Grantham, may lie on the same geological horizon as the Frodingham ore.

A series of papers on the iron industries of this country is in course of contribution to the "Mining Journal," by Mr. Richard Meade, Assistant Keeper of Mining Records. The series commences with an article on the ironstones of Northamptonshire, and the history of iron smelting in that country. From the writer's position in the Mining Record Office, the statistical information in these articles will be in the highest degree trustworthy.

METALLURGY.

The present position of our metallurgical industries is well shown in the following general summary of the quantities and value of the several metals obtained from ores raised in Great Britain, during the year 1873, according to the statistics recently published by Mr. Robert Hunt:—

		Quantity.	Value.
Pig-Iron	Tons	6,566,451	£18,057,739
Tin	,,	9,972	1,329,766
Copper	,,	5,240	502,822
Lead	,,	54,235	1,263,375
Silver	Ounces	537,707	131,077
Zinc	Tons	4,471	120,099
Other metals (estimated)		—	5,000
		Total value	£21,409,878

Some beautiful bronzes from China and Japan, remarkable for their fine dark patina, contrasting strikingly with the silver work with which they were inlaid, have been examined by M. Henri Morin, of the Conservatoire des Arts et Métiers. His results have been laid before the Academy of Sciences of Paris, and are reproduced in the "Annales de Chimie et de Physique." M. Morin finds that all these bronzes contain lead, the amount in some cases rising to upwards of 20 per cent. As the proportion increases with the intensity of the patina, it is inferred that this patina is due to the actual composition of the alloy, and not to any external application. All the specimens contain zinc, and in one group the proportion of this metal rose to 6 per cent of the alloy. From some synthetic researches M. Morin is enabled to give the following recipe for the production of a bronze strongly resembling the finest productions of China and Japan:—Copper, 83; lead, 10; tin, 5; zinc, 2.

Nickel-plating has of late been very popular in this country, the metal being generally deposited, we believe, from a double sulphate of nickel and an alkali. Messrs. Baker and Unwin, of Sheffield, have recently patented an invention for the electro-deposition of nickel, their improved solution consisting of nickel oxide, and an alkali or mixture of alkalies, dissolved in tartaric acid. The following proportions are found convenient:—100 lbs. of nickel, and 67 of cream of tartar; or 100 lbs. of sulphate of nickel, 53 lbs. of tartaric acid, and 14 lbs. of caustic soda.

Under the name of *Galenite* M. J. David, of Paris, has patented a compound of lead, prepared from galena, to be used as a substitute for white-lead and red-lead. The powdered galena is roasted, at a low red heat, in open retorts, and the sulphide is thus oxidised into sulphate of lead: this product, having been ground in water, is dried, and introduced into commerce as galenite. The name appears to be very badly chosen, as some mineralogists are in the habit of applying it to the native sulphide, in order to bring the word "galena" into harmony with most other mineral names.

It is worth recording that Dr. Reichardt, of Jena, has been led to conclude that the presence of even a very small proportion of silicon in platinum may confer considerable brittleness upon this metal. A platinum vessel used in distilling oil of vitriol was found to be extremely crystalline and brittle, although new, and on analysis the following composition of the metal was obtained:—Platinum, 99·43; copper, 0·473; iron, 0·01; silicon, 0·03. The brittleness is referred not to the copper in the iron, but to the small proportion of silica.

MINERALOGY.

At Spring Creek, near the town of Beechworth, in Victoria, there are found —and, so far as we know, found there only—certain mineral structures known popularly as "water-stones," and scientifically as *Enhydros*. These peculiar bodies are irregular polyhedra, of chalcedony, having a hardness equal to that of topaz; in some cases dark brownish-yellow and nearly opaque, and in others colourless and transparent. They are generally hollow, and enclose a liquid with a movable bubble, like the air-bubble in a spirit-level. The enclosing shell is usually thin, and either smooth on the inside or encrusted with quartz. In the recently-published part of the "Transactions of the Royal Society of Victoria" will be found two papers descriptive of these enigmatical bodies. One of these papers, by Mr. J. E. Dunn, describes the geological occurrence of the Enhydros in granite and Silurian sandstone, at Spring Creek; whilst the other paper, by Mr. G. Foord, discusses the physical structure of the stones and the chemical nature of the enclosed liquid. This liquid appears to be a weak saline solution, consisting of water with a small proportion of the chlorides and sulphates of sodium, magnesium, and calcium; and it is believed that a soluble form of silica is also present.

In compliment to Prof. Dawson, Principal of McGill College, Montreal, the name *Dawsonite* has been bestowed upon a new mineral species, found in the joints of a trachytic dyke, near the western end of the College. This mineral has been studied by Dr. Harrington, the chemist and mineralogist to the

Geological Survey of Canada, who has recently described it in the "Canadian Naturalist." Dawsonite is a white, transparent, or translucent mineral, with a bladed structure, believed to crystallise in the monoclinic system. Two analyses revealed a remarkable composition, seeming to show that the mineral might be regarded as "a hydrous carbonate of alumina, lime, and soda; or perhaps a compound consisting of a hydrate of alumina combined with carbonates of lime and soda." If the existence of a native carbonate of alumina be admitted, the mineralogist will have no difficulty in understanding the composition of *Hovite*—a mineral described by Messrs. J. H. and G. Gladstone, in the "Philosophical Magazine" for 1862. It was suggested that Hovite might be a double carbonate of alumina and lime; but there has been some hesitation in accepting this suggestion, as chemists are not acquainted with a definite carbonate of alumina, or at least the existence of such a compound, as a laboratory product, seems doubtful.

A mineral, reputed to occur in rather large quantity near the town of Noumea, the capital of New Caledonia, has been examined by Prof. Liversidge, of Sydney, whose results have been published in the "Journal of the Chemical Society." The mineral is a soft, amorphous substance, of a fine apple-green colour, occurring with chrome iron-ore and steatite, in veins traversing serpentine rock. It appears, from Prof. Liversidge's analyses, to be a hydrous silicate of magnesium and nickel. We understand that the species is to be called *Noumeite*.

Some mineralogical notes have been communicated to Leonhard and Geinitz's "Neues Jahrbuch," by Dr. August Frenzel, who describes, among other minerals, a new species from Schneeberg, in Saxony, to be called *Miriquidite*—a name referring to the old Miriquidi forest, which formerly stretched over the whole Saxon Erzgebirge. The new mineral crystallises in the rhombohedral system, and contains oxide of lead, peroxide of iron, water, and arsenic and phosphoric acids; but a quantitative analysis has not yet been made,

Among some Russian minerals Frenzel has observed a specimen of native platinum in grains which were strongly magnetic. On analysis they were found to contain 76·97 per cent of platinum, and 10·97 of iron.

The extremely rare mineral to which Levy, many years ago, gave the name of *Roselite*, in honour of the late Gustav Rose, has been recently monographed by Prof. Schrauf, of Vienna. On a previous occasion we called attention, in these pages, to the recent discovery of this rare species at Schneeberg, in Saxony. The Daniel mine there has yielded some very fine examples, which have been thoroughly examined physically, crystallographically, and chemically, by Prof. Schrauf, whilst they have also been analysed by Dr. C. Winkler, of Freiberg.

Prof. Tschermak, of Vienna, has recently studied some of the remarkable meteoric masses found at Ovifak, in Greenland, and has published the results of his studies in the last part of his "Mineralogische Mittheilungen." This paper is preceded by a translation of Nauckhoff's Swedish memoir, descriptive of Nordenskjöld's discovery of the occurrence of native iron in a dyke of basalt. Tschermak's studies support the view that this iron is of meteoric origin.

In the last part of the "Mittheilungen" Mr. E. S. Dana, of New Haven, U.S., publishes the results of his morphological studies of the specimens of *atacamite*, or oxychloride of copper, from the Wallaroo mines, South Australia, now in the Imperial Cabinet at Vienna.

Microscopic mineralogy has come to be pursued almost as actively in this country as in Germany. Mr. Allport, of Birmingham, has recently recorded, in the "Geological Magazine," the occurrence of *Nosean* in the phonolite of the Wolff Rock—a rock which rises from the sea between the western extremity of Cornwall and the Scilly Isles. This is the first time that the mineral species nosean has been discovered in this country. Prof. Hull, of Dublin, has published a paper on the structure of the beautiful dark green porphyritic rock which occurs on Lambay Island, off the coast of Dublin. Nor should we forget a paper of considerable merit which has recently been communicated to the Geological Society by Mr. J. Clifton Ward, of the Geological Survey. In this memoir the author compares the microscopic and mineralogical composition of the modern lavas of Vesuvius with that of some of the old Welsh felstones, and of the eruptive rocks of the Cumberland Lake District. He finds that, as a rule, the Cumbrian rocks are intermediate in chemical and mineralogical composition between the *felstones* on the one hand and the *dolerites* on the other, whence he proposes to term them *felsi-dolerites*. Among foreign papers recently published we may refer to Prof. Möhl's "Mikromineralogische Mittheilungen," in which he describes some German basalts, and certain eruptive rocks from Java, Flores, and Arden; Baronowski's paper on the mineralogical and chemical composition of granite-porphyries; and Kalkowsky's memoir on the augitic felspar-porphyry near Leipzig. Finally, we may observe that a valuable monograph, entitled "Die Krystalliten," has recently been published as a posthumous work of Prof. Vogelsang, under the editorial care of his brother-in-law, Prof. Zirkel, of Leipzig.

In the admirable course of lectures on Crystallography now being delivered in London, by Prof. N. S. Maskelyne, an attempt is made to elucidate the Millerian system, which, to many English mineralogists, seems to require a greater amount of mathematical knowledge than the old and well-known system of Naumann. As we have so few writings upon Prof. Miller's system, except his own, the student may be glad to know that a capital outline of its principles has been published by the Smithsonian Institution in their last Report. The Essay to which we refer is translated from the German of Aristides Brenzina, by Prof. Egleston.

GEOLOGY.

Foreign Geology.—It is interesting to know that a Geological Survey is in progress in Japan. A preliminary report on the first season's work has been issued by the Chief Geologist, Mr. B. S. Lyman, an American Mining Engineer. It relates to the Island of Yesso. Among his assistants, Mr. Lyman speaks favourably of eleven Japanese, who are the first Asiatics to undertake the study and practice of geology. The formations determined consist of a variety of volcanic, Tertiary, crystalline, and other rocks; at present they have been for the most part classified according to certain systems of disturbance. The useful minerals and rocks noted are coal, limestone, ironsand, sulphur, gold, rock-tar, silver, lead, zinc, manganese, and copper.

Dr. Feistmantel has noticed the occurrence of workable coal-beds in Bohemia of Permian age. The lower part of the series contains the coal-seams, but both the upper and lower beds contain plants usually considered typically Carboniferous, *e.g.*, *Stigmaria ficoides*, and species of *Sigillaria*, *Pecopteris*, *Calamites*, &c. The Permian coal-beds are separated from the Carboniferous beds below, by shales with characteristic Permian animal remains. These Bohemian lower Permian beds may be paralleled, according to Dr. Feistmantel, with the beds containing *Archegosaurus* in the Saar and Rhine district, which he also considers as Dyas, and which lie upon the true Saarbrücken coal-measures.

The coal-bearing beds of Sweden, referred to in a recent report by Dr. Erdmann, are of Jurassic age, but the precise horizon to which they belong is not quite settled. Prof. Hébert considers them of the age of the Lias.

Among the fossils are *Amphidesma donaciforme*, *Avicula inæquivalvis*, *Pecopteris*, and Cycads. Prof. Torell notices the affinity of the flora to that of the Yorkshire Jurassic beds, and mentions the occurrence of *Solenites Murrayana*.

The relations between the Cretaceous and Tertiary strata in the United States have been the cause of considerable difference of opinion. The officers of the Geological Survey have lately take up the subject: Profs. Leidy and Cope, through the extinct Vertebrate fauna; Mr. Lesquereux, through the Fossil flora; and Mr. F. B. Meek, through the study of the Invertebrata.

The study of the fauna and flora of these different formations in different districts yields somewhat discrepant results; and there appears to be no alternative but to accept the conclusion that a Tertiary flora was contemporaneous with a Cretaceous fauna, establishing an uninterrupted succession of life across what is generally regarded as one of the greatest breaks in geologic time.

Palæontology.—Mr. L. C. Miall presented to the Geological Section of the British Association at Belfast, a Tabular View of the Classification of the Labyrinthodonta, which was divided into ten sections. Forty-two genera and 126 species are now known. Some of these animals, in their mode of life, appear to have been fish-like. Some resembled serpents, others crocodiles, whilst those of Kilkenny appear to have been salamanders.

Mr. Waterhouse Hawkins has recently expressed his opinion that the *Iguanodon* was a marsupial animal.

Remains of the Lemming have been found in lacustrine brick-earth at Salisbury, associated with bones of the Mammoth.

Prof. Rupert Jones records the occurrence of *Gyrogonites* (fossil seed-vessels of *Chara*) in the London clay of Islington. He also adds *Cythere plicata* to the known fauna of the London clay.

Lythology.—The study of rocks has of late become a popular one, and many important observations have been made.

A detailed examination of the Cumbrian ash-rocks has convinced Mr. J. Clifton Ward that, in many cases, most intense metamorphism had taken place, that the finer ashy material had been partially melted down, and a kind of streaky flow caused around the larger fragments. There was every transition from an ash-rock, in which a bedded or fragmentary structure was clearly visible, to an exceedingly close and flinty felstone-like rock, undistinguishable in hand specimens from a true contemporaneous trap. Such altered rocks were, however, quite distinct in microscopic structure from the undoubted lava-flows of the same district, and often distinct also from the Welsh felstones, although some were almost identical (microscopically) with the highly altered ashes of Wales, and together with them resembled the felstone-lavas of the same country. This metamorphism among the Cumbrian rocks increases in amount as the great granite centres are approached; and it was believed by the author that it took place mainly at the commencement of the Old Red period, when the rocks in question must have been buried many thousands of feet deep beneath the Upper Silurian strata; and when probably the Eskdale granite was formed, perhaps partly by the extreme metamorphism of the volcanic series during upheaval and contortion. The author stated his belief that the Cumbrian volcanoes were mainly subaërial, since some 12,000 feet of ash and lava beds had been accumulated without any admixture of ordinary sedimentary material, except quite at the base, containing scarcely any conglomeratic beds, and being destitute of fossils. He believed also that one of the chief volcanic centres of the district had been the present site of Kenwick, the low craggy hill called Castle Head representing the denuded stamp or plug of an old volcano.

Prof. Hull has described the microscopic structure of a porphyry from the Island of Lambay, a few miles to the north of Dublin Bay. The order in which the different minerals seem to have been formed is as follows:—First, during consolidation, the crystals of orthoclase; next, the crystalline grains of magnetite; and lastly, the felsitic base itself. Then, after consolidation, chlorite, calcite, and pyrites.

Researches upon the thermal conductivity of certain rocks have been made by Prof. A. S. Herschel and Mr. G. A. Lebour. Among these, granite has been found to offer the least resistance to the passage of heat, and coal the greatest. Shale comes next below coal; but between these two and basalt there is a gap of considerable extent. Between basalt and granite come all the other rocks examined, including a number of limestones and sandstones of different varieties.

Physical Geology and Geography.—The Gippsland lakes, as described in a Colonial Report by Messrs. A. J. Skene and R. Brough Smyth, occupy extensive but shallow depressions in a great extent of level Tertiary country, and have been formed by elevation of the land. They are being gradually filled with mud and sediment; and every year, with the advance of settlement, the work of filling up will proceed more rapidly.

Professor Archibald Geikie has pointed out facts which lead him to conclude that the granite of the Isle of Arran is of Tertiary date.

The occurrence of erratics at higher levels than the rock masses from which they have been derived has been discussed by Mr. James Geikie. He points out that stones introduced into the body of a glacier, whether from above or below, tend to rise upwards in the ice, as the glacier flows on its way. This fact appears to him to solve the question; for a glacier travelling 50 or 100 miles may encounter many obstructions in the ground; and, if it surmount these, boulders may be extruded at its surface and stranded on the side of some rocky hill, many hundred feet or yards above the level from which they originally started.

The origin of the Cheddar Cliffs has been briefly treated of by Mr. H. B. Woodward, who advocates their formation by rain and rivers with both chemical and mechanical action.

Mr. John Horne, in a sketch of the geology of the Isle of Man, directs attention to the glacial beds. The Till (he remarks) is in all respects similar to that of Scotland, and is a product of land ice. There is abundant evidence of a strange intermingling of foreign rocks in it, which must have travelled from the coast of Cumberland, the south of Scotland, and the west coast of Ireland.

Sub-Wealden Exploration.—This great work progresses slowly and steadily. The Oxford clay, last reached, was still present at a depth of 1018 feet. Mr. Topley mentions that *Ammonites Jason* was met with at a depth of 990 feet. There does not appear to be the slightest break between the Kimeridge and Oxford clays; the Coral Rag and Calcareous grit being unrepresented, as is also the case near Aylesbury, and in Lincolnshire and Norfolk.

Mr. John Gunn has discussed, on several occasions, the probability of finding coal in the Eastern Counties, and he has recommended that an experimental boring be made at Hunstanton. It will be well, however, to await the results of the Sub-Wealden Exploration before attempting to prove the Palæozoic rocks elsewhere. We learn that at Sperenberg, about twenty-five English miles south of Berlin, a boring has been made by the Government engineers to the extraordinary depth of 4040 feet. This boring was made, after the first 283 feet, in salt-bearing strata; the great depth reached shows us what can be done with energy, and with funds!

Records of Geological Literature.—Mr. Whitaker has done great service to Geology by preparing lists of all papers on Geology, Mineralogy, and Palæontology, referring to certain districts. He has published those relating to London and Hampshire Basins, to Devonshire, Cambridgeshire, and Witshire. It is now proposed to publish, as a yearly volume, a record of all works (in the shape of short abstracts) that relate to Geology, whether British or Foreign. The first volume will be printed by the middle of 1875, and will contain records of papers, books, maps, &c., published during the year 1874.

PHYSICS.

Light.—M. Craveri has devised a new helio-photometer. It consists of a box of hard wood, 280 m.m. long, 145 m.m. wide, and 200 high, forming a parallelopiped placed upon a pedestal in an open situation, where nothing impedes the direct action of the sun. The upper surface of the apparatus cannot preserve, during the twelve months of the year, a horizontal position, because when the sun sinks below the equator in winter its rays would fall too obliquely. It is therefore necessary at that season to follow approximately the movement of the sun. This result is obtained by gradually inclining the instrument towards the south from September to December, and gradually diminishing the inclination again till March, when it is replaced in a horizontal position. One of the principal sides of the parallelogram represents the door, fixed on hinges, and giving access to all the interior. At the side opposite the door is fixed a clock, the dial of which is seen through a circular aperture in the side. To this clock is adapted a toothed wheel, moved by the drum containing the spring. This wheel only performs one revolution in twenty-four hours. To its axle is fixed by a movable screw a large drum of brass, the circumference of which is 520 m.m., and the breadth 16 m.m. Upon the surface of this drum is fixed a slip of paper, as is done with the Morse telegraphs. A few seconds are sufficient for fixing or for removing this band. A slit in the box is so arranged that the sun's rays, shining through it, fall upon the band, even when the luminary is very near the visible horizon. The bands are prepared with chloride of silver by being steeped, first in a solution of common salt, and then, shortly before being used, in solution of nitrate of silver.

The students in the Physical Laboratory of Owens College having occasionally experienced some difficulty in obtaining the spectra of some salts with the ordinary Bunsen, through apparently a deficiency of pressure in the gas, it occurred to Mr. F. Kingdon that the amount of light, even at this deficient temperature, might be increased by multiplying the number of luminous points. This is accomplished by broadening out the flame of the Bunsen; that is, causing the gas to issue through a narrow slit instead of a round hole. So far, only a rough experiment has been made, the slit being about $\frac{7}{8}$in. long and $\frac{1}{8}$in. wide. The result, as expected, was a more brilliant spectrum.

Microscopy.—In the Report of the Geological Survey of New Hampshire, some valuable information respecting the preparation of specimens of Diatomaceæ for examination and study by means of the microscope is contributed by Dr. A. M. Edwards, of Newark, New Jersey, U.S. Respecting the collection of Diatoms, some valuable hints are given. *Arachnoidiscus*, *Triceratium Wilksii*, and *Aulacodiscus Oregonensis*, may be looked upon for logs of wood which have been floating in the sea and imported from New Zealand or Vancouver's Island. So on logs from Mexico and Honduras may be found the curious *Terpsinæ musica*. The nets of fishermen from deep water may yield algæ bearing such forms as *Rhabdomena arcuatum* or *Adriaticum*, *Grammatophora serpentina* and *marina*, various *Synedras*, and other fine forms. On oyster shells may be found algæ bearing upon their fronds *Biddulphia regina*, *Bayleyii* or *aurita*. After a ship is unloaded, and as it floats higher in the water, its sides may be searched for treasures of the Diatom world. The sea-grass, *Zostera marina*, often bears upon its waving ribbons fine forms of Diatoms, and that used for stuffing chairs and imported from abroad will yield foreign species to the collector. The apparatus and materials used are those found in the possession of most well equipped microscopists. The chemicals required are nitric acid, sulphuric acid, hydrochloric acid, bichromate of potash, caustic potash, alcohol, distilled water, and washing soda.

Recent Gatherings.—Sand to be removed by shaking in clean water and pouring off before the diatoms, which are lighter than the sand, settle. After the diatoms have settled as much as possible of the water is to be poured off from the test-tube containing them. They are now covered with nitric acid and allowed to stand a few minutes. Usually some chemical action takes place, and it will be well to wait until it subsides. The tube or beaker is then held over the lamp and carefully heated until the reaction of the acid upon the organic matter of the diatoms ceases. Then, while the liquid is still hot, a few small fragments of bichromate of potash should be dropped in. The organic matter is more thoroughly destroyed in this way than when the acid is used alone. The acid and diatoms should then be poured into a capacious beaker of clean water, washing the test-tube out with a little water and adding this to the other. After the diatoms have settled, the supernatant fluid is carefully poured off, and a fresh supply added; this must be repeated until all the acid and coloured chromium compound has been removed. When this point is arrived at can only be ascertained from experience.

Muds will have to be treated in a somewhat different manner from recent gatherings. If the mud is dry it must be broken down by boiling for a few minutes in a solution of caustic potash, the strength of which must be apportioned to the particular specimen under treatment. After it has been broken down into a soft mud, all the potash is thoroughly washed away by means of clean water, and replaced by nitric acid as in the case of recent gatherings. This is boiled and a little bichromate of potash added as before, and the whole washed. Diatoms occurring in mud are very seldom sufficiently cleaned by this process, so that it it has to be supplemented by another. The sediment is washed into an evaporating dish and allowed to settle, and as much of the water poured off as possible. Then sulphuric sufficient to cover the deposit is poured in and the vessel gradually and carefully heated. As soon as the liquid shows signs of boiling, bichromate of potash is added a very little at a time, until the green colour first formed begins to assume a yellowish tint when no more is dropped in; but a few drops of hydrochloric acid are permitted to fall in, and the liquid is allowed to cool. As soon as the liquid has cooled a little, water should be added cautiously, as great heat will be generated and there will be danger of boiling over. It is then to be poured into a large beaker of water and washed as before. If it be found that the precipitate is not quite white, it will be necessary to boil it again in sulphuric acid, with bichromate of potash and hydrochloric acid until it is quite clean. If upon examination under the microscope much flocculent matter is present besides the diatoms and sand, this can be removed by boiling for a few seconds in a weak solution of caustic potash and washing quickly with plenty of clean water.

Guanos.—The preparation of these substances is rather tedious, difficult, and dirty. The ammoniacal guanos are those which contain most diatoms, that from the islands upon the coast of Peru may be taken as a typical specimen. As it comes into commerce, it is a moist powder of a light iron-rust colour, smelling strongly of ammonia and having scattered through its mass lumps of ammoniacal salts of a more or less solid consistency. The guano should be thinly spread out on a stiff sheet of paper and exposed to the air at a moderate heat for a few days, until the moisture and most of the ammonia have evaporated, and less acid will be required to clean the guano. It will now have become much lighter in colour, and crumble to a dry powder. A tin pan is now about half filled with a solution of common washing soda in clean filtered water and placed over some source of heat, as on a stove. The strength of the solution is not a matter of any great moment and must vary with the guano manipulated. As soon as it begins to boil the guano is dropped in, a little at a time, while the liquid is stirred with a glass rod or stick of wood. Considerable effervescence takes place, ammonia being given off; therefore it must be kept continually stirred, and care exercised to prevent its boiling over. It is then poured into a plentiful supply of clean water and washed several times, care being taken to permit all the diatoms to settle. As soon as the wash-water is only slightly coloured, the guano is transferred to a suitable evaporating dish and covered with nitric acid and boiled. While boiling a few crystals of bichromate of potash are added, and the material treated as in the case of muds. Phosphatic guanos, are somewhat more difficult to treat. They are generally drier than the ammoniacal kind, and must be boiled in a large quantity of hydrochloric acid as many as three times, and the acid must be poured off while still hot. Afterwards, nitric acid and sulphuric acid and bichromate of potash must be employed as in former cases.

Lacustrine Sedimentary Deposits.—For the most part these are pulverulent, and easy to clean. Some, as found in nature, are so pure that they require no cleaning except washing in clean water. Burning on a plate of platinum will often serve to clean some specimens; but it will, in general, be found best to boil in nitric acid with a little bichromate of potash, and subsequently in sulphuric acid and bichromate of potash with the after addition of hydrochloric acid. Occasionally an amount of flocculent matter will be left, which can be removed by very careful heating (not boiling), in a weak solution of caustic potash, and immediately pouring into a large quantity of clean water and thoroughly washing.

Marine Fossil and Sub-Plutonic Deposits, being stony and possessed of very much the same physical characters, are manipulated in the same manner. A small lump of the deposit is placed in a test-tube, and covered with a strong solution of caustic potash. It is then boiled for a few minutes, and usually it immediately begins to break up and fall down in the shape of a soft mud-like material. At once the liquid, with the suspended fine powder, is poured off into a large quantity of clean hot water, and if the whole of the lump has not broken down into a powder, what remains has a little water poured over it in the test-tube, and is again boiled. It will be found that a little more will now crumble off. This is to be added to the rest in the large vessel, and if the lump has not now broken down, it is again boiled in the alkaline solution and in water alternately, until it has all been disintegrated. It is then all permitted to settle for at least three hours, when it is thoroughly washed and boiled in hydrochloric acid for about half-an-hour. There is then added an equal amount of nitric acid, and the boiling continued for a short time. It is then washed and heated in sulphuric acid with the addition of bichromate of potash and hydrochloric acid.

The following preservative fluid is recommended for mounting recent specimens. To one ounce of distilled water add two or three drops of wood creosote and a sufficient quantity of alcohol to make the creosote soluble in the water; this will about equal double the quantity of the creosote. The methods of mounting recommended by Dr. Edwards are those well known to all skilled microscopists.

In a paper on blue and violet stainings for vegetable tissues, Dr. C. Johnson, of Baltimore, gives the following formulæ for a useful stain for this purpose:—Ordinary logwood extract is finely pulverised in a mortar, and about three times its bulk of alum (in powder) added; the two ingredients are well rubbed up together, and mixed with a small quantity of distilled water. The complete admixture of the alum and hæmatoxylin is necessary, and this will require fifteen or twenty minutes vigorous trituration. More water may now be poured on, and the solution, after filtration, should present a clear, somewhat dark violet colour. If a dirty red be obtained more alum must be incorporated and the mixture again filtered. After standing several days add 75 per cent alcohol in the proportion of two drachms to one ounce of the fluid. Should a scum form on the surface of the liquid at any time, a few drops of alcohol and careful filtering will be all that is required. This fluid colours very rapidly, requiring but a few minutes, whereas, if a slower tinting be desired, the fluid may be diluted with a mixture of one part alcohol and three parts water. The sections to be stained having been kept in alcohol, place them in a weak dilution of the fluid and watching the result, transfer the morsel to dilute alcohol for washing, and afterwards to strong alcohol in anticipation of mounting. If it be intended to display the general structure let the tint be decided; but if it be wished to give prominence to the vessels a faint blue only should suggest the other parts. In treating thin leaves or fresh green sections, the colour must first be removed or else staining will be of little service. The bleaching is to be accomplished through the agency of Labarraques's solution of chlorinated soda, in which the objects should be macerated until perfectly colourless and transparent. They should then be immediately transferred to distilled water for an hour or two, and then to a 3 per cent solution of oxalic acid in 50 per cent alcohol, which neutralises the soda and prepares the tissue to take the dye, particularly if aniline blue is employed. The oil of cloves process is to be employed in the final mounting in balsam. The aniline solution preferred by Dr. Johnston, is "Bower's Blue Ink" slightly acidulated with oxalic acid, Dale's soluble blue No. 3 (Robert Dale and Co., Hulme, Manchester) is also recommended. Staining with aniline blue is somewhat more uncertain than that with the hæmatoxylin solution, and the colour of the latter is preferable, especially for observations by artificial light.

The antennæ of the male gnat, *Culex pipio*, have long been favourite objects with microscopists, and when viewed under the binocular and suitably illuminated are scarcely surpassed by any low power object. Some experiments on an allied species, *Culex musquito*, by Alfred M. Mayer, go very far to prove that the beautiful whorls of fibres with which the antennæ are armed serve the purpose of very delicate auditory organs. Observations under the microscope upon living mosquitos show that the delicate setæ respond to the vibrations of tuning-forks, some vibrating to certain notes while the others remain at rest. Further experiments would seem to determine the function of this singular auditory apparatus, namely, to guide the male in the direction of the peculiar humming made by the female. The paper is too long for the quotation in full.

Mr. J. K. Jackson communicates to "Science Gossip" a process for fixing diatoms in devices. The operator is supposed to start with a stock of perfectly clean material, and that a "dip" has been evaporated on a slide. The tool used is a hair from a cow's neck, mounted in a light wooden handle.* The diatom selected is picked with the hair under a 1½-inch power and placed on a prepared thin cover, made as follows:—Take filtered gum tragacanth, and mix five or six drops with one ounce of distilled water. The covers are then cleaned and placed on a rack formed of six pieces of wood, six inches long, a quarter of an inch broad, and one-eighth of an inch thick, made into a "rack" by having pieces of copper wire run through the lot near to each end, so that the bars of the rack slid on the wires may be altered to suit different sizes of covers. Each cover has then a minute drop of the gum placed on its centre with a fine pipette, special precautions being taken to prevent the introduction of dirt. The loaded rack is then transferred to the hot plate and the gum dried as speedily as possible. The covers when dry are stored for use under a well closed glass shade. When required for use, one of the covers is placed on a wooden slide having a hole suitable to the size of the thin glass employed. On this cover the diatoms selected are placed with the cow's hair. When sufficient frustules have been collected to form the device, they are "pushed, coaxed, or driven," into the required form, under an inch and a half objective. When this has been accomplished, after the exercise of much trouble, patience, and dexterity, the cover with its device must be brought close to (just within) the mouth, and breathed upon—one slow, long breath. The cover must then be dried upon a hot plate and turned over on to a drop of balsam and benzol, or damar, in the centre of a slide, and if the glaze on the cover be of the proper thickness, the balsam may be boiled without displacing a single diatom. The writer adds the following hints:—(1). Never hope for one drop of clean water unless you distil it yourself. (2). Place your diatoms "on their backs" in the gum, else they will retain air, which nothing can expel. (3). "Mind your eye," or rather your eyes, as the process is a very trying one to the sight.

Mr. G. J. Burch has communicated to the Quekett Microscopical Club his method of making extremely thin glass covers. Take a piece of glass tube about a quarter of an inch bore, seal up the end with the blowpipe, and continue the heat until the glass is so soft that it will fall out of shape, unless you keep turning it round; remove it from the flame, and blow into it with all your strength. It will be seen to swell, at first slowly and then suddenly, to a large bubble of very thin glass. Supposing the tube to have been sealed up with as little glass as possible, it may be blown out to about four inches diameter. When cold break it up and cut the pieces to shape with a writing diamond. The glass is of course curved, but it may be flattened by being placed on a perfectly flat piece of platinum foil, and depressed for a moment into the Bunsen flame; as soon as it is red hot, it will sink down to the flat foil. This has also the effect of annealing it. A piece of this glass was found upon measurement with the micrometer to be only 0·0004 of an inch.

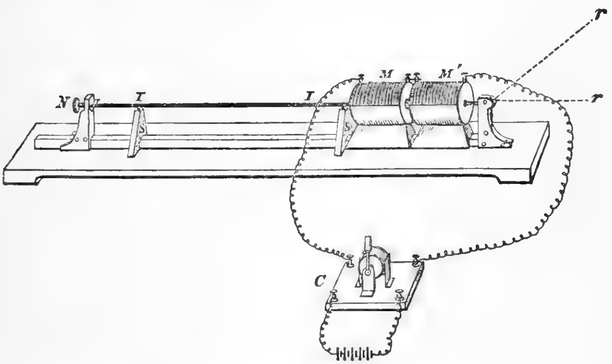
Heat.—Dr. G. Krebs, in a paper on the determination of the freezing-point for delicate thermometers, says:—Schultz, in his treatise on the freezing-point of the water of gaseous solutions and the regelation of ice, shows that the freezing-point of water is lowered by dissolving gases, the change being nearly proportional to the amount of gas dissolved. That water holding solids in solution freezes at a lower point is well known. Thomson and Clausius have shown from the principles of mechanical theory of heat that the freezing-point

* A fine bristle split at the end is preferred by some authorities.

of water falls 0·007° C. for every additional atmosphere of pressure. To determine the true freezing-point, take a glass tube closed at one end, 20 centimetres long and 2 wide, fill it almost full with sulphuric acid, and heat. Then pour out the acid, and rinse repeatedly with pure distilled water. The tube is then two-thirds filled with distilled water, which has been boiled for some time in a clean beaker, and a small quantity of filtered oil of turpentine (about 1 centimetre in depth) is poured upon the water. The tube is then carefully heated in the oil-bath, without allowing the temperature to rise to the boiling-point lest an explosion should ensue. The object of the heating is to remove any air bubbles which may adhere to the side of the glass or may remain between the turpentine and the water. When the water has been thus exposed for a considerable time to a temperature very near to the boiling-point, the tube is taken out of the oil-bath, cooled in cold water, and then placed in a freezing mixture (water and nitrate of ammonia). After a few minutes the water is cold, and in most cases a portion of it freezes at once if a thermometer is inserted, and moved up and down. If this does not take place the tube must be returned to the freezing mixture, and cooled more strongly. The thermometer may be previously placed in an empty test-tube, which is then plunged in the freezing mixture. It is very important that the thermometer should be cooled down close to the freezing-point before being introduced into the water. The best thermometers when tested in this manner show a freezing-point too high by about 0·1° C.

ELECTRICITY.—In a paper read by Prof. W. F. Barrett, F.R.S.E., M.R.I.A., on the "Molecular Changes that accompany the Magnetisation of Iron, Nickel, and Cobalt," at the British Association Meeting, at Bradford, it was shown that certain molecular phenomena known to attend the magnetisation of iron were found to accompany the magnetisation of nickel and cobalt. Notably this was the case with the peculiar sound emitted on magnetising and demagnetising these metals; with cobalt the note was clear and metallic, and louder than in the case of iron. The physical as well as the chemical properties of the three magnetic metals were, in every case, proved to be so closely similar that it was reasonable to expect that any cause producing a molecular change in the one metal would produce a similar change in the other. A series of preliminary experiments established the fact that a change in the dimensions of nickel and cobalt occurred on their magnetisation by an electric current, corresponding to the elongation and retraction of iron investigated by Dr. Joule. To pursue this enquiry further a committee was appointed, consisting of Prof. Balfour Stewart, and subsequently Prof. Clerk Maxwell, in conjunction with the author, and the results were embodied in a report read before Section A

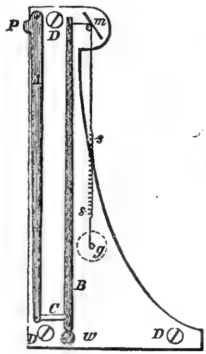
FIG. 1.



of the British Association at Belfast; after the trial of various arrangements an instrument was devised, and has been constructed by Messrs. Yeates, of Dublin and London, by which it is hoped precise determinations may shortly be made

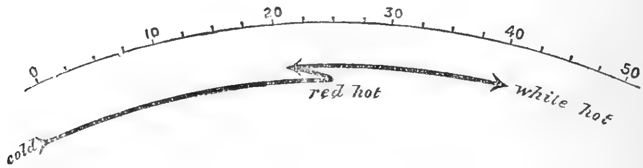
(Figs. 1 and 2). Associated with these molecular changes are others relating to the heating and cooling of wires of the magnetic metals. Mr. Gore discovered that an iron wire, when raised to a white heat, underwent an anomalous expansion at a certain point during its cooling. On repeating Mr. Gore's

FIG. 2.



experiments, the author found a similar anomalous deportment during the *heating* of iron wire,—the wire momentarily contracting when the temperature of a dull red heat was reached, and, as far as the eye could judge, this was also the temperature at which the elongation occurred in cooling (Figs. 3 and 4).

FIG 3.



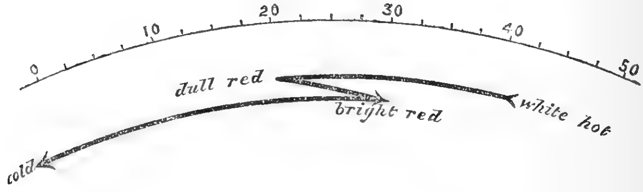
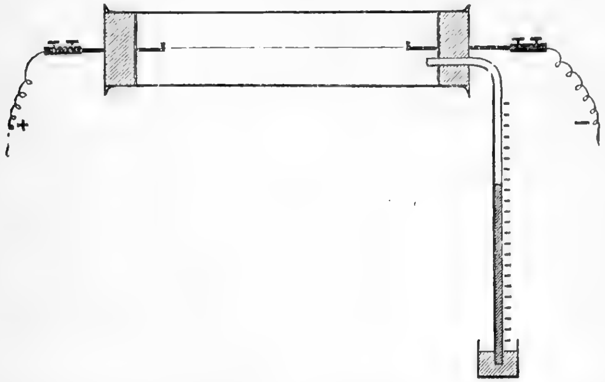


FIG. 4.

Examining the wire during the experiment, in a darkened room, another remarkable phenomenon was revealed, viz., that at the critical point during the *cooling* of the wire a sudden glow diffused itself over the wire, whereby it was raised from a dull to a bright red heat. During the heating of the wire—whether by an electric current or by a row of gas flames—a momentary cessation of heating occurred at the critical point. Enclosing the wire in an air thermometer, these effects were made more evident,—the expansion of the air during the heating of the wire ceasing for a moment, when a dull red heat was attained; and on cooling, the after-glow caused a momentary and violent depression of the liquid index (Fig. 5). A crepitation in the wire was heard at the moment this change took place. Moreover, the critical temperature was

FIG. 5.



approximately that at which iron lost its magnetic character upon heating or regained it on cooling. Further Prof. Tait has shown that at this temperature the thermo-electric condition of iron changes, and that at a lower temperature nickel undergoes a similar change. Hence it is most probable that nickel and cobalt will be found subject to analogous molecular changes at the temperature wherein their transition from the magnetic state takes place,—nickel at a lower temperature than iron, and cobalt at a higher. Experiments have also been made to exclude the effect of oxidation during the heating of the wires. Iron and steel wires have been enclosed in atmospheres of hydrogen, nitrogen, and carbonic acid gas, and in each case similar effects are produced to those already described. In carbonic acid the gas is decomposed around the heated wire, and brilliant scales, apparently of graphite, are deposited on the iron. The foregoing remarks the author begged to be regarded as merely fragmentary observations, preliminary to a more detailed investigation which he hoped shortly to undertake. After Prof. Barrett's paper had been read the following discussion took place.

Prof. CLERK MAXWELL said—In theoretical investigations an assumption is often made that if you make a slight change in the circumstances a slight change will be produced in the result; but this is not always the case in Nature. It seems that in the cooling of iron wire, described by Mr. Barrett, something goes off at a certain moment—something tumbles over at the moment of the kick. Other things tumbled over too,—traps for catching mice, for instance,—and the parts might not have the power of getting up again. If a row of bricks were placed on end, and the first one knocked down, it would knock down the next, and so the whole row would tumble down. When I was a boy we used to call this "Sending Jack for mustard." This mode of the transference of motion was not the same thing as wave motion, because in a wave the particles swing back, but the bricks have to get up again how they can, and won't return to their former position automatically. So in Prof. Barrett's experiment, a critical point is reached where a break down occurs among the molecules. In the process of cooling iron wire something happens which makes a noise, makes the wire lengthen, and make it glow again. A slight change in the circumstances has made a great change in the phenomena. Seeing Mr. Herbert Spencer present reminds me he has written that something of the kind may take place among the nerves, though I do not agree with the assumption he has made.

Prof. Guthrie—Having had the opportunity of seeing Mr. Barrett's experiments, I think Mr. Barrett remarked the re-heating passed along the wire in a definite direction; the glow did not appear immediately all over, but travelled along the wire.

Prof. G. C. Foster—The wire is not likely to rise equally in temperature, or to fall equally, through the whole wire. The ends are fastened to masses which tend to cool them quicker than the central parts of the wire. Is there evidence of anything more than this in the phenomenon described?

Prof. Barrett—It is the fact, as Prof. Guthrie has mentioned, that the "after-glow"—as Prof. Tait has called it—does not appear simultaneously throughout the wire. A wave of heat seems to travel along the wire, and the sudden rise of temperature and measured progress of this wave are very beautiful to observe. I do not think the existence of this wave is due—at any rate not wholly due—to the unequal distribution of heat caused by the masses of metal to which the wire is attached. For, in reply to Professor Foster's question, I may remark that if an iron wire, such as that tying down the cork of a soda-water bottle, be coiled round a pencil, and the helix of wire thus formed heated in some dark corner in a Bunsen gas flame, on the withdrawal of the wire from the flame the after-glow will be readily seen, generally traversing the wire in the manner already described. I am inclined to think the existence of this wave may be due either to an inequality in the diameter of the wire, to a want of homogeneity in its structure, or to the accidental passage of air-currents; for the progress of this wave is sometimes in one direction and sometimes the opposite.

Mr. Herbert Spencer—Is it the fact that the wire having once undergone this transformation, when it passes to a lower temperature, and is again heated, it will undergo a similar change?

Prof. Barrett—This is the case. The after-glow can be made to appear a dozen or more times upon the same wire. But the phenomenon becomes fainter each time. The gradual extinction of the after-glow which I have thus obtained seems to depend very much on the degree of tension to which the wire has been subjected during the heating and cooling; but this is a point I have reserved for future experiments, and cannot therefore speak positively upon it now.

Prof. Everett—Is there a want of symmetry between the sudden change which occurs during the heating and that which occurs during the cooling? Would Prof. Barrett be good enough to draw the temperature curves for both?

Prof. Barrett—I am much obliged to Prof. Everett for his suggestion. The following curve will approximately represent the progressive temperatures of the iron wire during its heating: where A B, in Fig. 6, represents the time

Fig. 6.

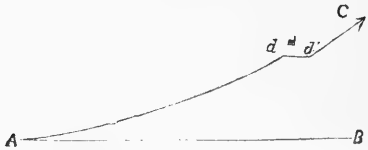
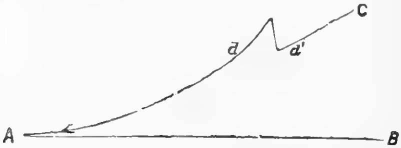


Fig. 7.



the wire is exposed to the source of heat, and A C the increment of temperature, judged by the eye or an air thermometer. A pause in heating, *d d'*, occurs at a dull red heat. On the other hand, the cooling of the wire may be represented as in Fig. 7. Here there are two points, *d d'*, at different intervals of time in cooling, where, nevertheless, the temperature of the wire is the same. The curves, therefore, are not symmetrical. Of course the continued accession of heat in Fig. 6 would mask any small fall in temperature if such occurred. In both cases the anomalous temperature is seen only as the result of a differential action.

After a remark by Professor Clerk Maxwell on the analogy of these phenomena with Mr. Spencer's statement on the propagation of nerve force already referred to,—

Mr Herbert Spencer replied—I rather think that Mr. Barrett's experiments confirm my hypothesis. When the nerve impulse has been expended, I infer that the nerves are reinforced from the neighbouring tissues. They fall in temperature, and instantly absorb heat from adjacent tissues. As a verification of this inference, I may mention we have evidence that the velocity of nervous transmission is variable according to the temperature of the body—the current being swifter in summer than in winter, which corresponds with the fact that the "personal equation" is a variable element in different individuals, and may be presumed to vary in consequence of their slight difference of temperature.

Professor Maxwell—I attribute to Mr. Spencer the view that the nerve recovers its power of acting by absorbing heat. The people who read such a statement might attribute to him the notion that the nerve acts as a heat-engine. It may be that when the nerve is raised to a higher temperature, some chemical action may be able to take place which, though it is a lowering of the available energy of the system on the whole, may make it more available for giving another impulse—another wave of nerve force—but at the same time, after all is done, the nerve is worse off than before, so that it has to receive from the blood something to bring it up to its former state. The two sides of the interpretation are these: We all admit that nerves won't go on without food, but it may be possible that some action, depending upon the absorption of heat from the surrounding tissues, may make the nerve more instantaneously available. But those who believe in thermo-dynamics, and the dissipation of energy, cannot admit that any purely material system can convert heat into work when the system and its whole environment are, at the commencement of the operation, at the same temperature. There is a statement in Sir W. Grove's address on continuity, in which he mentions that Berthelot had discovered that a certain salt could undergo chemical change in which its energy was increased by absorption of heat from the surrounding medium, at a temperature no higher than its own. That statement has not been attacked; but, though it supports Mr. Spencer's view, it cannot be understood literally by anyone who believes in Carnot's law, which expresses under what conditions heat can do work in passing from one thing to another.

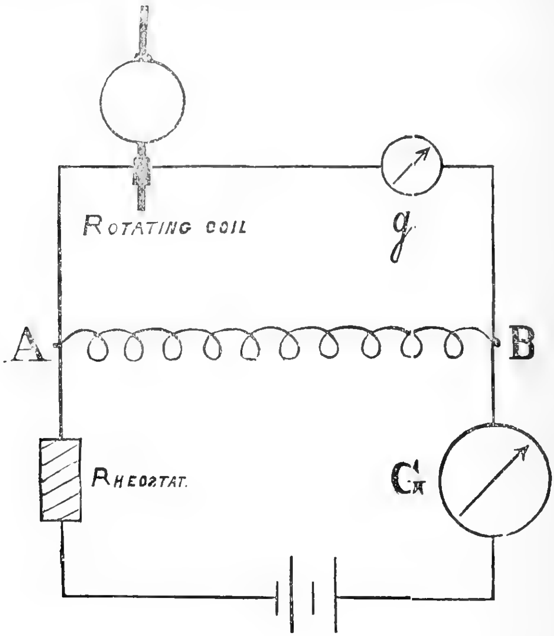
Mr. Herbert Spencer—I by no means assume that there is any genesis of force made possible in a nerve by any such absorption of heat. Although not very familiar with thermo-dynamics, I am sufficiently so to be aware that there is no possibility of such an evolution of force in the nerve except at the expense of the body at large. I am merely endeavouring to show that we have in the heat of the tissues surrounding the nerve a reserve of force ever present, which may be transformed into the force that traverses a nerve simply by the instrumentality of the change in the isomeric forms of the molecules of nerve; which change is a change of a kind permitting them, each time they absorb heat, to assume a less stable form, and therefore again to fall into the more stable form. It is a perfectly reasonable hypothesis that a nerve may be composed of material which, like all the other forms of protein, admits of extremely easy change from one isomeric state to another. If it is supposed that by the absorption of a small amount of heat, the molecules of the nerve assume such a relation, that by a slight disturbance they will again resume their more stable state, and send a cumulative wave of disturbance—like Professor Maxwell's line of bricks:—I say it is a feasible assumption that when they have done that, they have lost heat to the extent that they have given out that wave of motion, and are in a condition to absorb heat from the tissues. That the ability of the nerve to perform its function depends on absorption of heat from the surrounding tissues is a supposition confirmed by the fact that, if you reduce the temperature greatly, the nerve ceases to act.

Professor Clerk Maxwell—No doubt there is plenty of power in the heat of the body to renovate a nerve. But as heat can do work only by passing from a hotter body to a colder, "all the king's horses and all the king's men" could not renovate the nerve by the aid of heat alone.

At the late British Association Meeting at Belfast an important communication "On the Absolute Electro-Magnetic Units of Resistance and Electro-Motive Force, with Suggestions for their Re-determination," was made to Section A by Prof. G. Carey Foster. The method consists in determining the ratio of the electromotive force to the strength of the current produced by it in a given conductor; and then, since—

$$\frac{E}{C} = R,$$

the resistance of the conductor is determined. For this purpose a large coil of covered copper wire, like a hoop placed vertically, is made to rotate about a vertical axis, so as to have a certain electromotive force E generated in it by



the horizontal component of the earth's magnetic force. During each half-revolution beginning and ending with the plane of the coil at right angles to the magnetic meridian, the mean value of E is—

$$\frac{2a\,H,}{t}$$

where a is the effective area of the coil, t its time of revolution, and H the earth's horizontal intensity; but during successive half-revolutions the sign of

E is alternately positive and negative. The ends of the wire of the coil are connected with a commutator on its axis (as in an ordinary Delezenne's circle), which causes the current in all external portions of the circuit to flow in only one direction. This being arranged, all that would be wanted, in order to know the resistance of the wire of the coil, would be to measure the strength of this current, and calculate the electromotive force by observing the speed of rotation. But it is plain that the resistance so calculated would include the resistance of the commutator, which is variable and wholly irregular, so some modification is necessary which shall entirely eliminate this source of error. Prof. Foster manages this by applying Poggendorff's method of measuring E.M.F.—that is, by stopping all current in the coil with a branch of an auxiliary stronger current. A constant battery (shown in the diagram) has its circuit completed through a Siemens unit, a standard coil, or other conveniently-mounted resistance, A B, which it is desirable to know in absolute measure. A certain difference of potential exists between the ends of the wire A B, and this can readily be altered by changing the resistance of other parts of the circuit till it is precisely equal to the electromotive force generated in the coil when revolving at some constant rate. There is a delicate Thomson's galvanometer, *g*, in the coil circuit, which shows when the two differences of potential are equal by the absence of all current through it; and when this point has been attained, the battery circuit is totally unaffected by its connection with the coil. The strength of the current in A B is now to be very carefully measured in absolute measure by an accurate absolute tangent-galvanometer, G; this current is produced by an electromotive force E (which we have arranged to be equal to the difference of potential between the ends A and B) acting against the resistance R of the wire A B; but the electromotive force of the coil is also E, and therefore it could produce exactly the same strength of current against the same resistance R. Thus the strength of the current which the calculated electromotive force E of the coil could produce against the required resistance R is measured when there is no current at all circulating in the coil. The absolute resistance of the wire A B is now known, for—

$$R = \frac{E}{C} = \frac{\frac{2aH}{t}}{kH\tan\delta} = \frac{2a}{kt\tan\delta};$$

k is the constant depending on the particular tangent-galvanometer, and has of course been previously determined. The horizontal intensity of the earth is eliminated from the result, as it appears both in E and C; and this is important, since it is a thing very difficult to determine accurately, and is liable to local disturbances. The advantages of the whole method which are peculiar to itself are—first, that the wire whose resistance is to be measured may be of the most convenient form and material for accuracy and constancy, and any wire whatever may be measured just as easily as any other; second, that the strength of the current is not measured by some arrangement inside the revolving coil, but by an independent accurate tangent-galvanometer, whose sensitiveness may be so adjusted that the needle may be deflected about 45°; and, further, that by applying Bosscha's modification of Poggendorff's method (that is, after measuring as before, add a certain resistance to the wire A B, and then compensate it by a known addition to the external circuit, and so on) the E.M.F. of any cell may be compared directly with that of the coil, and so found in absolute measure. In the discussion which followed, Prof. Guthrie and the President (Prof. Jellett) both doubted whether the irregularities in the resistance of the commutator were wholly got rid of by the method. Prof. Foster explained that, as when the measurement is to be made there is no current in the coil, a change of resistance can have no effect. If the circuit were properly balanced, the needle of the galvanoscope would remain at rest, whether the resistance of the commutator were 0 or ∞; the only thing is that if the resistance of the commutator were infinite, the coil would be disconnected, and there would be no experiment; so the effect of an increase of resistance at the commutator is to diminish somewhat the *sensitiveness* of the method, but it has no effect on anything else. Prof. Clerk Maxwell said that the method seemed capable of furnishing very good results, and was well worth putting into operation. He said, also, that the change of resistance at the commutator was entirely eliminated, but that the change of E.M.F., due to heat at the contacts, was not; however, Prof. Lorenz had used a revolving commutator and had not been much troubled by this effect, and he got rid of what little there was by reversing the current in different experiments. The commutator must be accurately made, so as to cut off the current a definite proportion of each revolution. Prof. Wiedemann said it would be important to measure the area of the revolving coil accurately, and this, he thought, might be found the most difficult part of the experiments. Prof. Maxwell then explained a contrivance he had devised for ascertaining the dimensions of large coils, by letting a wheel (on whose axle a string could wind itself) roll on the wire as it was wound on, and afterwards running this wheel along a scale till the string was unwound. The length of the wire and number of turns is thus known, and from these the effective area of the coil is easily found. Prof. Maxwell further suggested that the commutator should only make contact for a very short time in each revolution, as this would do away with any self-induction (or extra currents) which might take place if contact were made long enough for the current to vary much in intensity.

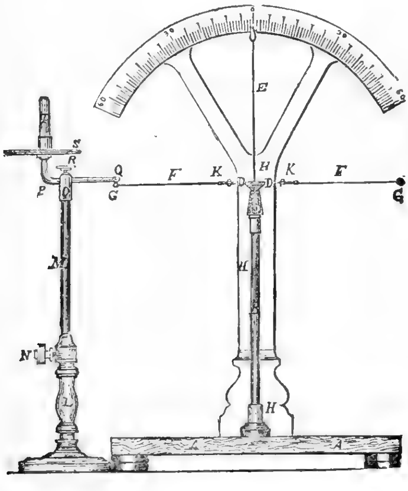
Mr. S. M. Yeates, the excellent philosophical instrument maker, in Dublin, has invented a most important modification of the step-by-step telegraph. In all alphabetical telegraphs hitherto made it is essential that the operator should always move the handle round in one forward direction, and therefore, if a letter be required which precedes the one last indicated, it is necessary to carry the pointer all the way round the dial in order to indicate the letter required; if, for instance, the word "day" be transmitted, the operator moves the index from zero to D, passing over A, B, and C; having thus passed A, it becomes necessary to go all round the dial to arrive at A, and having passed Y must again go round the whole alphabet before the word is completed. As each letter passed by the transmitting instrument sends a current which actuates the receiving instrument, and makes its index perform the same motion, it follows that, in transmitting this little word "day," there are no less than 49 currents sent along the line, and, as any one of such currents may miss, there are 49 chances of the instrument "tripping," as it is called. In Mr. Yeates's new instrument both the transmitting and receiving instruments work backwards or forward indifferently, and therefore—in the case supposed of the word "day"—the operator, having moved the index to D, has only to move it back to A, and then back to Y, thus transmitting the word with only eleven contacts, instead of forty-nine as in the other cases. The great advantage gained by this will at once be seen:—First, it greatly increases the speed of the instrument; and secondly, it greatly lessens the chances of tripping.

M. Gaiffe has devised apparatus used to light the chandeliers of the Paris National Assembly Hall. The hall is lighted by means of chandeliers containing 256 burners. To each chandelier a wire conducts the electric charge, but the return currents traverse a single cable to which all the wires converge. Each wire at the battery end—or rather induction coil end—starts from a separate insulated metallic button, which are put in communication with the secondary wire of the coil by means of an excitor terminated by a ball, and attached to the coil by a chain; the other end of the coil's secondary wire is is in connection with the return cable. The wire starts from the button and terminates at the burner; it resumes its course onward to the return cable, the circuit being complete, with the exception of a small breakage at each burner. To light the hall, the coil is first worked; then the gas is turned on, and the excitor applied to the several buttons causes sparks to be emitted at each burner, and thus enflames the escaping gas.

M. Charles Vavin has invented a magneto-mechanical sorter. Its object is to mechanically separate iron chippings and dust from the copper found mixed with the detritus and filings of factories. This sorting is ordinarily accomplished by hand. It takes up much time, and injures the workmens' health, on account of stooping all day over powdered materials containing copper. M. Vavin's apparatus consists of two hollow superposed cylinders revolving in the same direction, upon which the material to be sorted is spread by a hopper. The surface of these cylinders is formed by soft iron bands maintained in a continuous magnetic condition by magnets. To the surface of these cylinders then the iron particles attach themselves, and at a certain time they are detached by revolving brushes, and thrown into a side box, whilst the copper and earthy particles fall to the bottom of the apparatus. M. Mangon says he has used this apparatus to search for titanous iron in arable lands with such successful results as could never have been attained by any other means—chemical or otherwise.

The following interesting discovery, which is likely to be of considerable practical importance in telegraphy, was communicated by Mr. Edison to a recent number of the "Scientific American." The salient feature in Mr. Edison's discovery is the production of motion and of sound by the style of the Bain telegraphic instrument, without the intervention of a magnet and armature. By the motion thus produced, he works any of the ordinary forms of telegraph printing or sounding instruments, or relays, and is enabled to send messages, by direct tranmission over thousands of miles of wire, at the highest speed, without re-writing, delay, or difficulty of any kind. More than this, his apparatus operates in a highly effective manner, under the weakest electric currents, and he is able to receive and transmit messages by currents so weak that the ordinary magnetic instruments fail to operate, or even give an indication of the passage of electricity. Thus, when the common instruments stand still, owing to feebleness of current, the Edison telegraph will be at full work. The author has named his discovery the electro-motograph. The instrument attracted considerable notice at the recent *soiree* of the Society of Telegraph Engineers.

Dr. Ph. Carl has arranged a new tangent-electrometer for lectures, which unites very great sensibility with the advantage that the electric phenomena can be recognised at a great distance, and can be adapted for the execution of absolute measurements. The instrument is represented in the figure one-



twelfth its size. Upon a wooden support A A, stands an insulating glass rod, B, which supports a brass fork, C, provided above with steel bearings. In these lies the knife-edge of a short brass balance-beam, with which is connected a long index E. In the ends of the arms of the balance fine glass tubes (such as are used for vaccine lymph) are cemented, F F, covered with shellac varnish, and bearing at their ends the elder-pith balls, G G. Behind this balance there stands upon the same footboard, A A, a wooden support, H H, with the scale, J J. On the balance-arms is cut a screw-thread, by means of which the weights, K K—made in the form of female screws—can be set so as to bring the index E to the zero point of the scale. Below the balance-beam there is a weight, Z, to regulate the distance from the centre of gravity to the fulcrum. It is concealed by the fork C, and is therefore represented in the figure by a dotted line. If vulcanite is very slightly rubbed upon fur, and approached to the elder balls, G G, even at a considerable distance, the maximum indication of E is obtained. The electrometer can be combined, in a very simple manner, with a condenser. In the wooded foot, L, the perpendicular glass rod, M, can be raised or lowered, and fixed at any height desired by means of the screw, N. The rod M terminates above in a vulcanite support, O, through which passes the brass wire, P, bent at right angle, and bearing at one end the small brass ball, Q, and at the other the horizontal copper disc, R, upon which the condenser plate, S, can be placed by means of the insulating glass handle, T. The plates R and S can be unscrewed and replaced by others, as may be requisite. In cases where no condenser is required the plate R may be removed, and a simple brass ball fixed in its stead. The apparatus is not merely an electroscope, but an electrometer. If the disc R is replaced by a brass ball connected with any source of electricity, the ball G will be repelled to an angle a, and the magnitude of the repelling force is then proportional to the tangent of the angle of deviation.

LIST OF PUBLICATIONS AND PERIODICALS RECEIVED FOR REVIEW.

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A Treatise upon Railway Signals and Accidents. By Archibald D. Dawnay, A.I.C.E. *E. and F. N. Spon.*

Synopsis of the Flora of Colorado. By Thomas C. Porter and John M. Coulter. *Trubner and Co.*

Results of Astronomical Observations made at the Melbourne Observatory in the Years 1869 and 1870, under the Direction of Robert L. J. Ellery. *Melbourne: Mason, Frith, and M'Cutcheon.*

An Experimental Enquiry into the Nutrition of Animal Tissues. By William Marcet, M.D., F.R.S. *Longmans and Co.*

The Transit of Venus. By George Forbes, B.A. *Macmillan and Co.*

Pharmacographia—a History of the Principal Drugs of Vegetable Origin met with in Great Britain and British India. By F. A. Flückiger, Ph.D., and Daniel Hanbury, F.R.S. *Macmillan and Co.*

Sun and Earth as Great Forces in Chemistry. By T. W. Hall, M.D. *Trubner and Co.*

Reliquiæ Aquitanicæ. Part XV. Edited by T. Rupert Jones, F.R.S. *Williams and Norgate.*

Brinkley's Astronomy. By J. W. Stubbs, D.D., and F. Brunnow, Ph.D. *Longmans and Co.*

The Protoplasmic Theory of Life. By John Drysdale, M.D., &c. *Bailliere, Tyndall, and Cox.*

Patents and Patentees. Vol. vii. Indexes for the year 1872. By W. H. Archer, Registrar General of Victoria. *Melbourne: John Ferres.*

Insects Abroad; being a Popular Account of Foreign Insects, their Structure, Habits, and Transformations. By the Rev. J. G. Wood, M.A., F.L.S. *Longmans and Co.*

Polarisation of Light. By William Spottiswoode, M.A., LL.D., F.R.S., &c. *Macmillan and Co.*

The Elements of Embryology. By M. Foster, M.A., LL.D., F.R.S., and F. M. Balfour, B.A. *Macmillan and Co.*

Popular Treatise on the Patent Laws. By John Brown. *E. and F. N. Spon.*

The Elements of the Psychology of Cognition. By Robert Jardine, B.D., D.Sc. *Macmillan and Co.*

An Elementary Exposition of the Doctrine of Energy. By D. D. Heath, M.A. *Longmans and Co.*

The Aërial World: a Popular Account of the Phenomena and Life of the Atmosphere. By G. Hartwig, M.D. *Longmans and Co.*

Transits of Venus: a Popular Account of Past and Coming Transits. By R. A. Proctor, B.A., &c. *Longmans and Co.*

Economic Geology, or Geology in its Relation to the Arts and Manufactures. By David Page, LL.D., F.G.S. *W. Blackwood and Sons.*

Evolution and the Origin of Life. By H. Charlton Bastian, M.A., M.D., F.R.S. *Macmillan and Co.*

The Doctrine of Descent and Darwinism. By Oscar Schmidt. *Henry S. King and Co.*

Elements of Animal Physiology. By John Angell. Enlarged Edition. *William Collins, Sons, and Co.*

Principles of Metal Mining. By J. H. Collins, F.G.S. *William Collins, Sons, and Co.*

Elements of Magnetism and Electricity. By John Angell. *William Collins, Sons, and Co.*

The Methods of Ethics. By Henry Sidgwick, M.A. *Macmillan and Co.*

Heredity: a Psychological Study of its Phenomena, Laws, Causes, and Consequences. By Th. Ribot. *Henry S. King and Co.*

The Origin of Civilisation and the Primitive Condition of Man. By Sir John Lubbock, Bart., M.P., F.R.S. *Longmans and Co.*

PERIODICALS.

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Naval Science.

The Popular Science Review.

The Geological Magazine.

The American Chemist.

The Westminster Review.

PROCEEDINGS OF LEARNED SOCIETIES, &c.

Monthly Notices of the Royal Astronomical Society.

Monthly Microscopical Journal.

Proceedings of the Royal Society.

Transactions of the Manchester Geological Society, Vol. xiii., Part V.

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I. NIAGARA.

Glacial and Post-Glacial Phenomena.

By Thomas Belt, F.G.S.

THE glacial phenomena of the district of Niagara have been so often described, and the cause of, and the time occupied in, the excavation of the river gorge, so often discussed, that I did not expect, when, on Christmas-day, I made my first visit to the great falls, to have anything new to record, but went quite prepared to acquiesce in the conclusion that has been received for more than thirty years, that the whole of the gorge, from Queenstown to the falls, has been excavated since the glacial period. Since this theory was first advanced, many geologists have visited the district, and, so far as I can learn, no one has called in question this verdict ; it has been accepted as an established fact, and various calculations of the time necessary to excavate the gorge have been made, throwing back the occurrence of the glacial period from 30,000 to 300,000 years ago.

It was with great surprise, therefore, that I found, that at first sight this conclusion was not evident, and that, on further examination, it was not tenable. I feel that in having to oppose the theory that the gorge of Niagara has been excavated since the glacial period, I shall be adding another scientific heresy to the many that are recorded against me; but the heresies of to-day are the truths of to-morrow, and I shall at least give my reasons for believing that my explanation of the problem ought to be classed in the latter, and not in the former category.

The question of the excavation of the gorge cannot be clearly understood without some knowledge of the glacial deposits, and I shall in the first place describe the glacial, and afterwards the post-glacial phenomena. So many authors have written on the subject, that I shall only mention those from whose works I have obtained information of importance. Foremost in the list stand the names of Sir Charles Lyell and Professor James Hall, who visited the district together in 1841, and who afterwards published the conclusions, that they appear to have arrived at together. Sir Charles Lyell, in the "Proceedings of the Geological Society of London" for 1842 and 1843, and more fully in his "Travels in North America," where there is an excellent coloured bird's-eye view of the falls of Niagara and adjacent country, and also a geological map of the district, in which the reader who has not visited Niagara may correct the false impression he is likely to obtain, from the necessary foreshortening in the bird's-eye view, of the small distance between the falls and the whirlpool, which are, in reality, four miles apart. Professor James Hall published nearly identical opinions in the "Boston Journal of Natural History" for 1843-44, and more fully in the "Geology of New York," Part IV., in 1843. The latter work contains not only a bird's-eye view of the district, but an excellent map of the falls, constructed from a trigonometrical survey made in 1841, by Mr. Bakewell: afterwards in 1842, corrected by Professor Hall and two engineers. The whole of Professor Hall's observations on the glacial phenomena of the State of New York should be read by those interested in the study of the glacial period. They abound in original remarks, and in clear descriptions of the succession of the superficial deposits, and many of the conclusions at which this eminent state geologist arrived more than thirty years ago are only now receiving in England the attention they deserve. Professor Hall also describes other rivers running into Lake Ontario from the south, which, like Niagara, have had their pre-glacial channels filled up, and have since taken a more westerly course to the lake.

In 1859, Professor Ramsay published, in the "Quarterly Journal of the Geological Society," his observations on the glacial phenomena of Canada, made during a trip to that country in the preceding year. In this memoir he pointed out, I believe for the first time, that the river must have commenced to cut back the gorge at Queenstown, before the close of the glacial period.

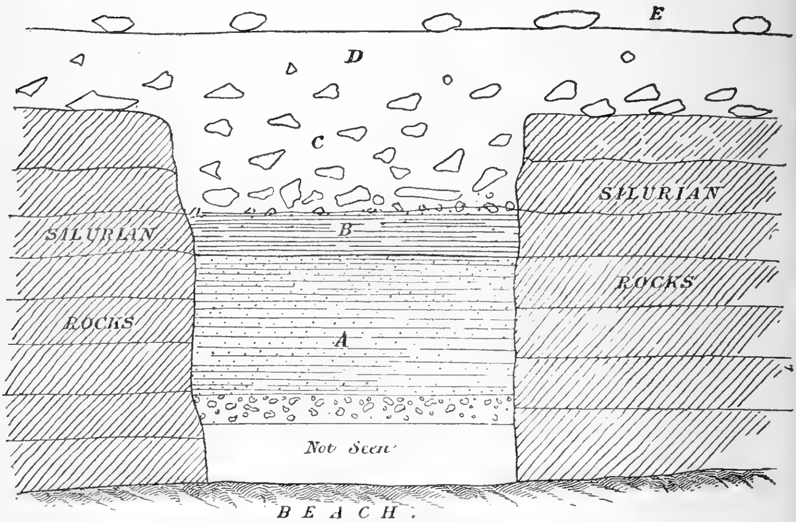
To Dr. Newberry, the accomplished chief of the Geological Survey of Ohio, I am greatly indebted, not only for much personal kindness and assistance, but for an early copy of his "Surface Geology," to be published in the forthcoming volume on the "Geology of Ohio," from which I have obtained a vast amount of information respecting the glacial deposits of the district of the great lakes. A very large amount of information is contained in the well-known works of Prof. Dana, not only in his admirable "Manual of Geology," but in various memoirs, amongst which I may especially mention his "Geology of the Newhaven Region," published in the "Transactions of the Connecticut Academy" in 1870. I may mention, that with the exception of Dr. Dawson, of Montreal, the whole of the most eminent of the geologists of eastern North America are now agreed that the principal glaciation of America was effected by land-ice, though there is abundant proofs, as I shall have occasion to show in this paper, that at a later stage, boulders were scattered over the country by floating icebergs. That later stage of floating ice was due, however, I contend, both in America and Europe, not to a submergence of the land below the ocean, but to the production of immense lakes of fresh water, by the damming up of the drainage of the continents by ice that flowed principally down the ocean depressions. In this conclusion, I have as yet no supporters amongst the geologists, either of America or Europe, if, indeed, I may not except Professor Hall, who informed me, in conversation, and authorised me to publish his opinion that the sea has never encroached on south-eastern New England since the deposition of the "*till*," and that the terraces of the Hudson and Connecticut were produced by the blockage of their waters by ice that flowed down the ocean bed, and of the presence of which we have proof in the immense moraines that compose the whole of Cape Cod.

Glacial Phenomena.

The rocks through which the gorge of Niagara is cut are limestones, sandstones, and shales. These rocks are all rounded and smoothed, and the limestones are frequently scratched and grooved. Besides the coarser ice marking, the rounded and smoothed surfaces of rock, when examined closely, exhibit innumerable fine scratches, which have been ascribed by Hall to small particles of sand imbedded in the ice that moulded the rocks, and he has shown the improbability that this moulding and fine scratching, which is universal over the whole northern part of the State of New York, wherever the rocks are of sufficient hardness to receive and retain striæ, could have been effected by icebergs. Lying on these glaciated rocks are superficial deposits of drift, containing beds of unstratified clay, with boulders, sands, and loam. These are spread over the whole district like a mantle, so that natural exposures of the bed rock are rare, excepting in the gorges cut by the river.

The cliffs bordering the gorge, from the falls to Queenstown, are everywhere capped by these deposits; one of the most interesting and instructive sections of which is exposed at the whirlpool, four miles below the falls, at the end of the filled-up pre-glacial gorge that runs down to St. David's. The following section exhibits the succession of deposits that fill the old gorge:—

Fig. 1.



Section through the old gorge at the whirlpool, along the line A B in Plan, Fig. 3.

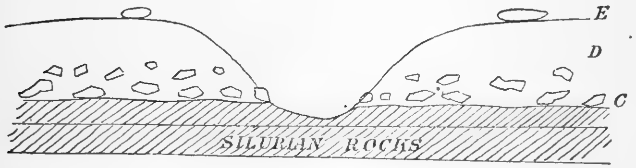
The lowest bed seen by me at the section is that marked A, which consists of clean yellow river sand, with occasional seams and rolled lumps of clay. Below these sands there were exposed, when Lyell described the beds,* strata of pebbles, cemented together by carbonate of lime, overlying laminated clays. I saw one large mass of the pebbly conglomerates lying on the beach, and have shown its position in the section underlying A, but I have not inserted the laminated beds mentioned by Lyell, as some that I saw low down in the gorge had evidently slipped down from A and B, the whole face of the unconsolidated materials filling the gorge, showing many slips produced by rain and frost.

The bed of laminated sands (A in section) graduates upwards into fine laminated silt (B in section), the powder of

* Travels in North America, vol. ii. p. 95.

which is almost impalpable when rubbed between the fingers. Higher up this silt is unlaminated, and contains a few small angular stones. It gradually changes upwards into C, which is a true "*till*," or "*grund morane*," containing large angular and subangular stones, many of great size, scratched and grooved. All the blocks imbedded in the clay are of the local limestones. A few rounded boulders, of northern origin, lie on the surface slope, but they have evidently rolled down from above. The higher part of C contains fewer and fewer stones, until it merges into D, which is composed of unstratified clay or "*till*," without stones. Of this there is from twenty to thirty feet in thickness, the upper part being more sandy than the lower, and sometimes obscurely stratified. On the surface are a few rounded boulders of granite or gneiss, all far travelled from the north, and it is noticeable, that whilst the angular blocks in the "till" are all of local origin, those lying on the surface are almost, if not quite, always of distant derivation, and are invariably rounded or subangular. Those seen on the surface, near the whirlpool, were all of granite or gneiss. The continuation of the till (C and D in section) is shown in some small valleys that run into the gorge at the whirlpool.

Fig 2.



Section of small valley running down to the gorge at the whirlpool. C, till with stones; D, till without stones; E, rounded boulders of northern origin on surface.

Here the till (C and D in section) lies upon the rounded and smoothed surface of the limestone. Some of the blocks of limestone at this point are of great size; one I measured was 9 feet by 6 feet, its thickness not seen, as it was half buried in the ground. On the opposite side of the river, and about a mile distant from it, I saw exactly the same succession of beds exposed in the cutting of the railway, half way between the railway bridge and Lewiston. The northern end of the old filled-up gorge at St. David's shows a similar succession of beds, with the addition, that on mounting the plateau from the lower one of the lake of Ontario, I found, exposed in railway cuttings, that the till without stones was capped by stratified beds of clay and sand, with a few lines of small pebbles.

The above sections may be taken as typical ones of the superficial beds that mantle the whole of the northern part of the States of New York and Ohio, and much of Canada, and I proceed to show that they are exactly what would be produced by the accumulation of a great mass of ice in the north, that gradually progressed southward, and that afterwards melted back again as gradually as it had advanced.

Let us carry ourselves, in imagination, back to the preglacial times, when the old river ran through the filled-up gorge from the whirlpool to St. David's, and try to follow the successive steps by which it was filled up, and ultimately completely obliterated, or rather concealed. Let us bear in mind that the Niagara runs northward, in the direction from whence the ice came. Hall, Dana, Newberry, Lyell, and Ramsay, have all pointed out both from the scratchings of the rocks and from the transported blocks in the till, that the movement of the ice was from the north. It has also been clearly shown that the ice flowed up the St. Lawrence valley, from the north-east. It advanced up the slope of that great valley principally by the overflow of the higher parts of the ice over the lower. That there was some movement of the lower part of the ice from the pressure from the north is, however, sufficiently proved by the different formations that crop out from east to west, having furnished stones to the till that covers the rocks immediately to the south of them. Thus, according to Hall, huge blocks of Medina sandstone are moved southward unto the top of the Niagara limestone. In like manner, numerous masses of the Niagara limestone are drifted forward unto the Onondago salt group, and still further south, on the Chemung limestone, lie great numbers of immense blocks from the Onondago salt group to the north. The size of these fragments bears a proportion to the distance they have been transported from the parent block, the largest being nearest to it. This is characteristic only of the till, and not to the northern boulders that are strewn over the surface, and which have not been transported from their distant northern homes by land ice.

The immediate effect of the ice, as soon as it had dammed up the mouth of the valley of the St. Lawrence, must have been to form a great fresh water lake in front of it, on which it was constantly advancing. When, after filling the basin of the Lake of Ontario, it had, in its progress south-westward, reached the base of the cliffs of the Queenstown escarpment, or had dammed back the water to that level, the commencement of the filling up of the old Niagara gorge was at hand, and from that time, during the advance of the ice and its subsequent retreat, the deposits of sand, till, and boulders shown in the sections were made. The first step was the partial arrest of the flow of the old river, causing it to deposit at a higher level than its original bed, first pebbles forming the conglomerate at the base of the section in fig. 1, and then the thick bed of river sand, when the current was still more impeded. The bed of fine silt (B in fig. 1) marks the time when the flow of water to the north-east was completely stopped. Dr. Newberry, several years ago, first drew attention to the fact, that at some time during the glacial period, all the great lakes of North America drained towards the Ohio and the Mississippi, and since then, several deep channels by which they did so have been described.

I did not see this fine bed of silt in any of the other sections I examined, and I think its preservation in the one at the whirlpool must have been entirely due to the protection the nearly perpendicular walls of the gorge afforded against the great pressure of the ice that passed over it. Its upper portion contains small angular stones, and it gradually merges into the unstratified till, containing large angular blocks.

It is probable that, during the advance of the ice, no till or *grund morane* was formed below it, but that the smoothing and scratching of the surfaces of the solid rocks were then effected, and that the till was deposited beneath the ice when it was melting back, and its pressure being gradually lessened. Mr. Bonney has objected to the theory of both the erosion of rock surfaces and the deposition of till having taken place below the ice.* But the two actions belong to different times; the one was accomplished during the advance, the other during the retreat of the ice. The effects are similar to those of a mountain torrent, which, when full, carries all before it, but which, when its waters lessen, deposits stones and mud in its course. During the advance of the ice, there could be little deposition below it, all the stones held at the bottom of the moving mass being probably ground to powder; but, as it melted back, the stones and clay held within it would be deposited at its foot.

* Nature, vol. x., p. 85.

Dana, in an excellent paper on the Glacial Era in New England, has ably argued this question, and has shown the enormous power that moving ice, 6000 feet thick, with a pressure of at least 300,000 pounds to the square foot, would have in abrading the rock surfaces below it, and carrying forward in its lower part the loose material it had broken off or caught up from the rocks below, and how the whole of this would be deposited at the melting of the ice.* It would greatly conduce to clear descriptions of glacial phenomena, if the old term "till" were confined to this deposit. It is the "Erie clay" of Dr. Newberry, the "Lower Boulder clay" of Wood, the "*grund morane*" and the "*moraine profonde*" of others. "Erie clay" is a local name, and includes stratified beds of a different origin. "Boulder clay" is often a misnomer, as frequently this clay contains no boulders. "*Grund morane*" and "*moraine profonde*" indicate a particular mode of origin, which, though probably correct, is still theoretical. "Till" is an old English word, long applied to this deposit, and may be used by every one, whatever theory of origin they may favour. I suggest, therefore, that it should be confined to designate the unstratified clay with angular blocks, generally of local origin, that lies at the bottom of all the glacial beds, and that the term "boulder clay" should be applied to the higher beds, which show the action of water as well as of ice. The term "drift" might be applied to any glacial deposit the nature and origin of which is doubtful, in the same way as the name "trap" is used for many igneous rocks of unascertained composition.

The preservation in the St. Lawrence valley, and in the Great Lake district, of beds of loose laminated sands and clays lying below the till is due, as has been shown by Dr. Newberry, to the fact, that the ice was rising against the slope of the land. It had, in consequence, little erosive power, but advanced principally by the slipping of the higher portions of the ice over the lower. When it topped the southern water-shed of the valley of the St. Lawrence, its action produced a different set of phenomena, for its motion was down the slope of the land, and its erosive power was vastly increased. With this subject I shall not here deal, nor shall I attempt to trace the limits of the ice in its greatest extension, as that would lead me into a dissertation on the whole of the glacial period in North America, far beyond the scope of this paper.

* American Journal of Science and Arts, vol. v., March, 1873.

As the ice melted back, it deposited the unstratified till under its receding foot, leaving a continuous mantle of it behind. Lying on the top of the till are seen scattered rounded boulders (E in section), often of great size, of granite, gneiss, and other crystalline rocks, that must have travelled from the Lawrentian hills in the far north. Amongst these, rocks of local origin are as scarce as in the till below those of distant derivation are rare. These foreign boulders are scattered over the surface, as if dropped by some agent that has left no other record of its movements. The rounded far-travelled blocks lie on soft unconsolidated beds that have not been disturbed. In some places, as on the top of a low hill on the Canadian side of the falls, I found great numbers of these blocks, and in some parts of northern New England and New York, great trains are found in lines along the sides of hills, as if stranded on a beach. They are found on the western prairies, according to Professor Hall, in long trains, "where, for many miles, the difference in elevation is not more than 50 feet; and here we observe long lines of boulders stretching away for miles beyond the reach of vision, as if once formerly a line of coast."* Speaking of the valley of the Hudson, Professor Hall says:—"In the vicinity of Albany and Troy, I have searched in vain for a boulder or pebble of granite, or of any rock older than the Potsdam sandstones in the deposits *below* the clay, while, in a period subsequent to the deposition of the clays and sands, boulders of granite are by no means rare."†

Only one satisfactory explanation has been given of the presence of these far-travelled blocks on the surface of the undisturbed loose beds of sand and clay, namely, that they have been dropped from floating ice, and most writers on the subject have concluded that they are proofs of the submergence of the land below the sea. There is certainly an area of land running from Lake Champlain northwards that has been elevated from below the level of the ocean since the glacial period, but there is no evidence whatever that the sea extended over the plateau of Lake Erie, and the entire absence of marine remains renders the supposition untenable. And if we follow the natural sequence of events that must have ensued during the retreat of the ice, we shall see that there is no occasion to call in the agency of the sea. For just as, during its advance, the ice from the north-east had blocked up the great valley of the St.

* Natural History of New York, part iv., p. 321.
† *Ibid.*, p. 320.

Lawrence and changed it into an immense lake, so, during its retreat, it must have done the same. Probably it did so to a greater extent, not only because, in its retirement, it had left moraines and deposits of till, blocking up the deep channels draining into the Ohio and the Mississippi, but because, during the greatest accumulation of ice, the land northwards, and especially the area of the St. Lawrence, had been depressed, and an immense sheet of fresh water, dammed back by the lower part of the valley of the St. Lawrence, being still filled with ice, stretched south-westward and northward. On the northern shores of this great lake glaciers still came down from the Lawrentian hills, and gave birth to icebergs that floated southward, dropping boulders of granite, gneiss, and other crystalline rocks, on the bed of the lake, or stranding on its shores, and there depositing their freight. During this time were also formed many stratified beds of sand and gravel that lie above the till, and to it belong most of the deposits of the "terrace epoch" of Dana which were formed, not after, but during the glacial period.

Before leaving this branch of my subject, I must again advert, as I have done in previous papers, to the great importance of a proper appreciation of the effect of the stoppage of the drainage of the northern parts of the continents during the glacial period. It was not only a period of erosion and transportation of rocks, but of great fresh water deposits; and I fully believe that the fresh-water and inland sea beds that Professor Ramsay proves to have been deposited in old red sandstone and Permian times were due to former glacial periods, that of the Permian epoch being greater than the last one, and resulting in such a lowering of the level of the ocean, that there was great destruction of marine life by the increased salinity of the sea.

There are many proofs that the ice was thickest and highest during the glacial period in the bed of the Atlantic. That which advanced up the valley of the St. Lawrence came from the direction of Greenland, and the whole of the eastern coast of America, down as far south as New York, must have been blocked up by it. This is proved, not only by the many fresh-water beds and terraces due to the damming back of the rivers, but by the direction taken by the continental ice. Thus, over the higher summits of New England, the scratches point to the south-east and not to the east, as they would have done if the ice had been free to move directly towards the ocean. I think that this shows that the bed of the ocean was then occupied by ice, and it could not fail to be so, for to the land it occupied the position of a great valley, down which the ice from the north would naturally flow. I do not think, however, that the time of the greatest extent of ice in the sea-bed was the same as that of the greatest thickness of ice on the land; for, as the margin of the ice of the ocean-bed moved southward, it would cut off the moisture-bearing currents travelling towards the land, and gather to itself the precipitation from them. Thus, I think it was that the ice on the land shrunk back, at the time of the greatest extent of that which occupied the bed of the Atlantic; and we have, both in America and Europe, a period of land-ice, followed by one of fresh-water deposits and fresh-water borne icebergs.

I endeavoured to show in my paper, published in this journal in October last, that the ice from Greenland also reached the western coast of Europe. It passed across Iceland, and overflowed Caithness. Iceland is so hugely glaciated, that we may conclude the northern ice invaded it also; and, extreme as the view may seem, I can find no other satisfactory explanation of the fact, that the whole of the south of England is mantled by fresh-water glacial beds, than on the supposition that, at the height of the glacial period, the English Channel was blocked up to the south-west by ice that extended in an unbroken mass from Greenland. I sought in vain, before my last visit to North America, for a satisfactory solution of the presence of the fresh-water gravels and floated boulders of the south of England, and was driven to suppose that one or more barriers of land must have existed in the western part of the British Channel; but, after seeing how the ice in the bed of the Atlantic blocked up the water-shed of the eastern seaboard of North America, ten degrees of latitude further south, I have no difficulty in imagining that it may also have blocked up the English Channel, and caused the formation of the high and low-level gravels, the beds of the Rhine, and the floated boulders of Devonshire, Somersetshire, and Wales. I venture to predict, that evidence will yet be found of the encroachment of the edge of this ice from the north-west upon the Continent, probably upon the coast near Brest, and I also expect that traces will be discovered of the great flow of water that must have taken place, either round the south-eastern termination of the ice, or around the mountains of Britany, into the valley of the Loire.

Post-Glacial Phenomena.

When the ice had retired so far back as to leave the channel to the St. Lawrence valley again open, the waters of Lake Ontario began to re-flow in that direction. From the whirlpool, northward, they did not run in their old channel, but took a more easterly course. This may have been because the lowest outlet through the moraines left by the ice was in that direction, as Dana has suggested; but I think it more likely that it was because the ice retired from the eastward first. If we look at a map of the southern side of Lake Ontario, we shall find that most of the rivers have been diverted in the lower part of their courses to the eastward, indicating that the cause was not one of accidental configuration of the ground, but some such general one as the early retirement of the icy barrier from the eastern part of the lake.

Wherever the river first commenced to flow, there it was likely to cut down through the rocks, for it would soon make for itself a channel through the loose drift lying on the surface; and between the banks of that channel it would be confined, and there only operate on the hard rocks below, just as, in copper engraving, the acid only acts on the plate in the lines cut through the soft wax covering it. To make clear the argument in the question we have to discuss, namely, how much of the gorge in which the river now runs has been excavated out of the solid rocks since the glacial period, I must, in the first place, direct attention to the sketch plan (Fig. 3) of the old and new gorges at the whirlpool, four miles below the falls. The sketch is founded principally on a small plan in Lyell's "Travels in North America," and partly from my own observations and sketches on the spot. I regret that I cannot give an accurate plan, and I could not learn that any complete survey has ever been made.

Standing at the summer-house, on the American side, at the point where the river takes a sudden bend to the eastward, I looked across the whirlpool to the old gorge opposite, and the question at once presented itself to my mind—from this point there are two channels downwards, one excavated before, the other after, the glacial period; to which does the one upwards to the falls belong? This question does not appear to have occurred to the authors of the theory, that the whole of the gorge through which the river now runs, from the falls to Queenstown, is post-glacial. But why might not the old pre-glacial river have excavated the gorge above the whirlpool, as well as the old one below it, and the present river have only cut back the gorge from Queenstown to the whirlpool, and, from that point upwards, have re-occupied and cleared out the old channel? On the face of it, the latter alternative seemed to me more likely, for the river above the whirlpool is running in a direct line for the old gorge, and is, moreover, about the same width as it is, the gorge to Queenstown being narrower. I found, with surprise, that this important

Fig. 3.

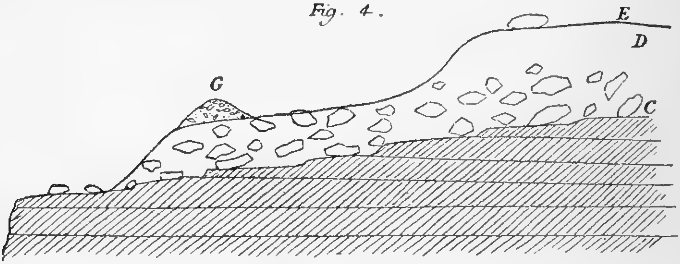
Plan of the old and new gorges of Niagara river at the whirlpool.

point had been overlooked, and that it had been assumed, without discussion, that the gorge above the whirlpool belonged to the new, and not to the old river.

I determined at once to devote the time I could spend at Niagara to the elucidation of this question, and soon found some data bearing on the subject. Lyell and Hall both noticed the terraces formed in the superficial deposits, when the river commenced to cut back the new gorge from Queenstown. These mark its course when it flowed along the top of the plateau, and it seemed to me unlikely, that if the gorge above the whirlpool was pre-glacial, that the post-glacial river would have followed exactly the same line when it was bounded only by the superficial deposits that marked the older features of the country. I found that, above the whirlpool, the post-glacial river had run in different channels, having, apparently, often changed its course in the superficial deposits. Thus Lyell has described one of these deserted channels that ran from the muddy river to the whirlpool.* Another, I noticed, ran down from behind the town of Niagara-falls. In some places, the terraces and ridges that bounded the old river come down to, and are cut off by, the present gorge; at other places they retire back for at least 100 yards from it. They prove that, before the present river was confined in its rocky gorge, it often changed its channel, as rivers do now that run through superficial deposits over a nearly level plain.

On the Canadian side, a little above the whirlpool, two of these terraces come down to the gorge, and are cut off by it. Their direction is shown by dotted lines in the sketch plan of the river gorges at the whirlpool (fig. 3, p. 147), and the following figure is a section through them at the line C D, in plan.

Fig. 4.



Section through the line C D in Plan, Fig. 3, showing two terraces excavated in till and a river ridge capping the lower one. C, till with angular blocks; D, till without stones; E, rounded boulders of northern origin; G, river ridge.

There are here shown two river terraces, of which the highest and oldest has been formed by the washing off by the river of the clay without boulders (D), from that with boulder (C), leaving a level terrace, excepting where it is capped by the river ridge (G). The river at the whirlpool has cut back into the old gorge, clearing out the entrance to it and cutting off this terrace, but on the other side of the old gorge it re-appears and continues on, parallel to the course of the present river, and without any reference to

* Travels in North America, vol. i., p. 42.

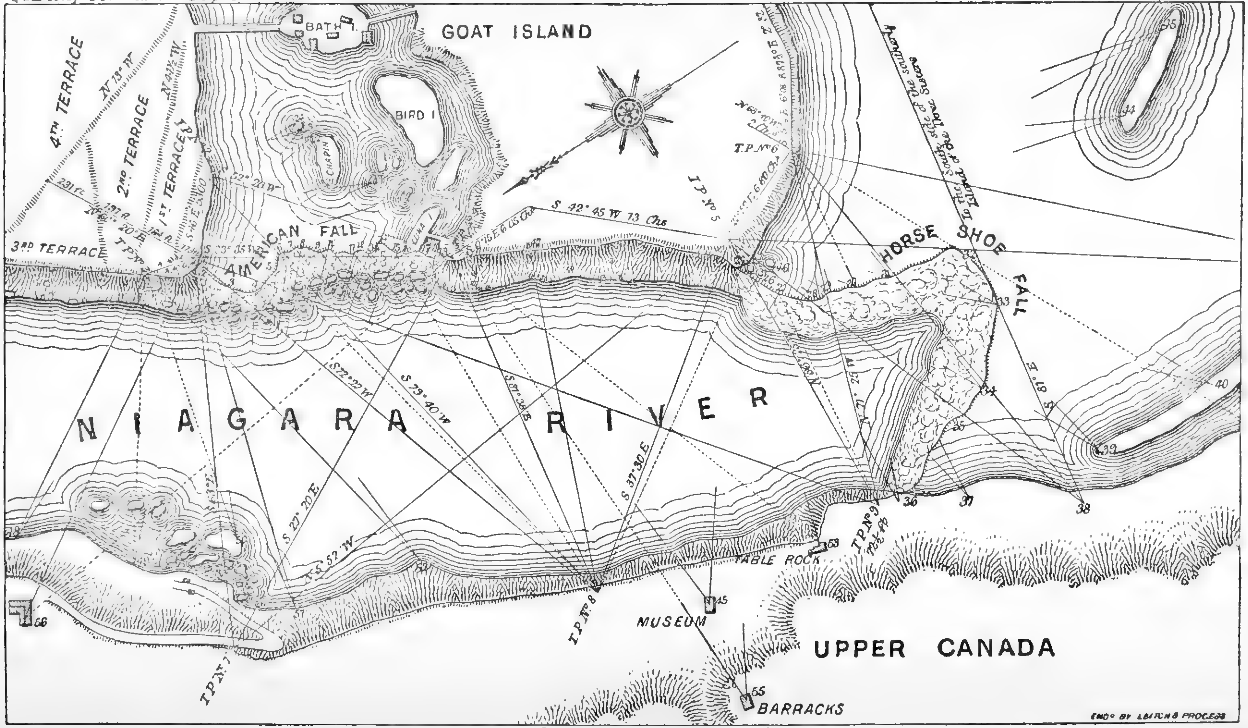
the old blocked-up channel, across which it had evidently been at one time continuous.

This terrace must have been formed when the river was much wider than now, and flowing so slowly, that it had only power to cut through the unconsolidated sandy clay, without stones. The lower terrace marks a later stage, when, by the cutting back of the gorge, the river ran with a swifter current, at a lower level, and in a narrower channel, and cut through the lower till to the solid rock below. It marks the last stage in the present river's course, before it occupied the gorge cut through the Silurian limestones. The ridge capping the bank of the old river-bed must have been heaped up during floods. It is well exposed, though in danger of total obliteration, as it is being carted away for gravel. The stones in it are all rounded, like true river gravel. Mixed through it, often filling the interstices between the stones, are multitudes of fresh-water shells belonging to the genera *Melania*, *Limnea*, *Unio*, and *Cyclas*. I was able to confirm the observation of Lyell, that the assemblage of species is the same as that now found in the river above the falls. Amongst the stones of the present beach above Goat Island, I found shells of the same species as I did in the old ridge above the whirlpool, and in not much better state of preservation; indeed, excepting that I had labelled the boxes containing the different sets, I could scarcely now tell which were the older of the two. This assemblage of dead shells in the gravel of the beach differs, both in the older and newer deposits, from that found living in the present river, in that many delicate shelled species are scarce, or not found at all, owing, no doubt, to only the more robust shells being preserved. Thus, thin-shelled species of *Physa* abound in the river, but I did not find any of their shells, either amongst the stones of the present or the old beach.

A little above the whirlpool, the gorge widens out abruptly, as shown in plan, and the terraces and ridge are cut off by it, so that they cap the gorge, and are exhibited in section almost as clearly as I have depicted them in fig. 4. The river ridge, composed entirely of loose gravel and sand, is seen running to the edge of the cliff. The widening of the gorge extends for some distance beyond it, cutting off the upper terrace in the same manner. At this spot the upper layers of limestone project beyond the lower beds, just as they do now at the table rock at the falls, forming an overhanging precipice, so that the widening of the gorge cannot have been caused by weathering. I cannot conceive how the present river could excavate the gorge beyond the loose ridge of gravel, without washing both it and the terrace of unconsolidated till away; and it seems to me that we have here a proof, of what appeared before to be probable, that the river has at this point only re-opened the ancient gorge, the clay and stones that filled the widened part having been washed out by water from below, not from above, as would be necessary to excavate the gorge itself. I concluded, therefore, that the pre-glacial falls had been situated at least as high up as this point, and I thought that the narrowing of the gorge upwards, though it was still wider than that leading to Queenstown, might mark the commencement of the present river's work above the whirlpool.

On examining the gorge higher up, I however discovered that there were several places where it widened suddenly out, and at two of these I found similar proofs of the gorge not having been excavated by the present river. Thus, on the American side, between the railway and the suspension bridge, there are two or more widenings of the gorge, and I noticed a terrace of till at the upper one cut off by the setting back of the gorge. On the Canadian side, about a quarter of a mile below the suspension bridge, there is one of these sudden widenings or bulgings out of the gorge. Higher up a ridge of till, capped by river gravels containing fresh-water shells, marks a former channel of the river, and runs down about 50 yards from the gorge. This ridge does not wind round the widened part of the gorge, but runs down to it, and is abruptly cut off by it, similarly to the one above the whirlpool. Exactly the same argument may be used to prove that the present river has not cut out the gorge at this point, but only emptied it of the glacial clays and sands with which the old pre-glacial gorge was choked up. This example of a river ridge cut off by the re-excavation of an older gorge, is the nearest to the fall that I could find. From thence, upwards, the river terrace is back from the gorge, and uninterrupted by it.

The argument resolves itself into this form: above the whirlpool the gorge approaches both in direction and width nearer to the old one leading to St. David's than to the post-glacial one leading to Queenstown. That it is pre-glacial is strongly indicated by the fact of the post-glacial ridges being cut off by it in consequence of its re-excavation, whilst there has not been a single argument advanced in favour of the theory that it is post-glacial, which was simply founded on an assumption that does not bear investigation. The conclusion at which I arrive is, that the gorge was cut back from the whirlpool up to at least within three-quarters



Trigonometrical Survey of the Falls of Niagara.
(Reduced from Plan published in Geological Report of New York, 1843.)

of a mile from the falls before the glacial period. It may have existed to within a few yards of the falls, for anything that can be seen to the contrary, whilst, in favour of such a supposition, there may be advanced the great width of the gorge up to the commencement of the horse-shoe fall, the very small indentation that the American fall has made in the side of the gorge over which it leaps, and the appearance the plan of the falls presents, that the river is now cutting back a much narrower gorge, one as narrow as that leading to Queenstown from the whirlpool.

I hoped to have been able to find at Goat Island some evidence bearing on this question, as both Lyell and Hall have described river gravels capping the till there, and also indications of a pre-glacial channel excavated in the silurian rocks, but the whole of the island was covered with a glassy surface of ice produced by the frozen spray from the cataract, that made it most difficult to get about the sloping banks, and masked the beds I wished to examine. On the Canadian side there rises a high ridge of till, overlain by a thick bed of boulder clay, with large stones; and on the American side, there is what appears to be a continuation of this ridge, now cut through by the river. Around this ridge, on the American side, there are indications, as I have already mentioned, that the river once flowed. Goat Island seems to be a remnant of this ridge, and I imagine that it has been pierced, not from above, but from below, that it overlaid the pre-glacial gorge, and was undermined in the same way as the clay filling up the end of the old gorge has been at the whirlpool. This and other questions that arise I must leave to observers with more time at their disposal, and a more favourable season of the year to make their investigations than I enjoyed. The observations that I made are the result of three days' holiday from business, and I am sure, from what I saw, that three months close application would not exhaust the many points of interest that present themselves. The geologists of Canada and New York could not have a more interesting question to work at than this, and if they did no more than correctly and fully map out the gorge, the terraces, and the river ridges, they would confer a great benefit on geological science. The absence of such maps I found to be a great drawback to my investigations. The only part of the gorge that has been surveyed with minute accuracy is that at the falls. This was done by Professor Hall, assisted by competent engineers, in 1842, and permanent marks were at the same time fixed in the rocks. The first step of a new survey should be a trigonometrical re-measurement of the rocks at the fall. This, compared with that made thirty-three years ago—one-third of a century—could not fail to afford data for calculating the present rate of retrocession, and would be a fitting compliment to the veteran geologist under whose auspices the first survey was made, and to whom the whole scientific world is a debtor for a lifetime spent in geological research.

Whenever that survey be made, I believe it will decide that the present river is cutting back the gorge much more slowly than Lyell estimated; that, instead of one foot yearly, the retrocession is not more than, if it is as much, as one foot in ten years, and that, allowing for the comparative softness of the rocks below the whirlpool, we shall have to put back the occurrence of the glacial period to at least 200,000 years ago, if we conclude that the whole of the gorge, from the falls to Queenstown, has been excavated since that time. But if the conclusion at which I have arrived is correct, that the gorge, from the whirlpool to the falls is pre-glacial, and that the present river has only cut through the softer beds between Queenstown and the whirlpool, and above the latter point merely cleared out the preglacial gorge in the harder rocks,—20,000 years, or even less, is amply sufficient for the work done, and the occurrence of the glacial epoch, as so measured, will be brought within the shorter period that, from other considerations, I have argued has elapsed since it was at its height.

Simply looked at from a geological point of view, the time occupied may not seem important, and it has been usual for geologists to ask for an unlimited duration, though, even from that standpoint, it is difficult to reconcile the small amount of denudation that glacial moraines exhibit with the remote antiquity that some physicists assign to them. In Ohio and Illinois, the mounds of the old Indians do not look more recent than the ridges and gravel hills of glacial origin, and in some parts cannot be distinguished from them until excavations are made into them. In England I know we have a school of geologists who have taught that the river valleys of the south of England have been excavated since the glacial period; but wherever we find undoubted glacial deposits, as in the north of England and in Scotland, we find them scarcely altered from the time when they were laid down.

But the student of the succession of changes in the organic world will have a serious difficulty removed, if it be proved that the glacial period occurred not more than twenty thousand years ago. In the northern temperate zone, so far as we can learn, there has been little variation in the animal or vegetable world since the glacial period. In the tropics, the formation of specific differences has been probably more rapid, but in northern Europe, the species now living differ but little, if at all, from their pre-glacial ancestors. Some of the large mammalia have become extinct, but the fauna and flora are essentially the same as they were before the glacial periods—that is, though some species have died out, we have no proofs of any new ones having come in. There is not a single example of a distinct species having been formed since that time, though some varietal differences may be detected. Even man himself has, I believe, varied but little, physically, since pre-glacial times.

In the paper already referred to, published in No. 44 of this journal (Oct., 1873), I assumed that the arguments brought forward by distinguished geologists, to prove that the palæolithic implements and the mammalian remains found with them were post-glacial, were founded on a sound data. There were great difficulties to be surmounted if that conclusion was correct; but the published sections of the superficial beds, at Bedford and at Hoxne, seemed to admit of no other explanation. Since then I have been able to examine for myself some of the supposed post-glacial beds, and to devote more time to the study of the whole of the valley gravels in the south of England, at the bottom of which the palæolithic implements and mammalian remains are found. The conclusion at which I have arrived is, that so far as the British Isles is concerned, palæolithic man, the mammoth, the woolly rhinoceros, and the hippopotamus are entirely pre-glacial, and that the great and distinct break between the palæolithic and neolithic deposits in that area was caused by the culmination of the glacial period, when to the north of a line drawn irregularly from Lynn Regis, in Norfolk, through Birmingham westward, nearly the whole country was covered by land-ice, that destroyed the mammalian bones and the palæolithic implements, excepting where preserved in fissures and caverns, or in a few spots in the eastern counties, to which the ice did not reach; and when, to the south of that line, a great lake or sea of fresh water, dammed back by the ice that blocked up the German ocean to the north, and the British Channel to the west, covered the pre-glacial remains beneath a mantle of beach gravels as it rose and fell.

Dr. Falconer long ago argued that the older cave mammalia were pre-glacial; Mr. Tiddiman has found a human bone beneath glacial *débris* in Yorkshire; in America Professor Whitney has announced the discovery of a pre-glacial human skull, and I hope soon to be able to lay before geologists the evidence I have collected, that I think proves that the tools of palæolithic man in the British Isles are all of pre-glacial age. Nearly all ethnologists are agreed that the representatives of palæolithic man are the Eskimos of the far north, and probably, in glacial times, they held much the same relation as they do now to more civilised communities, living further south in more congenial climes, and I have suggested that the records of a glacial civilisation still exist in the statues and cyclopian ruins of some Pacific Islands.*

If we have to go back 200,000 years to the glacial period, the small amount of change in the organic world, and the slow progress of civilisation northwards, from its southern home, are difficulties not easily surmounted by the evolutionist, for he has not unlimited time at his disposal. This world and its inhabitants do show signs of a beginning, and he will have to put that beginning back far beyond the time that physicists and astronomers will allow him, if 200,000 years scarcely takes us one step backward in the long succession of changes in the organic world, of which we have proofs in the strata of the earth's surface. These difficulties will be greatly lessened if the period of the glacial epoch has to be put back only 20,000 years; and, so far as the excavation of the gorge of Niagara affords a scale of measurement, there is no reason to ask for a longer time.

II. HEREDITY.†

IT is recorded that towards the end of the seventeenth century two Highland chieftains were condoling with each other on the unpleasant fact that "the law" had penetrated within fifty miles of their mountain-fastnesses, and that soon not a spot would be left where the claymore was the only arbitrator between man and man. In our day,

* Naturalist in Nicaragua, p. 269.

† Heredity: a Psychological Study of its Phenomena, Laws, Causes, and Consequences. From the French of TH. RIBOT. London: H. S. King and Co.

Heredity and Hybridism: a Suggestion. By EDWARD W. COX, S.L. London: Longmans and Co.

minds of a certain class perceive with alarm that order after order of phenomena is being brought within the grasp of what the scientific world not very happily terms "law," and that the very last strongholds of the arbitrary—the regions where force was supposed to be created, and where events arose without any natural sequence—are now invaded.

The doctrine of heredity, as applied to man, is one of the latest conquests of the scientific spirit, and, as such, it is still viewed with dislike and suspicion.

That like produces like has, indeed, been an article of popular faith for untold centuries, and has been embodied in the proverbial philosophy of all but the merest savages. If a son resembles his father in person, in habits, or in character, we are told that he is a "chip of the old block," or reminded that "the apple does not fall very far from the tree." But, with curious inconsistency, man often revolts at conclusions even when he has loudly proclaimed their premises. So long as the law of heredity was tacitly assumed to apply merely or mainly to racers and greyhounds, to short-horns and to crève-cœur pullets, it was admitted as a matter of course, and facts which placed the induction upon a yet firmer basis were welcomed. But now the result appears: now it is plain that the law embraces not brutes only, but man also, and that it extends from his outward structure to his intellectual powers, his vices, and his virtues; there is dire confusion, and a wish to retract former admissions were it possible. Such men as M. Ribot and Mr. Galton, who have collated facts bearing upon this important subject, and who, consistently following up the clue, have pointed out the only legitimate interpretation, have incurred no small share of obloquy.

There have been, hitherto, two hypotheses professing to account for the varying aptitudes, faculties, passions, and tastes of men. The one—to which we may refer hereafter—put forward by the revolutionary philosophers of the eighteenth century as a corollary of their doctrine of universal equality, held that all men were essentially and originally alike, and that the diversities which they exhibit were due solely to training, education, early associations, and other post-natal circumstances. The experience of every man is quite sufficient to render any formal refutation of this view needless. The other hypothesis, though still widely entertained by divines and moralists, and accepted as a point of conventional orthodoxy, is, if possible, still more outrageous. It regards every man, at least,* as the creator

* We are not aware that this doctrine has been extended to the lower animals, who have their differences of character as well as man.

of his own character. Genius, heroism, crime, insanity, every exuberance, every deficiency, and every aberration of our inner life, are self-caused,—coming into existence without any regular and unvarying antecedents. Let us take as a specimen the following passage, which M. Ribot quotes from Heinroth :—

"Insanity is the loss of moral freedom ; it never depends on a physical cause ; it is not a disease of the body, but a disease of the mind, a sin. It neither is nor can be hereditary, because the thinking *ego*—the soul—is not hereditary. What is transmissible by way of generation is temperament and constitution, and against these he must react whose parents were insane if he would not himself become lunatic. The man who, during his whole life, has before his eyes and in his heart the image of God, need never fear that he will ever lose his wits."

It is scarcely necessary to say that if we admit this hypothesis we must, to be consequent, reject not this or the other of the teachings of Science, but abandon altogether the scientific spirit and the scientific method. We may not be able to foresee—as we predict a solar eclipse or a transit of Venus—whether or no a newly-born child will prove a genius. The laws involved are beyond our present grasp ; possibly altogether beyond the powers of the human intellect. But if we maintain that the result is independent of law we may as well deny the conservation of force, question the axiom that like antecedents will be followed by like consequents, and abandon our faith in the uniformity of Nature. For us, then, Science has no longer any existence. But the advocates of the hypothesis assert that man makes himself a genius, a lunatic, an idiot, a criminal, or a common-place citizen, in virtue of his own "free will,"—an explanation which is far from mending their case. Not being either a Calvinist or a fallen angel* we shall not enter into such regions. To pursue a metaphysician into that Serbonian bog is as bootless as to chase a Cateran into his own wilderness. Still we may remark that this free-will view leaves the question unaffected. The difficulty, like a sturdy beggar in some parts of Germany, is merely "abgeschoben,"—laid on a barrow and wheeled into the next parish. Why should the "will" of John produce effects which the will of Thomas does not, except there be in the two some difference, innate or connate ? And whence springs this difference ? Is it, too, self-caused ? How,

* Milton happily represents his devils debating on free will.

moreover, can the will create faculties which are in their origin and development synchronous with itself?

But perhaps the Accidentalist—for so we must term him who denies the law of Heredity—may ask, in turn, whether that law is not also open to the charge of merely pushing away to a little distance a difficulty which it cannot solve? You derive your genius from your father, who, in turn, received it from your grandfather. Is not this like the Hindoo Astronomy which supported the earth on an elephant, and the elephant on a tortoise, but left the tortoise to struggle as he might for a footing among the Immensities? Certainly not, for here we must point out as a happy coincidence that the doctrine of heredity was not formally promulgated until after the great law of organic evolution had been made known. Obscurantists have, indeed, sneered at modern science as denying—by the mouth of Messrs. Galton and Ribot—what she had just declared by the mouth of Darwin, Wallace, Haeckel, and Schmidt. The joke is too poor to be unpardonable. The doctrines of evolution and of heredity are not contradictory, but harmonious. When we account for genius by pronouncing it hereditary, we do not mean to say that it suddenly made its appearance in some remote ancestor, or that it has been from eternity an heirloom in certain families. We conceive of it as being formed, in the course of generations, by small successive increments. A man of fair average intellect and good constitution marries a woman similarly endowed. He leads a life free at once from excesses and from asceticism, from torpor and from exhaustive over-work,—free, above all things, from anxiety. Suppose one of his children, born under such favourable circumstances, well nurtured and well educated, repeats the process. The grandchildren will, in all probability, show marks of decided superiority. If, now, such conditions are continued, the result in the fourth or fifth generation may be a Shakspeare, a Bacon, or a Humboldt. If we reflect how rare must be the concurrence of conditions needful for this gradual elevation of a family, we need not wonder at the scarcity of genius.

The question may be raised, on the other hand, not why children inherit the faculties and dispositions of their parents, but why such resemblance is not more complete and more uniform? What, simply, is the cause of that striking diversity often recognised in one and the same family? In short, admitting the general principle of heredity, what are its conditions, its modes of action, and its limitations?

Here it must be admitted that, although M. Ribot has collected a most valuable mass of facts, and has given us many interesting reflections upon their bearings, he still leaves much work for his successors. The case is one of extreme complication. Every human being, and every animal of the higher orders, has two parents whose influence is blended in a manner which to us appears, so far, inexplicable. We do not even know whether one parent, as a rule, determines the outward form, and the other the constitution and character; or whether, in each of these points, a resemblance may be detected to both. It has been suggested that something may here be traced resembling the chemical law of combination in multiple proportions, the male or the female parent, according to its general or momentary predominance, affecting more or less the character of the offspring. Thus children of the same parents might differ from each other, just like compounds formed from the same elements, but in varying proportions, as $A + B$, $A + 2B$, $2A + B$, &c.

Here, then, already is scope for great diversity. But in addition to the influences of the immediate parents, sometimes acting in conjunction and sometimes in opposition, and varying with their degree of vigour and mental state, both at the moment of procreation and during gestation, we have the phenomena of atavism. It has long been remarked by physiologists, physicians, breeders of cattle, &c., that attributes which have been observed in grand-parents, or in more remote ancestors, but which have lain dormant for one generation or for more, suddenly reappear. In this fact M. Ribot sees a certain analogy with alternate and serial generation, as it may be observed in the salpæ and the medusa.

We have, further, what is called indirect heredity, "the representation of collaterals in the physical and moral character of the progeny." Between uncle and nephew, or grand-uncle and grand-nephew, and even in still remoter degrees, we not unfrequently find "striking resemblances of conformation, face, inclinations, passions, character, deformity, and disease." Such resemblances have been ascribed to chance, and used as an argument against the very existence of heredity. M. Ribot very justly points out that "indirect heredity is only a form of atavism—a form which is rarer and less easy of apprehension than direct atavism, but differing from it only in appearance."

The most perplexing case is that which M. Ribot designates as "heredity of influence," a form rare indeed, but still thoroughly established. Any female animal may produce offspring resembling not her present, but a former mate, who may have been dead for years. However interesting this phenomenon may be as a physiological problem, it is too rare among the human species to have any important bearing upon our subject.*

But when the action—often conflicting—of all these forms of heredity has been duly taken into account, there is still a residuum, to which M. Ribot gives the name of "spontaneity." Occasionally, though not often, a man appears, eminent for good or for evil, whose genealogy throws no light at all upon his peculiar attributes, or seems even in flat contradiction to what the law of heredity would at first sight lead us to expect. But we must remember, that in many instances nothing is known of the ancestry or of the collateral relatives of a man of genius. Before he rose to celebrity these points excited no curiosity; when his reputation is established, the inquiry comes too late. It is very true, as M. Ribot remarks, that "merit does not belong exclusively to history." If we look upon the intellect, not as a single force, but as a complex of faculties, we shall find little to perplex us in the phenomenon of spontaneity. Suppose a family who have possessed some of the attributes of greatness, but who, in virtue of a principle equally true in psychology and in mechanics, that "nothing is stronger than its weakest part," has remained in obscurity. Let a man of this family marry a woman whose faculties are the complement of his own. It is possible that a child of such a couple may combine the defects or the weaknesses of both parents, and we have then the case of spontaneous imbecility, or criminality. But it is also possible that he may combine the excellences of both, and burst upon the world as a spontaneous genius. As to the laws which determine whether of these two cases shall be realised, we are still utterly in the dark. Perhaps the state of the parents at the moment of procreation is the clue to the difficulty.

Again we must remember, that even if we consider the intellect as "one and indivisible," it is far from being the only faculty needful for the attainment of excellence, even in the fields of pure science. Combined with it there must

* There is a form of heredity still more obscure, and requiring further investigation. If a female animal, pregnant by one male, has during pregnancy frequent connection with another male, the offspring may, to some extent, resemble the latter.

be the moral faculties* of patience, perseverance, and concentration. The will must be strong enough to overcome all distracting temptations, whether in themselves good or evil. Lastly, there must be constitutional energy and endurance. Failing these, the man will merely leave among his friends the conviction that he might have achieved greatness, if ——. We once knew a physician, resident in a small country town, who, from time to time, startled his associates by some profound and suggestive idea—some brilliant *appercu*. But a constitutional languor prevented him from ever completing an investigation, or from leaving the world one written line.

Nor need we feel greatly surprised if the son of an illustrious man proves, intellectually or morally, a failure. The world often expects from such a one too much. He is lost to sight amidst his father's fame, like Mercury in the blaze of the Sun. Further, we must remember that men of the very highest order often fail to leave any posterity at all,—a circumstance too common to be merely casual, and pointing, we think, to the truth, that in producing a genius a family exhausts itself. Is it, then, very wonderful if men —eminent, though not of the foremost rank—should leave progeny inferior to themselves?

The suggestion of Mr. Cox—that every organism is produced by the union of two germs, one supplied by the male and the other by the female parent—seems to us certainly to throw a clearer light upon some of the phenomena connected with hybridism. "All living forms," he writes, "are *double*; that is to say, they are not one whole constructed as a whole, but two halves joined together, always more or less unlike,† having points of resemblance and of difference obviously derived some from one parent and some from the other, and which indicate a greater share by both parents in the product than has yet been assigned them. The suggestion of a double germ (one supplied by each parent) accounts naturally for hereditary characteristics and tendencies, mental and bodily, and for the mysteries of hybridism. . . . If it be true that Nature's method is by the union of two germs, practical horticulture, no less than physiology and psychology, will have secured a firm foundation, from

* Are we right in drawing such a rigid line of demarcation between the intellectual and the moral powers? Do they not rather exist in what might be called "mixed solution?"

† Entomologists occasionally meet with moths one-half of whose body, taken laterally, is male, whilst the other half is female. We have ourselves captured a specimen of this kind.

which a new departure may be made into the region of speculation, no less than into the domain of fact." The learned author asks whether, in such cases as the "two-headed nightingale," there is "duplicity of structure in any portion of that part of the united structure which is common to both. Are the bones formed by the junction of two bones? Are the muscles duplex? Are there two sets of nerves? Is there any abnormal condition of the muscles or tendons indicating the original presence of two formative forces acting together?" He suggests the careful examination of the next double-headed calf or lamb produced.

We cannot, however, agree with Mr. Cox in his views on hybridism. The arguments he applies to the mule—which is usually, though not invariably, barren—would apply also to the *léporide*, and to certain hybrid birds which are fertile. Nor can we see that this hypothesis would at all aid us in understanding the "heredity of influence."

M. Ribot, though in part dealing with questions already handled by Mr. Galton, takes wider ground. He treats not alone of the heredity of genius, but of instincts, of the sensorial qualities, the memory, the sentiments and passions, of national character, and of morbid psychological heredity. To examine in detail all his views is a task which we must leave to the reader. To some of his remarks upon instinct we feel compelled to demur. Thus, he declares that "A bird hatched in a cage will, when given its freedom,* build for itself a nest like that of its parents, out of the same materials, and of the same shape." Has this experiment ever been tried with a pair of birds hatched in confinement, and then building where they cannot associate with others of their own species? The alleged stationariness of instincts is another questionable point. For how many thousand—or even hundred—years have we possessed detailed and trustworthy descriptions of the nests of birds and insects? Suppose a naturalist were to perceive, in the architecture of a wasp or of an ant, something entirely new,—an improvement on the part of the insect,—would he recognise it as such, or would he not rather pronounce it something old which his predecessors had failed to observe? Mr. Wallace has very justly pointed out that if the nests of birds are unvarying, so are the tents of nomadic Arabs, the wigwams of Red-skins, and the huts of Greenlanders. The author maintains that "we may have the same organisation

* "When *given* its freedom." We regret to see this slovenly expression finding its way into a work like that before us.

with different instincts," and remarks, in confirmation,—" Birds have their beaks and feet as their only instruments for building, yet how great are the differences of the form, architecture, and position of their nests." This instance is not happily chosen. The beaks and feet of birds differ so much in structure that they alone will go far to account for the varied architecture of their nests. Of what use would be the feet of a duck, or the beak of a parrot, in constructing the pendent nest of an oriole?

In treating of the heredity of the sensorial faculties, the author refers to the prevailing short-sightedness of the Germans, and ascribes it to their studious habits. We believe that the German type is mainly to blame, and that a nation equally studious, but making use of the Latin character, would escape.

The section treating on the heredity of the intellect might, we think, have been advantageously extended. Several cases, which decidedly support the view taken by the author and Mr. Galton, are omitted. The scientific world knows two Liebigs, two Fresenius', two Mitscherlichs, two Mulders, two Thenards, two Persoz', two Becquerels, two Berthollets, two Sainte-Claire Devilles. It is strange, also, that no mention is made of such men as Laplace, Lagrange, Latreille, Lavoisier, Lacepede, Gay-Lussac, Guyton Morveau, Wœhler, Lamarck, Blainville, Oken, Carus, Faraday, Dalton, Boerhaave, Swammerdam, Harvey, Volta, Nobili, Biot, Scheele, Schleiden, Chevreul, Agassiz, Dumas, &c. Surely the lineage of many of these men could be accurately traced, and could scarcely fail to throw an additional and welcome light on the subject. For, admitting the law of heredity in its broader outlines, we have yet to ask whether "genius" be general or special? Would the man who achieves greatness in one department have been equally great in another, and is the choice of studies merely determined by circumstances? Could an eminent chemist, if placed in youth under other influences, have become an eminent lawyer, divine, classical scholar, musician, or man of business? M. Ribot and Mr. Galton appear to answer this question in the affirmative. Hence, if a man has become illustrious in any respect, they cite—as cases confirmatory of the doctrine of heredity—any of his relatives who have in anything risen above the average level. Thus, in connection with the great German poet Heinrich Heine, Mr. Galton mentions his uncle, Salomo Heine, an eminent banker and philanthropist. Along with Francis Bacon, M. Ribot notes his half-brother Nathaniel, "a clever painter." To the name of Jeremy Bentham, legist and moralist, the author appends—"His *brother*, General Samuel Bentham, a distinguished officer. His *nephew*, George, an eminent botanist, president of the Linnean Society."

Of course this view renders it very easy to find evidence of hereditary genius. Our own observations lead us, however, to the antagonistic opinion—the speciality of genius. To us it appears that men of universal powers—"admirable Crichtons," who adorn whatsoever they touch—are of all psychological phenomena the rarest. The man who in one faculty rises high above the average of his fellows is very likely, in accordance with the great law of compensation, to fall below them in others. The mind, or the brain, if the latter term be preferred, has its idiosyncrasies no less than the stomach. To take a striking instance:—Towards the beginning of the present century, at a German educational establishment of the old type, where pupils were gauged solely according to their proficiency in classics, there was a boy who was the acknowledged dunce of the school. Sneered at by his companions, and denounced by the masters as little better than an idiot, his declaration that he intended to become a chemist was received with a general outburst of contemptuous laughter. But the boy knew his own speciality. His success in chemistry was yet more decided than his failure in classics, and when he passed away from our midst he left the name of Justus von Liebig, second to none in the annals of Science. There are, of course, groups of sciences or of studies within which the choice of an aspirant may be determined by what is commonly called accident. Thus a great chemist, doubtless, if placed under slightly different influences, might have reached equal distinction as a physicist. The same powers which render a man eminent in botany would have done him equal service in zoology, palæontology, or physiology. But to maintain that a distinguished cultivator of any of the above sciences could therefore, *if he liked*, have attained corresponding success as a barrister, a preacher, or a parliamentary orator, seems to us hazardous in the extreme. In these latter spheres eminence absolutely requires great power of expression, and the faculty which is aptly—though clumsily—called ready-wittedness. Now a man of science may be very poor in expression and very slow in thought, provided only that he thinks profoundly and originally. If some evil star had placed Henry Cavendish in the bar, the pulpit, or, we may add, at the merchant's desk, would he not have made a most deplorable figure?

The author handles the heredity of the imagination and that of the intellect in two distinct sections. Under the former he gives the genealogies of poets, painters, and musicians; and under the latter men of science and prose authors, even novelists. This arrangement seems to us objectionable. The gifts requisite to produce a prose work of fiction are surely more akin to the faculties of the poet than to those of the mathematician, even although the imagination plays no unimportant part in scientific discoveries. It is interesting that, in treating of painters and musicians, M. Ribot recognises—implicitly at least—that speciality of talent which he overlooks among philosophers. With scarcely an exception, when tracing the lineage of artists and musicians, he enumerates those only of their kindred who were themselves artists or musicians.

Into the interesting and suggestive chapters on the relation between the physical and the moral,—on heredity in connection with the law of evolution, and on its psychological, moral, and social consequences,—we must, though with reluctance, abstain from entering. To two, however, of the practical applications of the truths brought forward by Mr. Galton and M. Ribot, we will briefly call attention.

The doctrine of Heredity throws a needful light upon the question of national education. What shall we gain by catching every little boy or girl and consigning him or her to school, whether "Board" or denominational? Some of us, especially the neophytes of the movement, expect too much. It was held by Helvetius, in accordance with the revolutionary hypothesis of universal equality, that the differences, intellectual or moral, between man and man were simply due to early training. It was believed that genius could be raised to order, and the market supplied with original thinkers as easily as with early peas or hothouse grapes. Nor has this delusion totally disappeared. M. Ribot, as a matter of course, sees that the power of the schoolmaster over the raw material put into his hands is strictly limited. The mind of the child has been likened to a blank sheet of paper, but it is paper upon which certain characters may be traced with readiness, others with difficulty, and others, again, not at all. Or if we adopt the Socratic view, that the educator is like a sculptor who cuts out a beautiful statue from the crude block, we may say that every such block has a determined cleavage, which limits him, in each case, to the production of a certain class of forms. M. Ribot maintains that upon minds of the highest and lowest classes the influence of circumstances in general, and of education in particular, is comparatively small:—"We restrict education, as we think, within its just limits, when we say that its power is never absolute, and that it exerts no efficacious action except upon mediocre natures. Suppose the various human intelligences to be so graduated as to form a great linear series, rising from idiocy, the bottom of the scale, to genius, which is at the top. The influence of education is at its minimum at the two ends of the series. On the idiot it has hardly any effect: unheard-of exertions and prodigies of patience and ingenuity often produce only insignificant and transient results. But as we rise towards the middle degrees this influence grows greater. It attains its maximum in average minds, which, being neither good nor bad, are much what chance makes them; but as we ascend to the higher forms of intelligence we see it again decrease, and as we come nearer to the highest order of genius it tends towards its minimum." So long, however, as persons of "mediocre natures," intellectual and moral, constitute, as they are always likely to do, the great majority, the importance of education—both in a national and in an individual point of view—remains unshaken. Men who have not that magnificent spontaneity which enables its possessors to open new epochs in the history of Science, may yet, if properly trained, be capable of useful research. It will never be theirs to discover a law of gravitation, an atomic theory, a conservation of forces, or a doctrine of organic evolution; but they may discover and analyse new compounds, natural or artificial; they may detect new forms of organic life, observe their structure and habits, and determine their geographical distribution.

If, then, education can do such things,—if it can call into being discoverers of the second order, inventors, and appliers,—it must always constitute a main element of national greatness, and stands in no need of fictitious or exaggerated recommendations.

Not less important is the bearing of the law of heredity upon crime and its treatment. There are evidently two very distinct classes of criminals,—the casual offender, who in some unguarded moment has yielded to temptation, and the normal hereditary villain, such as the Liverpool "cornermen." As regards the latter, we have to consider not merely the garotter or ruffian brought before the courts, but his descendants, actual or possible, of whom the bulk will as surely be criminals as the children of a consumptive person will be prone to pulmonary disease. Attention has lately been drawn to the fact that the ancient sanctuaries of

England, which were nests where the vilest criminals swarmed and multiplied, are to this day characterised by the low moral type of their inhabitants. Whitefriars, Fulwood's Rents, and certain districts of Westminster, are sufficiently well known in London. Scarcely an episcopal city but has a similar rookery beneath the very shadow of its cathedral. A young woman of remarkably depraved character, who infested the districts on the Upper Hudson at the beginning of the present century, has left eighty direct descendants, of whom one-fourth are convicted criminals, whilst the rest are drunkards, lunatics, paupers, prostitutes, or otherwise useless members of the community. If the lineage of criminals is inquired into, as it doubtless henceforth will, we shall find that crime literally reproduces crime. So long as we imagined that the criminal, if placed under more favourable circumstances, might have grown up a virtuous man, we were justified in a policy of leniency, and might entertain the hope of reclaiming the offender. This hope—except as regards the casual, incidental criminal—is now taken from us. The hereditary ruffian must be "stamped out," by death or by penal servitude for life, as the circumstances of the case may indicate.

We do not set ourselves to work to train tigers and wolves into peaceful domestic animals; we seek to extirpate them. Why should we act otherwise with beings who, if human in form, are worse than wild beasts, since they slay and maim out of pure wantonness? Education, moral and religious influences, may doubtless arrest the formation of criminal races, and bring them back to a normal condition; but can we allow a criminal family to go on for four or five generations, murdering intelligent, moral, industrious citizens, whilst we are trying experiments in reclamation? To educate the son of a garotter or of a "corner-man" into an average Englishman is about as promising a task as to train one of the latter into a Newton or a Milton.

III. THE LATE TRANSIT OF VENUS.

By RICHARD A. PROCTOR, Author of "Past and Coming Transits," &c.

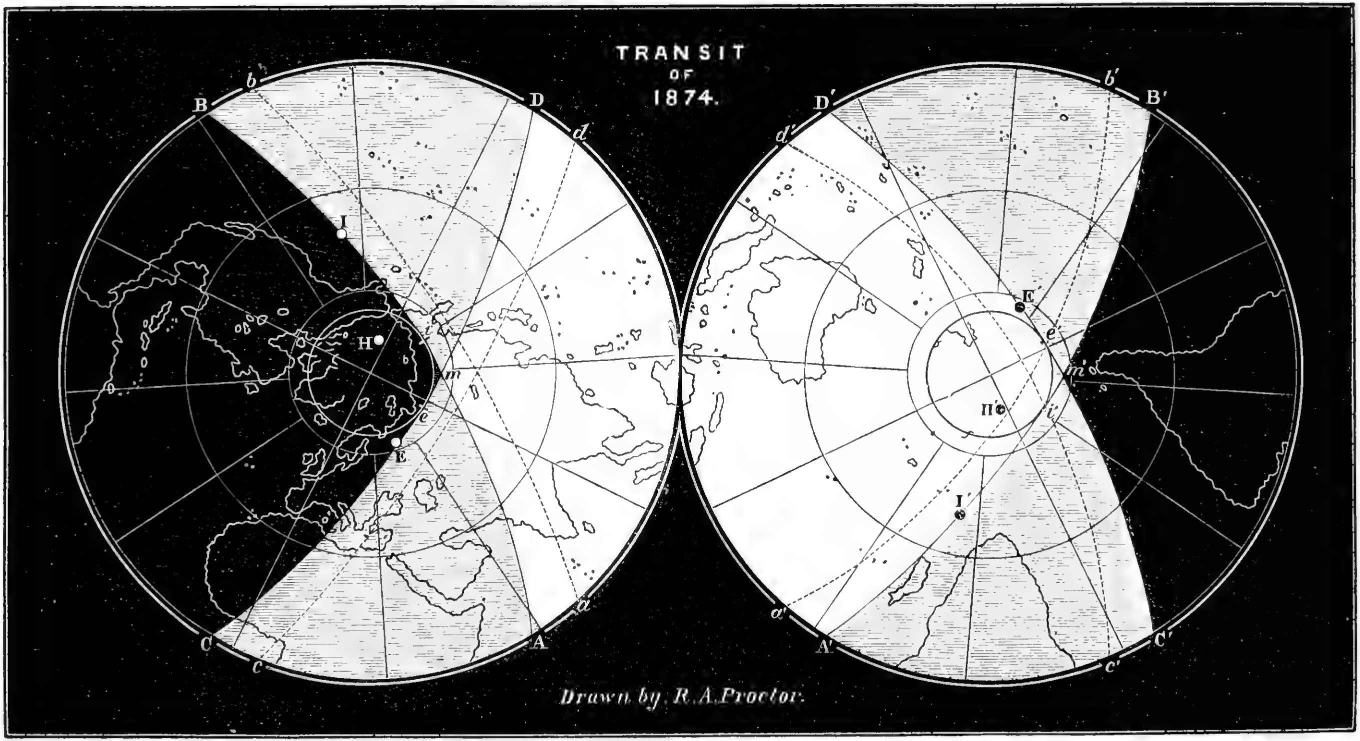
AT the moment of writing, news has been received from all the stations selected for observing the late transit, excepting two French stations—St. Paul's Island and Campbell Island—and though as yet the news from most of the stations is far too imperfect for any attempt to be made to calculate the solar parallax, while from others we only have news that observations were successfully made or otherwise, yet we know enough already to be able to form an opinion as to the general success of the operations. The result cannot but be considered as on the whole extremely favourable. It was certain, or all but certain beforehand, that there would be fine weather at some of the stations and bad weather at others; and the feeling was universally entertained amongst those best competent to judge that, if fine weather prevailed at half the selected stations, astronomers would have every reason to be satisfied, provided always that the fortunate stations were not all in one or two regions, or in the same hemisphere. It was also considered that, if one half of the various methods on which reliance was placed should succeed, the result would be quite satisfactory. It will appear, from the facts I am about to bring before the reader, that these hopes have been much more than fulfilled. A large proportion of the stations had weather either altogether favourable, or sufficiently so to enable the observer to secure satisfactory results; while all the methods proposed for observation have been successfully applied (unless the Janssen turning-wheel arrangement be regarded as a distinct method rather than a modification of Delisle's).

For convenience, it will be well to consider the several methods separately, rather than to take the stations in order—in fact, no otherwise can clear or satisfactory ideas be obtained of the success of the whole plan of operations.

We may consider the methods in use as divided into three broad classes—the Delislean, the Halleyan, and the mid-transit method. This classification includes the photographic and heliometric operations, as well as the spectroscopic method for observing external contact. For, both photography and heliometry may be regarded as means for determining the position of Venus at given instants, either near the beginning or end of transit, or near the middle of her chord of transit; and observations of external contacts, either at the beginning or end of transit, come under the head of Delislean operations, while the observation of the interval of time between the two external contacts is a Halleyan operation. But also the Halleyan and mid-transit methods can be considered together, because all good Halleyan stations are good stations for observing mid-transit. Let us take the classes in the order indicated, viz., first, the Delislean; secondly, the Halleyan.

Speaking generally, it may be said that the English scheme was the only one which made direct provision for Delisle's method. Originally, as most persons know, the idea was that no other method was available at all; and, accordingly, we find not only that our official folk invited England to provide exclusively for Delisle's method, but assigned to other scientific nations their Delislean parts. Russia was to fortify the region for observing earliest ingress (around the point I in the plate), and France also was to provide stations for this phase, England occupying the Sandwich Isles (aided perhaps by an American party there, and another at Tahiti, unless it so chanced that America reserved her strength for the transit of 1882, her share in which was also assigned her by our official astronomers). France was to strengthen the forces near I′, the region where retarded ingress was to be observed, though here again England undertook the chief part. For the region around E′, where egress occurred earliest, England made herself entirely responsible; and, lastly, she undertook to occupy Alexandria, leaving to Russia the provision of stations on Russian territory around E, the place where egress occurred latest.

If this programme had been carried out, England would have reaped the chief honours, always supposing that the plan of operations had been justified by the actual conditions of the proposed work. England would of herself have made sufficient provision for applying Delisle's method, both at ingress and at egress, other nations taking only a subordinate part in the arrangements for either phase. Of course, if it had been discovered after the event that the proposed plan of operations took no account of at least one-half the chances of success, and left regions unoccupied which were even more important than those considered in the English official programme, England would have reaped something far different from honour. Other nations might then have been able to say that trusting in the unquestionable skill and



A B and A′ B′, separate sunlit and darkened hemispheres at ingress; along *a b*, *a′ b′*, sun 10° high at ingress.
C D and C′ D′, separate sunlit and darkened hemispheres at egress; along *c d*, *c′ d′*, sun 10° high at egress.
I, I′, are the Delislean poles for ingress; E, E′, those for egress; H, H′, the Halleyan poles.

the presumable diligence of official astronomers in England, they had been misled, and had missed a fine opportunity of advancing scientific knowledge. But as the matter presented itself six years ago, there seemed every reason to believe that England was about to effect an achievement more than worthy of her ancient fame in such matters.

It turned out, however, rather inconveniently, that "some one had blundered." Other methods were available than Delisle's—were even better suited to the occasion. Other regions than those indicated were of extreme strategic importance, one of them being indeed the key of the whole position. So far was England from having provided adequately for the whole scheme of operations—that is, from having prepared to do more than her fair share of work—that actually certain regions in British territory, which it behoved her and her only to occupy, had not even been mentioned in the official programme.

When this was discovered, it fortunately was discovered at the same time that other nations were willing to take at least as large a share in the work as England. It was also found that the other nations (whether convinced by the reasoning urged in England against the official arrangements, or for whatever cause) attached so little value to Delisle's method proper, that they might not unsafely be expected to devote almost their whole strength to Halleyan and mid-transit operations. This was convenient in more respects than one. It not only ensured adequate provision for the method hitherto so strangely neglected; but it left it in the power of English official astronomers to adhere to their original programme (or nearly so) without absolutely endangering the success of the whole scheme of operations. As they were apprised early of the views of other nations, official astronomers quickly adopted this course. They even overacted their persistent adherence to the Delislean as the sole available method. Where the durations of transit could be observed, they said that instead of observing the duration they would observe the absolute time of the beginning and ending, which is a very different thing—at least so they asserted. Nay, so completely were the other nations about to provide for Halleyan and mid-transit observations, that our official folk felt free to add new Delislean stations, or rather stations professedly Delislean; for some among them were open to the objection of being excellent also as Halleyan stations. As regards the neglected region in British territory (North India), official astronomers would at first make no concession; then they would have a photographic station there; next they would make the station also a Delislean one; and, lastly, they conceded all that had been asked, a thing hitherto unheard of in the annals of officialism.

While, however, no inconsiderable proportion even of the English preparations were directed to Halleyan operations (more or less disguised), it was to England, almost alone, that Delislean operations were left. The Americans declined *totidem verbis* to occupy any station where the whole transit could not be seen; the French refused to occupy the Mauritius or Suez (calling forth from English official astronomy an expression of strong dissatisfaction), and the Germans, though they occupied a station in Persia where the ingress could not be seen, yet specially provided for photographic work there during the middle of the transit. The Russians alone, having provided eleven Halleyan stations in Siberia, consented also to have observers at Delislean stations nearer to Russia in Europe, extending around E′ to the Black Sea, Caspian Sea, and Sea of Aral.

The only Delislean stations properly so termed near I were those occupied by the three English parties under Captain Tupman, in the Sandwich Isles, at Honolulu, Atooi, or Kanaü, and Owhyhee (or Hawaii). At the two first observations were successfully made, but at Hawaii, the least important fortunately of the three, the observers Forbes and Barnacle had bad weather. Detailed information has not yet reached us respecting the observations made by Tupman and his party at Honolulu, but it is known that the Janssen apparatus failed. The reader is doubtless aware that this apparatus was so arranged as to allow sixty photographs to be taken at intervals of a second—a large circular plate being so turned that picture after picture would be taken around the border of the plate; so that if the turning began half a minute or so before the true moment of internal contact, one or other of the pictures would depict Venus in true internal contact. Whether the manager of this organ-grinding arrangement (to adopt a humorous simile of the Astronomer Royal's) began turning too soon or too late is not known, for the telegraphic announcement from Honolulu states simply "Janssen failed." Sixty photographs were obtained there, presumably by this method, and these either represent Venus drawing nearer and nearer to the position of internal contact, or else they were all taken after she had broken away from the sun's edge. Unfortunately the telegram indicates only too clearly the probable cause of failure, in these words, "internal contact uncertain several seconds."

This points to a disturbed condition of the air, which would render all the observations unsatisfactory. One hundred and twenty micrometric measurements were made at this station; but if the cusps were affected by a disturbed atmosphere, it is to be feared that little reliance can be placed on them.

If the observations made in the Sandwich Isles should prove to be unsatisfactory, the only observations of ingress to be depended upon are those made at stations outside the region B *m* D, in Japan, North China, and the adjacent part of Siberia. At Port Possiet and Wladiwostock, near the western shores of the Sea of Japan, the ingress was well seen by Russian and American observers; but the acceleration there only amounted to about 6½ minutes, whereas at the Sandwich Isles it amounted to 11 minutes. From Yokohama and Nagasaki, the news which has hitherto arrived has been somewhat confused. First, news arrived that the Americans had been partially successful at Nagasaki, but had been troubled by clouds; next we heard from Nagasaki that the French, under Janssen, who had intended to observe at Yokohama, had been successful, presumably at Nagasaki. Then a telegram arrived correcting the Nagasaki date for Yokohama; and a writer in the "Times" seems to have regarded this as signifying that the French had observed at Nagasaki instead of Yokohama; but, possibly, what was really meant was that the French observed at Yokohama, though the telegram came from Nagasaki. Next news came from Yokohama through an Austrian source, that good observations had been made there, which the "Times" interpreted as meaning that an Austrian party had been stationed there, though until then nothing had been heard of such a party. Lastly, Janssen telegraphed that good observations had been made at Nagasaki and Kode. It seems probable, as I have remarked in the "Observer," that in reality one series of good observations was effected at Yokohama by the French under Janssen, and another at Kode; that the news was forwarded through Nagasaki, and that the Austrian news relates to the success at Yokohama, while Janssen's later telegram about observations at Nagasaki may relate to the work of a subordinate party. My reason for so interpreting the telegrams is that the Russians announced complete success at Yokohama, while the Americans announced only a partial success at Nagasaki; now the French party under Janssen achieved full success, which implies that they were at the station favoured with the better weather.

In any case it appears that accelerated ingress is fairly provided for; though at the best English stations for that phase Janssen's method failed, and the ingress was not very satisfactorily seen.

In the opposite region around I′ there were three English observing parties, two in Kerguelen Island, and one on Rodriguez; while Lord Lindsay's station at the Mauritius, though not specially intended for Delislean operations, was even better placed than the Rodriguez station. I shall speak more at length of Lord Lindsay's operations further on. Here it is only necessary to say that his party missed ingress, though otherwise successful. At Rodriguez ingress was successfully observed, but as yet no details have reached England. It is known that the various parties stationed on Kerguelen Island were successful; but the nature of their success is not known, all the news yet received from that dismal island having been derived from the interchange of signals between the parties stationed there, and a passing ship.

It would appear that a fair success has attended the employment of Delisle's method, as applied to ingress. At least, whatever defects may appear hereafter in the results by this method will be due to the inherent defects of the method itself, requiring as it does the observation of contact when the sun is not far from the horizon. Janssen's contrivance, by which it was hoped that this difficulty would be removed, failed entirely for ingress.

Turning next to egress, we have first to consider the operations around the point E′, where accelerated egress was to be observed. The stations provided by England for observing this phase were in New Zealand, though the Government Observatory at Melbourne, and the Observatory at Sydney were fairly placed. It must be remembered, however, that everything depends on securing observations at both ends of either Delislean base-line by similar methods and by observers similarly trained. The only stations thus provided for were those in New Zealand; and unfortunately bad weather prevented the observation of egress at any of these stations. The American party, stationed at Queenstown, in Otago, were able to observe and photograph all the transit except egress, but the English parties were not even favoured with this partial success—or rather, I should say, with a degree of success which, in their case, would have been but partial; for to the Americans the observation of egress was a matter of small importance. It is impossible not to sympathise with Major Palmer, on account of this unfortunate end to a campaign for which he had made very complete arrangements. As a member of his party, writing from Wellington, remarks, "it certainly seemed not too much to expect that, towards evening of a summer day in December, one of the finest months in the year (in New Zealand), an hour's clear sunshine might be vouchsafed to at least a considerable part of a colony somewhat larger than Great Britain and famed for the beauty of its climate. Unhappily, these hopes and expectations were crushed by a day of pitiless weather. All through the 8th, and till mid-day on the 9th, from every quarter of both islands, telegrams conveying the same dismal tidings of mist and rain, cloud, gloom, and falling barometers, poured in on Major Palmer, the English chief, warning him that unless some sudden and unlooked for change should very soon take place, the careful plans to which he and his colleagues had for so long given their time and energies would prove to have been made in vain. No change came, and failure was the result." "To crown their trouble, the day after the transit was provokingly, almost mockingly, fine. Some excellent sun-photographs, which were taken at Burnham on that day, showed how carefully the dry plates had been prepared, and how successful this, the least certain branch of the work, would have been. That the choice of stations was judicious, and that all was done that could be done with the means at command, is the opinion expressed everywhere." This opinion will certainly be shared in England also; nor will astronomers be slow to accord to Major Palmer their fullest sympathy for his misfortune. As was remarked of Le Gentil's failure in 1761, Major Palmer has "experienced one of those mishaps which assume to the man of science all the proportions of a real misfortune—to have traversed so large a portion of the globe, to have endured all the weariness, all the privations, all the perils of a long sea-voyage, and to have been able to effect nothing."

The Germans at the Auckland Islands, a station superior in value to any in New Zealand, achieved great success. Ingress, indeed, was obscured, "but ten minutes later the sun shone out, and, the sky remaining clear till the end of the transit, some 150 photographs, as well as the observation of egress, were obtained." The French at Caledonia Island saw everything except the egress. No news has yet arrived from Campbell Island, where the French had a station. If success was achieved there also, then egress has been well provided for; though the failure in New Zealand remains scarcely less to be deplored, because of the special arrangements made there for securing observations directly comparable with those made by the Government parties in Egypt.

Turning next to the region around E, where retarded egress was to be observed, we have to consider results of a mixed character. At all the best stations, viz., those occupied by the Russians, bad weather prevailed. The Russian Delislean observing parties were spread over Western Siberia, and included such important stations as Erivan, Tiflis, Taschkent, Astrakhan, Ornsk, Blagowestchensk, &c. At some of these stations the retardation would have been fully twelve minutes, whereas, at the English stations in Egypt and North India, the retardation amounted only to ten minutes. At Ispahan, where a German party was stationed, there would have been a retardation of $10\frac{1}{4}$ minutes; and as the results would have been directly comparable with those obtained at the Auckland Isles, a very valuable Delislean success would have been obtained had good weather prevailed at Ispahan. Unfortunately, though some good mid-transit photographs were secured, the contact at egress was missed through clouds. A Russian party, but not provided with first-class instruments, were successful at Teheran. But the chief success, so far as retarded egress was concerned, was obtained in Egypt and in North India, a circumstance which makes the ill-success of Palmer in New Zealand the more unfortunate, as it was with his observations that the Egyptian results were to have been compared. In Egypt thick haze prevailed on the important morning, until within a few minutes of the moment of contact. Then, however, the sun passed clear of the low-lying haze, and the contact was well observed. It was at first announced that Captain Abney had succeeded in getting a photograph of the contact by the Janssen instrument, but this news turned out to be incorrect. He just missed the actual contact; though it would appear that the moment of contact can be approximately determined from the photographs.* The German observers stationed in Egypt also made very satisfactory observations. At Roorkee, in North India, egress was well observed (as also the whole transit).

On the whole, the Delislean observations for egress have been fairly provided for. The special English plans have been defeated by bad weather in New Zealand; but the observations at Melbourne and Sydney will combine tolerably

* It seems tolerably clear that in future applications of the Janssen arrangement provision must be made for a longer interval than one minute.

well with those made in Egypt and at Roorkee. The German observations in the Auckland Isles will combine admirably with the German observations in Egypt.

Before passing to the operations by other methods, it may be well to consider the general Delislean results, and the lessons they teach us as to future operations. It is, in the first place, certain, that if only Delislean operations had been provided for, as in the original programme, the measure of success achieved would have been very far from satisfactory. Early ingress does not appear to have been well observed, and Janssen's method failed, so that the complete ingress operations are to some degree imperfect. The English special operations for observing egress have completely broken down through the failure in New Zealand. The success of the Germans is a strong point, but one success counts for little in a process where everything depends on reducing the final error through the *number* of successful observations. It is to be noted that the German success results entirely from the fact that the Auckland Isles were occupied. These are among the island groups to which I directed attention in 1869. Chatham Island, from which we hope to hear of good American egress observations, was another—rejected by the Astronomer Royal, because, by a miscalculation, he set the solar elevation 7° too low. Campbell Island, where the French have a station (not yet reported from), was yet another of these islands. All of these were specially named by me as suitable for applying the Delislean method at egress, and also for observing the whole transit. Their occupation by America, France, and Germany, and the success of the Germans (even though we should not learn that the Americans and French had also been successful), sufficiently justify my suggestions, and more than meet the comments made on these suggestions by official persons. On the other hand, the partial failure of the English Government operations in no sense supports my position, except in so far as to justify the anxiety I expressed. Bad weather might equally have thwarted the arrangements I proposed, which other nations carried out.

Turning now to Halleyan and mid-transit operations, we have a series of excellent results to consider.

In the first place, let us examine the northern successes. At Nertschinsk, where the duration fell short of the mean by fully 15½ minutes, the Russians observed the whole transit with excellent telescopes, and obtained a number of measures and photographs. This success is particularly gratifying, as Nertschinsk was the best of all stations for applying Halley's method. So far back as 1869 I called special attention to this point. But even so late as February, 1873, the Astronomer Royal declined to believe that observations would be made there. "Nertschinsk," he said, "is a station in high latitude, nearly 1000 miles from the nearest sea. I presume that its climate is truly continental. At St. Petersburg, in the winter, the sun sometimes is not seen for several weeks together. I suppose that the same may happen at Nertschinsk. I doubt greatly the probability that any observations can be made there." This he assigned as the sole reason why England should not occupy Southern Halleyan stations. Though four years had passed since I pointed to Nertschinsk as a suitable northern station, the fact remained still unknown to the person principally responsible for the English arrangements that *eighty per cent* of winter days are clear at Nertschinsk. Of course Russia occupied this excellent station. She also occupied ten others in the region extending thence to the Sea of Japan, obtaining more or less complete success at six of them. The Americans were successful at Wladiwostock and Possiet, the Germans at Chefoo, the French in Japan and at Pekin. The duration was not, indeed, secured at all the northern Halleyan stations, though it was at most of them; but where either ingress or egress was missed, the position of the chord of transit was effectually secured by mid-transit photographs and heliometric measurements. At Roorkee, in the long-neglected Indian region, the whole transit was observed and photographed under Col. Tennant's skilful supervision. The Germans photographed mid-transit at Ispahan, the Russians at Teheran. The whole transit was also observed by amateur astronomers at Kurrachee, Indore, and Calcutta, a fact rather showing what ought to have been done by official astronomers in England to strengthen the north Indian position, than (in all probability) adding much to the value of northern Halleyan operations.

In the southern hemisphere corresponding successes have been already reported, though as yet we have not heard from some of the best southern stations, nor have we sufficient news of the nature of the success known to have been achieved at Kerguelen Island. But the Germans were successful in securing mid-transit photographs in the Auckland Isles, and the Americans at Otago and in Tasmania. At Sydney, Melbourne, and Adelaide, the chord of transit has been well secured. At Melbourne, in particular, the observations may be regarded as presumably most valuable, on account of the Government observatory there. I do not know whether Mr. Ellery was able to make use of the instruments (belonging to the Astronomical Society), which Mr. Lockyer had so generously presented to him after the Indian eclipse of 1871. If so the Astronomical Society, though it should reclaim its property, may be disposed to pardon the slight mistake made in this matter. From the two French stations in St. Paul's Island and Campbell Island (two of my myths I suppose*) no news has yet been received. At Rodriguez the whole transit was seen, and mid-transit well photographed. The French also secured more than two hundred daguerreotypes at Caledonia Island, and these will prove exceedingly useful.

But the most complete mid-transit success was that achieved by Lord Lindsay's party at the Mauritius, where photographic arrangements much more satisfactory than those made at the Government stations were combined with heliometric observations of mid-transit and spectroscopic observations of exterior contact, neither of which

* I have been in considerable doubt as to the localisation of my "geographical myths." Admiral Richards was sufficiently ready to describe me as suggesting "geographical myths," but he has never indicated what he really intended by that remark. Of southern islands which I recommended, if accessible (and which I supposed, after Admiral Richards's rebuke, to be inaccessible, to say the least), the Germans have occupied one, the Americans another, and the French two others; while the French say, in their report, that they would have occupied more stations in the southern seas, if they could have supposed England would have left the field open to them, as she actually did. Another, Crozet Island, was to have been occupied by America, having been pronounced accessible by experienced sealing merchants, but bad weather prevailed for some time after the *Swatara* had reached the islands, and having to convey all the American observing parties, she was compelled to proceed on her way without effecting a landing there. (England, with ample time, and colonial possessions close by, might very easily have occupied this station.) Macdonald Island proved indeed to be barely accessible, and was not occupied, though this was not proving it to be a geographical myth. I do not know whether Royal Company Island and McQuarie Island are myths; but they are certainly marked in admiralty charts. Possession Island was described by Admiral Richards himself as easily accessible. Sabrina Land was the selected station for the transit of 1882. Where, then, are the "geographical myths?" Great indignation was expressed with me when I said that the islands in south seas seemed to be described by the Admiralty as myths or realities, "as might be most convenient to those in authority;" and it was absurdly stated, that in so saying I was accusing the Admiralty of making false entries in their charts. The exact reverse is the case. I believe in the Admiralty charts; but the statement that my proposed stations are geographical myths I consider inaccurate. It is not possible to land on myths, or to set up observatories on myths. As I explained to the Astronomical Society, I attributed nothing dishonourable to Admiral Richards; his myths were a mistake—that is all. I should have thought his remark ought long since to have been withdrawn (as publicly as it was advanced), and not without some sort of apology. But officialism sometimes overrides good manners.

methods had been provided for at all by our official astronomers.*

On the morning of the transit, at Mauritius, the sun rose concealed by clouds. Minute after minute passed without any sign of a break in the cloud-bank behind which the sun lay hidden. The important period of ingress, from first external contact to first internal contact, passed without a single opportunity of observing the motions of Venus as she entered on the sun's face. Had Lord Lindsay's party been relying upon the views entertained by astronomers five years ago, according to which only the Delislean method could be applied, their whole scheme of operations would have already failed, for the egress at Mauritius was not worth observing for that method. But the failure thus far signified only that one method out of three which the party had hoped to apply had failed them. We may say, indeed, that Halley's method

* It seems to me that Lord Lindsay's recent work in the cause of science is worthy of more than ordinary recognition. It is no new thing, indeed, for men of wealth and leisure to devote large sums to the advancement of science, though even in this respect what Lord Lindsay has done is noteworthy, seeing that the expedition fitted out by him will involve, first and last, a cost little short of the entire amount devoted to the British Government expeditions. The remarkable feature in the present case is the personal activity displayed in the cause of science by one who might well have been content with the contribution of some fifteen thousand pounds to a single scientific expedition. Moreover, it is to be remembered that this is not by any means the first of Lord Lindsay's services to science. On the occasion of the eclipse of December, 1870, Lord Lindsay fitted out an expedition at his own expense, all the preparations being much more complete than those made for either of our Government expeditions. On that occasion he accompanied his party, and took an important share in the work of observation. The next great astronomical event in which he assisted was the total eclipse of December, 1871. On that occasion he fitted out an expedition at great expense, the results obtained by which were the most important ever obtained in eclipse observation, seeing that Lord Lindsay's party stationed at Baikul secured a series of photographs of the corona which finally established the solar nature of that wonderful object. Lord Lindsay did not himself join the expedition to India, but he took a large part in the work of analysing the results then obtained. A laboratory was fitted up by him in London, where, in company with Mr. Davis (to whose skill the success of the operations was in large part due) and other skilful assistants, he investigated carefully all the details of the photographs obtained in India. Even at that time preparations were being made and experiments carried on for the expedition to observe the transit at the Mauritius. Lieut. Gill was placed as superintendent over Lord Lindsay's observatory at Dunecht, where all the instruments and methods to be employed during the transit were carefully tested. At the laboratory in London experiments were made by Lord Lindsay and Mr. Ranyard on the peculiarities of solar photographs, in order that the best method of photographing the progress of the transit might be adopted. Lord Lindsay not only took a share in such work, and in superintending the preparations at Dunecht, but visited continental astronomers, physicists, and instrument-makers, inquiring into the qualities of various methods, processes, and instruments, in order that every available means for securing the best results might be employed. Lastly, he was not content to send out the expedition thus carefully provided for, but himself went to the Mauritius and shared in the work of observation.

had also failed, seeing that the duration of the transit could not now be determined; but the essential object in Halley's method is to determine the position of the chord of transit, and it still remained possible that this end might be secured. Fortunately the clouds which had so long concealed the sun cleared partly away about one hour after transit began, and, though the sun was visible only for a few minutes, photographs and measures were obtained. It was not till 8 o'clock in the morning that it became fairly fine, and from that time the course of observation steadily proceeded until the end of the transit.

Lord Lindsay's place was in the photographic room. "I took," he says, "271 plates, out of which number perhaps 100 will be of value: cloud and the high temperature of the camera were much against me, the temperature varying from 96° to 100°. With the heliometer Mr. Gill obtained five complete determinations of greatest and least distance of the centres of the sun and Venus, besides nine measures of cusps, and two determinations of the diameter of Venus near the end of the transit. Dr. Copeland obtained fifteen measures of least distance of Venus from the sun's limb, and ten measures of cusps. The last internal contact was observed successfully, as also the last external contact.

I come next to a point which I would willingly pass over, but that the history of the transit would be incomplete without some account of it. It had been shown by me, in 1869, that Cape Town would be an excellent station for observing the middle of the transit. A letter published in the "Times," for February 15th, from Mr. Dunkin, informed astronomers that the egress of Venus was satisfactorily observed at Cape Town by Mr. Stone, Astronomer Royal at the Cape. The satisfactory observation of egress at Cape Town unfortunately counts for very little more than—say—the satisfactory observation of a sun-spot on the morning of the transit. The valuable stations for egress were those already considered, where egress was either notably accelerated near E′ or notably retarded near E. Cape Town is remote from either region, and observations of egress there had scarcely any value whatever. But Cape Town as a southern mid-transit station was absolutely better than any station occupied either by our own country or by others. Mid-transit photographs secured there, especially with all the advantages of a well-provided national observatory, would have been invaluable: so, also, would have been heliometric measures of Venus at the time of mid-transit, and afterwards until the end of the transit. It appears only too certain, from the absence of all mention of any such work in Mr. Dunkin's letter, that absolutely no provision whatever had been made for these most important purposes.* If the opportunity of utilising the Cape Town Observatory for mid-transit observations had simply been overlooked, little need have been said. That would have been no novelty, unfortunately. But special attention had been directed to the value of the station. In reply to a letter by Admiral Sir H. Cooper Key, inquiring whether Sir G. Airy had made arrangements for photographing mid-transit, the answer came that the method had been amply provided for; yet at the very best station for the purpose no provision had been made, though the station was exceptionally suitable, because of the Government Observatory there and the presence of skilled astronomers. This would have been unfortunate as a mere case of negligence, but in its real aspect the matter is much more serious. It will not readily be forgotten.

In summing up the results of Halleyan and mid-transit operations, we must distinguish between contact observations, photographic results, and heliometric measurements. We must also draw a distinction between the various modes of photographing the transit employed at different stations.

It seems probable that in future transits less reliance will be placed on contact observations than on photographic work. It is true that the results obtained during the recent transit show that the phenomenon of the "black drop," which in 1761 and 1769 occasioned so much trouble, depends on instrumental imperfections and atmospheric disturbances,† and can be practically eliminated by employing good telescopes and choosing stations where the sun will not be too low at the time of either internal contact. Nevertheless, a "personal equation" comes in, depending on the fact that the eye itself is part of the optical arrangement for observing

* It is impossible not to connect this with what happened in the case of the important total solar eclipse of April, 1874, when totality lasted more than four minutes. On that occasion, though the track of total shadow passed close by our Cape Colony, Mr. Stone received no assistance whatever towards the proper observation of the eclipse. He had not even an equatorial telescope; but was obliged to observe with an altazimuth telescope "borrowed from Mr. H. Solomon," to which Mr. Stone attached a spectroscope with "wrappers of wash-leather," for want of more suitable appliances.

† Mr. Stone's ideas on this subject, on which so much stress was laid in 1868, have been entirely overthrown by the recent transit observations. Of all whose results have reached us, Mr. Stone himself was the only observer of skill who, with a good telescope, saw any approach to the "black drop" required by his theory. Certainly he saw and pictured what accorded most perfectly with his own ideas; but that only shows how likely preconceived opinions are to make the observer fancy he sees what he thinks he ought to see.

contact, and thus the observed moment of contact depends—to the extent of three or four seconds at least—on the observer's idiosyncrasies. This equation may be estimated by practice on models; but it must always remain doubtful how far, in the excitement of transit observation, the personal equation remains the same as in the calm of "model transit" operations.

Heliometric measurements, again, seem inferior to the instantaneous work of photography. On any reasonable assumption as to the skill of the observer, it is impossible to believe that he can measure the position of Venus with an accuracy comparable to that with which the photographic picture can be measured, if only that picture is clearly defined, and not affected by imperfections such as would render the process of measurement uncertain. For instance, specks and stains on the photograph do no harm. A contraction of the film in photographs on glass would be mischievous, as also would be the effects of so-called photographic irradiation, if measurement depended on the size of the photographic image of the sun as affording a scale of measurement, while of course any optical defects would be fatal. But if such sources of error as these last can be in any way avoided, then photography must take its rank as *facile princeps* among the available methods for dealing with transits of Venus.

Now here, again, we approach one of those delicate questions unfortunately so numerous in the history of recent transit operations. English official astronomers were content to take the opinion of Mr. De la Rue, a skilful photographer of celestial objects, doubtless, but in the first place likely to view this matter solely in its photographic aspect, and secondly (as is manifest by his own account of the qualities of the method he advocated), unaware of the exact astronomical requirements for success. In this method, that used in the Kew Observatory, the image formed at the focus of the object-glass is optically enlarged before it is received on the photographic plate, and consequently its proportions depend on the instrumental adjustment, so that the only means of determining the scale of the sun picture is by a reference to the picture itself. But if there is any photographic irradiation or shrinkage of the collodion film, the sun image will be affected; in other words, *the scale of measurement will be falsified*. Now it is asserted by nearly every one who is at once competent to pronounce on photographic matters and acquainted with the mathematical aspect of the question, that there is no such accuracy in the outlines of the sun and Venus, obtained by De la Rue's method, as is requisite in the delicate problem of measurement involved. The photographic delineation ought to be not only the best possible, but ought to be practically perfect. What, however, do we hear (when it is too late) from the very person who was responsible for the employment of the best available method, and at whose instance not only the English, but the Russian and German, arrangements for photographing the transit had been selected? The Astronomical Society is calmly informed that the French method of photographing the transit (on silver) is *infinitely* more exact than the method of photographing on glass. I do not know in what sense the Astronomer Royal used the word infinitely; but he is a mathematician, and therefore probably used the word in its mathematical sense; that is to say, regarding the trustworthiness of the French photographs on silver as represented by some finite quantity t, the trustworthiness of the photographs taken by the English, Russian, and German Government parties, is in the Astronomer Royal's opinion,—

$$= \frac{t}{\infty} = 0.$$

We cannot blame Mr. De la Rue for proposing a method thus rejected as utterly untrustworthy. He was not the responsible person. Dr. Rutherford, in America, advocated the same method, which probably seems to the photographer the best available. But the American astronomers responsible for the photographic arrangements rejected this method, showing by their calculations that it could not be trusted. Our official astronomers have also found this out. But there is this trifling difference—American official astronomers made the discovery before selecting a method for photographing the transit; ours made the discovery a month after the transit had taken place.

The Americans met the difficulty, not as the French did, by taking daguerreotypes, but by using an object-glass of great focal length (40 feet). It is manifest that with such an arrangement, the true focal length being known, the scale of the sun picture is determined independently of the apparent size of the solar image. For the focal image is as large as a disc which, seen at the focal distance, would look just as large as the sun; and the sun's apparent diameter at the time of transit being known, as also the true focal length, we can calculate how large such a disc would be. The photographic picture may be slightly larger or slightly smaller through photographic defects,—expansion by irradiation or shrinkage all round the border; but, as we have no occasion to measure the diameter of the disc, this is a matter of perfect indifference. All we require to know is the distance, in any picture, between the centres of the discs of the sun and Venus, and whether the outlines of these discs have contracted or expanded the positions of their centres can be accurately determined. The great difficulty to be surmounted consisted in the construction of suitable mirrors for the heliostats. An ordinary telescope, 40 feet in length, was out of the question, since no available machinery could direct such a telescope continually towards the sun. The construction of a flat mirror was a matter of extreme difficulty. As Prof. Newcomb pointed out—"The slightest deviation from perfect flatness would be fatal: for instance, if a straight-edge laid upon the glass should touch at the edges, but be the 100,000th of an inch above it at the centre, the reflector would be useless." But with such opticians as Alvan Clark and his sons this difficulty was not found insuperable, and the mirrors provided for the eight American parties were to all intents and purposes perfect.

Lord Lindsay selected the same method. The mirror for his heliostat was constructed by Repsold, and worked excellently.

We must, then, in considering the photographic results, attach chief—if not sole—value to the successes obtained by the Americans and Lord Lindsay using the long focal method, and by the French using Daguerre's process. Fortunately a sufficient number of results have been obtained by both methods, and in both hemispheres, to ensure the determination of the solar parallax to a much greater degree of accuracy than heretofore. I think it is not too much to hope that the sun's distance may now be ascertained within limits of error not exceeding 100,000 miles on either side of the true distance.

On the whole, Science has every reason to congratulate herself on the results achieved during the observation of the late transit. There was a good deal of blundering at the outset, and there were some points to be regretted in the final arrangements, but the more important mistakes were corrected in good time. In portioning to the different nations the honours due to them, we must assign to some countries special credit for some matters, while in other matters other countries took the lead. America devoted a larger sum to the expenses than any two nations together, and adopted excellent arrangements. Russia provided for the greatest number of stations; in fact, for more than England, America, and France together. Germany alone combined photographic and heliometric operations in both hemispheres. France distinguished herself by occupying the greatest number of difficult island stations in the southern seas. Lastly, to England must be assigned whatever credit is due to the first discussion of the subject, and the promulgation of a programme for the whole scientific world: had this programme been but correct, and had other nations only accepted the parts assigned to them, England would have been as easily first as she was in 1769, and as she may be—who knows?—in 1882.

IV. THE QUESTION OF ORGANIC EVOLUTION.*

WE, in the latter half of the nineteenth century, are fated to be present at one of the most striking revolutions which have ever occurred in the interpretation of the universe. The announcement of the heliocentric theory of the heavens, the discovery of the law of universal gravitation, the overthrow of the phlogistian chemistry, the creation of the science of geology, and the modern doctrine of the conservation of force are, indeed, philosophically, steps of equal value. But probably none of them, not even the first mentioned, has given rise to so much and so intemperate controversy. Forty years ago, when the "Beagle" was prosecuting her voyage of discovery with the young and unknown naturalist Charles Darwin on board, zoology and botany were the quietest, the least exciting of the sciences. Deemed altogether remote from human interests, with scant fame and scanter material rewards to confer upon their votaries, they were sneered at by poets,† and viewed by statesmen, ecclesiastics, and men of business with supreme indifference. Now all is changed: over a large portion of the civilised world, a zoological question is debated with an interest passing into acrimony. "With the exception of ecclesiastico-political topics, no sphere of thought agitates the educated classes of our day so profoundly as the doctrine of descent." "Darwinism is meat and drink to the daily papers, and to the political and theological periodicals." The terrible truth has dawned

* The Doctrine of Descent and Darwinism. By OSCAR SCHMIDT. London: H. S. King and Co.

Evolution and the Origin of Life. By H. CHARLTON BASTIAN, M.D., F.R.S., &c. London: Macmillan and Co.

† For instance, Ebenezer Elliot, the Corn-law Rhymer.

upon the world that laws, at first deemed applicable solely to "dumb animals"—to "mere brutes"—extend to man. The theory of ferns and flowers, of beetles and butterflies, has become a theory of life. Notions which have been unfortunately entangled with our creeds, intertwined with our earliest education, and which underlie many of our colloquial expressions, are threatened. No wonder that, as Professor Schmidt expresses the matter,—"Since to non-scientific persons the notorious relationship with apes is the alpha and omega of the Doctrine of Descent—since the most confused heads are often most thoroughly convinced of their own pre-eminence—on no subject do we so frequently hear superficial opinions, mostly condemnatory, and all evincing the grossest ignorance."

But whilst the new doctrine has met with so hostile a reception from those ignorant of the organic sciences, if not of science altogether, or at least dominated by a radically unscientific spirit, its welcome among candid, working naturalists, capable of understanding the evidence on which it rests, is equally remarkable. Agassiz, one of its bitterest opponents, though his own researches had helped to prepare its advent, exclaimed almost passionately that he did not expect to see so many of the best minds of the age range themselves under the standard of Evolutionism. To use an expression of Professor Schmidt's: "Scales fell, as it were, from the eyes of fellow-labourers and spectators, and the rapidity with which the theory makes its way affords the best evidence that it took shape, and was proclaimed at the proper moment." Its effect upon the sciences concerned has been the best argument in its favour. Stray facts, unintelligible, or at least unaccountable, observations, at once arrange themselves in definite order, like well-disciplined soldiers at the tap of the drum. Zoology and botany have received a purpose which before they lacked. Best of all, the necessity of verifying the "Darwinian" doctrine has produced and is still producing a plentiful harvest of discoveries. The works of Evolutionists are not barren apologetics for a stationary creed, but successful applications of a vital principle. Different thinkers may attach greater or less importance to "natural selection," the power by which, according to Darwin, the mutation of species, is mainly effected; other powers, modifying, controlling, and extending, may and probably will be brought to light. But the doctrine of evolution itself, we believe, has scarcely failed to receive the assent of any conscientious and qualified naturalist who has given it a fair trial. "The proof of the pudding," says a homely old adage, "is in the eating," and the proof of a theory, in the same manner, is in the working.

Professor Schmidt points out that, although the honour of this great discovery belongs incontestably to Mr. Darwin, yet previous thinkers had been turning their inquiries in the same direction. Of these, the foremost place, perhaps, belongs to Lamarck, who, in the earlier part of the century, was loudly shrieked over by the Cuvierian school (then dominant). As early as 1804, in his "Philosophie Zoologique," he enunciated views which, had they been worked out in detail, and verified by carefully selected evidence, would have almost anticipated Darwin. He held that our systematic definitions and gradations are purely artificial; that nature has produced neither orders, families, genera, nor even immutable species. He regards, as the main cause of divergence from the original type, peculiarities acquired by the efforts of animals to accommodate themselves to a change in external circumstances, and the use or disuse of organs—causes which certainly cannot have been operative in the vegetable world. Of our great comparative anatomist, Richard Owen—another herald of the avatar of Evolution—Professor Schmidt remarks: "From the ichthyosaurus to man he sees the connection of descent; he denies that the influence of circumstances is decisive; he rejects a dozen times any kind of miracle; but the next moment he cleaves to miracle again, namely, to an innate tendency towards a certain future development, not imposed by circumstances and dependent on them, but conducive to a special purpose. Thus deal the trimmers, who through fear of consequences appease their scientific consciences with a word."

The opinion that "Goethe actually proclaimed the Doctrine of Descent, or was, even in a poetical sense, its inspired prophet," though entertained by Haeckel is rejected by Schmidt.

It may prove interesting briefly to examine some of the leading doctrines of the old school, and to note the transformation they have undergone in the light of Evolutionism.

By the old Natural History, species—in contradistinction to variety or sub-species on the one hand, and to sub-genus on the other—was viewed as a something objective, and capable of a rigorous demarcation. For its recognition a two-fold criterion was put forward; a certain degree of mutual resemblance or morphological unity in all essential characters, and a kindred connection by the ties of a common descent from a hypothetical ancestral pair. On the faith, chiefly, of one well-known negative instance,* it was assumed that all the individuals of a species—even though belonging to distinct varieties—were capable of producing fertile progeny, whilst sexual intercourse between members of different species resulted either in sterile offspring or in none at all. It is surprising how much was here taken for granted. Both these tests utterly failed in practice. The physiological criterion, baseless as we now know it to be, was neither applied, nor indeed capable of application, in one case in a thousand. Who ever tried whether every two supposed species of some closely connected group of insects were or were not capable of producing prolific offspring? Experiments of this nature, continued for a few generations, can only be carried out upon animals in captivity. Now it is well established that, under such circumstances, certain animals refuse to breed at all. Undoubted varieties have been found which will not propagate with the original race. Thus "the variety evolved in Paraguay from our domestic cat pairs no longer with its ancestral stock, nor does the tame European guinea-pig with the wild ancestral stock of Brazil." Where, again, is the proof that the forms of dogs, known to be mutually reproductive, are all sprung from one common ancestry? The balance of evidence rather shows "that in various parts of the world, and at various periods, wild species of the genus Canis have been domesticated, of which the crosses produce fertile progeny to an extent almost unlimited." The orthodox zoologists of the schools of Linnæus and Cuvier thus reasoned in a circle, proclaiming that "forms belong to the same species, because they may be fruitfully crossed, and because they may be fruitfully crossed they belong to the same species; and, on the other hand, because such and such forms, when crossed, produce no fertile progeny, they constitute different species, and because they are different species, they generate no fertile offspring."

Nor was the morphological test more happy. To decide what variations of form indicated mere "sports," sub-species, and varieties, and what, on the other hand, were grave and persistent enough to constitute species, was left to the tact or the prepossessions of each systematist. The results were as discordant as might be expected. Thus, taking the true

* That of the mule. It has since been established that the hare and the rabbit, two species quite as distinct as the horse and the ass, are capable of producing offspring which, after many generations, show no decline in fertility. Among birds, fruitful hybrids are known to be produced both amongst the warblers and the gallinaceous tribe.

Geotrupidæ found in Britain, some entomologists resolve them into ten species, and others only into two.

Pages might be filled with cases where certain forms have been classified as distinct species, till the discovery of a series of intermediate individuals has reduced them to one. Haeckel points out that the calcareous sponges may be either comprised under one species, or, with equal justice, divided into 591. Certain characteristics are supposed sufficient to prove specific diversity among the true cats,* whilst in the genus homo more essential features are held, merely to indicate "varieties" or "races."

The stern logic of facts, therefore, compels us to admit that "species" is not capable of rigid definition; that between it and "race," or "variety," there is no absolute demarcation, the one passing by insensible gradations into the other; the one as well as the other signifying merely a group of beings, simultaneous or successive, which resemble each other more closely than they resemble anything else. Yet this ill-defined something the old school call upon us to admit as, in its essence, immutable!

It is still urged that vague, indefinable, and subjective as the idea of a species may be, we have never witnessed one species actually transmuted into another. This objection is, however, little better than irrational. An ephemeron may never have witnessed the evolution of the frog from the tadpole. Is the change the less real on that account? When we shall be able to compare series of specimens, anatomical preparations, and carefully-executed photographs of animal species extending at least over a few thousand years, it will be quite early enough to raise this point. To draw any conclusion from the identity of the domestic animals as preserved in Egyptian mummies or figured on Egyptian monuments, with the same species as now found in modern Europe, is presumptuous in the extreme.

But the question may be retorted: who has ever known any organic being produced by the alternative method, a direct and immediate act of special creation? The stone record tells us of species that have disappeared, and of others that sprung up in their stead. Have the latter arisen in full maturity from the ground, or been condensed

* The domestic cat occasionally exhibits all the range of colour and design found in the genus Felis, exclusive of the Lynxes. We find concolorous individuals reminding us of the lion, puma, and black tiger; others with vertical, tiger-like stripes; others, again, with the cloudings of the ocelot, and others with bands of spots closely resembling those of the leopard. If irregular crossing were prevented, there can be little doubt but each of these types could be distinctly developed.

from the air, fitted up by a *Deus quidam deceptor*, with Mr. Gosse's ὀμφαλος? The individual animal is evolved according to unvarying law from antecedent animals, at first as a simple and rudimentary cell, but gradually becoming differentiated and individuated as a complex and separate being. Does not analogy indicate that such, too, must have been the origin of the groups we call species? Astronomy, physics, chemistry, can dispense with miracle; zoology and botany must prepare to do the same if they are to be recognised as sciences at all.

Turning from general considerations to individual cases, let us take the following evidence:—" In the whole series of strata," says Hilgendorf, " the varieties of *planorbis multiformis* are distributed in such a manner that individual layers are characterised as successive strata, by the exclusive recurrence or by the predominance of single or several varieties, which, within the layer, remain constant, or slightly variable, but towards the limits of the next layer, lead by transitions towards the succeeding forms. The intermediate layers furnish evidence that the other forms originated by gradual metamorphosis from the earlier ones; they, moreover, render it possible to range form to form, and to trace the evolution backwards; hence it becomes manifest that, what above seemed distinctly divided meets below." " The forms diverge so greatly," adds Professor Schmidt in comment, " and are so constant in the main zones, which tell of periods of repose, that in accordance with old conchological practice, they would be unreservedly claimed as species *if the connecting links were not too conspicuous.*"

Finding such connecting-links—such plain evidence of continuous evolution in case of animals which, like the shell-bearing mollusca, have a characteristic portion of their structure composed of somewhat permanent materials—is it too much if we infer that, were the stone record undamaged, and were all animals equally imperishable, a like continuity would meet us in every group?

Turning to species still existent:—" Who that has read the comprehensive investigations of H. Müller can doubt that the honey-bee, as it gradually attained its bodily advantages and peculiarities, developed likewise the higher mental powers, corresponding with the more minute and complex organism of her brain?" Or, again, look at the Ancon sheep —a mere variety, commonly so called by those who refuse to see that between species and variety no valid line of separation can be drawn. Yet had this mere variety been placed in sole occupation of some country where it might have multiplied undisturbed, there is no doubt that in the course of ages it would have reached a point at which it would no longer have been capable of productive intercourse with the original stock, as we have seen is the case with the tame guinea-pig. Let us then suppose that its *habitat* had been visited by Cuvier and a band of his disciples. Unless specially informed of its origin, would not these sages calmly note it down as a "good species?"

The origin of parasites, internal and external, has long been a stumbling block to naturalists. Were these beings created simultaneously with the species they infest, or have they been produced by some subsequent miracle? Such were the questions asked by the old school. It was admitted that these vermin are not now found living otherwise than parasitically; that their present structure unfits them for an independent life, and that many of them are peculiar to some one particular animal, or even to some part of an animal. They were the *bêtes noires* of teleologists, none of whom, we believe, has yet made the tape-worm or the Trichina the subject of an extra Bridgewater Treatise. But in the light of the doctrine of evolution the difficulty vanishes. In successive generations these animals have become gradually modified to the places they inhabit and the parts they play; so that to trace what were their original forms and attributes may now be beyond the reach of the human intellect. Do not similar considerations—the gradual modification of certain low organisms, animal or vegetable, or, perhaps, doubtful—explain the origin of such diseases as rabies, syphilis, and variola, which in our days never occur except in consequence of transmission from one animal to another, and which we cannot conceive as primordial?

We are thus naturally led to one of the cardinal doctrines of the old Natural History. Who of us entomologists, when curiously examining some specimen, has not been accosted, perhaps by a child, perhaps by a grown-up outsider, with the question: "What is the use of that creature, and to what end was it made?" On our replying, as might often be the case, that to the best of our belief and knowledge it was of no use at all, but decidedly pernicious, have we not been told that "every living thing was made for some good end?" This doctrine is somewhat inconsistent in the mouths of sparrow- and rook-shooting farmers, aphis and red-spider-poisoning gardeners, and a very large majority of the human race. It is, too, decidedly obscure. Are all animals useful to man, or to the universe at large? If we take the former view, we shall generally have to strike a balance between the good and ill deeds of a species, and shall very often find the latter preponderate. Even the Rev. J. G. Wood confesses himself unable to "make out a case" in favour of the earwig. If we attempt to prove all species beneficial to the universe at large, our difficulties become still greater. But the new Natural History solves these enigmas. It shows us that an animal exists, not because it is beneficial to man, or because it is an integral and necessary, though minute, part of some vast mechanism, but because it is in harmony with the conditions in which it is placed. The house-fly lives and multiplies, neither to scavenge our dwellings, nor to propagate disease by conveying putrescent and morbific matter to our food and our persons, but because it so far has come off victorious in the struggle for existence. The burying-beetle, which is a far better scavenger, and which does not distribute zymotic poisons, is rare, and becomes rarer, because the conditions under which it can multiply are less easily met with.

Another doctrine of the old school, now in the course of rapid decomposition, is that the fauna and flora of every district are perfectly adapted to the climate, soil, and other conditions of that district. So far is this view from according with facts, that we very often find a foreign plant or animal, on introduction into a country, flourish with such luxuriance as to interfere with, and even extirpate, certain native species. That such should be the case is perfectly conceivable to a Darwinian, and as perfectly inconceivable to the opponents of the doctrine of Evolution.

Professor Schmidt is one of the most consistent and thorough-going of his school. Like Messrs. Darwin and Haeckel, but unlike Messrs. Wallace, Henslow, Murphy, &c., he does not stop short at the anthropoid apes, but embraces man also in his deductions. But he differs from Darwin at the opposite end of the series—the first beginning of life. Here we would wish, without advocating, briefly to expound his views. He quotes from Du Bois-Reymond this passage: "It is once for all incomprehensible how, to a mass of molecules of nitrogen, oxygen, carbon, hydrogen, phosphorus, and so on, it can be otherwise than indifferent how they lie or move; here, therefore, is the other limit to the knowledge of natural science—the former limit being the incomprehensibility of matter and motion. Whether the two limits to natural science are not, perchance, identical, it is, moreover, impossible to determine." Professor Schmidt then adds: "In these last words the possibility is indicated that consciousness may be an attribute of matter, or may appertain to the nature of atoms. And we may add that the attempt has of late been repeatedly made to generalise the sensory process, and to demonstrate it to be the universal characteristic of matter, as by von Zöllner, in his work on the Nature of Comets. . . . We must either renounce the possibility of comprehending the phenomena of sensation in the organism, or 'hypothetically add to the universal attributes of matter, one which would cause the simplest and most elementary operations of nature to be combined, in the same ratio, with a process of sensation.'" In the lower animals the phenomena of life assume a more vegetal character; and in many groups of lower beings, which Haeckel has recently comprised under the name protista, we see the processes of metamorphosis of tissue, nutrition, and reproduction taking place indeed, but in a manner so simple and undifferentiated, that we must attribute to these beings a neutral position between plants and animals. We gain the conviction that the roots of the vegetal and animal kingdoms are not completely sundered; but, to continue the simile, merge imperceptibly into each other by means of a connective tissue. In this intermediate kingdom the much derided primordial slime of the natural philosophers* has regained its honourable position. Many thousand cubic miles of the sea-bottom consist of a slime or mud, composed in part of manifestly earthy inorganic portions, in part of peculiarly formed chalk corpuscles; and finally, which is the main point, of an albuminous substance which is alive. This living slime, the so-called Bathybius, does not even exhibit individuality, or the definiteness of a separate existence; it resembles the amorphous mineral substances, each part of which bears the characteristics of the whole. . . . . It is only with great exertion that we are able to accommodate ourselves to the idea of a living mass, either absolutely formless and undefined, or defined arbitrarily and accidentally."

We are far from desiring to encounter speculations like the above with ridicule. But before they are accepted they must undergo a verification, which must be left as a difficult and splendid task for posterity.

We are thus naturally brought face to face with the question to which Dr. Bastian has long devoted his attention, the Origin of Life.

It was once supposed, both by the learned and the vulgar,

* The German "Naturphilosophen," as applied to a certain school of thinkers, does not correspond to our "natural philosophers." Physiophilosophers, or philosophers of nature, might be the best rendering.

that animals were occasionally produced without parents, in the way known as "equivocal" or "spontaneous" generation. Virgil's recipe for producing a swarm of bees out of the carcase of a bull is a curious relic of this belief—the more valuable from the light it throws on the habits of thought prevalent among men of culture in the palmiest days of classic antiquity. In the dark ages, insects, reptiles, and the like, were supposed to originate spontaneously from putrefying organic matter and mud, or to be called into existence by the malice of witches. Rösel, in his quaint "Insekten-Belustigungen" and Swammerdam, in the "Biblia Naturæ," were at great pains in refuting this view by tracing beetles, butterflies, &c., from the egg, through the larva and pupa to the perfect state. By degrees the doctrine "*omne vivum ex ovo*" was established as the orthodox confession of the scientific world. Even the lowest types of animal and vegetable life were proclaimed, on experimental evidence, incapable of originating except from pre-existing germs, no matter how favourable the circumstances. Professor Lister declares that "the doctrine of equivocal generation has been chased successively to lower and lower stations in the world of organised beings, as our means of investigation have improved." Mr. Justice Grove, in his presidential address at the meeting of the British Association, in 1866, observes that, "if some apparent exceptions still exist, they are of the lowest and simplest forms." The experiments of Pasteur, who has taken up this question more in the spirit of a political or theological partizan than that of a philosopher, are generally supposed conclusive as against the doctrine of abiogenesis. The doctrine of Descent has led to a re-examination of the question. It must not, indeed, be supposed that a belief in Evolution necessarily implies the admission of equivocal generation. Darwin himself, as we have stated above, seems to contend that, though organic life being once enkindled upon our globe, its development and extension follow according to natural laws, yet that its origin may have been due to the direct intervention of creative power—in other words, to miracle. Sir W. Thomson derives the first rudiments of life from "a moss covered fragment of another world,"—a solution, or rather evasion, of the difficulty, the weakness of which has not escaped the notice of anti-evolutionists. Professor Huxley, whilst denying that life ever originates from lifeless matter *at present*, holds that were it given him "to look beyond the abyss of geologically recorded time," he might then indeed "be a witness to the evolution of living protoplasm from non-living matter." These admissions show that, consistently or not, abiogenesis is not the universal creed of evolutionists. On the other hand, it must be conceded that, if equivocal generation is once demonstrated, the opponents of Darwinism have no longer a cause to defend. Heterogenesis—the transformation of organisms already existing—is conceivable without abiogenesis, but abiogenesis necessarily involves heterogenesis.

Under these circumstances it is highly desirable that the question should receive the fullest attention, and be decided, if capable of decision, in a manner which will admit of no appeal.

Towards this final solution, Dr. Bastian furnishes an important contribution in the work before us. He describes the following experiments, which we are bound to pronounce most significant.

Experiment I.—A strong infusion of turnip was rendered faintly alkaline by *liquor potassæ*, and to this a few separate muscular fibres of a cod-fish were added. Some of this mixture was introduced into a flask of nearly two ounces capacity. Its neck was drawn out and afterwards hermetically sealed by the blowpipe flame, while the fluid within was boiling. When thus closed the flask was about half full of fluid. It was then introduced into a digester, which was gradually heated, and afterwards kept at a temperature of 270° to 275° F. for twenty minutes, though it seems well to point out that, if we include the time taken for the water of the digester (in which the closed flask was immersed) to attain this heat, and also again to cool down to 230° F.; this flask was exposed to temperatures above 230° F. for one hour, as I myself carefully noted at the time. When withdrawn from the digester the closed flask was kept at a temperature of 70° to 80° F. for eight weeks, and during part of this time it was exposed to direct sunlight. After it had been ascertained that the flask was free from all crack or fault, its neck was broken in order that its contents might be examined. The reaction of the fluid was found to have become decidedly acid, and it had a sour though not fœtid odour. The fluid was very slightly turbid, and there was a well-marked sediment, consisting of reddish fragments and a light flocculent deposit. On microscopical examination the fragments were found to be portions of altered muscular fibre, whilst the flocculent deposit was composed for the most part of granular aggregations of Bacteria. In the portions of fluid and of deposit which were examined, there were thousands of Bacteria of most diverse shapes and sizes, either separate or aggregated into flakes. There were also a large number of monilated chains of various lengths, of a kind frequently met with in abscesses and other situations where pyæmia or low typhoid states of the system exist in the human subject. There where, in addition, a large number of *Torula* corpuscles, as well as of brownish nucleated spore-like bodies, gradually increasing in size from mere specks, about 1-30,000th up to 1-2500th of an inch in diameter. Lastly, there was a small quantity of a mycelial *Fungus* filament, bearing short lateral branches, most of which were capped by a single spore-like body."

Here, therefore, both animal and vegetal organisms make their appearance. The second experiment is not less conclusive :—

"A strong infusion of common cress, to which a few of the leaves and stalks of the plant were added, was enclosed in a hermetically-sealed flask in the same way, heated in the digester at the same time (and therefore to the same temperature), and was subsequently exposed to the influence of the same conditions as already mentioned in connection with the last experiment. This flask was, however, opened one week later. Before breaking the neck of the flask, the inbending of the glass under the blowpipe flame showed that it was still hermetically sealed. The reaction of the fluid was distinctly acid, though there was no notable odour. The fluid itself was tolerably clear and free from scum, but there was a dirty-looking flocculent at the bottom of the flask. On microscopical examination much altered chlorophyll existed, either dispersed or aggregated amongst the other granular matter of the sediment, and amongst this three minute and delicate *Protamœbæ* were seen, varying in form, and creeping with moderately rapid slug-like movements. In the same drop of fluid more than a dozen very active monads were seen, each provided with a long, rapidly-moving lash. There were also several unjointed *Bacteria*, presenting most rapid progressive movements. Many *Torula* corpuscles and other fungus spores also existed, as well as portions of a mycelial filament, containing equal segments of colourless protoplasm within its thin investing membrane."

We especially call attention to the following sequel :—
"A drop of the fluid containing several of these active monads was placed for about five minutes on a glass slip in a water-oven, maintained at a temperature of 140° F. *All the movements of the monads ceased from this time, and they never afterwards showed any signs of life.*"

These experiments, which, as the author informs us, "are merely two selected from several others in which even higher temperatures, were originally had recourse to in order to free the fluids and flasks generally from anything like a trace of living matter," are very important. If we would deny that in such cases life is produced, not *ex ovo*, only two courses appear to be open. We must either—which Dr. Bastian himself calls "the shortest way out of the difficulty"—deny the facts, or we must, in the face of experiment, maintain that low organic forms, or their germs, can sustain a temperature of 275° F. without injury. This, as far as we are aware, has not yet been accomplished. On the contrary, numerous direct experiments prove that *Bacteria* and their germs "are uniformly killed by an exposure to 140° F. for five minutes." Some authorities "assume that the mere minuteness of the germs of *Bacteria* may serve to protect them from that destructive influence which heat exerts upon living matter generally." This doctrine, we need hardly say, has not the shadow of a fact in its favour. Not less baseless is the notion that some germs may escape the destructive action of boiling water by being spurted out of the fluid on to the sides of the glass, when the process of boiling commenced. In a sealed flask this is utterly out of the question, since every part of the interior is acted on by the steam. As to the assumed "protective action of lumps," we fail to see its validity. No matter how badly any kind of organic matter may conduct heat, a tiny particle placed in a relatively large quantity of boiling water, and kept for above an hour at temperatures exceeding 212 F°., must be heated through and through to a point utterly inconsistent with the maintenance of organic life.

As to the statements made by travellers concerning living insects found in hot wells, we quite agree with Max Schultze and Cohn that they ought to be received with extreme caution. But even if literally correct, they prove nothing. The animals found in such situations have been specially trained to withstand extraordinary temperatures by a process of natural selection. But the germs of *Bacteria*, &c., about which Dr. Bastian argues, have undergone no such training. The author calls attention to a very interesting fact, that the very process which Pasteur devised for the better preservation of wines, is based on the fact that at temperatures of about 140° F. (60° C.) organic germs and spores, ferments, &c., are destroyed!

Dr. Bastian deserves great credit for the manner in which he has narrowed the issue. Unless we can detect some fatal flaw in his experimental evidence, we must, perforce, admit that abiogenesis is not merely possible, but that as regards the lowest forms of organic life, it is a matter of daily occurrence.

We must, however, point out that, upon the first dawn of life on our globe, these experiments, interesting as they are, throw no light whatsoever. They all involve the use of matter, which, though in itself lifeless, is the result of antecedent life. Such matter in the primordial world is, by hypothesis, absent. To solve the question, it will be necessary to prove the spontaneous development of life in purely inorganic matter.

In Professor Schmidt's work we notice a curious misstatement, which we can only attribute to an oversight in transcription. We read :—" If throughout the great family of the Coleoptera, genera and species are to be found with imperfect flying apparatus, consolidated wing-covers, &c.,—if the *whole family of the Staphylinæ does not possess the organs of flight*, no one dreams of considering them as arrested forms." Now the fact is, that the majority of the Staphylinidæ have large and powerful wings, and use them sometimes too well for our comfort. Many of the smaller species fly into our eyes in still, warm, autumnal days, and cause exquisite pain by their habit of erecting their bristly abdomen.

Another curious error occurs on p. 53, where a diagram is described as representing the larva of the "great black beetle (*Hydrophilus piceus*)." The *Hydrophilus piceus* might be called a "great black water-beetle," but the term "blackbeetle" is in England unfortunately applied to the cockroach (*Blatta orientalis*), which is no beetle at all, but an orthopterous insect.

The great merit of Professor Schmidt is, that he enables the English public to become acquainted with the results of Haeckel, Wagner, Nageli, Graber, and other German naturalists, who have so ably and industriously applied the doctrines of Darwin in actual zoological research. His work will be further useful as an able exposure of the shadow-fighting of some of the most prominent anti-Darwinians. "A renowned zoologist," we read, "one of the few who adhere to the old belief, has taken the useless trouble of proving that the skull of the orang could not possibly be transformed into the human head. As if the doctrine of Descent had ever asserted such nonsense!" The *buffos* of the platform, the press, and the pulpit, are here told—what they ought to have ascertained previously—that "man does not stand in the direct line of development from the age. The cheap jest, produced with so much glee, of inquiring why we do not behold the interesting spectacle of the transformation of a chimpanzee into a man, or conversely of a man by retrogression into an orang, merely testifies the crudest ignorance of the doctrine of Descent."

We cannot help expressing our regret that England and Germany are left to work out this great question alone. Is there no place for France, once the home of philosophic zoology—the country of Buffon and Lamarck, of St. Hilaire and of Blainville, the "Cuvierivorous?" Is her "official science" fatally smitten with alethophobia?

V. SELENOGRAPHY: ITS PAST, PRESENT, AND FUTURE.

By E. Neison, F.R.A.S., &c., &c.

SELENOGRAPHY, or the study of the moon's surface, may be said to have been commenced by Hevelius, the celebrated observer of Dantzic; for, though earlier astronomers had examined the moon, and there had been even prior to the discovery of the telescope much speculation as to the nature of the surface, yet to Hevelius are due the first systematic observations of our satellite's surface. When Hevelius commenced his study of the details of the moon's surface, the subject was practically new; Galileo had already, it is true, discovered the moon's libration in latitude, and understood how a diurnal or parallactic libration must also ensue, though it is doubtful whether, with his means, it could have been detected; but his lunar observations, like those of Scheiner and others, were too imperfect to be of any value. A primary and important result of the observations made at Dantzic was the discovery of the libration in longitude, which from his far more accurate observation did not escape Hevelius, and which he explained from the circumstance that the moon from its uniform rotation on its axis always presented the same face to the centre of its orbit, while the earth was situated at one of the foci. Considering the optical means then at the disposal of Hevelius, his telescope not magnifying more than forty diameters, the completeness and general accuracy of his map shows the great care and success with which his observations were made, and for many years it remained the best chart of the moon existing. As is well known, he adopted as a method of nomenclature, a system based on the fancied resemblance between the lunar and terrestrial formations; whilst from estimating the distance within the dark portion of the moon, that the summit of the peaks remained visible, he computed that some of the lunar mountains must be 17,000 feet high—a very corect estimate of probably the Apennines, the mountains measured.

Four years after the publication of the "Selenographa" of Hevelius, in 1647, appeared the "Almagest" of Riccioli, of Bologna, containing a lunar map, less perfect on a whole than that of Hevelius, but still a very creditable result; and in it was introduced a much improved system of nomenclature, where the names of distinguished astronomers and mathematicians were introduced in place of the feeble terrestial analogies of Hevelius. On the Continent this new system met with general acceptance, and has, since the date of Schröter's observation, entirely superseded the earlier, though, until the close of the last century, it was still employed in England.

Newton turned his attention to the subject of the lunar librations, and in a letter to Mersenius, in 1675, amended Hevelius's explanation of the cause of the lunar libration in longitude, by showing it was a necessary sequence of the uniform rotation of the moon on its axis, in the same time as a complete revolution in its orbit, combined with the variable motion of the moon in its orbit; but, like Hevelius, considered the axis of rotation to be perpendicular to the plane of the ecliptic. Later, Newton also showed that, as a consequence of the earth's attraction and the coincidence of its period of revolution and rotation, the moon must be elongated in a direction passing through the centre of the earth when in mean libration, which was the cause of the moon always presenting the same hemisphere to the earth. From this spheroidal form of the moon, Newton pointed out a real libration of the moon must ensue from the retardation exerted by the earth's attraction to the longer axis of the spheroid, passing from a line directed to the earth's centre, though compelled to do so by the libration in longitude, causing thus a vibratory motion in the direction of this longer axis.

Selenography received its next advance from the great astronomer Dominic Cassini, who, by investigating the known conditions affecting the lunar observations, and making many new observations during a period of twenty-two years, announced in 1693 his complete theoretical solution of the problem of the lunar libration, and showed the peculiar relation between the nodes of the moon's equator and orbit on the ecliptic; so that planes through the moon's centre, parallel to the planes of its equator, orbit, and ecliptic, intersect one another on the same straight line, and the two former always on opposite sides of the third—the ascending node of the lunar equator being thus coincident with the descending node of the moon's orbit, both on the ecliptic. Cassini deduced from his observation the value 2° 30′ for the inclination of the lunar equator to the ecliptic, and a mean value of 5° 0′ for the inclination of the orbit to the ecliptic, thus completing a work of the very highest importance to selenography.

Cassini also published in 1680 a small map of the moon, some 20 inches in diameter, containing the results of his earlier observations, but, like its predecessors, only founded on eye estimates of the positions of the spots; whilst though, from his superior optical means, it was more complete, than Hevelius's, it was on a whole not more accurate, and for a considerable period remained little known.

In 1748 Tobias Mayer, the celebrated mathematician and astronomer of Gottingen, as part of his researches on the moon, investigated the problem of the lunar librations, and by determining the difference in right ascension and declination between the apparent centre of the moon and the lunar spots Manilius, Dionysius, and Censorinus, obtained material for ascertaining how far Dominic Cassini's theory of libration agreed with observations. By the middle of 1849 Mayer had obtained 27 measures of Manilius, 9 of Dionysius, and 12 of Censorinus; and in a memoir in the volume for 1750 of the "Societe Cosmagraphique," of Nuremburg, by employing equations of condition, showed that the inclination of the moon's equator to the ecliptic was 1° 29′, and that sensibly Cassini's theory was correct, though he found a slight difference between the positions of the nodes of the lunar equator and orbit, which he made 3° 45′ apart, a result sufficiently small to indicate that its origin was in the errors of observation.

Mayer had intended to form a complete map of the moon in 25 sections, and with this view made a series of 47 additional measures of 21 lunar spots, besides those already mentioned; whilst he determined the position of 63 other formations by careful estimates, in which he excelled, of their distance from the measured objects. From the few measures of each spot made, the positions of all except 8 were only approximate, the average error being about one degree. Pressure of other engagements, especially his lunar tables, and his early death in 1762, prevented his carrying out his intentions, but in 1775, amongst his "Opera Inedita," was published a small map 8 inches in diameter, founded on his measures, which remained for many years the best lunar map, and was employed by Schröter.

From a consideration of careful micrometrical measures of the position of Manilius on three occasions, in October, 1763, Lalande obtained for the value of the inclination of the moon's equator to the ecliptic 1° 43′, which he regarded as exact, though founded on so few measures; whilst he regarded his results as showing the coincidence between the nodes of the lunar equator and orbit, as demanded by Cassini's theory of the moon's librations.

From its interest and importance the question was attacked theoretically by Lagrange, in an important memoir in 1764, where, by an analytical investigation, it was shown that the equality of the periods of the lunar rotation and orbital revolution was a necessary consequence of the earth's attraction; and further pointing out that the earth's influence was capable of producing this equality, even had it not originally existed. Later, in 1780, Lagrange showed that the coincidence between the nodes of the moon's equator and orbit involved in Cassini's theory of its libration was a necessary sequence of the earth's attraction. It also was pointed out by Lagrange that the moon's form must be that of an ellipsoid whose shortest axis was its polar axis and longest axis directed towards the earth's centre.

During the period 1784—1802, Schröter, of Lienthal, in Hanover, with powerful reflecting telescopes, made many observations of the moon's surface, which he was the first to examine in detail, and he obtained what he believed to be proof of the existence of a lunar atmosphere and of active volcanic changes. Though many of Schröter's observations were important, and his drawings, though rough and wanting in detail, are faithful, he took insufficient precautions against changes produced in the appearance of lunar objects by variation of libration and illumination, which were, indeed, then little understood; consequently, many of his supposed changes and evidences of an atmosphere have been shown to be fallacious, and have involved the rest in perhaps undeserved discredit. The results of Schröter's observations were published in his "Selenotopographische Fragmente," the first volume appearing in 1791 and the second in 1802.

About the commencement of the present century, the theoretical results obtained by Lagrange, with respect to the lunar librations, were confirmed by a full investigation of Laplace, who showed, moreover, that the lunar secular inequalities were without influence on the relation between the periods of rotation and orbital revolution, and the nodes of the lunar equator and orbit. Later, the theoretical investigation of this question was completed by Poisson, who showed that some minor inequalities of the second order, affecting the lunar elements neglected by Lagrange, were likewise without influence on the above relations.

As already mentioned, owing to the ellipsoidal form of the moon, a real libration must ensue from the vibratory motion of the longer axis of the moon, due to the endeavour of the earth's attraction to keep it directed towards the earth's centre; whilst the causes producing the moon's optical libration carried it on alternate sides of this direction. Laplace and Poisson both investigated the condition of this real libration, whose amount depended on the mutual relations existing between the lunar axes, and from which it appeared that, though all the inequalities in the true longitude and latitude of the moon must produce an effect on the moon, yet all these would be insensible, except in the case of those inequalities known as the annual equation and the equation of the centre, both of whom might be expected from the probably theoretical form of the moon to produce perhaps sensible real librations in longitude, though the latter could not amount to one-fifth of the former.

At the desire of Laplace, Bouvard and Arago, in 1806, undertook the investigation of this point, to detect, if possible, this theoretical real libration, and made during that year 18 observations of the position of the lunar spot Manilius, but their plans were interrupted by other work. Bouvard, two years after, renewed his work, and between September, 1808, and October, 1810, obtained 124 measures of the position of Manilius. Employing these observations as a basis, Nicollet, in 1816-18, undertook the investigation of the lunar libration, with the view of confirming by observation the theory of libration, and detecting any sensible real libration, from whose amount the form of the moon might be determined. As a result from the 124 observations made by Bouvard during 1808-10, Nicollet showed the distance between the nodes of the lunar equator and orbit on the ecliptic could not exceed 9′ 19″, a quantity so far within the errors of observation as to show its origin was in them, thus affording a complete confirmation of the theory of libration first announced by Cassini, and shown to be theoretically correct by Lagrange, Laplace, and Poisson. From these observations Nicollet also deduced that there existed a real libration of the moon in longitude corresponding to the annual equation in the lunar theory, and amounting to 4′ 46″ of selenographical longitude.

With a view of further improving these interesting and important results, Nicollet made a series of 32 new measures of the position of Manilius during 1819-20, and combining these with the 124 measures formerly employed, and the 18 made by Arago and Bouvard in 1806, he determined that the maximum real libration in selenographical longitude corresponding to the annual equation in the lunar theory amounted to 4′ 49·7″, and the figure of the moon, as deduced from this value, Nicollet showed was inconsistent with the figure of equilibrium it would have possessed had it been originally fluid. Poisson pointed out, however, that the data was insufficient for the rigid determination of this point, and also computed, from Nicollet's results, that the real libration in longitude corresponding to the moon's ecliptic inequality, only amounted at a maximum to 39″, or less than one-fifth of a second of arc.

From the same observations Nicollet deduced the value 1° 28′ 45″ for the inclination of the lunar equator to the ecliptic, and found for the selenographical position of the centre of Manilius latitude 14° 26′ 54″, longitude 8° 46′ 56″.

These results were in the highest degree interesting, and possessed no small importance, but, as remarked by Nicollet, Bouvard's optical means were very small, and it was very desirable that the investigation should be repeated not only with far more powerful instruments, but a greater number of observations be employed.

For the really systematic observation of the lunar surface, with the view of the solution of the many interesting and important questions connected with it, the accurate determination of the position of all the principal formations was essential, as by no other means could the identity of the various objects observed at different times be ascertained, and any changes that might occur be recognised. Hitherto there had, however, existed no data on which to found a trustworthy map of the principal features of the lunar surface; for with the exception of some eight formations whose position had been fixed by Mayer, and the very accurate determination by Nicollet and Bouvard of the place of Manilius—one of them—all the other formations had been laid down by practically eye estimations. Measures of the position of the principal objects upon the moon were therefore requisite for further progress, and then the almost unknown smaller delait could be properly inserted.

In 1824 appeared Lohrmann's "Topographie der Sichtbaren Mondöberflache," comprising four sections of what was intended to be a complete lunar map, 37½ inches in diameter, divided into 25 sections, being the first detailed lunar chart based on scientific principles. As a foundation, employing a method devised by Encke, Lohrmann made a series of 150 good measures with a parallel wire micrometer, on a 4⅘ inch achromatic, of the distance of 21 spots from the limb, all the objects being within the area of his four sections, namely, from 12° east to 38° west longitude, and 12° south to 37° north latitude. Of these points of the first order, as they were termed, twelve, whose position rested on the results of from 8 to 12 measures, may be regarded as determined with fair accuracy, five based on from five to seven observations were not quite so satisfactory, whilst the remaining five were founded on too few measures to be considered of much value. These measured positions were, however, far too few for the great area that rested on them, and few, if any, auxiliary points were measured. The section of his map, based on these results, was, however, a great improvement on all former charts, and constituted the first trustworthy delineation of the moon's surface; it was therefore unfortunate that failing health prevented its ever being completed. In some points, however, Lohrmann's work was defective, as no height measures were made only those earlier made by Schröter with imperfect means quoted, and few formations had their dimensions measured.

In 1837 appeared Beer and Mädler's "Der Mond," founded on the results of observations made during the years 1830 to 1837, accompanied by a complete lunar chart, in four quadrants, on a scale of 37½ inches to the lunar diameter. As a foundation for their map, Mädler, whom it is understood was the principal observer, made a series of 919 direct micrometrical measures from the limb of the principal lunar points, adopting Encke's system of computing the moon's libration and reducing the measures. Rejecting 104 of these —principally the earlier—as discordant, imperfect, or doubtful, from the remainder, the position of 92 points were determined, 7 being, however, points already chosen by Lohrmann, whilst the position of each point rested on from 8 to 10 measures. In constructing their map, as Beer and Mädler combined Lohrmann's results with their own, they had 105 points of the first order, only two of which rested on under five measures, as well as a considerable number of auxiliary points of the second order.

Mädler's measures were made with a parallel wire position micrometer, with a power of 140 and a field of $14\frac{1}{2}'$, or about 20 revolutions in diameter, the thickness of the wires being 1·91″, but as the edge of the field was imperfect, distances exceeding 14 to 15 revolutions could not be measured direct; consequently, objects more than 10′ or 11′ from the limb were measured from an intermediate spot, generally Posidonius, Piccolomini, Messier, Gassendi, or Seleucus, whose distance from the limb was also measured. To eliminate as much as possible incidental errors, and to simplify the computations, measures were only made when the moon was near the meridian and possessed a greater altitude than 18°. The distances were found by bisecting the spot with one wire and bringing the other tangentially to the limb, a somewhat difficult operation to perform in a right ascension direction, owing to the rapid motion of the moon, and the difficulty of putting the two distant objects in position at the same time. Each result consisted of two measures in a right ascension direction, and one at right angles.

Beer and Mädler considered that from the difficulty of exactly adjusting the wires of the micrometer so as to keep both in position, and from the irregularity of the moon's limb and variability in the amount of irradiation at the edge, the results of a single measure could not be regarded as giving the position of the point within 30′ of longitude or latitude near the centre, and proportionately greater towards the limb, the uncertainty of a single result being about 8″ to 9″, a result borne out by their measures, which, rejecting the unfavourable points, show the uncertainty of a single measure to be 7·2″. From this the average probable error of one of their points of the first order, resting as already mentioned on from 8 to 12 measures, usually might be expected to be from 2·2″ to 1·8″, or 8′ to 7′ of selenographical longitude or latitude near the centre, a result sufficiently approximate to the actual probable errors for the more favourable points. In the above no account has been taken of the considerable personal equation now known to exist in the observations of the lunar limb; and this is a weak place in Encke's method of determining positions of the first order, as employed by Mädler. Of necessity, the position of all the points were measured by Mädler, mainly from one pair of limbs, either S. and W. or S. and E., or else N. and W. or N. and E., and therefore the resulting personality acts as a constant error always in one direction, and cannot be eliminated, whilst escaping consideration in the resulting "probable error." It is true, on some occasions, Mädler measured from opposite limbs, but then always through the aid of an intermediate point, consequently introducing double errors, from having to make two separate measures, whilst these measures were too few to exert much influence. By comparing Lohrmann's measures with Mädler's, the influence of this personal equation, combined with some differences due to the effect of aperture on the irradiation at the limb, is at once revealed, for there exist a mean difference of 23′ in longitude and over 17′ in latitude between the places of the points they have both measured; and this would appear to be too great a difference to be ascribed merely to errors introduced by the few observations made by each of these spots, even were it not noticeable that all Mädler longitudes are nearer the centre of the moon than Lohrmann's, and, with one exception, all his latitudes further north. This would indicate, probably, a personal difference in the method of observing similar to that known to exist in ordinary lunar observations, and it is unfortunate that the few spots observed by both astronomers renders its direct determination impracticable.

Besides measures of points of the first order, Mädler made numerous determinations of the position of points of the second order, as he termed those points of minor importance, whose place he ascertained by measuring their distance and position angle from a line joining two of his points of the first order. This method gives, however, very inferior results to the results of measures of the first order, as, in addition to errors incidental to measurement, the position carry all the errors in the determination of the points of the first order, and when the distance amounts to more than some five or six degrees, is liable to considerable errors of its own. The great majority of these points rest on a single measure, and as their distance from a point of the first order is usually considerable, much uncertainty must attach to their position, which cannot be considered as having a probable error of under three or four times that of the neighbouring point of the first order.

Mädler likewise determined the diameter of 148 of the principal formations, and made 1095 measures of the height of 830 lunar peaks, whilst he also considerably increased and improved the lunar nomenclature, adhering, of course, to Riccioli's system, with Schröter's modifications, distinguishing the smaller objects by Greek and Roman letters attached to the name of the nearest important formation.

Mädler's instrument was a Fraünhofer achromatic of $3\frac{3}{4}$ inches aperture, and though smaller than Lohrmann's, yet probably of finer quality, its excellence being apparent from its admitting commonly of a power of 300 being used for drawing the lunar details, but its moderate aperture possesses important bearings on many of the results given in Beer and Mädler's "Der Mond," where the influence of its want of capacity for dealing with the more delicate phenomena is often exhibited. There can be no question, however, of the great advance on all its predecessors of the "Mappa Selenographica," not only from the vast amount of detail for the first time shown, but in its general accuracy, owing to its being based on the result of actual measures. Though deficient in minuter detail from the small aperture of their instrument, and though not implicitly to be trusted on the more delicate features, yet in its general fidelity the "Mappa Selenographica" is superior, not only to all its predecessors, but likewise to its successors.

Upon the conclusion of Beer and Mädler's great work, it was generally considered as established that the great questions in connection with the condition of the moon were finally settled, with the exception of the reason why volcanic action had produced such different effects on the moon and earth, and the real origin of some of the more inexplicable formations, principally the great ray or streak systems, and the rills or clefts, whilst it was considered certain that the moon was to all intents an airless, waterless, lifeless, unchangeable desert. With this opinion prevalent, the natural effects of such great works as Beer and Mädler's shortly ensued; the attention of astronomers was directed to other fields, and selenography, resting on its laurels, made no further progress for many years; and until 1865 Schmidt, at Athens, may be regarded as the sole selenographical worker on an adequate scale.

The only point in connection with lunar physics that during this period excited attention was that of the real libration of the moon, and was due to the investigation of Bessel. In a memoir in the "Ast Nach," in 1839, he entered into an examination of the question of the moon's real libration, as presented by the theoretical investigations of Lagrange, Laplace, and Poisson, and pointing out the uncertainty attending the value deduced by Nicollet. As a basis for further investigation, he deduced the position of the crater Mösting A by two careful measures with the Konigsberg heliometer. This preliminary examination was carried on by Dr. Wichmann, in 1847-48, who, after a careful investigation of the problem to be determined, applied the results of 50 measures with the Konigsberg heliometer, in 1844-46, to deduce the real libration. The results obtained by him were still more inconclusive than those arrived at by Nicollet; Wichmann's conclusion being that the real libration of short period could not exceed 10′ of selenographical longitude, and probably not 7′; but as to its exact amount, the observations failed to afford any satisfactory information.

Though Wichmann's individual measures were considerably superior to those of Bouvard and Nicollet, yet their small number, only 44 separate results, and the short period of 14 months in which they were made, with the irregular interval between them, renders them inferior as a series for this purpose to those of Bouvard and Nicollet, whose 174 measures were spread over a period of 4 years, and, commenced in 1806, extend to 1820. The uncertainty with respect to the amount of the real libration of the moon remains, therefore, as great as ever. The value obtained by Wichmann for the inclination of the lunar equator to the ecliptic was 1° 32′ 9″, or over 3′ greater than resulted from Nicollet's investigation; and this discrepancy between the results obtained by the two investigators adds much to the uncertainty attending the whole question. Incidental to his investigation, Wichmann found for the position of Mösting A, longitude 5° 13′ 23″, latitude 3° 10′ 55″, and that Burckhardt's value of the moon's semi-diameter was considerably too small. The determination of the problem of the real libration of the moon still required to be performed, and as Wichmann observed, it must be based on a considerable number of observations; not less than two hundred at regular intervals appears suitable, and should extend over at least three years.

Schmidt devoted his attention mainly to the production of a lunar map that should adequately show the true details of the lunar surface which he recognised Mädler's failed to do, though representing most faithfully the greater formations. For this purpose he determined that a map 75 inches in diameter would be the smallest adequate size, being four times the area of Mädler's, though the results show even this to be too small for this purpose, producing too much crowding to be desirable. During his work, Schmidt has made over a thousand drawings, and three times as many height measures—but it is understood few (if any) measures of position of the first order, but based his map entirely on the result of Mädler and Lohrmann.

In 1865, when the deficiency of Beer and Mädler's map in the minuter detail had become more generally recognised, the British Association appointed a committee for the purpose of making further progress in the mapping and cataloguing of the lunar surface, and the result was the production of a new system of nomenclature, the commencement of a complete lunar catalogue, and the construction of four sections of a preliminary outline map on a scale of 200 inches to the moon's diameter. After three years' work, however, the committee was no longer reappointed, and the map and catalogue remained in an unfinished condition, making the very slowest progress.

Through both these means, though mainly through the labours of Schmidt, much new lunar detail has been observed, sketched, and mapped; but little progress has been made in a most important branch, namely position measures of the lunar formations, though these are absolutely essential for further progress. Mädler's results were insufficient even for the proper delineation of the "Mappa Selenographica," and are entirely inadequate for the foundation of a larger map like Schmidt's of four times the area, much less for one like the proposed final British Association map of over seven times as large, or its outline map, which has nearly thirty times as great an area; therefore, for the proper drawing of these charts; and any smaller than the proposed final map of the British Association—namely 100 inches to the moon's diameter—would be inadequate; more positions of the first order must be made if they are in any way to be trustworthy. To supply this want should therefore be the first object of further lunar work.

In October, 1866, Schmidt noticed that the lunar crater, Linne, described by Mädler, with whom Lohrmann is in accordance, as a deep crater 5½ miles in diameter, was no longer visible, and in November he looked equally in vain, and announced, therefore, that Linne had disappeared. This attracted at once the attention of astronomers, and numerous observations were made; but no trace of the crater, as described by Lohrmann, and Beer and Mädler, could be detected, though soon after a very minute crater cone was found to exist on the whitish spot occupying the site of Linne. Since then the crater seems to have widened, and the remains of a crater of dimensions nearly equal to those described by Beer and Mädler have been detected: therefore it is generally considered that no real change has occurred, though no explanation can be given of the appearance as seen by Lohrmann, and Beer and Mädler. The importance of measures of the principal lunar formations was, however, apparent; for had not Lohrmann and Mädler measured Linne, it is doubtful whether it would have received the attention merited on Schmidt's announcement; whilst had they and Schmidt measured carefully the dimensions of the crater, no uncertainty could have existed as to its real appearance. Schmidt and others have since pointed out cases of apparent change, but being in unmeasured points, have received little elucidation. During the period 1869-71, many observations were made of the relative visibility of the minute crater cones, white spots, and streaks on the surface of the walled plain Plato, and the results have been discussed under the direction of a British Association Committee, whose reports were published in 1871 and 1872, where it was shown that the minute objects on Plato underwent curious and inexplicable variations in visibility; whilst the whole floor showed an apparent darkening of marked extent during each lunation. Numerous observations have also been made, indicating peculiar changes in the visibility of different lunar regions, not dependent on variation of illumination; many new formations have been detected, now so conspicuous as to appear hardly capable of being overlooked by earlier astronomers, were they then as distinct; changes in tint and colour have been recognised; and finally, considerable differences have been noticed in the position of the light streaks from Mädler's drawings, whilst a number of new ones have been seen. The result, in short, of the observations of the last period of selenographical activity has been to reopen all the questions nearly previously considered settled by Beer and Mädler, before the true nature of the lunar details were generally understood, and with regard to which the small aperture of their telescope placed them under serious disadvantages.

The present condition of selenography is essentially, therefore, one of uncertainty; and though the earlier astronomers have by their work cleared the ground, and defined the questions to be solved, their solution is still to be accomplished, and for this end further observations are necessary to afford the requisite data.

The principal points to which this further micrometrical work should be directed are essentially the following:—The determination of an adequate number of lunar positions, comprising those of all the principal formations; the measurement of the dimensions of the principal craters and of the slopes of the walls of the chief classes of lunar formations, as well as the relative levels of the principal mares or great plains, both important features, with regard to which no data exists; and the determination of the dimensions under different illuminations of some of the peculiar white spots occupying at full the position of various lunar formations. Means should also be taken for a re-determination of the apparent brightness of the different portions of the lunar surface, and of their colour; for good reasons appear to exist for believing distinct changes have occurred since Beer and Mädler's time, and it should be remembered that such changes were suspected by Mädler himself. Measures of the height of many of the walled peaks, and of the depths of the chief formations, are especially valuable, after the apparent disappearance of several of Mädler's deep craters, although the many unpublished measures of Schmidt will be, when accessible, a great addition to our present data on this subject.

Measures of the lunar diameter, and observations of occultations of stars present themselves as highly important for the determination of the question of the existence of a lunar atmosphere, such as is at present indicated by the known apparent disagreement between the telescopic and occultation semi-diameter. Measures of the shadow length of different ring-plains at different periods might reasonably be expected to afford some indications of the presence of a denser local atmosphere, due to local causes. By determining the distance between some conspicuous lunar point and a star before occultation, evidence of the strongest kind might be obtained with regard to whether a lunar atmosphere exists, though at present no suitable lunar point near the limb has had its position sufficiently well determined to be available for this purpose.

Much, likewise, remains to be done in the observation of the details of the surface, and the careful construction from these of a trustworthy map of them. Based on sketches made at intervals during a period of thirty-five years, and unaided by the results of systematic measures of their positions, Schmidt's map, when issued, which is unfortutunately not likely to be for a considerable period, will, though of the highest value, add materially to the lunar work requiring to be done. It is certain to require much revision from its very nature, and must give rise to many uncertainties requiring settlement, whilst, as it differs in many points from Beer and Mädler's, whose general accuracy is of the highest, these discrepancies will need most careful investigation, a work necessarily of considerable labour. Moreover, it appears certain that the position of numbers of the formation will need alteration, when their true places have been ascertained by systematic measures. Being founded likewise on drawings made at different epochs, some uncertainty must attach to the combination of these into a single connected whole, whilst, as is known, a considerable personality attaches to lunar observation, one observer detecting certain classes of objects far readier than others. Much also remains to be done in the proper combination of the isolated details seen at different times into a connected whole, and here appears one of the most difficult selenographical labours. For on the moon, the details of the surface being nearly only rendered visible when thrown in relief by shadow, it is rarely that an entire formation is seen equally distinct at the same time, but nearly always the details come out little by little, those first seen disappearing as the newer come into view; so that it is necessary, from isolated observations, to piece together, as it were, the details into a connected whole. Considering the great difficulty in studying these details, and the fact that much is only visible under conditions when the rest is quite invisible, while some are rarely, if ever, seen at all, the great labour and difficulty in piecing all together so as to obtain the nature of the formation which connects together all the isolated detail seen is apparent; whilst it must be remembered that, owing to the effects of libration, the appearance of these details are constantly altering. Lohrmann, and Beer and Mädler, have seldom attempted to do this, but have merely mapped down the object seen; and a similar course seems to have been followed in the main by Schmidt; but the study of the real nature of the surface will necessitate its being carried out in full, if we are ever to properly comprehend the meaning of what is visible on our satellite.

So far no reference has heen made to the results obtained by the employment of photography in selenographical investigation, though many fine photographs of the moon have been obtained mainly through the labours of De la Rue and Rutherfurd. Hitherto, however, the results obtained through the aid of photography have been of less value than was generally anticipated, for though for many purposes the photographs have been and will continue to be of great service, with regard to the two principal ends, it was trusted they would be of the greatest value: they have failed to realise expectation. In so far from affording unerring representations of the real condition of the lunar surface, enabling its details to be examined with ease at leisure, they have hitherto proved entirely inadequate for this purpose, even the finest photographs exhibiting hardly a trace of this detail, but show only the coarser features of the larger formations and the mere existence of the smaller, whilst even what is to be seen appears at times, from differences in actinic and illuminating power, so different to its actual appearance as to render it recognisable only with difficulty. Those already obtained seem to be deficient in sensitiveness to slight variations in illumination, which prevents the smaller detail being separately distinguishable, but blurs the whole into one general mass, which no mere enlargement can rectify, and it is doubtful whether this fault can be overcome, except by taking a larger image directly, even if that proves successful. Whatever may be the cause, the result has been to render lunar photographs of little assistance to the study of the details of the surface of the moon, as Mädler has already pointed out.

The other respect in which much was anticipated from lunar photographs was in the accurate mapping of the surface of the moon, as it was considered they would afford a means by which the true positions of the lunar formations might be laid down by means of a series of measures. In this, however, they have hitherto been of little service, only having been employed by Birt to lay down secondary positions on the lunar map of the British Association, by whom they were also employed to give the approximate dimensions of the principal formations. For these purposes, were the number of well-determined points much increased, they might prove of considerable assistance, and more use made of them for this end than has been. But for the determination of points of the first order with any accuracy, they are far inferior to direct measures, and the results neither satisfactory nor sufficiently trustworthy perhaps. Even were there no uncertainty introduced by the want of sharp definition, or by variations of illumination, all certainty is lost in the errors that can be, or perhaps must be, introduced, when dealing with such delicate material as collodion film and paper. And it appears impossible to guard against the effect of the vagaries of such materials as collodion films to the desired amount of accuracy, and the use of daguerreotypes seems impracticable, considering the large scale necessary.

With regard to classes of measured points on the lunar surface, on which depend our power of constructing a trustworthy map on an adequate scale of the surface of our satellite, they should consist of standard points, and points of the first, second, and third orders. The accuracy of the first should be sufficiently great to enable all the others to be measured direct from them: the points of the first order would be employed for the same purposes as the present members of this class, whilst the points of the third order would comprise all those auxiliary objects whose, at least, approximate position must be known for the proper construction of the outlines of the larger formations, and to serve as nuclei round which group the smaller not measurable details.

In measuring points upon the moon, the limb is considerably inferior as the origin of measures to any small conspicuous object on the surface, such as a small bright craterlet, not only from its irregularity and the irradiation accompanying it, but also from the considerable personal equation known to exist in observing it. To eliminate as far as possible the effects of these, if measured from the limb it should be from all four, which would considerably weaken the above disadvantages. When practicable, however, far more accurate results might be expected by measuring from some of the smaller lunar formations, preferably one of the bright lunar craterlets, such as are equally distinct in high and low illumination. These objects would constitute the first class, or standard point, and should rest on the results of from 40 to 50 measures, so as to reduce the probable error of the position to a minute amount, and so render them available as points from which to measure the rest. The members of the second class, which were termed by Mädler points of the first order, should be founded on from 10 to 15, or, in the more important, 20 measures, so as to increase their accuracy, and would be generally measured from the last, unless, as might be in some special cases, the limb offered superior advantages. The third class, or points of the second order, as here termed, would consist of objects whose place had been determined from 5 to 8 measures from points of the first class, and therefore probably be as accurately known as most of Mädler's points of the first order, and could be employed alone or in conjunction with one of the last class, to determine points of the third order within a convenient distance from them. The formations included in the third class would consist of the points of the second order of Mädler, and comprise all the principal smaller lunar formations, whose position was necessary for the proper mapping of the surface, or else possessed some special importance or interest of their own. Measured in an analogous method to Mädler's, they should preferably rest on two separate measures from different points, and as the increase in the number of available points would enable their distances from these to be much reduced from what Mädler was obliged to use, their accuracy would be probably nearly tripled.

In employing, as a point from which to measure other positions, a lunar spot instead of the limb, a considerable advantage is gained, inasmuch as a comparative great discrepancy between the assumed libration of the moon and the true would produce a scarcely sensible error in the resulting position; whereas, as measured from the limb, any error in the assumed libration produces a corresponding error in the position of the points, and thus, in the former case, the reduction of the measures made is much simplified, an additional advantage of no slight importance. Any connection, moreover, in the assumed position of the standard point that further observations may indicate, would also admit of being applied without trouble sufficiently approximately to the positions deduced from it by measures, unless it was of very considerable magnitude, which would hardly be conceivable under the conditions; and this, likewise, is no slight advantage.

So far, with reference to the measurements for the determination of the position on the moon's surface of the principal lunar objects necessary for the proper construction of a trustworthy map of the moon, and for the further progress of those selenographical observations, requisite for any advancement to be made in the solution of the many problems in connection with our satellite, and in the construction from the visible records of the past history of the companion to the earth.

Intimately connected with the subject of the measurement of the objects upon the moon is that of its librations, and a proper knowledge of the conditions introduced by these into lunar observations is essential to accurate selenographical work. It has been already stated that the moon is subjected to two classes of librations, the greatest, which may be termed the optical libration, being due to the nearly uniform rotation of the moon on its axis, and its variable motion in its orbit, in conjunction with the results of the inclination of the lunar equator and orbit to the ecliptic; whilst the other may be called the real libration, and is due to the actual motion as if it were of the major axis which is directed towards the earth. The optical libration admits without difficulty of being computed for any given time, and may be held to include the parallactic or diurnal libration due to the same cause as the lunar parallax, and depending on the position of the observer with relation to the earth's centre. This libration, therefore, presents no difficulty: but with regard to the real libration of the moon, this is not the case, for there exists no trustworthy determination of its amount; and this introduces some uncertainty into observation depending on the true lunar libration being known, which it is desirable should not be allowed to remain. The existing uncertainty on this point lends additional advantage to the employment of positions on the moon itself, rather than its limb, to measure points on the surface from, as under these conditions a small variation of the true lunar libration from the assumed produces no sensible error. As measured from the limb, the libration must be known correctly, as any error produces a corresponding error in the resulting place; whilst this error cannot be eliminated by increasing the number of observations, but only by spreading them over a very considerable period of time, naturally a highly inconvenient condition for many reasons.

It has been already mentioned, that from the mathematical investigation of Lagrange, Laplace, and Poisson, the complete theoretical conditions of this real libration in so far as results from the theoretical ellipsoidal form of the moon, is known. Its amount depends primarily upon the relation holding between the axes of the lunar ellipsoid, and on this point theory can afford little trustworthy data, considering the imperfect condition of our present knowledge; while they depend in part upon the condition of the initial motions of the moon, which is at present unknown. The amount of the real libration must therefore be deduced from observation, or at least one of the principal periodical inequalities of the moon's axial rotation constituting the real libration must be so found, and from that, by theoretical considerations, the others found. Mention has already been made of the observations commenced by Arago and Bouvard, and finished by Nicollet, with the view of determining directly the amount of this real libration if sensible, and of the success attending Nicollet's research based on these observations, resulting in the deduction of a value of 4′ 49″ for the maximum value of the periodical real libration in longitude corresponding to the annual equation. The results of this investigation are, however, unsatisfactory, the value deduced being very uncertain, owing to the inadequate optical means employed by Bouvard; and Nicollet considered it advisable that the whole research should be repeated with the assistance of more powerful instruments. On these grounds Wichmann, in 1847, made a fresh attempt to determine the amount of real libration of the moon from a series of 44 independent observations, but entirely failed to obtain a satisfactory determination of the amount of real libration, whilst the discrepancies between the results of the two memoirs is additional reason for further investigation.

The results obtained renders a new investigation of this subject very desirable, for it is only by this means that the limb of the moon can be rendered a satisfactory origin of measures for determining the position of the standard points on the surface of the moon, to be afterwards employed to measure other lunar objects from; and it is only the limb that is available for this purpose, whilst the sole standard points already existing are the central mountain of Manilius and the small lunar crater Mösting A. Moreover, the disadvantages of being obliged to employ the lunar limb as an object to be measured, or to measure from, has long been recognised in astronomical work, apart from selenography, where exactness is necessary, as in the determination of accurate longitude from moon culminations. For such purposes it has long been proposed to employ, instead of the somewhat indefinite limb, with its variable irradiation and irregularities, a bright lunar crater, without these disadvantages; but this is inadmissible with the moon's real libration, still only indefinitely known.

In repeating, however, the research on the real librations of the moon, several modifications might be adopted, besides simplifying the preliminary reductions employed by Nicollet and Wichmann, and it might be made after the following plan. Mädler has already pointed out that a small brilliant crater is preferable as a point to be employed to measure to any central mountain, like that of Manilius, used by Bouvard and Nicollet, and has suggested Mösting A and Triesnecker C and B as suitable both in form and from lying close to the centre of the moon, a desirable feature; and the former consequently, at the instance of Bessel, was employed by Wichmann. Of these, Mösting A is alone well suited for this purpose, but a still better is almost the crater Triesnecker A; whilst a third can be selected from one of the three craters, Hipparchus C, G, or E, the last being most favourably placed, and all considered perhaps the most suitable for this object. Three points have been selected; for it would add comparatively little to the work, though much to the accuracy of the results, were the investigation made to depend on three separate series of objects on different sides of the lunar equator, as Poisson has shown; whilst by mutual comparison some of the incidental errors could be eliminated. By means of a series of simultaneous measures from the central mountain of Manilius, a series would be obtained of like nature to the former, but free from the effects of the real libration of the moon and the irradiation at the limb, which would of course be also eliminated as much as possible from the former series by measures being taken from all four directions. By the mutual comparison of the two series, the accuracy of the results of the discussion of the first would be improved, whilst means would be afforded of comparing with the results of Bouvard and Nicollet. The comparison with the investigation of Wichmann would be independently done, as the crater Mösting A would be embraced in both series. Employing suitable instrumental means, the errors due to mere instrumental effects might be reduced considerably from those of Bouvard, and rendered equal, perhaps, to Wichmann, and from a series of from two to four hundred measures a trustworthy conclusion with regard to the real lunar libration might be arrived at.

Attention has now been directed, not only to the present position of selenography, but also to the progressive stages by which it has arrived at the condition it now stands in—a little known history; whilst the direction where further selenographical work appears most requisite, and which should be the first to be supplied, have also been indicated in some detail, and our present knowledge on these points described. The necessity of the introduction into lunar observations of micrometrical measures has been again urged with special stress, for it is only on a foundation of exact micrometrical work that the desirable accuracy in the delineation of the lunar formations can be obtained; for, though practice renders the results of an experienced observer sufficiently faithful, as a rule, for many purposes, yet in the comparison of drawings of the same formation at different epochs, all certainty is lost with regard to differences, unless based on adequate measures, as is exemplified by the results of Linne, and still more so in the case of the twin Pytheas of Mayer, to say nothing of minor instances.

In all branches of science too much faith is put in the results to be achieved by mere piling up of observation on observation, and trusting, by so doing, some fortunate discovery may result; but it cannot be too strongly urged that the hugest mass of isolated observations made without scope or character is far inferior in value to a well-considered series of observations made in pursuance of some definite plan and directed towards some distinct end. The former, it may be said, will serve as a mine from which, by labour, much may hereafter be extracted to confirm or disprove theories that may later be advanced; but even for this purpose they possess little value: for, to search through the mass for those observations that may be useful, with the almost certainty of finding them deficient in some especial feature necessary for them to be of real value in the particular direction wanted, renders it easier to obtain the results required in a different manner. The erection of an elaborate edifice, intended to be enduring upon an uncertain foundation, has long been recognised as unsatisfactory; yet much of this characteristic attaches to many selenographical hypotheses that have, especially of late, been promulgated. The origin of most of these may be ascribed to the vagueness incidental to desultory lunar observations, which, seldom pushed sufficiently far, often, as in other branches of science under similar conditions, afford untrustworthy results. Too much stress, therefore, cannot well be laid on the necessity of systematic series of observations being made, rather than mere desultory work, which seldom afford anything in value adequately representing the time and labour spent.

Above all, it may again be reiterated, stands the necessity so often urged of the completion of the determination of the positions of the principal lunar formations that has, since the time of Beer and Mädler, remained apparently untouched, despite the earnest appeals from the British Association Committees; when this has been successfully carried out, and the foundations for a thorough trustworthy aquaintance with the physical condition of the moon's surface laid, then the detail can be filled in at leisure. Not until this is done can much success in solving the various selenographical questions be expected; but then, in the words of our greatest selenographer, the late Baron von Mädler, "may our satellite, after the monstrous fables which for almost the space of many thousand years have gained credence respecting it, begin, not only by its course, but also by its natural constitution, to permit us to pierce deeper into the secrets of the Fabric of the Universe!"

VI. MODERN ENTOMOLOGY.*

ENTOMOLOGY is, of all sciences, the least fashionable. Insects, indeed, form what may be called the round number of the animal world, all other tribes constituting a mere fractional overplus. They display the most wonderful variety of structure and development. They offer excellent opportunities for studying the origin, the mutations, and the geographical distribution of species. They present the most striking examples of the singular phenomenon of mimetism. Their transformations, their architecture, their intelligence, and their social institutions, are themes practically exhaustless. Among their ranks are included our most formidable enemies—beings far more difficult to deal with than the wolf or the tiger. For all this, few works on the natural history of insects make their appearance—fewer, we believe, now than was the case thirty years ago. Of these few, a large proportion are little more than compilations and manuals, chiefly of British species—to which a large part of our entomologists limit their attention in a spirit possibly patriotic, but certainly unphilosophical.

Another besetting sin of English lovers of insects—indeed, of English naturalists in general—is a proneness to teleology. We have heard it maintained abroad that a work on the organic sciences, free from all reference to final causes, was as rare in England as "unfortified" wine. It is curious how completely the old proverb, "It's an ill wind that blows nobody good," decomposes such speculations. Let us take the following passage from one of the works before us:—
"The first insect of which travellers unite in complaining is the hated and dreaded mosquito. In its perfect or winged state it is about as annoying a creature as can be, but then it must be remembered that the traveller is but a casual intruder in the natural domain of the mosquito, and must expect the consequences of his intrusion. Devouring travellers is not the normal occupation of the mosquito; for hundreds of successive generations of them may live and die, and not one of them ever see a human being. Their real object is a beneficent one. In their larval state they live in the water, and feed upon the tiny particles of decaying

* Insects Abroad: being a Popular Account of Foreign Insects, their Structure, Habits, and Transformation. By the Rev. J. G. WOOD, M.A., F.L.S., &c. London: Longmans, Green, and Co.

On British Wild Flowers considered in Relation to Insects. By Sir J. LUBBOCK, Bart., F.R.S., M.P., &c. London: Macmillan and Co.

matter that are too small to be appreciated by the larger aquatic beings; and by devouring them, purify the water, and convert death into life. Even in our ponds at home we are much indebted to the gnat larvæ for saving us from miasma; whilst the vast armies of mosquito larvæ that swarm along the edge of tropical lakes, and feed upon the decaying substances that fall from the herbage of the banks, purify at the same time the water and the atmosphere, and enable human beings to breathe with safety the air in which, without their aid, no animal higher than a reptile could have existed."

To this passage, ably written as it is, many exceptions may be taken. If man is not to intrude into the "natural domain of the mosquito," his choice of a dwelling will be very limited; since, with the exception of sandy and stony deserts, scarcely one-fourth part of the land on our globe is free from these pests. We are not sufficiently well acquainted with the nature and origin of miasma to decide whether the larvæ of mosquitos do really purify the water and the atmosphere. That they feed upon particles of putrescent or putrescible organic matter in the waters is very probable; but will not, in that case, their excrements and their dead bodies be as great a nuisance as their original pabulum? They are found in pestilential districts, which proves that their services in a sanitary point of view are at least questionable. They swarm also in regions free from malaria, and where its existence seems highly improbable, such as Lapland. They are known also to be spreading into parts hitherto free from them, and where no malaria had been met with in their absence. How is it that teleologists judge the "contrivances of Nature" so much more leniently than those of Art? What would they say, for instance, of a city where the scavengers and nightmen were employed occasionally as surgeons, hospital nurses, and provision dealers, so that they might distribute putrescent and infectious matter upon the food, the medecines, and the very persons of the inhabitants? Yet such a case would be precisely similar to that of some of Nature's scavengers—to wit, the common house-fly and its allies. These are one moment feasting upon carrion, excrement, and ulcers; and the next, settling upon food, and upon men and animals. That in this manner they are active propagators of disease is no mere supposition. Which, then, is the real function of the fly, scavenging or the spread of disease? We have no more right to assume the one than the other. Surely our most philosopical as well as most reverent attitude is to dismiss, as beyond our reach, all enquiries as to "why" animals exist.

Another error of entomologists is their tendency to digress into learned, but tedious and utterly irrelevant, disquisitions on the names borne by certain genera or species. This may be interesting to the antiquarian, but it is not entomology. Significant names have generally the fatal defect of being too long for men who cannot hope to exceed three score and ten years of life. We wish that, in this respect at least, our modern naturalists would follow the example of Linnæus.

Turning to the chapter on Ants—beings whose wonderful intelligence makes us overlook their destructive propensities—we quote the following passage as a fair specimen of the author's matter :—"In the various accounts of ant-life which have been narrated by observers, there is often an absolutely startling resemblance to the conduct of human beings. We have heard of ants which make regular slave-hunting expeditions into the territory of less powerful ants, carry off their captives, and make them their servants. We know of ants which build walls and domed roofs. We know of ants which have their milch-kine (aphides and scale-insects), and which tend and guard them as carefully as any dairyman tends his cows. We know of ants which cultivate the ground, keep it clear of weeds, sow the future crop, and when the harvest has come to maturity, get it in just like human beings. In the history which now follows, a new and unexpected phase of human life is found to exist among ants—namely, funeral honours paid to the dead and their burial in the earth." The author then quotes from the "Journal of the Linnæan Society," vol. v., p. 217, a communication from Mrs. L. Hutton, of Sydney. This lady, having killed a number of ants which were stinging her little boy, and having flung the bodies aside, witnessed afterwards the following scene: "I saw a number of the ants surrounding the dead ones. At last four ran off very quickly, and I followed them till I saw them enter a hillock containing an ants' nest. They remained here about five minutes, when a number more came out two by two, and proceeded slowly to the place where their dead companions lay. Here they seemed to wait for something, and presently we saw, coming from the other side near the creek, a number surpassing those I had followed, and halting at the same place. Then two ants took up one of the dead ones and marched off, followed by two others as mourners; then two others entered the procession with a second dead ant, succeeded in the same way by another pair, and so on until all the dead were taken up—a number of (I should think) two hundred bringing up the rear. Following the train I found that the two empty-handed followers relieved their fellows in advance, the latter following behind in the place of those who had relieved them, and thus continuing to alternate from time to time. They stopped at a sandy hillock, where those who marched in the rear of the procession commenced operations by making holes. When a sufficient number of graves had been dug, the dead bodies were laid in them, and those ants which had hitherto stood idle were deputed to cover them in. About six would not stir from their places, and on these the others fell and killed them; whereupon they made a single large pit at a distance from the other graves, into which all the six were put and duly covered up. I had frequent opportunities afterwards of seeing the insects act much in the same manner. If one of the workers, however, were killed, it was buried where it fell, and no friends attended the funeral. The ants buried in state belonged to the soldier caste." Many other interesting facts are given in illustration of the reason, or as some persons will still persist in calling it the "instinct," of ants. Dr. Lincecum, who for more than twelve years studied the proceedings of the Agricultural Ant of Texas (*Myrmica barbata*), and who has given an interesting account of his observations in the "Journal of the Linnæan Society," in 1861, mentions a case where these ants had their nest in an orchard. But after a while the orchard was opened to cattle, who naturally ate the succulent grass-grain which the ants had planted. Finding this to be the case the ants abandoned the orchard, and took to making their plantation in the garden and other spots where the cattle could not disturb them.

It is commonly asserted that man alone is a progressive being, and that all the lower animals remain without improvement at the very point which was occupied by their most remote forefathers. To this view it may be of course objected, that we do not possess accurate accounts of the habits of social animals extending over a sufficient length of time to decide such a question. But let us assume that some species of ant did effect a manifest step in civilisation. Any naturalist who observed the result would suppose that he had seen not something new, but merely something which former entomologists had overlooked, and would thus take the credit to himself instead of awarding it to the ants. It thus becomes very easy to deny that the lower animals are capable of progress.

From the ant to the ant-lion is not an unnatural transition. Of these insects, which have no representatives in England, Mr. Wood gives a very interesting account, taken from Mr. Gosse's "Naturalist's Sojourn in Jamaica." The ant-lion larva, it is said, is capable of existing without food for a long time; one of Mr. Westwood's specimens having lived for six months without any nourishment whatever. This, our author remarks, "is to be expected, as the supply of nourishment is very precarious." But if we turn to his account of the tiger-beetle, whose larva lives on the same kind of prey as the ant-lion, and captures it in a somewhat similar manner, we read:—"All carnivorous creatures require a constant supply of nourishment. The internal fire fed by animal fuel burns fast and fiercely, so that a tiger-beetle larva would die of hunger through a temporary deprivation of food, which would little affect the turnip grub or the cabbage caterpillar." This ill records with the remarks made anent the ant-lion. We have always found that carnivorous animals are better able to bear prolonged abstinence from food than are herbivorous species, whose supply of nourishment is, as a rule, so much less precarious.

The superior beauty of the insects in tropical countries has been frequently insisted upon by travellers, and it has been ascribed to the greater intensity of the solar rays in those latitudes. Against this theory Mr. Crookes has argued with much force in his "Handbook of Dyeing and Calico Printing." But the alleged fact is itself open to question. Our author remarks that some English groups of insects are quite as numerous, as large, and as handsome as their foreign representatives. Mr. Bates asks, very acutely, why, if climate have any direct connection with splendid colouration, do we find the females of so many Brazilian butterflies clad in such dull and sombre attire, whilst the males display an almost dazzling lustre? This one fact must, we think, suffice to overthrow the common notion of beauty as a result of light or of temperature.

There is some curious information concerning the habit of certain Carabidæ, which, when alarmed, eject noisome liquids as a means of defence. Some of these liquids are dark coloured and of evil odour, but not corrosive.

The Brachinides, familiarly called bombardier beetles, emit a fluid so highly volatile, that "when it comes in contact with the air it explodes with a slight report, leaving a cloud of thin smoke." According to Burchell, this liquid, in some of the larger tropical species, burns and stains the hands to such an extent, that the capture of the insect requires considerable resolution. The common *Cychrus rostratus* projects, without any report, a drop of colourless liquid, which burns the skin, and which we suspect to be formic acid in a very concentrated state. On this subject there is room for much interesting micro-chemical research.

The existence of blind species of insects is a fact which leads to certain difficult questions. The author describes a blind beetle, *Leptoderus sericeus*, found, as far as is known, only in the caverns of Carinthia, which are inhabited; also of a blind spider (Obisium). Several ants also are blind, including several of the terrible Ecitons, in which family we recognise a curious instance of serial degradation as regards the organs of sight. *Eciton legionis* and *prædator* are not blind, but, as Mr. Bates informs us, have eyes consisting each of a single lens, instead of the compound structure usual in insects. *E. crassicornis* has eyes sunk in rather deep sockets, and always avoids the light, moving in concealment under leaves. If obliged to cross a clear space, it constructs a covered way or tunnel with grains of earth, as do the equally blind soldiers and workers among the Termites. *Eciton vastator* has no eyes, although "the collapsed sockets are plainly visible," whilst in *Eciton erratica* both sockets and eyes have disappeared, leaving only a faint ring to mark the place where eyes are normally situated. It is curious that these totally blind species construct a covered way on coming into the open, proving that in some unknown manner they are aware of the presence of light. The question then arises, are these species blind, *ab initio*, being adapted to dark, subterranean abodes, or have they become blind by a process of gradual transformation? To us the facts of the case seem neither favourable to original "adaptation" nor to "natural selection," since, though eyes may in such situations be rarely or never of use, it is hard to see how their possession should be any inconvenience. Other species, which lead underground lives, are well known to be furnished with eyes. It is quite conceivable, that in the course of many successive generations, the eyes should be atrophied by disuse.

Where different geological formations meet, there the most interesting minerals are often found. Where land and water come in contact, there organic life is generally the richest and most varied. In an analogous manner, the boundary between two sciences often proves a fertile field for research. This is evinced in a striking manner in Sir J. Lubbock's work, which calls attention to a class of phenomena that have, till lately, escaped common observation.

It is familiarly known that plants in general, and flowers in particular, are necessary to the existence of a large proportion of the insect world. Honey constitutes the food of probably all butterflies and moths when in their mature state; of many bees, both social and solitary; of a multitude of Diptera, and even of certain beetles, such as the pretty black and yellow-banded *Trichius fasciatus*, which, from its colour, its general hairiness, and its habit of haunting flowers, is often mistaken for a bee.

But it has been, till lately, little suspected that the interdependence is mutual, and that without the visits of insects, a vast number of flowers would be incapable of fertilisation, and would consequently become extinct. Few persons are ignorant that flowers are the reproductive organs of plants; indeed, we are sometimes afraid lest the prurient prudishness of modern times may pronounce them an offence to delicacy, and hand them over to some self-constituted authority for "suppression." In some species—in analogy to animals—the male and female flowers are respectively assigned to different individuals. Thus all the specimens of *Aucuba Japonica* existing in England happened to be females, and they consequently never produced their brilliant scarlet berries, till a male tree was brought from Japan by Mr. Robert Fortune. In other cases—as in the cucumber and the vegetable marrow—there are male and female flowers on every plant. Lastly, in the majority of species, each flower is hermaphroditic, containing both the male organs or anthers, and the female, or pistil. In all these cases the question arises, how is the pollen needful for fecundation to be transported from the anthers to the pistil? In some cases this is effected by the wind. But in other instances the structure of the flower renders this utterly impossible. Even in the case where the stamens and pistil are in the same flower, there is what seems very like an elaborate arrangement to prevent self-fertilisation. Sometimes the structure of the parts renders this result practically impossible. Sometimes the anthers and pistil do not come to maturity at the same time. There is, therefore, need for some special agency to transport the pollen from one flower to the pistil of the other flowers of the same species, and thus ensure the fecundation of the seed. This purpose is effected by insects, which, flying from flower to flower, convey the pollen adhering to their bodies. Attention must here be called to an important fact, first pointed out by Mr. Darwin, that when a flower is so constructed as to be capable of fertilisation by the wind, it *never has a gaily-coloured corolla.* If a momentary digression may be allowed, we would urge that an observer capable of detecting so capital a point, after it had escaped the notice of generations of botanists, is evidently not the mere amateur theorist which he is represented by the "rash envy" of MM. Milne-Edwards and Quatrefages. It appears, then, that the beauty of flowers, as well as their odours, are not unessential attributes designed for the amusement of man, but subserve an important purpose in the life of a plant. The more conspicuous a flower by colour or scent, the more certainly it will be visited by insects, the more surely its seed will be fecundated, and it will thus be enabled to perpetuate itself. Here, then, we see "natural selection" at work, and in this case it really tends to the preservation and multiplication of the most beautiful, and to a continued increase in brilliance of hue and delicacy of perfume.

Sir J. Lubbock has proved by experiments, which, however, he does not here detail, that insects—bees at least—are really attracted by and can distinguish colours. This, we must remark, is a most interesting fact, as proving a certain community between them and ourselves, not merely in the process of vision, but in the mental faculties. Animals, too, have their æsthetics. But the author continues: "Flowers, however sweet-smelling or beautiful, would not be visited by insects unless they had some inducements more substantial to offer. These advantages are the pollen and the honey; although it has been suggested that some flowers beguile insects by holding out the expectation of honey which does not really exist, just as some animals repel their enemies by resembling other species which are either dangerous or disagreeable."

That such false pretences may really exist becomes all the more probable if we remember that certain plants attract insects by a carrion-like smell, and then kill and eat them. Of these carnivorous plants we find an interesting account in the present work, taken from the observations of Ellis, Hooker, and Canby. If it be asked why the self-fructification of plants should not be desirable, Sir J. Lubbock gives the reply: "Kolreuter speaks with astonishment of the *statura portentosa* of some plants thus raised by him; indeed, says Mr. Darwin, 'all experimenters have been struck with the wonderful vigour, height, size, tenacity of life, precocity, and hardiness of their hybrid productions.'

Mr. Darwin himself, however, was, I believe, the first to show that, if a flower be fertilised by pollen from a different plant, the seedlings so produced are much stronger than if the plant be fertilised by its own pollen. I have had the advantage of seeing several of these experiments, and the difference is certainly most striking. For instance, six crossed and six self-fertilised seeds of *Ipomæa purpurea* were grown in pairs on opposite sides of the same pots; the former reached the height of 7 feet, while the others were on an average only 5 feet 4 inches. The first also flowered more profusely. It is, moreover, remarkable that in many cases plants are themselves more fertile if supplied with pollen from a different flower, a different variety, or even, as it would appear in some instances (in the passion-flower for instance) from a different species. Nay, in some cases, pollen has no effect whatever, unless transferred to a different flower. Fritz Müller has recorded some species in which pollen, if placed on the stigma of the same flower, has not only no more effect than so much inorganic dust, but, which is perhaps even more extraordinary, in others he states that the pollen placed on the stigma of its own flower acted on it like a poison. This he noticed in several species: the flower faded and fell off; the pollen grains themselves and the stigma in contact with them shrivelled up, turned brown, and decayed; while other flowers on the same branch, which were not so treated, retained their freshness."

It may be urged that the fertilisation of flowers by means of insects is a process liable to be frustrated by a number of accidents. Thus a bee or a butterfly may go from one flower to another of a totally different order, when the pollen it conveys will of course be wasted. Sir J. Lubbock enumerates, however, several species of bees which confine themselves to particular flowers. Thus *Andrena florea* visits no other flower save that of *Bryonia dioica*, whilst *Macropis labiata* restricts itself to *Lysimachia vulgaris*.

Why certain flowers are fecundated by bees, and others chiefly by moths, is an unexplained question. In some cases the long proboscis of a sphinx may reach into the tubes of flowers where no bee could penetrate. It would almost seem that bees are more generally attracted by colour, and moths by odour. Every entomologist knows that vinegar, porter, or rum—all strong smelling liquids—is a necessary ingredient in the mixture used in "sugaring" for moths. From the wonderful development of their antennæ, we should infer them to possess the sense of smell in singular perfection.

It must be remarked that by far the majority of flower-haunting insects are hairy, a circumstance which enables the pollen to adhere more readily to their limbs and bodies. The gradation in this respect from Prosopis, through Sphecodes and Nomada to Andrena, Osmia, and Anthrophora, and finally, to the true Humble-bees, is described and illustrated in this work. A somewhat parallel arrangement is pointed out in those flowers which are fertilised by the agency of the wind, without the mediation of insects. In these, such as the alder, the hop, and the wheat, the stigma is branched or hairy, so that particles of pollen borne along by the wind may more certainly adhere. We find here some interesting facts which go to prove that bees, in their quest for honey, are not actuated by some unvarying instinct, but both vary individually in intelligence and are capable of modifying their operations. H. Müller "watched a female humble bee (*Bombus terrestris*) examining an Aquilegia; she made several vain attempts to suck the honey, but after a while, having apparently satisfied herself that she was unable so to do, bit a hole through the corolla. Having thus secured the honey she visited several other flowers, biting holes through them without making any attempt to suck them first—conscious, apparently, that she was unable to do so. . . . Any one who has watched bees in green-houses will see that they are neither confined by instinct to special flowers, nor do they visit all flowers indiscriminately. It would also appear that individual bees differ in their way of treating flowers. Some humble-bees suck the honey of the French-bean and scarlet-runner in the legitimate manner, while others cut a hole in the tube and thus reach it, so to say, surreptitiously. Dr. Ogle has observed that the same bee always proceeded in the same manner, some always by the mouth of the flower, and some always by cutting a hole. He particularly mentions that this was the case with bees of one and the same species, and infers, therefore, that the different individuals differ from each other in their degrees of intelligence."

Into the admirable exposition of the successive modifications which the mouths of insects have undergone, so as to profit by their visits to flowers, space does not permit us to enter.

Of both the works before us we feel bound, in conclusion, to express a favourable opinion. Sir J. Lubbock, whose treatise forms not the least interesting member of the valuable "Nature Series," points the way to a region where patient research cannot fail to be amply rewarded.

The Rev. J. G. Wood, though we cannot altogether agree with his speculations, is himself a patient and acute original observer, of rare merit, and tells his frequently wondrous tale in the happiest manner. The result is a work as fascinating as the well-known volumes of Kirby and Spence. The illustrations represent no fewer than six hundred species of insects, not copied at second-hand from the works of others, but all drawn from actual specimens.

VII. AËRIAL LOCOMOTION:

Pettigrew *versus* Marey.

By Professor Coughtrie.

THE great interest taken in aërial locomotion, and the increasing belief in the feasibility of a flying machine, invest works on natural and artificial flight with a certain significance and importance which cannot be over estimated in the present day, characterised as it is by unusual progress and invention.

The works to which we wish more especially to direct attention, and which have attracted an unusual share of notice, are those of Dr. J. Bell Pettigrew, of Edinburgh, and Professor E. J. Marey, of Paris.

The names of Dr. Pettigrew and Professor Marey are well known in the scientific world, and require only to be mentioned. Both gentlemen are physiologists of a high order, both have experimented largely on the subject under consideration, and both, as a consequence, are entitled to be heard.

The object of the present article is to show that these *savants*, notwithstanding certain apparent differences (and notwithstanding much that has been written to the contrary), essentially agree. The fundamental features of flight, according to both, are the same. If there be differences, they refer, for the most part, to time and the mode of treatment adopted, Dr. Pettigrew having published his views some two years before Professor Marey.

Dr. Pettigrew obtained his results by transfixing the abdomen of insects with a fine needle, and watching the wings vibrate against a dark background, by causing dragon-flies, butterflies, blowflies, wasps, bees, beetles, &c., to fly in a large bell jar, one side of which was turned to the light, the other side being rendered opaque by dark pigment; by throwing young pigeons and birds from the hand into the air for the first time; by repeated observation of the flight of tame and wild birds; by stiffening, by tying up, and by removing portions of the wings of insects and birds; by an analysis of the movements of the travelling surfaces of quadrupeds, amphibia, and fishes; by the application of artificial fins, flippers, tails and wings, to the water and air; and by repeated dissections of all the parts, directly and indirectly, connected with flight.

Professor Marey obtained his results by gilding the extremities and margins of the wings of the insect with minute portions of gold leaf; by the application of the different parts (tip and anterior margin) of the wing of the insect to a smoked cylinder rotating at a given speed, the wing being made to record its own movements; by the captive and free flight of birds, which carried on and between their wings an apparatus which, by the aid of electricity, registered the movements of the wings on a smoked surface, travelling, at a known speed, in a horizontal direction; and by the employment of an artificial wing, constructed on the plan recommended by Borelli, Chabrier, Straus-Durckheim, Girard, and others.

The treatises on flight and cognate subjects by Dr. Pettigrew and Professor Marey are so elaborate and so profusely illustrated,* that a digest of them cannot fail to be interesting to the general reader, the more especially as in that digest we hope to state in a few words, and in something like chronological order, not only the great leading features of flight, but also the points wherein Dr. Pettigrew agrees with and differs from Professor Marey—these not being generally known.

The parts of Dr. Pettigrew's and of Professor Marey's works which interest us most are those which deal with aërial locomotion and the flight of the insect and bird.

Professor Marey, in his recent book,† describes the figure-of-8 movements made by the wing in space, and for these he claims, and in some journals has obtained, considerable *cudos*, although it is difficult to understand on what grounds.

There can be no question of the fact, that the figure-of-8 movements made by the wing in flight *were first observed*,

* Dr. Pettigrew's memoirs alone contain over 200 original figures—those of Professor Marey considerably over 100.

† Animal Mechanism: A Treatise on Terrestrial and Aërial Locomotion. By E. J. Marey, Professor at the College of France, and Member of the Academy of Medicine. Henry S. King and Co. 1874.

described, and *delineated* by Dr. Pettigrew, and to this physiologist undoubtedly belongs the high merit of first discovering the true principles of flight.

Dr. Pettigrew published his discovery in the early part of 1867,* and Professor Marey did not write upon the subject of flight till the end of 1868.† There is, therefore, an interval of nearly *two years* in favour of Dr. Pettigrew.

We think it right to draw attention to this circumstance, because Professor Marey does scant justice to Dr. Pettigrew, and because we detect in all Professor Marey's writings on flight traces of Dr. Pettigrew's original discovery.

This remark applies equally to Professor Marey's theory and practice of flight.

We hope to be able to prove the validity of our position, as we advance, by a series of parallel passages. The history of science demands that this course should be taken. We begin with the figure-of-8 itself.

Professor Marey, in a letter addressed to the French Academy of Sciences, admitted Dr. Pettigrew's claim to priority in the matter of the figure-of-8 movements made by the wing in space in the following terms:—

> "I have ascertained that, in reality, Mr. Pettigrew has seen before me, and represented in his memoir‡ the figure-of-8 track made by the wing of the insect, and that the optic method to which I had recourse is almost identical with his . . . .
>
> "I hasten to satisfy this legitimate demand, and I leave entirely to Mr. Pettigrew the priority over me relatively to the question, as restricted." (Comptes Rendus, May 16th, 1870, p. 1093).

Since writing the above, Professor Marey has evidently been changing his views; for in his new work ("Animal Mechanism," p. 187) he states that, "notwithstanding this apparent agreement, our theory, and that of Dr. Pettigrew, differ materially from each other."

We have searched diligently for the points of *disagreement*, and find them trifling in character and few in number. The points of *agreement*, on the other hand, are numerous and important.

Dr. Pettigrew, in his letter of "reclamation" to the French Academy,|| to which the foregoing, by Professor

* "On the Various Modes of Flight in Relation to Aëronautics." Proceedings of the Royal Institution of Great Britain, March 22, 1867.

"On the Mechanical Appliances by which Flight is attained in the Animal Kingdom." Trans. Linn. Soc., vol. xxvi. (Read June 6th and 20th, 1867).

† Comptes Rendus. Tome lxvii., No. 26, p. 1341. Dec. 28, 1868.

‡ "On the Mechanical Appliances by which Flight is attained in the Animal Kingdom." By J. Bell Pettigrew, M.D., F.R.S. Trans. Linn. Soc., vol. xxvi. (Read to Linn. Soc. on June 6th and 20th, 1867).

|| Comptes Rendus. April, 1870.

Marey, is the reply, claims to have been the first to describe and illustrate the following:—

"1. That quadrupeds walk, and fishes swim, and insects, bats, and birds fly, by *figure-of*-8 movements."

"2. That the flipper of the sea bear, the swimming wing of the penguin, and the wing of the insect, bat, and bird, are screws *structurally*, and resemble the blade of an ordinary screw propeller."

"3. That these organs are screws *functionally*, from their twisting and untwisting, and from their rotating in the direction of their length, when they are made to oscillate."

"4. That they have a reciprocating action, and *reverse their planes* more or less completely at every stroke."

"5. That the wing describes a *figure-of*-8 *track in space*, when the flying animal is artificially fixed."

"6. That the wing, when the flying animal is progressing at a high speed in a horizontal direction, describes a *looped* and then a *waved track*, from the fact that the figure of 8 is gradually opened out or unravelled as the animal advances."

"7. That the *wing acts after the manner of a boy's kite*," both 'during the *down*' and the '*up*' strokes.*

Such are briefly Dr. Pettigrew's views; and if we compare what Professor Marey has written on flight with what Dr. Pettigrew here enunciates, we shall find the coincidences (to use no stronger terms) very striking.

Take the following passages from Professor Marey's recent work as examples:—

"If we gild a large portion of the upper surface of a wasp's wing, taking precautions that the gold leaf should be limited to this surface only, we see that the animal placed in the sun's rays *gives the figure-of*-8 with a very unequal intensity in the two halves of the image. . . . . It is evident that the cause of the phenomenon is to be found in *a change in the plane of the wing*, and consequently in the incidence of the solar rays. . . . . We shall find in the employment of the graphic method new proofs of changes *in the plane of the wing* during flight. . . . . [In this and other quotations the italics are ours.] It is therefore not necessary to look for special muscular actions to produce changes in the *plane of the wing;* these in their turn will give us the key to the oblique *curvilinear movements which produce the figure-of*-8 *course* followed by the insect's wing."—("Animal Mechanism," pp. 188, 197).

In the passages here cited, Professor Marey admits, not only that the wing of the insect makes *a figure-of*-8 *track in space*, but also that the figure-of-8 is produced by *a change of plane in the wing*.

This is an important admission, for Professor Marey copies at page 201 of his book a figure-of-8 representation from Dr. Pettigrew's 1867 memoir,† in which this change of plane is delineated, and states that the arrows in Dr. Pettigrew's figure all point in one direction, and are wrongly

* "On the Physiology of Wings." By J. Bell Pettigrew, M.D., F.R.S. Trans. Roy. Soc. of Edinburgh, vol. xxvi., p. 332.

† Marey's figure is "Fig. 86, Trajectory of the Wing," p. 201. Pettigrew's figure is at p. 233. Trans. Linn. Soc., 1867., vol. xxvi.

placed. This is a glaring inaccuracy on the part of Professor Marey.

He has in the first place reversed the direction of the arrows in Dr. Pettigrew's figure, and in the second place he makes the half of the figure represent the whole. In Dr. Pettigrew's original figure the arrows are pointing from left to right; whereas in Professor Marey's copy of it, they are pointing from right to left.

In the description given of Dr. Pettigrew's figure, it is distinctly stated that *in extension* the arrows of the figure-of-8 are directed from left to right, but that *in flexion* they are directed from right to left.*

In one complete revolution of the wing, therefore, according to Dr. Pettigrew, the arrows are directed alternately from left to right, and from right to left, and this is precisely what happens in every figure-of-8 delineated by Professor Marey.

Dr. Pettigrew, when speaking of the change of plane occurring during the down and up strokes of the wing of the insect, states that:—

"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 boy's kite."†

A little further on, and on the same page of his 1867 memoir, in which the figure-of-8 and waved tracks made by the wing in stationary and progressive flight are delineated, Dr. Pettigrew says:—

"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 affording support, but no propulsion. In these instances the backward and forward strokes are made to counterbalance each other. . . . . 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 fixed; it is otherwise when the down stroke is added, and the insect is fairly on the wing, and progressing rapidly.

"In this case the wing, in virtue of its being carried forward by the body in motion, describes an undulating or spiral course."‡

The figure-of-8 and undulating wave movements originally described and figured by Dr. Pettigrew, in March and June,

* According to Dr. Pettigrew *extension* in the insect signifies "the carrying of the wing in a forward direction, away from the body; *flexion* meaning the reverse, or the drawing of the wing from before, backwards towards the body."—(Trans. Linn. Soc., vol. xxvi., p. 226).

† On the Mechanical Appliances by which Flight is Attained in the Animal Kingdom.

‡ Trans. Linn. Soc., vol. xxvi., p. 233.

1867, have been reproduced by Professor Marey in a variety of forms since December, 1868. They are reproduced in a collective form in Professor Marey's work already referred to, published in 1874.

The importance of the figure-of-8 and wave movements cannot be over estimated, and no one appears to be more keenly alive to their value than Professor Marey himself. When speaking of the figure-of-8 made by the wing in space, originally discovered by Dr. Pettigrew by the aid of the optical method, Professor Marey remarks :—

> "We have seen, when treating of the mechanism of insect flight, that the fundamental experiment was that which revealed to us the course of the point of the wing throughout each of its revolutions. Our knowledge of the mechanism of flight naturally flowed, if we may so say, from this first notion."*

Professor Marey here admits that his knowledge of flight is derived from the figure-of-8 revealed by the optical method ; but he admitted, as already stated to the French Academy of Sciences, in May, 1870, that the optical method to which he had recourse was nearly identical with that which Dr. Pettigrew employed, and that in reality Dr. Pettigrew had seen before him, and delineated the figure-of-8 track made by the wing of the insect in flight.

If, however, Dr. Pettigrew was the first to observe, describe, and delineate the figure-of-8 made by the wing in space; and if, as Professor Marey states, his knowledge of the mechanism of flight "naturally flowed . . . from this first notion," then it is quite evident, even according to Professor Marey's own showing, that the discovery of the true principles of flight was made by Dr. Pettigrew, and *not by him.* This follows as an inevitable sequence.

It is easy to extend a discovery once made, but the true discoverer is he who first describes and delineates the fundamental principle, and in the present instance that is unquestionably Dr. Pettigrew.

Dr. Pettigrew not only described and delineated the figure-of-8 and waved track made by the wing in space; he also described and figured the several changes of plane occurring in the wing during an entire revolution.

To him, moreover, is to be traced the important discovery of the *torsion* and *forward action* of the wing both during the down and the up strokes. The torsion and forward action of the wing are indispensable in flight.

The body in flight is *dragged* forward, not pushed forward;

* Animal Mechanism, p. 234.

but unless the wings themselves fly forward in curves, both during the down and up strokes, as Dr. Pettigrew explains, the body cannot be transmitted from one point to another. Dr. Pettigrew's experiments with natural and artificial wings are quite decisive on this point, as we have ourselves verified.

Dr. Pettigrew was likewise the first to describe and figure *the ellipse* formed by the wing of the bird, and to point out the difference in the direction of the stroke in the wing of the bird and insect, the stroke in the insect being, as a rule, *nearly horizontal*, that in the bird *nearly vertical.**

Professor Marey, in his first paper on flight, communicated to the French Academy of Sciences,† delineates the wings of the wasp as making *vertical* figure-of-8 loops. Now this never happens in the wasp. The figure-of-8 loops made by the wing of the wasp, as Dr. Pettigrew has shown, are so oblique as to be *nearly horizontal.*

Professor Marey, in his latest work, has corrected this mistake‡, and has delineated the horizontal figure-of-8 loops made by the wing of the insect in a figure nearly, if not identical, with a similar figure by Dr. Pettigrew.

Professor Marey's figure occurs at page 200 of his new work (1874), that of Dr. Pettigrew's at page 338 of his memoir, "On the Physiology of Wings." (Trans. Roy. Soc. Edin., vol. xxvi., 1870).||

A careful comparison of the figures in question will show that Professor Marey's figure is, or may be, a transcript of Dr. Pettigrew's. And this remark applies not only to the figure as a whole, but to all its details; first, to the horizontal direction of the figure-of-8 loops, made by the wing

* The following is the account given by Dr. Pettigrew: "The direction of the stroke varies slightly according to circumstances, but it will be quite proper to assume that the wing of the insect is made to vibrate in a more or less *horizontal* direction, and that of the bird or bat in a more or less *vertical* direction. By a slight alteration in the position of the body, or by a rotation of the wing in the direction of its length, the vertical direction of the stroke is converted into a horizontal direction, and *vice versa*.

"The facility with which the direction of the stroke is changed is greatest in insects; it is not uncommon to see them elevate themselves by a figure-of-8 *horizontal* screwing motion, and then suddenly changing the horizontal screwing into a more *vertical* one, to dart rapidly forward in a curved line."—Trans. Roy. Soc. Edin., vol. xxvi., p. 335.

† Physiologie—Détermination éxperimentale du movement des ailes des insectes pendant le vol. Par M. E. J. Marey. Comptes Rendus, tom. lxvii., No. 26, December 28th, 1868, p. 1341.

‡ Professor Marey remarks—"We need only observe the flight of certain insects, the common fly for instance, and most of the other Diptera, to see that the plane in which the wings move *is not vertical*, but, on the contrary, *very nearly horizontal*."—(Animal Mechanism, 1874, p. 204).

|| Figs. 5 and 6 more especially.

in space; secondly, to the reversal of the planes of the wing as the wing flies to and fro, *i.e.*, during a revolution; and thirdly, to the varying angles made by the surfaces of the wing with the horizon, when the wing is made to vibrate.

Surely this is more than a mere coincidence!

Then how strangely Professor Marey has blundered as to the direction of the stroke, when this is vertical. Thus he represents the wing (p. 195, fig. 82) as descending in a *downward* and *backward* direction, and as ascending in an *upward* and *backward* direction. Now this is simply a *physical* impossibility, and clearly shows that Professor Marey has failed to interpret the tracings obtained from the wing by his so-called graphic method.

The arrows in Professor Marey's figure-of-8 (*vide* figure 82), depicting the movements of the wing in space, should, in reality, be reversed. To get a continuous series of figure-of-8 loops, or of forward curves, characteristic of progressive flight, the wing must descend and ascend always *in a forward direction*, as described and figured by Dr. Pettigrew.* The tracings obtained by Professor Marey himself show this conclusively.

At page 201 of the work under consideration, Professor Marey describes his artificial wing as consisting of a *rigid main rib* in front and a flexible sail behind, from which it follows that he is not even now aware that a natural wing, and a properly constructed artificial one, are *flexible* and *elastic throughout*.

Professor Marey is wrong, when he states that the anterior margin of the wing of the insect *is rigid*. The following are his words:—

"These experiments prove that the insect needs, for the due function of flight, a *rigid main rib* and a flexible membrane. If we cover the flexible part of the wing with a coating which hardens as it dries, *flight is prevented*. We hinder it also *by destroying the rigidity of the anterior nervure*."—P. 208.

Dr. Pettigrew, in his memoir "On the Physiology of Wings," expresses the facts in very few words:—

"The wing of a flying creature . . . . *is not rigid*.† . . . That the anterior

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* According to this authority, "a natural wing, or a properly constructed artificial one, cannot be depressed either *vertically downwards*, or *downwards* and *backwards*. It *will* (the writer would say '*does*') of necessity descend *downwards* and *forwards in a curve*. This arises from its being flexible and elastic throughout, and in especial from its being carefully graduated as regards thickness, the tip being thinner and more elastic than the root, and the *posterior margin* than the *anterior margin*."

† This is again insisted upon in "Animal Locomotion," p. 240, where Dr. Pettigrew remarks, when speaking of the construction of an artificial wave wing on the insect type, "It should be *flexible and elastic throughout*."

margin of the wing should not be composed of a rigid rod may be demonstrated in a variety of ways. If a rigid rod be made to vibrate by the hand, the vibration is not smooth and continuous; on the contrary, it is irregular and jerky, and characterised by two *pauses*, the one occurring at the end of the *up stroke*, the other occurring at the end of the *down stroke*. The wing to be effective as an elevating and propelling organ should have no dead points, and should be characterised by a rapid winnowing or fanning motion. . . . . If a longitudinal section of bamboo cane has added to it tapering rods of whalebone which radiate in an outward direction, and this (framework)* be covered by a thin sheet of india-rubber (gutta-percha tissue), an artificial wing, resembling the natural one in all its essential points, is at once produced. . . . . If this wing be made to vibrate by its root, a series of longitudinal and transverse waves are at once formed, the one series running in the direction of the *length of the wing*, the other in the direction of its *breadth*. The wing further *twists* and *untwists* during the down and up strokes. . . . . This form of wing, which may be regarded as the realisation of the figure-of-8 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."‡

In one part of his new work, indeed (viz., at p. 198), Professor Marey seems to have largely profited by the observations and experiments of Dr. Pettigrew, as given above; for he states that, if rapid to-and-fro movements in a vertical plane be given to a "*flexible shaft*" (mark, the shaft is no longer described as *rigid*), to which he affixes a membrane similar to that found in the wings of insects—to use his own words—this *flexible shaft* will then represent the main rib of the wing; and we shall see this contrivance execute all the movements which the wing of the insect describes in space." "If," he says, "we illuminate the extremity of this artificial wing, we shall see that its point describes the figure 8 like a *real wing*; we shall observe also that *the plane of the wing changes twice during each revolution*, in the same manner as in the insect itself."—("Animal Mechanism," p. 198).||

Professor Marey, it will be observed, claims for his artificial wing similar properties to those originally claimed by Dr. Pettigrew for his artificial wing. Thus Dr. Pettigrew states (*op. cit.*, pp. 421, 422), that if the anterior or thick margin of his artificial wave wing be directed upwards, and

* The words in brackets are ours.

† Trans. Roy. Soc. Edin., vol. xxvi., 1870, pp. 408, 419, 420, and 422.

‡ Physiology of Wings. By J. Bell Pettigrew, M.D., F.R.S., p. 422. (Trans. Roy. Soc. Edin., vol. xxvi., 1870).

|| The above remarks of Professor Marey are worth studying; for two reasons: first, because they are so confirmatory of all Dr. Pettigrew had written about the *flexibility* of the main nervure of an insect's wing; secondly, they contrast so strangely with the *rigid* main rib, at pp. 201 and 208 of Marey's work before cited."

the wing made to vibrate, it will fly in *an upward direction* with an undulating motion; that if the anterior or thick margin of the wing be directed downwards, the wing will describe a waved track and fly *downwards;* and 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.*

Similarly, Prof. Marey says (p. 207) that if the anterior margins of the main ribs of his artificial insect be inclined upwards, the insect *rises vertically*, and that if the anterior margins of the main ribs be turned downwards *a descending vertical force is developed*; and that if the main ribs be turned upwards, and slightly forward, it developes the force *which sustains it in the air*, and directs its course in space.

We may point out many other parallel passages. Dr. Pettigrew states (*op. cit.*, p. 335)—

"The direction of the stroke varies slightly, according to circumstances; but it will be quite proper to assume that the wing of the insect is made to vibrate in a more or less *horizontal direction*, and that of the bird and bat in a more or less *vertical direction*. By a slight alteration in the position of the body or by a rotation of the wing in the direction of its length, the vertical direction of the stroke is converted into a horizontal direction, and *vice versa*.

"The facility with which the direction of the stroke is changed is greatest in insects; it is not uncommon to see them elevate themselves by a figure-of-8 *horizontal* screwing movement, and then suddenly changing the horizontal screwing into a more vertical one, to dart rapidly forward in a curved line."

Compare with the foregoing the following from Professor Marey's new work (p. 207):—

"When an insect hovers over a flower, and we see it illuminated obliquely by the setting sun, we may satisfy ourselves that the plane of oscillation of its wings is *nearly horizontal*. This inclination must evidently be *modified* as soon as the insect wishes to dart off rapidly in any direction; but then the eye can scarcely follow it and detect the change of plane, the existence of which we are compelled to admit by the theory and the experiments already detailed."

When speaking of the wing of the bird, Dr. Pettigrew points out (Trans. Linn. Soc., vol. xxvi., p. 242) that—

"The anterior or thick margin of the wing and the posterior or thin margin present different degrees of curvature, so that under certain conditions the two margins cross each other, and form *a true helix*. The anterior margin presents two well-marked curves, a corresponding number being found on the posterior margin.

"These curves may, for the sake of clearness, be divided into *axillary* and *distal* curves; the former occurring towards the root of the wing, the latter towards the extremity.

"The anterior, axillary, and distal curves completely reverse themselves during the acts of extension and flexion, and so of the posterior, axillary, and distal curves."

In like manner Prof. Marey, in his *first* chapter on the flight of birds (at p. 210), says that—

"If we take a dead bird and spread out its wings . . . we see that, at different points in its length, the wing presents *very remarkable changes of plane*.

At the inner part, towards the body, *the wing inclines considerably both downwards and backwards*, while near its extremity it is *horizontal*, and *sometimes slightly turned up*, so that its under surface is directed somewhat backward."*

It is worthy of remark that the curves of the wing described and delineated by Dr. Pettigrew are reproduced by Prof. Marey (compare Figs. 68, 69, and 70 of Dr. Pettigrew's 1867 memoir, Linn. Soc. Trans., vol. xxvi., with the right wing, Fig. 89, p. 210, of Prof. Marey's new volume).

When speaking of the duration of the down and up strokes, Dr. Pettigrew observes (Trans. Linn. Soc., vol. xxvi., p. 261) :—

"In birds which glide or skim, it has appeared to me that the *wing is recovered much more quickly*, and *the downward stroke is delivered much more slowly*, than in ordinary flight; in fact, that the rapidity with which the wing acts in an upward and downward direction is, in some instances, more or less reversed; and this is what we would naturally expect if we recollect that in gliding the wings require to be, for the most part, in the expanded condition."

Prof. Marey writes in a similar strain. He states—

"Experiment proves that the wing of the bird is raised more quickly than it descends."—(P. 212.) . . . "Contrary to the opinion entertained by some writers, *the duration of the depression of the wing is usually longer than that of its rise.* The inequality of these two periods is more distinctly seen in birds whose wings have a large surface and which beat slowly."—(P. 228.)

Weight, according to Dr. Pettigrew, contributes to horizontal flight. In illustration he states (Trans. Roy. Soc. Edin., vol. xxvi., pp. 355, 356)—

"If two quill-feathers are fixed in an ordinary cork, and the apparatus allowed to drop from a height, the cork does not fall vertically downwards, but *downwards and forwards in a curve.* When artificial wings, constructed on the principle of natural ones, are allowed to drop from a height, they *describe double curves* in falling, the roots of the wing reaching the ground first, which proves the greater buoying power of the tips of the wings. Weight, when acting upon wings, must be regarded as an independent moving power." . . . "The *wings of the bird form a natural parachute*, from which the body depends both during the down and up strokes."—(P. 371.)

Prof. Marey performs similar experiments, and arrives at similar conclusions. Thus he explains (p. 217) that if a sheet of paper folded in the middle, with a wire loaded at one end and fixed in the bent portion, be allowed to fall, the apparatus will not descend vertically, but will follow an *oblique trajectory*; and that if the corners of the paper be bent, and the concavity directed downwards, the apparatus will in falling describe a *double curve.*

"The wings are attached exactly at the highest part of the thorax, and consequently, when the outstretched wings act upon the air as a fulcrum, all the weight of the body is placed below this surface of suspension. Thus the

* It is evident, from the succeeding paragraph to above quotation from "Animal Mechanism," that Prof. Marey had read Dr. Pettigrew's observations to which we have just referred.

heaviest part is placed as low as possible beneath the point of suspension. The bird, as it descends with its wings outspread, will thus present its ventral region downwards, without its being necessary to make an effort to keep its equilibrium; it will take this position passively, *like a parachute set free in space*, or like the *shuttlecock when it falls upon the battledore*."—("Animal Mechanism," p. 216.)

Dr. Pettigrew likens the wing of the bird to a boy's kite (Proc. Roy. Inst. of Great Britain, March 22, 1867)—

"The wing of the bird *acts after the manner of a boy's kite*, the only difference being that the kite is pulled forwards upon the wind *by the string and the hand*, whereas in the bird the wing is pushed forwards on the wind by *the weight of the body* and the life residing in the pinion itself."

Similar in substance is the subjoined passage from Prof. Marey (p. 220):—

"In the last two forms, the wing, directed more or less obliquely, derives its point of resistance from the air, *like the child's plaything called a kite*, but with this difference—that the velocity is given to the kite by *the tractile force exerted on the string* when the air is calm, while the bird when it hovers utilises the speed which it has already acquired either by *its oblique fall* or by the previous flapping of its wings."

Dr. Pettigrew attaches great importance to the activity of the wing and its small size. Thus he remarks (Trans. Roy. Soc. Edin., vol. xxvi., p. 408)—

"*The surface exposed by a natural wing*, when compared with the great weight it is capable of elevating, *is remarkably small*. This is accounted for by the length and *great range of motion* of natural wings, the latter enabling the wings to convert large tracts of air into supporting areas. It is also accounted for by the *multiplicity of the movements of natural wings*, these enabling the pinions to create and rise upon currents of their own forming, and to select and utilise existing currents." . . . "The *problem of flight would seem to resolve itself into one of weight, power, velocity, and small surfaces*, as against comparative levity, debility, diminished speed, and extensive surfaces." —(P. 386.)

Analogous in many respects to the foregoing is the following from Prof. Marey (p. 222):—

"The part played by the wing in flight is not merely passive, for a sail or a parachute ought always to have a surface in proportion to the weight which it has to support; but, on the contrary, when considered in its proper point of view, *as an organ which strikes the air*, the wing of the bird ought, as we shall see, to present a surface *relatively less in birds of a large size and of great weight*."

Again:—

"Animals of large size and great weight sustain themselves in the air *with a much less proportionate surface of wing* than those of smaller size."—(P. 222.)

Dr. Pettigrew dwells upon the relative speed attained by the different parts of the wing (Trans. Roy. Soc. Edin., vol. xxvi., pp. 399—442). He says the wing as a rule is long and narrow.

"As a consequence *a comparatively slow and very limited movement at the root confers great range and immense speed at the tip*, the speed of each portion of the wing increasing as the root of the wing is receded from." . . . "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 can attain the same object by flapping its large wings with a very slow and powerful movement." . . . "In the larger birds the movements are slower in proportion to the size, and more especially in proportion to the length, of the wing. This leads me to conclude that *very large wings may be driven with a comparatively slow motion.*"

Professor Marey illustrates the same points as under (p. 224):—

"It is not immaterial whether the surface which strikes the air has its maximum near the *body* or near the *extremity; these two points have very different velocities.* For an equal extent of surface the resistance will be greater at the point of the wing than at its base."

Again (p. 226):—

"It can be proved that, if the strokes of the wing were as frequent in large as in small birds, each stroke would have a velocity whose value would increase with the size of the bird; and as the resistance of the air increases, for each element of the surface of the wing, according to the square of the velocity of that organ, *a considerable advantage would result to the bird of large size, as to the work produced upon the air.*"

Dr. Pettigrew shows that the vigour with which the wing is propelled varies according as the bird is rising, falling, or progressing in a horizontal direction (Trans. Linn. Soc., vol. xxvi., pp. 227, 260, 261). He observes:—

"All birds which do not, like the swallow and humming birds, drop from a height, raise themselves at first by a vigorous leap, in which they incline their bodies in an upward direction. *By a few sweeping strokes*, delivered downwards and forwards, in which the wings are nearly made to meet above and below the body, they lever themselves upwards and forwards, and in a surprisingly short space of time acquire that degree of momentum which greatly assists them in their future career." . . . "The forward movement of the wing during the down or effective stroke is particularly evident in birds when rising, the wing on such occasions being urged with *unusual vigour.*"—(P. 227, *op. cit.*)

"When the bird has elevated itself to the desired height, *the length of the downward stroke is generally curtailed*, the mere extension and flexion of the wing, assisted by the weight of the body, in some cases sufficing for the ordinary purposes of flight. This is especially the case if the bird is advancing against a slight breeze."—(Pp. 260 and 261.) "If birds wish to descend, they may reverse the direction of the inclined plane, and plunge head foremost with extended wings; or they may flex the wings, and so accelerate their pace; or they may raise their wings, and drop parachute fashion; or they may even fly in a downward direction—*a few sudden strokes*, a more or less abrupt curve, and a certain degree of horizontal movement, being in either case necessary to break the fall previous to alighting."—(P. 262, *op. cit.*)

Prof. Marey, as the annexed passages show, also adverts to the relative frequency and force with which the wing is urged in ascending, descending, and horizontal flight, though less fully than Dr. Pettigrew does:—

"The frequency of the strokes of the wing varies also according as the bird is first starting in full flight or at the end of its flight."—(P. 228, "Animal Mechanism.")

Again :—

"Confining the question within these limits, experiment shows that the strokes of the bird's wing *differ in amplitude and in frequency* from one moment to another as they fly. When they first start *the strokes are rather fewer, but much more energetic;* they reach, after two or three strokes of the wing, *a rhythm almost regular, which they lose again when they are about to settle.*" —(P. 234, *op. cit.*)

Dr. Pettigrew lays especial emphasis on the elliptical movements made by the wing of the bird (Trans. Linn. Soc., vol. xxvi.) Thus he remarks :—

"During extension the elbow and bones of the fore-arm, particularly their distal extremities, describe an *upward curve.* During flexion the elbow and bones referred to describe another but *opposite curve.* The movements described by the elbow-joint during extension and flexion may consequently be represented by an *ellipse or ovoid.*"—(P. 248, *op. cit.*, Diagrams 8 to 13 more especially.)

Prof. Marey follows Dr. Pettigrew in the matter of these elliptical movements. He says :—

"During the whole of the bird's flight the registering lever described *a kind of ellipse.*" . . . "All our experiments have shown that birds of different species describe with their wings *an elliptical trajectory.*"—(P. 242.)

Again :—

"In fact, the bone of the wing in each describes a kind of *irregular ellipse,* with its greater axis inclined *downwards and forwards.*"

Dr. Pettigrew represents the wing of the bird as oscillating on two separate axes,—the one running parallel with the bird, the other at right angles to it,—and adds (in his 1867 memoir to the Linnean Society, p. 243)—

"The wing may be said to agitate the air in two principal directions, viz., from *within outward or the reverse,* and from *behind forward or the reverse,* the agitation in question producing two powerful pulsations—a longitudinal and a lateral."

Prof. Marey gives, more briefly but yet in very similar terms, the same statement. He says (p. 247)—

"That a bird passes through a *double oscillatory movement,* in a vertical plane, for each revolution of its wings."

Dr. Pettigrew describes and delineates the wing of the bird as advancing in a curved line, both when it rises and falls. He observes (Trans. Linn. Soc., vol. xxvi., pp. 214 and 233)—

"In the water the wing strikes *downwards and backwards* (and acts as an auxiliary of the foot), whereas in the air it strikes *downwards and forwards.* . . . To counteract the tendency of the bird in motion to fall in a downward and forward direction, the stroke is delivered in the direction in which falling would naturally occur,—the kite-like action of the wing, and the rapidity with which it is moved, causing the mass of the bird to pursue a more or less horizontal direction. I offer this explanation of the action of the wing in and out of the water after repeated and careful observation in tame and wild birds, and, as I am aware, in opposition to all previous writers on the subject."

Prof. Marey again corroborates Dr. Pettigrew's original observations, as the subjoined extract from "Animal Mechanism" will show (p. 254):—

"The inspection of the curve shows us also that the pigeon's wing was carried more especially in the direction of the upper parts, similar to the point A; in other terms, that *the forward predominated over the backward movement.*"

Dr. Pettigrew describes and figures the body of the bird in flight as alternately rising and falling in forward curves, the curves described by the body being the opposite of those described by the wing, those movements being due to a kite-like action of the wings (Trans. Roy. Soc. Edin., vol. xxvi., pp. 343, 344). Thus Dr. Pettigrew remarks:—

"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. In both cases the wing describes *a waved track*, which clearly shows that the wing strikes downwards 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.*" . . . . "As the body of the insect, bat, and bird, falls forward in a curve when the wing ascends, and is elevated in a curve when the wing descends, it follows that *the trunk of the animal is urged along a waved line.* I have distinctly seen the alternate rise and fall of the body and wing, when watching the flight of the gull from the stern of a steamboat."

Professor Marey writes in a very similar strain. He asks (pp. 264, 265):—

"But do we find that the bird, when suspended in the air, keeps at a constant level, or *does it pass through oscillations in the vertical plane?* Do we not experience, by the intermittent effect of the flapping of its wings, *rising and falling motions*, of which the eye can detect neither the frequency nor extent? Again, does not the bird advance in its onward course, with variable rapidity? Shall we not find in the action of its wings a *series of impulses*, which give to its advancing course a *jerking motion?* These queries can be answered experimentally." . . . . "To explain the ascent of the bird during the time of the elevation of the wing, it seems indispensable to refer to *the effect of the child's kite*, to which we have before alluded. The bird, having acquired a certain velocity, presents its wings to the air *as inclined planes.*" . . . . "Thus, by registering at the same time the two orders of oscillation in the flight of a buzzard, we find that the phase of *depression of the wing* produces at the same time the *elevation of the bird* and the acceleration of its horizontal swiftness."—(P. 269). . . . "A part of this resistance, viz., that which is applied to *the lower surface of the wing*, is utilised to sustain the bird by the kind of action which we have compared to that of *a child's kite.* It appears that this action is of primary importance in the flight of the bird."—(P. 275). . . . "In the bird, one of the phases of the movement of the wing is, to a certain extent, passive; that is to say, it receives the pressure of the air *on its lower surface, when the bird is projected rapidly forward by its acquired velocity.* Under these conditions, the whole bird, being carried forward in space, all the parts of the wing are moved with the same rapidity, to take advantage of the action of the air, *which presses on them as on a kite.*"—(P. 276).

Dr. Pettigrew explains that the wing is a screw structurally and functionally; that it revolves on two axes (the one running in the direction of the length of the wing, the other in the direction of its breadth), and that the action of the wing resembles the action of an oar in sculling. (Trans. Linn. Soc., vol. xxvi., pp. 206, 229, 231, and 266; Proc. Roy. Inst. of Gt. Britain, March 22, 1867). Thus, he states that:—

"In the fish, the lower half of the body and the broadly expanded tail are applied to the water *very much as an oar is in sculling.*" . . . . The fish may be said to drill the water in two directions, viz., from behind forward, by a *twisting or screwing* of the body on its long axis, and from side to side by causing its anterior and posterior portions to assume opposite curves. The pectoral and other fins are also thrown into curves in action, the movement, as in the body itself, travelling *in spiral waves*; and it is worthy of remark that the wing of the insect, bat, and bird obeys similar impulses.

"The fins *are rotated or twisted*, and their free margins lashed about by spiral movements, which closely resemble those by which the wings of insects are propelled. . . . . That the wing *twists upon itself structurally*, not only in the insect, but also in the bat and bird—anyone 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 wing of the bird acts *as a twisted inclined plane.* In this respect it intimately agrees with the wing of both the insect and bat. . . . . The twisting in question is most marked in the posterior, or thin margin of the wing, the anterior or thicker margin performing more *the part* of *an axis.* As the 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."

Professor Marey goes over the same ground in much the same way. Thus, at pages 107, 109, 198, 208, 210, 211, 259, and 261, he states:—

"The oar is found in many insects which move on the surface of the water. A contrivance is employed by other animals, which resembles the action of an oar used at the stern of a boat in the process called *sculling.* To the latter motive power may be referred all those movements in which *an inclined plane* is displaced in the liquid, and finds in the resistance of the water which it presses obliquely two component forces, of which one furnishes a movement of propulsion." . . . . "When a fish strikes the water with his tail, in order to drive himself forward, he executes a double work, a part tends to drive behind him a certain mass of fluid with a certain velocity, and the other to drive the animal forward, in spite of the resistance of the surrounding water. . . . . *Aërial Locomotion.*—This mechanism is still the same; the motion of *an inclined plane*, which causes motion through the air, the wing in fact, in the insect as well as in the bird, strikes the air *in an oblique manner*, repels it in a certain direction, and gives the body a motion directly opposite." . . . . . "Each stroke of the wing *acts upon the air obliquely*, and neutralises its resistance, so that a horizontal force results, which impels the insect forward. An effect is produced analogous with that which takes place when an oar is used in the stern of a boat in the *action of sculling.*" "Most of the propellers which act in water overcome the resistance of the fluid by the action of *an inclined plane.* The tail of the fish produces a propulsion of this kind. *Even the screw may be considered as an inclined plane*, whose movement is continuous, and always in the same direction. . . . . We see the main rib (anterior margin) of the wing remain *sensibly immovable, and around it turns the membranous portion* (posterior margin). . . . . "If this motion as on a pivot did not exist, the wing would cut the air with its edge, and would be utterly incapable of producing flight." . . . . The wing presents *very remarkable changes* of *plane* at the *inner part towards* the body; the wing *inclines considerably, both downwards* and *backwards*; while near its extremity it is *horizontal and somewhat slighty turned up*." . . . . We admit that the wing *revolves on an axis*." . . . . "It was necessary therefore, for the lever, while fixed to the feathers of the bird, to glide freely on the rod in the direction of its length; and yet that it should cause it to receive, under the *form of torsion*, all the changes of inclination that are transmitted to it by the wings of the bird." . . . . "For this purpose we must return *to the dotted figure* 8, which is the impression of the *torsions of the wing* of the different instants."

Dr. Pettigrew points out that the wing acts as a true kite, both during the down and up strokes. He remarks—(Trans. Roy. Soc. Edin., vol. xxvi., p. 343) :—

"If, as I have endeavoured to explain, the wing, even when elevated and depressed in a strictly vertical direction, inevitably and invariably darts forward, it follows, as a consequence, that the wing flies forwards as a true kite, both during the down and up strokes, 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. The angle made by the wing of the bat and bird with the horizon is constantly varying, as in the insect wing."

Professor Marey, in his earlier writings ("Revue des Cours Scientifiques de la France et de l'Etranger," Mars, 1869) describes the wing as making *a backward angle of* 45° *with the horizon during its descent, and a forward angle of* 45° *during its ascent.*

This view was shown by Dr. Pettigrew to be untenable, and we find it greatly modified in Professor Marey's later work. Thus, at fig. 111, p. 263, where the angles of inclination made by the wing during its rise and fall are given, the under surface of the wing is represented as *forming a kite*, during quite three-fourths of one entire revolution of the wing.

At no point is the wing represented as making, during its descent, *a backward angle of* 45°. Nor is this all. At p. 274, Professor Marey states that—

"In free flight the axis of the bird is horizontal, or rather *turned somewhat upward*. Restored to this proper position, a fresh direction would be given to each of the positions of the wing. Then, probably, we should see that the wing always *presents its lower surface to the air*, as the only one which can find in it a point of resistance."

In this modified statement, as may readily be perceived, we have simply a repetition of Dr. Pettigrew's view, viz., "*that the under surface of the wing acts as a kite, both when the wing rises and falls.*"

We might greatly multiply these parallel passages in proof of our original assertion, that in all Professor Marey's writings and experiments in flight, *Dr. Pettigrew's original discoveries and experiments may be traced*, and we may fairly emphasise our other statement, "*that Professor Marey has done scant justice to Dr. Pettigrew.*

This is the more evident, as we are given to understand that Dr. Pettigrew's original memoirs and papers were duly transmitted to Professor Marey immediately after their publication.

From the foregoing, it will be evident that Professor Marey has added comparatively little to the science of Aërostation. He has, for the most part, simply confirmed by experimental methods, in which he is an adept, Dr. Pettigrew's original observations and experiments, published nearly two years before his own experiments were undertaken.

Professor Marey is certainly not entitled to say, as he does at p. 187, that "*notwithstanding this apparent agreement, our theory and that of Dr. Pettigrew differ materially from each other.*"

Still less is he entitled, virtually, to appropriate Dr. Pettigrew's descriptions and figures, without full and fair acknowledgment. Least of all is he entitled to modify and misrepresent those descriptions and figures.

Such practices sap the foundation of science.

NOTICES OF BOOKS.

The Aerial World: A Popular Account of the Phenomena and Life of the Atmosphere. By G. Hartwig, M. & P.D. London: Longmans, Green, and Co. 1874.

In this work we find a complete chemical and physical history of the atmosphere; the facts of all the sciences are brought forward to elucidate the subject: the physical properties of the atmosphere, sound and echoes, light in connection with the colours of the sky, heat in connection with the temperature of the air, winds, &c., electricity in connection with thunderstorms, St. Elmo's fires, and electrical meteors; of each and all of these we have a full description.

The first chapter treats of the weight and pressure of the air. In regard to the effect of rarefied air on the system, the author remarks that it is scarcely likely that any one will in future be foolhardy enough to endeavour to ascend above six miles, seeing how nearly Messrs. Coxwell and Glaisher lost their lives at the time of their famous balloon ascent. It has, however been suggested, that people who are in the habit of living in rarefied air—as the inhabitants of Quito and Potosi—might be able to reach a greater altitude than those who live at or near the level of the sea; and this experiment, which has never been tried, is certainly worth trying. In the course of a few generations of life, at high altitude, it is by no means improbable that the system might adapt itself to the surrounding circumstances, and that an extension of this rarefaction might be borne without difficulty.

The second chapter treats of the chemical composition of the atmosphere, and the various gases which compose it are described. In speaking of ozone, the author quotes the "Odyssey" to show that the peculiar sulphurous smell which accompanies manifestations of electricity has been known from the time of Homer. He omits, however, that extremely interesting philological fact, that the Greek for sulphur, *θειον*, was derived from this cause. Zeus manifested himself by thunderbolts; the sulphurous smell accompanied such manifestations; hence, clearly sulphur, in some way or other, appertains to the gods; hence it is assumed that it was called *θειον* (*θειος*); and we have the echo of the old Greek word in some of our more complex sulphur acids—trithionic, tetrathionic, pentathionic. In the third chapter we are introduced to the science of Acoustics, and come again to the mighty Zeus, in whose forest of Dodona the rustling of the oak leaves was believed to be the voice of the god himself. It is thus that the author blends mythology and history with the more sober facts of science. The fifth chapter contains some interesting remarks concerning the colours of the sky, which question has been debated any time since that of Leonardo da Vinci. He and many subsequent writers attributed the celestial blue to a mixture of the light reflected from terrestrial substances with the darkness of space. Goethe, in his "Farbenlehre," revives the same idea: he points out that, when we look through a turbid medium which is illuminated, at a black background, the medium appears to be blue; while light itself, viewed through such a medium, is yellow or red. He points out also the blueness of distant mountains, and of smoke seen in particular lights. The author then alludes to Tyndall's experiments, and with him concludes that the blue colour of the sky is due to the presence of extremely minute particles of water. On pp. 80-81, we find a few inaccuracies of printing; thus temperatures are written like degrees of arc, $17^\circ\ 6'$ F. $50^\circ\ 4'$, and (p. 81) the *minus* is omitted in stating extremes of cold:—53°, 58°. The table pp. 82-83, is also printed rather carelessly in the case of the *Latitude* column. Among the low European temperatures we find -5° F. near London, $-10{\cdot}5^\circ$ in Paris, -22° in Hamburg, -37° in St. Petersburg, -44° in Moscow, and -58° at Enontekis in Lapland. The records of several winters in Europe are most surprising; it is said that in 860 and 1234 the Adriatic was frozen over, and goods were transported from Venice to the opposite Dalmatian coast, over the ice. In 1364 all the rivers of France were frozen over, and the ice on the Rhone was 15 feet thick. The winter of 1788-89 appears to have been one of the severest on record. The Rhone was frozen over at Lyons, and the port of Ostend was closed by ice; and the Thames was frozen as far as Gravesend. As to European extremes of heat, we learn that in 1793 the thermometer in London registered 89° F., and in Paris, 101° F.; in each instance in the middle of July. In 1852, the thermometer in London rose to 95° F. in the shade on July 12th.

In the seventh chapter, "On Winds," the author devotes several paragraphs to an account of the *Tower of the Winds* in Athens, using always the past tense, "Boreas *was* represented," other winds "*were* represented." But there is no need for this. We may use the present tense for many long years to come; the Tower of the Winds is quite perfect, and is of comparatively late erection, that is to say, 100 B.C. The description of the monsoons, and the causes which produce them, is as complicated and difficult to understand as it is in every book in which we have seen them mentioned and described. An interesting account of a thunderstorm seen from a balloon, and a "side view of a storm," will be found in the fourteenth chapter. We notice in the woodcuts the usual error in regard to lightning, which is made pointed, whereas, as Faraday pointed out, if we could see the end of the flash at all, it would be at least as thick as the rest of the flash. Remarkable examples are recorded of the power of lightning. Deaths from lightning were most numerous in 1870, 202 deaths were recorded throughout the whole country: the deaths from sunstroke are twice as numerous. The instantaneous appearance of *rigor mortis* in the case of persons killed by lightning appears to be well authenticated. A curious slip of the pen may be noticed on p. 289, where the author, in mentioning the fact that the Emperor Augustus used to retire to a cave on the approach of a thunderstorm, puts him and his adjective in the accusative case:—"divu*m* Augustu*m*." The Romans considered seal-skins a preventive to lightning, and the emperor Tiberius caused himself, on the approach of a storm, to be crowned with laurel, believing that lightning never struck the plant sacred to Apollo.

An interesting and novel chapter is devoted to "The Primeval Atmosphere." In this the author points out that during the Silurian epoch—the earliest of all—the atmosphere must have been as transparent as it is now; he questions whether the air during the luxuriant carboniferous epoch can have had much more carbonic acid in it than it has now, for Liebig has shown, that if all the carbon contained in all living vegetation and in the coal measures was in the atmosphere again in the form of carbonic acid, the quantity of that gas in the air would scarcely be doubled, and would not be unfit for the respiration of the higher animals. While the composition of the air may not have undergone much change the temperature certainly has, for coal has been discovered in the Arctic regions, and in it plants that could only grow in a temperate climate. Again, during the Miocene Period, the temperature of Europe must have been at least sub-tropical; the flora somewhat resembles that of South Carolina, while apes abounded in the Pyrenees and in Greece. The latter chapters of the work are devoted to flying and ballooning.

The work, from beginning to end, is pleasantly written, and is very readable; it is crowded with facts derived from every available source, and however dry they may be in the abstract, they are clothed here in popular language, and their true interest is shown. The work is well printed, and beautifully illustrated by chromoxylographic plates, and by a good map and woodcuts. It belongs to the "Guillemin" and "Flammarion" class. Such books do much to induce a love for science and for Nature, and we cordially recommend this work to the notice of all our readers.

Science Primers. Astronomy. By J. Norman Lockyer, F.R.S. Illustrated. Macmillan and Co. 1874.

This little work forms the sixth of the series of Science Primers, edited by Professors Huxley, Roscoe, and Balfour Stewart. Although modestly called a Primer, it contains an immense deal of information, and discusses sun-spots, nebulæ, and multiple stars, while it further makes clear to us the meaning of such terms as "polar distance" and "right ascension." The language throughout is of the clearest; the book is well illustrated, and the author has throughout described simple experiments to illustrate his meaning. By using a moderator lamp to represent the sun, an orange with a knitting-needle stuck through it to represent the earth, a tub of water with floating balls, a small suspended ball, a round table with a ball placed in the centre, and a few simple contrivances of this sort, a number of important phenomena are made clear to the learner. The following is an example of the author's style:—"Thus, besides the planets, there are other members of the system, namely comets and falling stars, which will be mentioned again more fully hereafter. All these bodies form a sort of family having the sun for their head, and on Plate II. will be seen a view of this system as it would appear when looked at from above; but it is impossible thus to give an idea of the true scale of the system. In order to do this, take a globe a little over two feet in diameter to represent the sun: Mercury would now be proportionately represented by a grain of mustard seed, revolving in a circle 164 feet in diameter; Venus, a pea, in a circle of 284 feet in diameter; the earth also a pea, at a distance of 430 feet; Mars, a rather large pin's head, in a circle of 654 feet; the smaller planets by grains of sand, in orbits of from 1000 to 1200 feet; Jupiter, a moderate-sized orange, in a circle nearly half-a-mile across; Saturn, a small orange, in a circle of four-fifths of a mile; Uranus, a full-sized cherry or small plum, upon the circumference of a circle more than a mile and a half; and Neptune a good sized plum, in a circle about two miles and a half in diameter."

Introduction to Experimental Physics, Theoretical and Practical; including Directions for Constructing Physical Apparatus, and for Making Experiments. By Adolph F. Weinhold, Professor in the Royal Technical School at Chemnitz. Translated by Benjamin Loewy, F.R.A.S. Preface by G. C. Foster, F.R.S. London: Longmans, Green, and Co. 1875.

In the Preface to this work, Professor Foster says:—"I am convinced that the true way to make the somewhat abstract notions necessarily encountered at the outset of the study of physics intelligible to beginners, is not to emphasize the abstractions, but to provide the learner with the clearest possible ideas of the concrete facts from which the abstractions are derived. In any sound system of teaching, particulars must come before generalities; for, unless a student has clear conceptions of individual phenomena, it is impossible for him to understand their mutual relations or the general conclusions that are based upon them." To provide an account of these concrete physical facts, and to show how they may best be wrested from Nature, has been the object of Professor Weinhold in the book before us. He takes us through a course of elementary physics, and shows us, not only how to make the experiments, but also how to make the necessary apparatus. If anything, his descriptions appear sometimes to be too tediously minute, but for the beginner they are certainly not so. Here, for example, we have directions for boring a hole in glass—an operation which is frequently necessary in physical experiments:—"The hole is bored with a round file, of which the point is broken off, so that a round surface of from 2 to 3 m.m. in diameter is obtained. The edge of this round surface is used for drilling, the file being held with the right hand in a slanting direction, and the thumbs being kept quite close to the broken end of the file, in order to increase the pressure, and also to prevent the file from slipping through the finished hole and breaking the glass. File and glass must, during the operation, be frequently welted with water, or, better still, with oil of turpentine. After piercing the glass completely by the point, the whole is enlarged by slowly turning the file from right to left in the manner in which a screw is drawn, repeatedly moistening the orifice and the file." The author truly says it is advisable to practise this operation first on pieces of broken glass; for, if ever patience be wanted, it is when one is engaged in boring a hole in glass, or endeavouring to render the double needle of a galvanometer astatic. Let us glance at the author's treatment of any individual science; for example, *heat*. Omitting all theoretical matter, the author plunges at once into the subject; tells us that there is a particular sensation depending upon change of temperature, and that to the cause of this sensation is given the name Heat. Of the various effects of heat upon matter, expansion comes first, and this is illustrated by various means in the case of solids, liquids, and gases; and various experimental hints and cautions are given. An account of the thermometer follows, and rules for the conversion of degrees, and for determining the fixed points; then various effects of expansion, Prince Rupert's drops and Bologna flasks; anomalous expansion of water, absolute and apparent expansion, and the expansion of gases. A section is then devoted to melting and freezing, followed by evaporation and ebullition; radiation and conduction; specific and latent heat, and sources of heat. There is thus nothing new in the arrangement of subject matter, or indeed in the treatment of it, save that each experiment is minutely described, the necessary precautions and the conditions of success. The young student of physics will find this work a most useful companion.

Elements of Magnetism and Electricity. By JOHN ANGELL, Senior Science Master, Manchester Grammar School, with 120 illustrations. London and Glasgow: William Collins, Sons, and Co. 1875.

THIS very cheap and comprehensive little manual belongs to the capital series of elementary works on science which Messrs. Collins are now issuing. It is almost entirely devoted to Magnetism and Frictional Electricity, one chapter only at the end of the book being devoted to Voltaic Electricity. The figures are plain, but quite sufficient for their purpose; we may particularly call attention to the representations of magnetic curves (Figs. 19—23), and to the figures showing electrical distribution on various surfaces (Fig. 89). Hints are frequently given for the construction of cheap apparatus, but these cannot always be relied upon; for example, we are told how "an efficient electrical machine," capable of performing all the ordinary experiments, may be constructed out of a common black wine-bottle for a "few pence;" but all who have tried to make an electrical machine know how very difficult it is to get anything like an efficient machine at a cost of much time and many shillings. There are a thousand difficulties in the way: to mount the cylinder—to make it rotate in a horizontal plane—to manipulate the even pressure of the rubber—are among them. Again, some of the explanations are insufficient; for example, in describing the electrical whirl, the author says:—"On working the machine, a powerful wind, the electrical aura, will be produced, which, *re*acting against the points, will drive the wheel round with great velocity." But he does not explain why the wind is produced. It is a general fault in elementary works, in which as much matter as possible is introduced at the expense of clearness, to describe experiments without fully explaining them. But the book is distinctly good, and a marvel of cheapness, and will be found most useful as a commencing text-book.

Elements of Animal Physiology, chiefly Human, with Hints on Practical Work, Dissection, &c. By JOHN ANGELL, Senior Science Master, Manchester Grammar School. London and Glasgow: William Collins and Sons. 1875.

THIS is a shilling book containing 92 capital figures, and more than 500 separate paragraphs of text. It is a marvel of cheapness, and although we must of course regard it as, to a great extent, a compilation, it contains a great amount of sound information and useful knowledge. It is divided into twenty chapters: the first two treat of the general structure and functions of the human body; here we notice a complete comparison of the actions of a living body with those of a steam engine, in which we learn that the total *internal* mechanical work of a living man is no less than 715,000 foot-pounds per day, which Helmholtz calculates is equal to the total *external* work of a hard-working man. It has also been calculated that 2¼ pounds of good coal, if its force could all be utilised, ought to produce in a steam-engine 2,145,000 foot-pounds of mechanical work, or about three times the external work of a man. The third chapter treats of the skeleton; the fourth and fifth of chemical and histological preliminaries; in the latter, very good drawings of voluntary muscular fibre and of the structure of nerve fibre. The sixth and seventh chapters discuss the blood; the eighth and ninth, respiration and animal heat; the tenth and eleventh, digestion (beautiful drawing of a vertical section of the coats of the stomach); an interesting chapter on animal mechanics, and concluding chapters on the voice, the ear, the eye, and the brain. The author appears to be quite *au courant* with the most recent results of physiology, and we heartily recommend his little treatise to those who desire to commence the study.

The Methods of Ethics. By HENRY SIDGWICK, M.A., Lecturer and late Fellow of Trinity College, Cambridge. London: Macmillan and Co. 1874.

DEFINING ethics as "the Science of Conduct," the author shows that it may conveniently include the related studies of politics and jurisprudence. These sciences alike attempt to determine the nature of the ideal, not the real; to demonstrate what ought to exist, not what does exist. The rational ends are divided into two only, *i.e.*, Perfection and Happiness, and the methods of Ethics are reduced in the main to three—Egoism, Intuitionism, and Utilitarianism. The end of Egoism is defined as "the sum of pleasures, valued in proportion to their pleasantness;" the fundamental assumption of Intuitionism is "that we have the power of seeing clearly what actions are in themselves right and reasonable;" while by Utilitarianism is meant the theory first distinctly enunciated by Bentham "that the conduct which, under any given circumstance, is externally or objectively right, is that which will produce the greatest amount of happiness to all whose interests are affected." The work, admirable as it is, possesses little interest for the man of science, as ethical modes of thought are altogether dissimilar to the usual modes of scientific thought; we may briefly glance, however, at the chapter on "Philosophical Intuitionism." A philosopher is allowed at the outset some amount of divergence from common sense, and he is expected to altogether transcend common sense in his premises, and to sink his plummet into profound and infinite depths. But such men have often, after a life of study, presented to the world a host of arguments and conclusions which move in a narrow circle, or a host of propositions swollen out with tautological excesses, and like a windbag ready to burst on the application of the least pressure. And here we venture to think the philosopher of science has the advantage of the philosopher of morals; for the former can scarcely argue in a circle when he is confronted at every step by facts wrought out of nature by legitimate experiment and exact induction.

The Transit of Venus: its Meaning and Use. By T. H. Budd, F.R.A.S. London: Longmans, Green, and Co. 1875.

In this clever and concise little pamphlet the author tells us, in plain untechnical language, all that a general reader interested in science, but not specially an astronomer, can require to know. We will briefly condense the main facts of the case. The transit of Venus means, we all know, the passage of Venus across the face of the sun; and since the moon eclipses the sun under like circumstances, and Venus has thrice the diameter of the moon, we may reasonably enquire why the sun is not eclipsed during the transit of Venus. But the fact that Venus is far more distant from us than the moon converts what would be an eclipse into a transit. The transit occurs twice in 116 years, with an interval of 8 years between two successive transits in that period, *e.g.*, in 1761 and 1769, and again in 1874 and 1882. The long interval is due to the fact that the plane of the orbit of Venus is inclined to the plane of the earth's orbit round the sun. The exact observation of the transit of Venus enables astronomers to ascertain our distance from the sun; and as it is believed that an error of three millions of miles exists in our present estimate of that distance, the necessity of a new determination is obvious. There must always be some error, but repeated observations will reduce it to a minimum. The Author points out and illustrates by a diagram how, by means of a theodolite and a simple trigonometrical method, it is possible to measure an inaccessible object, and how this method may be applied to the sun when a suitable base line has been obtained. The greatest possible distance between two observers on the earth's surface—that is, the greatest possible base line on one and the same meridian—is something less than 7000 miles, because the polar regions are inaccessible. The observation of the different positions of Venus when crossing the sun's face, by two distant observers, enables us to ascertain the diameter of the sun, and by comparing his apparent diameter with his real diameter we can ascertain his distance from us in the same way that we can determine the distance of a church steeple if the height be known.

The Origin of Civilisation and the Primitive Condition of Man: Mental and Social Condition of Savages. By Sir JOHN LUBBOCK, Bart., M.P., F.R.S. Third Edition, with numerous Additions. London: Longmans, Green, and Co. 1875.

THIS volume, which has already reached a third edition, is founded on the subject-matter of a course of lectures delivered at the Royal Institution in 1868. It embodies the result of ten years of study, and constitutes a considerable addition to our study of Anthropology. The great bulk of the work treats of the religion of savages, and this subject is treated very ably in some concluding remarks:—" Thus we see that as men rise in civilisation their religion rises with them. The Australians dimly imagine a being,—spiteful, malevolent, but weak, and dangerous only in the dark. The negro's deity is more powerful, but not less hateful; invisible, indeed, but subject to pain, mortal like himself, and liable to be made the slave of man by enchantment. The deities of the South Sea Islanders are, some good, some evil; but, on the whole, more is to be feared from the latter than to be hoped from the former. They fashioned the land, but are not truly creators, for earth and water existed before them. They do not punish the evil, nor reward the good. They watch over the affairs of men; but if, on the one hand, witchcraft has no power over them, neither, on the other, can prayer influence them; they require to share the crops or the booty of their worshippers. . . . It appears, then, that every increase in Science—that is, in positive and ascertained knowledge—brings with it an elevation of religion. Nor is this progress confined to the lower races. Even within the last century Science has purified the religion of Western Europe by rooting out the dark belief in witchcraft, which led to thousands of executions, and hung like a black pall over the Christianity of the Middle Ages."

Other chapters of the work are devoted to the art and ornaments of savages, their marriage rites, and ideas of relationship, their morals, languages, laws. The Author considers, finally, that he has conclusively proved—" 1. That existing savages are not the descendants of civilised ancestors. 2. That the primitive condition of man was one of utter barbarism. 3. That from this condition several races have independently raised themselves."

The Protoplasmic Theory of Life. By JOHN DRYSDALE, M.D. London: Baillière, Tindall, and Cox. 1874.

THE object of this work is to prove "that every action properly called vital, throughout the vegetable and animal kingdoms, results solely from the changes occurring in a structureless, semifluid, nitrogenous matter, now called Protoplasm." It lays claim to no originality, and the subject-matter is expanded from a lecture delivered before the Microscopical Society of Liverpool during the present year. The Author is evidently well acquainted with his subject; he is well acquainted with the literature of it, and quotes a multitude of men, from Schleiden, Schwann, and Leydig, to Beale, Herbert Spencer, and Huxley. He sums up the whole variety of opinion on the subject in the following opening sentences, which divide them into—" 1. Those which require the addition to ordinary matter of an immaterial or spiritual essence, substance, or power, general or local, whose presence is the efficient cause of life; and 2. Those which attribute the phenomena of life solely to the mode of combination of the ordinary material elements of which the organism is composed, without the addition of any such immaterial essence, power, or force)." Up to the year 1835 the old idea of a *vital fluid* was in some form or other dominant; in that year the *coup de grace* was given to the theory by Dr. Fletcher, of Edinburgh, who, in his "Rudiments of Physiology," discussed the merits of the theory in a perfectly calm and judicial manner, and decided against it. The protoplasmic theory was proposed so recently as 1860, by Dr. Lionel Beale, who describes fundamental germinal matter, or bioplasm, as "always transparent and colourless, and, as far as can be ascertained by examination with the highest powers, perfectly structureless, and it exhibits these characteristics at every period of its existence. . . . Nor can any difference be discerned between the germinal matter of the lowest, simplest, epithelial scale of man's organism, and that from which the nerve-cells of his brain are to be evolved." Then follow commentaries on Beale's nerve theory and muscle theory; then various definitions of life, among which we may notice De Blainville's "Life is the two-fold internal movements of composition and re-composition ,at once general and continuous."

The concluding chapter treats of Materialism. The Author shows how frequently the term is misunderstood or wrongly defined, and shows, further, that religion and science need not be at war. The facts of the theory of protoplasm are clearly set forth in this very readable work, and the non-scientific man may by its help make himself well acquainted with one of the most prominent physiological theories now before the world.

The Elements of Embryology. By MICHAEL FOSTER, M.A., M.D., F.R.S., Fellow of and Prælector in Physiology in Trinity College, Cambridge, and FRANCIS M. BALFOUR, B.A., Fellow of Trinity College, Cambridge. London: Macmillan and Co. 1874.

THE Cambridge Physiological Laboratory is already beginning to bring forth good work, and it is satisfactory to see the name of a young student side by side, as joint author, with that of the able Prælector in Physiology. A few years ago physiological laboratories were almost unknown in this country: the work before us promises a splendid proof of what may be done in them, and of the value of the results to be obtained by a term or two of patient work. This book is only the first part of a systematic introduction to the whole subject of Embryology, and since the chick is not only "of all embryos the best to begin with," and also the most easy to obtain, and in many respects to manipulate, the Authors have taken it, from the new-laid egg to the chicken, as their starting-point. And they express hope, at the commencement, that the student will not content himself with merely reading the book, but that he will dissect for himself, in which he will be aided by many practical instructions, and by some beautiful and original drawings. The work is divided into nine chapters, which treat of the structure of a hen's egg, of incubation, of the changes which take place during respectively the first, second, third, fourth, fifth, sixth, and so on to the end of incubation; a final chapter on the development of the skull; and an Appendix, containing practical instructions for studying these various changes. Of the woodcuts we may specially mention Fig. 1, which shows a section of an unincubated fowl's egg; Fig. 13, showing the circulation of the yolk-sac at the end of the third day of incubation; and Fig. 71, showing the embryonic skull of a fowl during the second week of incubation. The formation of each particular organ—eye, ear, heart, &c.—is traced and illustrated in a most masterly manner. The book will be a great addition to our study of physiology. Although not suitable for school use, we have no doubt that the work will soon be adopted wherever examinations in physiology are held, and by those colleges which require physiology as one of the subjects of their Science scholarship examinations.

An Elementary Exposition of the Doctrine of Energy. By D. D. Heath, M.A., formerly Fellow of Trinity College, Cambridge. London: Longmans, Green, and Co. 1874.

This is a capital and comprehensive little book. It embodies a series of lectures given in 1872 to the sixth form of the Surrey County School. Mr. Balfour Stewart has an "Energy Class" at Owens College, and in process of time we shall have this great and dominant doctrine taught in all our larger schools. The sooner that time arrives the better, for we have already a good text-book in the work before us. It might, however, with advantage be somewhat subdivided, and the addition of a table of contents and of an index would enhance its value for purposes of reference. The Author gives at the outset a series of typical changes:— 1. A head of water confined by a sluice gate. 2. The sluice being opened the water falls, gaining velocity as it loses height. 3. The falling water encounters a water-wheel, and gives up to it some of its motion, the wheel in its turn giving motion to a system of machinery. 4. Heat is produced both by the clash of the falling water and by the friction of the bearing parts of the machinery, or millstones. 5. The millstones, working in a vessel of water, raise it to the boiling-point. 6. But no higher, for the water then passes into steam. 7. The steam so generated may work a steam-engine. 8. Which may work an electrical machine. 9. By which we may produce, through the intervention of the spark, both light and heat. 10. Or may decompose water. 11. While, finally, the liberated oxygen and hydrogen may be re-composed, and the steam produced may work a pump and replenish the head of water in the original reservoir. "Our proposition then is," says the Author, "that we may represent numerically—(1) the power to produce changes which the water in the reservoir has in virtue of its quantity, and its height above the mill-wheel; (2) how much of the power due to motion the water gains as it loses height in falling; (3) how much in motion, and how much in heat it gives to itself and to the mill-wheel when it clashes on it; (4) how much heat goes to turn water into steam; (5) what the steam loses in working the steam-engine; (6) how much the steam-engine is retarded by working the electric machine, and producing heat, and decomposing the water." Then comes a section on fundamental units; mass and force, velocity, &c.; then the second and third laws of motion; the measurement of heat; and the measurement of work done. The second section treats of dynamical energy; the third of thermal and other energies,—electrical, chemical, and what he calls "animal energy," and the energy of vegetation; while a short section at the end treats of molecular theories. The work is very sound and clearly constructed, and we recommend it to the student as a very useful book on a very important subject.

The Elements of the Psychology of Cognition. By ROBERT JARDINE, B.D., D.Sc., Principal of the General Assembly's College, Calcutta, and Fellow of the University of Calcutta. London: Macmillan and Co. 1874.

THIS work is designed for the use of students who are beginning the study of philosophy, and its object is to serve as an introduction to the psychology of the intellectual part of the human mind. After an introductory chapter, we have chapters on the acquisition of presentative knowledge, on the theories of perception, on representation, and a final chapter on the elaboration of knowledge. In the latter some useful matter concerning induction and deduction may be found:—"Induction proper has for its basis an observed relation of phenomena, or the elements of phenomena, in a greater or less number of cases; and the inference asserts what will take place in all similar cases. Inductive inference affirms, regarding all instances of a particular kind, what is observed to be true of a certain number of instances of that kind." Then as to deduction, "here the mind passes in reasoning from the general or universal to the particular." Thus the chemist, having discovered the general laws of chemical combination, can predict the result in particular cases; and the naturalist, having ascertained the universal characteristics of some species, may with certainty expect to find these characteristics in any new specimen of this species. A short summary is given at the end. The matter is clearly stated, and the book will be found useful to all those students who intend to take up that vast and not too profitable study of philosophy.

Transits of Venus. A Popular Account of Past and Coming Transits, from the first observed by Horrocks, A.D. 1639, to the Transit of A.D. 2012. By RICHARD A. PROCTOR, B.A. With 20 Plates and 37 Woodcuts. London: Longmans, Green, and Co. 1874.

WE have before us another of those carefully written and beautifully illustrated books which Mr. Proctor, from time to time, provides for the purpose of popularising more or less abstruse science. This work can hardly hope to appeal to so large a section of the public as his "Sun," "Moon," &c., for the subject is one that rather concerns the astronomer than the general public; the objects at issue do not bear so directly upon everyday life; and if a man has never seen Venus, and has no chance of ever seeing a transit, and does not easily comprehend how his race can be benefitted by a knowledge of an exacter determination of the sun's distance (which need not have a greater error than 100,000 miles), then we say he is not likely to trouble himself about the subject at all. But if any book is likely to create an interest on the subject, and to make it popular, that book is surely Mr. Proctor's.

Economic Geology, or Geology in its Relations to the Arts and Manufactures. By DAVID PAGE, LL.D., F.G.S., &c. Edinburgh and London: W. Blackwood and Sons.

As the Author remarks in his Preface, "There are works on agricultural geology, on building and decorative, on mortars and cements, on coal-mining, on veins and lodes, and on ores and metallurgy, but there is no general treatise on geology in its numerous relations to the arts and manufactures." This deficiency he has therefore endeavoured to supply, and has produced a work which we feel convinced will be widely appreciated. The purpose of this treatise being purely practical, those interesting theoretical questions in which Geology is so rich are here passed over. For a history of the life of the primæval world, for the causes of the striking phenomena it presents, for palæontological research, the reader must look elsewhere. Dr. Page, we must add, has, in his former publications, proved his ability to deal with such subjects. After an Introduction, showing the importance of Economic or applied Geology to the farmer, the land agent, the builder, the engineer, the miner, as well as to most manufacturers, the Author gives a brief account of the "rocky crust" of the earth, of stratified and unstratified rocks, their relative positions, texture, chemical composition, and chronological arrangement. He then shows, in succession, the bearing of geology upon agriculture, upon the valuation of land, upon architecture, engineering (including road and railway making, the construction of canals and docks, and the supply of water), upon mining, the production of heat and light, the fictile arts. He then treats of grinding, whetting, and polishing materials, of fire-resisting substances, of pigments, dyes, and detergents, of salts and saline earths, of mineral and thermal springs, of mineral medicines, of gems and precious stones, of metals and metallic ores. A summary at the end shows the valuable products likely to be found in each geological formation.

We need scarcely say that to deal exhaustively with all the subjects here enumerated would require not a volume, but an encyclopædia, and that of the bulkiest. Still whatever could be done within the compass of some three hundred closely-printed pages has been done. It would be difficult, indeed, to compress a larger amount of sound useful matter within the same limits.

We may, however, venture to point out that in the chapter on dyes tin is omitted amongst the bodies mentioned as mordants. Nor do we think it strictly accurate to pronounce the coal-tar colours of inorganic origin.

The work is illustrated with engravings, and accompanied with a geological map of the British Islands, placed, for better preservation, in a pocket within the cover.

We cannot more fitly conclude this brief notice than by quoting the following excellent advice :—"Every surface efflorescence, however insignificant, every trickle of styptic water (and every issue of water should be tested by the field geologist), every mealy disintegration of a rock, and even the presence of such plants as affect saline soils, should all be duly noted as indications of the mineral treasures below."

CORRESPONDENCE.

RESPECTING THE PHENOMENA CALLED SPIRITUAL.

By Charles Francis Keary.

Sir,—You have the satisfaction of thinking that you, more than any one else, have been instrumental in directing public attention to the "phenomena called Spiritualism," and that a strong and growing interest with many thoughtful people—and with not a few of considerable scientific attainment—dates from the publication of your first "enquiry" in 1870. This arose, no doubt, in great measure from the fact that you were the first scientific man who had devoted much labour to these investigations, and had made the result of them known; but partly also, I conceive, from a general impression in the public mind that you had not received very fair or decorous treatment either from the Royal Society or from the writer of a well-known article in the "Quarterly Review."

Speaking of the influence which the growing practice of publicly reading the Bible had in promoting the spread of Protestantism, Hallam makes the acute observation that this influence was largely due to the opposition which the practice met with from the Catholic Church, and to the fact that people were thus led to interpret the Scriptures "with that sort of prejudice which a jury feels in considering evidence that one party in a case has attempted to suppress; a danger," he goes on to remark, "which those who wish to restrain the course of free discussion, without any sure means of success, will in all ages do well to reflect upon." This consideration should alone be sufficient to determine us to give a fair hearing to the new science, or philosophy, or religion, or whatever it may be, which is comprised under the name of Spiritualism.

It may well be to this very prejudice of which Hallam speaks that we owe, to some extent, the activity of the public mind on this subject, and the decided advance which in the last few years the facts of Spiritualism have made towards—however far they may yet be from—a recognition. We must not therefore allow ourselves to be much influenced by a specious argument drawn from this fact, as well as from the greater candour of our own days, which is often employed in favour of other beliefs or disbeliefs beside the one in question. Because some things of a nature not altogether dissimilar from Spiritualism, which were once scoffed at, are now admitted by scientific men,—therefore, it is said, Science is sure, sooner or later, to give a place to this new theory. The same argument is often employed in religious questions, where it is contended that, because the upholders of religion have made many admissions and yielded many points once only maintained by sceptics, we are all inevitably drifting towards a complete scepticism at last. The argument is too foolish a one to be employed save in the heat of theological controversy, and I have wondered to find it so often even there. It only requires to be pushed one step farther to become a *reductio ad absurdum*. Because the representatives of Science or Religion, at any particular epoch, have almost as often as not been opposed to new discoveries, therefore there is no truth in either Science or Religion. Because, for instance, Harvey's discovery was scouted by the Medical Science of his day, therefore the pretender of ours who has found out that the earth is flat, or that the moon does not turn upon her axis, is as likely to be right as the Astronomer Royal. The fact, then, that Sir George Lewis, writing in 1849, spoke of Mesmerism or Electro-Biology as a theory which had been quite long enough before the world to have established its truth if there had been any in it, while the very facts of Electro-Biology are by many looked to for a probable explanation of Spiritualism, should not have more influence with us than to prevent our being turned from a candid and thoughtful examination of the subject by the passing effect of mere prejudice or fashion.

It is necessary to make this remark, because the more enthusiastic spiritualists seem now to be sustained by a premature confidence that such prejudice and fashion are the only obstacles which they have to overcome. A cause which is numerically weak has some advantage when its opponents think that the weakest arguments against it will suffice. Mere laziness, rather than want of capacity, must be blamed for the silly things which are constantly being said to disprove the truth of Spiritualism; but its more thoughtful supporters will do well to consider carefully if any valid arguments can be produced against them. It is with the object of inviting such consideration, Sir, that I have written this letter, which must be looked upon as an attempt—however unsuccessful—to put the question in the light in which it is likely to be regarded when the effect of prejudice has worn away. In the first place, then, I think it must be at once conceded that the present popular explanation of Spiritualism cannot be sustained. This popular explanation we know is this:—All the phenomena are nothing but a happy mixture of cheating and delusion. All those—scientific men and others—who have tried experiments upon these facts are deceived, just in the same way in which the writer or "the general reader" would be deceived by Messrs. Maskelyne and Cooke, only with this difference—that we should know we had been tricked, and they do not. This general opinion, I think, must change. To suppose that one man after another should enter upon the examination of these phenomena with a firm determination to expose the deception, and a perfect confidence of being able to do so, yet should end at last by being the victim of common trickery; to find, too, that many of these enquirers were well trained in the habits of scientific experiment,—this gives too rude a shock to our belief either in the value of human testimony or in the power of human skill in detecting falsehood and discovering truth. For on the hypothesis of mere trickery, of these three conclusions one:—Either all those enquirers who are ready to vouch for the truth of some of the facts of Spiritualism were—excuse the discourtesy for the sake of the argument—deliberate liars; or else they were less capable of investigating these facts than we—the general public—are; or, finally, if we had investigated them we should have been likewise deceived. Now I think, without any excessive modesty, we may put aside the second hypothesis. The dilemma in which we stand, therefore, is this:—Either concurrent human testimony of the highest seeming character is valueless, for all those who give it may be deliberate liars; or else we only preserve ourselves from error by not investigating,—in other words, the use of enquiry and reason is not a method of discovering truth. It is difficult to say which of these two conclusions is the most untenable.

Must we, then, admit the facts, and endeavour to discover some natural law which shall account for them? If these facts had not been of a nature different from those which rewarded your earlier enquiries, it would seem as if they might be admitted on an hypothesis which would give no violent shock to our present World-Theory. I need hardly say I allude to the hypothesis which you yourself started under the name of Psychic Force. Science is disposed to admit the phenomena to which has been given the premature and rather meanless name of Electro-Biology. This series of phenomena includes the action of one mind or brain upon another, and through that other brain upon the muscles of the body to which it belongs. The Psychic Force theory extends this notion to the affecting of outward material objects by the same brain power. In the ordinary experience of life we know of but one way of affecting another brain,—by the transmission of ideas through the recognised channel of speech,—just as we know of but one way in which the brain can affect inanimate matter,—that is, by the movement of the muscles of the body. To suppose a new exercise of this brain power in the second case is scarcely more difficult than to grant it in the first. Yet this is all the Psychic Force theory demands. As in Electro-

Biology, it says, we see a new brain-power acting not through the ordinary means of speech, but directly from brain to brain, so in this case we see such a new power acting not only upon the muscles or nerve-vessels of the body, but upon external matter beyond the precise region of the body.

Thus can the theory be stated so as to seem not to make very large demands upon our tolerance; and though it must be admitted that the otiose assent which is at present given to Electro-Biology arises chiefly from our ignorance of—or the absence of—exact mental laws, and our comparative indifference to such as we do know, nevertheless some force which may be compared to Electricity proceeding from the brain, under as yet unascertained circumstances, is certainly conceivable. But it is obvious that the readiness with which we assent to such a theorem must depend upon—must, in fact, vary inversely with—the degree in which these new phenomena modify our former experience. When it is only a question of moving a plank or a human body a little way in the air, we might accept it; but if asked to believe that this Psychic Force had the power of attracting a fresh comet (for this is not a radiating force, but can, at will apparently, be made to act in any particular direction and on any particular object), we should certainly refuse. This may be illogical of us, seeing that, if there is a new force discovered or come into being *for us*, it is as likely to be a great one as a little one; but we should have this comfort—that it could not be long before we should be obliged to believe in this new force, were it so powerful as it was said to be. Now, your later discoveries seem to point out this Psychic Force as the most remarkable in Nature. It apparently has the power of changing the very character of matter. Thus you record the fact of a bell having been carried through a locked door without any subsequent change in the nature of either of these material substances. The Psychic Force, then, must be accredited with the power of suddenly changing entirely the arrangement of the atoms composing bell or door, or both, and then as suddenly making them return to the *status quo*. It must be confessed that nothing can be conceived more contrary to all our past experience of matter and of force than this. All other experience teaches us that things are kept in their places by a balance of opposing forces. During the sudden rarefaction of the atoms of the bell or door, it cannot but have been that some of them came under the influence of quite new attractions, and passed into an entirely new state. How, then, can we conceive a force which should exert on each atom such a nicely-adjusted action as to bring it back exactly *in statum quo*, and no more? Is it not clear that such a force differs, *toto cœlo*, from all others in Nature, with their "equal and contrary reactions," as far as we have yet gone?

Other instances might be cited if necessary; but this alone is, I conceive, sufficient to show that these facts cannot be included under the head of "natural" phenomena. For it will be most germane to our present purpose, as well as most agreeable with the facts of Nature, if we draw a distinction between "natural" and "spiritual" at that point which divides *us* from external nature, or mind from matter. For I hold it to be a mere misnomer to speak of the will as a "natural" force in the sense of coming under the analysis of Science. Science, indeed, rather professes to disbelieve in the existence of this force than to correlate it with the forces of Nature; but until any one will prophecy what a man is *going* to think or do, and not only safely after the event, I shall continue to believe in it. Yet it certainly lies entirely outside the view of Science. It has no discovered correlation with "natural" forces. Science has simply nothing to say to it whatever. A body falling from rest to the earth will reach it in a time dependent only on the space through which it falls,—but not if I catch it and hold it in my hand during a time dependent only on my own will. We are so used to make these tacit exceptions that we scarcely realise that the forces of Nature do not act here with the same regularity as if there were no *wilful* beings upon our planet.

Keeping ourselves, however, within the limit of a purely scientific method, we observe that though this force Will has the power of directing certain internal forces of the body, yet it never acts upon matter but by the use of natural force. The expenditure of a certain amount of heat is the equivalent of the muscular action by which I sustain the falling body; nor have we any experience of this force of will acting upon matter directly, without the agency of known and measurable forces.

Our spiritual theory, therefore,—though, as we have used the word, it makes no assumption of the agency of disembodied spirits,—will not aid us much here, because we have already agreed that it would require an almost inconceivable amount of evidence to make us believe in the existence of a force which could make a bell pass through a door without permanently altering the condition of either. If, therefore, we are to accept the spiritual explanation of such phenomena as these, it must be as instances of this mental force acting directly on matter in a way of which we have at present no experience whatever. So that should by any chance this "spiritual" theory be the true one—and I do not say positively it is not—it would seem that Science is very unlikely to gain much by these investigations. For she has never yet, I venture to think, gained anything by the study of Psychology: I mean that, for all practical purposes, the laws of Mind are as distinct from the laws of Matter as ever they were.

Before, however, Science abandons the field of these phenomena as quite beyond her sphere, she is sure to rest for a long time upon a different explanation of these facts, which, though it does not bring them within the domain of Science, takes away their importance by robbing them of their external reality.

The series of phenomena called Electro-Biology seem to be chiefly connected together by the effect which can be produced upon the brain of the patient or patients by some unknown force generally believed to be at the command of some person. This effect includes the production of all sorts of curious delusions, and though these delusions have in no respect—as far as has been ascertained—been confounded, after the event, with the experience of common life, yet, as we know so very little of our own mental constitution, there is no reason to feel sure that no force is capable of producing delusions which would be so confounded. The real difference between our ordinary thoughts and those of dreams, trances, mesmerism, &c., is, it is to be noticed, the break in their continuity. In dreams these breaks occur not only at the moment of waking, but constantly throughout the whole succession of ideas. Thus, in dreams we never walk upstairs, or from one place to another,—I think I am speaking advisedly when I say *never*,—but we suddenly *find ourselves* arrived at the fresh place; and the same breaks may be noticed all through the dream. On the other hand, illusions produced by illness occur to perfectly sane people, without break of continuity; though in these cases they are generally but of short duration. The Electro-Biological explanation of Spiritualism would suppose these delusions to be as long as those produced in mesmerism, but also to occur without break in the continuity of ideas. The fact of permanent witnesses remaining to testify to the occurrence will not in this case serve, because a perfectly natural occurrence may happen, and the extraordinary explanation of it may be the result of delusion. Thus it would, I believe, be quite possible to mesmerise a person, then to jump upon a chair and write upon the ceiling, making the person believe all the time that you were floating in the air.* Nor can we in this case apply the tests which would have served if we adopted either the Psychic Force Theory or the Spiritual Theory,—in the larger sense in which I have used this latter word,—the test, namely, of knowledge being communicated to one person in the room, such as could have been known to him alone; because all this, the whole idea of this knowledge or of any communication, may have been part of the delusion. Take, for instance, the case in which you received communications by means of the Morse alphabet. The whole idea, from beginning to end,

* The same explanation would, I think, apply—*mutatis mutandis*—to the spirit photographs. I only suppose cheating on the part of some person, to make the argument *clearer*—it does not make it any *stronger*.

may, according to this theory, be a delusion; all that originated with the Medium (or blind Biological Force, possibly) being the idea put into your mind of having received a special message, the rest being left to a certain unconscious constructive power which the mind has, as we know from dreams,* &c.

I can imagine that such an explanation may be to you and your fellow-investigators a distasteful one, though not, so far as I can see, altogether reasonably so; nor am I ignorant that some facts are conceivable which would make this theory untenable. But I do not think at present that these facts, if they exist, have been produced; and until they are produced I feel sure that this is the explanation on which Science will ultimately rest,—being, indeed, the most reasonable from a purely scientific stand-point, though not on that account necessarily the *true* one. For Science, I hold, is not the same thing as Philosophy, and does not embrace the whole World-Theory of any man. The consideration of the subject from a philosophical stand-point would be somewhat different, and is not at present within my scope. The difference may, however, be hinted in the following way:—If the Electro-Biological Theory is true, it would seem very probable that these *delusions* are the result of certain undiscovered physical conditions of the mind, rather than the conscious action of one person's will upon others. Now, suppose these conditions liable to affect all, so that—without any break in the continuity of our ideas—we were all liable, under certain circumstances, to see bells pass through doors. What would be the difference in this case from the fact that bells could, under certain circumstances, pass through doors? An attentive consideration of this question will show us that if these "certain circumstances" were the same for all, there would be no difference. But that they are the same for all those who simultaneously are affected by the delusion our theory presupposes. If therefore the delusion became universal, it would cease to be a delusion and become a *fact*.—I am, &c.,

CHARLES FRANCIS KEARY.

British Museum.

* As, for instance, when we hear a noise in sleep we almost always make it fit in with our dreams.

PROGRESS IN SCIENCE.

MINING.

SOME researches on the geology of the mining districts of Cornwall, with special reference to the formation of mineral veins, have recently been submitted to the Geological Society by Mr. John A. Phillips, whose extended study of the microscopic structure of Cornish rocks and veinstones, coupled with his chemical examination of the rocks, must needs give peculiar weight to the conclusions which he has reached. He believes that the vein-fissures of the tin- and copper-bearing lodes of Cornwall were the result of forces acting subsequently to the solidification of the elvans, but in the same general direction as those which gave rise to the eruption of this rock. The fissures produced by these forces afterwards became filled with minerals, which were deposited by chemical action from waters circulating through them: these waters were probably, in some cases, at a high temperature, through contact with highly-heated rocks at great depths, but in other cases the temperature of the circulating waters appears to have been but moderate, and the action consequently but slow. It seems impossible to determine to what extent these deposits were produced by waters rising from below, or how far they were influenced by lateral percolation, but the latter action has probably been important. Contact-deposits and stock-works have been formed by analogous chemical action, set up either in fissures resulting from the junction of dissimilar rocks, or in fractures produced during the upheaval of partially-consolidated eruptive masses. England, unlike Germany, has so few experienced miners who are at once good chemists, mineralogists, and geologists, that such a paper from a man like Mr. Phillips is certainly a rarity, and deserves accordingly to be thoughtfully studied by the scientific metal-miner.

A description of the deposits of phosphorite in North Wales has recently been laid before the Geological Society, by Mr. D. C. Davies, of Oswestry. The phosphorite bed varies from 10 to 15 inches in thickness, and occurs at the top of the Bala limestone over a considerable area, having been found in various localities from Llanfyllin to the hills north and west of Dinas Mawddy. The bed appears to consist of concretions of various sizes, embedded in a black matrix. It is notable that the nodules are coated externally with a thin black film of some graphite-like mineral. The concretions contain about 64 per cent of tribasic phosphate of lime, whilst the average proportion in the entire bed—including nodules and matrix—is about 46 per cent, This deposit has been worked at certain points, and promises to become an important object of exploration.

It is reported that a very large deposit of phosphate of lime has been discovered in the island of Amba, in the Caribbean Sea.

Some interesting relics of ancient mining have been recently brought to notice by Prof. Boyd Dawkins, who has discovered—in the old workings near Alderley Edge, in Cheshire—a large number of rude stone hammers, similar to those which have been found in association with old copper-mines at Lake Superior, in Spain, Portugal, and elsewhere. It is difficult to fix the age in which these rude mauls and picks were used in Cheshire, but it is probable that they go back beyond the range of our history, and may therefore be fairly called "pre-historic." They were found on Lord Stanley's property, where the copper-bearing sandstones of the Trias are now worked by the wet-way, It appears, however, that some old slags discovered in the neighbourhood were not found to contain any copper, and hence it is probable that other metals may also have been worked here at a comparatively early date.

As mining geologists must take a good deal of interest in the deep boring at Netherfield, generally known as the Sub-Wealden Exploration, we may mention that the Committee—after having spent many months in fruitless attempts to recover the boring-tools, which had unfortunately fallen to the bottom of the deep hole—came to the bold conclusion that the old bore should be abandoned, and that a new one should be commenced by the Diamond Rock-Boring Company. The original boring had reached a depth of upwards of 1000 feet, and was advancing well in the Oxford Clay, but it was found on survey to be slightly out of the perpendicular, and the recovery of the lost tools seemed altogether hopeless. Under these unfortunate circumstances the Diamond Company generously undertook to put down a 1000-foot bore, with lining, at the cost of only £600. After a good deal of discussion as to the selection of the new site, it was agreed that the second boring should be close to the first, and accordingly the Netherfield bore-hole No. 2 was commenced on February 11th. A 6-inch crown is employed, and cores of almost this size are now being extracted.

Since the Sub-Wealden boring has attained to a considerable depth, attention has been directed to other deep borings, and to the difficulties experienced in such undertakings. Perhaps the most interesting of these deep bore-holes is that which was sunk a few years ago at Sperenberg, about 25 miles south of Berlin. The rock at the surface was gypsum, but this passed down into an enormously thick deposit of rock-salt. The boring was commenced on the 25th of April, 1867, and on reaching a depth of 984½ feet (956 Prussian feet), in July, 1868, it was stopped, in order that steam-power might be substituted for hand-labour. After the arrangements for introducing steam were completed, the boring was resumed, and continued uninterruptedly from January, 1869, to September, 1871, when the hole had reached the extraordinary depth of 4172½ feet (4051·6 Prussian feet).

The "Colliery Guardian," in its issue for the 1st of January, published some interesting statistical tables, giving a complete record of the lives lost by colliery accidents during the thirteen years which ended on December 31st, 1873. Without analysing these figures, we may state that the grand totals show that the aggregate number of deaths during this period amounted to 13,756. Of this number, 2790, or only little more than one-fifth, were due to explosions. The improved condition of our mines is seen by the fact that in 1861 one life was sacrificed for every 91,240 tons of coal raised, whereas in 1873 one life was lost for every 133,677 tons of fuel.

It would be wrong to conclude this Chronicle of Mining without mentioning that the Murchison Medal has this year been awarded, by the Geological Society, to Mr. W. Jory Henwood, of Penzance, as a well-merited recognition of his labours in mining-geology. Mr. Henwood's valuable observations on mines in almost every part of the globe will be found in the fifth and eighth volumes of the "Transactions of the Geological Society of Cornwall," two volumes of which are entirely devoted to his papers, and are an enduring proof of the success with which he has brought a sound scientific knowledge to bear upon the miner's art.

METALLURGY.

Remarkably little progress appears to have been made during the past quarter, either in the development of any branch of our metal industries or in the publication of papers bearing upon the scientific aspect of metallurgy. Our report is consequently somewhat meagre.

With respect to the metallurgy of iron, we may mention that certain improvements in puddling have been patented by Sir J. G. Alleyne, of the Butterly Iron Works. He provides his furnaces with four regenerative chambers; two for gas, and two for air. The rabble used consists of iron bars or tubes, bent to a loop or crank form, and made to revolve on a horizontal axis, so that the loop or crank can sweep through the metal in the basin of the furnace.

An improved rotary puddling furnace has been devised by M. Ehrenwerth. This furnace has a revolving sole, moved by a system of geared wheels. As the charge of pig-iron melts, the sole is put in motion, and the metal is worked by means of rabbles furnished with peels set obliquely. The rabbles rest on cones, and may be moved by either manual labour or steam-power.

Some improvements in the manufacture of iron and steel have been introduced by Mr. Willans. During the re-heating of the iron he injects into the furnace oil, creosote, carbonic oxide, or other substances containing carbon or hydrogen, in order to prevent the oxidation of the metal. Steel tubes may be made by employing soft steel, which is easy to draw, and then annealing this in the presence of deoxidising agents.

A process for tempering steel, invented by MM. Garnaut and Seigfield, has been acquired, it is reported, by the United States Government. It consists in heating the steel to cherry-redness, and then sprinkling it with common salt. At a later stage of the working a mixture is employed, containing chloride of sodium, sulphate of copper, sal-ammoniac, carbonate of soda, and saltpetre. Finally, the steel—having been brought to a cherry-red heat—is plunged into a bath containing alum, carbonate of soda, sulphate of copper, saltpetre, and chloride of sodium.

Mr. J. M. Oubridge, of Middlesbro', has recently read, before the Cleveland Institution of Engineers, a paper "On the Construction of Foundries." He sketches the way in which he believes a foundry should be constructed, and enters into technical details suggested by his own experience. The paper has been reproduced in the columns of "Iron."

For many years past antimony ore has been worked and reduced in Borneo and of late years mercury has also been extracted. A description of the methods followed in mining and smelting these minerals has been laid before the Tyne Chemical Society, by Mr. T. Down, jun. The antimony mines are at Jambusan, about 30 miles from Sarawak. The ore—which is principally the sulphide, although native antimony and the oxide also occur—is found in veins running through limestone, and in derived boulders. A charge of about 30 cwts. of ore is introduced into a specially-constructed reverberatory furnace, where it is liquated. The crude metal is run into cast-iron moulds, while the oxide which has been volatilised is collected in long flues. The cinnabar occurs disseminated through a rock, said to be a basalt, at Tegora, about 10 miles from Jambusan.

MINERALOGY,

In compliment to Hofrath Kopp, of Heidelberg, the name of *Koppite* has recently been given—by Prof. Knop, of Carlsruhe—to a mineral from the Kaisersthuhl, previously mistaken for pyrochlore. It is found to be a niobate of various metals, including calcium, cerium, lanthanium, didymium, potassium, sodium, &c.: a part of the oxygen appears to be replaced by fluorine. Koppite occurs, with apatite and magnoferrite, in the granular limestone of the Kaiserstuhl.

Some interesting researches on the composition of *Autunite*, with special reference to the condition in which the water exists, have been undertaken by Prof. A. H. Church, and the results published in the "Journal of the Chemical Society." Specimens recently raised in the neighbourhood of Redruth were subjected to examination, and compared with others from near Autun, in France. The powdered mineral submitted to dry air, confined over oil of vitriol, lost between 8 and 9 per cent of water, at the same time becoming fragile and losing its transparency. These changes seem to indicate that the water thus removed is not moisture accidentally present in the interstices of the substance, but water which is absolutely essential to the constitution of the mineral. Over oil of vitriol *in vacuo* a further loss of water occurs, reaching about 15 per cent. Prof. Church's analyses lead to the following formula for the unaltered crystals:—$(U_2O_3.CaO)P_2O_5.10H_2O$; and show that Autunite dried *in vacuo* has only two molecules of water instead of ten. *Torbnerite*, a phosphate of copper and uranium, closely allied to Autunite, did not exhibit a similar behaviour when dried.

Prof. Weisbach, of Freiberg, has described a mineral brought by Herr Simon from Mancayan, in the island of Luzon, to which the name of *Luzonite* has been given. It occurs, associated with enargite, in veins of copper ore. Dr. C. Winkler's analysis shows that it is an arsenio-sulphide of copper, having essentially the same composition as enargite, which is consequently dimorphous.

Rauite is the name which has been bestowed upon a new zeolitic mineral from Brevig, in Norway, described by Herr Paykull. It appears to be closely related to Thomsonite, and to result from the alteration of Elæolite.

Some interesting pseudomorphs from the Tilley Fort Iron Mines, in Putnam Co., New York, have been described in the "American Journal of Science" by Prof. Dana. After sketching the geological structure of the district, he describes the several kinds of pseudomorphs; some consisting of serpentine, or of serpentine and dolomite, whilst others are composed of brucite, magnetite, pyrrhotite, &c.

Dr. J. Lawrence Smith has called attention to a curious association of essonite, or cinnamon-stone,—a variety of garnet,—and green idocrase, with datholite, in limestone, at Santa Clara, California. This is the first instance in which datholite has been observed in association with garnet and idocrase. Analyses are published of the several minerals here described.

By the same chemist we have a note on *Warwickite*, recently presented to the French Academy. This rare mineral is a boro-titanate of magnesia and iron, originally described by Shepard.

It may be interesting to the mineralogist to know that M. Radominski has succeeded in artificially producing the two rare minerals *monazite* and *xenotime*. The former is a phosphate of cerium, lanthanium, and didymium, whilst the latter is a phosphate of yttrium with the monazite metals.

Some notes by Mr. W. Skey, of New Zealand, contributed to the "Chemical News," are of much interest to the mineralogist, from their bearing upon the origin of *Torbanite*, or the celebrated Torbane Hill mineral. By allowing petroleum to filter through clay, he obtained a substance strikingly resembling the natural mineral, and he concludes that Torbanite is not a coal, but a chemical combination of an acid hydrocarbon with silicate of alumina.

Attention has recently been called by M. Daubrée to some highly interesting examples of the formation of metallic minerals within a comparatively recent period. M. Daubrée's observations on the minerals formed in the Roman works at the hot springs at Plombières are well known; but the present illustrations have been presented by the hot springs of Bourbonne-les-Bains, in the Department of the Haute Marne. It appears that during some recent excavations the bottom of the old Roman well was laid bare, exposing a bed of mud, in which a number of bronze, silver, and gold antiquities were discovered. This bed rested on a deposit of fragments of rock cemented into a brecciated mass by the crystallised metallic sulphides, Among these were found examples of copper-pyrites (sulphide of copper and iron), copper-glance or vitreous copper-ore (disulphide of copper), purple copper-ore or erubexite (sulphide of copper and iron), and fahlerz or tetrahedrite (sulphide of copper and antimony). It is true that most of these had been found in other localities, under somewhat similar circumstances, but the occurrence of the fahlerz in sharp tetrahedral crystals has not been previously recorded from any deposits of this character. Altogether the association is strikingly like that in some of our old copper-lodes, yet the deposit in question is certainly not older than sixteen hundred years. The bronze objects have been much attacked, but the silver has not been affected.

GEOLOGY.

Physical Geology.—The plant-bearing series of India has been shown by Mr. H. F. Blanford to range from early Permian to the latest Jurassic times, indicating that, with few and local exceptions, land and fresh-water conditions had prevailed uninterruptedly over its area during this long lapse of time, and perhaps even from an earlier period. In the early Permian there is evidence in the shape of boulder-beds and breccias underlying the lowest beds of the Talchir group of a prevalence of cold climate down to low latitudes in India, and as the observations of geologists in South Africa and Australia would seem to show in both hemispheres simultaneously. With the decrease of cold the author believed the Flora and Reptilian Fauna of Permian times were diffused to Africa, India, and perhaps Australia; or the Flora may have existed somewhat earlier in Australia, and have been diffused thence. The evidence he thought showed that during the Permian epoch India, South Africa, and Australia were connected by an Indo-oceanic continent, and that the first two remained so connected, with at the utmost some short intervals, up to the end of the Miocene period. During the latter part of the time this continent was also connected with Malayana. The position of the connecting land was said to be indicated by the range of coral reefs and banks that now exists between the Arabian Sea and West Africa. Up to the end of the Nummulitic epoch, except perhaps for short periods, no direct connection existed between India and Western Asia.

Mr. J. W. Judd has carried his investigations into the structure and age of Arthur's Seat, Edinburgh; he believes that in it we have the relics of a volcano, which was at first submarine, but gradually rose above the Carboniferous Sea, and was the product of a single and almost continuous series of eruptions.

Palæontology.—Messrs. Hopkinson and Lapworth have described forty-two species of Graptolites from the Arenig and Llandeilo rocks of North Wales. The Arenig rocks contain a number of species, which ally them to the Quebec group of Canada.

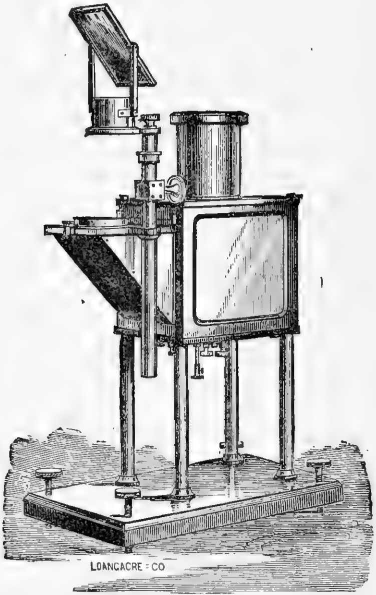
The publication of a series of decades, or numbers containing ten plates, illustrative of the palæontology of Victoria, has been commenced by Professor M'Coy. The first decade contains figures and descriptions of Graptolites, Marsupiata, Mollusca, Plants, and Star-fishes. Nearly all the species of Graptolites can be identified with species found in England or America. Extinct forms of Kangaroo and Wombat are described. Amongst the Mollusca, species of *Voluta* seem scarcely distinguishable from our Eocene forms. The plant-remains described include species of *Zamites* and *Lepidodendron;* the star-fishes belong to the family *Urasteridæ.*

Obituary.—By the death of Sir Charles Lyell, geological science has lost its greatest leader, one who, for a period of half-a-century, has been devoted to its advancement. He was born in 1797, and in 1819 was elected a Fellow of the Geological Society of London; he was President in 1835, and again in 1849. Sir Charles Lyell is deservedly designated the Historian of Geology; the amount of original research in the shape of detailed investigation which he performed has been exceeded by many of our distinguished geologists of the past and present; but the experience gained during travel, and the philosophical deductions he was enabled to draw in explaining the past history of the earth by the light of the present, in his "Principles of Geology," and in bringing together, in his "Elements" and "Manual," the known facts of geology, enabled him to influence the progress of the science more than any other man. The "Antiquity of Man, or early History of the Human Race," place all under a great debt of gratitude to the illustrious author. Although to the last he lost none of his interest in the progress of science, yet even in 1866, when he received the Wollaston Medal, he observed that every year he felt less able to keep pace with the ever-increasing rate at which geology is expanding, together with the numerous sciences which are so intimately connected with it.

Professor Marsh has recently returned from an expedition to the Rocky Mountains, where, south of the Black Hills, a deposit of Miocene age was found to be exceedingly rich in Mammalian remains. Nearly two tons of fossil bones were collected, including several species of gigantic *Brontotheridæ.* Professor Hayden has, however, lately expressed his opinion that the genus *Brontotherium* of Marsh is synonymous with the *Titanotherium* of Leidy, of which there are probably not more than two species.

PHYSICS.

LIGHT.—At the late Industrial Exhibition of the Franklin Institute in Philadelphia, as also at the like Exhibition of the American Institute in New York, silver medals were awarded for the vertical lantern shown in the accompanying figure, which is manufactured by the instrument makers to the Stevens Institute of Technology, Hoboken, N.J. The cut shows the apparatus as adjusted to exhibit on the screen such objects as waves in water, cohesion figures, and the like. The light from the lime cylinder or electric arc passes through two large lenses, by which it is thrown in a parallel beam on an inclined mirror in the triangular box in front. By this it is reflected upwards, and is condensed by a large lens standing horizontally on top of



the same triangular box, so as to pass into the objective above, and is then by the small upper mirror thrown on a screen. This upper mirror, with the objective, is carried by a bar provided with rackwork, by which an accurate adjustment for focus can be made. The objects rest directly on the plate holding the large horizontal condenser, When the apparatus is to be used for ordinary objects which can be held in a vertical position, a little screw in the front of the horizontal plate is taken out, and then the triangular box carrying the lower mirror is removed, allowing the horizontal plate to swing down into a vertical position, carrying with it the rackwork bar and objective into their proper positions.

MICROSCOPY.—Mr. T. Charters White, at a recent meeting of the Quekett Microscopical Club, gave the results of his experience in maintaining small Marine Aquaria, for the purpose of supplying objects for a microscopical study. Almost any vessel capable of holding a quart or more of water may be made use of, although the tank preferred by Mr. White is the well known form constructed of slate and glass, divided into two portions by a sloping partition, on which the rockwork is arranged, the lower part being devoted to the storage of sea-water away from the influence of light, and consequently in a quiescent state; this arrangement renders the maintenance of the water in a healthy condition much easier than when the whole contents of the tank are freely exposed. To establish a new aquarium requires considerable care, and, owing to the neglect of a few necessary precautions, failures are common. The tank, with its rockwork, should be filled with *fresh* water, and all the soluble lime salts in the cement used in attaching the pieces of stone soaked out; this may take a week or two, and must on no account be hurried. Afterwards the sea-water, natural or artificial, may be introduced,* and some ponds of *Ulva* or other suitable seaweeds placed in the tank, the object being to promote the growth of minute algæ on the surface of the rockwork. A seaweed already growing, although attached to stone, cannot be trusted to maintain the necessary supply of oxygen, as it but rarely establishes itself and forms vigorous plants. The rock should consist of sandstone or mica schist, both having surfaces favourable to the growth of the minute vegetation so much needed, and suitable for the attachment of zoophytes and other microscopic animals. When a good crop of weed has established itself, which will be easily known by the copious evolution of air bubbles from the coated surface of the stone, animal life may be *cautiously* added, a few sea-anemones and serpulæ; but fish, crustaceæ, or any of the more active creatures must be carefully excluded from an aquarium devoted to the culture of minute animal and vegetable organisms. The microscopic stock finds its way into the tank by means of germs, which are sure to come with weed and rock, brought fresh from the sea. The *Foraminiferæ*, *Hydroida*, *Polyzoa*, and other minute forms, are not long in making their appearance, and supply the microscopist with an unending source of material for investigation. Microscopic slides should be placed in various parts of the aquarium, as minute growths developing on them can readily be removed for examination. Artificial aëration, which is carried on with such great success at the Crystal Palace and elsewhere, is hardly needed in small aquaria containing only a few small and inactive animals, but it can be obtained to a sufficient extent, by allowing water from a suitable source of supply to fall into the aquarium through a glass tube drawn out to a point; this small stream will carry with it a large quantity of air bubbles, and greatly aid in maintaining the purity of the water. Sea-water must on no account be allowed to come in contact with any portion of metal.

Mr. Ingpen, Secretary of the Quekett Microscopical Club, communicated some notes on "Personal Equation" as affecting microscopical observations. The term is a well known one in practical astronomy, and the same causes disturb microscopical results, although, in the latter case, they are not so well recognised. Mr. Ingpen called attention to the following points:—

I. *Mental* equation, as causing differences in interpretation, particularly with regard to test objects.

II. *Nervous* equation, as shown by varied sensibility to tremors, &c.

III. *Colour.* Difficulty in estimating colour, as noted in Admiral Smyth's "Sidereal Chromatics." Right and left eye often differ in this respect. Effect of yellow crystalline, referred to by Professor Liebrich in his lecture on "Turner and Mulready." Difference in visibility of violet end of spectrum, amounting, in some cases, to a slight fluorescence. Effect of bluish mist, caused by slight opacity of cornea upon estimation of the correction of objectives. Colour blindness often existing in a slight degree unsuspected, and difficult of detection.

IV. *Focal Equation.* Differences in effect of long and short sight upon cover correction, &c.; also upon depth of focus, and power of resolving surface markings. Differences in size of images formed by right and left eye, and consequent effect upon binocular vision. Want of accommodation and pseudoscopic vision, &c.

* Where natural sea-water cannot readily be procured, the following artificial substitute from the formula of Mr. P. H. Gosse may be used:—

Chloride sodium (common salt)	3½ ounces.
Sulphate magnesia (Epsom salts)	¼ ounce.
Chloride magnesium	200 grains.
Chloride potassium	40 „
Water	1 gallon.

The iodides, bromides, and some lime salts found in minute proportion in sea-water are not included, but it is found that, when the aquarium has been established for a considerable time, these compounds may be detected.

V. *Form.* General tendency of the eye to show ultimate particles circular. Effect of square and triangular apertures. Effect of astigmatism upon form, particularly of lines and dots, as seen in different directions. Effects of diffraction upon points of light, &c. General considerations of the effects of unnoticed differences of vision, producing discrepancies often attributed to other causes.

ELECTRICITY.—The "Journal des Debats" notices a very ingenious application of electricity to voting in the National Assembly. "Before every Deputy two ivory buttons are placed, like the buttons of electric bells. If the Deputy wishes to vote 'Yes,' he presses the button on his right; if he wishes to vote 'No,' he presses the button on his left. The voter establishes by this means an electric communication, which is transmitted to an apparatus close to the President and his secretaries. Every time the electric current acts thus it opens the door to a ball, and the ball falls through a tube into the ballot box. The balls are made of glass or ivory, and are strictly identical in weight. The two ballot-boxes are then weighed, and the number of balls is indicated by the weight. Finally, by turning a handle, all the balls which have not been used are let out, and they give the number of members who have abstained, or were absent when the vote was taken. Nothing can be more simple. The inventor (M. Jacquin) has offered to set up his apparatus in the Versailles Assembly for the sum of 60,000 frs. Time is money." The "Debats" mentions also another plan invented by M. Martin, a well-known electrician. M. Martin's plan does away with the scales, which might not always be true. Accordingly, as the vote is black, a piece of coloured pasteboard appears instantaneously above a line bearing the name of the Deputy. Before each Deputy is a small box, supplied with two buttons. When he presses on one or the other, he discloses the piece of white or black card on the board. The sum total of the votes for either side is marked on a totalising board. The advantage of this system over that presented by M. Jacquin is, that it enables the President to see whether a Deputy has not voted because he abstained or because he was absent. A member can, by placing his hand on both buttons, vote at once "Yes" and "No," and be thus numbered among the abstainers.

TECHNOLOGY.—A recent number of the "Bulletin of the Industrial Society of Mulhouse" brings a very interesting report on "The New Dyes of Croissant and Bretonnière," from which we gather that a special committee has been investigating them in order to ascertain their value for the various purposes of dyeing and printing. At first the great durability of these colours engaged the attention of the members of the committee. Ink spots could, for instance, readily be removed by means of oxalic acid without the dyes themselves being changed in the least. The sunlight, as far as observations go, seems not to exert any influence on these colours; boiling soap-solution and oxalic acid are without effect, only chlorine or hypochlorites affect or rather destroy them.

In a paper on "Certain Properties of Weighted Black Silks" M. J. Persoz shows that weighting—which began with the modest aim of making up the loss sustained in ungumming—is now carried to the extent of 100, 200, and 300 per cent. This increase of weight is produced by treatment with salts of iron and astringents, salts of tin and cyanides. The bulk is augmented proportionably to the weight. As a matter of course, the chemical and physical properties of the silk thus treated are materially modified. What is sold as silk is, in fact, a mere agglomeration of heterogeneous matters, devoid of cohesion, held temporarily together by a small portion of silk. The elasticity and tenacity of the fibre are sensibly reduced. From being in its natural state one of the most permanent of organic bodies, and sparingly combustible, it burns like tinder if touched with flame. It is, moreover, liable to undergo spontaneous decomposition, and to absorb gases with the evolution of heat, which sometimes leads to actual combustion. The adulterated silk when burning scarcely gives off the characteristic odour of animal matter. It leaves an ash of oxide of iron exceeding 8 per cent.

ERRATA.—Page 145, line 18 from top, *for* "Iceland," *read* "Ireland." Page 148, line 4 from top, *for* "marked," *read* "masked." Page 155, line 20 from top, *for* "founded on a sound data," *read* "founded on sound data."

CONTENTS OF No. XLVI.

NOTICES OF SCIENTIFIC WORKS.

PROGRESS IN SCIENCE,

Including the Proceedings of Learned Societies at Home and Abroad, and Notices of Recent Scientific Literature).

THE QUARTERLY

JOURNAL OF SCIENCE,

AND ANNALS OF

MINING, METALLURGY, ENGINEERING, INDUSTRIAL ARTS, MANUFACTURES, AND TECHNOLOGY.

EDITED BY

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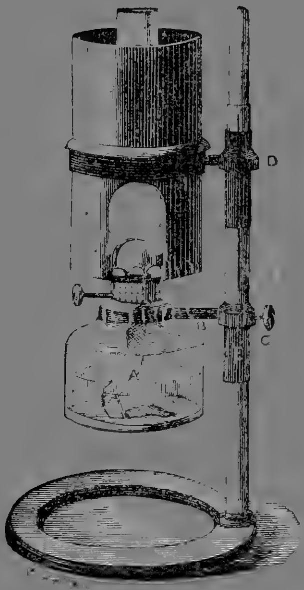

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I. VARIATION IN THE OBLIQUITY OF THE ECLIPTIC.

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THE problem of a variation, or possible variation, in the obliquity of the ecliptic, is one of the greatest interest and importance. As regards astronomy, it is a question which, although it may not materially affect observations made during short intervals, yet will produce most important changes after lengthened periods; and as regards our knowledge of astronomy, it is ten times as important to know whether the obliquity of the ecliptic can and has varied considerably, as it is to decide whether the sun is 90 or 95 million miles from the earth. When we examine this problem from a geological point of view, it is of even greater importance, for it would be the key to the solution of the climatic changes which are known to have existed on earth, and the records of which remain distinctly marked down to the present day.

This problem is one which has not for some years attracted much attention, in spite of its paramount importance; the reasons for this neglect or indifference are probably the following:

First, it has been generally supposed that the question of a variation in the obliquity was fairly and completely examined many years ago; that it was then proved as clearly as that the three angles of a plain triangle $=180°$, that the obliquity could vary only $1° 21'$; whatever, therefore, were the demands of geology for such an explanation as could be afforded by admitting a considerable variation in the obliquity, still it was supposed that the question had been long since disposed of, and nothing more remained to be done than to admit the impossibility of any variation greater than $1° 21'$.

The other reason why this problem has not been re-examined appears to be that there is a belief that it is one which can be judged of only by the most able mathematicians, and can be decided only by aid of the most complicated mathematical investigations; consequently that it was not competent for a mere philosopher or enquirer to offer any opinion on the subject.

With regard to the first of these two reasons, we trust we shall adduce sufficient evidence to show that the *real* question of a variation in the obliquity was not only not settled many years ago, but was not examined, except most partially and most superficially. With regard to the second reason, we believe it can be proved that the problem is one which must be solved long before it assumes the form of a mathematical problem; and, therefore, mathematics have little to do with the main point at issue.

Under these conditions, we purpose in this paper investigating the question of a possible variation in the obliquity, and calling attention to various facts which seem to have been overlooked, neglected, or undervalued, when the problem was examined many years ago.

In the first place, we will bring into notice the evidence in favour of a considerable variation having formerly occurred in the obliquity, and the general evidence in favour of a change being possible; secondly, the evidence or arguments that have been considered sufficient to prove that no greater change could occur than 1° 21′.

The first evidence in favour of a considerable variation in the obliquity is the evidence of geology. Whatever effects may be supposed to have been produced as regards changes in climate by the alternate elevation and depression of land, yet the facts of the glacial epoch demand some more powerful cause than can be attributed to this particular one; that the glacial epoch was universal in both the northern and southern hemisphere, and reached only to latitudes higher than 50°, except in mountainous ranges, are two most important facts, and conclusively demonstrate, we believe, that the elevation or re-distribution of land will not account for such universal results.

Let us for a moment compare the two explanations now before us, and note which appears the more satisfactorily to account for the conditions known to have prevailed during the past.

First, we have the alternations of land and sea, as an explanation of why an arctic climate prevailed down to about 50° latitude in both hemispheres, and produced also glaciers on the mountains in localities within the present tropics. We have this supposed cause to explain these known effects, and we cannot but consider the explanation feeble and inefficient.

Secondly, we have the explanation afforded by the variation of the obliquity to an extent of 12°, thus causing the arctic circle to reach to 54° latitude, and to actually bring this arctic circle down from its present boundary to that very parallel of latitude which the evidence of geology tends to prove was the boundary of glaciers in former times.

Of the two explanations as supposed causes, there is no doubt that the variation in the obliquity alone affords any satisfactory reason for the observed effects, and the re-arrangement of land and water can only produce minor changes, which do not touch the vast problem requiring solution.

The question as to a supposed variation to a great extent of the eccentricity of the earth's orbit, is one which has been most ably dealt with by Mr. Belt in the October Number of this Journal. The investigation given by Mr. Belt to the facts of the glacial epoch seem to show that the glaciation of the two hemispheres was contemporaneous, and that there are other effects which are not such as would receive an explanation, even granting that there had occurred formerly a much greater eccentricity in the earth's orbit than that which now occurs.

As an explanation of the known climatic changes which have occurred, we can best test the value of the theory of a considerable variation in the obliquity by endeavouring to explain the observed facts, even of the Boulder period, by this theory, and then by aid of either an increased eccentricity in the orbit or an alteration in the arrangement of land and sea. We believe that it will be admitted that the variation in the obliquity will alone account for the facts in a satisfactory manner, and that other climatic changes, shown by geology to have occurred, will alone be explained by granting a considerable variation in the obliquity. Thus the evidence of geology is so strongly in favour of a variation, both greater and less than at present in the obliquity, that we believe it may be claimed that no other theory has yet been advanced which will account for what is known.

If, as we believe will be the case, the future and present researches of geologists tend to prove that the northern and southern hemispheres were at the *same* time subjected to an arctic climate, it may be fairly claimed that no other theory will so completely and satisfactorily explain the phenomena as a great variation in the obliquity.

We believe also it will be granted that, if it had been long ago admitted that the obliquity of the ecliptic had been 35° instead of 23½°, geologists would at once have accepted this as a full and complete explanation of the great climatic change shown to have occurred on earth, and they would never have searched for those feeble or incomplete causes which are so inefficient when we endeavour to explain facts by their aid.

Thus it may be fairly claimed that geology, the science above all others of facts, is most completely in favour of a great variation in the obliquity, and almost demands it as a cause to explain the known effects.

We will next take the analogy of the planetary system, and we there find that there is no apparently arbitrary law as regards the inclination of the axis of a planet to the plane of its orbit. We find that the axis of Uranus lies almost in the plane of its orbit, so that the arctic circle extends from the poles almost to the Equator. In Venus, nearly the same condition exists, the arctic circle extending to within about 15° of the Equator. In Jupiter, however, the axis of diurnal rotation is nearly at right angles to the plane of the planet's orbit. The inclination of the axis of diurnal rotation of Saturn and Mars does not differ much from 66½°,—the earth's condition.

Analogy in Nature is an argument that must be used with great caution. It does not at all follow that because some planet has an axis of diurnal rotation, so inclined to the plane of its orbit that the arctic circle reaches nearly to the Equator, therefore our own planet was once in the same condition. Whilst, however, we use this caution, we must not forget that, if the analogy of the solar system had been allowed to weigh in the arguments long ago fought out, relative to the form and movement of the earth, its spherical or spheroidal form, and its rotation on its axis, might have been received without much opposition, when it became known that other planets were spheroidal in form, and that they as well as the sun rotated round an axis. The link wanting to bring that which seems probable into the region of proof, is to show that there is now actually going on some movement or some change in the earth's axis which will, may, or might formerly, have caused the angle of inclination of this axis to the plane of the earth's orbit to be a very different angle to that which it now is. This link, therefore, is *the* problem on which rests nearly the whole question connected with the probable or possible variation in the obliquity.

It is a fact known to observers that, during the past 2000 years at least, there has been a change in direction of the earth's axis of rotation, that the rate of this change is about 1° in 179 years; that it is now going on; and that, from the records of the past, this same movement and rate appear to have been nearly, if not quite, uniform.

It is also known from recorded observation that, whilst there has been this change in direction of the earth's axis, there is no evidence of any change of the position of the axis *when referred to the earth itself*. The poles of the earth appear from recorded latitudes to be situated now where they were situated 2000 years ago.

It is also known that the change in the direction of the earth's axis takes place in such a manner that it has produced, during the past 2000 years, little more than half a degree change in the obliquity of the ecliptic.

These recorded facts being before us, we may now advance to the next point in our enquiry, which is to investigate how this problem has hitherto been examined, and in what manner it is still assumed to be disposed of.

As soon as the revival by Copernicus and Galileo of the ancient theory of the earth's rotation on its axis became generally received, it was found necessary to attribute to the earth's axis a movement which should account for the decrease of 20″ per annum in the polar distance of stars, whose right ascension was near 24 hours. At that early date, and up to about 100 years ago, the fact of *any* decrease in the obliquity of the ecliptic was positively denied by the then ruling astronomers. Readers of the "Philosophical Society's Journal," from 1700 to 1750, will find that those persons who believed in any decrease in the obliquity were in the minority, and were treated with but slight respect by the paid official astronomers of the day. It being supposed that no decrease occurred in the obliquity whilst a movement of the pole of the heavens occurred of 20″ annually, it followed that the only possible course of the pole of the heavens to fulfil these assumed conditions was a circular course round the pole of the ecliptic *as a centre*.

The obliquity of the ecliptic being about 23° 28′, it followed that the pole of the heavens traced on the sphere a circle having a radius of 23° 28′ at the rate of 20″ annually. At the period when this supposed movement was agreed to, theory was in the ascendant; the explanation by gravitation of certain celestial movements caused every man who hoped to occupy a prominent position in the scientific world to be a theorist, and to follow and adhere to theories often to the exclusion of facts. The theory proposed by Newton to account for the gyration of the poles of the earth was that the protuberance of the earth about equatorial regions was acted on by the sun and moon, and caused the direction of the earth's axis to vary, though the pole of the earth traced always, it was supposed, a circle round the pole of the ecliptic as a centre.

When the accuracy of modern observation proved that there was a constant decrease in the obliquity of the ecliptic, at the rate of about 0·45″ per century, there seems to have been a great lack of geometrical knowledge among the astronomers occupying official positions, for they still wrote of and described the movement of the pole of the heavens, as tracing a circle round the pole of the ecliptic as a centre, and never varying its distance from this pole, yet admitting that there was a decrease in the obliquity of 0·45 per annum. Now the fact is, that any variation in the obliquity is the same thing as a variation in the angular distance of the pole of the heavens from the pole of the ecliptic, and of the circumference of the circle from its supposed centre, consequently that the pole of the heavens can describe *a circle* round the pole of the ecliptic *as a centre;* whilst we admit a decrease in the obliquity is granting as true that which is impossible. That a grave oversight was here committed it is useless to deny; and though many official astronomers have, since we pointed out* this error, attempted to defend their predecessors, or their own writings on this erroneous problem, yet their endeavours partake more of the attempts of advocates to defend a bad cause than of philosophers desirous of eliciting truth. Personal attacks on an author who has pointed out their errors, and assertions that his problems are by no means believed in by A, B, or C, are not proofs of error, nor can any amount of "authority" outweigh a geometrical proof or law.

When a certain school of teaching have agreed to some problem or theory, have written on this as if it were unquestionable, and have based their calculations on it, it naturally follows that they should at first oppose any innovation on their belief. It does not follow, however, that any number of such men, if they merely make assertions, should carry one atom of weight in disproving that which can be demonstrated by mathematics or geometry. What weight, for example, does Herodotus carry when he asserts that he cannot refrain from laughing when he hears men state that the earth is round, as though made in a machine, and that Asia is as large as Europe. He then asserts the relative

* In the "Last Glacial Epoch," and "The Proper Motion of Fixed Stars."

size of the two continents, and immediately—like many modern asserters—talks nonsense.

It is a law that a decrease in the obliquity of the ecliptic is the same thing as a decrease in the angular distance of the pole of the heavens from the pole of the ecliptic, and is the same thing as a decrease in the supposed radius of the circle described by the pole. Hence—in spite of assertions, of authority, and the many writers who have stated it to be so—it follows that the pole of the heavens does not, and has not at any time within historical periods, traced any part of a circle round the pole of the ecliptic as a centre.

We now come to the most modern explanation of the facts known in connection with the change in direction of the earth's axis, and the decrease in the obliquity. It has recently been stated that the plane (and hence the pole) of the ecliptic is slowly changing its position in the heavens, and thus bringing the pole of the heavens nearer to the pole of the ecliptic; that the pole of the heavens always moves at right angles to the arc by which it would be joined to the pole of the ecliptic; and that the two facts, viz., the movement of the pole of the heavens and the decrease in the obliquity, are thus accounted for.

This is a question either of fact or theory. If it be a fact, it follows that the angular distance of all stars from the pole of the ecliptic—that is, their co-latitudes—will be found to have decreased in one part of the heavens, viz., in that part towards which the pole of the ecliptic is supposed to be moving, and the co-latitude of stars in the opposite part of the heavens will be found to have increased. In order that there should be a decrease in the angular distance of the pole of the ecliptic from the pole of the heavens, it follows that the movement of the pole of the ecliptic should be approximately towards that part of the heavens in which Sirius is situated, and away from that part in which α Lyræ is located. To prove this movement, all stars in one direction must have decreased their co-latitudes; all stars in the other direction have increased their co-latitudes. After upwards of seven years' investigation, and after searching every catalogue of stars from that of Ptolemy to the latest, we fail to find *any* evidence of the assertion that such a movement of the pole of the ecliptic has occurred; for there is not any defined change in the latitude of stars to prove that the pole of the ecliptic has moved towards that part of the heavens near the meridian of 6 hours' right ascension.

We have, however, to deal with two well-defined facts, viz., a change in direction of the earth's axis of about

20″ annually, and a closing in—as we may term it—of the pole of the heavens to the pole of the ecliptic of about 0·45″ per annum, at the present time: that is to say, the pole of the heavens traces on the sphere of the heavens an arc, which, when referred to the fixed stars, indicates a rate of about 20″ annually, and towards that part of the heavens indicated by the first point of Aries. This arc approaches the pole of the ecliptic, and may be fairly represented by the arc P P′ P″ P‴, whilst E is the pole of the ecliptic.

Fig. 1.

The angular distances, E P, E P′, E P″, E P‴, will be the angular distances of the pole of the heavens from the pole of the ecliptic, at those dates at which the pole of the heavens was located at P, P′, P″, &c.,—and these distances represent the value of the obliquity at those dates. The records of these measures of the obliquity exist, and records of the rate of the movement of the pole exist; consequently the curve P, P′, P″, P‴ can be defined, as regards E, the pole of the ecliptic; and this curve is part of a circle, or part of an ellipse which does not differ much from a circle, the centre of which circle is 6° from the pole of the ecliptic, and so situated that at the date A.D. 2298 the pole of the heavens, the pole of the ecliptic, and the centre C, will be in the same great circle of the sphere. The curve thus traced out corresponds exactly with the recorded observations of the past as regards the decrease in the obliquity. It explains why the annual decrease in the obliquity is now less than it was found to be one or two hundred years ago, for it will be evident that the nearer we approach the date A.D. 2298 the less will be the rate in the annual decrease in the obliquity, whilst the further we go back the greater will be the annual rate; and this variation in the rate is in strict accordance with recorded observations.

We have, again, another fact to deal with, carrying back into the past the curve (either as a circle or an ellipse very closely corresponding to a circle) thus defined we should obtain a variation of at least 12° in the extension of the obliquity at the date when the pole of the heavens was situated 180° in the circle from the date A.D. 2298. Such an extension of the obliquity would bring England within the arctic circle, and would supply those climatic conditions which geologists consider prevailed during the Glacial Epoch. Thus we have, first, the actual recorded observational facts proving a certain curve to have been traced during the past four or five centuries: secondly, geological evidence, indicating such a climatic change as would result from an extension of 12° or more in the obliquity.

These two evidences are certainly strongly in favour of a possible increase in the obliquity, and, unless very powerful and convincing evidence can be brought in opposition thereto, must certainly tend to prove the fact.

Let us, however, examine another problem. Let us grant that the plane of the ecliptic does slowly vary (of which variation, however, there is no proof from the recorded changes in star latitudes), the course of the earth round the sun would slightly vary; but it is possible that the earth's axis would partake of this movement, and the pole of the heavens and pole of the ecliptic would then vary their relative distances just the same as if the plane of the ecliptic were at rest; and as it is a fact that the course of the pole of the heavens relative to the pole of the ecliptic is such a curve as that we have described it to be, we have actual facts on one side and theoretical conclusions on the other.

Taking an abstract view of the problem as far as it is here stated, we have the following as the two sides of the question relative to a possible variation of the obliquity:— On the one side we have a series of distances measured between the pole of the heavens and the pole of the ecliptic (termed the obliquity at various dates), and also the rate at which the pole of the heavens has decreased its distance from certain stars; consequently the nature of the curve thus traced by the pole of the heavens can be ascertained without any great difficulty. This curve is not one imagined by a theorist, or supposed to be traced in consequence of certain assumptions connected with attraction and repulsion, but the curve is one that recorded facts prove to have occurred during at least 400 years, and this curve— if continued during 13,000 years—will produce a change in the obliquity or angular distance of the two poles, viz., that of the heavens and of the ecliptic of $35\frac{1}{2}°$, and would consequently bring portions of Great Britain within the arctic circle.

That the changes in the latitude of stars in one part of the heavens, and an opposite change in another part of the heavens, indicate that the pole of the ecliptic has moved during past times in any particular direction is not true. This assertion we can bring to the test of recorded facts. The star catalogues of Ptolemy, Tycho Brahe, Hevelius, Halley, and others, can be compared with modern catalogues, and it will be found, by any fair and impartial investigator, that those persons who have so boldly asserted that the proof did exist are more remarkable for their zeal in endeavouring to support a popular theory than they are for that unprejudiced love of truth and fact which should alone be the desire of the man of Science.

The statement that the pole of the heavens always traces an arc at right angles to the arc joining the pole of the heavens with the pole of the ecliptic is a theory. To decide whether the pole of the heavens traces an arc at right angles to the arc joining the pole of the heavens with the pole of the ecliptic, or at right angles to the arc joining the pole of the heavens with the point C 6° from the pole of the ecliptic, is a problem almost impossible to decide by the actual changes in North Polar distance of stars near 24 and 12 hours right ascension. The means, however, by which it could be discovered which course is pursued we have pointed out in detail in our work "The Motion of the Fixed Stars," pages 112 to 170.

The important fact, however, remains,—viz., that if the pole of the heavens trace an arc at right angles to that by which it may be joined to the pole of the ecliptic, then, to account for a decrease in the obliquity, it must be proved that the pole of the ecliptic does move annually towards the pole of the heavens 45″ per century now, and at a more rapid rate formerly, as is shown by recorded observations. Yet no such evidence can be adduced.

Again, suppose it could be shown that the pole of the ecliptic has moved towards that part of the heavens in which are stars having from 5 to 7 hours' right ascension, we still have the stubborn fact before us, that the actual curve traced by the earth's axis is part of a circle having for its centre a point 6° from the pole of the ecliptic, and that if the pole of the ecliptic move towards that part of the heavens occupied for the time being by the pole of the heavens, still we have to account for the curve being such as it is; for to assert that it is a "strange coincidence" that during historical records this curve should coincide with such a circle is little better than a mere subterfuge to avoid admitting a fact which is not popular with a certain party.

Some writers who have opposed the idea of any change in the obliquity greater than 1° 12′ have asserted that it is easy to find a curve that will correspond with the recorded obliquity during three or four hundred years. Such a statement plainly shows that the writers have never attempted to test their assertion; for not only will no other curve agree with the arc thus defined, but if we assume a centre of even 5° 50′, instead of 6°, from the pole of the ecliptic, the curve then traced out will in twenty or thirty years differ several seconds from recorded observations, instead of agreeing with them to within one-tenth of a second for two hundred years. It is unfortunate that writers should make such incorrect statements, and also that persons, in their endeavour to oppose an original investigation, should publish assertions relative to this subject which—had they really read the books* which treat of it—they never could with truth have ventured to write.

As an example of the very small amount of enquiry or thought which has been devoted to this problem, we will call attention to the supposed unanswerable objection which certain writers have brought against the proof of the course traced by the earth's axis being *a circle* round the point 6° from the pole of the ecliptic. These objectors assert that such a course is "impossible," and is against the laws of gravitation, and is opposed to all the analogy of the solar system. They add that *the orbits* of all planets are ellipses, and that therefore no such thing as a circular course can be possible for the pole of the heavens. The repetition, by copying of this statement, shows how powerful it is supposed to be.

In the first place, these objectors forget that the present orthodox belief, which they are endeavouring to defend, is that the pole of the heavens traces *a circle* round the pole of the ecliptic as a centre. When they assert that because the orbit of a planet is an ellipse, therefore the pole of the heavens cannot trace a circle, as such a course is opposed to certain laws, they forget that every zenith traces *an exact circle* round the pole of the heavens every twenty-four hours,—also that the conical movement of a planet's axis is geometrically the same as a second rotation of that planet. Consequently it would be opposed to experience if the earth's axis traced anything but a circle during one

* The Last Glacial Epoch. The Motion of the Fixed Stars.

revolution of the equinoxes, or, as it may be termed, one second rotation of the earth; and the analogy between the "orbit of a planet" and "the course traced by the pole" is as appropriate as would be the course of a line of rails and the movement of the piston of a locomotive.

The problem, however, connected with the possible variation to a great extent in the obliquity of the ecliptic, is one which is considered entirely of a mathematical nature, and the assertion that no variation greater than about 1° 12′ could occur in the obliquity is the result of the investigations of theorists.

When we examine the manner in which this problem has been treated, we find that certain important omissions have occurred, on which the real question at issue is dependent; and instead of the question being one that can off-hand be resolved into a mathematical problem, it is in reality one that can and must be decided long before it assumes the form of a mathematical investigation.

The problem of a possible change in the value of the obliquity to any great amount has been confined almost entirely, hitherto, to an enquiry as to the extent to which the *plane of the ecliptic* could vary from a mean position; and when M. La Place concluded that the plane of the ecliptic could vary only 1° 21′, it was assumed that the obliquity of the ecliptic could vary only that amount. Such a conclusion is incorrect. The obliquity of the ecliptic being the complement of the angle which the earth's axis makes with the plane of the ecliptic, it follows that the direction in which the earth's axis moves is *the* problem for enquiry,—far more important as regards the solution than is that of the variation in the *plane* of the ecliptic.

That the earth's axis was formerly supposed to trace a circle round the pole of the ecliptic, as a centre, is evident from the writings and calculations of all the olden astronomers, and any change in this movement was supposed impossible. It is to this portion of the problem that we will now earnestly call attention.

The present accepted theory of the producing cause of the conical movement or change in direction of the earth's axis is as follows:—The earth is supposed to be a homogeneous spheroid, with a protuberance of about 1-300 at the equatorial diameter. This protuberance,* being acted on by the mass of the moon and sun, causes a gyratory movement of the axis of diurnal rotation, and produces a

* See Airey's Lectures.

change in direction of the axis, making it describe a small circle of the sphere round the pole of the ecliptic.

Let us grant that this theory is perfectly sound, supposing the preliminary data be correct, and we have yet not touched the main question at issue; but let us first examine the preliminary data.

How do we know that the earth is homogeneous?

Let us suppose that every part of the earth's surface in the southern hemisphere is at exactly the same distance from the true centre of the earth,—that the surface in a corresponding latitude in the northern hemisphere is from the true centre,—and it is quite possible that the mass of the two hemispheres is different, and hence the assumption that the earth is homogeneous may be a false one.

Let us, however, come to facts. Let us examine the earth as it is, and deal with what we know. Water finds its own level, and the surface of the ocean in say 50° N. latitude must be at the same distance from the earth's centre as is the surface of the ocean in 50° S. latitude, and so for every degree of latitude north and south. When we examine the globe we find north of the equator the enormous and elevated mass of land comprising Asia, also the whole of Europe, also fully two-thirds of Africa, and the whole of North and part of South America.

These vast continents of land in the northern hemisphere have as a set off, in the southern hemisphere, Australia, a few islands, one-third of Africa, and part of South America. At a rough estimate we may state there is four times as much land, above the sea, north, as there is south of the Equator.

Again, the great depths of the ocean in the Southern Pacific, in the Southern Atlantic, and Indian Ocean, admit of vast quantities of water being now in the southern hemisphere, whereas in the northern there are massive granite mountains, raised 3 and 4 miles above the sea-level, and having in place of water only, 5 miles below the surface, solid granite.

If, then, the materials of the earth be homogeneous, it follows that the preponderance of the land in the northern hemisphere must cause the centre of gravity of the earth to be situated considerably north of the Equator. Every competent mechanician knows that the second rotation of a rotating sphere is, as regards its amount and direction, mainly dependent on the situation of the centre of gravity. If the centre of gravity coincide with the centre of the sphere, regularity of movement may be expected; if the centre of gravity be not coincident with the centre of the sphere, eccentric motion occurs.

In the conclusions arrived at relative to the movement of the earth's axis being such as to produce only one uniform course in the axis, we fail to notice any reference, or any consideration, to the fact that the centre of gravity of the earth does not lie in the plane of the Equator.

But, again, let us examine the globe, and taking half the northern hemisphere—viz., from longitude 15° W. round to about 165° E.—we have the mass of Europe, Asia, and Northern Africa on one side of the sphere, whilst on the opposite side we have only the continent of North America. About three times as much land is above the ocean between 15° W. longitude and 165° E. longitude as there is in the other half, and it therefore follows that the earth's axis cannot pass through the centre of gravity of the earth.

We have, then, at present a spheroid of irregular form, in which the centre of gravity does not lie in the plane of the Equator, nor does the axis of rotation pass through the centre of gravity.

Let us now ask a question. There are forces acting by attraction externally on the earth, and supposed to produce a conical movement of the axis, in consequence of the protuberance at the Equator. Will the movement of the axis of diurnal rotation be exactly the same, no matter whether the centre of gravity of the earth lies in the plane of the Equator, and is passed through by the axis of rotation, or if this centre of gravity is located elsewhere? We need not remind the reader that the movement would not be the same.

Of all the conditions that produce a change in the second rotation of a rotating body—that is, in the change in direction of the main axis of rotation—none is greater than that of a displacement of the centre of gravity, or the fact that the centre of gravity of the rotating body is not coincident with the centre of the sphere. The divergence from a direct course or vertical plane of a spherical shot is always great when the centre of gravity of this shot is not equidistant from all parts of the surface, and this divergence is accompanied by—if not mainly dependent on—a rotation of the shot, in a manner dependent on the position of its centre of gravity.

The period occupied by the earth's axis in tracing a circle round the pole of the ecliptic would be about 25,000 years if the pole of the ecliptic were the centre of this circle, and the period would be about 31,000 years if the centre were the point 6° from the pole of the ecliptic. As the earth rotates about 366 times in one year, it follows that during one revolution of the equinoxes there would be at least 9,150,000 rotations of the earth on its axis. The fact that the centre of gravity of the earth is not situated in the plane of the Equator would produce a different movement of the earth to that which would occur if the centre of gravity were located in this plane ; and although during one rotation the differences would be very minute, yet when this difference is multiplied by 9,000,000 the variation would be considerable.

We have, however, other important matters to consider in connection with this problem. We have supposed that the earth is a homogeneous spheroid, but we do not know this, nor can any person know it,—we can only consider it probable ; but we can now state what we do know. We do know that in former periods of the earth's history there was a very different distribution of land and sea to that which now prevails, and the results of such changes we can define.

Let us take for granted that the earth is homogeneous, and that at present the centre of gravity is not coincident with the Equator, nor is it passed through by the earth's axis. Now the fact of an elevation of land—say in the southern hemisphere—by the action of internal forces, or the depression of land from any cause, would not make this land weigh more or less, and therefore the relative weight of the land in the two hemispheres would not be altered by any amount of elevation or depression, and no change in the position of the centre of gravity of the earth would be produced by any amount of elevation or depression of the land, *supposing the earth consisted of land only*. Immediately, however, there is an elevation of land, there is a transferral of the water of the ocean over the whole earth. The sea which before covered this land is spread over the whole globe ; and when such masses of land as the greater part of Europe and America rose from beneath the sea, millions of tons of water were transferred to other parts of the earth, and the position of the centre of gravity of the earth was consequently altered thereby. The equilibrium (as we may term it) of the earth would not be altered by the actual fact of the elevation or depression of land in either hemisphere, but it would be altered by the resulting transferral of vast masses of water from one hemisphere or one locality, which masses would then be spread over the whole surface of the globe.

The calculations made of the effect produced by the moon and sun on the rotating earth, when the centre of gravity was in one position, could not hold good,—nor would the effects be the same when the centre of gravity was situated in another position. It may appear that the effects resulting from such a cause would be slight, but it must be remembered that whatever effect is produced during one rotation of the earth would have to be multiplied by upwards of 9,000,000 to obtain the effect during one revolution only of the equinoxes; and as 25,000 years is but a small portion of time when compared to the long epochs required by Geology for different periods, it will be seen that this is by no means a fact to be neglected or ignored.

We know, from the researches of geologists, that there was in former times a vastly different distribution of land and sea to that which now prevails. We know that this cause would have produced, by the transfer of the water of the ocean, a change in the position of the centre of gravity of the earth; and, as mechanicians, we know that it is impossible to alter the position of the centre of gravity of a rotating body without altering the movement which the axis of this rotating body may at the time be making. Consequently it follows—if the statement made by geologists be true—that, in former times, continents now above the sea were formerly below that level, and that the change in direction which the earth's axis now exhibits is not that which it formerly pursued; and as the value in the obliquity of the ecliptic is dependent on the course pursued by the earth's axis, it may be affirmed that the variation in the obliquity to a considerable amount is not only possible, but is most probable.

The question of this great change in the obliquity is, we claim, one which cannot be definitely and arbitrarily settled by making certain calculations relative to the attraction of the sun and moon on the protuberant mass at the equatorial regions of the earth, and ignoring all other facts. It is one which no mathematician can deal with correctly, whilst he ignores or overlooks the fact of the present probable position of the earth's centre of gravity, and of the certainty of this centre having formerly been located elsewhere, and also of there having been many changes in its position due to the transferral of the waters of the ocean, in consequence of the many elevations of land in various parts of the earth in former times.

As a summary of this problem we may give the following:—

1st. The course traced by the earth's axis during the past 2000 years corresponds to an arc of a circle, having for its centre the point 6° from the pole of the ecliptic, and so situated that at about the date A.D. 2298 the pole of the heavens, the pole of the ecliptic, and this centre will be in the arc of a great circle. The result of such a course would be to cause an obliquity of about 35° at a date about 13,000 B.C.

2nd. The evidence of Geology is positive as regards the existence of glaciers, in former times, in localities down to about 54° lat. Thus astronomical facts and geological evidence agree as regards this course of the pole.

3rd. The fact that the elevation of land in one hemisphere, or in one locality, will cause a transfer of water, and hence a removal of the locality of the earth's centre of gravity, must produce a change in the movement of the earth's axis, so that this movement cannot be uniform for all time.

4th. The positive assertions of former theorists, that no change greater than about 1° 21′ could occur in the variation of the obliquity is based on two errors:—First, this conclusion was arrived at because it was supposed *the plane of the ecliptic* could vary only to this small amount, and, secondly, because the variation in the position of the earth's centre of gravity was entirely overlooked.

5th. The real problem relative to the variation in the obliquity has little or nothing to do with any change in the position of the plane of the ecliptic, but it has to do with the change in the direction of the earth's axis, and therefore is dependent on the change in position of the earth's centre of gravity produced by the transfer of water from one part to another.

6th. The present somewhat dogmatic theory, that no change whatever greater than 1° 21′ has occurred in the obliquity, assumes that the earth is homogeneous, that the earth's centre of gravity is situated in the plane of the Equator, *and was formerly always where it now is.* That the earth is homogeneous is a speculation only. That the centre of gravity is located in the plane of the Equator cannot be true if the earth is homogeneous. And it is an undeniable fact, due to the known distribution of land and sea in former times, that the earth's centre of gravity was not in the past where it now is.

It thus appears that this problem has yet to be investigated, for the present accepted theory is based on errors or omissions, and is opposed to facts. Its advocates have too often to ignore disagreeable facts, and to start on assumptions which geological knowledge shows us is erroneous. That a curve should now be traced by the earth's axis, which, carried back 13,000 years, will cause an obliquity of 35°, is a fact not to be disposed of by the mere quotation of what the present accepted theories are, especially when geological evidence so positively demonstrates that such climatic changes as would follow this condition actually prevailed on earth. Whilst, however, the earth's axis now traces this course, a different position of the earth's centre of gravity would cause a different movement, and hence the course might have been such in former times as to cause the obliquity to have been far less than it now is,—a condition which would produce almost an uniform climate over the whole earth, as seems to have been the case during the Miocene period.

The present accepted belief, therefore,—that the pole of the heavens always traces a circle round the pole of the ecliptic as a centre, and that the only producing cause of a change in the value of the obliquity must be a change in the position of *the plane* of the ecliptic,—appears erroneous, and to have been based on an imperfect collection of data. No allowance has been made for a change in the position of the earth's centre of gravity, and no knowledge has been shown that the course traced by the earth's axis has even now a definite character, which is not a circle having for its centre the pole of the ecliptic.

The statement, also, that the course traced by the earth's axis in all past times must have been the same that it now is, is the same as affirming that if you alter the position of the centre of gravity of a rotating body you still cause no change in its movements,—an assertion so palpably erroneous that we believe all unprejudiced persons must, when this omission is pointed out to them, admit that their position is untenable, and must grant that a considerable change, both greater and less, in the obliquity is not only possible, but must have occurred.

That vast volumes of water have at various times been transferred from one part to the other of our sphere is a well-known fact. That this transferral must cause a change in the position of the centre of gravity follows as a result; whilst a change in the movement of the axis of rotation also follows as a natural law.

Hence the evidence derived from the course now traced by the earth's axis, from the evidence relative to changes in the earth's centre of gravity inducing a different movement, from the convincing evidence of Geology relative to the former climate of high latitudes, and the absence of any sound arguments or proofs against such changes, lead to the conclusion that the obliquity of the ecliptic not only may have, but has, varied in former times considerably, having been both greater and less than it now is.

That a far less obliquity would give us the uniform climate of the Miocene, &c., whilst a greater obliquity would give us the conditions of the Glacial epoch, is undoubtedly true. It is also undeniable that the course now traced by the earth's axis, if carried back into the past for 13,000 years, would bring about conditions corresponding exactly to those indicated by the geological evidence of the Glacial epoch. That this same curve should be traced in endless gyrations is not, however, probable, and is in reality impossible, when any elevation or depression of the land occurs; and as such changes have occurred, we must trace—from geological evidences of climate—what the former course of the earth's axis probably was.

In conclusion, therefore, we must state that the evidence is considerably in favour of great changes having occurred in the obliquity in past times, that there is no sound mathematical objection to such change, and that the solution of geological paradoxes is probably to be found in this problem. And that the singular opposition offered to its reception by a certain class arises entirely from their not having examined *all* the preliminary data, and from a praiseworthy adherence to their early teaching.

II. THE RIGHTS OF THE THINKER.

A NEW Patent Law is before Parliament, and questions regarding literary copyright have lately been put before the Prime Minister: it is clear, therefore, that some people are interested in defending the men of thought and invention, but it is equally clear from the discussions that neither the lawyers nor the laity have arrived at any clear ideas of the right of property which a man has in his own thoughts. If discovery in the Arts is spoken of, we still have men telling us that they are all the result of accident; scholars have still some floating idea of glass being invented at the casual sight of it formed under the fire made for cooking on some Phœnician river; and Newton's apple accidentally falling seems to be an argument to others. We may throw aside all such fancies; for supposing the glass story to be true, which it almost certainly is not, how many millions of fires had been made in similar circumstances before, without finding an eye to see; and supposing the apple story to be true, as giving a momentary impulse to the thoughtful soul of Newton, how many millions had before seen the selfsame result of falling to the ground, as a characteristic of matter around us. To rear a germ seen in any natural or artificial act up to the fulness of life and usefulness is the work of invention or discovery, as the case may be, and the man who does so creates out of that which was as nothing to the world a property which his fellows value, at times, to an extent which is incalculably great. If a man takes unoccupied territory, producing nothing to his countrymen, and renders it productive, the trifle that he pays for it in any colony is as nothing to that which labour makes it, and no man thinks of denying his rights when he has made a rich estate. He has used the knowledge of Nature, and has made a permanent property for himself and his posterity. If another finds—as in this country—all the land occupied, and seeks, out of other departments of Nature, to bring results valuable to himself and others, he uses frequently much more labour, much rarer knowledge, more varied talents, and with difficulty he obtains a right to his property for a few years only, and then every one seizes it and uses it for himself. Why should a property which a man creates be allowed in his possession for a shorter time than if he finds it? There is one reason, and only one good one, that we know of. If the world of mind had been valued according to its usefulness it would have accumulated to an extent that it would be a burden to the living, and the very progress made would become a clog on its own continuance. This is a reason why the rights should not descend to posterity, but it is not a reason why they should not exist during a considerable part of a man's lifetime, or a certain time after death should he die early. Let a man make an invention every year for thirty years, and let each invention be worth an acre of land,—at the end of the fourteenth year the value of an acre is taken from him, and every year afterwards the same amount is taken away for thirty years, until at last nothing is left. This is the way inventors are treated, whilst peculiar laws are made to retain the property of land in the same family for generations. It is even proposed to take inventors' property away every seven years, as the thriving Jews and other wealthy persons—in times gone past—were robbed for the good of the country or the pleasure of the rulers.

The length of time suited for a patent is worthy of re-discussion. We are showing rather the error of those who would give none whatever. One reason for thus bleeding inventors is, that they take ideas out of a field common to all men. So are the fish of the sea, but he who catches is allowed to keep. Another form of this fallacy is, that some one else would have got the idea; but who knows? The world waits for thousands of years for very simple ideas. There are a few corollaries from other ideas that easily come to many minds, and that is held as a reason why no one should have any monopoly. This is a favourite fallacy: men forget that an idea cannot be made useful to the public without labour and expenditure, and unless a man has some protection he will not risk his money or his time. It is for the interest of the public that there should be a monopoly of a new invention for a time, that it may be worked out and completed, and the market supplied; and if twenty inventors claimed the idea at the same moment it would be a loss to the public if one out of the twenty were not chosen as the owner, or a joint ownership established. In a word, a manufacturer or capitalist refuses to aid in an invention without protection. There are ideas lying dead, because by some patent law they have lost protection. It would be well if the country would renew such patents to men who will undertake them. Why should it be impossible to patent them twice? If the first man in fourteen years could not repay himself, why not give other fourteen years to him, or to another man? And why should a fully-published idea be in all cases a bar to this kind of protection? The knowledge may exist, but the power, or knack, or tact may be wanting, and it is he who benefits the public that the public must reward.

The principle at the root of this reasoning is, that the public gains by giving—for a time at least—the monopoly to individuals. We believe this principle to be fully established.

If we take a country in which patents are not to be procured, we find invention repressed. There can be little doubt that the thoughtful German mind would have done more in industry—a direction extremely natural to it, as its early history shows—had not the laws been destructive of protection to the individual inventor. We do not know if the Government imagines that by refusing patents the country obtains inventions for nothing. The delusion is great; who will show his treasures if they are to be stolen?

The same reasoning applies to ourselves; it is by increasing the protection of the inventor that we shall gain more invention. We know well that many of the best men refuse to carry out ideas because of the expense, which patents will not enable them to recover, because the laws have been a confusion and the administration in the hands of men who do not pretend to a knowledge either of science or the industrial arts.

It has been a principle, on which the patent law is founded, that the good of the commonwealth is the aim. This is fair enough, so far, but we know what injustice was often done under the promising phrase, so powerful in Rome as to make the people blind to the misfortunes it entailed. If the individual must suffer for the commonwealth, it is only because the commonwealth is the protection of the individual, and if it is not he has no interest in suffering for it. We may say, therefore, that the rights of the literary and inventing class are such as the State must protect, as it protects the property in land or potatoes; and if it should not do so it can only be when reduced to a condition of such barbarism as can imagine no property that cannot be measured with a chain or weighed in sacks. In some nations a man might be said to live for the State; in newer times the State is seen, more and more, to be subservient to the individuals, and as new worlds are evolved from the old they must have their position recognised in proportion as they show their value. These new worlds cannot be formed of the same material as this old one, but they are no less admired, and if there is a certain endurance in the matter of this old world which seems destined to give it force as well as respectability, still its highest value is only as a foundation for the growth of the, apparently more transient, gifts of the mind.

Another point connected with patents is not to be neglected. It is said that they must only be given in the name of the inventor. This produces much evil, and causes some men to say that they are inventors when they are not. There is no harm whatever in giving patents in the name of anyone who may say that he is commissioned by the inventor, with whom there may be an agreement. This has been done, and is done, with foreigners, and no good objection has arisen. We have surely a right to this privilege as much as foreigners have. The advantage is, that men may give the benefit of their inventions without being dragged before the world or tormented in law courts, when by constitution they are unable to bear the annoyances. We know the want of such a permission to be a loss to the world of invention. Many who speak in public on this subject seem never to have known inventors or watched the growth of invention. We know that among patentees there are men who may more properly be called thieves, and frighten away some of the best; but as we have seen the growth of not a few inventions, which have been improving year after year, with careful study, labour, and expense, we have learnt, as we imagine any man who thinks at all must learn—in a very simple way—the great value of those of the creative race of Britain who take patents, and we marvel very greatly at legislators who are willing either to throw them aside as too common, or to starve them, in order that they may work hard and produce more. We give the unvarnished meaning of some of the arguments.

We have before us a sketch of a new patent law for Germany. The new patent court is proposed to be partly of lawyers and partly of experts, but there is not room here for a revision of the plan. In Germany the re-issue of patents has been very unsuccessful: the cause may lie very deep—viz., in the constitution of the German mind, which sees principles too much, and not the facts. Facts we know are endless, but if we take the principles out we find them few, and one thing becomes similar to many other things, and in a certain sense the same. The real practical meaning and value of an invention is therefore lost to minds of a certain stamp. In America this has not occurred. It is quite uncertain whether the English mind will be suited to form a court of patents,—it must be tried. To judge from the actions at law we should lose hope; but we may find another result when men are allowed to judge, freed from the trammels of precedent and ancient habit of thought, and free from the irritating arguments so often brought about apparently to complicate the subject. We are inclined to think that no men in Europe could do the work with less bias than the English. We have not studied the American habits on this subject. We may be satisfied, however, that any sensible men could weed out a great many of the applications for patents, and in any disputes come to safer conclusions than can now be obtained by expensive so-called legal processes.

When international law rises somewhat higher out of the region of those inferior quarrels which have so often led men to kill each other, the rights of the inventive and creative mind must take a more prominent place; and the natural tendency is so powerful in this direction that the inability to see it would appear to stamp a man as one of the past, having little sympathy with progress and little tendency to look forward.

We do not see that the time is come for an international patent law,—the interests of nations are not sufficiently alike, even if we include the more advanced nations only,—but in the matter of copyright the union is much nearer the possible, and that nation which advances most in justice and wisdom will assist most readily in forming it. This, however, requires discussion from another point of view, and we shall not continue the subject at present. We do not pretend to have discussed the patent question, but have only brought forward some of the points which seem to have been most neglected, and, being fundamental, are suggestive of many important details.

III. ANOTHER VIEW OF LEVITATION.

IN a recent number of this Journal several instances were given of the ecstatic rising of Saints during prayer or deep absorption of mind. Amongst other things it was said that these levitations, as they were called, did not occur among Saxon Saints. This, we think, is an oversight, as we have the liftings of St. Dunstan—a true Saxon, we suppose, the Dun is clearly not the Celtic noun—and St. Edmund (king), an Angle not easily distinguished from a Saxon. The first will be found narrated, under the name of that Saint, in Butler's "Lives of the Saints;" the second is mentioned in a note under "The Life of St. Philip Neri." It may, however, be true that the Saxon Saints are less connected with the subject than others. This leads to the question, Is it because of the less excitable character of that nation? and we next naturally ask if it is a matter of nerves physically, or is it because of the less imaginative character of that practical race? The tendency of the article connected levitation as among the Saints with modern Spiritualism, which certainly flourishes most in a very excitable, although also practical, nation.

It may be a little foreign to the point to introduce here circumstances, or rather narrations, from a literature almost unknown in England,—levitation of a character more romantic, incredible, and, we may add, fantastical; but, on the other hand, it is a part of scientific enquiry to neglect no view of a question, and if, by going in the direction now taken, we may appear in one instance to be throwing discredit on the whole matter by absurdities, and in another by what some persons might fancy to be profanities, it may, in another point of view, be shown that it is right, in order that we may see to what opinions may lead, or from what remarkable origins they may spring. This is said because it seems uncertain which is the character of the true beginners, although, as a rule, it is the smallest that must be held as such. It is true that we, with most persons, do consider all this Hibernian matter a fine exaggeration, not exceeded by any Americanisms, of which it is perhaps the ancestor; but we give it partly because of the matter being interesting in itself, as belonging to the history of ideas in Physics, and partly to show that the subject is by no means exhausted when viewed from one side only. Scientifically, however, we must not forget that the ideas in the battles of Ireland are like the religious narrations in England and the Continent in exciting times. In the history of the Irish Saints we have an exaggerated levitation, if we may so call it,—but that again runs into the absurd, and it is quite possible that it may have its origin in the minor, and apparently better authenticated, risings during prayer. One of these is about the wildest thing that man has imagined.

The flying of excited heroes is scarcely so well represented in the church, by any example known to us, as by "Christina, a virgin of Tron, who is said to have been carried into the church for burial, when her body ascended from the coffin, and, being recovered from her trance, she related her visions, and ever after was so light that she could outstrip the swiftest dogs in running, and raise herself on to the branches of trees or the tops of buildings."—("Encyclopedia Metropolitana,"—Occult Science.) This is more remarkable even than "Swift Camilla," who skimmed o'er the unbending corn. Savonarolo, as related there, quoting Calmet, is a remarkable instance.

Of course we shall not blame anyone for considering the whole of it like the tale of Mogh Ruith, who called for his white-speckled bird head-piece, with its fluttering wings, and his Druidic instruments, and flew up into the air to the verge of the fires, and turned them northwards against his enemies in a great Druidic battle.—(O'Curry and Sullivan's "Customs of the Ancient Irish," vol. ii.) Or they may be put among such exaggerations as in the fight between Cuchallain and Ferdiaidh, in the same volume, when they made such holes in each other that the birds could fly through them; or when "Cuchallain sprang from the brink of the ford, and alighted at the boss of Ferdiaidh's shield, —upon which Ferdiaidh gave the shield a blow of his left elbow, and cast Cuchallain off from him, as if he were a bird, back to the brink of the ford. Cuchallain again sprang from the brink of the ford, and alighted on Ferdiaidh's shield, for the purpose of striking him on the head." These are stages of the flying, and they can readily be traced through many degrees; and of course we can readily believe that the origin exists in the intensity of wishing in the human soul, by which the slow progress of human beings moving by machinery of flesh and bones envies the additional apparatus of the fowls, and finishes—if it does finish —by balloons. The feeling has its true expression, like many other of our greatest truths, in the words of the Psalmist:—"Oh that I had the wings of a dove, that I might flee away and be at rest."

If the following rather shocks Spiritualists, and sends them to re-consider their facts, it will do no harm, and it will give to those who have not seen any of the wonders of old Irish literature a curious taste, and may add a few readers of its remarkable writings to the small number among us:—In the "Battle of Magh Rath," on Moira, we have an account of the madness of Suibhne, or Sweeny. Taking O'Donovan's translation, as published by the Irish Archæological Society, we find, at p. 231,—"With respect to Sweeny, the son of Colman Cuar, the son of Cobhthach, king of Dal Araidhe, we shall treat of him for another while. Fits of giddiness came over him at *the sight of* the horrors, grimness, and rapidity of the Gaels; at the looks, brilliancy, and irksomeness of the foreigners; at the rebounding furious shouts and bellowings of the various embattled tribes on both sides, rushing against and coming into collision with one another. Huge flickering, horrible, aërial phantoms rose up, so that they were in cursed commingled crowds tormenting him; and in dense, rustling, clamorous, left-turning hordes, without ceasing; and in dismal, aërial, storm-shrieking, hovering, fiend-like hosts, constantly in motion, shrieking and howling as they hovered about them (*i.e.*, about both armies) in every direction, to cow and dismay soft youths, but to invigorate and mightily rouse champions and warriors, so that—from the uproar of the battle —the frantic pranks of the demons, and the clashing of arms, the sound of the heavy blows reverberating on the points of heroic spears and keen edges of swords, and the warlike borders of broad shields, the noble hero Suibhne (Sweeny) was filled and intoxicated with tremor, panic, dismay, fickleness, unsteadiness, fear, flightiness, giddiness, terror, and imbecility, so that there was not a joint or a member of him from foot to head which was not converted into a confused, shaking mass, from the effect of fear and the panic of dismay. His feet trembled, as if incessantly shaken by the force of a stream; his arms and various edged weapons fell from him, the power of the hands being enfeebled and relaxed around him, and rendered incapable of holding them. The inlets of hearing were expanded and quickened by the horrors of lunacy; the vigour of his brain in the cavities of his head was destroyed by the clamour of the conflict; his heart shrunk within him, with the panic of dismay; his speech became faltering, from the giddiness of imbecility; his very soul fluttered with hallucinations, and with many and various phantasms; for that (the soul) was the root and true basis of fear itself. He might be compared on this occasion to a salmon in a weir, or to a bird after being caught in the straight prison of a crib. But the person to whom these horrid phantasms, and dire symptoms of flight and fleeing, presented themselves, had never before been a coward or a lunatic devoid of valour; but he was thus confounded because he had been cursed by St. Ronan, and denounced by the great Saints of Erin, because he had violated their guarantee, and slain an ecclesiastical student of their people over their consecrated trench,—that is, a pure clear-bottomed spring, over which the shrine and communion of the Lord was placed for the nobles and arch-chieftains of Erin, and for all *the people* in general, before the commencement of the battle.

"With respect to Sweeny, let us treat of him yet another while. When he was seized with this frantic fit he made a supple, very light leap, and where he alighted was on the fine boss of the shield of the hero next him; and he made a second leap, and perched on the vertex of the crest of the helmet of the same hero, who, however, did not feel him, though the chair on which he rested was an uneasy one. Wherefore he came to an imbecile, irrational determination, —namely, to turn his back on mankind, and to herd with deer, run along with the showers, and flee with the birds, and to feast in wildernesses. Accordingly he made a third, active, very light leap, and perched on the top of a sacred tree which grew on the smooth surface of the plain, in which tree the inferior people and the debilitated of the men of Erin were seated, looking on at the battle. These screamed at him from every direction, as they saw him, to press and drive him into the same battle again, and he, in consequence, made three furious bounces to shun the battle, but it happened that—*instead* of avoiding it—he went back into the same field of conflict, through the giddiness and imbecility of his hallucination; but it was not the earth he reached, but alighted on the shoulders of men and the tops of their helmets.

"In this manner the attention and vigilance of all in general were fixed on Suibhne, so that the conversation of the heroes among each other was—'Let not,' said they, 'let not the man with the wonderful gold-embroidered tunic pass from you without capture and revenge.' He had the tunic of the monarch of the grandson of Ainmire upon him on that day, which had been presented by Domhnall to Congal, and by Congal to Suibhne, as he himself testifies in another place.

'It was the saying of everyone
Of the valiant beautiful host,
Permit not to go from you to the dense shrubbery
The man with the goodly tunic.'

His giddiness and hallucination of imbecility became greater in consequence of all having thus recognised him, and he continued in this terrible confusion until a hard, quick, shower of hailstones—an omen of slaughter to the men of Erin—began to fall; and with this shower he passed away, like every bird of prey, as Suibhne said, in another place.

'This was my first run.
Rapid was the flight;
The shot of the javelin expired
For me with the shower.'

And it was by lunacy and imbecility he determined his counsels as long as he lived."

Such is one of the most famous accounts of the Irish on the subject. It is in accordance with a notion there that lunatics lose their weight, at least, to a great extent; very different from the Red Indian who said—"When I am angry I weigh a ton." Can that Member of Parliament have had these stories in his head when he said—"A man cannot be in two places at once, unless he is a bird?" When St. Columba saw the angel flying through the air like a bird, to save a man from falling, the time spent was very short, and the messenger was almost "at once" there, at the end and the beginning.

If we take the ecclesiastical side we hear of many similar flights, such as the instantaneous passage from place to place in Ireland; but probably the Irish fancy could not be brought to tell a simple tale of rising into the air beyond reach : it immediately raised the whole question a few stages higher, and made the immeasurable and incomprehensible out of the strictly limited.

In the notes to the "Life of St. Columba," by Dr. Reeves, as published by Edmondston and Douglas, we have an account of two Saints fighting with demons for the soul of King Brandubh. "Brandubh was killed on the morrow, and demons carried his soul into the air, and Maedhog, abbot of Ferns, heard the wail of his soul as it was undergoing pain, while he was with the reapers, and he went into the air and began to battle with the demons. And they passed over Hy (Iona); and Columbkille heard them whilst he was writing, and he stuck the style into his cloak and went to battle to the aid of Maedhog, in defence of Brandubh's soul. And the battle passed over Rome, and the style fell out of Columbkille's cloak, and dropt in front of Gregory, who took it up in his hand. Columbkille followed the soul of Brandubh to heaven. When he reached it the congregation of heaven were singing *Te decet, &c.* Columbkille did the same as the people of heaven, and they brought Brandubh's soul back to his body again. Columbkille tarried with Gregory, and brought away Gregory's brooch with him,—and it is the hereditary brooch to this day in Iona,—and he left his style with Gregory."

Here we find the utmost exaggeration that one can imagine,—at the same time with limited ideas ; but let us go back to Sweeny, whose story, after that of Maedhog and St. Columba's, is like a common occurrence. Let us hear the remark, for a moment, of a modern student of those times. In Note 63 to "Congal," a poem by Dr. Samuel Ferguson, Q.C., we have—"That men should lose their reason in the horrors of hand to hand fighting seems not incredible; but the notion of this kind of phrenetic excitement being attended with a loss of material weight, is, so far as I know, peculiar to Celtic tradition. We have it in the legends of both islands : Merlin, at Arderidd ; Sweeny, in this incident of the battle of Moyra ; another in the battle of Ventry, and various other instances referred to in the extract following." This is from the "Quarterly Review" of April, 1868 :—"The idea, which had a strong and persistent hold of the Gaelic mind, was that excess of mental excitement—especially of the passion of fear—destroyed or counteracted the influence of gravitation. Excessive exaltation of mind arising from religious enthusiasm, whether Christian or Pagan, is alleged to have the same effect.

During the time of those epidemics called Witchcraft, in the Middle Ages, the trial by water was grounded on the assumption that a person demoniacally possessed could not sink; and the test of scales and weights was used, prompted by the same popular conviction. The Irish seem to have confined their belief of the capacity of the human body to receive some influence counteractive of gravitation to the case of lunatics, especially when the phrensy was induced by fear. Thus, in the battle of Moyrath, Sweeny, the young king of Dalaraidh, who has provoked the curse of an angry ecclesiastic, is visited on the battle-field with an excess of terror which deprives him at once of his senses and his bodily weight, and he rises like a leaf or a waif in the air over the heads and helmets of those around him, and so flits rather than—in the figurative sense—flies from the field. The battle of Moyrath is said to have been fought in 637 A.D., and certainly the belief may well be accepted as having been—even at that early period—settled in the popular mind; for here, A.D. 718, in the "Chronicle of the Monks of Clonmacnoise," we find the record of a furious battle between the northern and southern Hy-Niall, at the Hill of Allen, in Kildare, where—together with a great number of kings and chiefs, whose names are given—there perished *novem volatiles, i.e., gealta, i.e.*, nine volatiles or flying phrenetics, who, in the words of the old translator, 'flyed in the ayre as if they were winged fowle.' As usual, the visitation is consequent on the curse of a religious person,—in this case a leper, whose cow had been seized by some of the combatants." Dr. Ferguson continues, "One is naturally led to ask, Can there be such a body of tradition without such a foundation in fact; and, if so, is it a contradiction or negation of the Newtonian law that we are to admit as a solution?"

We may easily answer that, even if we found men floating, Newtonian gravitation would not be interfered with. Balloons, even when rising, still gravitate, and magnets—even rising from the ground by the attraction of another magnet—are not out of the influence of gravitation. It is not at all known whether mental excitement affects weight, but even if it does it can only be to a small extent, and the physical reasoning is quite against it; at the same time, neither the reasoning nor the experiments have been made with such care as men would give had they the slightest hope of gaining any knowledge. The connection of the body with magnetism and electricity is a most obscure subject: there evidently is much to learn; there are movements going on, but in a closed circle, where we cannot observe.

Still, allowing that there was a difference,—say that a man in violent excitement weighed half a pound less,—it would not bring us much nearer these wonderful accounts of flying, and so they must, it is to be feared, go from us like the man in the "Arabian Nights," who sat on a carpet and wished, and was anywhere; or the flying horse of wood, who was moved by the mechanism of a peg; or the older Pegasus himself. St. Columba is the foundation of legends told by men, so rude that the simple abbott was to them as an angel fit for heaven, and able to go when he pleased.

After viewing these ancient narrations we must come to this conclusion,—that there may possibly be something which caused the idea other than mere fancy, but whether that something were merely what we are accustomed to call light-headedness when it is unpleasant, or buoyancy when we feel bright, must, after all, be settled by modern enquiry. We are not wiser till we gain certainty, but we sometimes learn tendencies from indefinite narrations which it is profitable to trace, and it is extremely interesting to think that even the wildest brain has some little germ round which it makes its romance. It is the soul seeking to enlarge its empire of truth, sometimes doing it by empty boasting instead of by conquest. The question remains, What is the conquest in this case, and is there any?

IV. THE HISTORY OF OUR EARTH.*

ONE of the facts which first forced themselves upon the attention of the earlier geologists was the presence, in temperate and arctic regions, of the fossilised remains of animal and vegetable species, such as are now peculiar to tropical or sub-tropical climates. For a time this remarkable phenomenon was referred to the consequences of the Noachian deluge, which was assumed to have brought tree-ferns from tropical forests, and deposited them to form the coal-beds of England and of Nova Scotia. So soon as this crude hypothesis was found to be wholly untenable, it was felt that the climate of the earth must have

* Climate and Time in their Geological Relations: a Theory of Secular Changes of the Earth's Climate. By JAMES CROLL (of H.M. Geological Survey of Scotland). London: Daldy, Isbister, and Co.

undergone important secular alterations. For a considerable time it was supposed that during the Cambrian, Silurian, and other pristine periods, the climate of our globe was much hotter than at present, and that it has ever since been gradually declining, as the internal regions of the earth have become progressively colder. Modern physical research, however,* has shown that the earth's general climate could not have been appreciably modified by internal heat for more than 10,000 years after its surface began to solidify, and that the present influence of internal heat upon the temperature does not exceed the 1-75th of a degree. Further, geological evidence goes to prove that—so far from a uniform or progressive decline of temperature having taken place from the earliest ages—there have been periods when the climate was very much colder than at present. There have been, in fact, not one, but several "Glacial periods,"—times when an arctic condition of climate prevailed in the British islands, and when they, along with the greater part of the European continent, were buried under ice. Between these periods, again, there intervened others, when Western Europe enjoyed a semi-tropical climate, whilst even Greenland and Nova Zembla were free from ice, and covered with luxuriant vegetation.

It must be remembered that a Glacial epoch is not merely a season of unusual cold. Its characteristic feature is the "glaciation" of a considerable portion of the land,—in other words, its becoming coated with a continuous mass of glacier-ice. This phenomenon cannot, obviously, arise from any general decrease of the earth's temperature. Were the sun to be extinguished, evaporation from the surface of the globe would cease, and all downfall, either of rain or snow, would cease likewise. Though, therefore, in such an assumed case the ocean would be frozen down to its very bed, the land would remain bare of ice. For glaciation it is necessary that evaporation should go on unhindered in some warm parts of the globe, and that the moisture thus volatilised should be transferred to colder regions, and there precipitated. This consideration at once disposes of a variety of theories concerning the probable cause of a Glacial epoch, as we shall see hereafter, and proves—what no strictly geological evidence could show—that no such epoch could simultaneously affect both hemispheres.

This premised, we may proceed to examine some of the more important of the numerous hypotheses put forward to account for these alternating epochs of heat and cold.

* Sir William Thomson, Phil. Mag., January, 1863.

Poisson supposes that the earth may have passed, in its earlier history, through regions of space respectively hotter and colder than the part where she now revolves. On this Mr. Croll remarks, that though there may be a difference in the quantity of force passing through different parts of space in the form of heat, yet we cannot imagine space in itself to be either cold or hot. During the hot periods, therefore, the earth must, on this supposition, have passed near to or among other sources of heat and light in addition to the sun. But masses capable of thus seriously affecting our climate could not do other than greatly perturb the mechanism of the solar system. The orbits of the planets might therefore be expected to give some evidence of such an occurrence.

Others have assumed that the sun is a variable star, its energies waxing and waning at regular or irregular periods. Against this it may be urged—which, indeed, applies also to the hypothesis of Poisson—that a general reduction of temperature over the whole globe could not produce the characteristic phenomena of glaciation.

A variation in the obliquity of the ecliptic is sometimes put forward in explanation of former fluctuations in the earth's temperature. But Mr. Croll declares that "it can be shown from celestial mechanics that the variations in the obliquity of the ecliptic must always have been so small that they could not materially affect the climatic conditions of the globe; and even admitting that the obliquity could vary to an indefinite extent, it can be shown that no increase or decrease, however great, could possibly account for either the Glacial epoch or a warm temperate condition of climate in polar regions."—(Page 8). On this point Mr. Croll is at issue with Mr. Belt, who maintains ("Quarterly Journal of Science," October, 1874) that the Glacial epoch resulted from a great increase in the obliquity of the ecliptic. "There exist," he says, "glacial conditions at present round the poles, due primarily to the obliquity of the ecliptic." "Were the earth's axis perpendicular to the plane of its orbit, spring would reign round the arctic circle," and "under such circumstances the piling up of snow, or even its production at the sea-level, would be impossible, excepting, perhaps, in the immediate neighbourhood of the poles, where the rays of the sun would have but little heating power, from its small altitude." In reply, Mr. Croll remarks that, were the earth's axis to become perpendicular to the plane of its orbit, the quantity of heat received by the polar regions would be far less than it is now. It is well known that at present at the equinoxes, when day and night are equal, snow, and not rain, prevails in the arctic regions, and can we suppose it would be otherwise in the case under consideration? How, we may well ask, could these regions, deprived of their summer, get rid of their snow and ice?" The circumstances which Mr. Belt considers that physical astronomers have left out of account in calculating the variability of the ecliptic, Mr. Croll does not think capable of materially affecting the question at issue. But in the chapter in which he criticises the views of Mr. Belt, Mr. Croll makes certain admissions which very materially qualify his declaration which we have just quoted. He tells us (p. 398) that the "conclusion generally come to by geologists and physicists" is that no great effect can be ascribed to a change in the obliquity of the ecliptic, but that, after giving special attention to the matter, he has been "led to the very opposite conclusion!" "It is quite true," he proceeds, "that the changes in the obliquity of the Equator cannot sensibly affect the climate of temperate regions, but it will produce a slight change in the climate of tropical latitudes, and a *very considerable effect on that of polar regions, especially at the poles themselves*." An increase of obliquity from 23° 28′ to the known maximum 24° 50′ 34″ would cause the poles to receive 1-18th more solar heat than they do at present, and, all other things being equal, would produce "a rise in the mean annual temperature equal to 14° or 15°, raising the temperature of the poles to that now prevailing at latitude 76°." But as the polar regions are covered with snow and ice, this extra heat would have little effect in raising the temperature, and would mainly serve to increase by 1-18th the total yearly amount of ice melted at the poles. Thus the maximum of obliquity, if coinciding with other phenomena to which we shall refer in expounding Mr. Croll's own theory, would have a decided effect in producing the warm interglacial periods.

Let us now examine the theory of Sir Charles Lyell. This great geologist contended that the extraordinary climatic changes of which the "stone book" bears record were due to differences in the distribution of land and water. Were the land all accumulated in high latitudes, and were the intertropical regions occupied exclusively by water, the general temperature of the earth would be lowered sufficiently to account for the glacial period. Conversely, were the land collected within or near the tropics, and were the polar regions occupied by sea, the temperature of the globe would be as strikingly raised. This doctrine

Mr. Croll repudiates. He considers that it "does not duly take into account the prodigious influence exerted on climate by means of the heat conveyed from equatorial to temperate and polar regions by means of ocean currents." Were it not for this heat he maintains that "the thermal condition of the globe would be totally different from what it is at present," and he declares, further, that "the effect of placing all the land along the Equator would be diametrically the opposite of that which Sir Charles supposes." Into this argument it will be necessary for us to enter. The more land is placed at the Equator the more the possibility of conveying the sun's heat from tropical regions by means of ocean-currents is reduced. Heat could, then, only be transferred to the colder regions of the world by means of the upper currents of the trades, since the heat conveyed along the earth's solid crust by conduction is obviously insignificant. But these upper currents, or "anti-trades," are not well adapted for conveying heat. Even at the Equator the upper currents are nowhere below the snow-line, and consequently they play in a region whose mean temperature is below the freezing-point. If warm they would, as a matter of course, raise the snow-line above their own level. The heat carried up by the ascending air-currents at the Equator is not transferred to higher latitudes to be there employed in warming the earth, but is thrown off into the cold regions of space above. The upward current is really one of the most effectual means which the earth possesses of wasting the heat derived from the sun. Consequently, of all places, here ought to be collected the substance best adapted for preventing this dissipation of the earth's heat into space. Now, of all known substances water seems to possess this quality to the greatest extent, and besides—being a fluid—it is adapted, by means of currents, to convey to every region of the earth the heat which it receives from the sun.

Without denying the force of these considerations, it still appears to us that there are certain circumstances which speak in favour of Lyell's supposition. The average temperature of Western and Southern Europe is admittedly higher, latitude for latitude, than that of Asia and North America. But Europe is precisely that portion of the globe which has in its proximity to the south the greatest amount of intertropical land,—to wit, the wide expanse of Africa,—and towards the north the least amount of frigid land. America becomes narrower as we approach the torrid zone, and widens out as we recede towards the pole. Again, hot winds, blowing along the surface of the land, and not depending upon oceanic currents, are not unknown. On the south coast of Australia, a hot north wind, blowing from the heated interior of that continent, is common. In the South of Europe and in South-western Asia warm winds are experienced which cannot be fairly ascribed to the Gulf-stream. On the other hand, the greatest climatic scourge of Western Europe is the north-easterly wind blowing from Siberia and Russia. In America, the "norther"—which sweeps "sword in hand" from the polar regions down to Texas and Mexico—is likewise a land-wind. These facts certainly lend some countenance to the view that the climate of any locality in the temperate zone is lowered by land lying between such place and the Pole, and raised by land lying between such place and the Equator.

Without, however, going further into this question, we can scarcely assume that the land preponderated alternately in the frigid and in the torrid zone for every alternation of hot and glacial periods that the world has experienced. Part of the evidence of a former milder epoch is the discovery of large trees, *in situ*, in Greenland, Spitzbergen, and similar regions. The comparative warmth of the interglacial periods cannot, therefore, be due to the absence of land in high latitudes.

It has been ingeniously suggested that a warm epoch might be produced by an alteration in the composition of the atmosphere. Carbonic acid gas, whilst perfectly pervious to heat-rays emanating from a body of high temperature, such as the sun, is almost impassable for heat-rays of low tension given off by non-luminous bodies. Did our atmosphere contain a few per cents of this gas, the earth would be warmed by the sun's rays, during the day, precisely as at present; but at night the loss of heat by radiation would be practically annulled, and the earth consequently would lose little or nothing of the heat absorbed during the day. But though this theory might account for a single warm epoch, it cannot explain a succession of intensely cold periods, separated by a series of warm intervals.

We must now consider Mr. Croll's views on the cause of the Glacial epochs,—a theory which has a certain resemblance to that of Adhémar, if we strip the latter of certain of its exaggerations.

There are two circumstances which influence the relative position of the sun and the earth, and which, to a very great extent, affect the climate of the latter. These are the precession of the equinoxes and the periodical changes in the eccentricity of the earth's orbit. In the northern hemisphere, at present, winter occurs when the earth is nearest to the sun. As at that period she travels in her orbit with the greatest speed, it results that our winter half-year—*i.e.*, the interval between the autumnal and the vernal equinox—is nearly eight days shorter than the summer half-year. In the southern hemisphere the conditions are, of course, exactly reversed. In virtue of the precession of the equinoxes, however, times have been—and will be again—when the northern hemisphere will have its winter when the earth is in aphelion, and its summer when she is in perihelion, and when it, in turn, will have a longer winter and a shorter summer. But the orbit of the earth is also undergoing a regular series of changes. Its "eccentricity is at present diminishing, and will continue to do so during 23,980 years from the year 1800 A.D." After this time it will again gradually increase. The earth's distance from the sun, when in perihelion, is at present 89,000,000 miles. But when the eccentricity of her orbit is at its superior limit it will have fallen to 84,000,000, whilst her distance when in aphelion will be no less than 98,000,000. Hence, when the eccentricity is greatest, that hemisphere which has its winter solstice in aphelion will have its winter longer than its summer, not by eight, but by thirty-six days. The direct heat of the sun would be one-fifth less during that season than it is at present. Hence there would be greater facilities for the accumulation of snow and ice, and less opportunity for their disappearance during the summer. Now there is perhaps no effect which reacts upon and intensifies its cause so decidedly as do snow and ice. These substances, when once accumulated, lower the temperature in various ways. No matter what the intensity of the sun's rays may be, the temperature of ice and snow can never rise above 32° F. "In Greenland," as Mr. Croll reminds us, "a country covered with snow and ice, the pitch has been seen to melt on the side of a ship exposed to the direct rays of the sun, while at the same time the surrounding air was far below the freezing-point." The atmosphere is chilled by contact with the snow-covered ground, and is not warmed by the radiation from the sun. Again, the greater part of the rays which fall on snow and ice are reflected back into space and wasted. Those which are not reflected away cannot raise the temperature, since they are consumed in the mechanical work of melting the ice. Further, snow and ice chill the air, and condense its vapours in the form of thick fogs, and thus effectually screen themselves against the rays of the sun.

Now we are well aware that at present the southern hemisphere is colder than the northern. "Sandwich Land, which is in the same latitude as the North of Scotland, is covered with ice and snow the entire summer; and in the island of South Georgia, which is in the same parallel as the centre of England, the perpetual snow descends to the very sea-beach. In the Straits of Magellan, lat. 53° S., the direct heat of the sun ought to be as great as that in the centre of England. Yet in the midst of summer, when the day was eighteen hours in length, the thermometer seldom rose above 42° to 44°, and never above 51° F." Icebergs 1000 feet in height have been sighted in latitudes as low as 37° S.

If we now imagine that the earth's orbit were at present about or near its limit of maximum eccentricity, the southern hemisphere, having its winter solstice in aphelion, would have a summer too short, not by eight, but by thirty-six days, and would now be suffering all the rigours of a Glacial epoch. Great part of Australia, New Zealand, South Africa, and of temperate South America, would be overlaid with "thick-ribbed" ice. The region of highest temperature, instead of lying, as now, near the Equator, would be driven northwards as far, perhaps, as the tropic of Cancer, whilst the southern half of the torrid zone would be barely habitable. Meantime the condition of the northern hemisphere would be just the reverse. Its winter half year would be thirty-six days shorter than the summer. Thus the portion of the year during which snow and ice could accumulate would be largely abridged. Every season would see a decrease in the circumpolar ice-mass, and a corresponding quantity of the sun's rays, no longer wasted in thawing snow, would be available for raising the general temperature of the arctic regions.

A natural result would be that trees such as now flourish in Central Europe, would grow in Greenland and Spitzbergen —as has been the case aforetime—whilst Western Europe would display a vegetation of a semi-tropical character. A great period of this kind, when a high eccentricity of the earth's orbit will produce Glacial epochs alternately in the northern and southern hemispheres, awaits us in the remote future. "It consists of three maxima, one hundred thousand years apart. These will occur at the periods 800,000, 900,000, and 1,000,000 years to come."

It is interesting to examine how far this view of a Glacial epoch, successively invading either hemisphere, harmonises with certain suggestive speculations put forward by Mr.

Belt in his delightful "Naturalist in Nicaragua." * This eminent investigator urges that in a Glacial epoch a vast proportion of water must have been piled up upon the continents in the shape of ice and snow; that the level of the ocean must have been thus greatly lowered, and that wide territories, now submerged, must have been the homes of man, and of organic life in general. By this assumption he explains certain points in the migrations and distribution of plants and animals, and throws a light upon the deluges of the ancient world, and upon the myth of Atlantis.

According to Mr. Croll, a vast amount of water would, indeed, be accumulated in the solid form in the glaciated hemisphere. He considers that even now the "antarctic ice-cap" extends from the pole down to lat. 70° S., being 2800 miles in diameter. The whole of this region, he concludes, is covered "with a continuous sheet of ice gradually thickening inwards from its edges to its centre. A slope of one degree, continued for 1400 miles, will give *twenty-four miles* as the thickness of the ice at the pole. But suppose the slope of the upper surface of the cap to be only one-half this amount, viz., a half degree—and we have no evidence that a slope so small would be sufficient to discharge the ice—still we have twelve miles as the thickness of the cap at the pole." As there are in the southern ocean icebergs above a mile in thickness, the great antarctic ice-cap must be in some places over a mile in thickness at its very edge! Were one mile of ice melted off the whole extent of this southern cap, it would raise the general level of the ocean 200 feet!

If the ice-mass at the South Pole is at present so enormous, what must the amount have been surrounding the pole of a glaciated hemisphere? Mr. Croll does not, however, consider that the total amount of ice upon the earth's surface during a Glacial epoch could be greatly in excess of what it is at present. He holds that what is now, with some approach to equality, distributed between both poles, was then accumulated around one. The effect of this upon the general level of the ocean would depend not so much upon the direct decrease of water owing to its being converted into ice, as to an indirect action. Remove all the ice from one pole and transfer it to the other, and you affect the centre of gravity of the globe. Two miles of ice withdrawn from the southern circumpolar continent would displace the earth's centre of gravity by

* Quarterly Journal of Science, vol. iv., p. 320.

190 feet. If one-half of this mass were further added to that existing at the North Pole, an additional displacement of 95 feet would ensue, making a total of 285 feet. The immediate result of this would be a rise of the sea level in the northern hemisphere—which in the latitude of Edinburgh would amount to 234 feet—and a corresponding fall of the sea level and laying bare of lands now submerged in the southern hemisphere. The removal of 12 miles of ice from the South Pole would effect in the latitude of Edinburgh a rise of the sea to the height of 800 to 1000 feet above its present level. This is on the supposition that the interior of the earth is solid to the centre. If it be fluid, the displacement of the centre of gravity and the consequent modification of the sea level would be much greater.

Both Mr. Croll and Mr. Belt, then, agree that the occurrence of a Glacial epoch must greatly affect the distribution of land and water, and might easily produce cataclysms,—in case, for instance, that ice-masses melted away very rapidly, and especially if they disappeared more quickly in one locality than they formed in another. Mr. Croll—though he does not enter at length into this matter—admits that the withdrawal of the water from shallow seas would connect lands now isolated, and allow of the migration of plants and animals from one continent to another, or from continents to what are now islands. The depth of Behring's Straits does not exceed 30 fathoms. A lowering of the ocean level in the northern hemisphere to the extent of 200 feet would therefore connect the Old and New Worlds, and allow of an interchange both between their plants and their animals. Such a lowering could only occur when the southern hemisphere was glaciated, and the northern was enjoying its warm interglacial period. Consequently organic forms, now only found in the temperate regions of Asia, would press on into Siberia, cross the bridge, and establish themselves in America, where, when the temperature became less genial, they would gradually migrate southwards. Similarly in Europe, during the interglacial period, England would be part and parcel of the Continent, and the beds of the North Sea and the Channel would be dry land. Hence animals of southern regions were enabled to enter Britain, and have left their fossilised bones in proof of their former presence. The existence of closely allied or identical forms in countries now separated by wide seas, has led some naturalists to the assumption of a double origin, whether by special creation or by development—a view for which there is now no necessity. It is a remarkable fact, easily understood on the supposition of such changes of sea level, that the animal population of two neighbouring countries separated by the sea is more evidently unlike in proportion as such sea is deeper. No probable alteration of sea level is likely to have established a bridge between Africa and Madagascar. Consequently we discern in their mammals, and even in their insects, the most remarkable discrepancy. Not one of the monkeys, the cats, or the antelopes, in which Africa is so rich, has been found in its island neighbour. Both territories are rich in Cetonias, but the species of South-Eastern Africa are not the same as those found on the opposite shore of the Channel of Mozambique.

One of the most interesting facts in animal geography is the existence, in southern temperate regions, of forms which belong to the northern temperate, or even the subarctic zone. Conversely a few species from extreme southern regions have reached northern temperate climates. The question has therefore been asked by Mr. Belt and others* how the plants and animals of temperate or arctic climates could cross the Equator in their migrations from one hemisphere to the other? The alternate occurrence of a glacial period in either hemisphere solves the problem, as was, in fact, pointed out by Mr. Darwin. Suppose a cold period prevailing in the northern hemisphere and a warm one in the southern, both at their maximum intensity. The thermal equator, instead of, as at present, approximately coinciding with the geographical equator, would be driven southwards to—or perhaps beyond—the tropic of Capricorn, whilst the northern half of the torrid zone would experience a temperate, or perhaps even arctic climate. That such a state of things has actually prevailed in inter-tropical regions, and that indubitable marks of glaciation exist at comparatively low levels in Central America, has been fully shown by Mr. Belt. The animals and plants of northern extra-tropical regions would then easily manage to reach the geographical equator. When, again, the glacial period of the northern hemisphere began to relax and the thermal Equator returned gradually towards its normal position, these organic forms would take refuge on the mountains, thence to re-descend and continue their southward journey, when the region of greatest heat had passed to the northwards. For a more detailed exposition of this process the reader may consult Darwin's "Origin of Species" (sixth edition, p. 339).

* Quarterly Journal of Science, Oct., 1874.

The differences of view between Mr. Belt and Mr. Croll may be summed up as follows:—The former ascribes the former emergence of regions now under water to the general decrease of the amount of water present in a fluid state, while the latter attributes the same phenomenon to the accumulation of water in the glaciated hemisphere. Mr. Belt considers that low-lying lands might be left bare in the immediate vicinity of glaciated regions—*e.g.* territories now covered by the Caribbean Sea during the glaciation of Central America—and might serve as a refuge for tropical forms of organic life. Mr. Croll, on the contrary, holds that the sea can only recede in the non-glaciated hemisphere, and must of necessity rise in the glaciated above its present level. Finally, as is implied in the foregoing remarks, Mr. Belt assumes—if we do not misunderstand him—that a Glacial epoch might simultaneously invade both hemispheres, whilst if Mr. Croll's hypothesis is well-founded, it is, of necessity, limited to one.

Both these authors consider—and we must admit that in so doing they are warranted by facts—that of the evident alternations of land and sea noticed even by Ovid, many have been caused by a local increase or decrease of the depth of the latter. But neither of them asserts that all such changes are to be ascribed to this cause, or denies the existence of those areas of elevation and of subsidence to which Mr. Darwin draws attention in his "Naturalist's Voyage." The question is one which can be settled by careful observation. If we find that the sea gains upon the land simultaneously throughout the whole of one hemisphere, most markedly in high latitudes, and to a less and less extent as we approach the Equator, we should then be warranted in concluding that the encroachment was due to an increasing accumulation of water in that hemisphere. But if we observe the land in South America gradually rising higher and higher out of the water, whilst in certain of the South Sea islands it is gradually sinking, we should then be justified in seeking the cause in the crust of the earth, and in pronouncing South America an area of elevation and the Pacific Isles an area of subsidence.

Incidentally we have referred to the theory of M. Adhémar. This author seems to have been the first who pointed out the possible displacement of the earth's centre of gravity by means of an ice-cap accumulating on either pole. But he supposes this cap to rest, not upon circumpolar land, but upon the bottom of the sea, and to attain the height not of 12 or 24 miles, as Mr. Croll supposes, but of

60! Each hemisphere, he assumes, is in turn colder and warmer. The sea retires on the warm hemisphere, where the ice-cap is decreasing, and gains ground on the colder hemisphere, where ice is accumulating. In a period of 10,000 years the conditions are reversed, the reversal being not incidentally, but inevitably attended with a universal deluge. The last catastrophe of this kind was the Noachian flood, when the great ice-cap then existing on the North Pole collapsed, and the preponderating amount of water previously collected in the northern hemisphere rushed across the globe to its present position in the south. On this hypothesis the southern ice-cap will continue to increase for about another thousand years from the present date, whilst the southern hemisphere becomes still colder, and the sea gains ground upon the land. At the expiration of that time the process is reversed; the southern ice-cap begins to decrease, and its northern antagonist to preponderate. The maximum amount of water recedes from the southern hemisphere and encroaches upon the land in the north. Finally, when 10,000 years from the days of Noah have expired, and when the winter of the southern hemisphere approaches perihelion, "the ice grows soft and rotten from the accumulated heat, the sea begins to eat into the base of the cap, which is so undermined as to be left standing upon a kind of gigantic pedestal. This disintegrating process goes on till the fatal moment at length arrives, when the whole mass tumbles down into the sea in huge fragments, which become floating icebergs. The attraction of the opposite ice-cap, which has by this time nearly reached its maximum thickness, becomes now predominant. The earth's centre of gravity suddenly crosses the plain of the Equator, dragging the ocean with it, and carrying death and destruction to everything on the surface of the globe."

We need scarcely say that this theory harmonises ill with positively established facts in physics, climatology, geology, and animal geography. We have no evidence that all land animals and plants have been periodically eradicated every 10,000 years. We have proof—almost, if not quite, absolute—that certain parts of the earth's surface at no extraordinary altitude, have not been submerged for a length of time greater than this theory would imply. We have good evidence that the polar caps rest, not upon the bottom of the sea, but upon land, and we consider that in the former case the probability of their melting away and breaking up would be but slender. In short, this theory may be held up as a warning to mathematicians who persist in rearing up imposing superstructures upon imaginary data. That the respective distribution of land and sea in the two hemispheres may have fluctuated greatly from changes in the earth's centre of gravity, and that partial and even extensive deluges may have occurred, is a perfectly reasonable supposition.

V. DIFFICULTIES OF "DARWINISM."

MUCH of the unprofitable ink-shed by which the so-called "Darwinian controversy" has been obscured has sprung from the neglect of a very plain distinction. Evolution — the gradual mutation of organic forms in accordance with definite laws, and the consequent origin of what we call "species"—may be a fact, as the writer is by no means disposed to question. But its truth does not necessarily involve the truth of the doctrine commonly known as "Natural Selection." It may yet be shown that this theory, supplemented by Sexual Selection, will account for all the unsolved mysteries of the organic world. Or it may—and probably will—be found that we require the aid of some law of a higher order, defining the limits and the conditions under which the great task of Evolution is carried out. Or, lastly, it is by no means impossible that the entire theory of Natural Selection may have to be thrown aside. Its future depends simply on its power of explaining facts. So long and so far as it is able to do this, it is our duty to follow it. But if we find it fail us, we must be prepared either to supplement it with some more efficient conception or to abandon it altogether. It is with a view of testing this theory that the following *nuces zoologicæ* are submitted to the world. The writer does not hold that they can be more fully solved on the hypothesis of distinct and special creation than that of Evolution. But the reverse ought to hold good. Unless a new doctrine does us better service than the old one, how is it to be legitimated?

Let us first examine the colouration of animals, and consider in how far it can be accounted for by Natural Selection supplemented by Sexual Selection. In the Mammalia the first point which strikes us is the very limited range of colour which they possess. We question if a single instance of a true prismatic shade can be shown in the whole class. The reds found in certain ruminants—*e.g.*, in domestic cattle—are merely reddish-browns. The yellows and oranges of the cats, the fox, and the jackal, are simply buffs or *cuirs* —"cures" as the English dyers call them. Oken, in his great work, concluded a chapter on the Races of Mankind by asking why there were no green or blue men? His question was received with a somewhat gratuitous amount of ridicule,—certainly the easiest, if not the most satisfactory, way of disposing of it. But it may surely be asked why there are no green or blue mammalian species?* In beasts, again, iridescent colours—so common in other departments of the animal creation—are wanting, save in the golden mole of Siberia. We find no other case where the fur of a beast displays different colours according to the position from which it is viewed. Nor is there any near approach to the metallic brilliance so commonly met with amongst birds, reptiles, and insects. The prevailing garb of beasts may, in short, be characterised as monotonous, flat, and sombre in its tones; nor is the pattern or design in which the colours are arranged much more striking than the shades themselves. The majority of species are either concolourous or irregularly blotched and clouded. Even the zebra, the striped and spotted cats, and the spotted deer display nothing which can be brought into comparison with the elaborate and delicate designs found in birds and insects. Whence, then, comes the difference? Not, surely, from any distinction in the diet of the two classes. There is no article of food used by birds which is not also eaten by some beasts. The hair, fur, and bristles of beasts cannot be said to differ chemically from the feathers of birds, so that we can see no reason why the one material should not display the same colours as the other. The old school of Natural History would cut, rather than untie, the knot, by referring all to the "*sic volo, sic jubeo*" of the Creator; but, without in the least questioning the existence of a superintending Intelligence, we cannot regard such an explanation as scientific: it amounts to little more than the well-known domestic argument —"It is because it is." What, then, can Evolutionism—as at present understood—do towards solving the difficulty? Certain points connected with the colouration of animals it has explained with no small felicity; it has shown that the uniform tawny hue of the lion, resembling the soil of

* Meaning, of course, species with blue or green fur, hair, or bristles. The blueness of the nose in certain baboons can scarcely be taken into account here.

the deserts which he inhabits, screens him from the observation of his intended prey; it has pointed out that the tiger, lurking amidst the perpendicular stems of reeds, flags, and high grasses, is concealed by his vertical stripes. On the other hand, the rosettes and ocellated spots of the leopard, and other truly arboreal cats, are equally well adapted for hiding them amidst the foliage of trees. But surely the concealment of these latter species would have been still more complete had their ground-colour been some shade of green instead of buff, or stone-colour, as we actually find it. If Natural Selection gave them their rosettes, why did it not act further in the same direction, and assimilate them to their haunts, not merely in design, but in colour? There are, also, several species of beasts, frequenting grassy plains, which would have been much less conspicuous, and would have more readily escaped the observation of their enemies, had their colour approximated to that of the surfaces around them.

The zebra is, in colouration and design, very similar to the tiger,—a resemblance which Swainson accounts for by declaring them both eminently sub-typical forms of their respective groups, as shown by their fierce and untameable dispositions. That the stripes of the tiger favour it in escaping the notice of its destined prey, and that they are therefore explicable on the principle of Natural Selection, is admitted. But is the zebra, when grazing, screened in the same manner from the observation of an approaching enemy? He frequents pastures similar in their general character to those sought by the concolourous horse and ass, and by the very imperfectly striped quagga. When feeding, further, he stands so much higher than a crouching tiger that he would be hidden only among very lofty reeds or grasses. Moreover, it may be remembered that a troop of zebras, when pasturing, generally have a sentinel placed in some suitable position, who warns his comrades if he observes anything of a suspicious nature. He has, therefore, less need of a colouration calculated to mislead an enemy.

The pattern of the giraffe somewhat resembles that of the leopard, but it is very questionable in how far it can promote his safety. He browses, indeed, upon the lower branches of trees in the outskirts of woods; but this is surely a very different position from that of the arboreal cats crouched aloft among the dense foliage. Even where his body is partially masked by bushes with which his spotted hide may harmonise, his long neck is very likely to attract attention. We can scarcely consider that either the zebra or the giraffe is a case of "mimetism." The rough resemblance of the former to the royal tiger could have no deterrent effect upon lesser beasts of prey in a country like Africa, where the scourge of India is unknown. The general structure of the giraffe differs far too widely from that of the leopard to admit of the one being mistaken for the other. But we may even feel some doubt whether a concolourous animal is more easily seen than one that is striped, spotted, or clouded. No animal presents such a vast extent of self-coloured surface as does the elephant; yet all eastern sportsmen admit that, whether escaping or acting on the offensive, he has a wonderful power of remaining unseen at remarkably short distances. We must, therefore, I think, admit that in the colouration of the Mammalia there are points which cannot be explained by the doctrine of Natural Selection.

Passing to the great class of birds, we find, as regards colour, the most complete difference. In place of the poverty of tone and design characterising the Mammalia, we have here, probably, every conceivable colour, and patterns the most elaborate. Why such meagreness on the one hand, and such prodigality on the other? Is it in virtue of Natural Selection? We are certainly warranted in assuming that a coat of many colours may draw upon either bird or beast the observation of its enemies. We may think that the bird, having the power of flight, can more readily escape from its pursuers, and can thus—if we may use the expression—afford to be more showy. But in many gaily clad birds the power of flight is so limited as to afford them small protection against carnivorous beasts and reptiles. Birds, too, are more exposed to the attacks of eagles, hawks, and other predatory members of their own class, than are any—save the very smallest—beasts. So that we may well doubt if the feathered species do really enjoy any greater immunity from danger than do Mammalia. Nor must we overlook the fact that the winged mammals, the bats, are sufficiently sombre in their attire. Is it Sexual Selection? We may suppose that hen birds select mates of the richest plumage, whilst female beasts display no such preference. But then comes the question—Why is there such a difference of taste between the two great classes of warm-blooded vertebrates? To this question it is difficult to return any answer which is not substantially a reference to some inscrutable decree of the Creator. But there is the further difficulty that all birds do not display splendid hues and exquisite designs; many species are as dull and plain as the beasts of the field.

It is impossible to deny that the plumage of the Raptores bears, in colour and design, a manifest resemblance to the fur of the Felidæ. There are the same blacks and deep browns, on grey, buff, or stone-coloured grounds; there is the same general tendency to bars and spots. Now whilst admitting that patterns of this nature may aid in the concealment of cats lurking in a forest, they can have no such beneficial effect in hawks soaring in the open air, and seen in relief against the sky; nor can much stress be laid on the fact that this spotted plumage may withdraw female birds of prey from observation whilst sitting on their eggs. The nest is generally placed in situations not easily accessible to any creature except birds; and few of these, indeed, will care to disturb a hawk or eagle in the act of incubation.

Passing over the reptiles, amphibians, and fishes, let us next look at the distribution of colour among insects. Here we are met by puzzles not a few. Natural Selection, Sexual Selection, and Mimetism have indeed thrown a welcome—and often a startling—light upon many difficulties, but they have left at least as many unsolved. In butterflies, design may be said to have reached its summit. But to what end? It is impossible to conceive that all these exquisite and elaborate patterns—the admiration and envy of human artists—can serve in any way to promote the safety of the insect, or to secure its well-being; nor can we think that they are needed for the mutual attraction of the sexes. If it were really necessary for either purpose we might ask again, Why have butterflies—or at least Lepidoptera in general—the monopoly of this kind of beauty? For it is singular that, though colours even more brilliant occur among beetles and certain Heteroptera, and though there is in a lesser extent brilliance of hue among the remaining insect orders, down to the very Diptera,—the pariahs of annulose life,—yet in design the Lepidoptera stand in all Nature alone and unapproachable.

The colouration of beetles has also its difficulties. Among them we find metallic and iridescent shades in abundance, and in a perfection certainly rivalling, if not surpassing, those of the most splendid birds and butterflies. But the following tones are rare, if not altogether wanting:—Pinks, roses, lilacs, peaches, pale blues, pale greens, lavenders. On the other hand, deep reds, purples, violets, golden and coppery greens, deep blues (verging both to the violet and the green), yellows, and oranges abound. Elaborate patterns are rare, the nearest approach to the beauty of the butterflies in this direction being found among the Cicindelidæ, the Longicornes, and the Curculionidæ. But the colours in many species, even where not iridescent, shade gradually away from one tone to another.

It is scarcely possible, with any degree of plausibility, to harmonise these characteristic features with the laws of Natural or of Sexual Selection. In some cases the colours of a species seem exactly calculated to betray it to its enemies, or make known its approach to its intended prey. Take, for instance, *Calosoma sycophanta*. You see it running about, in the full light of day, on the bare, sandy soil of a pine-forest, or scaling the trunks of the trees in search of booty. Its broad elytra flash with golden-green, changing to an orange-scarlet, to which its blue-black thorax forms a striking contrast. It would be difficult to select an arrangement of colour which, under the circumstances, would be more conspicuous. Or, again, if a mass of the dung of the horse or cow be turned over, it will generally be found full of *Aphodii*. Some of these have reddish, some brownish, and others grey elytra,—all strongly reminding the observer of half-digested seeds. These colours must, therefore, attract the attention of domestic fowls and other birds which love to scratch and peck in dung. Yet these very species which thus attract the attention of their enemies are the most common. All these facts do not accord well with the law of Natural Selection. How, again, are we to explain the colours of the *Geotrupes* and *Onthophagi* of this country, and especially of the *Phanæi* of Mexico and Central America? These creatures fly by night, and in the day lurk chiefly in dung, or tunnel into the earth beneath it; yet they display a rich array of golden-greens, violets, purples, and bronzes.

We have already referred to the rarity or total absence of certain colours among beetles; yet to some of them—as the flower-haunting *Cetonias*—these very hues would seem desirable. Pink, lilac, rose, and peach-coloured flowers are numerous; why, then, are these colours so rare amongst honey-loving insects?

The predominant hues among the Hymenoptera are yellow, orange, brown, and black, or, on the other hand, deep metallic blue and purple. Why this limitation? These colours are scarcely the best adapted for the concealment of the insects amongst flowers and leaves.

But it is not merely in colour and design that animals exhibit characteristics not readily explained on the principles of Natural Selection and Sexual Selection. There are also peculiarities of structure. We must here especially call attention to the Dynastidæ. These huge beetles are remarkable for protuberances which in colloquial phrase are known as horns. The parts in question differ most essentially from the horns of Mammalia: they are integral portions of the skeleton, and, instead of being confined to the head, they are in many—if not in most—cases developments of the thorax, a part analogous to the shoulders and upper portion of the back in vertebrate animals. Hence they are incapable of motion, except in common with the entire body of the insect: they are so far sexual in character that they are almost confined to the males, and they vary exceedingly in magnitude among individual specimens of one species found in the same locality. If their presence is to be explained by Natural Selection they must have some reference to the habits and functions of the insect. But to what habits or functions? They are evidently not organs of sensation; as little can they serve in procuring food. For weapons—either against enemies of the same or of different species—they are not adapted, in some cases by reason of their position and direction, and in all by their very imperfect mobility. As to locomotion, they must be rather an impediment than an aid. Imagine an insect boring into mould or decayed wood, creeping under fallen trees, or through twigs and the roots of herbage, whilst encumbered with horns like those of *Hoplites Pan*, *Dynastes dichotomus*, or the *Golofæ*. In short, we are unable to conceive what purpose they serve. We find promiscuously individuals with highly developed horns, and others in which these parts are almost obsolete. Nor can we say that either kind appears to have any advantage over the other. If, *e.g.*, we examine a plot of ground in Central or Southern Europe where the spent bark from tanneries has been deposited, we shall find male specimens of *Oryctes nasicornis* with large and with small horns, respectively in about equal numbers. Now if any decided benefit accrued from either conformation we might expect that it would rapidly preponderate, and that the other form would, in the course of Natural Selection, have become rare, or be eliminated altogether. Similarly, if the large-horned males were regarded with especial favour by the females, we should anticipate that the small-horned type would gradually disappear by the process of Sexual Selection.

MacLeay, Swainson, and their followers, explain the horns of the Dynastidæ, or indeed of the entire Saprophagous Lamellicornes, on the principle that they represent the Ruminant Mammalia, and that, as the one group is horned, the other—whose function is to bury the excrements of the former—must be horned likewise. The analogy, however, is somewhat strained. The horns in one case differ signally from the horns in the other. The Dynastidæ—the most decidedly cornuted of all the Lamellicornes—do not remove or burrow in dung, and their metropolis is a region remarkably poor in ruminants. Still the idea of certain structural types reappearing, in a more or less modified form, in different sections of the organic world, may yet prove fruitful, and should not be entirely lost out of view. How it may best be harmonised with the doctrine of Descent is not a subject for present consideration.

Another difficulty for "Selection" is the existence of trimorphous species. Thus in *Papilio Memnon* there are two distinct forms of females, the one nearly resembling the male, and the other decidedly unlike. A single brood of larvæ, the issue of either kind, comprises at once males and specimens of both kinds of females. The males do not appear to show any exclusive preference for either form. On what principle has this anomaly arisen? Is it the initial step to the formation of two distinct species? If not, what end does it subserve that would not be equally well secured were the females all of one type?

Turning to a totally different point, we find it a general rule that—in the Vertebrata and Articulata—the sexual organs are situate at or near that extremity of the trunk which is farthest from the head. In spiders, however, it is asserted that these organs in the male are placed in one of the legs. Now, that this very peculiar arrangement is an advantage to male spiders we do not dispute. It is nothing uncommon for the female spider to destroy her mate, and hence a conformation which permits of intercourse without the close contact required in other forms of animal life may be beneficial to the male.* But by what process has such a change been brought about? We feel utterly unable to conceive even of the first stages of the needful transitions. It would seem that any male animal in which the generative organs were by some malformation removed from their usual site, even to a small extent, would be placed at a disadvantage, and be less likely to leave posterity than one of the normal structure. If we consider the intermediate positions through which the male organs of the spider must have gradually travelled in the course of myriads of generations, we shall be convinced that whatever advantages their present

* A somewhat similar arrangement prevails in the *Octopus*, which might almost be characterised as a sea-spider, though not ranking among the Articulata.

situation may afford, those points of transit must have rendered fecundation more dangerous and less easy. We should therefore expect that the line of descent in which these variations were occurring would—in the ordinary working of Natural Selection—tend to become extinct.

The distribution in the animal kingdom of the power of secreting poisons requires, also, much additional light. We may distinguish here two stages:—In the lower, poisonous fluids are indeed elaborated, or some of the solids of the body possess venomous properties, but the animal has no special apparatus for injecting them into the tissue of an enemy or of a victim. In the higher, there exist—in addition to the secreting glands and the receptacles for the poison—either hollow fangs, or stings, or some analogous weapon. In neither of these forms does the poison-faculty occur among Mammalia and birds, at least in a normal, healthy condition.* Among reptiles it seems restricted to one particular group, the Ophidians, where it receives its maximum development. Why, then, has no similar power been evolved among warm-blooded animals and among non-ophidian reptiles? Why, again, is the venom of serpents much more intense than is required for the destruction of their prey? The cobra feeds chiefly upon rats, but its bite is amply sufficient to cause the death of a man, a swine, or a sheep, which it is utterly unable to swallow. Nor, after all, can the venom of snakes be of great value to them for defensive purposes, since the bite of few species, if of any, is sufficiently rapid to prevent the bitten animal taking instant revenge. As, therefore, increased activity of venom can have conferred little especial advantage upon its possessors, either in procuring food or for the purpose of self-defence, it is difficult to see how its development can have been effected on the principle of Natural Selection. Still less can we conceive of it as due to Sexual Selection. The power, possessed as it is by both sexes, does not appear calculated to render either more attractive to the other.

Passing over the fishes, in some of which the venom-faculty exists in its higher stage of development, we come to the Articulata. Here true venoms exist in the Hymenoptera, Heteroptera, probably in certain Diptera, as well as in many spiders, scorpions, and centipedes.† On the other

* Is hydrophobia, as occurring among wild Canidæ, such as wolves and jackals, invariably fatal? Its extreme frequency among those species leads us to doubt whether such is the case.

† The bite of the common English centipede, as the writer can testify, is more painful than the sting of a wasp.

hand, the Neuroptera, Orthoptera, Homoptera, and Strepsiptera, are devoid of venom. In certain Coleoptera, as in the Bembidiidæ, in *Cychrus rostratus*, and a few other Carabidæ, formic acid can be ejected either as a liquid or as a vapour; but there is no special weapon for introducing it into any given object. Certain Lepidopterous larvæ have hairs, which if drawn into the lungs, or even brought in contact with the skin, produce painful and dangerous inflammation. But even if this effect is not solely due to the mechanical structure of the hairs, they are not under the control of the larva, and are distributed at random by the wind. There arise, then, the three following questions:—

1. Why is the poison-faculty developed in the particular groups above mentioned, and not in others?
2. Why is the poison-apparatus seated in the head, in bugs, spiders, and centipedes, but in wasps and scorpions in the abdomen or in the tail?
3. Are all the venom-bearing animals members of one and the same line of ascent? That they are seems impossible to be maintained.

Can we construct a pedigree in which all the poisonous groups can be arranged in order of filiation without the intervention of non-poisonous forms? Did some articulate type once exist from which the bugs, the wasps, the centipedes, spiders, and scorpions have all diverged, whilst non-poisonous Articulata sprang from another source? If we take our second question into consideration the matter is further complicated, as we shall then have to seek a separate ancestry for the bugs, centipedes, and spiders on the one hand, and for the wasps and scorpions on the other. If no such lines of descent exist, we have then to explain the independent appearance of this faculty at a number of distinct points in the animal kingdom, and among groups which cannot be shown to require it more than do several others not thus armed. A tiger-beetle, a dragon-fly, or a *Mantis*, able to inflict a poisonous bite, would, in the struggle for existence, have enjoyed a marked advantage over others of its kindred. If, then, the occurrence of the venom-faculty be due to Natural Selection, why is it found just where it at present exists, and not elsewhere? In the Hymenoptera we meet with further difficulties, in the sexual distribution of the poison-faculty, and in the fact that the organ which in some groups is used merely as an ovipositor, serves as a sting in others. In all species where the poison-power lies in the jaws the males and females are armed equally, as is also the case in the scorpion. We must, however, remember that the sting of the latter, though corresponding in function with that of the wasp or bee, is essentially different.

From the poison-faculty we pass to a power widely distributed among the Articulata, and even found in some Mollusca, but totally wanting among vertebrate animals—the power of secreting a textile matter and of forming it into threads and tissues. Most insects possess this power when in the state of larvæ, though in very different degrees of perfection. Among spiders only it exists in perfection in the adult state, and is used as a means of procuring food, or, we might say, as a weapon. Are, then, the silk-giving insects, arachnides, and mollusks* descendants of one common stock, or has it also originated independently in different groups? Here, again, it must be remembered that the spinning organs are not homologous. The spider emits its silk from certain abdominal appendages, whilst all insect larvæ spin from orifices in the face. The limitations of this power are also perplexing. Very useful, or rather necessary to many who possess it, it would have been equally useful to many who do not. What, then, is the reason that Natural Selection has operated just as we find it to have done? To many birds, and to certain arboreal mammalia, the power of spinning would have been of striking advantage. Yet the process which has evolved spinning spiders has failed to produce spinning birds and mammals. Who can solve this riddle?

Yet, again, we find in certain animals the power of phosphorescence—of emitting at will a distinct light. This faculty is, I believe, confined to the Articulata, and appears there in certain detached groups. It is found amongst Coleoptera, as in the well-known glowworm and fire-fly; amongst Homoptera, as in the lantern-fly, and in at least one centipede. But would it not have been equally useful to many other nocturnal animals, who might thus have attracted each other? If the digression is permissible it is curious why nocturnal mammalia are scared and repelled by a light, whilst nocturnal insects are universally attracted. What charm can the candle have for the moth? Nocturnal birds, also, have been known to dash themselves against the windows of lighthouses.

To return. Either all luminous animals are descended from one common stock, or the power of emitting light has been independently elaborated at distinct points, and not elsewhere—a repetition of the difficulty we have already

* Pinna.

encountered as regards the production of poison and of textile matter.

There is a strange phenomenon known as Melanism. In the more southern portions of the globe animal forms have a striking disposition to assume a black colour. This tendency is well marked in the temperate latitudes of South America, in South Africa and Madagascar, in Australia, New Zealand, and New Guinea, where it reaches the Equator. Perhaps the most familiar instance that could be cited is the black swan of Australia. New Zealand has a black parrot, and the high latitudes of South America a black humming-bird. Among insects it is well known that few large groups display a more brilliant general colouration than the Cetoniadæ, or rose-beetles, which have been not inaptly characterised as walking jewels. Yet in South Africa and Madagascar, where they are richly developed, they, too, appear in mourning. The genera *Stenotarsia*, *Liostraca*, *Heteroclita*, *Callipechis*, *Rhinocœta*, *Xiphoscelis*, and not a few others that might be mentioned, have scarcely a species which is not in great part black. Why this should be the case—while groups which elsewhere affect light or brilliant colours should be represented in the regions just mentioned by black members—is an unsolved question. Peculiarity of climate or of soil is out of the question, since the countries in which Melanism has been recognised include every conceivable variety of both, and since they form in these respects no exception to the remainder of the habitable globe. Can Natural Selection be the cause? Take the case of a black swan. Will his opportunities of surviving in the struggle for existence, and of leaving posterity behind him, be greater than if he had been white, like the swans of the northern hemisphere? We see no reason why this should be the case. But supposing, for argument's sake, that the black swan has the advantage, it might then be asked why he had not also been produced and perpetuated in other regions where swans occur. We want to find the law which connects the black swan with Australia, in preference to any other region of the world, and so far, Evolution leaves us as much in the dark as the old doctrine of distinct and special creation.

But Melanism is only one single instance of a general principle. Almost every country sufficiently large and sufficiently rich in animal and vegetable species, has its local characteristics. Place a miscellaneous, but local, assortment of insects before an experienced entomologist, and, though he may know nothing of them, and have never seen one of them before, he will from these characteristics suspect their origin. Now in this there is, to a certain point, nothing difficult to explain in accordance with the doctrine of Descent. Isolate two countries so that the animals and plants of each are perfectly excluded from the other, then, if species do undergo any variation in course of ages, we may naturally expect that the Faunæ and Floræ of the two regions will differ respectively from each other, and that the more decidedly the longer and the more complete has been their separation. As Mr. Wallace tersely puts the matter, "the individuality in the productions of a country is the measure of the time of its isolation." But here arises a difficulty; the variation in each zoological or botanical district is apt to take some one specific direction. Thus there are in America, if we remember rightly, some thirty vegetable species which all differ from their nearest representatives on the eastern side of the Atlantic in one and the same manner. Mr. Wallace remarks in the butterflies of Celebes, as compared with those of the adjacent islands, a peculiarity of outline. Their fore-wings are either strongly curved or bent near the base, or extremely elongated and hooked. Now the climate of Celebes differs very little, if at all, from that of Borneo on the west, or of the Moluccas on the east. It is difficult in the extreme to imagine any reason why butterflies with the peculiarities thus indicated should enjoy any advantage in the struggle for existence in Celebes which they would not equally experience in the surrounding islands. How, then, can we conceive these features due to Natural Selection? But there are local peculiarities, not merely in structure, but also in colour and design. To what can these be due? It needs but little effort of the imagination to recognise in the patterns of Chinese butterflies, moths, and beetles, something of the stiff and grotesque character which strikes us in Chinese decorative art.

Again, there is a difficulty in the distribution of zoological forms. Take those coleopterous groups which are most closely and directly dependent upon the vegetable kingdom, such as the Buprestidæ, Cetoniadæ, Dynastidæ, Curculionidæ, and Longicornes. We might naturally expect that in every tropical region where there is a profuse, luxuriant, and highly varied vegetable life, especially in the form of large trees, there the development of these groups would be mainly equal and similar. Yet nothing can be farther from the truth. There are decaying trees and rich vegetable mould in Africa, in India, and the Malay Archipelago. Yet the head-quarters of the Dynastidæ are unmistakably in South America. There are leaves and flowers in the warm regions of the western hemisphere, as well as in Africa, Southern Asia, and Australia. Yet the Cetoniadæ of the Old World are in round numbers four times as numerous as those of the New. Why have not they in Brazil, Guayana, Venezuela, and Mexico, developed themselves into as great a variety of forms as in Madagascar, the Guinea regions, the Cape, and India? It may be urged that their career in South America has been checked by enemies, as, for instance, by the destructive ants. But Africa is no less infested by these, our "six-legged rivals," than is South America. It is also difficult to see why any enemy which should interfere with the development of the Cetoniadæ, should not to a very serious extent interfere with the multiplication of Dynastidæ and Longicornes. To sum up this part of the subject it may be said we want to know what determines the variation of species, to take in different countries different directions? What connects certain forms of life with certain regions? Why do groups whose conditions of existence are the same—as far, at least, as our most careful scrutiny can discover—make their election of localities, and what determines their choice?

We cannot refrain from noticing here certain objections to "Darwinism," or rather to Evolutionism, which have been recently promulgated, but to which we can attach no great value. It is said that no monkeys are carnivorous, whilst all the stronger races of man are omnivorous, and even primarily carnivorous. It is a common error to lay great weight on the natural diet of an animal in proof of its affinities. We find the greatest diversity of diet in one and the same group. Thus whilst the polar bear, and, I believe, the grizzly (*U. Ferox*), may be considered exclusively carnivorous, there are other species of bear which decidedly prefer a vegetable diet. Among the Rodents we find some purely herbivorous, as the hare, the rabbit, and the Guinea-pig, and others which, like the rat, have a strong inclination for animal food. Nor must we forget that the earliest traditions we possess represent primeval man as a pure vegetarian.

A kindred objection is that the "nearest creatures to man in form are not the nearest in intellect. The elephant, dog, and horse, which have no affinity to man, have a far closer intellectual affinity than those pets of Darwinism, the gorilla and chimpanzee." If the author of these lines had mentioned the ant as having a "closer intellectual affinity to man" than any vertebrate animal, he would have commanded assent. We should then have reminded him that Evolutionism is the only theory of the animal kingdom with which such facts harmonise. All other systems have found it a severe shock that mental development did not keep a parallel course with structural affinity. Place a white ball at the top of every twig of a large tree: the second highest ball will not necessarily be found on the same branch that supports the highest of all. Mr. Darwin expressly declares that he does not consider man's place to be on the same ascending line as that to which the gorilla and chimpanzee belong. The anonymous writer quoted commits the common mistake of confounding docility and subservience with intellect. Could we enter into as close relations with the ape as we do with the dog, we should find the former far superior in intellect. But, like the higher races of man, he is independent in character, and does not take kindly to slavery. A female ape will drive away the flies from her sleeping infant. If it dies, she will often fret herself to death. Does a bitch ever show such an approach to human nature?

We have thus briefly glanced at a number of facts which still remain isolated and unorganised, and of which we are scarcely more able to give a rational account than were our forerunners two centuries ago. Our survey has been far from exhaustive, its purpose being to furnish, not a catalogue, but merely characteristic specimens. How, then, are such phenomena to be regarded? To ascribe them to chance is, substantially, what "the fool hath said in his heart." It may be asked why not simply refer them to the good pleasure of God, as did our fathers before us? We reply that it is equally irreverent and unphilosophical to conceive of the decrees of Absolute Reason as mere casual arrangements, based upon no fixed principles, and which might as well have been quite otherwise. Let us be sure that all these apparent anomalies are the outcome of laws —laws certainly difficult, and perhaps impossible, for us to comprehend. But it is at once our duty and our privilege to make the attempt.

VI. THE MECHANICAL ACTION OF LIGHT.

By William Crookes, F.R.S., &c.

SOME experiments illustrating the mechanical action of Light, which I have recently exhibited before the Fellows of the Royal Society, having attracted considerable attention, I propose to give here a description of some of the instruments which my researches have enabled me to construct. But, to render the subject more intelligible, it will be necessary to give a brief outline of the researches which I have been carrying on for the last three or four years, so that the reader may see the gradual steps which have led up to the full proof that Radiation is a motive power.

The experiments were first suggested by some observations made when weighing heavy pieces of glass apparatus in a chemical balance, enclosed in an iron case from which the air could be exhausted. When the substance weighed was of a temperature higher than that of the surrounding air and the weights, there appeared to be a variation of the force of gravitation. Experiments were thereupon instituted to render the action more sensible and to eliminate sources of error.*

My first experiments were performed with apparatus made on the principle of the balance. An exceedingly fine and light arm was delicately suspended in a glass tube by a double-pointed needle; and at the ends were affixed balls of various materials. Amongst the substances thus experimented on I may mention pith, glass, charcoal, wood, ivory, cork, selenium, platinum, silver, aluminium, magnesium, and various other metals.

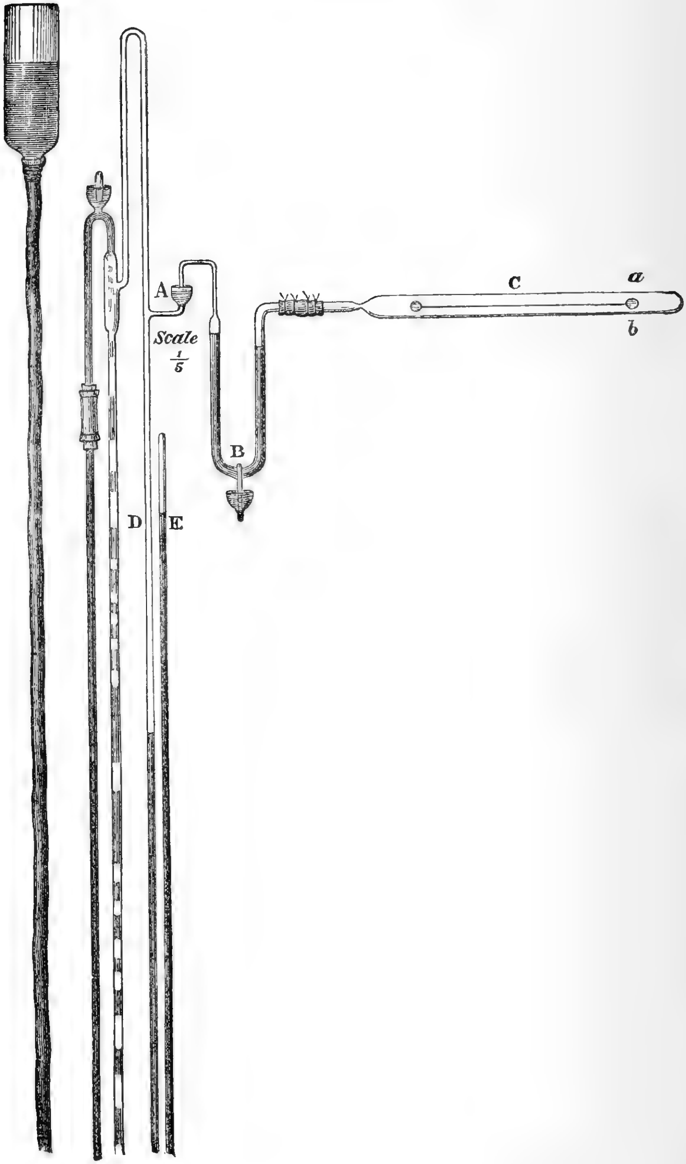
The most delicate apparatus for general experiment was made with a straw beam having pith masses at the end. The general appearance of the apparatus is shown in Fig. 1.

A is the tube belonging to the Sprengel pump.† B is the desiccator, full of glass beads moistened with sulphuric acid. C is the tube containing the straw balance with pith ends: it is drawn out to a contracted neck at the end connected

* "On the Atomic Weight of Thallium," Phil. Trans., 1873, vol. clxiii., p. 287.

† For a full description of this pump, with diagrams, see Phil. Trans., 1873, vol. clxiii., p. 295.

Fig. 1.

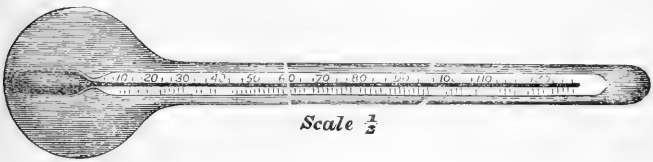


with the pump, so as to readily admit of being sealed off at any stage of the exhaustion. D is the pump-gauge, and B is the barometer.

The whole being fitted up as here shown, and the apparatus being full of air to begin with, I passed a spirit-flame across the lower part of the tube at *b*, observing the movement by a low-power micrometer: the pith-ball (*a b*) descended slightly, and then immediately rose to considerably above its original position. It seemed as if the true action of the heat was one of attraction, instantly overcome by ascending currents of air. A hot metal or glass rod and a tube of hot water applied beneath the pith ball at *b* produced the same effect as the flame; when applied above at *a* they produced a slight rising of the ball. The same effects take place when the hot body is applied to the other end of the balanced beam. In these cases air-currents are sufficient to explain the rising of the ball under the influence of heat.

In order to apply the heat in a more regular manner, a thermometer was inserted in a glass tube, having at its extremity a glass bulb about 1½ inches diameter; it was filled with water and then sealed up (see Fig. 2). This was

FIG. 2.



arranged on a revolving stand, so that by means of a cord I could bring it to the desired position without moving the eye from the micrometer. The water was kept heated to 70° C., the temperature of the laboratory being about 15° C.

The barometer being at 767 millims. and the gauge at zero, the hot bulb was placed beneath the pith ball at *b*. The ball rose rapidly. The source of heat was then removed, and as soon as equilibrium was restored I placed the hot-water bulb above the pith ball at *a*, when it rose again, —more slowly, however, than when the heat was applied beneath it.

The pump was then set to work; and when the gauge was 147 millims. below the barometer, the experiment was tried again: a similar result, only more feeble, was obtained. The exhaustion was continued, stopping the pump from time to time to observe the effect of heat, when it was seen that the effect of the hot body regularly diminished as the rarefaction increased, until, when the gauge was about 12 millims. below the barometer, the action of the hot body was scarcely noticeable. At 10 millims. below it was still less; whilst when there was only a difference of 7 millims. between the barometer and the gauge, neither the hot-water bulb, the hot rod, nor the spirit-flame caused the ball to move in an appreciable degree.

The inference was almost irresistible that the rising of the pith was only due to currents of air, and that at this near approach to a vacuum the residual air was too highly rarefied to have power in its rising to overcome the inertia of the straw beam and the pith balls. A more delicate instrument would doubtless show traces of movement at a still nearer approach to a vacuum; but it seemed evident that when the last trace of air had been removed from the tube surrounding the balance—when the balance was suspended in empty space only—the pith ball would remain motionless, wherever the hot body were applied to it.

I continued exhausting. On next applying heat underneath, the result showed that I was far from having discovered the law governing these phenomena; the pith ball rose steadily, and without that hesitation which had been observed at lower rarefactions. With the gauge 3 millims. below the barometer, the ascension of the pith when a hot body was placed beneath it was equal to what it had been in air of ordinary density; whilst with the gauge and barometer level its upward movements were not only sharper than they had been in air, but they took place under the influence of far less heat—the finger, for example, instantly repelling the ball to its fullest extent.

To verify these unexpected results, air was gradually let into the apparatus, and observations were taken as the gauge sank. The same effects were produced in inverse order, the point of neutrality being when the gauge was about 7 millims. below a vacuum.

A piece of ice produced exactly the opposite effect to a hot body.

The presence of air having so marked an influence on the action of heat, an apparatus was fitted up in which the source of heat (a platinum spiral rendered incandescent by electricity) was inside the vacuum-tube instead of outside it as before; and the pith balls of the former apparatus were replaced by brass balls. By careful manipulation and turning the tube round, I could place the equipoised brass ball either over, under, or at the side of the source of heat. With this apparatus I tried many experiments, to ascertain more about the behaviour of the balance during the progress of the exhaustion, both below and above the point of no action, and also to ascertain the pressure corresponding with this critical point.

In one experiment, which is described in detail in my paper on this subject before the Royal Society,* the pump was worked until the gauge had risen to within 5 millims. of the barometric height. On arranging the ball above the spiral and making contact with the battery, the attraction was still strong, drawing the ball downwards a distance of 2 millims. The pump continuing to work, the gauge rose until it was within 1 millim. of the barometer. The attraction of the hot spiral for the ball was still evident, drawing it down when placed below it, and up when placed above it. The movement, however, was much less decided than before; and in spite of previous experience the inference was very strong that the attraction would gradually diminish until the vacuum was absolute, and that then, and not till then, the neutral point would be reached. Within 1 millim. of a vacuum there appeared to be no room for a change of sign.

The gauge rose until there was only ½ a millim. between it and the barometer. The metallic hammering heard when the rarefaction is close upon a vacuum commenced, and the falling mercury only occasionally took down a bubble of air. On turning on the battery current, there was the faintest possible movement of the brass ball (towards the spiral) in the direction of attraction.

The working of the pump was continued. On next making contact with the battery, no movement could be detected. The red-hot spiral neither attracted nor repelled. I had arrived at the critical point. On looking at the gauge I saw it was level with the barometer.

The pump was now kept at full work for an hour. The gauge did not rise perceptibly; but the metallic hammering sound increased in sharpness, and I could see that a bubble or two of air had been carried down. On igniting the spiral, I saw that the neutral point had been passed. The sign had changed, and the action was one of faint but unmistakable *repulsion*. The pump was still kept going, and an observation was taken from time to time during several hours. The repulsion continued to increase. The tubes of the pump

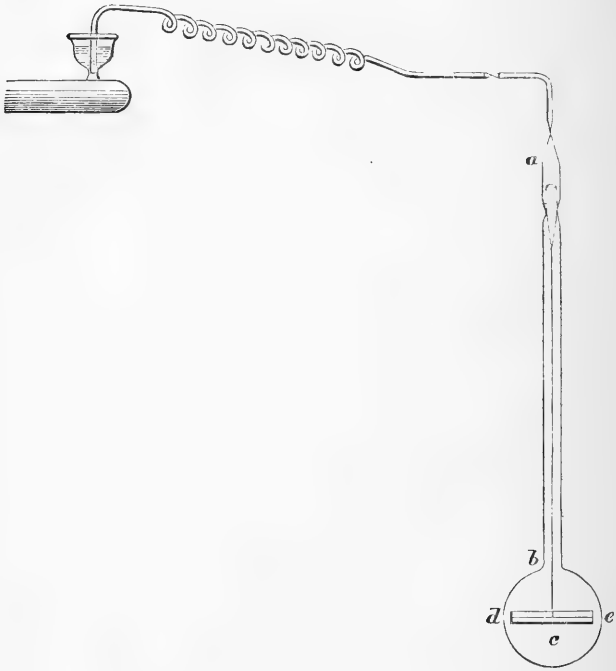
* Phil. Trans., 1874, vol. clxiv., p. 501.

were now washed out with oil of vitriol,* and the working was continued for an hour.

The action of the incandescent spiral was now found to be energetically *repellent*, whether it was placed above or below the brass ball. The fingers exerted a repellent action, as did also a warm glass rod, a spirit-flame, and a piece of hot copper.

In order to decide once for all whether these actions really were due to air-currents, a form of apparatus was fitted up which—whilst it would settle the question indisputably—

Fig. 3.



would at the same time be likely to afford information of much interest.

By chemical means I obtained in an apparatus a vacuum so nearly perfect that it would not carry a current from a Ruhmkorff's coil when connected with platinum wires sealed into the tube. In such a vacuum the repulsion by heat was still found to be decided and energetic.

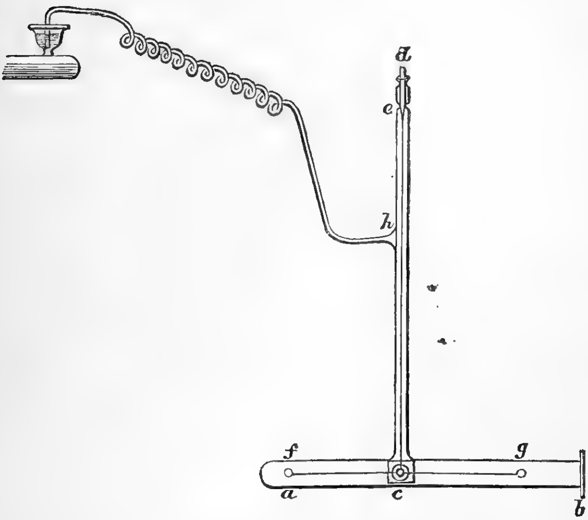
* This can be effected without interfering with the exhaustion.

I next tried experiments in which the rays of the sun, and then the different portions of the solar spectrum, were projected on to the delicately suspended pith-ball balance. *In vacuo* the repulsion by a beam of sunlight is so strong as to cause danger to the apparatus, and resembles that which would be produced by the physical impact of a material body.

A simpler form of the apparatus for exhibiting the phenomena of attraction in air and repulsion in a vacuum consists of a long glass tube, *a b* (Fig. 3), with a globe, *c*, at one end. A light index of pith, *d e*, is suspended in this globe by means of a cocoon fibre.

When the apparatus is full of air at ordinary pressure,

FIG. 4.

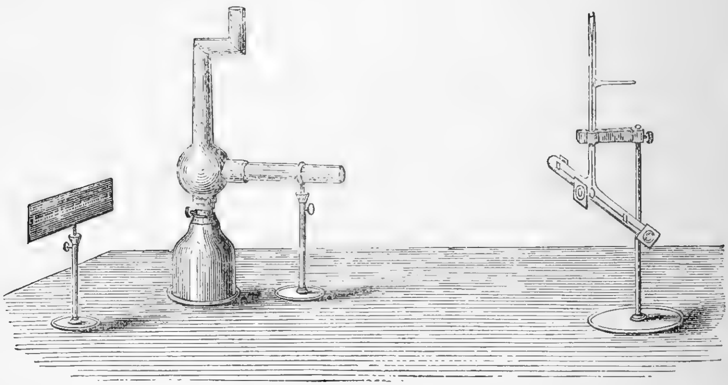


a ray of heat or light falling on one of the extremities of the bar of pith gives a movement indicating attraction. When the apparatus is exhausted until the barometric gauge shows a depression of 12 millims. below the barometer, neither attraction nor repulsion results when radiant light or heat falls on the pith, but when the vacuum is as good as the pump will produce strong repulsion is shown when radiation is allowed to fall on one end of the index. An apparatus of this kind constructed with the proper precautions, and sealed off when the vacuum is perfect, is so sensitive to heat that a touch with the finger on a part of the globe near one extremity of the pith will drive the index round over 90°.

while it follows a piece of ice as a needle follows a magnet. With a large bulb, very well exhausted and containing a suspended bar of pith, a somewhat striking effect is produced when a lighted candle is placed about 2 inches from the globe. The pith bar commences to oscillate to and fro, the swing gradually increasing in amplitude until the dead centre is passed over, when several complete revolutions are made. The torsion of the suspending fibre now offers resistance to the revolutions, and the bar commences to turn in the opposite direction. This movement is kept up with great energy and regularity as long as the candle burns.

For more accurate experiments I prefer making the apparatus differently. Fig. 4 represents the best form. *a b* is a glass tube, to which is fused at right angles another narrower tube, *c d*; the vertical tube is slightly contracted at *e*, so as to prevent the solid stopper *d*—which just fits the bore of the tube—from falling down. The lower end of the

FIG. 5.



stopper, *d e*, is drawn out to a point; and to this is cemented a fine glass thread about 0·001 inch diameter, or less, according to the torsion required.*

At the lower end of the glass thread an aluminium stirrup and a concave glass mirror are cemented, the stirrup being so arranged that it will hold a beam, *f g*, having masses of any desired material at the extremities. At *c* in the horizontal tube is a plate-glass window cemented on to the tube. At *b* is also a piece of plate glass cemented on.

* Some of the glass fibres used in these torsion balances are so fine that when one end is held between the fingers the other portion floats about like a spider's thread, and frequently rises until it takes a vertical position.

Exhaustion is effected through a branch tube, *h*, projecting from the side of the upright tube. This is sealed by fusion to the spiral tube of the pump. The stopper *d e* and the glass plates *c* and *b* are well fastened with a cement of resin and bees'-wax.

The advantage of a glass-thread suspension is that the beam always comes back to its original position.

An instrument of this sort, perfectly exhausted and then sealed off, is shown at work in Fig. 5. It has pith plates at the extremities of the torsion beam. A ray of light from the lamp is thrown on to the central mirror, and thence reflected on to the graduated scale. The approach of a finger to either extremity of the beam causes the luminous index to travel several inches, showing repulsion. A piece of ice brought near causes the spot of light to travel as much in the opposite direction. In order to ensure the luminous index coming accurately back to zero, extreme precautions must be taken to keep all extraneous radiation from acting on the torsion-balance. The whole apparatus is closely packed round with a layer of cotton-wool about 6 inches thick, and outside this is arranged a double row of Winchester quart bottles filled with water, spaces only being left for the radiation to fall on the balance and for the index ray of light to get to and from the mirror.

However much the results may vary when the vacuum is imperfect, with an apparatus of this kind they always agree among themselves when the residual gas is reduced to the minimum possible; and it is of no consequence what this residual gas is. Thus, starting with the apparatus full of various vapours and gases, such as air, carbonic acid, water, iodine, hydrogen, ammonia, &c., there is not found at the highest rarefaction, any difference in the results which can be traced to the residual gas. A hydrogen-vacuum appears the same as a water- or an iodine-vacuum.

The neutral point for a thin surface of pith being low, and that for a moderately thick piece of platinum being high, it follows that at a rarefaction intermediate between these two points pith will be repelled, and that platinum will be attracted by the same beam of radiation. This has been proved experimentally. An apparatus showing simultaneous attraction and repulsion by the same ray of light is illustrated in Fig. 6.

The pieces *f g* on the end of one beam consist of platinum-foil exposing a square centimetre of surface, whilst the extremities *f′ g′* on the other beam consist of pith plates of the same size. A wide beam of radiation thrown in the centre of the tube on to the plates $g f'$ causes g to be attracted and f' to be repelled, as shown by the light reflected from the mirrors, $c c'$. The atmospheric pressure in the apparatus is equal to about 40 millims. of mercury.

In a torsion apparatus similar to the one shown in Figs. 4 and 5 I have submitted variously coloured discs to the action of the different rays of the spectrum. The most striking results, as yet, have been obtained when the different rays of the spectrum were thrown on white and on black surfaces. The result was to show a decided difference between the action of light and of radiant heat. At the highest exhaustions dark heat from boiling water acts almost equally on white pith and on pith coated with lamp-black, repelling either with about the same force. The action of the luminous rays, however, is different. These repel the black surface more energetically than they do the white surface, and, consequently, if in such an apparatus as is shown at

FIG. 6.

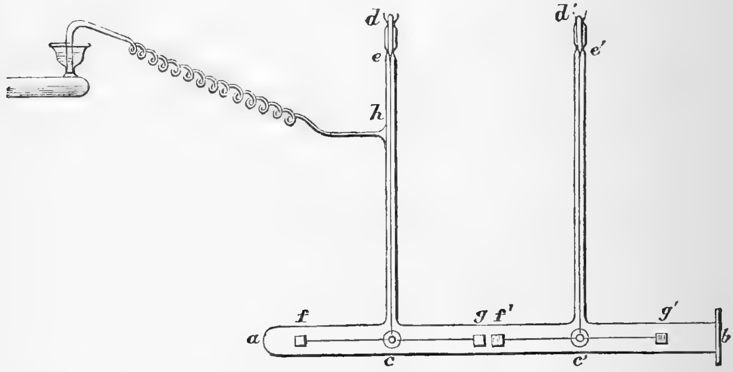
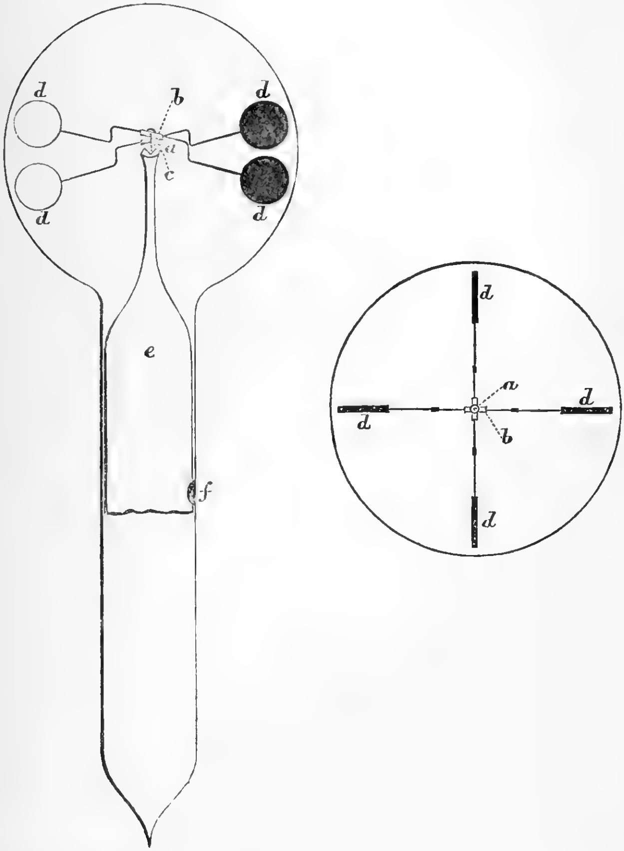


Fig. 4, one disc of pith is white and the other is black, an exposure of both of them to light of the same intensity will cause the torsion thread to twist round, owing to the difference of repulsion exerted on the black and the white surface. If, in the bulb apparatus shown in Fig. 3, the halves of the pith bar are alternately white and lamp-blacked, this differential action will produce rapid rotation in one direction, which keeps up until stopped by the torsion of the suspending fibre.

Taking advantage of this fact I have constructed an instrument which I have called the Radiometer, shown in section and plan at Figs. 7 and 8. It consists of four arms, of some light material, suspended on a hard steel point resting in a jewel cup, so that the arms are able to revolve horizontally upon the centre pivot, in the same manner as the arms of Dr. Robinson's anemometer revolve. To the extremity of each arm is fastened a thin disc of pith, white on one side and lamp-blacked on the other, the black surfaces of all the discs facing the same way. The whole is enclosed in a thin glass globe, which is then

FIGS. 7 and 8.



a. A very fine needle point.
b. Two pieces of straw.
c. Jewel cup.
d d d d. Four pith discs, blackened on one side. The arms between the straw in the centre and the discs are bent glass fibres.
e. Glass support holding cup.
f. Cement to keep the support *e* in its place.

exhausted to the highest attainable point and hermetically sealed.

The arms of this instrument rotate with more or less velocity under the action of radiation, the rapidity of revolution being directly proportional to the intensity of the incident rays. Placed in the sun or exposed to the light of burning magnesium, the rapidity is so great that the separate discs are lost in a circle of light. Exposed to a candle 20 inches off another instrument gave one revolution in 182 seconds; with the same candle placed at a distance of 10 inches off the result is one revolution in 45 seconds; and at 5 inches off one revolution was given in 11 seconds. Thus it is seen that the mechanical action of radiation is inversely proportional to the square of the distance. At the same distance 2 candles give exactly double, and 3 candles give three times, the velocity given by 1 candle, and so on up to 24 candles. A small Radiometer was found to revolve at the velocities shown in the following table, when exposed to the radiation of a standard candle 5 inches off.

Time required for One Revolution.

Source of Radiation.	Time in Seconds.
1 candle, 5 inches off, behind green glass . .	40
,, 5 ,, ,, blue ,, . .	38
,, 5 ,, ,, purple ,, . .	28
,, 5 ,, ,, orange ,, . .	26
,, 5 ,, ,, yellow ,, . .	21
,, 5 ,, ,, light red ,, . .	20

In diffused daylight the velocity was one revolution in from 1·7 seconds to 2·3 seconds, according to the intensity of the incident rays. In full sunshine, at 10 A.M., it revolved once in 0·3 second, and at 2 P.M. once in 0·25 second.

When heat is cut off by allowing the radiation to pass through a thick plate of alum, the velocity of rotation is somewhat slower, and when only dark heat is allowed to fall on the arms (as from a vessel of boiling water) no rotation whatever is produced.

In all respects, therefore, it is seen that the Radiometer gives indications in strict accordance with theory.

Several radiometers, of various constructions as regards details, but all depending on the above-named discovery, have been exhibited at the Royal Society, where their novelty and unexpected indications excited a considerable amount of interest.

This form of instrument is of too recent a construction for me to be able to do more than draw brief attention to a few of the many uses for which it is applicable.

By timing the revolutions of the instrument when exposed direct to a source of light—a candle, for instance—the total radiation is measured. If a screen of alum is now interposed, the influence of heat is almost entirely cut off, the velocity becomes proportionately less, and the instrument becomes a photometer. By its means photometry becomes much simplified; flames the most diverse may readily be compared between themselves or with other sources of light; a "standard candle" can now be defined as one which at x inches off causes the radiometer to perform y revolutions per minute, the values of x and y having previously been determined by comparison with some ascertained standard; and the statement that a gas-light is equal to so many candles may, with more accuracy, be replaced by saying that it produces so many revolutions.

To photographers the radiometer will be invaluable. As it will revolve behind the orange-coloured glass used for admitting light into the so-called dark room, it is only necessary to place one of these instruments in the window to enable the operator to see whether the light entering his room is likely to injure the sensitive surfaces there exposed; thus, having ascertained by experience that his plates are fogged, or his paper injured, when the revolutions exceed, say, ten a minute, he will take care to draw down an extra blind when the revolutions approach that number. Still more useful will the radiometer be in the photographic gallery. Placing an instrument near the sitter at the commencement of the day's operations, it is found that, to obtain a good negative, the lens must be uncovered—not for a particular number of seconds—but during the time required for the radiometer to make, say, twenty revolutions. For the remainder of the day, therefore, assuming his chemicals not to vary, the operator need not trouble himself about the variation of light; all he has to do is to watch the radiometer and expose for twenty revolutions, and his negatives will be of the same quality,* although at one time it may have taken five minutes, and at another not ten seconds, to perform the allotted number.

* In this brief sketch I omit reference to the occasions in which the ultra violet rays diminish in a greater proportion than the other rays.

I have long been experimenting in the endeavour to trace some connection between the movements of attraction and repulsion above alluded to and the action of gravitation in Cavendish's celebrated experiment. The investigation is not sufficiently advanced to justify further details, but I will give here an outline of one of the results.

I find that a heavy metallic mass, when brought near a delicately suspended light ball, attracts or repels it under the following circumstances:—

I. *When the ball is in air of ordinary density.*

a. If the mass is *colder* than the ball, it *repels* the ball.
b. If the mass is *hotter* than the ball, it *attracts* the ball.

II. *When the ball is in a vacuum.*

a. If the mass is *colder* than the ball, it *attracts* the ball.
b. If the mass is *hotter* than the ball, it *repels* the ball.

The density of the medium surrounding the ball, the material of which the ball is made, and a very slight difference between the temperatures of the mass and the ball exert so strong an influence over the attractive and repulsive force, and it has been so difficult for me to eliminate all interfering actions of temperature, electricity, &c., that I have not yet been able to get distinct evidence of an independent force (not being of the nature of heat or light) urging the ball and the mass together.

Experiment has, however, shown me that, whilst the action is in one direction in dense air, and in the opposite direction in a vacuum, there is (as I have already pointed out in the experiments described in the commencement of this paper) an intermediate pressure at which differences of temperature appear to exert little or no interfering action. By experimenting at this critical pressure, and at the same time taking all the precautions which experience shows are necessary, it would seem that such an action as was obtained by Cavendish, Reich, and Baily should be rendered evident.

It is not unlikely that in the experiments here recorded may be found the key of some as yet unsolved problems in celestial mechanics. In the sun's radiation passing through the quasi vacuum of space we have the radial repulsive force, possessing successive propagation, required to account for the changes of form in the lighter matter of comets and nebulæ, and we may learn by that action, which is rapid and apparently fitful, to find the cause in those rapid bursts which take place in the central body of our system; but until we measure the force more exactly we shall be unable to say how much influence it may have in keeping the heavenly bodies at their respective distances.

So far as repulsion is concerned, we may argue from small things to great, from pieces of pith up to heavenly bodies; and we find that the repulsion shown between a cold and warm body will equally prevail, when for melting ice is substituted the cold surface of our atmospheric sea in space, for a lump of pith a celestial sphere, and for an artificial vacuum a stellar void.

Throughout the course of these investigations I have endeavoured to remain unfettered by the hasty adoption of a theory, which, in the early stages of an inquiry, must almost of necessity be erroneous. Some minds are so constituted that they seem impelled to form a theory on the slightest experimental basis. There is then great danger of their becoming advocates, and unconsciously favouring facts which seem to prove their preconceived ideas and neglecting others which might oppose their views. This is unfortunate, for the mind should always be free to exercise the judicial function, and give impartial weight to every phenomenon which is brought it. *Any* theory will account for *some* facts; but only the true explanation will satisfy *all* the conditions of the problem, and this cannot be said of any theory which has yet come to my mind.

My object at present is to ascertain facts, varying the conditions of each experiment so as to find out what are the necessary and what the accidental accompaniments of the phenomena. By working steadily in this manner, letting each group of experiments point out the direction for the next group, and following up as closely as possible, not only the main line of research, but also the little bye-lanes which often lead to the most valuable results, after a time the facts will group themselves together and tell their own tale; the conditions under which the phenomena invariably occur will give the laws; and the theory will follow without much difficulty. The eloquent language of Sir Humphry Davy contains valuable advice, although in terms somewhat exaggerated. He says,—"When I consider the variety of theories which may be formed on the slender foundation of one or two facts, I am convinced that it is the business of the true philosopher to avoid them all together. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like Newton's."

NOTICES OF BOOKS.

The Origin of Creation; or the Science of Matter and Force. A New System of Natural Philosophy. By THOMAS RODERICK FRASER, M.D., and ANDREW DEWAR. London: Longmans and Co.

To some of our contemporaries the book before us will supply, doubtless, matter for jokes not a few. To us it is simply a psychological phenomenon, and one of the most puzzling, as well as the most painful, that have come before us for many years. The case is this:—Either the great bulk of our knowledge in all the Sciences, from Astronomy to Biology, is a delusion, and the most eminent philosophers of the past and the present are mistaken, or this work is the most singular collection of errors and fallacies ever brought together by misplaced human ingenuity. If we, for argument's sake, admit the former alternative, we shall find it in the highest degree improbable that any two men within the limits of an ordinary life-time could acquire knowledge sufficient to demonstrate the fundamental errors of every branch of Science, and, going still further, replace these errors with truth. The Authors certainly lay claim to very extensive experience. The data upon which their "theories have been built have, in all the subjects touched upon, been based on personal observations in chemistry, telegraphy, and marine diving; in an extensive experience in coal and gold mines; also, while travelling in the Gulf Stream, the calms of the Equator, the coasts of Brazil, California, and Mexico, the Mediterranean, the Bay of Fundy, the hot sulphur-baths of Salt Lake, the great Geysers of California, and the mangroves of the Isthmus of Panama." During these researches and travels they profess to have discovered "the duality of atoms, the properties and force of matter, the cause of life, the source of mind, the cause of chemical action, the cause of sunlight, the cause of variation in ships' compasses, the cause of boiler explosions, the cause of winds and storms, the process of digestion, the cause of the tides, that magnetism is weight and supersedes gravitation, that coral is a semi-mineral growth and not the work of insects (!), the cause of meteors, the cause of auroras, the cause of the circulation of the blood, that hydrogen gas has the properties not only of metals but of minerals, that oxygen gas has the properties of vegetable matter." This we must admit is a very extensive and important array of discoveries, and a small part only, if substantiated, will suffice to earn for them the highest honours that Science has to bestow. We note, at the outset, that they desire to be judged in a rather peculiar manner:—"By the light of their faculty of common sense—the name, we may remark, under which many people worship their own ignorance —and their own personal observation, without reference to any book whatever, except it may be the Scriptures!"

The reader looking over such a list of discoveries and refutations will naturally expect that the book contains a detail of carefully conducted experiments and observations, from which the novel and startling views of the authors have been concluded. But there is nothing of the kind. We find assertion, but very little, if anything, worthy of the name of demonstration. The Authors do not argue; they preach. Whilst ignoring all other authority, they expect their own to be implicitly accepted.

Let us take, as a specimen of the work, the following passage: —"The mineral elements have naturally, as inherent elements, the cold colours, blue, black, and white; while the vegetable atoms are naturally possessed of the warm colours, red, yellow, and orange. Of course there appear to be exceptions. Gold is yellow, but it is very scarce, and is prized accordingly. Sulphur is yellow also, but it sheds a blue light when burned. Sometimes we see blue flowers also, but they are very rare indeed."

The last assertion is truly astounding. What English wood is not, in spring, blue over with the wild hyacinth? What cliff or moorland in the north is not gay in August with the blue harebell? What river in Derbyshire is not bordered with the blue forget-me-not? How many hill-sides in the Tyrol and Illyria—as Mr. Ruskin has eloquently described—are clad with the blue gentianella! A flax-field in flower is no very rare sight. If we turn from the fields and the woods to the garden we find the lupin, the campanula, the monk's-hood, the lobelia, the convolvulus (major and minor), the salvia, the crocus, and many others, all bearing blue flowers. Turning over the catalogue issued by an eminent London firm of nurserymen, we find the blue-flowering plants form one-ninth of the species therein included. As the flowers were classified into white, blue, purple, pink, crimson, scarlet, yellow, orange, and brown, the blue species hold numerically a fair average position.

Further, on the author's showing, white flowers ought also to be "very rare indeed." Yet they are, if possible, still more abundant than the blue: they accompany us all the year round, from the snowdrop of January to the Christmas rose of December.

Sulphur, the authors admit, "is yellow," although indisputably a mineral body. They seek, however, to dispose of this unpleasant fact by adding that "it sheds a blue light when burned." Now this is what, on their theory, it ought not to do. Sulphur, when burning, combines with oxygen one of their "vegetable elements," the characteristic colours of which we are told are "yellow, orange, and red." So that we might expect, on this view, that its flame would be red instead of blue. Iron and copper pyrites, which are decidedly not scarce, are yellow, and have frequently been mistaken for gold. Metallic copper is of an orange-red, but assumes various shades of green and blue when oxidised and dissolved in acids, which again clashes with the doctrines of this new philosophy. The compound of chromium containing the largest proportion of oxygen, perchromic acid, is of a deep blue. Here, therefore, Messrs. Fraser and Dewar are again at fault. It will not avail for them to say, "Of course there appear to be exceptions." Facts are not to be set aside so easily, and it is with facts, as we see, not with theories, that the views of the authors refuse to agree.

As might be expected, the authors indulge in an attack on Mr. Darwin's "Origin of Species," and fancy that they have refuted it. By way of compensation they believe in Abiogenesis, and take M. Pasteur to task for the imperfection of his experiments undertaken to refute the notion of spontaneous generation.

On p. 163 we read:—"It may account for the Scotch, as a people, being so little troubled with indigestion, that the food, especially of the poorer classes, is mainly composed of oatmeal, salt fish, and cheese." Is this said in earnest or by way of a joke? Most people find by experience that salt fish and cheese are decidedly indigestible,

The Authors promulgate, also, a system of medicine. Turning to Chapter xxix., in the hope of meeting with some new light thrown upon diseases and their remedies, we find merely a recommendation of hot water, purgatives, and "in urgent cases, an emetic." Without at all questioning the value of these agencies, we submit that they are nowise novel. Morison, the "Hygienist," long ago proclaimed "proper vegetable purgation" to be the remedy for every disease. The "life-pills," one and all, which, with such touching faith, the British public has bolted for the last half century, are purgatives likewise. What prospect of success remains, then, for Dr. Fraser and Mr. Dewar?

The Authors seem to believe that they have the misfortune to be "too far in advance of their day and generation." We should rather consider them as altogether off the right track—hopelessly and helplessly astray: they seem to us loose, hasty, careless observers, and random theorists. We do not deny that, owing to the imperfection of language, many of the statements here laid down might be long defended on paper; but verbal discussion is not the ordeal which scientific theories have in these days to undergo: they must legitimatise themselves by coordinating facts hitherto unexplained and anomalous, and by pointing the way to phenomena as yet undetected. If we may venture to suggest anything to authors who are on such excellent terms with themselves, we would advise them to take up some one subject out of the many broached in their work, and try what they can do with it. Let them prove, for instance, that colour is inherent in objects themselves, and does not depend on the light by which they happen to be illuminated. Or, turning from the sphere of theory to that of practice, let them complete their magnetic machine and demonstrate its superiority to the steam-engine. Let them thus strike a death-blow at the rampant "coal interests," and the only danger will be that the public will accept their hypotheses without asking for verification.

A Manual of Hygiene, Public and Private, and Compendium of Sanitary Laws. By C. A. Cameron, Ph.D., M.D., &c. Dublin: Hodges, Foster, and Co. London: Bailliere, Tindall, and Cox.

This work is certainly a valuable addition to our sanitary literature. It is addressed not merely to medical men, chemists, officials, and others who may have a special or professional interest in matters relative to public health, but to the community in general. As such we trust that it will powerfully aid in creating a sound public opinion on matters which, hitherto, in spite of much talk, have been practically neglected. Without wishing to play the part of alarmists, we must assert that this neglect is imprudent in the extreme. We are too much in the habit of assuming that those fearful outbreaks of pestilence which ravaged Europe in the Middle Ages are now out of the question. The Author, after giving a brief account of the dreadful plague of the fourteenth century, remarks:—"I wish to direct attention to these almost forgotten calamities, because they are calculated to teach us important lessons. Are we sure that we are safe from another visitation of the black death? There are epidemiologists who believe that the germs of this disease still linger amongst the deep valleys of the Himalayas, and that they may yet be wafted to Europe. If such an event should ever unfortunately take place, I fear that in some of our towns the virus of the disease would find a congenial soil." He states that the Legislature is taking active measure to improve the sanitary condition of towns. We fear that in this respect he forms a too favourable view, both of the present and the future. The recent Act, by which a new lease of life was granted to the London slaughter-houses, was certainly a step in the wrong direction. On the water-supply of towns we meet with much valuable information. Dr. Cameron very judiciously doubts the safety, as well as the economy, of Mr. Bailey Denton's plan of furnishing villages and small towns with good water by a system of shallow wells and pools. The Author considers that "in seasons of drought and heat the water of such shallow ponds as those proposed by Mr. Denton would become greatly deteriorated in quality."

Like all chemists who have given the matter a really fair and unprejudiced examination, Dr. Cameron considers that the process of Wanklyn and Chapman is the only practical mode of water analysis for sanitary purposes. But he points out that though waters containing much organic nitrogen are certainly bad, those containing little are not necessarily good. He mentions a public pump in Waterford, the water of which contained more than 4 grs. of ammonia and 3 of nitric acid per imperial gallon. The question is sometimes asked by the unthinking, why, if polluted waters are so injurious, our ancestors, who were more ignorant and careless of such matters than ourselves, were not entirely extirpated? The answer is at once apparent:—The pollution of water increases with the density of the population, —a process which is at this moment going on in a most striking manner in some of the New England states.

In speaking of the sewage question, Dr. Cameron remarks that the A B C process has failed. We fear that he makes this rash statement on the misleading authority of the late "Rivers' Pollution Commissioners." Had he taken the trouble to examine into the matter himself, he would have come to a very different conclusion.

We can most sincerely recommend this Manual to officers of health, public analysts, municipal corporations, boards of guardians, and, in short, to all whose duties involve a knowledge of sanitary conditions, sanitary reform, and sanitary legislation.

The Identity of Primitive Christianity and Modern Spiritualism. By Eugene Crowell, M.D. Vol. I. New York: G. W. Carleton and Co. London: Trübner and Co.

To pass any formal judgment upon this work, without having previously made Spiritualism the object of exclusive study, would be at once difficult and unfair. Not feeling so qualified, we must be content with attempting to furnish a brief exposition of the author's views, without either advocacy on the one hand or refutation on the other. There are here a certain number of phenomena. What is their explanation? By the majority of the educated world, including most men of science, all so-called materialists (apneumatists), and the bulk of practical "common-sense characters," they are set down, often with little inquiry, as a mixture of delusion and of conscious imposition. A small class, whilst believing that the subject has been greatly complicated both by fraud and by enthusiasm, hold that there still remains a substratum of facts which, if rightly dealt with, might point the way to natural laws of the profoundest significance,—perhaps to some modification of force far more nearly connected with the phenomena of life than is electricity or magnetism. A third party—among whom rank, perhaps, the majority of orthodox clergymen and the bulk of "religious society"—view the phenomena in question as real and authentic, but as infernal in their character and origin. Lastly, spiritualists, including the author of the book, ascribe the incidents described to the agency of disembodied human spirits.

Dr. Crowell addresses himself in particular to the third class, and seeks to combat their objections. He dedicates his book "To all liberal minds in the Christian churches who are disposed to welcome new light upon the spirituality of the Bible, even though it may proceed from an unorthodox source, and who dare weigh and consider, even though they may reject, the claim herein made for the unity of the higher teachings of modern Spiritualism with those of early Christianity." He contends that the revelations of Spiritualism are the great and needed remedy against the development of Materialism, against which the Church has been powerless, on account of her unbelief in "spiritual gifts." These gifts, claimed by the early Christians, he urges, have not ceased, as Protestants suppose, but are still exercised. Spiritualism, therefore, is no novelty, but merely a re-manifestation of powers familiar to the first teachers of Christianity, and which, though obscured and neglected in succeeding centuries, has never ceased to exist. "I shall attempt," he writes, "to prove the genuine character of the so-called miracles of the Bible by evidence as strong as that required to decide the most important cases in our courts of law, by establishing the fact of the occurrence of similar miracles in our day constantly occurring in our midst, and which may be witnessed by all, and have been witnessed by thousands of persons of greater intelligence than most of those who witnessed the Bible miracles, and upon whose testimony these depend for credibility; and while I contend for equal credibility for both those which are recorded in the Bible and those which are now occurring, I shall be able to show that the different manifestations at the present time are fully as wonderful as those in ancient times, and that whereas they were little understood then, they are far better understood now."

As arguments against the asserted diabolical origin of Spiritualism, he contends that it has effected numerous conversions from Atheism, from Deism to Christianity, and from Unitarianism to orthodoxy. He maintains that Spiritualism has an elevating and purifying effect upon the inner life, and proclaims it to be simply "Christianity minus the framework of the ecclesiastical structure,—that is, Christianity stripped of the terrors with which superstition and error have invested it."

In support of these views Dr. Crowell displays great learning and research, and no small degree of acumen. Of his thorough sincerity the work seems, to us, to contain abundant evidence. But in how far his interpretation of certain passages of the Old and New Testaments will be admitted, by competent authorities, we entertain some doubt. Nor do we think that his comparatively low estimate of the Old Testament will greatly aid him in his attempts at reconciling Spiritualism with Christianity.

The whole question is one which obviously demands a broader, a more profound, and a calmer investigation than it has yet received. We fail to see what good end can be served by refusing such an inquiry, and by ridiculing scientific men who approach it in an impartial spirit.

The Blowpipe: a Guide to its Use in the Determination of Salts and Minerals. Compiled from Various Sources, by G. W. PLYMPTON. New York: D. Van Nostrand. London: Trübner.

THIS manual does not profess to be an original work. The first two parts, as we are told in the preface, " have been adapted, with but few emendations, from the work of Sheerer and Blanford. The alterations have chiefly been in the chemical symbols, the new nomenclature replacing the old." Symbols, of course, are invaluable where decompositions have to be explained, but we certainly do not see their utility in analytical manuals. When sesquioxide of chromium is mentioned, the Author's meaning would be fully understood without the addition of (Cr_2O_3).

The third part of the work is said to be translated from Gueront's "Guide Pratique pour la Détermination des Minéraux," which was originally written by Fuchs, of Heidelberg. We do not perceive anything erroneous or objectionable in the instructions given, and we have no doubt that the work will prove useful to students.

A Manual of Metallurgy. By WILLIAM HENRY GREENWOOD, F.C.S., &c. Vol. I.—Fuel, Iron, Steel, Tin, Antimony, Arsenic, Bismuth, and Platinum. Illustrated by 59 Engravings. London and Glasgow: William Collins, Sons, and Co. 1874.

AMONG the various branches of pure and applied Science in which the Department of Science and Art hold annual examinations, Metallurgy has for many years held a place, although the number of candidates for examination on this subject is, as might be expected, invariably small. Indeed it is difficult to see how the student can obtain the necessary information on this branch of Science, since our standard works on Metallurgy are, for the most part, beyond the reach of the class of students with whom the South Kensington system chiefly deals. It was therefore highly desirable that a cheap and trustworthy text-book should be compiled, and Messrs. Collins have done well to include a Manual of Metallurgy in their "Advanced Science Series." Mr. Greenwood has written the work with the view of meeting the requirements of the Kensington syllabus, and has succeeded in producing an extremely serviceable little Manual. Of course there is no room in so elementary a work for any original treatment of the subject, but the writer gives a succinct analysis of what others have written, and has evidently profited by his training under one of the best metallurgists in this country. After defining the terms peculiar to Metallurgy, Mr. Greenwood devotes a chapter to Fuel, and then passes to the study of Iron and Steel, which forms, indeed, the bulk of the book. The minor metals—tin, antimony, arsenic, bismuth, and platinum—are then briefly disposed of; but we fail to see what principles of classification have induced the writer to thus bring these rarer metals into juxtaposition with iron. The modern chemical notation, nomenclature, and atomic weights are employed, and the information generally is brought up to date. But although thus satisfactory as a whole, it would, of course, be easy enough to find fault with certain parts of the work. It is to be regretted that wherever German expressions are employed they are always bungled. Thus the writer cites, in his preface, "Bruno Kerl's *Handbuch der Metallurgischen*" *(sic)*; again, we are surprised to find the common word *Spiegeleisen* incorrectly spelt wherever it occurs. But trivial typographical errors of this kind, although annoying to an observant reader, do not, after all, detract much from the merits of the Manual. Indeed we hope that Mr. Greenwood will be as successful in his second volume—a volume which will deal with the remaining metals—as he has been, on the whole, in the present instalment of his work.

The Best Mining Machinery; an Essay. By Ralph Goldsworthy. Reprinted, with Alterations and Additions, from the Royal Cornwall Polytechnic Society's 41st Annual Report, 1873.

Originally printed in the "Mining Journal," and afterwards in the "Report of the Cornwall Polytechnic Society," this Essay is already known to a large number of those who are interested in mining. Nevertheless it is of so useful a character as to fully deserve a separate issue. Mr. Goldsworthy brings a good deal of practical knowledge to bear upon his subject, and gives the reader the benefit of a wide experience. It is a thoughtful and suggestive Essay, which will be read with interest by all who are connected with metalliferous mines.

Number, a Link between Divine Intelligence and Human: an Argument. By C. GIRDLESTONE, M.A. London: Longmans and Co.

THE object of this little work is to show that "man is made in the likeness of his Maker, instead of being, as surmised by some, developed from the lower orders of creation." We may remark, parenthetically, that the conditions here given as two alternatives are by no means mutually exclusive. Man may have reached the likeness of his Maker quite as easily by a process of evolution as by a direct, and, so to speak, mere mechanical mode of creation. "It is to the bearing of Number on this question of man's lineage, as pointing to an origin for the human race not below human nature, but above it, that the following argument will chiefly direct attention; pursuing throughout one definite chain of reasoning, which it is believed has not been elsewhere so distinctly set forth. And inasmuch as it rests mainly on grounds which underlie the foundations of modern Science, in most if not in all of its departments, it may perhaps have some little weight with those who follow after knowledge scientifically, out of a pure desire to discover and hold fast that which is true."

The Author's argument runs thus:—In the universe there appear certain numerical relations, which man is able to apprehend. Hence he may be assumed to bear a resemblance to the Creator of the universe. On the other hand, asserts the author, the lower animals are devoid of the faculty of number. Hence they differ from man not in degree, but in kind, and, as a consequence, man cannot have been evolved from any lower form.

We have thus, we believe, fairly stated the Author's train of reasoning, and have now to examine its validity. That there appear in the universe numerical relations, or at least uniformities, which man interprets and apprehends by number, no one will dispute. That man has, though to a very varying degree, the power "to calculate number and to apprehend its intimate relations" is also evident. But who tells us "that no (like) traces of a faculty to apprehend relations of Number have been met with in bird or beast is a proposition which may be confidently maintained"? This the Author has no right to take for granted. To give an illustration:—We stand before an audience, and hold up two minerals. "These," we say, "have a certain resemblance, but they differ herein that the one contains arsenic whilst the other does not." But if it turns out that we have not analysed the latter, but have merely assumed the absence of arsenic, we shall stand confounded. It is all very well for the Author to say "No one of these creatures, *it may be safely averred*, can discern the presence of number in things around it." Until the mental manifestations of animals have been investigated far more closely and impartially than they have been, it is the extreme of rashness to make any such statement.

But we can go further: animals do discern the presence of number in things around them. The Rev. J. G. Wood, in his interesting work "Man and Beast," gives a case of a dog whom his master, when drunk, had cruelly and unjustly beaten. The dog, after this, watched his master's potations, and as soon as it saw him take the fourth glass it invariably hid itself, and kept out of the way till he was sober.

Another instance, still more decisive, is the following:—A man wishing to shoot a crow which had her nest in a hollow near the top of a high tree, concealed himself in an outhouse close at hand, and waited for her to fly out. She remained concealed, however, till he had gone away. He then, to puzzle her, got another man to come with him into the shed, and go away after a time. Still she refused to come off her nest. Larger numbers of men were then tried; but it was not until seven men had gone in and six came away, leaving one behind, that her arithmetic failed her, and she was shot. Bitches, whose puppies have been removed and brought back, have been observed examining the lot with a puzzled air, as if in doubt whether the whole brood had been returned. The same phenomenon has been remarked with cats and their kittens. These facts, we submit, prove that animals can, though but to a small extent, recognise number. The crow saw that six was greater than five. The dog could count up to four, or, in other words, could perform a simple addition sum to that extent. This is very little less than what we observe among savages, some of whom can barely count as far as seven. The difference between man and beast, as far as number is concerned, is therefore one of degree, not of kind, and Mr. Girdlestone's argument must be set aside as null and void.

The Author seems not "to conceive it possible that quadrupeds might, in the course of long ages, become bipeds." Were there no mammalian forms on earth, living or fossil, save Hodge the ploughboy and Dobbin his horse, some difficulty might be experienced. But if we survey the whole mammalian series we perceive that the nearer we approach man the more the anterior extremities lose the character and functions of legs, and assume those of arms. Place side by side the arm of Hodge, the anterior limb of a gorilla and the fore leg of Dobbin, and no one can deny that the two former terminate in hands, and that the difference between them is trifling indeed compared to the contrast between the two latter.

Whilst holding, however, that Mr. Girdlestone has signally failed in proving his case, we consider that he deserves the thanks of naturalists for having drawn attention to a point which in their studies of animal intelligence has been too much neglected.

An Experimental Inquiry into the Nutrition of Animal Tissues. By WILLIAM MARCET, M.D., F.R.S. London: Longmans and Co.

THE nature of this interesting memoir may best be shown by a brief sketch of the conclusions at which the author has arrived. He holds that there is a safe ground for believing that the elementary constitution of muscle and other animal tissues is similar to that of a jelly, with this distinction—that its fibrinous or cellular form gives it due tenacity for the performance of its functions, but its water, albumen, and other constituents, hold the same physical relation as would water to gelatine in jelly. All tissues are formed of three different classes of substances,—those which constitute the ripe tissue, or the portion insoluble in water; those constituting the nutritive material of the tissues which are soluble in water, and *colloid;* and, lastly, the *effete* material which is also soluble but *crystalloid*, and consequently diffusible. In chemical composition the nutritive material and mature tissue do not differ, the change being simply morphological, and consisting in the assumption of an organised form. The whole of the phosphoric acid in muscular tissue is eliminated either as neutral tribasic phosphate or as pyrophosphate of potash. In flesh there exist, however, certain amounts both of phosphoric acid and of potash, which are not in the proportions of any known phosphate, and which take part exclusively in the actual formation of the mature tissue. The albuminous constituents of muscular tissue appear to be eliminated in the process of waste, in the state of kreatin, kreatinin, and other crystalloid bodies. Blood yields to flesh considerably more potash than is actually required in the formation of muscular tissue, the excess serving to eliminate the phosphoric acid by converting it into a crystalloid salt. In the lungs, however, Mr. Marcet thinks there is good reason to believe that the potash is eliminated, not exclusively as phosphate, but to a great extent as a crystalloid carbonate, in consequence of the carbonic acid emitted from the blood whilst passing through the lungs. The *effete* material in the muscles contains phosphoric acid and potash, in the respective proportions of 43 and 57; that in the lungs in the proportion of 11·32 to 88·68.

He finds that wheat flour, potato, and rice contain certain amounts of colloid phosphoric acid and colloid potash, which in these three kinds of vegetables exist nearly in the proportions of 1 part of total phosphoric acid to 0·55 of colloid phosphoric acid, and 1 part of total potash to 0·24 of colloid potash. This statement is based on the following experiment:—" 100 grammes of wheat flour were mixed with enough distilled water for the whole to be nearly liquid: this was placed in a dialyser, which was floated for 24 hours over a bulk of water equal to 8 or 10 times that of the contents of the dialyser: the volumes of the contents of the dialyser and of the solution outside were then determined.

The material in the dialyser was next dried and incinerated, and the ash analysed for the determination of the phosphoric acid and potash. On the other hand, a certain quantity of flour was carefully incinerated, and the phosphoric acid and potash were determined in the ash. A correction had to be introduced into the analysis by diffusion, owing to the colloid mass still holding a proportion of diffusible phosphoric acid and potash, depending on the relation existing between the volumes of fluid in and out of the dialyser."

We fear that this process is open to a serious objection. Analysis of the material in the dialyser will show not merely the phosphoric acid, soluble, though in a colloid state, but also whatever phosphoric acid may exist in a state insoluble in water, such as the phosphates of lime and magnesia. Before assuming any of the residual phosphoric acid to be in a colloid state, it seems necessary to show that it is, in part at least, soluble.

Passing over the Author's views on the modifications of the nutritive process in phthisis, we come to what may be regarded as the final summary of his investigations :—"That in Nature soluble matter is undergoing a perpetual transformation,—passing in rotation from the crystalloid into the colloid, and from the colloid into the crystalloid condition. Animal secretions and the products of decomposition of animal and vegetable tissues are crystalloid, admitting of their ready distribution through land and water by a physical process of diffusion. These crystalloid substances are transformed into colloids by plants, and used in that form as food for animals; and both plants and animals yield them back again in their original crystalloid condition. Chloride of sodium alone appears to be an exception to this rule."

To a very considerable extent this generalisation must be regarded as well founded. The chief constituents of food—albumen, fibrin, casein, gelatin, glucose—are undoubtedly colloid. Lactose, indeed, which is a truly crystalloid substance, is a necessary of life to the young of all mammalian animals. On the other hand, the excretions are rich in compounds admittedly crystalline.

La Terre Vegetale. Geologie Agricole. Par STANISLAS MEUNIER, Paris: J. Rothschild.

WE have here a terse, plain, and lucid manual of agricultural geology, with especial reference to the nature and formation of the vegetable soil. The main materials of mould—sand, clay, lime, and humus—are described, and their functions pointed out. Plain instructions are given for a physical analysis of soils, followed by a description of the various types of mould, such as the loamy, the clayey, &c

In the second part the Author explains the formations of soils, which he divides into *local*,—*i.e.*, such as are produced on the spot where they are found by the decomposition of rocks,—and *soils of transport*,—the deltas, polders, and warps brought down by rivers, and deposited at their mouths or along their banks. In some parts of the world, especially in Mexico and the island of Candia, transported soils are met with which appear due to the action of wind rather than to that of water.

Turning from theory to practice, the Author next treats of the amelioration of soils. He follows the useful French custom of dividing the substances which the farmer may have occasion to add to his fields into two great classes, amendments or correctives, and manures, properly so-called. Under the former head he ranks lime, marl, clay, and even water, whether the latter is added by irrigation or withdrawn by drainage. The circumstances under which these operations are necessary, and the effects produced, are well described. The proportion of lime used in France appears to be only one-fourth to one-fifth of the quantity applied to the same surface in England. Irrigation, as the Author judiciously points out, produces more beneficial effects in Southern France and in Italy than in colder and moister climates. On the other hand, it is in the latter that the results of drainage are most conspicuous. In treating of mineral manures the Author reminds his readers of a truth too often overlooked,—*i.e.*, that a manure cannot prove uniformly and equally beneficial upon all soils and under all circumstances. Thus, a phosphatic manure will be found useless in situations where the sub-soil yields a supply of phosphoric acid equal or superior to the amount annually withdrawn by the crops.

A valuable feature of the work is an agricultural map of France, drawn up by M. A. Delesse, Professor of Agriculture at the Ecole des Mines. This map shows not merely the respective amounts of land under the plough, and occupied by vines, forests, and meadows, but it indicates the average returns per hectare in different parts of the country. It appears that only 6 per cent of the land in France yields a net return exceeding 80 francs per hectare; 10 per cent yields revenues ranging from 80 to 60 francs per hectare; in 20 per cent the returns range from 60 to 40; in 44 per cent the yield is comprised between 40 and 20 francs per hectare; whilst in 20 per cent the proceeds of cultivation range between 20 francs and nothing. If we consider that a hectare is, practically speaking, 2½ statute English acres, we must admit that there still remains great room for improvement. The woodlands are stated as yielding a nett average return of 20 francs per hectare; arable lands, 42; vineyards, 69; meadows and pastures, 72; and gardens, 120.

This work is a valuable addition to the series of agricultural and horticultural manuals with which the name of the publisher is honourably associated.

Report of the Commissioners of Agriculture for the Year 1872. Washington: Government Printing Office.

WITHOUT pleading guilty to a wish to "Americanise" our institutions, we cannot help pointing out, as a feature worthy of imitation, the accessibility in the United States of all kinds of official reports, whether of Government departments, municipal boards, &c. Documents of such kinds are to be met with in all literary institutions, public libraries, &c., and fall into the hands of every newspaper editor. As a consequence, the valuable information which they contain is disseminated over the whole country.

The volume before us contains the statistical reports of the principal crops in every State of the Union, showing the total amount of crop, average per acre, number of acres in each crop, value per bushel, ton, or pound, and total valuation. There is a Report on the forests of the United States. Contrary to general opinion, the area of forest bears a smaller proportion to the total area of the country in the United States than in Russia, Norway, Sweden, and probably in Germany. Reports of the exports of agricultural produce are given in great detail.

The Report of the Entomologist and Curator of the Museum gives very interesting facts concerning the ravages of *Anarsia lineatella*, *Aræocerus prunella*, *Romalea microptera*, and other of the insect pests with which the American farmers are greatly exercised. A decoction of the berries and leaves of the pride of China (*Melia azedarach*) has been recommended as an insecticide, but no mention is made of *Pyrethrum roseum* and *carneum*, so much extolled in some parts of the world as the sovereign remedy for mosquitoes and sand-flies.

The Chemist to the Department gives the analysis of a number of natural fertilisers, among which we may mention a marl from Glymont, Maryland, containing 4·41 per cent of tribasic phosphate of lime, and another phosphatic marl from Charleston, containing 16·34 per cent of insoluble, besides 1·38 per cent of soluble, phosphoric acid. The "poison-soils" of Dallas county, Texas, are characterised by the entire absence of sulphur-compounds and the large proportion of humus in insoluble states. Cotton, fruit-trees, and root-crops invariably perish if planted in this soil. The chemist suggests, in our opinion quite correctly, thorough under-drainage, sub-soil breaking, and a heavy dressing of lime and gypsum. The paper on the refuse of cities and towns is worth careful perusal, trite as the subject has become. Mention is made of the pneumatic system of Liernur as worthy of more careful attention than it has yet received.

The work throughout is a perfect store-house of valuable information for all who are directly or indirectly interested in agriculture.

Report of the Sanitary Committee of the Board of Health on the Concentration and Regulation of the Business of Slaughtering Animals in the City of New York. New York: D. Appleton and Co.

We learn from this Report that New York, like London, suffers from the nuisance of intramural slaughter-houses. There are, in one district of the city, fifty-four of these establishments, "for the most part of the cheapest construction; the yards roughly paved, the sewerage imperfect, and the means of cleansing of the most imperfect character." As a necessary consequence there are, in the immediate neighbourhood, "large numbers of establishments devoted to fat-melting, lard-rendering, hide-curing, gut-cleaning, tripe-curing, glue-making," and other unsavoury trades. The reason why these *foci* of fever and zymotic disease are thus concentrated in a certain district is one from which we, in London, may learn an important lesson. "The slaughter-houses have occupied the area described since 1868, when they were removed from above 40th Street by the Metropolitan Board of Health. At that time this section of the city was sparsely settled, and the Board allowed them to locate above the street described without regard to the inevitable wants of the rapidly increasing population." Hence those interested in the nuisance now assert that "having removed their business above 40th Street in obedience to the orders of the Board of Health, in 1868, and having permits to occupy their present locations, this Board is bound in good faith not again to remove or disturb the business of the remonstrants." Is not this exactly analogous to the position of London as regards the burial of the dead? The old City churchyards have been closed, and the suburban cemeteries, foolishly permitted to be laid out in elevated localities, are fast becoming an equal nuisance.

The New York Sanitary Committee propose the abolition of private slaughter-houses *in toto*, and the opening of public abattoirs, where all cattle will be slaughtered under official inspection. Such a step, in addition to its direct sanitary action, would abolish the driving of infuriated oxen through crowded streets, needless cruelty in slaughtering, the introduction of diseased and putrid meat into the markets, and a long array of subsidiary evils. We fear that London is farther from reform in this point than New York. Recent legislation seems to have given a new lease to these nuisances.

The Safe Use of Steam, containing Rules for the Guidance of Unprofessional Steam-Users. By An Engineer. London: Lockwood and Co. 1874.

In plain-spoken words the writer, as a practical engineer, offers wholesome advice to all who have the care of steam-boilers and the control of steam-power. There can be little doubt that due attention to such rules as those laid down in this little work would go far to prevent boiler-explosions, a class of accidents which in most cases may be traced to the ignorance or recklessness of those who are in charge. "Be slow to believe, with some, that explosions occur from mysterious and unpreventible causes. For one explosion whose cause is obscure there are ten," says the writer, "which can be traced to causes which an observance of the foregoing rules would assist to remove."

Principes de Science Absolue. Questions de Science Absolue ou Science basée sur une réduction naturelle, intégrale, analogique de l'unité du fait absolu. Par M. JAMES THOMSON. Paris: J. Rothschild.

WE have here a goodly volume, superfine paper, acres of margin, clear bold type, and a singularly tasteful binding. But the contents! We find a string of rhapsodical sentences, printed in a fantastic medley of italics, small capitals, and common characters—not the first time, by the way, that typographical eccentricities have done duty for argument—and lines of asterisks, in the manner of Tristram Shandy, to be filled up at pleasure by the charitable reader, or to be taken as a proof that M. James Thomson, like the parrot in the story, "thinks more than he says." English, or possibly Scotch, as is the author's name, his language is purely French,—not, however, the French of science or philosophy, but of "fast" daily life, interspersed, like boulder-clay, with a new-coined terminology. He aims, evidently, at being profound, original, witty, and aphoristic. He succeeds, to a marvel, in being flippant, grotesque, absurd, sometimes positively loathsome. In support of so heavy a charge, we will quote a passage, after which few readers will care to inquire further:—

"Every individual convicted of *aggressive movements*, atheistic, socialistic, communistic, or anti-social (discourses, writings, conspiracies, rebellions, thefts, pillages, incendiarisms, assassinations, &c. . . . .) is *placed outside the law*, *degraded* from his *rank as man*, and *assimilated* to the *brutes*.

"This *degradation* and this *assimilation* signify that every individual thus designated is liable, according *to the degree* of his *criminality*, to be condemned to slavery, to torture, to the combats of the circus, whether against other *negativists* or against ferocious beasts; to death; to the shambles.

"It is certain that this last punishment involving the establishment of *shops for selling the flesh of atheists* constitutes a culinary question calculated to chill the souls of gourmets.

"But at any rate a cutlet of atheist is still better than a cutlet of horse, mule, ass, rhinoceros, cat, &c. . . .

"If Paris, when beseiged, had only consented to taste *the atheist*, it might have escaped easily, not alone from the enemies without, but from those within.

"If, in the future, silly *atheophagists* should pretend that MM. the *mimitaptap* of *negation* are not tender, we should advise to pickle them."

Whether this is a stupid jest, or sober (?) sadness, we must pronounce it the most revolting passage which it was ever our misfortune to read. We have here a man original in atrocity at least; capable of surpassing even the Commune and the Inquisition. We have little love or respect, truly, for Robespierre and Cluseret, for Torquemada and St. Dominic. But even they did not eat their victims. "Domini Canes," we suspect, if not too merciful, were at least too judicious.

We find, further, a declaration that the peoples of the true white races "(Germans, English, Americans, French, &c.) are called *naturally*, *necessarily*, *legitimately*, in order to procure the *social elevation* of their own *masses* to *dominate* and to *exploit** the inferior white races (Hindoos, Arabs, &c.), the yellow species, and those of the black species." This is evidently a fundamental idea of "Science Absolue," since it occurs more or less explicitly in several parts of the work. France is repeatedly advised to take possession of China, which would yield her an annual revenue of three or four milliards, whilst Africa would also prove very valuable. All this "domination and exploitation" is indeed to be restricted within the limits of "affirmation," to be marked out, we presume, by M. James Thomson. But whatever theoretical distinctions might be drawn in practice, the result would be simply slavery, and as such it could not fail to prove a curse alike to superior and inferior. The Author holds that the human race consists of three distinct and originally created species, the white, the yellow, and the black, and attempts to prove that this view is in harmony with the Mosaic cosmogony. He informs us that the serpent which tempted Eve was no reptile, but a "heathen Chinee," and that her fall—the eating of the forbidden fruit—was an act of conjugal infidelity of which the result was the birth of Cain.

We suspect that Biblical critics will find his exegetics as little satisfactory as is his ethnology. It is simply inconceivable that the Aryan race should speak of the Chinese as "sons of God," Elohim or Asar, beings superior to themselves. To the Teuton, whom the Author considers as the purest specimens of the "white race," the Asar were his own deified ancestors, ruled over by Odin, king of Gods and men.

M. Thomson informs us that "men are born atheists or communists, just as they may be born rachitic or scrofulous."

* We have no English word which exactly covers the French "exploiter" as applied to persons. It means to "use up," to "apply to our own purposes," &c.

Yet with admirable consistency he upholds the doctrine of free-will, and repeats the venerable fallacy that those who doubt it have no right to punish a transgressor. How? The wolf and the tiger must of necessity prey either upon our flocks and herds, or upon ourselves. Yet not the less, but rather the more, do we poison or shoot them. If a murderer pleads necessity, society replies that the very same necessity dictates his elimination.

The Author glorifies Adam Smith and Malthus, but rails at J. Stuart Mill, and denounces free trade. Wherever free trade exists, he tells us, "there may be found an Englishman and a dupe." Indeed for England and Englishmen he has little affection. He accuses our country of being the focus of his *bete noire*, the International. He has petroleum on the brain, and is delighted with himself for having turned "proletariate" into "petrolatariate."

But the reader will ask, where is the science? Echo answers "where?" There are, indeed, a few vague generalities, there are positives and negatives, animisms and dynamisms. We are told that the modes of contraction of dynamism are gravitation, weight, cohesion, capillarity, and acoustics, whilst its modes of expansion are heat, electricity, light, and magnetism. We are to ascribe "To the combination of *contractive* DYNAMISM with *infinitesimal* MATTER the apparition of the *germ* of the earth. To the combination of *expansive* DYNAMISM with *contractised* MATTER, the apparition of the *inchoative sketch* of the globe; in a word, the *terrestrial chaos*, organic and inorganic.

To the combination of AFFINITY, or soul of the fourth degree (eumorphism) with a portion of the *dynamised* MATTER, the apparition of the MINERAL kingdom. To the combination of RUDIMENTARY INSTINCT, or soul of the third degree, with a part of the *dynamised* matter, and with *affinity*, the apparition of the vegetable kingdom.

To the combination of TRUE INSTINCT, or soul of the second degree (*animal intelligence*), with a part of the *dynamised* MATTER, with *affinity* and with the rudimentary instinct, the apparition of the ANIMAL kingdom.

Finally, to the combination of INTELLIGENCE, properly so-called, or soul of the first degree (*true soul*, or *reason*) with a part of *dynamised* MATTER with the rudimentary instinct, the true instinct, and with *affinity*, the apparition of the human kingdom."

Such propositions as this can be produced by the mile as easily as calico. But they are not science. They shed no new light on any department of nature; they point the way to no researches, and are as incapable and as unworthy of refutation as of verification. They are essentially cold, sterile, lifeless.

A denunciation of Darwin occurs as a matter of course. The manner of the onslaught is quite in character. M. James Thomson does not argue—he shrieks, chatters, "mops, and mowes" at the great naturalist and his fellow-labourers, in a manner more simian than scientific. But there are no facts which have not been already taken into account; no arguments which have not been repeatedly refuted; no attempts to supersede the doctrine of Evolution by any profounder and wider generalisation, or to account for the phenomena which before its promulgation were unconnected riddles. There is, indeed, a new epithet, "pithecolatry!" It is remarked that if species are not permanent, then the title "Origin of Species" involves a contradiction. How little such an Old Bailey quibble affects the matter at issue is self-evident. To all this Mr. Darwin may well reply, "nolo laudari." M. Thomson is prone to Latin quotations, and can doubtless supply the rest.

It can serve no good purpose to wade any further into this chaos of philosophy, "falsely so-called," of theology, politics, morals, ethnology, and gossip, which the Author has presumed to designate "Absolute Science." Of that "*pourriture*" of the modern world to which he so often refers, this book is certainly not the least offensive symptom.

Fragmentary Papers on Science and Other Subjects. By the late Sir Henry Holland, Bart. Edited by his son, the Rev. F. J. Holland. London: Longmans and Co.

For us, in the last quarter of the nineteenth century, a strange interest attaches to the reminiscences of one who may be said to have stood by the cradle of modern science. "More than sixty years ago," says Sir H. Holland, "Davy showed me, at the Royal Institution, the minute globules of sodium and potassium just obtained from the fixed alkalies. In the same laboratory, the birth-place of so many great discoveries, I witnessed his first experiments on the chemical actions of the voltaic current. Very few years later I heard Dalton expound, for the first time, that Atomic Theory which gave the earliest impulse to those researches of which organic chemistry, present and prospective, is the most wonderful exponent. Yet later, in the theatre of the Royal Institution, I was one of a small party to whom Faraday showed the spark he had just succeeded in drawing from the magnet, the forerunner of those marvellous powers which have since been elicited from the same source." Nor did this early interest in Science forsake the author in his later days. To the last he must have watched with eager interest the progress of discovery, and have kept himself acquainted not merely with the methods and the general results of Science, but even with the minutest details of modern research.

Still it will be felt by the reader that Sir H. Holland writes about Science rather as might a refined and thoughtful scholar than as does the man of Science *pur sang*. His interest lies, after all, in persons rather than in things, physical facts serving him mainly as an introduction to metaphysical reflection. He contemplates rather than speculates. His mind ever reverts to Plato, Aristotle, Lucretius. A striking instance of this tendency may be found in the outset of his Essay entitled "Plurality of Worlds: Are Other Planets Inhabited?" He here informs us that "Neither in the Old or New Testament do we find a distinct answer to the question, though perhaps a few inferential allusions to it. The same may be said of the classical writers: Plato, Aristotle, Lucretius, and Seneca, as far as I can recollect, are silent on the subject. Pliny, who grasps at everything known or imagined, is equally so." This seems to us an instance of what Whewell calls the "commentatorial spirit," so prominent in the Middle Ages, which, instead of enquiring into things themselves, asked rather what former authors had said on the subject.

The most valuable of the Essays before us are, as might be expected, those bearing upon organic nature, especially "Life on the Earth: Relations of Man to Other Animals." Sir Henry here very justly pronounces Bichat's well-known definition of life, "La vie est l'ensemble des fonctions qui résistent a la mort," and that of the "Encyclopédie," "La vie est le contraire de la mort," as "too epigrammatically negative to serve any use." He might have gone further, and pronounced Bichat's utterance one of the greatest absurdities ever uttered by a man of mark. "Life is the sum of the functions which resist death." But what is "death"? The termination of life. If we, then, insert this explanation in Bichat's definition in place of its equivalent "death," we read—"Life is the sum of the functions by which the termination of life is resisted," which is nonsense. Sir H. Holland well points out that these definitions sin by omitting "that which is the very essence of life, viz., that of reproducing life more or less like in kind to itself." But is the search for such definitions scientific? We may ascertain the properties of life, as we do those of electricity or of gravitation. But can we possibly ascertain the essence of any of the three?

We are happy to find that the author has no sympathy with that purblind egotism which would represent man as differing from the lower animals not in degree, but in kind. "The philosopher," he writes, "looking on the dog crouched at his feet, sees in him an animal with organisation akin to his own; with intelligence, memory, feelings, and passions of the same kind, however different in degree and in manner of use; with appetites and necessities of life similar, also, though more in subordination to instincts and hereditary habits of the species. The idle spectator gazes on the anthropoid ape with mere merriment at this mockery of human form and gesture. The man of deeper thought cannot stand in face of these creatures without some feeling of awe in the contemplation of that mysterious scheme of creation which has brought them thus near to himself in the scale of animal being."

The view that the rest of the animal world exists solely in reference to man he rightly treats as a "vulgar notion." Every part of Natural History, and very especially the history disclosed to us by fossil remains, utterly annuls any such conception. It would not be too much to affirm that not one-hundredth part of the animal creation, counted by species, has relation, direct or indirect, to man's existence on earth.

Of the reasoning power of brutes, he remarks—"No happier definition can be given than that of Cuvier: 'Leur intelligence exécute des opérations du même genre.'"

Into a large part of these papers—such as those on Evil in the World, the Perfectibility of Man, Natural Theology, Differences of Religious Belief, Scepticism and Credulity, History, Shakspeare, &c.—we cannot here enter. If the work contains nothing novel in fact or in generalisation, it is, we think, not unsuggestive. The breadth and liberality of its spirit might well atone for even greater deficiency in originality and power.

On a Peculiar Fog seen in Iceland, and on Vesicular Vapour. By R. Angus Smith, Ph.D., F.R.S., &c. London: Taylor and Francis.

Meteorologists generally suppose that clouds, fog, &c., consist of particles of water in a hollow or vesicular form, something like minute balloons or soap-bubbles. One of the originators of this view was Edmund Halley, the astronomer, who, in the "Philosophical Transactions" (abridged, vol. iii., p. 428), explains the "rising of vapour by warmth, by showing that if an atom of water were expanded into a shell or bubble so as to be ten times as large in diameter as when it was water, such an atom would become specifically lighter than air." It is scarcely needful to remind the reader that water, in order to become specifically lighter than air, would require to be expanded very far more than ten times. In 1743, Gottlieb Kratzenstein, of Halle, in his "Theorie de l'Elévation des Vapeurs et des Exhalations" (Bordeaux, 1743), maintained that "vapours are hollow vesicles," though he admits that they have no absolute lightness and cannot lose their specific gravity. Hamberger, Professor of Physics and Medicine at the University of Zena, also published, in the same year, a dissertation on the subject, to which a prize was adjudged by the Academy of Bordeaux. In this he shows that the weight of water cannot be reduced by increasing the size of the vesicles, or, indeed, by making vesicles at all.

H. D. de Saussure, in his "Essais sur l'Hygrométrie" (p. 282), assumes the existence of vesicles, or hollow spheres, and endeavours to demonstrate their presence in vapour experimentally, and to render them visible.

The attention of Dr. R. A. Smith, from whose pamphlet the above sketch of opinion on this question is substantially taken, was drawn to this subject by a remarkable fog which he observed one afternoon in July, at Reikjavik, Iceland. The peculiarity of the phenomenon consisted in "a larger size of particles than I have ever seen, and that the flatness with which it fell on the ground, and the lumbering mode of rolling, distinguished it from all fogs which I have seen." The people of the town, as well as Dr. Smith and his party, took it at first for dust, or smoke conveying unusually large particles. The wind had nothing to do with the matter, as the fog arose simultaneously from the sea and from a small lake behind the town, and converged in the streets. The particles were found on examination to be at least ten times larger than those described by Saussure. They seemed, moreover, quite solid,—if the expression may be used for a liquid body,—without any hollow centre. This observation accordingly led Dr. Smith to examine the evidence offered by Saussure and others in favour of the vesicular theory. But he found nothing in the least conclusive. The notion of hollow particles "is a vague inference from certain of their qualities, and their supposed analogy to soap-bubbles." The Author contends that the hypothesis of hollow particles, besides being unsupported by facts, is unnecessary, as fine but admittedly solid particles can remain for a very considerable time suspended in the air. As an instance of this kind, he remarks—"Times out of number I have observed, on calm summer evenings, a cloud of smoke from a steam-boat funnel lying for miles in length, at a height very little different from that of the funnel out of which it issued. . . . In these cases have we anything to look to but the size of the particles? They are so small that their resistance to the atmosphere is diminished to the utmost, as the resistance of the air is increased so much in proportion to the weight that they cannot fall rapidly."

The subject is one which demands more attention from physicists and meteorologists than it has hitherto received.

Principles of Metal Mining. By J. H. Collins, F.G.S. London and Glasgow: W. Collins, Sons, and Co.

This little work gives a general and intelligible view of mining operations. One of its greatest merits is that the technical language of the Cornish miners—often a source of no little mistification to geologists, mineralogists, and chemists—is fully explained. The Author insists, very justly, upon the importance of scientific training to practical miners. Referring to instances of valuable minerals being thrown away from ignorance of their nature, he continues—"But it is only the mistakes *which have been discovered* that can be known: it is very probable that the majority of such mistakes are never found out at all. The only safeguard for the future is in the multiplication of mineralogical observers."

Prodromus of the Palæontology of Victoria, or Figures and Descriptions of Victorian Organic Remains. Decade I. By F. McCoy. Melbourne: John Ferres.

This work is the first number of an Appendix to the Geological Survey of Victoria. It is to contain figures and descriptions of the more characteristic fossils of each formation. The present number contains two plates of species of Graptolites, from which the Author determined the Lower Silurian geological age of the slates containing gold reefs. Then follow plates of the extinct fossil wombats from the gold cement of Dunolly, which afforded proof that the Victorian gold-drifts, like those of Russia, are of the age of the mammaliferous crag of the English Pliocene Tertiary age. Next are two plates of volutes, representing the *Volutilites* of the Barton clay formation of Hampshire; a plate of the *Cycadeous* plants characteristic of the oolitic coal-fields of India, China, Virginia, &c. Finally come plates of characteristic genera of fossil vegetation of the palæozoic coal formation, and of fossil star-fishes from the Upper Silurian rocks.

The work, when complete, will be a most valuable addition to palæontological literature.

A Handbook of Hydrometry. By James Boddey Keene. London: Pitman.

In this little treatise the Author gives an account of the construction and uses of the various instruments employed for taking the specific gravities of liquids. With hydrometers the same confusion prevails as with weights and measures. Almost every country has its distinct scale, and to convert the indications of one of these into another is not always an easy matter. Throughout the civilised world water has, indeed, been adopted as the starting-point, but it is not everywhere taken at the same temperature. "In France water is taken as unity at its greatest density, which is 4·1° C. or 39·4° F. This point has the advantage of presenting no appreciable change of density within a slight range above or below it. It is, however, so far below the average temperature of the year as mostly to require artificial cooling, and to be liable to rapid change of temperature."

In England, with our usual love for inconsistency, we have three standards. The degree ordinarily used is 60° F. = 15·5° C., but the imperial gallon is fixed at 62° F., and the proof point for Sikes's hydrometer, as laid down by Act of Parliament, is 51° F. "These differences," says the Author, "very justly serve no practical purpose, and only tend to confusion."

It is somewhat remarkable that amongst the various hydrometers here enumerated there is no mention of the two most generally employed in England—Twaddell's and the direct specific gravity instrument. Baumé's hydrometers, so much employed on the Continent in chemical works, dye works, &c., are worthy of all condemnation. They are graduated in a very arbitrary and often careless manner, and their indications are not readily calculated into direct specific gravity.

The author has devised a modification of Sikes's hydrometer, ordinarily used by Revenue Officers, which appears to have a decided advantage.

Consumption and Tuberculosis, their Proximate Cause and Specific Treatment by the Hypophosphites. By J. F. Churchill, M.D. London: Longmans and Co.

Medicine was reproached by the late Mr. Buckle as being still in the "theological stage." "Not one of its so-called specifics," continues the historian of civilisation, "has been discovered deductively, or even justified *a priori*." The Author holds that in the hypophosphites we have a specific for "pulmonary affections arrived at *a priori* by hypothetic induction, verified by clinical experiment, and confirmed by a concordance of the widest and most numerous array of facts ever brought forward in pathology."

Dr. Churchill was led, by his observations and experiments, to assume—provisionally, at least—the two following propositions:—

"The tubercular diathesis is owing to a diminution in the system of the phosphorus element."

"As this element fulfils the function of a combustible body, it must be in a degree of oxidation inferior to that of phosphoric acid."

He then proceeded to submit his theory to practical verification. Free phosphorus being, from obvious reasons, inadmissible, the choice lay between the oxide of phosphorus and the phosphorous and hypophosphorous acids. He selected the latter in the state of a lime-salt, and by experiments upon himself established the fact that it could be taken in 6-grain doses without any ill effects. He then proceeded to actual practice. Cases of consumption are, unfortunately, sufficiently common, and the results of the ordinary treatment are so little satisfactory, that the physician is certainly morally justified in adopting any new system which holds out any rational probability of success. A number of cases, with their results, are described. Not being ourselves connected with the Faculty we cannot, of course, view this matter professionally. But if we are asked whether we—judging from the "standpoint" of the man of science—consider that Dr. Churchill's facts support his theory, we reply without hesitation in the affirmative. Of course we do not admit, nor does the author contend, that hypophosphites, no matter of what base, no matter in what dose, or in what admixture, will inevitably cure phthisis, irrespective of the stage of the complaint and of possible complications. Such a proposition could only be upheld by the charlatan or the routinist, as distinguished from that too rare character—the thoroughly scientific physician. The following passage will show that Dr. Churchill by no means lays claim to such infallibility:—"What science or what art but that of medicine has ever pretended to an 'infallible' process? 'Infallible' means unconditional. Art, on the contrary, is the attainment of definite ends by definite means under certain given conditions, and the object of all science is the discovery of these means and of the conditions which are to guide us in their use."

There are few scientific men, in the true sense of the word, who will be able to read this book without pleasure and profit, and without recognising the profundity, the acuteness, and the originality of the author.

Reverting to the Preface we find a mournful, and only too true, picture of the difficulty experienced in obtaining fair play for a novel idea:—

"To whom could such an appeal be made?

"To the statesman?

"He will point a speech with a sanitary conundrum, and think no more of the matter.

"To the health-reformer?

"He is all eagerness that the people should be dragooned into health, even as their forefathers were once dragooned into holiness, and he hopes to be one of the dragooners: beyond that he goes not.

"To the man trained in the severe and patient methods of positive science?

"He will be afraid to venture amidst the discordant facts, the rash assumptions, and the ever-shifting fallacies of the so-called science of therapeutics.

"To the philanthropist, our poor clipt and crest-fallen substitute for the old knight-errant?

"Even he will fail to see that there is here the means of doing more good than by the endowment of many charities."

The Author quotes, from a medical authority whom he does not name, the theory that "pulmonary consumption is a bountiful dispensation of Providence to eliminate the weak and imperfect, and thus purify the earth, in order to save us from becoming a race of pigmies, of sickly dwarfs, and eventually dying out." It is distressing, and at the same time incomprehensible, how any man who has enjoyed any rudiments even of a scientific training can give vent to such an unscientific outbreak of gratitude. Pulmonary consumption prevails mostly during the *prime of life*, and numbers of its victims have consequently, before they are carried off, become parents! If it were the plan of Providence "to eliminate the weak and imperfect," the disease, surely, ought to occur in early childhood; or, better still, the first symptom of debility in any organism would be the cessation, temporary or permanent, of the reproductive power. But we often find that sickly beings, both animal and vegetable, are more prolific than the healthy. Shall we never be able to free ourselves from the delusions of teleology?

We can only hope that our voice may be, at any rate, of some little service in securing for Dr. Churchill that candid hearing which is all he demands.

Text-Book of Botany, Morphological and Physiological. By JULIUS SACHS, Professor of Botany in the University of Würzburg. Translated and Annotated by A. W. BENNETT and W. T. THISTLETON DYER. London: Macmillan and Co.

It has been insinuated, somewhat wickedly, that since the disuse of the Linnæan system Botany has become much less popular among ladies than it was heretofore. The change, indeed, is complete. We can imagine a botanist of the old school whose highest ambition it was to collect specimens from our scanty British flora, and to ascertain their names according to an artificial system, feeling bewildered at a work such as the one before us. He would find classification no longer the be-all and end-all of plant-lore, developed rather in its fundamental principles than in the minutiæ of detail. He would find species grouped together, not with regard to one single set of organs, but in accordance with their entire structure and the laws of their evolution. Had he the patience and perseverance to study the volume, he would be bound to confess that the botany of the present, if making larger demands upon the faculties of his votaries than did the botany—we might say the herbalism—of the past, it rewards their exertions in a far more bountiful manner.

The work of Professor Sachs is so thorough-going, so complete, and presents the results of the most recent investigators in such a clear and intelligible manner, that criticism becomes almost superfluous. We have certainly no English work which can take its place as a manual of vegetable morphology and physiology. The translators well deserve the thanks of all friends of the organic sciences for having placed this rich treasure within the reach of the English student.

The chapters on the elementary constituents of the food of plants; on assimilation and metastasis (*stoffwechsel*); on the respiration of plants; and on the action of temperature, light, electricity, and gravitation upon vegetation, are most excellent, and may be consulted with advantage by the physicist and the chemist, as well as by the botanist.

The last chapter is devoted to that difficult but fascinating topic, the origin of species. It may be interesting to learn what are the views of so eminent an investigator as Prof. Sachs upon this crucial question. He declares that—"The scientific basis for the theory of descent rests in the fact that it alone is able to explain in a simple manner all the mutual relationships of plants to one another, to the animal kingdom, and to the facts of geology and palæontology, their distribution at different times over the surface of the earth, &c.; since it requires no other hypothesis than descent with variation and the continued struggle for existence which permits those forms only to persist that are endowed with sufficiently useful properties, the others perishing sooner or later. But both these hypotheses are supported by an infinite number of facts. The theory of descent involves only one hypothesis which is not directly demonstrated by facts, namely, that the amount of variation may increase to any given extent in a sufficiently long time. But since the theory which involves this hypothesis is sufficient to explain the facts of morphology and adaptation, and since these are explained by no other scientific theory, we are justified in making this assumption."

The Transit of Venus. By GEORGE FORBES, B.A. ("Nature" Series.) London and New York: Macmillan and Co.

THE work before us is based upon a paper originally read before the Philosophical Society of Glasgow, and subsequently published in "Nature." There is scarcely any point connected with the entire question but receives a lucid explanation. The reader will learn what is a transit of Venus, the reason of its rarity, and its value in enabling us to calculate the true distance of the earth from the sun. We also find an explanation of the term "parallax,"—not, of course, of the gentleman who assumes that name,—and a brief notice of other methods by which the sun's distance may be determined, such as the parallax of Mars when nearest the earth, which occurs every fifteen years; the method founded on the velocity of light and the procedure of Leverrier, materials for which are gradually accumulating. The

Author then gives an account of the various methods in which a transit of Venus may be utilised, namely, the determination of the times of contact, at different stations, of known longitudes, and the determination of the least distance between the centres of the sun and Venus during the transit, as observed from different stations. This last determination may be conducted either photographically, heliometrically, or by the method of durations. These methods and the needful apparatus are described at some length. We cannot help referring to the photographic method as a beautiful example of the inter-dependence of the sciences, and how one may furnish methods to another. Who would have dreamed, half a century ago, that chemistry would—in photography and spectroscopy—furnish the astronomer with two novel and valuable means of research? The book concludes with an account of the stations selected by various nations, and an enumeration of the English observers.

To every one who, without making astronomy his speciality, wishes to have a full and clear knowledge of this subject in all its bearings, Prof. Forbes's book is indispensable.

The Province of Psychology. The Inaugural Address at the First Meeting (April 14, 1875) of the Psychological Society of Great Britain. By the President, Mr. Serjeant Cox. London: Longmans and Co.

This Address is, in the fullest sense of the word, "inaugural." Its object throughout is to define the work of the Society, and this object is admirably carried out. In the Preface we are told that the Psychological Society embraces no creed, supports no faith, contemplates no theory, has no latent designs, but proposes only to collect facts and investigate psychological phenomena, precisely as other scientific societies investigate the phenomena of their respective branches of knowledge; and, further, that a very erroneous notion respecting its objects appears to prevail, viz., that it has been established with a special view to the promotion of a new faith, to which the name of "Spiritualism" has been given. This notion is vigorously and authoritatively contradicted by Serjeant Cox, and the undeviating tone of the Address throughout is in perfect harmony with such contradiction. If Spiritualism be a delusion, and if the Psychological Society rigidly and vigorously follow the course which Serjeant Cox has mapped out for it, this Society will assuredly do more than has ever been done hitherto towards the removal of the delusion; if, on the other hand, the error is on the other side, its refutation will ultimately be effected by the same means.

The function of the Society, as defined in this Address, is to apply to all questions connected with "*Life, Mind, and Soul*" the strictest methods of inductive investigation. In reference to the evidence demanded, Serjeant Cox says—"It is an inflexible rule of our Courts of Law that the best evidence only shall be accepted, and that secondary evidence shall not be received when primary evidence can be had. It is a rule of reason and common sense. Its observance is no less essential to scientific investigation, and I trust that by this Society no relaxation of it will be permitted. Necessarily we shall be called upon to deal with some reports of alleged phenomena of rare occurrence and transcending common experience. It is scarcely necessary to remind the members that a higher degree of proof should be required in proportion to the strangeness of the phenomenon, and that strictest scrutiny must be made into the minutest details before the Society will be justified in giving to it a place among its records of psychological *facts*."

Theology is to be excluded from the deliberations of the Society. The desirability of this is unquestionable. The subjects of investigation are precisely those which, without such exclusion, would inevitably drift into sectarianism, and open the portals of superstition. At the same time, it is equally obvious that the difficulty of keeping clear of theology will be considerable, but this difficulty must be boldly faced, as the danger and mischief are precisely proportionate to the proximity of the subjects. Serjeant Cox treats this part of the subject but lightly; and we think that a clear definition of the boundaries between psychology and theology is much needed, and we venture to suggest the following for the serious consideration of the Society:—*Psychology is a science of experiment and direct observation, while neither direct observation nor experiment are possible in theology. Indirectly* the Society may do good service in promoting sound and wholesome theology; for although the Society, as a Society, will deal only with the facts and laws of the operations of human life, mind, and the "psyche,"—as Serjeant Cox prefers to name the possibly separate essence,—and thus will deal with matters of observation and experiment, the individual member, each for himself and on his own responsibility, may make such theological inferences as present themselves to his own mind. The separation of these subjects in the Proceedings of the Society does not necessarily imply any separation of them in the thoughts of the individual members, and still less does it suggest any exclusion of theology or religious sentiment.

Many special subjects of investigation are suggested in this Address,—such as the great question of matter and non-matter; whether our consciousness, our individuality, ourselves in fact, are merely cerebral functions beginning and ending with cerebral action, or whether a separate something or "psyche" exists which uses the brain and bodily mechanism as the organ of its manifestation. Whether the brain acts as a whole, or is composed of separately acting parts, as affirmed by Gall. Whether each hemisphere of the brain is a distinct organ conferring duality of mind, as also stated by Gall and re-affirmed by Brown-Sequard. What is matter? "Is matter merely the incrustation of spirit-atomic structure aggregated into molecular structure on the surface, as it were, and passing continually from one to the other—as the atmosphere becomes visible in the form of a cloud when it comes in contact with a colder body? Or is it that the vast interspaces between the worlds, those regions void to our senses, in which those countless worlds are as grains of dust, are really thronged with life which—because it is not of molecular structure—is imperceptible to our very limited material senses?"

Heredity and Hybridity are especially mentioned, and all those phases of the doctrine of Evolution which relate to the Descent of Man. Sleep, Dreams, Somnambulism (Natural and Artificial), Delirium, Insanity, and the Influence of the Mind upon the Body, are also among the subjects named and partly discussed in this Address.

We dare not go further than the mere enumeration of these, as any discussion of either would lead us far beyond the limits of this notice, and must conclude by heartily wishing that the Psychological Society will strictly adhere to the programme and principles prescribed by Serjeant Cox. If it does so, it cannot fail to do good service to the highest of all the sciences.

Principles of Mental Physiology; with their Applications to the Training and Discipline of the Mind and the Study of its Morbid Conditions. By William B. Carpenter, M.D., LL.D., F.R.S., F.L.S., F.G.S., &c. London: H. S. King and Co.

This work is described by its Author as "An Expansion of the Outline of Psychology, contained in the fourth and fifth editions of my 'Principles of Human Physiology'" (1852 and 1855). The keynote of the whole work is struck in the next paragraph of the Preface, where Dr. Carpenter tells us that "Not having seen reason to make any important change in my own psychological views since I first put them forward, but, on the contrary, having found them confirmed and extended by the experience and reflection of twenty years, I set myself to revise my former exposition of them." The present work is an expanded result of this effort. Readers who are desirous of admiring the immobility of Dr. Carpenter's "psychological views," and of studying the state of psychology at the period named, will find what they require somewhat irregularly diffused through the 708 pages of this work.

Dr. Carpenter's psychological immobility, his faithful adherence to the lessons of his youth, are well shown in Chapters i. and ix., which treat of the "General Relations of the Mind and Body," and of "The Will." The physiology and philosophy of evolution is ignored throughout, and the doctrine that the human will is a something which exists outside and independent of the cerebral functions, is laboriously advocated. Dr. Carpenter tells his readers that "the actions of our Minds *in so far as they are carried on without any interference from our Will, may* be considered as "Functions of the Brain." On the other hand, in the control which the Will can exert over the *direction* of our thoughts, and over the *motive force* exerted by the feelings, we have the evidence of a new and independent Power, which may either oppose or concur with the automatic tendencies, and which accordingly as it is habitually exerted, tends to render the Ego *a free agent.* And truly, in the existence of this Power, which is capable of thus regulating the very highest of those operations that are casually related to corporeal states, we find a better evidence than we gain from the study of any other part of our Psychical nature, that there *is* an entity wherein Man's nobility essentially consists, which does not depend for its existence on any play of Physical or Vital forces, but which makes these forces subservient to its determinations." This passage—the capitals and italics of which are Dr. Carpenter's—summarises pretty fairly the "view which it has been the special purpose of this Treatise to develop." It would carry us far beyond the space we can devote to this book to point out a tithe of the inconsistencies and contradictions which necessarily result from an attempt to reconcile with established physiological laws this notion of an outside will overruling cerebral function. The mere metaphysician, reasoning only on the data furnished by an examination of his own consciousness, might plausibly enough maintain such a doctrine; we might expect it from a certain school of theologians, but that a physiologist, and one who comes forward as a public teacher of this branch of science, should in 1874 publish the two chapters above referred to, is in itself a psychological anomaly sufficiently curious to be worthy of a place among the cases of aberration which Dr. Carpenter has quoted in the course of Chapters xiii. to xviii.

In these Dr. Carpenter treats of Unconscious Cerebration, Electro Biology, Sleep, Dreaming, Sonnambulism, Mesmerism, Spiritualism, Intoxication, Delirium, and Insanity. He is evidently more at home here than in the previous chapters on Memory, Common Sense, and Imagination, and hence Chapters xiii. to xviii. form the most interesting part of the volume, the illustrative cases being very numerous, and for the most part appropriate.

As a matter of course "Unconscious Cerebration" receives a large share of attention, and is made to explain a vast deal.

Dr. Carpenter's "unconscious cerebration" is evidently but another name for what Helmholtz has termed "unconscious judgment." We see no advantage in the more pedantic paraphrase which will not bear critical examination. We may arrive at a conclusion either in perception or reasoning by a sound, healthful, and conscious judgment, *i.e.*, by an intellectual effort, each step of which is consciously understood, and therefore capable of critical re-examination; or we may do the same by means which are hidden to ourselves, and which therefore we cannot thus critically examine. In the latter case the conclusion or judgment may be a delusion to the true nature of which our unconsciousness of its origin renders us blind. Such unconscious judgment would thus be an abnormal or morbid judgment. We therefore see no objection to this term of Helmholtz's, provided its application be not overstrained. But Dr. Carpenter's "unconscious cerebration," as applied to abnormal or morbid action, is a physiological contradiction, inasmuch as *cerebration, when normal and healthy, is always an unconscious operation.* A man with a healthy brain is conscious of seeing, hearing, feeling, judging, thinking, &c., but is not conscious of *cerebrating.* It is only when the brain is diseased or overworked that we are conscious of any cerebration whatever, and even then the consciousness is very indefinite. If the term unconscious cerebration is to be used at all, it should be applied to healthy action of the brain, while *conscious cerebration* would describe certain peculiar states of its morbid action.

On the whole this work of Dr. Carpenter's is by no means a satisfactory effort. Had it been published twenty years ago it might have had some value as a text-book, but a thick book on "Mental Physiology," that almost totally ignores the important contributions that have been made during the past quarter of a century by Helmholtz, Huxley, Herbert Spencer, and other leading philosophical physiologists, is of little use to anybody. It is true that Dr. Ferrier's recent investigations are described in an appendix, but the student who only takes Dr. Carpenter's book for his guide will naturally suppose that the method of investigation adopted by Dr. Ferrier was something new and original.

The methods of experiment, and the main results described in this Appendix as Dr. Ferrier's, are really those of Fritsch and Hitzig, Ferrier having merely followed up and confirmed the investigations made three years previously by these German physiologists. Dr. Hitzig and some of his compatriots have loudly complained of the unjust treatment which he and Fritsch have received in England, and these complaints appear to be mainly founded upon this ignoring of their work and existence by Dr. Carpenter when describing their methods and results. The German physiologists appear to have taken it for granted that Dr. Carpenter's "Mental Physiology" is one of our accepted text-books, and that it represents the English estimate of their work. The disclaimers that have been already made on the part of English physiologists have doubtless ere this convinced Dr. Hitzig and his friends that such is not the case.

Elements of Practical Hydraulics, for the Use of Students in Engineering and Architecture. Third Edition. Part I. By SAMUEL DOWNING, LL.D. London: Longmans and Co.

THE constantly extending use of water, and of machinery and appliances in connection therewith, renders it very desirable that the engineering student should be well versed in all branches of hydraulics, and every additional facility for acquiring a correct knowledge of the principles upon which that science is founded is well deserving of acknowledgment. The present work—which has now reached its third edition—is one of an eminently high character, and having undergone a careful revision prior to publication, is—what such a book ought to be—a very complete and valuable contribution to scientific literature, and is no mere science text-book compiled exclusively from other works, and edited by one having little or no knowledge of the subject treated on. Mr. Samuel Downing's name is well known as that of a careful author, whilst the manner in which the work now under review is arranged and compiled shows clearly that that gentleman thoroughly understands the subject on which he writes. This volume does little more than introduce us to the first principles of hydraulics; it consists of but three chapters, which treat respectively on the discharge of water through an orifice, and over weirs, waste-boards, or overfalls; on the flow of water under variable heads, with examples and practical applications as applied to sluices and weirs; and on the flow of water through pipes, channels, and rivers. Under the above headings we are introduced to the first principles of hydraulics as applicable to irrigation, river improvement, and water supply, the whole subject being thoroughly worked out; and, whilst the general explanations contained in the text are clearly and intelligibly given, very numerous formulæ are furnished, together with tables of co-efficients, as ascertained by different observers under varying circumstances. There are fewer works on hydraulics amongst English scientific literature than could be desired, but many are to be found of great value by French writers, and from these Mr. Downing has freely drawn, giving the results of experiments therein recorded, thus enriching the store of knowledge on this important subject with which his book abounds.

Elements of Practical Construction for the Use of Students in Engineering and Architecture. Part I. By Samuel Downing, LL.D. London: Longmans and Co.

This is another work by the same author as the above, and is noticeable on account of the great care and precision with which it has been compiled. In this volume the resistance of materials to direct compression and tension only is treated, leaving to another volume the subjects of elasticity, indirect compression and tension, transverse resistance and torsion, &c. It must be clear to every one interested in the subject of construction how very important it is that the real strength of materials employed should be perfectly understood, and that they should know how to calculate the factor of safety under varying circumstances. This is more difficult when varying or transverse strains have to be allowed for, but should be comparatively easy in the case of direct compression and tension only. These latter may generally be classified under four heads, viz.—First, the resistance to direct extension; a force being in action tending to tear asunder the fibres or particles of the body, as is the case in tie-rods, the links of the main chains of a suspension bridge, chain cables, screw-bolts, &c. Second, the resistance to direct compression; a force being present tending to crush the particles or fibres, as in pillars, piers of bridges, shafts of columns, &c. Third, the resistance of a beam supported at one or both extremities to a force acting in a direction transverse to its length, and in fibrous material, as wrought-iron and timber, perpendicular to the line of fibres, which force is illustrated in the girders of bridges, beams, &c. Fourth, the resistance to forces of torsion which act so as to twist the fibres, as in crank-axles, the shafting of machinery, capstans, &c. Besides the above, all of which are clearly dealt with in the present volume, many other complicated varieties in the mode of action of the resisting powers of bodies may be enumerated, such as that which takes place from the interior to the exterior surface of hydraulic presses, and of pieces of ordnance when fired, and so forth, to which some reference also is made in Mr. Downing's book. The principle upon which this work is based is that first giving a proposition for treating each material, stating its average ultimate resistance: this is followed by experimental proofs, and then are given illustrations of the material so strained, taken from completed and successful structures of eminent engineers.

The publication is a very valuable one, and seems especially adapted for the purpose for which it is primarily intended, namely, for the use of students, to whom we can confidently commend it for study, whilst it will, no doubt, also prove valuable for reference by those engaged in practical work.

CORRESPONDENCE.

THE POLE STAR AND THE POINTERS.

By Capt. ALLAN CUNNINGHAM, R.E., Hon. Fell. of King's Coll., Lond.

SIR,—Attention has been drawn (in No. xliii. of the "Quarterly Journal of Science) to an interesting phenomenon connected with the well-known rule for finding the Pole Star by means of the "Pointers" (α and β Ursæ Majoris). It is there asserted that it is matter of popular observation that the "Pointers" *do not at all times appear to "point" equally well* to the Pole Star, and an attempt is there made to prove that popular observation is herein *correct in fact*, that is to say that the "Pointers" *do not actually at all times "point" equally well* to the Pole Star.

The present writer, however, considers that the true explanation of the phenomenon has been quite missed in the Article referred to, and that the Article itself, indeed, contains its own refutation.

The true explanation to the present writer's mind is that—

1. The Pointers *do not actually* at all times (of the same night) "point" equally well to the Polar Star by an amount depending solely on aërial effects of refraction.
2. The Pointers *do not appear*—to the unaided human eye—to "point" equally well at all times (of the same night) to the Pole Star by an amount depending solely on an error of judgment (of the observer) in tracing the "directing line" in the heavens, and which *apparent change* of the obliquity of the "directing line" must therefore be considered an OPTICAL ILLUSION.
3. There is a minute annual change of the "directing line"—due to the earth's orbital motion—appreciable only with difficulty by the best instruments.
4. There is a slow secular change in the relative positions of the three stars—due to their proper motions—in consequence of which the "directing line" of the Pointers will in the course of ages cease to "point" towards or near the present Pole Star, but this change will be *very slow*.

The first two effects of *actual* and *apparent* change (during the same night) of the obliquity of the "directing line" of the Pointers are the only ones of present interest.

It may be premised that both effects will be zero at the North Pole, and extremely small in high northern latitudes; and will both increase with proximity to the Equator, the latter increasing much the more rapidly.

The former effect — the actual change—would be so small in Great Britain as to be appreciable only with instruments, but would probably become appreciable to the unaided eye in low northern latitudes: the latter effect—the apparent change (merely an optical illusion)—will rapidly increase with distance from the North Pole, and is perceptible in Great Britain.

As both effects increase with proximity to the Equator, the joint effect would of course be more noticeable in low northern latitudes, especially to one leaving a considerably higher northern latitude; indeed the attention of the writer of the previous Article seems to have been first attracted (see p. 286 of No. xliii. of the "Quarterly Journal of Science") to the phenomenon when on a voyage *towards* the Equator.

That the principal part of the apparent change of obliquity of the "directive line" of the Pointers from the Pole Star is really an error of judgment—*i.e.* only an *apparent* change, or an optical illusion—may be with some trouble *experimentally* verified by any one who has a small *portable* equatoriably mounted telescope (a "comet-seeker" would do very well)—it would be better if fitted with clockwork motion—or a small transit theodolite. The experiment would probably be more convincing to many than any amount of purely geometrical reasoning as to what *ought to be.* It will be described as performed with a small equatorial fitted with a set of hairs in its diaphragm, and means of illuminating the hairs.

Experiment.—The instrument must be set up so that the centre of the middle hair shall—by moving the telescope round the declination axis only—traverse both Pointers: the proper position will differ only slightly from the usual position of an equatorial, and may be found—by trial—by setting up the instrument as usual, *i.e.* with the polar axis pointing to the North Pole, and then correcting the position *by shifting the polar axis*, partly in azimuth, partly in altitude. The correct position, *viz.* that in which the centre of the middle hair traverses both Pointers *on rapidly moving the telescope round its declination axis*, will be secured after a few trials. When this position has been secured, the reading of the hour-circle should be noted: after which, turn the telescope *round its declination axis*, as near the Pole Star as possible—when, if the telescope be of low power, the Pole Star will be in the field of view: the centre of the middle hair must now be made to cover the Pole Star by use of the motions *round both polar and declination axes;* after which the hour-circle must be read again. One observation is now complete: the difference between the two readings on the hour-circle will be the *deviation* ($p\ m$ in Fig. 1) of the "directive line" of the Pointers from the Pole Star—affected, however, to some extent by the rotation of the earth during the interval of observation.

It is very desirable that the whole observation—from the moment the instrument is in proper position—should be rapidly performed, so as to reduce the last effect as much as possible: the apparent motion of the Pole Star is so slow, that, if the operation be rapidly performed, the earth's rotation will not sensibly affect the observation.

The above process should be repeated at other times of night: the *position* of the instrument will require slight shifting for every observation to secure the necessary condition that the centre of the middle hair should traverse both Pointers by the rapid motion of the telescope round the declination axis.

It is obvious that a transit theodolite might (with some trouble) be used in the same manner by fitting it with a strong universal joint and clamp, so that it might be possible to tilt its horizontal plates into the same position as the hour circle of the equatorial. Care must be taken that the joint and clamp are strong enough and stiff enough to hold the whole weight of the instrument steady throughout the observation.

It will be found that the values of the "deviation" of the "directive line" of the Pointers obtained from different observations will—if the observations have been well made—not differ from each other by any such large amount as would be appreciable by the unaided eye.

The result of this experiment is a proof positive that the principal part of the apparent change of obliquity of the "directive line" is only an *apparent change*, *i.e.*, an optical illusion.

The small difference in the values of the "deviation" *as obtained from different observations*, are due to the following causes:—1. An *actual change* due to aërial effects of refraction. 2. Errors of observation due partly to errors of the observer, and partly to the imperfection of the experiment in introducing improperly effects of the earth's rotation.

It will now be shown that the above result might have been expected *à priori*, *i.e.*, that—barring effects of refraction—there is no change in the obliquity of the "directive line" of the Pointers from the Pole Star at different times or places.

Consider first the meaning of the word "point" as here used of stars. The popular explanation would probably be—

Popular Explanation.—"Two stars are said to 'point' at a third when an apparently straight line joining them passes through the third, if produced."

Now this definition is only tolerably good for *very small portions* of the celestial sphere: apparently straight lines cannot be drawn at all over any considerable portion of the sphere. The only real analogue to straight lines in plane is that of GREAT CIRCLES on the sphere, which are really the *shortest lines* which can be drawn between two points: small portions of Great Circles do really seem *apparently straight:* no other lines on the sphere can possibly be apparently straight, but are both *actually* and *apparently curved*, and in general *twisted.* The proper definition of "pointing" as applied to stars is therefore—

Definition.—"Two stars are said to 'point' at a third, when a Great Circle through them passes through the third if produced."

Apply this to the case of the Pointers and Pole Star. Their relative positions are *plotted to scale* in Fig. 1 from data taken from the "Nautical Almanac." In the figure P is the north pole of the heavens, p is the Pole Star, α and β are the two Pointers (α and β Ursæ Majoris).

The lines Pp, Pβ, Pα, $\beta\alpha$, βp—straight lines in the diagram—represent the Great Circles in the heavens joining the points named: thus the (straight) line $\beta\,\alpha$ represents the Great Circle in the heavens, which is the true "directive line" of the Pointers, and along which, therefore, they must be said to "point." It will be observed that the Pointers (α, β) do not lie on the same Great Circle through P (the Great Circles Pα, Pβ, include, in fact, an angle of about 30′) and therefore do not "point" to the pole (P); the Great Circle arc $p\,m$, drawn perpendicular to $\beta\,\alpha\,m$, is "deviation" of the "directive line" $\beta\,\alpha\,m$ of the Pointers from the Pole Star (p), and subtends at the further Pointer the angle $p\,\beta\,\alpha$, which is what has been styled above the "*angle of obliquity of the directive line*" of the Pointers from the Pole Star.

Imagine this figure traced in visible glittering lines in the heavens at any instant: it will seem to swing round

the celestial pole (P)—in consequence of the earth's rotation—*as a whole, i.e.*, preserving an invariable figure (barring aërial effects of refraction), so that the "angle of obliquity" *$p\,\beta\,\alpha$ will remain constant.* It follows, therefore, that — excepting aërial effects—the Pointers do at all times of the same night, and at all places, "point" equally well to—(deviate by a constant quantity from)—the Pole Star.

It is easy to verify the above by a simple experiment :—

Experiment.—Take an orange (or a worsted ball) to represent the celestial sphere: drive a knitting needle through it,—this will be a tangible representative of the axis of apparent diurnal rotation of the heavens,—but only far enough to let one end just project; this end will represent the North Pole (P) of the heavens. Next drive three pins up to their heads into the orange in positions similar to p, β, α in the figure: the three pin-heads will represent the Pole Star (p), and the two Pointers (α, β). Strain a fine thread from the pin p round the pins β, α, and pin it down again at m, a point in the direction of $\beta\alpha$ produced. The thread (if properly strained) will cling to the orange along the Great Circles $p\beta$, $\beta\alpha m$. The construction is now complete.

Face the north, and hold the orange with the projecting end of the needle pointing towards the North Pole of the heavens (which it represents), and turn the orange slowly round the needle (held in the hand) in the same direction as the stars appear to move, *i.e.* rising from the east or right hand: the set of pin-heads representing the Pole Star and Pointers will now move in precisely the same manner as the apparent diurnal motion of the actual Pole Star and Pointers, and it will be at once recognised that the "angle of obliquity ($p\beta\alpha$) of the directive line" continues *invariable*, *i.e.*, that the Pointers do always "point" equally well to, *i.e.* with equal deviation from, the Pole Star.

It will also be understood that the Great Circle ($\beta\alpha m$) through the Pointers was the line traced out in the heavens by the centre of the middle hair of the equatorial in the first experiment, and that the arc pm was the angle obtained as the difference of the readings on the hour-circle.

Effects of Aërial Refraction.—The effect of the earth's atmosphere on the rays of light by which the stars become visible to us is to raise the apparent position of all by a small amount, so that all *appear higher* than they would if no atmosphere existed. The amount of apparent increase of altitude depends on the actual altitude, and is greatest (about thirty-three minutes) for a star in the horizon, and rapidly diminishes with increase of actual altitude, and vanishes at the zenith.

Now as the Pole Star and Pointers are generally at very different altitudes above the horizon, the apparent increase of altitude is generally different for each of the three, being always greatest for the lowest, and least for the highest; moreover this apparent increase of altitude changes in magnitude slowly from instant to instant, the variation being different for each of the three, *viz.*, least for the Pole Star, which can never be more than about $1\frac{1}{2}°$ above or below the pole, and greatest for the further Pointer (β), which may be about 32° 49' above or below the pole.

The effect of this varying increase of altitude *of the visible positions* of the three stars (different in amount for each star) on the problem in hand is a slight change both of *position* and of *shape* of the spherical triangle $p\beta m$ from the normal form (of the diagram) which it would have (excluding atmospheric effects), and this change varies slowly from instant to instant. Thus the *apparent shape* of the spherical triangle $p\beta m$ does really vary throughout the night, and therefore also the "apparent deviation" (pm), and also the "apparent angle of obliquity" of the Pole Star from the Pointers *do really vary throughout the night.*

This effect is, however, zero at the North Pole of the earth, for all three stars will there be *apparently* raised by a constant small amount (different, of course, for each star, but invariable for the same star): thus the *apparent shape* of the spherical triangle $p\beta m$ will be (slightly different indeed from its true shape, aërial effects excluded, but) of *invariable* form throughout the night, so that the "angle of obliquity of the directive line" of the Pointers is both actually and apparently invariable at the North Pole of the earth.

The effect due to refraction will increase at first very slowly from the North Pole towards the Equator, and will be sensibly zero in all high northern latitudes, and even in Great Britain will be so small as to be inappreciable to the unaided eye. The effect will increase more rapidly towards the Equator. The solution of the following Problem—

Problem—"In what latitude, and at what hour does the effect of aërial refraction cause most variation in the 'deviation' of the 'directive line' of the Pointers from the Pole Star (apparent positions only considered), and what will that maximum variation be?"

would be a matter of considerable mathematical, or rather numerical, complexity, and is hardly worth the great labour it would probably involve. It seems, however, *probable* that this maximum would occur at or near the Equator; and it is *possible* that it might be large enough to be appreciable by the unaided eye.

Explanation of Cause of Optical Illusion.—It having been shown that the principal part of the apparent change during the same night of the "angle of obliquity of the directive line" of the Pointers is *only an apparent change*, having no existence in fact, and which can therefore only be regarded as an OPTICAL ILLUSION, it remains to endeavour to give some explanation of the cause of so extraordinary an illusion.

It has been explained that the true meaning of the phrase "two stars *point* at a third" is that the Great Circle through the two stars passes—if produced—through the third. Unless, therefore, the eye can properly trace out and produce a Great Circle in the heavens, the mind cannot fairly judge of the manner in which two stars "point"—whether well or ill—at a third.

The illusion appears to depend entirely on the fact that the tracing of Great Circles in the heavens is an unfamiliar matter. This may readily be tested by trial. Let anyone endeavour to trace Great Circles in the heavens between pairs of stars not very far apart, and produce them as far as possible in both directions, and then compare them with the Great Circles obtained by straining a fine string through the same pairs of stars on a celestial globe; he will be astonished at the discrepancy when the Great Circles are not vertical or nearly so. Let it be remembered that *every* such Great Circle *ought to cut the horizon* in two points diametrically opposite. There will be found no difficulty in tracing out vertical or nearly vertical Great Circles tolerably correctly; but whenever the true Great Circle would be considerably inclined to a vertical circle, it will be found that—if the stars be not very far apart—the Great Circle as traced by the unaided eye will not cut the horizon in two points diametrically opposite, nay—if the obliquity be very great—will not cut the horizon at all!

The eye seems to be influenced in some way by an impression of the position of the horizon; so that if pairs of stars be selected at the same or nearly the same altitude, the impulse to draw a *horizontal circle*—which is, of course, a small circle of the sphere—through them will be found almost irresistible!

Anyone who tries this experiment fairly a few times will soon be convinced that the *apparent change* of "deviation" of the "directive line" of the Pointers from the Pole Star exists only in imagination, and is a remarkable instance of optical illusion.

The cause of the great influence of the horizon on the attempt to trace oblique Great Circles in the heavens is probably that all our associations of practical life are with figures drawn on the earth, *i.e.* on a horizontal plane, and that the recollection in the mind of these, the familiar work, insensibly influences the eye in endeavouring to do the unfamiliar work in the heavens.

An Article has been published in the "Quarterly Journal of Science," No. xliv., for October, 1874, with title "On the Curved Appearance of Comets' Tails," by the author* of the Article on the Pole Star and Pointers in No. xliii. of that Journal.

The Author endeavours to explain that the "curved appearance" of the tail is commonly only an *appearance* due to the position of the tail in the sky, and *changing in curvature from hour to hour* in consequence of the change of apparent position due to the earth's rotation. The Author's argument is virtually simply a reiteration of the argument in his Article on the Pole Star and Pointers in No. xliii. of the "Quarterly Journal of Science." Anyone who has understood the explanation advanced in the present Article of the difficulty experienced by the eye in tracing Great Circles in the heavens will see that any *supposed change of curvature*

* Lieut.-Col. Drayson, R.A., F.R.A.S.

of a comet's tail from hour to hour, due to its apparent change of position in the sky from hour to hour, is not—astronomically speaking — an "apparent change" of curvature, but is, in fact, for the most part, an OPTICAL ILLUSION.

There is, however, an "apparent change" of curvature of the tail from hour to hour, due partly to *real changes of figure* in the tail itself, and partly to the *real change of position* of the whole comet in space. This "apparent change" of curvature is not noticed in the Article referred to.

It is the "apparent curvature," and "apparent change" of curvature of comets' tails due to their real curvature, and real change of curvature or real change of position in space, which are referred to in works on Astronomy. It seems incredible that all writers* on Astronomy up to the present time should have confused the cause of supposed visible curvature or change of curvature from hour to hour—which is here explained to be a mere OPTICAL ILLUSION—with the "apparent curvature" and "apparent change" of curvature (due to real curvature and to real change of curvature or real change of position in space).

* An extract is given in the Article referred to from Herschel's "Outlines of Astronomy," Art. 557, as "showing some obscurity in the minds of former writers on astronomy on this subject of the curves of comets' tails." It seems needless to say, however, that Herschel there offers a physical explanation only of the *real curvature.*

PROGRESS IN SCIENCE.

MINING.

In a Memoir on the Geology of Darjiling, recently published by the Geological Survey of India, Mr. F. R. Mallet gives some interesting facts respecting the occurrence of coal in this locality. Although the late Dr. Archibald Campbell many years ago called attention to the reputed discovery of coal in this out-of-the-way corner of India, it was not until Dr. Hooker visited the country, in 1849, that true carbonaceous deposits were observed. The importance of a supply of coal for the great trunk railways of India has caused attention to be hitherto concentrated on the fields south of the Ganges; but the connection of Calcutta with the hill-country, by means of the Northern Bengal State Railway, has recently directed attention to the Darjiling coal, and has led to the investigation of the country by the Geological Survey. Mr. Mallet gives a list of localities where the coal-seams crop out. Most of these seams are highly inclined, and have been so violently disturbed that the coal is for the most part crushed, and even reduced to powder, so that it could be easily dug out, but would need to be compacted by artificial means in order to be available as fuel. The writer discusses the question whether it should be coked or prepared as artificial fuel, and inclines to the latter method.

Copper-mining has long been carried on in the neighbourhood of Darjiling, though only on a small scale and unsystematically. The ore is copper-pyrites,—not in true lodes, but disseminated through slates and schists. A description of the crude methods of mining and smelting the ores, as practised by the natives, is given by Mr. Mallet in his "Survey Memoir." He concludes that the prospect of copper-mining in this district is not inviting to European enterprise.

Some notes on the iron ores of Kumaon have been contributed to the records of the Geological Survey of India, by Mr. Theodore Hughes, who has also published some notes on the raw materials employed for iron-smelting in the Ranigang coal-field. The weak point in most of the Indian coals appears to be the large proportion of inorganic matter which they contain as compared with that of our own coals. A complete series of analyses of thirty different Indian coals has lately been made by Mr. Tween, under Dr. Oldham's direction.

In consequence of the reported discovery of a new coal-field in the Garoo Hills, a cursory examination of the district has been made by Mr. H. B. Medlicott, who thinks it safe to conclude, from his observations, that a coal-field of considerable extent exists in the very heart of these hills, the measures occupying an area of about 12 or 15 square miles.

Mr. V. Ball publishes, in the Records of the Indian Survey, a note of his visit to the coal-deposits recently discovered in the south-east corner of Afghanistan; and Mr. R. Bruce Foote describes his examination of the gold-deposits of the Dambal Hills, with the view of determining the sources whence the stream-gold of this region has been derived.

Large deposits of iron ore and extensive beds of coal occur in the neighbourhood of Wallerawang, about 100 miles from Sydney. These deposits have recently been visited and described by Prof. Liversidge, who has recorded his observations in an interesting paper. The ores—chiefly magnetic and brown iron ores—occur near the junction of the coal-measures with either Upper Silurian or Devonian rocks. The coal-measures contain several valuable beds, including three principal seams, of which the lowest has a thickness of 17 feet 6 inches, the middle seam 6 feet 6 inches, and the uppermost 4 feet 6 inches. Limestone occurs in the neighbourhood, and, on the whole, the district of Wallerawang appears destined to be one of the most flourishing mining centres in the Colony.

From Nova Scotia we have received the Annual Report of the Inspector of Mines, Mr. H. S. Poole. The statistics here given show that in 1874 there were in the Province 30 coal-mines, producing 872,720 tons of coal, and 33 gold-workings, yielding 9141 ounces of gold. Compared with the produce of the preceding year there is a falling off, which may in part be accounted for by the general dulness of trade. In addition to the statistical information, the Report contains some general observations, by Mr. Poole, on such branches of mining as are carried on in the Province.

The last number of the "Annales des Mines" opens with a long memoir on the Sulphur-Mines of Sicily, by M. C. Ledoux. During a visit to some of the more important sulphur-yielding localities, in 1871, he collected a good deal of information on this branch of industry; and although important memoirs on the subject have been written by Parodi and Mottura, the present paper presents—in a convenient and accessible form—a general sketch of the geological structure of the sulphur-producing districts, the working of the ore, and its treatment for the production of sulphur.

Mr. Warington W. Smyth, of the Royal School of Mines, recently delivered to the Iron and Steel Institute an interesting lecture on the Ores of Iron, considered in their Geological Relations. It is to be hoped that this lecture will be published in the Journal of the Institute.

METALLURGY.

At the recent metropolitan meeting of the Iron and Steel Institute the newly-elected president, Mr. Menelaus, devoted the greater part of his Address to a review of the chief modern improvements in the manufacture of wrought-iron and steel. The satisfactory working of Danks's system at the Erimus Iron-Works, near Stockton, was explained by Mr. J. A. Jones. The other forms of puddling machinery were noticed by Mr. Menelaus, who concludes that mechanical puddling is now an established success, and that better results may be thus obtained than by the old method of manual puddling.

Turning to steel, the president referred to the advantage of converting pig-iron into Bessemer steel by running the crude metal direct from the blast-furnace into the converter, and thus saving the cost of melting. With reference to the wide application of steel in the Arts, he gave the results of the experience of some of our leading mechanical engineers in favour of the use of a mild steel for purposes of construction.

The Bessemer medal has this year been awarded to Dr. C. W. Siemens.

In continuation of his valuable researches on the consumption of heat in the blast-furnace, Mr. I. L. Bell has recently read a paper "On the Sum of Heat Utilised in Smelting Cleveland Ironstone." He calculates that in the combustion of the best South Durham coke only 51·27 per cent of its heating power is utilised, the remainder being carried from the tunnel-head. In seeking to determine how this may best be utilised, he has been led to effect certain improvements in the construction of the boilers and hot-blast pipes at the Clarence Iron-Works, whereby the heating-power of the waste gases is so economised that all the heat needed for generating steam and heating air for furnaces is thus obtained, with some to spare, whilst in the old system, at the same Works, it was always found necessary to supplement the heat afforded by the waste gases.

An interesting process is now carried on by the Great Snowdon Copper Mining Company, for extracting copper from poor ores. The ore having been crushed is mixed with a small proportion of lime, and made up into a shape convenient for stacking in a kiln; the kiln of ore is then fired, and the roasted ore afterwards crushed and lixiviated. By this means the sulphide of copper originally present in the ore becomes converted into a soluble sulphate, and on passing sulphuretted hydrogen through the solution the metal is precipitated as a sulphide sufficiently pure to be at once smelted.

A paper on "The Extraction of Silver from Cupreous Iron-Pyrites," read before the Tyne Chemical Society, by Mr. T. Gibb. has recently been published in the "Chemical News" (April 16). After describing the ordinary process of treating burnt pyrites by the wet way, he refers to Claudet's method of extracting the small proportion of silver, and then describes his own process for effecting this extraction as practised by the Bede Metal and Chemical Company. This process consists in precipitating the greater proportion of the silver from the copper liquors by means of sulphuretted hydrogen, only a small proportion of copper being simultaneously thrown down. The sulphuretted hydrogen is economically obtained by the action of dilute hydrochloric acid on "tank waste." This sulphuretted hydrogen, mixed with carbonic acid, and diluted with a large volume of atmospheric air, is blown through the copper liquors for about twenty minutes. The precipitated sulphide of silver is calcined at a low temperature, and then ground and lixiviated, first with water, to dissolve sulphate of copper, and afterwards with a hot solution of common salt, to dissolve out the chloride of silver which is present. The solution is mixed with milk of lime, and the precipitate, having been washed, is digested in dilute sulphuric acid, and again washed and dried.

An improved method of preparing metallic barium has been devised by M. Sergius Kern, of St. Petersburg ("Chemical News," June 4). He precipitates iodide of barium by the action of iodine on barium hydrate, and then decomposes the iodide by heating it with metallic sodium. Barium is separated from the resulting mixture by means of mercury, and this metal is then distilled off from the amalgam, and the barium thus obtained in a free state.

MINERALOGY.

A short time ago a nickel ore from New Caledonia was described by Prof. Liversidge, of Sydney, under the name of *Noumeite*, after Noumea, the capital of the island. According to a paper since read before the Royal Society of New South Wales, it appears that the Rev. W. B. Clarke has suggested that the name of *Garnierite* should be bestowed on this mineral, since M. Garnier, a French geologist, some years ago obtained a similar mineral from New Caledonia. But the greater part of the nickel ore as shipped to Sydney consists of a similar mineral,—a hydrated silicate of nickel and magnesia,—but of a much darker apple-green colour than Garnierite, and to this second mineral Liversidge has now given the name of Noumeite, which he formerly applied to the paler variety. Both minerals are evidently products of decomposition, and Garnierite may perhaps be only a more highly altered form than Noumeite.

Prof. Church has contributed to the "Chemical News" (April 9) some "Short Notices of some Cornish Minerals." Specimens of a very pretty pink variety of soap-stone, resembling some of the Chinese figure-stones, were obtained from Mr. Talling, of Lostwithiel, and on analysis were found to consist of a hydrous silicate of magnesia, in which one-fourth of the magnesia is replaced by water: $3MgO.H_2O.4SiO_2$.

Some grains of gold from Ladock, in Cornwall, were found, by Prof. Church, to contain—Gold, 92·34; silver, 6·06; iron, a trace; silica and loss, 1·60. A fine specimen of filiform native silver, obtained many years ago from Huel Herland, in Cornwall, gave—Silver, 99·05; iron, 0·59. Neither gold nor copper was present, and the iron might be derived from traces of associated iron-pyrites.

Under the name of *Cossaite* Prof. Gastaldi has described a mineral apparently identical with paragonite, from which it differs in the absence of micaceous cleavage. It was found first in an antique ring dug up near Turin, but has also been found in other localities.

Several analyses of mineral phosphates extensively imported into Hamburg, from Estremadura, in Spain, have been contributed to a recent number of the "Chemical News," by Dr. B. C. Niederstadt. The average proportion of phosphoric anhydride is about 28 per cent.

An elaborate paper on the crystallisation of Galena, by Dr. A. Sadebeck, of Berlin, has been published in the "Zeitschrift" of the German Geological Society, where it is illustrated by three plates showing peculiar forms of this mineral.

Prof. Maskelyne, in a letter to Vom Rath, has announced the isomorphism of Asmanite, or orthorhombic silica, with Brookite, or oxide of titanium.

Some fine specimens of the American variety of Brookite (*Arkansite*) have recently reached this country; and we have also seen some splendid examples of plumbago, exhibiting in some cases a fibrous structure, brought over from Buckingham, in Canada, where it is now worked on a considerable scale.

ENGINEERING—CIVIL AND MECHANICAL.

Sanitation.—There are hardly two more important branches of engineering than sewerage and water supply, and these subjects have recently become prominent matters of interest by the introduction into Parliament of the Public Health Bill and the Rivers' Pollution Bill, two measures of the first importance, and largely called for in the interests of our town populations. It is not our intention to enter into any detailed examination here of the several clauses of these Bills, but we may do so upon another occasion. Everywhere the local authorities who have not already purified their sewage, are anxiously inquiring as to the best methods to be employed for that purpose; no scheme, however, yet invented has been found applicable under all the varying conditions that present themselves in different places, but amongst the numerous schemes that have already been put into practice, it may fairly be premised that one or other of them might be adopted—certainly with beneficial results, if not with absolute success—under all known circumstances. Where land is available, no doubt irrigation is an effective method of purifying the effluent water, but experience seems to teach us that in all cases some system of precipitation should be first adopted. The principal methods introduced for this purpose have been, on several occasions, described in the "Quarterly Journal of Science," so that it is not necessary to refer again to them now. A paper recently read by Mr. Charles E. Jones before the Chesterfield and Derbyshire Institute of Engineers, ably dealt with the subject of "Sewage; its Use and Abuse," and in the discussion that followed some particulars were given as to the value of excreta in Flanders, Belgium, and Bruges. In the latter place Mr. Chadwick stated that a system of employing servants existed whereby they were to receive a certain small money payment "*and the night soil*," and he further quoted statistics to show that the system of abolishing privies in England, and draining them into the sewers, has been followed by an enormous and alarming increase in the rate of mortality. The gist of Mr. Jones's paper was that the rainfall should go to the river, and the sewage to the soil, a distribution beyond doubt perfectly correct in theory, but hardly capable of being carried out except in comparatively small towns.

A paper on "Northampton and its Sewage" has also been recently read before the Association of Municipal and Sanitary Engineers, by Mr. J. H. Pidcock. Here the sewage was originally emptied into the river, but injunctions having been granted against polluting the river, notwithstanding the precipitation processes adopted there, it has been found necessary to purchase a sewage farm in order further to purify the effluent water before turning it into the Nene, thus bearing out what we have before remarked, that the soil must be depended upon to finish the purification of the sewage after the adoption of mechanical and chemical processes for extracting the solid matters contained in it.

In connection also with the general question of sanitation, we may also notice a paper read before the Institution of Civil Engineers on "The Systems of Constant and Intermittent Water Supply, and the Prevention of Waste," by Mr. G. F. Deacon. This paper had special reference to the restoration of constant service in Liverpool, and in it was explained the precautions taken by night inspectors to detect where a waste of water occurred, and, on the discovery of any such instance, measures are adopted to detect the cause and provide such remedy as might be necessary. The result of the trials thus far has been to show that with proper precautionary measures a constant service may be introduced so as to secure a lower consumption than under the intermittent system.

Ships.—The recent action taken in Parliament by Mr. Plimsoll has led to greater attention being given to the construction of shipping, and the extent to which this has taken place is shown in the nature of the papers read before the Institute of Naval Architects. Foremost amongst these is one by Mr. W. Froude on the "Rolling of Ships." This is a most important question as affecting the stability of vessels, but the difficulty in its proper determination consists in the theoretical nature of the necessary calculations, and the difficulty in ascertaining the actual resistance to oscillation presented under varying circumstances. In the absence of this resistance every ship of ordinary form would infallibly be overset the first time she encountered co-periodic waves. This is a very serious fact, and Mr. Froude has undoubtedly done good service in endeavouring to frame rules for the solution of so difficult a problem.

The superiority of steel over iron for shipbuilding has been ably explained by Mr. N. Barnaby, who showed that the French are in advance of this country in the adoption of the superior metal for that purpose. The principal care to be taken in its use is to avoid hammering the metal as much as possible, and to bend it to the necessary curves by means of pressure, so as to afford as little injury as possible during the process of construction. The chief advantage of steel is that it is homogeneous, and free from all laminations and blistering, and the recent improvements in its manufacture enable it to be turned out in sufficient quantities for the purpose in view.

An interesting paper by Mr. A. Sedgwick Woolley on "Spar Torpedo Warfare" was another contribution to the proceedings of the Institution of Naval Architects. In this paper the history of torpedo warfare is traced from the year 1775, and one Captain David Bushnell, of Connecticut, is said to have been the originator of the system, which, however, did not arrive at any really practical use until during the Civil War in America, when it was brought largely into use by the Southern States against the war vessels of their opponents. The results of experience gained up to the present time seems to be that the torpedo should explode only by actual contact with the vessel against which it is directed, because unless it is actually in contact with the side of a vessel, there is a great chance of the explosion failing to blow it in, since it will always take the line of least resistance. The plan of firing at will has the disadvantage of being left entirely to the discretion of the operator, who, in the darkness of night, under cover of which these attempts have to be made, may easily miscalculate the distance he is from a ship, and so fire his torpedo too soon. Some fast torpedo launches have recently been constructed by Messrs. Thorneycroft and Co., from the Swedish, Norwegian, and Danish Governments.

Within the last quarter there has been launched the twin-screw ironclad *Alexandra*, for the British Navy, which is justly described as the finest and most powerful broadside ironclad in the world. The following are her principal dimensions:—Length between perpendiculars, 225 ft.; extreme breadth, 63 ft. 8 in.; depth in hold, 18 ft. 7½ in.; tonnage, 6049 tons; indicated horse-power, 8000; and speed, 14 knots. The water-line of the *Alexandra* is protected by a belt 12 inches in thickness amidships, which tapers off towards the ends. The *Alexandra* is a central battery ship, and needs no bow or stern batteries to give her end-on fire; she has two gun-decks with end-on fire from both, and the batteries are armoured with 8 in. and 6 in. armour. The upper battery of this vessel consists of two 25-ton and two 18-ton guns, whilst in the lower battery are eight 18-ton guns.

Another vessel of special interest, which has recently been added to our commercial fleet, is the *Bessemer* steamer, designed primarily for cross

Channel traffic, and having a suspended saloon made unsusceptible to transverse oscillation by the application to it of special hydraulic machinery. This vessel is 350 ft. in length, and 40 ft. actual beam; she also possesses the peculiarity of having two distinct sets of engines, and two pairs of paddles, placed 105 ft. apart between centres; whilst, with the view of reducing the pitching motion as much as possible, she has been constructed with low ends. How far the vessel may prove a success is at present uncertain, and probably some further experience may be necessary before the swinging saloon is brought to such a state of perfection as fully to accomplish the desired end of insuring travellers against the terrors of sea-sickness.

Docks, &c.—A new public graving-dock has recently been opened at Cardiff in connection with the Roath Basin. The side walls of the dock are of Forest and Radyr stone, and the bottom of the whole is limestone, surrounded by a stone coping. Slips are provided each side, and the entrance, 60 ft. in width, is fitted with an immense caisson. The dock is 600 ft. in length, and 78 ft. wide; the depth of water provided will be, during spring tides, 23 ft., and during neap tides 13 ft., but the depth of water will rarely fall below 17 ft. The water can be discharged in about four hours, through the instrumentality of two powerful steam pumps.

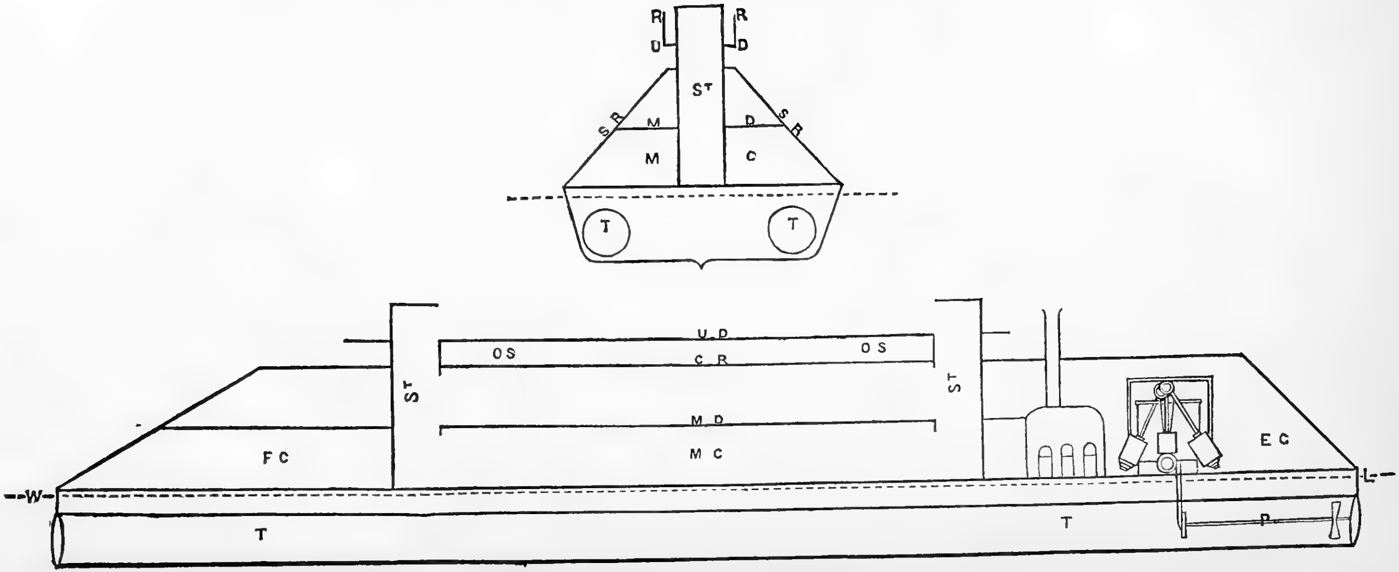
Railways.—A very great advance towards removing that element of human fallibility which has so often been the cause of serious railway accidents, has recently been made by the introduction of an electric block signal, the combined invention of Messrs. Farmer and Tyer. This apparatus comprises a combined mechanical and electrical arrangement by means of which the working of the semaphore signal can be controlled from any point, quite irrespective of distance. Electricity is employed to act on an electro-magnet, causing it to attract an armature; when the attraction ceases, a detent in connection with the armature is released, and the signal is set free from the hand-lever by a mechanical arrangement, and by means of the usual counter-weight the semaphore on the line rises to and remains at danger directly the connection between it and the hand-lever is broken. By the use of this invention the signalman at either end of any block section has the power individually to act upon the semaphore to signal danger, but it is only by the concurrent action of the two that the safety signal can be given.

A very simple contrivance has recently been introduced upon the Great Western Railway of Canada, which promises to be of service by giving the engine-driver always a view over the whole train, and which promises to be of valuable assistance in the operating of goods trains, and would also be a useful adjunct for passenger train service. The device consists of the introduction of a mirror placed on the engine weather plate, immediately in front, and over the heads of the driver and fireman, at such an angle as to reflect a view of the whole of the wagons or carriages attached to the engine, and thus render the train behind as distinctly visible to the driver and fireman as the line itself in front.

At the time we are writing some important experiments are being made on the Midland Railway with different systems of continuous brakes, an account of which we hope to be able to give in the next number of the "Quarterly Journal of Science."

In San Francisco a system of wire rope street tramways has recently been introduced, which presents some features of novelty. In consists of an endless wire rope placed in a tube below the surface of the ground, between the tracks of the line, and kept in position by means of sheaves, upon and beneath which the rope is kept in constant motion during the hours the traffic is running, by a stationary engine, the power being transmitted from the motor to the rope by means of grip pulleys, and from the rope to the cars on the street by means of a gripping attachment fastened to the car, and which passes through a narrow slot in the upper side of the tube.

Dr. W. S. Carmichael thus describes his Breakwater Steamer:—The invention is intended to modify the influence of the waves, &c., by covering certain generally exposed parts of vessels, and thereby to lessen the dangers and inconveniences of the sea. It consists of one general principle, viz., the defence of certain generally exposed parts of vessels by a covering. 1st. The defence of the sides and decks by sloping roofs, thereby enabling the vessel to retain a more horizontal position, and abating the dangers and inconveniences of the sea. 2nd. The defence of the screw or screws by a covering, thereby saving it or them from shot, &c., and enabling it or them to act with more effect. It consists of a hull up to the water-level, W L, somewhat like that of other vessels, except that the keel is straight and horizontal, the bow perpendicular, and it is perforated by one or more openings to be afterwards described. I propose that the vessel shall be nearly flat-bottomed, the sides below the water-level nearly perpendicular, and also that the heaviest parts shall be on the outside. Near the water-line, W L, or several feet above it, according to the size of the vessel, the sides all round slope inwards, become a sloping roof (S R) over the cabins (M C, F C, E C) and main deck (M D), &c., at an angle of 45°, more or less, forming a breakwater on which the waves in rough weather may harmlessly expend their force, instead of dashing against nearly perpendicular sides, or washing the deck. These sloping roofs at the front of the vessel meet and form a sharp ridge, by which a wave at the front is cut in two. The upper part of this sloping roof, S R, may be made mostly of sliding panels, which may be pushed aside in calm weather, so as to render the main deck, M D, at such times nearly as open as in other vessels. Of course in the harbour these panels will always be drawn aside, so as to leave room for the entrance and exit of passengers, the carriage of luggage and cargo, &c. Seven feet, more or less, above the main deck, M D, the sloping roofs end in a flat roof, G R, to allow the more violent waves to pass harmlessly from windward to leeward, and, by being partly glazed, to give light to the decks below. The cabins below may also be lighted by the usual bull's-eyes. Three or four feet, more or less, above the partly glazed roof, there is a smaller upper deck, U D, about half or a third of the size of the main deck, for captain, pilot, &c., and from which the vessel may be steered. This is supported on iron rods, braces, &c., not represented in the illustrations, and also by having a round enclosed staircase (St) at each end, leading down to the decks and cabins below. I think this upper deck should be surrounded merely by a light railing, R, so as not to catch the wind. The moving-power is generally one or two propellers, P, within or immediately behind one or two tubes, T T, 2 to 8 feet in diameter, more or less, extending from stem to stern. If, on account of friction or other cause, these long tubes be found inexpedient, I propose two shorter tubes near the stern, either passing through part of the vessel, or altogether on the outside, or, finally, one tube from the stern with a branch passing through each side. By such means I expect that in calm weather such a vessel will advance through the water pretty much like other vessels—perhaps a little faster; that in rough weather the waves will rush harmlessly up the sloping roof and through the open space, O S, to leeward, and that the windward side of the vessel will not be so inconveniently raised, both on account of the heaviest parts being on the outside and on account of the shock of the waves being received on a sloping roof, which will cause that part to be depressed rather than raised; and my object is so to balance these forces, by varying the angle of the slope if necessary, as to keep the vessel steady. I expect, also, that the bow, by modifying the slope if necessary, will advance straight through a wave, instead of rising over it, and thus that the vessel will remain much more steady than is usual, and, as there will be less pitching, rolling, &c., that foundering and sea-sickness, and other dangers and inconveniences will be less likely to occur, and as its course will be shorter, passing through instead of over the waves, that it will also be more rapid. An open space for the waves to pass through, and a high deck beyond their reach, is an old idea of mine revived by the late discussions about the means for preventing sea-sickness. I expect, also, that as the water reaches the screw more directly, this will have more power; that the loss of power from slipping will



THE BREAKWATER STEAMER.

not be so great; and that the screw will act more as if it were rotating in an unyielding substance, or pumping the water from stem to stern. The screw will also be less exposed to danger from ropes, chains, torpedoes, shot, &c. When the wind is favourable, water sails may be used, or other sails with jury masts. I think that flat-bottomed boats built on this plan might be useful for landing passengers through a surf, as at Madras, and a modification of it for life-boats. In these cases a screw driven by a small steam-engine or strong arms would be necessary. Such a plan might enable fishermen to bring themselves and cargo home in safety instead of losing both by being swamped in a sudden storm. Such a plan in sailing vessels might save sailors and deck cargo from being washed overboard, as sometimes happens, and the vessel itself from foundering. In this case a high deck would be necessary, either on the top of the sloping roof, or made by connecting the fore, main, and mizen tops, or by both plans. When I explained my invention to a friend of mine here, he said —"Would you have your vessel lie on the surface of the water like a log of wood?" He expressed my intention almost exactly. I would have my vessel, in rough weather, lie on the water, or rather advance through it, like a log of wood sharpened at the front, the water washing every exposed part of it except the high upper deck. Taking this view of it, there seems to me some similarity between it and the catamaran of Madras: this is merely three logs lashed together; upon it one, two, or three men kneel, and paddle through the surf when no other craft can venture. In this case the catamaran and the legs of the natives washed by the waves may represent the body of my vessel and its roofs; the heads, bodies, and arms of the natives may represent my upper deck and its occupants. Another objection is that a violent storm would soon wash away all my sloping roof. This is easily answered: the first 6, 8, or 10 feet above the water-line must be made as strong as the bulwarks, or rather the sides of other vessels. Beyond 8 or 10 feet the force of the waves is not against the sloping roof, except the mere weight of the water, but obliquely upwards at the angle of the slope; and I would have the movable panels only about 6, 8, or 10 feet above the water-level. Another objection is that straight lines are wrong, because seamen liked a lively boat. My object is to prevent liveliness. Another is, that there would be a choking action in the tubes. This seems to me a question of power: increase the power and strength of materials, or diminish the size of the screw. The engraving is meant to represent a transverse vertical section through the after staircase, St, and a longitudinal vertical centric section, both about $\frac{1}{300}$th of real size, 1 inch representing about 25 feet. T T, tube or tubes for propeller or propellers; W L, water-level; M C, F C, E C, main, fore, and engineers' cabins; M D, main deck; S R, S R, sloping roofs, the bow and stern slooping roofs not lettered; G R, flat roof, partly glazed; O S, O S, open space for waves to pass through below upper deck; U D, upper deck; R R, railings round upper deck.

GEOLOGY.

Physical Geology.—Professor Prestwich, in discussing the origin of the Chesil Bank, has concluded that the shingle was originally derived from the cliffs between Budleigh Salterton and Lyme Regis, and that it was propelled eastward by the action of wind-waves, due to the prevalent and heaviest seas. Referring to the raised beach of Portland, he pointed out that therein were to be found all the materials noticed in the Chesil Bank. Remnants of this beach could be traced in or on the present cliffs, at intervals from Brighton to the coast of Cornwall, and it was to the destruction of this old beach that the pebbles in the Chesil Bank were largely due.

The rocks of the mining districts of Cornwall have been lately described by Mr. J. A. Phillips. He observes that the clay-slates of Cornwall differ materially in composition, but no rearrangement of their constituents could result in the production of granite.

Speaking of the origin of continents, Prof. Seeley stated (at a recent meeting of the Geological Society) that remembering that the lifting power of the moon coresponded to about 1-250,000th part of the earth's weight, and that the sun supplemented this with a power half as great, he ventured to suggest, since the earth's surface is thrown into folds which are proved, by fringing reefs and atolls, to be alternately rising and falling, that these movements might be explained, in part at least, as the effect of tidal movement in the earth itself.

Stratigraphical Geology.—The line of demarcation between the Cambrian and Silurian rocks has ever been a source of dispute amongst geologists, according as they held to the views of Sedgwick, or adopted the classification of Murchison. When first the term Cambrian was suggested by Sedgwick, and the term Silurian by Murchison, these two observers had been working at different areas and independently, and subsequent researches demonstrated that some of Sedgwick's higher Cambrian rocks were equivalent to Murchison's lower Silurian rocks. The former first demonstrated that the only great break in the entire series occurs at the base of the May Hill Sandstone, or Upper Llandovery group, the lowermost bed of Murchison's Upper Silurian. Murchison's classification has, however, prevailed; it has been accepted by the majority of geologists, until the last year or two, when a tendency has sprung up to adopt the classification of Sedgwick, because the labours of geologists in Wales and in the Lake District go to prove its truth. It may be observed that no base-line has existed for the Silurian system, that it has been constantly shifted to agree with the results of palæontological research, until nearly the whole of the old Cambrian system has been swallowed up in it by some writers. Professor Hughes recently observed, at a meeting of the Geological Society, that the bottom of the May Hill Sandstone constitutes the base of the Silurian system, and that the beds below, called Cambrian by Sedgwick, form a well-defined natural group, which, following the true principle of classification and justice to our nomenclature, we must adopt.

The development of the Kimmeridge Clay in England, and its palæontogical contents, have formed the subject of an important paper by the Rev. J. F. Blake. He would divide the formation into two sections, attaining together in places a thickness of 650 feet. Professor Seely described a new Chelonian obtained from the Kimmeridge Clay, which he termed *Pelobatochelys Blakei.*

Mr. Jukes-Browne has brought forward reasons for concluding that the Cambridge nodule-bed belongs to the base of the true Chalk Marl. This opinion is borne out by Mr. Whitaker from experience gained in mapping the Geology of the Cambridge district.

The Geology of the Burnley Coalfield and of the country around Clitheroe, Blackburn, Preston, Chorley, Haslingden, and Todmorden, has been described in great detail in a memoir published by the Geological Survey, the work of Professor Hull and Messrs. Dakyns, Tiddeman, Ward, Gunn, and De Rance. The work contains a valuable list of the fossils by Mr. Etheridge, and a Catalogue of all the works, numbering 561, that have been published to illustrate the Geology of Lancashire.

Palæontology.—Fossils indicative of a transitional zone between the Permian and Carboniferous strata were obtained at Spitzbergen by the late German Expedition.

Mr. James Thomson has described some forms of corals from the carboniferous limestone of Scotland which he regards as new species and as belonging to three new genera allied to *Clisiophyllum*, which he names *Rhodophyllum*, *Aspidiophyllum*, and *Kurnatiophyllum.* The specimens are particularly interesting from the evidences of variation which they present.

Dr. Dawson has noticed the occurrence of *Eozoon canadense* in Lower Laurentian rocks at Côte St. Pierre, on the Ottawa River. He referred particularly to the resemblance of weathered masses of *Eozoon* to Stromatoporoid corals.

An interesting section of the Rhætic beds is being opened up on a line of railway in process of construction between Melton Mowbray and Nottingham.

Prof. Huxley, in describing some new remains of *Stagonolepis*, observes that in outward form it resembled the existing Caimans of intertropical America, except that it possessed a long, narrow skull like that of a Gavial.

Geologists' Association.—At the Annual Meeting of this Association, held on

February 5th, 1875, Mr. W. Carruthers, F.R.S., was elected President; Mr. Etheridge, Professor Rupert Jones, Mr. J. L. Lobley, and Mr. Henry Woodward being elected Vice-Presidents.

Sub-Wealden Exploration.—It is to be regretted that the bore-hole, which had been carried to a depth of over 1000 feet, has been abandoned, owing to obstructions caused by accident to the machinery and loss of the tools. Six ineffectual efforts were made to recover them. The Diamond Boring Company have made a favourable offer to commence again, and Mr. Willett, the energetic honorary secretary of the Committee directing the Exploration, has guaranteed £600, and appeals for funds to carry on the enterprise. So far as the old boring is concerned, the results obtained are of great interest, and in a scientific point of view they are by no means unsuccessful; but the immediate object being to prove the depth of the Palæozoic rocks in the Wealden area, it is to be hoped that the second boring may obtain the desired information, which, while of so much interest to geologists, is most likely to throw some light on the vexed question of the probability of productive coal-measures beneath the secondary rocks of the eastern and south-eastern counties. A new boring has actually been commenced near to the old one, and a depth of 40 feet has been reached.

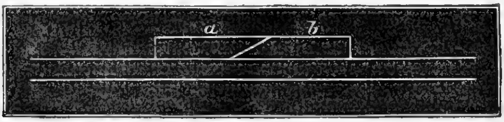
Geological Society of London.—At the Annual General Meeting of this Society, held on February 19, Mr. John Evans, F.R.S., President, in the Chair, the Wollaston Gold Medal was presented to Professor de Koninck, in recognition of his extensive palæontological researches, especially among the Carboniferous rocks. The balance of the proceeds of the Wollaston Donation Fund was awarded to Mr. L. C. Miall, to assist him in his researches on Fossil Reptilia. The Murchison Medal was presented to Mr. W. Jory Henwood, F.R.S., &c., as a mark of the Society's appreciation of his observations on metalliferous deposits; and the Murchison Geological Fund was presented to Professor H. G. Seeley, in recognition of his various palæontological researches. In his address, Mr. Evans discussed the geological evidence of the antiquity of the human race.

PHYSICS.

LIGHT.—Writing to the "Chemical News," Mr. W. H. Olley draws attention to an adaptation which he made, about two years since, of polarised light to the well-known and beautiful optical instrument, the kaleidoscope. This instrument, it need hardly be observed, consists of a tube, within which are fixed, at an angle representing an aliquot part of a circle, two or three plane mirrors, blackened on the outside, so as to give by repeated reflection symmetrical and constantly varying images of pieces of coloured glass or other transparent objects, placed in a cell at one end of the tube, the images being viewed through a small aperture at the opposite end. It occurred to Mr. Olley that if pieces of selenite were substituted for the fragments of coloured glass, and polarised light were made to pass through the instrument, there would be seen, with the aid of an analyser, the beautiful colours given by that familiar double-refracting substance, reflected like those of ordinary light. Accordingly he selected films of selenite, presenting, when analysed, various colours corresponding with their respective thickness, and mounted them on pieces of thin crown glass of different shapes and sizes, placed them in the cell of a kaleidoscope, and adapted a tube to the other end so as to receive a second tube fitted with a small Nicol prism, or a tourmaline. He then found that when light polarised by a plate of glass, or even by a cloudless sky, was transmitted through the cell, the reflected images of the selenite mountings were symmetrically and beautifully displayed. To enlarge these polarised images, and to increase the strength of their colours by concentrating the rays, he adapted in some of the instruments a lens of appropriate focus to the eye-end of the tube just below the Nicol, or analyser, but this addition is not necessary.

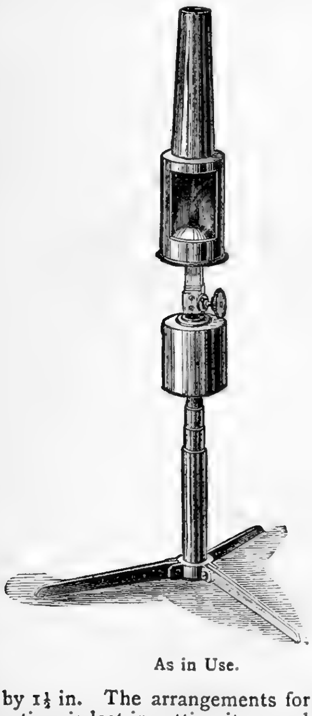
Microscopy.—Count F. Castracane ("Der Naturforscher") has demonstrated the existence of Diatomaceæ during the coal period. His first object of investigation was a piece of Lancashire coal, which was pulverised, and then exposed to a white heat. The decarbonised dust was then treated with nitric acid and chlorate of potash in test-tubes, and washed clean with distilled water, and then placed under the microscope. The Diatomaceæ found in this coal belong, with the exception of a *Grammatophora*, of a small *Coscinodiscus*, and of an *Amphipleura*, entirely to fresh-water genera and species, such as the following:—*Fragillaria Harrisonii*, Sm.; *Epithemia gibba*, Ehbg.; *Nitzschia curvula*, Kz.; *Cymbella scolica*, Sm.; *Synedra vitrea*, Kz.; *Diatoma vulgare*, Bong. The presence of the marine forms which were present among the very numerous fresh-water diatoms, only in one single specimen, appears to prove that, at one time, even sea-water found its way among the vegetation from which the coal originated. Besides this Lancashire coal, Count Castracane investigated coal of the carboniferous era from other localities, *e.g.* a piece of the so-called Cannel coal from Scotland, from Newcastle, and from the mines of St. Etienne. In every one of the pieces the presence of Diatomaceæ in greater or less numbers was proved. The species varied in the three different specimens of coal, but, as in the case of the Lancashire coal, not even a single new form was discovered, but all closely agreed with the existing fresh-water Diatomaceæ, from which they could not be distinguished by the most practised eye; all the signs by which the species of Diatomaceæ are generally distinguished are, in the Diatomaceæ of the coal period, identical with those of existing species; so that these organisms, in the indeterminably long period from the coal epoch to the present time, have undergone no perceptible modification.

Mr. Wenham communicates to the Royal Microscopical Society (March 3, 1875) an easy method of obtaining oblique vision of surface structure under the highest powers of the microscope. As the closeness of the high powers to the covering-glass will not permit the slides to be tilted to an extent to cause any appreciable difference in the appearance of an object, the following arrangement is to be adopted:—*a* is a slip of glass about $\frac{4}{10}$ths of an inch

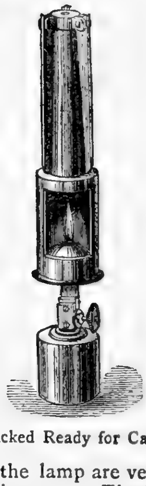


wide, ground and polished off to an angle. Objects to be mounted, such as diatoms or lepidopterous scales, are scraped up with the knife edge, so as to be distributed thereon along the sloping plane. Those situated near the edge may be viewed with the highest powers, as the glass is of course thinner here than any cover. The thickness of the remainder of the prismatic slip is of no consequence, and it may be of the same gauge as an ordinary thin slide. The slip is tacked on to a 3×1 slide with a dot of balsam or cement. Another similar slip *b* is then pressed endwise against it, so as to lay the objects flat between the two inclines. The lower prism is necessary, for without it a deal of offensive colour enters the object-glass from the decomposition of the transmitted light. This is recomposed or neutralised by the under prism, which also greatly increases the obliquity of the illuminating ray by refracting this to the same angle as that of vision, from the deflection of the axial ray of the object-glass. The degree of inclination of the facets of the prisms should be less than 40°, for on holding before a flame a slide having this angle, and tilting it slightly, the width of the junction of the prisms appears as a dark band impervious to light—the effect of total reflection. About 35° is therefore more suitable for objects mounted dry. If balsam is run in between the inclines, of course total reflection is eliminated and refraction nearly so, and we then see the object at an angle *the same as the incline*; therefore for objects in balsam 45° would be preferable. In using slides with these prismatic or inclined mounts, it must be borne in mind that nearly every object lies under a different thickness of glass, according to the distance from the keen edge; therefore having selected a suitable one, the objective is to be carefully adjusted to give proper definition, and a thickness of glass over the scale may probably be found that will best suit its correction. For dry mountings, the object-glass must be used dry, as water would run in and spoil the object. If this is in balsam, of course the immersion system can be employed. The prismatic mounting slips may easily be made by any ordinary glass grinder. Thin well-polished sheet glass (such as is used for microscope slides) is cut into pieces, three quarters of an inch long by four-tenths wide. Eight or ten of these are cemented together with hard Canada balsam, and their step-like projecting edges adjusted against a bevel set at the desired angle, say 35°. They must be pressed into very close contact, as it is important to have the edges worked fine and clean, and any thickness of balsam stratum between them will prevent this, for during the work close edges mutually protect each other. Having prepared a sufficient number of blocks in this way, they are cemented on to a runner or metal plate with the usual cement of pitch and wood ashes. They are then ground and smoothed on a flat metal lap, till all the steps are gone and keen edges shown, and are then finally polished.

A lamp for the convenience of microscopists who are in the habit of using their instrument away from home has recently been contrived by Mr. Swift. It possesses the great merit of extreme portability in combination with the rare merit of perfect efficiency. The dimensions of the cylindrical case are



As in Use.



Packed Ready for Case.

8¼ in. by 1½ in. The arrangements for packing the lamp are very simple, so that no time is lost in setting it up and putting it away. The burner is flat, of small dimensions, but the flame is quite large enough, and the light of good quality. The chimney is of metal, the lower part being cut away so as to allow a sufficient aperture for illuminating the microscope. The white lining is covered with glass, so that the smoke of the lamp cannot blacken it. Small as the reservoir appears to be, it contains oil enough to last four hours. The means taken to prevent leakage are very ingenious; a carefully-fitted screw-cap prevents leakage at the burner, but the greatest ingenuity is displayed in fitting the pinion, which raises the wick to prevent escape of oil. So effectual are the precautions taken to keep in the oil, that the writer received the lamp by carrier, and has since carried it inverted in his pocket, without the slightest inconvenience from leakage. The flame can be adjusted at any level from about 4 to 12 in. The general form of the lamp can be easily understood from the accompanying woodcuts.

Heat.—Under the supervision of Dr. R. Carter Moffat, a large party of gentlemen connected with shipping and the Board of Trade recently assembled at Greenhithe, near Gravesend, to witness the power of Paton and Harris's Pyroletor to extinguish fire in closed places. The pyroleter consists of a small double pump worked by hand, which sucks up from tubes on either side of it strong muriatic acid and a solution of bicarbonate of soda, which commingle in a generator forming part of the pump, and the carbonic acid gas and solution of salt pass at once down a metal pipe to the hold, along whose keelson runs a perforated wooden box which admits of the gas passing through to the burning material. The agent, therefore, for the extinction of fire is dry carbonic acid gas, which has no action on cargo. A steamer conveyed the party from Blackwall to Greenhithe, where a large wooden barge had been prepared for the experiments. Its entire hold was covered to a depth of several feet with wood shavings, cotton-waste saturated with turpentine, and naphtha. A temporarily-raised and by no means air-tight wooden deck, with loosely-fitting boards, formed the wide hatchway covering. After the apparatus had been explained by Dr. Moffat, the pipes to the chemicals were attached, and the signal given to set fire to the inflammable materials in the hold. Immediately the flames ran along the entire cargo and issued above the temporary deck, which was then covered with boarding. The pyroletor having been brought into action, and although nearly half a gale of wind was blowing, the fire was completely extinguished in four minutes. The experiments were so completely successful, and the efficiency of the apparatus so apparent, that the party at once agreed to sign a memorial to ask Government to compel all long-passage ships conveying passengers and cargo to carry one of these instruments. It is computed that a 1200-ton ship requires about half a ton of each of the chemicals, which, with their packages, cost about £20.

Electricity.—From a report by Count du Moncel on the thermo-electric battery of M. Clamond we gather that, in this battery the electro-positive element is of iron, and the negative element an alloy of antimony and zinc: and, as in the arrangement of Mr. Farmer, the elements are connected circularly for intensity, forming a kind of crowns, isolated from each other by plates of amianthus, and having their polar extremities placed in connection with a commutator, fixed tangentially to the cylindrical surface of the apparatus, and contrived so as to cause these crowns to be grouped either for intensity or for quantity, as may be requisite. The apparatus has been employed for six months at the galvano-plastic works of M. Goupil at Asnières. The gas consumed amounts to 2½ frs. per kilo. of copper deposited.

M. J. Morin has invented a new electro-medical galvanoscope. According to the description in the *Comptes Rendus* it consists of an ordinary two-branch electro-magnet, placed vertically, the breach being in the air. A magnetic needle is suspended by one of its poles over the breach, through which it penetrates by means of a large hole. The lower free pole of the needle descends as far as the level of the lower part of the electro-magnet's helices, between which it is able to oscillate. The needle is long enough to penetrate the breach to the height of its neutral point, thus nullifying at that spot all reciprocal action. On making a current circulate in the helices the two poles act in the same direction upon the free pole of the magnetic needle, causing it to be displaced towards one of the helices according to the direction of the current.

TECHNOLOGY.—A Board has been appointed by the Government of the United States to determine by actual tests the strength and value of iron, steel, and other metals which may be submitted to it, or by it procured, and to prepare tables which will exhibit the strength and value of said materials for constructive purposes. It has standing committees on abrasion and wear, on armour plate, on chemical research, on chains and wire ropes, on corrosion of metals, on the effects of temperature, on girders and columns, on metallic alloys, on physical phenomena, on steels produced by modern processes, &c.

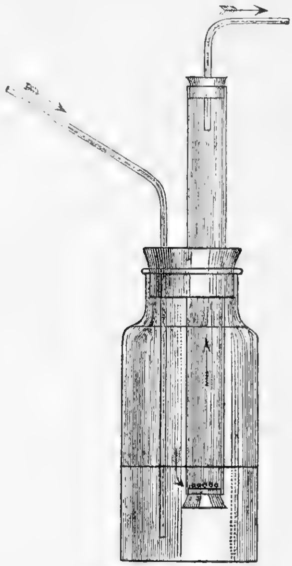
Th. Schlösing, in a course of studies on arable soils, has examined the influence of the salts present in a soil upon its openness of texture. Mould stirred up in water subsides the more rapidly the more salts are present, especially those of lime and magnesia. This phenomenon, which amounts to a coagulation of the clay, is thoroughly examined. The best precipitants for clay are caustic lime and the lime salts.

A good cement for marble and alabaster may be obtained by mixing 12 parts of Portland cement, 6 parts of slaked lime, 6 parts of fine sand, and 1 part of infusorial earth, and making up into a thick paste with silicate of soda. The object to be cemented does not require to be heated. It sets in twenty-four hours, and the fracture cannot be readily found.

M. C. H. Viedt has experimented on aniline inks. For a red ink he recommends a solution of 1 part of "diamond fuchsin" (we should say of Brooke, Simpson, and Spiller's rosein) in 150 to 200 parts of boiling water. For a blue, he dissolves 1 part of bleu de Paris in 200 to 250 of water. For a violet, 1 part of a Hofmann's violet (blue shade) in 300 of water. A beautiful green ink is made by dissolving 1 part of iodine green in 100 to 110 parts of boiling water. The yellow aniline inks are not recommended. These inks are not fit for copying, but they have the advantages of drying quickly, and of never clogging.

Prof. J. Nessler points out that genuine wines contain chiefly malic acid. Free tartaric acid is very rarely found, except in spurious concoctions. As a test the author uses a solution of 5 grms. acetate of potash, 5 grms. alcohol, and 25 grms. water. If an appreciable amount of tartaric acid is present, this test produces a crystalline deposit of tartar in a quarter of an hour; whilst, in genuine wines, even if they contain a trace of tartaric acid, no precipitate appears until some hours have elapsed. Genuine wine contains no citric acid. For its detection in small quantities, the wine is rendered alkaline and filtered, acidulated with acetic acid, mixed with chloride of barium, filtered, and a few drops of ammonia added to the filtrate until it has an alkaline reaction. If, on the addition of baryta water, a white precipitate appears, citric acid is present. Oxalic acid gives a white precipitate if lime-water is added in such small quantities that the liquid has still an alkaline reaction. Sulphuric acid in genuine wines is found only to the extent of 0·03 to 0·05 per cent.

When soluble gases are highly diluted with those which are insoluble, their complete absorption is a matter of difficulty when attempted by drawing the mixture through simple columns of the absorbent liquid, owing to the protecting effect of the particles of insoluble gas. In such cases, and with the ordinary wash-bottle arrangement, complete absorption can only be effected by greatly multiplying the pieces of apparatus. Mr. N. Glendinning has arranged a new form of apparatus, of which the annexed is a drawing. It consists of a bottle capable of holding 20 fluid ounces, which is provided with two tubes passing through a perforated india-rubber bung. The tube which has to convey the gases to the apparatus has a bore of about $\frac{3}{16}$ths of an inch, and reaches to within half an inch of the bottom of the bottle. The other tube is about 12 in. long and 1 in. in diameter. One end of this tube is closed, but is provided at about half an inch from this end with twelve or fifteen small holes distributed around it, forming a ring, the holes being equidistant from the end of the tube. The other end of this tube is fitted with a cork, through which passes a small tube. To prepare the apparatus for use, about 8 ounces of the absorbent liquid are put into the bottle, the larger tube partly filled with glass splinters, and depressed so that the apertures dip about half an inch into the liquid. The conveying tube is now introduced into the pipe or chamber from which the sample has to be taken, and the aspirator attached to the other. The liquid will now rise in the larger tube



until that in the bottle has been drained to the level of the apertures, and the gases which have already been partly washed in their passage through the stratum of absorbent liquid will now pass through the small apertures, and through the column of liquid in the tube, where their absorption is favoured by the breaking-up action of the glass splinters, and the agitation consequent on the passage of gas bubbles. When a sufficient quantity of the gases has been drawn through the apparatus, it is detached and shaken, so that the soluble gases existing in the atmosphere of the bottle may be absorbed. The cork is now removed, the glass splinters thoroughly rinsed into the bottle, and the liquid made up to any required bulk.

A paper on "Toughened Glass" was recently read before the Society of Arts by Mr. Perry F. Nursey, C.E. The subject being of importance, we do not hesitate to give a somewhat lengthy notice of the paper. After a reference to the origin and progress of the manufacture of glass, Mr. Nursey says:—Many years since M. de la Bastie was impressed with the desirability of rendering glass less brittle, and so extending the sphere of its usefulness. Aware that the fragility of glass results from the weakness of the cohesion of its molecules, he argued that, by mechanically forcing the molecules closely together, and rendering the mass more compact, the strength and solidity of the material should be increased. This is exactly the line of argument an engineer would follow—it is one which led Sir Joseph Whitworth to produce such splendid results in the well-known Whitworth metal, and it is one also which has led to success in casting in other departments of engineering. It is, however, not one which landed M. de la Bastie on the right side of all his hopes and fears, inasmuch as he found, after long trial and experiment, that mechanical compression failed to influence glass in the slightest degree, even when applied while the material was in a fluid or soft condition. He therefore changed his tactics, and commenced to apply to glass a system of tempering, such as is usually applied to steel, namely, submitting it to a bath of heated oil. He knew well that by immersing heated glass in cold water he would only put the material in a state of unstable equilibrium, so that the least shock would cause it to break up, as in the case of the Rupert drops. He then sought to invert this result, to diminish, or even to remove, the extreme fragility of glass by tempering it by immersion in a fluid other than water. In attaining this object two essential objects had to be determined. First, the point at which glass can be tempered without being put out of shape, and secondly, the medium to be employed for the immersion of the glass. The first condition M. de la Bastie found to be that degree of heating at which softness or malleability commences, when the molecules are capable of closing suddenly together, condensing the material when it is plunged in a liquid at a somewhat lower temperature. The second condition he found was satisfied by having a fluid capable of being raised to a much higher temperature than that of boiling water, without entering into a state of ebullition. For this purpose, and after a long series of experiments, M. de la Bastie devised an oleaginous compound, formed of oils, wax, tallow, resin, and other similar ingredients in certain proportions. Although the invention is apparently a most simple one, there are many delicate conditions involved, the disregard of any one of which constitutes the precise difference between success and failure. It thus happened that, seven years since, just as M. de la Bastie had perfected his invention, and had produced highly satisfactory results, he lost the clue to his success, and for two years was baffled in every attempt to re-discover it. He at length succeeded in regaining his secret, and has since been engaged in perfecting his invention, and putting it into a practical shape. He had to carefully adjust all the numerous details, for although the invention consists in simply heating the glass, and dipping it while hot into a heated oleaginous bath, there are many conditions involved. Thus glass articles may be underheated, and will not be susceptible to the effect of the bath, or they may be overheated, and will lose their shape; or, again, they may be heated to the right temperature, and yet be spoiled during the process of transference into the bath. Then, again, the exact proportions of the oleaginous constituents of the bath, and their precise temperature, have an important influence upon the ultimate result. All these points, however, with many others, have been definitely settled by M. de la Bastie, who has for some time past worked his process experimentally, and is now erecting a factory in France, in which to carry it on practically and commercially. It may be as well that I should here mention that it is recorded by Pliny, that in the reign of Tiberius a combination was said to have been devised by which a flexible glass was produced, and that the machinery by which it was made was destroyed in order to prevent a depreciation in the value of the precious metals. We have, however, no evidence that this was the toughening process invented by M. de la Bastie, and the statement to which publicity has recently been given, in no whit detracts from the merits of that gentleman as the inventor of an important economic process. Nothing more than the bare fact above alluded to is on record, except it be, perhaps, that the hapless inventor was destroyed as well as his apparatus. But there was no Society for the Encouragement of Arts, Manufactures, and Commerce in those days. In carrying out his process, M. de la Bastie finds it necessary to raise the glass to be tempered to a very high temperature. The hotter it is the less the risk of breaking the glass, and the greater the shrinkage or condensation. Hence the advantage, and often the necessity, of heating the glass to the point of softening, which is attended by the difficulty that glass in the soft condition gets readily out of shape, so that it must be plunged into the bath almost without touching it. In plunging the hot glass into a heated combustible liquid the latter is apt to take fire, and cannot easily be extinguished, so that time and material are lost. These difficulties M. de la Bastie has overcome by placing the tempering bath in immediate communication with the heating oven, and covering it so as to prevent access of air. The oven being charged with the articles to be tempered, they are made to slide into the adjoining bath without being handled, and the contents of the bath, having no supply of external air, are not liable to inflame. In order that the shape of the tempered articles may not be affected, particularly flat glass, the floor of the oven is made to cant, so that, when the glass is heated on it, it is turned to a sloping position, and the glass slides into the bath, along a surface arranged in it at the same angle as that of the oven floor. The clearness of the glass may be affected by the dust of the furnace flame, which is apt to settle on the glass and chill its surface. This is avoided by heating the glass in a muffle, to which the flame has no excess, being applied externally. The shock of the fall of glass into the bath is prevented by fixing in it a sheet of wire gauze, or asbestos fabric, for the glass to fall on. Of course the condition of working would be considerably modified where glass manufacturers adopted the toughening process in their own works. In such case the toughening process would simply take the place of the present annealing process, than which it is much more speedy and economical. The glass would then be treated just at the point at which it passes from the fluid to the solid condition, and would not require reheating. By the substitution of this process for that of ordinary annealing, the saving would be considerable. There would be, first, the saving of the fuel used in the annealing ovens; next, the saving of the time required for annealing; thirdly, the saving in breakages, besides a saving in labour as well as in other directions. The physical change which glass thus treated undergoes is no less complete than remarkable. Its extreme brittleness is exchanged for a degree of toughness and elasticity, which enables delicate glass articles to be thrown indiscriminately about the room, and more substantial ones to resist the impact of heavy iron weights falling from considerable heights. Upon my first making the acquaintance of toughened glass articles at the offices of Messrs. Abel Rey and Brothers, 29, Mincing Lane—Messrs. Rey being the representatives of M. de la Bastie—watch-glasses, plates, dishes, and sheets of glass, both coloured and plain, were thrown across a large room, and fell spinning on the floor. Water was boiled in a tempered glass saucer for some time over a brisk fire, and the saucer was quickly removed to a comparatively cold place, and stood on iron, but was in no way affected by change of temperature. A small piece of plate-glass was held in a gas flame until the corner became very hot. The glass proved a bad conductor of the heat, which did not extend any appreciable distance beyond the point of contact with the flame, neither was the glass cracked from unequal expansion, nor was it damaged by sudden immersion in cold water. In order to judge of the comparative resistance offered by untoughened and toughened glass to the force of impact, a piece of the former, measuring 6 inches by 5 inches, by one-eighth inch thick, was supported in a frame about half-an-inch from the floor. A 2-ounce brass weight was then dropped upon it from 12 and 18 inches respectively without damage, but on the height being increased to 24 inches, the glass was broken into several fragments. A piece of toughened glass of the same size, but rather thinner, was then treated in the same way, at heights increasing a foot at a time up to 10 feet, but without producing the slightest visible impression. I say "visible" impression, because it is possible that, by the repetition of the blows, the structure of the glass may have become imperceptibly altered. We all know that by repeated blows the fibrous nature of wrought-iron becomes exchanged for the crystalline character of cast-iron. Finding the 2-ounce weight to make no impression, an 8-ounce iron weight was substituted, and was dropped on the glass from a height of 2 feet, and then of 4 feet, without fracturing it. On the height being increased to 6 feet, however, the glass broke with a distinct report. But here another phenomenon presented itself; instead of the toughened glass being broken into some twelve or fifteen large angular pieces, as was the ordinary glass, it was literally reduced to atoms. There were, it is true, some pieces about half-an-inch square, but these were traversed in all directions by delicate lines of fracture, and, on being gently touched, crumbled into small pieces; and many of these small pieces were easily reduced by gentle pressure into mere atoms, so thorough and so complete does the disorganisation of the entire mass appear to be. All these points will be practically demonstrated at the conclusion of my paper. A similar result is produced by placing a piece of toughened glass flat on the table, with a corner projecting over, and endeavouring to chip the corner off with a hammer. The corner will, after a series of smart blows, be broken off, but the whole mass will be at the same moment disintegrated and reduced to atoms. Another peculiarity about toughened glass is that the fragments are by no means so sharp, and therefore so capable of piercing the flesh, or of causing incised wounds, as are those of ordinary glass. One important point of difference between M. de la Bastie's toughened glass and Prince Rupert's drops is that, although the skin of the former may be scored through with the diamond, the body cannot even then be broken through by ordinary force, much less does the mass fly to pieces and disintegrate, as in the case of the Rupert drops. Still wider will this difference appear when I state that toughened glass is readily susceptible of a high degree of polish, and it can be cut by the wheel for lustre-work and such like. The glass can likewise be engraved, either by hydrofluoric acid, or by Mr. Tilghman's elegant sand-blast process. It will thus be seen that toughened glass presents features which appear to some extent paradoxical. It would appear that toughened glass possesses enormous cohesive power, but that if the equilibrium of the mass is disturbed at any one point, the disturbance, or disintegration, is instantaneously communicated throughout the whole piece, the atoms no longer retaining the power of cohesion. It is as though the glass was endued with a nervous system, a shock to which at any one point instantly and utterly demoralised the whole. It is important to note that neither transparency nor colour in glass is in any way affected by the process of toughening, and the ring, or sound emitted upon the glass being struck, is nearly as clear in toughened as in plain glass. In order to determine the relative values of ordinary glass and the toughened material, as regards their strength, I suggested to Messrs. Rey the propriety of instituting experiments, with the view of ascertaining their respective resistances to ordinarily applied stress. In these experiments I have been ably assisted by Mr. Kirkaldy, whose perfect testing machinery has, for some years past, supplied a want long previously felt by the engineering profession. Twenty pieces of glass in all were submitted to bending stress, ten being toughened and ten untoughened. The glass was of French manufacture, and was that known as "Rive de Giers." Each piece of glass measured, as nearly as possible, 6 inches in length by 5 inches in breadth, and the samples had a mean thickness of 0·2259 of an inch. Each piece was placed with a bearing of half-an-inch at each end, and the weight was brought gradually upon the centre; in some instances by the testing-machine, and in others by direct weights. Taking two pieces of glass, having about the same sectional area—the one tempered and the other untempered—Mr. Kirkaldy's certificate shows that the untempered glass yielded under a strain of 279 lbs., whilst the toughened glass did not give way until a stress of 1348 lbs. had been reached. The same proportion, however, did not occur throughout the series, the toughened glass giving in some instances lower results. This arose from two causes—the diminished area of some of the samples of glass, and from the fact that, in some instances, the process of toughening had not been perfectly carried out; for the samples were prepared by M. de la Bastie under purely experimental conditions. The imperfect tempering was made manifest, after the destruction of the glass, in three ways chiefly; first, by the glass showing needle fractures, such as are seen in untoughened glass; secondly, by a faint milky line presenting itself in looking at the glass in section; and, thirdly, by portions of the glass, a square inch in area, remaining unfractured, whilst the whole surrounding mass was reduced to atoms. But above and beyond all this, it was evident that the strains applied were such as could not possibly come upon glass articles in ordinary use. They were long-sustained pressures, tending at every increment of weight to alter the relative position of the particles of the glass, but affording them no opportunity of returning to their normal position, or, in other words, of utilising the elasticity of the mass.

Glass articles in ordinary use are subject to sudden sharp blows, either from falling down, or from some extraneous substance being brought smartly in contact with them. Under these conditions the elasticity of toughened glass is called into play, and enables it to sustain a shock immeasurably beyond that which would suffice to destroy ordinary glass, as is shown by the experiments first described. Hence the proper tests for glass, either toughened or plain, are precisely those of smart and sudden impact, and not of prolonged stress. Examination and experiment with this remarkable substance have revealed a number of most interesting facts with regard to its physical character. The limits of a paper, however, forbid me entering upon these considerations at such length as I could wish, and as the subject deserves. I may, however, mention that the microscope reveals the fact that the fractures follow a regular order, which gives a uniform shape to the crystals which they produce. Large crystals can be subdivided into several smaller ones with a similar result. The edges of the atoms, too, are not jagged and serrated, as are those of ordinary glass, hence their diminished tendency to cause incised wounds, as already mentioned. This peculiarity would afford a means of ascertaining whether the glass had been tempered or not. The physical character of toughened glass has been made the subject of careful investigation by M. Victor de Luynes, who made the results of his researches the subject of a lecture, which he delivered at the annual meeting of the Société de Secours des Amis des Sciences. I hoped to have embodied in my paper some of the results obtained by M. de Luynes, but, unfortunately, the rules of the Société do not permit the publication of lectures until they have passed the examination of a committee, which process M. de Luynes' paper is only now undergoing. I may, however, mention that M. de Luynes had a furnace and bath in the lecture-room, and before his audience he tempered glass objects, which were afterwards successfully tested. As a general result, M. de Luynes has found that toughened glass will bear from 80 to 100 times the strain of ordinary glass. M. de Luynes also examined both plain and toughened glass by the aid of polarised light, the results of his examination going to show that toughened glass owed its peculiar characteristics to a condition of intensified compression. I have explained what toughened glass is, how it is produced, and what are its leading features, so far as at present ascertained. It therefore only remains for me to indicate the direction of its practical application. I say "indicate," for were I to enumerate all the purposes to which it can be usefully applied I should simply become wearisome. It is possible that there is not one corner in the whole domain of the arts, sciences, and manufactures, where its presence will not in time be made manifest in some way or other, and its usefulness appreciated, whilst for purposes pertaining to social life its application would seem to be unlimited. The miner would have a safer safety lamp than even Davy gave him, and the engineer's gauge glass would stand the highest steam pressure and alternations of heat and cold without fear of mischance. In chemical works it would supersede lead for tanks, and the present costly and unreliable glass pump-tubes would be far less expensive, and infinitely more durable. So with brewers; they would find it a most useful friend in their vats, which they could thoroughly and easily cleanse, and keep free from those secreted stale germs of organic life which develop and reproduce themselves in the process of fermenting beer, in a highly objectionable manner. For water-pipes it would offer the advantages of strength, without corrosion. Assayers, I am told, would use it instead of platinum in some processes. In silk-spinning machinery, slider-eyes, or guides, which are so soon cut through by reason of the speed at which the silk passes through them, would be rendered very durable if made of toughened glass. Another application, which has suggested itself to an ingenious American gentleman since the first notice of M. de la Bastie's invention appeared, is the manufacture of printing-types, and rollers for printing-presses, and this idea is now being developed into practical form. Seeing the wide range of domestic and social wants which toughened glass promises to meet, I know not where to begin, and were I to begin I should not know where to end. I can only observe in this connection that toughened glass promises to supersede porcelain and similar wares, and to add a real and permanent value to glass utensils of every kind. It will probably supersede enamel on culinary utensils, and in other similar directions. It might be thought that this invention would prove a disadvantage to the glass trade, but the widely-increased use of glass for purposes which its brittleness has hitherto unfitted it should be a sufficient answer to any such objectors. If there were any such, they would naturally be those connected with the glass trade. But I do not find them there. On the contrary, some of our most eminent glass manufacturers are now negotiating with M. de la Bastie's agents for licenses to work the patent in England. I may observe, that it is not at all improbable that the invention will receive its first practical application in the Aquarium now in course of erection within a short walk of the house wherein we are now assembled. Such, then, is one of the most notable inventions of modern times, an invention so remarkable, so unique, and apparently so fraught with import to the arts, sciences, and manufactures, as to render it probable that the name of De la Bastie will one day occupy no mean position amongst those of men by whose genius science has been enriched, and the nations practically benefitted.

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THE QUARTERLY
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OCTOBER, 1875.

I. ANIMAL DEPRAVITY.

"Selten habt ihr mich verstanden,
Selter auch verstand ich euch;
Nur wenn wir im Koth uns fanden
Da erkannten wir uns gleich."

HEINE.

"IT is of no use to talk about reason," said a friend with whom we had been discussing the subject;—"If you wish to establish man's kinship with brutes, you must prove that they, too, are capable of vice, his imagined prerogative." We could not deny that this was sound counsel. In sermons and platform-orations and in leading articles man declaims, indeed, in favour of "virtue." But listen to him in his more confidential moments when he flings aside his disguises. You will find that he then pronounces such of his own species as make some apparent approach to this official standard "nincompoops or hypocrites." The faint praise with which he damns goodness but half hides the underlying sneer. Scarcely can you, in the German language, speak of a man in terms which convey a lower estimate of his abilities or his energies than when you call him "eine gute Haut," or "eine gute Seele." On the contrary "ein böser Kerl" is always understood to be clever and plucky. Even the virtuous English, senior wranglers in the school of hypocrisy, have similar idioms. "A good boy," "a moral young man," "a very good sort of fellow," "a man with no harm in him," are terms used by no means in a complimentary sense. Of all the literary diseases of the day "goody-goodyism" is the one most despised by cultivated men of the world. On the other hand, when a woman is particularly well pleased with her lover does she not always call him a "naughty man?" Do all these phrases spring from a secret conviction that human vices are restrained less by conscience and high principle than by weakness or cowardice? Does the world suspect that the good man has often merely "nothing in him?"

But when we attempt to treat of the morals of brutes, in order to find whether in that region lies the much talked-of but evanescent boundary-line—when we seek to show that vice is, after all, not man's exclusive attribute, we are met at once with the objection—"Animals have, and can have, no moral life, as is man. They have no perception of right and wrong, but simply follow their propensities, and obey the laws of their being, from which, indeed, they have no power to depart." * This is, I think, a tolerably fair specimen of the language which *demi-savants* habitually use when treating of the lower animals. "The kingdoms of freedom and of nature" is an antithesis common in their mouths,—the "kingdom of freedom," forsooth, signifying mankind! It is, of course, exceedingly convenient to have some imaginary *a priori* reason which renders any appeal to facts superfluous, or rather altogether impertinent. Being neither lunatics, metaphysicians, Calvinists, nor fallen angels, † we shall not enlarge upon "freedom;" we will merely declare that if men's vaunted freedom relates to *action* it is shared by the gorilla. He is perfectly free to rise up or sit down, to come or go, to crack a nut, or to crush the skull of a "man and a brother," just as he may think proper. That he is "free" to love or to hate ‡ to fear or hope, to believe or disbelieve, or in short to experience any emotion, passion, feeling, sentiment, or frame of mind, we deny, just as we deny it of man. Now to the more immediate question.

In the first place we must judge every animal from what may be called its own point of view, not with reference to man and his notions of advantage or convenience. He calls the wolf and the tiger cruel, the viper malignant, and the spider treacherous. This is idle talk. The wolf can only subsist upon animal food, and is no more to be censured for devouring the lamb,—for which he may further plead man's conduct in precedent—than is the lamb for devouring grass. Why, moreover, should the vegetarian,—brute or human—presume to denounce the flesh-eater as cruel? Have plants no rights? Are we sure that, if they could be consulted, they would consent to be plucked and eaten? They have, it is true, no demonstrable nervous system. But in view of the manifold ways by which in creation we see one and the same end accomplished,—in view, too, of the facts on vegetal sensitiveness now ascertained—can we accept this as

* "Animals, as a rule, do no more than follow their natural instincts." Rev. G. Henslow, "Theory of Evolution of Living Beings."

† Milton most happily represents his devils discussing on free-will.

‡ "It Lies not in our Power to Love or Hate."—Marlowe.

conclusive evidence? A Society for the Emancipation of Vegetables should be formed at once and begin soliciting subscriptions. Such a movement would not be more unreasonable than certain other phases of modern British humanitarianism.

It is a great mistake to suppose that herbivorous animals are necessarily milder than the carnivora. The contrary is often the case. The flesh-eater attacks and kills for food. The grass-eater, *e.g* the Cape Buffalo, and even the domestic bull indulges in wanton outrages and "unprovoked assaults." His tendency to these peculiarly English offences is, perhaps, the reason why he has been, under the name of John Bull, chosen as the type of the nation.

The true question is, Does a brute, like man, ever violate "the laws of its own nature?" If it is found incapable of departing, whether to the right hand or the left, from one fixed line, we must then pronounce it, according to the commonly received notion, alike incapable of vice and of virtue, void indeed of moral life, in as far as this is deemed to be dependent upon choice.* But if it can deviate more or less from the norm of its existence, and especially if by such transgression it entails suffering upon itself and others, we are then, we submit, warranted in regarding its actions as morally good or evil,—good in as far as it conforms to the laws of its being; evil when it goes astray.

We may then judge it, just as man judges his own actions and those of his fellows; the full likeness of the cases justifying us in drawing like conclusions. It will be admitted that "brutes" have wills of their own which vary in intensity among individuals of any given species in the same manner as in man, if not to the same extent. Among domestic animals there are some, which, in spite of kicks and cuffs and general maltreatment, persevere in their own way. Such creatures, man taking, as usual, himself for the law of the universe, pronounces "vicious." There are others, again, which, under all circumstances, unhesitatingly submit their will to his, and these he praises.

The same method of judging, by the way, is applied to dependents and children. A child deficient in vital power implicitly obeys his parents and "betters" from want of energy to dispute their commands. He is, accordingly, held up to general admiration; his early death is pronounced a "mysterious dispensation of Providence," and his virtues

* If there were no evil, would there be also no good? If all matter were absolutely transparent and incapable of throwing a shadow, would light cease to exist?

and precocity are duly chronicled in a tract. On the contrary, the healthy and vigorous child, full of life and action, is apt to rebel against authority. It is, therefore, set down as a tiny incarnation of evil, and if it finds its way at all into a pretty story-book, is made to serve as an awful warning for the rising generation. There is wonderful virtue in listlessness, and in impotence lies an inconceivable amount of purity. Perhaps if we take the latter term in its modern cant sense the two may be regarded as practically synonymous.

The existence of a will, capable of acting at times in defiance of circumstances, is as clearly manifest in the horse, the ass, and the pig as in man himself, though in the three former it is little appreciated. Strange that what in animals is branded as stupidity should in man be deemed almost divine.

Were brutes devoid of freedom, unable to choose between two lines of conduct, we should find them in all cases simply obedient to their propensities, and intent only upon immediate gratification without any regard to ulterior consequences. Were such the case, for man to train them would be an impossibility. Yet we know that dogs, cats, hawks, &c., are trained to conduct quite different from their natural inclinations. A cat, though one of the most self-willed of animals, can be taught to abstain from molesting chickens, pigeons, and cage-birds, or from stealing, scratching furniture, &c. A dog can be brought to point to a covey of partridges instead of obeying his natural impulse to rush forward and endeavour to seize them. The following case is very significant:—" A fine terrier in the possession of a surgeon at Whitehaven, about three weeks ago exhibited its sagacity in a rather amusing manner. It came into the kitchen and began plucking the servant by the gown, and in spite of repeated rebuffs it perseveringly continued in its purpose. The mistress of the house, hearing the noise, came down to inquire the cause, when the animal treated her in a similar manner. Being struck with the concern evinced by the creature she quietly followed it upstairs into a bedroom whither it led her; there it commenced barking, looking under the bed and then up in her face. Upon examination a cat was discovered there quietly demolishing a beef-steak, which it had feloniously obtained. The most curious feature is that the cat had been introduced into the house only a short time before, and that bitter enmity prevailed between her and her canine companion."*

* Zoologist, p. 2131.

This is a capital case. "Instinct" might undeniably have led the terrier to attack the cat and attempt to deprive her of her booty. But we find this natural impulse here completely restrained for the attainment of a definite end. The terrier must have drawn the conclusion that his enemy, if detected in theft, would probably suffer severe punishment, —perhaps even death—and he therefore laid an information against her, calculating thus to get rid of her without compromising himself. This incident plainly proves that brutes are capable of self-control—that they do not always blindly and necessarily follow their physical appetites, but can, like man, forego a present indulgence for what appears to them a greater good in prospect. It is as clear a case of self-determination,—of appetite and passion governed by the will,—as any which human biography can show.

It will possibly be objected that we give no instance of self-control except in species which have been brought under human influence. The reply is obvious; if a free will or a power of self-determination has been created in such animals by man's intervention, its presence or absence is obviously a matter of small moment and quite inadequate to establish a "great gulf" between man and "brute." But if the will has *not* been thus created, it is probable, or rather certain, that were man better acquainted with the habits of wild animals he would find in their conduct also cases of self-control.

It will further be objected that in the vast majority of cases, animals merely act in accordance with the dictates of their ruling propensities. We grant this, and we ask whether this does not hold good to an almost equal extent with man? Analyse the actions of N'Kygntzgm, the blue-nosed baboon, and you will admittedly find little save the manifestations of ruling propensity. Sift in like manner the conduct of John Nokes, collier, of Hanley, and you will come to the same result. Surely, then, we can regard it as proven that in the matter of self-determination, in the supremacy of will over propensity, there is no difference of kind between man and brute.

Were animals really what vulgar human opinion supposes —did they simply and in all cases follow their propensities in the machine-like manner so commonly attributed to them, it is difficult to see how any individuality of character could exist. All the members of one species would have the same mental abilities and the same dispositions. But this is precisely what is not the case. Among a dozen animals of the same species and even of the same breed differences of character are found as decided as occur among a similar number of men. Any breeder or trainer of horses, cattle, dogs, or poultry, would greet with laughter,—loud, if not Olympian,—the theorist who should assert that these animals display anything like identity of disposition. There are the obstinate and the docile, the timid and bold, the open and the treacherous, the placable and the revengeful. In fact, to find two horses or two dogs precisely alike in every point of character that man can distinguish would be as difficult as to find two human beings similarly identical. How much greater, then, would be the range of character visible if we could see them with the eyes of their own species!

Perhaps the usual evasion may be attempted that such various development of temper and disposition is to be found among tame animals alone. The objection is baseless. Capture a number of wild elephants, hawks, ravens, parrots, and try to tame them. You will find still the same variety as you would among animals born in a state of tameness. The differences are found by man, not created.

We will next endeavour to show—what indeed, follows as a corollary from the foregoing considerations—that animals are capable of vice, hoping that this circumstance may lead man to recognise them as brothers.

To eat more than hunger demands merely for the sake of the sensuous enjoyment thus obtainable, has been always, in man, branded as a serious vice, and has indeed been classed among the "seven deadly sins" of mediæval tradition.* This transgression has been found to impair human health, and to blunt mental action. How is it in this respect with brutes? Do they never eat more than they can digest and assimilate? Do they never suffer consequently in their health? Most assuredly. Cows have been known to gorge themselves with clover till they have died from repletion. Ducks often suffer from their own greediness. Similar cases of gluttony are, of course, more rare among wild animals, who neither find food in such abundance nor are so undisturbed in its enjoyment. Yet even they, in homely phrase, at times eat more than does them good. Here, then, we see that brutes have a certain liberty of action. They can be either temperate or gluttonous. In the former case they preserve their health; in the latter case they bring upon

* It is a remarkable fact that the discharge of any voluntary physical function to which *no* pleasure is attached was never pronounced a vice, even if exercised in excess. But those whose importance the Creator has indicated by rendering them pleasant were branded as sinful not merely when discharged in excess, but even when kept within the bounds of moderation,—and this in the exact ratio of their pleasurableness.

themselves disease or perhaps death. If the gluttonous animal gives unchecked play to its propensities, does not the temperate animal, like the temperate man resist temptation and exercise a certain amount of self-restraint? Is it not, for so doing, equally entitled to credit?

The Rev. G. Henslow, in his able and interesting work on the "Theory of Evolution of Living Things," makes some remarks which must here be taken into consideration if only for their cool *naïveté* of assumption. Says this author:—"In obeying those laws of self-preservation and propagation which have been impressed upon it, it is extremely probable that wild animals eat and drink not for the purpose of eating and drinking, but to maintain bodily life only. The laws of propagation are obeyed, but union is probably not resorted to for mere union's sake. Animals show no signs of distinguishing the object from the means. Man alone can see that eating is pleasant, and so often eats for the mere sake of eating, and similarly of other pleasures."

If animals eat only to maintain life it is somewhat strange that they are so extremely nice in the quality of their food. Birds and wasps, in their visits to our gardens, select fruit with a care surpassing that of any human epicure. They attack only the finest pears, peaches, &c., and of these they eat only the sunny side. Mr. Henslow confounds the *result* of an action with the *motive*. Man, at least in his adult state, and possibly the higher animals, know that the result of eating is the prolongation of life, and that abstinence would be ultimately fatal. But neither man nor animal, as a rule, eats from any other motive than to avoid the pains of hunger and to secure the pleasures of eating. We will even venture to say that the less ultimate results are held in view in the gratification of any physical appetite the more perfectly those very results are obtained. As regards the "laws of propagation." We can bring forward facts proving that among animals union *is* resorted to for mere union's sake. Into what absurdities men are led by their notions of what is "extremely probable!"

It may be urged that the moderation of an animal may spring, not from its greater power of self-control, but from its feebler appetites. We cannot deny that this is a possible explanation. But it may, with equal right, be extended to man also. Who knows that the temptation which the saint resists is really as strong as that to which the sinner succumbs? Are we not, in cases of reformation of character, frequently left in painful doubt whether the "convertite" is forsaking his vices or his vices forsaking him?

Alcoholic excitement is not one of the prevailing vices of brutes, from the satisfactory reason that they are under the operation of a natural Maine Law.* Two cases of drunkenness, in a cow and a sow respectively, are on record. Both these occurred in Scotland. It is only fair to surmise that the offending animals, like some of their two-legged compatriots thought fit, in the words of Hudibras, to :—

> "Compound for sins they were inclined to,
> By damning those they had no mind to."

A later instance of undeniably "beastly" drunkenness is given in the "Greenock Advertiser." Two rats got "that fou" in the shop of a spirit merchant in the town by dint of consuming the dribblings from a barrel of strong ale, and were killed before they could stagger off to their holes.

It is generally known that most of the Quadrumana, when thrown among human society, learn very readily to like a glass of strong liquor,—a fact which should go far to establish their title to a place on the right side of the "gulf." It is no less certain that some of the less reputable monkeys are captured by leaving near their haunts vessels filled with a kind of beer. They come, drink and become drunken, and in that state commit the very venial error of mistaking the negro, who comes to lead them into captivity, for one of their own species.

From alcoholism we are naturally led to the love of the narcotics, as tobacco, opium, Indian hemp, coca, and the like. That man has a widely-spread craving for these so-called "keys of paradise," has been sufficiently shown. But apes, also, in captivity have been known to indulge in the "weed" with evident relish. Imitation, say you? Probably enough; but has imitation no part in the spread of these minor vices among mankind? Nine smokers out of ten first take to the pipe or the cigar from the tendency—common alike to man and brute—of doing what others do. A love for tobacco in the solid form, also, is not peculiar to man. At a tavern in Bradford there flourished some years ago a goat, whose exploits in tobacco-chewing were not unknown to fame throughout the "land of woollen." A frequenter of the house occasionally won money from strangers, by betting that "himself and another" would eat a pound of tobacco in ten minutes. If the wager was accepted he would order in a pound of ordinary shag tobacco,

* This is not literally true. Alcohol, in small doses, is being detected in natural productions, in which man has had no part.

put a modest pinch in his own mouth and call in the goat, who soon disposed of the remainder. It is not on record that Billy suffered in his health or displayed any marks of penitence after these performances.

Turn we next to dishonesty in the widest sense of the word,—the vice most in favour in this virtuous age. The lower animals labour under the disadvantage of having no stock-exchange and of not using bills of exchange. But they indulge to the best of their means and opportunities in deceit, affectation, and hypocrisy.

The Rev. J. G. Wood, in his recent interesting work "Man and Beast," gives an instance of a terrier who, finding that a companion had anticipated him in getting possession of a snug seat, suddenly pricked up his ears, dashed into a corner of the room, and begun scratching and barking furiously. The other dog, believing that this commotion indicated the presence of a rat, hastened to the spot, when the terrier at once ran back and secured the coveted cushion. Mr. Wood—we quote from memory—very justly brings forward this incident as a proof of intelligence in dogs. But it is equally a proof of dishonesty. It is a clear case of obtaining something desirable on false pretences.

Hypocrisy is almost as prominent among the Felidæ as among men. If a delicate morsel is thrown to a cat, she will, except very hungry, assume an air of utter unconcern. But all the while she knows its position to a hair's breadth, and when no one appears to be looking, it will be at once seized and swallowed. Or if a bowl of cream is standing in an accessible position, pussy appears lost in the brownest of studies. Her eyes are closed, or if open are directed anywhere save towards the tempting object; yet all the time she is watching her opportunity. Whether in cats or in man this failing is invariably the "homage which vice pays to virtue" we leave an open question.

The following instance of deceit and hypocrisy in a terrier is given by Mr. G. J. Romanes in "Nature" (May 27th, 1875, p. 66).

"He used to be very fond of catching flies upon the window panes, and if ridiculed when unsuccessful was very much annoyed. On one occasion, in order to see what he would do, I purposely laughed immoderately every time he failed. It so happened that he did so several times in succession—partly, I believe, in consequence of my laughing—and eventually he became so distressed that he positively *pretended* to catch the fly, going through all the appropriate actions with his lips and tongue, and afterwards rubbing the ground with his neck, as if to kill the victim: he then looked up at me with a triumphant air of success. So well was the whole process simulated that I should have been quite deceived had I not seen that the fly was still upon the window. Accordingly I drew his attention to this fact, as well as to the absence of anything upon the floor, and when he saw that his hypocrisy had been detected, he slunk away under some furniture, evidently much ashamed of himself."

This last point is most significant, fully overturning the vulgar notion of the absence of moral life in brutes, and of their total want of conscience.

That animals steal is a familiar expression. But we must here distinguish two different cases. We speak of hares stealing our corn, and of blackbirds plundering our cherries; but in neither case have we any reason to conclude that the offenders can distinguish between the crops in cultivated lands, and the spontaneous produce of woods and wastes. But not a few species both of birds, quadrupeds, and insects evidently recognise the idea of property. This is proved by the fact that they display far greater courage and pertinacity in defence of their nests, their haunts, and their accumulations, than under other circumstances. A dog, that when trespassing is put to flight by a gesture or a shout, becomes a formidable opponent in his own yard. If, then, such animals know what property is, and yet at times make free with it, we may justly pronounce them conscious thieves. Rooks are apt to purloin sticks from each other's nests; but if the offender is detected and cuffed by the rightful owner, conscience makes a coward of him. and he merely defends himself by flight. More than this—rooks have some rudiments of criminal law. Inveterate thieves are sometimes banished from the rookery, severely beaten, or even killed outright.* But law pre-supposes the notions of right and wrong, and could never, therefore, have arisen among beings incapable of making this distinction.

As another vice, we may take quarrelsomeness—a term which we need surely not define. This attribute is highly conspicuous in the human species, nowhere perhaps more strikingly than in that part of the English nation who inhabit the borders of Yorkshire and Lancashire. But certain dogs show the very same disposition, and without the smallest provocation take every opportunity of attacking horses, cows, sheep, and human beings. There is a well-

* A most interesting account of the habits of rooks was given by Mr. Ashley, of Sheffield, in a lecture delivered before the Mechanics' Institute of that town about twenty years ago.

authenticated instance of a terrier who, in picking a quarrel contrived, as skilfully as if trained in the Kanzlei of Prince Bismarck to place himself technically in the right. He would time his movements so that some passenger should stumble over him, and would then fasten on the calf of his leg. With a most statesmanlike aptitude he selected the aged, the infirm, and the ill-dressed, as the objects of his cunningly-planned attacks. Lord Lytton tells us that the dog is a gentlemanly animal!

Closely connected with quarrelsomeness is the most fiendish of all man's failings—overlooked, as it is, by world-betterers and vice-suppressers—his disposition to give pain, bodily or mental, for mere amusement. There are few human beings, of the male sex at least, who do not delight in tormenting other creatures, whether of their own or of some different species.* Yet even this kind of malignity is not unshared by man's poor relations. Fall among wolves, and they will kill you for the straightforward purpose of eating you. Fall among blue-nosed baboons and they will torment you to death "just for the fun of the thing." Could a Red Indian, or even a normal English school-boy, greatly improve upon this?

With the exception of a few genuine—not professional—philanthropists, man is remarkable for persecuting such of his own species as are unfortunate. This diabolical propensity shows itself in a variety of forms. "Hit him again, he has no friends," is scarcely a parody on the avowed opinions of the less hypocritical of the species. Those who lay claim to higher culture express their sorrow for the calamities of a neighbour by eschewing his society, or perhaps even by asking him whether he does not recognise in his sufferings a well-merited Divine chastisement?

Odious as is this trait of human character, man has no monopoly thereof. The wounded wolf is at once devoured by his comrades.

Cattle, both wild and tame, have been observed to gore and trample to death a sick or lame member of the herd. A rook, accidentally entangled in the twigs of a tree, is pecked and buffetted by its fellow-citizens. This, of course, has been pronounced "instinctive." Animals, we are gravely told, put an end to sufferings which they are powerless to alleviate. They do not wish that the herd should be encumbered with a sickly or wounded member. Taking these explanations for what they are worth, we still ask

* When an Englishman talks about amusement, it may be inferred as a general rule that he means killing something.

whether man's ill-treatment of his unfortunate fellows is not the ultimate transformation of the very same instinct.

But, further, the alleged instinct is not common to all gregarious animals. Monkeys and baboons cherish and defend the young, the helpless, and the wounded of their own species. Ants will take great pains to rescue a member of their community who is in distress.

Looking in a different direction, we must acknowledge that among viviparous animals and birds, the females are, as a general rule, no less careful of their young than are human mothers. In thus acting they are undoubtedly obeying one of the "laws" of their nature. But they can also transgress such law, just as we occasionally find a woman who will neglect, ill-treat, or even kill her child. So is it with female brutes. Sometimes, though rarely, they will abandon or destroy their young. This is a fact well known to the breeders of tame animals. The seller of a mare, a cow, or a sow, is often asked by an intending purchaser, "Is she a good mother?" It must be remarked that neglect of family is by no means the invariable result of want of food, or of danger and annoyance. Birds will, as is well known, sometimes forsake their nests from fear. But a hen has been known to leave her chickens to the mercy of accidents without any conceivable motive save caprice, or the want of ordinary natural affection. Cats, though ordinarily very affectionate mothers, and sows, sometimes devour their young. Here, therefore, we find, again, that the lower animals are not bound down by absolute necessity to one unvarying line of conduct. Like man, they have the power to deviate from what is for them natural, normal, or right. Occasionally they make use of such power. What may be the causes of, or the motives for, such transgression, is not here the question. Enough for us that it exists.

We now come to a part of the subject which, though essential to our argument, we cannot enter into at any length. Do brutes invariably obey the "law of their being" as regards the mutual relations of the sexes? Far from it. The nearer brutes approach to man, the more they are inclined to sin against what, in modern cantology, is exclusively styled "morality." With animals which pair conjugal fidelity is, indeed, more general than with mankind. A petty negro chief laughed at the notion of keeping to one wife, "like the monkeys." Still it is far from being universal, and nowhere are exceptions more frequently found than among pigeons, which with a rare depth of wicked satire, have been selected as types of matrimonial faith.

The existence of hybrids shows a departure from what nature should enjoin. Such beings have been produced respectively, not alone between the horse and the ass, but between the horse and the quagga, the horse and the zebra, the ass and the zebra, the lion and the tiger, the hare and the rabbit (leporides), and between a great variety of birds of the poultry, pheasant, grouse, duck, and finch groups. To the dismay and indignation of certain theorists some of these hybrids are capable of reproduction.

It has been objected that these instances occur only through human intervention. This is by no means the fact. Hybrids between distinct species of grouse have been met with in a wild state.

Instances of hybridism are likewise said to have occurred between animals much more widely remote in their respective natures. Such cases are doubtful, and are certainly not essential to our argument. But intercourse not unfrequently takes place between animals of different species where no offspring has been positively proved to result.

Many more instances of brute frailty might be given were it needful or desirable.

It has been asserted that "mere brutes" never commit suicide. This is a wanton, it might be said an impudent, assumption. If a negro, sold into slavery, refuses food and starves himself to death, as sometimes happened in the palmy days of the "black ivory-trade," men say that he has committed suicide rather than live in bondage; but if an animal, bird, or reptile, taken away from its native haunts and shut up in a cage, persistently refuses food and dies in consequence, why should not the same name be applied to conduct precisely similar? Yet cases of this kind, in which the love of liberty and independence asserts itself in flat defiance of the strongest of all instincts are by no means rare. There is great difficulty in inducing some animals to eat in captivity, even if supplied with the very kind of food which they select when at large. As an example, we may mention the common viper, which generally starves itself to death in captivity, regardless of the offer of the choicest mice. But there are many instances among domestic animals, proving that life-weariness and the determination to end miseries in a sudden manner are not confined to the human race.

"*Suicide by a Dog.*—A day or two since a fine dog, belonging to Mr. George Hone, of Frindsbury, near Rochester, committed a deliberate act of suicide by drowning in the Medway, at Upnor, Chatham. The dog had been suspected of having given indications of approaching hydrophobia, and was accordingly shunned and kept as much as possible from the house. This treatment appeared to cause him much annoyance, and for some days he was observed to be moody and morose. On Thursday morning he proceeded to an intimate acquaintance of his master's at Upnor, on reaching the residence of whom, he set up a piteous cry on finding that he could not obtain admittance. After waiting at the house some little time, he was seen to go towards the river close by, when he deliberately walked down the bank, and after turning round and giving a kind of farewell howl, walked into the stream, where he kept his head under water, and in a minute or two rolled over dead. This extraordinary act of suicide was witnessed by several persons. The manner of the death proved pretty clearly that the animal was not suffering from hydrophobia."—*Daily News.*

"*Suicide of a Horse.*—A correspondent writes:—'A few nights ago a poor creature, worn to skin and bone, put an end to his existence in a very extraordinary manner. His pedigree is unknown, as he was quite a stranger. A very worthy gentleman here met him in a public market, and thinking he could find employment for him, put him to work, but it was soon discovered that work was not his forte; in fact, he would do anything save work and go errands. His great delight was to roam about the fields and do mischief. People passing him used to ejaculate, "Ugh, you ugly brute," when they saw the scowl which was continually on his face. His master tried to win him by kindness. The kindness was lost upon him. He next tried the whip, then the cudgel, but all in vain. Work he would not. And as a last resort the punishment of Nebuchadnezzar of old was tried. He was turned out, "but house or hauld," to eat grass with the oxen. With hungry belly and broken heart he wended his lonely way down by the Moor's Shore, passed Luckyscaup, turned the Moor's Point, and still held on his lonely way, regardless of the wondering gaze of the Pool fishermen. At length he arrived at a point opposite the wreck of the *Dalhousie,* where he stood still; and while the curiosity of the fishermen was wound to the highest pitch as to what was to follow, he, neighing loudly and tossing his old tail, rushed madly into the briny deep, got beyond his depth, held his head under the water, and soon ceased to be. The fishermen conveyed the true, although strange and startling tidings, to the respected owner, that his horse had committed suicide.'"—*Dundee Advertiser.*

There are several other authenticated cases on record where dogs have committed suicide by drowning. It is important, as showing intention, that dogs are perfectly aware of the results of prolonged immersion in water, as evinced by their so frequently rescuing children when in danger of drowning. Were dead brutes honoured with a searching investigation, we might perhaps find such instances far more frequent than we suspect. They have, however, scantier facilities for self-murder than man, and possibly slighter temptations, as being doubtless, upon the whole, less miserable.

The various actions above mentioned are all departures from the normal or natural conduct of the species concerned, and of course lead us again to the conclusion that brutes *can* do wrong, and if wrong, that they are consequently able also to do right.

Perhaps it may be argued by the captious that though gluttony, neglect of offspring, suicide, and the like, are wrong in themselves, and are hurtful to the offending animal and its species, yet that brutes have no conscience, and neither feel any satisfaction in "obeying the laws" of their nature nor any remorse upon transgression. To this we may in the first place reply with a *tu quoque*—a retort for once satisfactory, as it withdraws the pretended distinction. Man does not appear to have any inborn and infallible knowledge of right and wrong. His vaunted conscience, when it is more than a mere figure of speech, is a creature of conventions and traditions. There is no vice, no crime even, how horrible soever, which at some time or in some part of the world man has not practised without a shadow of self-reproach. He has suffered, indeed, from his errors, but no more than the brutes does he, generally speaking, trace his sufferings to their true causes. Sir J. Lubbock states in his "Origin of Civilization" that after inspecting nearly all existing records of savage life, he was unable to find any case of a savage having evinced remorse after the commission of any crime.

But, on the other hand, does man really *know* that brutes are void of all trace of conscience—that they feel no joy when they have acted aright, and no sorrow when they have done amiss? He has no proof—merely wanton assumption. Facts prove that certain animals do feel shame, sorrow, or remorse, when they have departed from what to them is the standard of right; and what more can reasonably or fairly be demanded?

We have thus, we submit, established that the lower animals have a moral life, that they can do right or do wrong, and that like man they avail themselves of their power to do the latter. Surely henceforth a fellow feeling ought to make him wondrous kind to them all. Community in vice, or even in peccadillos, has always been a wonderful leveller of distinctions.

II. THE LONGEVITY OF BRAIN-WORKERS.*

By George M. Beard, A.M., M.D., New York.

THOMAS Hughes, in his life of "Alfred the Great," makes a statement that "the world's hardest workers and noblest benefactors have rarely been long-lived." That any intelligent writer of the present day, and especially a writer who, like Mr. Hughes, is a thoughtful student of mental hygiene, should make a statement so absolutely untrue, shows how hard it is to kill an old superstition.

The remark is based on the mischievous theory, which—against the clearest evidence of general observation—has been held for centuries, that the mind can be used only at the injurious expense of the body. This theory has been something more than a mere popular prejudice; it has been a professional dogma, and has inspired nearly all the writers on hygiene since medicine has been a science. On the basis of this theory, intellectual and promising youth have been dissuaded from entering brain-working professions; and thus, much of the choicest genius has been lost to the world; students in college have abandoned plans of life to which their tastes inclined, and gone to the farm or workshop; authors, scientists, and investigators in the several professions have thrown away the accumulated experience of the best half of life, and retired to pursuits as uncongenial as they were profitless. The superstition, for it hardly deserves to be called a theory, has therefore wrought immense evil specifically by depriving the world of the services of some of its best endowed natures, and generally by fostering a habit of accepting statement for demonstration.

* Communicated by the Author. Read before The American Public Health Association, 1874.

Between 1864 and 1866, while preparing a thesis for graduation, I obtained statistics on the general subject of the relation of occupation to health and longevity that convinced me of the error of the accepted teachings in regard to the effect of mental labour. These statistics, which were derived from the registration reports of this country and of England, and from a study of the lives of many prominent brain-workers, were incorporated in an essay on the subject that was delivered before an Association of Army and Navy Surgeons in New Orleans in 1863, and afterwards published in the "Hours at Home" Magazine. The views I then advocated, and which I enforced by statistical evidence, were:—

1st. That the brain-working classes—clergymen, lawyers, physicians, merchants, scientists, and men of letters,—lived very much longer than the muscle-working classes.

2nd. That those who followed occupations that called both muscle and brain into exercise, were longer lived than those who lived in occupations that were purely manual.

3rd. That the greatest and hardest brain-workers of history have lived longer on the average than brain-workers of ordinary ability and industry.

4th. That clergymen were longer lived than any other great class of brain-workers.

5th. That longevity increased very greatly with the advance of civilisation; and that this increase was too marked to be explained merely by improved sanitary knowledge.

6th. That although nervous diseases increased with the increase of culture, and although the unequal and excessive excitements and anxieties attendant on mental occupations of a high civilisation were so far both prejudicial to health and longevity, yet these incidental evils were more than counterbalanced by the fact that fatal inflammatory diseases have diminished in frequency and violence in proportion as nervous diseases have increased; and also that brain-work is, *per se*, healthful and conducive to longevity.

Many of these views have since received various and powerful confirmation, and by a number of independent observers.* The statistics on this subject I have endeavoured

* Those who desire to obtain the detailed facts on this subject are referred to my Essay in "Hours at Home" (Oct., 1867); to my series of papers on "Hygiene for Students," in the "College Courant" (1869); to my "Home Physician," p. 380; to Dr. Derby's "Registration Reports of Massachusetts" and Farr's "Registration Reports of England" (Supplement to 22nd); to Dr. Edward Jarvis's Papers on the "Increase of Human Life," in "Atlantic Monthly" (Oct., Nov., and Dec., 1869); to Dr. Elam's "Physician's Problems;" Hon. B. G. Northrup's "Report of the Connecticut Board of Education" (1869, pp. 61-74); and to the "Reports of the Life Insurance Company for Clergymen" (Bible House, N. Y.).

to use without abusing them; to draw from them only those lessons that they are really capable of teaching. Among those classes who live mainly by routine and muscular toil (mechanics, artisans, labourers, &c.) change of occupation is the rule rather than the exception, especially in this country; and any statistics of mortality derived from the Registration Reports, are, so far as these classes are concerned, of but little value in the study of the relative effects of the different occupations on health and longevity. Another important complication arises from the fact that certain occupations, as clerkships, positions in factories, teaching, &c., are followed almost exclusively by the young and middle-aged; while other callings, as judgeships, are filled only by those in middle and advanced life. Another difficulty arises from the fact that some important occupations, as journalism, for example, are adopted only by a limited number; and the number in them who annually die is too small to afford any basis for comparison. But this generalisation is, I am persuaded, admissible, that the greater majority of those who die in any one of the three great professions—law, theology, and medicine—have, all their lives, from twenty-one upwards, followed that profession in which they died. The converse generalisation, that the great majority of those who die in the muscle-working avocations have all their lives followed some kind of muscle-working employment, however frequently they may have changed from one to another at different periods, is also true. Very few who once fairly enter theology, medicine, or law ever permanently change to a purely physical calling; and, on the other hand, the number of those who begin life as farmers, labourers, and mechanics, and end it as lawyers, physicians, or clergymen, is quite limited, even in the United States, where every man has a better chance to follow the bent of his genius than in any other country.

A comparison, therefore, of the longevity of the professional and of the muscle-working classes, as derived from Registration Reports, such as I have made, is quite justifiable. The value of this comparison would be vitiated if it could be proved that those who enter the professions are originally healthier and stronger, and come from better stock, than those who enter physical avocations; but in this country the practice has been to allow the more delicate members of a family to enter a profession, whilst the tough and hardy work on the farm or learn a trade. Here, as in Europe, there is growing up a distinctively intellectual class who live solely by brain-work; it is, however, not from this class alone, but from the farming, mercantile, and artisan class, that the ranks of the professions are filled.

Great Longevity of Great Men.—I have ascertained the longevity of five hundred of the greatest men in history. The list I prepared includes a large proportion of the most eminent names in all the departments of thought and activity.

It would be difficult to find more than two or three hundred illustrious poets, philosophers, authors, scientists, lawyers, statesmen, generals, physicians, inventors, musicians,, actors, orators, or philanthropists, of world-wide and immortal fame, and whose lives are known in sufficient detail, that are not represented in the list. My list was prepared, not for the average longevity, but in order to determine at what time of life men do their best work. It was, therefore, prepared with absolute impartiality; and includes, of course, those who, like Byron, Raphael, Pascal, Mozart, Keats, &c., died comparatively young. Now the average age of those I have mentioned I found to be 64·20.

The average age at death at the present time of all classes of those who live over twenty years is *about fifty*. Therefore, the greatest men of the world have lived longer, on the average, than men of ordinary ability in the different occupations by fourteen years; six years longer than physicians and lawyers; nineteen or twenty years longer than mechanics and day labourers; from two to three years longer than farmers; and a fraction of a year longer than clergymen, who are the longest-lived class in our modern society. The value of this comparison is enforced by the consideration that longevity has increased with the progress of civilisation, while the list I prepared represents every age of recorded history. A few years since I arranged a select list of one hundred names, comprising the most eminent personages, and found that the average longevity was *over seventy years*. Such an investigation any one can pursue; and I am sure that any chronology, comprising from one to five hundred of the most eminent personages in history, at any cycle, will furnish an average longevity of from sixty-four to seventy years. Madden, in his very interesting work, "The Infirmities of Genius," gives a list of two hundred and forty illustrious names, with their ages at death. The average I found to be sixty-six and a fraction.

In view of these facts, it may be regarded as established that "the world's hardest workers and noblest benefactors" have usually been very long lived.

Causes of the Great Longevity of Brain-Workers.

The full explanation of the superior longevity of the brain-working classes would require a treatise on the science of sociology, and particularly of the relation of civilisation to health. The leading factors, accounting for the long life of those who live by brain-labour, are as follows :—

1. *The inherent and essential healthfulness of brain-work.* To work is to grow; and growth, except it be forced, is always healthful. It is as much the function of the brain to cerebrate as of the stomach to digest; and cerebration, like digestion, is normal, physiological, and healthful. In all organisations of force the exercise of force develops more force; work evolves strength for work. A plant that is suffered to bud and bloom is more sturdy and longer lived than the plant that is kept from the light or trimmed of all its blossoms, By thinking, we gain the power to think; functional activity, within limits, tends to vigour and the self-preservation of an organ and of the body to which the organ belongs. The world has been taught that the brain can be developed only at the expense of the other organs of the body; granting that brain-work strengthens the brain itself, the rest of the body is impoverished thereby—hence disease, and early death. But recent investigations in cerebro-physiology seem to indicate that the centres of thought in the anterior region of the brain are also the centres of muscular motion; and hence it may perhaps be inferred that to develop the brain may be one method of developing the muscles.* It is certain that the brain-working classes are, on the average, well developed muscularly; and in size and weight are superior to the purely muscle-working classes.

2. *Brain-workers have less worry, and more comfort and happiness, than muscle-workers.* Worry is the converse of work; the one develops force, and the other checks its development, and wastes what already exists. Work is growth; worry is interference with growth. Worry is to work what the chafing of a plant against the walls of a

* I here refer to the experiments of Hitzig, of Berlin, in the electrical irritation of the brains of living animals. These experiments have been confirmed by a variety of experiments undertaken by Ferrier, of London, by myself, and other observers. I use the word centre, in an experimental sense; and the above theory of the relation and definition of the thought centres, and muscle centres, is merely a provisional suggestion. (See "Archives of Electrology and Neurology," May, 1874, for a record of my own experiments, with remarks, and also a general *resumé* of facts).

greenhouse is to limitless expansion in the free air. In the successful brain-worker worry is transferred into work; in the muscle-worker, work too often degrades into worry. Brain-work is the highest of all antidotes to worry; and the brain-working classes are therefore less distressed about many things, less apprehensive of indefinite evil, and less disposed to magnify minute trials, than those who live by the labour of the hands. To the happy brain-worker, life is a long vacation; while the muscle-worker often finds no joy in his daily toil, and very little in the intervals. Scientists, physicians, lawyers, clergymen, orators, statesmen, literati, and merchants, when successful, are happy in their work, without reference to the reward, and continue to work in their special callings long after the necessity has ceased. Where is the hod carrier that finds joy in going up and down a ladder; and, from the foundation of the globe until now, how many have been known to persist in ditch-digging, or sewer-laying, or in any mechanical or manual calling whatsoever, after the attainment of independence? Good fortune gives good health. Nearly all the money of the world is in the hands of brain-workers; to many, in moderate amounts, it is essential to life, and in large or comfortable amount it favours long life. Longevity is the daughter of luxury. Of the many elements that make up happiness, mental organisation, physical health, fancy, friends,* and money—the last is, for the average man, greater than any other, except the first. Loss of money costs more lives than the loss of friends, for it is easier to find a friend than a fortune. Almost all muscle-workers are born, live, and die poor. To live on the slippery path that lies between extreme poverty on one side, and the gulf of starvation on the other; to take continual thought of to-morrow, without any good result of such thought; to feel each anxious hour that the dreary treadmill by which we secure the means of sustenance for a hungry household may, without warning, be closed by any number of forces, over which one has no control; to double and triple all the horrors of want and pain, by anticipation and rumination,—such is the life of the muscle-working classes of modern civilised society; and when we add to this the cankering annoyance that arises from the envying of the fortunate brain-worker who lives in ease before his eyes, we

* I do not here refer to accumulated wealth exclusively, but to income or sufficient amount to purchase comforts and luxury. Many persons (and notably successful professional men), live out their days in comfort and luxury, although they never succeed in accumulating fortunes; to them, their reputation is wealth and capital.

marvel not that he dies young, but rather that he lives at all.*

3. *Brain-workers live under better sanitary conditions than muscle-workers.* They have better food and drink, warmer clothing, breathe purer air, and are less exposed to fatal accident and the poison of disease. None of the occupations are ideal; none fulfil all the laws of health; but the muscle-working callings are all more or less unhealthy; tradesmen, artisans, common labourers, and even farmers (who combine muscle with brain-work), all are forced to violate sanitary law every hour of their lives; not one out of ten have enough good food; many are driven by passion and hunger to excess in the worst forms of alcoholic liquors; for a large number sleep is a luxury of which they never have sufficient for real recuperation; healthful air is but rarely breathed by the labouring classes of any large city; exposure to weather, that brings on fatal inflammatory diseases; accidents that cripple or kill—in all these respects, the muscle-worker, as compared with the brain-worker, is at stupendous disadvantage.

4. *The nervous temperament, which usually predominates in brain-workers, is antagonistic to fatal, acute, inflammatory disease, and favourable to long life.* Comparative statistics have shown, that those in whom the nervous temperament prevails live longer than those in whom any one of the other temperaments prevail, and common observation confirms the statement. Nervous people, if not too feeble, may die every day. They live, but they do not die; they talk of death, and each day expect it, and yet they live. Many of the most annoying nervous diseases, especially of the functional, and some even of the structural varieties, do not rapidly destroy life, and are indeed consistent with great longevity. I have known a number of men and women who were nervous invalids for half a century or more, and died at an advanced age. It is one of the compensations of nervousness that it protects the system against those febrile and inflammatory diseases that are so rapidly fatal to the sanguine and the phlegmatic. The nervous man can expose himself to malaria, to cold and dampness, with less danger of disease, and with less danger of death if he should contract disease, than his tough and hardy brother. This was shown in the late war, when delicate, ensanguined youth, followed by the fears of friends, went forth to camp and battle, and

* Those who question the truth of the above picture, are referred to any of the recently published essays and treatises on the condition of the peasantry of England. Observations show, that in our own country, not only in large cities, but in all manufacturing towns, and even in farming districts, the labouring classes are as badly circumstanced as I have stated.

not only survived, but grew stout amid exposures that prostrated by thousands the lumbermen of Maine, and the sons of the plough and the anvil. In the conflict with fevers and inflammations, strength is often weakness, and weakness becomes strength—we are saved through debility. Still further, my studies have shown that, of distinctively nervous diseases, those which have the worst pathology and are the most hopeless, such as locomotor ataxia, progressive muscular atrophy, apoplexy with hemiplegia, and so on, are more common and more severe, and more fatal among the comparatively strong and tough than among the most delicate and finely organised.* Cancer, even, goes hardest with the hardy, and is most relievable in the nervous.

5. *Brain-workers can adapt their labour to their moods and hours and periods of greatest capacity for labour better than muscle-workers.* In nearly all intellectual employments there is large liberty; literary and professional men especially are so far masters of their time that they can select the hours and days for their most exacting and important work; and when from any cause indisposed to hard thinking, can rest and recreate, or limit themselves to mechanical details. Thus, there is less of the dreadful in their lives; they work when work is easy, when the desire and the power are in harmony; and, unlike their less fortunate brother in the mill or shop, or diggings, need not waste their force in urging themselves to work. Forced labour, against the grain of one's nature, is always as expensive as it is unsatisfactory; it tells on the health and on life. Even coarser natures have their moods, and the choicest spirits are governed by them; and they who worship their moods do most wisely, and those who are able to do so are the fortunate ones of the earth.

Again, brain-workers do their best work between the ages of twenty-five and forty-five; before that period they are preparing to work; after that period, work, however extensive it may be, becomes largely a matter of routine. Lawyers and physicians do much of their practice after forty; but to practice is easy, to learn is hard—and the learning is done before forty or forty-five. In all directions, the French motto holds true, "It is the first step that costs." Successful merchants lay the foundations of fortune in youth and middle life, to accumulate, and recreate, and take one's ease in old age; thus they make the most when

* In my paper on "Spinal Congestion and Locomotor Ataxia," in the "Philadelphia Medical Times" for January 24 and 31, 1874, I have discussed this point in some detail.

they are doing the least, and only become rich after they have ceased trying to be so.*

With muscle-workers there is but little accumulation, and only a limited increase of reward; and in old age, after their strength has begun to decline, they must, with increasing expense, work even harder than before.

To this should be added the consideration that manual employments cost as much force after they are learned as before; they can never, like many intellectual callings, become so far forth matters of routine as to require little effort. It is as hard to lay a stone wall after one has been laying it fifty years as during the first year. The range of muscular growth and development is narrow, compared with the range of mental growth; the day-labourer soon reaches the maximum of his strength. The literary or scientific worker goes on from strength to strength, until what at twenty-five was impossible, and at thirty difficult, at thirty-five becomes easy, and at forty a pastime; and besides he has the satisfaction that the work done so easily at thirty-five and forty is incomparably better than the work done with so much difficulty at twenty-five.

6. *Comparative longevity of the professions.* Inasmuch as professional men do not usually change their callings, but die in the special profession in which they have lived, the vital statistics, at least of lawyers, physicians, and clergymen, become of value in determining their comparative longevity. I found in my researches, made several years ago, that lawyers and physicians lived to be about fifty-seven or fifty-eight. The difference in the longevity of lawyers and physicians is but trifling. My observations in this respect have been variously confirmed by other statisticians.†

Longevity of the Precocious.

That precocity predicts short life, and is therefore a symptom greatly to be feared by parents, has, I believe, never

* The whole subject of "The Relation of Age to Work," I have discussed in my pamphlet on "Legal Responsibility in Old Age," to which I may refer those who are interested in the subject. What is there written is preliminary to an exhaustive treatise now in the course of preparation.

† "An investigation made by a Berlin physician into the facts and data relating to human longevity shows the average age of clergymen to be 65; of merchants, 62; clerks and farmers, 61; military men, 59; lawyers, 58; artists, 57; and medical men, 56. Statistics are given showing that medical men in England stand high in the scale of longevity. Thus, the united ages of 28 physicians who died there last year, amount to 2354 years, giving an average of more than 84 years to each. The youngest of the number was 80; the oldest, 93; two others were 92 and 89 respectively; three were 87; and four were 86 each; and there were also more than 50 who averaged from 74 to 75 years."

been questioned. In poetry and in science, the idea has been variously incorporated that early brilliancy is a sure indication of a feeble constitution and an early death. This view is apparently sustained by analogy, and by facts of observation. Plants that are soon to bloom are soon to fade; those which grow slowly live long and decline slowly. Observing these facts, we naturally adhere to the opinion that the same principle should hold good as regards men, but in making the analogy we forget that it loses its force, unless the objects implicated start in life with the same potential force and are surrounded by the same external conditions. It is probable that, of two individuals with precisely similar organisations and under similar circumstances, the one that develops earlier will be the first to die; but we are not born equally endowed and similarly circumstanced. Not only are men unlike in organisation, but they are very widely unlike; between the brain of Shakspeare and the brain of an idiot is a measureless gulf, and we may believe that difference of degrees may be found between the greatest and simply great men. We may believe that some are born with far more potential nervous force than others. They are millionaires in intellect as well as in money, who can afford to expend enormous means without becoming impoverished. An outlay of one hundred dollars may ruin the mechanic working for his daily wages, while the royal merchant may spend a thousand, and barely know it. There are those who can begin their life-work earlier, toil harder and longer, than the average, and yet attain a very great age. The average age of 500 illustrious men, including those who did not exhibit any special precocity, was about 64·20. Of these 500 individuals, among whom there were 25 women, 150 were decidedly precocious, and their average age was 66·50, or more than two years higher than that of the list of 500, that included the precocious and non-precocious. So far as I could ascertain, the instances of extraordinary longevity were as great among the precocious as among those who were not.* My investigations in this department fully confirm the remark of Wieland, that "an almost irresistible impulse to the art in which they are

* A contributor to the "Galaxy" for August (G. W. Winterburn) thus discourses concerning musical prodigies. Investigating the records of the past two centuries, he finds 213 recorded cases of acknowledged prodigies. None of them died before their 15th year, some attained the age of 103—and the average duration of life was 58—showing that with all their abnormal precocity, they exceeded the ordinary longevity by about 6 per cent. Those who died before the age of 21 were, without exception, musicians of the very highest order.

destined to excel manifests itself in future virtuosi—in poets, painters, &c., from their earliest youth."

Not only in poetry and painting, but also in philosophy, in science, and in invention—indeed, in every great department in which human nature has displayed itself—it is true, as Milton beautifully remarks, "Childhood shows the man, as morning shows the day."

Madder, in his "Infirmities of Genius," says that "Johnson is indeed of the opinion that the early years of distinguished men, when minutely traced, furnish evidence of the same vigour or originality of mind by which they are celebrated in after-life."

The more closely I study biography, the more strongly I become convinced that the number of really illustrious geniuses who did not give early manifestations of their genius is very limited. I do not forget that some of the currently-reported exceptions are very striking. Thus we are told that Chalmers at school was stupid and mischievous; that Adam Clarke, as a boy, could do nothing but roll huge stones about; that of Sir Walter Scott, his teacher, Professor Dalzell, frankly said—"Dunce he was and dunce he would remain;" that Burns, though a good athlete, showed, in his boyhood, no unusual gifts; that Goldsmith was "a plant that flowered late;" that John Howard, and Napoleon, and Wellington were, to say the least, but little remarkable at school; and that the father of Isaac Barrow is reported to have said that "if it pleased God to take away any of his sons, he prayed that it might be his son Isaac, as being the least promising of them all."

These exceptions, apparent and real, may be explained in two ways:—

1st. The stupidity attributed to men of genius may be really the stupidity of their parents, guardians, and biographers.

Men are precocious, if they are precocious at all, in the line of their genius. It is observed, as Wieland has stated, that almost all artists and musicians are recorded as precocious, the exceptions being very rare. Music and drawing appeal to the senses, attract attention, and are therefore appreciated, or at least observed by the most stupid parents, and noted even in the most superficial biographies. Philosophic and scientific thought, on the contrary, does not at once, perhaps may never, reveal itself to the senses—it is locked up in the cerebral cells. In the brain of that dull, pale youth, who is kicked for his stupidity and laughed at for his absent-mindedness, grand thoughts may be silently growing; the plant which to-day looks stunted and dwarfed may hereafter quicken into life, rise into strength and beauty—to give fruit and shade to many generations. Scott, for example, though he stood low in his class at school, yet very early exhibited genius as an inventor and narrator of "tales of knight-erranty, and battle, and enchantments."

Newton, according to his own account, was very inattentive to his studies and low in his class, but a great adept at kite flying, with paper lanterns attached to them, to terrify the country people, of a dark night, with the appearance of comets; and when sent to market with the produce of his mother's farm, was apt to neglect his business, and to ruminate at an inn over the laws of Kepler.

It is fair to infer that the stupidity attributed to many other distinguished geniuses may be similarly explained. This belief is enforced by the consideration that many, perhaps the majority, of the greatest thinkers of the world seemed dull, inane, and stupid to their neighbours, not only in childhood, but through their whole lives. The brains as well as the muscles of men differ in the times of their growth. Of a dozen individuals of the same endowments and external conditions some will ripen early, others late. This is observed in colleges, where some who take the lead in everything make no further progress in after life. They "strike 12 the first time." Others who, between 15 and 25, are dullards, between 25 and 40 develop great powers.

It is probable, however, that nearly all cases of apparent stupidity, in young geniuses, are to be explained by the want of circumstances favourable to the display of their peculiar powers, or to a lack of appreciation or discernment on the part of their friends. It is very difficult to find any college graduate of remarkable ability who did not, during his collegiate course, in some way manifest the germs of that ability, but there are many who fail in the prescribed routine of studies in the race for literary honours, who yet, in some department or other, do attain distinction. As compared with the world, the most liberal curriculum is narrow; to one avenue of distinction that college opens the world opens ten. In order to learn the material of which a college class is made, it is necessary not only to look at the marks on the tutor's book and scan the prize list of the societies, but also to go out on the ball ground and down the river—we must mingle in the evening carousal and study the social life of the students in their rooms, or their walks, and in vacation.

Whether we regard those general considerations or not, the statistical fact remains that, in spite of the incomplete-ness of biographies, and the ignorance of parents and teachers, *a very considerable proportion of the greatest geniuses of the world are known to have been as remarkable in their precocity as in their genius; and in spite of this precocity were exceedingly long-lived.*

Great precocity, like great genius, is rare. Although I have known but few children whom fond parents did not at some time believe to be more or less superior to the average, yet I do not remember that I ever saw a very precocious child. There is in some children a petty and morbid *smartness* that is sometimes mistaken for precocity, but which in truth does not deserve that distinction.

The manifestation of genius in childhood is as normal and as healthful as its manifestation in maturity; but in childhood, as in extreme old age, the effects of overtaxing the powers are more severely felt than in maturity. Petty smartness is oftentimes a morbid symptom; it comes from a diseased brain, or from a brain in which a grave predisposition to disease exists. Such children may die young, whether they do or do not early exhibit unusual quickness.

The morbidly precocious soon wear themselves out, early find their level, and in after life are stupid or ordinary; the normally physiologically precocious go on from strength to strength, and do not reach their maximum until between thirty and forty; and live longer and are capable of working harder than those of average gifts. There have been noted and oft-quoted instances where the precocious geniuses have died in early manhood, or just at reaching the maximum of their strength, between thirty and forty. The names of Pascal, Mozart, Keats, will be at once recalled. But we forget the infinite number who have died at the same age or earlier, and of the same diseases; but who neither in childhood nor in manhood exhibited any superior genius. The only method of arriving at the truth on the question is the one I have adopted; that is, to obtain the average longevity of a large number, who were known to have been greatly precocious, and compare it with the average longevity of other able men in the same departments.

Those who have not given special thought to this theme will be suprised to learn how early and how strikingly the genius of some of the greatest and longest-lived heroes was displayed. Leibnitz, at twelve, understood Latin authors well, and wrote a remarkable production. Gassendi, "the little doctor," preached at four, and at ten wrote an important discourse. Goethe, before ten, wrote in several languages. Meyerbeer, at five, played remarkably well on the piano. Niebuhr, at seven, was a prodigy; and at twelve had mastered eighteen languages. Michael Angelo at nineteen had attained a very high reputation. At twenty, Calvin was a fully-fledged reformer, and at twenty-four published great works on Theology that have changed the destiny of the world. Jonathan Edwards, at ten, wrote a paper refuting the materiality of the soul; and at twelve was so amazingly precocious that it was predicted of him that he would become another Aristotle. At twenty, Melancthon was so learned that Erasmus exclaimed, "My God! What expectations does not Philip Melancthon create!"

Causes of the Exceptional Longevity of Great Brain-Workers.

The explanation of the surprising longevity of great brain-workers is quite complex. The readiest answer to the problem would be that brain work is healthful; and that, therefore, the better the brain, and the harder it is worked, the longer the life of its possessor. Such a solution would not be entirely true; and if it were true unqualifiedly, it would clear up but one side of the question.

The answer is to be found, not in any single consideration, but in many, as follows:—

1. *Great men usually come from healthy, long-lived ancestors.* Longevity is a correlated inheritance of genius. In order that a great man shall appear, a double line of tough, more or less vigorous fathers and mothers must fight in the struggle for existence and come out triumphant. However feeble the genius may be, his parents or grandparents are usually strong; or if not strong, are long-lived. Great men may have nervous if not insane relatives; but the nervous temperament holds on to life longer than any other temperament. The great man may himself be incapable of producing other great men; in him indeed the branch of the *race* to which he belongs may reach its consummation, but the stock out of which he is evolved must be strong, and usually contains latent if not active genius.* Longevity is, of course, hereditary, like all qualities or tendencies of organised life; and if great men come from long-lived stock, this fact is one most potent explanation of their exceptional longevity.

* That intellectual qualities are subject to all the laws of hereditary descent, so far as we know these laws, has been fully established by the researches of Galton in England, and of myself in this country. I therefore assume the fact without argument.

2. *A good constitution usually accompanies a good brain.* The cerebral and muscular forces are correlated. This view, though hostile to the popular faith, is yet sound and supportable. A large and powerful brain in a small and feeble body is a monstrosity. "In monstrosities Nature reveals her secrets," says Goethe. When a specially small and delicate frame sustains a specially large and potent brain, men wonder, as at a tree bowed to the earth by the weight of its over-abundant fruit. Everywhere Nature is a slave to the necessity of correlation or correspondence of parts and organs with each other; and unless she heeds it, all organised life would become awry and misshapen. In all the animal realm, there is a general though not unvarying relation between the brain and the body of which it is a part and to which it ministers. A hundred great geniuses, chosen by chance, will be larger than a hundred dunces anywhere—will be broader, taller, and more weighty. In all lands, savage, semi-civilised, and enlightened—the ruling orders, chiefs, sheiks, princes by might and mind, scientists, authors, orators, and great merchants, weigh more than the slaves, peasants, and riff-raff over whom they rule; and bear the evidences of their superiority so clearly that they need no other insignia. In any band of workmen on a railway, you shall pick out the "boss" by his size alone, and be right four times out of five. Those monstrosities where genius is cabined in a small body show the law by their very rarity.

3. *Great men who are permanently successful have correspondingly greater will than common men; and force of will is a potent element in determining longevity.* The one requisite for great success is "*grit*;" and, more uniformly than any other single quality or combination of qualities, it is found in those who attain high distinction. In the grand struggle for existence, it is everywhere the stiff upper lip that conquers; the timid and the yielding are cowed and crushed, and over them rise the courageous and the strong. In certain special lines, as poetry and art, extraordinary gifts may, as it were, draw their possessor into fame with but little effort of his own; but the highest seats in the temples both of art and poetry are given only to those who have earned them by the excellence that comes from *consecutive effort*, which everywhere tests the vital power of the man. That longevity depends not a little on the will, no one will dispute. The whole subject of the relation of mental character to longevity is one of vast interest, and is too far-reaching to be here discussed; but this single point

must be granted without argument, that of two men every way alike and similarly circumstanced, the one who has the greater courage and grit will be the longer-lived. One does not need to practise medicine long to learn that men die that might just as well live if they resolved to live; and that myriads who are invalids could become strong if they had the native or acquired will to vow that they would do so. Those who have no other quality favourable to life, whose bodily organs are nearly all diseased, to whom each day is a day of pain, who are beset by life-shortening influences, yet do live by grit alone. Races and the sexes illustrate this. The pluck of the Anglo-Saxon is shown as much on the sick-bed as in Wall Street or on the battle-field. During the late war, I had chances enough to see how thoroughly the black man wilted under light sickness, and was slain by disease, over which his white brother would have easily triumphed. When the negro feels the hand of disease pressing upon him, however gently, all his spirit leaves him. The great men of history are as much superior in their will-power to the average of their fellows, as are the races to which they belong to the inferior and uncivilised races. They live, for the same reason that they become famous. They obtain fame because they will not be obscure; they live because they will not die.

4. *Great men work more easily than ordinary men.* Their expenditure of force to accomplish great things is less plenteous than the expenditure of ordinary men to accomplish such things. A Liverpool draft-horse draws with ease a load at which a delicate racer might tug and strain without moving it. Ruskin is quite right when he says that the greatest work is done easily. The best action is the unconscious. It is the essence of genius to be automatic and spontaneous. The common mind cannot attain this spontaneity, or at any rate only to a slight degree. Many a huckster or corner tradesman expends each day more force on work or worry than a Stewart or a Vanderbilt. It is notorious that Beecher's great sermons cost him only an hour's musing or so, while many country pastors work for a week over "efforts" that suggest no thought, except pity for the composer. Great genius is usually industrious, for it is its nature to be active; but its movements are easy, spontaneous, joyous. There are probably many school-boys who have exhausted themselves more over a prize composition than Shakespeare over "Hamlet," or Milton over the choicest passages in "Paradise Lost." At one time I acted as surgeon on a gunboat of the United States Navy on the blockade, which was under

the command of a man who, I am sure, worried and exhausted himself more over that little craft than did Admiral Farragut over the entire squadron. When he died, shortly after the close of the war, I was requested by his widow to use my influence in procuring a pension for her. This I was able to do most conscientiously, for I knew that he had worn himself out in the service, although the vessel under his charge, while I was on board at least, never went into action, chased no blockade runner, and experienced not one moment of real peril.

5. *The advantages that belong to the brain-working orders in general.* Of these I have already spoken in some detail. The great brain-workers of the world have not all been rich; neither have they all been poor; some of them have lived a portion of their lives, but very few all their lives, in extreme want; and the majority have been most of the time surrounded with at least moderate comforts.

Causes of the Great Longevity of Clergymen.

When, in 1867, I first called attention to the fact that clergymen were longer lived than any other class of brain-workers, serious doubt was expressed whether there might not be some error in my statistics. So much had been said of the pernicious effects of mental labour, of the ill-health of brain-workers of all classes, and especially of clergymen, that very few were prepared to accept the statement that the clergy of this country and of England lived longer than any other class, except farmers, and very naturally suspected a lurking fallacy. Other observers, who have since given special attention to the subject, have more than confirmed this conclusion, and have shown that clergymen are longer-lived than farmers.

The Rev. Josiah F. Tuttle, D.D., President of Wabash College, Indiana, has ascertained the ages of 2442 clergymen—600 Trinitarian Congregationalists, 317 Presbyterians, 231 Episcopalians, 268 Baptists, 208 Methodists, 166 Unitarians, &c.,—and found that the average was "a little over 61 years." "Considerably over one-half of the whole were over 60 years of age at their death; three-fourths of the whole were over 50 years old at death; and seven-eighths of the whole were over 40 years of age at death." Dr. Tuttle found that the average age at death of 408 individuals (not clergymen), and who had died over 21 years of age, was a little over 51 years. This result pretty nearly corresponds with mine.

But by far the more thorough investigation on this subject, and one that must fully settle the question for all minds over whom facts have any influence, has been recently made by the Rev. J. M. Sherwood, formerly editor of "Hours at Home," and now Secretary of the Society for Promoting Life Insurance among Clergymen. This gentleman has laboured long and patiently in this department, and has ascertained that the average age of our ministers at death is sixty-four. The report (I quote from Document No. 3 of the Society) states: "this is four years more than the longevity of the most favoured (?) class; ten years more than in the other professions: and from twelve to nineteen years above that of mechanics, artisans, miners, operatives, and the like."

These conclusions differ slightly from mine, but the difference is in favour of clergymen. Mr. Sherwood informs me that he had obtained the average from a list of ten thousand clergymen, whose ages at death he ascertained at great labour by consulting "the minutes of ecclesiastical bodies for thirty years past, the catalogues of theological seminaries, Wilson's "Historical Almanack," Dr. Sprague's "Annals of the American Pulpit," biographical dictionaries, the files of religious journals, &c.

A list of ten thousand is sufficient and more than sufficient for a generalisation : for the second five thousand did nothing more than confirm the result obtained by the first. It is fair and necessary to infer that if the list were extended to ten, twenty, or even one hundred thousand, the average would be found about the same.

In England, also, clergymen live to a greater age than any other class. According to the report of the Secretary of the Clerical Mutual Life Assurance Society, the mortality is less than that in twenty other companies by a very important percentage.

Causes of the Exceptional Longevity of Clergymen.

The reasons why clergymen are longer-lived than any other class of brain-workers are these :—

1. *Their callings admit of a wide variety of toil.*—In their manifold duties their whole nature is exercised—not only brain and muscle in general, but all, or nearly all, the faculties of the brain—the religious, moral, and emotional nature, as well as the reason. Public speaking, when not carried to the extreme of exhaustion, is the best form of gymnastics that is known; it exercises every inch of a man, from the highest regions of the brain to the smallest muscle. In his public ministrations, in his pastoral calls, in his study, in his business arrangements, in his general reading, the

pastor exercises more widely and variously than any other calling.

2. *Comparative freedom from financial anxiety.*—The average income of the clergymen of the leading denominations of this country in active service as pastors of churches (including salary, house rent, wedding fees, donations, &c.), is between 800 dols. and 1000 dols., which is probably not very much smaller than the net income of all other professional classes. Further, the income of clergymen in active service is collected and paid with greater certainty and regularity, and less labour of collection on their part than the income of any other class except Government officials. Then, again, their income, whether small or great, comes at once, as soon as they enter their profession, and is not, as with other callings, built up by slow growth.

Worry is the one great shortener of life under civilisation; and of all forms of worry, financial is the most frequent, and for ordinary minds the most distressing. Merchants now make, always have made, and probably always will make, most of the money of the world; but business is attended with so much risk and uncertainty, and consequent worry, that merchants die sooner than clergymen, and several years sooner than physicians and lawyers.

By what I here say, I do not mean to give the impression that clergymen are properly paid: for it is thoroughly true, as was once remarked by a certain political economist: "We pay best,—1st. Those who destroy us—*generals*. 2nd. Those who cheat us—*politicians and quacks*. 3rd. Those who amuse us—*actors and singers*; and least of all, those who instruct us."

The average income of all classes in this country is small—about 700 dols. a year—and for the labouring classes not more than half that sum; and if the same efforts were made to obtain the details of the financial history of every family in the land, as has been done in the case of clergymen, there would be some very dreary reading.

3. *Their superior mental endowments.*—The law which I derive from the study of vital statistics is, that other conditions being the same, the greater and richer the brain, the greater the longevity.

Now I speak calmly and discriminately, and from a careful comparison of biographical data, when I say that the clergymen of this country—as represented by the Congregational, Presbyterian, and Unitarian denominations—have presented a higher average of the higher kinds of ability than any other equally large class, of any age or section, of recorded history.

During the past fifteen years there has been a tendency, which is now rapidly increasing, for the best endowed and the best cultured minds of our colleges to enter other professions, and the ministry has been losing while medicine, business, and science have been gaining.

4. *Their superior temperance and morality.*—Clergymen are more regular in their sleep, meals, and exercise than any other intellectual class; and are less exposed to injurious influences and contagious diseases than some other occupations. Very rarely, indeed, does a clergyman become grossly intemperate, or addicted to gambling, or to the exclusive and injurious pursuit of any animal pleasure.

III. ON THE CONDITION OF THE ATMOSPHERES OF THE PLANETS.

By E. Neison, F.R.A.S., &c.

The atmospheric conditions prevailing upon the planets of the solar system is a question of peculiar interest, for until these are thoroughly realised it is impossible to properly interpret the numerous observations of the physical condition of the planets. Moreover, seeing that in the great majority of cases, the observations deal only with variations in the appearance of the planet, and that these must arise almost entirely from atmospheric changes, the important bearings of the conditions regulating these is at once manifested.

The question of the height of the atmospheric envelope of the four great planets, Jupiter, Saturn, Uranus, and Neptune, derives peculiar interest from the influence it possesses over the physical constitution of the planet itself. It is known that the mean densities of these four greater members of the solar system are all very small, being in no case much greater than that of water, this peculiarity being of great interest and considerable importance in considering the probable physical constitution of these planets. It has long been suggested that this may be only apparent for the diameter of these planets measured, being that of the upper cloud-bearing strata of their atmospheres; if this be of great depth and yet of moderate density, the actual solid planet may possess a much greater mean density. If it be supposed in fact that the depth of the atmosphere was as much as one-tenth the apparent diameter of the planet, the mean density of this would be nearly doubled. It has been often urged, therefore, that the real average density of these planets may be much greater than that usually supposed, from their real dimensions being far less than their apparent; the augmentation arising from the presence of cloud-bearing atmospheres, that on Jupiter, it is assumed, may be as much as eight to ten thousand miles in depth. But no attempt appears to have been made to ascertain how far these supposed great depths of cloud-bearing atmosphere are consistent with those known dynamical laws which inflexibly sway the condition of gaseous envelopes on the other planets beside the earth; or whether, to render such immense atmospheres possible on these giant planets, conditions must not be assumed that are inadmissible.

The laws on which the physical condition of the atmospheric envelope of any of the planets may be regarded as depending have been long known, and have been applied with tolerable success in obtaining an approximate acquaintance with principal variations in the atmosphere of the earth, and for the higher regions of the gaseous envelope of any planet the theoretical constitution may be considered to have been determined, in so far as its solution is reduced to the ascertaining of the value of a single constant. But no attempt seems to have been made to apply these results to other planets than the earth, nor have they been placed in a distinct and convenient form for this purpose. A sufficiently approximate form of these equations can be derived very simply, and, as will be shown, made to afford results of considerable importance in considering the true nature of the planetary atmospheres.

Let

a = Radius of the planet.

g_o δ_o p_o t_o = Force of gravity, density, pressure, and temperature of the atmosphere at the surface of the planet, and

g δ p t = the same at a distance x above the surface.

Then—

$$d\,p = -g\,\delta\,d\,x \qquad . \quad . \quad . \quad . \quad (1)$$

and replacing g by it value $g_o \dfrac{a^2}{(a+x)^2}$ and change the variable to s where—

$$s = \frac{x}{a+x} \qquad . \quad . \quad . \quad . \quad . \quad (2)$$

and (1) becomes—

$$dp = -g_o \, a \, \delta \, ds \quad . \quad . \quad . \qquad (3)$$

From the theory of the expansion of gases

$$p = \delta \frac{p_o}{\delta_o} \frac{1+\varepsilon t}{1+\varepsilon t_o} \quad . \quad . \quad . \quad . \qquad (4)$$

and dividing (3) by this value of p—

$$\frac{dp}{p} = - g_o \frac{\delta_o}{p_o} a \frac{1+e t}{1+e t_o} ds \quad . \quad . \qquad (5)$$

This is the differential equation between the pressure and height above the surface, and when integrated by substitution in (4) gives the law of decrease of density, with increase of height above the surface. This equation cannot, however, be integrated unless the manner in which the temperature varies with the altitude is known. For the present this may be assumed to be given by—

$$\phi'(s) = \frac{1+\varepsilon t_o}{1+\varepsilon t} \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

and then very conveniently it may be considered that—

$$\phi(s) = \int \phi'(s) \, ds \quad . \quad . \quad . \quad . \qquad (7)$$

Substituting these values in (5) after integrating and determining the constant by the condition that $p = p_o$ when $s = o$; whilst replacing for brevity the ratio $\frac{\delta_o}{p_o}$ by ρ and (5) becomes—

$$p = p_o \; e^{-g_o \rho a \phi(s)} \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

and by substitution in (4)—

$$\delta = \delta_o \; \phi'(s) . \; e^{-g_o \rho a \phi(s)} \quad . \quad . \quad . \qquad (9)$$

These two equations, once the form of $\phi'(s)$ is known, are sufficient to entirely determine the normal physical condition of the atmosphere of any planet, in so far at least as depends on variations indensity, pressure, and temperature.

As the expansion of gases for a small increase of heat is very small, for conditions when the atmosphere undergoes only slow and slight variations in temperature as the height above the surface increases, the law of decrease of temperature possesses only secondary importance, and the condition of the atmosphere of the planet approximates to that it would possess were the temperature throughout uniform. For many purposes it may, indeed, be considered that the temperature of the atmosphere is constant without introducing any very material error, especially in the higher regions, where the decrease of temperature must be so slight as to approach very closely to this condition. This condition is, in fact, as far as any of the planets of the solar system are concerned, the limit on this side to any possible constitution an atmosphere can possess; and as it reduces the equations to their simplest form for the purpose of ascertaining the extreme in this direction, this condition may first be supposed to hold.

Assuming that the temperature is constant and equal to t_0 then $\phi'(s)$ becomes unity and $\phi(s)=s$, whence substituting these values in equations (8) and (9), the two equations become identical and can be expressed as—

$$\frac{p}{p_0} = \frac{\delta}{\delta_0} = e^{-g_0 \rho a s} \quad . \quad . \quad . \quad . \quad (10)$$

For atmospheric air it has been ascertained that—

$$\rho = \frac{0{\cdot}20131}{1+\varepsilon t_0} \quad . \quad . \quad . \quad . \quad . \quad (11)$$

and for any other gas it can be found by dividing this by the specific gravity relative to air of the gas in question. Computing the value of the coefficient of s for the six planets, Mercury, Venus, Mars, Jupiter, Saturn, and Uranus, and denoting by T the quantity $1+\varepsilon t_0$ then the coefficient βT^{-1} of s on the six planets will be, on—

Mercury	βT^{-1}	$= 0{\cdot}0086$	$. T^{-1}$
Venus	,,	$= 0{\cdot}0671$	$. T^{-1}$
Mars	,,	$= 0{\cdot}0246$	$. T^{-1}$

and on—

Jupiter	βT^{-1}	$= 2{\cdot}150$	$. T^{-1}$
Saturn	,,	$= 0{\cdot}860$	$. T^{-1}$
Uranus	,,	$= 0{\cdot}344$	$. T^{-1}$

The planet Neptune has been omitted from its great resemblance to Uranus, which renders the physical conditions regulating the atmospheres of the two planets almost identical.

The length in miles of a unit of s on the different planets has, for convenience, been taken so that n units of s will, in miles, on—

Mercury	$= 0{\cdot}148$	$+$	$0{\cdot}00002\, n^2$
Venus	$= 0{\cdot}376\, n$	$+$	$0{\cdot}00004\, n^2$
Mars	$= 0{\cdot}246\, n$	$+$	$0{\cdot}00003\, n^2$

whilst as on the larger planets the second term will be always insensible, it may be considered that on—

Jupiter	$= 4{\cdot}30\, n$	miles.
Saturn	$= 3{\cdot}60\, n$	miles.
Uranus	$= 1{\cdot}65\, n$	miles.

Moreover, as on the larger planets the depth of the atmosphere is to be measured, these values become rigorous.

Keeping for the present to the group of the three greater planets, Jupiter, Saturn, and Uranus, supposing the density of the upper limit of the cloud-bearing strata of the atmospheres of these planets to be known, it would be easy to compute at what depth beneath this the superincumbent pressure would be so increased as to crush the gases forming the atmosphere into so dense a condition that they would become, to all intents, liquids; though probably long before this could be accomplished the atmosphere would have come to an end through the condensation of its constituents. The measured diameter of Jupiter and, as far as can be ascertained, of Saturn and Uranus, isth at of the upper limit of the cloud-bearing strata of their atmospheres; and, consequently, at this point the density cannot be of extreme slightness, for masses of condensed vapours or clouds can only exist and remain suspended in a strata of comparative density. In fixing the limiting density, where clouds may be considered as possibly existing, at as low as one-thousandth of the surface density of the earth's atmosphere, an extreme value is probably taken; for not only is it questionable whether any mass of brilliantly illuminated condensed vapour could remain in suspension in air of this rarity, but it is doubtful whether condensation must not always ensue before reaching so low. Assuming, therefore, this limit, it remains to consider the maximum depths of the atmospheric envelopes of the three great planets; supposing the lowest limit marked by the point where the gaseous constituents would be crushed by the superincumbent pressure into a consistency which would class them amongst liquids, not gases, even were they not actually liquefied long before reaching this point. Supposing that the temperature of these three planets is, like our own, not materially different from $+100^{\circ}$ to -100° C. the atmosphere may be considered as possessing uniform temperature of 0° C.

On Jupiter at a depth of only 19 miles the density would be already over ten times as great as our own; at 28 miles it would be denser than water, and at 33 miles as dense and heavy as mercury; though long ere this the immense pressure would have, through liquefaction, put an end to the atmosphere and the laws of gaseous compression. On Saturn the same points would be reached at a depth of 38, 57, and 68 miles; and on Uranus at 41, 66, and 77 miles below the visible cloud surface. These comparatively trifling depths under the supposed conditions are the maximum that the cloud-limited atmospheres of these three giant planets can possess; and their insignificance is such that on the limb they would subtend an angle such a minute fraction of a second of arc as to be almost undetectable and quite unmeasurable. Yet as far as the observed phenomena of the cloud belts of Jupiter are concerned, the depth is ample to explain all that has yet been definitely ascertained. Considering these three planets from the point of view that has hitherto been adopted, these may be regarded as the maximum depths of their cloud-bearing atmospheric envelopes, because any law of decrease of temperature that may be adopted will be without sensible effect on these results. But there is another point from which Jupiter at least may be regarded, and which considers that perhaps this giant planet may have retained sufficient heat to entirely remove the conditions prevailing on its surface from any analogy in point of temperature to those on the earth. It has been urged with much force that, from its great mass, Jupiter may have been much longer in cooling down than the earth and other smaller planets, and that, therefore, its surface temperature may still be very high; perhaps approaching or even reaching a low red heat. Under these conditions the law of decrease of temperature rises so much into importance that it cannot remain neglected, as has hitherto been done; but some attempt must be made to take it into account. The researches into the spectrum of Jupiter show that the greater majority, if not all, the light it sends to the earth, is merely reflected sunlight that has traversed a sufficient layer of atmosphere to exhibit absorption lines which indicate the presence of aqueous vapour. Had Jupiter been actually incandescent, that is to say were its surface, which can be probably seen through the clouds environing it, so intensely heated as to become white hot, a very different result would have been expected, and only one uniform mass of permanent dense cloud would be shown by Jupiter. Moreover, the formation of persistent marking in the visible surface of the planet, the arrangement noticeable in their form and position, and their appearance all appear more consistent with a moderate surface temperature, and could hardly be expected to be screening a white-hot incandescent planet. Some striking evidence of the vast energies at work could not but become manifest in a most marked manner were the entire surface of Jupiter one molten and seething mass of fire, as such a supposition would render it, whilst the environing vapours would become the scene of titanic throes from the alternate decomposition and re-combination of those energetic elements whose most stable combinations would be destroyed by the great temperature at the surface, to reunite on ascending to the cooler strata, only to be again dissociated on sinking towards the surface.

If, therefore, it were assumed that the surface of Jupiter might be so intensely heated as to reach a temperature closely approaching a white heat, say 1100° C., an extreme view of the possible high temperature of Jupiter would have been taken, and the resulting depth for the atmosphere of Jupiter may be regarded as the maximum present under even these conditions. It would only remain, therefore, to frame some probable law of the decrease of temperature of the atmosphere from the surface, for it is impossible that the air could remain throughout at this great temperature; but seeing how quickly gases cool under the condition they exist under when forming portion of the upper strata of the atmosphere, a very rapid degree of cooling might be expected when at a little distance from the surface. The actual law of decrease of temperature is of course unknown but sensibly, except close to the surface; the results obtained by employing any approximate law will give results sensible the same as the true, for the purpose in view here, as well as for most others, including the computation of the refraction. In framing such a law it will be necessary to put it into such a form that it can be readily applied without alteration, not only to different views of the conditions prevailing upon any one planet, but to those upon the whole six. The temperature of any strata of an atmosphere must be held to vary in some manner inversely as its density, this law regulating implicitly not only the conduction but the absorption of heat, as well also as the radiation, although not explicitly. It will also be apparent that close to the surface the temperature will decrease slowly, owing to the influence of the hot surface, and that, from exactly the reverse cause, the temperature towards the outer portion of the surface will vary with extreme slowness, the quickest decrease being in the central portion of the atmosphere of any planet.

Returning to equation (6), assume—

$$\phi'(s) = \left\{1 - f + f\,e^{-r\,u}\right\}^{-1} \quad . \quad . \quad . \quad . \qquad (12)$$

where u is a new variable introduced to retain the equations in a simple form, and such that—

$$g_0\,\rho\,a\,s = (1 - f)\,u + \frac{1 + r}{r}(1 - e^{-ru}) \quad . \quad . \qquad (13)$$

Differentiate this with respect to u, and after multiplying (12) by $\frac{ds}{du}$ integrate when—

$$\phi(s) = \frac{1}{g_o \rho a}\{u - \log(1 - f + fe^{-ru})\}$$

substituting these values $f\,\phi'(s)$ and $\phi(s)$ in the two equations (8) and (9), and they become—

$$\frac{p}{p_o} = \{1 - f + f\,e^{-ru}\}\,e^{-u} \quad . \quad . \qquad (14)$$

and—

$$\frac{\delta}{\delta_o} = e^{-u} \quad . \quad . \quad . \quad . \quad . \qquad (15)$$

Upon the values given to the two constants f and r will the rapidity and degree of decrease of heat depend, the former influencing mainly the amount and the latter more immediately the quickness of the decrease in temperature with the variation in density. To determine f, the condition exists that f must be such a value that at the summit of the atmosphere the temperature will be the same as the temperature of space; or if this be supposed extremely low, that at which the elasticity of the atmosphere is equalled by the attraction of gravity. Accordingly if t' be this temperature,—

$$f = 1 - \frac{1 + e\,t'}{1 + e t_o} \quad . \quad . \quad . \quad . \qquad (16)$$

The other constant r can be determined if the temperature at any given great height were known; but this not being so, it must be arbitrarily fixed. It is noticeable that when r is unity, its influence vanishes, but as it approaches zero its influence is to make the condition of the atmosphere approach those of a uniform temperature; whilst as it increases from unity and approaches infinity, it gradually approaches the condition of an atmosphere of the uniform temperature given by—

$$\frac{1 + \varepsilon\,t}{1 + \varepsilon\,t_o} = (1 - f)$$

on a surface heated to t_o. In both cases it may be held to approach from the value unity the condition of a uniform temperature.

Consequently, as it is apparent for beyond any but small values of u, this variable is greater than $\beta\,s$; for any admissible assumed law of decrease of temperature, the density of an atmosphere at a given height is less than the condition of a uniform temperature. Therefore the maximum depth any planetary atmosphere can possess will be that when its temperature is uniform. Now, applying this to the three great planets, Jupiter, Saturn, and Uranus, and allowing that their surface may be so intensely heated as to be at a temperature of 1100° C., and the computation of their depths on the hypothesis of a uniform temperature will give their maximum depths.

On Jupiter, under these conditions, commencing from a point already mentioned, when considering their depth at a temperature near zero centigrade, then at 74 miles below the upper side of the visible cloud layers it would be ten times denser than our own atmosphere, at 110 miles compressed until denser than water, and at only 148 miles as dense as mercury; though, as before remarked, long before this could have happened, the condensation of its constituents must have occurred, as the whole ceased to be a gas. On Saturn, the depths beneath the surface, where the same would occur, would be 154, 231, and 308 miles, and on Uranus 178, 268, and 357 miles. The telescopic insignificance of the depths, in mere dimensions, of the greatest of these three quantities on each planet is best shown by considering that on Jupiter it subtends an angle of only 0·06″ of arc, on Saturn of 0·08,″ and on Uranus of 0·04″—quantities unmeasurable, and almost invisible even in the finest telescopes. It cannot be supposed, however, that these dimensions, small as they are, can ever be reached by any of the three planets, for a temperature such as supposed could not extend undiminished to the outer limits of the cloud-bearing strata; and, therefore, to obtain in any way the probable depth of the atmospheres to these planets use must be made of equations (15) and (13). To take the most favourable circumstance possible, suppose the temperature never to fall below zero C., then the value of f from (16) for the supposed intense surface heat of the planets will be 0·80, whilst to give a very favourable condition to a great depth, make r instead of near unity only 0·20; so that temperature decreases so slowly as to be even beyond the summit of the cloud-bearing strata, the atmosphere will be of still considerable warmth. Then the three respective points already mentioned would, on Jupiter, be reached at a depth of 60, 72, and 78 miles; on Saturn, at 126, 152, and 164 miles; and on Uranus at 144, 173, and 188 miles respectively, or scarcely one-half of that found on the previous assumption. These depths appear, therefore, to be the maximum possible on any permissible condition on these three giant planets of the solar system.

There is at present no reason for believing that the atmosphere of Jupiter possesses a greater depth than from 25 to 50 miles; the only observation that can be considered as indicating any very much greater depth being those of the form of the shadow of the satellite, which though highly interesting, cannot legitimately be regarded as pointing in this direction. Whatever, therefore, may be held to be the cause of the slight mean density of Jupiter and its companions in this respect, Saturn, Uranus, and Neptune, it cannot arise simply from their possessing an immense depth of atmosphere.

There is one other point in connection with the atmosphere of Jupiter requiring examination, namely, the influence of the presence of an atmosphere on the occultations of its satellites; for if a fringe of atmosphere of any recognisable breadth were present, the satellites, instead of disappearing sharply behind, and re-appearing as distinctly from beyond the planet's limb, would fade slowly away. Thus from the effects of its refracting the rays of light, it would appear that if any considerable layer of atmosphere to Jupiter extended beyond his disc formed by the cloud strata, it could not fail to be recognised by retarding occultation and eclipses, and accelerating the re-appearances. With sufficient approximation, the horizontal refraction for any layer of Jupiter's atmosphere assumed similar to our own and whose density is δ_0' would be—

$$r_0 = 3\cdot0^\circ \ \delta_0'$$

Accordingly, for a strata of air only one-thousandth the density of our own the horizontal refraction would be still strongly marked.

In all probability the real density of the strata of Jupiter's atmosphere immediately beyond the uppermost clouds is in density over one-tenth of our own, and would, therefore, exert a horizontal refraction equal to half of our own, and be therefore most marked. The true reason of its being undetectable consists of the very slight breadth of the fringe of atmosphere from its very quick decrease in density, so that only twenty-four miles beyond the upper cloud layers of Jupiter's, the density of the atmosphere would be utterly insensible, and with it the horizontal refraction. As this distance of twenty-four miles would at Jupiter's distance subtend an angle of barely one-hundredth of a second of arc, whatever changes might be produced by it would be entirely unrecognisable; so that whatever may be the density of its atmosphere, the effects of the refraction of light through it would be entirely undistinguishable from the earth. And the entire absence of any detectable retardation of the occultation of Jupiter's satellites is in itself sufficient to show the absence of any great depth of atmosphere; for if the increase of density was so slow as to admit of this, the decrease beyond the cloud layers would be sufficiently slow to admit of the refraction being detected.

There is one point in connection with the maximum depth of the atmosphere of Jupiter that requires further attention, and that is certain phenomena seen during the transits of the shadows of the satellites across the planet. By numerous observers it has been noticed that occasionally the form of the shadow is not a black circle, but a slightly elongated ellipse, which though it has been estimated to be as much as half as long again as broad, probably is very rarely so markedly elongated, but that from the character and very delicate nature of the observations, the ellipticity has been as usual considerably over-estimated. It has been noticed that this elongation only occurs when the planet is near quadrature, so that the shadow on Jupiter is seen from a point inclined at a considerable angle to the axis of the cone of shadow.

It has, however, been considered* that this indicates a peculiar character and great depth in the atmospheric envelope of this giant planet, and from a series of nine rough estimates of the amount of this elongation, an attempt has been made to deduce the depth of the atmosphere of Jupiter by assuming the cause of this appearance to be due to the passage of the shadow cone, though a multitude of very thin, delicate, almost translucent clouds, suspended in the atmosphere of Jupiter. For assuming the atmosphere of this planet to be filled with delicate clouds of this nature, and so extraordinarily transparent as to permit shades to be visible through immense thicknesses of it, it can be shown that when Jupiter is near quadrature, the shadow column of the satellite will appear as an elliptical dark spot, with an equatorial penumbra. Supposing these conditions to hold, from nine observations the depth of the atmosphere of Jupiter is shown by a rough method to vary in minimum depth from 3200 to 9200 miles, the mean of nine estimates being 5400 miles. Even granting that the constitution of the atmosphere supposed in this hypothesis can be considered conceivable, and crediting the atmosphere of Jupiter to be filled with delicate clouds sufficiently transparent to enable graduation of shadow to be distinctly visible through an

* BURTON, M. N. Roy. Ast. Soc., vol. xxxv., p. 65.

immense thickness that must generally be 5000 miles, though at times nearly 15,000 miles in depth, this explanation for the cause of the elongation is scarcely satisfactory, as no account is taken of the changes that must occur in the progress of the shadow across the disc. But apart from this, the supposition of such an immense depth of semi-transparent atmosphere is only possible if the surface of Jupiter possesses the tremendous temperature of 150,000° C. —a degree of heat impossible to realise; and that must render Jupiter a rival to the sun in intrinsic brilliancy. Now, the ground on which this hypothesis rests, which, considering the very small number of observations must be held to be extremely slight, may be divided into two: first, the equatorial penumbral fringe, and secondly, the elongation of the shadow. With regard to the former, as the true penumbræ of the shadows of the satellites is very considerable, being in the case of the nearest satellite nearly as broad as the dark shadow, whilst in the case of the farthest it is nearly four times as broad, and yet is rarely distinctly to be seen, any penumbral fringe that may be detected cannot be well distinguished from the effects of the true penumbræ; whilst the action of the atmospheres of the planet's satellites must still further complicate the appearance of the shadow. As far, therefore, as any deduction made from the presence of a penumbral fringe under these conditions, unless it could be shown that the appearances were independent of the true and atmospheric penumbræ, no legitimate conclusion could be drawn. But from the great variability in the size of shadows of the satellites on different occasions, it is known that the penumbral fringe must be variable. As there must be a penumbra entirely round the satellite, the mere inability on certain occasions to detect it near the Polar region cannot be held to show much.

It can, however, be easily shown that when Jupiter is in quadrature the true form of the shadows of its satellites is elliptical without resorting to any hypotheses of an extraordinarily translucent and deep atmosphere. For taking the most unfavourable case of an equatorial transit, and putting θ for the Jovian ecliptical longitude of the shadow, measured from the point where the sun is in the zenith, and considered positive towards the east, then denoting by θ_1 the ecliptical longitude of the centre of the earth, the length of the shadow of the satellite as seen from the earth will be obviously

$$\int_{\theta'}^{\theta''} \cos(\theta-\theta_1)\, d\,\theta = \sin(\theta''-\theta_1) - \sin(\theta'-\theta').$$

where—

$$\theta'' = \sin^{-1}(\sin\theta + d)$$
$$\theta' = \sin^{-1}(\sin\theta - d)$$

d being the radius of the shadow. The breadth of the shadow will be obviously equal to $2d$. Now, considering the mean radius of the shadow to be half a second of arc under these conditions when $\theta = 9°$ (for its maximum value 11° it will be much greater), the ratio of the equatorial diameter of the satellite to the polar will be, when the centre of the shadow is in the following apparent ecliptical longitude,—

$+80° = 1{\cdot}90$	$-75° = 0{\cdot}40$
$+60° = 1{\cdot}27$	$-60° = 0{\cdot}71$
$+45° = 1{\cdot}15$	$-45° = 0{\cdot}84$
$+30° = 1{\cdot}07$	$-30° = 0{\cdot}90$

The apparent centre of Jupiter being in $+9°$, of course all the positive longitude must be diminished by 9° and the negative increased by 9° to obtain the position relative to the apparent centre of the disc. It is evident, therefore, that for nearly two-thirds of its course after entering the disc before reaching the centre the shadow will appear as an ellipse, the elongation gradually diminishing. Near the centre it will be circular, and for about one-third of the final half of its passage elliptical in the reverse direction. These variations have been detected with a six-inch equatorial during this year; though the equatorial penumbral fringe which must necessarily be present has not been distinctly seen, probably on account of its exceeding delicacy.

Comparing the effect of this cause with the only six cases mentioned definitely in the paper referred to,* it appears that in four of these the ellipticity arising from this case must have been sufficient to render it distinctly visible; in one case the shadow was round, and in the other most markedly elliptical in the reverse direction to that seen. As under any conditions these variations must be undergone, the effect that they would have in combination with the hypotheses assumed, would be to make the dark shadow to all intents vanish on occasions before reaching the limb of Jupiter—a fact in itself sufficient to condemn the hypotheses. But other causes might readily produce the effects noticed, especially the clouding of the atmosphere of the satellites, whilst the extraordinary irregularities undergone by the satellites themselves show the uncertainty that must attach to such extremely delicate observations.

* M. N., vol. xxxv., p. 65.

There are other points of some question in connection with the condition of the atmosphere of Jupiter, more especially the shading noticed by Brett, Knobel, and Lassell on some of the minute white cloud-like spots. These appearances are, however, far from inconsistent with a moderate depth of atmosphere, such as it has been shown can well be supposed to exist on Jupiter. But in these, as in all other points, it is impracticable to found any trustworthy conclusion on so few observations as a score or so, and numerical data must be based on actual measures, for estimations on these delicate points can never be satisfactory.

In considering the probable atmospheric conditions of the three smaller planets less uncertainty with regard to the two principal unknown quantities exist, for not only is there no reason for believing that the temperature of these three planets differs materially from that of the earth, but neither can the density or nature of their atmospheres. Their comparative nearness also allows of telescopic investigation, affording a considerable amount of information with regard to their probable condition; whilst their astronomical character indicate a great analogy in constitution of the earth. It will, therefore, not be necessary to regard them from the extreme points of view necessary in dealing with Jupiter; whilst their proximity to the earth permits an additional method of obtaining information with regard to the atmospheres to be made use of, namely, its horizontal refraction.

Mars may be first considered as being the planet about whose physical constitution more information has been obtained, from telescopic and spectroscopic investigation, than any other. The appearance of Mars under favourable conditions is well known,—a distinct orange ground with faint shadings of brown, yellow, red, and grey, containing numerous permanent dull bluish grey markings, whilst the poles are marked by circular brilliant white spots. These last, from various considerations, have been, with justice, regarded as due to accumulations of snow and ice on the poles of Mars in the same manner as on the earth; and the dark bluish grey or, according to many observers, greenish spots appear to be true seas on Mars. The existence of an atmosphere to Mars has been demonstrated by telescopic observation, which has shown many clouds, but apart from this is a necessary corollary to the existence of moisture; and from the spectroscopic researches of Huggins and Janssen this atmosphere is very similar to our own, and according to the latter the presence of moisture is shown by its spectrum. Mars may therefore be considered to possess an atmosphere bearing a considerable resemblance to our own, though, from the small number of clouds and the general transparency, perhaps slightly inferior in density. The temperature at the surface is also probably very similar to our own, for the following reasons:—Were the temperature of Mars, in any marked degree, warmer than our own, from the presence of considerable bodies of water, an envelope of moisture of considerable dimensions would be formed, the polar ice zones would be much smaller, numerous clouds must at times occur, and the column of aqueous vapour traversed by the sunlight reflected from the surface of Mars would be so considerable, that the spectroscopic evidence of its existence would be of the most marked character.

Were, however, the temperature in any degree much colder than our own, the seas of Mars would be permanently frozen and snow covered; the general brightness of Mars being thus greater than the polar zones even, whilst the amount of aqueous vapour, though very variable, would be far too small to be revealed by the spectroscope, and clouds would be entirely absent. Only, therefore, if the temperature of Mars resembles that of the earth can its telescopic and spectroscopic characters be as they are. The conditions, therefore, of the atmosphere of Mars, in so far as its density and temperature are concerned, may be regarded as very similar to those of the earth; that is to say that its density is probably not over five times greater or five times less than our own, and its mean temperature at the surface within 40° C. of that of the earth.

Under these conditions the law of decrease of temperature of the atmosphere of Mars ceases to possess the all-important character it has when the variations in temperature are more marked, for the decrease must necessarily be, whether slow or quick, so small as to exert comparatively slight effect. The conditions are, however, sufficiently similar to those of the earth to render it probable that the law that best represents the rate of decrease of temperature on the earth will also best represent the same on Mars. In the equations (12), (13), and (15), putting $r=$ unity, and $f=\frac{1}{4}$, and they then reduce to the well known equations employed by Ivory, in his celebrated theory of the Astronomical Refraction, regarded by Plana and other eminent astronomers as best representing the condition of the earth's atmosphere. Substituting, then, the constant for Mars, and the height corresponding to any given density of the atmosphere of Mars will be found. From this it appears that, at a height of 8·59 miles, the density would be reduced to only half that at the surface; at the height of 15·84 miles, only one-fourth; at 24 miles, only one-tenth; at 44 miles, one-hundredth; at 64 miles, only one-thousandth; and at 130 miles, only one-millionth of that at the surface. When most favourably placed one second of arc at the distance of Mars is equal to 210 miles, and for a height of one-fifth of a second of arc at the limb the atmosphere would not be distinguishable from the surface of the planet. This corresponding to 42 miles, it is evident that nearly the entire atmosphere of Mars, at the limb, is so close to the surface as to be, to all intents, indistinguishable from it. This circumstance is important, for two occultations of stars by Mars have been seen when no trace of any retardation or distortion was observed, the star disappearing sharply and neatly at the limb. It has been considered that this indicates that the atmosphere of Mars must be of small density, or else the effects of its horizontal refraction would have manifested themselves. From the small diameter of Mars a horizontal refraction of only one four-hundredth the amount of our own would entirely prevent the disappearance of a star behind Mars, but would spread the rays into a distinct circle of light around the planet. But the breadth of the fringe of atmosphere being thus so small, only one-hundredth lying beyond one-fifth of a second from the limb, it is impossible to distinguish this circle of light, so that the star seems to disappear sharply at the limb, though in reality vanishing at a fictious border to the planet. To ascertain the effect of this small portion of the atmosphere beyond this distance from the limb, as the density of the atmosphere at this point is barely one-hundredth of that at the surface, the following approximate form of the equation to the horizontal refraction may be employed:—

$$r_0 = 60{\cdot}6'' \, \delta_0' \, \sqrt{\frac{\beta \pi}{2}}$$

where r_0 is the horizontal refraction and δ_0' the density of the air at the given point as compared with that at the surface of the earth.

Substituting the value of β and putting the density of the atmosphere at the point in question at only one-hundredth of that at the surface of Mars, the horizontal refraction, at a distance of only one-fifth of a second of arc from the true limb, would be—

$$r_0 = 0{\cdot}34'' \, \delta_0$$

whilst at a distance of only one-third of a second of arc from the limb, it would be utterly insensible for any possible value of δ_0, the density of the atmosphere at the surface of Mars, which cannot be supposed to be much more than 5, but is probably slightly less than unity. A distance of one-third of a second of arc from the true limb of Mars is to all intents a quantity perfectly indistinguishable, for the irradiation at the border of the planet must usually exceed this, so that no effects of refraction from the atmosphere of Mars during an occultation of a star can be expected to be telescopically visible. It will also be apparent that extreme limits of what can in any sense whatever be held to be an atmosphere to Mars must lie within two-thirds of a second of arc from the true limb of Mars.

In certain features the atmosphere of Venus seems to present a strong contrast to that of Mars; thus on the latter extensive dense masses of cloud are very exceptional, but on Venus they appear to be the rule, so that it is seldom that the true surface can be distinctly seen, whilst the earth appears to possess intermediate features. The evidence establishing the actual existence of an atmosphere to Venus, though of different character to that referring to Mars, is yet even stronger, depending principally upon the effects of the refraction exerted by it. The first observation pointing to the considerable horizontal refraction of Venus was an observation by Andreas Mayer, who observed the planet when near inferior conjunction; the horns of the crescent being prolonged until meeting, the dark planet was surrounded by a ring of light, throwing the dark body into relief. Since then the planet, when near conjunction, has repeatedly been seen as a dark body surrounded by a bright ring; whilst the prolongation of the horns of the crescent planet has, under favourable conditions, been noticed by a number of observers since Herschel and Schröter. In 1849 Mädler, from a series of careful measures of the amount of this prolongation, determined that the horizontal refraction necessary to produce this was 43′ 42″. Lyman in 1866, from similar measures, found it to amount to 45′ 18″, and from a further series of observations in 1874 obtained 44′ 30″, on both these occasions seeing the dark body of Venus surrounded by a beautiful silvery ring of light. From these three series of observations the horizontal refraction of the atmosphere of Venus can be considered as determined to be 44′ 30″ with tolerable certainty, and the density of the atmosphere can therefore be determined with a considerable approach to accuracy; for the spectroscopic observations of Huggins, Janssen, and Vogel show that this atmosphere is essentially analogous to our own. The amount of the horizontal refraction, fully one-sixth greater than our own, indicates that the density of the atmosphere of Venus must exceed ours; whilst the dense mass of clouds, by retarding radiation, must render the decrease of temperature comparatively considerably slower, and therefore in computing the amount of the horizontal refraction due to any atmosphere this must be taken into account. It has been shown by the investigations of Laplace, Ivory, Bessel, and Lubbock, that the influence of the law of decrease of temperature on the amount of refraction due to an atmosphere of given surface density is of comparatively secondary importance, so that the uncertainty that must be considered to exist with regard to this point will not prevent a tolerably accurate value for the surface density of the atmosphere being obtained. The great similarity between the physical conditions prevailing on the surface of Venus to those on the earth, in as far as they effect the atmospherical laws, renders it probable that the law of decrease of temperature observed will not be materially different from that on the earth, though, perhaps, slightly slower, whilst the greater density and the number of clouds may, with the greater proximity of the planet to the sun, render the mean temperature of Venus sensibly greater than the earth's. Returning, therefore, to the series of equations (12) to (15) it is necessary to ascertain the horizontal refraction due to the passage of a ray of light through such an atmosphere. The long and complicated nature of this prevents this being effected here, owing to the very considerable amount of horizontal refraction rendering it necessary to take into account a whole series of terms not necessary even on the earth, and the resulting expression for the horizontal refraction contains no less than twenty-three terms. Supposing, however, the value of the constant f to be one-third, or slightly greater than on the earth, owing to the probably higher temperature of Venus, whilst putting $r = 0{\cdot}9$, so as to ensure a slightly slower decrease of temperature, then for a surface density the same as, and an atmosphere of similar nature to the earth, the horizontal refraction on Venus would be—

$$r_0 = 1733'' = 28'\ 53''$$

By assuming Ivory's theory to hold, the result would be 28′ 55″, or sensibly identical, though the rate of decrease of temperature is sensibly different. Bessel's hypothesis gives 29′ 9″, and the assumption of a uniform temperature which gives the maximum refraction 29′ 25″; showing the comparative slight influence of very different laws of decrease of temperature. Adopting 28′ 53″ for the horizontal refraction corresponding to a density equal to that of the earth's, the true surface density of the atmosphere of Venus obtained from the value 44′ 30″ for the horizontal refraction is = 1·546. This corresponds to a pressure (measured in the height of the barometer on the earth) of 1175 millimetres of mercury, or 46·4 inches.

The surface density of the atmosphere of Venus being thus determined, its variation with the height above the surface can be ascertained. At 3·67 miles above the surface the density would be only half that at the surface, at seven miles rather less than one-fourth, and at 11 miles only one-twelfth; which, corresponding to a height of nearly eight miles on the earth, would be probably the limit of the great dense cloud-bearing strata of Venus's atmosphere. At a height of 20 miles, the density of the atmosphere would be only one-hundredth that at the surface, a height of 30 miles would reduce it to one-thousandth, 39 miles to one-ten-thousandth, and 58 miles to one-millionth, where it may be regarded as sensibly becoming evanescent. Under the most favourable condition,—it requiring over 100 miles to subtend one-second of arc at the distance of Venus,—the insignificant nature of the breadth of the layer of the atmosphere of Venus is apparent, and the great difficulty of detecting it manifest. Ninety-nine-hundredths of the atmosphere of Venus lies within one-fifth of a second of arc from the limb of the planet, and is under ordinary conditions absolutely indistinguishable from it; and almost the entire remaining portion lies within half a second of arc, the whole atmosphere becoming sensibly evanescent at a distance of two-thirds of a second.

In occultations of stars by Venus—a phenomenon comparatively rarely visible—as no sensible refraction would ensue from the effects of Venus's atmosphere until the star was within a third of a second of arc from the planet, it would be utterly undetectable at that distance, for the brightness of the planet is so great as to cause an amount of irradiation at the limb far greater than this, so that the star would disappear at the bright fictious limb, as if no atmosphere existed; but at the dark limb of the planet under favourable conditions a beautiful and delicate ring of light might be momentarily detected flashing round the dark body of the planet before immersion.

Considerable discussion has arisen in connection with the observations of the late transit of Venus from the observation of a beautiful silvery ring of light around Venus during ingress, enabling the entire disc of the planet to be seen long before reaching the interior contact. This very delicate ring of light appears to have been considerably brighter than the sun's limb, and forming a fine brilliant very delicate line of light round the portion of the dark planet off the solar disc. Though this appearance seems to have been quite unexpected to the majority of the observers, it is a feature that the results of Mädler and Lyman showed might be expected, and appears indeed to have been recorded by several of the observers during the transits of Venus of the last century. There can be no question but that it arises from the horizontal refraction of the atmosphere of Venus, though not at first sight quite in accordance with the results already obtained with reference to the dimensions of the atmosphere of that planet; for although it has been shown that the thickness of the sensible atmosphere of Venus was only one-fifth of a second of arc, the breadth of this ring of light was estimated to be less than one-second of arc, or probably about two-thirds of a second of arc. It is manifest, however, that this so far from being inconsistent with the small breadth of the atmospheric zone of Venus is exactly what was to be anticipated for the effects of the spurious disc arising from diffraction, which would increase the apparent breadth of the line of light from a thickness of only one-fifth of a second to nearly one second. The superior brilliancy of the line of light around Venus to the limb of the sun is also easily understood, for it is well known that the solar limb is far inferior in brightness to the central portion; but the light refracted in any one direction by Venus is composed of rays from every portion of the solar surface, and consequently would be far nearer the brightness of the central portions of the solar disc than that of limb, and even allowing for a considerable amount of absorption by the passage through the atmosphere of Venus would be still superior in brightness to the limb of the sun. Moreover, as the horizontal refraction of Venus is so far greater than the semi-diameter of the sun, it is evident that the principal rays which pass through the extreme low-lying strata of Venus's atmosphere would fail to reach the earth when the planet was crossing the solar limb, from being refracted too much. It is easily shown that the horizontal refraction of Venus's atmosphere being nearly 45′, the principal solar rays refracted to the earth by Venus's atmosphere, when the planet was crossing the solar edge, would traverse the atmosphere of the planet at a height of nearly six miles above the surface, far above any surface vapours that alone powerfully diminish the solar light and pass through a column of atmosphere at its densest point, not one-half the density of the air near the earth's surface. The solar rays would traverse, therefore, the atmosphere of Venus with scarcely any diminution in intensity, and appear thus brighter than the dusky limb of the sun, and from the contrast with the dull orange or reddish tinge of the solar border appear silvery in hue.

The rapid disappearance of the very delicate line of light after the planet had left the solar disc is a phenomenon not very satisfactorily explainable, and must be regarded as presenting a difficulty. The brilliancy of the light refracted by the atmosphere of course very rapidly diminishes with the increase of distance from the border of the sun, both from a less amount of light being refracted and from its traversing a longer and far denser column of atmosphere; but this by itself is scarcely sufficient to account for its complete disappearance. The presence of clouds or mist in the lower lying strata of the planet's atmosphere is of course adequate to entirely account for the observed phenomena, and may well have been its origin; but still it can scarcely be regarded satisfactory to have to assume the existence of a convenient band of clouds where they were wanted, and nowhere else. Probably the disappearance arose partly from the weakening of the solar light, but mainly from the disappearance of the dark body of the planet, which, when visible, would at once direct the eye to the position of the delicate ring, and by contrast throw it strongly into relief, whilst, when this had to a great extent disappeared, the delicate ring of light would be a very difficult object to detect through a dark glass. That the ring of light had not disappeared is unquestionable; for it was seen perfect by Lyman with great distinctness (without a dark glass) five hours before ingress, and still in part a considerable time after the transit.

A point of some interest in connection with this silvery ring of light was its forming close to the pole of Venus a bright spot, according to the Australian observers, which it has been suggested may arise partly from a greater refraction, owing to the greater density of the colder atmosphere near the poles, combined with reflection from the snow or ice zone of the planet. If the atmosphere was fairly free from cloud towards the poles, something similar to this might be expected; for the Italian observers under Tacchini have ascertained the presence of moisture in the atmosphere of

Venus, showing the presence of seas, and rendering it probable that this planet, like the earth and Mars, must possess arctic and antarctic cold ice zones which, reflecting those rays refracted obliquely on them, would appear as a white spot.

To this refraction of the solar rays by the atmosphere of Venus may perhaps, in great part, the appearance of the planet when near conjunction as a black disc on a lighter ground be ascribed; the dark portion of the planet being defined partly by light refracted, and partly by light reflected, from the planet's atmosphere.

A feature of peculiar interest noticed during the late transit was a faint grey halo around Venus, perhaps ten seconds of arc in breadth, or over one thousand miles, and this appears also to be detectable in the photographs. That it could possess nothing in common with the bright extremely narrow line due to the refraction of the solar rays by the atmosphere of Venus its great size leaves unquestionable; for the atmosphere of Venus must be considered absolutely evanescent at only one-tenth of the breadth of this halo of dull light, whilst its having been photographed shows it is not a matter of contrast. It appears to have been only visible around the portion of Venus on the sun; but bordering the planet on the solar disc the photographs show an extra deposit of silver, probably in some way connected with this remarkable halo, though possibly this last may be, as suggested by Ranyard, of independent origin, it having been found during photographs of solar eclipses. From its small breadth the atmospheric fringe of Venus will exert so small an influence that there will be no need to take its effects into consideration in determining the solar parallax from the results of the late transit, except in considering the time of contact obtained by the use of the spectroscope. In these observations the effects of the refraction of the solar rays by Venus might, perhaps, be found to require attention in determining the instant of contact, and indeed, from these observations, an independent determination of the horizontal refraction might be obtained.

With regard to Mercury, the slight acquaintance with the appearance of this planet renders its probable atmospheric condition less certain than in the case of its two companions, Venus and Mars. Yet the presence of an atmosphere indicated by the observation of Schröter, Mädler, and Secchi, is fully confirmed by the bright, though extremely delicate, ring observed round the planet during its solar transits. Like Venus, the real surface of the planet appears covered with dense masses of cloud usually, and only very rarely can perhaps its actual surface be seen. Supposing the law of variation in temperature to be analogous to that on Venus, the rate of decrease of density on Mercury would be as under, being slower on this planet than any other in the solar system; and thus, were not the planet always in so close proximity to the sun, would render Mercury the most favourable member of our system to study atmospherical effects.

At a height of 11 miles the density would be halved, at 33 miles reduced to one-tenth, at 62 miles to only one-hundredth, at 90 miles to one-thousandth, and at 200 miles become utterly insensible. As under the most favourable conditions one second of arc at the distance of Mercury from the earth is over 200 miles in length, it is evident that on Mercury, as all the other planets, the border of atmosphere is so shallow that on the limb it would be to all intents perfectly undistinguishable. From grey spots that have repeatedly been seen by observers during the transits of Mercury on the planet, it is possible that the atmosphere of Mercury may be of very considerable density, so as from the combined effects of refraction and reflection to produce these otherwise inexplicable markings. And the supposed great density of Mercury's atmosphere receives some support from the rosy tinge of the planet noticed by De la Rue, and the ill-defined border detected by Secchi.

From the formulæ already given, by assuming other conditions to hold upon the planets besides those that have been adopted in the above, any possible atmospheric condition can be easily found, it only being necessary to suitably determine the several constants. It will, therefore, be possible to test any supposed condition by assuming it to hold, and ascertaining whether it infringes any of the theoretical conditions; but it will appear that the two extreme limits of an atmosphere to a planet of the solar system will lie between the two conditions of a uniform temperature and uniform density, the one giving the maximum and the other the minimum depth.

IV. THE POSSIBILITY OF A FUTURE LIFE.*

GREAT, even in this self-applauding nineteenth century, is the amount of man's ignorance, and over much that he is supposed to know there brood not a few clouds of difficulty and doubt. Still it cannot be denied that we have made some progress. In certain directions at least we have been able to trace out the limits within which the knowable is included. Even concerning the origin of things,—confessedly one of the most difficult of all imaginable questions—if we have not come to the exact truth we have been able to narrow the number of conceivable hypotheses and to strike out for ever not a few errors. A century ago it was still possible for any one so disposed to maintain that the earth and the sun as we now see them had existed from all eternity, and would continue such as they are for ever and ever. His views would of course have been denounced by theologians as heretical, and as opposed to the Christian code of Revelation, but on purely scientific grounds and by purely scientific methods he could not have been fairly confuted. Now, from our researches into the attributes of Force, no less than from geological investigations, all this is changed. We know that neither sun nor earth can have existed from all eternity. That both had an origin in time, and that both, in time, must come to an end, are truths as firmly grasped by men of science as is the rotation of the planets. We can no longer conceive of the sun as an infinite source of force which can go on for ever pouring light and heat into space without being exhausted. Nor have we now any longer a warrant for regarding it, according to the clever device of some literal interpreters of the Mosaic cosmogony, as a mere light-bearer—as a body dark and cold *per se*, but specially invested some time after its coming into existence with a luminous and thermic external stratum, or "photosphere." Nor if, leaving earth and sun out of the question, we turn our attention to the distant stars, do we fail there also to recognise marks of a beginning and an end. We see stars of a pure white lustre, others which, like our own sun, burn with a dimmer and yellower fire, and others darker still, which pour out a radiance coppery-red or greenish-blue. We have even within the short space of our scientific annals records of stars which have grown less bright, or which, like the lost Pleiad, have become invisible. We read of stars which have

* The Unseen Universe; or, Physical Speculations on a Future State. London: Macmillan and Co.

suddenly blazed up with an unwonted splendour and have after a time gradually faded away. These phenomena we can only interpret in a manner which foretells the ultimate fate of our earth, of its sister-planets, and of the sun itself.

Again, we cannot fail to perceive that there is in all the forces of nature a gradual tendency to an equilibrium. One day that equilibrium will be attained. The last weight will have reached its level. The last molecule of matter will have satisfied its strongest affinity. All parts of the universe will be equally hot, and no light-wave will cross the regions of space. All these considerations throw a valuable, though indirect, light upon the origin of the universe. They compel us to infer that not merely this or the other heavenly body as now existing cannot have existed for ever, but that the whole must have had a beginning,—that the day was when it had not yet originated.

Now there are, we know, three theories, and as far as we are able to imagine only three, concerning the origin of the universe,—the atheistic, the theistic, and the pantheistic. The first mentioned may be presented in a double form. Either it regards the universe as having existed from all eternity, or it considers it as having been self-created. In the light of modern science neither of these views can be pronounced tenable. Its present activity, as far as we know, gives the lie to its alleged infinite past. Nor can we conceive how a universal and equally diffused medium could at some given point of time suddenly differentiate itself and begin the process of world-building. Did the primal atoms or the ether possess such power it could not have remained latent.

One refuge indeed remains, but this is in its nature doubtful in the extreme. There *may* be some power by which this general tendency to an equilibrium is counteracted, and in such a case the universe as a whole may go on from eternity to eternity. But in all our researches we have hitherto met with no proof of its existence,—no trace of its operations. This may not indeed be held to justify us in absolutely denying the existence of such a power or such a process. But still less shall we be warranted in building upon it, as if demonstrated. The advocates of the atheistic hypothesis can surely not blame us if we here quote the old saying, *De non apparentibus et non existentibus eadem est ratio.*

Such, then, is one of the results of modern physical science that it has rendered the idea of an eternally existing, or self-created, universe well-nigh unthinkable.

But there is also another principle perhaps of even wider bearing than the idea of the conservation of force—a principle which has slowly and gradually attained recognition, and which is known to some as the Law of Uniformity and to others as the Principle of Continuity. This principle is at once valuable and dangerous, safe-guiding and misleading. Without it science would be impossible, but with it grievous errors may gain recognition. The danger of the principle is that every theorist pronounces events or facts that suit his views in perfect harmony with its requirements. "Continuity, in fine," say two able modern writers, "does not preclude the occurrence of strange, abrupt, unforeseen events in the history of the universe." The principle is commonly based upon our almost instinctive tendency to expect that what has happened once will, under the same circumstances, happen again. It is a curious fact that those least willing and able to recognise the law of uniformity, as a general principle, are often most premature in expecting the recurrence of any particular case. The very dog or cat whom you have once fed expects a repetition of the gift whenever you appear.

The authors just quoted place it, however, on a new and strange basis: "Assuming," they say, "the existence of a Supreme Governor of the Universe, the principle of Continuity may be said to be the definite expression in words of a trust that He will not put us to permanent intellectual confusion." We cannot see that the principle—common in its outlines to man and beast—can be fairly connected with the God-idea at all. Nor must it be forgotten that its general application has been strongly insisted on by atheists and pantheists, and as strongly resisted by theists as being inconsistent with the belief in a special Providence and in the efficacy of prayer.

This brings us to the interesting and subtle attempt made by the late Charles Babbage, in his "Ninth Bridgewater Treatise," to reconcile miracles with law by showing that they were not infractions, but cases foreseen and preordained after the manner illustrated in his calculating engine. But it seems to us that our confidence in uniformity is equally violated by any exceptional case, as far as we are concerned, whether such case has been pre-arranged by any higher power or not. If we had the misfortune to see on some particular day a solid block of iron floating in the Thames, our confidence in what is called "law" would be at an end. For we should never be able to foresee whether such a case—pre-arranged if you will—might not recur on any future occasion. If once you show to us solid iron swimming, the idea of specific gravity has no longer for us any meaning. Nor can we refrain from pointing out that events which seem to be departures from uniformity become rarer and rarer in the exact proportion as nations attain a higher culture, and as the facilities for recording occurrences become greater.

Upon the doctrine of the Conservation of Force and upon the Principle of Continuity there has been based a new and original attempt to prove modern science compatible with the teachings of Christianity.

Reconciliation between Religion and Science, we may here ask—wherefore? What legitimate ground is there for a disagreement between them? Let us bring up as arbitrator not the much-talked-of "intelligent foreigner," but that still more mythological being the inhabitant of another world,—if in virtue of recent interpretations of the Law of Continuity our passions are not now supposed to extend over the whole universe.* Let us ask him as touching this supposed need of a reconciliation. Might he not well reply that Religion and Science address themselves respectively to different phases of man's nature; that their ends and their functions are widely distinct, neither being able to supply the place of the other;—that even when regarding the same object or fact, they view it from different aspects.† Might he not tell us that in his interstellar wanderings he had met with a race equal or superior to ourselves intellectually and mighty in science, yet devoid of those emotions and passions which make up the main tissue of normal human life and to which Religion addresses itself? Might he not remind us that in the person of Henry Cavendish our own species had produced at least an approximate instance of such a being? On the other hand, he might describe to us creatures far inferior to man in intellect and incapable of that systematised interpretation of the universe which we call science and yet filled with an intenser moral life.‡ We

* See Sir D. BREWSTER'S "Plurality of Worlds," *passim*. The same author in his inaugural address as President of the British Association in 1850 said:—"If men of ordinary capacity possessed that knowledge which is within their reach and had that faith in science (?) which its truths inspire, they would see in every planet around them, and in every star above them, the home of immortal natures, of beings that suffer and of beings that rejoice, of souls that are saved and of souls that are lost!"

† "The golden side of Heaven's great shield is faith; the silver, reason."—BAILEY'S "Festus."

‡ It will be understood that the term "moral" is not here used in antithesis to "immoral," which it in this sense includes, but in contra-distinction to "physical" and to "intellectual." It is well known that the religious world are generally more lenient to the man of strong, though ill-regulated, passions, than to one like Cavendish, whose moral nature is, so to speak, atrophied, and

do not think that it would be difficult to find examples of individuals,—perhaps even of races,—tending in this direction. There are, then, in mankind as it actually exists two types more or less strongly marked. On the one hand, we find the men of intellect, of speculation—using the term in its German acceptation—objective in their aims, and seeking every where for law and sequence. On the other hand, we perceive the men of moral, emotional life, subjective, craving in all things, and above all things for personality, will, purpose. The authors of the work before us recognise the existence of these two classes—or, better, two tendencies—under the names of followers of the "How" and disciples of the "Why,"—though they fail to see much which this distinction involves.

We have, then, before us two variables, each of which may be increased or decreased without producing any corresponding or even inverse augmentation or diminution in the other; nay, either of which might conceivably be wholly wanting. Is not this proof sufficient of mutual independence, isolation? Our imagined visitor from Procyon or Vega might surely then declare that between tendencies both perfectly legitimate, yet at the same time mutually independent, collision cannot have arisen without the aid of much depraved ingenuity. Disputes between Religion and Science—or, in other words, between man the believer and man the discoverer—are about as rational as war between two sets of beings inhabiting different media, and each incapable of existing in the sphere of the other.

But unfortunately the Christian records, like those of various so-called Heathen systems, open with a cosmogony. Or let us rather say unfortunately ecclesiasticism will persist in treating this magnificent poem as a literal history. Upon this cosmogony, coupled with certain passages in the Hebrew and Greek Scriptures capable of being treated as astronomical, physical, geological, and biological utterances, it has founded a code of natural philosophy. To reject this code, or to bring forward either deductions or facts at variance therewith, it has anathematised as "infidelity." *Hinc illæ lacrymæ!* The quarrel, if quarrel it be, has been begun, not indeed by Religion, but by those who took upon themselves to speak in her name and defend her supposed interest. On the other hand, the followers of Science, not content with insisting upon their own rights, executed from

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whose life is free from all vices because the energies of his being are totally absorbed in speculative research. Said an eminent German Pietist, Count Zinzendorf:—"Between devotion and lust there is but one step."

time to time counter-raids, more or less injudicious—of which Dr. Tyndall's Belfast Address is one of the latest instances.

Now the way to end this fruitless feud is the simplest thing possible. Let both parties enter into a treaty of "let be for let be" as it is called in homely phrase. Let divines cease to judge scientific theories by theological standards, and to preach against the researches of Darwin. Let men of science, in turn, cease to proclaim everything non-existent and inconceivable which they cannot weigh in the balance or observe with the spectroscope. Let each, Religion and Science, be content to pursue its own objects by its own methods, and for the reconciliation of difficulties let both wait in faith. Such a settlement was first hinted at by Giordano Bruno and was advocated in full by Galileo in his celebrated letter to the Dowager Grand-Duchess Cristina of Tuscany. Here he contends that the Scriptures have no claim to be received as a scientific revelation; that on astronomical and physical questions,—to which we might now add geological and biological,—they convey merely the notions current at the times when they were written.

If we accept this view all difficulty is at an end. But there are many who seek to bring about a reconciliation, not by relegating Religion and Science each to its own department, but of bringing them inco harmonious inter-dependence. Many have been the attempts made in this direction, agreeing perhaps in nothing save the unsatisfactory character of their results. Some have extracted systems of science from Hebrew roots under high-pressure philology, a mental diet analogous to the soup obtained from old bones in Papin's digester. Others have sought reconciliation by denying or explaining away the most elementary truths of science. As an instance of this method we may recall the existence of a book,—some few copies of which may have escaped the hands of the trunk-maker and the butterman—entitled "Errors in Electricity, Magnetism, and Chemistry," by a F.R.S. In this perhaps the only really noteworthy passage was the author's solemn confession that he had never made, individually or by proxy, a single experiment in any of the sciences for which he undertook to legislate.

The writers of the book before us are of a very different grade, and bring to their task far higher qualifications. They are beyond dispute men of science, well acquainted with the latest achievements of modern research and willing to accept them as true. They receive for instance the nebular hypothesis concerning the origin of the heavenly bodies,—a view not merely anathematised by divines, but which even a philosopher like Sir David Brewster could scarcely name with patience. They admit the still more dreaded doctrine of organic evolution, and do not even positively insist on the necessity of a special intervention for the origin of man. This is much. But they go still further; they distinctly formulate the view that man is not merely entitled, but even bound, to push back as far as possible all such Divine intervention.

We must beg our readers to make especial note of an admission so pregnant.

Nevertheless the writers are theologians even more decidedly than philosophers. They are at home in the sphere of the divine, and regard everything instinctively from his point of view. They are familiar with the Bible and with the views of commentators. They are versed in the mythology of all nations. "The existence of a Deity who is the Creator of all things" they "assume as absolutely self-evident,"—one of their critics remarking in reply that "it is evident" is merely another mode of saying "I do not know how to prove." But for all this we doubt whether religious orthodoxy will accept them as its duly authorised advocates and representatives. Their Deity, though tri-une, has a less personal and more pantheistic character than Christianity recognises. Nay, if we do not misunderstand the authors, they seem to surmise that the creation of the universe may have been effected, not by the Supreme himself, but by subordinate though still mighty agents. On the origin of Evil, also, their views can scarcely be accepted as orthodox. They regard it as "eternal," not confined to that planet which we inhabit, and they remark that "the dark thread known as evil is one which is very deeply woven into that garment of God which is called the Universe." They urge also that:—"The matter of the whole of the visible universe is of a piece with that which we recognise here, and the beings of other worlds must be subject to accidental occurrences from their relation with the outer universe in the same way as we are. But if there be accident, must there not be pain and death? Now these are naturally associated in our minds with the presence of moral evil."

Without entering into any criticism of the views thus expressed, we ask how they are to be reconciled with certain ordinary and supposed essential doctrines of Christianity? The Scriptures, it is generally conceded, teach that man was created immortal and perfect, in a perfect world, and that death, sin, and "all our woe" only entered through his disobedience. This is certainly not the place for a formal theological discussion, but it does seem to us that the authors, in surrendering the Fall of Man, have rendered a very questionable service to Christianity. This is a snare into which reconcilers are very apt to fall. They look attentively at the two theories, interests, parties, or the like which they would fain bring into accord. As they gaze the difficulties seem to vanish, and with a sincere Eureka, they turn to inform the world of their success. When, lo! it is found that they have harmonised, not what actually exists, but two creations of their own. They have made the boot fit the foot by liberally trimming and cutting both one and the other!

The immediate purpose of the work before us may be learned from its second title, "Physical Speculations on a Future State." The authors seek to show that immortality—a new life after the phenomenon known as death—is not impossible, and is not contradictory to, but rather in harmony with, the law of continuity. They declare that the "great mass of mankind have always believed in some fashion in the immortality of the soul; but it is certain that we may yet find disbelievers in this doctrine who yet retain the nobler attributes of humanity." The strength of this disbelieving minority they consider "has of late years greatly increased, until at the present moment it numbers in its ranks not a few of the most intelligent, the most earnest, and the most virtuous of men." To these accordingly they address themselves in the opinion that they are probably "unwilling disbelievers, compelled by the working of their intellects to abandon the desire of their hearts only after many struggles and much bitterness of spirit." Now we cannot, certainly, conceive the man or woman who does not crave for a further life, even for immortality.

Annihilation has no charms. We have read the "Logic of Death" of a celebrated sceptical writer, but it satisfies no one.* Yet even annihilation may not be the greatest conceivable evil. Our authors must know that many of the class for whom they write have been driven to the rejection of a future life less by the working of their intellect than by the revolt of their moral faculties. Such men,—we expound here without advocating their opinions,—much as they may shrink from an utter extinction of consciousness,

* "The Martyrdom of Man," by the late Mr. Winwood Reade, another book which seeks to reconcile man to the prospect of the utter and final cessation of consciousness at the moment of death, we must unhesitatingly declare one of the saddest works ever written.

look with far more loathing upon the future life of ordinary Christian eschatology—to wit, eternal torment for the many and a never-ending Sunday-school celebration for the favoured few. To such minds this prospect appears not merely inconceivable, but a libel on the Creator; and upon them, therefore, the argument of our authors, ingenious as it undoubtedly is, will be utterly thrown away. We must further call to mind that immortality, though an essential, is not a peculiar doctrine of the Christian religion, of which even its absolute demonstration furnishes no conclusive evidence. But no such demonstration is here attempted. We are reminded that the universal longing for immortality affords a presumption at least of a future life in which this yearning may be gratified. An eccentric friend of ours whose life has proved a failure, argued that this very craving is an evidence *against* immortality. But if it is an evidence of a future life at all, it must be taken in proof of a future of universal happiness. To implant such a craving, and then to fulfil it with an immortality of misery, would be more merciless than not to fulfil it at all.

The work before us, like certain recent legislative measures, is of a decidedly "permissive" character. It deals in the "may be" rather than in the "must be." It takes the conceivable as tantamount to the actually existing, and asks, "Is it not hazardous to deny?" where, to say the least, it is equally hazardous to assert. There is, we learn, around us, or among us, an "unseen universe." In density it bears about the same proportion to the interstellar, luminiferous ether which this does to ordinary molecular matter. From this unseen universe, or second ether, the visible universe and all that therein is originally proceeded, and into it they are being gradually re-absorbed. Beyond this second ether there is a third finer still, forming a second unseen universe, and beyond this another and another, each less dense than the preceding. These unseen universes, or at least the first of the series, receive the force which the ether loses. That such a loss takes place, the authors conclude from Struve's statement that there appear to be fewer stars of the tenth magnitude than should be visible if all their light reached us unabsorbed. The portion which thus disappears on the way is, they assume, taken up by the ether and handed over to the first "unseen universe." Of the properties and powers of this unseen universe we know, of course, nothing. When, therefore, the authors ask us in substance, "Why may it not" be the scene of the life to come, we are certainly unable to give any direct reply. For all we can show to the contrary, this second universe, if it exists at all, may contain anything. In this unknown and hypothetical region, a spiritual body—if such a term may be logically applied to what seems after all merely a matter extremely rarefied—is being elaborated for each of us after this fashion. Every thought or emotion which passes through our minds occasions, as is now generally admitted by physiologists, a certain molecular action in our brain. This action disturbs the ordinary luminous or interstellar ether, and a portion of such disturbance, like a portion of the light of the stars, is handed over to the second ether or unseen universe. By degrees, an organised structure is thus built up, though devoid of consciousness. When at last we die, our consciousness is, in some mysterious manner, transferred to this second body, and we begin a new life, though retaining our personal identity, and being mindful of all that has occurred to us in our present state of existence.

All this, the reader must carefully bear in mind, is supposed to happen, not by miracle, not in virtue of any direct interposition of God, but in obedience to the ordinary "laws of nature." Certain questions, therefore, arise, the answers to which are in the highest degree doubtful. That a molecular disturbance in the human brain may affect the ether, may be transmitted to an unseen universe, and may there produce physical effects, are propositions which we are not prepared to deny. But how are these effects to be localised and kept separate? We know that the light-waves thrown off from any illuminated body are capable of producing chemical effects upon certain substances, and of thus stamping the image of such body upon a duly prepared surface. But for this purpose care must be taken that the light-waves thrown back by other bodies do not interfere. If we place a sensitive paper, not in a camera, but in the open air, we do not obtain a landscape, but a blot. The undulations from each object do not select some part of the paper to impress themselves upon, but act indiscriminately upon the whole surface. What in the unseen universe is to play the part of the camera? How are the impressions emanating from the brain of each of the thousand millions of the human inhabitants of the earth—to say nothing of its brute population, and of the possible intelligences dwelling in other heavenly bodies—to be sorted out and compelled to act, each on the one spot required, and nowhere else?

The authors, indeed, make an attempt to meet this difficulty, an attempt so feeble that we marvel how it can proceed from men who elsewhere show themselves capable of reasoning most ably. They say, "We are as much puzzled by what takes place in our present body as we can be with respect to the spiritual. Thus let us allow that impressions are stored up in our brains, which thus form an organ connecting us with the past of the visible universe. Now thousands, perhaps even millions, of such impressions pass into the same organ, and yet by the operation of our will, we can concentrate our recollection upon a certain event, and rummage out its details along with its collateral circumstances to the exclusion of everything else. But if the brain, or something else, plays such a wonderful part in the present economy, is it impossible to imagine that the universe of the future may have even greater individualising powers? Is it not very hazardous to assert this or that mode of existence to be impossible in such a wondrous whole as we feel sure the universe must be?"

This illustration, we urge, is totally beside the question. An organism must exist before it can receive and individualise the impressions which occur. But here the organism has to be built up by the impressions themselves. Or, returning to our former illustration of the photographic camera, we know that it, with a duly prepared plate, can individualise into distinct images the light-undulations proceeding from bodies placed before it; but can we conceive of these undulations creating the camera and the plate on which they are to be recorded? For such a process we can find in the known universe no analogy. What it is possible for us to imagine is, we submit, quite unimportant. It may, indeed, be "very hazardous" to deny the possibility of anything. But is not the position of the authors still more hazardous? Before expecting our assent they are bound to produce some argument far more cogent than "why may it not?"

But the authors' entire line of reasoning, touching the unseen universe, is based upon doubtful assumptions, and is beset with difficulties little less formidable than the one we have just pointed out. Let us return to the fundamental fact, or alleged fact, of the small number of stars of low magnitudes. The calculation of Struve took for granted that the stars are uniformly distributed throughout space. This is a fallacy. The heavenly bodies within our range of vision belong for the most part to a single group, which appears to lie like an oceanic islet in vast starless regions. The interstellar spaces contain not merely the ether, but in all probability diffused gases, which may absorb the light of the more remote stars. The effect of the upper regions of our own atmosphere has also to be taken into account. It is further supposed by some that space may contain in considerable numbers the dead bodies of extinguished suns, which, of course, may eclipse to us the light of stars placed behind them. Hence there is absolutely no proof that any light is absorbed by the ether. The supposition of such a process is merely one out of several ways of accounting for certain facts, and it is in no manner forced upon our acceptance.

But supposing such an absorption to take place, is there any necessity that the force thus gained is handed over to an assumed second ether? None whatever: this force may, quite as probably, be restored to the visible universe in manners not dreamt of in our philosopy. An acute though sarcastic critic has pointed out that it may be transformed into other kinds of motion, or may serve in making up atoms, and even that the doctrine of the conservation of force may be only an approximate truth, and that the light, if any, absorbed by the ether, may be simply lost.

Among the possible objections to their views—it is curious, by the way, to note the kind of arguments which theorists put into the mouths of imaginary opponents,—we find it conceded that the unseen universe of the authors will find room for an immortality, not of man only, but also in brutes. How far down the organic series immortality is supposed to extend they do not say. Plants, indeed, have no brain and possibly no memory, but may not the same be said of many forms of animal life? We certainly should not object to the widest extension of a future existence. Flowers and trees are always pleasant companions, which is vastly more than can be asserted of animals, our own species by no means excepted.

But if we admit the unseen universe of our authors, and if we suppose our spiritual bodies duly elaborated there, and rendered the seat of the consciousness that has left its decaying tenement in this world, there is another difficulty remaining. How is our immortality in such future state to be guaranteed? Shall we be able to exert force without transformation of material, or, in other words, to create force out of nothing? This we have always held to be the prerogative of one only Being. But if not, will not the preservation of our life depend upon conditions strictly analogous to those by which it is governed in our present state? Nor is this all: by hypothesis our spiritual bodies will stand to their surroundings in a very similar relation to that which our present frames bear to the medium which they now inhabit. But if so, we may extend the argument which the authors have applied to the inhabitants of other orbs in the visible universe and we may in like manner infer that in the life to come we shall not be exempt from accident, and from its consequence—death. Possibly this point has been foreseen by the authors. Perhaps they may argue that if we die in unseen universe No. 1, we shall live again in unseen universe No. 2, and so on *ad infinitum.*

Now we are not aware that this supposition involves any new difficulties. Our transfer from No. 1 to No. 2 might be effected on the very same principles as our removal from this world to No. 1, and a virtual immortality, or at least continuity of consciousness, might thus be maintained. But such a future life would be reft of half its attractions. Is not the greatest pang of death the separation from those we love? Is not the hope of an eternal reunion the main source of our yearning for a life beyond the grave? But if such future life consists in a series of removals from one unseen world to another, the prospects of reunion become very slender.

The authors devote a chapter to discussing the possibility of intelligences superior to man existing in the visible universe. This inquiry seems at first glance irrelevant. But it is to some extent justified by the consideration that if such exist it might be "at least conceivable that man may be at death drafted off into some superior rank of being connected with the present universe, and thence ultimately removed into a new order of things when the present universe shall have become effete." The conclusion they reach is in the negative. Science tells us of no superior order of beings connected with the present universe, and in virtue of the law of Unity, they pronounce the conception of such beings altogether untenable." That beings analogous to man may exist in other worlds they do not dispute. Turning from the "verdict of science to the sacred writings of the Jews, we find," they continue, "that one grand idea which pervades the whole of the Old Testament is man's absolute superiority and practical sovereignty over all created beings whom he can perceive otherwise than with the *mind's* eye."

Now, that science has hitherto revealed to us no being superior to man is undeniable. But is it not "very hazardous" to conclude that none such may exist in other portions of the present universe? The authors admit beings "analogous to man," but a certain degree of analogy is not inconsistent with immense superiority. We know that the meteorites which from time to time fall to our earth have been found to contain no element hitherto unknown to us. We admit that the spectroscope points out to us in the sun and the fixed stars simple bodies, which, with the exception of helium, form part and parcel of our planet. We know that the most distant heavenly bodies are influenced by gravitation, light, heat, and probably by electricity and magnetism. But all this takes us a very little way towards ascertaining the nature of the living beings which may inhabit those orbs. To take an illustration from our own planet. We might find, say in Borneo, minerals very similar to those obtained in Australia. But would that warrant us in concluding that in Borneo the highest existing form of animal life was of the marsupial type?

Nor do we think that the Scriptures, Jewish or Christian, convey any decisive information on the subject. Concerning the population of other worlds in the present order of things, they are, we believe, totally silent. The sovereignty of the globe which they attribute to man, and the supremacy over all other visible and tangible beings thereon, are of a very doubtful nature. In how few parts has he succeeded in extirpating even the larger carnivora! How powerless is he in dealing with mosquitoes, locusts, and still more with those minute organisms believed to be the *materies morbi* of yellow fever, plague, and cholera! It may also be questioned whether the Scriptures do not grant the presence on the earth of beings heterologous to man, and possessing superior attributes. The idea of witchcraft is based upon the existence of such beings. Yet that the Bible recognises witchcraft not as an imposture but as a reality is admitted by theologians of repute. John Wesley declared that to give up witchcraft was tantamount to giving up the Bible. The General Assembly of the Church of Scotland denounced the repeal of the penal statutes against witchcraft as a national sin.

If, however, the authors consider man as belonging to the highest type of beings in the present order of things, they are of a different opinion as regards the unseen universe. This has its own population independent of the phantoms of men and animals who may have been transferred to it from worlds like our own. If we only suppose the unseen universe not separated from our world by distance, but interpenetrating its molecules, and thus being at once near us, and yet till death at least infinitely remote from us, we may here be reminded of some of the weird speculations put forward by the late Lord Lytton in his "Zanoni" and "A Strange Story." The "Dweller on the Threshold," and other beings, hideous or lovely, there represented as inhabiting the realms of space, are not spirits, but composed of matter so sublimated as to escape the senses of man in his ordinary condition.

The authors give it as their opinion that the "nebulous beginning and fiery termination of the present visible universe are "indicated in the Christian records." The "fiery termination" assigned to the world by the ordinary Christian eschatology does indeed present a certain superficial resemblance to what science points out as the probable ultimate catastrophe. But we must remember that certain sects of ancient philosophers held in like manner that the earth would ultimately be destroyed by fire, and that these views had been promulgated before the date of the New Testament. As to the "nebulous origin" of the universe, we may ask how is it, if such be the teachings of the Scriptures, that both divines and philosophers of orthodox tendencies failed to perceive it, and denounced the hypothesis of Laplace as essentially atheistic? Thus Sir D. Brewster in his "Plurality of Worlds" declares it "equally at variance with reason and Scripture" (p. 171).

We regret that we can continue no further our examination of a work so important in its subject—so thoughtful and so suggestive. To deal fully and fairly with all the issues here opened up would require the study of years. Widely as we differ from the authors, we are bound to pronounce it a book which few men of cultivated minds can read without pleasure and profit. Its main conclusions we are, however, unable to accept. Its arguments, solid and satisfactory at first sight, fade away in our grasp like elfin gold, and leave us empty-handed and disappointed.

V. THE CHANNEL TUNNEL.

By F. C. Danvers, Assoc. Inst. C.E., &c.

It is generally accepted as a geological fact that the high mountains and deep ravines that separate divers nations, and have been adopted by them as natural territorial boundaries, were developed, in some cases by gradual changes in the surface of the globe due to natural causes, whilst in others they are attributed, apparently with good reason, to violent convulsions of Nature, by the action of which the plane of the globe has been caused to undergo great changes. However these inequalities in the earth's surface may have been brought about, the necessities of man often demand that the inconveniences caused by them to free transit should be overcome, and to this end the services of the engineer are called into request—it may be to bridge over a river or chasm, to tunnel through a lofty mountain, under a river or across a channel of the sea. Further also the engineer may be called upon to unite two seas by means of a water communication, to divert the channel of a river, or to guard the coast against the errosive force of tidal action. Taking a general view of the engineering profession, it may be broadly stated that its chief and loftiest operations are undertaken with the view of accommodating the earth's surface to the need of mankind, either by counteracting the physical effects of forces that were in operation in ages past, or by neutralising present active forces by opposing to them means of resistance designed and based upon a knowledge of natural laws and physical science.

Many works of past ages may fairly claim to be classed in the general list given above, of which the most celebrated in modern times, at least, will readily present themselves to the mind of the reader. There is now, however, a grand work in contemplation, for uniting England and France by means of a railway tunnel under the English Channel, which in point of boldness of design and extensiveness—both from a material and commercial point of view—cannot claim a rival.

It is generally admitted by geologists that at one time England was connected with France by land, and formed a Peninsula. To all appearances also the separation from the Continent has not been effected by any violent convulsion, but by the slow and long-continued action of the waves. It was pointed out in an article that appeared in the "Quarterly Journal of Science" for April, 1872, on the "Geology of the Straits of Dover," that, very likely, at one time when the land stood at a higher level, and before the sea had eaten out the Straits, 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 English side, the Wimereux and the Slack on the French side.

The probability of this country having once been a peninsula, and of the land connecting it with the Continent being washed away by the action of the sea, was carefully considered by Verstigan, so long since as 1673, in a pamphlet dedicated to King James. He compared the identity of the strata, the composition of the cliffs, the similarity of their lengths, and arrived at the opinion that the surface had been gradually worn away by the action of the sea, and not by disruption. In the following century M. Desmarest wrote an essay on the same subject, arriving at the same conclusion; and in 1818 the question was philosophically treated by Richard Phillips, F.R.S., in an elaborate essay which he read before the Geological Society.

Although, however, there appears to be little doubt that the Straits of Dover have thus been formed by the continued action of natural causes, it by no means follows that at no previous period had the even lay of the strata between England and France been disturbed by volcanic or other violent terrestrial forces. Between Folkestone and Cape Gris-Nez there is the Varne in mid-channel of a formation belonging to the Portland beds, which are of a much older series than the deposits to be found on either coast, and this of itself should prepare geologists to anticipate some irregular trend in the direction of the strata between the two shores. It may be that the irregularity is not of sufficient extent at the proposed line of passage materially to affect the projected work, but where an evident upheaval of the lower strata is known to occur within so short a distance of the selected site, it seems but reasonable to suppose that the effects of the disturbance by which it was caused may have influenced the geological formations within a few miles at least on either side of the fault. We shall, however, refer more particularly to this subject when treating of the geological examinations of the Channel bed.

It is not proposed in this article to enter into any detail regarding all the alternative schemes from time to time projected with the view of spanning more conveniently the narrow channel that now separates us from our neighbours, but it may render the present examination of the subject more interesting to refer briefly to the various devices proposed for obviating or lessening the inconveniences felt by those who suffer in crossing between England and France from the too common complaint of *mal de mer*.

From M. Thomé de Gamond's publication on this subject we learn that the establishment of some means of direct communication between England and France was first proposed at the latter end of the last century. The earliest project of which there is any account on record for crossing the Channel by a tunnel was proposed by M. Mathieu, a

French mining engineer, in the service of the Department du Nord. His scheme was conceived at the close of the last century. It was laid before the First Consul in 1802, and plans illustrating the project were for several years exhibited, first at the Luxemburg Palace, then at the School of Mines, and afterwards at the Institute. Mathieu's project consisted of a subterranean way formed of two tunnels, one on the top of the other, forming in section an uneven line, the highest point being in the centre of the Channel, and inclining in opposite directions towards England and France respectively. The lower tunnel was to act as a canal to drain off any waters that might enter through leakage, and from which it would discharge at either end into drainage reservoirs. In the upper tunnel was formed a paved road, lighted by oil jets, and traversed by a service of diligences drawn by horses, which was the only known method of conveyance in those days. It is not shown exactly how the entrances to these tunnels were to be approached on either side, but they must necessarily have been situated at a great depth below the surface of the ground. For ventilating the tunnel, as well as for use in its construction, M. Mathieu proposed to erect circular iron chimneys rising above the surface of the water, and secured in position by masses of rock deposited at their bases.

When the Peace of Amiens was declared the author of this scheme thought that his project would at once be carried out. It was introduced to the notice of Fox on the occasion of his visit to Paris during the short peace that followed, and was received by him with favour: he regarded it as a most efficacious means of assuring peace between England and France. It is stated that Fox spoke on the subject to Napoleon, who exclaimed "Oh! c'est une des grandes choses que nous pourrous faire ensemble."

Subsequently one Dr. Payerne proposed to form a level bed at the bottom of the sea by depositing concrete, and upon this to construct a tunnel by means of diving bells. MM. Franchot and Tessier proposed to form a passage through a tube of cast-iron laid on the bed of the sea, but they appear to have suggested no means for securing a level surface. M. Favre designed a submarine tunnel having an outside casing of wood or sheet iron, and resting on piers of iron lined with brickwork in order to overcome the unevenness of the sea bed. In 1850 M. Ernest Mayer proposed a submarine tunnel between the South Foreland and Cape Gris-Nez, and some years ago M. S. Dunn laid out a plan for constructing a tunnel under the water, of which the principal novelty consisted in a cylindrical protector, fitted with a shield in front, within which the sections of the tunnel were to be fitted together.

The most indefatigable projectors amongst our neighbours has, however, been M. Thomé de Gamond. Whilst making a geological examination of the shores of the Channel in 1833, this engineer was first struck with the idea of making a way of communication between England and the Continent, and he accordingly proceeded to make a series of soundings between Calais and Dover. M. de Gamond's first project was for the submersion of an iron tube in sections, laid at the bottom of the Straits of Dover, and lined inside with masonry. The obstacle which soon presented itself to his mind was the difficulty of levelling the bottom of the sea in order to form a bed for the tube, and this operation alone he estimated at £12,000,000 sterling, after which the cost of the tube and approaches was set down at £6,400,000; so that the total probable cost of the project was £18,400,000. This scheme was no sooner finished than it was abandoned. In 1836, M. de Gamond gave his consideration to the construction of a bridge across the Channel, taking the line from Calais to Ness Corner Point, a line shorter by about two miles and a half than that between Dover and Calais. Five different plans of bridges, in granite, in stone and metal combined, and skeleton iron structures, were elaborated by him during a period of two years. The scaffolds for commencing the works were to be supported by buoys of great size, held in their place by metallic shrouds, fixed at the bottom of the sea to strong moorings. In making the foundations for the piers, it was proposed to drive piles into the ground by manual labour, the workmen being in a water-tight chamber at the bottom of the sea. Of all the different projects for this structure, the one for a granite bridge obtained most favour amongst scientific men. This structure was designed to be 131 yards broad, the arches, 162 yards wide, were to be built on piers 52 yards long and 131 yards broad. All the arches being 57 yards high above the sea could be passed under by most vessels. There was, however, to be one movable arch to admit the passage of vessels having still loftier masts. The greatest depth of sea between these points is stated to be 197 feet, and many of the arches crossing this depth would have been 126 yards in height from the sea-bed to the key-stone. The total cost of this structure was estimated by its designer at 160 millions sterling, but by others at 200 millions. At the advice of Messrs.

Stephenson, Brunel, and Locke, to whom all the plans were submitted in detail, this project was soon abandoned; and in conversations which M. de Gamond had on the subject with those engineers—and especially with Mr. J. Locke—an idea was suggested of improving the means of communication between the two countries by narrowing the Straits of Dover by throwing out piers from the two opposite shores, to be carried as far out to sea as possible, and establishing a steam communication between the two piers by means of an enormous raft moved by steam.

We defer for the present from entering further into detail regarding M. Thomé de Gamond's more matured schemes for a Channel tunnel whilst we refer briefly to other projects that have from time to time been put forward with the view of facilitating the Channel passage between England and France. Taking these in the order of their importance, we may perhaps devote a few lines first in considering plans that have from time to time been proposed with a view to counteract the motion of vessels in a rough sea, and so add to the convenience of travellers. The oldest invention of this sort of which we have been able to trace any record is mentioned in a curious old book, published in 1677, and named "Aero-Chanilos, or a Register for the Air." In it a sort of chamber is described, in which air might be rarefied or condensed, or otherwise changed for the use of invalids, so that they might have change of air without leaving home. Of this same chamber the writer says:—"Possibly, if the same might be made use of on board ship, it would (with the additional contrivance of a chair or bed, hung after the manner of a sea-compass) prevent that very troublesome affection whereto 'fresh men' are subject, called sea-sickness, and consequently become very serviceable to such whose emploiments engage them to undertake voyages into very remote parts, and there to reside far from their own countries."

The earliest patent on the subject appears to be that of Pratt (1826), in which a spring mattrass was fixed on a "swinging frame." A later invention by De Manara, in 1853, proposed to attach balloons to seats, in such a way as to keep them always horizontal. Another, by Ritchie, in 1866, describes a platform, resting on water in a tank, and having its edges attached to the edges of the tank by mackintosh, or similar fabric. Differing from all the above was a plan, patented in 1866, by M. Simpson, in which the body of the patient is firmly fastened down to the ship itself. In 1854, L. Wertheimher patented some improvements in apparatus for preventing sea-thickness, the first of which consists of a movable platform, to which chairs or couches may be attached. Connected with the platform is the piston rod of a steam cylinder, to which steam is admitted by a four-way cock, which may be opened and shut by a self-acting contrivance, so that when the ship sinks into the trough of the sea, steam is admitted beneath the piston, and the platform is caused to rise; on the contrary, when the vessel rises over the crest of the wave, steam is admitted above the piston, and the platform descends, and thus a motion opposite to that of the vessel is obtained. Another arrangement consists of three cylinders, one placed forward and two at the after part, connected with each other by pipes. The second part of the invention consists of a platform or chair, which is supported by a bracket attached to an upright shaft, which shaft passes through a hollow standard. The upper part of the shaft carries a rack in which gears a pinion, fitted with a handle, and a rising and falling motion is given to the platform by moving the handle to and fro. Or the platform may be moved by a perpendicular shaft or lever attached to a pinion gearing with a toothed rack. In the third modification, the effect is attained by interposing elastic bodies between the person and the deck.

In order to avoid sea-sickness, Mr. J. Scarth, in 1869, designed a swinging cot, which he hung from four hooks—two at each end—whilst, in order to counteract the tendency to extreme oscillation, he attached vulcanised india-rubber springs, or accumulators, below the cot, in the exact centre, directly perpendicular from the hooks by which it was hung, and this principle he proposed to apply to individual berths. In 1833, Sir J. Herschel designed a somewhat similar contrivance, which, he says, proved perfectly successful, one chief difference between his and Mr. Scarth's plan being that instead of india-rubber bands Sir John employed cord or pack-thread. Subsequently, in 1869, Sir John Herschel proposed, for swinging cots, to transfer the whole coercing power, operative in deadening the effects of oscillation, at once to the point of suspension; and so doing away with the necessity of attachment of any kind to the walls or floor of the cabin, whether by friction bands or by elastic straps, and this he proposed to accomplish by hanging the cot from the roof by a light but rigid iron framework, having a stiff ball-and-socket attachment lined with compressed felt or other similar material, in order to offer sufficient frictional resistance to oscillation.

A floating cabin was designed by M. Alexandrovski, which, instead of being attached to a pivot, as in the Bessemer saloon, floated in a kind of tank placed amidships between the engines. This invention, it is said, was tested by the Grand Duke Constantine, in his capacity as head of the Naval Department, with a perfectly satisfactory result. All efforts to shake the cabin proving utterly unsuccessful, the pitching as well as the rolling motion of the vessel being completely counteracted. A combination of both the Bessemer and Alexandrovski plans was also recently proposed by Mr. A. Allen, of Scarborough, the object of which was to give a steady saloon cabin or gun deck at sea. This cabin was to be constructed of two spherical segments, the outer segment or dock being fixed in the ship, and the inner segment being floated on a film of water in the dock, like one basin floated in another, the inner one or cabin being maintained at its proper height by being supported on a centre pillar passing up a conical passage in its centre, and which would allow 20 degrees of roll on either side, or 40 degrees in all.

We have now to consider the several plans proposed for conveying trains across the Channel by huge ferry steamers. In the Exhibition of 1862, a proposition by Mr. Evan Leigh for conveying trains across the Straits of Dover on board large ferry boats or rafts was exhibited by means of models, but it does not appear that his project ever found any substantial supporters.

In 1865 a company was formed to place on the Channel a line of steamers from Dover to Calais, so large that the railway trains should run on board them, and there bodily remain to be run out again on the other side after crossing the Channel, and which, by their magnitude, it was expected, would ride over the waves without putting the passengers to the slightest inconvenience. The miseries hitherto inevitable to this passage had, it was remarked, been chiefly entailed by the restriction to boats of a size proportioned to the shallowness of the water on either shore. The new pier at Dover has overcome that objection on this side, and it was rumoured that the French Government had granted a concession for the requisite extension of that at Calais on the other side. Such a scheme was indeed before Parliament in 1866, and was originated by Mr. J. Fowler, whose proposal was to construct steamers one-third longer than the vessels on the Kingstown and Holyhead service, and their decks were to be roofed over so as to protect the trains during the passage. The proposed dimensions of these vessels was—Length, 450 feet; breadth, 57 feet; with 12 feet draught of water. A Bill for effecting improved communication across the Channel by this means was three times before Parliament. In 1869 a project, having the support of Messrs. Fowler, Abernethy, and W. Wilson, contemplated the construction of very extensive harbour and dock works on either side of the Channel in addition to the construction of the large ferry steamers above referred to, and in November of that year an influential deputation from England laid the scheme before M. Gressier, the Minister of Public Works in France, by whom it was favourably entertained.

In consequence of the very defective state of the accommodation afforded by the Channel steamers plying between this country and the Continent, the Council of the Society of Arts, in 1869, offered the Gold Medal of the Society and the large Silver Medal of the Society for the best and the second best model of a steamer which should afford the most convenient shelter and accommodation to passengers on the deck of the vessel crossing the Channel between England and France. The size of the vessel was not to exceed in tonnage and draught the best vessels then in use between Folkestone and Boulogne. Seventeen models were sent in competition, which were referred to a Committee consisting of Lord Henry G. Lennox, M.P., Seymour Teulon, Rear-Admiral Ommanney, C.B., F.R.S., Admiral Ryder, E. J. Reed, C.B., Capt. Boxer, R.N., C. W. Merrifield, F.R.S., H. Cole, C.B., and Captain Tyler. From the report of this Committee it appears that three of the models only conformed to the conditions laid down by the Council, but none of these, in the opinion of the Committee, presented sufficient novelty or merit to justify the award of the medal.

In this year also Captain Tyler, R.E., in compliance with instructions from the Board of Trade, visited the French and English coasts with a view to preliminary enquiry as to the improvements which might be effected in the means of communication between the two countries. He reported that considering the restrictions as to dimensions imposed by the circumstances of the harbours and the various conditions of the service, the steamers employed in the Channel service were admirably constructed for the work they were required to perform. With regard to Mr. Fowler's proposal for large steamers and improved harbour accommodation on both sides of the Channel, Captain Tyler observed that it was a question whether it would be worth while to ferry the railway carriages as well as the passengers across the

Channel; but that the main features of an improved harbour at Dover and a new harbour south of Cape Gris-Nez were sound, if means could be found for meeting so great an expense as the works would entail. Captain Tyler then proceeded to consider the practicability of improving the existing harbours so as to fit them for a service of larger vessels, after which he directed attention to the bolder schemes which had been put forward from time to time for avoiding the use of steam vessels altogether by the construction of bridges, or tunnels, or tubes over, under, or in the bed of the Channel, with or without islands, piers, or air-shafts, so as to connect the railway system of England directly with that of the Continent.

After briefly referring to the plan proposed by M. Mathieu, Captain Tyler proceeded to observe that, after a series of geological investigations, M. Thomé de Gamond also proposed, in 1856, the construction of a tunnel, and his propositions were submitted to the examination of a scientific Committee by order of the French Emperor in that year. That commission appears to have come to the conclusion that it was desirable to test his investigations by sinking shafts and driving short headings under the sea at the expense of the two Governments. Mr. Low, an English engineer, also laid his plans for a tunnel before the Emperor in 1867, and Mr. Hawkshaw, whose attention had been for some years directed to the subject, caused a trial boring to be sunk on each side of the Channel in 1866 in order to test practically the result of his geological investigations. Mr. Remington published a plan for a tunnel in 1865, and deposited plans and sections of it with the Board of Trade. And amongst the names of other proposers or projectors in this direction may be enumerated Messrs. Franchot, Tessier, Favre, Mayer, Dunn, Austin, Sankey, Boutet, Hawkins Simpson, Boyd, and Chalmers. Of these various projects those which have of late made the most progress are the bridge scheme of M. Boutet, and the tunnel scheme presented under the chairmanship of Lord Richard Grosvenor, with Messrs. Hawkshaw, Brunlees, and Low as engineers on the English side, assisted by Messrs. Talabot, Michel Chevalier, and Thomé de Gamond on the French side. The result of the deliberations of a French commission, which was appointed by the Emperor, and presided over by M. Combes, the Director General of the Ecole des Mines, to inquire into this last-mentioned scheme, were on the whole favourable as regards the geological and engineering parts of the project, though the members of the commission were divided as regards its financial prospects, the president and two members attaching more importance to the "utility and grandeur of the undertaking," and three other members looking at it from a more strictly economical point of view. The General Council of Pont-et-Chaussées, presided over by the Minister of Public Works, to whom the matter was afterwards referred, were unable, "upon the documents submitted to them, to decide on the probability of success of the tunnel under the Channel," and considered that "if from political considerations, the undertaking should be considered useful, the Government should follow up the investigations at their own expense."

In reporting on the different projects put forward with a view to improving the means of communication between England and France, Captain Tyler, referring to the last mentioned project, remarked :—

"In this scheme it is proposed to commence by driving preliminary driftways through the grey chalk, at a great depth below the bed of the Channel, between a point near Dover and another point near Calais; as it is conceived that this material would be easily cut through, and would not be likely to present insuperable difficulties from the influx of water; whereas Mr. Remington selects the line from Dungeness to Cape Gris-Nez, in order to avoid the chalk, and the fissures which he fears to encounter in it, and to work in the Wealden formation, which would, he believes, afford a greater chance of success.

"In the case of M. Boutet's bridge scheme, an association has been formed for making experiments, two small bridges have been built in France, and arrangements are made near St. Malo for a third, a mile in length, to be constructed in two spans of half a mile each. The Emperor Napoleon visited the works of M. Boutet, on a site granted by the French Government, and His Majesty is stated to have expressed himself favourably with regard to the project. This bridge is intended to cross from Dover to Blancnez, and is advocated, in a paper forwarded on the 27th June to the Board of Trade as (1) being less costly than a tunnel; (2) occupying less time in construction; (3) giving no trouble in ventilation; (4) avoiding the danger of sudden inundations.

"Mr. Charles Boyd has forwarded to the Board of Trade a pamphlet containing his proposal for a 'marine viaduct' from Dover to Cape Gris-Nez, constructed with iron girders on 190 towers, 500 feet apart, and 500 feet above the sea, and he estimates the cost of such a bridge at £30,000,000.

"Mr. Hawkins Simpson has addressed the Board of

Trade on the subject of working a submarine tunnel on a pneumatic system, which he has termed his 'Eolian system,' for which he claims cheapness, expedition, superior ventilation, and greater utility.

"Mr. Alexander Vacherot has submitted to the Board of Trade a scheme on which he has several years been engaged, and which he laid before the Emperor of the French in 1856, for 'laying on the bed of the sea a tunnel made or formed of concrete, so as to form, when completed, a monolith.' He would construct it on the shore and 'draw it down to its place in sections.' And he considers that greater economy and security might thus be obtained than by the other methods that have been proposed."

After reviewing these several projects Captain Tyler, though unable to convince himself of the feasibility of any bridge scheme, considered "that it might be wise to test the practicability of a tunnel scheme by means of preliminary driftways. It is probable," he said, "that, even if any of them should hereafter be carried out in practice, they could not go forward otherwise than under the supervision of, and a previous guarantee from, the two Governments; and obvious that, as at least 10 or perhaps 15 years may elapse before they could be made available for traffic, improvements in the shape of more convenient and larger steam vessels are required in the meantime for the better performance of the service."

In the spring of 1870 Vice-Admiral Sir Edward Belcher read a paper before the Institution of Naval Architects wherein he expressed himself favourable to a proposed ferry scheme, and to the practicability of constructing ferry steamers suitable for the service. The outbreak of war on the Continent put an end to Mr. Fowler's project; but, in 1871, we find M. Dupuy de Lôme at the head of a similar undertaking in France. His project was for, first, the creation at Calais of a maritime station, with 16 feet 6 inches depth of water at the lowest tides, and about 30 acres in area, connected with the shore by an iron railway jetty, making a junction with a branch of the Northern Railway—and open to the sea by an entrance 260 feet wide, accessible in all weathers, and at every stage of the tide; secondly, the construction, for crossing the Channel, of steam vessels of large dimensions and of great power, embracing all of the most important conditions of speed and comfort, and able to carry 30 passenger carriages or goods wagons. These vehicles would be placed on a double line of rails running fore and aft; they would be shipped and unshipped by the assistance of a system of inclined planes leading to three landing stages of different heights, and alongside which the ferries would run according to the state of the tide. A paper on this subject was read by M. Dupuy de Lôme before the French Geographical Society in 1873. In 1871 the Society of Arts appointed a Committee to consider and report how far the existing means of crossing the Channel could be improved. In their report several modifications in existing vessels, and new boats about fifty feet longer than the existing boats, were suggested, but an opinion was expressed that no large measure of improvement could be effected in the Channel passage unless with vessels of much larger size, which would involve, in the first instance, considerable improvements in the French harbours of Calais and Boulogne, and subsequently the extension of the low-water pier at Folkestone.

Mr. Fowler again brought forward his ferry scheme in 1872, and Mr. Hawkshaw at the same time was supporting an alternative design for an improvement of the existing means of communication by the establishment of a service of vessels of considerable size, to which the existing harbours might be adapted without an excessive cost.

The Bill for Mr. Fowler's project passed the House of Commons in 1872, but it was thrown out in the Lords by a very small majority. From this date comprehensive schemes for a railway ferry across the Channel appear to have been abandoned, and in their place projects were started, the one by Mr. Dicey and the other by Mr. Bessemer, for constructing steamers of special and novel design, on board of which passengers would be enabled to undertake the passage across the Channel without fear, no matter how bad sailors they might be. Both of these vessels have now been constructed, and as they have been described in former pages of this journal it is not necessary to enter into any detailed description of them on the present occasion.

Having now made a rapid review of the general question of improved communication between England and Europe, and of the several projects that have from time to time been proposed for the purpose, it remains only to enter somewhat more fully into detail with regard to the great tunnel scheme which, to all present appearances, is about to be commenced. In doing this we shall purposely avoid all reference to the probable traffic and commercial results of the undertaking, and shall confine our investigations to the proposed line of route, the geological features of the strata to be pierced, and the general engineering features of the work.

Two principal schemes have been proposed for a tunnel under the Channel, the one by M. Thomé de Gamond between Eastwear Bay near Folkestone, on the English side, to Cape Gris-Nez on the French coast, and the other, which has already been referred to, and with which the names of Hawkshaw, Brunlees, and Low are associated, from between St. Margaret's Bay, near the South Foreland, to a point between Sangatte and Calais. The former of these, it was subsequently ascertained, would pass through a number of different beds. In mid-channel on this line there are two shoals, known as the Varne and the Ridge, which belong to the Portland formation, the same as that to which the cliffs at Cape Gris-Nez belongs, and under it are the Kimmeridge Beds. These rocks dip to the north-west, and it is supposed that somewhere between Cape Gris-Nez and the Varne the Kimmeridge clay wholly disappears, whilst somewhere to the west of the Varne and nearer to the English coast the Portland beds also disappear and are overlaid by the Wealden series. Probably the Hastings beds immediately overlie these, and above them again is the Weald clay; but where the outcrop occurs, or what is the thickness of the various beds, is not known. M. de Gamond's tunnel, therefore, as was pointed out by Mr. William Topley in an article on the "Geology of the Straits of Dover," which appeared in this Journal in April, 1872, would pass through a number of different beds; but how many is uncertain, 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 it would intersect these very near the coast; and it cannot be supposed 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 again it might intersect the Portland, and finally it would cut through the Kimmeridge. The various beds that would be intersected by this tunnel are of very different characters; some are highly porous and some wholly impervious.

The tunnel which it is proposed to take from St. Margaret's Bay to near Sangatte will, it is supposed, go entirely through the chalk without flints. The following particulars of this project are taken from a paper on the "Channel Tunnel" read before the Society of Arts on the 18th March, 1874, by Mr. W. Hawes, F.G.S., and from a "Statement by the Committee of the Channel Tunnel Company (Limited)" published in 1874.

In 1865 Sir John Hawkshaw began his practical researches into the nature of the strata beneath the Channel, which confirmed the theories of the geologists, threw new light on the subject, and put the question of a submarine tunnel in a position to be seriously discussed and considered by the public. Before that time he had given the subject much consideration, but in that year he caused careful geological surveys and investigations to be made of the Channel, and afterwards, in conjunction with the late Mr. Brassey and Mr. George Wythes, had borings sunk on each coast. Subsequently, by means of apparatus contrived for the purpose, he examined the bottom of the Channel all across in a great number of places, and raised specimens of the sea bed for examination, by which it appears to have been satisfactorily established that the actual position of the chalk across the Channel is very nearly identical with that deduced from previous enquiries, and its unbroken continuity placed almost beyond doubt. Its thickness, determined by deep borings on both sides, is proved to be above 500 feet below high-water mark, with an ample thickness of the lower or grey chalk between the bottom of the sea—which is nowhere more than 180 feet deep—and the crown of the tunnel, as well as between the bottom of the tunnel and the green sand or water-bearing strata underlying the grey chalk. The tunnel has been placed by the engineers at such a level that the depth of strata over it will nowhere be less than 200 feet, and this depth, which is desirable for security, will permit the railway approaches to be formed with not unfavourable gradients.

In the opinion of M. de Souch, Inspector-General of Mines in France, the line selected by the engineers of the Channel Tunnel Company is the only one which presents chances of success, and is the only rational one. The Commission appointed by the late Emperor of the French to examine this project reported that there was every reason to believe that the chalk formation extends under the Channel between Dover and Calais and that "the thickness of the grey chalk gives a certain latitude for the maintenance of the tunnel in the same direction, even where the level of the bed of the sea may be subject to some undulations; and they believe that the existence of any great fracture in the chalk is very improbable."

On the other hand, we have the opinion of so high an authority as Mr. Joseph Prestwich, Vice-President of the Geological Society, who does not entertain such sanguine views as to the suitability of the chalk stratum for the construction of the proposed tunnel, as will be seen from the following remarks extracted from a paper read by him before the Institution of Civil Engineers in December, 1873 :—

"The chalk formation, which everywhere in the south-east of England and the north-west of France underlies the Tertiary series, has a maximum thickness of from 1000 feet to 1300 feet; but, as much of it has in this area been worn away or denuded before the deposition of the Tertiary strata, its actual thickness in the district under notice varies from 300 or 400 feet to 800 or 1000 feet. The upper beds consist of almost pure carbonate of lime, easily worn by water, and being also soft and fissured they are readily permeable. But the Lower Chalk or Chalk Marl contains so large a proportion of argillaceous matter and silica in a state of fine division that some beds pass almost into a clay, and when unbroken and compact very little water can pass through them, and then only with extreme slowness, though this will increase under pressure. But although a small bore-hole or even a shaft may often be carried through a considerable thickness of Lower Chalk and no water obtained, the occurrence of fissures is too uncertain to render it a reliable medium over a large area. In some cases where the Lower Chalk comes to the surface, and is more broken and fissured, the quantity of water it yields is very large, as in the instance of the Tring cutting described by Robert Stephenson, where the discharge was at the rate of 1,000,000 gallons per day; or at Folkestone, where the town water supply is obtained from the Lower Chalk of the adjacent downs. On the other hand, the Chalk Marl in France and Belgium acts as an impermeable stratum in stopping the passage of water from the very permeable Upper Chalk into the underlying coal measures; and no water was found in it either at Kentish Town, Harwich, Southampton, or Calais; but the diameter of the bore-holes by which they were traversed were very small. At Calais one spring was met with at a depth of 70 feet in the Upper Chalk, and the water was brackish, showing communication with the sea. Nor must it be forgotten that wells in the Chalk under London have to be carried or bored to depths of from 10 to 300 feet before meeting with water-bearing fissures, or else headings have to be driven in search of one. Again, the escarpment of the North Downs and that of the chalk hills of Wiltshire, Oxfordshire, and Buckinghamshire are fringed with numerous springs, which issue at their base. These springs, although thrown out generally by the Chalk Marl, are apparently not always at the top of it, but often low down in the deposit, and they constantly wear their point of issue from a higher to a lower level. Whatever the level of the spring the water of course passes through all the superincumbent portions of the Chalk Marl. This very commonly impermeable character of the Chalk Marl has given rise to the hope that it might prove compact enough for a submarine tunnel under the Channel between Cape Blancnez and the South Foreland; but when it is considered that such a work would have to face the risks arising from the lateral passage of the inland springs and from the chance fissures so common to calcareous rocks communicating with the sea, it is feared that the difficulties would prove to be of a very formidable nature. It is to be observed that in the Channel the chalk is frequently bare, besides being unprotected by any overlying strata."

The foregoing observations demand our full respect, but, on the other hand, the possibility of tunnelling beneath the sea without being exposed to an irruption of sea water is shown in the submarine galleries of some mines in Cornwall, Cumberland, and elsewhere. In a treatise on Mines and Mining by Mr. Price, published in 1778, he treats especially of mining under the sea, and refers particularly to the freedom of water in such works.

It is probable, and the engineers anticipate that at the shore end of the Channel Tunnel, especially in constructing the shaft through the upper strata, a considerable quantity of water will be met with, but not sufficient to prevent the execution of the work, where pumping power of any magnitude could, if necessary, be applied. It is believed, and there appear to be reasonable grounds for such belief, that as the work attains a greater depth in the chalk, and especially after the lower or grey chalk is reached, the quantity of water will diminish, and that in mid-channel it will be less than at the sides.

The geological features of this project having now been duly considered, it remains to give some particulars of the engineering nature of the work. The distance across the Channel at the point selected is about 22 miles, but as considerable approaches will be necessary on either shore, in order to reach the level of the tunnel entrance, the entire scheme will embrace about 31 miles of railway. In the first instance shafts will be sunk on each shore to the depth of 450 feet below high-water mark, and, from the bottom of these, driftways will be driven for the drainage of the works whilst in progress, and for its permanent drainage after completion. The tunnel, which will be very similar to an ordinary railway tunnel having two lines of rails, will commence 200 feet above this driftway, and will be driven at an inclination of one foot in 80 to the junction with the drainage driftway, and then at a gradient of one to 2640 to the centre of the Straits, where the tunnel from the English shore will meet that driven exactly in the same manner from the French shore, and, being united with it, will complete the submarine railway under the Channel. The drainage will be from the centre of the tunnel to either end.

In the execution of this work a driftway, 9 feet in diameter, will first be carried right through, and this will afterwards be enlarged to the full size of the tunnel. The problem of the execution of the tunnel in a reasonable time has been simplified by the invention of tunnelling machinery, and the machine of Mr. Dickenson Brunton, which has been tried on a practical scale by the company in the lower or grey chalk, has been quite successful. The machine works like an augur boring a hole in wood. The chalk is cut off in slices, which break up and fall upon an endless band, which loads them into wagons behind the machine. The apparatus was tried by the Company at Messrs. Lee's Cement Works, Snodland, near Rochester, in the grey or lower bed of chalk, such as underlies the Channel. It made a driftway of 7 feet diameter, and it advanced at the rate of from a yard to a yard and a quarter per hour. At this rate it would only require two years to drive a driftway of 7 or 9 feet diameter from one side of the Channel to the other, a machine being started from each side. The cost of driving a heading would consist—1st, of tunnelling machines, pumps, and pumping engines; 2nd, the hand labour, which would not be considerable, as the machine requires but few hands to work it; and, 3rd, interest on the capital expended during the execution of the work, which might last two years or more. Taking these three elements of expenditure into consideration, and according to the calculations of experienced contractors, it has been found that the driftway could be executed for £800,000, if it required only two years to make it.

As soon as the driftway was completed the success of the undertaking would be assured. It would furnish the necessary data for an exact estimation of the cost of the whole work and the time necessary for its execution. In fact, all that would be necessary would be to enlarge the driftway to the dimensions of an ordinary railway tunnel. It has been estimated by some engineers and contractors of considerable experience that after the driftway was finished, four years' time and four millions of money would complete the work, including the junctions with the English and French railways on either shore. Sir John Hawkshaw and the engineers associated with him, however, think it prudent to double this estimate both of time and of cost, at least until the preliminary work shall have given them the necessary data for a more exact estimate of the duration and cost of the work.

Preliminary steps to test the practicability of the project are about to be put in hand without further delay, for which purpose an English and a French Company have been promoted to carry out experimental works on either side of the Channel. An Act has been passed by the British Parliament during the past Session to enable the English Company to acquire the necessary lands at St. Margaret's Bay, and it is understood that a *projet du loi* has also been passed in the French Senate to confer the necessary powers on the French Company. The works to be undertaken by these companies consist of sinking two shafts—one on either coast—about 150 yards deep, from which an ordinary mining drifting about half a mile long will be driven under the sea. This work would be a true beginning of the proposed permanent tunnel. Its cost is estimated at £160,000, of which sum it is understood the two companies will find £20,000 each; the Rothschilds of London and Paris have each undertaken to find similar amounts; the Chemin de Fer du Nord will contribute £40,000, and the London, Chatham, and Dover and South-Eastern Railways will respectively subscribe £20,000.

It may now be confidently anticipated that the commencement of this great work will not be delayed. In the foregoing account we have purposely refrained from entering into detail regarding means of ventilation and other minutiæ of construction. The progress of the work will, however, be closely watched, and we shall hope from time to time to give further particulars of its advancement in the chronicles of engineering in this journal.

VI. THE ARCTIC EXPEDITION OF 1875.

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AT frequent intervals during the last 300 years, the Arctic Circle has been crossed, and portions of the vast space within it have been explored. Foremost among the nations which have prosecuted such researches are the English, Russians, and Dutch, while the Americans, Austrians, and Norwegians have latterly done good work in the same fruitful field. Within that charmed circle of eternal snow and ice the difficulties to be encountered are prodigious, and the attractiveness of the exploration cannot compare with that of tropical or temperate portions of the earth's surface. "A region of thick-ribbed ice, the home of the walrus, seal, and bear, uninhabited by man; a stranger almost to flower and tree, whose forest giant is the dwarf birch, a tree 13 inches in height; the resting-place of iceberg and floe; the seat of land which is wrapped in a mantle of frozen water, and of seas whose solidity equals that of the rocks; a spot on which for four months the sun never shines, where the cold freezes the mercury, and the thermometer in March ranges 70° below zero." Such is this inhospitable region which it is the design of the Arctic Expedition of 1875 to open up to the further knowledge of mankind.

The causes which have induced enterprising maritime nations to risk the lives of their bravest men, and to expend vast sums of money, in endeavours to penetrate an unknown region, have been very various. In the case of most of the earlier and many of the later expeditions, the principal cause was commercial interest rather than discovery for discovery's sake. Hence Milton was led to remark that the early attempts at Arctic exploration "might have seemed almost heroic, if any higher end than love of excessive gain and traffic had animated the design." The various causes which have induced Polar research are the following:—(1). The search for a north-west passage from the Atlantic to the Pacific, in view of closer commercial relations with India and China. (2). The search for a north-east passage to the Pacific, in view of closer commercial relations with the north of Russia, Siberia, China, and India. (3). The search for fresh whaling grounds. (4). The search for gold mines. (5). The exploration of the Hudson's Bay Territory and of Northern Siberia, for the furtherance of commerce. (6). The search for the lost *Erebus* and *Terror*. (7). Rewards offered by governments or individuals. (Thus a century ago a reward of £20,000 was offered by Parliament for the discovery of the north-west passage; also a reward was offered for the first man who should sail beyond the 89° N. lat; and in 1818 Parliament offered £1000 to the first man who should sail beyond 83° N. lat., £2000 if he sailed to 85°, £3000 to 87°, £4000 to 88°, and £5000 to 89°; again £10,000 was offered for the solution of the mystery which so long overhung the fate of *Erebus* and *Terror*). (8). The prosecution of scientific research, including geographical surveys and deap-sea soundings. (9). In the case of our expedition of 1875, the acquirement of "important scientific and commercial results" . . . "the importance of encouraging that spirit of maritime enterprise which has ever distinguished the English people," . . . "the discovery of the conditions of land and sea within the unknown area, and the investigation of all the phenomena in that region in the various branches of science." (10). The discovery of the North Pole.

We have put last on our list, and would fain have omitted altogether, that which many will regard as the primary object of Arctic research; but in truth the expression "discovery of the North Pole" is scarcely more than a phrase. The North Pole is in some respects of less interest than the magnetic pole discovered more than 40 years ago. The North Pole of the earth is merely a point terminating the axis of rotation of the earth. It is a spot on the earth's surface where the altitude of the sun is equal to its declination; where the sun seems to revolve in a circle parallel with the horizon, and not to rise on one side of the horizon and set on the other, as with us. Nevertheless, if we like to make the discovery of this spot on the earth's surface, our goal in the Arctic race, well and good; the phrase may be retained, because we well know that the pole cannot be reached without a considerable exploration of the land lying around it, and this is the real present object of Arctic research. It is a useful phrase because you cannot explain to the nation in general, and to the sailors of the expedition in particular, the results geographical, magnetical, electrical, thermal, hydrographical, geodetical, meteorological, geological, botanical, zoological, and ethnological, which may be expected to accrue from the observations of the expedition; while the phrase "discovery of the North Pole" has in it a stimulating ring, a something tangible and real—albeit it conveys less meaning to nine people out of ten than if we spoke of the discovery of the Fountain of Jouvance, or the Garden of the Hesperides. To our enterprising spirits there is in the term something which excites and stimulates: the discovery of a spot of earth upon which man has never trod, the attainment of a goal for which many have started, and which none have ever reached; a something that shall call forth all the heroism, energy, and endurance of which a great nation is capable. Let us look back at our list of objects sought to be accomplished by Arctic expeditions, and see how many now remain to us. (1). The North-West Passage was probably discovered by the survivors of the *Erebus* and *Terror*, and was certainly discovered a few years later (1851) by Sir Robert McClure, who found a strait connecting Melville Sound with the Continental Channel, and who was thus enabled to sail from the Atlantic to the Pacific. (2). The North-East Passage has been proved to be altogether unpractical, if not altogether impossible. (3). The highest practicable whaling grounds have been discovered. (4). The gold mines have been proved to be fabulous, and the imagined gold ore to be iron pyrites. (5). The Hudson's Bay Territory and Northern Siberia have been fully explored. (6). The numerous expeditions sent out between 1847 and 1857 to search for the crews of the *Erebus* and the *Terror* have done their work. For a while their search ended as did the search of the sons of the Prophets for Elijah who had been taken from their sight; afterwards they found the records of the lives of brave men, the graves of heroes. (7). Of the rewards offered by Parliament, some have been won, others remain as yet unclaimed. (8). The prosecution of scientific research remains with as wide a field as ever, although much has been done. (9). The results of our present expedition, which we trust we may be able to give in October, 1876, and among them let us hope (10) the discovery of the North Pole. We suppose that a recent writer in the "Times," when he speaks of solving the "Polar mystery," means finding out whether the North Pole is surrounded by a great open sea, or whether it is situated on land; but Polar research, as we have shown above, means much more than the discovery of this spot on the earth's surface. In fact this is of less than secondary importance among the results of our expedition. As a means to an end it is alone important, for this one spot of earth cannot be discovered without the pre-discovery of ten thousand scientific facts.

Before we enter into an account of some of the more notable attempts to explore the Arctic Regions, let us open a map and glance at the region in question. Looking down upon the North Pole of the earth, we notice at a distance of about 1500 miles from it (about as far as from the North of Scotland to the South of Spain), the circumference of the Arctic Circle. Travelling east from Iceland, we observe that the latter just touches the northern border of that island, passes through the north of Norway, Lapland, and Russia, the south of the Gulf of Obi and Northern Siberia, passing a few miles to the north of the narrowest portion of Behring's Straits; continuing through Alaska, it cuts the northern portion of the Great Bear Lake and Cumberland Sound, the middle of Davis Straits, and so through the south of Greenland to Iceland again. Grouped around the Pole at distances of from a thousand to twelve hundred miles, we have the great northern limits of Europe, Asia, and America. The great space of more than one and a half million square miles marked "unexplored Polar Region" is entered from the Atlantic and Pacific Oceans by three channels—to wit, from the Pacific Behring's Straits, from the Atlantic, Davis Straits, and the great stretch of ocean extending between Greenland and Norway. If we finally narrow our gaze to the innermost circle, 10° (690 miles) from the Pole we notice that through more than half that circle no land penetrates; indeed it is an altogether unknown region. Above the north-west corner of Greenland, we have President's Land within 400 miles of the Pole, while above Novaya Zemblya we have Petermann Land, also within 400 miles of the Pole.

Iceland, the northern portion of which, as we have seen, is on the edge of the Arctic Circle, was discovered in the year 860 A.D., by a Norwegian Vikingr, and fourteen years later it was colonised by Norsemen. About the year 890, Audher, a Norwegian, gave an account of his voyages to Alfred the Great, and there can be no doubt from his story that he was the first to double the North Cape and to enter the Arctic Circle. Nearly a century later Erikr Rauthi, an Icelander, having been convicted of manslaughter, was banished from the country, and determining to pass a portion of his time of banishment in exploring the Northern Seas he fitted out a vessel and soon came in sight of the east coast of Greenland. He then doubled Cape Farewell, and landed on the western coast of Greenland, which he spent three years in exploring. In 986 he returned to Iceland and reported so favourably of the new-found country that he persuaded a large body of his countrymen to sail with him, and a colony was forthwith founded. The colony was divided into East and West Bygd (Ice., *byggia*, to build): the former ultimately contained 90 farms and 4 churches, and the latter 190 farms, a cathedral, and 11 churches. The colony was christianised towards the close of the tenth century, and sent tribute to Rome in the form of the tusks of the morse. Early in the fifteenth century the colony ceased to exist. It had undergone various misfortunes: a plague had broken out among the inhabitants; an irruption of the aborigines (Esquimaux) had disturbed the settlement; and finally a hostile fleet laid waste the country. During the occupation of Southern Greenland by the Icelanders America was discovered.

In 1492 Columbus discovered the West Indies, and in 1498 Vasco di Gama doubled the "Cape of Storms." The English merchants were cut out from any share of the commerce of the Indies, for the Pope had assigned the eastern route to the Portuguese, and the western to the Spaniards; hence it was that the Merchant Venturers of Bristol determined if possible to achieve a *North-West Passage* to the Pacific. In the reign of Edward IV. a Venetian merchant named Giovanni Gabotto had settled in Bristol, and according to Hakluyt his celebrated son Sebastian was born in that city in 1467. Letters patent were presented to John Gabot and his sons by Henry VII. permitting them to sail under the English flag, and to set up the flag in every newly discovered land. In 1497 John Gabot discovered Labrador and Newfoundland. He appears to have made two voyages in search of a north-west passage, and to have penetrated to the 58th degree of latitude. This was the first attempt of the English to make the North-West Passage. In 1527 one Robert Thorne presented an address to Henry VIII. praying that ships might be sent out to the North in quest of new lands, since other nations had already made important discoveries in the direction of South, and West, and East. He argued that if the seas above Newfoundland be navigable it would be possible to sail due North, pass the Pole, and descend to the equinoctial line near to the Spice Islands, and thus to outstrip the Spaniards and Portuguese. Two ships were equipped for this purpose by Henry VIII.; one was lost in a storm, the other got no further than Newfoundland.

An attempt was made in the reign of Edward VI. to sail to India by a north-east passage, and a company of merchants (afterwards incorporated and styled "The Fellowship of English Merchants for the Discovery of New Trades," but now commonly known as the "Muscovy

Company), sent out three vessels sailing under the directions of Sebastian Cabot, Grand Pilot of England. The crews of two of these vessels, together with the Admiral, Sir Hugh Willoughby, were frozen to death near the mouth of the Northern Dwina. The crew of the third vessel returned overland to England from St. Nicholas on the White Sea. The Muscovy Company, nothing daunted, fitted out a pinnace named the *Serchthrift* in 1556 for the search for the North-East Passage. Sebastian Cabot, although in his 88th year, was an active member of the company, and directed the new expedition, the command of which was given to Stephen Borough. "On the 27th of April," says Borough, "being Monday, the Right Worshipful Sebastian Cabot came on boord our pinnesse at Grauesende, accompanied with divers gentlemen and gentlewomen, who, after they had viewed our pinnesse and tasted of such cheere as we could make them aboord, they went on shore giving to our mariners right liberall rewards, and the good olde gentleman, Master Cabota, gave to the poore most liberall almes, wishing them to pray for the good fortune and prosperous successe of the *Serchthrift* —our pinnesse. And then at the sign of the Christopher hee and his friends banketed and made mee and them that were in the company great chere; and for very joy that he had to see the forwardness of our intended discovery, he entred into the dance himselfe among the rest of the young and lusty company: which being ended hee and his friends departed most gently, commending us to the governance of Almighty God." During this voyage Borough discovered the Strait between Novaya Zemlya and Vaigat Island sometimes called the Strait of Kara; but he did not prosecute his researches further to the East, having seen "a terrible heape of ice approach neere." The Dutch stimulated by the English attempts to effect a north-east passage sent out an expedition from Amsterdam in 1594 commanded by William Barentzoon, who made three voyages, the third of which was by far the most important. During this voyage he discovered Spitzbergen, and sailed round the north-west end of Novaya Zemlya. In the winter of 1596-97 Barents, with sixteen of his countrymen, found themselves entirely hemmed in by ice at the north of Spitzbergen, and they were compelled to land and build a house out of drift-wood and portions of their ship. In June, 1597, they set sail for the South, and a few days later Barents died. The survivors, after undergoing frightful hardships, reached Kola, in Lapland, after a voyage of 1600 miles in open boats through a stormy and icy sea, and were picked up by a Dutch vessel.

In 1871, no less than 278 years since the crew of Barents wintered in Ice Haven, the old house and numerous relics were found by a Norwegian captain, among them the clock and some books. The interior of the house was exactly in the condition described by Barents nearly three centuries before.

Seventy-six years after Sebastian Cabot's attempt to effect a north-west passage, Sir Martin Frobisher persuaded some London merchants to once more embark in the enterprise, and three small vessels were fitted up for the purpose and victualled for twelve months. They started on the 17th of June, 1576, and passed by Greenwich, "where we shotte off our ordinance and made the best show we could; Her Majestie (Queen Elizabeth) beholding the same, commended it, and bade vs farewell with shaking her hand at vs out of the window. Afterward she sent a gentleman aboord of vs, who declared that Her Majestie had good liking of our doings, and thanked vs for it, and also willed our captaine to come the next day to the Court to take his leave of her." Frobisher speedily doubled Cape Farewell and bore on to the West until he came to land, which Queen Elizabeth named Meta Incognita; a little to the north of this a large bay was found which was named "Frobisher's Bay." When Frobisher returned to England he brought with him not only the hopes of a north-west passage but a piece of black stone, which being examined by Baptiste Agnello, a quack, was found to contain a minute quantity of gold. Whereupon the London merchants again fitted out ships, and Frobisher was ordered to load them with the supposed gold ore, and to defer discovery till some more convenient time. The ships returned and the ore was found to be worthless, and for nine years the London merchants refused to send out a fresh Arctic expedition. In 1585, however, several vessels were fitted up and put under the command of John Davis with a view to the discovery of the North-West Passage. He discovered the straits which bear his name in 1585, and gave the name of Cape Walsingham to a prominent Cape on the East side of Cumberland Island. This is just below the Arctic Circle, and Davis is the first Englishman who reached so high a latitude on the American shore. Cape Chudleigh, the most northerly point of Labrador, was also discovered by Davis and named by him after Mr. John Chudleigh. In 1607 Henry Hudson was entrusted by the Muscovy Company with a small vessel for the discovery of the North-West Passage. There seems to be but little doubt that in the course of this voyage Hudson discovered Jan Mayen's Island, which is generally (but on weak grounds) said to have been discovered five years later by a Dutchman after whom it was named. Hudson also penetrated to a latitude of 80° 23′. In a second voyage Hudson discovered the river which bears his name. Hudson's Bay was discovered in 1609. In 1612 William Baffin and James Hall visited the West Coast of Greenland in order to search for a gold mine which was said to have been worked by the Danes. Twenty four years later the Danes sent out two vessels to the same spot with the same object, but they returned laden with iron pyrites. Baffin discovered the Bay which bears his name in 1616. Mr. Markham points out the curious fact that although the Dutch had established a flourishing whale fishery in Davis's Strait, the passage of the "Middle Pack" (that is the great mass of ice in the middle of Baffin's Bay) was not even attempted between 1616 and 1817. Baffin had himself passed the Middle Pack and reached the extreme north of the bay, but no one had followed in his footsteps, until in 1817 two whalers successfully passed the Middle Pack, and found an abundance of whales in the "North Water." A number of whalers now pass the Middle Pack every year.

We have seen to how great an extent our knowledge of Arctic geography is due to the Muscovy Company: another company had now arisen in the very heart of the Arctic district. A charter was granted in 1670 to the Hudson's Bay Company, giving them right to all the territories drained by rivers falling into Hudson's Bay, on certain specified conditions, one of which was the prosecution of geographical research. Expeditions were frequently sent out by the Company both for surveying the coast line and for the exploration of the interior. In 1741, Captain Middleton, who was a sound scientific man, well versed in nautical astronomy, was appointed by the Admiralty to explore Hudson's Bay. He penetrated to the extreme north, and there found a frozen strait, which seemed to cut off all communication with more northern seas. A few years later the Admiralty offered a reward of £20,000 for the discovery of a north-west passage, but the attempt, although often made, was fruitless.

In the other direction the Russians had often attempted a survey of the Siberian coast, but no European ship has ever been forced beyond the Sea of Kara. Our knowledge of the Polar Asiatic shore from Behring's Straits to Novaya Zemlya is due to the Russians. The region is barren and desolate beyond description, and the ground is frozen for many feet below the surface. The most northern points of Asia have never been doubled, although Peter the Great had himself given directions in 1725 that the whole coast of Siberia should be explored. The most important discoveries due to the Russians are Behring's Straits and the Islands of New Siberia off the mouths of the Lena. Baron Wrangell, in 1820-3, explored a good deal of the North Siberian coast by means of sledges, with which, drawn by teams of dogs, he once travelled no less than 1530 miles in eleven weeks. Mr. Markham considers that Russia is entitled to rank next to England in the matter of Arctic exploration; since to it is due the survey of *more than one-third* of the threshold of the unknown Polar Region.

Having thus briefly reviewed some of the more important Polar explorations prior to the nineteenth century, let us turn our attention to the work of the present century, which has far exceeded, at least in the number and magnitude of the expeditions, any previous period. For about 40 years after Cook's voyage, no attempt was made to discover a north-west passage, until early in this century Captain Scoresby and Sir John Barrow drew the attention of the Government to the subject, and four vessels were fitted up for Polar research. Two of these—the *Isabella* and the *Alexander*—under the command respectively of John Ross and Edward Parry, were to attempt the passage by way of Davis's Straits; the other two—the *Dorothea* and the *Trent*—under the command of David Buchan and John Franklin, were to attempt to cross the Polar Sea between Spitzbergen and Greenland. Both expeditions started in 1818. Ross sailed up the east coast of Greenland, examined Jones's Sound, and turning south arrived in Lancaster Sound, returning home by way of Cape Farewell. The most important results of the voyage were some magnetic observations made by Captain Sabine. Although the other expedition under the command of Captain Buchan had as its primary object the discovery of a passage through the Polar Sea to Behring's Straits, it was also enjoined to make various electrical and magnetic observations, to take temperatures, observe the refraction of the atmosphere, the velocity of ocean currents, and the height of the tides. An astronomer accompanied the *Dorothea*. The ships skirted the west side of Spitzbergen, and anchored in Magdalena Bay early in June. After encountering very severe weather, the ships, which were much injured, and unfit to attempt further exposure in Polar seas, returned to England. During the succeeding year (1819), Franklin was sent out in command of an overland expedition destined to explore the North American Coast to the east of the Coppermine River. Dr. John Richardson and Mr. George Back (then a midshipman) served under Franklin. The intolerable hardships and privations which they endured during their journey of 5550 miles, would have deterred any hearts less stout than theirs from ever again engaging in Arctic exploration. However, Franklin again started in 1825 for the Hudson's Bay territory—wintered on the Great Bear Lake, and in the following summer surveyed the coast line to the west of the mouth of the Mackenzie River. Between 1819 and 1827, Sir Edward Parry undertook four Polar voyages. During one of these by the use of sledges he reached the highest known land, N. lat. 82° 45′ (July 23rd, 1827). The north magnetic pole of the earth was discovered by Sir James Ross during his second voyage (1829-33); it was found in lat. 70° 5′ 17″ N., and long. 96° 46′ 45″ W., on the west shore of Boothia. The amount of dip was within one minute of arc of the vertical. A cairn was erected, and the British flag placed upon it. Large extents of the Hudson's Bay Territory and the adjacent lands were surveyed by Dease, Simpson, Rae, Back, and others. Sir John Franklin's last voyage was made in 1845. Its object was to make a new attempt to discover a north-west passage. The expedition consisted of two ships, the *Erebus* and the *Terror*, specially strengthened for Arctic service, and provided with every appliance which could lessen the danger and discomfort of the undertaking. The expedition sailed on the 19th of May, and reached Disco early in July.

The ships were last seen on the 26th of July on their way to Lancaster Sound. When no tidings of the expedition came to be received, the Admiralty lost no time in sending out searching expeditions. The private and public search expeditions sent out by this country and by America between 1847 and 1859 number no less than 40. The ships were ultimately found to have been abandoned near the entrance of the Great Fish River. If proper depôts of provisions had existed, there can be no doubt that the crew might have escaped to habitable regions. Traces of their winter residence were found on Beechy Island, and a cairn was discovered on the south-west cape of the island. Sir John Franklin died in 1847. Sir Leopold McClintock traced the route of the men after the abandonment of their ships from the estuary of the Great Fish River. Here he found three skeletons of men who had perished by the way. According to the Esquimaux, 40 men (out of the 134 who had left England in 1847) reached the Great Fish River, where they all died. It was in the conduct of one of the searching expeditions for the missing crews that Captain McClure, in 1851, proved the possibility of the North-West Passage by discovering the strait which bears his name, and which, dividing Melville Island from Bank's Island, connects Beaufort Sea with Melville Sound. The passage is not, however, practicable for ships, owing to the constant accumulation of ice, and the quantity of fast ice which exists even in the height of summer. Dr. John Rae, who was sent out by the Hudson's Bay Company, was the first to bring home (in 1854) the intelligence of the fate of the *Erebus* and *Terror*. He obtained evidence from the Esquimaux that the white men had died from hunger, and he discovered many graves and skeletons, together with numerous relics. The Admiralty now concluded that the evidence of the fate of the two vessels was complete, and the promised sum of £10,000 was paid to Dr. Rae. Still Lady Franklin determined that if possible more precise information should be procured, and in 1857 she sent out the yacht *Fox*, under the command of Captain McClintock. During the first summer he made for Baffin's Bay, and was there beset with ice and enclosed. During the winter the ice drifted southwards, at the rate of more than five miles a day, for no less than 1385 miles, but in the following April (1858) the pack was broken up by a heavy gale, and the *Fox* escaped, and again made for the north. In August North Somerset was rounded, and the vessel entered Bellot's Straits, passing thence into Franklin Channel. The *Fox* wintered in Port Kennedy, and long sledge journeys were made to the Magnetic Pole and elsewhere. Details of the ultimate fate of the *Erebus* and *Terror* were now learnt. One of the ships had been seen to sink, and the other was forced on shore by the ice. A number of relics were obtained from the Esquimaux on the west coast of Boothia.

Since the return of McClintock's expedition, various foreign nations have prosecuted Arctic research, while the English for the last 15 years have rested on their oars. The Americans seeking to approach the North Pole by way of Smith's Sound have reached a latitude of 82° 16′. This was effected in 1871 by Captain Hall in the *Polaris*. An expanse of open sea was seen to the far north, and was named Lincoln's Sea, while on the extreme west a coast line was discovered which was named President's Land. The Germans in 1868 sent out a privately-equipped vessel for the purpose of exploring the east coast of Greenland; but no results of importance were obtained. In 1869, a second expedition, consisting of two ships, commanded by Captain Koldewey, was again sent out to the east coast of Greenland. A certain amount of coast line below Cape Bismarck was surveyed, and the highest point reached was just above the 77° lat. The coast was carefully surveyed between the 73° and 77° N. lat.

The Swedes in 1870 explored the northern coasts of Novaya Zemlya, and in 1872 and 1873 they sent out an expedition to winter in Spitzbergen. No important results were obtained, but it is said that a new expedition will shortly be organised. A very important expedition was organised by the Austrians in 1872 under the command of Payer and Weyprecht. The object was to sail to the east of Spitzbergen, and if possible to follow the Gulf Stream into the sea which some suppose surrounds the Pole. Starting from Tromsö on July 14, the first ice was seen on the 25th, and on the 29th Novaya Zemlya came in sight. By the middle of August the vessel was near the north coast of Novaya Zemlya and was soon afterwards enclosed in the ice. Preparations for passing the winter had now to be made and the deck was converted into a house. The season had become excessively gloomy and the Arctic winter was about to commence. The sun disappeared for 109 days on October 28th, and for five months it was necessary to burn lamps. The sun appeared again on February the 16th and the severe pressure of the ice ceased. The mean temperature of the air was $-31°$ F., and towards the close of the month it reached $-51°$ F.; the maximum summer temperature was $-45{\cdot}5°$ F., and the mean temperature of the whole year $2{\cdot}75°$ F. Towards the end of October, still drifting with the floe, they came in sight of land within 3 miles in lat. 79° 54′ N., and to this was given the name of Wilczek Island. The sun again deserted them on October 22nd and they passed a second polar night of 125 days. During the winter they built a house upon the floe. The cold was very severe: mercury remained frozen for more than a week, and even brandy was changed into a solid mass. When spring returned they found that their ship was still firmly fixed in the ice, and as there appeared no prospect of its being liberated even in the height of summer, they determined to abandon it (May 20th, 1874), and to make their way to Europe by means of boats and sledges. In the preceding March a sledge expedition had started for purposes of exploration; the result was the discovery of a country as large as Spitzbergen, consisting of large masses of land, intersected by fiords and skirted by islands. The large sound was named "Austria Sound," and the land on each side of it Vichy Land and Wilczek Land, and more to the north Petermann Land and King Oscar Land. The latter is above 82° N. lat. On the 20th of May, having nailed the flags to the masts of their abandoned ships, they returned to Europe.

Since the return of McClintock's expedition in 1859, and the conclusive knowledge obtained by it of the fate of the crews of the *Erebus* and *Terror*, England has taken no part in Arctic research until the great expedition which started last May. Interest was revived in the subject in 1865 when Captain Sherard Osborn read his first paper before the Geographical Society on the further prosecution of Polar exploration. In this he pointed out the various advantages to science to be derived from such researches, and the value of the training to naval officers, and he strongly advocated the route by Smith's Sound. A second paper was read on the same subject in 1872. In the same year a deputation was received by Mr. Gladstone's Government which urged the sending out of an expedition in 1874. An unsatisfactory reply was received from the Chancellor of the Exchequer, and the matter remained at rest until, after further solicitation, the application was again refused by Mr. Gladstone, and soon after the Ministry resigned, and Mr. Disraeli came into office. In August, 1874, an influential deputation waited upon Mr. Disraeli, and in the following November a letter was forwarded to Sir Henry Rawlinson which stated that "having carefully weighed the reasons set forth in support of such an expedition, the scientific advantages to be derived from it, its chances of success, as well as the importance of encouraging that spirit of maritime enterprise which has ever distinguished the English people, Her Majesty's Government have determined to lose no time in organising a suitable expedition for the purposes in view." Whereupon an Arctic Committee of old and experienced Arctic explorers was appointed, Captain George S. Nares (then in command of the *Challenger*, and formerly mate of the *Resolute* in the Arctic expedition of 1872) was chosen to command the expedition, and two ships were fitted up with every modern appliance. For various reasons the route by Smith's Sound was selected. The general instructions given to the expedition were that the ships should proceed to Disco, an island on the west coast of Greenland, lat. 70° N., where they would touch, and afterwards continue their course further north to Upernavik, where dogs and Esquimaux drivers would be taken on board. Then, if the season were favourable, the ships should enter up Smith's Sound, and should winter if practicable in Lady Franklin Strait. The *Discovery* is not to go higher than the 82 parallel, and is to act as a relief ship to secure retreat in case of need; when the *Alert* has gone as far North as possible the journey to the pole is to be attempted by means of boats and sledges. It is probable that the two ships will not winter at a distance of more than 200 miles apart. The sledge operations—a main feature of the expedition—will commence in the spring of 1876, and if necessary a relief ship will be despatched in the summer of 1877 to the entrance of Smith's Sound. When in 82° N. lat. the ship will be within 500 miles of the Pole; then, if the ice is good, sledging will be comparatively easy. A portion of the ship's company will be broken up into parties of eight or ten men with an officer, and they will start in sledges provisioned for six weeks, and will probably make about twelve miles a day.

The *Alert* is a vessel of 1045 tons and 381 H.P., and the *Discovery* of 856 tons. The crew of each ship consists of about 60 officers and men, and they are provisioned for three years. The *Alert* carries 5 tons of spirits of wine, 10 tons of bread, 85 tons of beef, pork, bacon, coffee, sugar, flour, and preserved meats; while the *Discovery* carries 4½ tons of spirits of wine, 9 tons of bread, 78 tons of beef, bacon, and preserved meats. The greatest draught of either vessel will not exceed seventeen feet. The principal article of food is *pemmican*, which is more useful in the Arctic regions than the strongest spirits. It is prepared by drying lean beef, and then pounding it to powder, to which salt and sugar are added, and finally it is mixed with its own weight of melted suet and allowed to solidify in tins, each of which contains 56 lbs. Seventy pounds of this food will support a boat's or sledge's crew of eight men for a week. A quantity of unusually strong rum forms part of the stores. There is an improved cooking apparatus, and a large quantity of fuel in the form of coal, stearine, and spirits of wine has been provided. Every possible precaution has been taken to strengthen the vessels against the severe nips which they are likely to receive from icebergs; the screws can be raised before collision with the ice, and anchors of great power are provided. A number of ice saws are shipped on board (the largest nearly 15 feet long), and an experienced ice-master will superintend their use. There are no less than eighteen boats, some of which are 25 feet long; they are constructed of mahogany covered with marine glue and canvas, with an outside planking of elm and pine and a belt of cork. The sledge arrangements are particularly complete; all previous experience has been taken into account. The sledges hold eight men each; they are made of elm, and the runners are shod with steel. They are furnished with light masts, which also serve as tent poles. The drag ropes are of whale line, 26 feet long, and the drag belts are 3 inches broad, and pass over the men's shoulders. For a journey of seven weeks the weight of a loaded sledge will be 1646 lbs., or about 235 lbs. for each of seven men to drag. The weekly allowance of food to each man exceeds 19 lbs., and consists of daily rations of 16 ozs. pemmican, 4 ozs. boiled pork, 14 ozs. biscuit, 2 ozs. preserved potatoes, $1\frac{1}{2}$ ozs. chocolate, $\frac{1}{2}$ oz. tea and sugar, 1 oz. strong rum, and a weekly allowance of $1\frac{3}{4}$ ozs. salt, 1 oz. curry or onion powder, $\frac{1}{4}$ oz. pepper, and 3 ozs. of tobacco. Each individual uses about $1\frac{1}{4}$ lbs. of fuel (methylated spirits of wine or crude cocoa-nut oil) weekly. By a system of auxiliary sledges one sledge can advance several weeks' journey from the ship. Suppose six sledges start in company, at the end of the first week No. 6 sledge will distribute its provisions so as to make up the full complement of the remaining five, and will then return to the ship; at the end of the second week No. 5 sledge will make up the full complement of provisions of the remaining four, and will then return to the ship, and so on for Nos. 4, 3, and 2, leaving No. 1 to reach the extreme distance. Meanwhile the sledges which have returned will re-provision, and meet the returning sledge at various points of its journey. Great distances have been traversed by means of sledges; a sledge party led by McClintock walked 1210 miles in 105 days, while the distance from Cape Parry to the North Pole is only 484 miles.

The Admiralty shortly before the expedition started issued a very valuable Manual for the use of the officers accompanying the expedition. It is entitled "A Manual of the Natural History, Geology, and Physics of Greenland and the Neighbouring Regions; together with instructions suggested by the Arctic Committee of the Royal Society for the use of the Expedition." The instructions are discussed under two headings:—*Physical Observations* and *Biology*. The former relate to astronomy, terrestrial magnetism, meteorology, atmospheric electricity, optics, and miscellaneous observations; the latter to zoology, botany, geology, and mineralogy. The important suggestions on the determination of the magnetic elements, by Prof. J. C. Adams and Captain Evans, are accompanied by three capital maps showing the lines of equal declination, inclination, and horizontal force in the Polar Regions. Sir William Thomson contributes instructions for the observation of atmospheric electricity, while Prof. G. G. Stokes gives some useful hints in regard to spectroscopic observations. The instructions extend over 86 pages, while the manual embraces 750 pages, and consists of reprints or abstracts of papers relating to the natural history and physics of the Polar Regions. The natural history is compiled by Prof. T. Rupert Jones, and the physics by Prof. W. G. Adams. The physics is divided into (1) meteorology; (2) temperature of the sea; (3) physical properties of ice; (4) tides and currents; (5) measuring the motion of glaciers; (6) observations on refraction and on air, and observations on sound; (7) terrestrial magnetism; (8) aurora borealis. The scientific members of the expedition will have enough to do if, in addition to various other observations, they attempt to follow in the footsteps of the Swedish expedition to Spitzbergen under Prof. Nordenskiold. As part of the general observational work of this expedition we find the following:—

"1. Hourly meteorological observations.

"2. Astronomical positions of stations.

"3. Pendulum and refraction observations in great cold.

"4. Hourly magnetic observations, besides observations every five minutes on two term days a month, in connection with observations at Upsala.

"5. Tides and currents."

We come now to the results which we expect to obtain from the Arctic Expedition of 1875. Before we enumerate the more special objects to be attained let us call to mind the fact that all maritime enterprise is, and ever has been, popular among the sea-loving inhabitants of these islands. And when that enterprise involves unparalleled hardships, privations, and dangers, when it may result in the discovery of new lands and seas, the probable accomplishment of that which the world has for centuries attempted in vain, the old Vikingr blood is raised to fever heat in our veins, and the adventure becomes as exciting as the bombardment of a great sea fortress, or the investment of a walled and fortified city. We may not forget that Nelson began his career in an Arctic exploring vessel, and that the undaunted heroism of Franklin and a host of Arctic explorers is almost alone in these modern days worthy to be placed on a level with that of Leonidas. Once more, we who have ever been foremost in Arctic research must not allow other nations to step in and attain the final consummation. And if there is any more to be said as to the objects of the expedition, we say that the discovery of new truths, the enlargement of the sum of human knowledge, is object enough in itself for the expedition. In this land we should have no difficulty in manning a dozen Arctic expeditions having that object alone in view. The man who enquires the immediate *practical* use of the expedition is not of one blood with these men. What, pray, was the use of Galvani's experiments with frogs' legs or Benjamin Franklin's kite experiment? The practical man may answer if he pleases. However, there is something to be said as to the practical results to be expected from the members of the expedition. They will probably not discover new whaling grounds, or stores of fossil ivory, or gold mines, or easy routes to the Indies; possibly not even seams of coal and new veins of cryolite. But they will penetrate within an area which contains $2\frac{1}{2}$ millions of square miles of unexplored country, they will observe physical phenomena under extreme and unusual conditions, and they will survey many hundreds of miles of sea and land. An old Arctic explorer (Sir Edward Sabine) asserts that the completion of the circuit of Greenland and the survey of its northern and western coasts is "the greatest geographical achievement which can be attempted." Oceanic currents, tides, and deep-sea temperatures will be investigated; pendulum observations will allow us to determine the intensity of gravity at or near the pole, and will conduce to our knowledge of the figure of the earth. In meteorology we shall have determinations of the temperature and pressure of the air and the nature of prevailing winds. The magnetic and electrical results will be of peculiar interest; the spectroscope will be applied to the study of the Aurora; the nature of the electricity existing at any particular time in the air will be determined. In geology and mineralogy the field is likely to be as fruitful—a number of rare minerals have already been found in high latitudes; the existence of a palæozoic coal formation has been proved, and knowledge of its position and extent is much desired. Already the fossil flora of the east coast of Greenland has been proved to contain no less than 200 species of highly organised forms such as are not now found in the Polar Regions. As regards the existing flora of Greenland, 300 kinds of flowering plants are already known. In Zoology we have many facts to learn in regard to the migrations of birds to northern latitudes, and although the land fauna may not be extensive, the sea is full of creatures many of which have been but partially examined, and many of which are undoubtedly unknown. Finally, as to the ethnological results, it is probable that the most sterile wastes hitherto discovered in the Polar Regions were formerly inhabited. Remains have been discovered of races long since passed away. Many considerations point to the belief that, in the unexplored regions in the north of Greenland, inhabitants will be found ; if so the study of these isolated peoples will be of the greatest interest ; the form and measurement of their skulls, their stature, their moral intellectual condition, their whence and whither. "Snow huts will point to some devious wanderings from Boothian or American shores ; while stone *yourts* would indicate a march from the coast of Siberia across a wholly unknown region." The above are some of the results to be expected from the Arctic Expedition of 1875.

On Sunday, August 29th, the *Valorous*, which had accompanied the *Alert* and *Discovery* as far as Disco, returned to England, bringing dispatches from Captain Nares. The expedition left Portsmouth on May 29th, arriving in Bantry Bay on July 2nd. Thence they made for Greenland. On the 4th of June, a heavy wind began to blow from the west, and by the 11th it had increased to a gale. The first ice was sighted on June 27th, and on the following day they were off Cape Desolation. On the 13th of June the ships parted company in the gale, and they did not join company again till June 30th in Davis's Straits. On the 6th of July they both anchored in the harbour of Godhavn, at the south-west corner of Disco, 69° N. lat. From the 6th to the 13th of July, the ships remained at Godhavn, taking in coals and other supplies from the *Valorous*. Captain Nares reports :—"We are now complete in all respects for three years from July 1st, 1875." They also took on board 44 good Greenland dogs, and two whaling-boats, which had been lost during the gale, were supplied by the *Valorous*. On Thursday, July 15th, the *Alert* and the *Discovery* left Godhavn, and on the 17th they were last seen by the *Valorous* steaming down the Waigat Sound, and ultimately disappearing behind gigantic icebergs. Soon after a deep fog settled over the waters. The Danish officials at Godhavn reported that the last winter was much colder in South Greenland than in the north, and a great deal of ice had drifted south. At Godhavn the mean temperature of the winter months was from 5° to 13° higher than the average, but the spring was unusually severe. There is every reason, according to Mr. C. R. Markham, to hope that the ships will pass through Melville Bay during this season, and reach the north water of Baffin's Bay. When this is accomplished, a depôt will be established on the northwesternmost of the Carey Islands, and the ships will proceed to Smith's Sound. The *Discovery* will winter in lat. 82, on the north shore of Lady Franklin's Strait, if suitable quarters can be found; while the *Alert* will, if possible, push up as far as 84° N. lat., and complete the remaining distance to the Pole by sledging. The sledge travelling will commence early in April, 1876, and it is proposed that six sledges and 52 men should then start, leaving only 10 men in the ship. By means of the system of auxiliary sledges described above the foremost sledge will be enabled to be absent from the ship for four months, and probably travel 500 miles from the ship. Meanwhile the *Discovery* will send out hunting and exploring parties. If no news is received of the *Alert* in 1876, the *Discovery* will attempt to communicate by means of sledges drawn by dogs; and if during the early part of 1877 the *Discovery* is still unable to obtain news of the *Alert*, the former will return to England, and it will be concluded that the *Alert*, having passed to the north-east of Greenland, will return down the east side of Greenland. Letters and despatches will be left at Upernavik, 72° N. lat., for the last time before the vessels penetrate the unknown regions, and these letters we may expect to receive in England before Christmas. Then we shall probably hear no more of the expedition till the Autumn of 1876. Commander Markham, in a letter to Sir Henry Rawlinson, brought home in the *Valorous*, and read at the recent meeting of the British Association at Bristol, says:—"I think everything promises most favourably, and if we have only a reasonable amount of luck, I am confident we shall achieve success. We have a splendid lot of fellows, both officers and men in both ships, all eager for the fray and anxious to be at it. If we succeed in getting our ships to 83° or 84°, we shall be at the Pole in ten months' time."

No time could be more fitting than the present for an English Arctic expedition. We have enjoyed a profound peace for a number of years; the wealth of the country has prodigiously increased; taxes are low; the Exchequer is in a satisfactory condition; our most experienced seamen are at liberty to take part in the undertaking; and there is a lull in political affairs. The whole nation watches with one heart the progress of the work. In this too-practical age it does the world good to see Governments and men animated by the one desire of the discovery of new truths at any sacrifice. Whatever may be the result of the expedition, it must redound to the honour of this country and of the enlightened Government under whose auspices it was instituted.

NOTICES OF BOOKS.

The Sensations of Tone, as a Physiological Basis for the Theory of Music. By HERMANN L. F. HELMHOLTZ, M.D., formerly Professor of Physiology in the University of Heidelberg, and now Professor of Physics in the University of Berlin. Translated, with Additional Notes, by ALEXANDER G. ELLIS, B.A., F.R.S. London: Longmans and Co. 1875.

THE long looked-for translation of the *Tonempfindungen* has at length appeared, and it will be welcomed by all men of science. Hitherto those of us who can only read German with difficulty, or not at all, have made the acquaintance of certain portions of this work through Tyndall's "Lectures on Sound." Now we have before us the entire work, translated by a man most fitted for the task, and the well-known author of various papers printed by the Royal Society on collateral subjects. The original work appeared in Germany in 1862, and it has passed through three editions in that country. Its main object is to trace the precise relationships which exist between the Science of Sound and the Art of Music. It is the work of a man who, emulating the multifarious knowledge of Leonardo da Vinci, is at the same time an accomplished physiologist, physicist, mathematician, and musician; and it is the result of eight years of thought and labour.

According to the translator, "the great feature of this work is the proof that all musical sounds, whether proceeding from one or many sources, are heard by the ear as if they came from one or more distinct sources of a particular simple kind, combined by a well-known and definite law, so long as the excursions of the particles of air are small in comparison with the length of the wave, and by another and different law when those excursions are larger. From this flows the fact that any individual tone may be considered as compounded of the partial simple tones of which it is audibly composed, and the several combinations of such compound tones can be reduced to the combinations of the simple tones of which they are compounded, and of the other simple tones which result when the excursions of the particles of air are larger than usual." From it also can be derived the nature of consonance and dissonance and the theory of harmony.

The first chapter treats of "the sensation of sound in general," and at the outset a distinction is drawn between the various kinds of sound experienced by our ears, that is to say *noises* on the one hand, and *musical tones* on the other; the former being defined as being due to *non-periodic motions*—that is to say motions which do not return to the same condition after equal intervals of time; while the latter are due to *periodic motions*—that is, motions which do return to the same condition at the end of equal intervals of time. The production and propagation of sound is discussed, and the distinguishing features of musical tones—to wit, force or *loudness*, pitch or *relative height*, and quality; also the various means of indicating and examining these distinctive features. The musical tones which can be used with advantage are stated to be between 40 and 4000 vibrations in a second—a range of seven octaves; while the limits of audibility ranges over 11 octaves, from 20 to 38,000 vibrations a second. The range of the eye, on the other hand, scarcely exceeds one octave. Many terms are explained and definitions given, also a minute account of vibrations and of their graphic representation.

In the second chapter the composition of vibrations is discussed. As an analogy, the various simultaneous waves produced on water are exemplified, and we are told that we must imagine the same kind of action as taking place in the air. In a crowded ball-room, for instance, we have the various sounds of the musical instruments, the rustling dresses, the voices of men and women, and so on, and here "we have to imagine that from the mouths of men and from the deeper musical instruments there proceed waves of from 8 to 12 feet in length; from the lips of the women, waves of two to four feet in length; from the rustling of the dresses, a fine small crumple of wave, and so on: in short, a tumbled entanglement of the most different kinds of motion, complicated beyond conception." It is then shown that such composite masses of tone may be regarded as, and resolved into, simple tones. In the next chapters we plunge at once into the more complex technicalities of the subject, and the analysis of musical tones is discussed; first, by sympathetic resonance; secondly, by the ear. The whole structure of the ear is minutely discussed, and the functions of its various parts are described. Some new woodcuts, from Heule's "Manual of Anatomy," illustrate this part of the subject. An elaborate treatment of the Cortian fibres, and the office of the Cochlea, conclude the first part of the book.

The second part is "On the Interruptions of Harmony;" the third "On the Relationship of Musical Tones." Here we have an account of the development of musical style, and a division of all music into three parts, viz., the *homophonic*, or unison music of the ancients; the *polyphonic* music, with several parts, extending from the 10th to the 17th century; and, finally, *harmonic* or modern music, the special characteristic of which is the significance attributed to the harmonies.

The primitive form of music with all nations appears to be simple melody sung by a single voice, and it still remains among the Chinese, Arabs, Turks, Modern Greeks, and other peoples. Almost the only example that we now have of the "spoken song," or of something approximating to it, is in the intoning of Roman Catholic priests. Here we have differences of pitch, for emphasised words are spoken a tone higher than the rest; and modern recitative (invented by Giacomo Peri, about 1600) arose from the attempt to express these alterations of pitch by musical tones. Polyphonic music was invented by a Flemish Monk, Hucbald, in the beginning of the 10th century. It was a part music sung by two voices in fifths or fourths, with occasional doublings of the octave, and the same melodic phrase was repeated by the different voices. Modern harmonic music was induced by various causes, among which the Protestant ecclesiastical chorus is first cited, The founders of the new confession desired that the congregation itself should undertake the singing; hence harmonised chorales in which all the voices progressed at the same time had to be invented. The Roman Church also desired a reform in their music, and by order of Pope Pius IV., Palæstrina (1524-94) undertook the embellishment of ecclesiastical music.

We may give, in conclusion, a few important excerpts from the author's summary of results:—P. 564. "We are justified in assuming that historically, all music was developed from song. Afterwards the power of producing similar melodic effects was attained by means of other instruments, which had a quality of tone compounded in a manner resembling that of the human voice."

P. 568. "In the last part of my book I have endeavoured to show that the construction of scales and of harmonic tissue is a product of artistic invention, and by no means furnished by the natural formation or natural function of our ear, as it has been hitherto most generally asserted. Of course, the laws of the natural function of our ear play a great and influential part in this result. . . . . . But just as people with differently-directed tastes can erect extremely different kinds of buildings with the same stones, so also the history of music shows us that the same properties of the human ear could serve as the foundation of very different musical systems. . . . . The æsthetic problem" (in regard to music) "is thus referred to the common property of all sensual perceptions, namely, the apprehension of compound aggregates of sensations, as sensible symbols of simple external objects, without analysing them. In our usual observations on external nature, our attention is so thoroughly engaged by external objects that we are entirely unpractised in taking, as the subjects of conscious observation, any properties of our sensations themselves, which we do not know as the sensible expression of some individual external object or event." A curious and most interesting comparison of the resemblances between the relations of the musical scale and of space will be found among the concluding pages (p. 576).

An appendix of more than 200 pages will be found at the end of the work. Much of this has been added by the translator. Among other matters of interest which it contains, we may notice an account of an electro-magnetic driving machine for the siren. This machine gives perfect uniformity of rotation, and perfectly constant tones can be produced by the means. Air is driven through the siren by means of a small turbine of stiff paper. In discussing the size and construction of resonators, the author mentions that he first employed spherical glass vessels for the purpose, but now more generally vessels of brass. He also used tubes of tin and pasteboard. A table shows the exact size and capacity of resonators, adapted to tones of different pitch. A number of mathematical papers follow, and a very good index terminates the volume.

It is almost unnecessary for us to say that this already famous book will be welcomed alike by the physicist, the acoustician, and the musician. It is one of the most original works of the second half of this century, and may be placed in the same category as the "Natural Philosophy" of Thomson and Tait, and the "Electricity" of Clerk Maxwell.

Sound. Third Edition. By JOHN TYNDALL, F.R.S. London: Longmans and Co. 1875.

Six Lectures on Light. Delivered in America in 1872-73. By JOHN TYNDALL, F.R.S. Second Edition. London: Longmans and Co. 1875.

WE are very glad to welcome new editions of these standard works. One of the great merits of Dr. Tyndall as an expounder of scientific facts is that he is not alone content to read the memoirs of authors whom he is desirous of quoting, but that he himself goes through their experiments, and seldom gives an account of an experiment which he has not himself tried. His works thus become of especial value to the lecturer and science teacher. The main feature in the new edition of the "Sound" is the introduction (in chapter 7) of the author's recent experiments on "The Acoustic Transparency of the Atmosphere in relation to the Question of Fog-signalling." This subject has been so recently discussed in nearly all the scientific journals, that it will be familiar to all of us. The preface gives an account, in some detail, of the controversy between Dr. Tyndall and the United States Lighthouse Board, as to the priority of claims. A review of Dr. Tyndall's "Lectures on Light" appeared not long ago in this journal, and we have but little to add in noticing a second English edition; the only very noticeable alteration in this edition is the omission of the author's "Reply to the Edinburgh Reviewers," concerning the claims of Dr. Young, and the insertion of a portrait of Dr. Young at the commencement of the volume. In an appendix the Author gives the addresses of Profs. Barnard, Draper, and White, on the occasion of his departure from America, and the preface of the French translator of his work (the Abbé Moigno).

Natural Philosophy for General Readers and Young People. 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. Second Edition. London: Longmans and Co. 1875.

THIS is an abridgment of the well-known larger work of Ganot on the "Elements of Physics." It is divested of mathematical formulæ, and is specially adapted for young persons and for science classes in schools. The extent of its subject-matter may be defined by the fact that it represents the amount of physical knowledge necessary for the matriculation examination of the University of London. We are glad to notice a second edition of the work, because we believe it to be a really useful work; but we regret to notice that many clerical, and a few other, errors have not been expunged from the first edition. These will, however, be at once apparent to the teacher, and can be corrected by the student once for all. A second coloured plate has been added to this Edition, together with twenty-four new woodcuts, and twenty pages of new matter; the editor has also not contented himself with giving a mere literal translation of the French work, but has made various additions in order to render it more complete. We would suggest that in the next edition a number of well-selected questions on each chapter of the work be given, specially some questions of an arithmetical character (in regard to levers, the inclined plane, the laws of falling bodies, and of the pendulum), which are much affected now-a-days in the London Matriculation and other examinations.

A Short Manual of Heat, for the Use of Schools and Science Classes. By the Rev. A. IRVING, B.A., B.Sc. London: Longmans and Co. 1875.

An Elementary Book on Heat. By J. E. GORDON, B.A. Intended chiefly for the use of Candidates for the General Examination for the Ordinary B.A. Degree. London: Macmillan and Co. 1875.

IT is surprising that with such a book as Prof. Clerk Maxwell's "Elements of Heat," and the Clarendon Press Manual of Prof. Balfour Stewart, there should be room for any other elementary work on the subject; yet we have here two little treatises appearing within a very few months of each other. The first is by no means without merit; it embodies in an elementary form all the phenomena of heat, the arrangement is concise and comprehensive, a capital glossary of terms is given, and nearly twenty pages of questions. The book appears to us to be singularly well suited for the purposes of science teaching in schools; it contains about two 'Terms' work. It is a book which may be used by boys in conjunction with their school lectures, and is certainly adapted for Form questioning. It is divided into seven chapters, which treat respectively of the *Nature and Sources of Heat, Expansion and Thermometry, Changes of Physical Condition, Conduction of Heat, Specific Heat, Radiant Heat, Heat and Work.* A few simple, but in most cases sufficient, woodcuts are introduced. Mr. Gordon's book is written for a special examination (the Cambridge B.A.), and probably will not be much used beyond the circle for which it is intended. The "questions and problems" at the end are altogether deficient as to number, and we should have thought are in most instances too elementary, even for the examination for which they are intended.

Practical Hints on the Selection and Use of the Microscope. By JOHN PHIN, Editor of the "Technologist." New York: The Industrial Publication Company. 1875.

THIS little work describes in some detail the various forms of microscope, the manner of using them, and the preparation and mounting of objects; it possesses a very limited number of woodcuts, and it does not appear to us to possess any advantage over Dr. Beale's smaller book, or indeed to, by any means, come up to it in general usefulness and design.

Ure's Dictionary of Arts, Manufactures, and Mines. By R. HUNT, F.R.S. London: Longmans, Green, and Co.

URE'S "Dictionary" still maintains its place as the standard technological encyclopædia. There are, of course, treatises of some individual departments, such as metallurgy, calico-printing, manure-making, &c., which give more minute and complete instructions. But as affording a general survey of the whole field of industrial science, the work before us stands unrivalled, and may be regarded as a national trophy. The present edition has been carefully revised and brought up to the standard of the day. Many of the original articles having become obsolete, have been omitted, and others have been greatly curtailed, their subject-matter having fallen in importance since the appearance of the last edition. As an instance of such a relative decline, we may mention the prussiate of potash. A quarter of a century ago this was the ingredient necessary for the production of the most beautiful blues then known, whether upon cottons, silks, or woollens. It was also essential in the manufacture of Prussian blue, which was then a leading pigment. Now, however, blues far finer and brighter than those produced with prussiate are dyed or printed with coal-tar compounds. On the other hand, artificial ultramarine has greatly interfered with the use of Prussian blue. Hence the importance of prussiate of potash is declining, and there can be no longer the same interest felt in its manufacture. The whole field of technology—especially its chemical portion—is full of such revolutions.

As an instance of an article nearly re-written, or at least enriched with a great amount of new and important matter, we may mention that on calico-printing. Many capital discoveries have been made, or at least introduced, into practical use since the appearance of the last edition. Accordingly, we find now full notice of the methods of applying the coal-tar colours, especially the aniline-blacks—of the use of artificial alizarin and the improved extracts of madder, of the recent developments of pigment printing, and of the new indigo-vat of Schützenberger and De Lalande—improvements which may be said to have created a perfectly new epoch in this wonderful art. The kindred department of dyeing is scarcely treated in as thoroughgoing and as practical a manner. In the article on sulphuric acid, we are glad to find that the recent important researches of Mr. H. A. Smith have not been passed unnoticed. He maintains that the best form of chambers should be rather long than high, being about 150 feet in length, 35 to 30 feet in width, and 10 to 12 feet in height.

The account of the artificial soda, or, as it is generally called, alkali-manufacture, includes brief notices of the new methods of Mr. Tilghman and of MM. Schlœsing and Rolland, otherwise known as the ammonia-process. The arrangements for effecting the condensation of the muriatic gas, as required under the Alkali Act of 1863, are also described and illustrated with drawings of the towers at the celebrated works of Messrs. Alhusen.

In short, it may generally be said that few recent improvements in any branch of manufactures have been entirely overlooked. It would be, of course, easy to suggest additions which might have been made, and which would, doubtless, increase the value of the work. But the great point in a compilation of this kind is to include all that is really essential without rendering the result too bulky, and consequently too costly. This end, we think, the editors have reached to a very satisfactory degree. The present edition is a book which no manufacturer, merchant, miner, or technologist, should be without.

The Skull and Brain; their Indications of Character and Anatomical Relations. By NICHOLAS MORGAN. London: Longmans, Green, and Co.

A WORK on phrenology is, in these days, quite a portent. Some 30 or 40 years ago the case was different. Every large town and many a small one had its phrenological society; every popular literary institute had a class for the study of cerebral development in connection with character. We saw phrenological busts in shop-windows and in the libraries of our friends. We heard the subject continually discussed, not merely at gatherings of scientific men, but even in general society. Works assailing or defending the system were published, and received wide attention. Now all this is changed. The phrenological societies are, we believe, for the most part, defunct, and their museums broken up or disused. There does not appear to be, numerous as scientific and semi-scientific periodicals have become, a single phrenological journal published in London. We cannot lay our hands on any review-article on the subject which has been issued within some 15 years; nor, indeed, does our author refer to any such. Surely this fact—the gradual but general disappearance of phrenology from public attention in England—is one which Mr. Morgan should have noticed, and, if possible, explained. But he seems to have ignored it altogether.

Again, Mr. Morgan quotes without contradiction, from "Blackwood's Magazine" for 1857, a passage by Mr. G. H. Lewes, in which the latter declares that though "phrenology was born in Germany and reared in France, it has not standing-room in either country." On this he comments as follows: "This is highly suggestive. The seed of phrenology germinates and grows vigorously in these countries (England and Scotland), because it is sown in the soil of thought and is manured by impartial investigation, and warmed by the sunshine of sincerity. No wonder, then, that it withstands the blight of ridicule and the storms of prejudice." To this somewhat rhetorical passage we must object that it overlooks the recent decline of phrenology in Britain. Further, neither Mr. Lewes nor Mr. Morgan accounts in a satisfactory manner for the failure of phrenology on the Continent, as compared with its former popularity in England. "Thought," "impartial investigation," and "sincerity," are assuredly not the exclusive attributes of our island. Political investigations, indeed, when taking a direction unwelcome to the authorities, may have been suppressed in Germany; but scientific speculation has assuredly not been fettered, either by the censorship or by "Mrs. Grundy." We doubt, indeed, the power of the latter authority to hinder a German professor from following out to its remotest logical consequences any system for which he has sufficient evidence.

We do not, of course, mean to urge that either the decay of phrenology in Britain, or its rejection in Germany, is any direct argument against its truth. Still both are facts which an apologist like Mr. Morgan would have done well to examine.

The work before us begins with a survey of the objections to phrenology, those especially urged by Mr. Lewes in his "History of Philosophy," as published in 1871, and in his paper on "Phrenology in France," contained in "Blackwood's Magazine," for December, 1857. Mr. Lewes is charged with a strong anti-phrenological bias, and with the ready acceptance, without due verification, of evidence telling against the system. Thus, on the authority of M. Piesse ("La Médecine et les Médecins," Paris, 1857), he declares the head of the first Napoleon to have been "decidedly small." Mr. Morgan, from actual measurements, made on "an authentic copy of Dr. Antomarchi's cast of him," asserts that it was "considerably larger than the average European male head," exceeding in most directions the heads of S. T. Coleridge and Dr. A. Combe. The researches of Dr. Ferrier, which, by the way, will lay him open to the vengeance of the anti-vivisectionists, are cited in proof that the individual convolutions of the brain are separate and distinct organs. It is suggested that possibly Mr. Lewes "will deem it advisable in the next edition of the "History of Philosophy" to modify his views, and to eliminate the gratuitous assertion that the researches of anatomists have disproved every point advanced by Gall, in the same way as he found it necessary to re-write many portions of the last edition, so as to bring them more in consonance with the spirit of the times."

Dr. Ferrier's results, though accepted in support of the view that the brain consists of a number of distinct organs, are otherwise called strongly in question. This relates especially to his announcement that the cerebellum has no connection with the sexual instinct, since its excitement by a Faradic current failed to elicit any amatory symptoms.

Incidentally, too, Dr. Carpenter is criticised. He maintains* "that the *posterior* lobes of the cerebrum are the instruments, *not* (as maintained by phrenologists) of those passions and propensities which man shares with the lower animals, but of attributes peculiar to man, which we fairly may suppose to consist in such mental operations of a peculiar intellectual character as do not express themselves in bodily action."

Our author remarks that the brains of idiots are generally remarkable for the largeness of these parts and the smallness of the anterior lobes, whilst those persons who are marked for intellectual power are, as a rule, equally noted for the extraordinary size of the anterior lobes.

We should add that the number of attributes peculiar to man will be found, upon more careful and impartial scrutiny, to become "fine by degrees, and beautifully less." The last objection to phrenology which the author takes into consideration is

* Mental Physiology, pp. 714, 715.

the timeworn cavil that it leads to fatalism—a curious charge when brought, as it often was, by the followers of Calvin! It must be remembered that the plea of necessity is felt to be no longer any bar to the punishment of criminals. Whether a given man can "help" committing a murder or not, still, if he has been guilty of such a deed, society is in self-defence justified in his elimination.

A Digest of the Reported Cases Relating to the Law and Practice of Letters Patent for Inventions, Decided from the Passing of the Statute of Monopolies to the Present Time. By CLEMENT HIGGINS, M.A., F.C.S., Barrister-at-Law. London: Butterworths.

THE rights of inventors, the nature and even the very existence of patent-laws, are at present under examination and may be said to be trembling in the balance. A number of persons, noisy if not numerous, and strong in influence if not in argument, contend that patents are a hindrance instead of a benefit to manufactures, and deny the exclusive claim of the inventor to the beneficial use of his own ideas. That a view so essentially communistic should for one moment be tolerated in property-loving England may seem at first sight singular. But it is in reality the outcome of a principle but too common. Your *sans cullotte* communist, who has nothing in his purse, denounces property as robbery, and clamours for the confiscation of landed estates. Your capitalist, who has nothing in his head, denounces property in ideas, and seeks to confiscate all inventions for the good of the public, by which he means himself.

There are, moreover, two words commonly used in connection with patents which act upon the "rump" of the Manchester school in the same manner as a red cloth does upon a bull. We speak of obtaining *protection* for an invention, and we refer in treating of patents to the statute of *Monopolies*. These two unfortunate terms do the mischief. They are used, to be sure, in a sense quite different from what they bore in the great anti-corn-law agitation. But a free-trader when once he suspects economic heresy overlooks such distinctions. It is, of course, incontrovertible that if any kind of property should be sacred and inviolable, property in inventions should have precedence. The inventor may justly ask the memorable question "May not I do what I will with my own?" for he has created it. If a man may use our inventions against our will, and without recompense, *a fortiori* he is entitled to appropriate our money, even our personal labour, for his own purposes. If the anti-patent law agitators can show us any better way in which the interests of the inventor may be secured and recognised, we shall be well content. If not, we must protest against their views, and against the Bill for the "Amendment" of the Patent Laws now under the consideration of Parliament.

The work before us is a valuable and a welcome contribution to a knowledge of our present patent-law system, a system which, with all its admitted imperfections, has done much for our national manufacturers and much for industrial science. Those very men who are now barking against the system owe to it, indirectly, everything. It therefore deserves that we should make it the subject of careful study, so that its blemishes and shortcomings may be removed without the sacrifice of its merits.

Mr. Higgins has given us, in a very natural and convenient order, the recorded decisions of the courts of Law and Equity on every branch of this great and difficult subject. From these decisions the state of the law upon any point connected with patents may be deduced.

The learned author has not, indeed, sought to explain or justify the decisions, to reconcile real or apparent discrepancies, or to furnish any note or comment whatever. Had he done so the volume might have been expanded to an extent which would have greatly interfered with its utility. But he enables even persons not learned in the law to form a clear notion of the circumstances and conditions under which patents are granted, and of the rights, duties, and liabilities of patentees. The decisions are grouped under such heads as persons interested would naturally expect, and within these divisions they are arranged in chronological order. Does the reader wish to know what may be legally made the subject-matter of letters-patent? He finds that head in the copious table of contents and under it the decisions in point. Does he wish for information on sale and assignment, on licences and royalties, on joint ownership, or on infringement? It is all here. The work is further provided with a "table of cases" showing the original documents in which the decisions are found and the paragraphs in which they are quoted in this digest. There is also a very elaborate index. In fine, we must pronounce the book not only invaluable to patent agents, but likely to protect inventors, patentees, and manufacturers from much unnecessary trouble, anxiety, and expense.

Corals and Coral Islands. By James D. Dana, LL.D. London: Sampson Low, Marston, Low, and Searle.

Corals have long been a subject for conflicting theories, and a theme for sentimental declamation. They were once classified as vegetables. By certain speculators they are even yet imagined to be purely mineral developments, a kind of crystallisation, due to electrical forces, which Dr. Dana justly characterises as "the first and last appeal of ignorance." Some even of those who cannot deny the evidence of their animal nature entertain the wildest notions as to their manner of life and as to the mode in which coral reefs are produced. Those delicately branched and laminated structures, white, yellow, or red, which adorn our museums under the name of "corals," are commonly said to be the "work" of certain minute worms, or worse still of "insects," —a term which so applied always sets our teeth on edge. These little creatures by their "toil" and "skill" are supposed to build up islands from out of the profoundest depths of ocean. Each branch of coral is supposed to be "the constructed hive or house of a swarm of polyps, like the comb of the bee or the hillock of a colony of plants." The pores are described as cells into which the individual "insects" can retire for shelter or concealment.

All this, and more of a similar tendency, may be read in certain popular treatises on natural history, or, for those who prefer verse to prose, in Montgomery's "Pelican Island," concerning which our author remarks, with perfect truth, that "more error in the same compass could scarcely be found." In sober reality, the polyps elaborate coral, not by skill or toil, but by unconscious secretion, just as the bird forms the shell of its egg, or the quadruped develops its bones. Instead of the coral being a house, it is rather a skeleton, having its place within the animal. Polyps propagate by a process of "budding" as well as by ova, the former method being more general among the coral-forming species. This accounts for the branch-like structure of the corals.

The notion of polyps building up islands from the bottom of deep seas is now known to be mythological. Coral is certainly found at great depths, but it is never discovered in a living state beyond twenty-five fathoms. Hence, the origin of coral reefs was wrapped in mystery until Darwin, during the celebrated surveying voyage of the *Beagle*, succeeded in solving the question. Coral cannot live below twenty-five fathoms, and yet it is found extending downwards to far greater depths, dead in the lower regions, but living above. This is only possible on the supposition that the foundations upon which the reef was "built" have gradually sunk down, whilst the upper part continues to be extended, so as to keep its level, or even to rise to a certain height above the surface. Hence, these reefs become of the profoundest interest to geologists, serving as evidence of areas of subsidence, and, in tropical seas at least, marking the position of islands which have long ago disappeared.

Dr. Dana's views on the origin and signification of coral-reefs coincide in all main points with those of Darwin. The theory of our great English naturalist, which the author has studied, not in the closet, but among the living objects of which it treats, gave him, as he informs us, on his ocean journeys, "not only light but delight, since facts found their places under it so readily, and derived from it so wide a bearing on the earth's history." It is still more important for us to learn that Darwin's work on "Coral Reefs" appeared at a time when the author's "Report" was already in manuscript. The conclusions which these two eminent explorers had in a great measure independently reached were for the most part the same. It need scarcely be said that such a coincidence furnishes a very strong presumption in favour of the correctness of the theory.

Dr. Dana's work is well arranged. It may be divided into a zoological and a geological portion. In the former he describes and figures the principal species of polyps, hydroids, bryozoans, nullipores, and corallines. He then treats of their distribution in latitude and in depth, of the influence of local causes upon their development, and on their rate of growth. He then considers the structure of coral reefs and islands, their formation, and the causes modifying their form and growth, so as to produce barrier and fringing reefs and completed atolls. The author next surveys the geographical distribution of coral reefs and islands and passes on to the changes of level, in the Pacific Ocean especially, of which they afford us evidence. Finally he considers the geological bearing of the facts detailed in the earlier portions of the work, especially as regards the origin and characteristics of the great limestone formations. The work is plentifully and usefully illustrated,—two points which do not always go together,—and is furnished with an isochrymal chart of the oceans to illustrate the geographical distribution of corals and other oceanic species. There are also a good table of contents and index, a list of authors and memoirs quoted, and a list of the species described in the author's classical "Report on Zoophytes." We consider this a popular book in the highest sense of the word—clear, intelligible, readable, but at the same time thorough-going and accurate. Every student of natural history, geology, or physical geography ought to give this work a place in his library.

Chemical Examination of Alcoholic Liquors. By Albert B. Prescott, M.D. New York: D. Van Nostrand.

The objects of this manual, as stated in the preface, are to give in outline the chemistry of alcoholic liquors, including their current impurities and adulterations, in such terms as to be understood by persons having only an ordinary acquaintance with chemical science; and secondly, to furnish directions, as far as possible, for an efficient chemical examination; not more elaborate than is required for commercial, hygienic, and legal purposes, and containing all details except such as are to be found in every rudimentary treatise on chemical analysis. The writer, we are happy to find, holds it to be of absolute importance to society that all articles used as foods, medicines, or beverages be made subject to strict examination by authority of the law, and that impurities and additions be systematically exposed and suppressed." The author carries out his plan in a very satisfactory manner. Under each alcoholic liquor he describes the normal constituents, and then the fraudulent additions or substitutions.

Thus we are informed that genuine wine (the fermented juice of the grape without any addition) contains:—

(*a*). Alcohol, 7 to 20 per cent. by weight.

(*b*). Non-volatile substances, 3 to 10 per cent., including—

Grape-sugar 0·1 to 3 per cent (in a few varieties of wine 10, 13, 14 per cent.)

Free fixed acid, equal 0·3 to 0·6 of tartaric.

Tannic acid, 0·08 to 0·20 per cent.

Glycerine, 0·1 to 0·5 (maximum 2·0 per cent.)

Albumen (nitrogen from 0·02 to 0·06 per cent.)

Gum, pectin, fat, wax, colour, ash, 0·17 to 0·27 per cent. (Potassic phosphate, fully two-thirds the ash.)

Tartrate of ethyl (decomposed upon evaporation).

(*c*). Volatile substances, besides alcohol and water.

Ethers.

Fusel oil.

Acetic acid (0·06 to 0·12 per cent.)

We must remark that the author does not use the term "fusel" as a mere synonym for amylic alcohol, but extends it to all those products of fermentation which distil at a temperature higher than the boiling-point of ethylic alcohol. He quotes from Schmidt's "Jahrbücher Gesam. Med.," 1871, B. 149, p. 264, some interesting information on the physiological action of these compounds, which fully confirms the prevalent notion of their insalubrity. Amylic alcohol, it appears, produces poisonous effects, closely resembling those of ethylic alcohol, but of fifteen times greater intensity. Frogs were floated in a 0·002 solution of the alcohol (one part to 500 parts of water), and then in stronger solutions, and the effects of depressed action of the heart, congestion, anæsthesia, and death were timed. Amylic alcohol produced the same effects in the same time as did ethylic alcohol of fifteen times greater concentration or butylic alcohol of three times greater concentration; from which it was inferred that the poisonous action of butylic alcohol is five times more intense than that of ethylic acohol in the same quantity. Rabuteau also experimented on himself, taking from four to eight grains of amylic alcohol in a glass of wine, and the results confirmed the conclusions given above. On the other hand, Hermann, in his "Alcoholism in Russia," maintains that *delirium tremens* and acute alcoholism are not found more likely to result from the use of cheap spirits with much fusel oil than from the consumption of purer qualities. We may here state that in Poland and certain parts of eastern Germany, where highly fuseliferous potato-spirit is extensively consumed, the belief in its specially injurious character is common, both among professional and non-professional observers.

Apropos of poisonous beverages, our attention naturally turns to the worst of the class, absinthe. Dr. Prescott describes it as containing 40 to 60 per cent. by volume of alcohol, and several per cent of essential oils, those of wormwood, cinnamon, cloves, anise, and angelica being chiefly used, and coloured green with leaves of spinach and parsley, occasionally also with acetate of copper or with a mixture of indigo and gamboge. Doubtless by an oversight, he makes no allusion to the specifically noxious quality of this liqueur as compared with alcohol. We certainly think it essential that the manufacture, importation, or sale of this and of any analogous compound should be, on obvious sanitary grounds, totally prohibited.

Returning to wine, we find, among the list of impurities or additions, sugar, to increase the alcoholic strength of wines which otherwise would be weak. The author mentions that not more than 20 per cent of alcohol can be obtained by fermentation. If a greater amount be detected, the wine is sophisticated by the addition of spirits, generally of a low quality. Glycerine is occasionally added to the extent of 1 to 3 per cent., and if of good quality, free from traces of lead, is one of the most pardonable additions.

Calcined gypsum is sometimes added "to prevent viscous fermentation, to restore ropy wines, to fix colour, and to remove water." It is sometimes sprinkled upon the grapes, constituting the sin of plastering. This evil practice has not only been long and widely followed by the manufacturers—we use the term advisedly instead of growers—of sherry, but is finding its way into Australia. The result is that sulphate of potash is formed in the wine to an unnatural extent; its harsh, saline bitterness greatly injuring the flavour, and its well known action upon the heart rendering it hurtful, even dangerous. We should propose that all wines found to contain sulphuric acid in combination that above the normal amount should be at once condemned and run out into the river. Of course a brisk debate would then arise as to the normal quantity, and those chemists who stated the amount correctly would be pronounced "incompetent" by self-constituted judges. It is an important fact that in the juice of the grape magnesian salts predominate, whilst the fruits with which spurious imitations are chiefly got up are richer in calcium compounds. True wine contains malic and tartaric acids, whilst sham wines are often rich in citric, and, *horribile dictu*, in oxalic. The latter prevails when the fermented juice of rhubarb plays a leading part in the manufacture. Cane-sugar is never found in genuine wine, and its presence is, therefore, a mark of fraud. The author very judiciously remarks that "the artificial production of wines is not, like that of brandy, a task which chemical skill can hope to accomplish. Besides the great complexity of the ethers, the solid extractives are requisite. Then the peculiarity—in many cases the commercial value—of an actual wine depends upon certain proportions of the constituents named above, which proportions the chemist cannot fully determine. The ethers of wine elude quantitative analysis. Moreover there are doubtless substances in wine not identified. It may be perfectly true that a mixture of pure alcohol, water, glucose, bitartrate, and ethers, may be made in such carefully adjusted proportions that it will probably be capable of producing whatever effect wine would produce upon the system, and indeed may be less objectionable for administration, more agreeable, and more saleable than are many grades of actual wine; yet such a mixture is not actual wine, and should not be presented as such."

In treating of malt liquors, the author does not endorse the view that strychnia, if added to beer, would be thrown out of solution by the tannic acid of the hop. The tannin of the hop does not remain in beer. "Moreover the insolubility of the tannate of strychnia in 20,000 parts of water is by no means assured, and with the solvent action of acetic acid, as in beer, is quite improbable." Further, where strychnia is used, the hops will either be greatly reduced in amount or dispensed with altogether.

We may finally characterise this work as a valuable complement to the labours of Mr. Allen, Professor Wanklyn, and others of the much-abused public analysts who are successfully striving to place the chemical examination of food upon a sound and sure basis.

Report of the Geological Survey of the State of Missouri, including Field Work of 1873-1874. With 91 Illustrations and an Atlas. By GARLAND C. BROADHEAD, State Geologist. Jefferson City: Regan and Carter.

THIS is a decidedly unreadable book, and yet as a work of reference we must pronounce it a most valuable contribution to geological literature—a store-house of authenticated facts placed at the disposal of men of science.

After a historical sketch of the progress of mining and metallurgy in the State of Missouri, we find an account of its caves, water-supplies, soils, timber, minerals, and rocks. Then follow more detailed notices of the geology of the various counties, and a description of the lead regions of south-western and of central and south-eastern Missouri, and of the iron ore deposits of the State. In the appendices are given a description of the lead-mines of upper Louisiana, the metallic statistics and mineral springs of Missouri, and an abstract of the chemical analyses executed by the department. These latter refer chiefly to coals, limestones, waters, lead and zinc ores, and slags.

The presence of cadmium in the blende from the south-western region is a fact of some interest. In its determination the samples usually taken were of the weight of 20 grms. The cadmium was first thrown down by means of sulphuretted hydrogen, but as the precipitate was accompanied by much zinc it was re-dissolved in hydrochloric acid, and again thrown down in the same manner. It was next dissolved in sulphuric acid, the slightest excess being carefully avoided, and a third time precipitated as sulphide. This time the separation was found to be perfect. The cadmium sulphide was then re-dissolved, precipitated, and finally weighed as oxide. The cyanide method was also used for the separation of zinc and cadmium, and the mean results obtained all agreed within 0·05 per cent.

The mineral productions of the State appear extensive and varied. Iron occurs in the forms of bog-ore, limonite, goethite, red hematite, spathic ores, ankerite, specular iron, and sulphurets, The latter is generally diffused, "occurring in most of the coals, shales, and slates, and scarcely any limestone is free from it." Gold is found, though not to any great extent, in the drift sands of North Missouri. Silver is found only associated with lead. Blende abounds at Granby and Joplin and in other parts of the South-west; it is also found in the coal "pockets" of Central Missouri, in the lead mines of the same district, in the St. Louis limestone, and in the coal-measure limestone. "Iron-stone concretions often inclose a nucleus of zinc-blende. Fragments of plant-remains often have minute cracks filled with blende, and it occurs in the interior of fossil shells." Silicate and carbonate of zinc occur at Granby and Joplin, and zinc-bloom, though rarely in the central and southern parts of the State. Greenockite (cadmium sulphide) is associated with blende in the South-west. Copper is not specially mined at any part of the State, though it exists in small quantities widely diffused, as malachite and as copper pyrites. Nickel and cobalt occur as sulphides and arsenides, distributed through the galena, especially at Mine La Motte. At the Bluff Diggings the ore was found to contain—

Copper	1·00
Nickel	18·10
Cobalt	13·90
Lead	17·45 per cent.

These ores are not washed, but alternate layers of the ore and of charcoal are placed on a heap and roasted. When thus freed from sulphur they are smelted in a blast-furnace, which yields a matte of nickel and cobalt, some little copper, and traces of lead. Its sale price on the spot is 75 cents per pound.

Millerite (nickel sulphide) is found in the St. Louis limestone in beautiful hair-like crystals, occupying drusy cavities and resting on calcite or on fluor-spar. Lead occurs in profusion as galena and carbonate. Pyromorphite (lead-phosphate) is more rare. Wolfram is found in Madison County, and manganese and manganiferous iron in Iron County.

Sulphate of baryta, locally known as "tiff," is found in the lead mines of Cole, Miller, and Morgan, and is used, as in Europe, for adulterating white-lead. Mineral oils are widely diffused, but nowhere occur in quantity. The saccharoid sandstone, which is 100 feet thick at Cape Girardeau and 133 feet at St. Charles and Warren, is an excellent material for the glass manufacture.

Electricity: its Theory, Sources, and Applications. By JOHN T. SPRAGUE, Member of the Society of Telegraph Engineers. London: E. and F. N. Spon. 1875.

THE Author having contributed to the "English Mechanic" a number of articles on Electricity, now collects them for the first time; and his object in making this compilation has been to review and systematise the leading facts of the science, and thus to provide for general readers, interested in the subject, a *catalogue raisonnée*. The work is divided into twelve chapters, followed by a short "Dictionary of Terms." The first chapter is introductory, and at the outset our knowledge is divided into Metaphysics and Physics, the latter being designated "the basis of all our real knowledge." Various definitions follow:—Matter, elements, atoms, atomicity, molecules, chemical notation, ether. These definitions are slight, and sometimes very incomplete, and even inaccurate. Thus we are told that "the atom has several relations to force, while gravity, heat, and chemical affinity are the only forces specified." Again, of the ether it is said that astronomy affords us actual evidence of its existence, yet that we know "absolutely nothing" about it. Although, indeed, astronomy furnishes one proof of its existence, it is, we think, by no means the principal proof, and no one who has studied the phenomena of polarised light can say that we know absolutely nothing of the ether. In discussing valency, we are told that "one of the oldest ideas of the atom was that matter had no real existence, and that atoms were simply centres of force;" but surely this was one of the later phases in the history of the atomic theory. The division of molecules into "molecules which are also atoms," and "molecules formed of two similar atoms," is singularly perplexing to the student, and surely a very novel idea. Finally, we have a classification of the forces, and an expression of the belief that electricity, like heat, is a mode of motion. Altogether we think this introductory chapter will be of little use to the general reader unless he has already thought over for himself the various subjects which it discusses, and can thus discriminate between hypotheses and facts, and at the same time supplement the somewhat partial knowledge which it conveys. The second chapter, which occupies about one-seventh of the book, treats of static electricity. This subject is treated in a somewhat abstruse and occasionally very hypothetical form: for example (p. 22) it is pure hypothesis to assume that the molecules of bodies are spherical, and that if they are caused to rotate about a vertical axis, they will become ellipsoidal, and that a substance is then said to be polarised. The theory of induction developed therefrom, which we are told is very different from Faraday's theory, is equally hypothetical. The actual subject of induction (p. 31) receives but slight discussion, and the author passes on at once to electrical machines. The action of the Holtz machine is described, but in so complicated a form that we are convinced no general reader could follow it for a moment. That important instrument, the Leyden jar, is neither figured nor described, because it is so well known. The third chapter treats of magnetism, the fourth of galvanic batteries. This latter contains a very fair account of the existing batteries, including several of the new forms, and at the end of the chapter a list of 11 batteries is given, with the various purposes for which they are most suitable. The fifth chapter, occupying more than 50 pages, is on the very important subject of electrical *measurement*; the author has evidently given much attention to the matter, and the account which he gives is valuable, and will be appreciated by those engaged in practical telegraph work, as also will the following chapter on conductivity and resistance. In the seventh chapter the *current* is discussed, and this is commenced by a statement of Ohm's law. At curious diagram on p. 204 attempts to explain the action of the galvanic current by a mechanical analogy. This chapter is appropriately followed by one on electromotive force, wherein will be found (p. 217) a table giving the value of no less than 29 units. Final chapters treat of electrolysis, electro-magnetism, and electro-metallurgy. A short dictionary of terms concludes the work. The Electric Telegraph oddly enough receives only a few pages of notice at the conclusion of the book. One of the first things we look for now-a-days in a work on Electricity is a definition of that much-used (and by beginners much-abused) word *Potential*. Mr. Sprague does not employ it, because, according to his view, "at present it has really no definite and accepted meaning"; in support of this assertion he quotes the definitions of Latimer Clark and Fleeming Jenkin, and shows that they are at variance. He considers that the word is almost always used in place of tension or electromotive force. Again, the Farad is a term which does not always receive the same definition.

The book bears evidence of much thought, but it is somewhat patchy, and very jerky and variable in style. We do not think that it can be useful to the general reader, or indeed to anyone but the advanced student. To him we recommend a perusal of the work. It is often eminently suggestive, and if he be already well grounded in electrical matters, he will find much in Mr. Sprague's book to excite his thinking faculties.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE limited space at our disposal will not allow of more than a brief notice of the 1875 meeting of the British Association, which commenced on August 25th at Bristol. The local committee, of which Mr. W. Lant Carpenter and Mr. F. Clarke were secretaries, had made every possible arrangement for the comfort and convenience of the visitors. There were not wanting incidents to remind the members of the rapid progress in science during the 39 years which had elapsed since the previous meeting of the Association in Bristol. It was at Bristol in 1836 that the Mechanical Section was first instituted, and the President in his inaugural address drew attention to the fact that at a meeting of that section Dr. Lardner expressed the opinion that no steamboat would ever cross the Atlantic. Shortly after, however, the *Sirius* steamed from Bristol to New York in 17 days, and was soon followed by the *Great Western*, which made the homeward passage in 13½ days. We have also been reminded that at the Bristol meeting in 1836 Andrew Crosse expressed the belief that by means of electricity instantaneous communication with all parts of the world would one day be accomplished. At the second *soirée* this year a display of telegraphic instruments was made by the Post Office and telegraphic communications were maintained with Paris, London, and other places throughout the evening.

In introducing Sir John Hawkshaw as President for the year, Prof. Tyndall said: "In him I doubt not you will have a wise and prudent head, a leader not likely to be caught up into atmospheric vortices of speculation about things organic or inorganic, about mind or matters beyond the reach of mind, but one who, struggling Antæus-like, with his subject here to-night, will know how to maintain throughout a refreshing contact with his mother earth. I have looked forward for some time to the crowning act still in prospect of his professional career, to give our perturbed spirits rest in crossing the Channel in visiting our fair sister France. But pending that great achievement, it is his enviable lot to steer this British Association through calm waters to a haven of, at all events, temporary rest—rest all the more sweet and needful from the tempestuous weather which rasher navigators who preceded him thought it their duty to encounter rather than to avoid. To his strong hand I commit the helm of our noble barque, wishing him not only success, but triumph in that task he has undertaken, and which I now call upon him to fulfil."

Sir John Hawkshaw, F.R.S., delivered a long and able address on the origin, history, and progress of the science of engineering.

Several of the sectional addresses possessed unusual merit; they do not, however, admit of sufficient compression for us to give abstracts of them. The papers on subjects connected with physical science were not so numerous as on some former occasions. Several of those read, however, were of great value.

Professor Everett presented the report of the Underground Temperature Committee, which has now been in existence eight years. He gave an interesting account of investigations in the St. Gothard Tunnel. The distance now penetrated is more than a mile and a half at each end, and the temperature is found to be higher in the Swiss than in the Italian end, although the thickness of rock overhead is less. The length of the tunnel when completed will be nine miles, and the greatest depth beneath the surface two miles.

Dr. Guthrie, F.R.S., read a paper "On the Measurement of Wave Motion." By means of cylindrical troughs, he shows that the rate of undulation varies inversely as the square root of the diameter. This confirms the assertion that the rate of wave progress varies directly as the square root of the wave length; because the rate of recurrence must vary as the rate of progression divided by the path.

Dr. J. Hopkinson read an important paper by Professor Stokes and himself "On the Optical Properties of Titano-Silicic Glass." The principal object for the experiments, viz., the production of a titano-silicic glass suitable for destroying the secondary spectrum in an achromatic combination, has not been attained.

Professor Osborne Reynolds read a paper "On the Refraction of Sound by the Atmosphere." By means of an electric bell, the author found that with a cloudy sky and no dew the sound could be heard farther with than against the wind; but with a clear sky and a heavy dew, the sound could be heard as far against a light wind as with it.

Mr. J. A. Fleming read a paper "On the Decomposition of an Electrolyte by Magneto-Electric Induction." This paper appears in full in "The Electrical News and Telegraphic Reporter."

The Rev. S. J. Perry, of Stoneyhurst College, made some remarks on the transit of Venus. With regard to the stations in the North, Father Perry said they were left to the care of the Russians, and the English, Americans, Germans, and others confined their observations to the southern hemisphere. As it was mid-winter the sun was very nearly on the line of the southern tropics and nearly vertical over the eastern border of Australia. There were primarily five English expeditions, but as these were subdivided, there were nearly 20 stations of observation. His station was Kerguelen, to the south-west of Australia. On the morning of the transit they divided into three parties, and were so placed that, with the Americans, they formed four parties, about eight miles distant from each other. They saw the sun very well until almost the time that Venus was coming on to the sun's disc, and they had the external contact as well as could be expected, for there never could be absolute certainty with regard to such a point. They continued very well until they had taken the bisection by the planet of the sun's disc, but then there was just one little cloud that came and placed itself right over the planet and remained till 10 minutes after the commencement of the transit. At the other stations they were able to make observations of the ingress. At his station they were able to get very good observations and photographs of the internal points of contact and also of egress. The results of the observations would not be ascertained for at least 7 years, the determination of their longitude occupying a very long time.

M. Janssen contributed a paper on the same subject. By using coloured glasses, so selected as to allow the light of the corona to pass, he succeeded in seeing the planet for 10 minutes previous to its entering upon the sun's disc, His observations establish the existence of an atmosphere to Venus.

Sir William Thomson illustrated by diagrams the description of his experiments "On the Effects of Stress on the Magnetism of Soft Iron." With steel wire the magnetism diminished when weights were attached to the wire and increased when they were taken off, but when iron wire almost as soft as lead was used these results were reversed.

Professor W. F. Barrett read a paper on the Effects of Heat on the Molecular Structure of Steel. In the course of experiments he had found that steel of any thickness, if heated to a certain temperature, ceases to expand, and the steel wire does not increase in temperature. The length of time during which this abnormal condition lasts varies with the thickness of the wire and the rapidity with which it can be heated. No further change takes place till the heat is cut off, and then when the point is reached at which the change took place, an actual increase in temperature occurs sufficiently great to cause the wire to glow with a red heat. It was strange that this afterglow had not been noticed before.

Some interesting experiments on magnetised rings, plates, and discs of hardened steel were exhibited by Mr. P. Braham.

Mr. Glaisher presented the report of the Committee on Luminous Meteors. A mass of meteoric iron fell on the 24th August, 1873, at Maysville, California, and was one of the very few pieces of metallic iron the actual descent of which had been witnessed. In the following month a number of meteorites fell near Khairpur, in the Punjaub, and it is also related that in the month of December, when the British army halted on the banks of the Prah, an aërolite fell in the market-place at Coomassie, and was regarded as a portent of evil by the natives. On the 14th and 20th of May aërolites fell at Castalia, in North Carolina, and the last stone fall of the present year took place on the 12th February, 1875, near Iowa. In England no detonatory meteor had been observed this year, and the brightest meteor occurred on the 1st September, 1874.

A meteor burst with a loud detonation over Paris on the 10th February, of great size and brilliancy, and left a cloud like a streak of light in its track for nearly half an hour. No duplicate observation of it was obtained in England. Another fireball fell at Orleans on the 9th March, and of this two good observations were obtained in England—in London and Essex. The problem of the connection between comets and meteors had now become so great as to pass beyond the power of the Association to grapple with it in its fullest extent. There was a great deal of work yet to be done, and he was glad to state that Professor Le Verrier, of Paris, had made arrangements with some six thousand gentlemen who in different parts of France and her colonies would devote night after night to the study of meteoric astronomy.

Professor Herschell added some particulars of the work of the committee with regard to the connection between meteors and comets, and stated that the aërolites were stone bodies differing very little from volcanic rocks.

Sir William Thomson said the evidence they had with regard to the mass of comets was altogether negative. There was nothing in observations to justify the assertion of their being so extremely small as sometimes said. It would be interesting to obtain some definite knowledge of the mass of a comet, by comparison with some other body of the solar system. The whole subject still presented great difficulty, and the whole of the wonderful phenomena were not fully explained, but he thought that on the whole they might consider it quite established that the comet's tail was really a train of meteors.

Mr. G J. Symons presented the report of the Rainfall Committee. During the past fifteen years the number of stations has been raised from 241 to nearly 2000.

Relative to the rainfall in Monmouthshire and the Severn Valley on July the 14th, Mr. Symons said the rainfall was simply a mass of vapour that came up from the west, and it commenced at Tenby between midnight and one a.m. on the morning of the 14th July. It travelled at the rate of about 18 miles an hour, and at four p.m. in the same day it passed off by the north coast to Norfolk. As to its breadth it took about 36 hours to pass over any given points. The quantity of water that fell varied very much over the whole country, and was deepest the west side of a line drawn from the Isle of Wight to the Isle of Sheppey, and thence to the north-east. Some stations in Monmouthshire and Glamorganshire had a rainfall of over 3 inches; at fourteen stations the rainfall exceeded 4 inches; at six it was over 5 inches; and at Tintern Abbey, usually considered a dry station, the rainfall was 5·31 in 24 hours. This was followed with storms in which from 2 to 3 inches of rain fell. At Tamunk there was the extraordinary fall of 9½ inches in 8 days, or one-third of the average rainfall for a year.

Professor Hennessy read a paper on "The Influence of the Physical Properties of Water on Climate." The author believes that of all substances largely existing in nature water is the most favourable to the absorption and distribution of solar heat throughout the external coating of the earth.

The same author also read a paper "On the Possible Influence on Climate of the Substitution of Water for Land in Central and Northern Africa."

Captain Abney, R.E., in a paper "On the Increase of Actinism due to Difference of Motive Power in the Electric Light," described the results which had been observed in some experiments made for Government upon magneto-electric machines.

Prof. H. A. Rowland, of Baltimore, described his method of testing the distribution of magnetism in iron bars and also his experiments on the magnetising function of iron, nickel, and cobalt.

The report of the Committee on the Thermal Conductivity of Rocks affirms that quartz is the best and coal the worst conductor.

A paper was contributed by Prof. Osborne Reynolds "On the Force caused by the Communication of Heat between a Surface and a Gas."

The following papers were also read by M. Janssen "On the Eclipse of April 5th, 1875," "On the Actual Position of the Magnetic Equator in the Gulf of Siam and the Gulf of Bengal," "On Mirage at Sea."

CORRESPONDENCE.

AËRIAL LOCOMOTION.

Sir,—I received, some time ago, an article from your valuable paper entitled, "Aërial Locomotion; Pettigrew versus Marey."

I cannot leave unanswered the 20 pages devoted to me; but as I have little taste for polemics, I will not intrude to the same extent on your space. I intend to sum up the chief heads of that accusation, in order to answer them respectively.

1st. The writer states that in a work on the flight of insects, I had pointed out the figure-of-eight track made by their wings, and that, on a demand of Mr. Pettigrew, I have acknowledged that this physiologist had the priority over me relatively to that observation.

2nd. That, later on, "having evidently changed my views," I have declared that, in spite of this conformity, my theory and Mr. Pettigrew's differ materially.

3rd. That, in order to arrive at this demonstration, I had reproduced and altered Mr. Pettigrew's figures.

4th. That, concerning the flight of birds, I had copied Mr. Pettigrew in the same indiscreet manner, to substantiate which the writer of the article accumulates 14 pages of quotations, which, he says, might be more numerous.

I see in this article nothing but the sincere expression of a great love for scientific truth. This feeling finds way in the final sentence in which the author assures that my process "saps the foundation of science."

Before bringing such an accusation it would have been perhaps fair for the critic to be enlightened on the question. If he had consulted Mr. Pettigrew he would certainly have learned that I never accepted his theory on the flight of insects, no more than he accepts mine on the flight of birds. Under these circumstances it was not easy to plagiarise.

My severe accuser may have been misled by the excess of conciseness under which Mr. Pettigrew labours every time he happens to quote my answer to his complaints. In fact he presents it in the following way:—"I have ascertained that in reality Mr. Pettigrew has been before me, and represented in his memoirs the figure-of-8 track made by the wing of the insect, and that the optic method to which I had recourse is almost identical with his. . . . I hasten to satisfy this legitimate demand, and I leave entirely to Mr. Pettigrew the priority over me relatively to the question as restricted." — *Comptus Rendus*, May 16th, 1870, p. 1093.

Now, the suspensive stops which break the quotation stand for the end of the sentence, which runs thus: "But we differ entirely on the interposition of the trajectory seen by us both."

I do not suppose that Mr. Pettigrew has purposely suppressed those last words: perhaps they have escaped him, for I am loth to believe that any author would intentionally mutilate a text; the more so as my sentence quoted in that way has no meaning. What would be the meaning of "leaving the priority as restricted" if no restriction is mentioned?

Then I had not to change my views in order to reject Mr. Pettigrew's theory on the flight of insects. I rejected it as soon as I knew it, and precisely on account of the figure I am accused of having purposely altered.

By referring to page 233 of the Transactions of the Linnean Society, vol. xxvi., and to my work entitled "Animal Mechanism," fig. 86 (page 201 of the English translation), the reader will see that I have very accurately counterdrawn the figure given by Mr. Pettigrew, with arrows showing the direction of the motion. He will see, besides, that the English author expressly states that the wing of the insect is turned completely, and that its "posterior or thin" margin takes the place of the "anterior or thick" margin, *et vice versa*, which implies an active "retournement" of the wing.

But this figure must be looked for in the article of the "Transactions of the L. S.," published before Mr. Pettigrew's complaints and before my answer. It is no more to be found in the posterior publications.

This figure, without doubt, has been lost. It has even been forgotten to such an extent that when Mr. Pettigrew traces afterwards the trajectory of an insect's wing, he points his guiding arrows not in the same direction as formerly, but in the direction according to which I had always pointed them.

With reference to the flight of birds, my accuser piles up quotations. He compares texts, and every time he meets with similar expressions in the English and French books, he raises a cry of plagiarism, as if it were possible to treat of the flight of birds without speaking of wing, child's kite, inclined plane, sculling, &c.

This overflowing copiousness of quotations has the effect, perhaps not intended, of fatiguing the mind and misleading the judgment of the reader. Would it not be much more for the purpose to take Mr. Pettigrew's own opinions on the debate ?

Mr. Pettigrew, in his last work ("The Animal Locomotion"), devotes 10 pages to impugn my theory on the flight of birds, and a special chapter to show to what extent his own theory differs from it. This work of Mr. Pettigrew, as well as my book entitled "Animal Mechanism," having been respectively translated in both languages, every one can compare them, and by so doing, will ascertain that two authors could not easily have treated the same subject with more widely dissimilar methods and arrived at more different conclusions.

But those who have only read the article in the "Quarterly Journal" may form an estimate of me which I am anxious to correct.—Believe me, dear sir, yours very respectfully,

MAREY.

Professeur au College de France.

PROGRESS IN SCIENCE.

MINING.

From the Annual Reports of the Inspectors of Mines, issued during the past quarter, it appears that in the year 1874 there were 4332 mines working under the Coal Mines Regulation Act. The number of persons employed was 537,178 in Great Britain and 1651 in Ireland. The total number of lives lost in our coal and iron mines was 1056, as against 1069 in the previous year. In our metalliferous mines there were only 103 deaths from accidents.

A valuable paper by Mr. Handel Cossham, Mr. Wethered, and Mr. Saise, descriptive of a part of the Bristol coal-field, was read before the Geological Section of the British Association, and has since been published, with illustrations, in the "Colliery Guardian."

It is proposed by Mr. A. B. Boullenot, of Paris, that safety lamps in fiery pits should be furnished with air introduced entirely from outside the mine. To this end compressed air is forced from an air-pump at the surface down a system of pipes leading to the lamp. The lamp itself is of improved construction, and specially adapted for receiving compressed air.

An interesting paper on the history of the methods of draining mines by non-rotating steam appliances, by Mr. Stephan Holman, has been published in the "Transactions of the Chesterfield and Derbyshire Institute of Mining Engineers."

The gold districts of the Province of Otago in New Zealand have been reported on by Mr. G. F. Ulrich. The observations were made during a tour of inspection in the early part of this year, and refer not only to the auriferous quartz-reefs and crushing machinery, but also to the occurrence of copper ore, cinnabar, antimonite, and brown coal. Appended to the Report is a description of the German Treppenrost or step-furnace for the combination of brown coal.

Gold-mining in Japan has been the subject of a Report by Mr. H. S. Munroe, of the Imperial College of Tokai. The richest of these gold deposits are those at Toshibetsu, which are worked in a primitive manner by the Japanese; but even these are extremely poor, and would probably not repay the cost of introducing improved methods of working.

Mr. Mark Fryar his issued a Report from Moulmein, dated last May, in which he describes several mineral localities in Burma. Gold-mining is carried on at Shwe-gyeen, and iron ores appear to be generally distributed, but are worked only on a small scale and in primitive fashion.

It may be worth noting that the celebrated Adalbert shaft of the silver-lead mines of Pribram in Bohemia, has just reached the extraordinary depth of 1000 metres. The events was celebrated by festivities on the 13th, 14th, and 15th of September.

METALLURGY.

At the Annual Provincial Meeting of the Iron and Steel Institute, held this year at Manchester under the presidentship of Mr. Menelaus, a paper "On the Use of Caustic Lime in the Blast-Furnace" was read by Mr. I. Lowthian Bell, M.P. The author described in detail some researches carried out at the Clarence Works with the view of determining what advantage, if any, results from calcining the limestone before using it as a flux. In a furnace 48 feet high, of a capacity of 6000 cubic feet, and with a make of about 200 tons per week, the saving of fuel effected was only insignificant, but the yield of iron from Cleveland ores was increased, and the quality of the metal was improved. In a furnace of better construction, 80 feet high, holding 20,000 cubic feet, and yielding 350 tons weekly, there appeared to be absolutely no advantage in employing calcined instead of raw limestone. It appears, however, that in none of the trials was more than about one-half of the carbonic anhydride expelled from the stone.

Some improvements in the form of moulds for casting ingots of steel have been effected by Mr. W. Hackney, who explained to the Institute the construction of these moulds. Being subject to very sudden and violent alternations of temperature, the ordinary moulds frequently crack; but in Mr. Hackney's design the thickness of metal at different parts of the circumference of the mould is so adjusted that the expansion when heated is pretty equal all round, and the cracking is thus to a large extent prevented.

An improved form of hearth lately put in at one of the blast-furnaces of the Tees Iron Works was described to the Institute by Mr. Charles Wood, whose methods of utilising slag are now well known. Mr. Wood's hearth combines the closed-hearth system with an open fore-part, and a new water-plate arrangement over which the slag is drawn, and by which it is prevented from destroying the brick work or fire-clay through which it issues.

Price's Patent Retort Furnace, which was brought before the Institute by Mr. Lowthian Bell, is an arrangement for avoiding the loss of heat which occurs in the gas-producers of the regenerative furnace. Several of these retort furnaces have for some time been in use at Woolwich.

Certain improvements in rotatory puddling-furnaces have recently been effected by Messrs. E. A. and J. A. Jones, of Middlesbro'. The improvements relate chiefly to the means of admitting and expelling water from the casing of the furnace.

Some experiments on puddling by the aid of a blast have been made at the Bull Bridge Iron Works, near Wolverhampton, and appear to be of some importance. The blast is injected through four tuyères at the top of the furnace, and by impinging on the flame, as soon as it has come over the fire-bridge, produces an intense heat, which hastens the puddling and is said to improve the character of the metal.

In blowing out a blast-furnace in the Middlesbro' district some crystalline products were found in the upper part of the "dead horse," and these have been analysed by Mr. G. Johnston, of Leeds, who has communicated the analyses to the "Chemical News." The mass of slag and metal under the hearth had probably been exposed for 10 or 12 years to a temperature not much below the melting-point of cast-iron. The crystals found in this mass are octahedral in form, and may be resolved into two portions, one part being malleable and the other easily pulverised. A sample of the latter yielded as much as 29·58 per cent of graphitic carbon.

M. Boussingault's "Etudes sur la transformation du fer en acier par la Cementation" will be found in a recent number of the "Annales de Chimie." The objects of his experiments were to determine how long the iron and carbon are in contact at a red heat during the process of cementation; what is the temperature of the interior of the chests at different stages of the process; and what the nature and quantity of the substances acquired or lost during the changes effected by cementation.

It seems likely that metallurgists may soon be able to use petroleum as a fuel, if reliance can be placed on what has been called "A New System of Oil Metallurgy." Experiments made in New Jersey by Dr. Eames have shown, it is said, that petroleum may advantageously be used in the re-heating furnace for iron. The petroleum in the Eames furnace is converted into vapour in the "generator," where it drips from shelf to shelf, and during its flow meets a slow opposing current of steam. The mixed vapours pass into a chamber where they are brought in contact with a blast of air, and combustion consequently effected.

The metallurgist, not less than the physicist, is interested in Mr. W. C. Roberts's recent researches on certain alloys of silver and copper which have been brought before the Royal Society. These silver-copper alloys possess such molecular mobility that it is difficult to obtain an ingot of homogeneous composition. The melting-points of a series of alloys of silver and copper are represented graphically, and the curve exhibits a somewhat rapid fall from pure silver to the alloy employed for our silver coinage, which contains 925 parts of silver in 1000 of the alloy, and corresponds approximately to the formula Ag_7Cu. The alloy of lowest melting-point is represented by the simple expression AgCu. Liquidation appears to result from unequal cooling of the molten mass, and is modified if the cooling be retarded. The experiments made on cubes of various alloys showed that the molecular arrangement is largely dependent on the rate of cooling.

Under the name of *Dysiot* a new alloy has been introduced in Germany. Its analysis has yielded, copper, 62·30; lead, 17·75; tin, 10·42; zinc, 9·2; iron, traces.

According to Mr. Sergius Kern the chrome-iron ore of the Ural mountains has been used in the preparation of a chrome-iron alloy, which possesses a remarkable degree of hardness, and may be used as a substitute for spiegeleisen in the preparation of steel by the Siemens process.

Microscopy.—Mr. H. J. Slack* supplies the following valuable information respecting the use of Mr. Wenham's "Reflex Illuminator."† "If Mr. Wenham's Reflex Illuminator is used under the circumstances for which he especially contrived it little difficulty will be found with suitable objects. The light, as he explained, penetrates only where the object makes a new surface on the slide, and 'acts,' to use one of his familiar phrases, 'like a hole in a dark lantern.'" The effect is so admirable upon many objects, such as scales of insects, certain micro-fungi, minute algæ, desmids, diatoms, &c., that every one who has successfully tried it must wish to add to its range of utility, and this may be easily done. It will be found that most balsamed objects, and many in which the covering glass lies very close to the slide, give with it so much false light when ordinary objectives are employed that the result is very unsatisfactory. This false light will be found in many cases so oblique that it can be got rid of by using an objective with a small angle, or temporarily reducing the angle of an ordinary high power by a movable stop. For example, a slide of *Surirella gemma* and this illuminator exhibited no false light with a glass of about 7°; some, but not much, with a fine one-fourth made on Mr. Wenham's new formula, and having an angle of 150°; too much to be endurable with Powell and Lealand's immersion one-eighth full aperture; and none with the same glass and with a stop limiting the rays admitted to about 90°. Many slides of butterfly and other scales taken at random from a cabinet become manageable with reduced apertures, and the effects, when the plan succeeds, are very curious, beautiful, and instructive. Mr. Wenham has alluded to the changed aspects obtained by rotating the apparatus when employed upon the so-called Podura scale *Lepidocyrtus curvicollis*, and similar observations may be made with regard to Lepisma scales, and those of various insects allied to Podura. Indeed it is not prudent to pronounce an opinion upon any scale of difficulty until this method has been tried and all the aspects it produces considered in their mutual relations. It is by no means intended to advise microscopists against the use of this apparatus with large-angled glasses upon objects mounted so as to be fit for it; but when slides fail the observer is recommended not to abandon the plan, but to reduce the angle of the glass and try again, and with good chances of success. The apparatus has a remarkable power of increasing both the penetration and resolution of good objectives.

* Paper read before Royal Microscopical Society, 2nd June, 1875.
† "Quarterly Journal of Science," vol. ii., p. 400.

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