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I. CONTINUOUS RAILWAY BREAKS.

By Fred. Chas. Danvers.

THE Report recently issued by the Royal Commission on Railway Accidents brings very prominently to notice the fact that the break-power at present ordinarily applied to passenger-trains is inefficient, and that to this fact is due the occurrence of many accidents which, by the application of better mechanical contrivances, might have been avoided. Former Committees and Commissions appointed by the Legislature to enquire into the subject of railway accidents have generally in their reports assumed that the principle of self-interest, in its influence on Railway Companies, should be relied on as the best safeguard against accidents; that the liability of the Companies to pay a serious amount in compensation in individual cases was a strong inducement to railway directors to work the line carefully, the Companies having thus a direct pecuniary interest in keeping their lines safe. These assumptions have not been borne out by subsequent experience. One of the members of the late Royal Commission, in a separate report, has shown to what extent the self-interest of Railway Companies has any practical bearing on the subject, and, in order to make the matter more clear, he has deduced from the expenditure returns made by Companies to the Board of Trade the following figures, which represent an average on the several classes of expenditure for a series of years:—

	Per cent.
1. Locomotive power...	30
2. Traffic expenses	28
3. Maintenance of way and works, &c.... ...	18
4. Miscellaneous charges, general, legal, and Parliamentary; rates and taxes, &c. ...	14
5. Repairs and renewals of carriages and waggons, &c.	9
6. Compensation for personal injuries, &c. ...	1
	100

From these statistics it will easily be gathered how far the Companies have a direct pecuniary interest in keeping their lines safe. The amount paid in compensation for accidents exceeds but very little a half per cent on their gross receipts: to this, of course, must be added something for injury to the lines and rolling stock caused by accidents, but no data are available for the calculation of the amount.

It thus appears that the pecuniary loss to Companies caused by accidents is almost infinitesimal, but the cost that must be incurred by them in order to provide many of the necessary means of safety represents by no means an inconsiderable outlay. But if the Companies refuse to face the matter boldly on public grounds, and in the interests of the travelling public, such alterations as may be necessary in the present control over the Companies must be given to the Board of Trade, or other Government department, by special legislative enactment. The Companies have been repeatedly warned, and they have as often neglected the admonitions addressed to them. The question now, therefore, rests with the public, and they will assuredly now speak out, in their own interests, in a manner that cannot fail to command respect from the most unimpressible Boards of Directors. The subject has been taken up by the scientific press, whence it will, in due course, be introduced to the public by the daily press; and the ball, having been once started, will certainly not cease to roll until the public have secured for themselves such immunity from accident on railways as can be afforded by the forced adoption of any known means of precaution or mechanical application.

From the Reports by former Commissions, and by the

Board of Trade Inspectors, it is manifest that a deficiency of break-power is one not of the least important causes at present responsible for accidents to passenger-trains. It is true that the speed of railway travelling has not increased sensibly within the last thirty years, but the conditions of train-service have materially altered within that period, and they are, by the effect of natural causes, continually changing in a manner which demands constant attention and the application of such means for facilitating traffic as may from time to time be devised for that purpose; for instance, the trains are more frequent and following each other more closely than before, block signal-stations are necessarily brought nearer together, whilst at the same time the weight of trains, consequent upon improved and more capacious carriages, and the increased number of passengers, necessarily attain a greater amount of *vis viva*, demanding the exercise of greater force in order to bring them to a stop even within the same distance as was formerly the case. The old-fashioned hand-screw breaks—which ought to have been abolished some years ago on all passenger-traffic lines of railway—are, viewed with the light of improvements which modern science has introduced, at best both clumsy and make-shift appliances for the purposes to which they are applied; and it was stated in evidence taken by the Royal Commission that the break-power at present generally applied is insufficient to stop a train within the distance generally existing between the distant signal and the home signal of a station. In the case of heavy trains, running at a speed of from 35 to 40 miles an hour, the ordinary screw breaks are insufficient to bring them to a stand under half a mile, and when travelling at a higher speed under 1100 or 1200 yards, whilst heavy fast express trains cannot—with the ordinary break-power—be pulled up in many cases under a mile and a quarter.

Besides the inefficiency of the power of ordinary screw breaks, another important objection to their use is the time required to bring them into action. The necessity for promptness of action in pulling up a train will be at once realised when it is remembered that in one second a train travelling at 60 miles an hour passes over 88 feet; at 45 miles an hour, over 66 feet; and at 30 miles an hour, over 44 feet. A train travels, that is to say, 100 yards—at 60 miles an hour, in 3·4 seconds; at 45 miles an hour, in 4·6 seconds; and at 30 miles an hour, in 6·8 seconds.

It has been stated, by one of the Board of Trade Inspectors, that if the continuous break system could be adopted it would be one of the most fruitful sources of saving collisions; and he further remarked that out of eighty-one accidents into which he enquired in one year, had continuous breaks been able to have been worked by the engine-drivers, at least in thirty-five cases the accident would have been mitigated, if not prevented altogether. The Royal Commission, in their recent Report, remark—"Accidents of the nature of collisions are generally the result of several contributory causes, but the amount of available break-power is obviously a matter of the greatest importance as a means of preventing them and of modifying their consequences. Our own enquiries confirmed the impressions which the Inspecting Officers' investigations of accidents led us to form, that not only was there generally an insufficiency of controlling power in trains, but also that the distance within which a train running at high speed could be stopped by the break-power ordinarily in use was not ascertained with any approach to accuracy." Consequently the Commissioners applied to the Railway Companies to institute a definite series of experiments, to test the amount of control given by the break-power ordinarily applied to their trains, and the effect of various systems of improved or continuous breaks. From the experiments carried out for this purpose it appeared that the amount of hand break-power usually supplied with the trains of the respective Companies failed to bring up the London and North-Western train within 2374 feet, that of the Caledonian Company within 3190 feet, that of the Midland within 3250 feet, that of the Great Northern within 3576 feet, and that of the Brighton within 3690 feet, the speed of the trains varying from 45½ to 48½ miles per hour. It must, however, be borne in mind that the trains with which these experiments were made were in the most complete order, and the guards and drivers had notice of the exact spot at which the signal to stop would be given. A large addition must therefore be made to those distances in practice, and unless much greater control is obtained over trains by additional break-power the Commissioners consider that to ensure safety the distant signals must be, for a level line, carried back to the distance of a mile. From the experiments made with continuous breaks, however, it is evident that there are ample means of controlling trains within much less distance by some of the various systems already in use.

Besides an improvement in break-power, it is also necessary that—whatever improvement upon the present system be adopted—a large proportion of the available break-power should be under the control of the driver, who is generally the first to become aware of apprehended danger. No matter how small the interval of time required for the driver to attract the attention of the guard, it may be of vital moment.

On this subject Capt. Tyler observed, in a paper recently read by him before the Society of Arts,—"When an accident occurs in which a carriage leaves the rails from failure in any portion of a train, it may be of great, and even vital, importance immediately to reduce the momentum of every part of it; and every extra second expended before this action is commenced may be a question of life and death." It will readily be understood how the safety of a train is increased by having the break-power under the control of the engine-driver as well as of the guard. In the case of a signal failing to work and to show sign of danger, the engine-driver will naturally, if an obstruction exists on the line, be the first to discover it; and, supposing the break not to be under his control, he must intimate danger to the guard by whistle, in the ordinary manner; but all this takes time, and between the interval of the driver's signal and the application of the break by the guard two or three seconds must inevitably elapse, during which interval the train has probably approached not less than 100 yards nearer to the impending danger, or nearer to fatal results. An instance in point has been given by Capt. Tyler, who, in the paper above referred to, cited an accident which occurred last November, near Wincanton, on the Somerset and Dorset Railway. In that case an up-passenger train for Bath was travelling at a speed of 35 miles an hour, when the leading wheels of the engine left the rails, from a defect in the permanent way. The engine ran thus for 200 yards before the driving wheels left the rails, but it then turned over on its side, 240 yards from the point of first disturbance. The engine-driver was killed, and the fireman and guard, who narrowly escaped with their lives, were severely injured. If, says Capt. Tyler, the engine-driver had been able at once to apply a continuous break throughout this train, on finding his leading wheels off the rails, it might have been pulled up with scarcely any damage to the rolling stock, and no injury to himself or any one else.

The same officer remarked, in his Report for 1872, referring to the great Railway Companies,—"It is mainly because sufficient attention has not been paid in past years to the various means of safety that the greatest Railway Companies of England appear so unfavourably at the head of the accident list." "The 238 train-accidents which occurred this year were all more or less of a preventable character. The means of prevention are well known, and have sufficiently often been urged, as well in individual as in general reports." Why these means of prevention have not hitherto been enforced upon the Companies is, that although it has been the practice of the Railway Department of the Board of Trade to urge upon Companies, by way of advice, the adoption of measures from time to time, as tending to diminish the risk of danger, they have no power of compelling the Companies to adopt them against the advice of their own officers.

In recent years no doubt many improvements have been introduced in the working of railways, in view to insuring increased security for passengers, all of which have necessarily been attended by increase of expenditure on the part of the Companies. So far as can at present be ascertained the prevailing weakness in our railway system just now is a want of efficient break-power; not that efficient breaks do not exist, but that the Railway Companies have hesitated too long to adopt them. The Royal Commission recommends that Railway Companies "should be required by Law to provide every train with sufficient break-power to stop it absolutely within 500 yards, at the highest speed upon which it travels, and upon any gradient on the line." This break-power, they further explain, should be sufficient to stop trains within 500 yards "under all circumstances," and Mr. Galt—one of the Commissioners—further explained, in a separate Report, that "the break-power that brings to a stand both portions of a train in case of its being divided by an accident is certainly the only kind thoroughly effective."

It is clear that nothing but a continuous break will satisfy the necessities of safety for railway travelling, as is shown by the evidences above referred to. There are many kinds of continuous breaks, however, and they have not all the same powers or properties, and in considering which is really the most efficient several considerations must be taken into account. On this subject Capt. Tyler has laid it down that a break should possess the following properties in order to render it thoroughly efficient, and safe under all but the most exceptional circumstances, against which, of course, no human ingenuity could devise adequate safeguards. A break, then, should be—

1. Simple and easy of control, especially by engine-drivers, but also by guards of trains.

2. Automatic in action in the case of an accident, or of a division of a train, without any interference from engine-driver or guards, so that the break-blocks may —on the separation of the couplings between any two carriages—be self-applied on every wheel.
3. Adapted to fly on, and not off, so that the blocks may be applied to and not released from the wheels, on failure of any of the parts, which would prevent the train from being started unless the couplings were complete and the whole apparatus in working order.
4. Instantaneous in its application—say to full force within one second of time—when operated by engine-driver or guard, or when self-applied.
5. Safe and simple in working—which does not necessarily imply simplicity in construction, as distinguished from risk and confusion in working.
6. Moderate in cost, as compared with efficient durability of parts and easy maintenance.
7. Capable of constant employment in the conduct of traffic, and not merely for employment in tests and in cases of emergency.
8. Provided with indicators for engine-drivers and guards, showing at a glance the condition of the break-power and the continuity of the connections.

The above conditions, coupled with those laid down by the Commissioners, that the break should be capable of bringing a train to a stand *under any circumstances* within a distance of 500 yards, seem sufficient for all practical purposes, and, if capable of being realised, not less than should be demanded in the interests of the safety of the travelling public. These conditions necessarily imply the use of a continuous break ; but before considering which of the numerous inventions at present in use mostly fulfils them, it may be interesting to describe briefly the principles attached to each of the best of them respectively.

The several classes of continuous breaks at present in use may be divided according to the kind of power employed to actuate them. They may be classified as follows :—1. The chain break. 2. The hydraulic break. 3. The vacuum break. And 4. The air break. It is not pretended here to give descriptions of every kind of break that might be included under the above headings severally, but only of those which are unquestionably the representatives of each class, in consequence of their undoubted superiority, so far as has been hitherto ascertained by actual experience gained in the constant use of them in general work on lines of railway.

The following particulars relative to these breaks are taken from the Appendix to the Report of the Royal Commission, and may therefore be considered as describing those forms of each which are most perfect of their class:—

1. *The Chain Break.*—The latest improvement in this form of break is that known as Clark and Webb's continuous break. A chain—or, in the newest form, a steel rope—runs the entire length of the train, underneath the carriages, terminating in the guard's van, by means of which the breaks are applied to the wheels of every carriage. In the experiments made for the Royal Commissioners with this break, the train was divided into sections of four or five carriages, which were placed in the following order with reference to the break-vans:—First, there were four carriages together, then a break-van. From this van the breaks of the first four carriages were worked, and also those of the four carriages following the van. The remaining five carriages were under the control of the van at the tail of the train. Each carriage was provided with its own length of chain and couplings. The chain passed over seven pulleys fixed under the framings of the carriage, and under one pulley carried by levers which were in connection with tension-rods attached to the break-blocks. When the chain throughout a section was coupled up, one end of it was made fast to the end of the extreme carriage of the section, and the other was led to a chain barrel hung under the framing of the guard's van at the other end of the section, close to the centre axle of the van. On the chain barrel, and also on the van axle, there were friction-wheels, and by releasing a weighted lever in the van the friction-wheels were brought into contact, and, if the van was in motion, the chain barrel was made to rotate. By this means the chain throughout the section to which the barrel belonged was tightened, the pulley carried by levers under each carriage already mentioned was raised by the chain, and the breaks were applied to the wheels. The van from which the breaks of a section, both in front of it and behind it, were actuated, had two chain barrels set in motion by the release of one weighted lever. To release the breaks the guard in each van had to put back the lever into its normal position, and secure it with a catch, at the same time surrounding and releasing the breaks by a weighted lever arranged for the purpose. In order to place the continuous breaks of the whole train at the command of the driver, a cord was passed over the roof of the carriages to the engine. This enabled the driver to release the catch

holding the weighted lever in each guard's van, but the guards could only apply the breaks to their own section. There was, however, separate cord communication throughout the train. The breaks on the wheels of the vans themselves were applied by hand only. The guard could also, by pulling a signal cord attached to a whistle handle, call attention of the driver in case of danger.

However well this break may work under ordinary circumstances, it must be clear to anyone that it labours under several inconveniences, and could scarcely be relied upon in extremely exceptional circumstances. In the first place a chain is no stronger than its weakest link, and an imperfect weld or subsequent injury may remain undetected until the occurrence of an emergency, when the whole break-power of a train might be rendered useless by the breakage of a single link or the failure of a strand of wire-rope. Again, the means of placing the break-power within the control of the driver are complicated, and supplemental to the break-apparatus itself; besides which there is the difficulty of getting this break to act quickly, from the length of the buffer-strokes between the carriages, owing to which it was found, on the North London Railway, that more than 2 feet of chain had to be wound up for every carriage; and it was explained to the Commissioners that, where long buffers are used, in a train of eight carriages there would probably be sixteen revolutions of the carriage-wheels necessary before the slack chain was wound up sufficiently to put the breaks on to the wheels. This, with wheels 3 feet 6 inches in diameter, implies 168 feet run by the train, after setting the break-power in motion, before it begins to make itself felt in bringing up the train. In experiments on the Midland Railway accidents occurred through the break being applied too powerfully and too strongly, which caused the couplings to break and the train to part in two. The Carriage Superintendent of the Midland Railway further explained that, in working Clark's break it loses power every carriage away from the van, as the power gets less the further it is applied by the chain. Consequently the break bites tighter on some of the wheels than on others, which causes a slack or rebound. While some of the carriages are being very much retarded by the break, others are not so much retarded.

In pointing out the defects in this and other breaks which were brought to the notice of the Royal Commission, it must be understood that the object in view is not in any way to deprecate some and puff up other breaks, but to show in what respects each break requires improvements, so far as the evidence concerning them respectively given before the Royal Commission seems to indicate their weak points.

2. *The Hydraulic Break.*—Barker's hydraulic break, which is the representative of this class of break, was applied to the engine and the whole of the passenger carriages of an experimental train; but the tender and two vans were fitted with breaks worked by hand-power. The hydraulic apparatus, as used at the trials, consisted of the following parts:—On the engine there was a double-acting steam-accumulator, consisting of a large-sized cylinder with a piston in it connected with a plunger working in a second cylinder, which was kept filled with water from the tender-tank. The piston in the former was actuated by pressure direct from the boiler, without the use of any pump, and in making its stroke the plunger in the smaller cylinder was made to force water, with any degree of pressure, into pipes leading to small hydraulic rams attached to the engine and carriage breaks. These pipes led the whole length of the train, the connections between the carriages being made with india-rubber hose furnished with ordinary unions. Each carriage was fitted with two of the hydraulic cylinders and rams just mentioned, and these were connected directly to the break-blocks in such a manner that on the ram of each cylinder being moved by the pressure of water from the accumulator, the break-blocks on one side of the pair of wheels to which the ram belonged were forced against the wheels, while the pressure of water against the bottom of the same cylinder caused it to recoil, as it were, and draw the tension-rods of the break-blocks on the other side of the pair of wheels, thus clipping each wheel between its two blocks. In this arrangement, of course, the cylinders attached to the break-blocks have to be loosely suspended, and free to move horizontally for a distance nearly equal to the length of the arms. This description answers also for the action of the engine-breaks. By reverse action in the accumulator the pressure in the break cylinders can be relieved, and the blocks taken off from the wheels. The amount of pressure in the pipes can be regulated by a reducing valve; also, in order to keep the power always ready for immediate use, it is an essential part of this system that the pipes and cylinders throughout the train should be always full of water. The breaks were not arranged so that they would be self-acting in case of a train parting asunder.

The testimony given in favour of this break by those who had experience of its working was generally favourable; but it appears in the evidence that it is in no sense automatic, and it is entirely under the control of the guard, and not of the engine-driver. One necessity of its successful working is that the pipes should always be full of water, and in the event of a leak occurring in any part of the train its efficiency might be seriously interfered with, but when in proper working order it is no doubt a very powerful break.

3. *The Vacuum Break.*—Smith's vacuum break, which formed one of those experimented with by the Royal Commission, may be thus described:—On the side of the smoke-box of the locomotive there were two steam ejectors for exhausting air, operating conjointly, but acting independently in case one should part. Under each carriage and van throughout the train there was an india-rubber cylinder, of 15 inches diameter and 16 inches extreme stroke, stiffened with internal metal rings, and capable of collapsing and extending lengthwise. Under the tender there were two such cylinders. These cylinders were in communication with the ejectors on the engine by means of a double line of pipes, connected at the tail of the train and forming a complete circuit through it, with hose couplings between the carriages. On steam being admitted to the ejectors by the driver, the air is exhausted from the collapsing cylinders, and the movable end of each being connected with the ordinary break gear of each carriage, the breaks are at once applied. By opening an air-valve the cylinders refill, and the breaks are released. In addition to the above there was in the front and rear guards' van another arrangement for applying the breaks in case of emergency. This consisted of an air-exhauster in each van, nearly over one of the axles. On this axle a grooved friction-wheel was bolted and keyed, and in line with this another grooved friction-wheel of the same dimensions was suspended from the carriage in such a way that it could be thrown into gear with the first wheel or kept clear at pleasure. By means of a belt which passed up through the floor of the van, the second wheel, when set in motion, drove the wheel of a rotary pump-exhauster which was fixed in the van. The exhauster was made to run either way. Near the pump in the van there was a lever held up by a notch in a standard, and when the pump was required to work the lever was pushed out of the notch by a cam lever. By this operation the second friction-wheel under the carriage was thrown into gear with the axle-wheel, and, if the train was in motion, the pump was set to work and the air

exhausted from the collapsing cylinders under the carriages throughout the train. By means of a cord attached to the cam lever in the van, and running the whole length of the train and on to the engine, the driver, or any guard or passenger in the train, had the power of starting the exhauster. In addition to this there was a cord connected with the collapsing cylinders under the carriage, by which any movement in them was made to sound gong-bells in the guards' vans, and close to the driver on the engine. Also by the act of the driver exhausting the air from the collapsing cylinders, by means of the ejectors, the pump-exhausters in the vans are started.

The reports on the working of this break, where it has been practically tried, are generally very satisfactory, and there can be no doubt that it is a far more effective machine than either the chain or the hydraulic break. It however undoubtedly possesses two weak points; the one being the necessity for employing a cord communication to enable the driver to set in motion the break-apparatus in the guards' vans; and the other the employment of rubber sacks and rubber reservoirs, which, though somewhat cheaper in first cost than more durable materials, cannot be maintained as cheaply as iron cylinders and iron reservoirs, and, besides, they must be less reliable, and must at times be more apt to fail as they get old, when they are urgently wanted to act. The slightest cut or puncture would of course detract from their power, if it would not entirely neutralise their use; and this might easily be occasioned, in the event of a part of the train leaving the rails, by sharp stones thrown up from the ballast by the wheels, just when the efficient action of the break was of the greatest importance.

4. *The Air Break.*—The Westinghouse automatic air break is the best type of continuous air breaks brought before the Royal Commission. The mechanism of this break and the mode of operating with it may be thus described:—A small engine fixed on the locomotive, and deriving its steam direct from the boiler, worked a direct-acting pump, which forced air at pressure into a main reservoir, of nearly 9 cubic feet capacity, placed underneath the foot-plate. A line of tubing extended from the main reservoir longitudinally throughout the whole length of the train, with a cock at each end of each carriage. The connections between the carriages were formed of india-rubber hose and metal couplings. Under each vehicle a branch from the main pipe led through a triple valve—of special and complex construction—to a small supplementary air reservoir, and

also to two vertical cylinders provided with pistons fixed under the carriage and on either side of it, midway between the wheels. To the bottom of each cylinder was hung, by a pair of links, two cast-iron cams or quadrants. Each cylinder had a piston, the rod of which passed out through its upper end, and to the top end of this two links were attached. These links were of such form that they passed down the outside of the cylinder. Near their lower ends they were connected with the cams or quadrants just mentioned, and at their extreme lower ends they were connected with the thrust-rods of the cast-iron break-blocks. Thus, when the cylinder piston rises, it draws upwards with it the two links last mentioned, and in doing this the eccentric quadrants roll against each other, forcing the links apart, and, acting on the break-rods, thrust the break-blocks against the wheels. By the reverse of this action the breaks are made to leave the wheels. The breaks on the engine are similar in principle to the carriage-breaks, but the blocks were applied lower down on the wheels, and the arrangement of cylinders and links was somewhat different. There was one cylinder and set of break gear between the two coupled wheels on each side of the engine.

Upon a train being made up, compressed air may be allowed, by opening a three-way cock on the engine, to flow from the main reservoir and charge the whole of the main pipe and all the carriage reservoirs at a uniform pressure. When it is desired to apply the breaks, the compressed air is allowed to escape from the main pipe into the atmosphere through the three-way cock lately mentioned. The reduction of pressure to a small extent by this means operates upon a diaphragm in the triple valve under each carriage, instantly closing a port between the carriage reservoirs and the main reservoir, but permitting, at the same time, the air under pressure to pass from the reservoir to the break cylinders in proportion as the pressure in the main is reduced, thereby applying the breaks. By restoring pressure from the main reservoir to the main pipes the triple valves are shifted so as to charge again the carriage reservoirs, at the same time opening a discharge port in each triple valve by which the air can escape from the break cylinders, and thus release the breaks. The act of breaking asunder the train at any part would have the same effect as allowing the air to escape from the main pipe through the three-way cock on the engine, or through openings provided for the same purpose in the guards' vans or elsewhere.

It is impossible to read through the evidence given regarding this break before the Royal Commission without at once coming to the conclusion that in operation, both in this country and in America, its working has been all that could be desired. Besides possessing all the advantages of other continuous breaks, it can claim others which are not common to the latter. By no means an unimportant property of the Westinghouse automatic break is that its normal position is such as to apply the break-blocks to all the wheels of a train, so that the breaks have to be taken off by the driver before starting, and any defect in the mechanism would make itself at once known by the breaks refusing to be taken off in the ordinary manner. Thus infallible evidence is always given on the first starting of a train that the break gear is throughout in working order,—matter of no small importance so far as security in travelling is concerned. Another consequence of the automatic action of this break is, that if a train were broken into as many pieces as carriages, each carriage would be stopped by the self-acting application of the break upon its wheels. Also, the break can be applied by the guards as well as by the engine-driver, without the necessity of ropes or other extraneous appliances foreign to the mechanism of the break itself.

A description of the experiments conducted, with the several kinds of break in operation on different railways, by the Royal Commission, is given in Appendix F to their Report. From this some further very important particulars may be obtained relative to the comparative efficiency of the various continuous breaks at present in operation. Besides the general principles of their construction and application, their relative values must depend upon the results obtained from them in actual practice, and in calculating these several considerations must be kept in view; for instance, the speed with which the full force of the break can be applied; the mean retarding forces operating in each case; the distance within which a stop is effected; and the time necessary for taking off the break again—are all subjects of importance which must be taken into account in determining the relative values of different kinds of breaks.

In examining these several points it will not be necessary to review the results of experiments made with ordinary hand-breaks, as they are clearly so insufficient for the requirements of traffic at the present day that they may fairly be left out of consideration altogether. As regards the time occupied in the transmission of break-power through the trains, and in releasing breaks, the conditions under which the experiments were made did not admit of their being applied to Clarke and Webb's chain break. The observations made of several experiments with each of the other kinds of break gave the following average results :—

1. *Westinghouse Air Break.*—The time occupied in applying the break from the engine to the rear vehicle was from 1½ to 1¾ seconds, whilst the time occupied in taking it off was from 3 to 6 seconds.
2. *Smith's Vacuum Break.*—The time occupied in applying this break from the engine to the rear vehicle was from 4½ to 5 seconds, whilst to take it off required about 24¼ seconds.
3. *Barker's Hydraulic Break.*—With this break, if a coupling breaks, the rear portion of the train is placed beyond control of the continuous break, and therefore the report on its operations cannot be given as an average, but must be quoted in detail ;—

"First trial, from engine to fourteenth carriage, 5¼ seconds to put on, 8½ seconds to take off.

"Second trial, from engine to sixth carriage, 3 seconds to put on.

"Third trial, from engine to fifteenth carriage, not noted, 18½ seconds to take off.

"We subsequently severed the train between the eighth and ninth carriages after releasing the flexible pipe, leaving the valves open. When the engine moved on, a cord shut the valves, and the break could easily be applied by driver to the front portion of the train."

After giving particulars of the Westinghouse vacuum and Steele's air breaks, the Report proceeds :—"In these trials the most rapid action, both in putting on and taking off the breaks, was obtained from the Westinghouse air break."

The mean retarding force exercised by the several breaks was tried in a variety of ways, but it will not be necessary to note more than two methods which represented most nearly what would be the case in actual working. In both of these stoppage was ordered by flag signal, upon which all available break or other power was applied by driver and guards to the stopping of the complete train. In the first series of experiments sand was employed, whilst in the latter it was not used.

The following table shows the results obtained with the use of sand :—

Train.	Break.	Mean Retarding Force, Percentage of Gross Load.
Midland..	Westinghouse Air ..	10·64 per cent.
London and North-Western	Clarke and Webb's ..	7·79 "
Midland..	Barker's Hydraulic ..	7·64 "
Great Northern	Smith's Vacuum ..	7·47 "

In the last three cases the total retarding forces were very nearly alike, ranging from 7·79 per cent to 7·47 per cent of the weights of the trains, or within 0·16 per cent either way of the mean result. On the other hand, the force acting on the Midland train with the Westinghouse air break ranged as high as 10·64 per cent, being 3·01 per cent above the mean result obtained by the other breaks. Without the use of sand the results were as follows :—

Train.	Break.	Mean Retarding Force, Percentage of Gross Load.
Midland..	Westinghouse	10·04 per cent.
"	Barker's Hydraulic ..	6·47 "
London and North Western	Clarke and Webb's ..	6·21 "
Great Northern	Smith's Vacuum ..	5·72 "

It is only necessary to remark here that the Westinghouse air break showed a percentage of retarding force 3·95 per cent above the mean of the results given by the other three breaks.

We have next to consider within what distance a train can be brought to a stand with the several kinds of break above referred to, and it will not unreasonably be concluded that the break which can be applied most expeditiously, and at the same time exercises the highest retarding force, will be found at the head of the list in this case also. The following table gives the distances (approximately) in which the stops would have been effected in each case under the influence of the same retarding forces referred to above, upon the application of break-power to trains running at the speeds of 30, 45, and 60 miles per hour :—

Train.	Break.	With Sand. Miles per Hour.			Without Sand. Miles per Hour.		
		30	45	60	30	45	60
		Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
Midland	Westinghouse ..	282	634	1128	300	675	1200
London and N.W.	Clarke and Webb's	385	866	1540	480	1080	1920
Midland	Barker's Hydraulic	393	884	1572	465	1046	1860
Great Northern ..	Smith's Vacuum ..	403	907	1612	525	1181	2100

Besides the above, a most important series of experiments was undertaken for the Royal Commission, in order to ascertain the effective power of the several breaks in the event of an accidental severance of a train. This was done by slipping a portion of the train where the break couplings led past; the point of severance being, in each case, fixed by the Commissioners at the time of making the experiment, so that it might not be known beforehand to the train attendants or patentees. In this way the severance could be taken to represent that which might occur through the accidental breaking of a coupling. The carriages were slipped from the train at full speed, and with full steam on the engine, on signal being given to the guard entrusted with the slip. The speed of the train at the time of the slip, the distance run in performing the stop by the severed portion, and the time occupied in the stop were carefully noted. Four trials were thus made; but the accidental breaking away of a portion of the London and North-Western train in one of the earlier trials afforded a fifth example, of more value, perhaps, than the rest, inasmuch as the breaking away was entirely unpremeditated. The hydraulic break, not being specially adapted to meet the contingency of breaking away, it was not tried in this series. The series produced the following results in reference to the slipped portions of the trains:—

Train.	Break.	Number of Vehicles Slipped.	Speed of Train when Slipped.	Distance Run in Stopping.	Mean Retarding Force, Percentage of Gross Load.
			Miles per hr.	Feet.	Per cent.
Midland	Westinghouse ..	12	51¾	869	10·13
London and N.W.	Clarke and Webb's	6	50⅘	928	9·27
Great Northern ..	Smith's Vacuum ..	12	40½	2509	2·18

In the case of the Midland train the breaks were put on automatically over both sections of the entire train, with the exception of the engine and tender breaks, which were disconnected. The time of coupling up after the severed portions were brought together was noted: it took five seconds to couple the chains, and two and a half seconds to couple the hose-pipe of the breaks. In the case of the London and North-Western train a rather violent jar was produced when the coupling snapped. The severed portion consisted of five carriages and the rear van. The breaks of the said five carriages being actuated from the van, no collision took place between the two portions of the train, but, on the contrary, a space of 169 feet intervened between them after both had been brought to a stop. A special slip coupling with connecting-pipes and valves had been furnished in the case of the Great Northern train (Smith's vacuum break), but the Commissioners elected to effect the slip at another point, where no special provision for the contingency of breaking away existed, and where there was no valve in the hose-pipe. The action of the continuous break on the severed portion would doubtless have been more powerful had the influx of air into the severed pipe been prevented by a valve; but, as it was, the action was feeble compared with the others, as it amounted to less than one-fourth of that produced by either the Westinghouse air or by the Clarke and Webb's breaks.

These experiments having been conducted by wholly disinterested persons, and with the view of ascertaining really the most efficient break in existence, possess an especial value, and should unquestionably be relied on by Railway Companies as furnishing some guide to them as to which of the many breaks at present in existence should be adopted by them. No words that we might add could give additional force to the report on the experiments with the several breaks from which the foregoing particulars have been taken. From these it is clear that the Westinghouse air break is about 25 per cent superior to any of its competitors, and, from whatever point it is viewed, it is so pre-eminently more effective than the others that there should, we should think, be no hesitation on the part of the Railway Companies in adopting it. That it will be generally adopted by some of the leading companies there can scarcely be a doubt. Mr. Allport, of the Midland Railway Company, when under examination before the Royal Commission, expressed a very general approval of the Westinghouse break, and intimated that the Company was only waiting for the Commissioners' Report before adopting that break generally on their line; and it can scarcely be doubted that—except, perhaps, in cases where personal interest may bias the judgment of Directors, should such influence anywhere exist—the course adopted by the Midland Railway Company in this instance will be followed by other Railway Companies throughout the kingdom. It is most important that this should follow, for there is now so much interchange of traffic betwen railways, and the carriages of companies run through over several railway systems besides their own so generally, that some uniformity of system in the application of break-power should unquestionably be adopted. This, truly, is hardly a point for Government interference or for legislative enactment, but must be left to the good judgment and self-interest of the several Railway Companies, which ought to be sufficient to ensure the "survival of the fittest" amongst railway breaks. In conclusion, it may be remarked that if the conditions laid down by the Royal Commission be accepted as those which should be required to be fulfilled, there is only one at present in existence, viz, the Westinghouse automatic air break, which can be said to come up to the standard of efficiency laid down in those conditions.

Since the above was written a series of experiments were held at Cassel, from the 1st to the 4th of August last, the Report on which has just been published. Without entering into detail with regard to these experiments, it may suffice here to state that in each case the Westinghouse automatic break proved superior to the Heberlein, Steel, and Smith's breaks, with which it was in competition, to the extent varying in different cases from 16 to 60 per cent, thus completely corroborating the results obtained by the Royal Commission referred to above. With regard to the first cost of the breaks, the Westinghouse is slightly more expensive than either the Heberlein or Smith's, but it is far cheaper than the Steel break, whilst of the four the Westinghouse break is lighter in weight than any of the others by some hundredweights. It appears, however, from an official report on the subject to the Austrian Government, and from the report of the recent Belgian Commission, that the durability and small maintenance cost of the Westinghouse break compensate for its being somewhat dearer than others in the first instance.

In view of all these corroborating evidences it can be no source of surprise that the Westinghouse break has been adopted for the Belgian State Railways. It has, however, not yet been publicly notified, so far as we are aware, that the several Railway Companies in England have so far recognised their responsibilities towards the public as to fit up their several systems with the most efficacious break-power which has hitherto been introduced.

II. ON RESIDUAL PHENOMENA.

It is a well-known—indeed almost a trite—fact that the residues of manufacturing operations have proved a most fruitful source of novel and interesting bodies. If we wish to detect an element hitherto unknown we search for it among the refuse of metallurgical or chemical processes, in flue-dust, in furnace-soots, in slags, in burnt pyrites, in the mud of vitriol chambers, or in mother-liquors which have deposited their crop of crystals. If we are in quest of some new or valuable organic compound we operate upon the residue of the gas-works, in which Liebig prophetically declared that we might find whatever we wished if we would only seek intelligently; upon wood-tar, or upon natural products which have most probably undergone a process of destructive distillation in the recesses of the earth. In this manner we have obtained, on the one hand, bromine, iodine, selenium, cæsium, thallium, &c.; and on the other, phenol, rosanilin, artificial alizarin, and other the like marvels of modern chemistry. Nay, we sometimes elicit important products even from the residues of a residue. The "tailings" left from the manufacture of magenta are themselves a source of other colouring matters.

But we are not about to enlarge on the utilisation of waste products. That fecundity in novelties which seems to characterise refuse and residues is an apt type or illustration of the importance of "residual phenomena" as a source of unsuspected truths; and it is to such, as a sphere for discovery, that we wish to draw the attention of our readers.

What are "residual phenomena?" We will suppose a man of science setting to work to "verify" some theory—that is, to examine whether and in how far it accords with the facts it is intended to harmonise and to explain. There are here three cases possible:—In the first place, the theory in question may agree exactly and completely with the facts, leaving merely such minute errors as are plainly due to the shortcomings of experiment and observation. We may take, as an instance in point, the law of chemical combination in definite proportions. If we examine whether this is a correct statement of facts, we find it confirmed the more completely the more precise and accurate are our experiments. Secondly, the theory may distinctly fail to account for the phenomena before us, and may prove utterly contradictory to facts. Thus it was at one time maintained that the distinction between animals and vegetables lay in the presence of nitrogen in the former and its absence in the latter. But as soon as improved methods of analysis showed the presence of nitrogen in vegetable tissues the theory was recognised as at variance with facts, and was therefore abandoned. We may here briefly glance at certain phrases very common in the mouths of unscientific persons, of whatever stage of culture. They are apt, when encountered by an inconvenient fact, to tell us, in an off-hand manner, that there is "no rule without an exception," or sometimes even that "the exception proves the rule,"—a *dictum* quite on a par with Sir Thomas Browne's celebrated "*credo quia impossibile est.*" Now we should like any speaker or writer of this class to find us an exception to the law of universal gravitation, or to the doctrine of definite combination just mentioned. Let him produce, *e.g.*, an element which will combine with oxygen, or with sulphur, or with chlorine in every conceivable proportion, and yet form not mere mixtures, but a true compound or true compounds. Having found such an element, let him further show how by its existence the "rule" of combination in definite proportion is "proved," or, indeed, other than completely refuted. Still there is a sense in which this saying carries with it a certain amount of truth, and which perhaps explains its origin. An *apparent* exception, if it does not prove, may at least confirm a rule, so soon as its nature is understood. Thus we know that every kind of wood, if dry, is combustible. If some person asserted the existence of an exception to this rule, and brought forward in proof, *e.g.*, a piece of the fossil wood of Antigua,—*i.e.*, silica deposited in the exact texture of fragments of wood which have been long ago decomposed, it could of course be shown that this substance was wood in appearance only, and that its incombustibility was therefore no real exception to the rule. Or, to take another somewhat more complicated instance:—Everyone who has any acquaintance with chemical and physical science knows what is meant by the law of isomorphism; yet to this law, when first promulgated, there appeared a signal exception. Arsenic and phosphoric acids were at once recognised as compounds mutually analogous, and coming within the scope of the law; yet their soda-salts, as then known, did not exhibit that identity of crystalline form which the law of isomorphism requires. On closer investigation, however, it appeared that the ordinary phosphate of soda of commerce differs decidedly in its composition from the arseniate of soda, and, further, that there exists another phosphate of soda agreeing with the arseniate both in composition and in crystalline form. Such explanations of apparent exceptions may, perhaps, to some minds seem to "prove" or at least confirm the "rule," on the same principle that lawyers consider the title to an estate strengthened if it has been unsuccessfully called in question. Yet what truly confirms the rule is not the exception, real or apparent, but the detection of the fact that it is no exception at all. Before anyone can venture to pronounce the imagined "law" that all hybrids are necessarily barren to be "proved" by instances to the contrary, he should have prepared himself to show that such instances are contrary in seeming only, and not in reality.

We are thus brought to the threshold of the third possible case:—The theory or the law may agree with or account for the facts to a certain extent, leaving, however, a margin unexplained. Such margins are the "residual phenomena" which we are about to consider: their value as a clue to further discovery will best appear from example. We may begin with one of the simplest cases, which, nevertheless, led to the discovery of the alkaline metal lithium. One of the foremost consequences of the chemical law of combination in definite proportions is that all samples of any compound, supposing them pure, must contain the same ingredients in the same relative quantities. A specimen of sulphate of magnesia, whether natural or artificial, whether prepared recently or a century ago, contains exactly the same proportions of magnesia and of sulphuric acid. On a certain occasion the Swedish chemist Arfwedson, having been engaged with the analysis of a mineral petalite, obtained what he at first took to be sulphate of magnesia; but on closer examination he was led to doubt this conclusion, not by any striking discrepancy in colour, solubility, or taste, but by an excess of weight. His suspicions being thus aroused, he soon found that he had in his hands an element as yet unknown.

Turning from chemistry to physics, we find other examples equally striking and simple. Thus the speed with which sound travels through the air had been deduced with great precision, from its known cause and mode of propagation. But when the conclusions thus reached were brought to the test of actual experiment, the agreement, though approximate, was not complete. The result was sufficiently close to show that the calculation had been, in the main, based upon correct principles, but there was still a margin, a "residuum" of velocity unaccounted for. Whilst examining into the discrepancy, Laplace suggested that the heat liberated by the compression of the air resulting from sound-vibrations might be the missing factor. Its influence was accordingly calculated, and was found to supply the complete solution of the difficulty. Nor was this all ; the law of the evolution of heat on the compression of bodies received at the same time a most signal verification.

These few instances will suggest certain reflections. Residual phenomena, though possibly of very frequent occurrence, will be overlooked save by the patient and accurate investigator who is content with nothing less than certainty. Had Arfwedson quietly assumed that the saline body formed during his operations was sulphate of magnesia, without subjecting it to any quantitative examination, lithia would probably have remained undiscovered for some time, and the honour would doubtless have fallen to the share of some other chemist. Had the comparison between the experimental and the theoretical velocity of sound been made in a careless manner, the discrepancy would have escaped attention, and the question ultimately solved by Laplace might never have been raised. But there is another danger to which Liebig has drawn attention, and which he has happily illustrated by an incident in his own early career. The experimentalist may perceive a difference between theory and practice, between results or properties actually observed and those which in his opinion were to have been expected, but may content himself with framing some hypothesis to account for the difficulty without resorting to any actual investigation. Such a line of conduct prevented Liebig from anticipating Balard in the discovery of bromine. He had actually obtained the new element in a state of approximate purity, but had assumed it to be chloride of iodine, had imagined an hypothesis to explain its peculiarities, and had set it aside unexamined. This event, as he tells us, served him as a caution for the rest of his career. How many residual phenomena, which have been really noticed, escape investigation in virtue of some superficial and baseless explanation we can scarcely even conjecture.

But another question arises here :—How is a residual phenomenon to be distinguished, on the one hand, from those errors of observation and experiment which cannot be absolutely avoided, even by the most careful operators, and, on the other, from those discrepancies which prove that a law or a theory is essentially erroneous, and must be rejected. It is scarcely possible, we fear, to lay down any absolute rule which shall in all cases direct the enquirer how to overcome this difficulty. Certain suggestions, may, however, be given. In case of a theory or a law which is correct, and which fully embraces and accounts for all the facts, the discrepancies will be very minute; they will be smaller in proportion as the experimentalist is more skilful, and they will further decrease as suitable precautions are taken and sources of error are eliminated. In the case of a false theory, on the other hand, the errors will be much larger, will vary in different instances, and, instead of being reduced by improved methods and more finished manipulation, are more likely to become greater. Residuals differ from both these cases: the correspondence between the fact and the theory cannot to a certain extent be denied, but there is a margin which, though generally small, is constant, and cannot be lessened by any niceties and refinements of procedure. Where the law or the theory under investigation is merely qualitative, more must be left to the tact and judgment of the observer.

In carefully searching for the source of an anomaly we may discover someting other and much greater than we originally intended or hoped. The old apologue of the farmer's sons—who by digging for an unknown treasure increased the fertility of their land, and reaped a harvest of unimagined luxuriance—finds its frequent realisation in the history of scientific research.

We shall, perhaps, find it instructive to turn from the consideration of residual phenomena which have been recognised as such, and which have been fully traced to their causes, and examine certain laws which admittedly do not fully agree with the phenomena, and whose shortcomings demand explanation. Such laws may, of course, belong to our second class, and in that case require total rejection; but they may possibly be not so much erroneous as incomplete, embracing a part only of the phenomena, and leaving the rest still unaccounted for.

Considerable attention is now drawn to the relations existing between the atomic weights of our received elementary or simple bodies. If these bodies have been evolved from a common source, as a variety of considerations conspire to suggest, it is highly probable that such relations should exist. If, on the other hand, they are primordially distinct, and, as far as we can perceive, accidental in their number, in their distribution, their relative amounts, and their properties,—suppositions highly repugnant to our intellect,—then no laws of numerical relation between their atomic weights may be traceable.

The earliest attempt in this direction was made by Prout.

Perceiving that the atomic weight of hydrogen was lower than that of any other element, he put forward the law that bears his name,—*i.e.*, that if we take the atomic weight of hydrogen = 1, the atomic weights of all the other elements will be multiples of 1 by some whole number. The suggestion agreed tolerably well with the state of chemical knowledge in his day. Not a few atomic weights, calculated on the hydrogen standard, had been found to be whole numbers, and there was at least room to anticipate that, on further and more accurate research, the fractions with which the atomic numbers of the remainder were encumbered might prove to be the result of error. There was, moreover, about the proposed law an appearance of "simplicity" which could not fail to recommend it to general notice. Let us hasten to declare that we put little faith in such simplicity. It is a conception totally *ex parte hominis*. Wheresoever we have attempted to interrogate Nature somewhat closely, we have utterly failed to perceive that simplicity, as it appears to man, is any part of her plan. When, therefore, a law or a theory is recommended on the ground of its simplicity our suspicions are aroused, and we fear lest the facts have been garbled and manipulated.

On the other hand, a certain amount of confirmation is lent to Dr. Prout's view by the observations of astro-spectroscopists, that hydrogen predominates in the stars whose temperature seems highest, and where the dissociation-process must be most active. Hence it is contended hydrogen may be a more primitive stage of development of the primordial element, or elements, than any other substance with which we are acquainted. Still, admitting such to be the nature of hydrogen, the truth of Prout's law is by no means a necessary consequence. The more accurate determination of the atomic weights of the elements, again, has by no means, as was anticipated, tended to free them from fractions. Hence the value generally attached to Prout's law has not latterly been increasing. Dumas, who is one of its most distinguished upholders, proposes the modification that all atomic weights are multiples by a whole number, if not of 1, yet of 0·25 or 0·50, maintaining that out of fifty-eight atomic weights not more than half-a-dozen differ appreciably from multiples by whole numbers of half the atomic weight of hydrogen, whilst some of the exceptional elementary weights are multiples of one-fourth the atomic weight of hydrogen. Here, however, we enter upon dangerous ground. If, multiplying 1·0 by a series of whole numbers, we obtain products coinciding with the atomic weights of the elements, the coincidence is of considerable value, and may be fairly interpreted as indicating a profound relation between hydrogen and the other elements. But if we multiply 0·5 or 0·25 by the same numbers, and especially if we reserve to ourselves the right of using either of these factors if the other do not suit our purpose, the coincidences will naturally become more frequent and less significant. By taking a sufficiently minute fraction of the atomic weight of hydrogen (say 0·001), and multiplying it by a series of arbitrarily-selected whole numbers, we cannot fail to obtain the atomic weights of the elements, whether any natural connection exists between them or not.

In opposition to Dumas, Prof. Stas infers—from his own justly-famed researches on the atomic weights of nitrogen, chlorine, sulphur, potassium, sodium, lead, and silver—"that there exists no common divisor between the weights of simple bodies which unite with each other to form definite compounds." He considers, therefore, the hypothesis of Prout as altogether illusory, and regards the reputed elementary bodies "as distinct entities, having no simple relation of weight one to another." This conclusion he founds upon his own recent determinations of the weights of certain elements,—a train of research executed with such precaution, skill, and patience that it may serve as a model for all similar investigations.

Thus if we take hydrogen = 1, we shall have—

Oxygen	15·960
Silver	107·600
Nitrogen	14·009
Bromine	79·750
Chlorine	35·368
Iodine	126·533
Lithium	7·004
Potassium	39·040
Sodium	22·980

These numbers are, of course, utterly hostile to the pretensions of Prout's hypothesis to completeness. Not a single exact multiple of 1 by a whole number do we find. Nor are the deviations from whole numbers so small as to be neglected. Least of all can it be assumed that the fractions which occur are the result of any inaccuracy, and may consequently be obliterated by further and more minutely-accurate research. Those who read the detailed account of the experiments of Professor Stas, and note the precautions he has taken to eliminate all sources of error, will be unable to conceive of any further improvements adequate to convert the fractions here given into whole numbers.

But let us re-consider the atomic weights of the nine elements above quoted. In three cases only do we find the fraction extending to the first decimal place; in four others it commences with the second decimal, being always less than 0·05; whilst in the remaining two elements the difference between the atomic weight experimentally determined and the calculated figure is confined to the third decimal. If we suppose, with Dumas, that the common divisor may be 0·5, we have the following differences between the experimental and the hypothetical nnmbers:—

Oxygen	0·040
Silver	0·100
Nitrogen	0·009
Bromine	0·250
Chlorine	0·132
Iodine	0·033
Lithium	0·004
Potassium	0·040
Sodium	0·020

Or, as Prof. Stas himself expresses the matter, "The greater part of carefully-determined weights come so near to calculated ciphers that it has been necessary to have recourse to all the arts and refinements of analysis to prove that they are not absolutely exact." But if we take the weights as given by earlier authorities, and, it may be admitted, determined, if not with less skill, yet without the aid of the most recent appliances, we find the difference between the experimental result and the calculated number in many cases even greater than those shown by the researches of Prof. Stas.

Thus the older numbers are—

Oxygen	16·0
Silver	108·1
Nitrogen	14·0
Bromine	78·4
Chlorine	35·4
Iodine	126·0
Lithium	6·4
Potassium	39·2
Sodium	23·2

The difference between these figures and a whole number, or the fraction 0·5, is therefore—

Oxygen	0·0
Silver	0·1
Nitrogen	0·0
Bromine	0·1
Chlorine	0·1
Iodine	0·0
Lithium	0·1
Potassium	0·2
Sodium	0·2

Or an average of 0·0888, whilst according to the atomic weights as determined by Stas, the margin is only 0·0688. The divergence, according to the older atomic weights, reached the first decimal place in six out of the nine instances, whilst in the numbers of Stas we find this occur in three cases only. Thus far, therefore, the labours of the illustrious Belgian chemist are more favourable to Prout's hypothesis than might appear at first glance. An eminent authority also calls attention to the fact that "the smallest atomic weights, which, as a general rule, are those of the best known and most easily estimated elements, accord the most precisely with Prout's law." Thus in addition to the instances of oxygen, nitrogen, lithium, and sodium, we have carbon, whose atomic weight has been re-determined by Dumas and Stas as 6·0 and by Liebig as 6·008, and sulphur, which Stas has investigated along with the above-mentioned nine elements, and which—taking oxygen as 15·960—he finds to be 31·976, showing a divergence from the calculated cipher of 0·024. According to the old notation, oxygen being—

$$\frac{15{\cdot}960}{2} = 7{\cdot}980,$$

sulphur would be—

$$\frac{31{\cdot}976}{2} = 15{\cdot}983,$$

the difference from the whole number being then merely 0·017.

If, then, we take these eleven elements,—viz., oxygen, silver, nitrogen, bromine, chlorine, iodine, lithium, potassium, sodium, carbon, and sulphur, and fix our attention on the first decimal place, we shall find it scarcely what might have been expected if we had to deal with a set of distinct entities between whose atomic weights there was no definite relation. According to the doctrine of probabilities, there would be no reason why we should expect any of the ten digits to preponderate over another; yet in this instance the decimal place in question is occupied nine times out of the eleven by figures which are more in favour of the hypothesis than against it, whilst in two only—the cases of bromine and chlorine—do we find figures which hold an unequivocally half-way position between the whole number and the fraction 0·5. This coincidence we can scarcely attribute to accident. Some reason there must be why the fractions should be either a little in excess of the whole number, a little below it, or else bear a similar relation to 0·5.

If, then, this state of facts is utterly adverse to our reception of Prout's hypothesis as a complete and accurate law, it is, we submit no less incompatible with the antagonistic view of its being utterly baseless and imaginary. The case is apparently one of a "residual." Prout's hypothesis is interfered with and modified by an x, which prevents the resulting atomic weights from being either exactly whole numbers or merely fractions midway between two such whole numbers. What is this unknown law no one has yet, we believe, made any systematic attempt to discover?

The error, if we may so call it, is as we perceive by no means constant; sometimes it is in excess, and sometimes in deficiency. It appears also to vary with the class of the elementary bodies. It is considerable in the Halogens, chlorine showing a deficiency of 0·132, bromine a deficiency of 0·250, and iodine an excess of 0·33. These numerical peculiarities are not easy to account for, especially in the case of bromine. We might have expected that it would have shown a smaller deficiency, as compared with the calculated cipher, than does chlorine, forming thus a transition to the excess perceived in the atomic weight of iodine. It is remarkable that bromine shows in another manner this tendency to deficiency. Its atomic weight, as determined by M. Stas, is 79·750; but as calculated from the weights of its congeners, chlorine and iodine, it is—

$$\frac{35{\cdot}368+126{\cdot}533}{2}=80{\cdot}950,$$

thus showing in the actual weight a deficiency of 1·200 to be accounted for.

The group of the alkaline metals, lithium, sodium, and potassium, is less markedly exceptional. Here, as the atomic weights rise, we have in lithium an excess of 0·004, in sodium a deficiency of 0·020, and in potassium again an excess of 0·040. The atomic weight of sodium, calculated from those of lithium and potassium,—

$$\frac{39·040+7·004}{2}=23·022.$$

is in excess of its experimental weight only by 0·042.

The atomic weights of the alkaline earthy metals are unfavourable to the hypothesis of Prout, or at least point to the more decided interference of some unknown cause.

According to Marignac the atomic weight of calcium is 40·21, and that of strontium 87·25,—numbers of a very intractable character, and which, lying in an intermediate position between a whole number and the fraction x·5, would seem as favourable to the total rejection of Prout's law as to the prospect of its exceptions being explained as residual phenomena.

Lead, as determined by Professor Stas, has for its atomic weight 103·136 (o = 15·960), or an excess of 0·136. Thallium, according to the careful determination of Mr. Crookes, = 203·642. Here, then, the excess is also 0·142, very closely approximating to the unexplained margin in the case of lead.

Until, however, the atomic weights of all the elements shall have been verified with the precautions and refinements employed by M. Stas in the case of silver, nitrogen, the halogens, and the alkaline metals, and by Mr. Crookes in the case of thallium,—a task which no single chemist can possibly complete in the longest life-time,—all speculations concerning the relations of their combining weights must be regarded as premature. It may be that when all the required numbers are before us the deviations in excess or deficiency from what Prout's hypothesis would in strictness require will be found to display a "periodic" character. All that can be said at present amounts to very little more than the recommendation to suspend judgment till the facts of the case are fully before us—a consummation, we fear, scarcely to be hoped for in the life-time of the present generation. Seventeen years ago the late Sir John Herschel, in an inaugural address as Chairman of the Chemical Section of the British Association, remarked—"Not until these numbers are determined with a precision approaching that of the elements of the planetary orbits—a precision which can leave no possible question of a tenth or a hundredth per cent, and in the presence of which such errors as are at present regarded as tolerable in the atomic numbers of even the best determined elements shall be considered utterly inadmissible—I think can this question be settled; and when such gigantic consequences—so entire a system of Nature—are to be based on a principle, nothing short of such evidence ought, I think, to be held conclusive, however seductive the theory may appear."

The so-called atomic heats of the elementary bodies bring us in contact with another instance of residual phenomena. It has been observed that within certain limits the atomic weights of the simple bodies vary inversely as their specific heats, and that consequently the products of these two numbers, or the so-called atomic heats, are approximately a constant quantity. In very many cases the numbers thus found range from between 5·9, as in aluminium and rhodium, to 6·9, as in iodine. This is certainly a rather wide deviation from the requirements of the law. But there are several sources of uncertainty which may be supposed to affect the results of the calculation. The atomic weights themselves, as we have just had occasion to remember, are not yet ascertained with absolute certainty. The purity of the various specimens of the elementary bodies operated upon is in some cases open to doubt. The determinations of the specific heats themselves may not have been entirely free from experimental error, and the circumstances under which these determinations were made cannot be pronounced strictly comparable. The experiments were performed within temperatures nearly the same in an absolute point of view, but not relatively identical—*i.e.*, not equidistant from the fusion-points of the bodies concerned. The difficulty of obtaining the elements in comparable conditions is no little increased by the fact that not the temperature of a substance alone, but its physical structure and its state of aggregation, have a modifying action upon its specific heat.

But there are three elements, mutually analogous in many respects, which show a much wider discrepancy from the law than can be explained by such considerations. The atomic heat for boron in the graphitic state is 2·59, in the crystalline condition 2·75. Carbon, as wood charcoal, gives the number 2·90, as graphite 2·41, and as diamond 1·76. These figures, moreover, widely as they differ from what the law might require, are obtained by taking the atomic weight of carbon = 12. Silicon when crystallised shows the atomic heat 4·97, and when fused 4·90. It is obvious that these three elements cannot be made to harmonise with the presumed law by any known expedient. Even if we were to fix the atomic weight of carbon at 24, the results, 4·82 for graphite and 3·92 for diamond, would still remain distinctly anomalous. The only method of saving the law is the assumption that we have here before us a residual phenomenon, some other law exercising a modifying result, either on these three elements alone or at least to a greater extent than upon other simple bodies. It must be remembered that the atomic weights of the three elementary bodies concerned are low, and that of carbon, at least, may be regarded as most satisfactorily ascertained, so that the solution of the difficulty cannot lie in that direction.

It is a somewhat curious circumstance that though the law of Dulong and Petit, as it is called, has been now for a long time before the scientific world, and though the anomalies to which we have drawn attention have been recognised for more than a quarter of a century, the explanation is still wanting. Kopp, as the result of his investigations, concludes that the law does not hold good for all the commonly reputed elements in their solid state; but if so we naturally inquire into the reason of its limited scope, and without some reply we feel inclined to doubt whether its approximate applicability in other cases may be anything more than a casual coincidence. In the meantime the law of Dulong and Petit should be regarded as on its trial, and the weight allowed it should be limited indeed. It would, for instance, be scarcely justifiable to reject any new hypothesis because it should happen to lead to results incompatible with the law in question.

In the instances we have been considering, and in all others of an analogous nature, there can be no doubt whether, or in how far, the law is in harmony with the phenomena which it embraces. But where the results are incapable of being stated numerically a serious difficulty arises. Issue may be joined on the fundamental question whether the theory agrees with the facts or not, and a thoroughly indisputable decision is not easily arrived at. Hence the recognition of residual phenomena—not to speak of their explanation—is far less easy in biology or geology than in astronomy, physics, or chemistry. One and the same theory may be by different authorities accepted as satisfactory and complete, rejected as altogether illusory, and again admitted as a partial view of the truth, which, however, leaves many points still in the dark. To us it seems that the origin of species supplies an admirable instance of residual phenomena. The laws of "natural selection" and "sexual selection," collectively known as Darwinism, do, as far as we can judge, account satisfactorily for a large portion of the phenomena concerned; but as, undoubtedly, there is another portion which they cannot by any amount of ingenuity be made to explain. For instance, it has been well said that before natural selection—or indeed selection of any kind—can be brought into play, variation must have already set in. This will be at once apparent on the following consideration:—Suppose a pair of animals, or, still further to simplify the matter, a single hermaphrodite, being of low type, existing in the primæval world, had produced a hundred fertile ova. Two cases then are only possible: the young animals springing from these ova must be either all absolutely alike, or they must exhibit certain variations, however slight. In the former alternative there is no basis for natural selection to work upon, the very idea of selection implying differences among the objects among which a choice is to be made. In the latter case, the varieties, being *ex hypothesi* antecedent to the action of natural selection, cannot be its effects. Here, then, we have a residual phenomenon, a marginal fact for which the Darwinian hypothesis is unable to account, and which will yet have to be explained before we can understand the origin of species. Nor in the instance we have supposed can the explanation be furnished by Lamarck's hypothesis. The influence of all external circumstances acting upon the parent animal must affect the ova, if at all, equally; but, as regards the first beginning of variation among a number of creatures absolutely alike at birth, the Lamarckian view has the advantage over the doctrine of natural selection. If the young animals wander away from the place of their birth they may become exposed to different influences, from climate or from the quantity and quality of their food, and among their progeny slight variations may thus possibly appear, and thus enable the principle of natural selection to come into play. Successive broods produced by one and the same parent may also be supposed to differ from each other to a small extent if any change has occurred in the circumstances to which the parent is exposed. But it is difficult to avoid the conclusion that the "residual" here is an internal tendency to vary, faint instances of whose action we see in the facts that no two children of the same parent, no two symmetrical organs of the same animal, no two leaves even of the same tree, are absolutely alike.

If we carefully trace the instances in which both facts are imperfectly ascertained, and laws accepted or rejected without full verification, we shall feel that a new *Skepsis Scientifica* is urgently demanded in the present day.

III. THE ACTION OF LIGHT
UPON THE
COLOURATION OF THE ORGANIC WORLD.

UNTIL the earlier portion of the present century light, by the vast majority of civilised persons, was regarded as a medium for the sense of sight, and as very little more. With the discovery of its chemical functions, brought home to the popular mind by the invention of photography, a revolution in opinion took place, and the danger now is, not that its real powers should be overlooked, but that it should be credited with effects in which its part is very doubtful. It has been especially proclaimed to be at once the creator and the destroyer of colouration in the organic world. The superior intensity of the light to which they are exposed has been pronounced the chief cause why diurnal species are more gaily coloured than their nearest nocturnal allies, and why the flora and the fauna—especially the insects and the birds—of tropical regions are so rich in hues of a gorgeous character. It may therefore be not uninteresting to inquire into this supposed double function of light, and ascertain, if possible, its limits in either direction. In so doing it will be impossible for us to overlook the views put forward by Mr. A. R. Wallace in a recent issue of "Macmillan's Magazine."

The bleaching power of the sun's rays, and to a less extent of ordinary diffused daylight, has been fully recognised in the affairs of daily life. It has been observed that this same agency, utilised formerly in preparing vegetable fibre for the reception of colours, gradually destroys, in almost every instance, the work of the dyer and the printer, and exerts a corresponding influence upon the hues of plants. There is, however, a distinction by which its effects upon the integuments of animals are limited.

It is well known that what we designate as colour may be produced either by the interference or by the absorption of rays of light, and hence the colours of animals may be divided into two well-marked classes. On the one hand, especially in birds and insects, we find hues which are iridescent changeable according to the relative positions of the observer and of the light, and are possessed of an intense, so-called, metallic lustre. Such colours—to take familiar examples—may be seen in the plumage of the peacock, of the starling, on the wings of the "purple emperor" butterfly (*Apatura Iris*), on the entire coating of the rose-beetle (*Cetonia aurata*), of the fire-wasp (*Chryseis ignita*), and of many other common native insects. In the vegetable kingdom they may be pronounced unknown. Such colours are due to the interference of certain rays of light, whether reflected from superimposed transparent films or reflected from or refracted through minute striæ. These colours are permanent, even on the most prolonged exposure to air, to atmospheric moisture, or to full sunlight. Unless the very texture of the feather, the wing-scale, the elytron, &c., be destroyed by putrefaction or combustion, the colour remains unhurt. Nor can we by any means extract from such coloured surfaces a dye or pigment capable of being applied to other objects.

On the other hand, there are colours which do not change their shade from whatever position they are regarded, and which possess little of that intense lustre which marks the former class. To this kind belong the colours of all flowers, of caterpillars, of the great majority of our native butterflies and moths, and, in short, of the vast bulk of organic beings. These colours are due to the absorption of certain of the rays of light, such absorption being effected by substances known as pigments, and capable, when present in sufficient quantity, of being extracted by solvents, and used to dye or stain other bodies. Such colours have not the permanence of the first-mentioned class. Every entomologist knows that if a case of butterflies be kept constantly exposed to the sun, or even to diffused daylight, then—no matter how completely air, damp, and mites may be excluded—the specimens fade, even though the minute scales which clothe the wings may still be found in their places. Yet the golden spots on the wings of the *Plusiæ* and the pearl-mother markings of the "fritillaries" remain unchanged. The colours of most other insects behave in a very similar manner. Beetles are generally supposed to wear a more permanent livery; but every Coleopterist must have observed how the reds of ladybirds, of *Aphodius fimetarius*, of *Elater sanguineus*, &c., lose their purity and brightness on exposure, and to some extent even on preservation in darkness. Even darker and more intense colours are gradually affected. Thus in the collection of native beetles in the British Museum, which have doubtless been exposed to the light for some years, the jet-black *Typhœus vulgaris*—absurdly known as the "bull-comber"—has taken a decided chestnut-brown, whilst a similar change has come over the blue-black elytra of the common dung-beetle.

To test the speed of the bleaching power of light upon deep-coloured Coleoptera we placed in a glass case, outside a south-western window, specimens of the following species:—*Cetonia aurata*, *Eupœcilia Australasiæ*, *Typhœus vulgaris*, *Geotrupes stercorarius*, *Abax striola*, and *Sternocera orientalis*,—and exposed them to the sun during the months of June, July, and August, 1876. The *Cetonia* and the *Sternocera*, whose colours are of the interference-class, were unaffected; but the black of the *Typhœus*, the *Geotrupes*, and the *Abax* was changed to a brown, and the brown of the *Eupœcilia* to a very dirty yellow. Thus we see that even the darkest and most intense pigment- or absorption-colours are affected by light. This fact accounts for one class of the variations in colour met with in different specimens of one and the same species. An insect that has lived long and has been much exposed to the sun may have more degraded colours than such as are captured soon after reaching full perfection.

If we examine the nature of the changes produced by the action of light we shall notice the following facts:—Pigment greens, blues, lilacs, pinks, and roses—shades not very abundant in the animal kingdom—are the first to fade. Full reds, purples, and blacks resist longer. Oranges, yellows, fawns, drabs, browns, and olives have still greater permanence, merely taking a duller or dirtier tone. The changes ensue in a definite direction. Blues and pale greens turn to a grey or a yellowish drab; darker greens to an olive; lilacs, pinks, and roses to various shades of grey; reds become a reddish or yellowish brown; purples a very dirty brown; yellows and oranges verge more to a pale brown, and may rank as buffs or fawns. The alteration is therefore from the primary or secondary towards the tertiary colours, accompanied with a decrease in depth. But we have never seen a primary colour, when fading under the influence of light, pass into another primary colour; nor does any secondary or tertiary colour ever pass into a primary. The change which blacks undergo will not seem surprising if we reflect that in Nature, as well as in Art, they generally consist of an intense olive or brown to which a deep blue or purple is superadded. The latter hues, being the more fugitive, fade first on exposure to light, and thus a dirty olive or a rusty brown must remain.

These changes are in partial harmony with what we observe in the vegetable kingdom. A dull, dirty brown is the ultimate goal towards which leaves, flowers, and fruits, as well as insects, tend while fading; but those splendid intermediate changes which we find in autumnal foliage have nothing analogous in the decaying colours of insects.

It is curious that in the manufacture of those artificial colours which now play so important a part in tinctorial operations a corresponding rule holds good. If these dyes, during their elaboration, are submitted to a heat too high or too prolonged, the product becomes dusky, and a dirty brownish grey is the final result.

We must further note how, in the animal and vegetable kingdoms, pure and bright colours are connected with the highest vitality only. We plant the dusky seed in the earth amidst the dark remains of decomposing organic matter, and as it grows up we see it put on higher and higher colours, till, in the culminating moment of its life, in the act of inflorescence, prismatic hues are all but universal. Then begins the process of decay, attended by a degradation of colour. Similar changes may be traced in animals. Externally we need merely compare the dull-coloured larva with the brilliant imago, or the sombre-coated nestling with the brighter plumage of the mature bird. Internally we may contrast the intensely-vitalised scarlet arterial blood with the darker-coloured and more contaminated venous blood, and still further with excrementitious matters. The great truth to which we are here calling attention has not altogether escaped the notice of Mr. Wallace, who writes—"The very frequent superiority of the male bird or insect in brightness or intensity of colour, even when the general tints and colouration are the same, now seems to me to be due to the greater vigour and activity and the higher vitality of the male. The colours of an animal usually fade during disease or weakness, while robust health and vigour add to their intensity.* This intensity of colouration is most manifest in the male during the breeding season, when the

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* Those who are brought practically in contact with animals have long been familiar with the fact that a "dull coat" is indicative of disease, or at least of weakness.

vitality is at a maximum." But we are not aware that either Mr. Wallace or any one else has fully grasped the principle laid down above, or traced its numerous applications, æsthetic as well as biological.

But among the "pigment-colours" there is a very great diversity in permanence due to the nature of the colours themselves, or to that of the tissues in which they inhere. Dr. Hagen divides such colours into epidermal, placed in hair, in feathers, and in the chitinic exo-skeleton of insects, and hypodermal, situate in the softer internal layers of the skin. That the latter are the more easily affected by any external influence is natural.

Alterations and degradations of colour similar to those above-mentioned may indeed, under certain circumstances, be produced even in the absence of light. But we have direct experimental evidence to show that, other things being equal, animal matters retain their colours most completely in the absence of light, and fade the more rapidly in proportion to the intensity of the illumination to which they are exposed. Hence we are compelled to recognise light as a destroyer of animal colouration.

But light is generally regarded not merely as a colour-destroyer, but as a colour-producer, and it is with this its supposed function that we have now to deal. Those who take here the affirmative view rely mainly on two facts, or supposed facts, to which we have already briefly referred,—the higher colouration and the superior brilliance of the tropical fauna, and the sombre hues of nocturnal and subterranean beings. At these facts we must look, and seek to ascertain their meaning. We must of course admit that Europe produces no humming-birds or trogons, no *Belionotæ* or *Pachyrhynchi*; but we must also remember that the total number of species of birds, of reptiles, and of insects found, say in South America, is far greater than the sum total existing in Britain or on the European continent. Hence, even if the tendency to produce a gay colouration were equal in either case, the probability is that South America would be the richer in gorgeous species. Again, travellers who visit tropical countries not unnaturally select the most showy forms, and their collections are therefore not a fair average. Naturalists, such as Mr. Wallace, who have taken the trouble to examine closely, find that even in New Guinea, Borneo, or Brazil dull-looking species exist in numbers. Had we catalogues of the insects of such countries as complete as those we possess for Britain, France, or Germany, our views as to the general character of a tropical fauna would be doubtless modified. As the insects of warm climates, also, are upon the whole larger than those of our hyperborean latitudes, they necessarily attract attention, and their beauty does not pass unseen; yet every entomologist knows that even in Britain we possess "tiny miracles of Nature" which, if viewed with a lens of low power, display a splendour little—if at all—inferior to the most richly attired tropical species. We will merely mention, as instances, *Chryseis ignita*, *Chrysomela cerealis*, *Donacia proteus*, *Polydrusus micans* and *flavipes*, *Rhynchites betulæ* and *populi*, *Lampra rutilans*, and *Anthraxia salicis*. *Calosoma sycophanta*, also, if very rare in Britain, is very common in certain parts of Central Europe, and may be fairly considered one of the most gorgeous species of the entire family of Carabidæ to be met with in any part of the world.

The case, then, seems to stand thus:—We have in Britain certain species, small, and it may be rare, which display the very same shades of colour and the same brilliance as we find in the most admired forms of tropical life. This fact seems to us scarcely consistent with the theory that the more intense light of low latitudes is a prominent factor in the production of splendid colours. Were such the case gaily-coloured species in our climate would not merely be fewer and smaller; they would rather be altogether wanting.

Again, different portions of the torrid zone differ very widely as regards the number, and even the beauty, of the richly-attired birds and insects they produce. Thus, as Mr. Wallace has pointed out, in New Guinea 50 per cent of the birds are brilliantly coloured, whilst in the Malay Islands and in the Valley of the Amazon the proportion does not exceed 33 per cent. Can this distinction be rationally ascribed to any excess of light enjoyed by New Guinea over and above the amount received by the Valley of the Amazon? Both these respective districts lie under the Equator; both are fruitful, plentifully supplied with moisture, well-wooded, and exposed—as far as we can perceive—to very similar meteorological conditions. But if excess of light cannot be the cause of the superiority of New Guinea over equinoctial Brazil, why should it be put forward to explain the superiority of Brazil as compared with Britain? Why should the fauna of the Philippine Islands, as is remarked by Mr. Wallace in his invaluable "Glasgow Address," be so rich in species of exceptionally splendid colours? Can there be in those islands either any excess in the quantity or any peculiarity in the quality of the sunlight? That there is, no one has yet even attempted to show, and were such the case it would doubtless be traceable in a variety of phenomena not limited to the organic world.

Another important point has been raised by Mr. Bates. He shows that whilst in many tropical butterflies the males are most splendidly coloured, the females—in numbers of cases at least—are sombre and insignificant in appearance, so much so that in former times they were often regarded as specifically distinct from their mates. If excess of light, therefore, be the producing cause of the splendour of the tropical Lepidoptera, why should not the effect appear alike in both sexes? To this argument, however, the reply has been made that in these very species the females are exceedingly sedentary in their habits, remaining generally concealed in shady thickets, whilst the males flutter about in the sunshine, and, being thus more exposed to light, experience modifications which—transmitted with constant accumulation from one generation to another—have produced the splendour now characteristic of their sex. To this question of the relative amount of exposure to light in different stages of existence we shall have to return.

But the amount—or at least the intensity and clearness—of the sun does not necessarily vary with latitude alone. The air of some countries is more transparent, less obscured by fogs and clouds than that of others. More light evidently reaches the earth's surface on open plains or on table-lands and in deserts than in dense forests and in narrow valleys Do we find any corresponding variation in the prevalent hues of the animal population of these respective localities? Mr. Wallace points out that the most brilliantly-clad birds and insects are dwellers in the forests where the amount of light received is comparatively scanty. On the other hand, in the deserts, where—as we have already mentioned—light must attain its terrestrial maximum, the prevalent colouration, if not dark, is certainly neither light nor brilliant. As the Rev. H. Tristram remarks, in such regions the smaller Mammalia, the birds, the snakes, and lizards are alike sand-coloured, their hues having evidently more reference to concealment than to the influence of an intense illumination. There is indeed, if we wish to come to details, a curious want of harmony in the effects which light is expected to produce. We know that it bleaches in certain cases and darkens in others; but it is no easy task for us to predict when either of these opposite effects will be manifested. Still it is perfectly possible that light might have a bleaching power upon some living organisms, and a darkening effect upon others, according to their different molecular structure. There is, for instance, little doubt but that the air of Persia is, as a rule, exceedingly transparent; the climate is dry, mists and clouds comparatively rare, woodlands scanty, and the country generally open. We have even heard it stated that there the satellites of Jupiter are occasionally visible with the naked eye. Here, therefore, we have doubtless a case of light in its greatest intensity; but, according to Mr. Blanford, Persian specimens are generally paler than their nearest European representatives. Here, if light be directly concerned, its action must be of a bleaching character; yet we generally find in mammals, in birds and reptiles, as well as in insects, the upper surface, or portion most exposed to the sun, is darker than the under side, or than parts generally kept in the shade. An animal in whom the contrary arrangement prevails—such as the common badger—has much of the appearance of a caricature. This darkening of the superior surface of animals is again adduced as an instance of the chromogenic power of light, a view to which we shall afterwards take occasion to revert.

As regards the comparison between the tropical and the extra-tropical faunæ the case may, perhaps, be fairly summed up thus:—There are certain cosmopolitan groups whose members, wherever found, are alike devoid of rich or brilliant colouration; there are other groups—such as the Ornithoptera, the Papiliones, the Buprestidæ, the Cetoniadæ, the trogons, humming-birds, birds of paradise, &c.—which have a remarkable and hitherto-unexplained tendency to the development of splendid hues, and which, if not exclusively tropical, have their head-quarters and produce their largest representatives within the torrid zone. Other groups, again, attain their greatest splendour beyond the tropics, as, *e.g.*, the ducks, the pheasants, and among insects the ground-beetles or Carabidæ. It has, indeed, been suggested that if the colder regions of the earth are now inferior to the tropical districts in the beauty of their fauna, the cause may be sought in the ravages of the Glacial epoch. If the most magnificent species were forest-dwellers, as we now find it to be the case in warm climates, their destruction would be almost inevitably involved in the desolation of their haunts and the annihilation of their food. Perhaps, too, the very splendour of such supposed forms would render them more conspicuous to their enemies, and thus accelerate their extirpation. All such speculations, however, are little more than conjectural. We conclude indeed, judging from the fossil remains of insects discovered at Œningen and elsewhere (See "Quarterly Journal of Science," vii., 255), that certain groups, now mainly tropical or subtropical, were very extensively developed in Central Europe; but at the same time we find indications that the climate, at least as far as warmth is concerned, was almost tropical in its character.

We may next inquire whether the relative brilliance of colour in various animal groups is at all connected with their diurnal or nocturnal habits, or with their greater or less exposure to light at different stages of their development. It is a truism that the diurnal Lepidoptera are upon the average much more highly coloured than the nocturnal species, the moths. Some weight has been laid on the circumstance that in butterflies both sides of the wings are freely exposed to light, and that both are also adorned with a variety of hues, whilst in moths, where the under surface of the wings is not turned to the light, it generally exhibits a dull and uniform colouration; but these facts admit of much qualification. Even among the small number of beetles indigenous in Britain there are some—such as *Erebia Cassiope*, *Cœnonympha Davus*, and *Thanaos Tages*—certainly less brightly coloured than many moths. Many species of butterflies, also, if richly coloured on the upper surface of the wings, can boast no gay or varied tints beneath. We need only mention the common peacock (*Vanessa Io*). Again, in certain genera of moths we find colours as vivid as can be met with in butterflies—*e.g.*, *Callimorpha*, *Euchelia*, *Chelonia*, and *Catocala*. The most remarkable feature in these genera is that the chief display of colour appears on the upper surface of the hind wings—a part as little exposed to light as the lower surface, since when the insect is at rest, in the daytime, it is completely screened by the anterior pair of wings.

In the larva state it cannot be said that Lepidopterous insects are much exposed to light. As a rule the caterpillars of the diurnal as well as of the nocturnal species prefer shade to sunshine. It is perhaps somewhat curious that the habits of the larva stand in no regular connection with the diurnal or nocturnal character of the mature insect.

Turning to the Coleoptera, we find further facts unfavourable to the supposed predominant influence of light upon the development of colour. Such Coleopterous larvæ—and they are the majority—as live in total darkness are, indeed, generally of a dull dirty grey, contrasting strongly with caterpillars which are more or less exposed to light, and many of which exhibit a bright and pleasing colouration.

This circumstance, like the etiolation of plants reared up in darkness, is certainly in favour of the view that light is not without influence upon organic colouration; but, on the other hand, let us consider the after-life of some of these dull-looking beetle-grubs. The most gorgeous, perhaps, of all Coleoptera are the Buprestidæ. These creatures spend the whole of their larval and pupal life within the trunks of trees, and consequently in total darkness. When mature, indeed, they sport for a time in the chequered sunlight of the woodlands. But why, if light be the main cause of animal colouration, should they be so far superior in brilliance to the Longicornes, or wood-beetles, which from birth to death are exposed to precisely the same circumstances? Taking the opposite extreme, the Staphylinidæ—of which the common "devil's coach-horse" is a familiar example, rank in appearance among the dullest and least decorated of all the insect tribes, whether they inhabit cold or warm climates; yet these creatures, instead of leading the earlier part of their life in complete and constant darkness, are active when larvæ, and may be seen running about in the daylight, seeking for prey. Surely, therefore, being so much more exposed to light than the Buprestidæ or the Cetoniadæ, they ought, on the theory we are examining, to be at least correspondingly beautiful. Let us turn to the Melolonthidæ, of which the common and destructive insect known as the cockchafer may serve as the type. Their early life is spent in darkness, since when larvæ they live underground, devouring the roots of plants. When mature their colours must be pronounced far less brilliant than those of their near allies, the rose-beetles (Cetoniadæ), which are equally nursed in darkness. It will be of course objected that the adult cockchafer is a nocturnal—or at least a twilight-loving—insect, while the rose-beetle feeds and flies by day. We will therefore take another instance—that of the Elateridæ, or click-beetles. As larvæ they, like the immature cockchafer, live underground, but when mature they are diurnal in their habits; yet the general colouration of the family is what some people call "sober," scarcely more gay than that of the Melolonthidæ, and forming a most striking contrast to that of the Buprestidæ, whom they so closely approach at once in their structure and in the degree of light which they encounter, both in their earlier stages and in mature life. Again, we may consider the weevils (Curculionidæ), all of them when larvæ burrowing from daylight in the interior of fruits and in the buds and stems of plants; yet when mature some of them—*e.g.*, the diamond-beetle—are as remarkably brilliant as others are conspicuously sombre.

On the other hand, attention is drawn to the Chrysomelidæ, to which the redoubtable Colorado beetle—vilely called the potato-bug—belongs, a family very richly and brightly coloured. Their larvæ are active, and they are thus throughout their lives exposed to the sunshine.

Among the animal population of the seas and rivers, also, we meet with facts, not a few, difficult to reconcile with the hypothesis under examination. It must be admitted that in all waters, save the very shallowest, the amount of light enjoyed must be very decidedly less than that which falls upon the surface of the land in similar climates; yet we do not find that the denizens of the waters are, as a general rule, less vividly coloured than those of the dry land. On the contrary, fishes, crustaceans, molluscs, besides aquatic forms lower in the scale of existence, such as the sea-anemones, display all the colours of the rainbow in a purity and in a profusion rivalling what we observe in the most gorgeous birds and insects. We admit that splendid oceanic forms are more abundant in tropical waters than in higher latitudes, and also that in a majority of cases the inmates of shallow waters are more vividly coloured than the dwellers in deeper, and consequently darker seas. But what must be inferred from the following observations, extracted from a paper by H. N. Mosely, late Naturalist to the *Challenger* Expedition, read before the Linnean Society on February 15th, 1877?—"A species of *Edwardsia* from 600 fathoms has undergone but little modification from the littoral form. The *Cerianthus* from 2750 fathoms is like its shore-brethren. Thus one species is found in shallow water at the Philippines, under the full glare of the tropical sun, while another species exists at 3 miles depth, where solar rays never penetrate, and where the water is at freezing-point. The deep sea-anemones retain vivid colours in the dark."

This fact is very suggestive. It agrees ill with the often-expressed view of teleologically-disposed naturalists, that all the brilliant hues of animal and vegetable life have been called into existence for man's delectation; but no less does it clash with the conclusions drawn from the paleness and obscurity of certain nocturnal, subterranean, or cave-haunting animals, such as the Coleopterous larvæ to which we have referred, wood-lice, crickets, &c. Light, it would seem, is not the sole condition for the production of positive colour; nor are the dwellers in darkness necessarily restricted to a garb of whites, blacks, and greys. It can, further, scarcely be contended that the land-shells of any country are more vividly and intense coloured than the marine shells of its coasts, many of which are as highly decorated within as without; yet a land-shell will doubtless receive a larger share of the solar radiations than a sea-shell.

Again, whilst there is thus abundant proof that an aquatic or even a deep-sea existence is not necessarily incompatible with a rich colouration, we find certain groups—the aquatic insects—ordinarily plain in their hues. The water-beetles, chiefly frequenting shallow pools and rivers, present ordinarily a dark olive, black, or brown colouration, relieved at most with rusty yellow, and those of tropical climates show little, if any, pre-eminence in this respect over their allies from colder regions. But these beetles, be it noted, if devoid of splendour, are not etiolated. The water-scorpion, water-boatman, and other aquatic Hemiptera, though living rather on than in the water, and fully exposed to light, are also remarkably plain in their colouration.

We have repeatedly referred to nocturnal animals; but it will be observed that in the higher forms of life the common views concerning their dominant colours scarcely hold good. Thus the owls, though not decked out with any metallic hues, differ little in the general character of their colouration from their diurnal kindred, the hawks, presenting bold, well-defined patterns, and a variety of black, fawn, brown, buff, and white shades. Few mammals display more vivid hues than the Felidæ, most of which are unquestionably nocturnal. Many nightly or subterranean insects, also, such as *Sphodrus leucopthalmus* and *Pristonychus terricola*, show no signs of etiolation. Even the common cockroach makes no approach to that pallid, ghastly hue which is commonly supposed characteristic of animals inhabiting sunless localities. Amongst nocturnal species we believe few, if any, instances can be found where the male surpasses the female in brightness or depth of colouring.

Mr. Wallace, however, whilst going perhaps even farther than we should be prepared to accompany him in the rejection of the theory which regards animal colouration as directly proportionate to the intensity of solar radiation, gives some curious instances of phenomena proving that in certain cases light has a direct action upon the colours of organic beings. Thus Mr. T. W. Wood, some time ago, pointed out that the chrysalids of the small "cabbage white" (*Pontia rapæ*) varied in colour when the larvæ had been fed up in boxes lined with different coloured materials.

Those which were kept in black boxes were nearly black, whilst such as had lived in white boxes were almost white. He observed corresponding changes in the same species in a state of Nature: chrysalids fixed against a whitewashed wall being whitish; those secured to a red brick wall being reddish; whilst those fixed against a pitched paling were nearly black. The cocoon of the emperor moth is also observed to be either white or brown, in accordance with the colours of surrounding objects. A still more decisive instance of such changes has been observed in the chrysalis of *Papilio Nireus*, a South-African butterfly which has been studied by Mrs. Barber. It acquires, more or less exactly, the colour of any contiguous object. "A number of the caterpillars were placed in a case with a glass cover, one side of the case being formed by a red brick wall, the other sides being of yellowish wood. They were fed on orange-leaves, and a branch of the bottle-brush tree (*Banksia*) was also placed in the case. When fully fed some attached themselves to the orange twigs, others to the bottle-brush branch—and these all changed to green pupæ; but each corresponded exactly in tint to the leaves around it, the one being a dark and the other a pale faded green. Another attached itself to the wood, and the pupa became of the same yellowish colour; while one fixed itself just where the wood and brick joined, and became one side red, the other side yellow."

This Mr. Wallace pronounces "a kind of natural photography, the particular coloured rays to which the fresh pupa is exposed in its soft semi-transparent condition effecting such a chemical change in the organic juices as to produce the same tint in the hardened skin." This power of the pupa to assume the colour of closely adjacent objects, however, is limited, since when Mrs. Barber surrounded one of her caterpillars with a piece of scarlet cloth the pupa displayed its ordinary green tint, though the small red spots with which it is marked were rendered abnormally bright. It must be recorded, however, that these very interesting changes are confined to the chrysalis, and do not appear to have extended in any way to the mature butterfly. We have never been able to trace any modification in the colours of butterflies reared, for one generation, in abnormally-coloured light, nor, as far as we are aware, has any other observer been more successful.

A correspondence has also been in some instances traced between the colours of animals and those of the localities which they inhabit and the food which they eat. Spiders have been found of exactly the same tint as the flowers in which they lurk. Mr. Wallace, on the authority of Sir Charles Dilke, mentions a pink-coloured *Mantis* which, when at rest, closely resembles the pink flower of an orchis, and is thus enabled to seize unsuspecting butterflies. But we should be wrong in ascribing such similarity of colouration to the effects of reflected light, or, indeed, of any merely physical influence. They are almost certainly due to physiological causes, and are instances of what is called "protective colouration."

There is another class of phenomena which at first sight seems due to the action of light. Many insects when they first emerge from the pupa are abnormally pale, and do not take their full mature colouration until after a longer or shorter interval of time. It was in virtue of this property that an entomologist, commissioned by the German Government to inspect a field where the dreaded Colorado beetle had made its appearance, was enabled to decide that these insect enemies had only just appeared in the mature form, and that on turning up the ground a further stock would be found in a rudimentary state, as on actual trial was found to be the case. But this gradual development of colour has not been proved to be the result of light. We have reared up caterpillars in perfect darkness, and have found their colours on reaching maturity no less brilliant than those of their fellows which had been exposed to light in the ordinary course of nature. In the case of interference-colours a change in the physical condition of the integuments, consequent of their drying and hardening on exposure to the air, is doubtless necessary for their development. The evolution of the pigment-colours we are at present investigating, and believe that it is simply due to a process of oxidation.

Some other of the phenomena advanced in support of the "light theory" of organic colouration may also be, with great probability, referred to other causes. Thus some ascribe to light the fact that the upper surface of most animals is more intensely coloured than their under side. Many fishes have a dark back, and a pale greyish blue or greenish belly; but, as Mr. Wallace points out, this arrangement seems more protective than due to the action of light. An enemy—say a sea-bird—looking down from above will have difficulty in distinguishing the dark back of the fish amidst the water. On the other hand, an enemy looking from below will see the pale belly of the fish against the dull bluish colour of the sky as seen on looking up through the water, and will scarcely detect its presence. Now, were this arrangement of colours reversed the fish would be much more readily seen, either from above or from below, and the chances of its escaping from its enemies would be much reduced. At the same time it must be confessed that this explanation is not admissible in all cases of a similar arrangement of colour. Thus in many Crustaceans unable to swim, and therefore not liable to be seen by any enemy from below, the under surface is much paler than the back. Similarly slugs—whether creeping upon the ground, upon the stems or leaves of vegetables—are liable to be espied from the back or the sides, but never from beneath; yet in most cases their under surface is decidedly paler than their back. These instances, and others which might be adduced, certainly seem to agree better with the supposition of a darkening influence due to the freer action of light upon the upper side than with that of a protective distribution of colour.

But from the whole of the evidence before us, especial attention being paid to the case of the deep-sea anemones, we are forced to conclude that the colouration of an animal species is not, in the mathematical use of the word, a function of the amount of solar radiations to which it is exposed. That this conclusion does not compel us to deny the influence of sunlight upon the hues of all animals, under all conceivable circumstances, scarcely needs to be stated.

The fact that Lepidopterous larvæ are in a majority of cases, partially at least, of a green colour is not inexplicable. They retain in their bodies, in an undecomposed state, the chlorophyll of the leaves upon which they have fed. Larvæ, on the other hand, whether Lepidopterous or Coleopterous, which feed not upon leaves, but upon wood, roots, seeds, &c., not containing chlorophyll, may naturally be found deficient in this green colour, without our taking the presence or absence of light into account. Hence we need not wonder that the caterpillars of the goat-moth and the wood-leopard, the larvæ of the Longicornes, Buprestidæ, Dynastidæ, &c., are not green: they have not been consuming a green pigment. But why have we comparatively so few green butterflies and moths, and so many green birds and green beetles? The green colours found in birds and in beetles—with the exception of such forms as *Cassida*, a *leaf-feeder*, are due not to a pigment, but to the interference of light, so that their formation must be explained on different principles. The paucity of green butterflies may, perhaps, be traced to the fact that chlorophyll is a mixture of two colouring principles,*—cyanophyll, which is blue, and xanthophyll, which is yellow,—the latter of these colours being much more permanent than the other. Hence if, as appears exceedingly probable, chlorophyll is assimilated by leaf-eating insects, a number of phenomena connected with their colouration become at once intelligible. We have mentioned in an earlier part of this paper that among animal tints pigment-greens are generally the first to fade, and that they become a dull yellow or a yellowish drab, as may be observed in a specimen, say, of *Cassida equestris*, which, however carefully preserved, soon loses its pale green hue, and turns yellowish. The reason of this change, we contend, is that the cyanophyll or blue colouring-matter first undergoes decomposition, while the yellow xanthophyll alone remains. A similar change, taking place in the living animal in its pupa condition, is the cause why pigment-greens are so rare alike among Lepidoptera and Coleoptera, whilst yellows and browns of different shades are so exceedingly common, and relatively so permanent. We find also that certain caterpillars, green in the earlier part of their life generally, though not invariably, take a brown colour as they approach the time of their assuming the pupa state.

But even supposing that chlorophyll is demonstrably assimilated or deposited in the tissues of certain insects, the hypothesis we have been suggesting takes us but a very little way. We have still to ask why the green colour in certain species remains undecomposed to the mature condition, whilst in others it disappears in the pupal or even in the larval state, and how, after disappearance or absence in the larva, as in *Chœrocampa Elpenor*, it appears in the perfect insect ? We have to enquire why certain diurnal caterpillars, consuming as much chlorophyll as do any others,—*e.g.*, *Vanessa Io*, *V. xanthomelas*, *V. urticæ*, &c.,—are free from a green colouration ? At the same time we must admit that in caterpillars of this class a yellow pattern is very rarely absent, as if the xanthophyll had already been separated from the cyanophyll. We have to explain the pigment-blues, of which there seem to be two, if not three, the identity of which with cyanophyll must not be too rashly assumed, though in many cases we see both blue and yellow spots appearing in a butterfly, as if the two colours, which in its earlier state had been blended together, were now

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* Some authorities consider that chlorophyll is a mixture not of two, but of three colouring principles (Fremy and Cloëz), or of four (Stokes). As these, however, are in all cases found to be respectively blue and yellow, the view we have taken will not be affected by these discordant results.

separated, as in *Papilio Machaon.* We have, further, to throw a light upon the origin of the pigment-reds, to two of which Mr. Wallace refers as being different in their chemical constitution and behaviour.

But chlorophyll is not the only substance which has been called into requisition in order to explain the mysteries of animal colouration. It has not escaped the attention of biologists that all those creatures which develope, more or less frequently, beautiful hues are precisely the same in which uric acid is abundantly secreted,—*i.e.*, birds, reptiles, insects,—whilst in the Mammalia, in which the secretion of uric acid is trifling, the prevailing colours are dull. It was asserted that whilst uric acid is abundantly found in the excretions of parrots, humming-birds, &c., at other times of the year, during and immediately before the moulting season it was absent. Hence the inference that this compound might play a part in the elaboration of the new plumage was not unwarrantable. In addition came the fact that a beautiful violet colour, known as murexide, and capable of producing a variety of shades, was artificially obtained from uric acid.* Unfortunately when these investigations were carried on the distinction between interference-colours and absorption-colours had not been fully apprehended, and the iridescent hues of humming-birds, trogons, *Belionotæ*, were supposed to be due to some peculiar pigments of unknown composition. Nor has it, as far as we are aware, ever been shown that the excreta of splendidly-coloured birds are richer in uric acid than those of sea-fowl. For the present, therefore, the uric acid theory must be considered as useless.

A consideration of the food of different species might at first sight appear likely to throw some light upon the nature of their colouration. But we find intense splendour and varied tints alike among carnivorous species (Cicindelidæ and certain Carabidæ), wood-eaters (Buprestidæ), and leaf-eaters (Chrysomelidæ). We find dull and sordid colours among many carrion- and dung-feeders (Silphidæ, Aphodiidæ, Staphylinidæ), whilst others addicted to a similar diet—such as most species of the genus *Phanæus*—display the most splendid hues. Nor is an examination of the diet of birds more satisfactory.†

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* Murexide, known in the commercial world as "Roman purple" and "Tyrian purple," was some time ago prepared from guano,—*i.e.*, the excreta of sea-fowl,—and was in considerable demand among dyers and calico-printers. Being costlier than the coal-tar colours, it is now superseded.

† In addition to the case of chlorophyll above mentioned there seem to be individual instances where the colouring matter of a plant, if eaten by insects, may be traced in their secretions. We do not know whether the deep reddish

It may perhaps be thought that in an inquiry into the influence of light upon the colouration of animals a consideration of their diet, or of their peculiar secretions and excretions, is out of place. But whether solar radiations, or local atmospheric influences, or the need of protection take a greater or smaller share in the development of colour, there must be essential differences in the material upon which these causes act. Mammals are exposed to the same climatic influences as birds and insects, and are likewise exposed to dangers which they might escape by a colouration favourable to concealment. Their hair is, chemically considered, a material no less suitable for the display of gay and brilliant colours than are the feathers of birds, the scales of serpents, or the chitinous coating of insects; yet neither in lustre, in varying play of colour, nor in delicacy and elaborateness of design, do they make even the faintest approach to a rivalry with these groups of animals. There must therefore be an internal source of colouration, not everywhere present, upon which external influences may react.

Mr. Wallace, whilst rejecting the light-theory, brings forward certain principles which he considers throw a light upon the phenomena of colour in organic nature. Whilst demurring to the common conclusion that tropical light and heat are the cause of colour, he fully recognises the general fact that "all the more intense and gorgeous tints are manifested by the animal life of the tropics, whilst in some groups, such as butterflies and birds, there is a marked preponderance of highly-coloured species." This phenomenon he ascribes to a variety of causes, some of which yet remain to be discovered. The foremost place is given to the following consideration :—"The luxuriant vegetation of the tropics throughout the entire year affords so much concealment that colour may there be safely developed to a much greater extent than in climates where the trees are bare in winter, during which season the struggle for existence is most severe, and even the slightest disadvantage may prove fatal." Fully admitting the force of this consideration in the case of birds, we must yet, with all the deference due to so eminent an authority as Mr. Wallace, point out that it can have very little moment as regards insects which during the winter are in a dormant condition, as larvæ or

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violet liquid which exudes from *Timarcha lævigata*, an insect feeding upon bedstraw, a plant of the madder tribe, has ever been examined for alizarin or purpurin.

pupæ, either in the earth, in the trunks of trees, or other localities where neither beauty can betray them nor its lack screen them from the pursuit of any enemy.

As the first among the causes of colouration he places the need of protection. He points out that browns and other tertiary colours, being most readily produced by "an irregular mixture of many kinds of solar rays, are most likely to occur when the need of protection is slight, or even when it does not exist at all, always supposing that bright colours are not in any way useful to the species." Hence browns, olives, and other dirty colours may naturally be expected to predominate.

Brilliant colours, again, often serve as a sign that their wearer possesses some unpleasant or dangerous property, and hence warn possible enemies to pass on and seek some less nauseous prey. The number of apparently feeble and defenceless species which are clad in the most conspicuous colours, and which are avoided and refused by birds, monkeys, spiders, &c., is astonishing. The present writer, in a paper read before the Entomological Society (Trans. Ent. Soc., 1877, Part III., p. 205) has shown that, in a great number of cases at least, the most showy and conspicuous caterpillars feed upon plants either absolutely poisonous or possessing offensive flavours and odours, whence the rejection of such larvæ by insectivorous animals. Their brilliant colouration is therefore simply a danger-signal.

The theory of "Sexual Selection," upon which Mr. Darwin lays great weight, Mr. Wallace finds himself unable to accept as in any way an explanation of the distribution of colour in animals. He remarks that "whilst male butterflies rival, or even excel, the most gorgeous male birds in bright colours and elegant patterns, there is literally not one particle of evidence that the female is influenced by colour, or even that she has any power of choice, whilst there is much direct evidence to the contrary." In the case of the silk-moth Mr. Darwin admits that "the females appear not to evince the least choice in regard to their partners." On the principle of natural selection among a number of rival male butterflies, "the most vigorous and energetic" will probably be successful, and, as these properties are very generally correlated with intensity of colour, natural selection "becomes a preserver and intensifier of colour." Very similar is the case among birds. We know that in many species the male displays his colours and ornaments, but, as Mr. Wallace contends, there is a tota absence of any evidence that the females admire, or even notice, this display. "The hen, the turkey, and the pea-fowl go on feeding while the male is displaying his finery) and there is reason to believe that it is his persistency and energy, rather than his beauty, which wins the day." Here, again, vigour and intense vitality seem to be the chief recommendations of the male in the eyes of the female, and these—as is very strikingly manifest in the game cock—appear correlated with intense colouration. Mr. Wallace resumes:—"Evidence collected by Mr. Darwin himself proves that each bird finds a mate under any circumstances. He gives a number of cases of one of a pair of birds being shot, and of the survivor being always found paired again almost immediately. This is sufficiently explained on the assumption that the destruction of birds by various causes is continually leaving widows and widowers in nearly equal proportions, and thus each one finds a fresh mate; and it leads to the conclusion that permanently unpaired birds are very scarce, so that, speaking broadly, every bird finds a mate and breeds. But this would almost or quite neutralise any effect of sexual selection, of colour, or ornament, since the less highly-coloured birds would be at no disadvantage as regards leaving healthy offspring." Whilst accepting this conclusion, we may ask whether the same argument is not capable of further application? It is generally stated that the "fittest" male—*i.e.*, the one most in harmony with the circumstances in which he is placed—will have the best chance of securing a mate and of leaving offspring, whilst the feebler, the slower, the less energetic, and those least in harmony with the situation, will be left in a state of single blessedness, and will not transmit their attributes to posterity. But on the principles laid down in the passage we have just quoted the effects of natural selection will be greatly neutralised. It must, however, be remembered that the destruction of birds, especially in a state of Nature, will not fall exclusively or mainly upon those which have secured mates, but will likewise extend to the unwedded.

Whilst combatting Mr. Darwin's view, that the brilliant colours of butterflies have been acquired for the sake of protection, Mr. Wallace remarks:—"It is in fact somewhat remarkable how very generally the black spots, ocelli, or bright patches of colour are on the tips, margins, or disks of the wings; and as the insects are necessarily visible while flying, and this is the time when they are most subject to attacks of insectivorous birds, the position of the more conspicuous parts at some distance from the body may be a real protection to them." This rule, however, is by no means universal. The fire-wasp (*Chryseis*) and not a few other Hymenoptera have brilliantly-coloured bodies, but colourless and transparent wings, which when expanded and in action are scarcely visible. In numbers of Lepidoptera the more intense colours, especially reds, are found entirely or mainly on the posterior wings, which extend to a less distance from the body than do the anterior pair. In many cases again Lepidoptera, Coleoptera, and Hymenoptera display conspicuous colours at the extremity of the abdomen, where a blow from the beak of a bird would doubtless permanently disable.

A question may here arise concerning the use of the colouration of the posterior or true wings in certain beetles, a circumstance not sufficiently examined. Whilst these wings in the vast majority of Coleopterous species are colourless, or at most of a very faint yellowish hue, in the Colorado beetle they are pink, and purple in several *Chrysochroas*, *Pachnodas*, and *Lomapteras*. Why should these species thus differ from other closely-allied forms, with whom they appear to agree most closely in their habits?

We have no doubt that Mr. Wallace's formal declaration against the doctrine of Sexual Selection will attract the attention of disbelievers in Evolution, and we venture to hope that all the comments which will be elicited may not be beside the question.

IV. ON THE DISCOVERY OF STONE IMPLEMENTS IN GLACIAL DRIFT IN NORTH AMERICA.

By Thomas Belt, F.G.S.

THE discovery of great numbers of stone implements in New Jersey, by Dr. C. C. Abbott, in deposits which are probably of Glacial age, is of such great importance that a detailed account of the beds in which they have been found and a discussion of their antiquity will be interesting to many. I had, during the past autumn, an opportunity of studying these beds under the kind guidance of the discoverer of the implements; and I am also indebted to Prof. Cook and Prof. Smock, of the Geological Survey of New Jersey, for much information respecting the glaciation of the State. I shall, in the first place, give a brief statement of what was before known of the earliest traces of man in North America.

Before these discoveries there had been many intimations of the great antiquity of man in the western hemisphere. Probably one of the earliest of these was the discovery of the fragment of a human bone which was said to have been found at the base of 60 feet of loess, near Natchez, on the Mississippi, along with the remains of the megalonyx and other extinct quadrupeds. A full description of the deposits in which these remains were discovered has been given by Sir Charles Lyell, in his "Second Visit to the States."* We learn there that Dr. Dickeson, of Natchez, felt persuaded that the fragment of human bone had been taken out of the clay underlying the loam; but Sir Charles Lyell could not ascertain that it had been actually dug out in the presence of a geologist, or any practised observer, and he speculated on the possibility of it having fallen from above, into the bed of the ravine, from some old Indian grave. This was in 1846: long afterwards, when the discoveries in Europe had established the contemporaneity of man and the great extinct pachyderms, he recalled the fact

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* *Op. cit.*, vol. ii., p. 196.

that the human bone was in the same state of preservation and of the same black colour as the bones of the mastodon and megalonyx, said to have been found with it; and he was disposed to think that he had discussed its probable age with a stronger bias, as to the antecedent improbability of the contemporaneous entombment of man and the mastodon, than any geologist would now be justified in entertaining. *

The fragment of a human skull from Calaveras, in California, which was said to have been found in gravel beneath five successive overflows of lava, would, if authenticated, be probably the oldest record of man in North America. The same doubts, however, have been expressed about it as about the Natchez remains, no geologist being present when it was exhumed. In the newer gold-drifts of California, along with the remains of the mastodon, elephant, tapir, bison, and horse, the implements of man have been frequently found.†

In the auriferous gravels of Kansas and Georgia stone and flint implements have also been discovered.‡

Dr. Samuel Aughey, in his account of the superficial deposits of Nebraska, states that the remains of elephants and mastodons are often found in the loess that overspreads nearly the whole of the State. In this deposit, in a railway-cutting near Omaha, 20 feet from the surface, he dug out himself a large coarse arrow- or spear-head which lay 13 inches below the lumbar vertebra of *Elephas americanus*.||

Near Alton, in Illinois, stone axes and flint spear-heads along with the bones of the mastodon are reported from drift below loess.§

All the above discoveries are in regions that drain either into the Pacific or the Gulf of Mexico.

Mr. Chas. M. Wallace has described the discovery by him of flint implements in stratified drift near Richmond, Virginia.¶ These deposits seem to be similar to those in which Dr. Abbott has made his discoveries in New Jersey. The valley of the James River is mantled by thick deposits of coarse gravel covered with brick-clays. The implements have been found occasionally in the clay, and more frequently

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* Antiquity of Man, first edition, p. 200.

† J. D. WHITNEY, Geol. Surv. California, vol. i., p. 252.

‡ Dr. D. WILSON, Canadian Journal of Science, October, 1877, pp. 559, 560.

|| Geol. Surv. of the Territories, 1876, p. 254.

§ Geol. Surv. Illinois, 1866, vol. i., p. 38.

¶ Amer. Journ. Science, March, 1876, vol. xi., p. 195.

in the underlying gravel. One of the sections given by Mr. Wallace shows the following succession of beds:—

	Feet.
Brick earth underlying greyish clay ...	9
Rounded gravel, reddish hue	4
Fine bluish sand...	12
Gravel and bluish pebbles...	4
Compacted sand (probably Tertiary).	

Several implements were found on the surface of the lower bed of gravel. This lower gravel contains large numbers of the pebbles from which the implements, for the most part, appear to have been fashioned. In some parts large boulders (one 8 ft. by 12 ft.) rest upon the gravel, and appear as if they had been brought by floating ice and deposited in gentle waters. Mr. Wallace notes the similarity of many of the implements to those of palæolithic age in Europe. I believe this is the first notice of the discovery of palæolithic implements on the eastern sea-board of North America,

The report by Dr. C. C. Abbott of his discoveries of stone implements in the drift-gravels near Trenton, New Jersey, appeared in the "Tenth Annual Report of the Peabody Museum," issued during the present year.* My attention was drawn to it, soon after its publication, by Dr. D. Wilson, of Toronto,—who has since reviewed Dr. Abbott's paper,†—and in consequence I visited the locality. Dr. Abbott showed to me a great number of the implements he had found, and afterwards accompanied me to the principal places near Trenton from which they had been obtained.

Whilst a few of the implements resemble some of the palæolithic chipped flints of England and France, the general form and type is of a ruder and more imperfect character. Some are simply made from rounded flat pebbles by chipping a cutting edge at one end. Amongst them are many of what Dr. Abbott has named the "turtle-back" type. It appears to have been formed by using a pebble with one side naturally flat, or by producing a flat surface by artificial fracture and bevelling down the other side by chipping, so as to produce a cutting edge.

Whilst the general character of the implements is ruder than the European, a few appear more like a spear-head than I have seen amongst the latter. I have shown a few,

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* *Op. cit.*, p. 30.

† Canadian Journal of Science, October, 1877, p. 557.

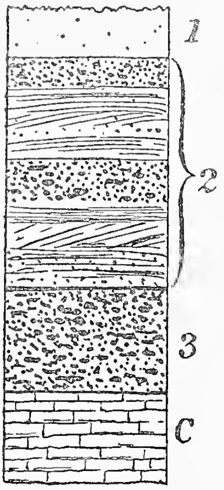
that Dr. Abbott liberally presented to me, to our greatest authority on stone implements, Dr. John Evans, and he considers they are different from the palæolithic type in Europe, and more resemble some of the ruder neolithic implements from Ireland. It is surprising that palæolithic implements from distant parts of the Old World resemble each other so closely, and it would have been more wonderful if those from America had been fashioned to the same type.

Most of these implements are from the high bluff facing the Delaware, near Trenton; some from other localities in the same valley. Amongst them is one which Dr. Abbott picked out himself from the face of a railway-cutting through a boulder deposit at Butzville, 50 miles north of Trenton. It is made from a flattened pebble, by one end being chipped to a cutting edge, the other being left in its natural rounded condition. Both on the rounded portion of this specimen and on the chipped part are what appear to be glacial scratches. Dr. Abbott informed me that nearly every stone in this deposit was covered with glacial striæ, and Professor Smock afterwards told me that the formation had been recognised by the Geological Survey as part of the terminal moraine of the great ice-sheet. I felt no doubt when I examined this specimen that it had been fashioned by man, but Dr. Abbott has since informed me that others to whom he has shown it think that it is barely possible that it may be of natural formation. I fully expect that it will be authenticated by further discoveries, but in the meantime it may be well not to base any theories upon it. I noticed small scratches upon some of Dr. Abbott's other specimens, and he kindly presented one to me showing these on one side. As I shall have to explain further on, the glacial age of at least some of these implements can be proved without reference to the one from Butzville, or whether the scratches on others are glacial or not; and I am disposed to place less value on the striæ on the Trenton specimens, because it is obviously possible that they may be artificial.

Opposite Dr. Abbott's house, about 2 miles below Trenton, the high bank bounding the river has been worn back into a deep bend, and a wide alluvial plain now lies between it and the Delaware. I sketched the section of the beds forming the bluff (Fig. 1) at this point. The top bed is here an unstratified sandy clay, with a few scattered pebbles, and occasionally very large boulders, none of which were, however, seen at this locality; beneath this lie alternations of fine sandy gravel, sand, coarse gravel, and boulder-beds. Dr. Abbott pointed out to me the upper layer of pebbles as the horizon from which he had obtained some of the implements in undisturbed ground.

Towards Trenton the bluff approaches the river, and just below the town forms its bank. The face of the bluff is mostly a talus; there are few good sections, and none that I saw were perfectly satisfactory. The best section was near the cemetery, where I made the sketch shown in Fig. 2. The lower bed (No. 3) is quite unstratified; only the upper 12 feet of it is seen where the section was taken, the lower part being covered with talus, but in other places

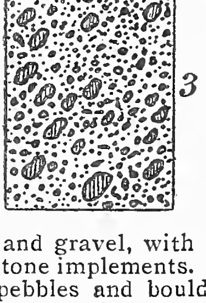
Fig. 1.



1. Sandy clay, 3 to 6 feet, with large boulders on surface in some places.
2. Alternations of gravel and sand, occasionally false bedded, 6 to 10 feet. Stone implements.
3. Unstratified pebble and boulder bed. Thickness very variable.

C. Cretaceous marls and clays.

Fig. 2.



2. Irregularly stratified sand and gravel, with occasionally larger stones intermixed, about 12 feet. Stone implements.
3. Thick unstratified bed of pebbles and boulders, mostly rounded, with many stones up to 15 inches diameter; larger ones rare. Base of deposit not seen.

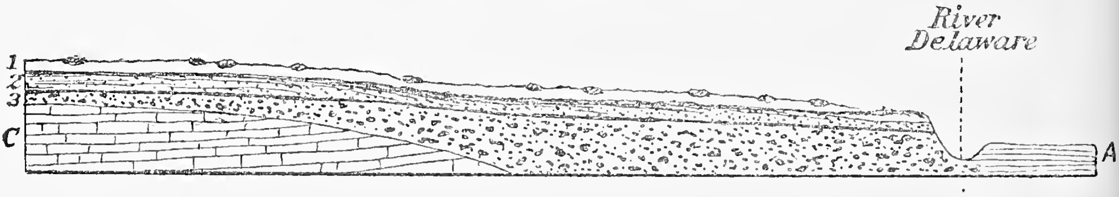
it was seen going down to and below the river, the surface of which is about 40 feet below the top of the bluff.

The irregularly stratified beds, No. 2, contain generally much smaller stones than No. 3. These beds, as in the former case, were pointed out to me by Dr. Abbott as the ones from which he had obtained the implements, and whilst I was present he discovered a rude one in the talus which appeared to have come from them, as we were too high up the slope for it to have come from the lower bed.

The bed of sandy clay (No. 1 in Fig. 1) has been denuded from the top of the bluff, but a little way back from the edge it appears, and contains great boulders scattered over its surface.

From all the sections that I saw, and from the information given to me by Dr. Abbott, I have constructed the general section (Fig. 3) showing the succession of the beds. Up to the time of my visit (October, 1877) no implements had been found in the lower quaternary bed (No. 3), though it seems to me extremely likely that they will yet be discovered. This bed, of mostly rounded boulders and large pebbles, is quite unstratified. Amongst the pebbles are many flattened ones, similar to those from which some, if not all, the implements found in the higher beds have been made. This rounded boulder-bed is of great extent, and has been described by Prof. W. B. Rogers* as extending from the Delaware into Virginia. At Washington the deposit covers the whole plain on which the city is built, rising to a height of about 200 feet above the sea over the low hills around it. Prof. Rogers has found in it stones containing *Scolithus linearis*, a well-known fossil of the Potsdam formation, having its nearest outcrop on the western side of the Blue

FIG. 3.—DIAGRAM SECTION BELOW TRENTON.



1. Sandy clay, unstratified, with a few pebbles, and with very large ar-transporte boulders on or immediately below the surface.
2. Irregularly stratified sands and gravels. Stone implements.
3. Pebble and boulder bed. Stones up to 15 inches across plentiful; larger ones rare. Stones mostly rounded, with a few subangular ones; no scratched ones seen.

A. Alluvium.
C. Cretaceous marls and clays.

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* "On the Gravel and Cobble-stone Deposits of Virginia and the Middle States." Proc. Bos. Soc. Nat. Hist., 1875, vol. xviii., p. 101.

Ridge. Some of the stones found near Richmond must have been brought 80 miles if they travelled in a direct line from the parent rock. At Richmond this deposit is said to present itself at various heights from the river-bank to the tops of the hills, mantling the irregularly denuded surface of the underlying formations, resting at one place on the Upper Miocene, at others on the Eocene, or yet older deposits.*

At Trenton this deposit is principally composed of crystalline rocks, many of distant origin, but none I believe have been noticed that may not have been derived from formations existing within the drainage area of the Delaware and its territories. Trenton is often mentioned as being about the southern limit to which the northern ice reached in the valley of the Delaware in glacial times, but I could find no traces of glaciation in the neighbourhood; and Prof. Cook informed me that it has not been noticed farther south in the valley than near Belvidere, about 50 miles in a direct line N.N.W. from Trenton. Prof. Cook and Prof. Smock have now traced the southern boundary of the land ice pretty clearly across the State of New Jersey, from the neighbouroood of Amboy, on the Atlantic coast, to Belvidere, on the Delaware. From thence it runs across to near Harrisburg, in the valley of the Susquehanna, where Prof. J. P. Lesley informed me were the most southern glaciated rock surfaces in that valley. North of an undulating line passing through these points the surfaces of the bed-rocks are rounded and polished, and scored with glacial striæ. By means of these markings, and by the direction from which transported rocks have been brought from their parent beds, the course of the ice has been mapped out from the Canadian boundary, where it was so thick as to be able to over-ride and move independently of the valleys, up to its southern termination, where it is found conforming with the direction of the main drainage channels. South of the line to which the northern ice extended the rock surfaces are often decomposed to a great depth. This is especially evident where the bed-rock is gneiss or granite. In such cases, for more than 50 feet from the surface, the rocks have often been changed to a clay that may be dug with a spade; whilst that it has not been otherwise reconstructed is evidenced by the fact that veins of quartz running through it remain in their original position.† Over the glaciated

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* *Ibid.*, p. 106.

† See T. Sterry Hunt, "Decomposition of Crystalline Rocks.' Amer. Journ. Sci., 1874, vol. vii., p. 60.

district, on the contrary, the decomposed crust has been removed, and the hard unaltered rock laid bare.

It has been shown that the decomposition of the rocks has been caused by the slow percolation of rain-water containing a little carbonic acid. It follows that the surface rocks had been exposed for long ages to this influence, and that the time that those of the glaciated districts have been subjected to the same action, since the Glacial period, is exceedingly short in comparison.

Another mark by which the glaciated may be distinguished from the non-glaciated country is by the occurrence of a deposit long known in Scotland under the name of "till," and on the Continent as "grund morane" and "moraine profonde." It is generally a stiff clay, full of small angular fragments of the rocks over which the ice has passed, and sometimes with large angular and subangular stones. It always more or less reflects the characteristics of the strata immediately in the neighbourhood and in the direction from which the ice has come. Thus in the vicinity of Toronto I noticed, as Mr. G. J. Hinde had before remarked,* that the till is packed with small fragments of black Utica shale and blue Trenton limestone—strata that the ice had passed over in its passage from the eastward. Along with these were larger fragments and slabs of the underlying Hudson River group, and a few rounded boulders of gneiss that were far travelled. Around New York many patches of till are left on the glaciated rocks. I visited Marion, near the city of New Jersey, by the advice of Prof. Cook, and found very fine sections of the glacial beds. The till is there not a stiff clay, but a rather sandy deposit, of a dark reddish brown colour, packed with the angular *débris* of the red triassic sandstones that form a large portion of the bed-rocks of eastern New Jersey. A few of the contained stones may have been brought from a distance, but the great bulk of them, as well as the sandy matrix, are of local origin.

The deposition of the till probably took place during the melting back of the ice-sheet. Dr. Dana has shown that the ice was very thick over New England, and that the pressure at its base would be so great as to force the plastic mass into the crevices of the rocks below, so as to tear off fragments from them, which, with any loose material it met with in its progress, would be gathered up and borne along in the lower portion of the ice-sheet.† Prof. Joseph Le Conte,

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* Canadian Journal, April, 1877, p. 8.

† "On the Glacial and Champlain Eras in New England." Amer. Journ. Sci., March, 1873.

in his study of the deposits left by the ancient glaciers of the Sierra Nevada,* has arrived at a similar conclusion. Prof. Hall, in his "Geology of New York," has given a large picture of a natural section on the shore of Lake Erie, in Chautauque county, where we may see as it were the till in course of formation. In some parts the top strata are only a little separated, with clay forced in between them; in others they stand on edge, or are broken up and the fragments scattered throughout the till.† I have seen similar instances on the shores of Lake Ontario. In our own country there are also many examples of the same action; one of the finest, that I have seen, being in the beautiful section of the glacial beds exhibited in the cliffs at the mouth of the Tyne, in Northumberland. In this way the lower portion of the ice appears to have become charged with stones and finer materials which were left on the surface when the great glacier melted back. The till is therefore restricted to the area that the land-ice covered, and is as much a memorial of its former presence as the scratched and rounded rocks on which it generally lies.

By the glaciated rock-surfaces up to the line I have already mentioned and their absence beyond, by the outspread of the till limited by the same boundary, and by the disappearance of the decomposed surface-rocks up to but not beyond that margin, we know that the land-ice did not reach, in the valley of the Delaware, farther than the neighbourhood of Belvidere, which is 50 miles to the north of Trenton. What relations, then, do the beds of drift that I have described at Trenton bear to the ice-sheet? To answer this question we must take into consideration what we know is taking place at the terminations of existing glaciers. Great streams of water run from underneath them, bearing along fragments of rocks that have been melted out of the glacier or fallen through crevices to the subglacial rivers. These are rounded by attrition, and spread out in sheets of pebbles often extending for miles below the terminations of the glaciers. From the enormous glacier that once filled the Delaware valley as far as Belvidere, and which was itself only a southern prolongation of the still greater northern ice-sheet, there must have been shed a vast amount of similar material. The large rounded drift that lies at the base of the quaternary beds at Trenton (No. 3 in Figs. 1, 2, 3) is almost undoubtedly a similar

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* Amer. Journn. Sci., 1875, p. 126.

† *Op. cit.*, Plate VIII.

deposit. It contains a mixture of all the various rocks over which the glacier of the Delaware would pass; those from the hardest formations being most abundant, as they would best survive the severe abrasions to which they had been subjected.

No implements are yet reported from this bed, so that, putting aside the disputed implement from the morainic accumulation at Butzville, we have no evidence at present that man frequented the valley of the Delaware before or during the greatest extension of the glacier of that valley. The deposits from which Dr. Abbott has obtained the implements lie above this great unstratified bed of rounded drift, and we should have great difficulty in fixing the approximate age of the implement-bearing strata if it were not for the fortunate occurrence of the sandy clay with large boulders (No. 1 in Figs. 1 and 3) clearly superimposed on the latter. We may now turn our attention to the consideration of this surface-bed and the relation it bore to the Glacial period.

In my discussion of the glacial and post-glacial phenomena bearing on the date of the excavation of the gorge at Niagara, published in this Journal,* I have described the occurrence of large boulders of crystalline rocks lying above all the other glacial beds. In the till which lies next the glaciated bed-rocks the stones are all of local origin; in the surface deposit they are all from the distant north.

Prof. James Hall, so long ago as in 1843, had fully recognised the importance of the occurrence of these far-transported blocks that lie scattered over the surface, and had noted the difference in the mode of their occurrence and in their composition from the rocks included in the lower glacial beds.† He shows that the glacial beds belong to two periods: one, the lower, which contains mostly local rocks; the other, the upper, containing far-transported crystalline rocks. He says that on the broad northern slope towards Lake Ontario, where hills are distant, there are numerous and extensive fields of boulders resting upon the surface, or but partially imbedded in the soil, and holding such a position that it is evident that they are of subsequent origin to the great body of detritus; and again, on the western prairies, long lines of boulders are to be observed stretching away for miles beyond the reach of vision, as if once forming a line of coast or deposited along some channel

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* *Op. cit.*, April, 1875.

† Geology of New York, Part IV., pp. 319 to 321.

or course of a current, though the general surface indicates no influence upon this portion beyond what is common to the whole. Prof. Hall considered that there was no explanation of the transport of these great blocks, excepting on the supposition that the whole surface was covered with water, over which they were floated on icebergs. "Had they been transported," he says, "by a powerful current over the bottom (which cannot be supposed from the inequalities of the surface) all the older drift would have been removed at the same time, and instead of finding them as we do now, mostly upon the surface, they would have been imbedded indiscriminately in the superficial detritus, and there would have been no means of recognising the products of different periods."*

Dr. Newberry, in his "Surface Geology of Ohio," has fully described the distribution of large boulders over the surface of that State. Even in Southern Ohio they are in some parts very numerous. He says that the large unscratched boulders are generally found on the surface, and that in the great series of excavations, which have been made in the construction of the railways and canals, they have been rarely met with below it. They are often seen resting on the fine stratified clays which form the upper part of the drift. And he observes that "it seems impossible that they should have been brought to such positions by glaciers or currents of water, as either of these agents would have torn up the underlying clays. We also learn, from their relative position, that these boulders were deposited at a later period than the most recent stratified beds of the drift series, and that they were floated to their present resting-places. In short, no argument is required to convince anyone who will glance at the facts that these boulders, and probably the gravel and sand with which they are sometimes accompanied, were floated on icebergs from the north shore of the great fresh-water lake which once filled the lake basin, and that as these icebergs melted, or when they stranded, their loads were discharged on the top of all the drift deposits which had been laid down in the preceding epochs of the Quaternary age."†

On the eastern side of the Appalachians, Prof. Hall has noticed the occurrence of these boulders in the valley of the Hudson, and says that he has searched in vain, near Albany and Troy, for a boulder or pebble of granite, or of

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* *Ibid.*, p. 336.

† Surface Geology of Ohio, 1874, p. 40.

any rock older than the Potsdam sandstone 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.*

In the southern part of the State of New York and in New Jersey they are not uncommon. At Marion, at the section of the till to which I have already referred, the top bed is a light-coloured sandy clay, similar to that at Trenton. Lying on and sometimes imbedded in this are large boulders, scattered over the surface. The sandy clay rests directly on the till, and is about 3 feet thick. Both here and at Trenton these great boulders were much larger than any I saw in the underlying till or drift. At Trenton they are often seen in the formation of new streets on the outskirts of the town. Some of them are 7 or 8 feet across, and most require blasting before they can be removed. I learnt from Prof. Smock that these blocks are distributed over much of the State, and he spoke of particular boulders occurring at a considerable altitude. I do not know, however, how high they occur, but probably this interesting question will be worked out by the Geological Survey of New Jersey, as well as the distances which they must have travelled from their parent rocks.

Nor does the Delaware form the southern limit of the far-transported boulders. They appear to bear the same relation to the drift-beds in Virginia, for Mr. Wallace, in his account of the discovery of stone-implements near Richmond, speaks of boulders in the surface-soil, and of large blocks (8 feet by 12) resting on the gravel.

It is obvious, as Prof. Hall and Dr. Newberry have pointed out, that these great blocks of stone must have been carried to their present position by floating ice. Any flood of water sufficient to move them would certainly wash away the sandy loam in and on which they rest, and such a mode of transport would not account for their position scattered here and there over the great undulating plain that extends from Trenton to the sea; nor could they have been left by the great ice-sheet, as they are found far beyond the limits to which it reached. Sometimes we hear the distribution of the upper glacial beds ascribed to a second Glacial period, when the ice again covered the land. But ice could not have moved thus for hundreds of miles over beds of gravel and sand without disarranging them, and nowhere in America has any sign been noticed of a second advance of

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* Geology of New York, Part IV., p. 319.

the northern ice. We have thus two distinct phases of the glacial era clearly marked in North-eastern America,—a Glacier period and an Iceberg period, just as we have in Europe. They are distinct in their range and distinct in their effects. In the first—the Glacier period—the ice, moving gradually southward, scored and polished the rocks over which it passed, and left behind it the unstratified till containing principally scratched fragments of local rocks. In the other—the Iceberg period—rocks were carried many miles beyond the limit that the glacier ice reached to, and were dropped on the top of loose unconsolidated clays and sands, which show no trace of any abrading or disturbing force. In Europe the ice from the Scandinavian mountains reached to the southern side of the Baltic, and for the whole distance the bed-rocks are glaciated; but beyond this the iceberg drift is scattered for hundreds of miles, and extends to the flanks of the mountain-chains that bound the German plain to the south; and that icebergs do not, as a rule, glaciate the beds over which they pass may be gathered from this,—that as soon as the boundary is left behind to which the land-ice undoubtedly reached no more glaciated rock-surfaces are seen; not even on the hills on which the icebergs must have grounded, as they have left there the greater part of the rocky burden they carried.

The agency of floating ice in the distribution of boulders was early recognised by geologists; but when, later on, Agassiz proved that land-ice had also played a most important part, it was not clearly perceived that both agencies were required to interpret the phenomena, and to this day the till—the product of the land-ice—is often confounded with the boulder clay, the product of the floating ice. In no other department of geology is far-travelled experience more necessary than in the study of the glacial beds. The knowledge to be acquired in a single province, or even in a single country, is not sufficient, for it will be well nigh impossible from that alone to separate what is particular and local from what is widespread and general. To limited experience I cannot help believing is due the obscurity to be observed in many of the memoirs dealing with glacial problems. One authority, who has perhaps lived amongst northern mountains, ascribes everything to the action of glaciers; another, whose home, maybe, has been on southern plains, sees nothing but the agency of water and floating ice.

In studying the glacial beds of North-eastern America we must seek to give their proper importance both to glaciers and icebergs, and to separate the phenomena into the two classes to which they belong. When we do this we find, as I have endeavoured to show, that the land-ice came down from the north to a certain well-defined line in New Jersey and Pennsylvania, and that after it melted back the country was submerged beneath a great expanse of water that covered the whole of the lower ground and reached far up the flanks of the hills, and that over this icebergs floated from the north, and dropped, as they melted, large stones brought from far distant ranges. This expanse of water was not limited to the area that the land-ice had covered, but extended far to the south of it into Virginia. After the land-ice retired, or whilst it was retiring, and before the country was submerged to such a depth as to permit the flotation of icebergs from the north, the upper pebble beds containing the stone implements were formed. Dr. Abbott has not only obtained his implements from beds that are clearly seen to have been spread out before the large blocks were scattered over the surface, but in one instance took one from the gravel below one of the large stones. From Mr. Wallace's description his discoveries appear to have been made in gravels of the same age.

West of the Appalachians the evidence all points to the same conclusion. We have in the Northern States, first, glaciated rock-surfaces and patches of till that witness the reign of land-ice; then we have on its retirement a land-surface, with remains of vegetation (peat and forest beds) and of extinct mammals. Along with the latter at some places, at the same horizon in others, have been found the bones and implements of man, as I have described at the commencement of this paper. The next stage is marked by widespread beds of gravel or rolled drift, that indicate the rising of the water. The gravel is covered with brown clay containing great far-transported boulders, witnessing the submergence of nearly the whole country beneath the flood. This brown clay covers the land everywhere in the States of Illinois, Iowa, Kansas, and Nebraska, and I have traced it myself up to the flanks of the Rocky Mountains. It marks as surely the culmination of the great flood as the beds that follow it, the loess, mark its subsidence; when the waters that had before covered the hills began to be confined to the valleys of the great rivers. From this time the mammoth, the megalonyx, the megatherium, the mylodon, the horse (until it was re-introduced from Europe), the gigantic beaver, and the lion were no more seen alive in North America, for their remains are not found in beds of later age. To the same horizon belong all the instances I have given of the earliest appearance of man in North America, and to it almost certainly must be ascribed the discoveries of Dr. Abbott and Mr. Wallace on the eastern sea-board. The extinct mammals and the earliest appearance of man in North America are therefore prediluvial, as I have urged is the case in Europe: indeed a most striking parallel may be drawn between the series of events that happened in the Glacial period in the eastern and the western continent.

We have first in Europe a great extension of land ice, that from Scandinavia reaching to the south of the Baltic, and that of the Alps to the Jura and down the valley of the Rhone as far as Lyons. We have then the retreat of the ice; and palæolithic man, the mammoth, and the rhinoceros occupying part at least of the area the ice had covered. Then we have a great outspread of gravels and clays, the latter in Northern Europe, with far-transported boulders, reaching up to 1700 feet above the present level of the sea.

I have endeavoured to explain this series of events by the theory that whilst the ice was accumulating on the mountains of Scandinavia and Central Europe, it was also being piled up at the northern end of the Atlantic, and in greater abundance there because of greater precipitation. When it there reached a sufficient height to intercept the moisture in the air-currents travelling northward it would advance down the bed of the Atlantic, partly by flowing as a glacier, but principally because the precipitation was on the southern slope and increased as the ice-ridge progressed southward. Whilst this ridge of ice was moving down the bed of the Atlantic, that from Scandinavia and the Central Alps had culminated, and began to shrink back, for the area of greatest precipitation was now on the Maritime Alps, the Pyrenees, the Cantabrian Range, and the mountains of Asturias, and the accumulation of ice there intercepted the moisture that had before supplied the glaciers of Northern and Central Europe. This I consider was the time of the principal distribution of the mammoth and the woolly rhinoceros, an earlier stage being marked by the presence of *Elephas antiquus* and *Rhinoceros etruscus*. The great accumulations of ice in the northern and southern hemisphere had before this abstracted so much water from the ocean that the level of the latter had been greatly lowered, the rivers cut deeper channels than they now occupy, the bed of the German Ocean was left dry, and many another tract that is now covered with a shallow sea then formed wide pasture-grounds for the great pachyderms and their associates. Palæolithic man likely lived on higher and drier land, and found a plentiful subsistence amongst the deer and the wild horses and oxen that appear to have abounded.

Probably this state of things was of long duration, but at last there came a catastrophe; possibly the greatest that has befallen the human race, yet of immense benefit in its ultimate results. The ridge of ice in the Atlantic had been slowly advancing, and through time it coalesced with that from the Cantabrian Range, whilst at the same time the gap between the Pyrenees and the Maritime Alps was also closed with ice. I suppose that the communication of the Black Sea with the Mediterranean had not then been effected, and that the ice of the Pacific blocked up the outlet of the waters to Behring's Straits. An immense basin was thus formed, the drainage of which to the sea was intercepted. The consequence would be that the low lands would be soon submerged. The pent-up waters ultimately reached a height of about 1700 feet above the sea, as evidenced by the great outspread of gravels at Munich, Bern, and Geneva; but whether this extreme height belonged to the first or second rise I do not yet know. To this great flood I ascribe the formation of the lower boulder clays and diluvium, and the destruction of the great mammals that were caught on the low plains and have left there their bones in great abundance. The first great European lake was apparently not of long duration, but was suddenly and tumultuously lowered by the breaking away of the ice-dam; probably, I now think, between the Pyrenees and the Maritime Alps. The rushing flood or debacle swept off from the flanks of the hills much of the detritus that covered them, and mingled all together in the great sheets of gravel that are now spread over much of the low country. Thus were formed, I think, the middle sands and gravels (including the Thames and other valley gravels), in which have been caught up or which cover the bones of the pre-diluvial mammals or the stone implements of pre-diluvial man. As no land surface has yet been detected between the middle sands and gravels and the upper boulder clay, it is probable that the break in the rim of the lake basin was soon filled with ice again, and the great lake re-formed. Over it floated icebergs from the north, carrying great boulders from the mountains of Scandinavia and scattering them over the German plain and as far as the flanks of the Carpathians. At this time was formed the upper boulder clay and diluvium of Europe. The second lake was gradually lowered by the cutting through of the Bosphorus or the Dardanelles, and the various stages of its subsidence are marked in all the great valleys of Northern and Central Europe. In England the upper boulder clay and the upper brick clays of the Thames and other river valleys were at this time deposited. Flint implements appear to have been found in clays of this age, but they do not indicate, I think, that palæolithic man existed, but that pre-diluvial man had left his nearly indestructible stone-work on the hill-sides, higher than the violence of the debacle reached to, and that shore-ice in the second-lake period sometimes carried these away and dropped them in the clay that was then forming. I noticed, in Dr. John Evans' noble collection of stone implements, with surprise, a fact with which he had been long familiar—the sharp, unworn edges of the implements from the brick clays, and also of a few that have been found on the surface at heights of over 300 feet above the sea. These implements have also a whitened bleached appearance, which may be due to long exposure on the surface before being imbedded in the brick clays.

It is now more than two years since I laid this theory before the Geological Society of London,* and no flaw has yet been pointed out in it, whilst in a series of papers published in this Journal I have shown that many other difficult problems in glacial geology besides those to the solution of which I first applied it find in it a simple explanation. I believe it is the only theory that explains the transport of northern boulders across the plains of Germany and Russia; and at the same time accounts for the absence of marine remains testifying that the sea had not occupied the area during the flotation of the blocks; and the absence of glaciated rock-surfaces showing that the Scandinavian land-ice had not extended so far. It is also the only theory of our day that deals with the difficult question of the origin of a great debacle of which De la Beche, Murchison, Sedgewick, and Prestwich have shown us there is so much evidence. The principal feature in the theory is that the advance of the ice of the Glacial period was mostly down the ocean depressions, partly because ice will gravitate towards the lowest levels, and especially because the precipitation of moisture is in our hemisphere much greater at the northern ends of the seas than in similar latitudes inland on the continents.

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* "Drift of Devon and Cornwall." Read November 3rd, 1875. Published in abstract only, Quart. Journ. Geol. Soc., February, 1876.

If the northern end of the Atlantic was so occupied with ice as this theory requires, the effects ought to be similar on the west coast of Europe and the east coast of America, which on this view form the left and right banks of the same great valley. I restrict my argument for the present, on the American side, to the country lying east of the Appalachians, but I hope at some future time to show that a similar explanation of the glacial phenomena west of that range is not improbable; this I cannot do now, as the preliminary steps of the discussion would occupy a greater length than the whole of this paper.

Owing to the influence of the Gulf Stream the ice occupying the bed of the Atlantic would probably extend much farther on the American side than on the European. Flowing down there—much influenced by the shape of the ocean bed, still more by the areas of greatest precipitation as affected by the advance of the ice itself, and not necessarily, nor even probably, thickest next the coast line and south of Cape Cod mostly distant from it—the ice, I think, reached so far at least as the 37th parallel of latitude. I suppose that the mass of ice had been increasing as it advanced southward, in consequence of the enormously greater precipitation not having yet been counterbalanced by the also increased waste from liquefaction, and that it flowed in upon the American coast somewhere south of Chesapeake Bay, and blocked up the eastern drainage as far as that point. Thus I think was produced the submergence of all the lower parts of the country. To what height the flood reached I have not information to guide me, but the water must have been deep to permit the tranquil deposition of the brown clays that cover much of the country, and the flotation of icebergs from the north, bearing the great rocks that were thus distributed over the land. I have found no evidence in North America of any great debacle, and the waters do not appear ever to have been suddenly and tumultuously discharged. In consequence, there has not been there the same mixing together of remains of different ages as occurred with us when the middle sands and gravels were spread out, and the relation of the beds containing the relics of prediluvial man and the pre-diluvial mammals to the other glacial deposits is more clearly defined. The more gradual and interrupted subsidence of the water is, however, marked by a series of terraces in the valleys. Excepting for this, the parallel between the series of events that occurred in the Glacial period, in Western Europe and North-eastern America, is complete. There is the same evidence of the advance of the land-ice, of the rivers running in deeper channels, of the formation of forest and peat beds in tracts now below the level of the sea, of the recession of the land-ice, of the interruption of the drainage of the country, and the formation of a continental ice-dammed lake over which floated icebergs. The same evidence, too, that palæolithic man and the extinct mammals were pre-diluvial, and were destroyed or driven out of the country by the rising of the great flood.

That American geologists will follow up the evidences of pre-dilvuial man in the western hemisphere we may be sure, and we may confidently expect that as great advances will be made by them in our knowledge of the relation he bore to the Glacial period as they are making in every other department of geology, and in fact in every branch of science.

It is a matter for congratulation that this question should be in the hands of such a skilled and enthusiastic archæologist as Dr. Abbott, and of such able and cautious geologists as Prof. Cook and Prof. Smock. I feel confident that we shall not have to wait long for confirmation of the position of the implements below the iceberg drift, and for more definite information than we now possess of the height above the sea to which the erratic blocks extend, and the distances they have travelled from the north or north-west. Nor need we despair of evidence soon being found that man was present in the country at the time of the greatest extension of the land-ice; the witness of which, so far, is the solitary scratched chipped pebble from the moraine at Butzville, the fabrication of which by man is doubted by some that have seen it.

I cannot conclude this brief view of the broad features of the glaciation of North-eastern America and the relation of palæolithic man to it, as seen from my standpoint, without again making an appeal for a more thorough examination of the records in our own country. It is susceptible of proof in East Anglia whether or not palæolithic man lived there in the Glacial period. Within a stone's throw at Hoxne lie all the glacial beds—the till, the lower boulder clay, the middle sands and gravels, and the upper boulder clay. There also are the gravels and clays in which Mr. Frere, nearly eighty years ago, found flint implements and bones of extinct mammals; and yet to this day we have not settled the relation that these bear to the glacial beds. Eighteen months ago, in the pages of this Journal, I gave my reasons for believing that the post-glacial age of these deposits, as assumed by most of our geologists up to that time, had not been proved; and I urged that we ought not to allow the matter to remain doubtful when it could be cleared up by sinking a few shafts in different parts of the ground. No action has been taken by our learned societies to whom I appealed, but now discoveries in other places have caused many to doubt the post-glacial age of some of the deposits containing implements, and they may be more inclined to listen to my appeal.

At Hoxne the expenditure of £200 would probably, and of £500 certainly, I think, show the relations of the deposits there to each other, and clear up the question of the glacial or post-glacial age of the beds containing the relics of palæolithic man and the great pachyderms. Large sums, and with results exceeding our anticipations, have been spent on the exploration of the cavern deposits, and we have ascertained definitely from them that man and the great extinct mammals lived at the same time. We should now take another step, and determine the exact position that the same fauna holds in the geological series; and this can be done at Hoxne. We send out scientific expeditions to the ends of the world, and rightly so I think, and yet here is one of the grandest problems that can interest mankind lying at our doors, and lying neglected. Granted that I may be mistaken, and that Prof. Prestwich—whose geological opinion is properly of much greater weight than mine—may be right; is it not worth while to set the question at rest, and not consume our time in fruitless discussions and barren congresses? My glacial theory is the outcome of many years of study of the phenomena with which it deals, and I know that it has been fashioned with sincerity; but it is not so dear to me that I should hesitate to put my own shoulder to topple over the edifice I have reared if I could find reason to believe that it was not founded on truth. If the explorations that I urge, ought to be undertaken at Hoxne, be carried out, and prove that the implement-bearing beds are post-glacial, I shall at least have the satisfaction of thinking that not only has my own geological vision been cleared, but that Mr. Prestwich—whose writings for more than twenty years have been my study and delight—has been proved to be right. But trivial and paltry are these personal considerations compared with the issues that are undetermined, and which it is our duty and privilege to clear up, when we have at Hoxne such an opportunity of doing so as is not known to exist anywhere else in Europe.

V. A NEW THEORY OF TRANCE.

THE subject of Trance has for centuries excited great wonder and curiosity, and although the essays and works on its nature and causes may be numbered by hundreds there are very few writers who have treated the subject fully and scientifically. It was therefore with great satisfaction that we received from Dr. G. M. Beard a paper on the subject, which he recently read before the New York Medico-Legal Society.* Our readers have already had an opportunity of judging of the care and originality with which Dr. Beard works out his ideas, from his article on "The Longevity of Brain Workers," which appeared in the "Quarterly Journal of Science" for October, 1875; and in this paper on Trance there is the same evidence of deep thought and mature consideration.

With regard to the value of human testimony our own views do not, in some points, coincide with those expressed by Dr. Beard; but from the exhaustive way in which the whole subject is treated we believe we are consulting the interests of our readers in placing before them an explanation of his new theory, and of giving in a somewhat abbreviated form the principal arguments which he adduces in its favour.

Under the word "Trance" Dr. Beard includes the real phenomena represented or suggested by terms which are more or less meaningless and incorrect—such as somnambulism, artificial and spontaneous mesmerism, animal magnetism, hypnotism, Braidism, catalepsy, ecstacy, biology, &c. He very rightly insists on the absolute necessity of having recourse to deductive as well as inductive reasoning when grappling with such subjects as trance, reasoning from generals to particulars, and drawing conclusions from principles already established, as well as from particulars to generals; and in opposition to the oft-repeated assertion that it is the wiser course for scientific men to let these subjects alone, and not to attempt their solution, he remarks—what indeed we have ourselves often urged—that such sciences as Astronomy, Physics, Physiology, and Chemistry were in their infancy opposed on substantially the same ground. These things, it was claimed, could not

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* "A New Theory of Trance, and its Bearings on Human Testimony." By George M. Beard, A.M., M.D.

be understood by the human mind; therefore we were counselled to join ourselves to the idols of our ignorance, and let them alone. But how far, we would ask, should we have advanced in civilisation had such advice been followed? We believe that by adopting true scientific method in such investigations as the one now under consideration the mysteries of Trance and the phenomena ascribed to Spiritualism will ultimately be unravelled. Let us see how far Dr. Beard's new theory contributes to this much-to-be-desired end.

The nature of trance is, he contends, a functional disease of the nervous system, in which the cerebral activity is concentrated in some limited region of the brain, with suspension of the activity of the rest of the brain, and consequent loss of volition. It is the prime requisite of a scientific hypothesis that it should account for all the phenomena embraced under the department to which it applies. The hypothesis that trance is a morbid state, consisting in a concentration of the cerebral force in some limited region of the brain, the activity of other portions being meanwhile suspended, seems to account for all the real phenomena of this state, all its different forms and stages.

Trance, like other functional nervous diseases, may be induced either physically or psychically.

Among the physical causes are injuries of the brain, the exhaustion of protracted disease or of starvation, or of over exertion, anæsthetics, alcohol and many drugs, and certain cerebral diseases. Ordinary sleep may act as an exciting cause, as is illustrated in the somnambulistic form of trance. Under the psychical causes are included all conceivable influences whatsoever, that may powerfully excite any emotion or group of emotions.

For the sake of convenience of description Dr. Beard divides trance into four varieties—the *spontaneous*, the *self-induced*, the *emotional*, and the *intellectual* trance. In strictness these varieties may to a certain extent include each other, and in using these terms this fact should be borne in mind. Thus, the intellectual trance is spontaneous, although the majority of the cases of spontaneous trance are not also intellectual. The self-induced trance may be partly emotional, but it is not entirely so.

A typical form of spontaneous trance is natural somnambulism, or sleep-walking—a term which is vaguely used by many writers to include all phases of trance, excepting those which are produced by performances of mesmerisers, which are called cases of artificial somnambulism. In sleep-walking the cerebral activity, which during ordinary sleep is more or less lowered throughout the brain, is suddenly concentrated in some limited region; the cerebral equilibrium being spontaneously disturbed through the subjective action of dreams, the subject, under the dominion of this restricted region of the brain (the activity of the rest of the brain being suspended), runs or walks about like an automaton, with exaltation of the sense of touch often, and of the co-ordinating power, as is shown in their capacity for balancing in difficult and dangerous positions, and climbing on heights where in the normal state he would not venture. Other senses may be sealed entirely, as in other forms of trance.

Under self-induced trance are comprised those cases where the subject can bring himself into this state at will, either suddenly or gradually. Of such subjects it may be said that they will to lose their wills; or it would be nearer the truth to say that they voluntarily put themselves under influences where the involuntary life becomes supreme.

All genuine trance preachers and speakers—and many of them are genuine—represent the self-induced variety. Dr. Beard states that he has studied the case of a famous trance preacher, who told him that when he began to go into this state the first symptom was only a thrill, or electric shock through his arm; then with more practice the whole arm became convulsed, then the whole body, until in time exaltation of the faculties of imagination and of language were developed, and he became a most successful performer before audiences.

In speaking of the terms often applied in spiritualistic circles to mediums as being "fully developed," or "partially developed," or as "developing," Dr. Beard concedes that there is a basis of truth in such expressions, inasmuch as it oftentimes needs practice to acquire the habit of readily, and at will, entering the trance. Further, that some have a habit of falling into trance spontaneously at regular intervals, the same periodicity oftentimes occurring in this disease that has long been observed in neuralgia.

Under emotional trance are included cases that are caused by the so-called mesmeric performances, or through the feelings of fear, wonder, reverence, and expectation, however excited. The majority of the cases of trance come under this head; for every one is endowed with these emotions, and in the greater part of the human race they are the controlling elements in character, and in the strongest and most intellectual minds they are apt to be in constant and usually successful rebellion against the authority of reason: any influences, therefore, that excite any or all of these emotions may be regarded as exciting causes of trance.

The almost universally held belief that the mesmeric form of emotional trance is caused by some force or fluid (animal magnetism) passing from the body of the operator into the body of the subject, is, according to Dr. Beard, as far from the truth as any view on any subject possibly can be; it makes no difference what is done to produce mesmeric trance; it makes no difference who does it; it is a *subjective* matter entirely, and all depends upon the emotions of the subject—what he fears, expects, or wonders at.

To intellectual trance belong the extreme cases of what are commonly characterised as absent-mindedness—a state which is quite distinct from simple mental attention. The popular term absent-minded, as applied to those who become so absorbed in thought that they are unconscious of what is going on around them, and perhaps respond automatically to external suggestions or influences, is, in view of the theory of trance advocated by Dr. Beard, a happy one, since it expresses with partial correctness the real state of the brain during an attack of that kind. A large portion of the brain is active, and, until aroused, is insensible to surroundings, and thus responds mechanically. The biographies of illustrious thinkers are filled with instances, some of which are probably correct, of this form of trance. Thus Walter Bagehot says of Adam Smith, the great political economist, that his absence of mind was amazing. On one occasion, having to sign his name to an official document, he produced not his own signature, but an elaborate imitation of the signature of the person who signed before him: on another occasion, a sentinel on duty having saluted him in military fashion, he astounded and offended the man by acknowledging it with a copy of the same gestures.

In these intellectual trances, great thoughts have been, without doubt, evolved, that would have been impossible to the brain in its normal state.

Dr. Beard then proceeds to show how by his hypothesis all the facts of all forms and phases of trance are explained, unified, and made harmonious; it is only by this hypothesis, he affirms, that it is possible to give any unity or solidarity to the phenomena of this state.

He argues, First,—That this hypothesis accounts for the loss of the control of the will and the automatism of trance, which is the first observed and most distinguishing feature. That, in his normal state, man is, to say the least, nine-tenths a machine. The involuntary life—that which acts without the will, or in spite of the will—is the chief fact in human life and in human history. Comparing life to a wheel, as Dr. S. S. Laws has also done, the voluntary functions may represent the narrow hub, while the involuntary functions are represented by all the area between the hub and the periphery. In this little inner circle lies all human responsibility and all the vast influence of punishment or reward; in all the rest of his functions man is as much an automaton as a tree or a flower. Now, in the trance, this little inner circle of what is called volition is encroached upon by the involuntary life, and in the deeper stages is entirely displaced by it. The fully entranced person has no will; what he wishes to do he cannot do; what he wishes not to do he does; he is at the mercy of any external or internal suggestions.

This hypothesis of the concentration of the cerebral activity in a limited region accounts for the displacement of the will in this way:—The will may be defined as the co-ordinated activity of all the faculties of the mind, including, in general terms, the perception, the emotions, and the intellect. Cerebro-physiologists will agree—all questions of phrenology, or cranioscopy, or minute specialisations of functions aside —that the brain does not act as a unit, but that different parts are the organs of different faculties. When the cerebral activity is harmoniously diffused, as in the normal state, through all the different regions, the man is said to be under the control of the will. When the cerebral activity is concentrated in some limited region of the brain—say that devoted to the emotions, or that devoted to the intellect, the activity of the rest of the brain being suspended for the time—the man would have no will; he would be under the control of that group of faculties; he would be a conscious living automaton, as a fully-entranced person always is.

Secondly,—That his hypothesis proves why trance is an abnormal state. It shows that it must be a morbid pathological condition, and also shows in what this morbidness consists. The man whose mental faculties are mostly suspended, who has no will, but is under the control of some single faculty, is surely in an abnormal state, and in this respect the popular idea is correct. It is a functional disturbance relating only to circulation and innervation, and not causing structural changes, although it may be caused by structural cerebral diseases, and not ordinarily permanently affecting the health in other respects. The liability to trance, like the liability to various other functional disturbances of the nervous system, does not conflict with general good health and longevity.

Thirdly,—That this hypothesis explains the difference between trance and ordinary sleep, which in some respects it so much resembles. Sleep is a normal state, a partial cessation of the activity of all the faculties, a lowering of the activity in all the regions, but not a suspension of the activity of any except the will, which, as we have seen, is simply a co-ordinated action of the faculties. When a person who is sleeping gets up and walks in his sleep—in other words, passes into the somnambulistic form of trance—the change that takes place in the brain is this; while sleeping the activity of all the faculties was lowered; on going into trance the activity of all the faculties becomes suspended, and the entire cerebral activity is concentrated in some one faculty or limited group of faculties.

Fourthly,—That this hypothesis explains the phenomenon of dual life and double consciousness, which has been regarded as one of the greatest and most inexplicable mysteries of trance.

In trance—even in the most profound instances ever observed—there is probably always consciousness at the time, but it is not always or usually remembered consciousness. On awaking, as on awaking from ordinary sleep, the dreams, that may have been active and numberless, fade as a cloud; possibly not even a glimpse of them may be caught and held before the mind long enough to become a permanent and recollectible impression during the normal state. But on resuming the trance state the exalted functional activity of the region of the brain in which the cerebral force is concentrated is able to bring these impressions of the previous attack of trance, forgotten during the intervening normal state, to consciousness, and thus the subject carries on an independent trance life, just as though there had been no intervening normal state. On returning to the normal state, the cerebral force being again diffused through the whole brain, is insufficient to enable the subject to recal the experience of the trance, but quite sufficient to enable him to recal the experiences of his previous normal state. Thus he leads two lives, the normal life and the trance life, and they are independent of each other.

In cases of dual life the trance life is the more brilliant and active in certain features, as by this hypothesis it naturally would be. In a case under the observation of the late Dr. J. R. Mitchell, a young girl in trance life was quick, energetic, and witty, and vivacious; in her normal life she was slow, indolent, and querulous.

Fifthly,—That the hypothesis explains the difference between the deeper stages of trance, and death, with which trance is sometimes confounded.

With this hypothesis of the pathology of trance before my mind, I have, continues Dr. Beard, been accustomed to illustrate the difference between ordinary sleep trance and death, by pointing to a chandelier of gas-burners. When all the burners of the chandelier are fully lighted, that is the normal waking state; when all the burners are turned down low, but not turned out entirely, as usually is the case in public halls before the opening of entertainments, that is ordinary sleep; if I turn out entirely all the burners except one, and that one, as often happens, flames all the more brightly from increased pressure, that is trance; if all the burners are turned out entirely and permanently, that is death. The only hold on life which the deeply entranced person has is through the activity of a limited region of the brain, through which feeble movements of the heart are sustained, the body being in other respects motionless. The popular belief that deeply entranced persons are liable to be buried alive is correct, but fortunately mistakes of this kind occur but rarely.

Sixthly,—That this hypothesis explains the exaltation of some of the physical and mental faculties in trance, and depression of others.

The exaltation of the physical and psychical faculties in trance cannot be questioned, but is readily demonstrated, and by this hypothesis receives an explanation that is both lucid and complete.

Representing, for the sake of comparison, the quantity of cerebral force in all parts of the brain by one hundred—if the activity of three-fourths of the brain is suspended, then the remaining one-fourth may be fourfold more active than when in the normal state. That there should be such a concentration of cerebral force in a limited range of faculties is in harmony with every day observed facts. Thus the intellect increases in vigour in any direction under exercise up to a certain point, and through over exercise becomes fatigued. In the brain are the centres of thought, of muscular motion, and of general and special sense. It would follow, therefore, that some one or several of the senses, or some one or several of the mental faculties, or some one or several groups of muscles, might be exalted in activity, with entire suspension of the activity of other senses, faculties, and muscles, according to the region of the brain in which the concentration of activity takes place. There is therefore no mystery in the frequently observed, though sometimes disputed, fact that entranced subjects can raise with ease weights which in their normal state they are unable to move. Mesmerised subjects sometimes exhibit this power. Persons entranced through fear, as by an alarm of fire, have been known to take up a stove and carry it out of the house; the next day they cannot, to save their lives, carry back that stove. The co-ordinating or balancing power may be so much exalted in somnambulists that they can climb without harm in most dangerous places. The exaltation of the time-telling power—which sometimes passes for second sight—has the same explanation as the exaltation of other faculties. Likewise the nerves of general and special sense are all liable to be greatly exalted in this state; the feeblest whisper in a distant room may be readily heard, and one can read by a dimmer light than is usually needed. The sense of touch may be so delicate that, when the sense of sight is sealed, the subject can find his way from room to room without injury; and it is claimed may, in some cases, recognise the presence of another person near at hand, by the temperature alone, even where there is no physical contact.

We are told these exaltations of the normal senses are the bases of many of the popular and professional delusions relating to "second sight," "clairvoyance," "thought reading," and the like. By this hypothesis, also, any of the mental faculties should be liable to be exalted. Observation shows that not only the imagination, but the reasoning faculty and command of language are oftentimes greatly enlivened in their activity in this state, as the performances of trance preachers illustrate. Weak-minded men and women, who in the normal state think little and say less, are sometimes able, when entranced, to speak continuously, and almost if not quite eloquently, and with slight apparent effort. While there has been much exaggeration of the originality and value of these trance speeches, yet it cannot be denied that they are—with all their wildness of fancy and repetition, and frequent senselessness—far beyond the capacity of the same persons when not entranced. On returning to the normal state they may be utterly stupid or commonplace; their cerebral force, when diffused through the whole brain, is unequal to even rapid and sustained small talk.

The converse of exaltation, depression of some of the senses and faculties of the mind, directly follows from this hypothesis of concentration of cerebral activity; those senses and faculties that belong to the entirely inactive regions of the brain must be for the time practically dead, as is found to be the case in some forms of trance. Thus the sealing of some of the special senses, and general anæsthesia, making it possible to perform without causing pain certain surgical operations, are accounted for.

Seventhly,—That this hypothesis explains all the familiar physical symptoms of trance, such as flushing of the face, fixity of position, sighing respiration, accelerated pulse, involuntary convulsive movements, and marvellous and numberless hysteroid sensations.

The effects of trance on the pulse and respiration, and on the circulation in general, are, according to Dr. Beard, what would be expected from the known inter-dependence of mind and body. The quite recently established fact of the existence of definite centres of muscular motion in the brain, however the fact may be interpreted, is of great significance in its bearings on this subject, since it shows clearly why convulsions so frequently accompany trance. The aphorism that when we think we move was based on our knowledge of the existence of these centres of muscular motion in that portion of the surface of the brain that is regarded as the seat of some of the mental faculties, and was first suggested to him while repeating the experiments of Hitzig and Ferrier in the electrical irritation of the brains of animals.

Eighthly,—That this hypothesis accounts for the illusions and hallucinations of trance.

Illusions, delusions, and hallucinations are, as is established, the products of cerebral activity, and are frequently the symptoms of some abnormal state of the brain. There is no proof that any other part of the body than the brain, as the spinal cord or nerves, can originate hallucinations, any more than there is proof that any other part of the body can originate the higher modes of conscious thought—all the facts and arguments that serve to establish that the brain is the organ of the mind in health, also establish that it is the organ of the mind in disease. For while automatic acts, as nursing and so forth, may be manifested by brainless infants, and while the spinal cord clearly contains centres of reflex action, yet there is no proof that any conscious thought, of the *higher* kind at least, attends the activity of these reflex centres, any more than in the familiar automatic movements of plants. The hallucinations of trance—the visions of heaven and other innumerable fancies—must then, like dreams and all mental operations, whether coherent or incoherent, have their seat in some part of the brain; and, according to this hypothesis, their existence, their coherency, and their extreme activity, are all explained.

Ninthly,—That this hypothesis accounts for the relation of trance to its admitted predisposing and exciting causes.

By this hypothesis any influence that tends to overthrow the cerebral equilibrium, to disturb the balance of innervation, would be likely to be a cause of trance: experience shows that this is actually the case.

The predisposing, like the exciting, causes of trance are both physical and psychical.

One is physically predisposed to trance, says our author, who inherits or has acquired a nervous system generally sensitive and impressible. One is psychically predisposed to trance who is mentally unbalanced through excessive and disproportionate endowment of imagination and emotion. One who is powerfully developed in reasoning and thinking qualities, and is badly deficient in observing, practical faculties is so far forth predisposed to the intellectual form of trance. The best subjects are those who are predisposed both physically and psychically, who have sensitive organisations, and unbalanced, ill-trained minds.

Trance is not, however, as many suppose, the peculiar gift of certain temperaments. It is the property of the human race. All persons are liable to become entranced, just as they are liable to become paralysed or epileptic, although all do not suffer in this way. All persons are not predisposed to the same form of this disorder; one can only be entranced through the intellect, another through the emotions; one person can only be frightened into this state; one needs the presence of a medium, another of a mind reader, another of a clairvoyant, and another of a mesmeriser; another of a magnetised letter, and another still of one who performs miracles of healing by the laying on of hands. Mr. Grimes, who has had much experience with the mesmeric trance, and who is accustomed to direct his subjects to stand still with closed eyes and folded hands, as a means of exciting the emotion of reverence, says that he failed with every one out of forty military officers at West Point, while just across the river, among the operatives, the same process was very successful.* This is easy

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* Mr. Grimes has recently published a work entitled "Mysteries of the Head and Heart," the latter portion of which especially is commended to the attention of those who are interested in these themes. Mr. Grimes is, says Dr. Beard, almost the only writer on trance who has had sufficient originality and

to understand from what has already been stated in regard to the predisposing causes, but it would be an error to infer that those officers were not capable of being entranced. If they should all sit in a circle around a table for half an hour or more, with the expectation that some strange things would develop, very likely some of them would become carried away, and, by unconscious muscular motion, would move the table; or perhaps they would feel sensations like electrical shocks through their bodies, or they might go into convulsions, or might experience wonderful visions, hearing the voices and seeing the faces of loved ones.

Tenthly,—That this hypothesis accounts for the periodicity of trance in certain cases.

It is the nature of all functional nervous diseases—neuralgia, sick headache, hay fever, inebriety, and some forms of insanity—to appear more or less periodically. It may be said that the majority of cases of spontaneous trance are periodic.

It may be opposed to all this process of reasoning that no one has ever seen with his eyes the brain thus concentrating its force during an attack of trance; but it must be remembered that only exceptionally can scientific hypotheses be verified by actual sight. Even in the material world the seen is but a fraction of the unseen. No man ever saw the waves of light; no man has ever seen gravity: these universal forces are studied only through their phenomena, by means of which we frame hypotheses of the law of gravitation and the existence of a luminiferous ether. In the realm of physiology and pathology the chances of verification by actual sense perception are more rare than in astronomy or physics.

We have now laid before our readers what we hope is a clear and concise account of Dr. Beard's theory of trance, and of the principal arguments which he adduces in its favour. In his paper he enters at very considerable length into the necessity, in such investigations as of spiritualism, of reasoning by the deductive method, &c. He moreover insists that it is only those who have made themselves masters of, and authorities in, cerebro-physiology and pathology, and who have thereby learned that the subject lies within the

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mental force to see more than one side of it. But, like Dr. Carpenter and most other writers on these subjects, he fails to see all sides, and he makes the mistake which is fatal to the scientific study of trance, of conceding the possibility of thought reading. This serious mistake results from the studying the subject inductively instead of deductively; it is indeed, as I shall show farther on, an inevitable mistake from that false method of reasoning.

grasp of the properly trained and properly furnished intellect, who are competent to unsolve such problems and to explain the mysteries in full detail.

Up to a certain point, we agree with Dr. Beard, but we maintain that the phenomena he discusses embrace two distinct branches of science, viz., Physiology and Physics, and that it is strictly within the province of the trained physicist, and outside the province of the physiologist, to apply such tests as the electrical and other tests which we ourselves have used when investigating the physical branch of the subject. There is abundant proof that the fact of being a specially trained physiologist may actually disqualify a man for investigating spiritualism. For instance, Dr. Carpenter is a physiologist, and it is chiefly to his researches in physiology and pathology that he owes his present position. He ought, therefore, to be an expert, and to be able to get at the truth by exactness. He certainly considers himself fully competent to conduct such an investigation, for here is his own estimate of his qualifications. He says*—"The training I originally received, and the theoretical and experimental studies of forty years, have given me what I honestly believe (whether rightly or wrongly) to be a rather unusual power of dealing with this subject. Since the appearance of my Lectures I have received a large number of public assurances that they are doing good service in preventing the spread of a noxious *mental* epidemic in this country; and I have been privately informed of several instances in which persons who had been 'bitten' by this malady have owed their recovery to my treatment."

Now, Dr. Carpenter has been investigating the subject of mesmerism, spiritualism, &c., for more than thirty years, and what has he done towards solving the problem? Absolutely nothing! The reason is, we believe, that his mind lacks that acute philosophic quality which would fit him to unravel the intricate problems which lie hid in the structure of the human brain. He has, moreover, shown himself unable to grapple with the reasons or to deal with the facts of opponents; he views their work through a special "focus of his own," and is incapable of projecting himself into the mind of another person. We wish to do justice to Dr. Carpenter's ability and usefulness. It is, indeed, whispered that he has rather talked than worked himself into position and reputation, and like the clever man in the market place, whose cries of "I'm a

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* Nature, vol. xvii., p. 26.

genius! I'm a genius!" drew a crowd round him, so Dr. Carpenter, by reiterated assurance that he alone as a physiologist possesses, in a supreme degree, a "trained and organised common sense," has led the public to yield to his views a certain degree of assent. But we would not for one moment underrate Dr. Carpenter's abilities: he has undoubtedly done a considerable amount of meritorious work; and if he does not hold any high rank as an original investigator, he distinctly occupies a noteworthy scientific position as an expositor and popular lecturer. He is, in fact, the indispensable middle-man between the original investigator and the public. He has compiled some useful books on physiology and on the microscope, and were he not in the habit of viewing unwelcome facts pseudoscopically, and describing them catachrestically, he might be said to hold, with regard to this generation, the same position that Dr. Lardner held in the last, though Dr. Carpenter lacks the bright, clear, unfatiguing style of his prototype.

But in his investigation of the phenomena ascribed to spiritualism, Dr. Carpenter has stepped out of his proper sphere. Physiologists are not authorities on the physical side of the question, and it is a pity that when dealing with the subject from a physiological point of view they invariably either ignore the physical tests altogether or they misrepresent them, for the purpose, apparently, of securing a little cheap applause by showing that the experimentalist is a dishonest or untrustworthy observer.

NOTICES OF BOOKS.

The Border Lands of Geology and History: an Inaugural Address. By T. W. KINGSMILL. Delivered at Shanghai on the 20th of February, 1877. Shanghai: Kelly and Walsh. London: Trübner and Co.

WHILST the facts generally summed up under the name of "glaciation" are admitted by every one fairly competent to form an opinion, there is no such happy accord concerning the cause assigned to these phenomena—the so-called "Glacial epoch." What were its causes? Did it occur once only, or repeatedly? Was the whole globe simultaneously attacked, or were the northern and southern hemispheres glaciated alternately? Was there an "ice-cap" extending regularly and uninterruptedly down from the poles to comparatively low latitudes, and obliterating every natural feature? Or did the glaciers, as in our day, though on an intensified scale, take an independent origin in the mountain chains, and move sometimes not from, but towards, the poles? On all these main questions, and on many collateral points, geologists of the highest merit disagree, and a controversy has been for some years going on. This discussion may be pronounced most fruitful, since it necessitates a more minute and searching appeal to facts than would otherwise have been undertaken. Some of the results of this appeal are very remarkable. The conclusion that in Europe and North America glaciation has prevailed is not in the least shaken, but rather confirmed. The evidence on which the late Agassiz inferred that even equatorial Brazil once suffered from this invasion of ice stands, indeed, in need of corroboration. But the proofs of glaciation found by Mr. Belt in Central America, and even at comparatively low altitudes above the sea-level, leave exceedingly little room for doubt. The question, however, naturally suggests itself—What about Asia? If the northern hemisphere has at any time been glaciated as a whole—whether simultaneously with the southern hemisphere or independently is a matter of no importance—North China, Mongolia, and Mantchuria ought to show the usual traces of the visitation. "Yet in no part of Eastern Asia do we hear of phenomena indicating a Glacial period." Mr. Belt, than whom no more competent observer could be named, has examined the regions between Ekaterinburg and the head-waters of the Irtish, lat. 51° to 55° N. and long. 60° to 76° E., "a situation which, according to usually-received glacial theories, should have been extensively glaciated." But from his description the author sees no proof that such was ever the case. "The sands and loams of the steppes, though covering extensive areas, in no place seem to exhibit the ordinary marks of glacial action. In the north they are composed of fine sediment, without a trace of pebbles; going south the pebbles increase in size and numbers; and along the southern boundary of the deposit abutting on the mountain chains they contain angular blocks of stone, growing larger as the foot of the ranges is approached. The rocks lying immediately south of these beds, according to Mr. Bates, show also no signs of glacial action.

To account for this absence of glaciation in North-Eastern Asia the author suggests a secular shifting of the axis of the earth's rotation, by which, of course, different regions would in turn become circumpolar, and experience the conditions now prevailing in Spitzbergen, New Siberia, North Greenland, &c.

"It has, however," he declares, "in my judgment not been proved that these glacial phenomena are contemporaneous, and herein lies what I conceive to be the fallacy of the reasoning of the glacialists. Perceiving in many portions of the earth's surface traces of a state of things only to be attributed to an access of intense cold, they have fallen into the error of classing them together, and thereby evoking the so-called Glacial epoch. Glacial epochs, I believe, have been numerous: nay, I will go further, and say that since the beginning of geologic time some portion of the earth's surface has ever been undergoing its own glacial stage. Sometimes a greater access of cold occurred than at others, and this period of greatest cold had doubtless a tendency to oscillate from one hemisphere to another, but I think that the evidence afforded by the geology of Northern Asia is too strong to permit of the belief in any universal Glacial epoch being acceptable.

"Traces of a Glacial epoch have been clearly proved to exist so far back as the time of the deposit of the Permian rocks. The valley of the Godavery, in India, seems then to have been within the glaciated area. Nearly, if not quite, contemporaneous with this are glacial deposits in Natal. But is this a proof of a general Glacial epoch? I trow not. The observed facts are very similar to what I have pointed out as occurring in Europe and Northern Asia in late tertiary times. The former was glaciated; the latter enjoyed a milder climate than at present. So while India and Southern Africa were having their Glacial epoch a diligent search amongst contemporaneous rocks in Australia fails to lend any countenance to the view that it extended to those regions."

This hypothesis, whether it be ultimately found to accord with facts or not, may seem at first sight to have little connection with the title which the author has selected. But if various portions of the earth's surface are being successively glaciated owing to a secular displacement of the poles, we may expect to find in some regions—those, to wit, from which the pole is receding—a gradual improvement of climate, whilst in others, to which the pole is approaching, there will be a correspondingly progressive deterioration. Upon such changes not merely geology, but myth, tradition, and finally authentic history, may contribute to throw important lights. From these various sources the author concludes that the climate of Northern and Eastern Asia is becoming colder, a view in which Mr. Geikie seems to concur. The proofs of this increasing refrigeration are to be found "in the comparison between its present and its recently-extinct fauna, as well as in the geographical distribution of the animals that remain." But ethnology contributes evidence of a similar tendency, which will be perhaps more widely understood. Central, Northern, and North-Eastern Asia are now but thinly peopled, and are unanimously pronounced by travellers as very ill adapted for the support of a dense population; yet history and tradition represent these now lone and silent deserts as being the *officina gentium*, a very magazine of nations, from which horde after horde issued to ravage the fertile lands of Southern and Western Asia and of Europe. Historians have recorded these successive migrations, which, commencing with the attack of the Hiung-nu upon China, some six or seven centuries before our era, culminated in the overthrow of the Roman Empire, and lasted down to comparatively modern times. But the cause which set these tribes in motion from their original seats has been overlooked. According to our author it was the commencement of that process of refrigeration and desiccation which has evidently prevailed for a long time in Central Asia. He contends that at a still earlier period a vast mediterranean stretched from the Black Sea eastwards towards the Pacific, cutting off the southern parts of Asia from the North. Of this sea the Caspian and Lakes Aral and Baikal are now the principal remains. By a gradual upheaval this sea was drained, except the portions just mentioned. Its former bottom is characterised by the "loess" formation, more largely developed in East Asia than in any other part of the world, and which strongly resembles the ooze dredged up by the Staff of the *Challenger* Expedition from depths of about 2000 fathoms. To counterbalance this great upheaval of so extensive a portion of the earth's crust, the author considers that the continent of "Lemuria," formerly extending across the Indian Ocean, was simultaneously depressed. A remarkable argument for the former existence of a Central Asian Sea is founded on the presence of seals in the Caspian, in Lake Aral (till very recently), and in Lake Baikal. The latter case is the most important, since in no other part of the world do seals inhabit fresh water. Now if we suppose that these lakes were formerly parts of an inland sea, it is very conceivable that seals might be prevented by the upheaval of the land from making their escape to the ocean, and might in successive generations become habituated to a gradually freshening water.

It is obvious that Mr. Kingsmill's views can only be thoroughly criticised in the deserts and the mountains of Eastern and Central Asia, hammer in hand, and there are many points which must be more closely examined before his hypothesis can be either accepted or rejected. From the point of view of the physicist there is, we believe, no preliminary objection to the idea of a secular shifting of the earth's axis of rotation. Sir W. Thomson considers it as possible that the poles may have, at one or other time, have occupied positions differing from their present locality by as much as 40°. Such a variation might place London alternately on the Pole and within about 10° of the Equator—a climatic range fully sufficient to account for the different faunas and floras of bye-gone geological epochs. But is there at present any indication that the axis of the earth is shifting its position? If the North Pole is receding from us and approaching the eastern parts of Siberia, our longest day ought to be growing gradually shorter and our shortest day longer, and the maximum apparent altitude of the sun in the heavens ought to be increasing. But we are not aware that even any suspicion of such changes has been aroused. If, therefore, a secular displacement of the earth's axis of revolution is in progress, it must be exceedingly slow—too slow, we think, to have effected any important climatic changes in Central and Eastern Asia within the limits of historical time.

Again, for the deterioration which those regions have admittedly undergone, the author proposes two causes apparently not standing in any necessary connection—viz., the displacement of the earth's axis and the elevation of the land in the centre of Asia. Is it not possible that the latter cause may alone suffice to account for the effects produced? A gradual drying up of rivers and a paucity of rainfall have also been traced in many parts of Central and Southern Europe and of Northern Africa, but there has been no distinct amelioration of climate beyond what is due to improved drainage and other local causes. Yet on Mr. Kingsmill's hypothesis Western Europe and Eastern North America ought to be enjoying a progressive elevation of their average temperature.

If the earth's axis is undergoing secular displacement this change will doubtless follow some law, and the poles would infallibly describe a regular curve. This curve it will be important to trace out as nearly as possible by observing in what order of time different parts of the world have undergone glaciation. Now it is quite possible, supposing Mr. Kingsmill's hypothesis to be correct, that Eastern Asia should exhibit no marks of a glaciation synchronous with that of Europe and North America. But the theory seems to us to require imperatively that Asia should exhibit marks of an earlier glaciation and to detect these is an essential step towards the verification of Mr. Kingsmill's supposition. Another important test will be a re-examination of the leading facts of animal and vegetable geography in the light of this new hypothesis. If it harmonises with the distribution of organic species its probability will be greatly augmented.

It would be unpardonable to conclude this hasty survey of a brief but most important treatise without putting on record our appreciation of its character. The author dissents, indeed, from certain geological theories widely—if not universally—received; but he thinks and writes as a man of Science, and he may therefore justly demand for his views a full and a candid examination. The advice which he gives, in concluding, to the members of the "North China Branch of the Royal Asiatic Society," is most excellent. "In the investigation of the loess," he says, "a wide field is open for research, and one in which many members of this Society can render a service. We are entirely ignorant of its microscopic composition, yet without a knowledge of this we can do no more than speculate as to the conditions under which it was deposited. What Dr. O. von Mollendorff has done for Chihli (Petcheler) is wanted for all the other provinces of China —a reliable catalogue of the animals resident within their limits. Of the botany of North China we are supremely ignorant, and we have still a very imperfect knowledge of its fishes. The interesting fossil flora of the Kinsin coal-field is still untouched, yet it is of special importance in its connection with the Arctic flora of similar age."

Proceedings of the Literary and Philosophical Society of Liverpool during the Sixty-fifth Session, 1875-76. No. 30. London: Longmans and Co. Liverpool: D. Marples and Co., Lim.

This volume possesses the character common to the Proceedings of most of our provincial learned societies: its contents are literary rather than scientific, and the papers of the latter class are devoted rather to popular exposition than to the results of research. This, of course, is to be expected: we reap just what we sow. As long as our education is based upon literature, as long as the art of expression is prized more highly than the power of origination, so long we shall abound in novels, sermons, speeches delivered in Parliament and elsewhere, historical, antiquarian, and critical dissertations, and be scanty in scientific work, in discoveries, and inventions. Whether we are acting wisely or foolishly it is now not the time to inquire.

The first paper in the volume before us is the Presidential Address on "The Tendencies and the Future of Modern

Civilisation." That it contains many interesting and suggestive passages cannot be denied; but it enters too largely into political and theological considerations to admit of analysis in these pages. We regret to find the author quoting Guizot, a writer whose "Histoire de la Civilisation" always reminds us of the old "conchological" systems—founded on an exclusive consideration of the shell, with complete neglect or ignorance of the vitals.

Dr. J. Campbell Brown's paper, on "Electricity compared with Heat as a Source of Mechanical Power," will, we hope, go far to dispel certain semi-scientific delusions current as to the probability of our finding some substitute for coal. The conclusion of the memoir deserves quotation:—"There neither is nor will be any substitute for coal if in that term we include petroleum, oils, peat, and wood,—in short, carbon and hydrocarbons. Force cannot be created; it must be obtained from previously existing stores of force, and when it has been equally distributed there is no collecting it again. We must economise our carbon, and when it becomes exhausted we may utilise the force of the sun, growing wood by its light, producing evaporation or obtaining mechanical power in the shape of wind from its heat, and obtaining mechanical motion from its attraction and that of the moon by means of the tides; but in the earth itself, apart from other worlds, we can expect to find no store of force, either in the form of chemical force, heat, light, or electricity, which will take the place of our carbon deposits. We may learn to do without coal, but we can hope to find no substitute." In all this, however, we see no justification for any increase of the enormous profits of the mine-owner and the coal-merchant, nor yet for any augmentation of the working colliers' allowance of champagne and boiled pine-apples.

Mr. T. Ward communicates an interesting statistical paper on "Salt and its Export from the Ports of the Mersey." The method of preparing the various commercial qualities of salt, as carried on in the Cheshire district, and the quantity exported to various parts of the world, are here given in detail. That it should be carried to India—where, in addition to the facilities for preparing salt from sea-water by solar heat, there are in the north-western deposits beds of rock-salt far surpassing in extent those of Cheshire—seems almost mysterious. But ships going out to India take a cargo of salt at mere nominal freight in preference to going out empty. The duty of £5 per ton, maintained purely as a source of revenue, is economically indefensible, and must ultimately be abandoned.

Mr. E. Nicholson's paper on "Indian Snakes" is intended to combat certain vulgar errors concerning the serpent tribe. He states that out of the 260 species of snakes found in India only five are dangerous to human life. Of these "*Ophiophagus elaps* is very rare" (it has, however, been killed in Calcutta); and "another replaces a congener, the two being rarely found together." Thus the formidable snakes would be reduced to three —the cobra, the bungarus, and the daboia. Nineteen out of every twenty accidents are due to the cobra. Some other species are mentioned which, although venomous, "are not dangerous to man." Under "Error XVI.—*There is some general antidote against snake-poison*," the author remarks, truly enough, that "all experiments in this direction have met with utter failure." But he adds—"If successful they would be of little or no practical use, and they keep up an unhealthy excitement, detrimental to the interest of Science." These assertions are to us utterly unintelligible. The discovery of an antidote seems to us certain to be useful; nor do we see how the interests of Science can suffer from any properly conducted experimental research. As to "Error XVII.—*Some animals are either proof against snake-poison or know of vegetable antidotes against it*," it is well known that the ichneumon and the secretary-hawk, in their combats with the "thanatophidia," depend for success not on any immunity from the effects of snake-poison, but on their dexterity in evading the deadly bite. But what of the hedgehog? We do not know that it has ever been inoculated with the venom of the cobra; but, independently of the authority of Dr. Lenz, we can from our own observation testify that it is absolutely unaffected by the poison of the viper, which in Central and Southern Europe proves fatal to about 20 per cent of the human beings bitten. The annual loss of life due to venomous serpents, Mr. Nicholson thinks, "when reduced to sober death-rates is really of trifling amount; it is on the average about one death in 15,000 inhabitants." Now one death per 15,000 inhabitants would amount in London to 200 fatalities yearly, and in the United Kingdom to 2000! Suppose so many persons came yearly in our midst to a violent and untimely death from one cause, should we call the amount "trifling"? Should we not demand that every conceivable means should be used for the diminution of the evil? Mr. Nicholson has certainly done good service in exposing certain grave errors in the natural history of serpents, but his practical conclusions are, in our opinion, no less to be deprecated than the superstitions he is denouncing.

Mr. A. E. Nevins contributes a paper on the "Method of Correcting the Rate of a Marine Chronometer for Changes of Temperature." Not being one of those omniscient sages who are equally versed in horology, in physics, and in biology, we are unfortunately not able to form any opinion as to the value of Mr. Nevins's method.

The Rev. T. P. Kirkman, known as the author of "Philosophy without Assumptions," writes on "The Janal 14-acral 14-edra." We imagine that all except mathematicians will be fully satisfied with his title, without enquiring further.

"Vegetation and Climate," by Mr. Richmond Leigh, is a sketch of botanical geography.

Mr. W. T. Black's "Natural History of the Grey-wing and Red-wing Partridges of South Africa" is a small but useful contribution to ornithological knowledge. The author recommends the "red-wing" for domestication.

Proceedings of the Bristol Naturalists' Society. New Series. Vol. i., Part 3. 1875-6. London: Williams and Norgate. Bristol: Kerslave and Co.

That the activity of the provincial scientific societies has increased of late years in its amount and has improved in its quality is very generally admitted, and must be received as a hopeful sign of the times. The Bristol Naturalists' Society has four sections—the botanical, entomological, geological, and zoological, the latter of which is at present in abeyance. Of the members eleven have contributed papers printed in the Society's Proceedings, a proportion which we should like to see increased. Concerning the state of the Museum there is no information, and the augmentations to the Library appear to consist to a great extent of the Reports and Transactions of various Societies, English and foreign. The papers which appear in the "Proccedings" are—"Geology of the Bristol District," by W. W. Stoddart, F.G.S.; "On Prof. Renvier's Geological Nomenclature," by E. B. Tawney, F.G.S.; "Birds of the Bristol District," by E. Wheeler; "Age of the Cannington Park Limestone," by E. B. Tawney; "Insect Anatomy," by E. H. Fripp, M.D., who likewise communicates three papers on Microscopy; "Notes on Carboniferous Encrinites from Clifton and Lancashire," by J. G. Grenfell, F.G.S.; and "An Account of the Rainfall at Clifton for 1875," by G. F. Burder, M.D.

History of the American Bison (Bison Americanus). By J. A. Allen. Washington: Government Printing-Office.

This valuable monograph derives additional importance from the fact that the American bison—or buffalo, as it is generally called in the United States—is doomed to early extinction. Perhaps, indeed, the American Government may protect a remnant upon some specially reserved plot of land as an interesting reminiscence of the past, just as has been done by the Emperor of Russia in the case of the kindred species, the European bison, or aurochs (*Bison bonasus s. Europæus*). Mr. Allen gives a full description of the American animal, with tables of measurement of the principal parts of the skeleton, and points out the characteristics which distinguish it from the bison of Europe. Entering next upon its geological history, he shows that its appearance in North America is of comparatively recent date, whilst its fossil remains have hitherto been found in no country outside its known geographical range. He concludes that it is "the descendant of *B. latifrons*, modified by existence in the new conditions of soil and climate to which it was driven by the great changes closing the last ice-age."

The geographical distribution of the American bison at the time of the first arrival of European settlers in the Western Hemisphere is next carefully investigated. According to common tradition it inhabited not merely the eastern half of the great valley of the Mississippi and the basin of the Ohio, but extended its range up to the Atlantic coast. But on carefully sifting the evidence there appears good reason for assuming that the buffalo was not found in New England, nor along the coast of the Middle States, during a long period antedating the exploration of the continent by Europeans, or during the period of the formation of the Indian shell-mounds of the Atlantic coast, which contain no traces of the remains of the buffalo." In the upper parts of the Carolinas the former occurrence of the bison is an established historical fact; but here, also, the evidence of their having been found on the coast is very imperfect. Nor do they appear to have been known within the present limits of Florida. Between 1540 and 1720, however, these animals seem to have temporarily extended their range south-eastwards, and to have penetrated into the country immediately bordering on the Gulf of Mexico, known at that time as West Florida.

A great part of the work is devoted to an account of the gradual extirpation of the bison over the vast tract of country which it once occupied, and its restriction within its present narrow limits. It is now found merely in two distinct areas. The more "southern of these is chiefly limited to Western Kansas, a part of the Indian territory, and North-Western Texas. The northern district extends from the sources of the principal southern tributaries of the Yellowstone northward into the British possessions, embracing an area not much greater than the present territory of Montana." Even within these two districts the author estimates that, at their present rate of decrease, they can scarcely outlive the present century.

To the scientific naturalist the most valuable part of this work is the chapter on the domestication and hybridisation of the bison. Certain theorists of the Swainsonian school maintained, on *a priori* grounds, that it must be irreclaimable. But the following facts are fully attested :—1. That the bison is readily susceptible of domestication. 2. That it interbreeds freely with the domestic cow. 3. *That the half-breeds are fertile.* And 4. That they readily amalgamate with the domestic cattle." For the detailed evidence upon which these inferences are founded we must refer to the work itself. This case, joined to that of the *Leporides*,—a confirmatory account of which has lately appeared in so hostile a quarter as "Les Mondes,"—ought, we think, to make a clean and final sweep of the so-called physiological test of species. It can no longer be safely argued that animals which interbreed and produce fertile offspring must be of necessity specifically identical, nor that those which are morphologically distinct must be incapable of producing fruitful descendants. By bringing before the world the facts just referred to Mr. Allen has rendered one of the main positions of the old natural history simply untenable.

The extirpation of the bison—or at least its very great reduction—is a result which must in the long run have inevitably followed upon the cultivation and enclosure of the country. But it has unfortunately been carried out in the most wasteful and reckless manner, and millions of tons of what might have been utilised as human food have been left to rot, or to become the prey of wolves and vultures.

Annual Report of the Board of Regents of the Smithsonian Institution, for the Year 1875. Washington: Government Printing-Office.

Amongst the varied and valuable matter contained in this volume we notice first a series of publications commenced under the title "Bulletin of the National Museum," intended to illustrate the collections in Natural History and Ethnology belonging to the United States. The total number of vertebrate animal species found in the Nearctic realm as given, is, we think, certainly below the truth, as many fishes must still be undescribed. The number of insect species is estimated at 50,000, of which 8000 are Coleopterous. In enumerating the main zoo-geographical provinces of the world, the "palæotropical" is by a curious typographical error transformed into "palæological." As regards the subdivisions of the Nearctic or North-American realm, the author proposes to make in the Eastern region four provinces dependent mainly on the isothermal lines—viz., the Carolinian, the Alleghanian, the Canadian, and the Hudsonian. The Central region is divided merely into two provinces, parted by the 100th parallel of longitude.

Amongst the memoirs forming the bulk of this volume attention may be drawn to one by Alphonse de Candolle, translated for the Smithsonian Institution, on "The Probable Future of the Human Race." The author's views may thus be summarised:—The coming thousand of years will be marked by a great increase of population, a mingling of races, and a prosperity more or less marked. Then will follow a long period of decrease of population and of general decadence. In this decline the increasing rarity of coal and of the useful metals will play a prominent part. These will by man's operations have become comminuted and equally mingled with the superficial strata of the earth, nor does it apppear that there is at work any natural process which will re-aggregate them in masses capable of new exploitation. The Darwinian theory lends very little countenance to the dreams of indefinite improvement cherished by the French philosophers of the last century. Should the struggle for existence become intensified, as there seems every reason to expect, the intellectual progress of the species must perforce slacken, because the pursuits of the inventor and the discoverer, however beneficial to the race at large, are very scantily remunerative to himself. Hence he will fare as would the vine, the wheat-plant, and others of the most precious and beautiful vegetal species, if left without human aid to compete with the brambles and thistles.

A higher development of the nervous system, holds Mr. Spencer, diminishes the increase of population. A time will therefore come when the chief additions to the population will be made by the less intelligent and less provident families. Hence, says M. de Candolle, "their numbers constantly renewed will greatly affect the supposed progression of intelligence."

It was contended by Malthus, by John Stuart Mill, and others, that almost all social evils spring from the fact that population tends to increase more rapidly than the means of sustenance, and that, whether by moral or physical means, the supply of human beings should always be kept below the demand. Now we grant that the Malthusian individual or the Malthusian family has a certain advantage over non-Malthusians; but unless the principle could be carried into effect all over the world the Malthusian nation must be overwhelmed by its neighbours, whether in war or by the silent—yet equally sure—method of immigration. A nation can only hold its place by unlimited increase, at whatever cost to some of its own members. The future has, for man, no "good time coming."

In a short biographical notice of the late Prof. Agassiz, also taken from the "Transactions of the Genevese Society of Physics and Natural History," we find the following very judicious remarks:—"He excelled in the examination of details and in the comparison of forms. I cannot say that he was equally superior in the principles of natural classification and in theoretic deductions. It may be considered at the least singular that the author of the immense discovery of a parallelism between the successive forms of the embryo of a fish and the successive forms of the class of fishes in general, in geological times, should persistently deny all evolution in the two kingdoms."

In a valuable paper on "Certain Characteristics pertaining to Ancient Man in Michigan," by Mr. Henry Gillman, we meet with the record of a very singular fact:—"About 50 per cent of the humeri are perforated." This is a Simian characteristic which, singularly enough, is found to pertain in the largest degree to the lower races of man, while it is rare or almost absent in the Caucasian (Aryan). "The predominance of the perforation (along with other degraded traits) in the chimpanzee and gorilla, as well as in the lower races of mankind, would suggest, if not a common ancestry in the remote past, at least some predisposing cause common to both the ape and the savage, and this connected with the use of the arm."

Of the long and elaborate paper on the "Stone Age in New Jersey," by Dr. C. C. Abbott, we can notice merely some of the author's conclusions as to the primæval tribes who made and used such instruments:—"What though the Mongol does resemble the American, does this in itself prove relationship?" And, also, it may be asked, *which* of the American aborigines, as they now are, does the Asiatic Mongol most resemble; or has each American tribe a representative in the other continent? Dr. Wilson asserts that "the theory of an aboriginal unity pervading our indigenous American race from the Arctic circle to Tierra del Fuego has been shown to be baseless;" but how can it be proved that the Indians apparently most nearly allied to Asiatic races are the oldest or *original* aborigines? We doubt that all American races are related, and if so different as Dr. Wilson assumes, who can demonstrate which type or pattern was the central, from which came the others that climate, food, and surroundings generally finally produced? "The Indian was once a palæolithic man, and, from whatever source he came, here advanced, without supernatural revelation or the missionary efforts of a superior people, to a condition which is best known as Neolithic." The memoir is profusely illustrated with figures of the stone weapons and tools described, and affords admirable evidence of the rapid progress lately made by American men of Science in investigating the condition of the pre-historic occupiers of the western continent.

Annual Record of Science and Industry, for 1876. Edited by SPENCER F. BAIRD. New York: Harper Bros. London: Trübner and Co.

THIS Annual Record appears to maintain its superiority over all publications of a similar nature. Alike in the selection and the arrangement of its materials, it has the advantage.

Among the recorded facts to which we may here briefly call attention is the hypsometric distribution of the Mollusca. It has long been known that on the slopes of mountains each species of plant has its peculiar zone, below or above which it does not prosper. A similar rule holds good with the terrestrial and fluviatile Mollusca. In the Central Pyrenees and the Alps M. P. Fischer has recognised five zones, each characterised by a species of *Helix*.

The subject of "seasonal dimorphism" receives notice. Certain insect forms, lately regarded as distinct species, are now found to be merely varieties, dependent upon temperature or upon the time of the year at which they appear. Chrysalids of *P. marcellus*, if exposed to severe cold in an ice-house, became *P. Telamonides*. Certain Australian moths, unlike ordinary Lepidoptera, do not suck the honey from flowers, but perforate fruits to feed upon the juices. For this purpose their proboscis is strong and sharply pointed.

According to Dr. Otto Hahn the much-disputed *Eozoon Canadense* is a "myth founded on a mistaken conclusion as to the micro-geological character of certain serpentines." If this statement is true it will be a heavy blow and great discouragement to Principal Dawson and other surviving members of the Cuvierian school. At the same time the *Bathybius* is becoming extremely questionable. It appears to be little more than a flocculent deposit of sulphate of lime.

An extract from the "Transactions of the Norfolk and Norwich Naturalists' Society" gives the "dimensions of some of the famous and larger oak-trees in England." With all deference to the Society we believe it would not be difficult to select oaks larger than many which they have described. We may instance the "Major" and the "Shambles" oaks, in Sherwood; the "Shire" oak, at the junction of the counties of York, Nottingham, and Derby; and the "King" and "Queen" oaks, in Dunham Massey Park, in Cheshire.

A paragraph, the origin of which has been accidentally omitted, states that "A comparison of the observations of naturalists with the weather-charts published in Europe and America makes it now seem certain that the weather immediately prevailing, and not that which is about to come in the near future, is the element which decides the movement of the greater number of migratory birds." This observation strips the migration of birds of no small part of the mystery in which it has been wrapped by unscientific writers.

The decrease of the birds of Massachusetts, both as regards numbers and species, has been examined by Mr. J. A. Allen. The mischief, for such it is, is ascribed in part to wanton shooting.

The well-known water-weed, *Elodea Canadensis*, is described by MM. Stein and Hanstein as having the power of evolving oxygen, or perhaps rather ozone, in considerable abundance. It may thus play an important part in burning up organic pollutions introduced into streams and water-courses, and in preventing—as Dr. Geisler has shown—the development of the lower forms of animal and vegetable life.

In an account of the controversy on Archebiosis, taken from the "British Medical Journal," we notice that the name of Dr. Bastian has been twice misprinted as Dr. Bartian.

Prof. Cope's theory of Evolution is noticed, but scarcely explained with the needful clearness. He considers that Darwin's doctrine of Natural Selection "has a secondary position in relation to the *origin* of variation which Lamarck saw but did not account for, and which Darwin has to assume in order to have materiais from which a 'natural selection' can be made." The author takes a very just view of the influence of Cuvier, which retarded the progress of philosophical zoology for at least half a century. Prof. Cope, in his "Origin of Genera" and "Method of Creation," points out that the most nearly related animal forms present a relation of repression and advance, or a permanent embryonic and adult type, leaving no doubt that the one is descended from the other. This relation was termed *exact parallelism*. It was also shown that if the embryonic form were the parent, the advanced descendant was produced by an increased rate of growth, which phenomenon was called *acceleration*; but that if the embryonic were the offspring, then its failure to attain to the condition of its parent is due to the supervention of a slower rate of growth: to this phenomenon the term *retardation* is applied. *Inexact parallelism* is the result of unequal acceleration or retardation. Additions appear either as *exact repetitions* of pre-existent parts or as *modified repetitions*, the former resulting in simple, the latter in more complex, organisations.

We can, however, no further multiply extracts from this interesting volume, which everyone connected with scientific pursuits ought to read for himself.

Transactions and Proceedings of the Royal Society of New South Wales, for the Year 1875. Edited by A. Liversidge.
Sydney: Richards.

To the scientific societies of the Colonies we look naturally and mainly for a very important class of investigations which they alone can furnish. We expect full accounts of the local fauna and flora, as well as of the palæontology, geology, and mineralogy of their respective regions. Nor are we altogether disappointed. The present volume contains a most interesting paper by S. H. Wintle, on the "Stanniferous Deposits of Tasmania." In this island tin appears to be abundant. In Mount Bischoff, Mount Ramsay, Wombat Hill, Mount Housetop, and in a variety of other localities, it occurs chiefly as ruby tin-ore. Bismuth is also found in Mount Ramsay, in a lode from 30 to 40 feet in width, which has been traced to a considerable distance. Wolfram, chrome-iron, titaniferous iron-sand, carbonate and sulphide of copper, appear also to abound. The climate and the face of the country offer considerable obstacles to the exploitation of these mineral treasures. At Mount Bischoff it is said to rain nine months in the year, and the forests are dense, dark, and compact to a degree almost unexampled. "The moisture is sufficient to render the country a fit habitation for a species of land lobster, whose circular mud-built walls and burrows are found everywhere."

Prof. Liversidge communicates a most valuable memoir on the "Minerals of New South Wales," which will be highly prized both by scientific mineralogists and by technologists.

The remaining pages in the volume are of less general interest. It is to be hoped that the Society will persevere in labouring upon the rich harvest-field of facts spread out before it. It has only to make use of its opportunities to win a high standing for "Transactions," and earn the gratitude of scientific men throughout the globe.

Journal of the Royal Society of New South Wales. 1876. Vol. x. Edited by A. Liversidge. Sydney: C. Potter.

The Anniversary Address, delivered by the Rev. W. B. Clarke, Vice-President, contains some useful remarks on the necessity of Specialism in Science, and especially in what he rightly considers the chief function of the Society—the study of the Physical History of Australia.

In a paper on the "Origin and Migrations of the Polynesian Nation," by the Rev. Dr. Lang, the author maintains that the Polynesian nation is of Asiatic origin and of Malayan race, and was separated from the rest of mankind at a period of the earliest antiquity; that spreading gradually over the Pacific Islands they reached Easter Island, and from thence the western coast of South America, whence they gradually became scattered over all the western continent.

An essay on "Meteorological Periodicity," by the Government Astronomer, Mr. H. C. Russell, is exceedingly interesting. The author examines the various periods or cycles which have been put forward by meteorologists. He mentions that in Tasmania a biennial cycle, consisting of a wet and a dry year alternately, was traced and found to recur regularly for about twenty-five years. Then, however, two wet years occurred together (1848 and 1849), followed by two dry ones, since which Tasmania has had an uncertain rainfall.

A triennial period was suggested by a Mr. Tebbutt, from observations taken at Windsor, in New South Wales, and it has been also traced to the climate of Ceylon. The author does not consider the evidence in favour of an eleven years' cycle—corresponding with the supposed period of the solar spots—to be at all conclusive. There is, at least as far as Australia is concerned, much to be said in favour of a nineteen years' period. The author further draws attention to the influence of cosmical causes in modifying the climatic conditions of our earth. Thus the earth may, and probably does, pass through regions of unequal meteoric density, the heating power of the sun being thus temporarily diminished or increased. The fall of temperature in February and May, and its increase in August and November, producing in the latter month the phenomenon known as the "Indian summer" in America, and as "St. Martin's summer" in Europe, are instances in point.

The Rev. W. B. Clarke contributes a valuable paper on the "Effects of Forest Vegetation on Climate," and shows clearly, from a mass of evidence collected with great patience and judgment, that to denude a country is to destroy its fertility, to expose it to the destructive alternation of flood and drought, and to injure severely its sanitary condition.

We regret to find that the Zoological and Botanical Section of the Society held no meetings during the year. Surely there is an almost unlimited amount of work awaiting its attention.

Transactions and Proceedings of the Royal Society of Victoria. Vol. xii. Melbourne: Stilwell and Knight.

THIS volume contains no small amount of interesting matter. The important question "Is the *Eucalyptus* a fever-destroying tree?" is discussed at some length by Mr. J. Bosisto, and the conclusion drawn is affirmative. The immunity of the Australian continent from fever, as compared with other countries of similar climate, is remarkable, and that in spite of grievous sanitary neglect in many towns. "The various fever-types as found existing among us at times appear malignant, arising either from importation or from the existence of bad sanitary regulations; but medical testimony is that their virulence is meteor-like, and dies at its opening day. No credit can be taken for any improved sanitary condition of our surroundings by ourselves in our towns and cities; the influences operating there *entice* the fever-germ to fructify and abound."

Mr. F. J. Pirani contributes a memoir on "Some Processes of Scientific Reasoning." The following passage may interest some of our readers:—"Chemistry is almost entirely based on ideal construction. We popularly employ the term 'gold' to denote various objects which possess certain properties of weight, colour, &c.; but the gold of the chemist is an ideal conception, bearing the same relation to real gold as a geometrical sphere does to a real sphere."

"Notes on the Discovery of some Keys in the Shore Formation of Corio Bay, near Geelong," by Mr. T. Rawlinson, conveys the impression that in 1845 or 1846 a bunch of keys, of modern make, had been found *in situ* in a shelly bed on the shore, at the depth of about 15 feet below the surface. A note by Mr. R. C. Gunn, F.R.S., however, renders it probable that the keys, though found at the bottom of the shaft, must have been accidentally dropped in from above. The lime-burner who found them admits that he did not pick them out of the stratum of shells.

Mr. R. Etheridge, jun., contributes a paper on the Upper Palæozoic Polyzoa of Queensland.

Mr. A. M. Smith's "Notes concerning the Phenomena of the Approach and Recession of Bodies under the Influence of Radiant Energy" summarise the history of the Radiometer down to June, 1875.

The papers on "Surcharge of the Bullion Assay," by Mr. R. Barton, and on a "Proposed New Method of Weighing applicable to the Gold Bullion Assay," by Mr. G. Foord, though interesting, cannot be rendered intelligible without the accompanying diagrams.

Papers and Proceedings and Report of the Royal Society of Tasmania, for 1875. Hobart Town: Mercury Office.

The papers here presented to the learned world relate principally to botany, malacology, and geology. In a discussion which took place after reading the Rev. J. E. T. Wood's paper, on the "Fresh-water Shells of Tasmania," some interesting statements were made concerning the date of extinction of the moa (*Dinornis*) of New Zealand. The Governor, Sir Aloysius Weld, announced that he had been the first European who had visited the Kaikora country, in the southern island, and that he had been warned by the natives to beware of approaching the moa from behind, as it would kick, and might probably break his leg. They thus showed their acquaintance with the habits of birds of the ostrich family. With reference to the gigantic extinct eagle (*Harpagon Moorei*) he was told by an old chief that on the tops of the mountains an enormous bird, of a rufous colour, built its nest, and that in their forefathers' time it sometimes descended suddenly, and was large enough to carry off a good-sized boy or girl. Mention was made of *Scalaria Australis*, a sea-shell which yields a beautiful purple dye. It will be interesting to ascertain whether this colour is identical with that formerly obtained from certain molluscs inhabiting the Mediterranean, and which was known as Tyrian purple.

Dr. G. Bennett describes the "Frilled Lizard" (*Chlamydosaurus Kingii*) of Queensland. This creature has the curious habit of sometimes standing on its hind legs, and in that position walking, or rather hopping, like a bird. The lace lizard (*Hydrosaurus varius*) has a similar habit. The reader will be at once reminded of certain extinct saurian or sauroid beings which seem to have held an intermediate position between true reptiles and birds, and to have occasionally at least resorted to bipedal motion.

Dr. J. Milligan has presented to the Royal Society his valuable local herbarium, which we devoutly hope will be extended and carefully preserved. It is perfectly humiliating to hear of the fate of the Colonial Herbarium at Cape Town, which is being allowed to perish from damp and the ravages of insects.

We learn that a number of Australian trees introduced into the Isle of Arran are found capable of bearing the winter in the open air. The Australian palm (*Corypha Australis*), the silvered tree-fern of New Zealand, and the great Australian bush-fern were quite untouched by the frost.

A specimen of argentiferous galena, of Tasmanian origin, has been found to yield over 60 per cent of metal, rather more than half of which is silver.

The Royal Society of Tasmania evidently displays a very creditable amount of activity, though we regret to perceive, from the financial department of the Report, that "some members have not yet paid their subscriptions for the past year, and several are even in arrear for former years." It is sad that a Society which has such fine opportunities for research, and which numbers some most zealous members, should be hampered in its career by the want of funds.

Catalogus Polyglottus Historiæ Naturalis. By Prof. C. J. Wheeler. Chicago: S. J. Wheeler.

This singular work gives the names of certain animals, plants, and minerals in Latin, Spanish, French, English, and German, all arranged in parallel columns. Singularly enough many of the English names are incorrect, literal translations from the German being given. Thus no English writer would use "bear-pavian" as a synonym for *Cynocephalus porcarius*, or would call the common badger a "badger-dog." Porcupine and hedgehog are not synonymes for one and the same animal. The wren and the hedgesparrow, again, are two distinct birds. There is also a difference between the misteltoe, or missel thrush, and the fieldfare: the former is the *Turdus viscivorus*, and the latter the *T. pilaris* (Linn.). The common European viper is never known in England as "crossed adder" or "copper snake." *Lampyris noctiluca*, the glow-worm, is not called "John's beetle," nor did we ever hear it spoken of in Germany as "Johanniskäfer," a name we always knew to be applied to *Rhisotrogus solstitialis*. "Purple emperor" is the English name not of *Argynnis Paphia*, but of *Apatura Iris*. Many more mistakes of a similar kind might be enumerated if it were necessary. Accuracy is of course the one thing needful in a work of this class, and if found wanting it will only serve to promote misunderstanding and confusion.

Statistics, Medical and Anthropological, af the Provost-Marshal-General's Bureau, derived from Records of the Examination for Military Service in the Armies of the United States, during the late War of the Rebellion, of over a Million Recruits, Drafted Men, Substitutes, and Enrolled Men. By J. H. BAXTER, M.D. In two volumes. Washington: Government Printing-Office.

THE work before us consists of two goodly quartos, of about 600 pages each, profusely illustrated with diagrams. The Introduction gives an account of the working of the conscription system during the American civil war, and of the difficulties encountered by the medical officers in judging of the physical fitness of a recruit. The methods adopted for examining the soundness of the men are next described. Then follows a comparative view of the instructions issued by the United States Government and by the principal Governments of Europe for the guidance of the medical officer in the examination of recruits, every particular being given in detail. A great part of the work is devoted to a comparison among the men of different nationalities who enlisted in the United States Army, as regards height, weight, girth of chest, and liability to various diseases. The value of the results thus obtained entirely depends on the question whether the recruits examined can be considered as fair average representatives of their respective nationalities—a point on which, in some cases, very grave doubts may be entertained. The work, however, will be of very great value to the medical authorities of every army.

*The Influence of English Quakerdom upon German Culture, and upon the Anglo-Russian Project of a Universal Church.** By Bruno Bauer. Berlin: Grosser.

We have here a work published, it would seem, in the year 1878, and certainly deserving profound attention. To discuss a politico-ecclesiastic treatise in the peaceful pages of the "Quarterly Journal of Science" is of course out of the question; but we recommend Herr Bauer's views to the careful consideration of our political, religious, and social contemporaries. We were about to say that some of them might possibly have their eyes opened, but we remember that the infatuated are always able to interpret both facts and arguments in favour of their own prepossessions. To the author we would merely say that he has seen much, but not all, and that England has before now recovered from hallucinations not less threatening.

United States Commission of Fish and Fisheries. Part III. Report of the Commissioner for 1873-4 and 1874-5. Washington: Government Printing-Office.

This Report comprises an inquiry into the decrease of the food-fishes, and an account of experiments on the propagation of food-fishes in the waters of the United States. Among the interesting appendices we find a treatise on the condition of the fisheries among the ancient Greeks and Romans, and on their mode of salting and pickling fish. The share which the seas and rivers may be made to take in contributing to human subsistence was thoroughly understood in the days of classic antiquity. No fewer than four hundred kinds of fishes are mentioned by Greek authors—a proof of the great attention paid to the population of the waters. Artificial pisciculture, an art which is now only beginning to revive, was extensively practised.

Further sections give the statistics and other facts connected with the most important fisheries of the North Atlantic and of the Arctic Ocean, such as those of Norway, Sweden, Denmark, Britain, and Russia. The amount of valuable products thus obtained and the magnitude of the interests involved will be surprising to the general public. Thus in the year 1872 1,210,000,000 lbs. of herrings were taken in the Bay of Malanger, on the Norwegian coast. At Söndmöre the yield of the spring cod-fisheries was nineteen and a half millions of fish,—110,000,000 lbs. of liver, or at least 55,000,000 lbs. of oil, and 39,600,000 lbs. of roe. An especial notice is given to M. Sar's

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* Einfluss des englischen Quäkerthums auf die deutsche Cultur, und auf das englisch-russische Project einen Welt-kirc.

new theory of the migrations of the herring. He denies that it at any time of the year inhabits the deep-sea bottoms, where its favourite food, small oily crustaceans, cannot be found. During the summer it lives scattered, in the open seas, between Iceland, Scotland, and Norway, and approaches the Norwegian coast in a south-easterly direction at the beginning of the spawning-season. The Nordland *great* herring lives, in Sar's opinion, to the north-west of Nordland and Finmarken, but somewhat nearer the coast. The periodicity in the herring fisheries, the occurrence at certain intervals of years unusually productive, is a question still in dispute. It would be very rash to deny the possibility of such a phenomenon.

The artificial cultivation of carp in ponds, as carried on in Holstein and East Prussia, is described at some length. We have heard it maintained that land laid out in well-arranged carp-ponds will yield a larger return than the same superficies devoted to grazing oxen or sheep. The profits in Silesia are given as about £7 10s. per acre.

All persons who take an interest in fish, whether zoological, economical, or gastronomical, will find this volume most instructive reading. Great credit is due to Prof. Spencer F. Baird for the zeal with which he pursues the important investigations with which he has been entrusted.

The Lazy Lays and Prose Imaginings. Written, printed, published, and reviewed by W. H. Harrison. A.D. 1877 (Popular Chronology). A.M. 5877 (Torquemada). A.M. 50,800,077 (Huxley). London: 38, Great Russell Street.

We have here a somewhat singular book, partly in prose and partly in rhyme, sometimes comic and sometimes sentimental. On its cover it bears emblazoned a griffin who is to keep watch on the Arimaspians of the nineteenth century—*i.e.*, American publishers. The author gives, among other things, instructions how to "double the utility of the printing-press." The simplest way to effect so desirable an end would be, in our opinion, to print less rubbish, in the prevalent shapes of novels, stump-orations, biographies, quack advertisements, and the like, so that books and papers worth reading might stand out more distinctly before the world. Mr. Harrison unfortunately takes a very different plan: he aims simply at economising paper by introduction of characters which would none of them take up more room than does the letter "i." We can see not the least good in his proposals. The introduction of a new character, which would certainly not be adopted all the world over, would retard instead of promoting civilisation, and would be particularly unfortunate at a time when there is some hope that the Germans may abandon their peculiar type, and thus place themselves in easier mental communication with the rest of cultivated nations. Our present characters, however irrational their origin, which is a totally irrelevant consideration, have the incalculable advantage that they are distinguishable from each other with little strain to the eye, and that very difference in size to which Mr. Harrison objects is therefore a valuable attribute. Letters formed on the author's plan would require much closer examination to distinguish one from another, and would, from their very trifling difference in size and shape, cause the eyes to "swim," occasioning serious injury to sight in people of studious habits.

"Why should any word," asks Mr. Harrison, "be more than one or two syllables in length?" And yet he is a poet! We have always protested against the length of the technical terms introduced into the sciences under pretext of "significance." But a language framed on our author's principles would rival in "mad monotony" the "Mission-room" bell which is at this very moment throbbing and tingling through our brain.

Mr. Harrison's strictures on some of the materialists of the day who yet cannot agree what matter is are much more valuable than his proposed printing-reform. His poems sometimes recall those of Hood.

Does Vaccination afford any Protection against Small-Pox? By T. B. Sprague, M.A., a Vice-President of the Institute of Actuaries.

Most persons are aware that compulsory vaccination is one of the "burning questions" of the day, and that, instead of being calmly discussed in medical and sanitary circles, it is taken up by political agitators as a point of their creed. On the one hand we have the orthodox medical practitioners, who as a body put a somewhat exaggerated faith in the prophylactic virtues of cow-pox. On the other hand is arrayed a league who consider vaccination as not merely futile and dangerous, but when enforced by law as an outrage on their civil and religious liberties. But there is still a third party, who, whilst utterly scouting the vested rights of disease and considering the State fully justified in enforcing precautions for the promotion of public health, do not feel altogether satisfied with the evidence adduced in favour of the efficacy of vaccination. They ask how is it that we have still small-pox epidemics, assuming almost pestilential proportions, if vaccination is indeed a safeguard? There are very few persons who do not undergo the operation once in life, and multitudes are re-vaccinated whenever the disease breaks out afresh in the neighbourhood which they inhabit. It is also declared that in the years 1815 to

1835, when vaccination was comparatively rare, small-pox epidemics were commonly spoken of as a thing of the past, and a pitted face among young people was almost unknown. These facts have not been satisfactorily explained, and any additional light is therefore welcome. Mr. Sprague, however, after a careful examination of the statistics of the case, draws no very decided conclusion. He thinks the inferences condemnatory of vaccination founded by Dr. Keller on the vital statistics of the *employés* of the Austrian State Railway scarcely warranted by the figures, but admits that the Doctor has established that "the rate of mortality among the vaccinated persons who were attacked was quite as heavy as that among the unvaccinated." In English statistics, and in the comments made upon them, he can "see nothing that at all explains why, notwithstanding the introduction of compulsory vaccination, the deaths from small-pox should have risen in the year 1872 so far beyond their number in any of the previous seventeen years."

Mr. Sprague is a spelling reformer, and a friend of Mr. H. Pitman; consequently the orthography of his pamphlet reminds us of our old acquaintance the "Fonetic Nuz."

Dynamics, or Theoretical Mechanics. In accordance with the Syllabus of the Science and Art Department. By J. T. BOTTOMLEY, Lecturer and Demonstrator in Natural Philosophy in the University of Glasgow. London and Glasgow: W. Collins, Sons, and Co.

THIS book belongs to "Collins's Elementary Science Series," and is specially prepared to meet the requirements of Science Classes under the "Science and Art Department." In the Introduction the author refers to a late change in nomenclature. "The name *Mechanics*," he remarks, "which properly denotes the science of machines, and was used by Newton in that sense, came for a time into use, instead of the appropriate word *Dynamics*, for the science which treats of force, and under that name—*i.e.*, Mechanics—there was a peculiar cross-division of the subject into Statics and Dynamics, in which the proper signification of the latter name was altogether departed from." We fear there is, in the present day, too strong a tendency to abolish names, inappropriate, indeed, if we look to their derivation, but which are thoroughly understood in favour of such as are more "significant" and etymologically more correct. It is a singular fact that the terms "mechanical" and "dynamical," now regarded as synonymous, were at one time treated as decidedly antithetical.

The work before us is divided into eight chapters, treating of the measurement of time, space, mass, velocity, acceleration, and momentum; of force, and the composition and resolution of forces; of the properties of matter; of gravitation; of the equilibrium of simple mechanical arrangements; of falling bodies; of curvilinear motion; of work and energy. The author, laudably enough, advises his readers to make themselves familiar with the metric system of weights and measures, but we fear that he is too sanguine in holding that these must shortly become universally employed.

In the "Exercises" at the end of the book we find the very remarkable statement that "a gallon of water weighs 18 lbs." This is doubtless a typographical error, but it calls for prompt correction. The work is well arranged, and the explanations lucid.

A Treatise on Chemistry. By H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S., Professors in the Owens College, Manchester. Vol. I. The Non-metallic Elements. London: Macmillan and Co. 1877.

It is now some years since a treatise on chemistry of any magnitude has been published in this country, and all students of chemistry will welcome the volume of Profs. Roscoe and Schorlemmer as a real addition to their science. Embracing the newest discoveries and the newest methods, it aims to give a clear and detail account of Modern Chemistry, taken in its broadest significance. When we call to mind the large experience and untiring industry of the authors, both as regards original research and professorial teaching in lecture-room and laboratory, we have a right to expect a very able treatment of the subject at their hands. This expectation is fully realised.

Out of the 750 pages of which this first volume consists, 40 are given to an historical introduction, 54 to the general principles of the science, 610 to the non-metallic elements, and 46 to crystallography. The Introduction gives a brief but very lucid account of the rise and progress of the science, traced from the earliest times to discoveries of Priestley, Scheele, and Lavoisier, and the development of the atomic theory by Dr. Alton. The more prominent facts in the history of organic chemistry are also detailed. In the second section the laws of chemical combination are clearly explained, and the manner in which the combining weights of bodies are obtained from the analysis of compounds. The kinetic theory of gases and diffusion and effusion are discussed, and a brief but all-sufficient account of chemical nomenclature concludes this section.

The non-metals are divided into the four usual groups, according to their atomicities, and they are described in the order in which they there occur. The account of ozone contains all that we know on the subject, and is fuller than is usually found in our manuals. It is called "Active Oxygen," and is represented as $O_3 = 47{\cdot}88$, having a density $= 23{\cdot}94$. That is, three volumes of oxygen form two volumes of ozone, and, when acted upon by potassium iodide, one-third of the ozone is used up in liberating the iodine, and the remaining two-thirds go to form ordinary oxygen—

$$O_3 + 2KI + H_2O = O_2 + I + 2KOH.$$

Group 5.—Besanez has recently shown that ozone is invariably formed when water evaporates, and it is mainly to this source that its presence in the air has to be traced. Ozone has been recently used for bleaching engravings which have been discoloured by age. They are rolled up and placed in a glass globe containing water and moist phosphorus. Ozone has also been used for the purpose of oxidising alcohol, C_2H_6O, to aldehyde, C_2H_4O, a substance employed in the manufacture of aniline-green. In the next section the proofs of the composition of water are elaborately discussed, and an excellent engraving shows the completest form of apparatus for the oxide of copper synthesis. A best method of exploding a mixture of oxygen and hydrogen is shown in Fig. 68; and of the energy developed we are told that 1 grm. of hydrogen, in burning to form water, produces an amount of energy sufficient to raise 24,577 kilogrms. through the space of 1 metre. An ingenious apparatus for showing that sulphur dioxide contains half its volume of sulphur vapour is figured on page 307. An exhaustive account of the manufacture of sulphuric acid, with diagrams of the newest forms of apparatus for its distillation, forms a prominent feature of the article on Sulphur. This is followed by a description of the "Chlorides and Bromides of Sulphuric Acid"—names, we think, which have not been very happily chosen. A detail account of carbon, and its compounds with hydrogen, chlorine, oxygen, sulphur, and nitrogen, conclude the volume.

Throughout the book the style is very lucid. The engravings, which are quite equal to those we meet with in French and German scientific works, are directly taken from photographs of apparatus in actual use, and the entire get up of the book is excellent. We trust that the second volume will soon make its appearance.

An Elementary Treatise on Physics, Experimental and Applied. Translated from Ganot's "Eléments de Physique," by E. Atkinson, Ph.D. Eighth Edition, Revised and Enlarged. Longmans. 1877.

This well-known work, which has long since established its reputation, has reached an eighth edition. Since its first appearance it has, in each succeeding edition, been enlarged and rendered more complete. The present additions include sixty-two new illustrations, and about sixty pages of new matter. It presents in all respects remarkable completeness; there are numerous tables, and four coloured plates, including two which represent respectively the isogonic and isoclinic lines for the year 1860. Few changes have been made in the earlier parts of the book. In "Sound," however, we find several additions notably relating to the synthesis of sounds, Kundt's determination of the velocity of sounds, and Kœnig's manometric flames. In Heat, the subject of Radiant Heat is treated more fully, an account of Crookes's Radiometer and the theory of its action, Bunsen's Ice-Calorimeter, and the sources of cold. An interesting description of Horn's method of determining the mechanical equivalent of heat will be found on p. 410.

In Light, the construction and use of Hedley's Reflecting Sextant is given, and the measurement of small angles by reflection from a mirror. The Spectroscope is treated much more fully than before, also the whole subject of the eye. The chapters on Magnetism are enlarged and much improved since the earlier editions, and the plates showing isogonic and isoclinic lines are beautifully clear and intelligible. In Electricity, the applications of the discharge to the firing of mines has been introduced; also a woodcut, and brief account of Leclanché's battery (unaccompanied, however, by the equation which expresses the change within the battery). A graphic representation of the heating effects in a circuit is given on p. 710, and the mechanical effects of the battery on p. 718. The science of Electro-dynamics is treated at greater length than before, and several new woodcuts have been introduced to illustrate this portion of the subject. The sounder telegraph receives no more than a passing notice; a woodcut in the next edition would be acceptable in this connection. Becquerel's electric thermometer is described in detail and figured, and the subject of animal electricity receives greater attention than hitherto. In the Meteorology and Climatology a description, accompanied by a good engraving of Secchi's Meteorograph, will be found; also Tyndall's researches on the influence of aqueous vapour on the radiation of heat, and on the blue colour of the sky. The volume is concluded by 221 well-selected problems and examples in various branches of Physics, the answers to which are given.

We congratulate Prof. Atkinson on the completeness and thoroughness of his work in connection with the new edition of Prof. Ganot's "Physics."

Stability of Motion. By E. J. Routh. (Adams's Prize.)

This is an essay of somewhat uneven interest. The able and elegant discussion of the conditions that an equation may have no positive roots and no imaginary roots with the real part positive is very interesting, both in method and result; but the remainder of the work takes (perhaps necessarily) too much the form of enquiry into special and limited cases. We are inclined to think that the line of investigation as slightly sketched in Chapter VIII. would have yielded results of more general theoretical interest. But the subordination of theoretical developments to practical application seems to us the defect of this laborious and original essay.

Spherical Harmonics. By the Rev. M. M. Ferrey.

An exceedingly clear and well-arranged account of the leading properties of these functions, with continual reference to their application to the theory of the potential. Possibly too great care has been devoted to giving algebraically simple proofs of the preliminary propositions, which might have been exhibited in closer connection by a free use of Green's theorem. One is surprised, too, to find no mention of Prof. Maxwell's representation of solid harmonics as derived from $\frac{1}{r}$ by axis-differentiation. But if these are faults they are alone. The concluding chapter on Ellipsoidal Harmonics, as the author happily terms Lamé's functions, is an especially good introduction to this elegant extension of the spherical analysis.

Inductive Metrology; or the Recovery of Ancient Measures from the Monuments. By W. M. Flinders Petrie. London: Hargrove Saunders. 1877.

This work gives a very elaborate comparison of the measures ot all the great nations of antiquity with those of modern times. Measurements from more than 600 buildings have been considered, and as many as 10,000 measurements have been examined. Among the results obtained may be mentioned the proof of "the exact identity of the American mound-builders' unit with the Hebraio-Persian cubit, which had a wide and ancient diffusion in the old world. The close similarity of the Mexican unit with the widespread 21·4 unit of the old world; and the similarity also of the pre-historic British, and Christian Irish unit to this. The close similarity of the Phœnician unit to a principal unit of pre-historic British remains, and also to the Polynesian unit. The identity of the Pelasgic with the Etrurio-Roman foot. The continuance of the Romano-British units into mediæval times, the resemblances being generally inexact, and far within the probable errors. Also the similar continuance of the classical units into the Mohammedan times in Turkey and Persia."

Incidents in the Biography of Dust. By H. P. MALET. London: Trübner. 1877.

THIS work reminds us in its style of some of Victor Hugo's rhapsodies concerning matter and force. A few quotations from the work will be more effective in showing its general character than any more formal notice, for which we have neither the inclination to write nor the space to give:—

"Air is composed chiefly of oxygen, hydrogen, and carbonic acid gases."

"Air and water are active, dust is the passive element."

"Pressure condenses dust; dust condenses heat."

"Heat acts on the dust."

Southern Stellar Objects for Small Telescopes, between the Equator and 55° *South Declination.* By J. E. GORE, Asst. Engineer, India. Lodianet. 1877.

THIS carefully compiled work embodies the results of observations made in the Punjaub with an achromatic of 3 inches aperture and four feet focal length. All the best catalogues of southern stars have been consulted. The work will be of much use to observers in India.

Physiography and Physical Geography. By the Rev. ALEX. MACKAY, LL.D., F.R.G.S. London: William Blackwood. 1877.

IN September, 1876, the Committee ot Council on Education decided that Physical Geography, as defined in the Science Directory, was not a subject in which the special aid of the Science and Art Department should be continued. But the Committee substituted for it *Physiography*, a subject designed to embrace "those external relations and conditions of the earth which form the common basis of the sciences of nautical astronomy, geology, and biology, as treated in the Science Directory." Dr. Mackay's work was written in order "to meet the wants of such an examination. It is a good book, but too overcrowded with facts. It is a condensation of what might fill a dozen volumes. In the short space of 143 pages it treats of Mathematical Geography; the Relation of the Solar System to the Universe; the Earth Viewed Individually; Configuration of the Surface; the Atmosphere; Climate; Electricity and Magnetism; Biology; Geology; Optical Instruments; How to Find the Distance of the Heavenly Bodies; Map Projections and Geodetical Surveys. Such extraordinarily condensed matter can never be properly digested, and it tends to foster that system of "cram" which is the curse of our modern education.

The London Science-Class Books. Elementary Series. Edited by Prof. G. C. FOSTER and PHILIP MAGNUS. *Thermodynamics*, by RICHARD WORMELL, D.Sc., M.A. *Astronomy*, by R. S. BALL, LL.D., F.R.S., Royal Astronomer of Ireland. London: Longmans and Co. 1877.

THE works of this series have special reference to science teaching in schools, but surely not to science as now taught in the generality of schools. If the time comes when there are regular science "sides" in schools, as we now have "modern side" and "classical side," these works will be invaluable, but they are far above the capacity of boys who are only able to devote two hours a week to science. Mr. Wormell's "Thermodynamics" is a very dry book, only suitable for the most advanced boys in large schools, who would use it in lieu of some mathematical work. It is absurd to put into the hands of boys, who do not understand the construction and use of a thermometer, a work which embodies in a condensed form some of the mathematical deductions of Clausius, Thomson, and Clerk Maxwell.

Professor Ball's "Astronomy" is written in a clearer and more interesting style, and will be decidedly useful for those who, having some knowledge of mathematics, desire to make themselves acquainted with the first principles of astronomy.

The Voyage of the "Challenger": The Atlantic. A Preliminary Account of the General Results of the Exploring Voyage of H.M.S. "Challenger" during the Year 1873 and the early part of the Year 1876. By Sir C. WYVILLE THOMSON, LL.D., F.R.SS. L. and E., F.L.S., &c. In Two Volumes. Published by Authority of the Lords Commissioners of the Admiralty. London: Macmillan and Co.

IN forming an opinion of these two volumes regard must be had to the limitations which appear upon the title-page. The account of the exploring expedition here given is only "preliminary."

As we are reminded in the preface, some years must elapse before the facts ascertained can be all thoroughly digested. "The observations are still unreduced, the chemical analyses are only commenced, and there has not been time even to unpack the natural history specimens." Hence to give anything like a detailed account of the additional data which have been acquired by the "Challenger" expedition, or of their bearings upon the various problems of physical geography, must be quite impossible. To pronounce, therefore, upon the value of the expedition from a mere perusal of the present volumes, and to declare it a failure, either relatively or positively—as some persons are doing —appears to us in the highest degree rash and presumptuous.

Another point to be remembered is, the work before us embraces only the less interesting portion of the expedition, the exploration of the Atlantic. The transactions of the second and third years, in the Indian and Pacific Oceans, cannot as yet be placed before the public in their entirety.

To form some idea of the amount of work actually performed we must bear in mind that the explorers took up 362 special observing stations, as nearly as possible at equal distances, and at each made the following observations :—

"The exact depth was determined. A sample of the bottom, averaging from 1 oz. to 1 lb. in weight, was recovered by means of the sounding instrument, which was provided with a tube and disengaging weights. A sample of the bottom water was procured for physical and chemical examination. The bottom temperature was determined by a registering thermometer. At most stations a fair sample of the bottom fauna was procured by means of the dredge or trawl. At most stations the fauna of the surface and of intermediate depths was examined by the use of the tow-net variously adjusted. At most stations a series of temperature observations were made at different depths from the surface to the bottom, and samples of sea-water were obtained from different depths. Atmospheric and other meteorological conditions were carefully observed and noted ; the direction and rate of the surface current was determined, and at a few stations an attempt was made to ascertain the direction and rate of movement of water at different depths."

But it must not be thought that the duties of the expedition were limited to the observations just detailed. The islands and the coasts of continents visited were also to be explored from several points of view. It was especially enjoined upon the voyagers to "obtain information about the vegetation of oceanic islands." These, as is very justly remarked in the code of official instructions drawn up by a committee of the Royal Society, "are in many cases the last positions held by floras of great antiquity, and, as in the case of St. Helena, they are liable speedily to become exterminated, and therefore to pass into irremediable oblivion when the islands become occupied." We may even ask whether it would not be possible to secure an agreement among civilised nations against turning loose pigs, goats, or rabbits on any island not inhabited by carnivorous animals. Had it not been for goats much of the original flora of St. Helena would be in existence at the present day. The terrestrial faunæ of the islands and coasts visited were doubtless examined in the same manner as the floræ.

Thus, in addition to details of what may be called sea-work, we find a very complete account of the Bermudas, the Falkland group, Tristan d'Acunha, Ascension, the Açores, besides notices of Madeira, the Cape Verde islands, the Rocks of St. Paul, and Fernando Noronha. In this latter island, a Brazilian convict station, distant about 180 miles from the mainland, and hence likely to present interesting correspondences or differences, the expedition met with a strange reception. The governor would allow them to land, offered them horses and guides and every possible accommodation, but on condition that no scientific work was to be done. "Captain Nares asked, if we saw a butterfly might we not catch it, but he said he would prefer that we should not!" This was a great disappointment, as some of the party had intended to prepare a monograph of the natural history of the island. On approaching Bahia they encountered one of those strange hosts of butterflies which are occasionally met with on the coasts of Brazil, soaring out seawards. They belonged to the slight and delicate genus *Heliconia*, and fluttered all day over the ship and over the sea as far as the eye could reach, like the flakes in a heavy snow-storm. This curious phenomenon is not by any means perfectly understood.

At Sto. Amaro the party met with an ornithological incident which may be worth mentioning. "As the truck ran quickly down the incline the swarthy young barbarians, attracted by the novelty, crowded round it, and suddenly the agonised cries of a child, followed by low moanings, rang out from under the wheels, and a jerk of the drag pulled the car up and nearly jerked us out of our seats. We jumped out and looked nervously under the wheels to see what had happened, but there was no child there. The young barbarians looked at us vaguely and curiously, but not as if anything tragical had occurred, and we were just getting into the car again, feeling a little bewildered, when a great green parrot in a cage close beside us went through another of his best performances in the shape of a loud mocking laugh. A wave of relief passed over the party, and the drivers expressed to the parrot their sense of his conduct, I fear strongly. For a fortnight dredging was carried on with great success in the shallow waters of the bay, almost every haul bringing up large numbers of fine tropical shore forms, when one of the men on leave was suddenly and fatally attacked with yellow fever, and it was deemed prudent to weigh anchor at once.

Dredging and trawling in the deep sea is an operation not exempt from difficulty and even danger, and often disappoints the eager naturalist. Between St. Thomas and the Bermudas "the large iron dredge which we were using in preference to the trawl caught upon a rock, or a mass of coral, and brought a sudden strain upon the dredge-rope; and before the rope could be veered or any other steps taken to relieve the strain, the hook of the foremost span carried away, and the leading block which was hooked to it flew back and struck William Stokes, one of the sailor lads, with such violence that he was driven against the ship's side. His thigh was broken in two places, and he was so seriously injured otherwise that he never recovered consciousness, and died a few hours afterwards." An accident, unattended with any tragical features, occurred on the run between Bahia and the Cape. "The trawl was lowered, and on heaving in it came up apparently with a heavy weight, the accumulators being stretched to the utmost. It was a long and weary wind-in on account of the continued strain; at length it came close to the surface and we could see the distended net through the water, when just as it was leaving the water and so greatly increasing its weight the swivel between the dredge-rope and the chain gave way, and the trawl with its unknown burden quietly sank out of sight. It was a cruel disappointment,—every one was on the bridge, and curiosity was wound up to the highest pitch: some vowed that they saw resting on the beam of the vanishing trawl the white hand of the mermaiden for whom we had watched so long in vain; but I think it is more likely that the trawl had got bagged with the large sea-slugs which occur in some of these deep dredgings in large quantity, and have more than once burst the trawl-net."

We find no mention of portions of the skeletons of the higher animals having been brought up by the dredge, save some sharks' teeth and the ear bones of whales fished up in one case from the depth of 2275 fathoms. No saurian or ophidian remains are mentioned as having been met with.

The floating islands of sea-weed, the Sargasso sea of the old Spanish navigators, offer in their fauna some curious examples of protective resemblance. "Animals drifting about on the surface of the sea with such scanty cover as the single broken layer of the sea-weed must be exposed to exceptional dangers from the sharp-eyed sea-birds hovering above them and from the hungry fishes searching for prey beneath; but one and all of these creatures imitate in such an extraordinary way both in form and colouring their floating habitat, that we can well imagine their deceiving both the birds and the fishes."

A curious observation, recorded in a former work of the author's and confirmed during the cruise of the "Challenger," is the absence of eyes in certain deep-sea animals, whilst in others they are fully developed. In *Ethusa granulata*, a stalk-eyed crustacean, well-developed eyes are found in specimens taken in shallow water. In deeper water, from 110 to 370 fathoms, the eye-stalks are still present, but the animal is apparently blind, the stalks ending not in eyes, but in mere round calcareous knobs, whilst at still greater depths, from 500 to 700 fathoms, the eye-stalks themselves lose their specific character and become fixed. Here, then, as the author remarks, we "have a gradual modification, depending apparently upon a gradual diminution and final disappearance of solar light." But in *Munida*, a form from equal depths, the eyes are "unusually developed, and apparently of great delicacy."

It is certainly conceivable that as the light gradually decreases in amount one of the changes may take place; the power of vision may fade away *pari passu* as no longer needed, and its organs themselves may ultimately become abortive, or, on the other hand, it may become more and more acute as the solar light declines, and, as Sir Wyville Thomson suggests, may "become susceptible of the stimulus of the fainter light of phosphorescence." But on what does it depend in any given case which of these two kinds of progressive modification shall take place? Why do we find the influence of identical circumstances so different, if not diametrically opposite? This is a question which the biologist has often to put to himself in vain.

A most interesting phenomenon met with in the Falkland Islands may require a somewhat lengthened notice on account of its connection with the "glacial question." We refer to the so-called "stone-rivers," a natural feature not occurring elsewhere on such a scale. Many of the valleys in the East Island are occupied by pale grey masses, varying in width from a few hundred yards to a mile or upwards, and which, from a distance, simulate glaciers descending from the ridges and stretching down from the sea. They consist, however, not of ice, but of blocks of quartzite, from two, twenty feet in length by about half as much in width, resting irregularly upon each other, and supported by the edges and angles of those below. They are not weathered to any extent, though the edges and points are in most cases slightly rounded, and the surface also perceptibly worn, but only by the action of the atmosphere, is smooth and polished, and a very thin, extremely hard white lichen, which spreads over nearly the whole of them gives the effect of their being covered with a thin layer of ice." Water is heard murmuring down below, and occasionally becomes visible where the interval between the blocks is exceptionally large. At the mouth of the valley the section of the "stone-river" is like that of a large stone drain. The only difficulty in accounting for this phenomenon is, according to the author, the slightness of the slope; that from the ridge to the valley not exceeding six degrees, whilst the inclination of the valley itself is only two or three, so that blocks of such a form are obviously unable to roll or slide down. How, then, is the effect produced? The beds of quartzite vary greatly in hardness; some crumbling readily away, whilst others scarcely yield at all to ordinary weathering. The softer portions being thus worn away, the harder beds are left as long projecting ridges along the crests and flanks of the hill ranges, and ultimately, being deprived of their supports, give way, and the fragments fall over upon the sloping hill-sides when they become imbedded in vegetation, a slight inequality in the surface of the turf merely indicating the blocks buried beneath it. From a variety of causes, the whole soil-cap, blocks included, gradually creeps down even very gentle slopes. Thus the alternate expansion and contraction of the vegetable mass, according as it is soaked with water or comparatively dry, gradually pushes the blocks downwards. The rain-water as it runs down clears away the movable matter before them, and the vegetable mould on which they rest "is undergoing a perpetual process of interstitial decay and removal." In this manner the blocks are gradually swept down the slope and collected in the valley.

The author gives analogous cases, though on a less striking scale, which he has observed in the West Highlands of Scotland and elsewhere. He adds:—"It seems to me almost self-evident that wherever there is a slope, be it ever so gentle, the soil-cap must be in motion, be it ever so slow; and that it is dragging over the surface of the rock beneath the blocks and boulders which may be imbedded in it; and frequently piling these in moraine-like masses where the progress of the earth-glacier is particularly arrested, as at the contracted mouth of a valley where the water percolating through among them in time removes the intervening soil. As the avalanche is the catastrophe of ice-movement, so the land-slip is the catastrophe of the movement of the soil-cap. As I have already said, I should be the last to undervalue the action of ice, or to doubt the abundant evidences of glacial action; but of this I feel convinced, that too little attention has been hitherto given to this parallel series of phenomena, which in many cases it will be found very difficult to discriminate; and that these phenomena must be carefully distinguished and discriminated before we can fully accept the grooving of rocks and the accumulation of moraines as complete evidence of a former existence of glacial conditions."

The theory thus suggested evidently demands very careful examination. Every geologist must have observed blocks embedded in vegetable mould at considerable distances from the rocks whence they were obviously derived upon slopes too gentle to admit of rolling or sliding and in situations where the accumulation of a head of water sufficient to bear them down seems utterly improbable. If they have travelled since these slopes can have been glaciated, if they are still travelling, Sir W. Thomson's theory must be accepted. But the movement is exceedingly slow, so as to render direct evidence of its existence difficult to procure. By putting forward this view, valuable as it is to all true geologists, the author will certainly offend that class of dogmatists who are now presuming to call themselves sceptics in geology.

But we must now turn to the author's general results as laid down provisionally. Till lately the most eminent naturalists, judging from such limited evidence as was at their disposal, were of opinion that life at the bottom of the sea was confined to a narrow border around the land, and that at the depth of about 100 fathoms plants disappeared almost totally and animal life became scarcer and scarcer, representing only such groups as are among the simplest in their organisation, whilst at greater depths—300 fathoms and upwards—organic life was absent, the physical conditions precluding the possible existence of living beings. Gradually, however, facts came to light which could not be reconciled with this view of the distribution of organic life. Samples of the sea-bottom brought up from the depths of the Atlantic by Brookes's sounding machine were found, on microscopic examination, to consist largely of the shells, entire or broken, of certain Foraminifera, which appeared to have lived where found rather than to have floated on the surface of the sea. Star-fishes with their stomachs full of deep-sea Foraminifera were brought up from depths of 1200 fathoms. Still this important evidence was scarcely received as sufficient. Men of science hesitated to admit the possibility of animals flourishing, not merely in the total absence of day-light, but under a pressure amounting at 1000 fathoms to 1 ton on every square inch of surface. The results obtained by the author and Dr. Carpenter during their voyages in the "Lightning" (1868) and the "Porcupine" (1869 and 1870) followed up on a far wider scale by the great expedition of the "Challenger," may be said to have fully decided this important question.

The views formerly entertained concerning the limitations of oceanic vegetable life are indeed unchanged, the author finding that vegetation is "practically limited to depths under 100 fathoms." Very few of the higher Algæ inhabit the surface of the sea, the chief exception being the gulf-weed, *Sargassum bacciferum*. Thus marine plant life may still be regarded as substantially littoral. But with animals the case is totally different. It is present at the bottom of the sea at all depths, though much less abundant in the deepest parts. Still, as well-developed members of all the marine invertebrate classes are found at all depths, their distribution does not appear primarily connected with any of the conditions immediately depending upon depth. It appears, however, that the oceanic fauna is chiefly confined to two strata; the one on and near the surface, and the other on or near the bottom. In the intermediate stratum the larger animal forms, whether vertebrate or invertebrate, are almost, if not entirely, wanting. The chief invertebrate marine groups, though all represented in the fauna of the abyss, occur in peculiar proportions. Mollusca (shell-fish), brachyurous Crustaceans, and Annelida are scarce, whilst Echinodermata and Porifera were largely preponderate. Below 500 fathoms the fauna presents the same general features in all parts of the world. Deep-sea genera are either cosmopolitan in their distribution, or if differing in distant localities are to a marked degree mutually representative. It was anticipated that the deep-sea fauna would be more closely related than the littoral fauna to the faunæ of the secondary and tertiary periods. This expectation has been realised to a certain extent, though the number of types discovered which had been supposed extinct is not large. The most characteristic deep-sea forms, including those most closely related to extinct types were found in the greatest abundance and in the largest size in the Southern ocean, and the "general character of the faunæ of the Atlantic and of the Pacific gives the impression that the migration of species has taken place in a northerly direction." This capital observation, we must remark, confirms the converse conclusion formed as to the distribution of land-animals, which are supposed to have spread from the northern hemisphere southwards. In other words, terrestrial forms have originated in and have been distributed from the greatest mass of land, just as marine forms have originated in and have spread out from the greatest mass of water. The deep-sea fauna generally, as might be expected, resembles the littoral fauna of high latitudes, the physical conditions to which it is exposed being nearly identical. A new order of animals approaching to but still distinct from the Radiolarians has been discovered and has received the name "Challengerida."

No less important are the physical and hydrographical results. We find indeed, in the present volumes, no observations throwing any light on former westward extensions of the eastern continent or eastward prolongations of America. The question whether Atlantis was a large island south-westward of the Canaries, or a large and level tract reaching to the east of the Lesser Antilles, according to Mr. Belt's suggestion, remains precisely where it was. It is indeed remarked that the Açores form the culminating points of a plateau of considerable extent, and that the Bermudas present the appearance of an "atol." But it may now be considered as definitely established that the bed of the Atlantic is divided by an axial ridge, and its branches into three basins—the eastern, extending from the West of Ireland southwards nearly to the Cape of Good Hope, with an average median depth of 2500 fathoms; the north-western, occupying the great eastern bight of the American continent, and having the average depth of 3000 fathoms; and a third opening southwards and running along the coast of South America to Cape Orange, and having also an average depth of 3000 fathoms. The Atlantic, and even the North Pacific, may be, in fact, regarded as mere gulfs or prolongations of the great preponderating water-mass of the southern hemisphere, from which a cold under-current is steadily flowing northwards. This arrangement depends on the circumstance that the rainfall in the Southern sea is in excess of evaporation, whilst in the North Atlantic and Pacific the reverse rule holds good. The temperature of the Atlantic does not appear sensibly affected by any influx of cold water from the Arctic Sea. The greatest depth found in the Atlantic—3875 fathoms, an abyss in which Chimborazo would be "taken overhead"—was observed a little to the north of the Virgin Islands.

At all depths between 400 and 2000 fathoms the bottom of the Atlantic—save where the *débris* brought down by rivers or due to the disintegration of rocks interferes—is covered with "Globigerina-ooze," a calcareous deposit mainly produced by the shells of Foraminifera, but mixed with fragments of pumice, felspar, sanidine, angite, hornblende, quartz, leucite, and magnetite, generally in very small particles. As the globigerina-ooze is decomposed, it gradually passes into so-called "red clay," more or less homogeneous, which covers the floor of the deep depressions below the 2000-fathom level. In this substance recognisable mineral fragments were found in greater abundance. The pieces of pumice were most numerous in the neighbourhood of well-known volcanic centres, such as the Açores and Philippines, and they are supposed to be all due to sub-aërial volcanic action. It does not appear easy to identify any of the beds now in course of formation at the bottom of the ocean with the rocks belonging to any of the past geological periods. The rocks of the Mesozoic and Kainozoic series seem to have been formed in shallow water. The author believes, however, that the globigerina-ooze, if elevated and slightly metamorphosed, would yield a limestone very closely resembling a bed of grey chalk, whilst the "red clay," under similar circumstances, might approximate closely to one of the palæozoic schists. He does not wish, however, to offer a definite opinion until the comparative chemical and microscopic examination of the specimens dredged up is completed.

The volumes before us must produce a favourable impression upon every candid reader. Though necessarily "dry" in parts, they are perfectly free from any gratuitous parade of technicalities, as well as from "fine writing," and, above all, from any obtrusion of the author's personality. Sir Wyville Thomson sees, and tells us what he sees, without "posing" for our admiration. The work is also not like too many records of voyage and travel, ballasted with irrelevancies. If we further bear in mind that we have here merely an instalment, and that probably the less interesting part of the work done by the "Challenger," we must pronounce the results of the highest service to Science, and creditable to the Government which sent out the Expedition, to the illustrious Society by which it was planned, and to the eminent men by whom so arduous a task was successfully executed.

The Different Forms of Flowers or Plants of the Same Species. By Charles Darwin, F.R.S. London: Murray.

Though modestly proclaiming himself no botanist, Mr. Darwin continues to produce botanical researches whose sterling value must be admitted even by the most determined adherents of the old school of Natural History. The work before us treats of some very interesting points connected with the reproduction of plants. A certain number of vegetable species were grouped together by Linnæus as hermaphrodites, and amongst these are a class which Mr. Darwin studied and described some years ago as "dimorphic" and "trimorphic," and which have since been named "hetero-styled" by Hildebrand. In plants of this class there are individual flowers of two, or in other cases of three, forms, differing principally in the relative length of the pistils and stamens. A familiar instance may be found in the common cowslip, polyanthus, and auricula. In some of these flowers the globular stigma appears at the mouth of the corolla, whilst in others the stigma does not protrude, and in its stead appear the anthers as an annular tuft. These different forms of the flower are named by florists respectively "pin-eyed" and "thrum-eyed." The former type is named by our author the long-styled, and the latter the short-styled. These two kinds of flowers are never found upon one and the same plant. Mr. Darwin has investigated the meaning of this diversity, and finds that it is by no means accidental or unimportant. If the long-styled form is fecundated by the pollen of the short-styled form, or *vice versa*, we have complete fertility; the seed is abundant and good, and the author accordingly speaks of this as a "legitimate union." If, on the other hand, a long-styled flower is fertilised by the pollen of a long-styled flower, or if a short-styled flower is fecundated by the pollen of a short-styled flower, we have two cases of "illegitimate union," in which the seed produced is not merely much less in quantity, but inferior in quality.

There are other plants, again, in which we find not two, but three different forms, as in certain species of *Lythrum*, *Nesæa*, *Oxalis*, and *Pontederia*. Here, again, we mark the same difference between the effects of legitimate and of illegitimate union. The author remarks that there is a wonderfully close parallellism between the effects of illegitimate and of hybrid fertilisation. "It is hardly an exaggeration to assert that seedlings from an illegitimately fertilised hetero-styled plant are hybrids formed within the limits of one and the same species. This conclusion is important, for we thus learn that the difficulty in sexually uniting two organic forms, and the sterility of their offspring, afford no sure criterion of so-called specific distinctness. If anyone were to cross two varieties of the same forms of *Lythrum* or *Primula* for the sake of ascertaining whether they were specifically distinct, and he found that they could be united only with some difficulty, that their offspring were extremely sterile, and that the parents and their offspring resembled in a whole series of relations crossed species and their hybrid offspring, he might maintain that his varieties had been proved to be good and true species, but he would be completely deceived. In the second place, as the forms of the same trimorphic or dimorphic hetero-styled species are obviously identical in general structure with the exception of the reproductive organs, and as they are identical in general constitution (for they live under precisely the same conditions), the sterility of their illegitimate unions, and that of their illegitimate offspring, must depend exclusively on the nature of the sexual elements, and on their incompatibility for uniting in a particular manner. And as we have just seen that distinct species when crossed resemble in a whole series of relations the forms of one and the same species when illegitimately united, we are led to conclude that the sterility of the former must likewise depend exclusively on the incompatible nature of their sexual elements, and not on any general difference in constitution or structure. We are, indeed, led to this same conclusion by the impossibility of detecting any differences sufficient to account for certain species crossing with the greatest ease, whilst other closely allied species cannot be crossed, or can be crossed only with extreme difficulty. We are led to this conclusion still more forcibly by considering the great difference which often exists in the facility of crossing reciprocally the same species, for it is manifest in this case that the result must depend on the nature of the sexual elements, the male element of the one species acting freely on the female element of the other, but not so in a reversed direction. And now we see that this same conclusion is independently and strongly fortified by the consideration of the illegitimate unions of trimorphic and dimorphic hetero-styled plants.

It can scarcely be denied that these latest researches of Mr. Darwin have dealt a most serious, if not an absolutely fatal, blow at the so-called physiological test of species, and consequently at the line of absolute demarcation by which some naturalists still consider species—as distinct from varieties—to be bounded.

The Methods of Physical Science. A Lecture delivered at University College, Bristol, as Introductory to the Course of 1877-78. By SYLVANUS THOMPSON, B.Sc., &c. Bristol: T. Kerslake and Co. London: Longmans and Co.

WE have here a lecture which brings forward not one novel fact, and probably no conclusion which has not been arrived at and pointed out before, but which is yet of exceeding value as clearly and authoritatively summing up the methods by which alone scientific truth may be attained. These methods are too apt to be sneered at by mere paper philosophers, the professors of the so-called "Wissenschaft in der phrase;" they are practically overlooked by the young, the inexperienced, and the superficial, who have yet to learn the great lesson of thoroughness. Hence it is most appropriate that they should be fully explained and enforced at such a time as the opening of a course of study in a new institution for higher—and we hope for scientific—education.

Mr. Thompson naturally addresses himself to the student of physics or of chemistry, but he says little, if anything, which has not an equal claim of the biologist or the geologist, in short, of all who in any department are engaged in questioning nature.

The lecturer takes for his text, if we may use the expression, the "Rules of Philosophising" laid down by Newton in his "Principia."

I. "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances."

II. "To the same natural effects, therefore, we must, as far as possible, assign the same causes."

A striking example of the breach of these two rules is given by certain biologists in their attempts to deny intelligence to the lower animals. If a man acts in a certain manner to attain a given end, they declare he has been guided by intelligence, but if a fox or an ape proceeds in a manner totally analogous they then ascribe his conduct to *quasi*-intelligence, and thus assign different causes to the same natural effects.

The author refers to a "certain note of dissatisfaction at the methods of modern science, and that in quarters where it cannot be passed by; a dissatisfaction amounting almost to disgust. This "revolt from the experimental method" is manifested in the writings of Hegel, and finds an echo in a passage quoted by the author from Buckle's "History of Civilisation" (vol. iii., p. 379). We may even doubt whether Comte can be altogether acquitted of having given utterance to similar views. This same tendency to undervalue experiment and observation and to return to the vicious methods pursued in the dark ages may be traced in an advice given in a certain quarter to naturalists that they ought by all means to study metaphysics. But it is not from open attacks upon the inductive method that we have the most to fear. It is manifestly not merely the right but the duty of science to subject every phenomenon to the test of experimental study. No matter how difficult the question, or how much it may have been complicated by imposture, folly, and superstition, this obligation still remains. Yet we have men of science, so-called, who rest satisfied with superficial perfunctory researches, mere apologies for a foregone conclusion, and who, on the faith of their own "educated common sense," adjudicate not merely theories or hypotheses, but facts. What is still worse, these men make it their business to ridicule and disparage the earnest enquirer who is searching for the pearls which they trample under foot.*

Mr. Thompson gives a survey of "some of the more special methods of physical science which have been of service in the acquisition of facts." He enumerates—*Casual Observation*, *Methods of Comparison*, *Methods of Precision*, *Methods of Analogy*, *Methods of Hypothesis, and Residual Methods*, each of these being explained by reference to cases of its successful application. With the last-mentioned method he connects the names of Boyle, Wollaston, Faraday, and Brewster in the past, and that of Mr. Crookes in the present.

"As a mental and moral training," the author observes," "the pursuit of the scientific method is priceless." "He who will be warped by prejudice, by passion, or by fear cannot investigate nature rightly."

The students of the Bristol University College may well be congratulated on a teacher whose conceptions of the methods and the duties of science are so clear, so wide, and so lofty.

The Canadian Journal of Science, Literature, and History. Vol. XV., No. VI., July, 1877. Toronto: Copp, Clark, and Co.

This issue opens with an interesting paper on "Left-handedness," by Dr. D. Wilson, in which the important question is raised whether any phenomenon corresponding to the preferential use of the right hand in man can be traced in the inferior animals and especially in apes. It is curious that the handle of a bronze sickle found at the lake-dwellings of Möringen, on the Lake of Brienne, in Switzerland, is adapted for use with the right hand. But the deer-horn picks recovered from the "Grime's Graves" flint pits are not invariably right-handed. "Left-handedness," the author considers, "is inherited and transmitted, though in an irregular manner and with varying intensity; that the range of the influence, to whatever source we may trace it, affects other organs of the same side only partially and uncertainly." He declares that with the majority of mankind the left hand is systematically reduced to the condition of a comparatively useless member. He does not, however, give sufficient prominence to the fact that this uselessness among right-handed persons varies very considerably in degree. We have met with persons who, without any previous training, could on the very first attempt write legibly with the left hand, whilst others failed completely even after many trials.

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* This treatment of truth is not, it appears, confined to the *pachy*-dermata.

There is a valuable synopsis of the Flora of the Valley of the St. Lawrence and of the Great Lakes, by Dr. J. Macoun, the Botanist to the Canadian Geological Survey, giving the localities of each species and descriptions of the rarer plants.

Proteus, or Unity in Nature. By C. B. Radcliffe, M.D. Second Edition. London: Macmillan and Co.

We do not wish to be irreverent, but this work irresistibly reminds us of an old wooden frigate hastily converted into an iron-clad in some real or fancied emergency. "Substantially," as the author tells us in his mythological and mystic introduction, "a second and much enlarged edition of a very immature work published under the same title more than twenty-five years ago," it is now refitted, and sent out to cruise against the doctrine of Evolution. He considers that much of what he has to say is likely to find "little favour in a materialistic age like the present." Has he yet to learn that between Evolutionism and Materialism there is no necessary connection? He gives us "traces of unity" in plants, in the limbs of vertebrate animals, in the appendicular organs of invertebrate animals, in the skull and vertebral column, in the animal as a whole, in plants and animals, in organic and inorganic forms. Passing from form to force, he lays before us traces of unity also in the various modes of physical force, in vital and physical motion, in the "vivifying power of light and heat," in the phenomena of "instinct," of memory, of imagination, volition, and intelligence, as well as in the personal, social, and religious life of man. All this is worked out with abundance of learning, with references to and quotations from numerous authors of all ages, though also with an obvious leaning to far-fetched analogies which almost reminds us of G. H. v. Schubert, or of Heinrich Steffens. But accepting in substance what Dr. Radcliffe has to urge, and without at all seeking to impair its value by facts of an opposite tendency, we have to inform him that this very unity is confirmation strong of the doctrine of Evolution! It is precisely what we should expect if the whole organic world had been developed from some common primitive form. It is precisely what we should not expect if each plant and each animal were of independent origin. Let us take a parallel case: the chemical unity of the heavenly bodies, as shown almost demonstrably by spectroscopic research and by the analysis of meteorites, is justly considered as a valuable piece of evidence in favour of the "nebular hypothesis," *i.e.*, the evolution of suns and planets out of one common material. It is strange to find an author, after elaborately collecting instances of unity in creation, breakind down the barriers between the plant and the animal,—a task already accomplished, —and even attacking that "which has been erected between the domains of organic and inorganic nature;" it is strange, we say, to find such an author suddenly arrested by the dim and evanescent boundary line which alone lies between those respective groups which we call species, and which he considers as primordially distinct. He declares, indeed, that "the doctrine of unity in diversity, and diversity in unity, has little or no bearing, direct or indirect, upon the doctrine of Evolution." If so, why does he append his attack upon Evolution to a work devoted to pointing out "traces of unity?" His only argument against Organic Evolution, beyond his declared ignorance of "any evidence which ought to lead to a different conclusion" seems to lie in the following passage:—"And what other conclusion can be drawn from the infertility of mules than this—that there *is* a barrier between different *species*, even between those which are most closely akin to each other, by which they are kept apart most effectually." That such lines can still be written by a man of learning is to us a mystery. Shall we never be delivered from the errors deduced from that unfortunate fact that the best-known hybrid, the mule, happens to be barren? The fertility of the léporide—the hybrid between the hare and the rabbit—has been recently most fully confirmed in a paper in "Les Mondes," an organ bitterly hostile to Evolutionism. In a recent and valuable monograph on the American bison we have satisfactory evidence that this animal can produce permanently fertile hybrids with the domestic cow; yet the two belong not merely to distinct species, but to distinct genera! But to any candid enquirer we think the recent researches of Mr. Darwin on the reproduction of plants must supply full proof that the difficulty of producing hybrids, and their infertility where it exists, is due to causes very different from a supposed barrier between species, and supplies no valid argument against Evolution. Dr. Radcliffe concludes that "each creature was created as a necessary part of a great whole, perfect in itself, and perfect in its relations to other creatures and to the universe to which it belongs!" But he gives us no proof of this alleged perfection, no standard by which it can be judged or recognised. If each animal was necessary, why do we see so many blanks in the animal series which have not been filled up? Why do strange plants and animals, artificially introduced into any country, often displace its original flora and fauna? In fact, there is scarcely a phenomenon connected with animal geography, or with the powers and functions of animals, which can be fairly reconciled with the creed of the old Natural History as here summarised by our author.

We have no sympathy with materialists, but we cannot forget that every important step in the development of science has been met with charges of materialism and atheism. From all we can gather from this work, its author, both by disposition and by the character of his studies, is better fitted to deal with words or with dreams than with things, and would shine much more in fanciful interpretations of myths than in the sober domains of Natural Science.

The Geological Record for 1875. An Account of Works on Geology, Mineralogy, and Palæontology, published during the Year. Edited by W. WHITAKER, F.G.S. (of the Geological Survey of England). London: Taylor and Francis.

IN few sciences is an annual record or general summary of work done so needful as in geology. So wide and so manifold is the subject, so closely blended with physics and chemistry upon one side and with biology upon the other, and so numerous are the organs in which matter of geological interest may first see the light, that without some journal like the one before us it is scarcely possible for the student to keep himself abreast of the current of discovery. We hope, therefore, that there may be no question as to the success of so necessary an undertaking.

The information given is arranged under the following heads:—Stratigraphical and Descriptive Geology, Physical Geology, Applied and Economic Geology, Petrology, Mineralogy, Palæontology, Maps and Sections, Miscellaneous and General. Most of these classes are again subdivided, and each such division is placed in the hands of a responsible sub-editor, To extract even a tithe of the interesting matter contained in the work would be of course impossible, but we may, as specimens, lay a few fragments before our readers. G. Krefft, in his "Remarks on Prof. Owen's Arrangement of the Fossil Kangaroos," considers that the whole of the Australian marsupials, extinct and living, are descendants of an extinct animal comprising the dental structure both of the carnivorous and herbivorous sections of the sub-order, and of which *Thylacoleo* was the last representative.

O. C. Marsh, in a paper on a "New Order of Eocene Mammals" contained in the "American Journal" (vol. ix., p. 221), gives an account of the Tillotherium, which combines the characters of Carnivora, Ungulata, and Rodentia. This is another of those discoveries which so powerfully support the doctrine of Evolution, presenting, as they do, characters in combination which have been since separated out and distributed among different animal groups.

Rütimeyer's work, "Traces of Man in Swiss Interglacial Deposits" ("Verhand. Nat. Ges. Basel," vi., 333 to 342), describes wooden rods, artificially sharpened, and remains of coarse wickerwork, found in the interglacial lignite along with known extinct animal species (elephant, rhinoceros, cave-bear, *Bos primigenius*, &c.

Brief notice is taken of Principal Dawson's "Address on the Origin and History of Life on our Planet." The author concludes that "the introduction of new species of animals and plants has been a continuous process, not necessarily in the sense of the derivation of one species from another, but in the higher (?) sense of the continued operation of the cause or causes which introduced life at the first." This seems very like the demand for a continuous miracle. Against this view the objection urged against Darwin weighs with tenfold force. For if no one has witnessed an ape becoming a man, still less has anyone witnessed a new animal condensing out of the atmosphere, crystallising out of the waters, or emanating from the ground. Principal Dawson holds also that "Palæontology furnishes no direct evidence, perhaps never can furnish any, as to the actual transformation of one species into another, or as to the actual circumstances of the creation of a new species; but the drift of its evidence is to show that species come in *per saltum*, rather than by any slow and gradual process."

S. H. Scudder's important paper on "Fossil Lepidoptera" is somewhat severely abridged. We are told that the author "discusses the comparative age of fossil butterflies, the probable food-plants of Tertiary caterpillars, the present distribution of butterflies most nearly allied to the fossil forms, and the affinities of certain fossil insects which have been referred to butterflies," but there is given no summary of the conclusions arrived at.

The evidence of glaciation in India is becoming stronger, but the details bearing on this question, as well as interesting accounts of the coal-fields and the other mineral wealth of the Empire, are to be met with in the Reports of the Geological Survey of India.

The geology of Africa is still in a very imperfect state. The British provinces in the South ought at any rate to be thoroughly explored.

Mr. J. C. Brown has written an important memoir on the hydrology of South Africa, discussing the causes of its present aridity, and suggesting appropriate remedial measures.

The geology of Australia is engaging the attention of a greater number of explorers. Mr. J. Bonwick estimates the carboniferous area of New South Wales at 15,419 square miles, and expects that part of the comparatively unknown western interior of the colony may prove to be carboniferous. The coal-field of New South Wales and Queensland he considers to be the western edge of a larger area now under the Pacific.

A paper contributed by Mr. J. Goodhall to the "Transactions of the New Zealand Institute" is of great importance as regards the early existence of mankind—or at least of some tool-using animal—on our earth. During excavations in the city of Auckland a tree-stump was discovered in its natural position, upright, with roots penetrating the surrounding clay, and covered by about 25 feet of volcanic *débris*, consisting of stratified beds of ooze and volcanic ash adjacent to a volcanic centre. The clay in which the roots occur rests upon Tertiary rocks, and is 10 to 15 feet thick. The stump is said to be a "tea-tree" (*Manuka*), and to have been cut with a tool. The author holds that it proves the existence of man long before the period indicated by the traditions of the Maories of their advent in this island, and at a period before what is probably the oldest volcano in Auckland became extinct.

At Mercedes, near Buenos Ayres, human bones have been discovered at a depth of 4 metres in undisturbed Quaternary beds, mixed with burnt wood, earth, and flint-implements; also bones of about fifteen species of mammals, mostly extinct.

This interesting volume finishes with "Addenda," a "Supplement," and a "Postscript," which reminds us of the headings "Finally," "Lastly," and "To conclude," in a Puritan sermon.

Is Scientific Materialism Compatible with Dogmatic Theology? The Inaugural Address delivered before the Literary and Philosophical Society of Liverpool on the Opening of the Sixty-Seventh Session, October 1, 1877. By JOHN DRYSDALE, M.D., President of the Society, and author of "The Protoplastic Theory of Life." Liverpool: Adam Holden.

THE Literary and Philosophical Society of Liverpool, as we were on a former occasion compelled to remark, seems to have a strong propensity for the discussion of subjects which can scarcely be held to fall under its legitimate cognisance. Dr. Drysdale's able and interesting address may doubtless rank as "philosophical" in the more recent and etymologically more correct acceptation; but when the literary and philosophical societies of England were founded, and when their aims and functions were defined, the word "philosophical" was, we submit, used in a sense which now survives mainly in the phrase "philosophical instruments." In the first quarter of the present century, and even down to, say, the year 1840, the word "philosopher" was the exact equivalent of the German "Naturforscher," and of the recent American word "scientist." Hence were it not that the Society bears also the very vague name of "Literary," Dr. Drysdale's address could only be regarded as a breach of order. As it is the discussion of his views, in a strictly scientific organ, is scarcely within the limits of possibility.

The author answers his own question—the momentous character of which we do not seek to under estimate—in the affirmative. An evolutionist, a believer in the mere phenomenal character of life and mind, and a disbeliever—if we understand him aright—in teleology, at least as it has been hitherto taught; he accepts, "on supernatural authority, the knowledge of the existence of personal conscious-thinking beings other than man, and whose substance is non-material, and that man in a personal, conscious, and responsible state shall live again for ever." He shows that man's utmost power and skill applied to the interpretation of the Universe only leave him the choice between materialistic Pantheism, as expounded, *e.g.*, by Haeckel, Deism, and "negative Atheism.*" After pointing out the utterly unsatisfactory character of the two former hypotheses he declares his conviction that the third "is not incompatible with dogmatic theology, and not even unfavourable for the reception of it. Our attitude in this category may be compared to that of the humble publican who prostrates himself on the floor of the Temple, and cries aloud in agony, overwhelmed by the cruel and crushing power of natural laws, and the blank emptiness of all visible signs of the presence of God in nature. Is the cry to go up to Heaven for ever and no answer to be vouchsafed? No! a thousand times No! For from the depths of the unseen world the voice of the Almighty Himself has been heard, declaring His will and His nature and purpose, so far as seemed to Him good, and as we are fitted to comprehend. . . It is only in the external bearing of revelation, in contrast to science, that we are at present concerned, and therefore we must speak of it explicitly, as the special revelation bound up with the history of Moses and of Christ. Whatever may be the defects of historic evidence in comparison with the proofs, repeatable at will, of physical science, it is obviously all we have to depend on, and by it the whole of dogmatic religion as something apart from all other knowledge and truth must stand or fall. For the—to us—supernatural revelation is inseparably bound up with the historic truth of the persons and miracles of Moses and of Christ; the so-called internal evidence of the truth of the teachings of Christ, valuable as they may be, are wholly insufficient of themselves; and no rational thinker can accept the Gospels without accepting the miracles, which form an essential part the narrative, or rather, they are the essence of christianity itself. This is conclusively shown by the destructive criticism of the German school represented by Strauss and Baur. That the moral elements of Christianity could survive the destruction of the historic personality of Christ and the whole supernatural element

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* By this term we presume the author means not the dogmatic denial of a god, but merely a confession of inability to infer from the Universe the existence of a First Cause, personal, just, and benevolent.

is a mere chimera; the notion never had any rational foundation, and if any belief in it ever existed it must have been dispelled by Strauss's last book. It is only, therefore, as a supernatural element of actual knowledge that Christian dogmatics can take a place in our knowledge, and prevail over, or rather, supplement the conclusions of scientific materialism. Without the supernatural element there is, as far as I can see, no choice except between Pantheism and negative Atheism; for the flimsy barrier of Deism is swept away at once whenever the hold on the supernatural is abandoned."

Now, we are certainly not prepared to say that Dr. Drysdale's views in this respect are not here and there open to criticism, but they involve, at least, no fundamental absurdity, and it will be worth carefully considering whether they do not open out the only possible way of escape from the difficulties which we encounter whenever—as none of us can help at times attempting—we would rise from the details of research to first principles. It must be confessed that the Universe, as we now behold it with the eye of unaided reason, is a ghastly spectacle. Time was when we fancied we saw in the woods and the fields realms of peace and joy. We knew, of course, that the hawk and the weasel were preying upon the small birds, that the spider was sucking the life-juices of the fly, and that the ichneumon larva was feasting among the vitals of the caterpillar. But all this seemed merely an exceptional disturbance of the general bliss. Now all this is changed; whether Natural Selection be a true cause of the formation of species or not; nay, whether species have been evolved at all, or primordially created, we cannot get rid of the Struggle for Existence, which, like Lytton-Bulwer's "Dweller on the Threshold," haunts every man whose eyes have been opened. Well might Winwood Reade, quailing before this phantom, and finding no solution to the mysteries of the Universe, exclaim "Creation is murder."

We need scarcely say that Dr. Drysdale, in accepting the Christian revelation as fully compatible with the results of the latest advances in science, makes the necessary reservation, old as the days of Galileo, that the Scriptures are not a code of geology, physics, biology, or psychology. This simple principle that where matters of observation and experience are touched upon Revelation speaks a merely popular language—with the same right as the most precise of us all still speaks of the sun's rising and setting—does away with the necessity of all attempts to reconcile "geology and Genesis," and at the same time cuts away the ground from beneath those who contend for the rejection of revelation on the ground of its technical inaccuracies.

We have no doubt Dr. Drysdale will encounter some rough usage. Those who presume to say that the object of Evolutionism is to undermine religion will doubtless brand him as an infidel. On the other hand, he will be denounced as an abject slave of superstition by such men as Posner, who, in a passage here quoted, declares that we "must deny God, and trample the Cross under foot, before we can become even scholars, far less masters in Natural Science." With this vicious and unphilosophical outbreak Dr. Drysdale deals with just severity. He admits that thoughts of First Causes and teleological ideas "tend to check the progress of investigation at numerous points." The fact that the majority of scientific men are not religious in the conventional and restricted sense of the term cannot, he thinks, be denied. But this adds very little strength to the fanatically intolerant position of Posner, since "the field of science is now so enormous that a man must not only give himself up wholly to it, but even to a very small part of it, in order to make any new conquest for the domain of knowledge. Hence even an incapacity to judge of religious truths." That there is no essential antagonism between religion and science the author considers is proved "by a long roll of illustrious names, from Newton to Faraday." To all the numeroust emptations to premature hypothesis and baseless speculation the infidel is at least as open as the believer. "For it is possible for the latter to free his mind from all bias, and push back creative interference as far as the search for phenomenal causes can possibly demand, and then he is in a better position for calm judgment than the fanatical infidel-evolutionist, for example, who is compelled to find a natural origin of life, and thus is tempted to fantastic speculation on the spontaneous generation of completely organised living beings with a life-history in a few hours from fermenting chemical compounds. Even on the descent of man the believer can rise to a calm and more unprejudiced standpoint than the Häckelian." Our author also thinks that the moral causes of mental disturbance must be taken into account. He remarks that "envy, jealousy, hatred, and prejudice are as rife among men of science as among other men, and these dim the pure love of truth, which is the essential condition of all discovery in science. Where are the qualities to counteract these most likely to be found? Surely among those desiring honesty, honour, truth, love, esteem, veneration, and beauty, to which the highest ideal is presented in Christian dogmatics!" Inverting Posner's position our author asks: "May we not, therefore, say, No one can truly become a master in science unless he first takes up the Cross and blends indissolubly the perfect love of truth, as a moral duty, with the love of truth in nature, which is the foundation of all scientific method?"

United States Geological Survey. Miscellaneous Publications. No. 8. *Fur-bearing Animals: A Monograph of North American Mustelidæ.* By ELLIOT COUES. Washington: Government Printing Office.

WE have here a most valuable monograph of a group of animals not merely important on account of the furs they yield, but interesting to the naturalist from many strange points in their character and habits. Our notice is first claimed by that most unsavoury beast the skunk, *Mephitis mephitica.* The genus *Mephitis*, and the American species in particular, their outward and inward structure, geographical distribution, and habits are described with elaborate care. The offensive secretion whose horrible properties have not been exaggerated by popular tradition, and the excretory organs or "atomisers" for its ejection and distribution in the atmosphere, have been examined by Dr. J. M. Warren, who, undeterred alike by the dreadful odour, and by the possibility of anti-vivisectionist denunciations, examined the glands in a living specimen which had been placed under the influence of chloric ether. The fluid does not appear to have been hitherto submitted to chemical examination, but we trust that before long some enterprising student will ascertain its properties and its "constitution." It is described as being phosphorescent, of an altogether indescribable odour, at once pungent, penetrating, and intensely nauseating. Both men and dogs have been permanently blinded in consequence of a drop having come in contact with the eye-ball. According to Sir John Richardson a dead skunk, thrown over the stockades of a trading post, produced instant nausea in several women in a house with closed doors upwards of a hundred yards distant. The vulgar notion that the offensive liquid is the urine of the animal is utterly unfounded. As regards the distance to which this fluid can be propelled the author remarks that "the squirt reaches several (authors say from four to fourteen) feet, while the *aura* is readily perceptible at distances to be best expressed in fractions of a mile.

The authority and the reasoning of Waterton, notwithstanding, there is no doubt but that this creature is fully aware of the offensiveness of its secretion to other species and uses it as a defensive weapon. "Its heedless familiarity, its temerity in pushing into places which other animals instinctively shun as dangerous, and its indisposition to seek safety by hasty retreat, are evident results of its confidence in the extraordinary means of defence with which it is provided." In the woods of Nicaragua Mr. Belt repeatedly observed the calm assurance of the skunk, who "goes leisurely along holding up his white tail as a danger-flag for none to come within the range of his nauseous artillery."

But this animal, it is now known, possesses a far more dangerous attribute than that of the emission of an execrable odour, but which certainly contributes nothing to his defence. His bite is usually fatal, inducing a disease very similar to if not identical with canine rabies. One remarkable point is that the skunk possesses this power, not when himself attacked with a fatal disease, as is the case with the dog, but when apparently in a normal state of health. Some unsolved questions here present themselves. *Rabies* is said to be not uncommon among wolves on the European Continent, and very general among jackals in India. Do these animals invariably, or even commonly, die of the disease, or are they, like the skunk, capable of inflicting a poisoned bite when in good health? Again, is there any authenticated case of idiopathic hydrophobia in a cat or in any other feline animal? The skunk displays what may be called gratuitous malignity. Its bite is inflicted not in defence of itself or its young, but without any provocation and without any prospect of advantage. A trapper or a traveller may be asleep on the ground of his camp-fire when a prowling skunk will stealthily approach the sleeper and bite the lobe of his ear or his little finger. Under such circumstances as this the majority of the cases of skunk-bite occur, and there is only one recorded instance which has not terminated fatally. *Mephitis gorilla*, the Californian species, is equally to be dreaded. There almost seems to exist among the Mustelidæ a "superfluity of naughtiness" such as no other of the lower animals display, but which are shown by the ermine, the stoat, and the pekan in their wholesale slaughter of birds, &c., far more than they require for food, and in the useless mischief committed by the wolverine or glutton. Of the extraordinary intelligence and thievish propensities of this strange animal we find here many most surprising but apparently authentic instances. We are of opinion that no one can read the work of Dr. Coues without finding a new light dawn upon him both as o the intellectual and the moral faculties of "brutes."

Bulletin of the United States Entomological Commission. On the Natural History of the Rocky Mountain Locust and on the Habits of the young or unfledged Insects as they occur in the more fertile country in which they will hatch the present year. No. 2. Washington: Government Printing Office.

This report, drawn up by the Commissioners, Messrs. C. V. Riley, A. S. Packard, Jr., and Cyrus Thomas, gives a full account of the habits of the *Caloptenus spretus*, the Rocky Mountain Locust, so as to facilitate arrangements for their destruction. The deposition of the eggs, their curious arrangement in the soil, and the method in which the young locust ruptures the shell are clearly described and figured. An appended map shows the country east of the Rocky Mountains which has been already overrun, and from which they will too probably spread still further to the eastward. The best remedy, in our opinion, would be the introduction of live hedges and hedgerow trees capable of affording abundant cover to insectivorous birds in place of the rail and wire fences so common in the Western States, and which find their thoughtless advocates in England.

A Guide to the Determination of Rocks; being an Introduction to Lithology. By EDWARD JANNETTAZ. Translated from the French by G. W. PLYMPTON, C.E., A.M. New York: D. van Nostrand.

THIS work, in its original French form, came under our notice some time ago ("Quarterly Journal of Science," v., p. 109), on which occasion we ventured to pronounce it a not unsuccessful attempt at supplying an important deficiency in geological literature. Until the student is able to identify with moderate accuracy the rocks which he meets with he has not yet learnt the alphabet of the science, and is unable either to make original observations or to verify the researches of others. Any work which may enable him to recognise the different rocks he sees when out on a geological ramble contributes something towards transforming him from a mere reader into a worker in the science. It is very true that the determination of geological species is not always easy,—instance the discrimination of granite and gneiss. Still, if the student fixes in his mind the chemical and physical characters of the rocks, or rather of their chief mineral constituents as here laid down, and continually applies this knowledge in practice, he will find the difficulty not insurmountable.

The first part of the book gives the properties of the principal minerals which compose rocks. The second is devoted to a description of the rocks themselves; whilst the third lays down systematic procedures for the practical determination of any rock which falls into the hands of the student. Part fourth, the appendix, translated from M. Stanislas Meunier's "Cours Elementaire de Geologie Appliquée," consists of a dichotomic system for the determination of rocks. Having no respect for dichotomy as a key to classification, we must yet admit its extreme value for the diagnosis of species.

It may not be out of place to notice the exceedingly elegant binding of this little volume.

CORRESPONDENCE.

THE HALO IN THE ZENITH, AND THE HALO OF 90°.

By the Rev. S. Barber, F.M.S.

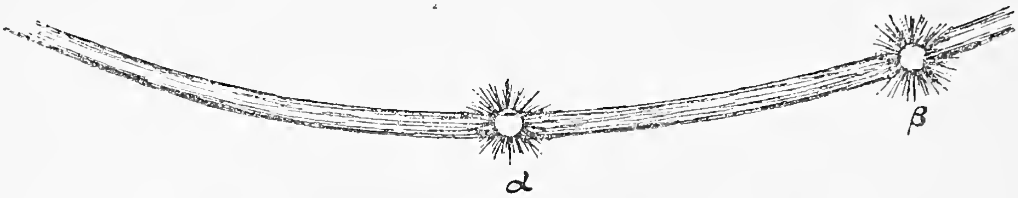
Sir,—Both of these rare phenomena having occurred during the present month (September, 1877) a brief notice is now given by an observer in the neighbourhood of Eythorne, near Sandwich, Kent.

The halo in the zenith was seen on September 8th, from about 4.30 to 5.30 p.m. The weather was fine, and the sky marked by a number of bands and patches of cirrhus cloud. A single parhelion appeared, in an opening of the clouds, where the sky was clear. This was distinctly chromatic, and globular in form. It lay to the north of the sun.

The peculiarity in this appearance was in the bright colouring of the halo (which was, indeed, almost as distinct as that of a rainbow) and in the character of the originating medium. It is well known that the cirrhus cloud—consisting, as it does, of snow-crystals floating at a considerable elevation—often gives rise to halos and prismatic bands of light. On this occasion the line of the halo was intersected by well-defined bands of cirrhus, lying almost parallel to one another. There was no break in the arc of the halo, which was as visible upon the blue sky as on the cloud. From this observation we may make the inference that the actual limits of the cloud constituents are not always confined to the visible limits of the cloud itself. This perhaps may be thought to be only another way of asserting the fact that, even in an apparently clear sky, there may be above us vast quantities of snow crystals and vapour particles.

In regard to the weather succeeding this appearance, we may note that within forty-eight hours there were heavy showers, and one very violent squall.

The halo of 90°, which was seen from the same neighbourhood as the preceding, occurred at about 12.30 on the morning of Tuesday, September 25th. This was also attended by a single mock-sun, through which the arc of the halo passed (see figure). This line also passed through the sun itself,—or, rather, seemed to branch out from the sun, on either side, and made a circuit of the heavens. It was not prismatic, but of a grayish tint, and was broken in its outline, in the part of the sky opposite the sun, by patches of cirro cumulus.



Halo of 90° with Parhelion. α. The Sun. β. Parhelion.

It is remarkable that the weather following, in this case, was finer than had been experienced for some time previous to the manifestation.

Until October 8th, when there was a gale, the air was clear and fresh, and sunny during the day, with the usual autumn mists in the morning and evening (wind easterly for the most part). It may be remarked, however, as I have noted on previous occasions in this Journal and elsewhere ("Quart. Journ. of Science," Nos. 26 and 41, and "Nature," March

23, 1871), that optical phenomena of the kind here referred to indicate a transition state of the weather; and this inference has recently been again substantiated (October, 1877) by the occurrence, on two successive days, of imperfect parhelia at the end of a period of fine weather, and before the advent of storms and rain. That this is the true prognostic value of these appearances will scarcely be disputed now by meteorologists. How to account for the fact that ice-crystals become chiefly visible at these times of transition does not appear easy. It is well known that in the finest weather abundance of crystals may be found at high elevations, even with a clear sky; but it seems perfectly clear that optical phenomena do not always attend them.

I venture, therefore, to propose the following explanation of the fact that halos generally attend a " transition " state of the atmospheric vapour, viz., that *they result from the transmission of the solar rays through their substance while in a partially melted state*, or, rather, while slightly melted on the surface.

It must be observed that in dry frosty weather the longer ice remains exposed to atmospheric influences the more uneven its surface becomes, by the accretion of vapour upon its prominences, and consequently the less capable it becomes of giving a clear refraction and exhibiting chromatic effects. Now if we allow that, previous to the appearance of a halo or parhelion, the floating ice may have been present in this condition, a rise of temperature would produce the optical phenomena referred to, by making the refraction more perfect; and, on the other hand, a fall of temperature sufficient to freeze would have the same effect, by origination of new ice-prisms from existent watery vapour.—I am, &c.,

S. Barber.

Tilmanstone, Sandwich.

SCIENTIFIC NOTES.

The most important scientific achievement we have to record in the present number of the "Quarterly Journal of Science" is that of the liquefaction of oxygen, which was accomplished on the 22nd of December by M. Raoul Pictet. When a gas is compressed to 500 or 600 atmospheres and kept at a temperature of −100° or −140°, and it is allowed to expand to the atmospheric pressure, one of two things takes place—Either the gas, obeying the force of cohesion, liquefies and yields its heat of condensation to the portion of gas which expands and loses itself in the gaseous form; or, on the hypothesis that cohesion is not a general law, the gas must pass to the absolute zero and become inert,—that is to say, an impalpable powder. For some time past M. Pictet's object has been to demonstrate experimentally that molecular *cohesion* is a general property of bodies, to which there is no exception. The apparatus he employed in his experiments may be briefly described as follows: Two pumps, such as are used in M. Pictet's ice-making apparatus, are coupled in such a way that the exhaustion of one corresponds to the compression of the other. The exhaustion of the first commences with a tube 1·1 metre long and 12·5 centimetres in diameter, and filled with liquid sulphurous acid. Under the influence of a good vacuum the temperature of this liquid rapidly sinks to −65°, and even to −73°, the extreme limit allowed. Through this tube of sulphurous acid passes a second smaller tube, of 6 centimetres diameter and the same length as the envelope. These two tubes are terminated by a common base. In the central tube M. Pictet compressed carbonic acid produced by the reactions of hydrochloric acid on Carrara marble. This gas, being dried, is stored in an oil gasometer of 1 cubic metre capacity. At a pressure of from 4 to 6 atmospheres the carbonic acid easily liquefies under these circumstances. The resulting liquid is led into a long copper tube, 4 metres in length and 4 centimetres in diameter. Two pumps, coupled together like the first, exhaust carbonic acid either from the gasometer or from the long tube full of liquid carbonic acid. The ingress to these tubes is governed by a three-way tap. A screw valve cuts off at will the ingress of liquid carbonic acid in the long tube: it is situated between the condenser of carbonic acid and the long tube. When this screw valve is closed, and the two pumps draw the vapour from the liquid carbonic acid contained in the tube 4 metres long, the greatest possible lowering of temperature is produced, the carbonic acid solidifies, and descends to about −140°. The subtraction of heat is maintained by the working of the pumps, the cylinders of which take out 3 litres per stroke, and the speed is 100 revolutions a minute. In the interior of the carbonic acid tube passes a fourth tube, 5 metres long, intended for the compression of oxygen. Being immersed in solid carbonic acid, the whole surface is brought to the lowest obtainable temperature. These two long tubes are connected by the ends of the carbonic acid tube, consequently the small tube extends about 1 metre beyond the other. The small central tube screws into the neck of a large wrought-iron shell. This shell contains 700 grms. of chlorate of potash and 256 grms. of chloride of potassium mixed together, fused, then broken up, and introduced into the shell perfectly dry. When the double circulation of the sulphurous and carbonic acids has lowered the temperature to the required degree, the shell is heated over a series of gas-burners: the decomposition of the chlorate of potash takes place at first gradually, then rather suddenly towards the end of the operation. When the reaction is terminated the pressure exceeds 500 atmospheres; but it almost immediately sinks a little and stops at 320 atmospheres. If at this moment the screw tap which terminates the tube is opened, a jet of liquid is distinctly seen to escape with extreme violence. If the tap is closed a second jet can be obtained in the course of a few moments. If pieces of charcoal, slightly incandescent, are put in this jet they inflame spontaneously with great violence. M. Pictet has already announced his intention of attempting the condensation of hydrogen and nitrogen by means of a similar apparatus to that we have described We shall hope to give the results of his further experiments in a future number of this Journal. Meanwhile we would refer our readers to the "Chemical News" of January 4 for a detailed description and diagrams of the complicated apparatus, by means of which M. Pictet has succeeded in liquefying oxygen.

Mr. Charles Stodder, in the "American Journal of Microscopy" (vol. ii., page 142), recommends the use of chloroform as a preparation for mounting dried Algæ in balsam. The immersion in chloroform has the effect of restoring the Algæ to their natural shape: the portions selected are then placed on a slide, and just before the chloroform evaporates the balsam is dropped on, and the object covered in the usual manner. The balsam should be allowed to harden slowly, as if heat is applied the specimen is liable to be shrivelled. The process is of especial value in mounting diatoms *in situ.*

The "American Journal of Microscopy" (vol. ii., p. 129) contains some remarks on the examination of objectives, by Ernest Gundlach, which may prove interesting to microscopists. The way in which various forms of spherical and chromatic aberration render themselves apparent are familiarly described. The subjects of angular aperture and working distance are discussed, and it is shown that considerable resolving power is only obtainable at the sacrifice of some portion of the latter quality of the objective. The immersion system is shown to be a substitute for the extreme thickness of the front lens needed in correction, formed by a corresponding stratum of water, to gain with the highest performance a tolerable working distance.

Our readers have probably noticed a recent addition to our fictile manufactures of a number of ornamental vases, cups, bowls, &c., of clear white glass, covered with beautiful iridescent films of different colours. At first it was thought that the process consisted in submitting the glass to the action of a deoxidating flame, and that the colours were caused by the reduction of the lead; from the specification of the patent, however, we find that the inventor of the process is M. L. Clémandot, a French civil engineer, who has patented it in France, England, and America, and that the principle of the process consists in submitting the glass vessels to the action of dilute hydrochloric, sulphuric, or other acid, under a pressure of from two to six atmospheres. M. Clémandot claims to be able to imitate the beautiful nacreous films on ancient glass which has been submitted to the combined action of air and water for two or three thousand years; but the ornamental vessels already exhibited, although very pretty, are a long way off the poorest specimens of Assyrian or Egyptian glass in any ordinary collection. Time is evidently an important factor in bringing about this singular change.

The injurious influence of the products of combustion of coal-gas upon the leather bindings of books is only too well known. Vellum seems unaffected; morocco suffers least; calf is much injured, and Russia still more so. The disintegration is most rapid with books on the upper shelves of a library, whither the heated products of combustion ascend, and where they are absorbed and condensed. By comparing specimens of old leather with specimens of new it is quite clear that the destructive influence of gas is due mainly to its sulphur. True there are traces of sulphates in the dye and size of new leather bindings, but the quantity is insignificant, and there is practically no free sulphuric acid. That leather may be destroyed by the oil of vitriol produced by the burning of gas in a library is proved by the following observations and analyses of Prof. A. H. Church, of the Royal Agricultural College, Cirencester. The librarian of one of our public libraries forwarded to him the backs of several volumes which had been "shed" by the books on the upper shelves in an apartment lighted by gas. The leather of one of these backs (a volume of the "Archæologia") was carefully scraped off so as to avoid removing any paper or size from beneath. This task of scraping was easy enough, for the leather was reduced to the consistency of Scotch snuff. On analysis of the watery extract of this leather the following figures were obtained:—

Free sulphuric acid in decayed leather	6·21 p.c.
Combined ,, ,, ,, ...	2·21 ,,
Total ,, ,, ,, ...	8·42 ,,

Writing of rare minerals found in Colorado, Mr. T. F. Van Wagenen, in the "Engineering and Mining Journal," says that thallium, indium, and cadmium have lately been detected in ores from that state. Of the rarer metals there have been found in Colorado, besides the three mentioned above, nickel, cobalt, selenium, tellurium, uranium, bismuth, molybdenum, and platinum, and there is scarcely a doubt that columbium, thorium, titanium, and vanadium will be recognised as soon as proper search is made. A belt of telluretted veins is believed to traverse the entire state from north to south. Two years ago sylvanite and alaite were found in San Juan county. The principal locality for bismuth ores is in Geneva, where two mines are being worked that carry a considerable quantity of schirmerite. Sulphide and carbonate of bismuth occur on Sugar-loaf Mountain, Boulder county. Nuggets of native bismuth are common in the upper gulches of the Blue Valley; the same metal has been found also in the Arkansas Valley. Nickel ore, ranging from 2 to 5 per cent, has been found in three localities. Among the mineralogical curiosities of the tellurium belt may be mentioned a telluride of mercury found in the Mountain Lion Mine. Native mercury and amalgams of both gold and silver have also been found at several points along this belt.

We are likely soon to hear of columbium-plating, a metal which is very much like nickel, but whiter, like tin. It was first found, more than fifty years ago, in the United States, near Middletown, Conn., and named in honour of America. It was thus far very scarce, but has now been found at Marion, N.C., and also in the Amazon stone of Colorado. If it becomes abundant enough to be used in the arts we may soon have a valuable addition to the useful metals, and also to their compounds or salts. The metal forms an acid like tin, called columbic acid; tin forms stannic acid, and the compounds of this acid with bases are stannates, very useful in the arts; the compounds of columbic acid with bases must be called columbates. They have not yet been investigated, but will likely be found extremely useful for some purposes.

As a means of preventing the explosion of fire-damp in coal mines M. Basin recommends ventilation with compressed air, watering the galleries (in dry mines), and the use of Bastie's toughened glass instead of wire-gauze for the cylinders of safety lamps. The tube which gives vent to the products of combustion will contain several superimposed layers of gauze.

From the "Mineral Statistics of Victoria for the Year 1876" we learn that the total yield of gold for that year in this colony has been 357,901 ozs. from alluvial sources, and 605,859 ozs. from quartz rock. The total yield of silver was 26,356 ozs., the greater portion of which was parted from gold smelted at the Mint. The exports of tin were 34 tons 9 cwts. During the year 2529 tons of antimony ore were raised, and 606 tons of ore, 474 tons of regulus, and 254 tons of crude antimony were exported.

THE QUARTERLY

JOURNAL OF SCIENCE.

APRIL, 1878.

I. ECONOMY OF NITROGEN.

We have repeatedly heard the observation that chemists in complaining of "waste" are guilty of inconsistency. If matter, it was argued, is indestructible, and if no element can be converted—whether designedly or accidentally—into another, waste is simply an impossibility. Such censures are the natural result of an education based rather upon Literature than upon Science, and are in themselves of no moment. Still, as in England—unlike Germany—a man of general "culture" is considered entitled to lay down the law upon any subject whatsoever, and is often listened to with more attention than the specialist or the man of original research, it may be useful to point out the fallacy involved.

Waste, from a chemico-technical point of view, depends not on the fancied destruction of some element, nor on its transmutation into some other simple body, but on useful matter being thrown into a state where it is no longer immediately available for our purposes. It is thus, so to speak, locked up; withdrawn, for a longer or shorter time, from circulation, just like specie buried by a miser of the old school. Such transmutations may take place in various manners. A useful substance, whether simple or compound, may be brought into the state of a highly dilute solution, or may be mixed with solid matter in so minute a proportion as to be practically irrecoverable. Nature presents us with many instances of bodies, valuable, indeed, in themselves, but rendered useless by admixture with a vast excess of alien substances. We need only mention, as cases in point, the silver and iodine, whose presence has been demonstrated in sea-water, the gold in many varieties of quartz and pyrites, and the sulphur in coal. To obtain a shilling's worth of silver from sea-water costs more than a shilling, and the amount of precious metal thus dissolved in the ocean—immense as it doubtless is—may be, in our present state of knowledge, pronounced practically non-existent.

Now Art is constantly engaged in reducing useful substances into a similar condition. Every time a sovereign is handled in the course of business, or shaken against other coins in the purse or the cash-box, some small trace of gold is abraded. The quantity lost may be too trifling to be visible, and might even elude spectroscopic detection; but that loss does constantly take place we know, since by the mere continuance of such ordinary wear and tear the current coin of the realm ultimately becomes "light." The silver fork or spoon, the brass-work of the microscope and the balance, the leaden gutter or spout, the iron key, spade, or wheel-tire, are all in like manner gradually wearing away, losing their substance in particles so minute as to elude observation, and becoming thus distributed no one can say where. Nothing is indeed destroyed, but everything is becoming mixed. There may be natural processes by which all these microscopic films and fragments of metals, and other useful substances, are being sorted out and collected together, like to like, and are being formed into veins and beds of ore, &c., for future generations to extract and again bring into use; but such processes, if they exist at all, are assuredly very slow. We scatter more rapidly than Nature gathers. We are living beyond our income, and, except as far as some few of the commoner elements are concerned, we must ultimately, in the words of the old adage, "find the bottom of the bag."

Another form of waste consists in taking an elementary substance, useful in its free state or in some particular combination, and transmuting it into compounds no longer capable of the same applications. Of this class of waste the consumption of coal may serve as the type. Carbon and certain compounds of carbon and hydrogen combine with oxygen, the main resulting product being carbonic acid, accompanied, in the case of the hydrocarbons, with watery vapour. Now carbonic acid is certainly not useless in the economy of Nature, being one of the most important ingredients in the food of plants, but in our hands it is what the old alchemists called a *caput mortuum*—a residue which, economically speaking, must be pronounced intractable. There is no need to insist here upon the vast supply of this gas poured daily into the atmosphere from the combustion of coal and other fuel, from the respiration of animals, and from the fermentation and putrefaction of organic matter,—nor to show how during all these processes carbon, either free or existing in some organic combination, is being transformed into carbonic acid at an astonishing rate. That at the same time the carbonic acid of the atmosphere is being decomposed by growing vegetation, its oxygen set free, and its carbon assimilated,—re-converted into organic compounds,—is not capable of question. But is fuel being generated as rapidly as it is consumed? Evidently not; otherwise there would have been slight ground indeed for the outcry which was so injudiciously raised about the prospective exhaustion of our coal-fields. We say "injudiciously raised" because the warnings were at once seized hold of by the coal interest, and employed as a weapon for striking an almost fatal blow at our manufactures, our commerce, our national wealth, and our domestic comfort. The waste of carbon can be checked only by the sacrifice of that profligate source of warmth the "cheerful fire,"—by the substitution, wherever possible, of water-power and tidal-power, and in brighter climates of direct solar-power, for steam-power obtained by the use of fuel,—and, where no such substitution is possible, by penalties upon a consumption of coal greater per horse-power than what can be proved practically necessary.

Phosphorus is an element which we are wasting in a different manner; not by oxidation, for it is most useful in combination with oxygen, but like the metals, by extreme comminution and promiscuous distribution. Here the total supply existing in Nature is small compared with that of carbon. Though phosphorus plays, most fortunately, a comparatively insignificant part in our industrial operations, yet in the chemistry of life it is second in importance to none of the elements. Without phosphorus none, at any rate, of the higher organisms, animal or vegetable, can possibly exist. All our crops require a due supply of it, and being the constituent of plant-food of which most soils are soonest exhausted it becomes, upon the principle laid down by Liebig, the measure of their fertility. Amongst the methods in which this important element is wasted, a prominent place belongs to the manufacture of lucifer matches. Three hundred thousand lbs. of phosphorus are made yearly in England and France alone, nearly the whole of which is absorbed in the match-manufacture. More than ten years ago Wagner calculated the total annual production for the whole of Europe at about 540,000 lbs., requiring the consumption of about $6\frac{1}{2}$ millions lbs. of bones, which would otherwise be available as manure. If we assume that the skeleton of an average man, when dry, weighs 20 lbs., this weight would furnish the bony framework for the bodies of 325,000 human beings. As each match is struck, its proportion of phosphorus is converted into phosphoric acid. Such conversion does not necessarily imply waste, since this is precisely the form in which phosphorus enters into the food of plants. But the phosphoric acid is scattered abroad in such minute portions that we may well question whether much of it will, in any reasonable time, find its way to its legitimate place, our fields and gardens. If we duly weigh how much nutritive matter is thus in effect destroyed, and how much potential life is prevented, we may well question whether the invention of "lucifers" has been quite as great a boon to mankind as is sometimes represented by platform-orators, and whether the power of instantaneously obtaining a light at all times and in all places is not being, to say the least, somewhat dearly purchased. It is perfectly possible to make matches without the consumption of any phosphorus at all—a fact too much lost sight of and too little acted upon, but which will ultimately force itself upon the public attention.

Bone-black or animal charcoal is largely used for water-filters, in sugar-refining, in the manufacture of blacking, and as a pigment, and contains about 70 per cent of phosphate of lime. That portion applied for filtering and for sugar-refining finds its way in course of time to the manure manufacturer, and ultimately to the soil. The amount used for blacking and for pigments is evidently squandered about in traces far too minute to be again collected by any human agency. To protest against so long-established and general a custom as giving a black surface to coverings for the feet is of course a hopeless task; yet if we consider that any material coloured black radiates heat more strongly than the same material coated with any other colour, we must admit the folly both of blacking, as applied to boots and shoes, and of the original blackening of shoe-leather before it leaves the hands of the currier. Our universal practice can only be considered rational if we wish to have our feet as hot as possible in summer and as cold as possible in winter.

Bone-ashes consist, of course, chiefly of the phosphate of lime, and are mainly utilised in agriculture. But a small portion is used in making cupells for the assayer, and in the manufacture of opal glass. The phosphorus that enters into these compositions is doubtless lost,—certainly in the latter case,—but its quantity is not sufficiently large to render it worthy of especial attention. Of far more moment is the bone-ash wasted in refining argentiferous lead, no less than 17 lbs. being consumed for every ton of the metal operated upon. This is, if we refer to our former standard, a quantity nearly sufficient to build up the skeleton of a man. We are not aware that the bone-ash thus employed in refinery hearths is afterwards collected and purified from adherent traces of lead, so as to be fit for agricultural uses.

Many iron-ores contain phosphorus. Thus in vivianite, phosphoric acid may be present to the extent of 30 per cent. In a few instances this constituent is separated in an available form, but in the majority of cases it remains partly in the iron, to the great dissatisfaction of the manufacturer, who finds his product much impaired, and partly in the slags. Under both these circumstances it is generally wasted.

Phosphoric acid, combined with soda or potash, enters into the composition of certain "dung-substitutes" used by the calico-printer. Here, again, it must be regarded as ultimately wasted, since, when its purpose is served, it is washed into the sewers along with a variety of substances, many of them hostile to vegetable life, and therefore ill-fitted for application to the soil.

But even the phosphoric acid that is supplied to the fields in the forms of superphosphate, of bone-dust, Peruvian guano, farm-yard manure, night-soil, &c., and is there assimilated by the crops, is still afterwards in great danger of being turned aside from its normal channel of circulation, and thus of being substantially lost. Everyone in these days has learnt that animals, in their excreta,—solid, liquid, and aëriform,—give back all the matter which they have assimilated, and which has for a time formed part and parcel of their bodies. If the solid and liquid excreta, therefore, of all the animals fed upon the produce of a plot of land, of say 10 acres in size, together with all the waste or residual portions of the crops, are returned to that plot, its fertility—*i.e.*, its power of producing grain or vegetables, or of feeding cattle—will remain substantially unimpaired. But if this is neglected, or done only in part, a gradual decline of fertility must take place. We may, indeed, by importing bone-ash, Peruvian guano, and the like, keep up the needful supply of phosphoric acid in a 10-acre plot, or possibly in the whole of England; but this is, in familiar phrase, simply "robbing Peter to pay Paul." The whole accessible store of phosphoric acid on the face of the globe, and consequently the total quantity of wheat and of beef that can be produced and the number of human beings that can be maintained, is lessened by every gallon of sewage we pour into a tidal river or into the sea. To put the fact in another light: every stroke of the costly and ornate pumping-engines at Barking Creek and at Crossness is destroying potential life, or rendering existence more difficult throughout the world! Surely this is an outcome of engineering skill on which the nineteenth century has small cause for self-congratulation.

It is indeed true that the phosphoric acid thus so bountifully poured into the sea may be, in the lapse of years, recovered in the shape of fish, serviceable for food or for manure; but this is a long, a tedious, and a doubtful circulation. If the excreta of men and animals are placed upon arable soil, their valuable constituents are rapidly made available and assimilated by plants, and may re-appear within a twelvemonth in the shape of food; but if poured into the sea their phosphoric acid, &c., will probably be first absorbed by low organisms, animal or vegetable. These will sooner or later become the food of fishes, and the matter in question may thus ultimately become available for human support.

In one case indeed we may, on the present system, receive back sewage matters much sooner than we wish. The particles of solids cast into the sea "hug the shore," just as corks floating in a basin of water keep to the side, and there are swallowed by those "scavengers of ocean," shrimps. Thus they may happen to be re-eaten by man even *before* assimilation. A chemist of the present day, who occasionally writes on sanitary topics from an original and independent point of view, remarks that thus—

> ——— "Even-handed justice
> Commends the poisoned chalice to our lips."

A still more glaring example of waste occurs in the case of nitrogen, an element having important analogies with phosphorus, rivalling the latter in its relations to organic —and especially to animal—life, and playing a far more important and more varied part in the arts and manufactures. Manifold in appearance as are the methods in which this king of the elements is wasted, they may, with few exceptions, be reduced to one. To understand this great mode of waste we must glance at the two almost contradictory aspects which nitrogen is capable of assuming. On the one hand, in its free gaseous state, as present in the atmosphere, it is the very type of indifference and negation. We can merely say what it will not do. But on the other hand, when existing in its solidified combined condition, and especially in its organic compounds, it has more striking positive attributes than perhaps any other of the elements. No longer *azote*, as our French neighbours persist in calling it, it might rather be termed "zote,"—if such a word may be framed,—the very essence of life. It is an indispensable constituent of every vegetable seed and of every animal ovum. Without it blood, muscle, nervous tissue are impossibilities. But in addition to its functions in the processes of life, and therefore to its necessity in our diet, nitrogen—combined nitrogen, we must remember—has other properties which render it useful, or rather necessary, in numerous arts and manufactures. As a rule we shall find that if any organic compound, whether existing naturally or only produced by human intervention, possesses very striking attributes, such compound is nitrogenous. The most valuable fibres available for our clothing, the richest dyes procurable for their embellishment, the most precious medicines, the deadliest poisons, and last, though not least, those explosives so largely prescribed by modern humanitarianism in treating the diseases of bodies politic,—all these various bodies contain nitrogen as an essential constituent. Such manifold utilisation, in the present imperfect state of our knowledge, and in our still more imperfect disposition to make a rational application of what knowledge we possess, leads to a waste equally manifold. We say manifold in appearance, but still resolvable into one common principle—the conversion of solidified combined nitrogen into the free, inert, gaseous nitrogen which exists in the atmosphere. How vast are some of these forms of waste we will endeavour to show in some detail.

Let us, first and foremost, look at the very defective economy of nitrogen in the maintenance of human life, under existing conditions. In speaking of phosphorus we have already adverted to the truth so perseveringly and authoritatively enforced by Liebig,—that in growing crops upon land we take away from the soil certain constituents, and that its crop-producing power can only be prevented from diminishing by a systematic return of such constituents in the shape of the excretions of all living beings fed on the produce of the soil. Our neglect in fulfilling this condition is, perhaps, even more conspicuous in the case of nitrogen than of phosphorus itself. On an average the daily excretions of every human being contain half an ounce of combined nitrogen. If we assume the population of the British islands at 32 millions, this amounts to a yearly quantity of 365 million lbs. This weight of combined nitrogen, if applied without loss to the soil and absorbed by food-plants, would, if calculated in the form of bread, be equivalent to 4380 million 4-lb. loaves! But how much of this fertilising matter is really returned to the land? The whole of the sewage of London—in other words, the whole of the excretions of its four million inhabitants—is substantially wasted by conveyance into the sea. Here alone we have a national loss of about 91 million lbs. of combined nitrogen. The same disastrous game, differing merely in unimportant matters, is played wherever sewage is being allowed to flow either directly into the sea or into a river or a canal, without any attempt to separate and retain its valuable constituents. Unfortunately the mischief is not confined to places where water-carriage has been adopted for the removal of excrementitious matter. If we look at the case of small country towns, of villages and of detached houses, where, theoretically speaking, the excreta are returned to the soil, we shall find that there is in practice a most sad deficiency. The dung-heaps and cesspools, in which the night-soil and urine are supposed to be stored up along with all other domestic refuse, betray their trust. On the one hand, the soluble matters, dissolved out by atmospheric waters, to which they are as a rule freely exposed, find their way into wells, ditches, ponds, and thus gradually into the next stream. We have often observed the completeness with which this form of waste is carried on in many villages in the Eastern counties. On one side of the road—or perhaps on both sides—is a ditch which receives from every cottage, every farmyard, as well as from manured fields, a tiny drain, rich in soluble organic matters. That these ditches are on amicable terms of "give and take" with the wells and pools which supply the village with its supposed potable water, is a by-evil with which we have at present no concern. On the other hand, the cesspools and manure-heaps are giving off their combined nitrogen to the atmosphere no less liberally than to the streams. Fermentation is going on unchecked; there is nothing which may serve to arrest and condense the volatile nitrogenous products, and when ultimately the contents of the heap or the pool are dug into the garden or the allotment-field, instead of containing, as they should, the entire fertilising ingredients excreted by the inmates of the cottage, they consist chiefly of an impotent residuum. To call this a restoration to the soil of what has been taken from it is mere mockery. Not one-half the compounds withdrawn from our fields and gardens for the food of man, whether in town or country, are ever returned to where they belong.

So far, however, we have dealt merely with the waste of the fertilising matter in human excreta; but there is also the case of our domestic animals. When cattle and sheep are pasturing, and when horses are working in the fields, not only their dung, but their urine is deposited upon the soil. Under most other circumstances, however, the latter, which is the more valuable of the two, goes to swell the waste we have already mentioned. In the towns it is conveyed into the sewers, and in the country it escapes into ditches, adding thus no inconsiderable item to our yearly loss of combined nitrogen.

The remarks which we made on the waste of nitrogenous matters by the processes of putrefaction or fermentation going on in cesspools admit of a more extended application. Let us take the case of ordinary town-sewage. We can calculate with tolerable exactness how much nitrogen it ought to contain per gallon, if we know the gross number of the population and the total volume of the sewage. The figure thus obtained will, however, fall below the exact truth, since the proportion of nitrogen entering the sewers will be increased by the excretions of domestic animals and by the washings of slaughter-houses, &c. Yet if we take a gallon of the sewage, and submit it to actual analysis with the utmost care, we shall obtain a result very much short of the calculated percentage; some 20 to 50 per cent of the combined nitrogen, more or less according to circumstances, has escaped. In what form this loss takes place can scarcely be doubted. Were it given off as ammonia it could easily be detected; but ammonia does not readily evaporate from such dilute solutions, and at such low temperatures. We conclude, therefore, in accord with two chemists who have made the analysis of waters their speciality, and who on most points differ very widely in opinion, that the loss takes place in the form of free nitrogen. This we shall find to be the case whenever vegetable or animal matter containing nitrogen passes into a state of fermentation. A part of such nitrogen reappears as ammonia, and may be arrested by the aid of absorbents and of certain acids and mineral salts; but a further portion is evolved as free nitrogen. As far as we are aware this loss becomes the greater the larger the quantity of organic matter which is allowed to collect.

Here, then, in the very outset of our enquiries, we have already met with a fearful waste of nitrogen committed in the normal course of daily life, and without taking industrial operations into account.

Let us next examine the manufacture of gunpowder, and its allies, gun-cotton and nitro-glycerin. The first of these substances contains on an average 75 per cent of saltpetre (nitrate of potash), equivalent to 10·2 per cent of combined or available nitrogen. Of this 10·2 per cent, 9·98 per cent —we might consequently say, practically speaking, the whole—escapes in the form of free nitrogen, and is consequently rendered useless. This waste will appear the more serious if we consider the enormous scale upon which gunpowder is now manufactured. It was calculated some years ago that our annual exports of this article alone amounted to 19 million lbs. weight. We cannot estimate the total quantity of gunpowder produced in the whole world at less than 100 million lbs. This represents 10 million lbs. of combined nitrogen withdrawn yearly from the world's available resources. Translating this into the shape of human food, the annual consumption of gunpowder means the destruction in advance of no less than 500 million lbs. of bread. We must remember, too, what this destruction means. If we were to take the ripe crops of wheat in some country to an extent capable of yielding 500 million lbs. of bread, and if—instead of allowing the grain to be gathered and put through the various processes which fit it for use—we were to have it ploughed into the soil, this conduct would be pronounced a criminal waste of the means of human subsistence; yet we should in reality have committed, comparatively speaking, little destruction. The buried nitrogenous matter would be still available for the growth of crops in succeeding seasons, and our supposed course of action, foolish and reprehensible as it would undoubtedly be, would delay rather than destroy the appearance of such combined nitrogen as food. But if we work up the combined nitrogen into gunpowder, and explode it, there is not merely delay, but what amounts to destruction—a destruction going on from year to year, and continually diminishing the amount of food which the earth is capable of yielding.

These considerations seem to us to supply a new argument against war, to which we would call the attention of such Peace Societies—if any there be—whose hatred of war is logically consistent, and is not merely assumed at times to suit the purposes of faction. Hitherto political economists and social reformers, especially of the Malthusian school, have looked upon war as one of the "positive checks" upon the increase of population. Whilst deploring the other evils brought on by international conflicts, they have regarded it as a set-off that every battle must decrease the proportion of eaters to the total amount of food. Had they, before advancing such opinions, taken counsel of the chemist, he would have told them that although their view might have been correct in the olden time, when men shot each other with the grey-goose shaft or cleft each other's skulls with the battle-axe, yet since

> "That villainous saltpetre hath been digged
> Out of the bowels of the harmless earth"

the case is totally altered. Our modern battles, how sanguinary soever, reduce the supply of food along with the demand. Every rifle and every cannon discharged *may* destroy actual life, but certainly *must* destroy the means of life. Each ounce of powder burnt means so much more nutriment withdrawn from our crops, so much less bread or beef producible, and so much less human life rendered possible in the future. Speculators on the population question must henceforth cease to regard war as one of their "positive checks." Indeed these same "positive checks" are becoming somewhat abridged. War, as we have seen, though waged upon a larger scale than ever, has been converted into an agency of an opposite nature. Pestilence is to be abolished by "sanitary reforms." What remains, then, but famine; and, unless we cease our systematic waste of the elements of food, that we shall certainly experience.

We shall, perhaps, understand more clearly the essential antagonism between food-raising and gunpowder-making if we consider the process by which saltpetre was formerly obtained in most countries of Europe. If we, in England, saw less of this operation than did most of our neighbours, it was because we imported the nitre from other parts of the Empire, where it was produced substantially on the same principle, even though without the conscious and intentional intervention of man. So-called nitre-beds were formed and worked. To make these, earth was collected together from the sides and bottoms of cesspools, drains, urinals, and dunghills. It was mixed with plaster from old walls, and stratified with decaying animal and vegetable refuse. These beds or heaps were sheltered as far as possible from cold and rain, and were sometimes watered with urine. In course of time a whitish efflorescence began to appear in different parts of the heap. The earth was then dug up, lixiviated with hot water, and the solution thus obtained on concentration yielded crystals of saltpetre. Now it is evident that all the substances of which these nitre-beds were formed were in themselves nitrogenous, and capable of being used as manures. What was sold as saltpetre was simply so much robbed from the farm and the garden. As to the composition of the soil taken from the foundations of cesspools, stables, cow-houses, dunghills, &c., there can be now no doubt. Even the ground all about ordinary dwelling-houses was in former days saturated with excrementitious matter. We have adopted another form of waste; what our forefathers allowed to soak into the earth beneath and around their houses we run into the streams and seas. Our method may be a little more favourable to health, but it is waste all the same. The great fact that the saltpetre thus obtained from the nitre-beds was merely a transformation of the combined nitrogen put in them in various organic compounds was formerly not perceived. The nitre-bed was supposed, in some unexplained manner, to have the power of fascinating that chainless wanderer the free nitrogen of the atmosphere, and chaining him down as a slave to do man's work. Had, however, the constituents of the heap been accurately analysed,—a requirement utterly impossible when nitre-beds first came into use,—it would have been found that the nitrogen in the saltpetre obtained, instead of being more than equivalent to the nitrogen originally present, invariably fell short of it, and that a portion consequently of this element made its escape in the uncombined gaseous state. To this subject we shall be compelled to revert more fully below.

The Indian saltpetre, which till lately formed the bulk of our supply in England, was also obtained by the lixiviation of soils, which contained it as saline crusts. It was not, however, any and every soil which could be made use of, but such only as had become saturated with nitrogenous matter capable of yielding nitric acid by oxidation. The importation of saltpetre from India, therefore, was a robbery of the soil of India, and lessened the crop-producing power of the world in general just as much as did the nitre-beds of Europe.

It is true that at the present day the great bulk of the saltpetre used, whether in the manufacture of gunpowder or in other arts, is obtained from a different source,—the deposits of nitrate of soda found on certain parts of the western coast of South America. From these vast deposits more than 4,000,000 cwts. are yearly exported to different parts of the world; yet even these beds are supposed to have been originally formed by the decomposition of nitrogenous organic matter, and though large they are by no means incapable of exhaustion. So long as we take combined nitrogen and set it at liberty then, no matter whence our supply is obtained, the ultimate result must be complete sterility.

The manufacture of gunpowder involves another kind of waste, which is more conspicuous in the present state of the trade than it was formerly. The nitrate of soda imported from South America, unlike the nitrate of potash, has the property of absorbing moisture from the air, and is hence utterly unfit for the preparation of gunpowder. Before it can be used for any such purpose it must be converted into common saltpetre by appropriate treatment with some salt of potash. Now, most unfortunately, potash—just like phosphorus and like combined nitrogen—is an important constituent of the food of plants, and we can only procure it by the old sin of robbing our crops. Formerly the salts of potash were obtained by burning timber to ashes and lixiviating the residue. Next, the salts left in the preparation of beet-root sugar were employed. In either case the substantial result was the same: unless we can show that pine-trees or beet-plants have the power of creating potash out of nothing, or out of some other elementary body, we must confess that they obtain it from the soil, and that the soil must ultimately become exhausted. But potash compounds are now obtained by mining, at Stassfurt and elsewhere? Granted, yet the supply—like that of nitrate of soda in Atacama—is not infinite.

The gunpowder manufacturers, or those who prepare their materials, compete with the farmer for the one and the other of these products, raising the price and hastening the day of exhaustion. As regards potash, however, the waste is of a less fatal character than the expenditure of combined nitrogen. Gunpowder, indeed, contains the equivalent of 34 per cent of potash, and, according to our estimate of 100 million lbs. as the world's total annual production, about 34 million lbs. of this valuable body are thus withdrawn from the immediate service of agriculture. But the potash is not, like the nitrogen, withdrawn from utility. When a battle takes place all the potash contained in the gunpowder exploded gradually settles down upon the earth in the form of a fine dust, and, being washed into the soil by the next shower, becomes again available for the food of crops, and shares with the "red rain" the honour of making the harvest grow!

The other explosives in practical use—such as gun-cotton, nitro-glycerin in its manifold disguises and modifications, fulminate of mercury, &c.—all share with gunpowder the cardinal fault of unlocking combined nitrogen and returning it as an inert gas to the atmosphere. Wheresoever, therefore, man works by dint of explosions he is warring against life itself. The glorious victories of the warrior, the triumphs —often little less costly—of the civil engineer, the very "set pieces" of the pyrotechnist, the red fire in which the villain of the stage meets his retributive fate, and the squibs and crackers which have now for some two centuries been annually blazed away in honour of Guy Fawkes, are all bought at the cost of potential life, and all tend to make life more difficult.

We have now seen two of the great forms of the waste of combined nitrogen: wasted, as far as the excreta of man and animals are concerned, partly by conveyance into rivers and seas, partly by destructive fermentation, and in case of the explosives by a reduction differing from destructive fermentation merely in its more rapid character. But for both these kinds of waste there is at any rate the apology to be advanced that the nitrogen brought into play was essential. In other words, combined nitrogen is absolutely indispensable in the food of man and animals, and if their excreta are subsequently misapplied, and not returned to the soil, that does not condemn the original use of the nitrogenous matter. In the explosives, again, the nitrogen is not an inert constituent, but in the cases we have taken is essential. We cannot make either gunpowder, gun-cotton, nitro-glycerin, or picric acid without oxidised nitrogen, though we may and should most earnestly seek to produce explosives available for practical use which shall not require this expenditure of one of the main elements of life.

But there is another form of the waste of nitrogen more unjustifiable still. In pointing out some instances of this kind of loss we must protest against the possible imputation of seeking to play into the hands of any sect of world-betterers and social reformers. We are merely judging certain practices which mankind adopted in days of greater ignorance than the present, according to the light of modern chemical science, and pronouncing our verdict without fear or favour, and utterly indifferent to the possible tendencies or applications of such decision.

It is a threadbare story to tell our readers that Nature does not present us with her treasures in a state ready for our immediate use. Sometimes they are mixed or combined with things useless, or even pernicious, as pyrites with arsenic, or coal with sulphur. More frequently the admixture may consist of an ingredient inferior indeed in value, but still capable of use. But if we, in endeavouring to turn to account some such plentiful and inferior article, waste a far more valuable product with which it is blended, we may confess ourselves guilty of egregious folly. Let us suppose an ore containing gold and copper in the relative proportions of a shilling's worth of the former to a pennyworth of the latter. Suppose, then, some metallurgist using this ore for the extraction of copper, and operating in such a manner as to render the gold utterly incapable of separation. He would be regarded as foolish and his process as wasteful, whether it was in itself remunerative or not. To waste a shilling's worth of gold for the sake of extracting a pennyworth of copper would be considered as a culpable abuse of natural resources. Now in the cereals we find a mixture of substances somewhat analogous to the case which we have, for illustration's sake, assumed. There is in them a very precious substance,—combined nitrogen in the form of gluten,—relatively small in quantity, but predominating in value; and there is a substance of a very much lower value,—a compound of carbon, hydrogen, and oxygen, in the form of starch. We are by no means denying that starch has its important uses, but it is a substance which Nature produces in almost indefinite amount, and its constituents are far more plentiful in the earth than is combined nitrogen. Hence we contend that if mankind, when in quest of starch or of some product of starch, take a substance containing starch in admixture with gluten, and waste the latter and more valuable product, they are acting just like the metallurgist whom we have been supposing, and are either ignorantly or knowingly squandering the resources of the world.

As the first instance of this economic sin we may mention the use of wheat-flour for purposes other than food. A very serious quantity is consumed, without having undergone any previous chemical conversion, in the textile manufactures. It serves the sizer for communicating an artificial body to flimsy cotton goods, and is employed by the calico-printer in thickening his colours to the desired consistence. To give some idea of the extent of this waste we quote the following recipe from a recent work* :—

For medium sizing, take—

Flour, 3½ sacks	980 lbs.
Tallow...	180 ,,
Brown paraffin wax	5 ,,
White soap...	105 ,,
Soft soap	15 ,,
China clay	448 ,,

We have heard of 6½ sacks of flour mentioned as the daily consumption in a single manufacturing establishment.

From this we turn to the manufacture of starch. The demand for this substance in its ordinary commercial state is both varied and wide-spread. Its domestic use in stiffening linen, though most generally known, is probably not the most extensive application which it undergoes. Like flour, it serves for stiffening calico and for thickening colours, and is the raw material for the manufacture of so-called British gum. Of its functions in the sophistication of various articles of diet this is not the place to treat. Indeed none of the uses to which starch is put would in the least concern us were it obtainable, or at least were it ordinarily obtained, without the misappropriation of gluten, and the consequent waste of combined nitrogen. Unfortunately the chief materials selected, for the sake of convenience or on account of the quality of the product, by the starch manufacturer rank among the most important articles of human food, such as wheat, rice, maize, the potato, &c. If the whole of the nitrogenous matters could be separated out undamaged and in a state fit for human food, or even for the support of cattle, there would be little reason to complain; but no inconsiderable share of the gluten in the cereals undergoes fermentation, and much of its nitrogen is consequently wasted. Among the tasks which the industrial chemistry of the future will have to master are, therefore, some of the following :—

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* THOMSON'S Sizing of Cotton Goods.

The obtaining a sufficient supply of starch from materials either non-nitrogenous or at least not adapted for human food, such as, *e.g.*, the horse-chestnut.

Or, if food-materials are still used in the starch manufacture, the complete utilisation of the nitrogenous compounds present.

Or, the complete replacement of starch, flour, &c., in textile manufactures, and in calico-printing by some inorganic substance.

Or, the supercession of "thickeners" by some improved method of applying colours—a step which some practical men consider as by no means out of the question.

From starch we pass to vinegar. This useful and extensively employed acid contains not a particle of nitrogenous matter, save in the form of impurities, not merely unessential, but damaging to its quality. All that is required for its manufacture is simply sugar, which is in the first place split up into carbonic acid and alcohol, which latter then undergoes a process of oxidation. If vegetable acids—such as the tartaric, citric, &c.—are present in the raw material, these not only effect an agreeable modification of the flavour, but contribute to the formation of traces of ethereal compounds, of most refreshing odour. This explains the excellence of the wine-vinegars of France, the apple-vinegars of America (which preserve the delightful aroma of ripe apples), and the vinegars occasionally met with in rural districts in England—made from cane-sugar, flavoured with the juices of fruits, or even flowers, such as the primrose or the cowslip. But the vinegar made for sale in England is unfortunately prepared from a nitrogenous matter, *i.e.*, malt, and we have thus combined nitrogen wasted in obtaining a product which can be procured in far superior quality from non-nitrogenous, or at least very sparingly nitrogenous, matter. True economy demands that bodies rich in combined nitrogen should be used for no purpose save where such nitrogen is essential, and should be restricted as far as possible to the production, directly or indirectly, of articles of food and medicine. The enunciation of this law brings us into open collision with widespread customs. We do not belong to those who pronounce alcohol altogether objectionable as an article of diet, and neither by moral suasion nor by legislative interference do we seek to abrogate its use; but as combined nitrogen is not required for the production of alcohol, we are compelled to include the use of nitrogenous products in the manufacture of alcoholic beverages among instances of that waste which we are explaining and deprecating. The annual amount of malt made in this country is somewhere about 47 million bushels, or, in round numbers, 2000 million pounds. A very large part of the nitrogen originally present in the grain is wasted. A small quantity is indeed to be found in malt liquors as supplied to the consumer, but too little generally to be of appreciable dietetic value. The bulk is lost in the various stages of the process. Hence, from a chemico-economical point of view, the repeal of the malt tax, or any fiscal measure which shall encourage the application of grain or other highly nitrogenous matter to the manufacture of fermented or distilled liquors, would be a mistake. Well would it have been if the art of malting had never been invented, and if mankind had been content to procure their alcoholic beverages from fruits and from saccharine juices of comparatively low dietetic value, and have kept grain of all kinds for exclusive use as food.

But there is a use of nitrogenous matter even more palpably and lamentably wasteful than the manufactures of starch, of gum-substitutes, of vinegar, alcohol, &c., from grain. Indian corn is actually to some extent used as fuel,—a misappropriation rendered more feasible by the proportion of oily matter which it contains. That under such circumstances the bulk of its nitrogen will be wasted, by escaping in the free state, admits of no doubt.

Nitrogenous matter of animal origin is perhaps less systematically wasted than is the gluten of grain. Albumen obtained from eggs is indeed somewhat extensively applied by calico-printers as a mordant for inducing cotton to take up colours, especially the coal-tar dyes, in the same manner as do silk and wool. Unfortunately the albumen of blood, which is highly improper as food, has not yet been made available in all cases as a substitute for egg-albumen, inasmuch as it has not been practically procurable in a colourless state. Hence there is here room for a mordant capable of "animalising" vegetable tissues, and yet involving no waste of human food.

The use of wool in clothing does not involve the loss of combined nitrogen which might be at first suspected. The ultimate destination of the dust from the shoddy mill, and of the fibre itself when no longer capable of being re-spun, is to the manure manufactory and to the fields. With leather the case is less favourable. The raw hides of animals, though probably worthless as the food of man, are of great value as a nitrogenous manure; but after the tanning process they are scarcely, if at all, available as plant-food.

Leather-waste will lie for years in the soil undecomposed, and appears to exert no appreciable fertilising influence. Hence a non-nitrogenous substitute for leather would be a boon of no small magnitude to the world.

We have thus given a catalogue, by no means exhaustive, of the operations and processes, industrial and domestic, by which nitrogen is wasted. This waste, as we have seen, turns mainly on its transformation from the combined or solidified state to the free or gaseous condition as it is found in the atmosphere. But in this state, we shall be asked, is it not plentiful, almost to infinity, and does there not exist here one of those beautiful processes of circulation, of which we often read, by which it is restored to the combined or solidified state, and made again available? Yes, the store of atmospheric nitrogen is all but infinite, and we dare not assert that there is absolutely no natural process by which it can be re-combined with other elements; but these processes are slow in their operation, and, as far as we can judge, they cannot keep pace with our waste. They do not build up as quickly as we can destroy. We know that the atmosphere contains traces of ammonia, and that nitric acid in small quantity may be detected in rain, especially that which accompanies a thunderstorm; but we are still doubtful how much of this combined nitrogen has been really formed at the expense of the inert atmospheric nitrogen, and how much, on the other hand, is merely the decomposition of organic matter upon the earth's surface. It is ascertained that the electric spark, *i.e.*, lightning, on passing through a mixture of oxygen and nitrogen,—in other words, through common air,—effects the combination of these two elements so as to form nitric acid. Water holding in solution atmospheric air, as do all normal waters on the earth's surface, is found to yield nitric acid at the positive pole of the battery and ammonia at the negative. Hydrogen and nitrogen are also, according to the researches of Donkin, induced to combine by the action of the effluve, or silent electric discharge. But all such processes, however varied, have been found to be exceedingly slow in their action, and hence incapable of practical application in the arts. Ozone was once appealed to in every difficulty, but it has been proved incapable of oxidising free nitrogen to nitric acid in presence of water, as was suggested. Other reactions have been proposed, but none of them has proved distinctly and unequivocally successful as a means of combining the free nitrogen of the air into ammonia, or nitric acid, or cyanogen, or any other available compound. To solve this, the king-problem of practical chemistry, is a duty still unfulfilled and a triumph still unearned. It may again be urged that processes which do not "pay" in our hands may yet prove successful in those of Nature, with whom time is no object. If ammonia is but sparingly manufactured in the upper regions of the atmosphere, it may perhaps be evolved by the aid of non-nitrogenous organic matter in some of the phases of fermentation or of eremacausis. Or the vital power of vegetables may solve the difficulty: the growing plant may arrest atmospheric nitrogen, and employ it in the formation of albumenoids. Method after method, thus pointed out as possible, has been carefully scrutinised. In most cases the experimental reply has been decidedly negative, in a few equivocal, in none distinctly affirmative. It is undoubted that the atmosphere superincumbent upon an acre of ground must supply combined nitrogen enough for all the vegetation naturally growing upon this acre, and must be able to keep up the supply from century to century. Were this not the case old Mother Earth would long ago have lost her green, flower-embroidered mantle. But to grow the crops needful for the support of our human population we are obliged to make, upon every single acre of arable land, demands a hundredfold greater than Nature ever makes upon an acre of forest, or prairie, or swamp, or moorland.

As we have thus increased our requisitions, and as we have still in another point deviated from Nature's plan by not restoring to the soil what we take from it, we meet the universally admitted fact that without a supply of nitrogenous manures the fertility of our fields declines. What proof other than this one fact is required to show that there is in the economy of the world no recuperative power equal to our present power of waste, and that we are thus rendering nitrogen unavailable more rapidly than it is being combined or made available? Our stock of solidified nitrogen, like our supply of solidified carbon, is decreasing, and must ultimately come to an end. The case of nitrogen is, of the two, by far the worse, because Nature is manufacturing fuel for us far more rapidly than she is producing albumenoids.

What then remains? The world may not, perhaps, be upon the whole more populous than it has ever been before; but thanks to some of the inventions upon which we so much pride ourselves, thanks to the industrial development which has characterised the last three centuries, and to which history records nothing similar, waste—destruction of the matter most essential to life—has reached a height truly ominous and alarming to contemplate. It is sad to think that our civilisation, with all its glories, may be most aptly described in those lines from the "The Devil's Walk on Earth:"—

> "The pig swam well, but every stroke
> Was cutting his own throat."

We know now, approximately at least, the extent of the earth's resources. We have no more vast and scarcely trodden continents for us to discover. We can no longer calculate on finding coal, or phosphates, or other useful products, at any point where we may choose to dig. No more can we comfort ourselves with vague hopes that emptied mines and exhausted soils will spontaneously grow rich again, or that plants can create their own nourishment. Nor can we lay the flattering unction to our souls that for every utility no longer procurable a substitute will be found. The dreams indulged in by enthusiasts in the earlier portion of the present century—of a future measured perhaps by millions of years, in which mankind will be constantly improving in power, in knowledge, and in happiness—are being somewhat rudely broken. Science tells us, in unmistakable tones, that this earth cannot for ever afford a home to a race like ours. What, for instance, would be the condition of mankind were all the habitable parts of the globe as populous, as industrial, and as luxurious as are Western and Central Europe and the eastern part of North America? Whence would they all be able to import their needed supplies of food, of manures, and of raw materials for manufacturing purposes? Time was when England produced a sufficiency of food for her own inhabitants. Then came a time when she began to import, and that in ever-increasing proportions, both food and manures. Next we mark that the countries which formerly exported food and manurial matters—*i.e.*, ground bones, &c.—ceased to do so, became importers instead of exporters of both, and competed with us in the market. The Atlantic States of the American Union import manures, and can scarcely supply food for their home population. By-and-bye must come a time when Chili, California, South Russia, Hungary, will require all the wheat they can produce for their own consumption. The eyes of our political economists will perforce be opened to the truth that every country unable to feed its own population is in a dangerous predicament. The civilised world is now in the position of a spendthrift who is gradually awaking to a faint consciousness that his resources are limited, and that "something must be done." Before us—and by "us" we mean not merely the British nation, but the entire civilised world—there lie open two courses. We may go on as we are now doing, increasing in wastefulness even more rapidly than in numbers, and presenting our drafts upon Nature till the reply at last comes "No effects." Or we may take the advice which Science has given us by the mouth of Liebig, and amend our ways. Pending the great discovery of the industrial utilisation of atmospheric nitrogen, we must look with a jealous eye upon every application of nitrogen, and also of phosphorus and of potash, other than for food or medicine. We must proscribe the use of nitrogenous bodies for all purposes to which such nitrogen is not essential. We must restore to the land all that we take from the land, in forms capable of assimilation. We must either seek to restrict the use of explosives, or we must find bodies of this class whose manufacture shall not rob our fields and gardens. We must obtain our sizings, our thickeners, our mordants, our vinegar, our alcohol, from bodies free from or at least poor in nitrogen. How much of the programme may lie within the bounds of possibility the future only can show. Shall we ever succeed in obtaining food direct from its inorganic elements without the tedious and circuitous interposition of plants and animals? If so, the future of the human race may be both longer and brighter than we can at present dare to hope. Meantime, to economise nitrogen, phosphorus, and potash, to recover these bodies from waste, and to find substitutes for their present "profligate" applications, is the most sacred task which the chemist can take in hand. The reforms which may shield us from occasional pestilence sink into insignificance compared with those required to guard posterity, in a not very remote future, from chronic scarcity, from recurrent famine, and from a wolfish struggle for food, in which man must relapse into a worse savagery than that from which he has emerged.

II. ON THE MOVEMENT OF MICROSCOPIC PARTICLES SUSPENDED IN LIQUIDS.

By Professor W. Stanley Jevons, LL.D., M.A., F.R.S.

IF we see a thing moving of its own accord, this spontaneous motion is thought to be a sure sign that the object is an animal. Such an inference may be true of large bodies, but when we treat of microscopic objects the case is very different. Not only are minute vegetables, on the whole, as active as animals, but even mineral particles are found to be capable of moving as if endowed with life. This is a fact which has been familiar to microscopists for half a century at least. The phenomenon was made widely known by the celebrated botanist, Robert Brown, of the British Museum, who, in the course of some observations on the pollen of plants, discovered what he called *active molecules*, both in organic and inorganic bodies. He published his observations in two brief papers, which will be found in his "Miscellaneous Works," reprinted by the Ray Society (vol. i., p. 463). Considerable attention was drawn to the subject at the time, and the animated behaviour of particles became known as the *Brownian movement*.

Brown, however, as he himself points out, was not the first discoverer of the phenomenon. A few years previously John Bywater, a Liverpool man of some scientific merit, had published a tract (in 1819), re-issued in 1824 with additions, under the title "Physiological Fragments; to which are added Supplementary Observations to show that Vital and Chemical Energies are of the same Nature, and both derived from Solar Light." The greater part of this book is occupied with vague and worthless speculations, but his observations are better. He found that "small active linear bodies" can be obtained from matter in general. That they could not be really animated he inferred from the fact that they were yielded by coal just drawn from the pit, by the white ashes of coal, or by sandstone which had just been heated red-hot. Moreover, boiling kills animalcules, whereas he found that these lively particles could be boiled without suffering any loss of activity. Bywater came to the conclusion (p. 59) that these active bodies derive their vital power from the water, and he acutely remarked that rocks generally undergo change when exposed to air and moisture.

Bywater, indeed, was far from being the first who examined the motion of minute particles; he was only the first who distinguished between the motions of living and dead particles. A series of physiologists—such as Buffon, Needham, Gleichen, Wrisberg, Müller, and Spallanzani—had been misled by these dancing particles into the belief that all bodies are formed of certain ultimate organic globules, which Spallanzani described as "animaletti d'ultimo ordine." Needham had, in 1749, published a quarto tract called "Observations upon the Generation of Animal Substances," in which he described the motions of little *eels* and other small bodies. Buffon, who was acquainted with Needham, seems to have been thus led to start his celebrated theory of *organical parts*. He says*—"God . . . has not only given form to the dust of the earth, but he has rendered it living and animated by enclosing in each individual a greater or less quantity of active principles, of organic living molecules, indestructible and common to all organised beings."

We may go back still further; for in the "Philosophical Transactions" for 1696 (vol. xix., p. 280) is to be found a remarkable account of some microscopical observations by Stephen Gray, made by microscopes, or rather lenses, of very primitive construction; yet Gray seems to have succeeded in viewing these animated particles, and, considered by the light of later observations, the following passage is well worth quoting:—"I have examined many transparent fluids, as water, wine, brandy, vinegar, beer, spittle, urine, &c., and do not remember to have found any of these without more or less of the bodies of these insects; but I have not seen any in motion except in common water, that has stood for sometimes a longer, at others a shorter time, as has been observed by M. Leeuwenhoek; though I do not remember he has observed that they are existent in the water before they revive. In the river, after the water has been thickened by rain, there are such infinite numbers of them that the water seems in great part to owe its opacity and whiteness to these globules. Rain-water, so soon as it falls, has many, and snow-water has more, of these globules." It is probable that these moving globules were not really insects, but inorganic particles moving in the way we have

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* Histoire Naturelle, redigé par Sonnini, tome xxiii., p. 2. See also tome xvii., ch. 6, p. 231.

to investigate. Leeuwenhoek was the most celebrated of the early microscopists, and appears to have frequently observed the motions of small bodies.

One astonishing fact about this subject is the small attention paid to it in recent years. Microscopists have continually had the phenomenon under their eyes, and it has often been mentioned as a mysterious one which might lead to great discoveries. Faraday's attention was drawn to the matter by Brown's tracts, and he gave a Friday evening discourse on the "active molecules." Part of the notes of this lecture have been published by Dr. Bence Jones, in his "Life of Faraday" (vol. i., p. 403) and are very interesting; they end thus:—"*Lastly, the relation of these appearances to known or unknown causes.* Analogy to other moving particles. Camphor. Supposed facility of explanation, *not camphor motion.* Takes place within pollen. Under water, inclosed by mica or oil—not *crystalline* particles—not attraction or repulsion. Does not consist in receding and approaching—*not evaporation* answered as before—not currrents, too minute—oscillation—consider current in a drop—when currents, motion very different—not electricity of ordinary kind—because do not come to rest, seen after hours—so that the cause is at present undetermined.

"Mr. Brown supposed to be careless and bold, is used to microscopical investigations—has not yet been corrected—assisted by Dr. Wollaston—so that carelessness can hardly be charged. Then, what does Mr. Brown say? Simply that he cannot account for the motions.

"Many think Mr. Brown has said things which he has not—but that is because subject connects itself so readily with general molecular philosophy that all *think* he must have meant this or that—as to molecules, by no means understand ultimate atoms—as to size, says that solid matter has a tendency to divide into particles about that size—pulverisation and precipitation—if smaller, which may be, *are very difficult to see*—does not say that all particles are alike in their nature, but simply that organic and inorganic particles having motion—motion cannot be considered as distinctive of vitality—connection with atomic or molecular philosophy."

It is plain that Faraday, although he correctly puts aside many erroneous suggestions, could not explain the phenomenon; but it is strange that in his long life of indefatigable research he never recurred to the subject, nor did any other eminent physicist undertake the task. A few incidental remarks upon the subject are to be found in books on the microscope, as in that of Dr. Carpenter (4th edition, p. 169), or in Griffith's "Micrographic Dictionary," article "Molecular Motion" (3rd edition, vol. i., p. 498).

The French naturalist and microscopist, Dujardin, appears to have paid special attention to this Brownian movement, which he describes with more minuteness and accuracy than any previous observer.*

Griffith, the editor of the "Micrographic Dictionary," seems to be almost the only other observer who has specially investigated this curious subject. His results are mostly correct as far as they go.† He finds that all kinds of ponderable matter, whether organic or inorganic, exhibit these movements when reduced to a fine state of division, and suspended in a liquid not too viscid or tenacious. He satisfied himself that a single particle moves independently of surrounding particles. He rejects the explanations previously suggested, such as the influence of evaporation, and, after unsuccessfully trying some experiments with electricity, leaves off with the candid conclusion that he knows no cause for the phenomenon.

For a number of years past‡ I have occasionally occupied leisure time with experiments on this subject. For the microscopic observations a good compound instrument is desirable, if not indispensable. I have used an excellent instrument by Dancer, of Manchester, the quarter-inch objective of which, with the lowest eye-piece, usually suffices; but, to observe the movements in all their perfection, I have found a good one-eighth inch objective, with a C or D eyepiece, to be useful.

One difficulty is to find a convenient name for the phenomenon. Brown called it *molecular movement*, and it is often so called in microscopic books; but this is a very bad name, as the particles moving are of course not molecules. Some writers have called it the *Brownian movement;* but this two-worded expression is very inconvenient, is not in any way descriptive, and gives perhaps undeserved honour to Brown, who was not its first discoverer, and did little more than make it generally known. Dujardin described the motion as one of *titubation*, from the Latin *titubatio*, which means staggering or wavering; but this word is not elegant, and

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* Nouveau Manuel complet de l'Observateur au Microscope. Manuels Roret, Paris, 1843, pp. 58 to 60.

† London Medical Gazette, 1843, vol. ii., pp. 502 to 506.

‡ A brief preliminary account of my experiments was given in the Proceedings of the Manchester Literary and Philosophical Society for January 25th, 1870 (vol. ix., p. 78).

has not since been used. I have therefore ventured to coin, or rather adopt, a new word, and call the motion *pedesis*, from the Greek πηδησις, *leaping* or *bounding*, this being the correct description of the phenomenon when seen in perfection. We also have the advantage of the perfectly classical adjective *pedetic*, from the Greek πηδητικος.

The pedetic movement cannot be better seen than by taking a drop of old common ink which has been exposed to the air for some weeks, and examining it under thin glass with a magnifying power of 500 or 1000 diameters. An infinite multitude of minute black particles will be seen, all in such rapid motion as to cause a boiling or swarming appearance. This is, in fact, a wonderful sight. In new clear ink the particles are fewer and very minute, but all in motion, as far as can be seen. Almost any kind of fine clay mixed with pure water shows pedesis, and muddy rain water from a macadamised road, if properly examined, will soon convince the observer what a common phenomenon it must be. Perhaps the best possible exhibition of the motion is to be got by grinding up a particle of pumice-stone in an agate mortar, and mixing it with distilled water. The minute angular particles will be seen under the microscope to leap and swarm about with an incessant quivering movement, so rapid that it is impossible to follow the course of a particle, which probably changes its direction of motion fifteen or twenty times in a second.

It is very difficult to measure or describe the motions with accuracy, and they vary much according to circumstances. The distance through which a particle moves at any one bound is usually less than 1-5000th part of an inch. The motions much depend upon the sizes of the particles. Those of a greater diameter than 1-5000th of an inch are seldom seen to move, and the motion is more marked as the particles are smaller. Exceedingly minute particles may sometimes be seen literally to skip and dance about, as in the case of some minute particles of metallic antimony which Mr. Dancer,* the microscopist, of Manchester, showed me with an excellent 1-8th inch objective.

There is no apparent lower limit to the size of moving particles, and down to the 1-50,000th or 1-70,000th part of an inch the movement certainly becomes more and more remarkable.

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* Mr. Dancer, F.R.A.S., studied this subject at intervals for thirty years, but I do not know that he has published any results except those given in the Proceedings of the Manchester Literary and Philosophical Society for January 25th, 1870 (vol. ix., p. 82). Mr. Dancer found that diamond-dust and graphite could be made to show the motion.

The substance which I have found to be most convenient for experiments on pedesis is fine pure *china clay*, or *kaolin*. This clay is used in photography, and is to be purchased very carefully washed and prepared for the purpose. A small quantity of the clay shaken up with pure water makes a milky liquid, a drop of which being placed under a piece of thin glass will show the motion in great perfection; but finely-powdered glass, earthenware, and in fact almost any very finely-powdered substance, will do nearly as well. Among vegetable substances gamboge may be mentioned as readily giving minute particles, which quiver and dance about in a wonderful way.

In endeavouring to discover the cause of this phenomenon I made a certain number of experiments to test the validity of various explanations which had been offered. It has often been suggested that the motion is excited by rays of light or heat falling upon the liquid, and Crookes's radiometer now shows that in some cases it is possible to convert radiant energy directly into the energy of visible motion. But this idea was easily and completely disproved. Taking several active substances, such as kaolin, road-dust, red oxide of iron, and mixing them with distilled water, I examined them in the microscope, causing the direct rays of the sun, somewhat concentrated by a lens, to pass straight through the glass slide into the microscope. I then interposed a dark glass alternately between the sun and the specimen, and between the eyepiece and the eye. While the amount of light reaching the eye remained nearly the same, the intensity of the rays falling on the moving particles was probably several hundred times as great in one case as the other; yet the vibrations of the particles proceeded in exactly the same manner in comparative darkness and in intense light. Different coloured glass screens—purple, yellow, rose-coloured, &c.—were also tried, but no difference in the motions was apparent. The same conclusion was indirectly obtained, after the connection of pedesis with the supension of particles in water had been ascertained, by taking two tubes of china clay and pure water, putting one in a dark place, and exposing the other to the direct rays of the sun for three hours. No difference in the rapidity of subsidence was apparent.

It is asserted in Dr. Carpenter's work on the microscope (§ 130) that this so-called molecular movement is greatly accelerated and rendered more energetic by heat. "This," he says, "seems to show that it is due either directly to some calorical changes continually taking place in the fluid, or to some obscure chemical action between the solid particles and the fluid, which is indirectly promoted by heat." Although I believe there is obscure chemical action, my experiments lead me to think that the effect of heat is rather the other way, and that increase of temperature decreases the motion. I was not able, indeed, to perceive any difference produced by warming the microscope plate; but as this is a difficult and uncertain experiment, I tried the matter indirectly. A mixture of charcoal-powder and boiled water was surrounded with ice, and a similar mixture was placed in boiling water and maintained at 100° C. At the end of one hour the heated mixture had deposited nearly all the charcoal, whereas the ice-cold water still had as much in suspension after eight hours. A similar experiment with china clay gave like results. This is the more surprising and conclusive inasmuch as the heated mixture would be the most likely to be disturbed by convection currents. It is well known that heat causes the aggregation of many precipitates, and I suspect that this will in most cases be due to the diminution of pedesis, and I shall point out that this is probably explained by the increase of electrical conductivity of liquids produced by rise of temperature.

It may still be urged that a liquid while receiving or parting with heat might show increased pedesis. To test this I took a tube containing china clay and water, and during two days frequently suspended it before the fire, allowing it to cool in the intervals. An exactly similar tube was sunk in sawdust which had been lying for several years undisturbed in a wine-cellar. After remaining fifty-two hours in nearly perfect darkness and equilibrium of temperature (about 9° C.), the latter tube was found to contain more clay in suspension than the one which had been moved about and warmed many times. Even after seven days the buried tube still showed a slight cloudiness. This was a very striking experiment, and quite convinced me that no extrinsic causes are concerned in the matter.

As it might be thought that the movement is connected in some way with the shape of the particles, I compared in the microscope the fine needle-shaped particles of asbestos dust with the spherical globules of milk, the minute spheres of gamboge, the flat particles of talc, the small cubes of galena, and the wholly irregular fragments of glass. As particles of all these various forms show pedesis, no particular form can be essential to its production; but it seems likely that, *cæteris paribus*, sharp-pointed and irregular particles oscillate most intensely and rapidly. Spherical particles exhibit rather a gentle swaying backwards and forwards in no determinate direction; this motion can be well studied in common milk, or in a mixture of oil and water thoroughly rubbed together.

It is natural to enquire how long the pedetic movement will go on. Does it exhaust itself rapidly? The experiments which I have made lead to the opposite conclusion in a wonderful degree. Ink which is many months—and possibly years—old, and which has been long exposed to the air, exhibits the motion in perfection, as already stated. Again, by means of a pipette, I took a little of the lees from a bottle of port wine which had been lying undisturbed in a wine-cellar for several years, and placed a drop under the thin glass cover of the microscope plate, with the least possible exposure to air. A slow distinct motion of the particles was apparent, and it was not increased when some of the dregs had been shaken up in a bottle with air. This experiment leaves no doubt in my mind that the sediment of port wine is in a state of perpetual motion, until it finally settles down and attaches itself to the glass. But the most surprising fact elicited in the whole enquiry was furnished by two glass tubes containing china clay and distilled water, which I corked up in November, 1869, and laid in a drawer usually opened several times in the day, so that they would be shaken up every now and then; frequently, too, they were shaken up by hand. At long intervals the tubes were opened, and drops of the milky liquid examined. Comparing the motion of the particles with that of newly-mixed china clay and water, *no diminution of motion was apparent;* on the contrary, *the motion seemed to be even more remarkable than in a fresh mixture.* This observation was confirmed by the suspensory power of the liquid. On several occasions, in 1876 and 1877, I have shaken up the tubes and placed them upright in a spot shielded from light. After a lapse of eleven days I have found a slight cloud of clay still in suspension in the lower part of the water. Thus, after a trial of eight or nine years' duration, we meet with the astonishing fact that the suspensory power and the pedetic motion apparently increase with time. In fact this pedetic motion seems to be the best approach yet discovered to a perpetual motion. The above observations are quite in agreement with the statement of Dr. Carpenter ("Microscope," § 130, p. 169), that the movement has been known to continue for many years in a small quantity of fluid enclosed between two glasses in an air-tight case. Faraday, as we have seen, discarded ordinary electricity as the cause of motion, because the particles do not come to rest after hours. Nevertheless I believe that motions lasting for several years are not inconsistent with an electrical explanation.

Prof. Tyndall has quite recently expressed his opinion that this motion of particles is due to "surface-tension," but he cannot be aware of the length of time during which the motion will continue. It is obvious that from surface-tension we cannot derive a long-continued supply of energy, as is required to explain the phenomena.

One of the most important questions to decide is naturally what substances exhibit these movements, or do all substances exhibit them? I have tried many experiments with a view of answering these questions, and though at times there seemed to be particles of the proper size which did not move in pure water, subsequent trials often threw a doubt upon the conclusion. I hesitate, therefore, to affirm that there is any substance which may not be reduced to such small particles, and so suspended in water as to show pedesis; but there are certainly great differences in activity. Silica, in all its insoluble forms and compounds, is especially remarkable for the activity of movement. Clay of several kinds, earthenware, fire-brick, common brick, clay slate, silicified wood, glass of several kinds, talc, mica, pumice-stone, asbestos, basalt, granite, slag from iron and other furnaces,—also many silicious minerals, such as topaz, tripoli powder, glacier sand,—all exhibit the movement in a high degree of intensity; and these instances are probably sufficient to show that all silicious compounds, and accordingly the greater part of known minerals and rocks, yield very active particles. But the state of composition is not requisite to the effect; for the purest quartz crystal, agate, cornelian, chalcedony, or the finest white sand, when reduced to dust, show almost equally active motion. On the whole the silicates and silica exhibit pedesis in the highest perfection.

The substances, on the other hand, which seem to be least remarkable for pedesis, are oxides and other compounds devoid of silica. I may mention especially chalk, fluor spar, hematite iron ore, galena, oxide of tin, barium sulphate, calcium sulphate, titaniferous sand, bone-dust. Various other substances—such as charcoal of several kinds, coal, coke, emery powder, amorphous phosphorus—show the motion readily. The metals iron, steel, gold, and platinum were tried at first without success, probably because it was not easy to reduce them to sufficiently minute particles;

but the brittle metals, antimony, arsenic, and bismuth, were readily ground into fine powder, and showed active motion. Afterwards I obtained much motion with steel, lead, bronze, and standard silver and gold coin, the particles being produced by rubbing the metals on a smooth hone. Sulphur could not be ground up fine enough; but some sulphuretted hydrogen water having become decomposed, the precipitate of sulphur was found to consist of very minute particles, probably about 1-30,000th of an inch in diameter, in a state of extraordinarily active motion. It is certain that substances of the most widely different chemical characters will exhibit the phenomenon, and it is difficult to establish any clear differences in the activity of the motion as connected with the chemical nature.

As the form and chemical nature of the particles and the physical circumstances of the liquid seemed to throw no light on the cause of pedesis, it remained to consider the chemical character of the liquid. Striking differences in this respect were soon detected. If, instead of mixing china clay with pure water, we mix it with a very dilute solution of sulphuric acid, say one part in a thousand, an extraordinary difference results. Under the microscope the particles of clay are seen to aggregate together, and rapidly sink down upon the glass slide. The pedetic movement is almost entirely destroyed, or, if any particles move at all, it is only exceedingly minute ones. The same effect is produced by almost any mineral acid, and, as a general rule, by all salts and other soluble substances, with certain significant exceptions afterwards to be described. Before describing the experiments in this direction, however, it is necessary to point out the intimate connection which exists between pedesis and the suspension of particles in liquid.

It seems surprising that the conspicuous phenomena arising from the suspension or precipitation of solid particles in liquids have not been more carefully studied. Besides being full of scientific interest, they have great practical importance in questions of water supply and sewage treatment. A few experimenters have touched upon the subject. The microscopist Dujardin, already quoted, hits the right nail on the head when he says (p. 60)—" Ce mouvement Brownien joue un rôle important dans certains phénomènes physiques; c'est lui qui empèche les eaux troubles de se clarifier promptement par le repos."

A few other casual remarks to the same effect may be quoted from several sources, but I am not yet aware of any connected series of experiments upon the suspension of powders prior to those which I made in 1869, the results of which were briefly published in the "Proceedings of the Manchester Literary and Philosophical Society" for January 25th, 1870 (vol. ix., p. 78). Since then several experimenters have treated the subject. Mr. Wm. Durham, F.R.S.E.,* has correctly apprehended that the suspension depends upon the non-conducting nature of the fluid; but he is evidently wrong in supposing that the electricity is generated by the falling of the particles through the fluid, inasmuch as the pedetic movement, which favours suspension, is greatest when the falling of the particles is slowest. Mr. David Robertson has tested the precipitating power of salt, or even slightly brackish water,† and Mr. Wm. Ramsay, principal assistant in the Glasgow University Laboratory, has attempted an explanation of the fact with which I cannot agree.‡ He attributes the varying rapidity of the settling of clay suspended in saline solutions to the varying absorption of heat by the solutions, whatever this may mean.

In the "Chemical News" for August 31, 1874 (vol. xxx., p. 97) it was pointed out that Dr. Sterry Hunt read a paper before the Boston Society of Natural History, on February 18th, 1874, in which he alluded to the rate of settlement of clay in the water of the Mississippi River. The suspended matter, chiefly clay, amounts to about 1 part in 2000 parts of the water, and takes from ten to fourteen days to subside. But he observed that additions of sea-water, salt of magnesium sulphate, alum, or sulphuric acid, rendered the turbid water clear in twelve or eighteen hours. He thus explained the ready precipitation of the suspended clay when the river-water comes into contact with the salt water of the Gulf of Mexico, causing great deposits of fine mud, and helping us to understand the origin of the accumulation of clay slates in various geological formations. My experiments in 1869, and since, entirely confirm these observations of Dr. Hunt, and lead me to think that the pedetic movement must have played a most important part in geological processes.

The power of salts to precipitate suspended particles has long been known practically. In some parts of the world

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* Abstract of a Paper read before the Royal Physical Society of Edinburgh, January 28th, 1874 (Chemical News, August 7th, 1874, vol. xxx., p. 57).

† "Note on the Precipitation of Clay in Fresh and Salt Water."—Transactions of the Geological Society of Glasgow, vol. iv., p. 257. See Whitaker's excellent Geological Record for 1874, p. 182. I have not been able to refer to the original paper.

‡ Abstract of the Proceedings of the Geological Society of London, No. 315, March 8th, 1876, p. 1.

alum has been dissolved in river-water to clear it. I presume that we may in the same way explain the use of the *Strychnos potatorum*, or clearing nut, the seeds of which are used in the East Indies for the purpose of clearing muddy water. One of the seeds is well rubbed for a minute or two round the inside of the vessel containing the water, which is then left to settle; in a short time the impurities fall to the bottom. Bitter almonds are employed for the same purpose in Egypt, and those of the Kola or Sterculia in Sierra Leone. These methods are probably due to the destruction of pedesis.

The connection between pedesis and suspension is simply this;—In the absence of pedesis suspended particles attract each other and become aggregated together into little groups, which then acquire sufficient weight to force their way down through the resisting liquid. This effect I have observed some hundreds of times, both in and out of the microscope. If a little china clay be mixed up with water containing 1-1000th or 1-10,000th part of sulphuric acid, the milky liquid will presently assume a flocky curdled appearance, and after a time the little flocks will subside, streams of clear water making their way up between the interstices. The rate of subsidence is doubtless affected by other circumstances, such as the specific gravity of the liquid, the smallness of the separate particles, and their shape; but it is the aggregation of particles into groups which mainly determines precipitation or suspension. As a rain-drop is to a cloud particle, so is a group of clay particles to the minute suspended particles. Prof. Stokes has shown that the resistance experienced by the minute particle in falling through a fluid is comparatively enormous, so that there is really no further mystery in the falling of curdled precipitates. But pedetic motion prevents the formation of groups; it keeps each minute particle—say 1-1000th of an inch—from each other particle, so that each encounters the separate resistance of the fluid.

Dujardin, indeed, thought that the pedetic motion caused particles to diffuse themselves through water. "C'est lui aussi qui répand dans toute une masse d'eau, et y tient en suspension les particules désagrégés d'un corps animal ou végétal qui se décompose." In this he is probably wrong, for if a layer of clear distilled water be placed above a mixture of china clay and distilled water no diffusion of the clay upwards will be detected. It is difficult to make the experiment in an accurate manner; but if we consider that the pedetic motion, as seen in the microscope, is alternating and vibratory, and quite indeterminate in direction, and that any one jump of a particle probably never exceeds 1-1000th part of an inch at the most, it becomes exceedingly unlikely that any particular particle should make ten such steps in the same direction. According to the theory of probability the proportion of particles which would succeed in moving any perceptible distance would be imperceptibly small.

Proceeding with experiments upon many solutions and liquids, this fact came out by degrees more and more clearly—that it is pure water which exhibits pedesis in the highest perfection. So remarkably is this the case that *the suspension of china clay may be used as a test of the purity of water.* Even the air and carbonic acid, which is usually dissolved in water, produces a perceptible difference. If a milky mixture of china clay and distilled water be divided into two portions—and while one is well-boiled in a glass tube and then corked up, the other is shaken in a large bottle with air, and then left to stand in a similar glass tube —the clay will be found to remain longer in suspension in the boiled water. In a similar way, if china clay be mixed with various specimens of water,—such as spring water, river-water, rain-water, boiled rain-water,—it is possible to detect differences in the rate of precipitation. In experimenting upon such mixtures it will be noticed that the topmost layer of water, to the depth of one-eighth or one-quarter of an inch, becomes very clear before the rest of the liquid : this may probably be attributed to the air dissolved by the water with which it is in contact. The quantity of air absorbed by water is probably quite competent to produce this effect, as it often amounts to several hundred-thousandths by weight of the water.

It has been stated that, as a general rule, all substances dissolved in water tend to prevent pedesis ; but it is easy to detect differences in this effect. The mineral acids—sulphuric, nitric, or hydrochloric—have an extraordinary effect, and it is possible to detect one part of sulphuric acid in a million parts of water by the precipitation of china clay. Caustic alkalies and metallic salts have a less, but still a great, influence. I have tried the nitrates of copper, mercury, and silver, the sulphates of copper and zinc, chloride of gold, acetate of lead, bichromate of potash. Various other acids—such as oxalic, fluoric, hydro-fluosilicic, citric, arsenic acids,—also have considerable precipitating power. Somewhat lower in the scale may be placed such salts as carbonate of potash and soda, chlorate of potash. Among solutions of still less power may be mentioned common salt, sulphate of potash, iodide of potassium, nitrate of potash, cyanide of potassium, ferrocyanide of potassium, lime-water, &c. My recorded observations amount to nearly eight hundred, and the solutions named were tried not only in different strengths, varying according to circumstances, from one part in ten to one part in a million, but they were tried with various suspended powders, such as charcoal, red oxide of iron, amorphous phosphorus, precipitated carbonate of lime, red oxide of lead, black oxide of manganese, and occasionally with other substances. I do not think, then, that I can be much mistaken in my chief conclusions.

While salts and substances dissolved in water prevent pedesis, as a general rule, there are certain remarkable exceptions of great significance. One of these is pure caustic ammonia, the liquor ammoniæ of the apothecaries. One per cent of ammonia will be found to have little or no effect in precipitating powders from water. The compounds of ammonia are quite different from ammonia itself, and, so far as I have observed, all have precipitating power, though apparently less than that of most other salts. Other very notable exceptions are boracic acid and silicate of soda. Upon these, especially the latter, I have tried many experiments. With 1 per cent solutions of these substances clay was observed to remain in suspension for twenty-four hours, and in some experiments a slight cloud of clay remained after five days. Indeed I believe that silicate of soda has a positive retarding power, and prolongs the suspension of fine particles. This salt will also neutralise the precipitating power of the acids: when clay is mixed, for instance, with water containing one per cent of silicate of soda and one-tenth per cent of nitric acid, the acid combines with the soda, liberating silicic acid. Now soluble silicic acid has no perceptible power in preventing pedesis, as proved by experiments upon two samples of very pure silicic acid, carefully prepared for me by Prof. Schorlemmer, of Owens College, for which I am much indebted to him. Clay, mixed even with a *concentrated* solution of *pure* silicic acid, will show pedetic motion of minute particles in the microscope.

One of the most extraordinary facts connected with this subject is the power which gum arabic possesses of increasing the pedetic motion. The more inactive substances, such as powdered galena, oxide of iron, or carbonate of lime, when examined with a weak pure solution of gum arabic (say 5 per cent), exhibit much motion, and there is the corresponding absence of precipitating power, or rather a power of maintaining substances in suspension. It is obviously on this account that gum arabic is an invariable component of common ink, though the quantity specified in various recipes ranges from about 1 to 5 per cent. The extraordinary pedesis seen in old ink is due to the gum arabic, and perhaps also that seen in gamboge.

It will not be difficult now to arrive at an explanation of the pedetic motion. When we compare the substances which do not prevent the motion with those which do, it becomes apparent that, with some doubtful exceptions, they differ widely in the power of making water a conductor of electricity. The following is a quotation from one of Faraday's earlier researches:*—"Some acids, as the sulphuric, phosphoric, oxalic, and nitric, increase the (conducting) power of water enormously, whilst others, as the tartaric and citric acids, give but little power; and others, again, as the acetic and boracic acids, do not produce a change sensible to the voltameter. Ammonia produces no effect, but its carbonate does. Sulphate of soda, nitric, and many soluble salts produce much effect. Percyanide of mercury and corrosive sublimate produce no effect; nor does iodine, gum, or sugar; the test being a voltameter."

The argument in the case of pedesis is exactly analogous to that which Faraday employed in his inquiry into the production of electricity by Sir W. Armstrong's electrical boiler. Faraday found that the machine must be supplied with pure distilled water in order to yield much electricity. The smallest drop of sulphuric acid, or a little crystal of sulphate of soda, dissolved in the water, prevented the evolution of electricity. "So also did the addition of *any* of these saline or other substances which give conducting power to water." Then Faraday argued in this way:—"As ammonia increases the conducting power of water only in a small degree, I concluded that it would not take away the power of excitement. Accordingly, on introducing some to the pure water in the globe, electricity was still evolved. But the addition of a very small portion of dilute sulphuric acid, by forming sulphate of ammonia, took away all power."† Even common town's water was found by Faraday to be unsuitable to the production of electricity. The analogy of these circumstances to those of pedesis is so remarkable that little doubt can be entertained that the same explanation applies. *It is perfectly pure water which produces electricity and pedesis.*

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* Experimental Researches in Electricity, vol. i., p. 432, § 1355; see also p. 161, § 554.

† *Ibid.*, vol. ii., pp. 110, 111, § 2090 to 2094.

Almost all soluble substances prevent both one and the other phenomenon; but ammonia is one of a few exceptions—it allows both of electric excitation and pedesis. Boracic acid is another exception, and gum a third one. There are difficulties, no doubt. Silicate of soda and silicic acid were not tested by Faraday. Tartaric, citric, and acetic acids are mentioned by him as giving little conducting power to water, but they arrest pedesis in a considerable degree. But it is to be remembered that the conducting power of liquids and the connected effect of electrolysis are very imperfectly understood, and few researches on the subject have been undertaken since these old ones of Faraday.

The proper way of settling the question, no doubt, is to make direct experiments upon silicic acid and the other substances which allow or promote pedesis. This I attempted to do, immersing two platinum points, about 1-60th of an inch apart, in various solutions, and connecting them with two of Grove's cells and an ordinary galvanometer. The results did not altogether agree with the theory, but there are many doubtful points about the matter. It does not follow that the conducting power of a liquid through 1-60th of an inch, with the power of two of Bunsen's cells, will correspond to the conducting power through 1-5000th of an inch at a very low tension; and there are other difficulties in the matter. In spite of some discrepancies and failures, I still think the analogy between pedesis and Armstrong's electrical machine so strong as to leave little doubt that pedesis is an electrical phenomenon. Various reasons may be given for regarding this conclusion as probable.

There is no doubt that pure water may produce electric tension. If pure water be put into an ordinary electric battery, the tension produced is supposed to be the same as with dilute sulphuric acid. In fact the acid is only added to make the water a conductor, without which the current is imperceptible. Then, again, there is no doubt that, when water is in contact with silicates, some chemical action goes on, for silicic acid is invariably found in all water which has been in contact with the earth. It is perfectly well known that all clays, and also all rocks exposed to air and water, decompose slowly, as Bywater acutely remarked. The action is very slow, so that there is nothing absurd in supposing that chemical action may go on for eight years, or more, as in the case of my tubes of china clay and water. The silicic acid dissolved out of the clay promotes rather than prevents pedesis.

In attempting to explain the exact *modus operandi*, we cannot rest, as hitherto, upon experiments. We can only speculate that the action upon a minute irregular fragment will never be exactly equal all round. Differences of potential will exist, owing, in the first place, to the non-homogeneous nature of the particle. In accordance with all known laws of electro-dynamics, motion of the particle will result, which, by exposing new points of the particle to the action of fresh liquid, will alter the potentials, and lead to motion in a new direction. In order that a particle shall rest motionless in a non-conducting fluid, it must be in exactly equal chemical and electric relation to the fluid on all sides. That this should happen is almost infinitely improbable. A condition of unstable equilibrium within limits is the result—a condition partially analogous to that of a ball suspended on a jet of water, which is always falling off one way or another, but is always brought back again.

Having once begun to speculate it is easy to go a little farther, and to point out that there is probably a close connection between pedesis and the phenomena of osmose so carefully investigated by Graham. The connection is probably that of action and reaction; for if a liquid is capable of impelling a particle in a given direction, the particle, if fixed, is capable of impelling the liquid in the opposite direction by an equal force, just as a steamboat, if not allowed to move forwards, will throw the water backwards. The earthenware jars used by Graham in many of his experiments consisted of silicious minerals, which would give pedesis in great perfection; and Graham came to the conclusion that whether the septum consisted of earthenware, animal membrane, or other substance, the motive force arose from chemical action upon the matter of the septum. The membrane was always corroded to some extent—a fact which quite accords with my explanation of pedesis. The solutions employed by Graham were also such as would admit of this view, containing usually about 1 per cent of the salt. It is obvious that if a pedetic particle be in contact with precisely the same kind of liquid all round, there would be no determinate direction of motion either of particle or of liquid; but if one end of the particle be immersed in different liquid from that which surrounds the other end, a tendency to motion in a determinate direction will result, and this would be the condition of things in the porous earthenware jars of Graham. I do not pretend, however, to explain the exact *modus operandi*, which must be left to some one well versed in electrical science, but it is probable that the ultimate explanation must take into account the remarkable phenomena of electric osmose investigated by Wiedemann.

It was first observed by Porret that when the poles of a battery are placed in two portions of water separated by a porous division, not only is some of the water decomposed, but another and far larger portion is impelled towards the negative pole. Wiedemann found that for 1 part of water decomposed, 5000 parts were transported through the porous septum. This impulsion is greater as the electric resistance of the liquid is greater, and ceases altogether when sufficient acid or salt is added to render the liquid a good conductor. There is an obvious analogy to the circumstances of pedesis, except that the energy is derived not from the septum, but from a cell. If these surmises should prove correct the phenomenon of pedesis will acquire great importance, because it was believed by Graham and others that osmose plays a large part in the functions of life, probably causing the motion of sap in plants and of juices through the walls of animal cells and vessels generally. It is certain that the solutions of gum, albumen, &c., which permeate the vessels both of animals and plants, are generally well fitted for the production of pedesis. From this point of view it is much to be desired that the inquiry should be extended, so as to include the relations of organic particles to solutions both of salts and of organic substances. I have hardly been able to enter upon this part of the subject. The motion of particles of gamboge has already been mentioned, and other organic paints, such as sepia and indigo, give like results. The yolk of an egg rubbed up in water showed intense pedesis. These motions are far from being prevented when the medium is a solution of albumen, gum, glycerin, gelatin. Some experiments seemed to show that, in the presence of these organic substances, small quantities of acid or alkali would not prevent pedesis. Observations of this kind duly extended might have a most important bearing upon many motions, such as that of cyclosis observed in vegetable cells, or even upon the whole subject of the motion of fluids in vegetable and animal organisms. I tried a few experiments with the sap of a tree, and found that gamboge rubbed up with it gave *a perfectly boiling appearance*. Other organic mediums—such as alcohol, ether, amylic alcohol, chloroform, spirits of turpentine, wood spirit, &c.—seemed to arrest pedesis; but they ought to be much more carefully tried with different suspended particles, and in mixtures of different proportions with water.

It is very interesting to follow out the effects of this pedetic motion from the geological, physiographical, and economic points of view. Its importance is immensely great, and it is not too much to say that the surface of the globe would be very differently shaped were the phenomena otherwise than they are. The disintegrating and carrying power of pure water mainly depends upon the pedetic motion, which, as we have seen, prevents rapid aggregation and subsidence. Now, the rain-water which falls upon the surface of the earth is sufficiently pure to produce pedesis in considerable perfection. Every little stream and pool of muddy water bears evidence to this effect, and fresh water as it flows down the rivers, and even into the sea, continues to hold great quantities of silicious particles in suspension. But no sooner has the fresh water become mixed with salt than the pedesis is destroyed, as I have ascertained by direct trial with sea-water, and the particles subside rapidly. Thus we account for the wonderful clearness of ocean-water, and the fact that the ooze from the ocean bottom consists almost entirely of organisms produced out of the substances contained in solution in the salt water. In the same way we may account for the peculiar clearness of alkaline solutions, with which I have often been struck. A pan of brine at a salt-works, for instance, if allowed to stand undisturbed for a little time, becomes as clear as crystal. This is due to the perfect subsidence of all foreign particles, in spite of the greater specific gravity of the solution which would tend to buoy them up. In the same way we may explain the dark blue colour of the Dead Sea, which is due to the clearness of the dense solution.

It now becomes easy to understand the peculiar turbidity of glacier streams, for the water proceeding from melting ice is in the most perfect condition for producing pedesis. I have examined water derived from melting snow, and find that the contained air is not sufficient to diminish the sustaining power much, and I have shown that the suspension of clay is continued longer in ice-cold water than in warmer water. That the particles derived from glacier erosion exhibit pedesis in a high degree is evident from the silicious nature of the rocks eroded, but I have confirmed this inference by direct trial upon the sediment of glacier streams both in Switzerland and Norway. As the water from a glacier becomes gradually warmed and aërated, the particles subside and the water becomes clear; but there is some difficulty in understanding how this process is carried out with sufficient rapidity to produce the clear blue water of the Lake of Geneva. It is well known that for domestic purposes water ought to be well aërated, so that the water from a pure rivulet is better than that taken direct from a mountain bog. This is at least partly due to the decrease of the suspensive power by the dissolved air; and the brilliant crystal clearness of well-water is probably to be explained by the mineral salts held in solution, which favour the throwing down of all sediment.

III. ENERGY AND FEELING: ALTERNATE AND MUTUALLY CONVERTIBLE AFFECTIONS OF MATTER.

By W. S. Duncan.

IN the philosophy which is the offspring of modern science there is believed to be but one substance with twofold properties or affections. That substance is matter, and its twin properties are energy and feeling.

The present position of physico-psychology, however, is logically imperfect, inasmuch as it places an impassable gulf between the two manifestations of matter—energy and feeling. These two affections, it is said, lie side by side, and in parallel lines. Energy never once begets feeling, nor does feeling ever beget energy; they never even affect each other in the slightest degree. Language of this import has been used, even quite recently, by some of our highest scientific authorities and by clear thinkers, though admitted by these authorities to be not unattended by difficulties.

But a philosophy such as that is not only attended by difficulties, but is so illogical, so discordant with human language and experience, that I feel sure it must ultimately give place to one much more consonant alike with sound logic, scientific truth, and human experience. First, what does experience say? Do we not say, when we wish to explain our actions, "Such an idea, or such a feeling, was my motive for so doing,"—meaning that the subjective motive preceded, and was the cause of the physical action? Do we not know that most of our actions are aimed at subjective results, as so much pleasure or so much pain, rather than at mere physical or material well-being? Has not the ethical basis of the "*happiness* of mankind" been approved of by the highest scientific authorities as the best principle of moral action? Why, then, should mere physical energy aim at *subjective* results unless it càn *cause* them?

But Science replies, "The doctrine of conservation of energy and the fundamental difference between energy and feeling exclude the intermixture or alternation of these two affections of matter." If any theory involved the rejection of the doctrine of conservation of energy, it would of course condemn itself, for nothing in Science is more satisfactorily established than that doctrine. But this objection could not apply to a theory which described energy and feeling as alternate and mutually convertible affections of matter; for if energy merged in feeling, and feeling in energy, no energy would be lost, and no new force imported into the realm of Nature. Spontaneous origination of energy or feeling would be both alike unknown. The objection that energy and feeling are fundamentally different is at first an apparent obstacle to the acceptance of a theory of alternation. But this objection is only valid if both aspects of matter are viewed from *different* standpoints, the one objectively, the other subjectively. View both from the same standpoint, and the difficulty vanishes.

I, a part of matter, experience that alternation of feeling and action as if my actions proceeded from my feelings and my feelings from my being acted upon; why may it not be so with every other part of matter, as well as that I call "me"? To feel and to energise seem both to be very much akin, if we regard matter as a living substance, and exclude from it the old notion that it requires something immaterial to move it and something immaterial to feel its motion. But it is also objected that the moment a part of matter is moved by a neighbouring part, the former moves and feels simultaneously; that No. 2 movement is the true consequent of No. 1 movement; and that feeling is merely *concomitant* in time, and never intervenes between movements 1 and 2. Now, is this objection sound? Does not the swiftest known force take time for its passage? If so, we have but to divide the whole time by the number of parts taking share in a vibration, when we have the mathematical equivalent in time which elapses between the receiving and the discharging of an impulse by an elementary part, and we may still farther divide this time into the parts required for the duration of feeling and acting respectively by the elementary part. Now receiving energy takes *precedence* of discharging energy, as is very obvious, since no force originates spontaneously. That the affection of matter in the receiving stage is somewhat different from that in the discharging stage is implied by the very term "receiving," being suggestive of passivity in contrast with the discharging stage, which latter stage alone answers to our notion of activity or energy.

Thus the fact of precedence in time between receiving and discharging energy removes the objection of apparent concomitance of feeling with energising states of matter, leaving time for the fact of alternation to transpire. Then the difference in nature between receiving and giving helps us to recognise affections of matter differing as much as do our ideas of the nature of feeling and acting respectively; while the possession by ourselves of powers to feel and act alternately as characteristic living powers helps us to understand how Nature, in part and in whole, may be endowed throughout, and perpetually manifest these twofold affections of feeling and energising in continuous alternation.

Let us consider what are the advantages of this theory of alternation of feeling and energising affections of matter.

First.—It agrees with human experience and language.

Secondly.—It seems logically more consistent than the theory of concomitance.

Thirdly.—It preserves the doctrine of conservation of energy, and agrees with the great laws of continuity and causation in all their manifestations.

Fourthly.—It requires no exclusive confinement of consciousness to ganglionic matter, to a central nervous sensorium, or even to organised matter at all, but places feeling in all matter at the stage of receiving energy.

Fifthly.—It does not involve intelligence while predicating sensibility of all matter, for the terms feeling and intelligence are far from synonymous, the former being simple and elementary, while the latter is compound or complex. But it teaches us to search for intelligence elsewhere than in man and animals: for wherever groups of force channels are organised, there groups of feelings are to be inferred,—therefore intelligence; for intelligence is the product of groups of feelings in one organised whole.

Sixthly.—In the event of discovering intelligence higher than man's the old polytheistic notions may not revive, inasmuch as those superior intelligences, equally with ourselves, must be subject to the great law of causation.

Seventhly.—Much of the mystery of adaptation in things, many of them removed distantly in time and space, seems removed; for if feeling alternate with energy, what wonder that they should result in adaptations favourable to subjective as well as physical harmony or well-being.

Eighthly.—Nature is regarded in the light of the theory of alternation of energy and feeling, as a living whole, conscious as well as active; conscious because active, and active because conscious. How infinitely superior is it to the theory that regards Nature as the home of a sort of Siamese twins,—energy and feeling,—accompanying and complimenting one another, yet quite unnecessary the one to the other,—the theory of concomitance!

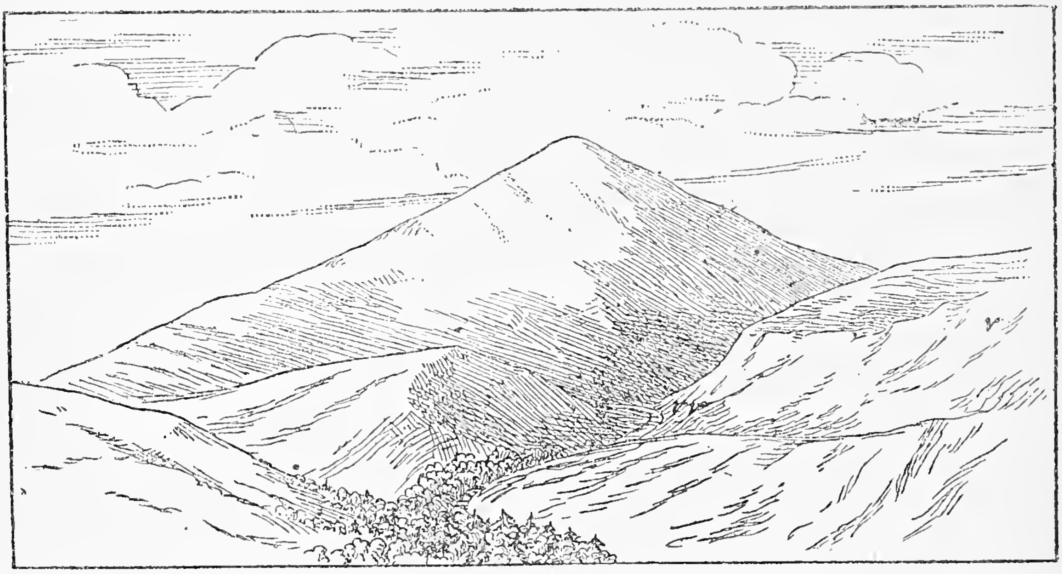
IV. THE GOLD AND PLACER MINES OF WICKLOW.

By G. H. Kinahan, M.R.I.A., &c.

LENIGAN and other English writers represent the Ancient Irish as perfectly ignorant and barbarous. Nevertheless, from the Annals, and the "finds" of gold ornaments and weapons in numerous localities, it would appear that at a very early age the use and manufacture of the precious metals were known to the natives of the country. That gold was much more abundant in Ireland than in England is shown by the fact mentioned in De Larne's "History of Caen," when, after the Norman Conquest of the British Islands, treasures were exacted from both to the exchequer of Normandy; the tribute exacted from England was 23,730 marcs of silver, but from Ireland 400 marcs of silver and 400 ounces of gold.

According to Dr. Joyce ("Origin and History of Irish Names of Places") the earliest known record of the manufacture of gold in Ireland was about 1000 years before Christ, when the monarch Tighernmas caused goblets and brooches to be manufactured by an artificer named Uchadan, who resided in Fercualan, or that portion of Wicklow which is now the Barony of Powerscourt. Besides, other monarchs are also mentioned who had gold manufactured, or who introduced the use of golden ornaments. On account of the abundance of gold in Dublin and Wicklow in ancient times the inhabitants of this portion of Leinster were called Laighnigh-an-oir, or the "Lagenians of the Gold." as mentioned by O'Curry in his lectures (Lecture 1, p. 5.)

From the records in the Annals it would also appear that the ancient placer mines were principally situated in the Dublin and adjoining mountains. In these hills in recent times very little gold has been found. Now and then pieces of gold are picked up in Glenismole, or the upper portion of the Valley of the Dodder; and recently in Stephen's Green, Dublin, a small nugget was found in a load of gravel brought from the Dodder Valley. In some of the valleys eastward of Blessington, on the Liffey, are broken patches of ground that have an appearance like the sites of ancient placer mines; and farther south, workings of the ancient Irish were discovered about thirty-five years ago, in the placer mines at the Hill of Lyra (*anglice*, junction or fork of two glens), townland of Knockmiller, nearly a mile S.W. of Wooden Bridge. In connection with these ancient works, under a depth of about 5 feet of meteoric drift, a black oak frame was found. It was 14 feet long by 10 or 12 feet wide, the shorter beams being morticed into the others about a foot from the ends, and on the outside of all the beams were carving of animals, some having men mounted on them. These consisted of "animals like mules with long bob-tails, others like goats or deer, and some of a nondescript character." The beams were cut up to make ground joists for the east wing of Wooden-Bridge Hotel.



Croghan Kinshella. *r*, Iron mine; *rr*, Gold placer mines.

The Lusceans, or the inhabitants of the Lusca or Earth-caves, must have been rather a primitive race, if we are to judge from their habitations, that were rarely high enough for them to stand up in, and into most of which they had to creep through passages scarcely 2 feet high; yet in these caves most highly-wrought gold ornaments and weapons are not uncommon; while under some of the deep bog they also occur. Of the "finds" in bogs, those in the Bog of Cullen, at the junction of the counties of Tipperary and Limerick, are the most remarkable, as here were found not only innumerable golden articles, but also the various implements used by the goldsmith, such as crucibles, ladles, and the like, among the "corkers" or roots of the trees of an ancient forest.

As pointed out by O'Curry, this locality seems to have been the habitat for years of a race of goldsmiths, who carried on the manufacture from one generation to another in the wood there situated, long before the bog began to grow. O'Curry has tried to identify these goldsmiths with a race of artificers whose genealogy is given in the book of Lismore, who were direct descendants from Olioll Olum, king of Munster, and followed the trade from about A.D. 300 to A.D. 500; but the thickness of the bog over the ancient forest, among the remains of which the articles are found, would seem to suggest a far greater age.

The numerous and rich "finds" in the Bog of Cullen during the last two hundred years has made it proverbial in Munster and celebrated in song.

It would seem that after the conquest of Ireland by the English the existence of gold in the country was unknown or forgotten; but in recent years it was remarked that from time to time the natives of Wicklow brought up small quantities of gold to sell in Dublin. This did not create much inquiry till 1795, when a large nugget was offered for sale. The exact weight of this is uncertain, some say 21½ ounces, others only 18; while some authorities mention two large nuggets. Whatever its weight, on enquiry it was found that it had been picked up by a girl driving cattle over the ford of Ballinasilloge, in the stream now called the Gold-Mines River. This runs eastward from Croghan Kinshella to join the Aughrim River, and eventually the Ovoca, at Wooden Bridge.

The find of this large nugget caused a rush to the place, and for over six weeks from 600 to 700 people were working in the valley and neighbouring streams, till the Government, fortified by a special Act of Parliament, took possession, and established more systematic placer mining, under Messrs. Weaver, King, and Mills. The Government mining seems to have been mostly carried on in the Gold Mine Valley, while at the same time adventurers were working in the neighbouring streams of Ballintemple, Clonwilliam, Knockmiller, Coolballintaggart, a stream on the other side of the hills running north from Croghan Kinshella, and, farther northward, at Mucklagh, Ballinagappoge, and Sheanmore, the last four being on tributaries of the Aughrim River.

The Government Works continued till May, 1798, when they were interrupted by the Rebellion, and were not resumed till 1801, after which time, for reasons fully stated by Weaver in the "Transactions of the London Geological Society," a level was driven into the east side of Croghan, while miles of trenches or "open cast" were made round its summit, in search of the "mother rock" of the gold: all these works resulted in failure, for, although numerous veins of quartz were discovered, not a particle of gold was found *in situ*. After the failure of the trials the Government were advised to abandon the works. Since then placer mining has been carried on by companies and private individuals, but not successfully; Prof. W. W. Smyth, in his Report states "partly it may be presumed from the rarity of the precious metal, and partly from the difficulty experienced in all gold-streaming or gold-digging regions of obtaining from the workmen the full produce of their labours."

The Government placer mining, both before and after the Rebellion, is said to have been remunerative, and the quantity of gold returned was 944 ounces, of a total value at the time of £3675; the assay of 24 grains, by Weaver, gave pure gold 22·58, and silver 1·43; while a second, by the Assay Master of the London Mint, Mr. Alchorn, gave, for the same weight, 21·375 fine gold, 1·875 of silver, and 0·375 of an alloy of copper and iron. The streamings carried on prior to those conducted by the Government are said to have been even more remunerative, and Sir R. Kane states that it has been calculated that over £10,000 were paid for the gold sold by private individuals.

The minerals found with the gold, as recorded by Weaver, are magnetic iron ore (sometimes in masses over half a hundredweight), titaniferous iron, specular red and brown iron-ores, pyrites, tin-ore and wolfram, manganese ore, garnet quartz, and lepidolite; to which Mr. Mallet has added platinum, galenite, chalcopyrite, molybdenite, sapphire, topaz, zircon, and spinella ("Journ. Geol. Soc. Dublin," vol. iv., p. 271). This observer seems to have come on an extraordinary prolific "run," as he records 3·5 lbs. of tin ore from 150 lbs. of sand; while all other observers have found this valuable ore only in small quantities. It may be mentioned that in the old working, in the wood, tin ore was more frequently found than higher up, or to the south-west, where it is rarer. Tin ore is also recorded for the working at Ballinagappoge, one of the tributaries of the Aughrim River.

Most of the gold is in "eye-sills," or small particles; in some places, however, "large gold" or grains occur, while here and there are nuggets, ranging from 21·5, the larger, to half an ounce; in one placer, a little N.E. of Ballinasilloge ford, many nuggets averaging from 3 to 5 ounces, one being 11 ounces, are said to have been lifted. In Ballintemple there was a great deal of gold, but it was nearly all in "eye-sills." The placer of Knockmiller is said to have been very productive, "large gold" and "eye-sills" occurring together, while in Coolballintaggart "large gold" was principally found.

In the "Elements of Geology," recently published by Prof. Le Conte, of the University of California, we learn that the California placers are below slopes on which there is always an outcup of an auriferous quartz vein. In Wicklow no auriferous quartz vein has been discovered, but in all cases the placers are below the slopes on which certain iron ore lodes crop out. On this account all the different observers seem to believe that there must be relations between these lodes and the gold in the placers; furthermore, fragments of these ores always occur in the sand associated with the gold. However, in none of the lodes above the placers has gold been found, although farther north-east, west and east of the Ovoca, it occurs in the gaussen at Ballymurtagh and Cronebane, and in the remarkable mineral Kilmacoite, or "silver-blende," at Connarry.

The major portion of the gold is abraded, and apparently has been drifted to its present site; but some nuggets and many of the eye-sills are frosted, as if they had grown in the drift, similar to some of the gold found in the "deep placers" of California; other pieces are *attached to quartz*, especially in the vicinity of the "Red Holes," a swampy patch at the mearing of Ballyvally and Ballinasilloge, as if somewhere in the latter townland there is a still undiscovered auriferous quartz vein. It must, however, be allowed that the chances in favour of the latter are small, as hundreds of tons of the quartz erratics were brought to Ballintemple, and crushed, without even a particle of gold being got to repay the trouble and expense.

In the Red Hole Mines, at present being worked by Mr. F. Acheson, we learn that the surface-accumulations in the ravine consists of—above meteoric drift, under which is water-formed drift, and at the base of the latter in places is the auriferous or "black sand." The black sand is found in "runs" or lines, and these occur in channels or slight hollows that have been denuded or worn out along nearly parallel lines of dislocation, or master-joints, in the underlying Cambro-Silurian rocks. The miners know when they are coming to a "run," as the lamination of the "bottom rock" is "crumpled," as the twisting and breaking-up of the rock adjoining the fault or dislocation lines is locally called. In each section of the valley these breaks have general bearings, so that when the direction of one run is known all others are nearly parallel. One set of parallel breaks may, however, be crossed by another, and at the junction of the two systems the channels are deeper, and consequently in such spot more gold collects than elsewhere; so that on a map of a placer the rich spots occur somewhat like the corners of the squares on a chess-board, only more oblique to one another.

In the Red Holes Mine the surface of the "bottom rock" in general is ground smooth, as if a rapid torrent had ran over it for years. In the overlying water-drift all the rock fragments are abraded, and rarely—even on the Gabbro, quartz, and other hard rocks—was a trace of ice-work detected; but a few ice-dressed fragments do occur in the higher meteoric drift. It would appear that when the ice was melting off the Wicklow hills great torrents were flowing down the different ravines, and when the ice had all melted and the water-supply was gone the torrents dried up, while subsequently the marginal cliffs of the ravines weathered into slopes, their detritus forming the meteoric drift now found above the water-drift.

All the modern mines in the neighbourhood of Croghan Kinshella are "shallow placers," the deepest being less than 30 feet, while no deep trials have been made. In nearly all other gold regions the precious metal has been worked not only in shallow but also in "deep placers," and, if we may reason from analogy, it appears probable that there are vast supplies of unknown auriferous sands under the deep river and estuary accumulations in the flats at Wooden Bridge, and other places in the valleys of the Ovoca and its tributaries.

V. ON THE RELATION OF MOISTURE IN AIR TO HEALTH AND COMFORT.*

By Robt. Briggs, C.E.,
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It may be accepted that the most pleasant condition of the air, in our portion of the globe, will be found to exist on a fair day in early summer, when the temperature ranges from 62° to 68° F., and the moisture present in the air is from 85 to 80 per cent of saturation. On a day like this no thought of the weather is taken, and the passage of the conversational compliment of a "fine day" becomes a needless reminder, to be accepted without discussion or thought. Admitting this proposition as a fact, it is the purpose of this paper to show that in our climate this summer condition of relative heat and moisture is not desirable, or even attainable, at other seasons, in the ventilating or heating of occupied places. And in presenting this view to the American Society of Architects I do so with a full knowledge that it does not accord with the opinion of most American writers on the subject of ventilation, who have derived their information and their arguments mainly from the study of English and French books, and have only endeavoured to reconcile the data found in these to American wants and practice. Even Wyman, who is by far the most original as an observer, as well as the most thorough as the collator of information from all sources, hardly makes the distinctive effect of our low temperature, combined with a comparatively lower dew-point, sufficiently evident.

Except one investigates the relation of moisture to temperature of air in the two countries, it is impossible to reconcile our facts with the statement of good foreign authorities,† that 56° is comfortable, and 62° is warm, in

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[*] Paper read before the Am. Institute of Architects, at Boston, Oct. 18th, 1877. From the Journal of the Franklin Institute.

[†] The comfortable warmth of air indoors is given by various authorities, as follows:—Peclet, Traité de la la Chaleur, gives 15° C., 59° F. Morin, Etudes sur la Ventilation,—for nurseries, schools, &c., 59°; hospitals, 61° to 64°; theatres, assembly halls, &c., 66° to 68°! Tredgold, Principles of Warming and Ventilating, &c., 56° to 62°. Reed, Illustrations of the Theory and Practice of Ventilation, 65°. Hood, Treatise on Warming Buildings, &c., inferentially 55° to 58°. Parkes, Manual of Military Hygiene, 48° to 70° (this author has encountered the difficulty of naming a fixed temperature, and avoids the issue). Box, Practical Treatise on Heat, 62°. Others might be quoted, but these are amongst the best authorities on Heating and Ventilation.

living rooms in mid-winter; while the American shivers with cold at 70°, and is not over-warmed at 76°, in the apartments of his own dwelling, although clad in the thickest of under-clothing. Investigation, however, shows that the deprivation of heat from the person is more due to evaporation from the lungs or throat, and from the skin, than from heat otherwise dispersed, whether carried off by the breath, imparted to the air, or radiated to surrounding objects. And further investigation will show that the hygrometric state of the air has so much effect in inducing or retarding evaporation as to make 56° F. in the West and South of England, in Ireland, and in Normandy, sensibly as warm as 80° in Canada or Minnesota at the same season. A brief statement of the difference of climatic condition of England and of America may show why we cannot import English theories of ventilation and heating, and apply them at once, without modification, to American residences. The English climate affords nearly eight months in every year when the thermometer ranges between 40° and 60° in the shade, with a dew-point so high that it is a pleasure to exercise in the invigorating air; one month of 60° to 80°; and three months from 25° to 50°; there being no term, except a part of the one month of heat reaching to 80°, when any person cannot, with suitable clothing, enjoy the open air. While in America there is scarcely one month (or thirty days) out of the year having an average temperature of 50° to 75° (which temperatures, from the difference of dew-point, correspond sensibly with 40° to 60° in England); and there are three months of 75° to 90°; three months of 30° to 50°; and five months of excessive variation of temperature of from 0° to 50°. During the three hot months, and also during most of the five cold, open-air exercise to those whose avocations are within doors is, if not impossible, at least very uncomfortable, however clad or covered. Anyone who is called upon to endure the fervid summer heat, or who can habituate himself to the inclemencies of our arctic winter, will not suffer great discomfort, nor experience much injury to his health therefrom; but the weak and tender—the merchant from his counting-house, the student from his closet, the workman from the shop, the women and children of the house—cannot acquire the endurance or the habit, and must shelter and protect themselves.

This preamble to the subject has been intended to impress the fact that its consideration must be on its own merits, and not through the light thrown upon it by general writers, that its investigation shall be original from physiological considerations, and not based upon authorities.

§. Comfort, if not existence, depends upon a constant loss of heat from the person. The internal natural warmth of the body is very nearly 100° F., regardless of the heat of the external air; and the personal comfort which proceeds from the temperature and humid condition of the air, proceeds from the cooling effect which must then occur with constancy and regularity, and yet not so fast as to produce the sensation of cold. The origin of the natural heat is well established. There is inhaled by each adult in comparatively still life, each three to four seconds, from 30 to 40 cubic inches of air, at such temperature as may exist at the place, with extreme differences of temperature ranging from −40° to +140° F., and with extremely variable proportions of humidity, from the point of saturation on the one hand to that of nearly an anhydrous air on the other. The practical extremes in our country are from little below zero to about 100°, accompanied also with great variation of humid condition. A portion of the oxygen of the inhaled air is consumed in the system, and the exhalation, which follows each inhalation, emits about 4 per cent of carbonic acid and 1½ per cent of vapour of water. Two or three grains of carbon are consumed in the system each minute, giving out 3¾ to 5¾ units of heat, the unit of heat being the equivalent to a pound of water heated 1° F. It is the dispersion of this heat which establishes the sensation of comfort. Modern theory has established the convertibility of heat to work or power, and some portion of the heat evolved by the air of respiration will have been converted into labour or effort, but far the greatest portion will have been utilised in preserving the temperature of the body from the losses by evaporation of moisture, by conduction, and by radiation. One portion of the loss is readily estimated. The breath is inhaled at whatever temperature and humidity may subsist at the place, but is exhaled at all times at 90° (when the temperature of the air is not above that degree), and it is saturated with moisture at that temperature. If it is supposed that the temperature of the external air is 62°, and the dew-point 54° (= 65 per cent humidity), from 0·35 to 0·56 unit of heat will have been expended in evaporation of moisture in the lungs and throat, and 0·10 to 0·17 unit of heat in imparting heat to the exhaled air each minute. About three and a half times as much heat will have been expended in supplying the moisture as in heating the air. The loss of other portions of heat cannot be as definitely estimated. It is evident that it must mainly be dispersed from the skin, and it is pretty certain that a large, if not much the larger, portion must pass off in insensible perspiration, which will be greatly affected by the condition of humidity of the surrounding air. Here clothing becomes an important element. We protect ourselves against the inclemency of winter, or the heat of summer, by coverings more or less non-conducting or non-radiating, leaving but a small portion of the person unprotected by direct exposure. An almost instinctive preference is given by all people, of all times, and at all places, for porous clothing; even the skins which clothe the inhabitants of the coldest regions, although quite impervious to moisture and to currents of air, are very open for the passage of vapour of water, or of diffused gases. Evaporation, and consequent cooling of the skin, takes place in great measure, or is influenced by the relative vacuum which the quantity of vapour present in the air establishes. The transfer of vapour is then one of diffusion, and follows the law of diffusion of gaseous bodies. A partially anhydrous air, external to the clothing, is a partial vacuum to the vapour of 90°, existing in ducts of perspiration, and this vapour rushes towards the vacuity without encountering the resistance of any circulation, and meet no considerable obstacle in the porous coats and overcoats. It is in this way possible to explain why, in mid-winter, with the room from 65° to 70°, heavy underclothing is not only endurable, but necessary. The overcoat may be removed on entering the well-warmed house, but no discomfort follows from the retention of warm garments that, with a summer condition of air of the same temperature would be oppressive. We sleep in rooms which, if not warmed to the full heat of our living rooms, have yet the "temperate" point of indication by the thermometer, and in this case enjoy a pile of bed-clothing, which would be suffocating in weather of the same natural temperature. The American requirement of comfortable drawing-room clothing is strikingly different from that of England. The ladies' English drawing-room dress is an impossibility in America. Even the rigorous laws of fashion fail to conform themselves in this case, and yet our American drawing-rooms are *hotter* than the English ones.

What proportion of the heat generated by formation of carbonic acid to be dispersed from the body after taking out what is abstracted by exhalation and by labour of work or animal life is expended in vaporisation of water is of course doubtful. Some authorities give 2 to $2\frac{1}{2}$ lbs. of water to be evaporated each day by insensible perspiration. These quantities would give (nearly) 2000 to 2500 units, or $1\frac{1}{2}$ to $1\frac{3}{4}$ units of heat per minute, and, together with the quantity of heat dispersed in breathing (on the previous supposition), account for one-half of the heat produced; leaving one-half the heat to be dispersed in work, life, or conduction to air, or radiation to other bodies. These authorities quoted are, however, English, and it is uncertain what quantity of moisture is evaporated from the skin in heated rooms in mid-winter in our climate.

It is probable that in still air, with the person in repose, the transfer of heat, either from the person or the clothing, whether from radiation or from conduction, is nearly equal; but in any current of air or movement of the individual the effect of conduction will much exceed that from radiation. It should be remembered, however, that a current of air always exists about any person. The comfortable temperature of the air being lower than that of the person, there is established, by the heat imparted to the air by the person, an ascensional current surrounding and enveloping him, sufficiently defined to be measurable by a delicate anemometer, which is effective in augmenting considerably the convection of heat over what would occur in entirely still air. Assuming any comfortable temperature for air between 60° to 80°, the exhalations of breath, by virtue of extra temperature and the presence of vapour to saturation, notwithstanding the addition of some carbonic acid gas of greater density than that of the air, are still so much lighter than the air as to ascend at once after the directional impulse from the mouth or nostrils will have expended itself, which, when the act of breathing has its normal force, and is not made violent by running or exertion, occurs within two feet.

In all cases the sensibility to loss of heat, whether from the breath as exhaled moisture or heated air, or from the person as evaporation from the skin, or as conduction to air, or by radiation to cooler objects, this sensibility, I say, varies in the several regards with different persons, with different races or nations, and, above all, with the habits from business pursuits or occupations, or the customs or fashions of the place of living; any of which causes may and will have established a *régime* in each individual, and their comfort will depend upon conformity thereto. The occupation, business, or habits of individuals as regards their labour or exercise, both when at labour or exercise, or when at rest, cause much discrepancy in demands for heat. In the coldest and driest day, few young persons can fail to warm themselves to the point of comfort in skating; many of the trades demand special temperatures for the workmen, some requiring special temperature and moisture condition of the air for the work, to which temperature and condition the workmen must conform.

There are three means provided for the healthful dispersion of animal heat into the atmosphere; the first is radiation to surrounding colder objects; the second, conduction to the atmosphere, which, for comfort, must be sensibly cooler than the body; and the third is evaporation from the moist surfaces of the lungs, throat, and the roots of the pores of the skin. The first of these means, to the clothed person at least, is comparatively ineffective, while the relative quantities of heat which may be eliminated in any given time or locality, by the two last, will probably be found nearly equal in an atmosphere of about 70° temperature and 65 to 70 per cent. of humidity. In all cases of *excess* of animal heat, the animal, and mankind as an animal, find relief in evaporation of water secreted in the system, showing that vaporisation is the ultimate means of dispersion of heat.

Even the races of animals exhibit diversity of natural methods of dispersing the surplus animal heat. Thus the dog obtains relief through the breathing functions, and extends the surface of evaporation by exposure of the tongue, while the horse breaks out into profuse perspiration of the skin.

The relation of what is indicated by the sense of cold or warm to definite temperature with varied proportions of humidity may be examined at this stage of the argument. Considering a nearly saturated atmosphere, it will be found that its effects differ with the temperature altogether. Such an atmosphere at from 35° to 50° is found to be intolerably chilly, and although evaporation may be checked, and this source of loss of heat be removed, yet the conductive and radiating value of the vapour in the air is now elevated enormously. The cooled surface of the cuticle absorbs the natural heat of the skin, and represses the evaporation of secretions almost entirely. An actual transfer of heat from the skin to the vapour in contact with the surface occurs, the superheated vapour no longer rushes away from the skin in search of that vacuum, which is the accompaniment of a usually low dew-point, but merely transfers its heat to the next particle of cold vapour, which is packed in convenient juxtaposition to receive it; or else an actual movement of the heated vapour effects a circulation or current which brings a new cold particle to receive a new increment of heat. In an atmosphere of this nature the exhaled breath and the exhaust steam from the workshops evolve a cloud of apparent vapour, which must condense in cooling as the air absorbs its heat, for the saturation of the air forbids its absorption as an invisible gas. A Scotch mist of 36° (which is only a supersaturated air with vapour in excess at a slightly higher temperature than the air) penetrates clothing, and reaches every part of the person with distressing frigidity.

Passing upwards in the scale of temperatures from 50° to 65°, the point of equilibrium of cooling action by conduction or radiation of vapour in the air, with supply of heat from checked evaporation of the skin or lungs for attainment of comfort, seems to be reached. Perhaps the most healthful, or at least most stimulating, atmosphere for human breathing is found within these limits, when of natural and continued existence, so that within and without the doors the same condition exists, and the *régime* of the bodily system is not disturbed from hour to hour. This, if not the ruling climatic condition of English life, at least is the presumed theoretic standard of English writers. Mental or physical exercise alike, either separately or in conjunction, are supported by this condition of the atmosphere to an extent which no inhabitant of the frigid north or enervating south can imagine.

Some curious physiological phenomena accompany this atmospheric condition, one of which is the possibility of use of stimulants of the milder nature (wine and malt liquors), in quantities which would be immoderate in our climate. With the comparative cessation of cutaneous evaporation, it seems as if action of the alcoholic ingredient of the liquors were much changed and rendered more stimulating and less intoxicating.

From 65° to 80° a saturated atmosphere is sultry and oppressive. The surplusage of heat cannot be removed by conduction, and the natural effort of the system is to induce evaporation. The least physical effort produces, in the healthy person, abundant sensible perspiration, and the cooling effect of evaporation of a heated surface of water into a cooler air is the natural remedy. The lassitude and enervation of this step in the scale is eminently unfavourable to mental as well as to physical labour.

Above 80° a saturated air becomes burdensome; it is even questionable if life could be prolonged in a saturated air of 90° to 100°, and it is certain that at some point, not much above 100°, suffocation would ensue when any exertion should raise the animal heat above its normal degree. The deaths in the Black Hole of Calcutta were the result of excess of moisture, rather than of heat or want of air *per se*. There are travellers' tales of regions on the Red Sea, and near the mouth of the Persian Gulf, where men cannot breathe in summer for the heat combined with moisture.

In the same way that the effect of a nearly saturated atmosphere has been examined, that of a very dry one may be investigated. To the sense of feeling all air may be said to be dry below 35°. The small amount of vapour present, and possible to exist as vapour below this point, reduces the conductivity so that the chilliness, to a great degree, disappears, even in a saturated air. Yet even here the cold-producing effect of a high dew-point is felt in a wind, so that from 15° to 35° the N.E. wind of our Eastern States is a very raw one. But, on the other hand, with a dry air from 40° below zero to the freezing-point, the immediate sensation of cold by the active man, well clad with porous clothing, is yet endurable, and with habit becomes almost pleasant. Still these temperatures are not those suited to civilised life, either physical or mental. As has been before noticed, we have in the Northern States about five months of the year when the temperature ranges from 0° to 50°, and consequently when our civilised avocations demand artificial heating. The winter climate of the Eastern, Northern, and Middle States is one of great vicissitudes, with extremes, both of temperature and of hygrometric conditions, following each other rapidly. In the North-Western States it seems that a somewhat greater uniformity of temperature and a much more uniform hygrometry exist during the winter months, but in the Middle Western States the irregularities appear to be as frequent as in the Eastern States. Except that the length of the winter season is a little cut short, and the excessive cold is a little alleviated in the southern portion of the belt of country I have designated, much the same phenomena of climate exist all through the States north of the 40th parallel of latitude. Throughout this territory it has become recognised that the minimum temperature of comfort for heated and ventilated rooms can be stated at 70°, with an admitted—and generally supposed inexplicable, if not unreasonable—demand for 75° to 78° in some localities and at some times.

§. Ventilation means a supply of fresh air to the occupants of a house, workshop, or meeting-room of any kind; and, as a final result, the quantity of such supply needed to attain the desired purity is from 40 to 60 cubic feet of air for each person each minute, where the contents of the rooms can be considered as furnishing a portion of the supply when occupancy is only for a part of the time. Much of the air may not be supplied through the heating apparatus. In cold weather, when the levity of the heated air within a building, compared with the colder air on the outside, produces a great pressure of outer air near the ground, the leakage of air at cracks of the door and window-frames, at the top of the building, and its replacement by colder air through similar apertures at the bottom, furnish a much larger volume of air than is generally supposed. The strong winds also seek such leaks. Some permeability of walls, even of boards well painted, is available for the diffusion of vapour and of gases in a measure. So that the proper quantity of air to be supplied by an apparatus becomes a question to be considered, in each instance, on its own merits. But the fact still remains, that for each adult or child in health, 40 to 60 cubic feet of fresh air must be estimated as provided, either by arrangement or surreptitiously, for each minute they may occupy a room or place, although not necessarily during each minute of the day and night. This fresh air must be derived from out of doors.

Accept, for the sake of argument, the average temperature and dew-point of Philadelphia, in January, February, and March, of 1844, as reported in Prof. Bache's meteorological and magnetic observations. The mean temperature of those three months was 34°, with an average dew-point of 25°; barometer 30 inches, from hourly observations, giving 68·8 per cent of saturation. Using Guyot's Psychrometrical Tables, Regnault's data, 1·57 grains of aqueous vapour exist in a cubic foot of such air. These, in Philadelphia, at this season, are the unquestioned properties of the air from which is to be furnished the fresh air of ventilation. If heated to 70° without increment of moisture, the dew-point remains unchanged, and the same 1·57 grains of moisture appertain to the enlarged volume of air, now increased 8·2 per cent by expansion. The hygrometric condition of this air is but 1·44 grains per cubic foot, or but 18 per cent of saturation. The summer hygrometric condition of air can be derived from the same source. The three months of July, August, and September give 71° average temperature, with 60° dew-point, or 68·3 per cent of saturation. Suppose we take the 68·3 per cent, and consider it the proper condition for the air of ventilation at 70°; it then follows that

5·46 grains of moisture should accompany each cubic foot of air in winter. One more step in calculation, and I have done. A cubic foot of air at 70° weighs 0·074 lb., and if it has been elevated in temperature from 34° or 36°, then 0·635 unit of heat will have been expended in warming the air. Again, if the amount of moisture present in this cubic foot of air has been increased from 1·44 grains to 5·46 grains = 4·02 grains, then 0·612 unit of heat will have been expended in evaporation of water to supply the moisture to vaporise the air. The quantity of heat for warming very nearly corresponds with the quantity of heat for vaporisation! The tension of vapour of the external air at 34°, with 25° dew-point, is 0·155 inch of mercury; that at 70°, with 59° dew-point, is 0·515 inch of mercury. It follows that the pressure tending to diffuse the aqueous vapour from the "hydrated" room to the external air would be 0·365 inch of mercury. The vapour itself, within the room at the same time, possesses but 1-48th the tension of that of the air present, and hence, as it is endeavouring to escape under the pressure of about 25 lbs. per square foot (which corresponds to the 0·365 inch of mercury column), it becomes evident that it would diffuse through cracks, outlets, and even through the passages for supply of fresh air, with great rapidity, and that this ratio of saturation is practically impossible to maintain in any *ventilated* room, or even in any room whatever, as ordinarily enclosed and built.

These estimates and considerations show fairly what would result from the attempt to produce an artificial summer climate in the houses of our Northern States in winter; but while the futility of the effort in its complete accomplishment is made evident by them, it by no means follows that some degree of hydration of warmed air is not the requisite of health or of comfort, and the question recurs—What proportionate hydration is needed for these ends?

§. It is with some reluctance that I refer to the "Theory of Ventilation." The past eighty years have witnessed the growth of chemical science, which, after passing through numerous stages of development, as witnessed by the different nomenclatures, has finally reached the point that only the chemists of continual study can comprehend it, and at which point he who knows most about it is the least satisfied with its present condition.

But thirty or forty years since chemistry was supposed to have unlocked the mysteries of matter, and, by the extension of the simple rules applicable to the gaseous and metallic elements, it was thought that the cause of disease or health was to be discovered. Careful observers then examined, from the chemical standpoint, the constitution of the air, both fresh and vitiated; and writers, with good logical conclusions, enunciated a *theory*, by which it was made evident that chemistry had uncovered the root of disease, and carbonic acid gas was the fatal cause.

The real facts are these:—An adult in still life inhales each minute about 480 cubic inches of fresh air, and exhales 488 cubic inches of vitiated air, of which vitiated air about 4 per cent is carbonic acid, and from which about 19 per cent of the oxygen originally supplied by the fresh air has been abstracted; the original quantity of vapour in the fresh air at mean temperature and hygrometric condition (62° and 65 per cent) will have been increased from 1·1 to 3·08 grains.

Carbonic acid gas was made the scapegoat. It killed dogs at the Grotto del Cane, as was happily exhibited to numerous travellers in Italy at that time—and both before and since, the same unfortunate dog serving to be killed, to the satisfaction of admirers, that had been resuscitated the day before, after the visitors' backs were turned. It was heavier than air, and in some conditions of temperature would not so readily diffuse, but form a layer of distinct gas, like water beneath oil. Altogether it answered the conditions of hypotheses, and it was decided to be vile, deleterious, poisonous. To be sure we devoured it in bread, and drank it in beer or aërated waters, but then the poison was to the lungs, not to the stomach! This theory found promulgators in the lecture-rooms and advocates in the household thirty years ago, and has become to-day the *traditional* belief of the middle-aged and elderly. If a room is hot or close from excess of temperature, or from a crowd of occupants, carbonic acid gas is the difficulty; if malaria is developed in a jail or hospital, or typhus or scarlet fever exist in the dwelling, carbonic acid gas in excess is the poison. "The gas is heavier than air, and must necessarily sink to the floor, where all the air of vitiation will be found." These notions continue to have advocates and supporters to the present time, and the popular lecturer or writer gives a half assent to them to secure the favourable opinion of audiences or readers. But step by step, during the past thirty years, it has come to be perceived that the causes of disease are not to be found with organic matter, and carbonic acid has been removed from its elevated place in ventilation, with the fullest admissions that in the quantities ever present in the living rooms, except by accident, it is quite harmless; and, finally, its presence has been accepted as merely a measure of other more dangerous vitiations, in that as it is a definite product of respiration, and as the proportion present in any room, at a given moment, can be ascertained with tolerable exactness, an indication can be derived thereby of the extent of organic vitiation with some degree of certainty.

The unquestioned theory of malaria, the meaning of which word can be extended to embrace diseases arising from deficient or defective ventilation, to-day, is organic vitiation, and the probability of this theory holding its place in future is, I think, a very fair one. The exhaled and exuded vapour from the human body is known to be laden with organic matter: much of this organic matter is within the range of the microscope, by means of which the local derivation of many particles can be determined; but some of it is in the form of effluvia and odours, which pass the limits of visual observation in the smallness of the atoms, notwithstanding such effluvia or odours are decidedly perceptible to the sense of smell. With a dry external atmosphere and a reasonably free ventilation the exuded vapour and the organic matter pass away, or are diffused as rapidly as supplied. *It should be remarked that the organic matter appears mainly to be in connection with the vapour in the air, and not to exist as a separate gas, diffused in the dry air when the vapour is removed by natural causes.* With an imperfect or insufficient ventilation the upper parts of rooms become filled with air, which will be found to contain a much larger proportion of moisture than the lower portions, and will be shortly found to be exceedingly offensive from the rapid decomposition which the exuded organic matter undergoes in a moist air. This will happen more frequently when the internal and external temperatures are about the same, and when it is so cold, raw, or windy as to require closed doors and windows, with only a small addition of heat, and when with these conditions the natural dew-point is high. These circumstances are in concurrence frequently in England, where probably 120 days out of 365 call for but small addition of heat in rooms, while they rarely exist with us, the climate of our northern United State not giving 30 days in any year of similar kind. The objection of *effluvia*, which forms the distinctive one in audience-rooms in England, and is so noticeable to the American visitor of such rooms, is replaced in our halls by a simple sense of oppression—a mere feeling of discomfort—which, on the other hand, is particularly noticeable to the English visitor of our halls, who is apt to associate it with a supposed excess of heat.

But this organic matter of exhalation is still one step removed from malaria; it is only the ground of malaria—the soil on which a malarial growth will propagate; its *decomposition* is held to supply the means of fecundity to the germs of disease. In the warm air confined in the upper parts of rooms, with excess of moisture, it may undergo a rapid and somewhat fœtid decomposition; under such circumstances it is found to become offensive in six to ten minutes. With a smaller proportion of moisture, or when it is rapidly absorbed with the moisture by diffusion into air of American dryness, it does not decompose so rapidly, but is likely to be absorbed by any hygroscopic substance the air containing it may come in contact with. The walls of rooms—especially the porous plastering, stone, or bricks, and possibly the papered and painted walls—will take up the excess of moisture with its organisms, and give up at another time, wholly or in part, the moisture without them. There is a characteristic smell of walls of kitchens, cabinets, hospitals, jails, court-rooms, and similar permanently occupied places, which can be developed in intensity by simply holding the half-closed warm hand against the face of the wall, and testing the result by the sense of smell of the hand. In such instances of retention of the results of imperfect ventilation, the eventual propagation of disease is a certain one.

To the vegetable and lower animal growth the presence of moisture in the air seems a positive necessity; where it is absent they perish, or at least no longer grow or propagate. The drinking animal apparently suffers the least of injury and but little discomfort in the dryness of the air, of whatever temperature. The immunity from disease to the human race which accompanies the drier regions of the earth has been frequently remarked, and the fact meets general assent. The discomfort of the colder countries, even to the limits of the arctic regions, is one of cold, not of absence of moisture. The assertion of relative insensibility to cold air devoid of moisture is the common report of all travellers in such regions, while the dreaded malarial diseases of more genial lands do not exist in them at all. In the temperate zone, in countries or localities which possess the driest of atmospheres, with the least variation of hygrometric condition, mankind is most free from disease of all descriptions. The elevated dry and barren lands of our midland country, from Minnesota southwardly, are the healthiest regions of the United States; and where, together with the *dry* atmosphere, some uniformity of temperature exists, as in Mexico,—where the height above the level of the sea reduces the sensible heat of the air which is usually found in that latitude to a comfortable one,—there we have the acknowledged climate of utmost healthfulness. Going into more torrid lands, the dryness of the air alleviates the heat of the deserts of Arabia, and of Africa, of Peru, and Bolivia, where the temperature rises at times to 110° or even 120° in the shade. I have the assertion of a friend that on a hot day, with the thermometer nearly 100°, he has known the wind on the Arabian Desert to be searchingly cold, when everything was shrivelling up for want of moisture. My attention was called by Dr. J. S. Billings to the following extract of his Report on the Hygiene of the U.S. Army in 1875:—"Description of military posts, Fort Yuma, California. Reports of Asst.-Surgeons Lauderdale and Ross, U.S.A. After describing the locality of the post, near the junction of the Giler and Colorado Rivers, they say:—'The heat increases rapidly from the latter part of May, and in June, July, August, and September may be said to be intense. . . . During the months of April, May, and June no rain falls; then, with the thermometer at 105°, the perspiration is scarcely seen upon the skin, and it becomes dry and hard, and the hair crispy, and furniture falls to pieces, . . . ink dries so rapidly upon the pen that it requires washing off every few minutes. . . . A No. 2. Faber's pencil leaves no more trace on paper than a piece of anthracite, and it is necessary to keep one immersed in water while using one that has been standing in water some time. Newspapers require to be handled with care; if rudely handled they break. Twelve pound boxes of soap, when re-weighed, gave but ten pounds. Hams had lost 12 per cent and rice 2 per cent of original weight. Eggs that have been on hand for a few weeks lose their watery contents by evaporation; the remainder is tough and hard; this has probably led to the story that our hens lay hard boiled eggs.

"'The mercury gained its highest point last summer on the 2nd day of July, when for two hours it stood at 113° in the shade. All metallic bodies were hot to the touch; my watch felt like a hot boiled egg in my pocket. . . .

"'This post, although not the most southerly, is the hottest military post in the United States: the highest temperature recorded on our books since 1850, when the post was established, is 119°, observed at 2.25 p.m., June 16th, 1859. A temperature of 100° may exist at Fort Yuma for weeks in succession, and there will be no additional cases of sickness in consequence. . . . We have none of the malarial diseases. . . . No ice is formed at any time; 29° has been indicated by a registering thermometer, in January, 1872. The mean temperature, day and night, of January, however, is 57°; that of July is 95°. The average rainfall during four years was a litttle over 2 inches each year.'"

Any who wish to corroborate or question these views as to the healthiness or unhealthiness of dry air, hot or cold, can examine authorities, or investigate or observe for themselves; the conclusions they will reach can be confidently anticipated. But the proof or argument cannot be further extended in this paper, and it must be claimed that there exist good grounds to believe that dry air, *per se*, of whatever temperature it may be found on the surface of the earth, is not unhealthy; that, as regards disease, such air possesses both preventive and curative qualities of great value; and that on the other hand, moist air, such as promotes vegetable growth, is not desirable for breathing, on sanitary grounds. Asserting these views, the question narrows down to—Given a habitat or place of residence, where some degree of moisture and vegetation does thrive for a portion of the year at least, what effect on the system do the variations of moisture produce, from season to season, from day to day, and during such of the seasons as the comfort of inhabitants may call for artificial warmth, from one place to another on the same day?

§. Clothing, houses, and fires are the means by which mankind is enabled to inhabit the face of the earth. It is an artificial existence for an animal whose natural life would otherwise be limited to a small belt of the torrid zone, where the temperature never falls below about 80° nor rises above 100°. As residents of the northern United States we cannot expect to avoid, and do not expect to ameliorate, the vicissitudes of climate *out of doors*. Hot or cold, rainy or dry, with air relatively humid or otherwise, life in all countries means endurance under artificial guards or protections from natural inclemencies. We clothe ourselves by the umbrella on the one hand, or the great coat on the other; open or close our doors; induce cool breezes, or gather around fires, in search of the *comfortable uniform* loss of heat by the system. The efforts to accomplish this end, by means of change of temperature or relative moisture of the air, are of necessity confined to the cold, or at least cool, season, with infrequent attempts to obtain an artificial degree of cold in extreme hot weather. In moderate weather the vicissitudes of temperature, and of humid condition of the air, are endured with the expression of discomfort and the tacit admission, on all hands, that our great day-by-day variations of the mild season are harmful to the feeble or sickly. But these daily changes of temperature and of hygrometric condition are of small account with those which in the Northern States accompany the season of cold. The change of climate from that which accompanies, during any of the winter months, our warm south winds, to that which accompanies a great north-western wind-wave, which may follow the southerly breeze within twelve hours,—a change from 50° to 60°, with 80 per cent saturation, to even below zero, with an unascertainable dew-point,—such a change is trying in the extreme. The prevalent disease of the land is consumption of the lungs, and these changes are disastrous to those who are suffering from this complaint, and to the healthy these changes are held to be fraught with danger.

§. A very simple and commonplace observation will make the general condition of air in rooms in winter, as regards humidity, the subject of positive demonstration. During the season of winter in our climate, after a continued spell of cold weather, the exhibition of condensed moisture or of frost on the window-panes is very infrequent. The usual provision of glass in windows throughout the northern United States is in single thickness, not double plates; the latter arrangement being decidedly exceptional as a means of preventing transfer and loss of heat at the windows. The temperature of a pane of glass which is interposed between two temperatures of still air—that is, of air devoid of currents, except those generated by the differences of temperatures of the air on either side and the glass—is obviously that of a mean between the said two temperatures of air. With so good a conductor as glass, and with plates as thin as ordinary window-glass, the conductivity of the glass may be assumed to be perfect, and both sides of a pane can be deemed to have the same mean temperature. But the temperature of the air on each side of a pane differs from either that of the external air on the one side, or of the room on the other. On the outside, the layer of cold air in contact with the pane ascends slowly as it is heated, and the vacuity which is formed at the bottom of a pane is supplied by fresh cold air, so that the layer at this point approximates to the temperature of the outer air closely. On the inside, the layer of warm air in contact with the pane descends as it is cooled, having, perhaps, an approximation to the inside temperature at the top of the pane, but by the time the layer has flown downwards to the bottom of the pane its temperature will have become materially lower than that of air of the room generally. So that while the law of mean temperature of the pane at the bottom of the pane is yet good, the real temperature may be much lower than a simple mean between thermometers hung in the room and out of doors. Beside the supposition of still air, much allowance must be made for the effect of winter winds in accelerating the flow of cold air on the outside of the pane until the outer layer is very nearly of uniform coldness, which favours the greater abstraction of heat from the internal descending current, and cools down the lower part of the pane still further. Curtains, shades, or internal blinds, while they aid in protecting the room from loss of heat, also protect the glass from acquiring the temperature due to the heat of the room. Until with the supposed case of external air at 10° above zero, and a moderate wind out of doors, and of a room warmed to 75° inside, the temperature of the panes near the bottom, or even well up their height, will be much below 42½° as the mean; and 27° to 28° may fairly be taken as giving the real temperature near the bottom. This pane of glass becomes, then, a dew-point thermometer at all times in the winter, ready for indicating the humidity in the air. Except, however, a crowd of people, or some artificial "hydration" carried possibly to momentary excess under the stimulus of the last theoriser, or a very new house, or a damp cellar in an old one, we but rarely see any indication of presence of moisture in our dwellings in cold weather. This simple test will show that our dwellings, although the water-troughs of the hot-air furnaces do supply limited quantities of vapour with admitted comfort, do not, as a rule, have over 30 to 40 per cent of humidity in the air within them.

The entire range of our Atlantic coast is only removed from a region of perpetual spring from 200 to 500 miles; and a south-east wind, from December to April, may bring a vapour-laden air, which, in a few hours, will have changed our frigid winter to a genial spring. The succeeding wave of north-west winds—a great current which the Signal Service has traced up to the Arctic regions—may, with great violence, restore the winter with all its rigour, within another few hours. Under this change heavily-frosted windows become the rule, and they indicate a dew-point above the temperature of the panes of glass, at once. So dry and arid are these winds, however, that with the continuance of a north-west wind for a single day all traces of window-frosting will have disappeared.

This test of the dew-point by the window is very accurate. As an instance I will quote that on one day in this present month, in Philadelphia, with the thermometer in-doors at 64°, without fire, a fall of temperature to 54° outside, produced immediate condensation on windows, showing 75° to 80° of humidity. Now if a difference of only 10° produces this indication for *humid* air, the want of such an indication with difference of up to 65° must manifestly be a very dry air. By actual trial in well-warmed and ventilated rooms, the writer has found the dew-point far below the freezing-point of water, in rooms where the sensation of dryness, which is held to accompany the heat of the furnace when not supplied with water for evaporation, certainly did not exist.

It is proper, when alluding to the dampness of houses, to advert to one of the most striking differences between England and Western Europe and the northern United States, in the necessities which climate imposes in the relation of humidity in air to the health, in this particular regard. We find all foreign authors speaking in unequivocal language of the great danger of inhabiting a newly-built and consequently imperfectly dried house. One English writer, whose name I cannot recall, but I remember to have been of much eminence, asserted that no house ought to be occupied until a year after completion. Many writers on ventilation join in estimating by figures the quantity of moisture in the new walls, and *demonstrate* the dangers of a residence where the excess of moisture in the air, the want of permeability of the walls, and the increase of conductivity of heat through the damp walls, will have produced such a tomb-like house. While in America we are fully alive to the danger in the house, from air overladen with moisture, as from a damp cellar or location in a damp place or vicinity, and appreciate that rheumatism and consumption, with scarlet fever for the children in winter, when the house is thoroughly warmed, and typhus for adults in mild weather, are the possible, if not probable, penalties for living in such a house; yet the new house, from its inherent dampness, only is considered at least as but objectionable in a small degree. Except that dampness exists from other sources than that which comes from the walls, our new houses are quite as healthy during the first year, or the first season's occupation, as afterwards. No preliminary drying out is needed as a rule. Our summers or winters are dry enough to take up all the moisture which walls may give, without overlading the air with humidity; although it is noticeable that new houses require more fuel to warm them the first winter than afterwards, as the supply of heat must be sufficient to evaporate an excess of moisture, and the conductivity of the walls is somewhat greater before they are thoroughly dried out. Few dwellings are completed for occupancy at the end of winter or of summer—residence generally begins with the beginning of summer or of winter, and the seasons when the dampness of walls would be dangerous from the existence of a humid air are not those when new houses are generally first occupied.

§. Although not directly related to the subject, I will mention here one curious demonstration of the effect of atmospheric humidity and impurity which is peculiar and common in England. All American readers or observers become aware of the great importance attached by the English public, as a people, as writers, and as a government, to the relative purity of illuminating gas; but it is not generally known, or even asserted, that the cause in England which makes the impurities of gas obvious, and renders them seriously objectionable, is to be found in the air of the room, and not alone or even mainly in the gas itself. The sense of oppression from the burning of gas in dwellings in England is one that can be appreciated only by being felt; any description fails to convey to the mind of the untravelled American the burden on the breathing functions which results from gas burning in a humid and impure air. It is enough to say that throughout England gas-lighting is regarded as only suitable for shops, work-rooms, warehouses, public rooms, and such other places, within or without of doors, as demand light for passengers, rather than for occupants. When halls are lighted by gas, the chandeliers ("gasoliers" is the English word) and bracket lights are not considered to be well arranged unless "ventilated;" in other words, provided with especial means—air-passages or outlets—for removal of the gases of combustion, with the accompaniment of a volume of heated air. The dwellings—dining and drawing rooms, passages, and chambers—of the more wealthy are lighted nearly universally by candles of wax, sperm, or some prepared fatty substance, either animal, vegetable or mineral, with the occasional or frequent use of the carcel-lamp as a table light. From any English gas-light there arises a current of impurities, which in a brief space of time discolours and coats with black the interior of the ventilating tubes, or, in absence of such protectors, the canopies of glass or metal which are usually supplied for unventilated burners; or when this last protection is wanting, the ceiling, even when several feet above the burner, is quickly marked by a halo of greasy soot, which adheres to it where the ascending current impinges.

The heat emanating from a gas-burner, as compared to that from a number of candles giving the same amount of light, is very nearly the same, about 7 per cent more heat being given out by the candles; but the fourteen to sixteen candles, which represent the single gas-burner, will have been dispersed about a room, or even when grouped in threes or sevens—as usual in sconces, candelabra, or chandeliers—will be spread widely asunder, so that the current proceeding upwards from each separate candle will have become diffused before reaching the ceiling; and, if the candles give out the same impurities (which they do not) as the gas-burners, the obvious impurities which make a mark will not be precipitated to show themselves as spots. Besides this, a room lighted by candles will be considered brilliantly illuminated by three or four candles, where one gas-burner would have been used; so that one-fourth the quantity of light will be made to suffice with candle illumination, to what is requisite for gas lighting. The numerous candles come in proximity to the objects to be lighted, while the gas-burner, with its volume of light and of heat, must be further removed to be tolerable, and there is an actual requirement for more light from the latter than the former source to give the same effect in the room. The other substitute for the gas-burner, the carcel-lamp, is frequently used in England as the centre-piece, singly or in numbers, of the tables in the dining-room, drawing-room, or library. Its illuminating power is about two-thirds that of a gas-burner, and the quantity of heat given out bears very nearly the same relation,—that is, the heat from a carcel-lamp is about two-thirds that from a single gas-burner of fourteen to sixteen candle-power. As the carcel-lamp is movable in a room, and is usually placed low down, the chance of the current of air proceeding from it defacing the ceiling is much less than from a fixed gas-burner, in its usual position of height from the floor.

What may be the exact physical or chemical conditions accompanying the manifestation of *bad* gas in England might call for a long discussion in reply. It can be said at once that the indications on the ceilings are almost entirely those of atmospheric impurities and condensation of water. Soot, dust, and organic matter in suspension in a saturated humid air—wherein the humidity is the vehicle which carries these substances, and is not free to disperse them from the general saturation—will be and are deposited on the first object of cooling and contact. This view of the case is not a mere guess. The fact that there is a general discoloration of ceilings from gas-burners in England, as contrasted with those of the Continent, where the air is uniformly a little more dry, and where the use of bituminous coal is unknown, so that its particles of carbon dust do not form a part of the common impurities of the air; and also as contrasted with the ceilings in this country; this fact is certain, and from it the connection of pure air with the *cleanliness* of gas burning is made evident. While English gas is much more carefully purified and treated than any in the world, the standard of its excellence is not only the highest, but it is made strictly up to this standard. Yet any sulphurous or sulphuric acids which emanate from gas lights are at once absorbed by the vapour present, and if the atmospheric condition does not facilitate the diffusion of this vapour, these acids are retained in the ascending column to exert their energies on the objects of first contact, and afterwards retained in the room to act generally on any sulphur absorbent, colours or materials, as they do in England and do not in this country. It is probable in our climate, and in that of the Continent, much of the humidity of combustion, and of the deleterious gases, either evolved by or inherent to the illuminating gas, as well as the organic impurities, dust or soot, in the air burnt or heated by the act of burning, is diffused at once into the tenuous vapour of the surrounding air. Not only before passing four or five feet from the burner, so that no condensation takes place on the ceiling, but also so thoroughly diffused as to prevent, in great degree, those chemical actions which prove so objectionable from the burning of the best purified coal gas in England.

§ It has been shown how nearly impracticable it is to procure in winter, with the average temperature of winter, which is 34°, a summer temperature and humidity in our houses. The difficulties of effecting this with 34° for the temperature of the external air are enhanced greatly as the minimums of cold are reached, and at zero the production of a summer air in a house or place of residence may be claimed to be impossible. If the effect of the changes of out-of-door temperatures and humidities, which can happen with, at the worst, eight to twelve hours of change, and which on the average gives twenty-four to seventy-two hours of interval, is as objectionable, what words can express the effect of the mere passing from a room at summer humid warmth to the open anhydrous air at zero? There are few readers of this paper who have not tried the experiment of leaving some crowded hall, where the closed doors and windows and many breaths had made an approach to the summer condition, and felt the cold air of winter at the bottom of the lungs, as the inactive membrane parted with its unexpected supply of moisture to the anhydrous air. At whatever temperature or moisture condition air be inhaled, it will be exhaled at 90°, and laden with moisture nearly to the point of saturation. Of the heat given out by the lungs, that which proceeds from evaporation is generally largely in excess of what is required to impart heat to the air. Even in the extreme case of breathing air at −40° (or the temperature when mercury freezes, which the writer once observed at Vassalborough, Maine), the heat of evaporation of moisture from the lungs is but a little greater than that for heating the air, being 2·22 units in one case to 2·18 units in the other, per cubic foot of exhaled air. The skin gives out its heat through insensible perspiration, or through heat imparted to air in similar, if not the same, proportion.

The establishment of a régime of evaporation from the lungs or skin—of a constancy of secretions—appears to be more essential than the establishment of a uniform temperature, either of the air of respiration or of contact with the person. The stability of the moisture condition, whether in the external air from time to time, or between inside and outside of the room, is what is desirable for health; and this stability, from inside to outside of room, is what we must maintain if we are to live in active, healthful life in our climate. The transfer of heat through the skin or membranes is merely conductive, not involving organic action; while the supply of moisture incident to the maintaining of evaporation brings into service vessels, ducts, or pores, whose healthful action depends in great measure on the regularity and continuity of the said service. This hypothesis will explain at once the healthfulness of the climate of Florida or that of Minnesota in cases of pulmonary disease, and in other parts of the country will account for the prevalence of colds and coughs or the occurrence of rheumatic affections. The diseases of the changeable climate and water-laden winds of our colder Eastern States are bronchial and pulmonary; and, without desiring to entrench on the province of the medical profession, whose known duty at all times it has been to find the cause of disease) it may be safe to attribute them, to a great extent, to the effect of alternate dry and damp air on the evaporating surface of the lungs when the skin has the protection of clothing to keep it warm. The disease of the warmer mountain-sides of our Middle States are rheumatic, and may be attributed to the same cause operating, by warm currents of air, on the less unprotected skin.

Beside this view, it can be averred that, for the resident or native of any country, there will have established as a habit a connection of moisture of air relative to its temperature which is national, so to speak, in which the *variations* of humidity and heat are accepted as a general average. Thus, the American in England is chilled to great discomfort by the low temperature endured by Englishmen, whose systematic evaporation provides for small loss of heat; while the Englishman in America finds a suffocating heat in the dwelling of the American, from the fact that his lungs and skin do not afford the moisture requisite for evaporation and consequent dispersion of heat. A long residence, often two or more years, is needed before the system of either is adapted to the novel condition.

§. Although somewhat late in the course of this argument, and perhaps somewhat elementary as information, it may be well to state the physical laws of the relation of moisture to air. The property of water is to possess in contact with itself, at any and all temperatures, from the boiling-point downwards, an atmosphere of vapour, which vapour has an elastic force, or exerts a pressure bearing some definite relation to the sensible temperature of the water and of itself. The English measures of this elastic force are generally expressed in inches of height of a mercury column, in the same way as shown by an English barometer, which of course applies to any unit of surface, and may be transformed to pressure in pounds per square inch or square foot, by a similar process to what we use for atmospheric pressures. According to Regnault (as quoted in Guyot's tables), some of the elastic forces are as follows:—

Degrees Fahrenheit.

—30	—20	—10	0	10	20	32	40	50	60	70	80	90	100	212
0·0092	0·0163	0·0270	0·0434	0·0684	0·1078	0·1811	0·2476	0·3608	0·5179	0·7329	1·0232	1·4097	1·9179	2·9922

Inches of Mercury Column.

From which it will be seen that the elastic force falls off rapidly with the temperature. At any given temperature vapour, possessing the above value of elastic force, exists in the atmosphere as a part of its volume, *whenever there is water present to supply it*, and such an atmosphere is said to be saturated. When there is not sufficient water at hand to supply the vapour due to the temperature, what there is of vapour in the air is slightly superheated, as it accepts the temperature of the air and not that of its tension, but this effect is so small that it may be neglected. The air is then said to be dry; the usual way of estimating such dryness is by naming the percentage of vapour present to that which would fully saturate the air at a given temperature. Dry air seeks moisture from any source, and the difference of elastic forces, between that of the vapour in the air at any time, and that of saturation of the same air, represents the *avidity* for moisture of such dry air as a species of partial vacuum.

Now, the quantity of moisture as vapour accompanying a given quantity of air is neither increased nor diminished in the same air by heating or cooling it (of course, in the latter case to the temperature when the air is saturated, below which point the moisture condenses). Hence it does not matter, so far as moisture is concerned, at what degree of heat we introduce the air for warming a room; if only the final temperature of the room be 70° or 80°, then the drying effect of the air of that room upon the persons occupying it, or its furniture, or its materials of construction, is that due to air of 70° or 80°, which air shall contain only the normal moisture of supply. In other words, our hot air furnaces which supply air at 150° to 200° do not (except, perhaps, close to the mouths of supply, before the heat is distributed) dry up wood-work or absorb any more moisture from the persons occupying a room any more than do currents at 80° or 90°, provided the general temperature of the room is the same in both cases. But a yet more important truism can be stated, which is that the drying effect of air of a ventilated room at 70° or 80° which has received no increase of humidity in the hot air of ventilation from out of doors—air that has been warmed artificially from zero, let us say—the drying effect of this heated air upon a person occupying it is very nearly the same as if he or she were to maintain the same temperature through active exercise and warm clothing out of doors. The exception expressed by "very nearly" relates to the clad portion of the body—the obstacle presented by the clothing to the free diffusion of aqueous vapour is more effective between the cold air which is but little warmed to demand moisture, and the skin which will give it out if the vacuum demands it, than between the arid air of the room, which takes up every particle of moisture as it transpires through the clothing. As regards loss of moisture from the throat or lungs, however, there is absolutely no difference in breathing the air of the supposed room and that which is then found out of doors, although the one be at 70° to 80° and the other at zero; reiterating the former statement:—"In either case the exhaled breath is at 90° as it passes out from the nostrils or lips, and is saturated or nearly saturated with moisture." No one ventures to assert that it is unhealthy, as an act of breathing, for the human race to breathe freely the coldest dry air of winter, because of the supplying of moisture to the anhydrous air. There is an evident fallacy in the assumption that it can be healthy to check instantly that copious secretion which has been supplying moisture to the fresh cold air of zero, by going into a room of summer hygrometric condition, or to demand such an effort of the tissue of the lungs suddenly by leaving such a room for the outer air. Fortunately, Nature is more lenient than the theorists, and we do not get 70° to 80° with 70 per cent. of saturation in the most unventilated or uncomfortably heated houses, and with all efforts to the contrary, even 40 per cent. is of unusual attainment when the external air has a temperature of zero.

It must be admitted, however, that some small degree of hydration is a necessity for comfort, and, with comfort for a demand, some reason may be found to establish the healthfulness of the small supply. It is certain from all experience that from 5 to 10 per cent. of moisture can be added to air after it is heated, certainly with much relief, especially to the eyes, with apparently little harm, although such addition may make the occupant of a heated room a little delicate as to out-door exposure. Moisture may to some small extent be abstracted by the means of heating, especially when the heating is by stoves or hot-air furnaces; at all events, the presence of a sheet or surface of water over which the heated air is allowed to pass is now a recognised means of supplying a small quantity of aqueous vapour to air of ventilation. But the quantity supplied in this way is very small in comparison with what is needed for complete "hydration," or even for what can be denominated "hydration" at all in the sense of a summer condition. From an estimate based on several winters' experience, a vaporisation of water which supplied a half grain of vapour per cubic foot of air introduced, when an increment of four to six grains for the same volume of air would be requisite to get the summer condition of humidity corresponding to the internal temperature, has proved sufficient to give a sensibly pleasant air, while the absence of this supply was at once perceptible in the house. A low pressure hot-water apparatus, whose temperature never reached the boiling-point, and rarely exceeded 180°, giving heat to a large volume of fresh air, which, at the mouths of supply to the rooms, was not much above 90° at any time, was the source of heat.

§. It is very difficult to find any hypothesis which will account for this requirement of a small supply of vapour with heated air when we admit, or can demonstrate, that the sufficient quantity to the senses is so far below what is needed for hydration, and so independent from the moisture condition of the air; for nearly the same small quantity of vaporisation seems desirable in air heated from any temperature. The explanation of the offensiveness of heated air currents has been sought with much diligence, and, at times, causes have been assigned with much positiveness. One of the earliest of these explanations (but one which yet finds supporters) was found in the substitution of transferred or convected heats by currents of air for the radiant heat of fire. Open fires were to be restored as the means of securing pleasant air. The healthfulness and comfort of our ancestors were to come back to their degenerate children. Gathered around a blazing fire, roasted on one side and frozen on the other; restricted to one fire in the house, as all the others *would* smoke; the chamber-heating apparatus reduced to the warming-pan; victims of rheumatism, sciatica, tic-doloreux, and ague—the diseases of fifty years ago—the good old times were to come back. Alas! there were obstinate innovators, and the world would not be convinced of the advantages of radiant heat as the sole means of warming.

This point being established, at a later time, surfaces at high temperatures for imparting heat to the air of a room, either by ventilating currents or direct heating, including all fire-heated surfaces, together with steam-heated ones, above the boiling-point, or 212°, were utterly condemned.

Somebody discovered burnt atoms of dust in heated air, and its destructive, pernicious effect on the health was at once apparent! But the comfort of the community some way overruled the theory, and hot-air furnaces, with admitted deficiencies in quality of air, met greater favour than ever. It is allowed generally that the expensive steam-heating apparatus is at once the more pleasant, controllable, and durable; and that the yet more expensive hot-water apparatus, with its great volumes of low temperature currents of air, is the best of all means of heating. The cost in fuel by these several apparatuses becomes nearly the same, for equal effects, in warming of houses.

The next demonstration was the chemical one. An occult effect is most conclusively, if not convincingly, explained by an occult cause. Ozone is a favourable object to carry a theory, and it really is possible, if we knew anything definite and certain about the origin or the effect of ozone; some relation of this phenomenon of the requiring the evaporation of a small quantity of water, when heating air, might be traced. But no blinder pathway in science was ever opened than the ozone one. After this came Deville and Troost's discovery of the permeability of some metals, when heated at, or nearly at, red heat, to some of the gases. In the language of one of the most prominent writers on ventilation, this "explains the very injurious and even poisonous effects produced by the use of stoves in the rooms of a dwelling!"

The last resort of the unreasoning theoriser in physics is always to electricity, and efforts have not been wanting to show that either the presence or the absence of electricity, in some form or condition, ought to have something to do with the discomfort arising from heated air. The only answer to this hypothesis is, that heated air is equally oppressive in entire absence of water supply, whether highly electrical or otherwise; our vicissitudes of climate and of humidity enabling a test of electric condition in extreme cases. There are times in any winter, in the Northern States, when it is possible to gather enough electricity, by walking over a carpet, to make the spark from the finger which will light a gas-light. The quantity of water demanded at such times by a heating apparatus is no greater than at other times. There is not the least positive proof of relation of electricity to the healthfulness of air. Altogether, the whole resolves itself to the reiteration of the bare fact that it is comfortable to evaporate a small quantity of water in heated rooms, and that it can be done without marked injury to the occupants or to visitors. The quantity itself seems to be almost constant for all temperatures or hygromations of the air, and to be a slight addition only to the moisture in the normal air out of doors at any time.

§. The effect of draughts or currents of air upon any person exposed to them is materially modified by the hygrometric condition. In still air in winter the comfortable temperature of a room in general hygrometric condition has been stated at 70°, but a current of air upon the person at this temperature is uncomfortably cold from the rapid abstraction of heat, while in summer the same temperature, accompanied by the summer dew-point, will be warm enough to demand light clothing, and a current of the same velocity will not be objectionable; in other words, draughts which cannot be tolerated in our houses in winter become pleasant breezes in summer. Not only the speed or rate of velocity of the current of air is to be taken into account, but its avidity to take up moisture from the skin as indicated by its dew-point. So long as the hygrometric condition of the air is such that will absorb moisture below 98°, a blast of it at some rate of current will be a cool one.

One fans himself in our climate, when the thermometer stands at 100°, with a sensation of relief. This feeling of cold, from air of high temperature, when in motion, proceeds from the rapid removal of the stratum of warm and nearly saturated air in contact with the person, and its replacement by fresh air, which is not only cooler, but which has not yet become saturated or charged with moisture by contact with a moist surface like that of the skin. In no one of the changes in the three forms of matter—solid, liquid, and gaseous—is there so much heat taken up as in the change from a liquid to a gaseous (or vaporous) form, and in no other body or substance is so much heat absorbed or become latent as in the formation of steam from water, or, in other words, in the process of evaporation; and the quantity of heat taken up by the moisture which a dry air abstracts from the skin is so great that the mere differences of temperature of the air from that of the skin may almost be neglected in the statement; and it is very nearly correct to assert that the cool sensation from a breeze in summer proceeds entirely from the evaporation of moisture thereby induced.

Upon this basis it may be noticed that a current of saturated air at 100° would neither remove heat by its contact nor by induced evaporation, and consequently would be incapable of producing a cooling effect, while as the temperature or the dew-point should fall the current would become a pleasant one. With a high temperature and dry air the cooling effect of a current of air (even at 100°) may be very pleasant in the sensation, but will be attended with sun-burning (even without exposure to the sun), and blisters will be produced by the excessive deprivation of moisture from the cuticle or surface of the skin. With 80° of temperature and a high dew-point a strong breeze is not unpleasant, nor likely to be injurious after the person shall have acquired some *accustomed* habit of body to endure it; but at 70° and a low dew-point, which is the only possible condition of heated air in mid-winter, the annoyance of a current of even 5 feet per second and its unhealthiness are positive facts.

§. There remain to be considered two more relations of moisture in air to health and comfort. First, the effect of evaporation of water by the air itself in summer, in reducing the temperature to one of comfort; and secondly, the effect on the moisture condition of the air of summer, when it is attempted to cool air by artificial means.

The cooling of air, by spontaneous evaporation from extended surfaces, is of frequent practice in hot countries by the wealthy inhabitants near the banks of rivers where the water-supply is abundant. The condition of the air which makes it practicable is one of great heat, and of a relatively low dew-point; and the summers of Egypt and of parts of India, especially of Bengal, give opportunity to employ this method of cooling air. If it is accepted that the temperature of the air in the shade, in the localities referred to, will sometimes run from 85° to 105° for many consecutive hours, accompanied with, say, 50 per cent of moisture for the 85° of temperature, or, say, 30 per cent for the 105°, then the evaporation of moisture from wet surface can be relied upon to produce a comparatively comfortable atmosphere. Air at 85°, with 50 per cent of moisture, has quite exactly the same quantity of moisture, per given volume of air, as that at 70° and 70 per cent of moisture. So that if it could be cooled *without* adding moisture at all it would then reach the point of comfort for the clothed inhabitant of the temperate zone. If the dew-point of 85° is brought up to 80 per cent, or above, the air becomes intolerably sultry, and at 90 per cent quite suffocating; so that the greatest addition of moisture practicable to the supposed air of 85° and 50 per cent dew-point may be taken as 10 per cent, or 1¼ grains to the cubic foot. The resulting figures which the latent heat demanded for the evaporation of these 1¼ grains of moisture into the air supposed is 74½°, and 82 per cent of humidity. How far this condition of the air may be more comfortable than the original one of 85° and 50 per cent of humidity is questionable; but it is apparent that the limit of possible cooling of air of 85°, by evaporation of moisture, is found when its humidity is not to exceed 50 per cent of saturation. A similar computation applied to air of 105° (in which air there is a little more moisture than in that supposed at 85°) gives, for the addition of 1¼ grains of moisture to the cubic foot, air of 94½° temperature and 48 per cent of humidity—an atmosphere which *may* be, in some degree, more comfortable to a person at rest than the normal one. It is evident that, for efficiency in cooling air of 105° by evaporation from moistened surfaces, the humidity of the air to be cooled should be less than 30 per cent. As to ultimate healthfulness of the moistened air, it *seems* to be unquestionable that the supply of moisture ought to promote disease.

We have, however, in each year in our country, a few days or parts of days (perhaps, in the Southern Middle States, ten to twenty days in different years) when the range of thermometer and the dew-point make it feasible to adopt this means of reducing the apparent heat of the air. The attempt has been frequently made, with provoking failure to the projectors. Its success depends upon not only the exact condition of relative humidity and temperature, but also on the proportion of surface of evaporation to the quantity of air to be supplied. The Indian ratio is one or two persons to 6 to 8 square yards of wet surface. But the most provoking cause of failure has been, that while there are ten to twenty hot dry days in any year, there are also twenty to thirty hot damp ones, and the application of the *cooling* apparel to the hot air on these days has produced such an effect of sultriness that the whole has met with instant condemnation.

The last relation of moisture to air to be considered at this time is that which proceeds from the effort to cool air artificially. The fallacies of the attempts to effect this purpose can be made very apparent. Even the smallest decrement of heat is obtained only at great cost. The *quantity* of cooling of air in summer is, to be sure, only about one-fourth that of heating in winter. Taking the ideal temperature of 70°, there may be 15° to come off in summer as generally as 60° to be added in winter; and supposing iced water to be the cooling medium, and steam of low pressure to be the heating one, the relation of difference of temperature of the air to be cooled or heated to that of the iced water or steam is such that about the same extent of surface would be required in either case to transfer the heat. But a pound of coal produces, in ordinary steam-boilers, quite 9000 units of heat on the average, while a pound of ice (in cooling to 60°, let us say) will produce only 170°, so that about 54 lbs. of ice would be demanded to effect the transfer of an equal quantity of heat, to what would be effected by 1 lb. of coal; or, accepting the one-fourth cooling effect, then 13½ lbs. of ice would be demanded for the cooling of air in summer against each pound of coal required for warming in winter. Unfortunately for the proposition for cooling *air* in summer, even this statement is too favourable, for the requirement will be found that the air must not only be cooled, but must be divested of a portion of its moisture, unless the cooled air is deemed satisfactory in the form of a cloud of vapour. Air at 85° and 70 per cent of humidity must be taken to be cooled to 70° and 70 per cent of humidity, and one and one-fourth times as much cold will be demanded to condense the vapour 2·3 grains per cubic foot as that which is requisite to cool the air the 15° supposed. This leaves the ratio of ice needed to obtain a spring condition for the air on a hot day in summer to be that of 30 to 1 of coal usually demanded to heat air on a cold day in winter, or assuming that the air on such a day is so dry that no moisture should be removed, about 15 to 1. Our ideas of the necessity of civilised wants, as compared with civilised luxuries, scarcely yet reach high enough to warrant the expenditure of money to cool air under the circumstances stated.

In view of the great cost of cooling air by ice, it has been proposed to cool it by mechanical means on a large scale. Air, if compressed, becomes sensibly hotter. In fact the compression can be carried to the extent that the heat will ignite tinder, as the cigar-lighters of twenty or thirty years since bear witness. And it has been proposed to use large air-pumps which shall compress the air until its temperature is elevated sufficiently for it to give off heat to surfaces cooled by currents of water, at such temperature as water is to be had from streams or aqueducts in summer. This compressed air, when deprived of a portion of its heat, is then allowed to expand, and the result is a cooler air. This process has, in reality, much merit, and it is probable that the cost of producing cold in this way compares favourably with the use of ice; in fact it has been shown that ice itself may be manufactured with profit by this process in some localities, where transportation enhances the price of ice largely; but it is preposterously expensive in apparatus and in cost of working as a means of cooling air in the great quantities demanded for ventilation, and the humidity of the cooled air would still be objectionable. The comparatively high temperature of the surface for cooling the air would fail to be very efficient in condensing the vapour thoroughly. In both the methods of cooling air, whether by ice or by water (of which great quantities would be needed), the cooling surface must be copper, brass, or tin, as the rusting of iron, when exposed to condensing vapour, is extremely rapid.

The most probable result of cooled air would be a thunder-cloud in miniature. The atmosphere on one of our hottest and most sultry days of summer is on the verge of a tempest. Cooling of air of 85° to 90°, to the extent of 20° or 30°, produces a dense mist of super-saturated cooled air. The equilibrium of the atmosphere on a still, clear summer day, when every growing thing on the surface of the ground is supplying moisture, and the radiation of the ground itself is supplying heat to increase the relative levity of the strata of air next the ground—the equilibrium of such an atmosphere is very unstable. Let an upward flow be established anywhere, and the air will rush in all directions along the surface to supply the partial vacuum. The ascending column, as it reaches the region of lower barometrical pressure, will expand, become sensibly cooler, and in a short 5 to 8 miles of height the region of frost and ice will be reached, and hailstones will be returned from the condensation of the transparent vapour which had existed in the air when it left the surface of the ground. The writer once saw in a little ball-room, on a Christmas Eve, a miniature snow-storm deposit a little bank of snow, from the opening of windows to air the room when the dancers had retired, the night being a clear moonlight one, with the thermometer a little above zero.

The difficulty of absence of moisture in air that is heated in winter is a matter to be disposed of with some happiness, by asserting it is not wanted, but the objection of presence of moisture in cooled air can be only overcome by not cooling the air. It does not seem that the successful cooling of our summer air, so as to produce a comfortable or healthful condition at spring temperatures, has any probability of accomplishment.

Quoting, as an appropriate final remark, the words with which I concluded another paper having an especial application to a system of ventilation:—"I will not follow further the proposition to change the seasons into a perpetual springtime—practical ventilation is the supply in dwellings of an abundance of fresh air at the several seasons, warmed to the temperature of comfort in winter, and supplied in quantity to the volume of comfort (as near as possible of the quality and condition of out of doors in the shade) in summer. The truth is, all our heating and ventilating appliances are the compromise of condition—a truth extending beyond all mechanical operations to the phenomena of Nature itself."

VI. ON SPACE OF FOUR DIMENSIONS.*

By J. C. FRIEDRICH ZÖLLNER,
Professor of Physical Astronomy in the University of Leipzig.

IN the first treatise the author shows that both Newton and Faraday were advocates of the theory of direct action at a distance through a vacuum, in opposition to the views of many modern scientific men. In the last

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* Wissenschaftliche Abhandlungen von JOH. CARL FRIEDRICH ZÖLLNER, Professor der Astrophysik an der Universität zu Leipzig. Erster Band. Leipzig: L. Staackmann, 1878. (With portraits and facsimiles of Newton, Kant, and Faraday. 8vo., 732 pages.)

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3. Newton's Law of Gravitation and its Derivation from the Static Effects of Electricity.
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13. Thomson's Demons and the Phantoms of Plato.

treatise, which is of the highest interest, the author describes experiments which he made in Leipzig, in December, 1877, with Mr. Henry Slade, the American. These experiments were only the practical application of Gauss's and Kant's theory of space, which these two eminent men imagined might contain more than three dimensions. The author will try to give to the readers of the "Quarterly Journal of Science" an idea of this theory, though he must of course refer to the work itself for a more ample explanation of it.

In accordance with Kant, Schopenhauer, and Helmholtz, the author regards the application of the law of causality as a function of the human intellect given to man *a priori* —*i.e.*, before all experience. The totality of all empirical experience is communicated to the intellect by the senses—*i.e.*, by organs which communicate to the mind all the sensual impressions which are received at the *surface* of our bodies. These impressions are a reality to us, and their sphere is two-dimensional, acting not in our body, but only on its *surface*.

We have only attained the conception of a world of objects with three dimensions by an intellectual process. What circumstances, we may ask, have compelled our intellect to come to this result? If a child contemplates its hand, it is conscious of its existence in a double manner—in the first place by its tangibility, in the second by its image on the retina of the eye. By repeated groping about and touching, the child knows by experience that his hand retains the same form and extension through all the variations of distance and positions under which it is observed; notwithstanding that the form and extension of the image on the retina constantly change with the different position and distance of the hand in respect to the eye. The problem is thus set to the child's understanding, How to reconcile to its comprehension the apparently contradictory facts of the *invariableness* of the object, together with the *variableness* of its appearance. This is only possible within space of three dimensions, in which, owing to perspective distortions and changes, these variations of projection can be reconciled with the constancy of the form of a body.

So, likewise, in the stereoscope, the representation of the corporeality—*i.e.*, of the third dimension—springs up in our mind when the task is presented to our intellect to refer at once two different plain pictures, without contradiction, to one single object.

Consequently our contemplation of a three-dimensioned space has been developed by means of the law of causality, which has been implanted in us *a priori*, and we have come to the idea of the third dimension in order to overcome the apparent inconsistency of facts, the existence of which experience daily convinces us.

The moment we observe in three-dimensioned space contradictory facts,—*i.e.*, facts which would force us to ascribe to a body two attributes or qualities which hitherto we thought could not exist together,—the moment, I say, in which we should observe such contradictory facts in a three-dimensioned body, our reason would at once be forced to reconcile these contradictions.

There would be such a contradiction, for example, if we were to ascribe to one and the same object at once mutability and immutability, the most universal attribute of a body being the quantity of its ponderable matter. In conformity with our present experience we consider this attribute as unalterable. As soon, however, as phenomena occur which prove it to be alterable, we shall be obliged to generalise our representation of the ideality of a body so as to bring the observed change in the quantity of its matter in accordance with its hitherto-imagined unchangeableness.

On page 235 of his book the author quotes the celebrated mathematician Riemann, who says in his work "Concerning the Hypotheses upon which Geometry is Founded:"—

> "The explanation of these facts can only be found by starting from the actual theories of the appearance of all phenomena which are confirmed by experience, and of which, as they now are, Newton has laid the foundation. Urged forward by facts, which we cannot explain through our hitherto-conceived theories, we slowly remodel our conceptions. If phenomena occur which, according to our conception, were to be expected with probability, our theories are confirmed, and our confidence in them is founded upon this confirmation by experience. If, however, something occurs which we do not expect, which according to our theory was improbable or impossible, the task is imposed on us to remodel our theory, in order to make the observed facts cease to be in contradiction with our improved theory. The completion of our system of ideas forms the explanation of the unexpected observation. Our conception of nature by this process grows slowly to be more complete and more just, at the same time it retreats more and more beneath the surface of appearances."

I now proceed to apply the higher conception of space to the theory of twisting a perfectly flexible cord. Let us consider such a cord to be represented by *a b*, showing us, when stretched, a development of space in *one* dimension,—

(*a*————*b*).

If the cord is bent so that during this action its parts always remain in the same plane, a development of space in *two* dimensions will be required for this operation. The following figure may be given to the cord,—

and all its parts, if conceived of infinite thinness, may be considered as lying in the same plane, *i.e.*, in a development of space in two dimensions. If the flexible cord, without being broken, has to be brought back into the former figure of a straight line, in such a manner that during this operation all its parts remain in the same plane, this can only be effected by describing with one end of the cord a circle of 360°.

For beings with only *two*-dimensional perceptions these operations with the cord would correspond to what we, with our *three*-dimensional perception, call a knot in the cord. Now if a being, limited on account of its bodily organisation to the conception of only *two* dimensions of space, possessed, nevertheless, the ability of executing by his will operations with this cord which are only possible in the space of *three* dimensions, such a being would be able to undo this two-dimensional knot in a much simpler way. Merely the turning over of part of the cord would be required, so that after the operation, when all parts again lie in the same plane, the cord would have passed through the following positions:—

By the same operations, but in an inverted sense, such a being would be able again to form the knot without needing that circumstantial process, during which all parts of the thread have to remain in the *two*-dimensional space of perception.

If this consideration, by way of analogy, is transferred to a knot in space of *three* dimensions, it will easily be seen that the tying as well as the untying of such a knot can only be effected by operations, during which the parts of the cord describe a line of *double* curvature, as shown by this figure :—

We three-dimensional beings can only tie or untie such a knot by moving one end of the cord through 360° in a plane which is *inclined* towards that other plane containing the two-dimensional part of the knot. But if there were beings among us who were able to produce by their will four-dimensional movements of material substances, they could tie and untie such knots in a much simpler manner by an operation analogous to that described in relation to a two-dimensional knot.

It is by no means necessary—nay, not even *probable*—that such beings should have a contemplative consciousness of these actions of their will. For all our conceptions in relation to the movements of our limbs, and to those produced by their means in other bodies, have been acquired by us solely by way of *experience.* Having observed from childhood that a voluntary movement of our limbs is always connected with a corresponding change in our visional impressions, accompanying the action of our will, it is only in this way that we are now able to connect the movements of our body or of other objects with a corresponding conception of such motion.

Berkeley demonstrated this truth in the year 1709 in his "Essay Towards a New Theory of Vision" and in his "Principles of Human Knowledge." In the last-mentioned treatise he remarks, on the relation of our visional perceptions to the sensations of touch :—

> "So that in strict truth the ideas of sight, when we apprehend by them distance, and things placed at a distance, do not suggest or mark out to us things actually existing at a distance, but only admonish us what ideas of touch will be imprinted in our minds at such and such distance of time, and in consequence of such or such actions."—Berkeley, *Principles of Human Knowledge,* (Fraser's Edition, vol. i., p. 177.)

Lichtenberg, in 1799, expresses himself in like manner when he says :—

> "To perceive something *outside* ourselves is a contradiction; we perceive only *within* us; that which we perceive is merely a modification of ourselves, therefore, *within* us. Because these modifications are independent of ourselves, we seek their cause in *other* things that are outside, and say, there are *things beyond us.*

> We ought to say, '*præter nos;*' but for '*præter,*' we substitute the preposition '*extra,*' which is something quite different, *i.e.*, we *imagine* these things in the space *outside* ourselves. This evidently is *not* perception, but it seems to be something firmly interwoven with the nature of our sensual perceptive powers; it is the form under which that conception of the '*præter nos*' is given to us—the form of the sensual."

The want of these conceptions would necessarily be felt by us, if in some individuals, and these only occasionally: the will should be capable of producing physical movements, for whose geometro-mathematical definition a four-dimensional system of co-ordinates is necessary.

To my knowledge Gauss was the first to direct from the point of view of the "Geometria Situs," his attention to the theory of the twistings of flexible cords. In his manuscripts left behind (Gauss's Werke, vol. v., p. 605), we find the following remarks:—

> "Of the '*Geometria Situs* which Leibnitz foresaw, and on which to throw a feeble glance was allowed only to a few mathematicians (Euler and Vandermonde), we, after a lapse of 150 years, know and possess hardly more than nothing. One of the principal problems on the boundary of the *Geometria Situs* and the *Geometria Magnitudinis* will be to calculate the number of the twistings of two closed and endless cords."

In my first treatise, "On Action at a Distance," I have discussed in detail the truth, first discovered by Kant, later by Gauss and the representatives of the anti-Euclidian geometry, viz., that our present conception of space, familiar to us by habit, has been derived from experience, *i.e.*, from empirical facts by means of the causal principle existing *à priori* in our intellect. This in particular is to be said of the three dimensions of our present conception of space. If from our childhood phenomena had been of daily occurrence, requiring a space of four or more dimensions for an explanation which should be free from contradiction, *i.e.*, conformable to reason, we should be able to form a conception of space of four or more dimensions. It follows that the *real* existence of a four-dimensional space can only be decided by *experience*, *i.e.*, by observation of *facts*.

A great step has been made by acknowledging that the *possibility* of a four-dimensional development of space can be understood by our intellect, although, on account of reasons previously given, no corresponding image of it can be conceived by the mind. (Dass die moeglichkeit eines vierdimensionalen Raumgebietes *begrifflich* ohne Widerspruch *denkbar,* wenn auch nicht *anschaulich vorstellbar* ist.)

But Kant advances one step further. From the *logically* recognised *possibility* of the existence of space having more than three dimensions, he infers their "very probably *real* existence when he verbally remarks:—

> "If it is *possible* that there be developments of *other* dimensions in space, it is also very *probable* that God has somewhere produced them. For His works have all the grandeur and variety that can possibly be comprised."

> "In the foregoing I have shown that several worlds, taken in a metaphysical sense, *might* exist together, but, at the same time, here is the *condition,* which, according to my belief, is the only one which makes it probable that several such worlds *really exist.*"—(Kant's Works, vol. v., p. 25.)

I may further cite the following observations of Kant:—

> "I confess I am much inclined to assert the existence of immaterial beings in this world, and to class my soul itself in the category of these beings."

> "We can imagine the possibility of the existence of immaterial beings without the fear of being refuted, though, at the same time, without the hope of being able to demonstrate their existence by reason. Such spiritual beings would exist in space, and the latter notwithstanding would remain penetrable for material beings, because their presence would imply an acting power in space, but not a *filling* of it, *i.e.*, a resistance causing solidity."

> "It is, therefore, as good as demonstrated, or it could easily be proved, if we were to enter into it at some length; or, better still, *it will be proved in the future—I do not know where and when—that also in this life the human soul stands in an indissoluble communion with all the immaterial beings of the spiritual world; that it produces effects in them, and in exchange receives impressions from them, without, however, becoming humanly conscious of them, so long as all stands well.*"

> "It would be a blessing if such a systematic constitution of the spiritual world, as conceived by us, had not merely to be inferred from the—too hypothetical—conception of the spiritual nature generally, but would be inferred, or at least conjectured, as probable from some *real and generally acknowledged observation.*"—(Kant's Works, vol. vii., p. 32.)

I have already in the above-cited treatise discussed some physical phenomena, which must be possible for such four-dimensional beings, provided that under certain circumstances they are enabled to produce effects in the real material world that would be visible, *i.e.*, conceivable to us three-dimensional beings. As one of these effects, I discussed at some length the knotting of a single endless cord. If a single cord has its ends tied together and sealed, an intelligent being, having the power voluntarily to produce on this cord four-dimensional bendings and movements, must be able, *without* loosening the seal, to tie one or more knots in this endless cord.

Now, this experiment has been successfully made within the space of a few minutes in Leipzig, on the 17th of December, 1877, at 11 o'clock a.m., in the presence of Mr. Henry Slade, the American. The accompanying plate shows the strong cord with the four knots* in it, as well as the position of my hands, to which Mr. Slade's left hand and that of another gentleman were joined. While the seal always remained in our sight on the table, the unknotted cord was firmly pressed by my two thumbs against the table's surface, and the remainder of the cord hung down in my lap. I had desired the tying of only *one* knot, yet the *four* knots—minutely represented on the drawing—were formed, after a few minutes, in the cord.

The hempen cord had a thickness of about 1 millim.; it was strong and new, having been bought by myself. Its single length, before the tying of the knots, was about 148 centimetres; the length therefore of the doubled string, the ends having been joined, about 74 centims. The ends were tied together in an ordinary knot, and then—protruding from the knot by about 1·5 centims.—were laid on a piece of paper and sealed to the same with ordinary sealing-wax, so that the knot just remained visible at the border of the seal. The paper round the seal was then cut off, as shown in the illustration.

The above-described sealing of two such strings, with *my own* seal, was effected *by myself* in my apartments, on the evening of December 16th, 1877, at 9 o'clock, under the eyes of several of my friends and colleagues, and *not* in the presence of Mr. Slade. Two other strings of the same quality and dimensions were sealed by Wilhelm Weber with *his* seal, and in his own rooms, on the morning of the 17th

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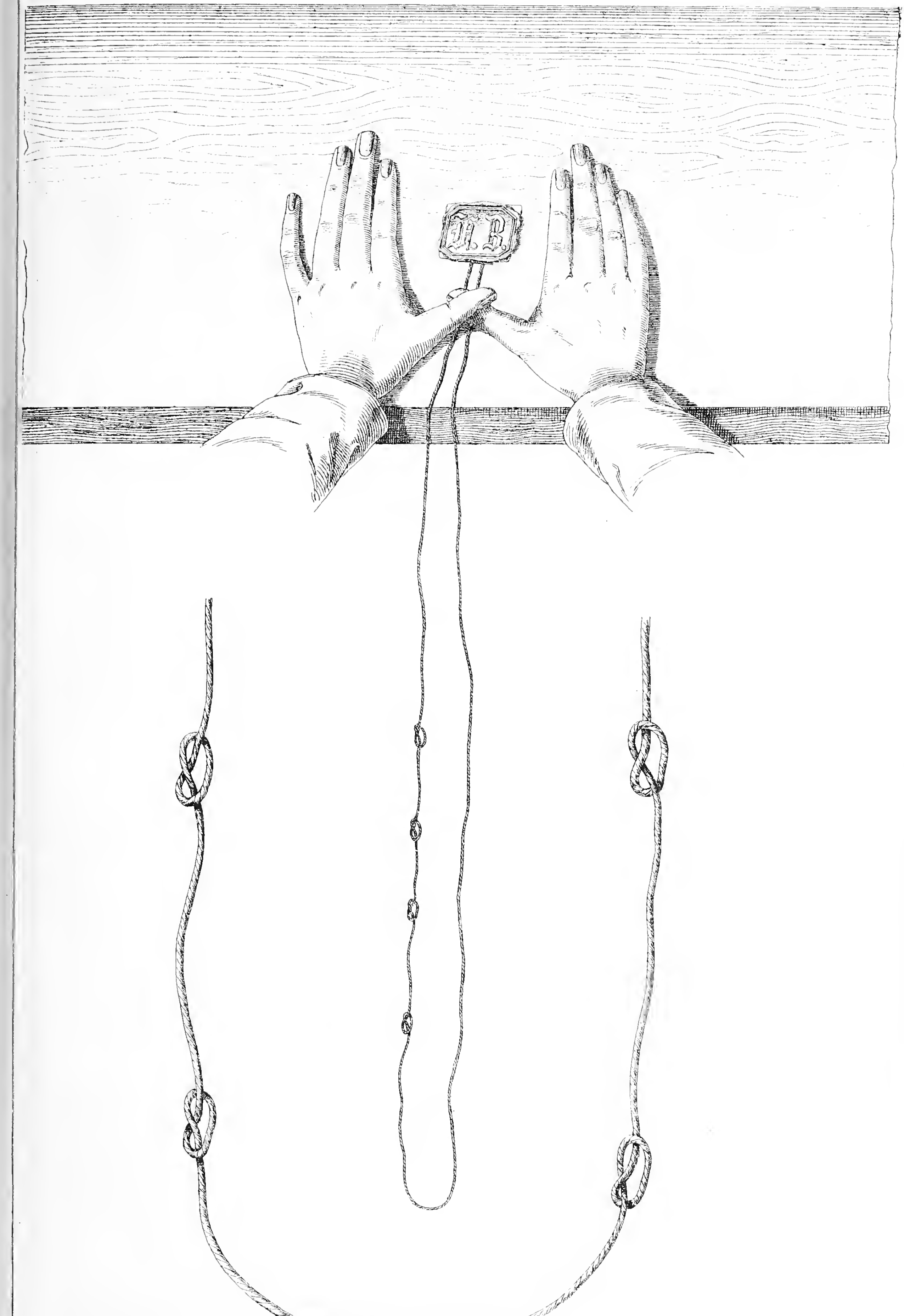
* In the enlarged drawings the knots have been represented by mistake symmetrical; they were tied on one side, in accordance with the small figure of the cord.



Lith.Anst.v.E.A.Funke, Leipzig.

of December, at 10.30 a.m. With these four cords I went to the neighbouring dwelling of one of my friends, who had offered to Mr. Henry Slade the hospitalities of his house, so as to place him exclusively at my own and my friend's disposition, and for the time withdrawing him from the public. The *séance* in question took place in my friends' sitting-room immediately after my arrival. I myself selected one of the four sealed cords, and, in order never to lose sight of it before we sat down at the table, I hung it around my neck—the seal in front always within my sight. During the *séance*, as previously stated, I constantly kept the seal—remaining unaltered—before me on the table. Mr. Slade's hands remained *all the time* in sight; with the left he often touched his forehead, complaining of painful sensations. The portion of the string hanging down rested on my lap,—out of my sight, it is true,—but Mr. Slade's hands *always* remained visible to me. I particularly noticed that Mr. Slade's hands were not withdrawn or changed in position. He himself appeared to be perfectly passive, so that we cannot advance the assertion of his having tied those knots by his *conscious* will, but only that they, under these detailed circumstances, were formed in *his presence* without *visible* contact, and in a room illuminated by bright daylight.

According to the reports so far published the above experiment seems also to have succeeded in Vienna in presence of Mr. Slade, although under less stringent conditions.* Those of my readers who wish for further information on other physical phenomena which have taken place in Mr. Slade's presence, I refer to these two books. I reserve to later publication in my own treatises the description of further experiments obtained by me in twelve *séances* with Mr. Slade, and, as I am expressly authorised to mention, in the presence of my friends and colleagues, Prof. Fechner, Prof. Wilhelm Weber, the celebrated electrician from Göttingen, and Herr Scheibner, Professor of Mathematics in the University of Leipzig, who are *perfectly* convinced of the reality of the observed facts, altogether excluding imposture or prestidigitation.

At the end of my first treatise, already finished in manuscript in the course of August, 1877, I called attention to the circumstance that a certain number of physical phenomena, which, by "synthetical conclusions *à priori*," might

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* "Mr. Slade's Aufenshalt in Wien: Ein offener Brief an meine freunde." Wien: I. C. Fischer, and Co., 1878. "Der Individualismus im Lichte der Biologie und Philosophie der Gegenwart von Lazar B. Hellenbach." Wien: Braumüller, 1878.

be explained through the generalised conception of space and the platonic hypothesis of projection, coincided with so-called spiritualistic phenomena. Cautiously, however, I said:—

> "To those of my readers who are inclined to see in spiritualistic phenomena an *empirical* confirmation of those phenomena above deduced in regard to their *theoretical* possibility, I beg to observe that from the point of view of idealism there must first be given a precise definition and criticism of *objective reality*. Indeed, if *everything* perceivable is a conception produced in us by *unknown* causes, the distinguishing characteristic of the *objective* reality from the *subjective* reality (phantasma) cannot be sought in nature, but only in accidental attributes of that process producing conceptions. If causes unknown to us produce simultaneously in several individuals the same conception, only subject to those distinctions which depend upon differences in the position of the observers, we refer such conception to a *real* object *outside* of us; this conception not taking place, we refer that conception to causes *within* us, and call it hallucination.
>
> "Now, whether the spiritualistic phenomena belong to the first or to the second category of these conceptions, I do not venture to decide, so far never having witnessed such phenomena. On the other hand, I do not possess, with regard to men like Crookes, Wallace, and others, such an exalted opinion of *my own* intellect, as to believe that I myself, under similar conditions, should not be subject to the same impressions." (Written in August, 1877.)

This supposition received, four months after my writing it down, a full confirmation by the above-mentioned experiments with the American, Mr. Henry Slade. In making them I was intent upon giving full consideration to the above-cited distinction between a subjective phantasma and an objective fact. The four knots in the above-mentioned cord, with the seal unbroken, this day still lie before me; I can send this cord to any man for examination; I might send it by turn to all the learned societies of the world, so as to convince them that not a *subjective* phantasma is here in question but an *objective* and lasting effect produced in the material world, which no human intelligence with the conceptions of space so far current is able to explain.

If, nevertheless, the foundation of this fact, deduced by me on the ground of an enlarged conception of space, should be denied, only one other kind of explanation would remain, arising from a moral mode of consideration that at present, it is true, is quite customary. This explanation would consist in the presumption that I myself and the honourable men and citizens of Leipzig, in whose presence several of these cords were sealed, were either common impostors or were not in possession of our sound senses sufficient to perceive if Mr. Slade himself, before the cords were sealed, had tied them in knots. The discussion, however, of such a hypothesis would no longer belong to the dominion of science, but would fall under the category of social decency.

Some other still more surprising experiments—prepared by me with a view to further testing this theory of space—have succeeded, though Mr. Slade thought their success impossible. The sympathising and intelligent reader will be able to understand my delight caused thereby. Mr. Slade produced on me and on my friends the impression of his being a gentleman : the sentence for imposture pronounced against him in London necessarily excited our *moral* sympathy, for the *physical* facts observed by us in so astonishing a variety, in his presence, negatived on every reasonable ground the supposition that he in one solitary case had taken refuge in wilful imposture. Mr. Slade, in our eyes, therefore, was innocently condemned—a victim of his accuser's and his judge's limited knowledge.

VII. LIQUEFACTION OF OXYGEN.*

By M. Raoul Pictet.

THE object which I have had in view for more than three years is to demonstrate experimentally that molecular *cohesion* is a general property of bodies, to which there is no exception.

If the permanent gases are not capable of liquefying, we must conclude that their constituent particles do not attract each other, and thus do not conform to this law.

Thus, to cause experimentally the molecules of a gas to

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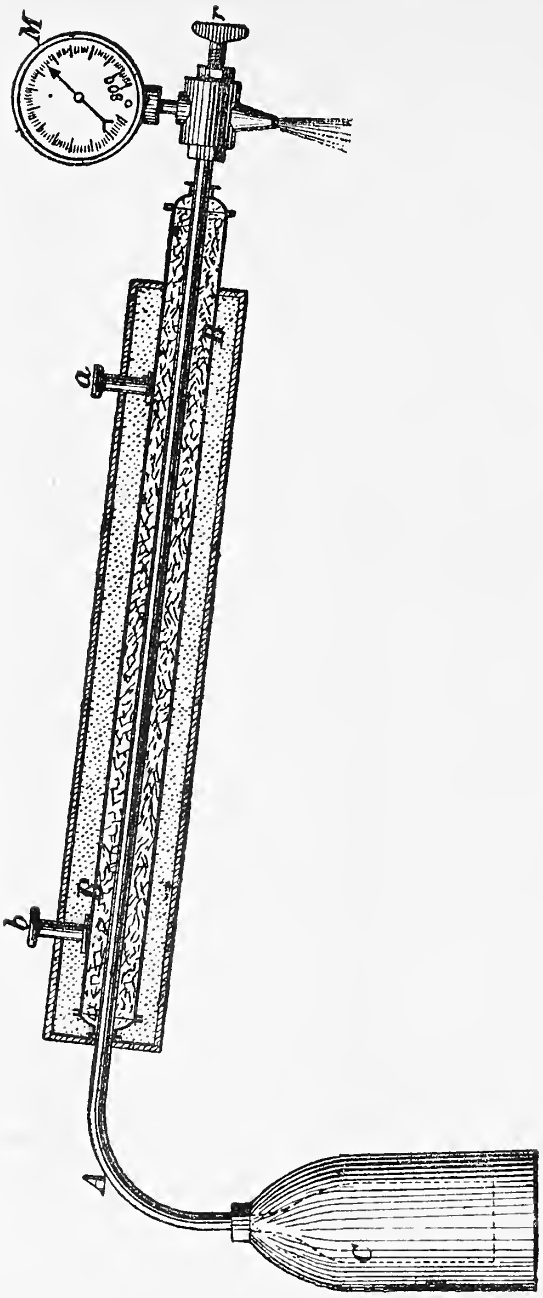
* The *liquefaction of oxygen* is so important a scientific achievement that we have much pleasure in laying before our readers the following detailed account of the means employed and diagrams of the apparatus used, which were communicated to us by M. Pictet himself.—Ed. *Q. J. S.*



DESCRIPTION OF THE DRAWINGS.

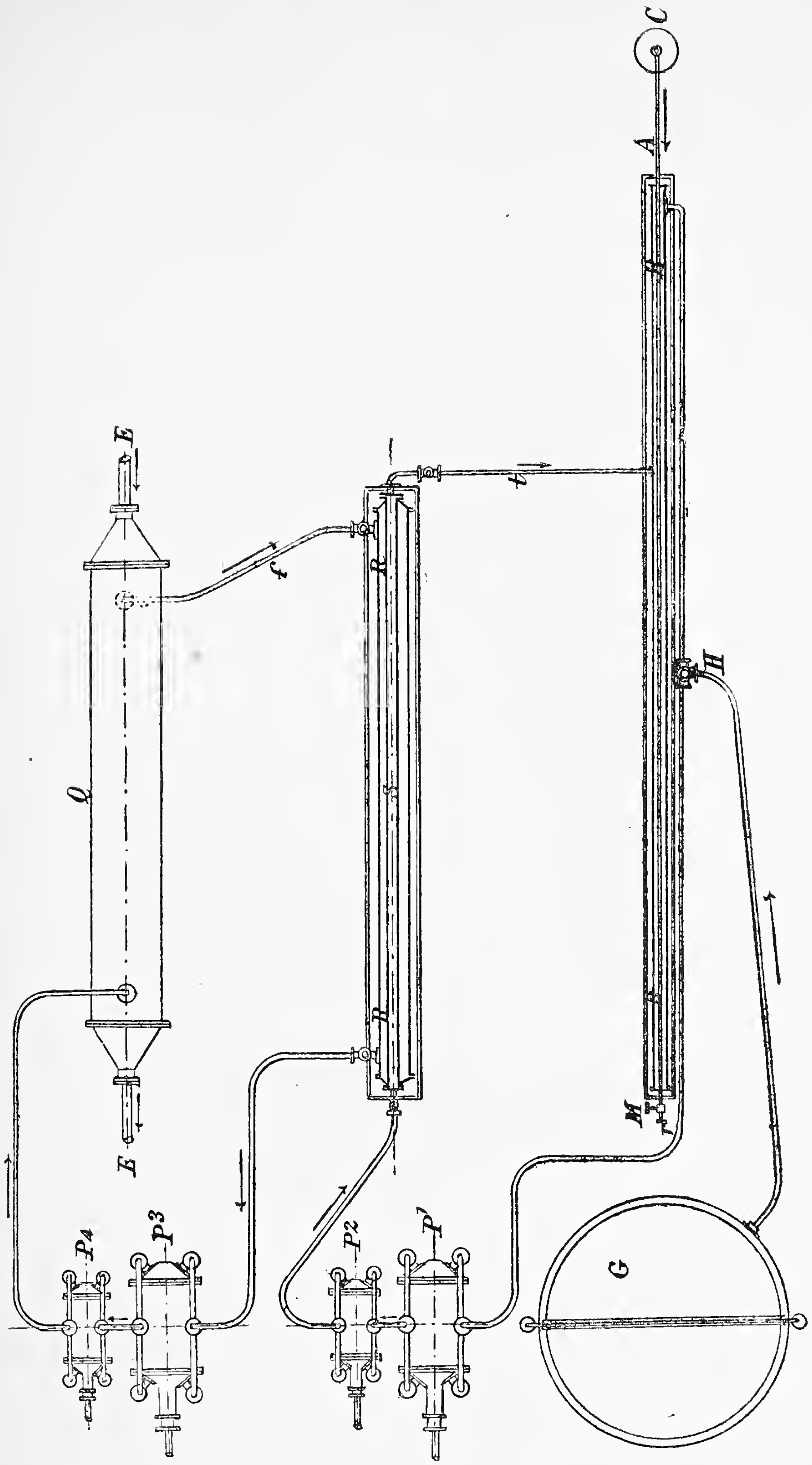
A. A tube, 5 metres long, 14 millimetres external diameter, and 4 millimetres internal diameter, in which the oxygen condenses. It is furnished with a screw-tap, *r*, from which the liquid oxygen jets out. A pressure-gauge, M, measures the pressure up to 800 atmospheres.

B. A tube, 4 metres long, in which is solid carbonic acid. The stock of carbonic acid is contained in a gasometer, G, of 1 cubic metre capacity. A three-way tap, H, puts it when desired into communication with the apparatus.

C. A howitzer shell, containing 700 grms. of chlorate of potash mixed with chloride of potassium. It is heated with gas.

P1, P2. Double-action exhaustion and force pumps, drawing carbonic acid from the tube B or the gasometer G, according to the position of the tap H.

S. A tube, 60 millimetres diameter and 1·1 metres long, in which is condensed the liquid carbonic acid compressed by the pumps. This liquefied gas returns by the small tube *t* to the tube B.



R. A tube, 125 millimetres in diameter and 1·1 metres long, containing liquid sulphurous acid.

P_3, P_4. Double-action exhaustion and force pumps, exhausting sulphurous acid gas from the tube R.

Q. A tubular condenser of sulphurous acid compressed by the pumps. This body, when liquefied, returns by the small tube *f* to the tube R. The cold water for condensing the sulphurous acid passes through the apertures E E.

a. Entry for liquid carbonic acid.

. Exit for the vapourised carbonic acid caused by the suction of the pumps.

approach each other as much as possible, certain indispensable conditions are necessary, which may be expressed thus :—

1. To have the gas absolutely pure, with no trace of foreign gas.
2. To be able to obtain extremely energetic pressures.
3. To obtain intense cold, and to subtract heat at these low temperatures.
4. To utilise a large surface for condensation at these low temperatures.
5. To be able to utilise the rapid expansion of the gas from extreme condensation to the atmospheric pressure—an expansion which, added to the preceding means, will *compel* liquefaction.

Having fulfilled these five conditions, we may formulate the following alternative :—

When a gas is compressed to 500 or 600 atmospheres, and kept at a temperature of −100° or −140°, and it is allowed to expand to the atmospheric pressure, one of two things takes place :—

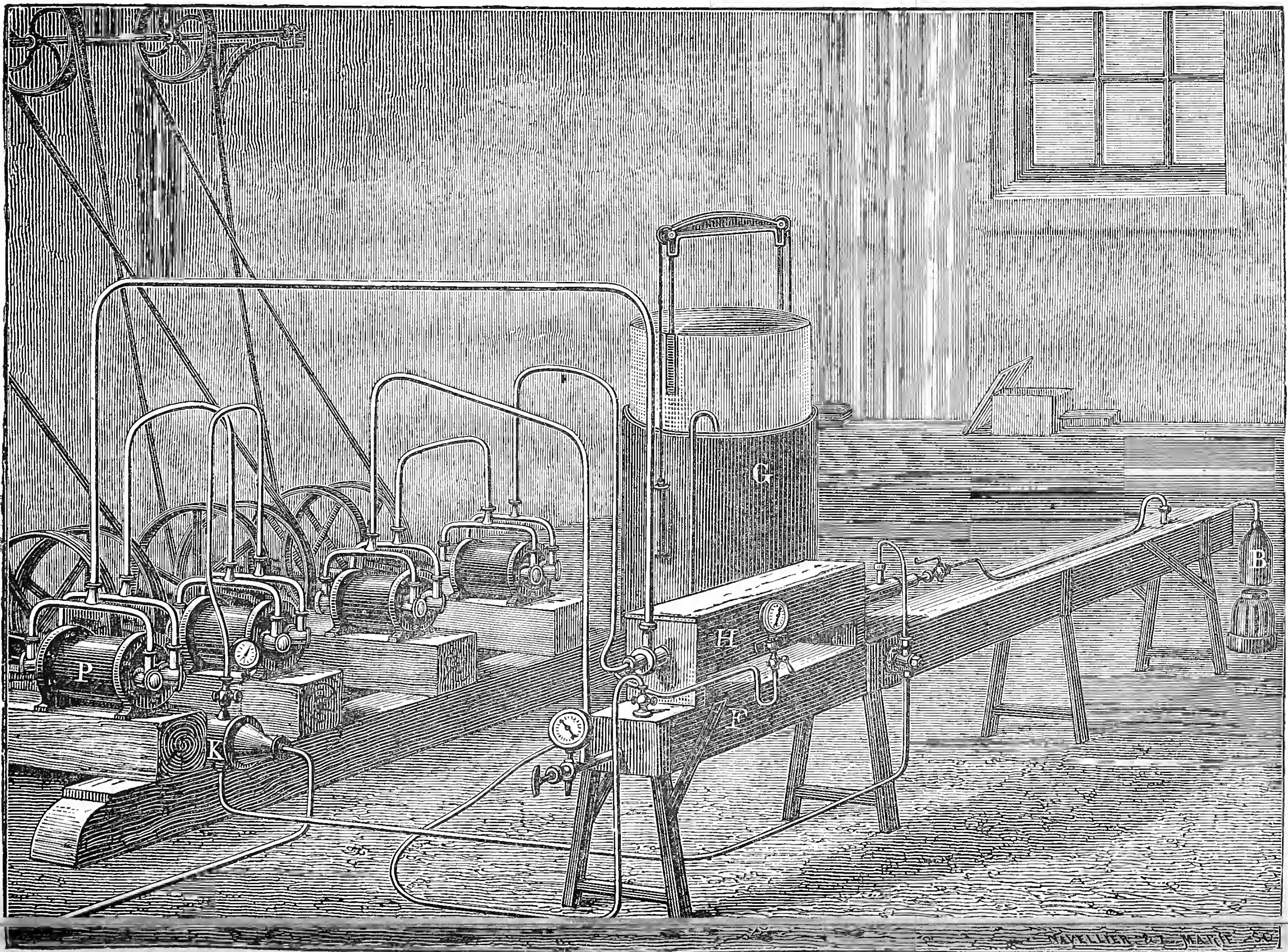
> Either the gas, obeying the force of cohesion, liquefies, and yields its heat of condensation to the portion of gas which expands and loses itself in the gaseous form; or, on the hypothesis that cohesion is not a general law, the gas must pass to the absolute zero and become inert,—that is to say, an impalpable powder.

The work done by expansion will not be possible, and the loss of heat will be absolute.

Struck with the truth of this alternative, which is rendered certain by thermo-dynamic equations based on accurate data, I have sought to produce a mechanical arrangement which should entirely satisfy these different conditions, and I have chosen the complicated apparatus of which the following is a brief description :—

I take two pumps, P_3 and P_4, for exhaustion and compression, such as are used industrially in my ice-making apparatus. I couple these pumps in such a way that the exhaustion of one corresponds to the compression of the other. The exhaustion of the first communicates with a tube (R) of 1·1 metres long and 12·5 centimetres in diameter, and filled with liquid sulphurous acid. Under the influence of a good vacuum the temperature of the liquid rapidly sinks to −65°, and even to −73°, the extreme limit attained.

Through this tube of sulphurous acid passes a second smaller tube (S), of 6 centimetres diameter, and the same


P
K
G
H
F
B
NAVELLIER & L. MARIE SC

length as the envelope. These two tubes are closed by a common base.

In the central tube is retained compressed carbonic acid produced by the reaction of hydrochloric acid on Carrara marble. This gas, being dried, is stored in an oil gasometer (G) of 1 cubic metre capacity.

At a pressure of from 4 to 6 atmospheres the carbonic acid easily liquefies under these circumstances. The resulting liquid is led into a long copper tube (B), 4 metres in length and 4 centimetres in diameter.

Two pumps, P_1 and P_2, coupled together like the first, exhaust carbonic acid either from the gasometer (G) or from the long tube (B) full of liquid carbonic acid.

The ingress to these pumps is governed by a three-way tap, H. A screw valve cuts off at will the ingress of liquid carbonic acid in the long tube; it is situated between the condenser of carbonic acid and this long tube. When this screw valve is closed, and the two pumps draw the vapour from the liquid carbonic acid contained in the tube 4 metres long, the greatest possible lowering of temperature is produced; the carbonic acid solidifies and descends to about −140°. The subtraction of heat is maintained by the working of the pumps, the cylinders of which take out 3 litres per stroke, and the speed is 100 revolutions a minute.

Both the sulphurous acid tube and the carbonic acid tube are covered with a casing of wood and non-conducting stuff to intercept radiation.

In the interior of the carbonic acid tube, B, passes a fourth tube, A, intended for the compression of oxygen; it is 5 metres long and 14 millimetres in external diameter. Its internal diameter is 4 millimetres. This long tube is consequently immersed in solid carbonic acid, and its whole surface is brought to the lowest obtainable temperature. These two long tubes are connected by the ends of the carbonic acid tube, consequently the small tube extends about 1 metre beyond the other. I have curved this portion downwards, and given the two long tubes a slightly inclined position, but still very near the horizontal, as I have shown in the accompanying drawing.

The small central tube is curved at A, and screws into the neck of a large howitzer shell, C, the sides of which are 35 millimetres thick; the height is 28 centimetres, and the diameter 17 centimetres.

This shell contains 700 grms. of chlorate of potash and 256 grms. of chloride of potassium mixed together, fused, then broken up, and introduced into the shell perfectly dry. When the double circulation of the sulphurous and carbonic acids has lowered the temperature to the required degree, I heat the shell over a series of gas-burners. The decomposition of the chlorate of potash takes place at first gradually, then rather suddenly towards the end of the operation. A pressure-gauge, M, at the extremity of the long tube, lets me constantly observe the pressure and the progress of the reaction. This gauge is graduated to 800 atmospheres, and was made for me expressly by Bourdon, of Paris.

When the reaction is terminated the pressure exceeds 500 atmospheres; but it almost immediately sinks a little, and stops at 320 atmospheres. If at this moment I open the screw-tap, *r*, which terminates the tube, a jet of liquid is distinctly seen to spirt out with extreme violence. I close the tap, and in the course of a few moments a second jet—less abundant, however, can be obtained.

Pieces of charcoal, slightly incandescent, put in this jet inflame spontaneously with inconceivable violence. I have not yet succeeded in collecting the liquid, on account of the considerable projectile force with which it escapes, but I am trying to arrange a pipette, previously cooled, which possibly may be able to retain a little of this liquid.

Yesterday I repeated this experiment before the majority of the members of our Physical Society, and we had three successive jets, well characterised. I cannot yet determine the minimum pressure necessary, for it is evident that I have a surplus pressure produced by the excess of gas accumulated in the shell, and which could not condense in the small space represented by the interior tube.

I hope to utilise a similar arrangement in atempting the condensation of hydrogen and nitrogen, and I am especially occupied with the possibility of maintaining low temperatures very easily, thanks to four large industrial pumps which I have at my disposal, worked by a steam-engine.

The following experiment was performed for the fourth time on Thursday, December 27th, in the presence of ten scientific men—among others, Prof. Hagenbach, of Bâle, who came expressly to assist at this important experiment, the success of which called forth the applause of all present:—

At 10 o'clock in the evening the manometer, which had risen to 560 atmospheres, sank in a few minutes to 505, and remained stationary at this figure for more than half-an-hour, showing by this diminution in the pressure that part of the gas had assumed the liquid form under the influence of the 140 degrees of cold to which it was exposed. The tap closing the orifice of the tube was then opened, and a jet of oxygen spirted out with extraordinary violence.

A ray of electric light being thrown on the escaping jet showed that it was chiefly composed of two parts;—one central, and some centimetres long, the whiteness of which showed that the element was liquid, or even solid; the other exterior, the blue tint of which indicated the presence of oxygen compressed and frozen in the gaseous state.

VIII. THE PHONOGRAPH.

NO sooner had the wonderful simplicity and marvellous capabilities of Mr. Graham Bell's articulating telephone been practically demonstrated in England than we were startled by an announcement in the American journals that another instrument had been invented, by Mr. T. A. Edison, which would not only receive and register, but also reproduce at any distant period, whatever sounds were uttered into it by the human voice. The first accounts of the wonders of the instrument were evidently somewhat coloured: that it does, however, actually re-produce vocal sounds was demonstrated by Mr. W. H. Preece, at his Lecture on the Telephone, at one of the Friday evening meetings at the Royal Institution, when he exhibited the Phonograph for the first time in England. By the courtesy of the Editor of "Engineering" we are enabled to place before our readers drawings and descriptions of different forms of this instrument.

Fig. 1 is a general view of Mr. Edison's instrument, which has recently been brought to this country by Mr. Puscus, his representative. It consists of a brass cylinder, which, by a winch handle, can be rotated on a horizontal axis, upon which is fixed a heavy fly wheel for the purpose of controlling, to some extent, its speed of rotation. One end of this horizontal axis is screwed, and turns in a screwed bearing, so that the cylinder is not only rotated on its axis, but has imparted to it a lateral movement from end to end when the winch is rotated. Around the circumference of the cylinder is turned a spiral groove, the pitch of which is the same as that of the screw on the horizontal shaft, so that if a fixed pointer were to be set in the groove at any portion of its

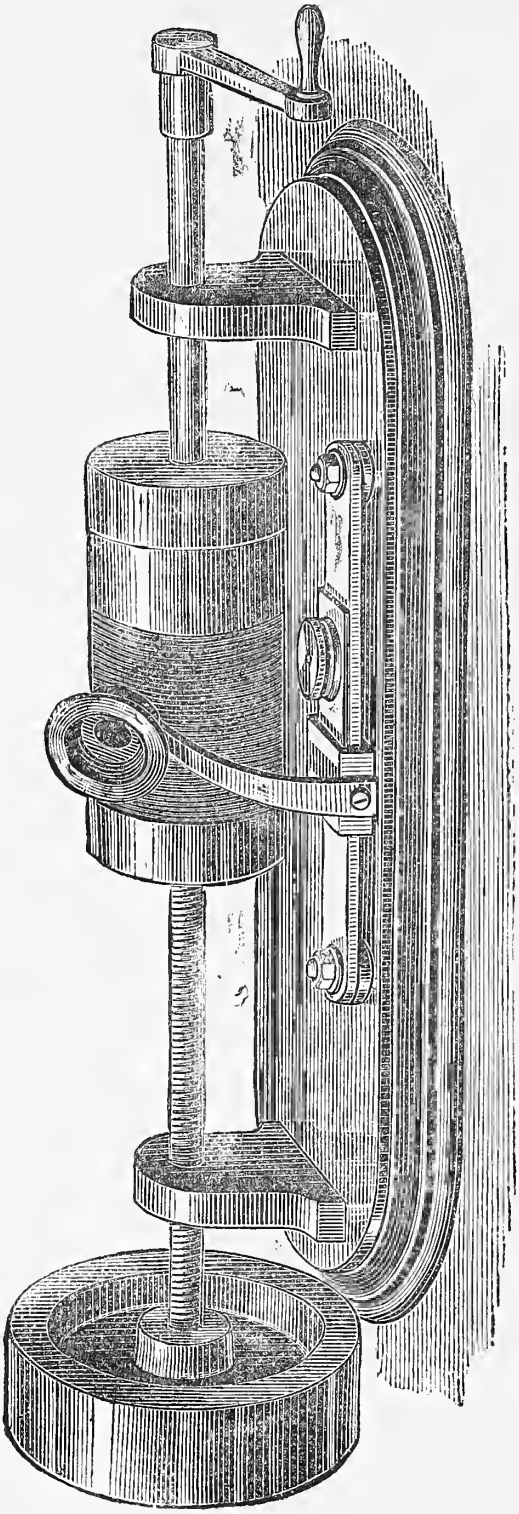


Fig. 1.

length it would remain in it as the cylinder was rotated until it worked out at either end.

In front of the cylinder and directed to its axis is fixed a thin metallic diaphragm, carried by an arm attached to the stand of the instrument, and provided with adjustments by which a steel pin projecting from its centre may be accurately set in the middle of the groove and at a proper depth; and in front of the diaphragm a mouthpiece is fixed, very similar in form to that employed in Professor Bell's telephone.

From the above description it will be evident that if the diaphragm be set into vibration by sounds being uttered into the mouthpiece, the steel pin attached to it, partaking of that vibratory motion, will enter into greater or less depths into the groove on the cylinder, according as the amplitude of vibration of the diaphragm by which its motion is controlled be large or small. If, while this vibration is going on, the cylinder be rotated in its screwed bearing, the point of the pin will trace out a spiral undulating path of motion within the groove, the amplitude of whose waves will be equal to that of the vibrations of the diaphragm, their length and form being dependent upon the rapidity and character of the undulations of the metallic membrane combined with the surface speed of the cylinder.

In order to obtain a permanent record of this wave-like path a sheet of ordinary tin-foil is fastened round the cylinder, being secured in its place by brass caps, shown on the drawing; and, as the centre of the diaphragm is adjusted so as always to be opposite to the middle of the groove, which is bridged over by the tin-foil, it follows that the pin in vibrating with the diaphragm must indent the tin-foil, which, at any spot below it, is unsupported by the resisting surface of the cylinder, having nothing but a groove behind it, and as the cylinder is rotated a chain of indentations is produced, which is in every particular a record of the sounds which originated them.

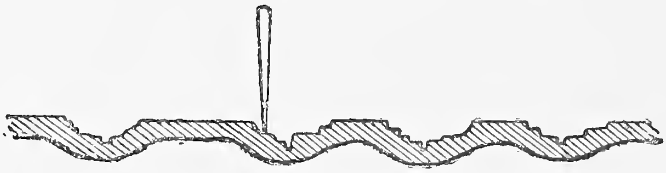
So far the apparatus is complete as an instrument for recording sounds, and as such is not superior to many of its predecessors,—such as the very beautiful logograph of Mr. W. H. Barlow, F.R.S., the phonautograph of M. Leon-Scott, or the instruments of Prof. Marey and the late Sir Charles Wheatstone,—but the most wonderful feature of Mr. Edison's phonograph is that it not only interprets its own record, but does so by re-converting it into sonorous vibrations, repeating the sounds, whether articulate or otherwise, in the actual voice in which they were originally communicated to the mouthpiece.

In Mr. Edison's first apparatus this was accomplished by employing a second diaphragm of paper, fixed on the opposite side of the cylinder to the first diaphragm, and thrown into vibration by being attached by a silken thread to a light steel spring, which carried at its extremity a blunt metallic point, which was held by the elasticity of the spring against the tin-foil covering the cylinder, but with sufficient lightness to allow it to be thrown into vibration as the indentations on the tin-foil passed beneath it.

Mr. Edison has, however, in his more recent instrument, of which Fig. 1 is an illustration, dispensed with this second membrane, making the one metallic diaphragm do double duty, first by receiving vibrations from the voice and impressing them upon the tin-foil, and afterwards by being thrown into vibration by the passing below its projecting pin of the indentations so produced, and thus giving out a repetition of the original sound.

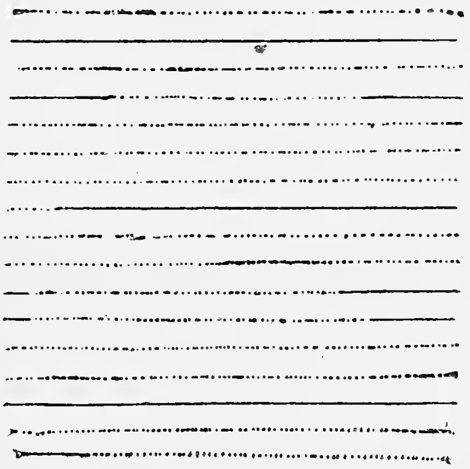
The diagram Fig. 2 will serve to illustrate the principle upon which the tin-foil is impressed by the action of the vibration of the diaphragm, and how the latter is again thrown into precisely similar vibration by the movement of the embossed foil below it. It represents a magnified section of the tin-foil taken along the line of the indentations, showing the position of the pin as it rides over its surface while the foil is travelling below it from right to left. The indentations, of which three only are shown on the diagram, having been produced on the foil by the sonorous vibrations ot the diaphragm, it follows, from the construction of the instrument, that if the foil be drawn under the pin at precisely the same speed as it was travelling when the impressions were made upon it in the first instance, it will cause the diaphragm to vibrate in an exactly similar manner to that in which it vibrated under the influence of the voice, and, from what was pointed out at the beginning of this article, it would therefore emit a similar sound. This diagram is, of course, greatly exaggerated, and must be taken only as an illustration of a possible explanation of what goes on in the action of the instrument, but which is by no means certain. It presupposes that each indentation is made up of a minute structural surface, the details of which are so small as to be quite indistinguishable under

Fig. 2.



comparatively high powers of the microscope, and yet must be sufficiently pronounced to impart to the diaphragm through its projecting pin those minute variations of vibration by which the proper form is given to the sound-bearing waves of the air, and the exact quality of the sound is conveyed to the ear. It is almost impossible to conceive that microscopical striæ (for they can be nothing more) upon such a substance as tin-foil can impart, by mechanical means, to a diaphragm as rigid as that employed in the phonograph, such niceties of motion; but the phenomena connected with the telephone have shown that metallic diaphragms are capable of imparting to the air, and the human ear is capable of detecting, sonorous vibrations whose amplitude is so minute as to have been altogether unsuspected before. Some idea of the minuteness of the indentations may be formed from Fig. 3, which is printed from an electrotype cast of a piece of the foil, and is therefore a facsimile on a plane surface of the marks recorded by the instrument.

Fig. 3.



It will readily be understood that, in order to obtain a perfect reproduction of the original sounds, the tin-foil must travel below the diaphragm at precisely the same speed as it was turning when it was receiving the impressions, and therefore in Mr. Edison's second instrument, which we have been describing, the heavy flywheel was added to render the speed of rotation as uniform as possible; but in turning the instrument by hand it is impossible, notwithstanding this addition, to insure the surface speed of the cylinder being always the same. In order to meet this requirement Mr. Edison has since applied clockwork mechanism for driving the apparatus, with such marked success that twelve clerks were lately able to take down correctly portions of newspaper articles from dictation spoken to them by the instrument. At a recent exhibition of the phonograph in New York its articulation was distinctly heard and understood at a distance of 425 feet from the instrument. Still more

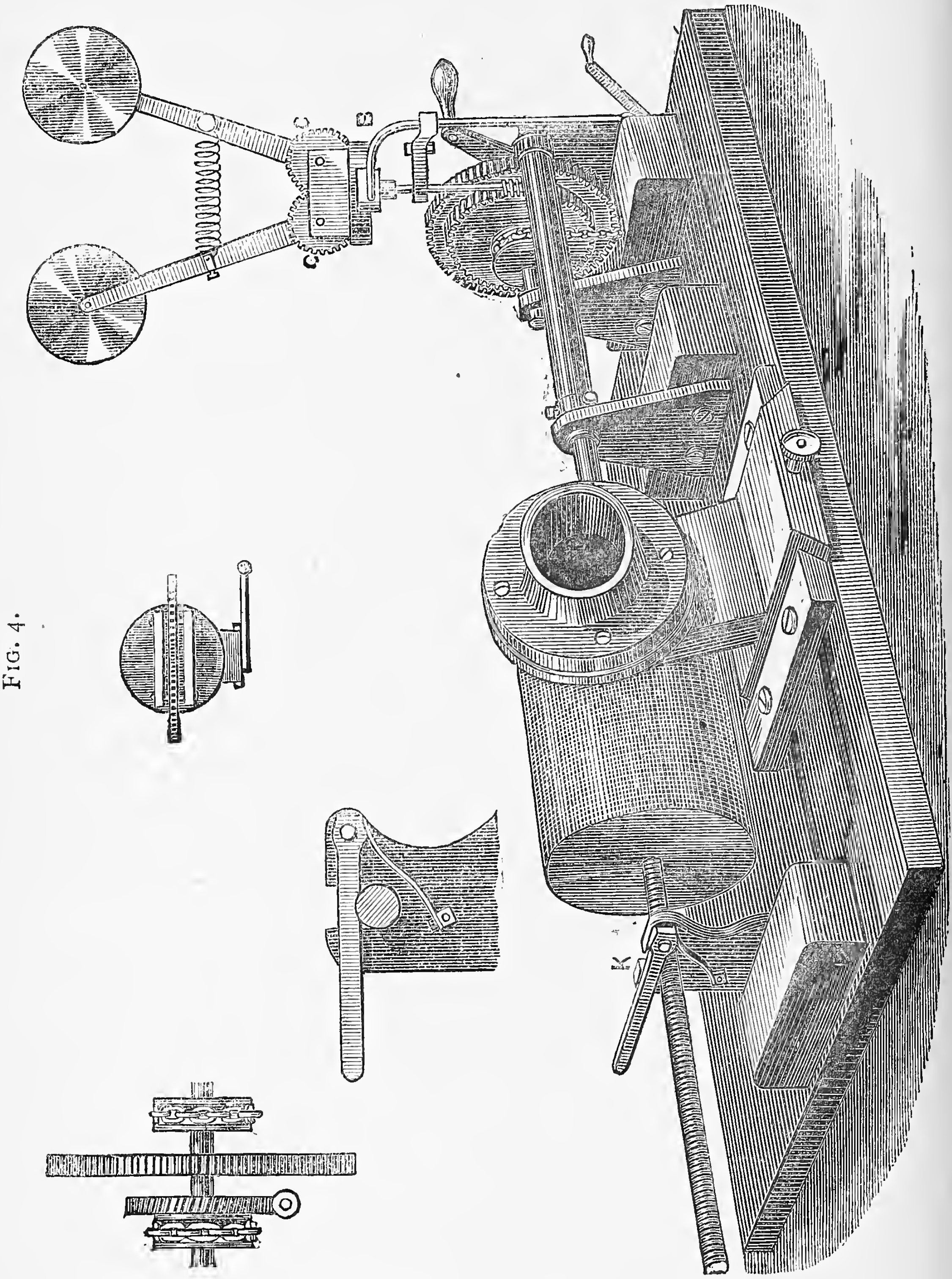


FIG. 4.

recently Mr. Edison has made records on copper-foil that could be read at a distance of 275 feet in the open air.

Fig. 4 is a view of a very beautifully arranged instrument, designed and constructed for his own use and instruction, by Mr. Augustus Stroh.

The cylinder is driven round at a surface speed of about 1 foot in a second, by means of exceedingly simple controlled clockwork mechanism actuated by a descending weight, attached, upon Huyghen's maintaining principle, to an endless chain passing over a pulley fixed upon the principal axis of the instrument, so that it is possible to wind up the weight while the cylinder is rotating without affecting its speed.

The controlling fan, or governor, which is beautifully simple and efficient, consists of two circular disks of brass mounted at the upper ends of two light levers, which are geared together at their lower ends, so as to cause them to fly out symmetrically on each side of the axis of rotation of the vertical fly-shaft to which they are pivoted. When the machine is started (by taking off the pressure of a small cork-lined brake-block, shown in small detail sketch, which presses against the cylindrical head of the fly-spindle) the disks fly out under the influence of centrifugal force, and the resistance of the air to the motion of the spindle is increased by the increase in the diameter of their path of rotation if the speed become too great. Should, however, the speed of rotation tend to fall off, a spiral spring, which can be attached to the fan levers at any position in their length, draws them together, and, by reducing the circle of their path, offers to the mechanism a diminished resistance to rotation.

At a recent meeting of the Society of Telegraph Engineers both the instruments which we here describe were exhibited in illustration of a very interesting paper, by Mr. W. H. Preece, C.E., upon this last and perhaps greatest marvel of the application of Science which this or any other age has seen.

Mr. Edison's first form of phonograph was represented by a very successful instrument made by an amateur, Mr. Pigeon, from descriptions received from America, and in which the two diaphragms, the one of paper and the other of metal, were employed.

The second form of apparatus was represented by Mr. Edison's own instrument (Fig. 1), and the more perfect form driven by controlled mechanism was represented by Mr.

Stroh's instrument, which has been described, and which is illustrated in Fig. 4.

The first words were spoken into the mouthpiece by Mr. Puscus. The mouthpiece was then withdrawn, the cylinder turned back until the pin was at the beginning of the groove, a cone of paper or speaking trumpet was put on in front of the mouthpiece, and the handle once more rotated, when the instrument shouted out, in a perfectly clear voice, "The Phonograph presents its compliments to the audience." This was heard in every portion of the hall of the Institution of Civil Engineers, and brought forth rounds of applause, to which were added roars of laughter, when it again called out in a voice still clearer than before, "How do you do? How do you like the Phonograph?" and then began to laugh in veritable hearty human laughter, "Ha! ha! ha! ha! ha! hurray!"

Mr. Pigeon's instrument was next tried, and the sublime words of the national war song—

"We don't want to fight, but by Jingo if we do,"

followed by the recital of the equally ennobling creation of the poet—

"Twinkle, twinkle, little star,
How I wonder what you are."

were given by the instrument with an emphasis entirely its own, which caused great merriment.

A song was next sung into the mouthpiece, and was reproduced amazingly out of tune, in consequence of the impossibility of obtaining a perfectly uniform speed of rotation.

When, however, Mr. Stroh's instrument was brought into use, the value to the phonograph of controlling the speed of the cylinder by mechanical means was at once apparent, for not only was the articulation of spoken words more perfect, but songs sung into it by Mr. Spagnoletti, Mr. Edmunds, and Mr. Preece, were reproduced with very respectable correctness; and even the breakdown of one of the singers at a high note, accompanied by a little impatient remark, was faithfully recorded, and given out again with exasperating fidelity.

At the Physical Society, on March 2nd, the instrument was again described by Mr. Preece, followed by a similar series of experiments, with the addition of causing the instrument to perform the wonderful feat of reproducing a duet sung into it through a double mouthpiece by Mr. Spagnoletti and Mr. Sedley Taylor. The result would certainly not make the musical reputation of either gentleman, did it stand upon no intrinsic merits of its own, but it was a remarkable experiment as showing the marvellous powers of which the phonograph of the future may be capable. At this meeting it was further shown that when an indented sheet of tin-foil has been employed to emit sounds it retains its form with such perfectness that the sounds can be reproduced by means of it a second, and even a third, time, with nearly equal distinctness.

NOTICES OF BOOKS.

Some Chemical Difficulties of Evolution. By J. J. MACLAREN, M.A. London: Bumpus.

PERHAPS our readers may remember that about two years ago there appeared a book entitled "A Critical Examination of some of the Principal Arguments for and against Darwinism." Whilst cheerfully acknowledging the candour shown by the author, we had but too much reason to declare that his work was more calculated to retard than to promote the definite solution of the question which he had attempted to discuss. We urged that for men to deal with the most complicated problems in any science before having made themselves familiar with its rudiments was an absurdity at once painful and ludicrous. We have met with no instance where a working naturalist—a man who had really studied organic life in the fields, the woods, the museum, and the biological laboratory—formed a more favourable opinion of Mr. Maclaren's book than did we. But, unfortunately, special knowledge of the subject in question is not considered more necessary for reviewers than for authors. Certain writers in the political and social papers of the day—on the principle, doubtless, that "a fellow-feeling makes us wondrous kind"—pronounced the book "useful and acceptable," and thus encouraged the author to further efforts in the same direction. In our notice of Mr. Maclaren's former work we pointed out that he failed to "distinguish with sufficient clearness between *Evolutionism*—the doctrine of a progressive mutation of species—and *Darwinism*—the explanation of such changes by the hypothesis of Natural and Sexual Selection." In the present volume the confusion is worse confounded, in a manner which we think indicates, to say the least, very culpable carelessness. He tells us that there have been put forward two views—the "Creationists' view," the view of the "Evolutionist properly so-called, besides Mr. Darwin's view." We are not sure whether the author is not here taking leave of that fairness which characterised his first work. To give to the opponents of Evolution the name of "Creationists" seems very like the *suggestio falsi* that Evolutionists as such deny creation. The "Creationists," according to Mr. Maclaren, hold—

(1.) "That life was originally called into existence by a mighty power whose commands matter and force are obliged to obey.

(2.) "That the same mighty power, after having originally called the first living beings into existence, has continued to take a direct part in calling into existence the various new forms which have from time to time appeared on the earth."

It would possibly surprise Mr. Maclaren were he told that there are multitudes of Evolutionists who fully accept the first of these propositions, and, perhaps with the sole exception of the word "direct," which might require some qualification, even the second! On the other hand, there are, or at least have been, men who, whilst disbelieving in the transmutation of species, repudiate equally the necessity of a Creator, asserting that each form of life sprang into existence—

> —— "When fatal chance
> Had circled its full orb."

The old school of Natural History has therefore no exclusive right to the title of "Creationist," its sole fundamental principles being that species are non-transmutable, and that each form of life originated from inorganic matter.

In opposition to the "Creationist" we have next the "Evolutionist," of whose opinions the author gives a singularly travestied summary:—

(1.) "That the first combination of atoms which possessed the distinctive properties of life was called into existence by the action of the physical forces which still surround us, on a portion of the matter of which our earth consists.

(2.) "That the first form of living being when once called into existence, and afterwards its more or less modified descendants, have been made to vary gradually by changes in the external forces incident; that the actual variations produced were due in part to gradual changes in the external incident forces, and in part to internal variations, the more or less remote consequences of previous changes in the external forces surrounding living beings; that any variation arising in this way which gave the living being in which it occurred an advantage in the struggle for life under the conditions which then prevailed was picked out by the survival of the fittest to compete in this struggle, or, as it is generally called, by Natural Selection; that all the known forms of life, both existing and extinct, have in this manner been developed from the first simple form of living being under the influence of repeated slow changes; and that all change of form among living beings has been very gradual."

Further, we find Darwinianism not placed as it ought as one of the forms of Evolutionism, but classified as a distinct and independent doctrine. If Mr. Maclaren had taken the trouble to master even the outlines of the subject upon which he is, for the second time, coming forward as a teacher, he would have found that the "view of the Evolutionist properly so called" merely amounts to this—*viz.*, that organic species are capable of transmutation, and that new species, like new individuals, take their rise not from inorganic lifeless matter, but from antecedent living beings. Within this common doctrine shades of opinion exist in vast variety, that of Mr. Darwin being one and that which Mr. Maclaren coolly ascribes to "the Evolutionist" another. An Evolutionist may, certainly, in common alike with Mr. Wallace and Mr. Darwin on the one hand, and with Professors Haeckel and Oscar Schmidt on the other, consider that the origin of species is due wholly or in great part to the agency of Natural Selection; or he may, with Mr. Mivart, ascribe it rather to an internal force which occasions modification of structure in certain divinely pre-ordained directions. He may regard Evolution as a slow, gradual process, moving on at one uniform rate; or he may, with Dr. Leconte, ascribe to it a more paroxysmal character, having its periods of permanence followed by shorter intervals of rapid change. Our author, overlooking all these important distinctions, and imputing to all Evolutionists, as a body, notions which many of them would most earnestly repudiate, has, at best, set up a straw puppet to show his skill in demolishing it, and has given most abundant proof of his own inability to handle the subject. The chemical difficulties which he points out, and which might have been summed up in much fewer words, are not devoid of a certain weight as against Haeckel. As against Darwin and Wallace, the author himself is not very confident of their validity, whilst there are other schools of Evolutionists whom they do not touch at all.

We notice the following passage:—"Consider such a compound as the frightful poison aconitine, which is met with only in the roots and other parts of the genus Aconite. This is a distinct and specific chemical compound, and is one of the most powerful and active poisons known, and it will at once be said the possession of such a poison must be of great advantage to the plant as a protection from enemies which would otherwise devour it. No doubt; but the point is whether anything was gained by the elaboration of so very intense and active a poison. Would not the same end have been practically attained by the development of an acrid poison of far less intensity? And, if so, how came it that this special compound was wrought up to so unnecessary a point of perfection, when the energy of the plant would have been more usefully directed into other operations?" But how does Mr. Maclaren know that the production of an intense poison is a greater strain upon the energies of a plant than the secretion of a colouring principle, of an odour, or of a non-poisonous organic base? Further, much of his question can with great effect be turned upon the teleologists of the Old School. Why, for instance, is the bite of the cobra so much more venomous than is required for the destruction of the animals upon which it preys? Against larger animals it is no real defence, for though mortally wounded they have ample time to take summary vengeance.

That chemistry has evidence to give upon the question of

Organic Evolution we do not dispute, and Mr. Maclaren may have rendered some service by calling the attention of enquirers in that direction. But to find out such evidence and to ascertain its value will require the labour of many years—perhaps of more than one life-time.

Annual Report of the Board of Regents of the Smithsonian Institution. Washington: Government Printing Office. 1877.

In the official reports with which this volume commences we find an announcement that the National Museum, which has hitherto been merged in the Smithsonian Institution, is about to be placed on a distinct basis, it being considered that the two establishments have now reached such a state of development that they can scarcely be continued under one common organization. The appropriation for the maintenance of the Museum now amounts to 20,000 dollars annually.

Among the memoirs included in the volume are the *eloge* on Gay-Lussac, delivered by Arago; a biographical sketch of the present Emperor of Brazil; a paper by Mr. W. B. Taylor, of Washington, on "Kinetic Theories of Gravitation." After reviewing all the speculations which more or less directly bear upon the subject from the days of Dr. R. Hooke down to the present time, the author, in summing up, remarks that *every* kinetic system suffers from the "culminating vice" of an "utterly reckless violation of any rational conception of the conservation of energy." He continues: "And yet, remarkably enough, the ostensible impulse and occasion of such creeds have usually been a strong veneration for this much-abused principle and the consciousness of a special mission to restore and to vindicate its neglected authority! Not unfrequently the vibrations communicated to the telegraphic æther by a trembling atom have been supposed to be transmitted unimpaired to that or to other atoms and back again in endless and magnificent cycles of perpetual motion. And as there is no limit to the *vis viva* which such a medium may conserve within its boundless bosom, such projectors have the Bank of the Infinite on which to draw in every dynamic emergency, without the fear of a depleted treasury, and without any necessity being felt for inquiring too nicely into the balance of the depositor's account. And thus, as Leray has intimated, suns and stars are maintained blazing for ever on a borrowed capital of motion."

In opposition to such views the author maintains that we have, from experience, no reason for believing the æther to be in any case a source of energy.

There is also a long and not unimportant paper by Prof. G. Pilar, on the "Revolutions of the Crust of the Earth." The author starts with the hypothesis of M. Faye, who, in opposition to Wilson, Herschel, and Arago, maintains that the mass of the sun is entirely gaseous, and that by the contraction of this mass is furnished the enormous amount of heat radiated into space. Reviewing the gradual cooling and condensation alike of stars and of planets, he brings us to the dawn of the geological life of our earth, which he describes in accordance with views generally accepted. The elevatory action of the central heat he considers as "slow and constant." In his views of areas of elevation he mentions Australia, as not merely now rising, but as having been covered by the sea at no very distant time. Yet it seems perfectly clear that at dates geologically not very remote Australia must have passed through a stage of subsidence, and that it extended eastwards as far as the great "Barrier Reef," and to have been almost, if not quite, connected with New Guinea to the northward, as has been shown by Mr. A. R. Wallace.

In the next chapter, treating of the "Fluid Envelope of the Earth," he calculates that at the present rate of denudation the entire American continent must be levelled and buried in the sea in four million years, supposing no upheavals to intervene. The denudation of India proceeds at a rate at least three times more rapid.

The author does not consider the atmosphere as permanent either in its mass or its composition. "According to Ebelmann (?), if the stratified rocks had contained 1 per cent. of protoxide of iron, this would have been sufficient to absorb all the oxygen of the air." He maintains that the (free) nitrogen of the atmosphere is also diminishing in quantity. He thinks that nitrogen is "taken out of the air by guano and other analogous deposits;" and, on the authority of D'Archiac, asserts that the nitrogen withdrawn from the atmosphere cannot unassisted return to its source, and that we know of no natural agent for its restoration! By "Swiss Saxony" (p. 310) Prof. Pilar probably means the district usually known as the Saxon Switzerland, or better as the Highlands of the Elbe, extending from the southern side of the Dresden district to the frontiers of Bohemia. Speaking of the fact, recorded already by Pliny, that mineral spring water is unsuited for culinary purposes, the author tells us that "Chemistry teaches us that selenitic waters hold in solution carbonate of lime, which combining with the legumin render these fruit unfit for alimentation." We should rather suppose that selenitic waters must contain selenite, *i.e.*, sulphate of lime.

The author's remarks upon the fossil fauna and flora of the world and upon the origin of species cannot be considered remarkably happy. Speaking of coal, he says:—"England, Belgium, France, Prussia, Silesia, Bohemia, Hungary, possess mines of more or less importance. Scandinavia, Russia, Greece, Italy have, we may say, no coal-deposits." Here is a serious error; the coal-beds of Russia are large and important. He proceeds:—"Of other parts of the world America is the most richly gifted; while Australia possesses the least." Here we must again convict him of error; there are undoubtedly regions totally without coal, whilst Australia contains very considerable deposits.

The view that the desert of Sahara was once a sea, communicating through the gulf of Gabes with the ocean, is by no means universally accepted.

The author's fifth chapter, on "Ice," deals with the interesting and complicated question of glaciation. We have first the old dispute as to whether the climate of Europe is improving or deteriorating. M. Pilar evidently takes the latter view. He writes:—"We may, on the contrary, attribute to a slow cooling the increased extent of the Swiss glaciers which from the twelfth century have more and more obstructed the ancient passes of the mountains, destroying forests and habitations, and reducing the temperature of the surrounding country. In France and Belgium the culture of the vine has ceased in many regions where formerly its products were of great importance. The disappearance of pines in Ireland indicates that Britain also experiences this decrease of temperature. But the most striking proof of a considerable cooling is furnished us by the dwindling and decay of the birch forests of Iceland. This island, eminently volcanic, was in the middle ages the seat of an advanced civilisation. The magnificent woods were peopled with numerous animals, and the nightingale made music in the groves of this island now ravaged by cold and fire." This passage he substantially repeats elsewhere, maintaining that the temperature of the northern hemisphere has been decreasing since A.D. 1248, whilst that of the southern has been improving. Upon these supposed changes is based to a great extent his adhesion to the famous theory of Adhémar. We must therefore examine the alleged facts a little more closely. The Swiss glaciers, so far from having been gradually extending since the twelfth century, are now well known to be shrinking, and have in many striking cases a much less extent than when Switzerland was first recognised as the "play-ground of Europe." As regards Ireland, where fuschias and myrtles—and, according to the author, even palm trees—flourish in the open air, it is very certain that the disappearance of pines, if a historical fact, cannot be due to a fall of temperature. The position and circumstances of Iceland are so peculiar that the decay of the birch forests there is not a sufficient proof of the deterioration of the climate. The demands of the inhabitants for fuel, the action of the tremendous volcanic outbursts which desolated the island, and the ravages of goats, may amply account for the decline of these forests, and when trees are once swept away from any country, by what cause soever, their re-introduction is a difficult task. The Iceland nightingales are probably due to the imagination of the poets to whom Professor Pilar appeals. We can scarcely suppose a bird of such limited powers of flight able to cross the wide and stormy seas which intervene between Iceland and either Norway or Scotland. In his hypothesis of a deluge every 10,500 years, proceeding alternately from the north and from the south, M. Adhémar not only assumes the existence of polar ice-caps larger than observation justifies, but takes for granted that they rest upon the bottom of the ocean. Nor is the supposition of one unbroken ice-cap covering the whole of Northern Europe scarcely tenable in the face of the observations of Mr. Mattieu Williams.* Whilst fully admitting also that submergence and emergence have alternated over many, if not all, parts of the surface of our globe, can we recognise the probability of a universal deluge, previous to which the preponderance of water was in the northern hemisphere, having occurred less than 10,000 years ago. All that we know of the distribution of organic life, as well marine as terrestrial, seems to point to the conclusion that for a very prolonged period land has predominated in the northern hemisphere and water in the southern.

A Treatise on the Cycloid and all Forms of Cycloidal Curves. By Richard A. Proctor. London: Longmans and Co. 1878.

This is a work or two hundred and fifty pages, devoted to the investigation of a family of curves which have lost most of the interest which formerly attached to them, and which, so far as they are worth knowing, are studied with far greater ease and perspicuity by the analytical method. The mass of details of which the book is composed is hardly relieved by any real generality in treatment in spite of the parallel sequence of the propositions relating to the different curves. An effort is made at the close of the work to exhibit a problem in whose solution a knowledge of the properties of the cycloid is capable of application; but even the approximate solution of Kepler's problem there given fails to account for the fact that "students at the Universities" are expected to find the work of "use."

The plates, which are due to Mr. Perigal and Mr. Boord, are excellent. The beauties and variety of these curves, together with the ease of their mechanical description, will always make them favourites with the amateur turner; but the interest of the mathematician is soon satisfied with noting the effect of changes in the mode of their generation on their form, without enquiring

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* Quarterly Journal of Science, vol. vii., p. 537.

into such recondite properties as those referring to the centre of gravity. More plates and less letter-press would have made this book more useful to the designer and certainly not less useful to the student.

Photographed Spectra. Printed by the Autotype Process. By G. RAND CAPRON, F.R.A.S. London: E. and F. N. Spon. 1877.

THIS work contains no less than one hundred and thirty-six photographs of metallic and gaseous spectra, accompanied by a full description both of the conditions of each experiment and of the spectra themselves. The spectra range from about the line *b* to beyond H_2 in the violet, and the method of reproduction enables much more of the spectrum in the direction of the violet to be delineated than is ordinarily found in representations of spectra. Each spectrum in its photographed form was found to present a readily recognised individuality, and the author was thus induced to imagine that the permanent reproduction of them might furnish a handy book of reference to spectroscopists.

The spectroscope employed was by Browning, and was specially constructed for auroral observations. The prism was an inch aperture compound, easily dividing the D lines, and the collimator carried a 1¼-inch lens of 6 inches focus. The camera was in immediate connection with the spectroscope, and the images were taken upon collodion wet plates, 4¼ by 3¼ inches. The metallic spectra were produced by the spark, and also by the electric arc. The former was obtained from a large Ruhmkorff coil giving a 2-inch spark, and the latter by forty pint Grove cells. The width of the slit averaged 0·003 inch. Several attempts were made to obtain photographs of the red end of the spectrum. A full explanation of the various photographs is given. All the elements, metallic and non-metallic, with a few exceptions which could not be photographed, are given.

The work will be found of great service to the spectroscopist, and of interest even to those who do not work practically.

Miscellaneous Papers connected with Physical Science. By HUMPHRY LLOYD, D.D., D.C.L., Provost of Trinity College, Dublin. London: Longmans. 1877.

THIS work contains a reprint of twenty-three papers, reprinted from the "Transactions of the Royal Irish Academy," the "Reports of the British Association for the Advancement of Science," and elsewhere. Four of these relate to Optics, and include the important paper on Conical Refraction; twelve relate to Magnetism; two to Meteorology; and five are Addresses delivered on various occasions, on various subjects. The papers extend over many years: we have a Report on the Progress and Present State of Physical Optics, which was read before the British Association in 1834; and the Introductory Address delivered before the same Association twenty-three years later. The Report on Physical Optics extends over 128 pages, and is of much value for reference. The magnetic observations go back as far as 1834, and they were made in conjunction with Capt. Sabine and Capt. James Ross. The observations are accompanied by some useful charts. The chapters on the Meteorology of Ireland will always be valuable, and will no doubt form the basis of a complete system of meteorological observation, which may hereafter be established in the island. The Introductory Lecture, on the rise and progress of mechanical philosophy, contains an interesting historical account of the earlier researches in this branch of natural philosophy. Altogether the work will be found of great interest to the man of Science.

Physiography: an Introduction to the Study of Nature. By J. H. Huxley, F.R.S. London: Macmillan. 1877. 8vo., 377 pp.

This work embodies the subject-matter of a course of educational lectures delivered nine years ago at the London Institution. It is a Physical Geography containing the newest and most accurate details, and divested of the formal treatment which usually characterises works on that subject. The opening chapter treats of "The Thames," and this river was chosen because the lectures were addressed to Londoners by a Londoner; but the author points out that any intelligent reader or teacher will have no difficulty in transferring the ideas herein expressed from the Thames to the river and river-basin of his own district. The general formation of rivers is traced, and the nature of watershed and water-parting, land-drainage and springs, is discussed. A map shows the principal river-basins and water-partings of Great Britain.

The second chapter is on "Springs," the formation of which is illustrated by some excellent woodcuts. It is shown that all such sources of water owe their origin to the rain which sinks into the earth, and descends until it finds an impermeable stratum. This naturally leads (Chap. III.) to an account of rain and dew. The formation of clouds is discussed, and illustrated by some good chromo-lithographs; a coloured hyetographical map shows the annual rainfall in different parts of

England, and the formation of dew is described. The account of liquid water is followed by that of solid water (Chap. IV.), and the principal physical properties of ice and snow receive full attention. A chapter on evaporation shows the connection between it and the rainfall.

The sixth chapter is devoted to the atmosphere, the chemical nature of which is fully discussed. This is followed by an account of its physical properties, and of the perturbations which arise owing to alterations in the pressure. "The Times" weather-chart is reproduced and described; also the barometer-charts of the "Standard" and "Daily Telegraph." The chemical and physical history of the air is followed by a chapter in which the composition of pure water is demonstrated. The constituents of mineral waters and of sea-water are discussed, and analyses given.

The ninth chapter is of a more geological character: it treats of the work of rain and rivers, the power of running water, and processes of denudation. The Thames is said, on the authority of Prof. Geikie, to discharge annually 1,865,903 cubic feet of sediment, to which must, of course, be added the mineral matter carried away in solution, which brings up the amount to 14,000,000 cubic feet of solid matter. "Imagine a huge die-shaped mass of stone 100 feet in length, 100 feet in width, and 100 feet in height: this would contain one million cubic feet. No fewer, then, than fourteen of these gigantic cubes appear to be quietly stolen from the surface of the Thames basin by means of running water, and transported to the sea, in the course of a single year. But the Thames basin covers a very large area, and it will be found on calculation that, admitting the abstraction of this vast mass, the entire surface of the basin would be reduced in level by only 1-800th part of an inch every year. At the present rate of wear and tear, therefore, denudation can have lowered the surface of the Thames basin by hardly more than an inch since the Norman Conquest; and nearly a million years must elapse before the whole basin of the Thames will be worn down to the sea-level." Prof. Geikie has calculated that, at the present rate of denudation, it would require 5½ million years to reduce the British Islands to a level with the surface of the sea.

The tenth chapter is devoted to "Ice and its Work:"—the mechanical expanding work of water in the act of freezing, the motions of glaciers, and so on. A capital engraving of the glacier of Zermatt is given on page 156. It is shown that the passage of a glacier across a country produces peculiarities which are not caused by any other process of denudation, and it is thus possible to infer with certainty that ice has been at work in a district in which ice is now never seen. The flat-domed hillocks known as *roches montonnées* are thus produced, and they may be detected in Ireland, Cumberland, Scotland, and North Wales, together with *bloce perchés*, and vestiges of old moraines.

The chapter on the "Sea and its Work" is illustrated by a capital section of the Atlantic between Sandy Hook and Bermuda, in which the soundings are given at short intervals, the temperatures, and the position and dimensions of the Gulf Stream. Here, also (p. 182), we find a chart of the estuary of the Thames between the Nore and Margate. The account of volcanoes and earthquakes includes geysers, and is illustrated by a representation of the Beehim Geyser of Yellowstone Park, Colorado, which throws jets of hot water to a height of 200 feet. It is said that there are no less than 10,000 hot springs, geysers, and hot lakes within the area of Yellowstone Park.

The slow movements of the land (Chapter XIII.) are shown to be altogether more important than the sudden paroxysmal changes produced by earthquakes. The best-known example of such changes within the memory of man is perhaps to be found at Puzzuoli, where the land near the Temple of Serapis appears to be sinking at the rate of one inch in every four years. This occurs in the midst of a volcanic district; but in Scandinavia, a country peculiarly free from earthquakes, we have positive proof of similar slow changes. The northern part of the peninsula is rising, while the southern part appears to be undergoing depression. Evidences of similar changes are not wanting in some parts of Great Britain.

A long chapter (XIV.) is devoted to "Living Matter, and the Effects of its Activity on the Distribution of Terrestrial Solids, Fluids, and Gases. Deposits formed by the Remains of Plants." Herein it is shown that the gaseous and liquid constituents of the earth are being constantly reduced to the solid form, either temporarily or permanently, by living matter. Prof. Huxley objects to the term organic matter, "because all forms of living matter cannot be strictly said to be organised." Vegetable life is traced up from its most primitive beginnings, by a gradual process of evolution, until it attains full perfection, and then passes to decay. The same is done with animal life, commencing with the egg; and the analogy between the growth of the plant and of the animal is shown. Then the principal fossil vegetable forms are described, and the formation of the coal-measures. The whole of this chapter is treated in an original and highly suggestive style. A natural continuation leads, in Chapter XV., to the formation of land by animal agencies, such as "Coral Land," and (Chapter XVI.) "Foraminiferal Land." The latter embraces an account of some of the deep-sea soundings of the *Challenger*, the bed of the Atlantic, and globigerina-ooze.

Since at the commencement of the volume we find an account of the surface configuration of the Valley of the Thames—that river being chosen for the reason stated above—it is appropriate that now the geological structure of the Thames basin should be discussed (Chapter XVII.): this is accompanied by a geological map and section of the district. The history of one single river-basin leads to a more general account of the distribution of land and water over the surface of the globe (Chapter XVIII.)

The penultimate chapter treats of the figure of the earth and the construction of maps, and the final chapter of the sun.

It will be seen from the above that no formal sequence of subject-matter has been attempted by the author. A book which commences with "The Thames" and ends with "The Sun," and discusses midway geology, palæontology, and physical geography, interlaced with chemistry and physics, cannot be regarded as an example of great continuity of structure and design. Only a most comprehensive and original mind, such as that of Prof. Huxley, could make such a work at once intensely interesting and highly instructive. The book will be welcomed alike by the general reader, the teacher, and the student. For the upper forms in our modern school divisions it is admirably suited. Its great value to every class of reader depends not only on the matter which it contains, but upon its suggestiveness and upon its excellent literary style.

An Elementary Treatise on Physics, Experimental and Applied. Translated from Ganot's "Eléments de Physique," by E. Atkinson, Ph.D., F.C.S. Eighth Edition, Revised and Enlarged. London: Longmans. 1877.

This is a new edition of a very well-known work which has been reviewed at length in our columns. It is not often that a scientific book of this magnitude reaches an eighth edition in this country, and the fact speaks for itself without any further comment. The present edition contains sixty pages of new matter and sixty-two illustrations. All the most recent discoveries in Science have been introduced, and the work is a complete exponent of the present state of general elementary physics.

A Star Atlas for Students and Observers. Showing 6000 Stars and 1500 Double Stars, Nebulæ, &c., in Twelve Maps on the Equidistant Projection; with Index Maps on the Stereographic Projection. By Richard A. Proctor. Fourth Edition. London: Longmans. 1877.

Three editions of this work having been sold rather rapidly, the author determined to bring out a cheaper edition, and this is the result. The maps are constructed in reference to the year 1880, and they will continue to be more and more correct than in the year when first published, until 1890; moreover, it will not be as far from correctness, on account of precession, as existing Star Atlases, until the year 1927! The stars which are mapped include all down to the sixth magnitude inclusive, and are taken from the B. A. Catalogue and all other most available sources. The maps are drawn by Mr. Proctor himself. We predict a long-continued career of usefulness to this laborious work.

Transits of Venus. A Popular Account of Past and Coming Transits. Third Edition.

The Universe of Stars, Presenting Researches into, and New Views respecting, the Constitution of the Heavens. Second Edition.

Other Worlds than Ours. The Plurality of Worlds Studied under the Light of Recent Scientific Researches. Fourth Edition.

By RICHARD A. PROCTOR. London: Longmans. 1878.

THESE works are second, third, and fourth editions of well-known treatises on popular astronomy, by that most indefatigable and industrious writer Mr. R. A. Proctor. In each instance the former editions have been revised, and in some respects modified. New discoveries have also been introduced, and these have increased as much in astronomy as in any other science. Of recent matters we have Dr. H. Draper's discovery of oxygen in the sun by means of its bright lines; also the discovery of the two small moons of Mars, which revolve around him in respectively 30¼ and 7⅔ hours, at distances of 14,000 and 5600 miles from his centre. A new star, which appeared in Cygnus, has faded into a planetary nebula, and it emits the monochromatic light of certain gaseous nebulæ whose spectrum is a single nitrogen line. The calculations regarding the distance of the sun from the earth, founded on the recent observations of the Transit of Venus, appear to be drawing to a close, and they give an approximate distance of 93,000,000 miles. Mr. Proctor unites much learning with an easy popular style: he thoroughly understands his subjects, and has carried out much observational research; moreover, he is an excellent draughtsman, and frequently gives us his own designs. Thus his books are at the same time popular and scientifically accurate, and they must always commend themselves to every class of reader.

Pocket Altitude Tables. Short and Simple Rules for Accurately Determining Altitudes Barometrically. By G. J. SYMONS. London: Stanford.

WE once heard of an amateur who fell to work to measure the height of one of the loftiest peaks of the Carpathian range. He made his observations with great precision, calculated out the results, and found—a height, or rather depth, considerably below the sea-level! Mr. Symons's little book will, we think, effectually prevent such an undesirable consummation. By following the author's instructions fairly accurate results may be obtained with a minimum of time and trouble, and without the necessity of employing two observers and two sets of instruments, which on an exploring expedition is not always practicable.

How to Work with the Spectroscope. A Manual of Practical Manipulation with Spectroscopes of all Kinds, &c. By JOHN BROWNING, F.R.A.S., &c. London: John Browning. 1878.

THIS little work, from the pen of our leading spectroscope maker, will be found useful to the student who is commencing the study of spectroscopy. It gives practical directions for working with spectroscopes of all descriptions, from the simple pocket form to the large compound prism apparatus. The various kinds of instruments at present in use are also figured and described, so that the beginner may gain an insight into the cost of an outfit before taking up this branch of scientific study. The paragraphs giving directions for mapping spectra might have been extended with advantage. The whole is illustrated with thirty woodcuts and diagrams of apparatus and spectra.

As an introduction to the more expensive works of Schellen, Roscoe, and others, Mr. Browning's little book may be safely recommended, especially if used in conjunction with Mr. Proctor's shilling manual of the Spectroscope.

Mineralogy. By J. H. COLLINS, F.G.S. Vol. I., The General Principles of Mineralogy. London and Glasgow: W. Collins, Sons, and Co. 1878.

THIS book, which forms part of Collins's Advanced Science Series, has been specially written to enable those practical working miners, quarrymen, field geologists, and students of the

Science Classes of the Department of Science and Art, who wish to submit to the Government examinations, to pass with credit. The present volume treats more especially of the physical characters of minerals generally, eighteen of the twenty-seven chapters being devoted to a description of Miller's system of crystallography, all mathematical formulæ being avoided. The other physical properties of minerals—such as cleavage, structure, magnetism, optical properties, &c.—are also well described; the analysis of minerals by the blowpipe is, however, dismissed in ten pages. There are but few elementary manuals of chemistry in which any space is given to blowpipe analysis. It seems a pity, therefore, that the opportunity was lost of giving a more detailed account of a subject of such vital importance to the working mineralogist. The use of the spectroscope in the qualitative analysis of minerals is hurried through a dozen lines. We fear that the system of publishing books in series of volumes, each containing an exact number of sheets, is a mischievous one, for in too many instances it compels authors to restrict certain portions of their work within inconvenient limits. The work is illustrated by nearly six hundred diagrams. The second volume, which will shortly be published, will treat of descriptive mineralogy.

Building Construction, showing the Employment of Timber, Lead, and Iron Work, &c. By R. Scott Burn. Vol. I., Text; Vol. II., Plates. London and Glasgow: W. Collins, Sons, and Co. 1878.

The fame which Mr. R. Scott Burn has gained for his numerous works on the constructive arts renders it almost unnecessary to say very much about the latest production of his pen and pencil. We need only open it at any page to see that it is written by a thoroughly practical man, possessed of the power of explaining his meaning in clear homely language. The text is illustrated by nearly five hundred woodcuts, besides which there is a quarto volume of plates, containing thirty pages of plans, working drawings, and diagrams, illustrative of the text. Amongst them are also a number of plates containing examples explanatory of the excellent instructions for mechanical and free-hand drawing.

We cordially recommend these volumes to all who are connected with the constructive arts. The work forms part of Collins's Advanced Science Series.

Botany: Outlines of Morphology and Physiology. By W. R. McNab, M.D., F.L.S., Professor of Botany, Royal College of Science, Ireland. London: Longmans and Co. 1878.

This little work forms part of the series of London Science Class-Books, edited by Prof. G. Carey Foster and Mr. P. Magnus, and is stated to be intended for pupils who have already acquired a rudimentary knowledge of botany. The growth of cells of different kinds, their aggregation to form tissues, and the general external conformation of plants are treated of in the first three chapters. The next five are devoted to the nutrition of plants, their general conditions of life and growth, their movements and modes of reproduction. The book ends with a chapter on classification. So far the general arrangement, which is excellent; but we regret to see a constant tendency on the part of the author to use terms derived from the Greek and Latin, most of them neologisms, when English ones would serve just as well. What possible end can it serve to call the stem of a plant a *caulome*, the leaf a *phyllome*, and its hairs *trichomes*, and that, too, without giving the pupil the least clue to the derivation of these and a hundred other similar words? We must really warn teachers against using a book for any class of school-boys whose chief end seems to be to cram their minds with such facts as these—that "imperfect self-fertilising flowers are kleistogamous;" that "flowers fertilised by the agency of birds are called ornithophilous;" or that "in sympodial dichotomies the sympodium consists either of the fork-branches of the same side, right or left, the bostrychoid (helicoid) dichotomy, as seen in the leaf of *Adiantum pedatum;* or the branches are alternately right and left, the cicinnal (scorpioid) dichotomy of many of the *Selaginellas*." We thought that pedantic cramming was being speedily eradicated from our educational system, but the noxious plant seems to be still flourishing.

A Treatise on Photography. By W. de Wiveslie Abney, F.R.S., &c. Text-Books of Science. London: Longmans and Co. 1878.

Captain Abney has already earned the gratitude of both professional and amateur photographers by his little work "Instruction in Photography," published some twelve months since. His first book, however, was almost entirely practical in its nature, the theory of photography being only treated of incidentally. The present work fully supplements the first, and enters into full details with regard to the theory of the subject. It must not be thought that the author confines himself entirely to theory, for there are sufficient technical instructions for all the best known wet and dry processes to enable anyone possessing the book to become an accomplished landscape photographer. The history of photography—from the days when Scheele first saw *luna cornua* blackening in the sun, to the latest improvements in photo-engraving processes—is succinctly given in the first chapter; after which the author gives a series of experiments on light, which form an admirable introduction to his third chapter, which treats of the theory of sensitive compounds. This chapter is undoubtedly a tough one, but it will fully repay any amateur or professional for the time and trouble which he may take in mastering it. The next two chapters—on "The Action of Light on Various Compounds," and on "The Support and Substratum"—may be looked upon as a continuation of the third. We are now introduced to the more practical part of the subject, the daguerreotype being the first process treated of. Some practical photographers may smile at this exhumation, but they are perhaps not aware that it was employed by the French astronomical expeditions which were sent out to observe the transit of Venus in 1874. Where minute measurements of the photographic image have to be made, as in astronomical or spectroscopic work, the advantage of having a rigid immovable surface to work on, instead of an unequally contractile film, will be readily understood; in fact, we are surprised that so many photo-spectroscopists should still adhere to the wet or dry collodion process. This being the case, the chapter in question might have been longer. The various collodion processes are next described, the practical instructions being very copious. As might have been expected, they take up a large portion of the book. The gelatino-bromide process and the old-fashioned catotype, which Capt. Abney informs us is still practised in remote districts in India, next claim our attention. We next come to the various printing and toning processes, both on glass and paper; printing with ferric, uranic, and chromic salts, the various autotype processes being as fully described as the secrets of trade will allow. The Woodburytype process is also well described. The next chapters are devoted to the photo-lithographic processes of Col. de C. Scott and Sir Henry James,—much used by the Ordnance Department for the reproduction of enlarged or reduced copies of charts and maps. It may interest our readers to know that large numbers of maps reduced by a modification of this process, introduced by Captain Ali Bey, the chief of the Photographic Department of the Seraskierate, were largely and profitably used by the officers of the Ottoman army engaged in the late disastrous campaign. Photo-engraving and relief processes are next described, but of course practical details are wanting, so many of the operations connected with them being kept secret. A print from a photo-relief plate, by Warnerke, produced by a secret process, is given, and is one of the best specimens of the kind that we have seen. The two chapters on Lenses and Apparatus will be of great assistance to the amateur purchaser, who frequently wastes an immense deal of time and money through being ignorant of his real requirements. The chapter "On the Picture" is a most valuable one from an artistic point of view. Not only are minute instructions given for obtaining artistic pictures instead of mere photographic transcripts, but the photographer is shown what is right and wrong by means of nearly a dozen beautifully executed woodcuts of scenery of all descriptions, taken from photographs by Manners Gordon, Woodbury, H. P. Robinson, and other masters of the craft. A chapter on actinometers and actinometry follows, and the remainder of the book is devoted to photo-spectroscopy, celestial photography, micro-photography, and the miscellaneous applications of photography.

We most cordially recommend Capt. Abney's book to our readers. We regret to say that the Index to this important work is so meagre as to be almost useless.

A Critical Examination of the Flints from Brixham Cavern. By A. Whitley. London: Hardwicke and Bogue.

This pamphlet, which is a reprint from the "Transactions of the Victoria Institute," has for its object to disprove that the flints found in the Brixham Cavern were knives, or otherwise show traces of human labour. The author brings to his task no small ingenuity, and a too obvious desire to make the most of every circumstance upon which the faintest doubt may be founded. This is the weak side of his argument; we cannot trust a critic who speaks not as a judge, but as a most passionate advocate. That he has established a charge of carelessness against some of the discoverers and custodians of the relic in question is undeniable.

Zoology of the Vertebrate Animals. By Alex. Macalister, M.D. London: Longmans and Co.

This treatise is one of a series entitled the "London Science Class Books," edited by Messrs. G. C. Foster and P. Magnus. According to their Preface, these gentlemen consider that there is "still a want of books adapted for school purposes upon several important branches of Science." Their object being to supply this want, they have sought to obtain the co-operation of men who "combine special knowledge of the subjects on which they write with practical experience in teaching."

Within the very brief space allotted—some 120 small pages—the author has given a fair, but of course very sketchy, account of his subject. We are somewhat surprised to find it stated that the common viper is "easily recognised by its dark green colour." Among the hundreds we have captured, from England to Dalmatia, we never met with a specimen that could be called green; the males were various shades of grey, and the females copper-colour. We fear the author's estimate that over 10,000 deaths take place annually from snake-bites is far too low. As an oversight, we may also notice that the swift is ranked among the birds which leave us "about the first week in October."

Report of the Board of Health of the City of Nashville for the Year ending July 4th, 1877. Nashville: Tavel, Eastman, and Howell.

In addition to a Report on Sanitary Reform in Nashville, this volume comprises papers on the topography, the geology, the water-supply, and the climate of the city. Nashville is a place whose sanitary history, as here given, is exceedingly instructive. It was once a summer health resort. By neglect of proper precaution and regulation during its increase from a village to a city—as has happened in too many places on both sides of the Atlantic—its death-rate increased; but now intelligent attention has been awakened, the tide is turning. Mention is made of one J. M. Bass, who, "as Receiver, replaced the entire City Government," and who "made the fatal mistake of economising at the expense of the public health." It would be well, indeed, if so-called economists, who are always counting the cost of any projected measure, would be fair enough to add the reverse of the medal, and count the cost of letting things alone; it would often prove much the heavier. We find here a high compliment paid to our country of which we are scarcely worthy. The Report speaks of "Great Britain, the acknowledged leader in all sanitary reform." When we read these words we remembered the multitudes of inhabited cellars in our English towns—an abomination entirely absent in Paris.

The following facts, if no mistake prevails, must rank among the unsolved mysteries of medical science:—"that cholera is never seen in Iceland, Siberia, Greenland, or Australia; that phthisis is never seen in Iceland, and only rarely in Norway, Madras, or the elevated plains of Mexico."

We are glad that the sanitary value of trees is fully recognised in this Report, and that planting is urged as a public duty.

A List of Writings relating to the Method of Least Squares, with Historical and Critical Notes. By Mansfield Merriman, Ph.D.

A bibliographical treatise of great value to mathematicians, physicists, and engineers.

Sketch of the Origin and Progress of the United States Geological and Geographical Survey of the Territories. By F. V. Hayden, U.S. Geologist in Charge. Washington: Darby and Duvall. 1877.

This pamphlet contains an interesting and useful sketch of the rise and extension of that continuous scientific exploring condition for which the Federal Government is deservedly reaping golden opinions. The Survey it appears took its origin in 1867, when Nebraska was admitted as a State of the Union, the unexpended balance of the sum appropriated for its legislative expenses as a territory being set apart by Congress for a Geological Survey of the new State. The utility of such an undertaking, executed in a spirit of thoroughness, having become evident to the Legislature (whose wisdom in this respect might be advantageously imitated in other countries), further sums of money were voted, and the Survey was gradually extended, not alone as to the country to be explored, but also as to the subjects included. Not alone the geology—using the term in its widest sense—but the ethnology, the natural history, the meteorology, and the agricultural resources of the territories are carefully studied and reported on. Men of proved eminence are appointed to the various departments, and they are allowed both time and appliances to carry out their task in a manner creditable to themselves and their country, and highly useful to men of Science throughout the civilised world.

Among the most valuable results of the Survey may be mentioned the observations glanced at in the following passage:—"Accumulated experience has shown that the various evolutional tides of organic life have not advanced at the same *rate* in all parts of the world. Thus while we find that a certain grade of vertebrates, invertebrates, and plants are associated together in the strata of, and collectively characterise a certain geological period in, Europe, in America we find that the same grade of plant life was evidently reached much earlier, and the same grade of vertebrate life was continued much later."

One of the features of this Survey has been the employment of a skilful photographer, Mr. W. H. Jackson, whose success in conveying the characteristics of rocks and mountains we have on a former occasion had the pleasure of pointing out. The value, or rather the necessity, of photography in scientific exploration is scarcely even yet fully understood by the public at large. We are justly told that "twenty years ago hardly more than caricatures existed, as a general rule, of the leading features of overland exploration. Mountains were represented with angles of 60 degrees inclination, covered with great glaciers, and modelled upon the type of any other than the Rocky Mountains; the angular lines of a sandstone mesa represented with all the peculiarities of volcanic upheaval or of massive granite."

The total number of negatives in the possession of the Survey amounts to nearly four thousand.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iii., No. 4; Vol. iv., No. 1. Washington: Government Printing-Office.

This number is devoted to zoology, recent and fossil. The first paper, by Mr. S. H. Scudder, contains a description of certain fossil insects discovered in the Tertiary beds of White River, on the borders of Colorado and Utah. With the possible exception of four specimens from the Miocene of North Greenland, they are the first insects found in the Tertiary strata of America. There are in the collection no Lepidoptera, nor has the author yet met with any fossil species of this order of American origin, and no Orthoptera. More than one-half the species are Diptera, thus showing that these creatures—whose absence would be one of the essential requisites of a "golden age"—must have existed from a very early date in the Western Hemisphere. There are three Hymenoptera, four Hemiptera, one Neuropterous insect, and nine Coleoptera. The relative proportion of insects of different orders found in various deposits is of course a document of the highest value as regards their origin. If we consider that the Diptera, from their fragility, are about the least likely insects to be preserved, it will, we think, be allowed that in the Tertiary times they must have been relatively more abundant than in the present day. Among the Coleoptera the absence of Buprestidæ is remarkable.

There is a description of two fossil Carabs found in the Interglacial deposits of Scarboro' Heights, Toronto; a catalogue of the insects collected by Dr. Uhler during the explorations of 1875, with notices of their localities, times of appearance, &c. There are also papers on *Cambarus Couesi*, a new crawfish from Dakota; on a carnivorous Dinosaurian from the Dakota beds of Colorado; a notice on the ichthyological fauna of the Green River Shales; and an account of the genus *Erisichthe*.

The first number in Vol. iv. gives the records of much good work. The first paper, "Notes on the Ornithology of the Lower Rio Grande of Texas," by G. B. Sennett, may give the general public an idea of the difficulties which naturalists have to encounter on their explorations. "While we were constantly on the alert for huge rattlesnakes, tarantulas, and centipedes, still more troublesome enemies were with us continually in the shape of wood-ticks and red bugs, to say nothing of the fleas. The wood-ticks we could pick off or dig out, but the abominable red bugs, too small to be seen, work themselves through the clothes and into the skin, making one almost wild with incessant itching. We only obtained partial relief by giving ourselves from head to foot, before going to bed, a bath of ammonia, and a daily bath of kerosene oil before going into the brush."

The author secured five hundred birds, one altogether new to Science; about a thousand eggs, many of them new or rare; a few mammals, all of which proved interesting; and quite a collection of insects.

Drs. Coues and Yarrow's "Notes on the Herpetology of Dakota and Montana" include a most valuable account of the rattlesnake, of which five species, belonging to the two closely-allied genera *Crotalus* and *Caudisoma*, infest the region in question. The authors do not believe in the alleged power of fascination possessed by this reptile, and consider that the use of the rattle is a problem still unsolved. "One thoroughly established fact concerning the rattle is that its practical operation is injurious to its possessor, by provoking attack from those who can cope with it successfully." There is no known specific for the poison of the rattlesnake, but its bite, unlike that of the cobra, is by no means invariably fatal.

Geological Survey of Victoria. No. 4. Report of Progress by the Secretary for Mines, with Reports on the Geology, Mineralogy, and Physical Structure of Various Parts of the Colony. By Messrs. MURRAY, KRAUSE, TAYLOR, HOWITT, NICHOLAS, McCOY, and NEWBERRY. Melbourne: Ferres. London: Trübner and Co.

IT is satisfactory to know that the exploration of Australia—geological, palæontological, and mineralogical—is not standing still. Attention is mainly directed, not unnaturally, to the occurrence of gold-fields and auriferous quartz veins. Still the existence of other useful minerals and the occurrence of animal and vegetable remains are not overlooked. Prof. McCoy makes the following interesting remark:—"As in South America the Geological period, just before the creation of man, had the gigantic *Megatherium* to pre-figure the little sloths of the present day amongst the characteristic edentate group of mammals of the fauna of the same country, so the little native "bears" (*Phascolarctos*) of Victoria in our day were preceded, at the same late Tertiary period, by equally huge animals of their same general marsupial type, as characteristic of the existing Australian fauna as the edentate is of that of South America. The *Diprotodon* of Australia, curiously enough, like the *Megatherium* of South American deposits, was obviously a feeder on the twigs and foliage of trees, like their diminutive representatives of modern times."

Among the fossils figured is *Asterolepis ornata*, which occurs, though rarely, in the Middle Devonian Limestones of the Buchan River, and is almost identical with specimens found in the Russian Old Red Sandstone. Upon the occurrence of this species Prof. McCoy remarks:—"The great ganoid armour-plated fishes of the genus *Asterolepis* are amongst the most abundant and striking characteristics of the Devonian rocks of Russia, and it is certainly a most extraordinary circumstance to find them here in Australia in limestones of the same age, and accompanied by the corals and shells of the Plymouth and Eifel limestones of similar age, with which they are not known to occur in England or Germany, and which do not occur with them in the Russian beds."

As additional instances of the permanence of local type in Australia, both among plants and animals, may be cited the occurrence, in the Pliocene Tertiary argillaceous strata at Daylesford, of *Eucalyptus Pluti*, the foliage of which is in size and shape almost identical with that of the living *Eucalyptus globulus*. Relics of gigantic extinct kangaroos, *Macropus Titan* and *M. Atlas*, are found in the newer Pliocene Tertiary, 25 feet below the surface, at a place called Duck Ponds.

Several of the iron ores of Victoria have been analysed, and are considered valuable. Certain samples, from Lake Tyers and from Bonang, have been pronounced by an experienced English iron-master worth carriage from Melbourne into Shropshire. The reporter, however, thinks it doubtful whether even the best of the Victorian iron ores would, in their crude state, pay for export to England, owing to the expense of land-carriage to the sea-board.

The volume is illustrated with maps, diagrams, and plates, showing microscopical rock-sections, sections of strata, and views illustrative of the general character of the scenery.

Records of the Geological Survey of India. Vol. x., Parts 1, 2, 3. 1877.

The Annual Report of the Survey for the year 1876 contains some judicious strictures upon men of science who wish to force upon facts an interpretation favourable to their pet theories. The foreign relations of the Gondwana system are becoming, it seems, a burning question. "Palæontologists come from cabinets in Europe with the fixed idea that the 'laws' they have seen to work so well as between Bohemia and Bavaria, or from Durham to Dorsetshire, will apply equally between India and and Australia, or Europe, and the eager aim of their labours seems to be to tally off our Indian rock-groups as the representatives or equivalents of certain fossiliferous series in Europe, or elsewhere. From the beginning this palæontological fallacy has been the chief obstruction to our knowledge. When first the Gondwana fossils were taken, pure geology being in the ascendant, the fact that certain plant forms of the lower Gondwana rocks were somehow associated with beds having a carboniferous marine fauna in Australia was made the basis of a special pleading to show that the Damudas, their flora and their coal, were palæozoic. The materials have now come into the hands of a pure palæontologist. He has shown, I believe, conclusively that the Gondwana flora is wholly mesozoic, nailing its several phases to certain representative zones in Europe. But it so happens that on the confines of India, east and west, the upper Gondwana groups are associated with beds having a marine fauna, according to which these said groups have already been attached by palæontological experts to other standard groups in Europe. It is true that the study of this fauna was only partial, but the experts were very accomplished in their line, and their judgment was quite unprejudiced, so that it must carry great weight. Here then, again, is an opening for the procrustean method of research; and there are symptoms that it is to be duly applied, this time, to make the fauna conform to the flora. The expression 'palæontological contradiction,' which has been applied to this fact of association, exhibits the predicament in a very naive manner. The contradiction is certainly there, but only as a rebuke for those who can look upon it in that light. No theologian could be more impious in reducing the mysteries of existence to the compass of his narrow thoughts than are often scientific specialists in imposing crude conceptions upon the proceedings of Nature. Yet these ought to know better that truth is discovered, not invented."

Mr. W. T. Blanford reports on the Great Indian Desert, between Sind and Rajpootana, part of which at no remote date appears to have been an arm of the sea.

Dr. Feistmantel describes the occurrence of the cretaceous genus *Omphalia* near Namcho Lake, in Tibet, and gives also a note on *Estheria* as occurring in the Gondwana formation.

Mr. Lydekker describes certain new and other vertebrates from Indian tertiary and secondary rocks, such as *Bos acutifrons* and *planifrons*, *Bubalus platyceros*, *Stegodon ganesa* (which elephantoid, like the allied *S. insignis*, lived down to the Nerbudda period, and must have been contemporary with the early human inhabitants of India), *Sivalhippus Theobaldi* (an aberrant horse from the Siwaliks), *Ictitherium Sivalense*, *Hyænarctos Sivalensis*, and certain Saurians, including the new genus *Titanosaurus*.

Part 2 comprises an account of the rocks of the Lower Godavari; notes on the Atgarh Sandstones near Cuttack; a paper on the Fossil Floras of India; a notice of some new or rare Mammals from the Siwaliks; a note on the Arvali Series in North-eastern Rajputana; and an account of some borings for coal, which describes the cost of the operation and the time consumed in perforating different rocks, but omits to state whether coal-beds were reached, and, if so, what were their thickness and quality.

There is subjoined a paper on the geology of India, which the late Dr. Waagen, formerly palæontologist to the Survey, had contributed to the "Zeitschrift der Deutschen Geolog. Gesellschaft. The author pronounces his opinion that the peninsula of India belonged to a great continent, which probably included China, the (Malayan) Archipelago, and Australia, perhaps even a part of Oceania.

Part 3 comprises "Notes on the Tertiary Zone and Underlying Rocks in the North-west Punjab," by A. B. Wynne, illustrated with a geological map of the district. In this region occur the celebrated salt deposits of the western frontier and certain petroleum springs of no great importance.

Dr. Feistmantel gives an account of a tree-fern stem from the cretaceous rocks near Trichinopoly.

Mr. W. Theobald treats on the occurrence of erratics, and concludes that at one time "glaciers were ploughing their way down the great Himalayan rivers and valleys, to within 2000 feet or so of the sea," whilst the Potwar was one great lake, with an exit probably near Kalabagh as now, and into which lake glaciers descended, freighted with the *débris* of the hills of Hazara and Kashmir.

Mr. F. R. Mallet gives an account of recent coal explorations in the Darjiling district. The coal found is extremely friable, and could not be worked without great expense, especially as the Assam coal could easily be brought to the foot of the Darjiling hills.

The same author likewise describes the limestones found at Barakar, and some blowing machines used by the smiths of Upper Assam.

The analyses of Raniganj coal, by Mr. A. Tween, show that the Indian samples are less contaminated with sulphur than the average of English specimens, but contain more ash and more oxygen. The Dumakunda and Benodakotta coals, however, have a good heating power, since, if the combustion of pure carbon yield 8080° C., they afford respectively 7040° and 7023°. The best gas-coal is that of Sanktoria, which furnishes about 9000 cubic feet per ton.

Memoirs of the Geological Survey of India. Vol. xii., Part 2. MALLET: Coal-fields of the Nágá Hills bordering the Lakhimpur. Calcutta: Geological Survey Office.

THE deposits of coal on the Brahmaputra and its affluents in Upper Assam are exceedingly valuable. The coal is of good quality, and nothing seems requisite save the improvement of the navigation of the river to bring it into use in Bengal, and supersede the necessity of importation from England. Petroleum is also found in quantity, but until the means of communication are improved it cannot compete in the market with that brought from Rangoon and from America. Some of the coal-beds are of very considerable thickness. Thus in a total section of 47 feet 10 inches the coal-beds alone amount to 37 feet, the thickest bed being 25 feet. It is an unfortunate circumstance, however, that the measures have a high general dip towards the hills, and the seams must rapidly sink to a depth below which the coal could not be profitably worked. Still the author estimates that as a minimum 9,000,000 tons of marketable coal would be easily procurable from the seams already known to exist. Between Tipam and Bornarchali there is a further minimum of say 10 millions of tons in a marketable state, whilst the Nazira field will add a further supply of 7 millions. In the unsettled state of the political horizon the existence of a supply of coal for naval purposes independent of importations from Britain is of the highest importance. Pyritous shales seem abundant, and the manufacture of copperas and alum could probably be conducted with success. The iron ore in the sub-Himalayan beds is inexhaustible, but the quality is very poor, and the rarity of limestone in the Nágá hills must always be a difficulty in the way of smelting operations on a large scale.

The Origin of the World, according to Revelation and Science. By J. W. Dawson, LL.D., F.R.S., &c. London: Hodder and Stoughton.

This work may be characterised as one of the many attempts to reconcile the doctrines of revealed religion with the results of modern science, or, as the author himself puts it, to "aid thoughtful men perplexed with the apparent antagonisms of science and religion, and to indicate how they may best harmonise our great and growing knowledge of Nature with our old and cherished beliefs as to the origin and destiny of man."

To those who hold fast the luminous principle laid down by Galileo, that on what may be called scientific subjects the Hebrew and Greek Scriptures speak merely the ideas of the times when they were written, and, though a moral, have no claim to be regarded as a physical revelation, such books as that before us present a curious and not altogether pleasing phenomenon. We may well ask whether Dr. Dawson and those who agree with him are not to a great extent responsible for the perplexity which they seek to remove? We deprecate as much as any one the gratuitous, and in our opinion unscientific, attacks upon religion to which such writers as Buchner have given way. But it is still very probable that much of this spirit springs from disgust at the attempts made to extract from Hebrew roots, under high-pressure philology, doctrines conformable with or anticipatory of the teachings of modern science.

To go through Dr. Dawson's treatise paragraph by paragraph would be a tedious and not very remunerative task, and could only be fairly undertaken by a writer well versed in theology, mythology, and philology.

It is very significant that we find the cosmogony of Laplace—the so-called nebular hypothesis—accepted, and proclaimed in harmony with the teachings of the Scriptures! But the other day, so to speak, this doctrine was denounced by Sir D. Brewster and others as an atheistical speculation, and an attempt to construct the universe without God. Just as Brewster opposed the nebular hypothesis, Dr. Dawson struggles against the doctrine of Evolution, each in turn insinuating that to throw a novel light on God's *modus operandi* is to deny His existence. The moral of this coincidence is too plain to be missed.

The author is, perhaps, in this work less magniloquent than in some of his former productions, but towards those who do not see what he sees—or rather who see what he cannot, or will not—he is scantily courteous. "Shallowness," "mere folly and presumption," are the attributes which he finds in all dissentients.

Pollen. By M. P. Edgeworth, F.L.S., F.A.S. Illustrated with 438 Figures. London: Hardwicke and Bogue.

This interesting monograph was originally laid before the Linnæan Society; circumstances, however, determined the author to withdraw the paper, and it now appears in the present altered form. The introduction explains the varied forms which pollen takes in different plants, and gives the bibliography of the subject. This is followed by a list of plants the pollen of which has been examined, with references to authors who have made the observations; the species examined by the author are marked with an asterisk, and form a considerable part of the catalogue. This valuable portion of the work occupies sixty pages. The most important part of the book, however, consists of the twenty-four plates, containing four hundred and thirty-eight figures, and in this, with the explanation, is contained the result of the author's own microscopical observations. Many of the pollens are figured as they appear when viewed under different conditions, *e.g.*, dry or opaque objects or in various fluid media, as water, oil, vinegar, &c. The author has not put his own drawings on the stone, but they appear to have been rendered with tolerable fidelity by the lithographer. It is to be regretted that so few observers will take the small amount of trouble needed to acquire the various lithographic processes, and prevent the risk of misinterpretation so liable to occur when drawings are copied by those who, however skilled they may be in the technicalities of their art, can hardly be expected to understand the nature of unfamiliar objects.

The drawings are all to scale (1-500th of life size), a matter too often neglected, and when this is the case detracting from the value of otherwise good figures; for rigid adherence to this principle the author is highly to be commended, as the comparative dimensions of any pollen can be readily estimated.

Mr. Edgeworth's monograph will prove of great value to the student of this department of minute vegetable structure.

CORRESPONDENCE.

RESIDUAL PHENOMENA.

To the Editor of the Quarterly Journal of Science.

Sir,—In the article on "Residual Phenomena" published in the last issue of your Journal it is stated that the actual atomic heats of carbon, boron, and silicon are much lower than those required by the law of Dulong and Petit, and it is assumed that there is some residual phenomenon modifying the result in these three instances. It is also stated that "though the anomalies to which we have drawn attention have been recognised for more than a quarter of a century, the explanation is still wanting."

I would draw the author's attention to the researches of F. Weber respecting the specific heats of the three above-mentioned elements (Ann. Chim. Phys. [5], viii.; abst. Journ. Chem. Soc., June, 1876). Weber found that the specific heats of these bodies increase *rapidly* with the temperature, and at high temperatures tend to become constant. If we multiply the specific heats at these high temperatures into the atomic weights, we obtain the atomic heats. Carbon—(1) diamond, 5·5; (2) graphite, 5·6; silicon, 5·75; boron, 5·5. Thus it appears that the true atomic heats of these three elements do not vary from the mean number 6·4 to a greater extent than those of other elements of low combining weight, such as aluminium.

Here, then, we have the residual phenomenon alluded to; and it is interesting to note that its explanation affords a further confirmation of Dulong and Petit's law.—I am, &c.,

C. H. Bothamley.

The Yorkshire College,
February 20, 1878.

SCIENTIFIC NOTES.

Continuing his researches on the repulsive force resulting from radiation transmitted through rarefied gases, Mr. Crookes constructed a torsion balance in which the beam for carrying the experimental disks was a straw suspended by a very fine glass fibre. His experiments extended over one hundred different substances, and he measured the effect produced by them, not only under the influence of simple radiation from a luminous source, but also by removing the invisible heat rays by the interposition of a water screen. In this manner he found that each substance exercises a more or less distinct action on the absorption of rays. Thus most white powders powerfully absorb the invisible heat rays, while they are almost without action on the luminous rays. On the contrary, black powders powerfully absorb the luminous rays, and only slightly absorb the obscure heat rays, whatever their intensity may be. The different metals present great differences in their action. Iron, for instance, chiefly absorbs the invisible heat rays, while gold is principally acted on by the luminous rays. The substances experimented with may be divided into two classes: 1st. Those whose action is increased by the interposition of water screens with regard to the effect produced on the standard disk; 2nd. Those in which the contrary is the case. Amongst the former may be mentioned copper tungstate, saffranin, precipitated selenium, and copper oxalate; these are more affected by light than by invisible heat. Amonst class 2 may be mentioned chromic oxide, persulphocyanogen, zinc oxide, barium sulphate, and calcium carbonate; these are acted upon more by the ultra-red rays than by the luminous rays. Remarkable effects were also obtained by combining the substances in these two categories on the disk of the radiometer. Mr. Crookes has previously shown that when the exhaustion of a radiometer is carried beyond a certain limit its sensibility gradually diminishes until it becomes absolutely null. He has now come to the conclusion —1st. That there is a gradual increase in the sensitiveness of the radiometer until the pressure has attained 50 millionths of an atmosphere (0·038 millim.); 2nd. That beyond this limit to 30 millionths of an atmosphere (0·013 millim.) it remains stationary; 3rd. Further still, it sinks rapidly until at 1 millionth (0·00076 millim.); 4th. That at 0·2 millionth of an atmosphere (0·00015 millim.) the radiometer refuses to turn even when five candles are put near it. He has also examined the effects of molecular pressure produced directly by heat upon a radiometer with the following results:—(1.) When the apparatus is full of air at the normal pressure of 760 m.m., and a platinum ring was rendered incandescent by an electric current, the direction of the rotation of the vanes and disk was *positive*,—that is to say, that which would be produced by a current of air coming from the platinum ring; this effect must be attributed to the ascending current of hot air. (2.) At a pressure of 80 millims. the disk did not turn. The vanes turned slowly in the positive direction. (3.) At 19 millims no movement was produced either by the disk or by the vanes. (4.) At 14 millims. the disk remained stationary, but the vanes began to turn gently in the negative direction,—that is to say, in a way inverse to their first direction. (5.) At 1 millim. the disk turned in a continuous manner in the positive direction, whilst the vanes turned rather fast in a negative direction. (6.) At 0·224 millim. the speed of the disk or the vanes was the same, and their rotative movement was the same. Below that pressure the speed of the rotation of the vanes diminished gradually, whilst the speed of the disk increased, and at a pressure of 0·107 millim. the disk turned rapidly in the positive direction, whilst the vanes were motionless. (7.) With a more perfect exhaustion still, at 0·098 millim. a sudden change was seen. The vanes which had been stationary then began to turn in the positive direction at a speed of 100 revolutions per minute, whilst the disks turned as before, positively, but with less speed. (8.) When the exhaustion was carried beyond 0·098 millim. the speed of the two disks and of the vanes increased till it exceeded 600 revolutions per minute, and it did not seem to diminish with the highest rarefaction, which was at 0·0001 millim. According to the most recent determinations the number of molecules contained in a cubic centimetre of air at the ordinary pressure is probably something like—

1,000,000,000,000,000,000,000 (one thousand trillions);

consequently, at an exhaustion of 0·0001 millim., 100,000,000,000,000 are still left. This number is sufficiently large to justify the hypothesis—That when the molecules are set in vibration by a white-hot platinum wire they are still capable of exercising an enormous mechanical effect.

Mr. J. W. Groves, of the South London Microscopical Club, after cleaning glass slides for mounting microscopical objects, by one of the usual processes, fastens them together by their edges, after the manner of the well-known artists' sketching-blocks. This is easily done with a pile of slips, by fixing round their edges a piece of ready-gummed tissue-paper, 10 inches long, and of a width suitable to the number of slides, so that, although they are firmly bound together, their surfaces are left uncovered. The block is left to dry, when each slip may be detached by running the thumb-nail round its edges. The surface next the adjoining slip should be used for the preparation to be mounted on, as it is, of course, quite clean, although the exposed one may have become dirty: the fragments of tissue-paper are removed after the mount is completed.

Mr. H. C. Sorby, F.R.S., has described to the Royal Microscopical Society a new arrangement for distinguishing the direction of the axes of doubly-refracting substances. The usual method is to employ plates of selenite of various thickness: this, however, involves great difficulty in selecting one of nearly the same tint as that of the crystal examined under the polarising microscope; and unless this can be done it is impossible to obtain so decided a result that the direction of the positive and negative axes can be seen at once and no consideration be required. Mr. Sorby employs a wedge-shaped plate of quartz, cut parallel to the principal axis, 1¼ inches long and ½ an inch wide. At its thickest end it is 1-20th of an inch thick, and thins off to the sharpest possible edge: this is fixed on a glass plate so as to leave a space of glass 4-10ths of an inch long by ½ an inch broad beyond this thin end of the quartz. The combined plates are fixed in a brass frame, like that for a micrometer, which slides into the eye-piece. On using polarised light with a crossed analyser over the eye-piece, and arranging the plate so that the part with only glass is in front, we see the object in its normal state, and the rest of the field black, and on pushing forward the quartz wedge we see the field of the microscope crossed with coloured bands, gradually rising from the bluish white of the first order, through all the brighter orders of colours, to the faint reds and greens, and upwards to what cannot be distinguished by the unaided eye from white light. If some crystal, giving any tint, be on the stage of the microscope, we can usually see at once whether the tints are raised or depressed, by the manner in which it alters the colour of the bands; and by pushing the quartz wedge backwards and forwards, there may be no difficulty in finding the exact place where the plate of quartz so exactly neutralises the action of the crystal that it appears black. If this does not occur in any place, and, on the contrary, the tints appear to be raised, the eye-piece and the plate must be rotated through an angle of 90 degrees, and the requisite place can then be easily found. The plate of quartz being so cut that its longer axis is parallel to the principal axis of the crystal, we know that this longer axis is positive, and thus also at once know which is the positive and which the negative axis of the crystal under examination. We can also at once see what is the true order of colour which it gives, since we can readily count it up from the bands due to the quartz alone seen crossing the field of the microscope. We are also by no means limited to visible tints. The crystal may have such a powerful double refraction, or be so thick as to give apparently white light, and yet, by using the thicker end of the quartz wedge, the tints may be reduced down to those easily distinguished. This simple arrangement secures all the advantages of an unmanageably large number of selenite plates, and all necessary observations can be made with ease and expedition.

A valuable paper on the "Application of the Micro-Spectroscope to the Study of Evergreens" was read before the Royal Microscopical Society (November 7th, 1877), by Thomas Palmer, B.Sc. As it would be impossible to give an abstract that would be of any use to the student, and, moreover, the reproduction of Mr. Palmer's spectrum charts would be required, reference is made to the original paper.

Owing to the death of Dr. Henry Lawson, F.R.M.S., and Assistant-Physician and Lecturer on Physiology at St. Mary's Hospital, and formerly editor of the "Monthly Microscopical Journal," that publication will be discontinued. The "Transactions of the Royal Microscopical Society" will for the future be issued by themselves, after the manner of the larger societies.

A mode of examining water microscopically has been contrived by W. L. Scott, Public Analyst to the County of Glamorgan and Borough of Hanley. The chief point in the process consists in the manner of filtering the water, by which the organisms contained in a large quantity of material are retained on a very small portion of the filter-paper. The centre of the filter is rendered impervious by means of a fatty composition, and the texture of the paper rendered more obstructive to the passage of minute organisms by being dipped in a very thin structureless collodion. The process is described in detail in the "Monthly Microscopical Journal" (vol. xviii., p. 239).

In the obituary notices of the Royal Microscopical Society we remark the death of Mr. J. L. Denman. Although not distinguished for any published work in the department of minute structure, he has done that which will endear his memory to every analyst. An honest tradesman, disgusted with the adulteration practised—and unfortunately approved of by the public—in the article in which he dealt, he devoted much time and expended considerable capital in obtaining pure wine. Failing to obtain it from the usual sources, he sought it in other countries, and, in spite of popular prejudice, imported the unadulterated wines of Greece and Hungary. Just before his death he had the satisfaction of introducing pure wine from Spain—a country which, like Portugal, had always prepared its exports to the vitiated taste of the British consumer. His definition of wine was simply "fermented grape-juice," in old times coupled with bread as a necessary of life.

The Telephone continues to engage the attention of physicists. A very complete and concise description of the construction and action of the instruments of Reiss, Yeates, Gray, Edison, and Bell is contained in the report of a Lecture by Prof. Barrett, of the Royal College of Science, Dublin, and published by Mr. Yeates, of King Street, Covent Garden.

In a paper "On some Physical Points connected with the Telephone," read before the Physical Society, Mr. W. H. Preece has pointed out that this instrument may be employed both as a source of a new kind of current and as the detector of currents which are incapable of influencing the galvanometer. It shows that the form and duration of Faraday's magneto-electric currents are dependent on the rate and duration of motion of the lines of force producing them, and that the currents produced by the alteration of a magnetic field vary in strength with the rate of alteration of that field; and further, that the infinitely small and possibly only molecular movement of the iron plate is sufficient to occasion the requisite motion of the lines of force. He also pointed out that the telephone explodes the notion that iron takes time to be magnetised and de-magnetised. The instrument forms an excellent detector in a Wheatstone bridge for testing short lengths of wire, and condensers can be adjusted by its means with great accuracy. M. Niaudet has shown, by employing a doubly wound coil, that it can be used to detect currents from doubtful sources of electricity, and it is excellent as a means of testing leaky insulators. Among the facts already proved by the telephone may be mentioned the existence of currents due to induction in wires contiguous to wires carrying currents, even when these are near each other for only a short distance. Mr. Preece finds that if the telephone wire be enclosed in a conducting sheath which is in connection with the earth, all effects of electric induction are avoided; and, further, if the sheath be of iron, magnetic induction also is avoided, and the telephone acts perfectly. It appears that conversation can be carried on through 100 miles of submarine cable, or 200 miles of a single wire, without difficulty, with the instrument as now constructed; but the leakage occurring on pole-lines is fatal to its use in wet weather for distances beyond 5 miles.

M. Demoget, of Nantes, has experimented with two Bell telephones in an open field. He held one of these to his ear, while his son at a distance kept repeating the same syllable with the same intensity of voice into the second instrument. He compared in this way the sound heard from the telephone with that heard from the speaker, and calculated their relative intensities from the relative distances of their sources. From the results M. Demoget concludes that the telephone as a machine leaves much to be desired, since it can only transmit 1-1800th of the original work.

Dr. R. M. Ferguson has contributed some valuable "Notes on the Telephone" to the Royal Scottish Society of Arts. He takes exception to the vibration theory of Bell, viz., that it is the vibrations of the disk to and from the pole of the magnet, in excursions proportionate to the intensity, pitch, and quality of the vocal sounds, that electrically affect the instrument. He submits that at the receiving station it can be proved well nigh to demonstration that it is a molecular tremor or vibration, and not a vibration mechanically produced, that emits the sound; and that this molecular vibration becomes louder the easier the sounding body vibrates. Seeing that there is the most perfect correspondence between the sending and receiving instruments, there is, he says, every reason to believe that the sending instrument exhibits the converse action to the receiving instrument. and that there again sound acts on iron so as to produce molecular changes, the electric power of which is much enhanced by the vibration of the sounding body.

The Molecular Theory of the Telephone has also been the subject of a paper at the King's College Engineering Society, by Mr. C. W. Cunnington, who considers that the vibration of the iron disk of the sending instrument under the influence of sound is amply sufficient to induce currents of electricity strong enough to cause the plate of the receiving instrument to vibrate, and thus to reproduce sound: he also referred some of the sound produced in the receiving instrument to the effect of the undulatory currents on the magnet itself, thus explaining results obtained without the use of a vibrating disk. In comparing the vibrating disk of the telephone with the membrana tympani of the ear, Mr. Cunnington pointed out the fact that the predominance of the fundamental note of a flat plate would drown a large number of the overtones of the voice, thus causing many of the observed peculiarities of the sound transmitted in ordinary telephones; the membrana tympani being funnel-shaped, whilst peculiarly susceptible to the influence of sound, had no fundamental note of its own, and therefore transmitted all of the sound vibrations impinging upon it (within certain limits) without giving undue preponderance to any particular note.

The Count du Moncel finds that the vibratory plate of the recipient cannot merely be replaced by a very thick and massive armature without affecting the transmission of speech, but these vibratory plates may be formed of non-magnetic substances. The vibratory plate may even be totally suppressed without hindering the telephonic transmission provided the polar extremity of the magnet is placed close to the ear. Hence the vibrations which reproduce speech in the receiving telephone are principally produced by the metallic nucleus infolded by the coil. The vibratory plate serves merely to react for the production of induced currents, when set in vibration by the voice, and by its reaction upon the polar extremity of the magnetic rod to reinforce the magnetic effects produced by the latter.

At a meeting of the Physical Society, in February last, Mr. F. J. M. Page exhibited the action of the telephone on a capillary electrometer. He first explained the construction of Lippmann's electrometer, as modified by Marey, and threw the meniscus of the mercury in the capillary tube on the screen by the electric light. The delicacy of the instrument was shown by passing a current of 1-1000th of a Daniell, which caused a distinct movement of the mercury. Resistances of 500 ohms and 1-50th ohm gave approximately the same deflection, so that, in practice, the instrument may be considered to be independent of resistance, in addition to which it possesses the great advantage of portability, and its indications are almost instantaneous, To illustrate the use of the electrometer for physiological investigations, a frog's heart was connected by non-polarisable electrodes with the instrument: each beat of the heart caused a considerable movement of the mercury column. A telephone was now connected. On pressing in the iron plate the mercury moved, and on reversing the wires the movement was seen to be in the opposite direction. On singing to the telephone each note produced a movement; but the fundamental note of the plate, as well as its octaves and fifths, had the greatest effect. On speaking, the mercury oscillated continually: some letters of the alphabet had scarcely any effect, and the *w* was especially curious, producing a double movement. Reversing the wires did not alter the character or *direction* of these movements. The same effect was observed when the telephone was in the primary and the electrometer in the secondary coil of a Du Bois Reymond's induction coil. In conclusion, Mr. Page showed the contractions produced in a frog's leg. On inserting under the sciatic nerve two platinum wires coupled with the binding-screws of a telephone, and talking to this instrument, violent contractions ensued. In the course of the discussion which followed, Prof. Graham Bell said he had made very many attempts to ascertain the strength of the current produced by the human voice in vain; he considered, however, that the present method will in all probability give some most valuable results.

On Thursday evening, January 10th, M. Raoul Pictet succeeded in liquefying hydrogen gas in the laboratories of the Society for the Construction of Physical Instruments, at Plainpalais. The experiment, which was performed in the presence of several people, succeeded perfectly. The process consists in decomposing formiate of potash by caustic potash, a reaction which, as proved by M. Berthelot, gives hydrogen absolutely pure. The pressure commenced to rise at half-past eight; gradually and without any stoppage it attained, at seven minutes past nine, 650 atmospheres, at which point it remained steady for a few instants. At this moment the tap was opened, and a steel-blue jet escaped from the orifice, producing a hissing sound like a bar of red-hot iron plunged into water. The jet suddenly became intermittent, and there could be observed a hail of solid particles projected violently to the ground, on which their fall produced a crackling noise. The tap was closed, and the pressure, which was then at 370 atmospheres, gradually descended to 320, where it remained for some minutes. It then rose to 325. At this moment the tap, on being opened a second time, only allowed a jet to escape intermittently, rendering it evident that a crystallisation had taken place in the interior of the tube. The proof of this can be established by the escape of hydrogen in the liquid state when the temperature begins to rise on stopping the pumps. Thus has been experimentally demonstrated the liquefaction, and especially the solidification, of this gas, which from its properties has always been considered probably to belong to the class of metals.

M. Cailletet sent a paper on the "Liquefaction of Nitrogen, Hydrogen, and Atmospheric Air" to the Academie des Sciences, which was read at the meeting on the 31st of December, 1877. M. Cailletet showed that pure and dry nitrogen compressed to about 200 atmospheres, at a temperature of +13°, then allowed to expand suddenly, condenses in the most perfect manner: it first produces an appearance like that of a pulverised liquid in small drops of appreciable volume; this liquid then gradually disappears from the sides to the centre of the tube, at last forming a sort of vertical column following the axis of the tube. The duration of these phenomena is about 3 seconds. On experimenting with hydrogen—in the presence of MM. Berthelot, H. Sainte-Claire Deville, and Mascart—M. Cailletet succeeded in observing indications of its liquefaction under conditions of proof which left no doubt in the minds of the scientific men who witnessed the experiment. It was repeated a great number of times. Operating with pure hydrogen compressed to about 280 atmospheres, and then suddenly allowed to expand, they saw form an extremely attenuated and subtle mist suspended in the gas and disappearing suddenly. Having liquefied nitrogen and oxygen, the liquefaction of air is thereby demonstrated. It appeared, however, of interest to make this the subject of an actual experiment, and, as might be expected, it succeeded perfectly. I need not say that the air was previously dried and freed from carbonic acid. The accuracy of the views expressed by the founder of modern chemistry, Lavoisier, is thus confirmed as to the possibility of causing air to assume the liquid state, and of producing matter gifted with new and unknown properties.

At a recent meeting of the Académie des Sciences M. Lecoq de Boisbaudran exhibited a bar, a sheet, and several crystals of the new metal, *Gallium*, which is harder than iron, yet melts at under the heat of the finger, its freezing-point being at about 30°. It is proposed to use it for a thermometer going up to red-heat. 5000 kilogrms. of ore had to be worked down to get 60 grms. of metal. It will now be possible to investigate its physical properties. It adheres to glass, and is very brittle; the colour is nearly that of steel; and the crystals are octahedra. M. Lecoq de Boisbaudran has since determined the atomic weight of gallium; it is 69·9.

A photo-lithographic plat of the primary triangulation carried on during the summer of 1877, by Mr. A. D. Wilson, Chief Topographer, has just been published by the U.S. Geological Survey, under the charge of Dr. F. V. Hayden. The area covered by these triangles extends from Fort Steele, in Wyoming, *Zy.*, westward to Ogden, in Utah, *Zy.*, a distance of about 260 miles, and north as far as the Grand Teton, near the Yellowstone National Park, including Fremont's Peak, of the Wind River Range of the Rocky Mountains. The area embraces about 28,000 square miles, and within it twenty-six primary stations were occupied and their positions accurately computed. Besides these occupied stations a large number of mountain peaks were located, which in the future will be occupied as points for the extension of the topographical work of the Survey. A base line was carefully measured near Rawlins' Springs, on the line of the Union Pacific R.R., and from this initial base the work was extended north and west to the valley of Bear River, in Idaho, Zy. Here a check base was measured, and the system expanded to the neighbouring mountain peaks, to connect with the triangulation, as brought forward from the first-mentioned base. Along the line of the Union Pacific R.R. the work was connected at six points with the Triangulation System of Clarence King's 40th Parallel Survey. In addition to the importance of this sheet as the base of the season's topographical work, it presents a most striking feature in the number of remarkably long sights which were taken from some of the most lofty mountains in the area explored. Many of these sights were over 100 miles in length, while some reach a distance of 135 miles. From Wind River Peak all the prominent points in the Big Horn Mountain were sighted; also the loftier peaks of the Uinta Mountains: the former are located 165 miles to the north-east, while the Uinta Mountains are situated about the same distance to the south-west. As these ranges were not in the scope of the season's work they are not given on the chart.

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JULY, 1878.

I. THE SENSES OF THE LOWER ANIMALS.

EXPANDING the aspiration of the Scottish poet, most of us at times crave to see, not merely ourselves, but the whole world as it appears to the senses of others. We cannot help questioning what portion of our perceptions is purely objective, depending solely on the nature of the things seen, heard, or tasted, and what portion, if any, is due to our own subjectivity, and is consequently liable to vary in different individuals ? For instance, we see the flower of a field poppy. It impresses our retina with a certain sensation which we have been taught to call "redness." We can find in the solar spectrum, or in Chevreul's chromatic circles, regions which make upon us the like impression. Our neighbour, unless affected with colour-blindness, gives the same name to the effect produced by the flower upon his sight.* But who guarantees, after all, that the impressions made respectively upon him and upon us are identical ? Neither of us can make use of the optic nerves of the other. Much more strongely does this doubt crop up as regards the senses of smell or taste. One of the most familiar facts in daily life is to find one man praising an odour or a flavour which is to another person simply loathsome. Where lies the difference ? In the very impressions made upon the nerves of smell and taste, or in the judgment formed by some inward faculty on reviewing such impressions ? But if we have already grounds for question as to whether our sensuous perceptions are identical with those of our fellow-men, the difficulty

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* It has been pointed out that persons suffering from colour-blindness have often great facility in accommodating their language to that of their colour-discerning neighbours, and thus, unless systematically tested, often escape detection. Many persons who are red colour-blind do not merely fail to distinguish red from its complementary green, but red light, as light seems to make no impression on their retina—a fact of some importance.

becomes vastly greater when we cross the " Rubicon " of a certain learned, but unbiological, professor, and strive to form some notion of the world as it appears to the lower animals. Have they the same senses as ourselves? At the first glance the educated public will be disposed to assume that all living beings must as a matter of course be able to see, hear, smell, taste, and feel just as we do, and equally of course must possess no inlet of knowledge to which we are strangers. Closer examination, however, will throw doubts on both members of this proposition. Many animals are blind, not incidentally from disease or accident, but normally, the eyes being sometimes covered over with opaque membranes, sometimes merely rudimentary, and sometimes even totally wanting, the very optic nerve itself being obliterated. Such arrests of development occur in almost every department of the animal kingdom, even among the mammalia. Two species of mole are not figuratively but absolutely blind, *Talpa cæca*, found in Southern Europe, and the golden mole of the Cape of Good Hope (*Chrysochloris inaurata*). A rodent found in the eastern parts of Europe (*Spalax typhlus*) has eyes so small, so deeply buried in its head, and furnished with so narrow an aperture, that it may be regarded as blind, functionally if not structurally. In the caverns of Kentucky two sightless rats and two bats equally deficient are said to occur, though whether they are totally blind is somewhat doubtful. No blind bird has hitherto been discovered, but amongst amphibians the renowned *Proteus anguinus*, found in subterranean pools in the caverns of Carinthia, is a signal instance in point. Two blind fishes have also been detected, one of which, *Amblyopus cæcus*, is peculiar to the Mammoth Cave of Kentucky.

The other senses proper to man seem to be shared at least by all the Vertebrata. They possess organs which are the homologues of our own, and which in most cases evidently discharge the same function. One exception on the large scale must not be overlooked. If we consider that smell bears upon substances either themselves aëriform or suspended in air, whilst taste relates to bodies either liquid or capable of solution in liquids, we can scarcely see how smell as differentiated from taste can exist in animals which are always immersed in water, and which respire through gills. With the exceptions thus pointed out, that of blindness in certain species and of scentlessness* in fishes, the

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* We want a word here. The one we use on compulsion might mean not only the inability to perceive odours, but the non-emission of any smell.

vertebrates may be considered to see, hear, smell, taste, and feel. Along the so-called lateral line in fishes there appear to be arranged organs of sense, whose nature is not perfectly understood, but which are possibly capable of being acted upon by different kinds of undulations. Still, it would be an act of most unscientific rashness to infer that they must see the world around them as we see it. Let us take the most highly-specialised of our senses, the one most precise and definite in its indications—our light-sense. We all know how widely it varies among individual men. Almost telescopic in the Red Indian on his prairies, the Arab on the deserts, or the sailor on the ocean,* accustomed perhaps, through many successive generations to scan the horizon for indications of game, for an enemy, or of an approaching tempest, it becomes nearly microscopic in certain men of science and certain artizans constantly in the habit of scrutinising the most minute objects. It is said that Wollaston could write upon glass with a diamond characters which none of his friends could distinguish with the naked eye, but which, when examined with a good lens, were found to be beautifully distinct and regular. Nor does human eyesight differ merely in its range. Some men can discern objects clearly under a degree of illumination much more feeble than is requisite for others. Some again can detect the most minute variation in the shades of any colour, whilst to others, as Dalton and an eminent chemist and physicist now living, scarlet, green, and gray are all one.† If, then, such diversity prevails in the sense of sight

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* This last instance has latterly been called in question.

† According to the *Times of India*, a phase of colour-blindness, or, at any rate, of incapacity to distinguish colours, is very common among the lower caste natives. "Our natives cannot distinguish between blue and green. They apply the word *lal* to a variety of objects we should describe as yellow and brown, and apply the generic epithet 'tambada,' corresponding to Homer's *Chalkos*, to all the bright red tints. Like Homer, they speak of the blue sea as black (*Kala pani*). They apply the word *nila*, dark blue, to a grey horse, and their notion of the colour of the sky, or *Asmani rung*, is a light grey. The subject can be readily tested by anyone by telling his 'boy' or some less civilised native to choose a blue, red, or green book from a pile on the table. I have just tried a puttawallah with different coloured books. Between green and blue he cannot properly distinguish; *tambada* he applies to vermillion, and the rainbow he protests is simply red or green. This is just what Mr. Gladstone says about the colour sense in Homer's Greeks."

If, then, the colour-sense in man is of recent origin, and has made notable progress since the days of Homer, the question further arises as to the date of its development among the lower animals? If the perception of colour was feeble among primeval animals their colouration must have been dull and sombre, on the hypothesis of sexual connection. Are the least splendidly coloured groups survivals of earlier days? On this subject the "stone-book" within the range of our own species, if we compare the visual powers of man with those of other animals, much wider differences must be expected. Nor is this mere matter of conjecture. Direct observation proves that numerous species are able to seek their food or their prey, meet with their mates, distinguish friends from enemies, and find the way to and from their dens or nests in what to man appears complete darkness, and where he can scarcely move without running into danger. With what skill does the owl, hovering over the harvest-field, discern the grey mouse upon a soil from which it differs little in colour even by day? Other birds, on the contrary, such as the Gallinaceæ, if disturbed in the night, seem perfectly stupefied, and are utterly unable to find a way of escape from danger.

As regards the range of vision, from what a height and to what a distance do eagles, hawks, and other diurnal birds of prey espy a suitable victim! On the other hand, if the sight of small animals is to be of much use to them, it must enable them to distinguish objects which to our unaided vision are simply imperceptible. Concerning discrimination between different colours, the evidence that it is possessed by the lower animals though indirect is conclusive. Were it otherwise the doctrine of "sexual selection," instead of being as it is, a hypothesis, not indeed demonstrated, but still conceivable, would be a simple absurdity. It is still, however, doubtful why sexual selection, if it has been a *vera causa* in the production of colour, should have led to brilliant hues in one group of birds or insects, whilst it has permitted the existence of sombre shades in another class closely allied and similar in habits. Have we here to do with a difference in the perceptive organ similar to colour-blindness, or with a difference in some inward faculty? The well-known case recorded by John Hunter where a female zebra rejected the advances of a male ass till he was painted so as to resemble a zebra is in itself quite conclusive. According to Sir S. Baker, the African elephant and rhinoceros show an especial enmity to white and grey horses, and attack them with remarkable fury. Here also belongs the antipathy of the bull and the turkey-cock to red-coloured objects. The recognition of

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throws no light, since fossils, as a rule, betray not the slightest indication of the hues they may have worn when living. We know that a preponderant proportion of the fossil insects found at Schambelen belong to the gorgeous family of the *Buprestidæ*, but even that family includes so many plainly coloured groups that no certain conclusion can hence be drawn. A parallel question of course arises as regards the fructification of flowers by the agency of insects in the primeval world.

colour by small birds generally is indisputable. Everyone must have observed with varied feelings the discrimination with which they select the "sunny side" of a pear, a plum, or a peach. It is also an established fact that they will attack the red currant in preference to the white variety, though the latter is much the sweeter of the two. Many observers during the last few years have pointed out how the yellow crocus is torn to pieces by sparrows and other birds, while the white and other varieties are unmolested. We are reminded of a curious fact which we do not know whether to ascribe to imperfect perception or to animal stupidity. A bullfinch which we kept in our chambers long ago was seized every evening with the desire to go to roost upon a line painted upon the wall about twenty inches from the ground. Time after time the poor fellow flew up only to be disappointed before he would consent to perch upon something tangible, and the next day he resumed his vain attempts.

The phenomena of mimetism or of protective resemblances, of which so many signal instances have been pointed out by Messrs. Wallace, Bates, and Belt, necessarily involve a tolerably nice discrimination of colour. Brilliantly-coloured serpents are often especially dangerous, and are hence mimicked protectively by innocent species. But unless such colouration were distinguished by mammals and birds, they would neither avoid the former nor be deterred from attacking the latter.

Caterpillars displaying gay and striking hues are generally offensive in smell and taste, or even poisonous, and are hence avoided by birds. But if the latter were incapable of recognising colours, such a conspicuous dress would not deter them from seizing the unsavoury or unwholesome morsel.

But the sight of some at least of the lower animals may very possibly differ from our own in a manner not indicated above. The human retina is sensitive only to a portion of the sun's rays. Thus, if we allow a pencil of sunlight to fall into a darkened room through a slit in the shutter and to pass through a prism, we have the well-known spectrum, all the seven primary colours of which are visible to the eye. But above and below this spectrum, where our sight finds nothing but darkness, there are solar rays whose presence may be demonstrated by appropriate chemical and physical means. Now, if the retina of any creature is sensitive to the whole or a portion of these, to us, dark rays it will be able to see where we should find total obscurity.

Again, we see all objects by means of the so-called luminous rays which they reflect. Whether they at any time reflect any of the "dark rays" is to us immaterial, because of such rays our vision is unable to take cognisance. But an animal whose retina were endowed with a wider range of sensibility would perceive the appearance of bodies modified accordingly as they reflected or absorbed such "dark rays," and would hence detect differences between substances which to our sight appear absolutely alike.

There is another point of some moment which here forces itself upon our attention. It is well known that animal organs deprived for successive generations of the opportunity of exercising their function become abortive. Of this truth several of the blind animal species enumerated above have long served as striking instances. Why should the Proteus possess an eye—a light-organ—living as he does where no ray of light ever penetrates? But as it has been pointed out by Sir Wyville Thomson in his account of the results of the *Challenger* expedition, whilst in certain animal groups permanent and total darkness leads to the total uselessness of the eyes; in others, under precisely similar circumstances, the very contrary result ensues, and the visual organs, as in the *Monida*, an animal living at the depth of 700 fathoms below the surface of the ocean, where no gleam of sunlight can reach, are "unusually developed, and apparently of great delicacy." We shall have difficulty in conceiving in a series of successive generations the continued development of an organ which is of no use in the economy of the animal. The greater weight, therefore, attaches to Sir W. Thomson's suggestion that the eyes of deep-sea creatures under such conditions may "become susceptible of the fainter light of phosphorescence." That many of the inhabitants of the ocean, both when living and when passing into decomposition, are phosphorescent to an extent visible even to eyes adapted to the stronger stimulus of direct sunlight is indisputable. The light which at certain times and places follows in the wake of a ship and marks every stroke of a boat's oars has often been described, and is due both to living animals, Salpæ, Medusæ, Pyrosomæ, Nereids, &c., and to decomposing animal matter. So powerful is this light at times that by its aid fishes have been discerned at the depth of several feet below the surface. There can hence be no doubt that an eye sensitive even to the faintest phosphorescence would be of great service to marine animals, enabling them either to discover their prey or to shun an approaching enemy.

But we have not yet done with phosphorescence. The question has often been asked why certain nocturnal animals seem greatly afraid of fires, torches, &c., whilst others, on the very contrary, are attracted by a light, and seek eagerly to plunge into it to their own destruction? One explanation proposed is that the candle or torch seems to the soaring moth an opening, or way of "escape," to which it accordingly rushes. "Escape?" we ask; wherefore, or from what? Why should the nocturnal insect which carefully shuns the daylight feel less at home in the fields or gardens by night than does the bee or the butterfly by day? An anonymous writer in "Hardwick's Science Gossip"* suggests what seems to be the true explanation. Many flowers are, even to our eyes, phosphorescent; to the vision of moths, &c., many more, if not all, possess this property, which serves to entice night-flying insects. Such creatures, attracted by what to them is the usual announcement of honey, make towards the flame, and then on nearer approach become dazzled and bewildered by its superior and unaccustomed intensity and perish in their confusion. Has this any connection with the fascination which a white cloth spread upon the ground or hung upon a tree seems to have for nocturnal insects?

It has been urged in objection that insects of carnivorous habits—amphibians, fishes, and even certain birds—are attracted by a light. Owls and nightjars have been known to flutter against the window of a lighted room in the "small hours." When sickness has been the cause of the unwonted lamp or candle, such visits have been regarded with superstitious dread. Fishes are allured to a light allowed to float along the stream or placed on the bank, a circumstance which has been taken advantage of for spearing salmon, both by poachers in the Scottish rivers and by the Red Indians in the streams of Oregon and British Columbia. A light held near to the side of a fresh-water aquarium by night brought newts, water-scorpions, and water-boatmen to the glass.

But all these facts, so far from refuting the hypothesis that moths are guided to flowers by a phosphorescent light seem to us merely to indicate the necessity for its further extension. What if phosphorescence is a very general attribute of organisms, living or dead, and if those animals which are specially attracted by a light possess eyes sufficiently sensitive for its recognition?

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* Vol. v. p. 138.

However this may be, enough has been advanced to show that the sense of sight even in animals whose eyes are perfectly homologous with our own may supply them with information which to us is inaccessible.

Very similar is the case with the sense of hearing. The human auditory nerves are sensitive to a certain range of sounds, and no further. Vibrations either more or less rapid do not to us break silence. But we have no right to infer that they may not be distinctly heard by other animals. That they actually are perceived has in many cases been shown by direct experiment with the instrument known as "Galton's whistle." Upon this a line is marked indicating the usual upper limit of the sense of sound in the human species, and corresponding to from 41,000 to 42,000 vibrations per second. But when this limit is passed, and when the whistle no longer produces a sound appreciable to man, several animals still indicate by their movements that they hear. Cats, birds, and some insects seem decidedly more affected by high than low tones. In *Sphinx Ligustri* and *Metopsilis Elpenor* the reverse is said to hold good. In short, we may conclude that many living creatures recognise sounds which utterly escape our ears, and thus receive through the medium of the sense of hearing impressions perhaps more varied and numerous than, or at least different from, our own. A wonderful instance of combined delicacy and discrimination in the recognition of sounds is shown by deer in America. It has been repeatedly observed that if one of these animals is browsing in a forest in windy weather, when the trees are groaning and creaking to the blast, and branches are occasionally snapping, yet, if amidst all this uproar a rotten twig happens to crack beneath the foot of an approaching hunter, the animal at once stops feeding, gazes carefully around in every direction, and remains unusually suspicious often for hours. The ears which discriminate so nicely must be uncommonly sensitive.

We come now to scent, which is to civilised man probably the vaguest of all the senses in its indications. It may yield us a certain amount of pleasure and a very palpable amount of annoyance. It may warn us against eating, touching, or approaching certain noisome or putrescent substances; but in this respect its warnings are not trustworthy. It is far from being proved, and indeed is highly doubtful, that the stage of animal or vegetable decomposition most offensive to our nostrils is the one which presents the greatest amount of danger. The most deadly atmosphere of the

Terai, of the Gold-coast, or of the Tierras Calientes smells to us exactly like pure, ordinary, wholesome air. On the other hand, a wholesome and even delicious fruit, like the durrian, may have a most disgustingly repulsive odour. Nor can we by this sense discover what track a missing friend has taken, or pursue the footsteps of an enemy. In the lower Mammalia this is almost reversed. Concerning the delicacy of scent in dogs many interesting facts have been brought forward, and not a little "tall talk" has been perpetrated. But their scent differs from our own not merely in its power of detecting odours which escape us altogether; they are evidently gratified by the smell of carrion, of ordure, of animal excretions and secretions, which to us are utterly offensive. But if the scent of dogs has attracted more prominent attention from the circumstance that it is at the disposal of man in hunting, that of other Mammalia is little, if at all, inferior. Animals of the weasel tribe pursue their prey by scent, and even hunt in packs. Swine can follow their companions by the same clue, and, as it is well known, have been trained to discover truffles under ground. Elephants, deer, bears, and in fact many other wild animals, can only be approached with safety from the lee-side, as they otherwise soon become aware that an enemy is at hand. Rats have a delicate scent, and in setting traps or poison for them it is necessary to avoid touching the bait with the hand; otherwise their suspicions are at once aroused. Almost all animals seem to know—at least as far as the productions of their own locality are concerned—by the odour whether any substance is suitable for food, and this fact has accordingly been enlarged upon as a marvellous case of "instinct." Very many species are also decidedly attracted by odours which have no connection with their food, and which are to them, therefore, a mere luxury. The fondness for valerian, lemon-thyme, chamomile, lavender, and many plants rich in essential oils, is common to the whole feline family. On the other hand, they have their dislikes. We have often observed the domestic cat smell at a fig-tree, and turn away with the air of a disgusted connoisseur. Carbolic acid and the coal-tar products generally seem to be an abomination to everything that comes under the head of "vermin." Oil of rhodium, on the contrary, has a wonderful fascination for rats and mice, and is said to be a prominent ingredient in the mixture which enables the modern housebreaker to lull even the most vigilant watch-dog into a temporary neglect of duty.

The question of the scent of the vulture, so hotly discussed by Waterton, Audubon, and others, can scarcely be regarded as definitely settled.

Serpents have a more powerful sense of smell than is generally imagined. They do not, indeed, pursue their prey by scent,—or, rather, they do not pursue them at all,—but lie in wait, and seize any suitable victim that presents itself. But we have been repeatedly struck with the commotion excited in a reptile vivarium on the introduction of a mouse, shrew, or other small warm-blooded animal; whilst the arrival of a frog, toad, newt, or lizard passed quite unnoticed. Our observations lead us to believe that among vipers the male, who is of wandering habits, seeks the more sedentary female by scent. Some country people hold that serpents are remarkably fond of milk, and will follow by scent a farm-labourer who is carrying a full pail from the fields, or even a woman who is suckling her child. But we never met with any authenticated facts in support of this tradition.

Fishes are, as is well known, attracted by various substances thrown into the water; but it can scarcely be said that, in their case, the two senses of smell and of taste are thoroughly differentiated.

Of the sense of taste in animals we know but little, save that many species are exceedingly nice in the selection of their food, and reject with considerable obstinacy an article which does not meet their requirements. Thus an owl can rarely be induced to eat meat in the slightest degree tainted. This should argue no small delicacy of taste. On the other hand, those creatures which swallow their food whole—such as serpents, lizards, frogs, toads, and many birds—can scarcely be supposed to recognise its taste. We have heard of a python, in a fit of hunger, swallowing a blanket.

Touch, the least localised of our senses, is too often confounded, in popular apprehension, with feeling. But surely the mere recognition of resistance, in certain directions and to various degrees, has no necessary connection with the idea of pleasure or pain possibly excited in the system. This is a fortunate circumstance, as it saves us from discussing the vexed question as to how far and to what extent feeling extends down the animal, or even the vegetable, kingdom. But that touch exists in all animals will scarcely be disputed. Its seat, however, varies greatly in different groups. The hands or fore feet in the Primates and Carnivora, aided in many of the latter by the whiskers; the snout in the lower mammals, the beak apparently in birds, the tongue in many reptiles, the feet (sometimes aided by the antennæ) in articulate animals—all exercise this function. With the exception of a doubtful case in bats, to which we shall have to refer below, we doubt if any of the Vertebrates possess the sense of touch in as high a degree of perfection as does man.

Reviewing the facts already adduced, we are surely warranted in concluding that, even as regards those animals which possess organs of sense clearly homologous with our own, and exercising demonstrably the same function, the sense-perceptions are not necessarily the same as our own, the probability being that—so far at least as sight, smell, and hearing are concerned—we are surpassed by not a few of our humbler fellow-tenants of the globe. They may, or rather they actually do, see where to us there is mere darkness or a void, hear where we find utter silence, smell what to us is inodorous, and distinguish grades and modifications where our senses pronounce there to be no difference. In fact, just as our bodily nakedness, slowness, feebleness, and lack of weapons require to be supplemented by clothing, vehicles, arms, and machinery, so our dull senses demand the aid of the telescope, the microscope, the spectroscope, and other the like aids.

But we have still to consider those beings whose organs of sensation are not homologous with our own, and exercise function as yet imperfectly ascertained. Above all, we have to keep in view the possibility that certain animals may enjoy senses whose nature is to us altogether unknown. To deny, *a priori*, the existence of such senses is as if we were to assert that because we possess no poison-fangs, therefore the bite of the cobra is harmless,—or that because we do not secrete silk, the spider and the caterpillar are unable to spin. It is, in short, that ever-besetting error "Man the measure of all things."

Taking up first the latter question, we will call attention to the wonderful dexterity with which a bat will flit about in a locality full of prominent or pendent objects, without coming in collision with any. It is sight, you say? But an experiment has been tried which will, we fear, be pronounced a case of "violationism." A bat has been deprived of sight, and turned loose in a room where a number of rods, strings, and other objects were suspended from the ceiling; but it avoided these obstacles just as well as if still possessed of its eyes. Feeling or touch? We know that men perfectly blind, when moving slowly along, can judge whether they are approaching an object, such as a wall.

But here is a creature darting rapidly about, and yet able to turn and wind between a number of small obstacles. If this is touch, seated in the wings, in the lobes of the ears, or in the leaf-like appendages which in some species adorn the end of the nose, it is so highly subtilised as almost to deserve the rank of a distinct and independent sense, fulfilling, as it does, functions for which the ordinary feeling or touch of man and other animals is as impotent as it would be for recognising a colour or a sound.

The possibility of other senses than the five with which we are endowed will at once appear if we reflect that of all the so-called "physical forces" light is the only one of which we have a direct perception. We only recognise heat and electricity when they give rise to some phenomenon which appeals to our sight and our feeling. Now, it is plain that senses may exist which take direct cognizance of the electric or thermic state of bodies. Had we such senses they would evidently enable us at a glance to distinguish bodies which, without formal scientific examination, appear to us identical, and to recognise changes of condition which now escape us. An animal possessed of a magnetic sense would, for instance, be able, without any mental effort, to direct its course northwards or southwards, and in crossing an unknown region would enjoy all the advantages which we derive from the use of the compass.

The possibility of new, undiscovered senses is of course greatest in the Invertebrates, and chiefly in the Articulates. We find these creatures executing actions which from our point of view demand reason of a higher grade than we are willing to concede to beings so widely different from ourselves, and—what is much more to the purpose—in which we fail to trace in other points such effects as we might naturally expect to flow from the possession of a highly-developed intelligence. There appears in their economy a mixture of wisdom and folly more incongruous than we at least can imagine may be detected by higher beings in our own. When, further, we examine their structure, we find organs of sensation upon whose functions we are far from being able to pronounce with full certainty.

Concerning the organs of sight in insects there can fortunately be no dispute, but their eyes differ so widely in structure from those of the vertebrate animals as to offer not a few unsolved problems.

Many groups possess two distinct kinds of eyes—the larger or compound, and the smaller, simple, or so-called *ocelli*. The former—found in all mature insects, save certain blind species—occupy a position similar to that of the eyes in the Vertebrates, but, instead of consisting each of a simple lens, they are formed of an aggregation of lenses, varying greatly in number, and in some groups amounting to many thousands. From each of these lenses or facets a crystalline rod radiates downward to the nervous ganglia. The *ocelli*, or small eyes, placed on the top of the head are simple in structure, absent entirely in many species, and in others covered over with hair.

We can scarcely doubt that a structure so widely differing from that which prevails among the higher animals must be accompanied with corresponding differences in function to some of which we have already referred. We know that the sight of many insects must require a wide range. If they are to find their way back to their nests, hives, or other haunts, they must be able to recognise objects at a very considerable distance, whilst in other cases their vision to be useful must be almost microscopic in its character. In accordance with this twofold need it has been observed that in many groups the upper lenses of the facetted eyes are considerably larger than the lower. Hence the former may possibly serve for the recognition of distant objects, and the latter for the examination of such as are near at hand.

Graber, however, maintains that the compound eyes are telescopic in their function, whilst the *ocelli* are adapted solely for the perception of proximate objects, whence their strong convexity. He remarks that these simple eyes occur especially in insects whose locomotive powers are feeble, and whose entire sphere of action is restricted. To this view exception must be taken: the Neuroptera, such as the dragonflies,—perhaps the most locomotive of all insects,—the great majority of the Hymenoptera and Diptera, all of them swift, strong, and agile on the wing, possess these organs; whilst in the Coleoptera, comparatively clumsy and imperfect flyers, they are generally absent.

The *ocelli*, in the Hymenoptera at least, are, according to F. Müller, adapted to a very feeble illumination, their size increasing as the habits of the species are more nocturnal.

But even insects in which no eyes can be traced—such as the blind grubs of *Lucilia Cæsar*, *L. eristalis*, and other flies—have, according to Pouchet, a perception of the intensity and the direction of incident rays of light, diffused apparently over the whole surface of the body.

The eyes of spiders are not facetted like those of insects, and occupy a position more corresponding to the ocelli than to the compound eyes of insects. This position, awkward as it may seem at first glance, is a great convenience to the spider, whose enemies—such as birds and certain wasps—are sure to attack from above, and whose cell or hiding-place is generally underneath the web. From our own observations we do not think that the sight of spiders has a very long range.

Insects are sometimes led astray by their unaided sight. The "humming-bird hawk-moth" has been seen examining artificial flowers on a lady's bonnet, and even the coloured designs on walls,—a fact which confirms the attractive influence of colour upon insects, and corroborates Mr. Darwin's views on the fecundation of flowers.

The light-organs of snails offer most interesting peculiarities. In the genus *Oncidium*, found in the Philippine Islands, and recently studied by Dr. Semper, there are upon the back small specks, which are in reality eyes, essentially similar in structure to those of vertebrate animals, and quite distinct from the well-known tentacular eyes. What may be the difference of function in the two organs has not yet been ascertained.

Passing to the sense of smell, we may be met at the very outset by the inquiry whether insects have any perception of odours at all? This question is of great moment in the general economy of organic Nature. According to several modern investigators, among whom we may especially mention Darwin, the fecundation of flowers, and consequently the propagation of vegetable species, depends in a large number of cases upon the intervention of moths, butterflies, bees, and other insects, which convey the pollen from the male organs of one flower to the female organs of another. These creatures are attracted to blossoms, in some cases of brilliant colour and in others of odour, this being in fact the final cause of such beauty and fragrance. If, therefore, insects are not endowed with the sense of smell, one part, at any rate, of this theory must be given up, and the perfume of flowers must—as far as the welfare of the plant itself is concerned—be pronounced purposeless. It will therefore be useful to review the evidence which proves the existence of an acute and delicate odour-sense in insects, the more as a recent experiment has been by some writers supposed to demonstrate its absence.

We will first glance at some of "Nature's scavengers," such as the sexton-beetle and the dung-beetles. One of the most familiar facts in the economy of these creatures is the ease and certainty with which they discover their quarry. On a calm spring evening nothing is more common than to see a *Geotrupes* come flying along in a straight line, not hawking or searching about, and drop at once upon some recently deposited ordure. Where burying beetles are common they may be seen, in like manner, flying one after another to some dead mole, or shrew, lying amidst high grass, and certainly incapable of being seen from a distance. Every housewife must have noticed with what importunate assiduity the common blow-fly will hover about a cupboard or safe enclosing meat, even when the contents are totally hidden from view. It is especially interesting to note that certain plants which to our senses emit the odour of carrion prove attractive to carrion-flies. It is less generally known that some of the most beautiful butterflies are attracted by excrementitious and putrescent matters, and may even be lured within reach of the entomologist's net by such baits. This scarcely agrees with the poetical notions of butterfly-life; but alas! *Psyche* will sip the fœtid moisture from carrion as eagerly as the nectar from the purest flower. A dead weasel or rat nailed against a tree-trunk will often induce the "purple emperor" to descend from the tree-tops over which he is wont to flutter. The *Papilios* and giant *Ornithopteras* of warmer climates may also be captured by similar stratagems. According to Mr. W. M. Gabb,* the brilliant *Morphos* of Nicaragua may be caught "by baiting with a piece of over-ripe or even rotting banana," whilst at other times they were almost unapproachable. The same author adds that his native servants "always carried with them a fermented paste of maize-flour, which they mixed with water to the consistency of gruel, as a beverage. On our arriving at the side of a stream in a narrow gorge, invariably, within a few minutes after they opened a package of this paste, although there might not have been a butterfly in sight before, these most brilliant of their kind would come sailing up, always from the leeward." Our common *Vanessa Atalanta* is, in like manner, attracted by the smell of over-ripe or rotting fruit, especially plums.

One of the commonest and most successful methods of capturing nocturnal Lepidoptera, *i.e.*, sugaring, depends on an appeal to their sense of smell. A thick syrup of coarse brown sugar is mixed by some experts with rum, by others with porter, and by others again with a little vinegar,—all strong odorous fluids,—and the composition is smeared upon the trunks of trees. The moths come to sip the syrup, and are caught.

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* Nature, February 7, 1878.

But there is another method of catching Lepidoptera which is even yet more convincingly demonstrative of the wonderfully acute scent possessed by these creatures. We refer to the practice of "sembling." If a virgin female moth of certain species is shut up in a box, males of the same species will make their appearance, even from a very considerable distance. Thus Mr. Wonfor, in a paper read before the Brighton and Sussex Natural History Society, mentioned that he had by this method captured, in two days, fifty males of *Saturnia Carpini*. He declares that the attraction "cannot be by sight, for the females were in a box on the side of a slope, and the males flew across the valley and close to the ground. When trying similar experiments with other species we purposely selected a field with a wood at the end, and saw the males flying over the tops of the trees." They always, further, approach against the wind. Two additional circumstances have to be taken into consideration: as soon as the female is impregnated the attraction ceases, and, further, the moths in question are by no means common. In the same district where Mr. Wonfor made his experiments any person, not having with him a female *S. Carpini*, could scarcely count upon meeting with a single male of the species in the course of a day's ramble.

The following instance, recorded by Mr. J. H. Davis, Curator of the Portsmouth Philosophical Society,* is conclusive against the supposition that the males are attracted to the female in consequence of discerning her afar off:—"Another female of the same species (*Sphinx Convolvuli*) had been produced; three males found their way into my study down the chimney." Many more instances of male moths coming and hovering round, or settling upon perfectly opaque boxes in which females of their own species were imprisoned, might be adduced did any necessity exist. It is, however, contended by some that the attraction, though not sight, may be a sound. The virgin female, they argue, produces a sound inaudible to human ears, but distinctly heard and understood by the males of her species, and becomes silent as soon as her love-call has been answered. There is not in this supposition anything necessarily absurd; but the following fact proves it to be utterly inadmissible. Mr. J. H. Davis, in the Journal above cited, tells us:—"On going into my study, in the evening, I found a female *Sphinx Convolvuli* fluttering on the floor. On lifting it up it ran up

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* Zoological Journal, vol. v., p. 142.

my coat and several times round the collar before I could place it in safety. I went from thence into my garden, to shut some hot-bed lights, where I was occupied about ten minutes; from thence again to my study, where I found two fine males of *Sphinx Convolvuli* had, whilst in the garden, attached themselves to the collar of my coat, where the female had previously been." This instance, we think, is absolutely decisive. A scent might be easily left adhering to the collar of the coat, whilst no one can conceive of a sound remaining. Male moths have further been known to flock to the empty cocoon from which a female had recently escaped, though she had in the meantime been removed. Sight and hearing being evidently thus out of the question, there remains only one sense known to us capable of acting at a distance. Judging from our own powers, indeed, scent of such delicacy and subtlety is simply incomprehensible. We wonder at the accuracy with which the harrier can track a hare, or the bloodhound a fugitive criminal or slave, over meadows, marshes, ploughed lands, and trodden roads; and if common observation were not against us we should doubtless proclaim this also inconceivable or impossible. But the task of the hound is vastly easier than that of the moth. He is guided by scented surfaces to which the specific odour of the animal pursued adheres in a concentrated form. The emanations from the female moth, on the other hand, are diffused through space and diluted with an excessive quantity of air, whether scentless or saturated with other odours. Yet this infinitesimal trace is sufficient to guide the male towards her with unfailing accuracy, and from distances of at least 200 yards, or about 3600 times the length of the insect's body. This is as if a human being were able to detect the presence and the condition of an individual of his own species at the distance of more than four miles! Inconceivable, however, as such a power may seem to us, there stand the facts before us, and we have merely the option of admitting this wonderful delicacy of scent or assuming the existence of some sense totally foreign to us, and possessed of equal delicacy in its indications.

According to the observations of Mr. Belt,* confirmed by those of others, the *Ecitons* and other ants follow each other by scent. Each exploring party marks out thus the road it has travelled. He says:—" I one day saw a column of *Eciton hamata* running along the foot of a nearly perpendicular tramway-cutting, the side of which was about 6 feet

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* Naturalist in Nicaragua, p. 23.

high. At one point I noticed a sort of assembly, of about a dozen individuals, that appeared in consultation. Suddenly one ant left the conclave, and ran with great speed up the perpendicular face of the cutting without stopping. It was followed by others, which, however, did not keep straight on like the first, but ran a short way, then returned, then again followed a little farther than the first time. They were evidently scenting the trail of the pioneer, and making it permanently recognisable. These ants followed the exact line taken by the first one, though it was far out of sight. Wherever it had made a slight *détour*, they did so likewise. I scraped with my knife a small portion of the clay on the trail, and the ants were completely at fault for a time which way to go. Those ascending and those descending stopped at the scraped portion, and made short circuits until they hit the scented trail again, when all their hesitation vanished, and they ran up and down it with the greatest confidence." Mr. Belt further thinks that ants "can communicate the presence of danger, of booty, or other intelligence, to a distance, by the different intensity of the odours given off." In this hypothesis of a scent-language addressed to the organs of smell, taking the place of a sound-language addressed to the organs of hearing, strange as it may seem at first glance, there is nothing impossible or even improbable. The language of man and of many other Vertebrates demands the power of producing at will sounds, joined to the faculty of recognising them ; in other words, the joint possession of vocal and auditory organs, In like manner, the language which Mr. Belt thus attributes to ants requires merely the power of producing odours, and of distinguishing them when produced. Now, one of the most striking peculiarities of insects, as compared with vertebrate animals, is the variety and intensity of the odours which they emit, even as appreciable to our olfactory nerves. There are genera, and even species, which an experienced entomologist can recognise, even blindfold, by the smell alone. How distinct these scents must be to such sensitive organs as are evidently possessed by insects will be understood from the facts stated above. It appears also that in certain cases these odours can be emitted, suppressed, or varied at will. Here, therefore, are all the facilities needed for a scent-language. Our want of some standard for remembering and recording odours renders the study of this subject peculiarly difficult.

Curiously enough, the notion has been lately taken up that insects, after all, do not possess the sense of smell.

An experimentalist placed some caustic ammonia close to the head of a moth which was either asleep or "shamming" death, and was surprised to find that it took little or no notice of the pungent vapour. On the other hand, he observed that a loud and sudden sound seemed to startle the moth. To do the writer in question justice, he does not appear to have drawn from this single experiment the rash and sweeping conclusion that insects are incapable of smelling. Others, however, were less cautious, and not a few paragraphs have appeared in consequence, in political and literary journals, to the effect that the supposed possession by insects of a sense of smell must now be regarded as an exploded error. We have found, however, not indeed an insect, but a spider perfectly sensitive to the odour of ammonia. Being once annoyed out of all patience by a *Tegenaria*, which would persist in attaching her web to a burette-stand in the window of our laboratory, we unstoppered a 20-oz. phial of the strongest ammonia, and presented it at her, in the hope of putting her to final rout. With the usual courage of her race she charged the strange object with so much eagerness as nearly to fall into the open bottle. Catching the fume, however, she at once turned tail, and fled precipitately. Perhaps if we fully understood the structure of the olfactory organs in moths we should see a reason why they are little affected by a dose of ammonia from which we should shrink back half-stifled. Their nerves of smell do not appear to terminate in a mucous membrane liable to be irritated by ammonia. Furthermore, we have observed cases where odours repulsive to man appear indifferent, or even attractive, to insects. That gnats possess the power of smell is demonstrated beyond all reach of doubt; yet we have seen a crowd of these little beings dancing merrily over the ventilators of a shed from which the orange fumes of hyponitric acid were escaping in torrents. It would have been easy for them to have found an equally eligible place free from the pungent vapour, but they showed no disposition to withdraw. In summer mornings, also, we have frequently observed moths of various kinds drowned in bowls of solution of tin in *aqua regia*, whilst none of these nightly visitors had thought proper to commit suicide in a cistern of water close at hand. We were hence led to conclude that the odour of the tin solution was to them positively attractive. Such solutions indeed have, under certain circumstances, a faintly aromatic smell which might be likened to that of decaying fruit.

But if insects thus—as we are warranted in concluding—

possess the sense of smell in a degree of perfection quite unknown to vertebrate animals, the question follows, What is its organ? Here there is a want of accord among authorities who have made this subject their study. Still we hold that a preponderating body of evidence points to the antennæ as the seat of this sense. These organs occupy a situation exceedingly appropriate for the purpose; they are exposed to currents of air, and can be readily applied to or held over any substance which the insect may wish to examine more closely, and they are most abundantly supplied with nerve-fibre. In flies the third or terminal joint receives thousands of such filaments, each apparently terminating in a small open cell. In some of the Buprestidæ the antennæ display multitudes of pores or open cells, scattered uniformly over the entire surface, whilst in others they are concentrated in a small depression upon each joint. The development of the antennæ, which varies greatly in different groups, presents, in the main, the features which we should expect in an organ of scent. We know that in insects—as indeed in all animals—the male seeks out the female, who in most cases is more sedentary in her habits, and is in some instances even devoid of wings. Hence we may fairly conclude that the male will require to possess the sense of smell in a higher degree than the female. This is accordingly the invariable rule: if the antennæ of the two sexes are not absolutely alike, those of the male are more highly developed. This is especially the case in such moths as are usually entrapped by the process of "sembling," as above mentioned. In these, as for instance in *Saturnia Carpini*, the antennæ of the female have the form of a bristle, while those of the male have a series of minute plates, like the barbs of a feather, projecting out on both sides, and exposing a great amount of surface to the air.

We should also expect the organs of smell to be more complicated in species which feed on a very narrow range of substances, and have consequently more difficulty in finding support than in omnivorous species whose food is everywhere. We should also suppose that a less delicate sense of smell, and consequently a less highly developed organ, would be necessary in insects possessing great locomotive powers than in such as travel slowly and awkwardly. Further, if the antennæ are the organs of smell, their development should be to some extent inversely as that of the eye, and should be relatively higher in nocturnal than in diurnal species. All these suppositions may, generally speaking, be pronounced to agree with observed facts. The dragonfly, with his wonderful speed and command of wing, and with his scarcely less marvellous development of the visual organs, has comparatively little need for the sense of smell, and his antennæ are small and simple. The locust, strong on the wing, able as a leaper, and prepared to eat almost every green thing upon the face of the earth, has small occasion for minute discrimination of odours: his antennæ, accordingly, are mere bristles. The tiger-beetle—able to run, bound, and fly with extreme velocity, and preying upon every animal he is able to overcome—needs little delicacy of scent, and his antennæ therefore are of the same simple type. The ground-beetles, though in many cases wingless, and in others nocturnal, are willing to feed both on living prey or dead animal matter, and in case of need upon certain vegetable substances. We need therefore feel little surprise that their antennæ, too, should be plain in structure. The common house-fly is swift and powerful on the wing and unlimited in its diet; it therefore has less need for a nicely appreciative scent, and in consequence for highly developed antennæ.

On the other hand, the male moth who has to seek both his food and his partner, often in the night, has in a majority of instances highly developed plumose antennæ. The butterfly—who, though feeding on the honey of flowers, does not appear to be promiscuously attracted by all plants—has antennæ furnished at the end with buttons or knobs. The common dung-beetle, who crawls slowly and flies heavily, and who depends upon one class of substances alone for his own food and for the *nidus* of his offspring, has at the end of his antennæ a club that opens out in plates, like the leaves of a book, and thus exposes a very large amount of surface to the action of the atmosphere.

We may further remark that the antennæ in larvæ are in a rudimentary state, and merely become developed when the insect has reached the reproductive stage of its existence.

This also is in favour of our view that they are the organs of scent—a sense which throughout the animal kingdom seems to stand in a close and particular relation to the sexual functions. It has been observed that in our own species the olfactory nerves are comparatively inactive prior to the age of puberty.

The question has often been raised—By what means does the ichneumon-wasp, or other parasitical insect, discover the presence of the larvæ or pupæ destined to become its victims, hidden, as the latter frequently are, among closely folded leaves, or in fruits, in the stems of vegetables, or in masses of earth? The sense of smell seems by far the most likely guide; and if we watch a female ichneumon on the search for larvæ in which to deposit her eggs, and note the rapid and systematic play of her long flexible antennæ over the surface of the objects she is examining, we cannot help comparing her movements to those of a hound searching for the trail of a fox or a deer. If we admit that the antennæ are the olfactory organs all becomes intelligible.

Actual observation, as far as it has been carried, testifies in the same direction. We have often offered fruit or flowers to captive rose-beetles, and have always found that their first action was to stretch out the antennæ and expand the leaflets of their terminal clubs. Dung-beetles act precisely in the same manner if suddenly presented with a piece of excrement. Indeed all insects whose antennæ are large and conspicuous enough to be conveniently observed act as if these organs played a most prominent part in the recognition of food placed in their way. That they may be, at the same time, organs of touch is perfectly possible. The proboscis of the elephant, the snout of the swine, &c., fulfil this double function.

Provisionally, then, we think it may be admitted that the antennæ are the organs of smell. But till we are able to show some correlation between the form of these organs in each group and its peculiar habits and requirements, or its general structure, our knowledge must be confessed to be exceedingly imperfect. We are perfectly aware of Dr. Wolf's supposed discovery of an organ of scent in insects, which is merely a "specially differentiated portion of the membrane" which extends from the labrum inwards. We admit that the part examined by this naturalist is an organ of sensation. But we do not see that even the attempt has been made to trace any connection between its development and the varying degrees of olfactory power.

Though second in moment among the senses in man, the faculty of hearing must, among invertebrate animals, receive a lower position in accordance with the part which it plays in their economy. On this subject, however, we are almost daily receiving new and often startling revelations. Naturalists have long known that some insects possessed the power of producing sounds at pleasure, and have very justifiably inferred that such species cannot be devoid of the sense of hearing. The responsive chirp of the cricket, the cicada, and the grasshopper; the peculiar note uttered by the queen bee, and which produces such an effect upon her subjects; the importunate or querulous hum of many Hymenoptera when angry; and the wailing buzz of the common house-fly when captured in the web of a spider, on hearing which all other flies beat a retreat from the spot—all these instances of insect-voices and insect-hearing are well known. But till very lately the vast majority of insects and of other articulate animals were considered literally dumb. Now, however, vocal powers are being discovered in spiders, scorpions, butterflies (hair-streaks), moths, beetles (*Cychrus*, *Prionus*, &c.), as well as in the groups formerly known to be noisy. Several *Vanessæ*—members of the group to which our common "peacock butterfly" and "red admiral" belong—are known to stridulate, as also the Brazilian butterfly (*Ageronia feronia*), and the moths *Chelonia pudica* and *Euprepia matronula*. The "death's-head" (*Sphinx Atropos*) was formerly supposed to be the only Lepidopterous insect capable of emitting any sound. The organs of sound are often very curiously constructed, and are provided in some cases with a resonant cavity for the purpose of intensifying the effect. If we further take into consideration the circumstance that the sounds produced by such minute animals may easily be too acute to be recognised by human ears, we shall not be far wrong in supposing that the majority of the Articulata can emit sounds at will, and that they therefore are probably endowed with the sense of hearing.

It is to be remarked, however, that the recent discoveries of the production of sound by insects refer more to the solitary species than to such as live in organised societies. Ants have not hitherto been observed to utter any sound, save a kind of hissing when on the march. Their antennal language—whether or not we regard it, with Mr. Belt, as depending upon the production and recognition of odours, or view it merely as a system of movements and touches, somewhat resembling our "deaf and dumb alphabet"—can scarcely be referred to sounds. The stridulation of solitary insects is often, doubtless, a love-call; often again, as in the scorpion, a note of defiance. Predatory insects are doubtless, like larger animals, often guided to their prey by the sense of hearing, and the feebler species may in some cases be made aware of the approach of danger. Gilbert White* curiously enough accuses bees of deafness:—"It does no appear from experiment that bees are in any way capable o being affected by sounds; for I have often tried my own

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* Selborne, Letter XXXVIII.

with a large speaking-trumpet held close to their hives, and with such an exertion of voice as would have hailed a ship at the distance of a mile, and still these insects pursued their various employments undisturbed, and without showing the least sensibility or resentment." The rustics who, when pursuing a stray swarm of bees, kept up a horrible dissonance with rattles, cows' horns, frying-pans clashed together, and the like, seem to have taken a different view of the hearing of bees.

The question as to the ears of insects is not satisfactorily settled. It would almost seem that these organs, or what stands in their stead, are placed differently in different groups. In the two-winged flies (Diptera) the so-called poisers or halteres—the small knobs which take the place of the posterior wings of other insect-orders—have been supposed to be the organs of hearing. The Orthoptera are said to have ears on their fore-legs, and other insects seem to possess similar organs in the subcostal vein of the wing. It may be asked, how can these points be ascertained? There is certainly, in observing and experimenting on such subjects, wide scope for error. We once saw it gravely maintained that the antennæ of insects served as auditory organs because when an insect is startled by a loud and sudden sound a convulsive movement is sometimes observed in these members. This, however, proves nothing: a man under similar circumstances will often give a sudden jerk with his arms; yet no one will maintain, on that account, that we hear with our hands. To recognise the organs of sensation in the vertebrate animals is generally easy, because they occupy positions answering to those which they hold in our own system. But among Invertebrates the case is different: organs which in mammals, birds, &c., are concentrated in the head or on the trunk, may there appear on the limbs. If then we find, *e.g.*, on the wing or the leg of an insect some apparatus specially supplied with nerves, and yet obviously adapted neither for locomotive, prehensile, vocal, secretive, or reproductive functions, &c., we arrive, by a process of exhaustion, at the inference that it is most probably an organ of sensation. Concerning the eyes there can be fortunately no doubt. The senses of smell and taste we may reasonably expect to be in close proximity to the mouth. So that a highly specialised organisation found on the wing or leg, as above mentioned, may with great probability be pronounced either to be an ear or the seat of some sense totally unknown. If, on experiment, the removal of such organ is found to bring with it the incapacity to recognise sounds, the evidence may be considered as complete.

Concerning taste in the Invertebrates we know very little beyond the bare fact that many of them are extremely scrupulous in the selection of their food, and will in many instances accept death by hunger as a preferable alternative to a change of diet. Here, however, the functions of smell and of taste are so interwoven that we are not yet competent to draw the boundary line. What are the organs of taste in insects is still a matter of doubt; the palpi, the so-called tongue, and the inner surface of the alimentary aperture have all been suggested.

Touch plays a very important part in the economy of the Articulates, and attains in some of them a delicacy at least as great as in man. Pope's assertion that the spider's touch "lives along the line" is the expression of a literal truth. If an *Epeira* is sitting in the centre of her geometrical web, and a gnat becomes entangled towards the circumference, she may be seen applying her feet in succession to the different radii, and then bounding off in the right direction. That she is guided by touch rather than by sight appears from the circumstance that she may be induced to rush out in the same manner if the web is gently tickled with a straw, or if a fine jet of water is cautiously directed against it from a syringe or washing-bottle. We once saw a Harry Long-legs—as they are familiarly called—fall into a web, but manage to escape, leaving one of his ungainly limbs behind him. The spider, hurrying up and finding the foot, ran along, naturally expecting, doubtless, to find a body at the other end. Being disappointed she returned to the foot, and, when finally convinced that she had merely a trunkless leg to deal with, she fled with precipitation, as if utterly staggered at such a violation of the fitness of things.

The antennal language of ants—if it does not, according to Mr. Belt's interesting conjecture, turn upon the emission and recognition of odours—must depend upon touch. But if we look through the whole extent of what has been called "insect architecture" we meet with one unbroken series of instances, proving the utmost delicacy of touch in articulate animals.

If we try to discover the seat of the sense of touch in insects, we find several organs to which it has been assigned—the antennæ, the palpi, the paraglossæ, and even the feet may take a share in the process. It is, of course, probable that where so many organs exist there must be some variety in the nature of their functions. We may here be on the track of modifications of the faculty of touch or feeling which may be substantially to us unknown senses. Our power of touch refers mainly to solid bodies; we can decide whether they are moist or dry, hot or cold, rough or smooth, &c., and we can discriminate between solids and liquids. But we have no organ that informs us of the state of the atmosphere, except in as far as we perceive its temperature by the whole surface of our bodies. It is far from improbable that some of the organs of insects may give them information of the condition of the atmosphere—baroscopic, hygroscopic, or electroscopic. This is the more likely since some of the parts which have been suggested as the seats of touch are, in multitudes of cases, exceedingly ill-adapted for being applied to the examination of solid bodies. The antennæ are often too short and too sparingly mobile.

It is plain that the more numerous and complete the data laid before us, the easier does the solution of any problem become. If, then, certain of the lower animals possess more delicate, and possibly more numerous senses, than do we, they are in a position to acquire, by direct perception, knowledge which we can only gain by trains of inductive reasoning and by the use of instruments of precision. Even yet Natural History is haunted by a phantom known as "Instinct," which is invoked in every case of difficulty, just as was phlogiston by the chemists of the last century, and which is invested, *pro re natâ*, with attributes not always the most conceivable or the most mutually consistent.* But very probably those instances of supposed instinct which are not resolvable into hereditary habit may be traced to the simple following of the guidance of senses more acute than our own. Thus we are told that certain birds, beasts, and insects have an instinctive foreknowledge of coming storms and of other meteorological changes; that migratory birds leave us when the weather is still warm and sunny, and the insects upon which they feed are still plentiful; that wild geese, fieldfares, stormcocks, &c., arrive earlier than usual on our shores, not because unusual cold has already set in at their ordinary resorts, but because it is going to do; that the bees contract the inlet into their hive in proportion to the degree of cold which is about to prevail, &c. These observations, in so far as they are really founded

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* Our great objection to "instinct" is that it is too often a word hiding ignorance under the veil of pretended knowledge. There are in Biology unsolved questions in abundance, some of which are possibly beyond the scope of the human intellect. But in such cases, instead of talking about "instinct," let us frankly confess that we do not know.

in fact, are merely instances of warnings furnished by sensuous perceptions more acute than our own. We pronounce it a mystery that the swift should leave us by mid-August, when, according to our feelings, we are in the height of summer. But could we see and feel with the swift we might perceive a change amply sufficient to prompt migration.

Even the attraction of a virgin female moth for the males of her species, as already described, has been referred to the convenient class of "instincts." It must be a curious instinct, indeed, which will act to the windward and not to the leeward. We never think of saying that the hound pursues the fox by instinct. If unwilling to concede to him the possession of reason, we may say that it is by instinct he knows that the odour he detects upon the grass or the soil has been left there by an animal which he may overtake if he follows up the trail. But the act of recognising this odour we ascribe simply to one of his senses. Why take another course as concerns the *Saturnia Carpini?* If an animal detects the presence of any object at a distance, it can only be effected by one of two methods; either material molecules, solid or gaseous, are given off by the object, and brought by atmospheric (or aqueous) currents into contact with the sense organs of the observer, or else certain vibrations or undulations, sonorous, luminous, &c., reach him through the medium of the atmosphere or the ether. Surely neither of these processes can be described as "instinct," and he who by implication admits the possibility of a third way should at least give us some hint concerning its nature and mode of action.

Here, therefore, is a point of departure for animal psychology which has not received a due share of attention. We must study the senses of the lower animals both structurally and functionally, more especially in the Articulata, where the departure from the human type of organisation is so complete, and where the manifestations of intelligence are so complex and so nearly rival our own.

II. THE SUPERFICIAL GRAVELS AND CLAYS AROUND FINCHLEY, EALING, AND BRENTFORD.

By Thomas Belt, F.G.S.

I. *Introduction.*

I PURPOSE in the present paper to show the relation of the Glacial-beds at Finchley to the implement-bearing gravels at Ealing, and the mammaliferous gravels and sands at Brentford.

Possibly the objects I have in view—to demonstrate that the formation of the valley-deposits took place in the Glacial period, and that palæolithic man was pre-diluvial—might have been more clearly attained by a thorough study of one of the more northern valleys; but I have had much greater facilities for making myself acquainted with the district I have chosen. It has also this great advantage, that it is close to London, so that my descriptions and conclusions may be readily checked by an inspection of the numerous gravel- and clay-pits from which I have obtained the facts described in these pages.

II. *Description of the Deposits.*

1. *Finchley and Neighbourhood.*—The Glacial beds in the neighbourhood of Finchley were described in 1835 by Mr. Edward Spencer,* who traced the boulder-clay and underlying gravels from Muswell Hill to Finchley Common. Mr. Whitaker has mentioned them in his "Memoir on the Geology of Parts of Middlesex, &c.,"† and Mr. Henry Walker has described the beds exposed in the cuttings of the Great Northern Railway and the clay-pits in the vicinity.‡

My own observations date from July, 1875, when I first visited the clay-pits near Finchley under the guidance of Mr. J. J. B. Ives. Since that time I have neglected no opportunity of examining the numerous sections that have been exposed in excavations for the foundations of new

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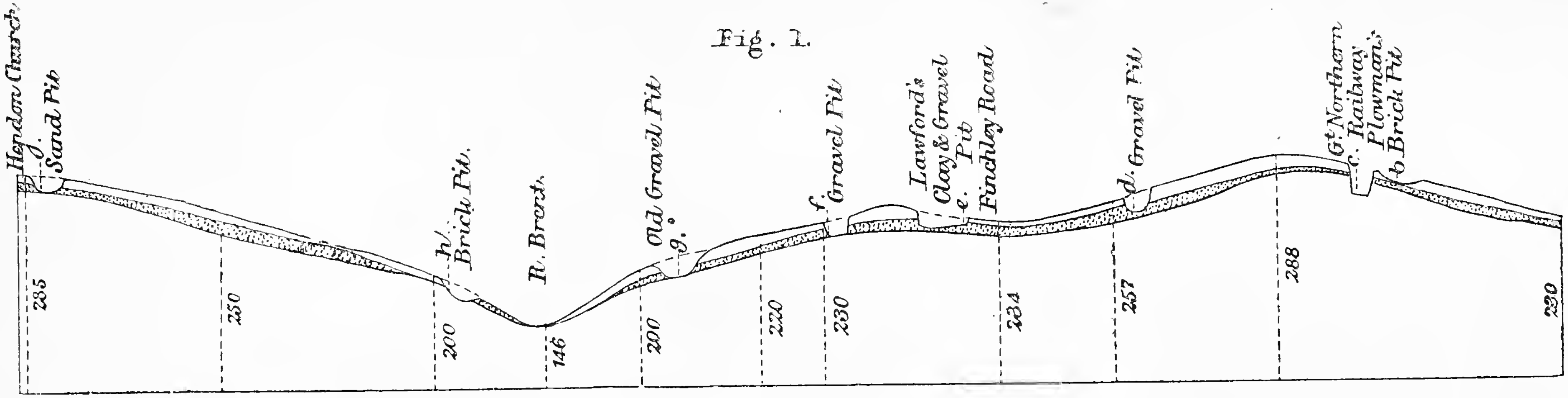
* Proc. Geol. Soc., vol. ii., p. 181.

† Mem. Geol. Survey, 1864.

‡ Proc. Geol. Assoc., vol. ii., p. 289, 1871.

Fig. 1.



SECTION ACROSS FINCHLEY TO HENDON.

Horizontal scale, 3 inches = 1 mile. Vertical scale, 1 inch = 200 feet. The figures express the height of the surface, in feet, above the Ordnance Datum-line. The dotted stratum is the Middle Sands and Gravels; the unshaded part above, the Upper Boulder-clay, as more particularly shown in the sectional details.

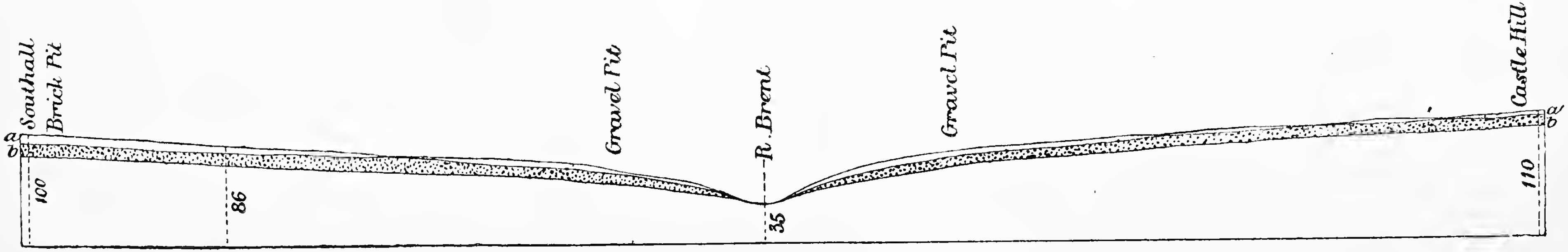


FIG. 2.—SECTION FROM ARGYLE ROAD, CASTLEHILL TO SOUTHALL.

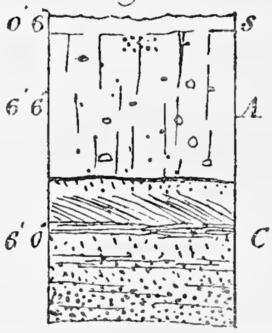
Scale as above. *a*. Brick clays. *b*. Sands and gravels.

houses, as well as the more permanent ones in the gravel- and clay-pits. In this way I have been able to trace the beds right across from the drainage area of the River Lea into and across that of the Brent.

The general distribution of the deposits is shown in Fig. 1, which is a section extending from a little east of the "Green Man" public-house, on the St. Alban's Road, to the top of the hill at Hendon. The following are the details from which this section has been constructed :—

At an old brick-pit at the eastern extremity of my section the Upper Boulder-clay has been worked, but the surface is now grassed over. Lower down the slope I found a gravel-pit open about 250 yards directly west of the "Green Man" public-house, on the south bank of a small stream running to the Lea, and obtained the section shown in Fig. 3.

Fig. 3.

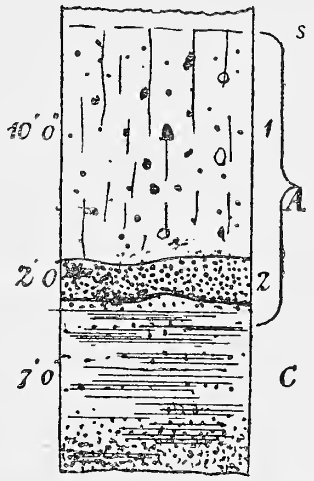


GRAVEL-PIT NEAR EASTERN END OF GENERAL SECTION.

S. Surface soil. A. Brown boulder-clay, unstratified, with bluish vertical partings. Stones mostly flints, with a few pieces of quartzite, sandstone, chalk, &c., the chalk mostly decomposed. C. Sands and gravel. Mostly sand at top, with lines of small gravel. Sandy coarse gravel below. Pebbles mostly subangular. Base not seen.

In this section the chalk has been mostly dissolved out of the clay, but pieces, generally quite soft, are still to be found in places, and in others the calcareous matter has entirely disappeared, leaving behind, nests of siliceous grains originally contained in the chalk. Ascending the hill westward, a good section of the boulder-clay is exposed in Mr. Plowman's brick-field. The clay here contains much chalk *detritus* and many other travelled stones, including pieces of granite, lias, and red chalk. Just beyond this is the cutting of the Great Northern Railway. The slopes are now mostly grassed over, but the beds were examined and described by Mr. Henry Walker when the section was well exposed. He found a chalky boulder-clay overlying a blue boulder-clay, with occasional patches of sandy gravel between them. At Finchley Station, about 550 yards to the north-west of the line of section, the chalky boulder-clay is exposed in patches overlying coarse gravel, and that again a sandy gravel. Further up the line this sandy gravel is superimposed upon a dark blue clay with much liassic *detritus*. When the cutting was being made Mr. Walker traced this blue clay for a distance of a mile and a half towards East End, considerably past my line of section. A little south-west of the railway the line of section crosses the top of the watershed between the Lea and the Brent. Descending the slope I was so fortunate as to find a large excavation for obtaining gravel and sand in the grounds of the Avenue, and the following section exposed :—

Fig. 4.



SECTION AT THE POINT MARKED *d* IN FIG. 1.

s. Surface soil. A, 1. Chalky boulder-clay. A, 2. Coarse gravel. Pebbles mostly rounded. C. Very sandy gravel. Mostly sand at top; coarser towards bottom of section. Base of gravel not seen.

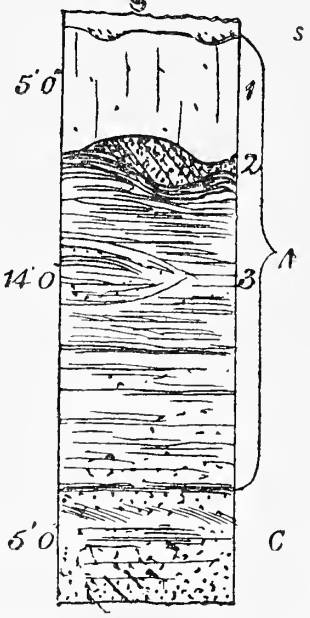
The upper part of the clay, both here and at Mr. Plowman's clay-pit, is brown and without chalk.

The line of section crosses a small valley running into the Brent, and, after passing the Finchley Road, reaches Mr. Lawford's brick-field at Church End. The beds exposed in this pit are shown in Fig. 5.

In this section the Upper Boulder-clay is much thinner than higher up the hill, and contains very few stones excepting in nests next the surface. These surface patches of pebbles mark the extent to which the clay has been subjected to subaërial denudation, the finer materials having been carried away and the coarser left behind. This denudation has been greater on the slopes than on the plateaux, so that it is on the latter that we find the clay now thickest.

The patches of gravel marked 2 in Fig. 5 are very irregular. Some of them look like lumps of the Middle Sands and Gravel that had been picked up and deposited in a frozen state, the originally horizontal stratification having in their new position been turned on end.

Fig. 5.

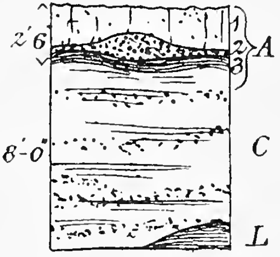


SECTION AT MR. LAWFORD'S CLAY-PIT, CHURCH END.

S. Surface soil. A, 1. Brown boulder-clay with few stones, excepting in nests near the surface. A, 2. Irregular patches of sandy gravel. A, 3. Alternations of laminated clay, sandy clay, and sand, with some lines of fine gravel. C. Sandy subangular gravel, with rounded quartz pebbles.

The line of section now crosses to Hendon Lane, where many gravel-pits have been opened, but most of them are now filled and houses built over them. In July, last year, I visited the spot, in the company of Mr. Henry Hicks, and obtained the section shown in Fig. 6, a little to the north of the line of the general section.

Fig. 6.



GRAVEL-PIT IN HENDON LANE.

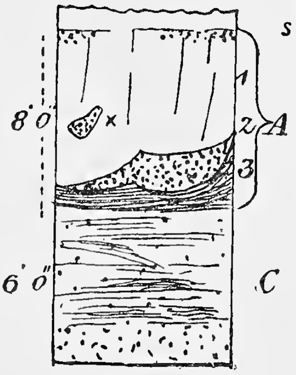
Brown boulder-clay, with few stones. A, 2. Irregular patches of gravel. 3. Laminated dark sandy clay. C. False bedded sands and small gravel. London Clay

The Middle Glacial Gravels here and throughout the Finchley district contain a considerable proportion of rounded pebbles from the Eocene beds.

Farther down the slope, in the field on the north side of Oldershill Lodge, the gravel has been largely worked. The following important section (Fig. 7) was exposed when I visited the pits in January last. The gravels extend to within about 40 feet of the level of the Brent, or Dollis Brook, as the stream is here named.

The upper clay A, 1, is here nearly without stones, excepting patches near the surface. It very closely resembles the Upper Brick-clay of the Thames Valley, and, as it is undoubtedly a continuation of the Upper Boulder-clay, and overlies the Middle Glacial Sands and Gravels, this change to the usual form of the brick-clay, as it descends into the valley, is very significant. In this section there was an angular piece of reddish sandy gravel, lying completely surrounded by the clay, at the point marked × in Fig. 7.

Fig. 7.



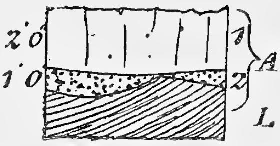
Gravel-pit in Field near Oldershill Lodge.

s. Surface soil. A, 1. Brown unstratified clay, with vertical joints. A few nests of pebbles at surface. Angular patch of gravel at ×. A, 2. Coarse gravel in sandy clay. A, 3. Dark, sandy, laminated clay. C. Sand and small sandy subangular gravel.

Descending Hendon Lane there are more old gravel- and sand-pits, in which exactly the same succession of beds is shown as in Fig. 7. These extend down to the line of 200 feet above the ordnance datum. Below this the ground slopes more rapidly towards the brook, and the middle sands and gravels thin out, but the Upper Clay, with its gravel patches at base, overlaps the Middle Sands and Gravels, and continues down to the bank of the brook, where it is seen as shown in Fig. 8.

Crossing now to the Hendon side of the stream, the surface deposits are shown in a large brick-field on the south side of Finchley Lane. The Upper Clay, with its accompanying patches of gravel, is thin, but it is seen to be continuous nearly to the brook. On the north side of the lane, along the line of section, it has been dug in several places; in others the clay has been denuded by natural agencies, and the coarse pebble gravel that lies at its base comes to the surface. The Middle Sands and Gravels come in below the clay at about the same height as on the Finchley side of the brook—that is, at about 200 feet above the Ordnance datum-line.* A little above that level, at the point marked *i* in general section (Fig. 1). I saw some sandy subangular gravel that had been thrown out in digging the foundation for a house. I have not seen anywhere on the Hendon slope these beds so strongly developed as they are on the opposite side of the brook, but Mr. Henry Hicks has informed me that near Heriot House they were 30 feet in thickness.

Fig. 8.



SECTION IN BANK OF BROOK BELOW THE HENDON LANE BRIDGE.

A, 1. Brown unstratified clay. A, 2. Coarse pebble gravel. L. London Clay.

Ascending the hill towards St. Mary's Church, the Upper Glacial Clay is seen in every road cutting and in excavations for new houses; but I did not see any deep enough to expose the sands or gravels until near the summit of the hill, where sand below the clay has been dug in several places, but the sections were not good enough for me to determine whether it belonged to the Middle Glacial beds or not.

On the west side of Hendon, down as far as the Midland Railway, the Upper Clay is everywhere present. To the south it is also seen wherever there are cuttings, but the Middle Sands and Gravels are not exposed, and I think they are mostly absent. To the northward the Chalky Boulder-clay is continuous from Finchley along the ridge past Whetstone. By the side of Church Lane, in Whetstone, the Middle Sands and Gravel are worked. I obtained here the section shown in Fig. 9. The surface of the ground is at this place about 290 feet above the Ordnance datum-line.

Mr. Caleb Evans has noticed the occurrence of the

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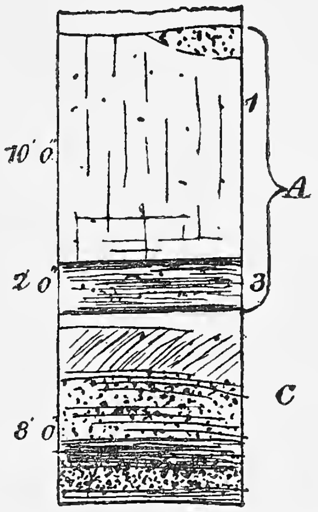
* The Ordnance datum-line is the mean level of the sea at Liverpool.

Glacial beds at Fortune Hill,* and on the slope to the north I saw both the Upper Clay and the Middle Sands and Gravels. To the east the beds extend as far as Muswell Hill.

Over the whole of this district the Upper Boulder-clay is spread, and conforms to the slopes of the hills, lying over them like a mantle. It contains most chalk fragments and other travelled materials on the flat-topped ridge on which Finchley is built, at a height of about 300 feet above the sea. On descending the slopes on either side the number of included stones and of chalky materials rapidly diminishes until it becomes a brown clay, containing only a few scattered pebbles. Mr. Whitaker records a great thickness of brown clay near Finchley Church, which, although it contains no boulders, he recognises as boulder-clay.† In the various sections I have examined, all the steps are to be seen in the gradation from a clay packed with travelled stones up to that in which only a pebble is to be found here and there. It is the non-recognition of the latter form of the deposit as a glacial clay that has led to the supposition that the Upper Boulder-clay is confined to the tops of the hills, and does not extend down their slopes.

Fig. 9.



GRAVEL-PIT, CHURCH LANE, WHETSTONE.

S. Surface soil. A, 1. Bluish chalky boulder-clay, with rather few scattered stones. Red and white hard chalk, not uncommon. Slightly stratified towards base. A, 3. Sandy loam, with patches of chalk *detritus*. C. Sand and sandy subangular gravel, with many rounded tertiary pebbles.

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* Proc. Geol. Assoc., 1873, vol. iii., p. 30.

† Guide to the Geology of London, 1875, p. 55.

2. *Ealing and Neighbourhood.*—The superficial deposits around Ealing have been noticed by many observers. In the works of Prof. Prestwich and Mr. Whitaker are to be found many references to them. Colonel Lane Fox,* in 1872, gave an excellent detailed account of the gravels and clays around Acton, and showed in several sections the position of the flint implements and of the mammalian remains found in them. This memoir has been of great assistance to me in the study of the same deposits in the adjoining parish of Ealing. My own opportunities have been numerous for gaining a knowledge of their distribution, and of the nature and relation of their different component parts. In addition to the large gravel-pits which are always open, the small pits which are sunk at the building of every new house, for the purpose of obtaining sand and gravel, have supplied sections all over the district. In the cuttings made for widening the Great Western Railway unusually fine and continuous sections were exposed between Acton and Hanwell, or right across the parish of Ealing from east to west.

Westward from Ealing there are large gravel-pits on each side of the Brent, at Hanwell, and in making the foundations for widening the railway-bridge, sections of the lower part of the valley of the Brent at this point were exposed. Deep cuttings for sewers have given nearly continuous sections in a north and south line across Ealing; and, lastly, the construction of the new branch railway from Turnham Green to Ealing, now in progress, has given me opportunities for checking the results obtained in the other sections.

I made careful notes of all the sections at the time they were exposed, and soon found that there were certain dominant features that ran through the whole with remarkable persistency. Further study showed that these features ran parallel with those observed in the glacial beds at Finchley.

In the large gravel-pit at Castlehill Station the gravel comes to the surface at the western end, but at the eastern side it is covered with about 2 feet of unstratified brown clay, and presents the section shown in Fig. 10.

Both westward and southward the clay, *a*, 1, thins out, and the subsoil consists of the gravel, *a*, 3, and the flints that had been scattered through the clay before it was denuded. In the railway-cutting opposite the gravel-pit the gravel, *a*, 3, came to the surface. Going eastward it was

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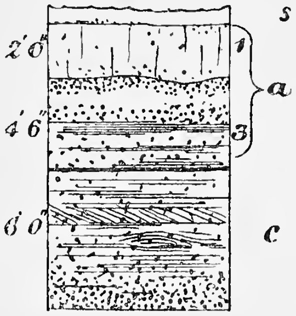
* Quart. Journ. Geol. Soc., vol. xxviii., p. 449.

seen to be gradually covered by the brown clay, and at about 100 yards east of Castlehill Station the section exhibited was as shown in Fig. 11.

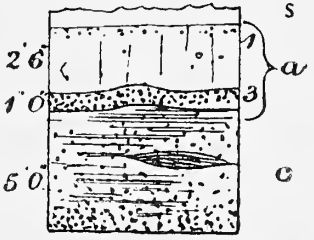
Fig. 10.



EAST END OF CASTLEHILL GRAVEL-PIT.

Surface soil. *a*, 1. Unstratified brown clay, with a few scattered pebbles. *a*, 3. Whitish gravel in clayey matrix, succeeded by alternations of ferruginous gravel and seams of sandy clay. c. Sand and very sandy gravel, sometimes false-bedded and with lenticular patches of sand in upper part. Base not seen.

Fig. 11.



RAILWAY-CUTTING 100 YARDS EAST OF CASTLEHILL STATION.

s. Surface soil. *a*, 1. Unstratified brown clay, with a few scattered pebbles. *a*, 3. Gravel in brown clay. c. Very sandy subangular gravel, with lenticular seams of yellow sand. Base not seen.

At Ealing Station a fine section was for a long time exposed, and I had the pleasure of showing it to Professors Morris and Bonney. It is represented in Fig. 12.

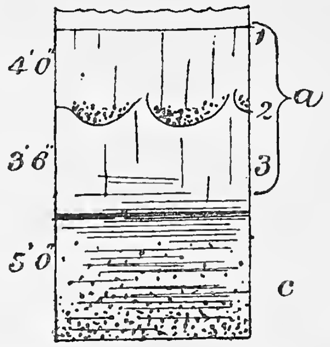
In it, the irregular patches of gravel *a*, 2 are more fully represented than is generally the case. Usually this division in the valley beds is only marked by a waved line of pebbles. At Ealing Station it consists of lenticular patches of mostly rounded pebbles lying on an extremely irregular surface of the clay below. They look as if they had been dropped whilst the beds below were so soft that the masses of gravel sank into them where they fell.

I might multiply instances and heap example on example to show the persistency and regularity of the beds, as I have great numbers of sections figured in my note-book, but no good purpose would be gained thereby, as everyone in the neighbourhood of London interested in the matter can observe them for himself.

The sections I have given, however, are all along an east and west line, with the surface of the ground a little over the 100-feet contour-line, and it will be well to describe the deposits at lower levels.

At Ealing to the south of the line of railway, the numerous sections shown in the small gravel-pits, dug when houses were building, all showed the same divisions. At about 90 feet above the Ordnance datum-line the brick clay is very thin, and sometimes absent through denudation; but below this line it again thickens. That it was originally present over the whole district is shown by the fact that patches of it are preserved all over the denuded areas in hollows or irregularities of the beds below. Thus in a long excavation that was made for a main sewer at Beaconsfield Villas, the trench ran north and south, and crossed two channels cut into the gravels. These channels were filled with the clay *a*, 1. I have represented one of these filled-up channels in Fig. 13. The other was about 80 yards to the north, and was deeper than that figured. The filled-up channels ran east and west.

Fig. 12.



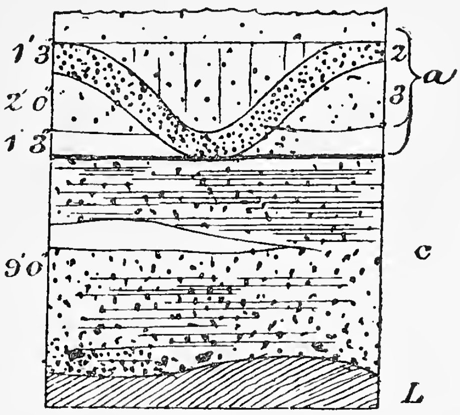
RAILWAY-CUTTING, EALING STATION.

s. Surface soil. *a*, 1. Unstratified brown clay. *a*, 2. Irregular patches of small pebbly gravel in clay. *a*, 3. Yellowish brown, rather sandy clay, a little stratified towards base. *b*. Sand passing downwards into sandy subangular gravel.

South of this the gravels continue down the slope, and at about 70 feet above the Ordnance datum-line are covered continuously with the clay down to near the 50-feet contour-line. At Ealing Cemetery, which is just above the 50-feet line, the gravel occurs in patches only, in hollows in the surface of the London Clay, and the brick-clay is thin, though still present. Below the 50-feet line the surface dips more rapidly, and on the steeper part of the slope the gravel is absent, but the clay, with its accompanying pebble-bed, though thin, is continuous.

Going westward from Ealing, towards the Brent at Hanwell, the beds down to the 50-feet line have been well exposed along the Uxbridge Road, in gravel-pits and other excavations. On the western side of the Brent the large gravel-pits opposite the Lunatic Asylum, and the Southall clay- and gravel-pits, afford good sections of the beds. The gravels descend both slopes of the valley, as shown in Fig. 2 (page 317). The lower gravel thins out at about the 50-feet contour-line, the upper beds overlapping it, and continuing nearly to the brook. Both conform to the slopes of the valley.

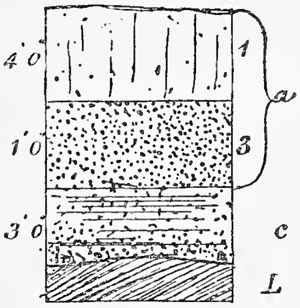
Fig. 13.



SECTION AT BEACONSFIELD VILLAS.

Surface soil. *a*, 1. Brown clay, with some pebbles. *a*, 2. Pebbly gravel in clay. *a*, 3. Irregular ferruginous sandy clay and gravel, with whitish stiff clay at base. *c*. Sandy gravel, with lenticular bands of yellow sand. Upper part of gravel irregularly stratified. Large flints and stones of quartz and quartzite at base. L. London Clay.

Fig. 14.

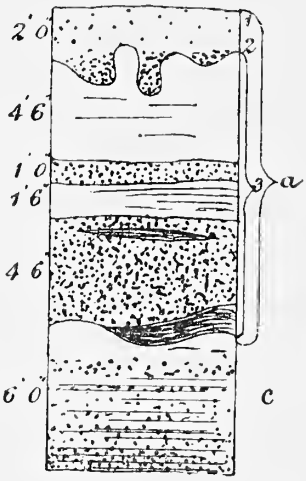


WEST SIDE OF BRENT, NEAR HANWELL RAILWAY BRIDGE.

a, 1. Unstratified brown clay, with a few scattered pebbles. *a*, 3. Gravel in clay, with many rounded pebbles. *c*. Yellow sandy subangular gravel, with a few large stones (12 inches diameter) at base. L. London Clay.

On the western side of the stream, a deep drain on the north side of the railway-embankment gives a continuous section down the lower slope of the valley-bank. Fig. 14 shows the section exposed, at about 20 feet above the Brent and about 50 feet above the Ordnance datum-line. Higher up the slope, at the large gravel-pit, the upper beds show more variety than is usual, the beds *a*, 3 being composed of a succession of clays and pebble-beds. Farther westward the clay *a*, 1 thickens, and is extensively worked for brick-making at Southall, where the sections correspond exactly with those at Ealing.

Fig. 15.



Gravel-pit opposite Hanwell Lunatic Asylum.

a, 1. Sandy clay, with pebbles scattered through it. *a*, 2. Pebbly gravel in clay. *a*, 3. An irregular seam of dark loamy clay at base; then a thick bed of pebble gravel in clay matrix; then dark loamy clay overlaid by pebble gravel, and that by sandy clay. *c*. Yellow sand passing downwards into sandy subangular gravel, with large stones at base.

The characteristics of the lower gravel are persistent throughout the whole district. It consists principally of angular and subangular pebbles of flint, with some rounded ones from the Eocene beds, and also pebbles and stones, generally rounded, of quartz, quartzite, Lydian stone, granite, and porphyry. It is extremely sandy, and the sand is distributed throughout it, and occurs also in lenticular patches. At its base it contains many large pebbles of quartzite, some a foot in length, and large unworn or slightly worn flints from the chalk. Large Sarsen-stones, 3 to 4 feet in diameter, are also found scattered over the district, at the base of the gravel.

The peculiar subangular character of the pebbles forming the bulk of the gravel, and the loose yellow sandy matrix, are the most characteristic features of the deposit. I had the pebbles in some samples of the gravel counted, and found that from 80 to 90 per cent were broken, or more or less angular. Thin chips of flint are abundant. Nearly all have the edges of the fractures a little rounded, as if, after they had been broken, they had been shaken together. About one-quarter of the rounded pebbles are of quartz.

There are slight and irregular signs of stratification in the lower part of the gravel in some instances; in others it is quite unstratified. Towards the top the stratification is more decided, but not continuous. Oblique lamination is frequent. It has the appearance of having been suddenly accumulated, and at one time. The irregular and fitful stratification, the short lenticular patches of sand, the mixture of sand throughout the gravel, the large stones at the base, the great proportion of broken flints with slightly worn angles, and the occasional oblique lamination, are all opposed to the theory that the deposit is the result of successive layers of materials brought down by a river at different times.

All over the district flint implements of the palæolithic type have been found in the lower gravel. According to the unanimous testimony of the workmen they occur nearly always amongst the larger pebbles at the base of the deposit, and close to the surface of the London Clay. Col. Lane Fox has described the position at which were found several flint implements at Acton, where the surface of the ground was from 75 to 83 feet above the Ordnance datum-line. Most of these were obtained from the base of the gravel, and the angles were worn and rounded. At one place, flakes were found in a thin seam of sand lying below the gravel, and in this instance the edges were "as sharp as when they were first flaked off the cores."* At Ealing Dean, Col. Lane Fox obtained two implements from gravel taken out in the construction of a sewer which was carried down below the general run of the excavations for the foundations of houses. According to this excellent authority they occur always at the base of the gravel. He says—"Here (in the lowest stratum of the gravel) the largest flint stones lie, and with them the implements, mostly of the dimensions of the larger stones, so that it was common for the more experienced workmen to say that they should find no implements till they got down into the coarse gravel; the smaller flakes, however, were not so invariably at the bottom."

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* Quart, Journ. Geol. Soc., vol. xxviii., p. 457.

Mr. Peter Crooke, of Turnham Green, has been very successful in finding implements in the Lower Gravel all over the Ealing district. One of these was obtained from the foundation of a house in Argyle Road, at the highest point the gravel reaches to, where the surface of the ground is about 120 feet above the Ordnance datum-line. Several were obtained from Grove Road, in Ealing, at about 80 feet above the same line, and many others in Gunnersbury Park a little above the 50-feet contour-line.

One fine specimen was found in Beaconsfield Villas, at the bottom of the gravel, a few yards south of the part represented in Fig. 13. The man that discovered it assured me that it lay directly on the surface of the London Clay. Mr. Crooke bears the same evidence to the position of the implements as Col. Lane Fox. He says that all the large ones come from the very base of the gravel. He has given me an interesting instance. He had for years watched the gravel-pit on the east side of the Brent, at Hanwell, but never could find an implement. Lately, however, the workmen dug down a little deeper than usual, and got down to the big pebbles at the base. Amongst the stones thrown out was a fine pointed implement, which is now in Mr. Crooke's collection. At Bollow Brook, near Acton, the gravel contained many fragments of wood in the same stratum in which some flint flakes were found. The only mammalian remains that have been found in the Ealing gravels are some rolled teeth of the mammoth; they occur at the base of the gravel, and are seldom met with.

The upper division of the superficial deposits that I have grouped together under the symbol *a* is made up of a series of beds differing greatly in composition. The lowest bed is often a seam of dark sandy loam, or of layers of ferruginous gravel, or of alternations of gravel with the dark loam. Very frequently it consists of gravel in a matrix of brown clay, and often there is a thin layer of sandy silt at the base.

Since the description of the sections has been written out I have found a buried forest bed in the cutting for the new railway from Turnham Green to Ealing, about 300 yards south of the east end of Ealing Common. The stumps of the trees are all small, the largest being 3 inches in diameter. They are rooted in the surface of the subangular gravel, and are all upright as they grew. The stumps are about a foot long, and buried in silt. They terminate upwards at the top of the silt, having apparently rotted off there. Above the silt there is from 2 to 4 feet of the brown clay, with patches of pebbles at its base.

The patches of gravel *a*, 2 are peculiar. They nearly always lie on an irregular surface of the beds below them, and look as if they had been dropped into the places they now occupy. Their place is often taken by an irregular line of pebbles.

The bed of clay marked *a*, 1 in the sections is the widespread deposit so extensively dug for brick-making. It is entirely unstratified. Pebbles are scattered here and there throughout it; in some parts they are scarce, in others numerous. These pebbles, by the partial denudation of the clay, are often collected into nests next the surface. The clay appears to have been originally distributed over the whole district, and to have been since partly removed by the action of the elements, the fine material of which it is composed and its position next the surface rendering it very liable to be washed away, especially on the slopes. When thin, it, with the irregular pebble bed at its base, forms the deposit named "trail" by Mr. Fisher. No organic remains nor implements have been found in the brick-clay in the Ealing district.

3. *Brentford and Neighbourhood.*—The gravels and clays at Brentford were brought before the notice of geologists in 1813, by Mr. W. K. Trimmer,* who described the occurrence of mammalian remains in the lowest division of the deposits. He stated that the uppermost beds were unfossiliferous, and that the bones occurred most plentifully at the base of the gravel, in hollows in the surface of the London Clay. In 1849 Prof. Morris† described a section exposed in the construction of a branch of the South Western Railway, at Brentford, near Kew Bridge. This locality was not far from one of the pits described by Mr. Trimmer, and Prof. Morris again notices the absence of bones and shells from the uppermost beds, and the great abundance of the bones at the base of the lower gravel. Col. Lane Fox, in the paper I have already mentioned, has described and given figures of the sections exposed between Acton Green and the Brentford Road, and has shown very clearly the position of the mammalian remains at and below the base of the gravel.

My own observations have been made in various gravel- and sand-pits in the neighbourhood of Brentford, Chiswick, and

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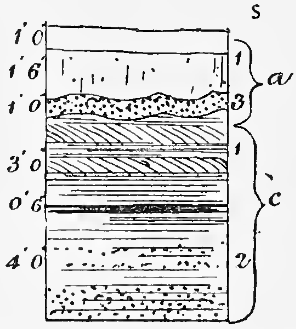
* Phil. Trans., 1813, p. 131.

† Quart. Journ. Geol. Soc., vol. vi., p. 201.

Turnham Green. Near the Kew Bridge Station of the South Western Railway the line has been widened, and a long section is exposed, showing a brown unstratified clay, with patches of pebbles at its base, overlying sand and sandy gravel. From the latter I obtained a small portion of a deer's horn. These beds were exposed in some sand-pits about 50 yards east of the station, as shown in Fig. 16.

Fig. 16.

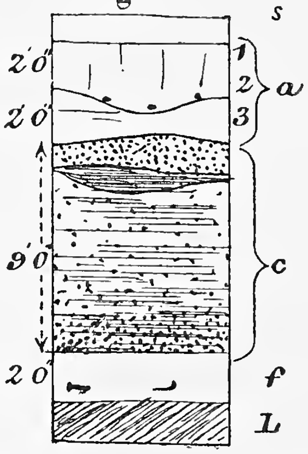


SAND-PITS NEAR KEW BRIDGE RAILWAY STATION.

s. Surface soil. *a*, 1. Unstratified brown clay. *a*, 3. Gravel in brown clay. *c*, 1. Reddish, rather coarse, sand, with oblique stratification overlying a thin seam of dark loamy clay. *c*, 2. Yellow sand overlying very sandy subangular gravel.

I watched these pits for months, but could never find a fragment of a shell in them. A pit was sunk through the gravel to the London Clay below, for the foundation of a crane, and a seam of sand was met with at the base of the gravel from which several mammalian bones were obtained, now in the possession of Mr. T. Layton, of Kew Bridge.

Fig. 17.



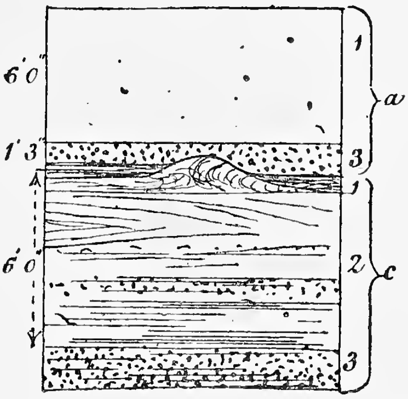
GRAVEL-PIT NEAR STYLE HALL.

s. Surface soil. *a*, 1. Brown unstratified clay. *a*, 2. Scattered pebbles along an undulating line. *a*, 3. Sandy clay. *c*. Ferruginous gravel at top, with lenticular patches of sand passing downwards into sandy subangular gravel, with a few stones of quartzite at base 7 inches diameter. *f*. Sharp sand, with bones. L. London Clay.

Nearer the river, close to Style Hall, I observed the section shown in Fig. 17.

Near to the place where this section was taken Mr. Crooke found a flint implement which had been thrown out from a depth of about 17 feet from the surface.

Fig. 18.

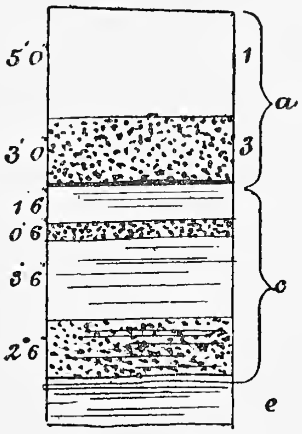


SECTION N.W. CORNER OF MR. TRIMMER'S OLD BRICK-FIELD.

a, 1. Brick-clay, removed by Mr. Trimmer. *a*, 3. Gravel in sandy clay, with some dark brown sand at base. *c*, 1. Curled yellow sand, with fragments of fresh-water shells. *c*, 2. Sand and sandy loam, with thin layers of clay, and some small angular gravel and minute fragments of shells. *c*, 3. Sandy subangular gravel.

Towards the western end of Brentford a very interesting series of pits have been opened to obtain sand and gravel. They are all on or near to the site of Mr. Trimmer's old western brick-field. Fig. 18 shows the beds exposed in a pit near the north-west corner of the old brick-field.

Fig. 19.



SECTION EAST END OF MR. TRIMMER'S OLD BRICK-FIELD.

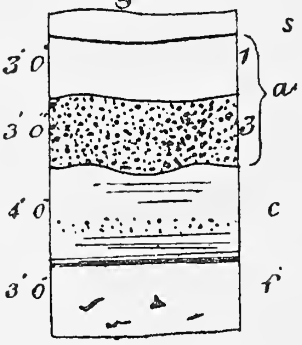
a, 1. Brick-clay, removed by Mr. Trimmer. *a*, 3. Gravel in clay. *c*. Layer of sand at top; then 6 inches of grave lin which was a broken valve of *Unio pictorum*; then sand and gravel. *e*. Yellow sand. *Unio pictorum* and *Cyclas rivicola*, common.

At the eastern end of the field the section shown in Fig. 19 was exposed.

About 100 yards north-east of the last section some pits have been sunk at various times, opposite Flora Villas, to obtain a peculiar sharp dark-yellow sand that is confined to a small area. The section exposed at one of these sand-pits is shown in Fig. 20.

There was water at the bottom of this pit. One of the workmen, at my request, pushed his spade down about 18 inches into the soft wet sand, and brought up from that depth a great number of shells, amongst which were specimens of *Unio littoralis* and *U. pictorum* with the two valves united. Many of the small cyclades also were perfect.

Fig. 20.



SECTION OPPOSITE FLORA VILLAS.

s. Surface soil. *a*, 1. Brown clay. *a*, 3. Gravel in clay. *c.* Coarse yellow sand, with a band of subangular gravel; a few broken shells in lower part. *f.* Sharp yellow sand, with mammalian remains and fresh-water shells, common. Base not reached.

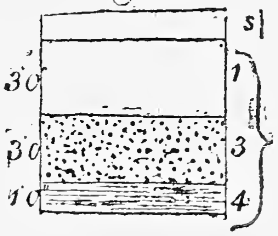
This sand is full of shells, amongst which the little *Hydrobia marginata* is abundant. Neither *Unio littoralis* nor *Hydrobia marginata* now live in England. Their shells have been found in the Lower Thames brick-earths, but were not before this discovery known to occur so high up the valley as Brentford.

The superposition of the subangular gravels to the shell-bearing sand was clear and decided. The workmen informed me that a stiff dark clay came in below the sand, and this is doubtless the London Clay. I obtained several mammalian bones from the same sand-bed, but they were soft and decomposed. The shells also were soft, and those of the *Unios* split up as they dried. Dr. Gwyn Jeffreys has kindly examined the shells from this pit, and has given me the following list of their names :—

Pisidium fontinale, var. *Henslowana*.
,, *amnicum*.
Unio pictorum and *U. littoralis*.
Bythinia tentaculata and *B. Leachii*.
Hydrobia marginata.
Valvata piscinalis and *V. spirorbis*.
Planorbis complanatus and *P. nautilens*.
Limnea peregra and *L. truncatula*.
Succinea oblonga.
Helix pulchella.
Pupa marginata.

Of the above shells the little *Hydrobia marginata* was the most abundant, though, from its small size, easily overlooked.

Fig. 21.



SECTION 30 YARDS SOUTH OF SECTION SHOWN IN FIG. 20.

s. Surface soil. *a*, 1. Unstratified brown clay. *a*, 3. Gravel in clay. *a*, 4. Laminated sandy clay or silt, with shells of *Helix pulchella*, *H. caperata*, and *Zua lubrica*.

In some other pits sunk near this the subangular gravel (*c* in section) was much more developed, and the underlying shell-bearing sand confined to a thin drifted seam. One of the sections (Fig. 21) exposed was noticeable as the only one at which I obtained any shells in the Brentford district from beds belonging to the upper division. They occurred in a thin seam of sandy silt, occupying the same position in the series as the silt enveloping the buried forest near Ealing, already described. The most abundant shell in the silt was the little *Helix pulchella*, and no aquatic species were found.

The gravels pass down below the Thames, and the bed of the river at Brentford is principally composed of reconstructed subangular gravel, with many shells of *Unio*, *Anodon*, *Nerita*, *Paludina*, *&c*.

I have the details of numerous other sections in my notes, all showing the same succession of beds, with the exception that I nowhere—excepting opposite Flora Villas—saw the bed of dark yellow sand with *Hydrobia marginata* and *Unio littoralis*. The bed of sand with mammalian remains, described by Col. Lane Fox as occurring at the base of the gravels in Brown's Orchard, may be the same, but he does not mention any fresh-water shells. As at that locality it also contained some rounded and angular pebbles, it was probably a slightly drifted bed mixed with the lower part of the sandy gravel. At Flora Villas the perfect shells of the species of *Unio* and *Pisidium*, with the two valves united, and the absence of all foreign materials, leads me to think that it was in place, and quite undisturbed since its original deposition.

I believe there are two distinct faunas represented in the lower beds, though the remains are often now found mixed together. The oldest of these two faunas is characterised by containing the remains of *Elephas antiquus*, *Rhinoceros hemitœchus*, *Hippopotamus major*, and *Cervus dama* var. *Clactoniensis*. It also contains the following species that occur in the succeeding formation:—*Equus caballus*, *Bos primigenius*, *Elephas primigenius*, *Cervus elephas*, *Ursus ferox priscus*, and *Felis leo*. This is the fauna that Col. Lane Fox found in the low-lying ground between Acton and Brentford. A single piece of the antler of a reindeer was found at the same place, but I think its presence may be explained by a slight mixture of the two faunas, as there are other signs of the partial reconstruction of the deposit.

The characteristic mammals of the second fauna are the woolly rhinoceros and the reindeer. Both faunas were originally contained in loose incoherent sands or in peat beds, and at the time of the violent outspread of the subangular gravels were more or less mixed together, and even caught up and distributed through the gravel. As they lived at the same time, one occupying a northern and the other a southern zone of country, they may sometimes have overlapped each other's range in their summer and winter migrations, as has been urged by Sir Charles Lyell and Prof. Boyd Dawkins.

The mammoth appears to have had a very wide range, and occurs in all the deposits from the Cromer forest-bed up to the diluvium, and is found associated, on the one hand, with such a southern form as *Rhinoceros etruscus*, and, on the other, with such northern forms as the reindeer and woolly rhinoceros.

The molluscan remains found in the Brentford beds favour the supposition of two faunas. In the lower beds occur *Unio littoralis* and *Hydrobia marginata*, both of which are southern mollusks not now found in England, and which lower down the Thames are found associated with the still more southern species, *Cyrena fluminalis*. In higher beds at Brentford, and mixed through the lower part of the gravel, I found a more robust form of *Unio pictorum* than occurs in the lower sands, along with a peculiarly thick form of *Cyclas rivicola*, but without any of the two southern shells. Prof. Morris, in the description of the beds near Kew Bridge, in which he found numerous remains of the reindeer, states that he could not find a fragment of *Cyrena fluminalis*, *Unio littoralis*, or *Hydrobia marginata*, although the northern species of mollusks were abundant.

The sandy subangular gravel lying above the mammaliferous sands is simply a continuation of that found at higher levels at Ealing. It only differs in containing more sand at the lower levels, and occasionally drifted shells and bones. The largest stones lie at the bottom of the gravel, as in the Ealing district: they consist of slightly worn flints, and more or less rounded stones of quartz, quartzite, and sandstone. At low levels—that is, below the 30-feet contour-line—bones of mammals are often found in this gravel, especially when, through the denudation of the older sands, it lies directly on the London Clay. These bones have been derived from the older beds. Mr. Trimmer states that the remains he found were most abundant at the base of the gravel, in hollows in the London Clay, where the deposit consisted of a heterogeneous mass of clay, sand, and gravel. In no instance, he states, have two bones been found together which were joined in the living animal. That the gravel is largely mixed with the pre-deposited sands is evident from the patches of the latter that occur in it full of the comminuted shells that are found uninjured in the undisturbed beds. Excepting these derived shells I have never seen any in the subangular gravel.

Passing on to the beds lying above the subangular gravel, we have again a most remarkable resemblance in the colour, composition, and succession of the deposits to those I have grouped under the same symbol in the Ealing district. In both cases the beds consist of unstratified brown clay, containing a few pebbles scattered throughout it and overlying patches of pebbles. Below the latter there are, occasionally preserved, remnants of a silty clay, in which I have found land shells near Brentford, and remains of a buried forest bed near Ealing, as already described. The brick-clay appears originally to have covered the whole district, but has been removed over much of the area from ancient brick-fields, the limits of which may still be traced by the abrupt slopes left at their boundaries.

III. *Mode of Formation of the Superficial Beds.*

From the time that the deposits at Finchley were described by Mr. Spencer, in 1835, they have been recognised as of Glacial age. On my first visit to Mr. Plowman's brick-field with Mr. Ives, we found pebbles of hard red and white chalk and granite, and specimens of *Gryphea incurva* in the Upper Boulder-clay. Various Liassic fossils, and fragments of granite, porphyry, micaceous sandstone, mountain limestone, coal, and oolite have been recorded from it. Mr. Searles Wood, jun., has recognised its identity with the Upper Boulder-clay of the eastern counties. He states that, even in South and Central Lincolnshire, and to the north of Gainsborough, "the material of the deposit is so identical with that on the brow of the Thames Valley that a basket of clay taken from either extremity of this area could not be distinguished, although these extremities are 140 miles apart." *

With regard to the origin of the Upper Boulder-clay there have been various theories propounded, but the majority of geologists seem to have come to the conclusion that it was spread out when the country was submerged and floating ice brought the stones contained in it from the north. Mr. James Geikie has indeed advanced the theory that the boulder-clay is the product of land-ice, and he would dispense with the action of floating ice altogether.† Even if some general arguments that have been urged against the theory of a mantle of clay having been spread over a country by the action of land-ice could be met, there are special objections to its application to the Finchley district. There, the boulder-clay caps the ridges between the valleys, and descends their slopes, lying like a cloak over loose beds of sand and gravel. Not a trace of the passage of Glacial ice is to be seen in these deposits, although the slopes are sufficiently great to have caused considerable movement of the ice down them if it had existed in the form of a glacier. The clay is thickest on the crests of the ridges, and contains there the most stones. Many of these stones have been transported from Yorkshire and Lincolnshire, and the ice that brought them, if it was land-ice, must have been sufficiently thick to cover all the low lands of Suffolk, Norfolk, and Essex, and pour over into the valley of the Thames through passes in

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* Quart. Journ. Geol. Soc., vol. xxiii., p. 395.

† See Geol. Mag., 1878, page 73, for Mr. Geikie's latest views on this question.

the northern watershed, more than 300 feet above the sea. Yet from the neighbourhood of Ely, southward, not a trace of the passage of this immense mass of ice has been recorded in the eastern counties, although the boulder-clay generally overlies loose beds of sand and clay. That in some parts glacier ice might have passed over small areas under peculiar circumstances, and melted off again, without leaving any mark on the beds below, seems probable enough; but the distance from Ely to Finchley is about 60 miles in a direct line, and that a glacier of that length, and of sufficient thickness to over-ride the watershed of the Thames Valley, should have left behind it no trace of its passage, nor any of the moraines that mark the line of retreat of other glaciers, is a theory that cannot at present be accepted, notwithstanding the faith of its talented author in its efficiency.

With the theory of floating ice—by which is meant both that of coast ice forming during the winter, breaking up in the spring, and carrying away from the shores and distributing materials frozen to it, and that of icebergs breaking off from the ends of glaciers terminating in water—the phenomena seem to be in entire accordance. Local rocks are scarce; indeed nothing is more remarkable in the Upper Boulder-clay at Finchley than the absence of fragments from the underlying London Clay. The far-travelled stones are most abundant near the top of the ridge, where floating ice was likely to ground and deposit its freight. The stones are also scattered here and there through the deposit, as if dropped at different times during its formation. There are faint traces of lines of deposition in some of the exposures, and the general absence of stratification is not more marked than it is in the Upper Thames brick-clays, the formation of which is universally ascribed to deposition from water.

The much greater abundance of transported stones on and near the ridge of the hill, and the gradual diminution in their numbers and size on the lower slopes until the clay at the bottom of the valley contains only a very few scattered pebbles, is the very opposite to what we should have expected from the action of a glacier, and just what we should expect as the result of floating ice; for the former gravitates to the bottom of the valley, and deposits there a great proportion of the stones it carries along, but the latter generally in confined waters leaves its burdens on the shores. Around the lakes, and at the heads of the fjords of Nova Scotia, there are lines of great stones left by the ice when it breaks up in the spring. Sir Roderick Murchison, in his description of the distribution of the northern drift in Russia, shows that hills and slopes 200 or 300 feet high are often covered with large blocks, whilst the valleys between them, often wide, are free from them ; and again, that the northern rocks are scattered plentifully over the plateaux, and rare on the low plains, though not entirely absent.* Prof. James Hall has described similar facts with regard to the distribution of the northern drift in America.†

As the transported stones are most abundant at Finchley at a height of about 300 feet above the sea, we must suppose —if they were brought by floating ice—that the water at the time stood sufficiently above that level to permit of the flotation of the ice, and at the same time not too high to prevent it stranding on the ridge. The most of the stones have been brought from the north, from districts where we know there were glaciers, from the marks they have left behind them ; and it is probable, therefore, that the carriers of the transported stones were icebergs. If it had been shore ice that brought them we might have expected to find a preponderance of local stones from the hills in the vicinity, instead of which there is a great abundance of far-transported materials.

For icebergs, a depth of from 50 to 100 feet at least would be required for flotation, giving about 400 feet above the sea as a probable depth of water at the time they stranded on the hills at Finchley. That this really was about the height of the water at that time is probable, for if we colour a map of the drainage area of the Thames, so as to show the ground that would be then submerged, we shall find that we have opened avenues from the north-east connecting the Finchley deposits with wide areas in that direction that are covered with the same boulder-clay.

The Upper Boulder-clay overlies at Finchley, as it does farther north, sands and gravels with much oblique stratification, generally known amongst geologists as the Middle Glacial Sands and Gravels. The gravels contain many stones not found in place in the present drainage area. Patches of a boulder-clay, known as the Lower Boulder-clay, are found beneath them, from which these foreign materials seem to have been derived. We are thus led to conclude that there was an earlier submergence, during which the Lower Boulder-clay was spread out, and that afterwards it was subjected to some action by which it was greatly denuded

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* The Geology of Russia in Europe, pp. 510, 513, and 519.

† Natural History of New York, Part 4, p. 321.

and the materials derived from it re-arranged, the clay being washed away, and the sand and gravel now constituting the Middle Sands and Gravels left behind.

The explanation I have to give of this series of events is the same that I have offered with regard to the formation of the superficial deposits of Cornwall and Devon, of the loess of the Danube and the Rhine, and of the diluvium of the South of Russia: it is—that during the Glacial period a ridge of ice, starting from Greenland, gradually advanced down the bed of the Atlantic, and ultimately blocked up the drainage of Europe as far as it extended. A continental lake was thus formed, over which floated icebergs carrying the northern drift. The first lake thus formed was suddenly discharged by the breaking away of part of the ice-barrier. During the tumultuous outpouring of the pent-up water the materials deposited during the submergence, and others derived from the pre-glacial denudation of the surface rocks, were caught up and spread out in great sheets over the lower ground, thus forming the Middle Sands and Gravels. After a time the ice-barrier was re-formed, the country again submerged, and the Upper Boulder-clay spread out.

Passing on now to the consideration of the origin of the clays and gravels at Ealing and Brentford, the first noteworthy feature is the similarity in the succession of the beds to that of the glacial deposits at Finchley. This is brought out prominently in the figures by the nearly similar symbols under which I have grouped the divisions, but not more prominently than an inspection of the beds themselves will fully justify. The boulder-clay on the top of the ridge at Finchley, full of chalk fragments, is sufficiently distinct from the brick-clay at Ealing; but when we descend the slopes at Finchley the chalky boulder-clay gradually changes to a clay without chalk, until, in the lower parts of the valleys, it becomes a brown clay, with a pebble here and there, exactly similar to the brick-clays. The Middle Sands and Gravels at Finchley only differ from the subangular gravels of Ealing and Brentford in that the former contain a larger proportion of unbroken pebbles from the Eocene beds. This is only what we might expect on the theory of origin I have offered, for Finchley is nearer to the patches of Eocene gravels left on the hill-tops, and must have been largely made up from them. The greater distance the lower gravels had been carried, their exposure to the whole sweep of the torrents rushing down the Thames Valley, and their greater admixture with broken flints derived from the west, fully account for what small amount of difference exists between the contents of the two deposits.

I should, I believe, have little difficulty in establishing the position I have taken up,—that the gravels and clays of Ealing are simply the valley representatives of the glacial beds at Finchley, and that they were deposited at the same time,—if I had only the facts of the case themselves to deal with. I have, however, a much harder task before me; I have to contend against the general acceptance by geologists of Prof. Prestwich's theory,—that the valley clays and gravels are quite distinct from and newer than the drifts on the slopes and tops of the hills. I shall, in consequence, have to examine this theory in detail.

Briefly, Prof. Prestwich's celebrated theory is,—that after the distribution of the glacial drift, the valley of the Thames was gradually excavated, and the gravels and clays deposited during the process. It implies, therefore, that the whole of the Thames Valley, from the level up to which the gravels in question extend, has been cut out since the Glacial period.

The objections that may be urged against this view are many and serious. The gravels at Ealing reach to a height of 120 feet above the river, and the width of the Thames Valley at that level is more than 9 miles. If filled to that height there would be an expanse of water stretching from the northern side of Haven Green, at Ealing, to Morden, in Surrey, with some small islands consisting of the tops of the hills near Kingston, Richmond, Wimbledon, and Putney. A few miles farther west the valley is still wider, and without hills rising above the 120-feet line. All the valley below this line is supposed to have been excavated since the distribution of the glacial drift. Col. Lane Fox has shown us that there has been no appreciable change in the course or level of the river during the last two thousand years.* The theory therefore requires that we should put back the occurrence of the Glacial period many tens—and possibly many hundreds—of thousands of years, to allow time for the excavation of the valley from the 120-feet contour-line.

When, however, we go northward, to the region of undoubted ice-action, we find everywhere traces of the recent action of the glaciating agent, and that very little change has taken place in the configuration of the country since it was covered with ice. Thus Mr. Goodchild, in his description of the glacial phenomena of the North-west of England,

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* Quart. Journ. Geol. Soc., vol. xxviii., p. 463.

states that "So little indeed has the aspect of the country changed in post-glacial times, that in many places the larger rivers are even now above the bases of the adjoining drift-mounds, whose present form can hardly be referred to any other than glacial action; and post-glacial denudation generally has effected so little that by far the greater part of the present surface-configuration has, in one way or another, resulted from the former presence of the great ice-sheet."*

Mr. D. Mackintosh, and I can quote no higher authority on the glacial beds of the West and North-west of England, in a recent lecture, at Chester, brought forward a great number of facts tending to show that the time which has elapsed since the close of the Glacial period need not have been so much as ten thousand years. Mr. Geikie and other geologists often refer to the recent appearance of the ice-markings in glaciated districts, and to the very small modification of the surface that has taken place since the outspread of the drift; but I need not refer more particularly to these opinions, as the neighbourhood of London itself affords sufficient evidence to the same effect.

At Finchley we see the Upper Boulder-clay mantling the hills and descending the slopes into the valleys, proving conclusively that the surface-features of that district, when it was spread out, were the same as they are now. The upper part of the valley of the Brent was then in existence, and at the most has not since been lowered more than 10 or 12 feet. The unconsolidated clay lies as it was deposited, and runs down to about 150 feet above the sea-level. It covers the hill continuously, and even on the steeper slopes has not been entirely denuded. Can it be for a moment considered possible that the great valley, several miles in width, has been worn out by denuding agencies below the level to which the boulder-clay reaches, and that above that line the action of the elements has been stayed, no appreciable change effected, and not even the uppermost bed of the glacial series denuded? How often must the river have changed its course, and travelled across from one side of the wide valley to the other, to remove the enormous mass that has been swept away in its excavation! Yet we are asked to believe that during all this time the denuding agencies did not operate on the uplands, but were stayed in their course, like the sun on Gibeon and the moon in the valley of Ajalon.

If we look at the country below the level of 150 feet above

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* Quart. Journ. Geol. Soc., vol. xxxi., p. 99.

the sea, and compare it with that above it, we discover no alteration in the slopes, which blend the one into the other and are continuous, so that it is impossible to distinguish that horizon amongst the contours of the country. We might have expected that there would be some distinctly marked line, when, after the boulder-clay had been spread out, the river recommenced the excavation of the valley; but there is none. None, I mean, in Nature, though in the fanciful diagrams shown in most of our geological manuals the boulder-clay is represented capping the hills and presenting an escarpment to the valleys. If these sections were true ones, or even if there were one such in Nature, my opposition to Prof. Prestwich's theory would be greatly weakened, but I can find none, excepting in our books. In every valley I have examined, including that of the Ouse, at Bedford,—often appealed to in support of Prof. Prestwich's views,—the boulder-clay is not confined to the tops of the hills, but descends the slopes as it does at Finchley.

Prof. Ramsay and others have taught us that rivers excavate their channels from the sea backwards, somewhat in the same manner as the recession of the Falls of Niagara is effected. This is proved to be the case when, in new countries, the forest is cut down and valleys begin to be formed: they commence next the rivers and progress backwards, as Sir Charles Lyell witnessed in Georgia.* I have seen similar instances in Nicaragua and Southern Missouri. But on Prof. Prestwich's theory the upper parts of the valleys must have been excavated first and the lower afterwards—the tributaries of the river before the river itself; for it is evident that the valley of the Brent, at Finchley, was formed at the time the Upper Boulder-clay and the Middle Sands and Gravels were spread out.

From the bottom of the valley near Finchley, where the road to Hendon crosses the brook, to the line of my section shown in Fig. 2 (page 317), where it crosses the same stream at Hanwell, the distance down the valley—without following the minor sinuosities of the brook—is 9 miles. The boulder-clay comes down between Finchley and Hendon to 150 feet above the Ordnance datum-line; the valley gravels and clay at Hanwell rise to about 120 feet above the same line. The stream itself falls more than 100 feet in the 9 miles, or about 11 feet per mile. At the end of the distribution of the Upper Boulder-clay, if the lower part of the valley was not excavated, the stream would only fall 30 feet between the same

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* Principles of Geology, ninth edition, p. 205.

points, or a little over 3 feet per mile, and yet, according to the theory of the post-glacial origin of the valley, have had power to spread out a continuous sheet of gravel, without at the same time being able to wash away the loose Middle Sands and Gravels with their thin covering of clay on the slopes at Finchley.

These seem to me formidable preliminary and general objections to the theory; but let us suppose that they have been overcome or explained, and try to follow in our mind what would happen if the valley below the 150-feet contour-line had been left, at the end of the Glacial period, an undulating plain, covered more or less with boulder-clay and underlying sands and gravels, through which the streams began to cut their channels. We have no great body of water to do this in the valley of the Brent below Finchley, if between Finchley and Hendon the stream was not powerful enough to wash away the glacial beds. The drainage area of the Brent is also too small a one to have supported a large stream. To excavate the wide valley from below the "Welsh Harp" to the Thames, it must therefore have often changed its course and wandered from side to side, so as to operate at different points. It would first have to clear away the glacial beds, and then cut down into the London Clay. We find, however, no bluffs in the latter such as we might expect, but a long gentle slope, from Ealing down to the stream at Hanwell, covered continuously with a mantle of gravel and clay. The superficial beds could not be deposited without break or overlap during the excavation of the valley, but only after the escarpments, formed whilst it was being cut out by the stream, had been sloped down by subaërial denudation. Every stream that is lowering its bed leaves precipitous banks, but the sides of the valleys of the Thames and its tributaries had been bevelled off into gentle slopes before the deposition of the gravels.

We may next inquire if the gravels and clays in question resemble those that are now being deposited by streams. When travelling in Russia I examined the sands and gravels of the beds of the large rivers. These rivers are frozen over for several months every year, and it seemed likely that the conditions would be somewhat similar to those that are supposed to have existed in the valley of the Thames at the close of the Glacial period. Much of the sand and gravel from the rivers of Southern Russia was used for ballasting the railways, so that I had many opportunities of examining it. I found it in every case full of river-shells, and sometimes one-half of the whole mass seemed to be shells. There were three species of *Unio* and the little *Neritina fluviatilis* especially abundant. When gravel from the bed of the Thames is now dredged up, it also contains many shells. In the sections exposed in 1875, during the extension of the embankment west of the Houses of Parliament, the bed of the river was seen to be crowded with shells, principally *Unio pictorum* and *Bithynia tentaculata*. Now, it is another objection to Prof. Prestwich's theory that the gravels that he supposes were deposited in the old river do not contain river-shells. I never saw even the fragment of a recent species of river mollusk in all the sections I have examined around Ealing, and none have been recorded by others. At Brentford, when we get down nearly to the level of the present river, drifted shells are often met with in the gravel, but they have evidently been obtained from the older sands of the pre-diluvial river, and do not belong to the time of the deposition of the gravels.

The brick-clay that overlies the gravel is considered by Prof. Prestwich to be inundation mud. We have silty deposits now forming or lately formed on the low flats adjoining the Thames: they consist of dark blue clay, with remains of vegetation and land and fresh-water shells. Sometimes peat beds occur. The brick-clay, on the other hand, is a homogeneous unstratified brown clay, without any organic remains. We have thus the formation of two deposits, the valley gravels and the brick-clays, ascribed to river action, and yet, in all the sections exposed, differing in most important particulars from the sediments now being deposited by rivers.

The absence of these remains from some of the beds might be explained, but it seems impossible to believe that the excavation of the valley could have progressed during several thousands of years, and the river everywhere have left behind it thick deposits of gravel and clay, without any river shells. The preservation of the fragmentary shells at Brentford, derived from the older sands, proves that their absence is not due to their destruction since the outspread of the gravels.

The structure of the deposits is opposed to the origin claimed for them. A continuous sheet of gravel covered by another of clay, spread over the slopes of a valley, is not what we should have expected. The river, in wandering from side to side of the wide valley it was wearing down, should often have cut into and truncated the deposits it had left at a higher level, and could not have exactly joined on its later sediments to its earlier ones. Nor does river action explain the angular and subangular character of the gravel, nor the presence of the largest stones at the base of the deposit.

If, then, the physical evidence is so opposed to the theory, why should it be upheld? Only, so far as I can understand, because the conclusion is started with that palæolithic man is post-glacial, and only by the theory of the post-glacial excavation of the southern valleys can the post-glacial age of palæolithic man be defended. But why should we commence with an assumption like this? At Bedford the evidence is a repetition of that which we have in the Thames Valley, and I have shown in a former paper that at Hoxne* there is no proof of the post-glacial age of palæolithic man. The palæontological evidence is also hostile to Prof. Prestwich's theory. At low levels in the Lower Thames Brick-earths the remains of *Rhinoceros megarhinus*, *R. hemitæchus*, and *Elephas antiquus* occur, all of which belong to the oldest pleistocene fauna of Europe. At much higher levels the stone-implements of palæolithic man are found, along with the remains of the mammoth and the woolly rhinoceros. On the supposition that the remains have been entombed during the gradual excavation of the valley, the latter would be the oldest of the two faunas. The great mammals, the bones of which are found so plentifully at Brentford, would have lived long after the men who left their chipped flints at the much higher levels at Ealing. The affinities of the fauna found in the Lower Thames Brick-earths with that of the Cromer forest-bed are much closer than that found in the gravels. And in the valley of the Rhine the deposits, with the woolly rhinoceros and the mammoth, overlie those containing *Elephas antiquus*, whilst they ought, if Prof. Prestwich's theory was correct, to underlie them.

Mr. Alfred Tylor has propounded the theory that the gravels were spread out after the excavation of the valleys to their present depth. He supposes that the Glacial period was succeeded by a Pluvial period marked by an enormous rainfall, so that the river-floods reached far above their present limits, and deposited gravels and clays high up the slopes of the valleys. The highest beds may, he thinks, have been formed by rain-wash. He has also suggested that the gorging by ice of the mouths of the Somme, the

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* Quart. Journ. of Science, July, 1876.

Seine, and the Thames, may have assisted in the production of some of the gravel-beds.*

I am so much indebted to the suggestive papers on the superficial deposits, by Mr. Tylor, that I regret that I cannot accept his theory as satisfactory. The gravels and clays of the lower slopes do not differ from those of the higher ones, and it is not probable that rain-wash would produce the same effects as a flooded river. The fact that the glacial deposits at Finchley remain intact nearly to the level of the present brooks shows that they have not been exposed to the action of any extraordinary denuding agents, and floods and rain-wash that did not remove them could not spread out great sheets of gravel lower down the valley. The gorging of rivers by ice is doubtless a true cause of great floods, but water so dammed back could not reach to the levels we require, and would only deposit silt, and not spread out gravels. The theory also offers no explanation of the close similarity in the succession of the glacial and the valley deposits, nor of the resemblance of the Middle Glacial Sands and Gravels to the subangular gravels of the valleys.

The theory that I offer in the place of these is in complete harmony with both the physical and the palæontological evidence. It asks for no other agency than that concerned in the outspread of the glacial beds, and explains the similarity of those to the valley deposits by recognising in the latter the representatives of the glacial clays and gravels of the hills. It offers no opposition to the evidence of the comparatively short time that has elapsed since the Glacial period, as it admits the pre-glacial age of the southern valleys as well as that of the northern ones. It does not require us to believe that in the one case the configuration of the country has been completely changed, whilst in the other it has remained unaltered. It affords an explanation of the disappearance of many large animals that formerly abounded in Northern and Central Europe, and accounts for the multitude of their remains at a particular horizon. A new light is thrown by it on the cause of the present distribution of animals and plants. The strongly-marked break that exists between the times of palæolithic and neolithic man finds in it a solution, and the traditions of a great deluge a foundation.

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* Quart. Journ. Geol. Soc., vol. xxv., p. 10, and Geol. Mag., 1875, p. 437.

TABULAR VIEW OF THE RELATION OF THE SUPERFICIAL DEPOSITS OF THE THAMES VALLEY TO THE GLACIAL PERIOD.

F. LOWER BRICK-EARTHS. OLDER MAMMALIAN FAUNA	Atlantic ice reaches the North-west coast of Scotland. Straits of Dover cut through by the drainage being turned southward.
E. NEWER MAMMALIAN FAUNA	Atlantic ice progressing slowly southward. River-beds lowered through the depression of the sea-level.
D. LOWER DILUVIUM	Atlantic ice reaches the Western end of the Pyrenees. First European Lake, and great destruction of Mammalian life.
C. MIDDLE SANDS AND GRAVELS	Break in the ice-barrier. Sudden and torrential discharge of the water of the European Lake.
B. LAND SURFACE	Gap in the ice-barrier still open.
A. UPPER DILUVIUM	Gap in ice-barrier closed up again. Second European Lake gradually lowered by the cutting through of a channel between the Black Sea and the Mediterranean.

I have described the main features of my theory in several papers published in this Journal and in the "Journal of the Geological Society, and I shall now seek to apply it more particularly to the deposits described in this paper. To render the subject as clear as possible I have drawn up the preceding tabular statement, showing the relation that the deposits of the Thames Valley bear to the Glacial period.

F. *Lower Brick-Earths. Older Mammalian Fauna.*—The Lower Brick-Earths are only represented in the Brentford district by the small undisturbed patch that I found opposite Flora Villas, and by more or less drifted remains of the older mammalian fauna. They are more fully developed in the lower part of the Thames Valley at Erith, Crayford, Ilford, and Grays Thurrock. The depositions of these brick-earths in the south-eastern counties was preceded by a land surface represented in the rootlet beds of Kessingland, Hasborough, and Runton.

The fauna found in the deposits is closely related to the still older Cromer Forest-bed, by the occurrence of *Elephas priscus*, *E. antiquus*, *Rhinoceros megarhinus*, *R. hemitæchus*, and *Cervis dama*, var. *Clactoniensis*, amongst the mammals, and *C. fluminalis* and *Hydrobia marginata* amongst the mollusks. *R. etruscus*, another Cromer Forest species, has been found in beds, apparently of the same age as our Lower-Brick earths, in the valley of the Rhine, along with *Elephas antiquus*.

In the Thames Valley there are few deposits of this age that were not more or less affected by the cataclysm that spread out the middle sands and gravels. The small patch found opposite Flora Villas at Brentford, some of the lowest beds in the Lea Valley at Clapton, and the Grays Thurrock brick-earths are the only instances I know of where the sands appear to be quite undisturbed. The mammalian beds at Ilford, Crayford, and Erith are, I think, all slightly drifted, and contain some admixture of remains belonging to a later date.

Before the Atlantic ice reached the north-west coast of Scotland, the Straits of Dover do not appear to have been cut through, and the German Ocean only communicated with the Atlantic northward. By the arrival of the ice from the north-westward on the coast of Scotland, the water of the German Ocean area was dammed back, and rose until it flowed across the neck of land joining England to the Continent. A lake was thus formed draining to the southward, which was gradually lowered by the cutting through of the Straits of Dover.

This was probably the time of the greatest range of the Hippopotamus which ascended the great river to the Germanic lake. Possibly the river, by the lowering of the sea level, flowed as far southward as the Bay of Biscay, and if, as Dr. Gwyn Jeffreys supposes, the Mediterranean then communicated with the Atlantic to the north of the Pyrenees, the hippopotamus would not have to travel far from the Mediterranean area, where we know it abounded, to the mouth of the great river.

Palæolithic man appears to have penetrated into Britain at this stage, as his implements are found associated with the older mammalian fauna in the caves of the west.

E. *Newer Mammalian Fauna.*—The last stage gradually merged into the present one by the climate—affected by the continued advance of the northern ice—becoming too cold for the southern fauna, which retired further south; its place being taken by the mammoth, the woolly rhinoceros, and the reindeer. The great ox, the wild horse, the bison, the red deer, the Irish deer, the lion, the bears, and the hyænas appear to have lasted on from the former period, and the three first to have abounded. The hippopotamus still occasionally came up the great river in the early part of the period. The mammoth, which during the time of the deposition of the brick-earths had reached as far south as the Thames in its winter migrations, now became a more permanent resident. The most characteristic mammals are the woolly rhinoceros and the reindeer, though even they, in the early part of the period, must have overlapped in their winter migrations the range of the more southern animals in their summer wanderings. The reindeer is absent from the Lower Brick-earths, and from the beds containing the older fauna at Brentford, excepting a small portion of an antler found by Colonel Lane Fox, which may have been drifted at the time of the debacle, when the remains of the two faunas were partially mixed together. In the section at Brentford, described by Prof. Morris, the reindeer was abundant, and was associated with the mammoth and woolly rhinoceros. Not a fragment of the southern shells, *Hydrobia marginata*, *Unio littoralis*, and *Cyrena fluminalis*, occurred at this spot, whilst in the older sands in the same district the two first-named are abundant.

Mr. Godwin Austen has described peat and sedimentary beds of this age underlying the gravels at Pease Marsh, near Guildford.* In these the nearly perfect skeleton of a mammoth was found. Remains of trees occurred, rooted in the underlying Neocomian clay, the whole being overlaid by the subangular valley gravels.

To this period belongs most of the flint implements of the Thames Valley gravels. When found in the gravels they are mostly worn by rolling, but at Acton in the deposit, described by Colonel Lane Fox, below the gravel they were quite unrolled just as Mr. Godwin Austen found the uninjured bones of the mammoth in a similar situation at Pease Marsh.

The localities where the flint-implements occur in most abundance are just such as might have been chosen by palæolithic man if the configuration of the country was then the same as it is now. Perhaps the most conspicuous instance of this is the Milford Hill Station near Salisbury. Milford Hill is described by Dr. Blackmore as forming a buttress between the two valleys of the Bourne and the Avon above their junction. It is separated from the main tract of high land behind by a transverse valley, thirty feet in depth, so that it forms an isolated hill.† Such a position was an admirable one for a rude tribe dependent for their living on hunting and fishing; giving them the command of two valleys, and being easily defended from attack. In our own district it is noticeable that the implements are found in greatest abundance in situations presenting somewhat similar features. Thus at and near Mill Hill at Acton numerous flint-implements have been discovered in the lower part of the gravel. Mill Hill lies between Acton Brook and Bollow Brook, and to the south, slopes rapidly down towards the low flat bordering the Thames. The locality in Gunnersbury Park, where Mr. Crooke obtained many implements, is on the crest of a similar slope, and immediately to the east of another small brook. In such situations palæolithic man might easily secure himself from the great wild beasts that frequented the lower ground near the river; he held a position of defence with regard to other tribes, and the brooks in the vicinity would probably be frequented by many of the smaller animals the objects of his chase. Mr. Crooke found numerous semi-fossilised pieces of wood along with flint implements in the gravel on the east side of Bollow Brook, and it seems not unlikely that these were the remains of a stockade that had surrounded the old palæolithic station.

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* Quart. Journ. Geol. Soc., vol. xi., p. 112.

† *Ibid.*, vol. xxi., p. 250.

The significance of these facts is entirely lost if we have to suppose that the configuration of the prediluvial land was not the same as it is now.

D. *Lower Diluvium.*—During the last stage I suppose that the Atlantic ice had slowly progressed southward, the climate of the temperate zone becoming every year more vigorous in consequence. In those last days of palæolithic man the reindeer lived the whole year through in Southern France, and the musk sheep reached in its migrations the same low latitude.

The readers of this journal who have followed me in my former papers will recollect that I have already given some explanation of the cause of the accumulation and of the mode of advance of the Atlantic ice. I feel, however, that I have not dealt with this important phase of the question with the fulness it deserves as lying at the very foundation of the theory. I must ask my readers to bear with me whilst I speak of this advance of the Atlantic ice as a fact, fully recognising my obligation in so doing to address myself at the first opportunity to the solution of the physical difficulties of the problem. They evidently weigh heavily with many scientific authorities without being actually defined. They are indeed more fanciful than real, more a failure of the imagination to conceive a state of things so different from that now existing than any clearly perceived objection; but reason has a stronger pinion and a more piercing eye than imagination, and leads the way to regions that would remain unknown to her sister excepting for her guidance.

I suppose that the Atlantic ice at last reached the Pyrenees, or coalesced with the ice flowing from that mountain-chain, closing the outlet of the northern drainage of the Continent to the Atlantic. The gap between the western Alps and the eastern Pyrenees was also filled with ice, and Behring's Straits similarly closed. The communication between the Black Sea and the Mediterranean not having yet been cut through, the waters began to rise over all the northern parts of Asia and Europe. The bulky mammoths and rhinoceroses frequenting the low plains would probably be the first to be overwhelmed. Some might find a temporary refuge on low isolated hills only to be overtaken by the rising water. The higher ranges of hills would form the chief places of refuge, but many of these, especially in western Europe, were covered with ice. The Altai mountains in Central Asia, the Urals, the Caucasus, the Carpathians, and the mountains of Armenia were likely the ranges on which many animals and plants found security, and which formed centres of dispersion in post-diluvial times. The ethnologist now finds on some of these mountains remnants of diverse peoples, and the naturalist animals and plants preserved there and absent from intermediate tracts of country.

The non-occurrence of human bones along with the flint-implements and the remains of the great mammals in the British Isles suggests that the palæolithic people of these parts escaped, at least for a time, from the rising waters. Their numbers may have been few, and they may have lingered for a time, and more gradually have died out on the hill tops to which they fled, or they may have taken to canoes and rafts and tried to reach more southern shores across the great waste of water that surrounded them.

Some of the palæolithic people of Central Europe may have escaped into Italy, but that country was probably inhabited by a different and a hostile race, the ancestors of neolithic man who spread over central Europe when the great floods abated; finding the land covered with a fertile soil, freed from the great carnivores and without inhabitants to contest their occupation of the country. Possibly the ancestors of the Basques lived to the south of the Pyrenees in the Glacial epoch, and spread in like manner into Southern France in early post-diluvial times.

C. *Middle Sands and Gravels.*—The Middle Sands and Gravels were spread out, according to this theory, by the sudden and torrential discharge of the pent-up waters, caused by the breaking away of the ice dam between the Pyrenees and the Alps. The course of the flood across the south of England was from the north until the water was lowered, and the rushing torrents confined to the valleys, the direction of which they then followed. In the Thames Valley, as soon as the height of the water fell to the level of the water-shed to the north, the rush would be down the valley or from the west; though until it was lowered to below 200 feet above the sea, some of the flood would escape southward through the low pass between the Wey and the Arun.

The materials that the flood would find on the surface, in a loose and unconsolidated state, would be varied and numerous. Over all the chalk area there would be the flints left during the long subaërial denudation to which the chalk had been subjected in pre-glacial times. There would be the patches of Bagshot sands, only small remnants of which are now left on some of the hills, and other tertiary sands and gravels where they came to the surface. To these local materials would be added all those brought by floating ice before the first lake was discharged, and constituting the Lower Boulder-clay.

These materials would be caught up by the mighty flood, swept away, mingled together, and ultimately spread out over the lower lands, when the violence of the debacle abated. If it had not been checked it would have probably carried all its spoil into the sea. The lessening of the fury of the flood, when it reached a level of about 200 feet—and still more below 100 feet—above the sea, must have been due to the outlet of the water being contracted or obstructed. The channel across Languedoc to the Mediterranean may not have been broad enough for the rapid discharge of the great lake below 200 feet above the sea. Whatever was the cause, the evidence shows that the violence of the flood was checked, though not entirely stopped, and the materials that had been swept off from the upper slopes were spread out over the lower ground.

At Finchley we find the Middle Sands and Gravels, and even patches of the Lower Boulder-clay, at heights from which they are entirely absent on the flanks of the hills bordering the Thames Valley, as, for instance, the southern slopes of Harrow and Hampstead Hills. The preservation of these deposits at Finchley is due to the fact that to the north and north-west there is a ridge of high land rising to over 400 feet above the sea, which would protect them from the violence of the flood when it was moving from the northward; and when the water was lowered to below 400 feet the current would be from the westward, and Finchley would be completely sheltered from it by the high chalk ridge running south-westward from Tring. The southern slopes of Harrow and Hampstead would, on the other hand, be exposed to the full violence of the torrent rushing down the Thames Valley, and if, as is likely, gravels had been deposited on them whilst the current was from the north, they must have been utterly washed away later on when it was from the west.

Below 200 feet above the sea, when the violence of the flood was somewhat checked, the outspread of the materials it was hurrying along began; at first in sheltered places; then, as the water was still further lowered and its velocity still more lessened, more generally and continuously.

The level at which these deposits began to be the rule, instead of the exception, was at about 120 feet above the Ordnance datum-line. Below this level they were spread out over all the flat or gently sloping ground, and only on the steeper gradients did not find a lodgment. As the water still further fell it drained into the old pre-diluvial smaller valleys, and in some cases the gravels have been swept out of these for some distance above the levels of the present brooks. The gravel and sand being all in motion together, there was a rough sorting of the materials, according to their weight; the largest and heaviest stones found their way to the bottom, whilst the sand was more generally distributed near and at the top of the gravel.

Pre-diluvial man had left his stone-implements around his old settlements, and many of these were mixed up with the materials brought down from the west by the flood and deposited with them, the larger flint-implements sinking to the bottom along with stones of the same size. At still lower levels many of the implements were not moved far from where they had been left on the surface, and in some instances were not moved at all. Above the 40-feet contour-line the bones of the large mammals that may have been lying on the surface deposits before the debacle, have been mostly swept away, excepting where preserved in hollows from the violence of the flood. A few of the large heavy teeth of the mammoth have been found in the gravels of the Ealing district, but the lighter bones are absent. In 1875, in digging gravel at the site of the New Museum of Natural History, at Kensington, the tooth of a mammoth was found at the base of the gravel, within 6 inches of the surface of the London Clay. No other bones occurred with it. A little farther west, in digging the foundations for houses in Cromwell Road, many fragments of bones belonging to the great ox, the red deer, and the mammoth were found near the top of the gravel, but none of the heavier teeth. It would appear from this that the remains were, like the stones of the gravel, deposited according to their specific gravity.

Below the 40-feet contour-line, in the Brentford district, mammalian remains are most abundant. The reason why they should have been preserved there, is apparent when we look at a map showing the contour-lines of the district. To the west of Brentford, a spur of high land, all above 70 feet above the sea-level, comes down from Southall, as far as Hounslow. To the east of this the 50-feet contour-line runs to the north from Brentford, up past East Acton, towards Wormwood Scrubs, forming an inlet protected from the violence of the western flood. It is in this sheltered bay that the mammalian remains, so abundant between East Acton and Brentford, have been preserved. Mr. Alfred Tylor, some years ago, drew attention to the fact that all the mammaliferous deposits of the Thames Valley occur in situations similarly protected by escarpments to the westward; and my experience has been to the same effect.

We have thus, on the one hand, deposits completely broken up by the violence of the flood, and, on the other, completely preserved from it. All the gradations between these two extremes should and do exist. In some the remains have been broken and mixed through the gravel, and even sorted according to their specific gravity. In others they have been drifted for a short distance, and the two mammalian faunas are mixed together without the bones being broken or much rolled. In others they have been only a little drifted from their original position; and in others, again, remain as they were first deposited, the bones of the same animal lying together.

Much confusion must thus arise, and much difficulty in determining the succession of the beds. The difficulty is often increased when the remains lie at the junction of tributaries with the main stream. In such cases it has sometimes happened that the water filling the secondary valleys has been pounded back by the greater flood coming down the main one, so that, after the gravels have been spread out by the latter, the contents of the former have been washed over them. And thus, pre-diluvial mammaliferous sands lying in the tributary valleys, and protected from the violence of the flood coming down the principal channel, have, by the change in the direction of the currents during the falling of the water-level, become interstratified with—or even shifted unto the top of—much newer deposits. The only way to guard against being deceived by instances of this kind is not to accept any evidence of superposition as conclusive where the bones of the same animals do not lie near together, where the lamellibranchiate shells have not their two valves united, or where there are other signs of the deposit being a drifted one.

In Norfolk and Suffolk the Middle Sands and Gravels often contain fragments of marine shells derived from the Weybourne Sands and other beds. In Yorkshire they are known as the Hessle Sands and Gravels.

B. *Land Surface.*—How long the gap in the ice-barrier remained open after the discharge of the water of the first lake it is impossible to say. We have evidence, however, that it was long enough for some of the land-shells to occupy the surface again, and also, as the buried forest-bed at Ealing witnesses, for small trees to grow up. Until very recently I had not obtained this evidence, and was under the impression that the lake had been quickly re-formed. The forest-bed at Ealing shows clearly, however, that some years at least must have elapsed before this took place. I have found land-shells at Brentford, and near Bletchley, in Buckinghamshire, at this horizon.

The Rev. H. M. De la Condamine described, in 1853, a deposit containing land and fresh-water shells on the top of the Middle Sands and Gravels, in the valley of the Ouse, between St. Ives and Huntingdon.* At Crayford there is a grey sandy clay containing shells of the fresh-water mollusks, *Planorbis*, *Bythinia*, *Lymnea*, and *Anodon*. The *Anodon* has the two valves united. The clay with these shells overlies the mammaliferous gravels and underlies the deposits that appear to be the representatives of the Upper Boulder-clay, so that I think it must belong to this stage. It was probably at this time that the land shells contained in the Upper Loess and Diluvium multiplied greatly, and occupied the land, many of their natural enemies and competitors having been destroyed by the flood.

It seems not impossible that remnants of palæolithic tribes or some of the great extinct mammals might have escaped destruction at the time of the rising of the waters of the first lake, and have spread over the area again before it was again submerged. I have, however, so far found no evidence whatever that they did so. The scarcity, if not absolute non-existence, of the bones of the species of deer, horse, and ox, that we know were not exterminated, makes it probable that the gap in the ice-barrier did not remain many years open.

A. *Upper Diluvium.*—The Upper Diluvium, under which name I include the Upper Boulder-clay, the Upper Loess, the Hessle Clay, and the Upper Brick-clays, is the most widespread and the best preserved of all the glacial deposits. The gap in the ice-barrier had been closed and the lake re-formed, but this time the water was not suddenly discharged, and gradually cut out a channel between the Black Sea and the Mediterranean. There are several facts pointing to the probability that it was the channel of the

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* Quart. Journ. Geol. Soc., vol. ix., p. 271.

Bosphorus that was thus formed, and not the Dardanelles. The deposits that were spread out during the continuance of the second lake were not subjected to the action of any sudden and tumultuous outpouring of the water, but only to its gradual subsidence. They have been but little denuded, and are still to be found nearly everywhere over the area that was submerged.

During the rise, greatest extension, and subsidence of the water, sediments, varying greatly in character, were formed. The earliest of all in the Ealing district—the first sign of the second rise of the flood—is the bed of silt enveloping the stumps of the small trees of the buried forest. The similar silty clays at Brentford and elsewhere, with land shells, mark the same event. There are, in the alternations of gravel and sandy clay in some of the sections at Ealing, signs of small oscillations of the level of the water, during which the subangular gravels were partly denuded, and furnished some of the materials for the new deposits. Above this we have a bed of clay, showing a greater depth of water, and stones scattered through it, indicating the agency of floating ice.

In addition to the single pebbles, and sometimes larger stones, there are patches of gravel and sand that appear to have been dropped in a frozen state into the bed of clay as it was forming. They are often angular, and show the lines of their original stratification, now lying at all angles or turned completely on end. They occur still more frequently in the Upper Boulder-clay of Norfolk and Suffolk than in the clay of the Thames Valley, and form one of the many evidences of the identity of the two deposits. The most probable explanation of their presence in the clay is, that the rising of the second lake took place wholly or partly in the winter season, and during an extreme frost; that ice was continually forming along the ever-widening shore, and, being broken up and floated off by the rising water, bearing masses of frozen gravel and sand with it; that thus a wide area of the lake was covered with gravel- and sand-laden ice, and that, on the melting of the latter, the frozen masses fell to the bottom and were imbedded in the clay.

Very little *detritus* that can be traced to a northern source has been found south of the Thames, and this has led some geologists to hold the opinion that the country south of the river was not submerged when the Upper Boulder-clay was spread out. It is difficult to believe that this can have been the case, when we find the drift covering the northern brow of the valley. Prof. Prestwich has also observed transported rocks on the top of Well Hill, in Kent, at 600 feet above the sea. Along with large rolled flints were found a few fragments of chert and ragstone, which he refers to the Lower Greensand of the Sevenoaks range, some six miles further south, separated by the broad and deep valley of Holmsdale.* Mr. Whitaker has also seen lumps of hand-chalk, in Kent, at a high level, on Crocken Hill, eastward of St. Mary's Cray;† and Prof. Morris has informed me that there are patches of the loess, with its characteristic shells, preserved in fissures of the chalk at Bensted's Quarry, near Maidstone.

We have an obvious explanation of the nearly total absence of northern drift south of the Thames in the theory that the Upper Diluvium was distributed by ice, floating over a great lake draining to the Mediterranean by way of the Black Sea; for the currents from the South of England would flow to the north of east, to get round the Hartz Mountains, and might be deflected still more to the north by those from the valley of the Rhine, and from the area of the English Channel.

III. ON THE POSSIBILITY OF EXPLAINING PAST CHANGES IN THE UNIVERSE BY CAUSES AT PRESENT IN OPERATION.

By S. Tolver Preston.

ANY attempt to explain the phenomena of Nature by the recognised working of physical causation has been invariably welcomed. Thus the explanation of geological changes through the influence of time, by the recognised working of natural causes, is now generally accepted with satisfaction, and the idea of cataclysms or catastrophes has been abandoned. Might not the same thing apply to cosmical changes (or to changes in the Universe), or would not an attempt to explain them by the

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* Geological Magazine, October, 1874.
† Geology of London, p. 52.

recognised action of physical causation be as desirable as in the case of geological changes?

One insuperable barrier in the way of explaining past changes in the Universe, by causes at present in operation, has been the supposition—in the absence of any explanation of the mechanism of gravity—that the range of gravity extends to indefinite distances, and consequently that every portion of the Universe tends to agglomerate or collect together into one mass, whereby all conditions of stability or permanence are excluded. Another difficulty has been to conceive how the energy (in the form of heat) dissipated by matter in the ether of space could ever again be made available or re-transferred to matter. Now, in the first place, it has never been *proved* that the range of gravity extends to indefinite distances, and, on examination, conclusions so incongruous are involved in this assumption that they themselves render the assumption unlikely. Thus imagine the incongruity of the idea of an *unstable* Universe tending to agglomerate into one (perhaps infinite) mass, gravity itself becoming (in the limit) infinite, so that the conditions of life would be rendered utterly impossible. Can it appear reasonable to conclude that physical causation should be so adapted as inevitably (in time) to produce such a state of things as this, or to involve instability and confusion?

According to the only physical theory of gravitation which has yet withstood the test of criticism (viz., that of Le Sage*), and which has been received with some favour by some of the most eminent physicists, it is *certain* that the range of gravity cannot extend to indefinite distances. It is therefore quite possible that the range of gravity may not extend to the stellar distances so that the stars do not gravitate towards each other.† This would evidently be the condition required to satisfy the very important point of *stability* in the Universe. The range through which gravity has been actually observed to prevail is known to be infinitesimal compared with the stellar distances, so that the range of gravity may perfectly well lie within these limits. Thus the condition for *stability* is the first important

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* In three papers published in the Philosophical Magazine (September and November, 1877, and February, 1878) it has been my object to show how Le Sage's original theory may be modified, and certain difficulties in the way of its acceptance (in its original form) removed by the light of the modern investigations connected with the kinetic theory of gases. A fourth paper (after the MS. of the present paper was written) has also been published in the Philosophical Magazine for April last.

† Of course we do not allude to *double* stars, in close range.

conclusion following on the recognition of the *limited* range to gravity required by the physical theory.

Since an explanation of the mechanism of gravity is attracting attention at the present time, Le Sage's theory (or a modified form of it) being apparently the only conceivable one that can satisfy the various conditions of the problem, it therefore behoves us to inquire—as a point of great interest, and for the furtherance of scientific truth—what modification the acceptance of this explanation of the mechanism of gravitation (and the important connected inference that its range is limited) would make in the views as to the operation of physical causation in the Universe; and this inquiry would appear to be all the more desirable in view of the accepted fact that gravitation is really the most important physical agency in the Universe, and therefore any modified views regarding its nature and range of action would naturally entail important deductions and modified views in regard to the working of natural phenomena.

The second fundamental conclusion that follows by the recognition that the range of gravity is *limited*, and is within the stellar distances, is that the stars must be moving in *straight lines*, and not in orbits, as supposed on the assumption of an indefinite range to gravity. It follows therefore, by the ordinary laws of probabilities, that if a star continue to move in a straight line for a sufficient time it must inevitably *come into collision* with another star situated somewhere in the line of the star's proper motion; or that, in general, collisions among the stars (in a state of proper motion among each other in straight lines) must be inevitable. This, therefore, may be regarded as the third fundamental conclusion that follows by the recognition of a limited range to gravity. In the "Quarterly Journal of Science" for July, 1877,* is a paper by Mr. James Croll, in which he calls attention to the fact that some explanation is required in order to bring the age of the sun's heat up to the length of time required by the teaching of the facts of geology, and has suggested the collision of matter (in a state of proper motion) for that purpose. The mere approach of the matter forming the sun under the action of gravity without a collision (due to a previously existing proper motion) appears to be demonstrably insufficient to account for the age of the sun's heat. It is therefore so far satisfactory to observe that the inference of the collision of

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* Also Phil. Mag., 1868.

masses (in a state of proper motion) arrived at on independent grounds by Mr. Croll is precisely the same conclusion that necessarily follows from the deduction that the range of gravity is *limited*, and therefore that the stars necessarily move among each other in *straight lines*, whereby, in the natural course of things, collisions are rendered inevitable. The eventuality of collisions among the stars is also treated of in a paper by Mr. Johnstone Stoney, "On the Physical Constitution of the Sun and Stars" (Proc. Roy. Soc., 1868-69).

The question of difficulty that remains would be to explain how the colliding matter of the Universe is separated and prevented from aggregating together to an indefinite extent. The *limit* to the range of gravity and the *limit* to the intensity of gravity required by the physical theory would appear to have a bearing here. It may be observed that there are two opposing agencies, the expansive action of heat and the contractive action of gravity. By the continued aggregation of matter the expansive action of heat increases, while the intensity of the contractive action of gravity attains a final limit. It would therefore appear reasonable to conclude that a balance would naturally set in, at which the expansive action of heat would compensate the contractive action of gravity, and thus a natural limit to the aggregation of matter would be reached. Thus the general result led up to by these conclusions would appear to be that the parts of the Universe are in motion among themselves in straight lines (much in analogy to the molecules of a gas, as far as regards character of motion); that, under the collisions that occur, a general balance is maintained in the quantity of heat, and general distribution and state of aggregation of the matter; that some stellar suns are in the act of cooling down, while others are renovated by fresh collisions, changes continually occurring in parts of the Universe, but the whole remaining unchanged in character. That sudden developments of heat among the stars and (as it were) the flashing out of suns do occur at times, is a notorious fact in astronomy. It should be noted that the above fundamental deduction, that the parts of the Universe are moving in straight lines (which entails collisions), cannot be regarded as a mere speculation, but is a necessary inference following on the recognition that the range of gravity is *limited*, and on the fact that the stars are actually observed to possess proper motions in various directions. The deduction that the range of gravity is *limited* is itself a necessary consequence of the one explanation of the mechanism of gravity that has received support by competent judges.

From the deduction that the stars are moving in *straight lines* in various directions, the analogy (as regards character of motion) with a gas becomes natural and inevitable; for dynamical principles are independent of *size* or relative scale, or it is the same as if we were dealing with molecules observed to have a proper motion among each other in various directions. The analogy of a gas presents itself, therefore, rather as a necessary consequence, not as a speculation.* The *degree* of aggregation of the separate parts of the Universe moving among each other in straight lines would therefore appear to be necessarily dependent on the mean velocity of proper motion, just as the degree of aggregation of the components of the molecules of a compound gas depends on the mean velocity of their proper motion. If the velocity of the compound molecules of the gas be increased (which is synonymous with what is called "raising the temperature") they begin to separate into their components (or to become "dissociated"), and if the molecules have a very high complexity the extent to which this separation occurs (*i.e.*, the final *degree* of aggregation) depends on the temperature, or on the *velocity* of the molecules. If the velocity be very high (by a high "temperature") the molecules split up—"dissociate"—into their *ultimate* components. By gradually lowering the temperature (velocity) the converse process may take place, or the components may gradually aggregate together to form massive molecules. This analogy is applicable also to the larger scale parts of the Universe moving in straight lines among each other, since dynamical principles are independent of scale. Such an excessive velocity of the component parts of the Universe (the stars) among each other is quite conceivable, at which the whole would break up into *single* molecules. By a lower velocity these molecules collect together under

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* The *relative* velocity of the stellar masses (meaning by this the relative time taken by them to traverse a distance equal to their own diameters) is almost indefinitely slow compared with the case of the molecules of gases, which traverse a distance equal to many millions of times their own diameters in a single second. The absolute velocity of the stellar masses, however (as far as observed), is very great compared with that of the molecules of gases, and consequently the heat developed at their encounters would be very great. On account of the large multiple the distance of the stellar masses is of their diameters, they would, no doubt, traverse vast distances before an encounter, or their "mean length of path" would be very great; and (considering the relative length of time a stellar mass takes to traverse a distance equal to its own diameter) an enormous epoch of time would in general elapse between the encounters.

the action of gravity into states of aggregation, or groups, on the same principle (and perhaps even much on the same process) as the constituents of the molecules of a compound gas group together under "chemical" action. The range of "chemical" action, like the range of "gravific" action, is *limited;* the constituents of the stellar masses, which group together under the limited range of "gravity," being in this respect comparable to the constituents of the molecules of a compound gas which group together under the limited range of "chemical" action. Just as in the case of a compound gas (on account of some molecules possessing much higher velocities than others) the components of two molecules frequently become dissociated by a collision, and group together again in a different part of the gas; so in the Universe, where some stars (no doubt for analogous dynamical reasons) possess a higher proper motion than others, we have complete disintegration by collision in some parts, aggregation in others, continual change, but the *mean* aggregation remaining unchanged.

It may be observed that in principle, in order to explain the continued working of physical phenomena, or the continuance of change in the Universe, the existence of some process of *recurrence* is absolutely essential. The particular material that is utilised for the development of fresh suns or centres of heat must be the cooled down or dead material of former suns. For if this were not the fact there would be a continual accumulation of the matter of extinct or useless suns in the Universe, and processes of renewal and maintenance of the energy of the Universe would come to a deadlock in the absence of matter to operate upon. It therefore becomes absolutely necessary to show, in any attempt to explain the continued working of physical phenomena under present causes, how the material of extinct suns can—in accordance with recognised dynamical principles—be made available for the development of fresh suns (or centres of heat). This conclusion would seem to evolve itself naturally on the basis of the deductions following from the physical theory of gravitation—in regard to the motion of the stars taking place in straight lines, which involves collisions and an alternating renewal and loss of heat. This appears on broad principle the only conceivable way in which there should be *recurrence*, under the condition that the *same matter should be used again.* On account of the relatively enormous area of free space, compared with the relatively very minute portion of space occupied by each star, a stellar sun would, in the course of its proper motion in a straight line, probably as a rule traverse an immense distance before an encounter, and thus have time to cool down before the renovation of its heat by a collision; the dead or used-up material of former suns being thus available for fresh suns.

There is one point in connection with this subject that may be worth noticing:—The sun, as is known, is giving off an enormous amount of energy in the form of heat into the ether of space, no less than about 1720 foot-tons of energy being thrown off from every square foot of the sun per *second*. Can it be imagined that this enormous total of energy can be given off in a particular direction—*i.e.*, *from* the sun—without any reaction in the opposite direction; or would it be thought that if the sun were emitting all this energy from *one side only* there would be no reaction in the opposite direction? To us it seems incredible, whatever the constitution of the ether may be imagined to be, that there should be no reaction at all by this enormous total of energy thrown off.* Admitting that there is a slight reaction, then it is probable that it would not be absolutely equal all round the sun, owing to accidental irregularities in the temperature and radiating power of the materials of the sun's surface. If this reaction were only a few *grains* per square foot (due to 1720 foot-tons of energy thrown off per square foot, per second), the resultant or unbalanced reaction, owing to irregularities in the distribution of the sun's heat and radiating power, might amount to a considerable *total* quantity, owing to the enormous area of the sun. This reaction, even if relatively small, might, in acting for ages (millions of years), in the end sensibly affect the proper motion of the sun; or this might be a cause having

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* We do not mean to infer that the reaction due to radiation is the cause of the motion of the radiometer, as we think there are grounds for believing that (in the case of the relatively extremely feeble heating of the radiometer) it would not be a *sufficient* cause to produce the motion, or probably any measurable effect. There can be little doubt that the *effective* cause of the motion of the radiometer has been quite successfully traced to the action of the molecules of the residual gas, as shown by Mr. Johnstone Stoney. This, however, by no means proves that there is *no* reaction by radiation. In considering the reaction in the case of the sun, the relatively enormous value of the energy continually thrown off from its surface (amounting to thousands of horse-power per square *foot*) must be duly realised. It may be estimated that a portion of the sun's surface equal in size to the disk of an ordinary radiometer (say a quarter of a square inch in area) is throwing off a wave energy equal to twelve horse-power. A vertical column of ether of this sectional area, situated on the sun, is therefore transmitting the same energy as the belt of a twelve-horse steam-engine. Can it be imagined that it can do so without any reaction at all? Consider, also, the millions of square miles of the sun's surface, each minute portion of which is throwing off this same energy.

some influence on the proper motion of the stellar suns, in addition to the repulsive action of the heat generated by the mutual collisions.

In the absence of any explanation of the mechanism of gravity, or idea of the nature of gravific energy, it naturally becomes impossible to see how heat once dissipated in the ether could ever be recovered again, or how heat-energy derived from gravific energy could ever be re-converted into gravific energy. But when it comes to be recognised (as it logically and inevitably must) that gravific energy resides in a medium pervading space, and that therefore this medium is necessarily immersed in the ether or heat-conveying medium, then we have data for new conclusions. It appears clear, or almost a necessary deduction, that the waves of heat cannot be conveyed through the gravific medium (in which the ether is immersed) without becoming to some extent dissipated in the gravific medium: for it seems incredible how two media should be immersed in each other without interfering with each other's motions at all. If we admit that the waves of heat, though no perceptible diminution occurs in planetary distances, are at last in the course of ages frittered away or dissipated in the gravific medium in which the ether is immersed, then, since energy cannot be annihilated, the full equivalent of heat-energy (represented by the waves) is finally converted into gravific energy (by dissipation in the gravific medium). The energy, therefore, which came from the gravific medium, and was converted into heat-energy in the action of gravity at the approach of the masses, is finally restored to the gravific medium again, in the frittering away of the waves of heat in that medium. Thus the total amounts of gravific energy and of heat-energy may remain constant in the Universe, the one being convertible into the other, and back again. It may be observed that, besides the above apparently unavoidable conversion of heat-energy into gravific energy by dissipation of the heat-waves in the gravific medium (and the consequent equalisation of the energy in the two media), it would seem a reasonable conclusion that two media (the gravific medium and the heat-conveying medium), *immersed* in each other, must naturally maintain an equilibrium of motion or energy between themselves, as, for example, is known to be the fact in the case of two gases immersed in each other or mixed, however diverse their qualities may be. It should be observed that the gravific medium *must* be constituted as a gas (according to the kinetic theory) in order to accord with the observed effects of gravity.

These considerations would point to the general conclusion that *recurring* changes take place in the Universe whereby the continuance of useful activity and life is insured, and the deadlock of useless inaction and uniform repose prevented. In other words, they would lead to the inference that physical causation is not so constituted as to defeat itself and bring the operations of the Universe to a standstill.

It should be noted that, as a matter of theoretic principle, the problem of recurring changes in the Universe undoubtedly admits of solution, since it is an admitted dynamical principle that masses immersed in media whose particles are in a state of translatory motion must themselves inevitably acquire *some* translatory motion, and the stellar masses moving freely in straight lines in the gravific medium represent (as it were) a larger scale gas immersed in a smaller scale gas (viz., the gravific medium). It is a mere question of *scale*, and dynamical principles are (admittedly) independent of scale. The difficulty, if any, would therefore be one of *degree*, not of principle. There might be a difficulty in accounting for the high value of the translatory motion of the stellar masses that observation appears to point to. But it should be noted that it does not follow that the value of the translatory motion we observe is the true *mean* value; for it might be exceptionally above it. For it is known that the translatory motion of the molecules of gases (or of any bodies moving freely among each other according to kinetic theory) varies from zero towards infinity. It might therefore well be that the translatory motion we observe in our immediate neighbourhood might be exceptionally above the mean value, and we happen to observe this *particular* value because it is suited to the conditions of life (or the low translatory motion, on account of the feeble heat developed, would not be so suited).*

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* Possibly there may be some supplementary cause tending to produce translatory motion in the stars. The spectroscope shows the molecules of matter to possess a considerable complexity, or their *parts* have a considerable capacity for taking up motion. It would appear reasonable to conclude that the molecules of matter of the Universe, immersed in a medium constituted according to the kinetic theory, must acquire also in their *parts* a certain degree of motion, owing to the dynamic action of the impinging particles of the medium in which they are immersed. The intensity of motion thus acquired would (as is known) be greater in proportion as the parts of the molecule which are capable of motion are smaller; and this motion of the parts of the molecules would apparently tend to produce translatory motion in the molecules as wholes—much as the development of motion in the *parts* of the molecules of a gas (as occurs when they are exposed to the pulsations of waves of heat) is known to produce translatory motion in the molecules as wholes, thereby producing expansion in the volume of gas. Indeed a balance

It may be remarked that the conditions above arrived at, as a basis for recurring changes in the Universe, were not deduced solely with this object in view; but these very conditions follow independently as necessary consequences of the explanation of gravitation afforded by the kinetic theory, so that it becomes a remarkable fact that the very conditions that follow on the explanation of gravitation are precisely of that character required to account for *recurring* changes in the Universe. The more the subject is reflected on, the more apparent will it become that *in broad principle* other conditions for producing *recurring* changes are not conceivable—keeping in view the necessity (in order to effect recurrence) that the cooled down material of the Universe should be that material which is utilised for the development of fresh centres of heat; and there would appear to be a simple grandeur (not out of harmony with the recognised characteristics of Nature) in this great result being brought about by the mere movement of the stars according to the kinetic theory. Also there would be a sort of harmony or

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(as is known) tends to establish itself between the energy of the translatory motion of the molecules as wholes, and the energy of the motion of their parts (internal motions). Thus the throwing of the parts of molecules into motion (from any cause) would tend to produce motion in the molecules as wholes. In principle, therefore, the development of motion in the smaller parts of matter tends to produce translatory motion in the larger parts (and that whatever the relative scale may be). This might possibly have its application as an auxiliary cause for the development of translatory motion in the larger scale parts of matter (represented by the stellar masses), through the intermediary of the smaller parts (molecules), and the minute moving particles of the medium in which these smaller parts are immersed.

Another point may perhaps be worth noticing here. When the molecules of a compound gas (immersed in another gas) break up from any cause into their components, each of these smaller components tends to acquire from the rest of the gas the same *absolute* kinetic energy of translatory motion as the entire compound molecule possessed (before it was broken up); so that therefore the total energy of a given portion of gas may be suddenly greatly augmented by the mere breaking up of its (compound) molecules into parts. If we imagine any limited portion of a gas to be immersed in another gas of unlimited extent (or if the portion of gas be supposed enclosed in some way so as to permit expansion, and at the same time to allow a free exchange of energy between it and the other gas), then a change in the state of aggregation—by the breaking up into parts of the compound molecules of the enclosed gas—will be followed by a considerable transference of energy from the outer gas to the enclosed gas; for when equilibrium is again attained, each of the small parts into which the compound molecules of the enclosed gas are split up will have acquired from the outer gas the same absolute kinetic energy of translatory motion as the *entire* molecule possessed before it was broken up. Thus the mere act of breaking up (disintegration) of matter, *immersed in a medium enclosing a store of motion*, may cause considerable transference of energy from this medium to the immersed matter, tending to produce expansion and rebound. Possibly this principle might also have some bearing on the disintegrating changes continually taking place in the Universe.

analogy in the stellar masses and the media in which they are immersed moving under the same dynamical principles. Moreover, the kinetic theory has been mathematically proved (when a large number of masses are concerned) to produce a system of order and symmetry (or *mean* similarity of the conditions in all parts of the system) which is rigidly and automatically maintained by a process of self-correction under dynamical principles—a self-acting adjustment of the motions continually taking place, whereby a system of harmony and order is maintained everywhere, a perfect state of mobile equilibrium existing in all parts.

Those who regard the physical causation of the past in the light of the physical causation of the present, or who look upon the principle of the conservation of energy as a truth as necessary in the past as in the present (or who look upon physical truths as independent of time), are bound to believe that some process of recurrence must exist, whereby useful change and activity are continued in the Universe, and the purposeless end of a changeless chaos prevented; and that we should seek for the explanation of this, not so much with the view to prove the fact thereby, but rather as a satisfaction or confirmation of a fact we already had logical grounds for believing to exist. As we happen to have given considerable time and thought to the problem of gravitation on the basis of Le Sage's fundamental idea, we have considered ourselves justified in calling attention to the important consequences and modified views regarding natural phenomena that the acceptance of the physical theory of gravitation entails.

The phenomena of Nature would thus consist in *cyclical* processes involving transferences of motion from physical media in space to matter, and back again to these media (in a circle), the collective sum total of energy in matter and in these media remaining at every instant constant, in accordance with the principle of the conservation of energy. The cyclical processes thus taking place on a great scale in the Universe are imitated on a smaller scale in the ordinary interchanges of energy on the earth's surface, or all the ordinary processes of "work," or the derivation of power, show themselves to be *cyclical* processes on the recognition of the stores of motion in space rendered necessary by the physical theory of gravitation. For suppose, for example, we derive power from the fall of water, and use it through machinery for lifting weights or elevating materials,—then the motion derived at the descent of the water comes from the store of motion in the gravific medium, and passes to the gravific medium again in a *cyclical* process, in the operation of elevating the materials in opposition to the dynamic action of the gravific medium which tends to urge them towards the earth. If, instead of elevating materials, the work be done in friction, as in dragging masses (vehicles, &c.) along the earth's surface, then the waves of heat due to the friction are radiated into the media of space, and, becoming naturally dissipated in the gravific medium through which they have to pass, the energy thus finally returns to this medium, to be available for utilisation again. Winds or currents of air are due directly (as is known) to variations of pressure in the earth's atmosphere, which re-adjust themselves through the action of gravity. The gravific medium is therefore the agent directly concerned in producing the current of air or wind, and therefore is the motive agent concerned in developing motion in the machinery driven through the intervention of the wind. In the case of the ship propelled by the wind, the power is converted into heat in the passage of the ship through the water. The heat then passes in waves to the media in space, or is dissipated in the source whence it was derived in a *cyclical* process. It should be observed that all these deductions are necessarily true on the acceptance of the explanation of the mechanism of gravity afforded by the physical theory.

There can be little doubt that the explanation of gravity (the fundamental agency in Nature) will naturally and inevitably entail with it, in principle, analogous explanations of the other molecular motions ("chemical" reactions, &c.). Thus the operation of the steam-engine will show itself as a *cyclical* process, the motion passing from the media in space to the coal, and thence (in a circle) to the media in space in the various operations of the engine.* Thus considerations of great practical interest present themselves on the recognition of the existence of the stores of concealed motion in space revealed by the kinetic theory of gravitation, and problems of the highest practical importance may suggest themselves as to possible methods of utilising this store of motion to greater advantage than at present. There might be a tendency to regard considerations of this nature (independently of their truth or error) as in some degree *occult*. This could only arise from the kind of prejudice that tends to beset every new path, or possibly in the

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* See also a paper in Philosophical Magazine for April last (" The Bearing of the Kinetic Theory of Gravitation on the Phenomena of 'Cohesion' and Chemical Action ' ").

absence of realisation of the *mechanical fitness* of the scheme due to *attention* not having been given to the subject. What may be justly regarded as occult are the effects themselves (or the developments of motion in matter, occurring on every hand) without any explanation at all. Since the store of energy simply consists of finely subdivided matter in a state of rapid motion, and since it is obviously just as easy to reason of matter of one dimension as of another, there can be nothing whatever occult about the subject at all. The conclusion deducible with certainty beforehand that this store of motion—*if* it did exist in space—*would be concealed*, ought to go far to remove all preliminary doubts as to its existence; and unless we are ready to surrender the right of using our reason, the developments of motion occurring on all sides (as combustion, the detonation of explosives, and the varied movements developed in masses and molecules of matter, &c.) are utterly inexplicable without the existence of this store of motion in space. Minuteness of size of the particles is evidently adapted to rapidity of motion, and this rapidity of motion is itself essential to an intense store of energy, and this rapidity of motion, combined with minuteness of size, obviously and necessarily renders the motion of the particles concealed; indeed the more perfect the concealment, the higher should we be warranted in estimating the intensity of the store of energy to be (from the known fact that the higher the velocity of the component particles, the more impalpable does the medium become). It is very important, therefore, to keep in view that—independently of all question of the existence of this store of energy in space—it is so far an indisputable fact that *if* it did exist it would be perfectly concealed. There can therefore be no logical *à priori* reason for doubting its existence, and the admirable manner in which (on analysis) the mechanical conditions show themselves to be adapted to allow the existence of an available store of energy in space, to any intensity consistent with concealment, and in harmony with the conditions of life, ought to render the contemplation of the problem one of great mechanical interest. Surely, for example, to realise how the motion is transferred from the concealed store of motion in space to a mass of gunpowder (as a shell for instance) in the act of explosion, is a problem of the highest mechanical interest, and no one surely would mistake theories which may serve as a convenient temporary refuge in the absence of any conception of the process involved, as *explanations* of the process involved. Clearness of conception is the test of truth, and constitutes the real dignity of Science, and theories, however elaborated, if vague, have no real dignity; and the aim ought surely to be to endeavour to think out the *simplest* method of arriving at a result, keeping in view the fact that simplicity is *necessary* to the orderly working of mechanical processes, and that the real point to admire is a *simple* method of attaining a mechanical result, because it is *unique*, not a complicated method (which may be conceived to be indefinitely varied). The beautiful kinetic theory of gases affords the simplest method to the solution of the problem of the constitution of the physical media in space, and in the case of the gravific medium it may be proved to be the only solution which can harmonise with observed facts.*

When we realise the practically unlimited intensity of the concealed energy that may thus exist in space (in the simple form of finely subdivided matter possessing a high velocity) one may cease to wonder at the sudden energy transferred to a mass of gunpowder (or a shell) in the act of explosion, and realise how this otherwise incomprehensible and extraordinary effect can take place. It would be illogical to consider a fact less extraordinary, or that a rational explanation for it is less urgent, because it is commonplace, and precisely because there is this tendency it ought to be specially guarded against. In the case of the explosion we have in principle simply the transference of motion from minute particles of matter (where the motion is therefore invisible) to grosser matter, in the form of clouds of vapour and palpable fragments of matter (when the motion is visible); and therefore to the superficial bodily eye there naturally *appears* to be an actual *creation* of motion. If the motion (before transference) were not invisible, it would not be *mechanically efficient* for the object to be attained; for unless the moving particles of matter constituting the medium were small, it would be impossible to *concentrate* a considerable intensity of energy within a small volume of space. In the case of any powerful or intense motive source, *velocity* of the particles must evidently be relied on rather than mass (since mass occupies space, and prevents concentration of the energy. There could be no logical ground why problems of this nature should be regarded in

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* It may be observed that in the case of *any* medium constituted according to the kinetic theory, the velocity of the particles of the medium is equal to the velocity of propagation of a wave in the medium—

$$\times \frac{3}{\sqrt{5}}.$$

See result appended by Prof. Maxwell to paper "On the Mode of Propagation of Sound" (Philosophical Magazine, June, 1877).

any other light than as ordinary engineering or mechanical problems, to be explained by the light of reason and common sense. There would appear to be a kind of prejudice in regard to thinking on these subjects which requires to be surmounted or dissipated by logic. It may seem bold to say it, but scientific *reform* is called for in this respect. Ability is by itself of no avail without *attention*. Reform, in the sense of a more general attention being directed to subjects whose real interest would disclose itself on examination, is what is required.

In regard to the subject of recurring changes in the Universe, this opinion appears to have been held by Sir W. Grove (" Correlation of Physical Forces," p. 67), though he does not go into any explanation as to the particular conditions required to bring about the result. He remarks relative to this subject (p. 67):—" Enlarged observation may prove that phenomena seeming to tend in one direction will turn out to be recurrent, though never absolutely identical in their recurrence; that there is throughout the Universe gradual change, but no finality; . . . that no star or planet could at any time be said to be created or destroyed, or to be in a state of absolute stability, but that some may be increasing, others dwindling away, and so throughout the Universe, in the past as in the future."

Humboldt also says, as regards this point (Preface to " Cosmos ")—" I would therefore venture to hope that an attempt to delineate Nature in all its vivid animation and exalted grandeur, and to trace the *stable* amid the vacillating ever-recurring alternation of physical metamorphoses, will not be wholly disregarded at a future age."

IV. THE EVOLUTION OF BEAUTY.

By F. T. Mott, F.R.G.S.

TO estimate the comparative value of Constitution and Education in producing the net result of character is one of the most difficult problems of physiology.

The influence of Education being the more easily recognised of the two, it is probable that this factor has rarely been undervalued, and the Darwinian philosophy has turned upon it a special share of attention. Inherited "tendencies" are indeed admitted by modern evolutionists, but with a very vague conception of what it is which "tends;" and generally the evolving organism is treated rather as a *tabula rasa,* on which surrounding conditions paint themselves in a cumulative manner, than as a centre of force to whose inherent activity is largely due the character of the complex product.

The existence of a "vital force," under some name or other, is not perhaps denied, but it is comparatively ignored; the main arguments being expended in attempting to show that the variations of form, colour, ornament, and disposition may be accounted for by the mere action of surrounding conditions; that Education is everything, original Constitution of quite secondary importance. Yet the fact is surely evident that every organism has within itself a force which will expend its activity in a definite direction, and that its environment has only power to modify that line of activity within very narrow limits.

An acorn cannot be made to grow into anything but an oak, with perhaps some small variation of form or colour; and though it be granted that variations in the same direction, impressed upon many successive generations, may effect large changes in the end, this is the result of guiding rather than of creating. It is the turning of the stream into a new channel, not the making of the river; and the most important factor in the product is, after all, the current with its perpetual activity, rather than the physical conditions to which it partially submits.

A recent article in the "Quarterly Journal of Science," on "The Action of Light upon the Colouration of the Organic World," presents a fair example of the manner in which this question is commonly treated. Impartiality and sound logic are not wanting, but the argument is invalidated because it ignores one of the primary factors of the problem.

To discuss the manner in which the surface-colouring of plants and animals is produced by insects or by light, without reference to the internal forces upon which these influences act, is like calculating the velocity of the hands of a watch upon the basis of the decreased temperature by which it is slightly accelerated, without taking account of the spring, or of the fact that the velocity will remain nearly the same whether the temperature be lowered or elevated.

Is it not possible to get a more satisfactory view of organic phenomena by going a step lower in the chain of causation? By turning our attention to the internal sources of activity, as well as to external influences, and by endeavouring to estimate each factor at its true value?

We have no knowledge of Matter, except as accompanied by Force. The ultimate atoms we imagine to be related to each other by attractions and repulsions. The compound molecule is the seat of equally compound Forces. The possibility of such a perfect balance of Forces as to produce absolute rest may be argued, but can scarcely be proved, and a probability remains that no such thing as absolute rest is known throughout the Universe. Everywhere the concomitant of Force is motion or activity, either atomic, molecular, or molar; and the universal character of such activity is *alternate acceleration and retardation.*

We need not discuss the problem whether the Force or the Matter is the actual *substans*. The same general laws of activity hold good both for Matter and Mind, and one of the most fundamental of these laws is that of alternate acceleration and retardation, of expansion and compression, of increase and decrease, whether in relation to time, to space, or to intensity. In modern terminology this type of activity is known as *wave-motion*, and the total activity of the Universe, as far as we can penetrate it, is made up of such waves, in what may be called—without extravagant metaphor—an " infinite " variety.

The wave form originates in the compound action of initial impulse with surrounding resistances. Both factors are essential, and if space is everywhere occupied by matter, or is everywhere under the influence of attractions and repulsions, there can be no initial impulse which is not instantly met by resistance, no motion therefore which does not partake of the wave character.

An initial impulse, if unresisted, would, it is supposed, continue with unvarying velocity and direction through space and time for ever. But by the action of surrounding resistances, which are themselves perhaps but modifications of other initial impulses, its equilibrium is upset, its velocities made inconstant, its direction and intensity altered, divided, scattered, or concentrated, in a thousand different ways. How the initial impulses originated we need not inquire. We have to deal with a Universe in which they have been operating practically for infinite time, and to estimate as accurately as we can their present condition.

It seems correct to speak of these impulses as separate and individual phenomena, because, although the whole

Force of the Universe is probably a unity, it presents itself to our senses and understandings as divided everywhere into distinct units. In their simplest form we conceive of these units of Force as associated with material "atoms."

Wherever heat exists the material atoms are supposed to be in a state of perpetual agitation, and every minute atomic movement originates a "wave" of force in its simplest character, the differences between such an atomic wave and the great secular waves with which astronomy and geology mainly deal being probably differences due to increased intensity and complexity alone.

The *normal life* of a wave dates from the starting-point of its initial impulse to its maximum of compression or retardation, and thence through the period of reaction till it returns to its original condition. But in vast numbers of cases this normal life is not completely fulfilled. The surrounding conditions being continually varied, in a succession of similar impulses no two will meet precisely the same fate. Many of them will be checked at different periods of their career, absorbed, divided, repelled, turned aside, or compounded with others; and out of this complex action new impulses will arise to pass through similar changes, and so keep up the perpetual agitation and life of the material Universe.

The *forms* of wave-motion may be indefinitely varied, but the three leading types are—(*a*) the wave of oscillation; (*b*) the wave of undulation; (*c*) the centripetal wave; the wave of oscillation being represented by the simplest atomic movement, while all compound movements tend towards the undulatory or the centripetal forms.

A simple atomic vibration in a molecule of unstable composition may be followed by molecular change and recomposition, the effects of which are passed on through surrounding matter in a succession of compound waves. These compound waves, meeting everywhere with other compound waves of varied form and intensity, may either be dissipated early in their career, or may pass through their normal life, or may be taken up without dissipation into the life of another compound wave of greater intensity or wider scope. By this latter process of absorption, combined with the reverse process of subdivision, a wave of great initial intensity may become complex to a degree scarcely conceivable by the human understanding.

From atomic vibration to a solar system is a long leap, but it is no more than the result of such composition of wave-motion carried on through indefinite time.

From a solar system to an acorn is a long leap also, yet it is probable that this, too, is only a result of the same law, only a still further development of the complexity of wave-motion. An organic unit seems to be the latest outcome of compounded force—the most complex form of wave yet attained to, in at least this corner of the Universe.

If we examine the structure of a complex wave we find that it consists of an organised body of waves, arranged after the manner of a disciplined army. There is first a single inclusive impulse, of the highest intensity and widest scope, holding in its grasp the total body of subsidiary waves, and representing the General in command. Then there are a few large, well-marked, secondary impulses, like separate brigades, working towards special goals, but with a common end in view. Each of these secondary waves is made up of a number of tertiary waves, which may stand for regiments, and within each regiment are again companies, squads, and finally individuals, the number of component waves increasing with each step downwards. But this organised arrangement of a crowd of units is not the only kind of complexity to be considered. Besides having its definite place in the system, each subsidiary wave has also its own normal period of life, and its own *velocity*, which varies not only in its totality, but in its *two phases*, viz., the ascending phase, in which it approaches towards its climax, and the descending phase, in which it recedes from it. These phases may either be equal or only equivalent, one being long and slow, while the other is short and rapid, the varieties of proportion in the latter case being unlimited.

Note what takes place in the growth of an acorn. While it lies brown and dry upon the earth, apparently lifeless and motionless, atomic and molecular motions are unceasingly at work among its tissues. This is proved by the fact that if left dry too long the living forces will be dissipated, the germ will perish and decay. But the presence of moisture and the communicated energy of external heat give rise to a re-composition of forces without dissipation. A wave of high intensity and rapidly increasing complexity is originated. Surrounding waves of force are absorbed into its vortex, and material molecules are built up in the track of the advancing wave, and of each unit in all its army of subsidiary waves. The normal form of the great inclusive impulse is such as to build up that material structure which we call an oak tree, and nothing else. The wave originated from a germinating acorn is always of that form, and no other. Its subsidiary waves, although unable to escape from the general law impressed upon them by the primary impulse, assert their individuality within that limit, and are slightly modified at the same time, as is also the primary itself, by surrounding resistances and attractions. Hence the result that no two oak trees are precisely similar in details, though identical in general plan.

Of the three leading wave-types, by far the most complex is the *centripetal.* This type is especially associated with organic life. The primary impulse in every entire organism is of this form; so also are all the great subsidiary waves. An organic being is a living and active centre, around which force and matter are alternately concentrated and dispersed. The normal life-period of each organism depends upon the character of its primary centripetal force-wave; and the apex or grand climacteric of its life is the point of greatest concentration, from which point dispersion sets in, ending in partial or in total dissolution.

The subsidiary waves being also of the centripetal type, but of varied periods and intensities, their climacterics are not synchronous, but successive, so that nearly always some portions of the organism are at their climax, while some are tending towards decay, and others still developing.

Among the characteristics of the organic centripetal wave there is one of peculiar significance. It is this—that the necessary result of centripetal action is concentration, and concentration implies the bringing into closer relations with each other of elements which had previously been more scattered. There is an important correlation between this process and our human senses.

Organic development takes place by the process of assimilation, by continually adding to the original impulse the energy of surrounding materials, by complicating the primary wave with an ever-increasing army of subsidiary waves. The intense attractive power of the organic life-wave has this especial capacity of accumulation and concentration, which is continued until the whole kinetic energy of the primary wave is changed into the potential form. At this point reaction sets in, and the descending phase of the wave commences, accompanied by decay of the organism and re-dispersion of its constituents.

During the career of the primary wave many subsidiary waves of all ranks will have run their course. In the life of a deciduous tree the *annual* wave, accompanied by evolution of foliage and blossom, and leaving part of its energy stored up in the seed, has almost a primary character, though it is in fact a subsidiary of the larger wave which carries the tree through many seasons to its normal limit of growth. But it furnishes also an analogy suggesting that even the primary life-wave of the individual organism is in truth subsidiary to a still larger and more inclusive wave controlling the development of the *species;* that each specific wave may be part only of a *generic* wave, which in its turn may be included in some wave of longer period and wider sweep; and that so all finite waves may, in their ultimate relationships, be but ripples of the one universal impulse.

Organic growth is, then, always a process of concentration, an accumulation of minute waves of force, each accompanied by some particle of matter. Of these minute waves many probably run together, and form the subsidiary waves of various ranks, while others remain distinct as superficial undulations. The constant tendency is to bring into relationship a vast number of force-waves which were previously scattered. These constituents are arranged, as they arrive, first in a rough outline indicating the tendencies of the primary wave; and this outline is gradually filled up as force and matter are accumulated, and drawn closer to the point of ultimate concentration of the primary wave.

In all developing organisms the order of growth is from the general to the particular, from the less to the more differentiated, from the scattered to the concentrated, from the outline or framework to the clothed and completed form; and although organic growth may appear to be an expansion rather than a concentration, it is not so in reality. When the bugle-call of a regiment in the field summons the skirmishers to retire upon their supports, the result is a concentration of force, although accompanied by an enlargement of the central mass.

In the process of organic development there appear to be at least four well-marked stages, which seem to indicate the existence of four large secondary waves immediately subsidiary to each organic primary. In the animal kingdom these mark the development of the four great systems of organic tissue—the cellular, the osseous, the fibrous, and the nervous. In the vegetable kingdom they are represented by the cellular tissue, the trunk and branch system, the foliage, and the blossom.

The *visible beauty* of the organic world depends upon the correlation between the sense-organs of the human race and these concentrating organic waves of force. That an object should appear to be beautiful is not the result of accidental surroundings, nor of any superficial garment spread over an ugly and repulsive interior. The elements of the beautiful are inherent in all things; that we cannot always recognise them is due to the limitation of our senses.

Beauty is an abstract idea, of the same nature as Goodness, Truth, Power, Charity, &c., and that which causes this idea to present itself in the human consciousness is *the perception of relationship among a number of diverse sensations*, of unity coexistent with variety. The mental sense of *ordered activity* is always accompanied by the idea of Beauty more or less vividly impressed. When the attention of the mind is focussed upon a variety of points in rapid succession, and the intellect is able to recognise *relationship* among all those points as members of one group, then arises the idea of Beauty. It can only present itself under the conditions of mental activity coexistent with the perception of relationship, proportion, unity.

Variety is necessary in order to secure the condition of mental activity, which is effected by the perpetual change implied in the successive contemplation of many points. Each act of attention is a force-wave which is very rapidly dissipated. No natural phenomenon can ever be exactly repeated; but if there is repetition, with too little variation, temporary paralysis follows, the well-known result of monotonous sensation. To keep up a vigorous mental activity the force-waves of attention must be sufficiently varied. Hence it follows that the primary condition under which any object can appear beautiful to the human mind is that it be compounded of a variety of parts, and that these parts be so much varied that the mind which contemplates them is not paralysed by monotony. Every object in Nature is so compounded of various parts, but human minds are not equally sensitive to small shades of difference. Obtuse minds are paralysed by a succession of acts of attention whose difference is sufficient to keep wide awake and active other minds of more delicate perceptivity. If any mind were absolutely sensitive to all shades of difference the paralysis due to monotony would be impossible to it.

Activity alone, however, is not sufficient to arouse the idea of Beauty; it must be recognised as *ordered* activity. The mind requires not only the perception of change, but also of relationship in passing from one act of attention to another, the perception that there is *similarity* as well as difference in those acts of attention. Similarity implies *identity* in at least one direction, with difference in other directions; and the point of identity which must be perceived in order that the group may appear beautiful is, that all those phenomena *belong* to that group, all take a necessary part in its formation, are bound together by a common bond, and form one unity. There may be many aspects of unity in a single group of concrete phenomena,—as unity of form, unity of colour, unity of motion, unity of purpose, &c.,—and the vividness with which the idea of Beauty is presented will be intensified as both variety and unity are perceived in a larger number of such aspects.

As different minds vary in their sensitiveness to differences between the acts of attention, so they vary also in their sensitiveness to *relationship* among those acts. A group of phenomena may appear to one mind to be closely related, while to another no element of identity among the varied parts can be perceived, so that the group appears a chaos, and the idea of Beauty is not evolved. Every object in Nature is a group of parts related to each other in ways more or less complex and subtle. If any mind were absolutely sensitive to all degrees of relationship, in all its aspects, nothing would appear chaotic. A mind absolutely sensitive to all shades of difference, and to all degrees of relationship at the same time, would see everywhere throughout creation variety bound up in unity, would find neither monotony nor chaos, discord nor ugliness, but only a universal Beauty.

The human mind, however, as it actually exists, has no such absolute sensitiveness. It is, in fact, sensitive either to variety or to unity only within narrow limits. Whatever groups of phenomena present variations or similarities within the range of those limits may be recognised as beautiful, but no others. To the naked eye no differences can be recognised among the minute grains which constitute the seed of the common musk-plant; and to the ordinary mind there appears at first to be no relationship between four such numbers as 264, 330, 396, 462; while the simplest of us can see that the stones on a gravel-walk are not all alike, and that the numbers 2, 4, 6, 8 are related to each other by a common law.

There is no doubt that culture increases the sensitiveness of the mind both to difference and to similarity, and it is certain that the cultured mind perceives more beauty in the Universe than the savage or the untaught. But the universal Beauty which exists throughout every corner of creation may be brought within the sphere of man's recognition by *changes in the grouping* of material forms, as well as by education of the mind.

Reduce to their simplest form the four numbers 264, 330, 396, 462, by dividing them by their greatest common measure, viz., 66, and you have the series 4, 5, 6, 7, the relations of which are perceptible at once to the most ordinary mind. Place before a person ignorant of plants the little yellow celandine and a red pæony, and he would not recognise any family resemblance; but fill up the gap between them with a buttercup, a globe-flower, and a Christmas rose, and he would perceive the gradation from one to the other and admit the relationship.

The closer the relationship, the more easily is it recognised; therefore the objects which appear beautiful to human minds must be those in which the parts are closely related, the proportions simple, the design obvious. Among mathematical figures the pentagon is one of the most elegant, because there is in its outline a sufficient variety with such simple proportions that the unity is easily grasped. The circle also is beautiful, so is the hexagon, the square, and the equilateral triangle; but in each of these the variety is less than in the pentagon. The dodecahedron, of twelve pentagons, is scarcely beautiful, except to a mathematician, because the variety is so confusing that part of the unity is lost, and the figure appears almost chaotic.

Divide a hexagon into six equilateral triangles, and let these be acted upon by an accelerated repulsive force so that they are driven radially from the centre to distances represented by the numbers 1, 3, 6, 10, 15, 21. They will then stand in a spiral of such form that the majority of persons would not recognise any law of relationship binding them into one group. Let, now, the repulsive force be changed into an attractive one whose formula is precisely reversed, and, as the triangles are drawn gradually towards the centre, their relationship will become more and more easily discerned, till, as they close up into the original hexagon, every human mind will perceive the unity of the group, and a sense of beauty will inevitably follow.

It is the character of the centripetal organic wave to be continually simplifying the relationship of its component waves with each other, both by concentration and the filling up of gaps; and it results from this that the nearer any organic wave approaches towards its climax, the closer are the relations of the component waves, and the more readily will its beauty be perceived by the human mind.

Beauty is thus shown to be an index of organic maturity, and not to depend upon any accidental or external influence, but to be inherent in every object, and only unseen during its embryonic stages.

The blossoming of plants indicates that the climacteric of their highest secondary life-wave has been attained. From that point reaction and sleep or decay set in. In perennials the great primary life-wave keeps its course through many flowering seasons; but there comes one season in which its power to blossom is at its height: this is the ultimate climacteric, and from that point there is gradual decay of the whole organism.

The *specific wave,* however, does not terminate with the individual. It is carried through the seed from generation to generation, and its climacteric is not reached until the highest blossoming power of the species has been attained. When this happens the species itself must gradually pass away.

The four great secondary organic waves, the cellular, the osseous, the fibrous, and the nervous, may exist in *unequal proportions* in a specific wave. The cellular is always first developed, afterwards the others in succession. But the grand climacteric of organic development is not attained until all four have reached the highest stage of concentration possible to them; and it is probable that such conditions have never yet existed on this earth.

Geologic history shows us, in vegetable life, first, a wonderful development of the cellular wave in the cryptogamic type, everywhere predominant; and then a similar epoch in which the osseous wave overtopped the cellular, clothing the world with mighty Conifers—huge masses of trunk and branches, with little foliage or blossom.

This was succeeded by an epoch of fibrous development, represented in the vegetable world by foliage. Forests of the broad-leaved Amentiferæ, the oak, the birch, the poplar, and the alder,—with elm, and maple, and plane, and other trees conspicuous for foliage, but not for blossom,—became the striking feature of the landscape.

Finally, as the nervous wave advanced towards its climax, blossom began to be developed in varied and conspicuous forms. Magnolias, roses, mallows, lilies, orchids, and many other "flowering" plants, appear in the latest tertiary deposits; and plants of this type still adorn the world, and probably become more numerous and more beautiful century by century.

It has been shown that plants with inconspicuous blossom have the widest geographic range, that those with white flowers have a range more limited, while species with brilliantly coloured blossoms have a still smaller range of distribution. Taking the area of distribution to indicate roughly the length of time during which a species has been in existence, these facts go to prove that the nervous wave of vegetable life, represented by the blossom,—the most sensitive, the most delicate, the most complex, and the most vitalised of vegetable organs, is still approaching its climax, and has not yet attained to it. They show, also, that visible beauty is to us the index of approaching climax, and not a quality which can be added at any epoch at which a temporary utility might seem to demand it.

In the light of this reasoning, the doctrine that "except for insects we should have had no flowers" cannot be maintained. Insects have doubtless aided the development of the life-wave among the complex resistances through which it has to make its way, but the beauty of the world, of which flowers form so interesting a part, depends upon laws far more profound, more deep-rooted and far-reaching, and which would surely have attained in due time their ultimate goal, even though the race of insects had never formed part of the same marvellous and admirable Cosmos.

V. FEELING AND ENERGY: ALTERNATE AFFECTIONS OF MATTER.

By W. S. Duncan.

In a former paper I boldly controverted the hypothesis of concomitance between the mental and physical states attendant on nervous action. That hypothesis I endeavoured to show was inimical to a perfect psychological philosophy on a scientific basis, inasmuch as it placed an impassable gulf between consciousness and action, between feeling and energy. It was opposed to universal experience and language. It even jarred with the best system of ethics, based on the *happiness* of mankind.

With equal boldness I ventured to advance a rival hypothesis, namely, that Feeling and Energy could be regarded as strictly alternate and convertible affections of matter—a hypothesis which not only agreed with the language of mankind, scientific and untaught alike, but bridged the logical gulf, and accorded with the laws of continuity and causation in all their manifestations.

Anticipating some objections that might be urged against the view I had taken, I briefly stated and endeavoured to refute them. The importance of the subject, however, demands a closer examination of its bearings; and the object of this paper is to deal with some of the more salient objections liable to be urged by a careful critic.

At the outset I would desire to be distinctly understood as making no claim to *prove* the existence of feeling in any object, organised or not organised. We *infer* feeling to exist in our fellow-creatures, by observing actions performed by them which in ourselves seem to be the outcome of feeling. The inference may be a correct one, but it rests on analogical reasoning which is liable to error. Neither the denial of feeling, on the one hand, nor its affirmation, on the other, are valid in establishing a fact which from its very nature is incapable of absolute proof.

But while admitting this much, I am not precluded from submitting the question to a philosophical examination in view of discovering truth, which, though not self-evident nor perfectly demonstrable, may yet be supported by fair deduction.

Starting, then, with the assumption that MATTER in an *organised* form and possessed of life has the quality, or affection, or property of Feeling, I think it is only the first step towards consistency to admit that feeling being found in that form of matter must have previously existed as a fundamental property in matter before it became organised, and must remain in matter after disorganisation has occurred. For it is utterly out of keeping with all scientific teaching to believe that an entirely new thing like feeling, a state believed by many to be unique, or without parallel in any other aspect of Nature, should be the mere product of organisation. What is organisation? Unless we change the meaning of the term it implies but the combination, in an orderly connection, of elements which—with all their individual properties—existed before organisation. It is therefore as illogical to regard matter not organised as devoid of feeling, as it is illogical to attempt theoretically to evolve it, after a creational fashion, as a "something out of nothing," —a something *created* by organisation,—called into being out of nothing, by a mere process of orderly arrangement! Feeling, being found in organised matter, must exist fundamentally as an affection of matter in all its forms.

A superficial objection to this conclusion, not likely to be seriously urged by thorough-going philosophers of the type of Herbert Spencer, is the absence of "purposive" action in all inorganic forms of matter. The absence of purposive action in the operations of chemical combination, the propagation of heat, light, and electricity, the play of gravitation, and the distribution of forces by machinery, seem all to the common mind utterly inconsistent with the presence of feeling in the unorganised forms of matter in which these physical manifestations are displayed. The philosopher would reply to this objection that absence of purposive action did not necessarily imply the absence of feeling, any more than that the actions of a madman destitute of purpose proved the madman to be without feeling. It is the characteristic of the speech and action accompanying insanity in its most acute forms to be purposeless. Indeed the actions of a lunatic seem to be far more erratic and unexpected than the ordinary physical activities going on in the inorganic world. Yet the absence of purpose in the lunatic is not taken to imply absence of feeling. No more would the inference be warranted in reference to ordinary physical forces.

But some may think that actions even of the erratic kind are far more indicative of the presence of feeling than those that can be predicted and calculated with mathematical precision. The latter are so evidently subject to law that it seems to reduce feeling to the position of a "slave to blind forces" if we regard ordinary physical forces to be accompanied by feeling. This kind of reasoning I imagine would not have much weight with those philosophers who have come to the conclusion that all animal action is automatic; the action of mankind not excepted. And the objection is clearly not a logical one, for the presence of law and order in the connections of feeling with action cannot be shown to be either inconceivable or inconsistent with psychological facts. On the contrary, the regularity with which like feelings are observed to be followed by like actions is so constant that the actions not only of men, but of all animals, are generally calculated and predicted therefrom. If, then, the connection of all "feeling-prompted actions" with law is recognised, both by philosophers and by people in daily life who have no interest in psychological theories, what argument can be drawn therefrom against the connection of feeling with the orderly operation of physical processes? Clearly none. If order and law be observed in the one case, why not in the other?

But this orderly operation of physiological activity has been supposed to go on sometimes in the *absence* of feeling. The incessant action of the heart and its vessels, that of the organs of digestion, and many if not most of our "involuntary" actions, are alleged to be unattended by feeling. In answer to this I should say that it is a question whether the thing which is alleged to occupy no share of our attention should not be described as simply occupying our attention *least.* The fact, as has been pointed out by some writers, that, when the heart and digestive organs are in any diseased or abnormal condition, sensations arise forcibly detaining the attention, proves that what may thus command at occasions much attention may continually be a factor in our feelings, though intellectually we take no note of it. What I mean by this will be best illustrated by dealing with a class of alleged facts of the same kind comprehended under the heads of "feelingless automatic action" or "unconscious cerebration."

Nothing is more common as a fact of experience than our forgetting that we used certain words in conversation or debate. We cannot believe now that we said so-and-so; or, admitting that we may, we explain it as a *lapsis linguæ*, as if due to the mere machinery of vocal utterance unattended by consciousness. Then, again, we sometimes lay past an object, as the banker cited by Dr. Carpenter laid by his safe-key, and forget where we have laid it. Or we may often do something very different from what we originally intended, like the gentleman, again mentioned by Carpenter, who went up-stairs to dress for dinner and "unconsciously" undressed and went to bed.

The statement that we do anything unconsciously, in the sense of *not associating with it all the circumstances* that an ordinarily rational being will usually include, is quite a truism; but the absence of the usual compounding of the separate cognitions of surrounding circumstances with our cognitions of the act we are performing is one thing to admit, and the entire absence of consciousness or feeling in connection with any act is quite another. The former is what we might expect from the complex character of the nervous system as a compound of feeling organs, which complexity renders it liable to operate in parts more or less in isolation from each other. Recollection means a feeble revival of the excitement originally made in the nerves by that object or set of circumstances which we are said to remember. When we fail to remember it the revival does not take place, because the original excitement was *not* made in nerves directly connected with those nerves which are the seat of our mental state when we are trying or wishing to remember. This is proved by our recollecting unexpectedly (after giving it up in despair) in *a different mental* connection—that is, in connection with nerves having a different physical connection. Then are we convinced that it is possible that what we supposed was not attended by feeling or consciousness was really so attended after all.

The mere fact that feeling organs may not operate in connection with one another is no proof that they do not feel individually. In any case the statement as to the absence of feeling in any organ is on a par with the statement that a given organ does feel: neither statement can claim to be supported by absolute proof. The question, therefore, is resolved into one of consistency with known facts, and I have endeavoured to show that the facts cited under the terms "unconscious cerebration" and "feelingless reflex action" are explainable quite consistently with the supposition that all action is prompted by antecedent feeling, whether or not the feelings concerned have connection with other feelings in neighbouring organs.

At this point of the discussion I am reminded that it is incumbent on me to explain consistently with my hypothesis, the conditions falling under the terms sleep, swoon, coma, death, and the inorganic state. In this connection I need hardly remark that it is equally necessary to both hypotheses—that of concomitance and alternation alike—to admit that dynamical equilibrium in an organ presupposes the absence of what we ordinarily call feeling; for it is the disturbance of this equilibrium that creates molecular vibration or nervous action—what is ordinarily deemed a mere train of physical sequences. The hypothesis of alternation is quite consistent with unconsciousness where dynamical equilibrium exists, for where no energy is being *received* no feeling can, by the hypothesis, exist. It is impossible to say with absolute certainty that in sleep, coma, or swoon there is total unconsciousness; but even if there is an entire absence of feeling, it is doubtless due to the supervention of a state of equilibrium in the grouped organs of feeling—the nerves. This state of equilibrium may be considered as arising from the withdrawal of the blood-supply which seems necessary to preserve the semi-fluid condition of the "axis-cylinders" or true nerves—a condition in which doubtless vibration can alone arise from the ordinary exciting causes, and therefore a condition indispensable to the presence of feeling. The withdrawal of the blood from the nervous system seems to be the true cause, for it has been observed that sensibility appears to diminish as the circulation is lowered, especially when it is withdrawn from the higher organs of feeling, namely, the co-ordinating and generalising organs that make up the brain.

Death would seem to bring about the perfect condition of equilibrium of the complex system of feeling and acting organs and the supervention of the inorganic state.

Inorganic matter, strictly defined, is a condition of matter in which feeling and acting organs are not arranged to operate in connected series or groups. Now, although all elements that receive energy must by the hypothesis feel and act somewhat like nerves *individually*, yet, inasmuch as these feelings have no grouping organ to connect them, in the case of inorganic matter they must feel as individuals. And since individual feelings are not feelings of feelings, so to speak, from the want of a grouping organ, they are therefore destitute of intelligence (sense of difference), which can alone arise in a grouping organ—an organ connected with several organs so as to feel their different states, or to have their different states communicated to it or combined with its own. But the condition of the inorganic world, commonly so-called, may be the seat of organisation of the kind necessary to intelligence in a degree of which we have never yet dreamt, just as the tissues of some Medusæ, destitute of nerves in the ordinary sense of the term, have been discovered by Mr. Romanes to be the seat of virtual nerves or lines of organised molecular activity. This, however, is for future investigation. Possibly, by the aid of the microphone and extensive research, much truth may yet be gained to connect the so-called inorganic world with what is at present called organic. Life will then be regarded as pervading all Nature, and a Cosmic Philosophy will become possible.

The most formidable objections, however, which the hypothesis of alternation has to answer are objections arising from the custom of regarding the *mental* condition of the thinking and feeling substance, matter, as something quite unique, and opposed to every other condition of that substance.

Mind has been defined by one representative of this mode of thought, namely, Professor Bain, as "opposed to the external world." He says—"The External or Object World is distinguished by the property called Extension. The Internal or the Subject World is our experience of everything *not extended*. A tree which possesses extension is a part of the object world; a pleasure, a volition, a thought, are facts of the subject world." The same writer again says—"We may have a simple *name* for the whole phenomena of mind, as 'The Subject,' 'The Unextended.'"

Now, since Feeling is the general term for all phases of consciousness, for all mental states, Feeling is therefore regarded as Unextended. Mr. Bain does not, in defining mind, say in so many words that the object world includes not only matter and space, but *energy*. Yet he speaks of "resisting matter" and "unresisting empty space," which implies that energy is included under the term "Object." Besides, although he speaks of feelings prompting actions all through his elaborate works on Mind, he speaks of the physical sequences as running side by side with the mental sequences, and of the feelings or mental states as being concomitant with the physical processes in nervous action. It is clear, therefore, that he does not mean to exclude energy from his definition of the object world.

The late Mr. Spalding, also a representative of the same school, evidently included energy along with matter under the term "physical' as opposed to the mental, "the unextended." "The physical and mental," he said, "stand over against each other, a fundamental duality of being which no effort of thought has been able to transcend."

Now, if Feeling be unextended in the exactly opposite sense that energy has extension (namely, of extension in time), feeling will certainly never merge into or alternate with energy, for energy has extension in time. But if it be true that energy has time-extension it is equally true that feeling has time-extension. By energy having extension in time, we mean that it endures in its action for a longer or shorter period. We must apply the same quality to feeling it is evident, for feeling endures also in the same sense as energy. If by applying the term "extension" to energy is meant merely the mental association with our idea of energy of an idea of time-extension, and not that energy actually endures, the same subtle distinction must be applied to feeling, namely, that feeling as extended in time should not be spoken of as a reality, but merely our mental association with the conception in our minds of feeling of an idea of extension is all that we mean when we describe a feeling as extended in time. In this very subtle but hazy language there will be found after all no real contrast between Feeling and Energy, for both must be regarded as having extension in time in exactly the same sense. If we admit, as we must, that Feeling has extension in time, there appears to be no necessity for any longer regarding it as something totally unlike anything else in Nature.

Further, if we institute a comparison between feeling and energy, I think we shall find that they are wonderfully alike. We cannot speak of the length, breadth, or volume of energy or of feeling, and therefore neither can be said to be possessed of space-extension. But we say of energy or feeling equally "it is here, it is there" in an extended body, and therefore, though these affections of matter have no space-extension in themselves they are both *related* to that which has space-extension (as well as time-extension), viz., matter. But their likeness is still more confirmed when we remark that they possess in common other characteristics which we do not observe in the mere substance-matter. We speak of energy or feeling as being "intense or violent, weak or dull," both being qualified as manifesting *degrees* or *quantity*.

Can these strong features of resemblance between feeling and energy justify the ascription of uniqueness to feeling or its opposability to all that is termed physical? So long as the term physical includes energy it is evident that no such contrast can be legitimately drawn. If the term "physical" merely comprehended the substance-matter it would be legitimate to contrast feeling with matter in the same way that a property is distinguished from that in which the property inheres. But energy, being an affection of matter also, could in the same way be distinguished from the substance wherein it was manifested. There is, indeed, no sense I am aware of in which Feeling can be contrasted with Matter that is not equally applicable to energy.

But if feeling and energy are, after all, so very similar that we apply almost invariably the same language to each, I am at a loss to see wherein they should be considered so dissimilar as that the hypothesis of alternation I have advanced should be deemed untenable. The language of experience describes these two affections as differing only in the sense of receiving and giving. We say "I feel, I act; I receive an influence, I give forth an influence; I am passive, I am active." When we use such language we are describing two states of the same being; subjecting these states to the only contrasts of which they are capable. My experience tells me of no other difference between feeling and energy than that the former is the aroused, awakened, affected; the latter the arousing, awakening, affecting—phase of my being.

Having met the objection of the alleged total dissimilarity between the mental state, feeling, and the physical state, energy, and endeavoured, as far as my language and logic can, to repel the allegation, I must now deal with the next objection likely to be urged against the hypothesis of alternation.

It is objected that, even admitting the similarity between feeling and energy to be sufficient to satisfy a theory of alternation, *there is yet wanted a locus in time* wherein the supposed alternation may transpire. The "physical processes concerned in nervous action," say the objectors, "are complete in themselves without the intervention of mental states. The dynamical activity of an excited nerve is believed to be perfectly unbroken from first to last. No interval of time, however brief, is regarded as possible during which the dynamical state is suspended. Now it appears to me there are innumerable intervals during the action of a nerve when the dynamical state, strictly so-called, must be suspended, though this may to physicists sound new, strange, and absurd.

Nervous action has been described as molecular vibration, or the to-and-fro movement of the minute constituents of the axis-cylinder or nerve-proper. The rapidity of transit of a nervous impulse is claimed to have been measured, and a very humble speed it is. But even if it were as swift as the swiftest known force, it would still be a period of time. And just as the time-period of the motion of the first molecule of the nerve is distinctly separated from the time-period of the last molecule's motion by the times of motion of each of the intervening molecules, so there is a period during which each molecule is in the attitude of receiving energy from its preceding neighbour *distinct from the period* during which it is in the act of imparting its energy to the succeeding molecule. Receiving and imparting are not simultaneous in occurrence, else time would be blotted out, the last molecule would move simultaneously with the first. The very fact that time is an element at all involves the admission that the whole time of vibration is divisible into the individual times of the vibrating elements, and the times of each of these into times of receiving and giving energy. And since the imparting stage is the only one of the two conditions of a vibrating molecule which can be strictly called dynamical, active, energising, it follows that the dynamical periods of the entire nerve are interrupted by periods that cannot be strictly called dynamical, since they are periods of passivity; periods of being affected, aroused, awakened; periods during which a condition exists which exactly corresponds with the condition we call feeling, or the mental state. These periods furnish, then, the desired *locus* in time, in which feeling may transpire and alternate with energising affections.

I am aware that a physicist might reply that the reason why nervous vibration—like all other vibration—involves the element of time is that space has to be travelled over, and that all the period between the receiving of energy by the first molecule and imparting it to the second molecule is occupied by traversing the distance between them, and that if such distance did not exist the motion of the first and last molecules would be simultaneous, and the condition of all the molecules would be a dynamical one. Therefore, that the condition of a molecule separated by distance from a second molecule is simply a dynamical one from the instant of its first receiving energy. Doubtless, I reply, if we obliterate space we obliterate time, and if we in theory blot these out there will surely not be much power left in us to imagine either feeling or energy! But we cannot conceive of energy without supposing something upon which energy is to be exerted. This implies plurality of objects or parts of matter, and this again implies parts of space and time. That the time of receiving energy by a molecule is not identical with that of its imparting the received energy is proved even by the law of inertia, which will admit of no beginning of movement till the requisite energy is all received. Now there must be a period, in the case of every molecule about to take part in vibration, during which it is in the condition of receiving its quota of energy necessary to overcome its inertia *prior to its beginning to move*, or impart the energy being communicated to it. This is the necessary *locus in time* where feeling may come in as incipient energy, or an affection of matter resembling energy, yet differing from it by its characteristic feature, *passivity*.

Thus far the hypothesis of alternation between mental and physical, or feeling and energising, affections of matter has its difficulties explained away.

In conclusion, it may be remarked that while the hypothesis links feeling with energy in a causal relation, yet no harm will accrue to physical science by its acceptance; for the dynamical links, though deemed alternating with links of feeling, need not to be reckoned in any problems of physics as affected by the mental states intervening, for the mental are the results of the physical, and the physical of the mental, in such a manner that no energy is lost, and all is exactly as if the dynamical sequences were uninterrupted. Again, no discrepancy need arise in psychological science by the intervention of dynamical with mental affections, for the actions alternating with feeling are those only which arise in the nerves,—or organs of feeling and action,—and not in the muscles, whose action, though doubtless preceded by feeling in themselves, are no more to be considered as exponents of the nervous action than the explosion of a powder-magazine is an exponent of the energy present in the ignited match. Even the hypothesis of concomitance is by the new hypothesis accounted for, by reason of the brevity of the period of transformation of feeling into energy, which practically makes the two series of affections have the appearance of concomitance.

The new hypothesis sheds a new light upon Feeling and Energy alike ; for if it be correct, then there is no "enduring" feeling strictly so-called. What is called an "enduring" feeling is really a succession of inconceivably brief mental states interrupted by dynamical states, as a musical note is the repetition of sound-impulses interrupted by periods of silence.

NOTICES OF BOOKS.

The Lake Dwellings of Switzerland and other parts of Europe. By Dr. Ferdinand Keller. Translated and Arranged by John Edward Lee, F.S.A., F.G.S. In Two Volumes. London: Longmans and Co.

Nature sometimes seems impatient at man's slow progress, and as if she desired to assist him in his researches. A great storm sweeps away the sand and shingle from the coast, and bares to the eye of the geologist buried forests and fossiliferous strata containing some of the missing links in the great chain of life. At another time the waves cast up on the shore some curious inhabitant of the deep, before unknown to Science. And so, in Switzerland, the pile dwellings might long have remained unexplored if it had not been for the great drought and extended frost of the winter of 1853-4, which caused the rivers to shrink to their smallest compass, and the level of the lakes to fall lower than is ever recorded in history, before this time.

Then were exposed, in a way that could not but arouse attention, the remains of habitations some of which are probably as old as the mysterious dolmens, and were used by the earliest of the neolithic immigrants. Notwithstanding their vast antiquity. the sediment at the bottoms of the quiet lakes had preserved not only the piles themselves, and articles of wood, stone, and pottery, but the provisions that had been stored for winter use, linen cloth, and fishermen's lines and nets. The great opportunity thus afforded for examining these remains was not neglected, and under the enthusiastic yet cautious leadership of Dr. Ferdinand Keller the explorations have been zealously carried on from that time to the present day.

A translation of Dr. Keller's earlier reports was published by Mr. Lee in 1866, and has been the principal work of reference for English students. Since then a great many additional discoveries have been made, not only in the lakes, but in many of the peat-bogs which have been proved to be filled-up lake-basins. The area over which the lake dwellers lived is shown to have extended far beyond Switzerland, into Italy, Austria, and Hungary. Now, more than ten years after the first edition, Mr. Lee lays us under a fresh obligation by a second, in which it is scarcely too much to say that the additional information is nearly equal to the whole contained in the first, Modestly terming it a translation of Dr. Keller's memoirs, Mr. Lee gives us what is in reality the work rather of a student who, wishing to ascribe all the honour to the master under whom he had studied, lays his own labours at his feet. A mere translation it is not. The antiquities themselves have been carefully studied by Mr. Lee, who brings, from a variety of sources, information to bear upon them, and appears to have neglected nothing that he thought might afford help in their elucidation. He has done for the Lake Dwellers of Switzerland and Italy what Prof. Rupert Jones —in his edition of Messrs. Lartet and Christy's "Reliquiæ Aquitanicæ"—has done for the Cave Dwellers of France; and it is instructive and suggestive to study the two works together, and to note the few points of resemblance and the many of contrast in the two peoples who occupied Central Europe at far removed periods, and in that area appear never to have come in contact. That the lacustrine people did not differ in race from those who lived on the dry land is proved by the fact that the implements of stone and bronze found in graves and tumuli, and on the surface in parts where there are no lakes, are the same in material, form, and ornamentation as those obtained from the pile-dwellings. Articles of bronze, stone, horn, bone, and earthenware, exactly similar to those used by the lake dwellers, were found in a settlement on the mainland at Ebersberg.

The people living on the land chose, for their settlements, positions on the sides and crests of hills that offered facilities for defence. These were hill-forts, and so the settlements in the lakes may be considered water-forts; the object in both cases being safety against the attacks of neighbouring tribes. In the early history of mankind, as well as amongst the lowest of existing races, every small community was or is at enmity with neighbouring ones; and so amongst these earliest neolithic settlers of Switzerland, some entrenched themselves on the hills, others in the lakes as far from the shore as they could drive their piles, and were the same people adapting themselves to the necessities of their surroundings.

The lake settlements were formed by first driving piles into the sand or clay in shallow parts; sometimes the piles were strengthened by cross timbers mortised into them, or by stones being brought from the land and heaped up around them. The tops of the piles were brought to the same level, and on them was formed a platform of small stems of trees, covered with mud, loam, and gravel. On this platform the huts of the settlers were reared. The walls consisted of upright posts, between which a wattle-work of small branches was interwoven, and the whole filled up and covered with clay. The roof was thatched with straw or reeds.

A narrow bridge or platform, built on piles, connected the settlement with the shore. It might have been safer to have done without this, and to have kept up the communication by means of canoes only; but the settlers stalled their cattle in the lake dwellings, and it would be necessary to have a more permanent connection with the land to get them to and from their pastures.

Many of the settlements were destroyed by fire, to which, from the combustible character of their materials, they must have been particularly liable. It is to the occurrence of these fires that we owe the preservation of many of the articles that have been obtained from the bottoms of the lakes. The whole of the provisions stored up for winter use have in some cases been precipitated into the water by the giving way of the burning piles. Rare implements of jade and personal ornaments have at these times been lost and covered up amongst the *dêbris*. Even loaves of bread and fruits have been preserved through having been charred before falling into the water.

The oldest of the lake dwellings appear to date back as far as the earliest traces we have of neolithic man in Central Europe. In these the implements are of stone, bone, and horn, and the pottery is rude and entirely hand-made. At this time the lake dwellers subsisted largely upon wild animals and fruits, though domestic animals and cultivated plants were not unknown. Weaving linen cloth was practised in the oldest settlements, and hanks of unspun flax and thread, cord, nets, and cloth of the same material have been found.

Up from the rudest stage of the stone age there is a regular progression to be observed. The bronze age comes in gradually; at first a bronze celt being found amongst the stone ones; then stone-implements become scarce, and those of bronze common and of improved form and ornamentation. Throughout, the evidence points to the gradual progression in civilisation of the same people, influenced, doubtless, by others in a more advanced stage living to the south and east, but without any sign of the sudden intrusion of a new race bringing with them new weapons and customs. Prof. Desor has also shown that the form of skull prevalent in the stone age continued through the bronze and iron ages, continually increasing in size, and showing a broader and higher forehead. It is the same type of skull that prevails in the Swiss valleys at the present day, showing that the direct descendants of the builders of the pile dwellings still live around the shores of the lakes.

Some of the breeds of domestic cattle they kept and of the varieties of grain they cultivated have also survived to our time, though greatly improved. The "marsh cow" of the lake dwellings is represented by the common Swiss "brown cow," and some of our varieties of wheat are, according to Dr. Heer, the descendants of those grown by the lake dwellers. None of the animals known in these ancient times have become extinct, though the domestic breeds have been improved and the wild species have in some cases deteriorated,—probably, in the latter case, through the curtailment of their feeding-grounds by man.

The remains of the common fowl have not been met with in the lake dwellings, and this gives us an approximate date for the latest of those belonging to the bronze age, as it is not mentioned by Homer, and is first referred to by Greek authors about 400 B.C. In the time of Pericles it was known as the Persian bird, from which we may gather it was brought from that country to Europe. It was unknown to the ancient Egyptians, and is not mentioned in the Old Testament. It had, however, spread into Western Europe before the Christian era, as it appears on the earliest of the Gallic coins, and was found by Julius Cæsar in Britain. Probably, therefore, we may place the date of the latest of the settlements of the bronze age at from 2400 to 3000 years ago.

It is often stated that Europe was peopled directly from Asia, but the relics found in the earliest of the lake dwellings do not favour this conclusion, but rather show an intimate relation with Egypt and the shores of the Mediterranean. Rye, which was cultivated in the east in the bronze age, is not found in the lake dwellings, and it was equally unknown to the ancient Egyptians. The cereals cultivated by the lacustrine people were all Egyptian or Italian. The eastern hemp was not used by either the lake dwellers or the ancient Egyptians; flax was largely grown, spun, and woven by both. Even the weeds introduced with the seed flax and corn point to the southern origin of the people. Dr. Heer finds the seeds of the Cretan catchfly in the remains from the lake dwellings. This plant does not now grow in Switzerland or Germany, but is found everywhere in the flax fields of the countries bordering the Mediterranean. The corn bluebottle, a native of Sicily, also grew in the fields of the lake dwellers; and the water chestnut had probably been introduced from Italy.

The influence of Egypt is shown in another way. A number of large crescent-shaped objects, made of pottery and wood, have been found amongst the remains of the lake dwellings. At first some thought these were indications of the worship of the moon, others that they were rude representations of the heads of bulls. Similar crescents have, however, been discovered in abundance amongst the Egyptian antiquities, and there is no doubt but that they were used as pillows by a people who wore their hair in the form of thick plaited head-dresses. The sleeper rested his neck in the hollow of the crescent, so as to prevent his carefully prepared head-dress from being disarranged. That the lake dwellers really wore their hair in thick plaited masses is proved by the discovery of very many long hair-pins. Even at the present day similar pillows are used by different tribes in Africa and Polynesia, who fasten up their hair in thick masses with pins 9 inches long.

The only fact pointing to a communication with the east is the presence of celts made of jade. This stone must have been brought from the east, and the advocates of the Oriental origin of the first settlers in Europe have supposed that they brought the implements made of it with them. But in that case, either the first settlers must have come laden with these implements to supply both themselves and their descendants, or the latter must have obtained them by barter from the east. In one of the latest settlements of the stone age—at Gerlafingen, in the Lake of Bienne—some of the finest jade-implements have been found. Two chisels of pure copper and some bronze celts of primitive type indicate that the people of this settlement lived at the close of the stone age. As they have been shown to be the descendants of the earlier lake dwellers, they could only have obtained these articles through traders, and, if they did so, their forefathers might have done the same. It is known that in ancient America copper articles found their way from the northern lakes, passing from tribe to tribe far to the south, whilst, on the other hand, the obsidian implements of Mexico were carried northward. The system of barter exists now amongst tribes more rude and savage than the Swiss lake dwellers. In those parts of Asia where jade occurs amongst the rocks, the ancient inhabitants would soon discover that they possessed an article of export for which they could obtain whatever they might wish in exchange from the nations of the West. And the latter, in the linen cloth they fabricated, had one article at least that they might barter for the coveted jade implements.

About the origin of the still earlier people that lived in Switzerland and other parts of Europe long before the lake dwellers—the people of the reindeer period—we know absolutely nothing. We know that they lived in Europe along with extinct species of elephant and rhinoceros, and many other animals not now found in Europe, and that in the latter part of their time the reindeer abounded throughout Central Europe and as far south as the Pyrenees. We know, too, that the climate was much colder, and that the musk sheep and other arctic animals lived in the South of France. But where palæolithic man came from there is as yet no evidence to show.

His outgoing is almost as much shrouded in mystery as his incoming. He simply disappears, and with him vanish the great beasts amongst which he lived. There is not a single point of connection between the latest of the palæolithic and the earliest of the neolithic tribes. Both, it is true, used stone-implements, but they are so essentially distinct in type that they can be recognised at a glance, and there are no intermediate forms showing the development of the one from the other. We have seen that there has been a regular, quiet, and gradual progression of the people of the stone age of the lake dwellings, through the bronze and iron periods, up to the present time. The people themselves are still represented, their arts remain, and every species of animal and plant they knew still exists.

But when we go back beyond the age of polished stone-implements there is a complete break,—a period unrepresented, excepting by physical changes in the appearance of the country, and by the deposition of clays and gravels, beneath which the relics of the earlier people and the remains of the extinct animals lie buried.

The evidence of the completeness of the break, and of the importance of the interval that separate palæolithic and neolithic man, is to be found in these volumes, and in Prof. Rupert Jones's edition of the "Reliquiæ Aquitanicæ." The reindeer is absent from the lake dwellings. The dog and the sheep are found in the earliest settlements, and both were unknown to the cave dwellers. The latter knew nothing of agriculture, of stock-keeping, or of weaving, and yet in the figures of the animals that they have left behind them—engraved on horn and ivory—they show a phase of art culture to which the lake dwellers never attained.

Had these different races ever come in contact as enemies, we ought to have found amongst the remains of the pile settlements some trophies taken from the people they displaced. Tusks of the brown bear and of the wild boar, perforated so that they might be worn as ornaments, are not uncommon, and show that they would have carefully preserved the proofs of their victories over nobler foes if they had ever encountered them. But there are none, and this—considered in connection with the other facts mentioned, and with the physical evidence that a long time intervened between the two peoples and faunas—indicates that they never met in Central Europe. As the evidence now stands it seems to warrant the conclusion that palæolithic man and the extinct animals were destroyed in Northern and Central Europe by some physical catastrophe, and that, after a long interval, neolithic man migrated northward from the shores of the Mediterranean and from the Iberian peninsula, and found the land unoccupied excepting by species of wild animals that had escaped extirpation, and which, notwithstanding the persecution to which they have been subjected since, still exist amongst us.

The questions touched upon in this review are only a few of those suggested in reading through Mr. Lee's most valuable work. It should be in the hands of every student of anthropology. The letterpress extends to 725 pages, and there are no less than 206 plates, with admirable figures of the lake dwellings and of the multitudinous objects found in them.

Acadian Geology. The Geological Structure, Organic Remains, and Mineral Resources of Nova Scotia, New Brunswick, and Prince Edward's Island. By J. W. Dawson, F.R.S. London: Macmillan and Co.

We have here the pleasure of meeting with Principal Dawson not as the scarcely-candid anti-Evolutionist zealot, prone to "high-falutin" language, and to bringing against opponents charges of intellectual and even moral obliquity, but as the earnest and successful worker in geological research. In this field he has made his mark, and, even though the evidence in favour of the organic nature of his *Eozoon* is not increasing, he has an indisputable claim to the gratitude of the scientific world.

The work before us is full of important observations, and brings us face to face with some most interesting questions, at which we regret that want of space allows us to take merely a passing glance.

Is Nova Scotia, with the adjacent parts of the Dominion, subsiding? We have heard it maintained that the harbour of Halifax was being gradually upheaved, and must at no very distant date become useless. But Prof. Cook, in a paper here referred to, gives a summary of indications of modern subsidence observed on the coasts of New England and New York, and estimates the average rate of sinking, as now in progress, at 2 feet per century. In some parts of Nova Scotia the tides are found to rise higher than formerly, and the reclaimed marshes—an exceedingly fruitful tract—are exposed to some peril.

A question of wider interest is the existence of a former Glacial epoch as the cause of certain geological phenomena observed both in Europe and America. The existence of such a period "when the whole of the northern parts of Europe and North America are imagined to have been covered with glaciers, or rather with an universal glacier like that of Greenland, but on an enormously larger scale," the author considers improbable, whether considered "in a mechanical, meteorological, or geological point of view." He contends that floating ice and the Arctic currents have been the grand agents in the distribution of erratic blocks, and in the production of scratched and polished rock-surfaces. He suggests that, as the country was gradually subsiding, blocks imbedded in ice were driven against the base of the hills. As the land continued to sink, the ice-fields of successive years gradually pushed them higher, until the summits of the hills were submerged so deeply that the ice could no longer take up the blocks. Concerning Prof. Frankland's hypothesis of a higher temperature of the sea conjointly with a lower temperature of the land, Dr. Dawson remarks that such an "inversion of the usual state of things is unwarranted by the doctrine of the secular cooling of the earth; it is contradicted by the fossils of the period, which show that the seas were colder than at present, and if it existed it could not produce the effects required, unless a preternatural arrest were at the same time laid on the winds which spread the temperature of the sea over the land."

The author has "failed to find, even in our higher mountains, any distinct signs of glacier action, though the action of the ocean-breakers is visible almost to their summits; and though I have observed in Canada and Nova Scotia many old sea-beaches, gravel-ridges, and lake-margins, I have seen nothing that could fairly be regarded as the work of glaciers." He holds that while a great and marked climatic revolution has occurred in Europe, the evidences of such a change are very much slighter in America, where the causes of the coldness of the post-pliocene seas to some extent still remain. The author, in the following passage, admits substantially the existence of a former epoch of intense cold:—"In the Tertiary era there was much dry land in the northern hemisphere, and multitudes of large animals now extinct inhabited it, apparently under a climate milder than at present. Great changes, however, took place in the relative positions of land and water, inducing very important changes of climate, *which finally became of an almost arctic character over all the present temperate regions.* The greater part of Northern Europe and Asia appear to have subsided beneath the waters of the boulder-bearing semi-arctic ocean." It would almost seem that Dr. Dawson attributes the climatic deterioration to a decrease of land in the higher latitudes, taking thus a view opposite to that of Lyell. We fear that the very foundations for a thorough settlement of the glacial question stand in need of careful revision.

In closing this interesting and instructive volume we can only pronounce it indispensable to every geologist.

A Manual of Zoology for the Use of Students. By H. ALLEYNE NICHOLSON, M.D., D.Sc., &c. Fifth Edition, Revised and Enlarged. Edinburgh and London: W. Blackwood and Sons.

WHEN a scientific work has, like the volume before us, reached its fifth edition, and has met with the general approval of the highest authorities, the task of the critic is greatly simplified. There are indeed points on which issue might be joined, and there also topics on which somewhat fuller information might be desirable. Thus, we cannot pronounce the common British centipede harmless, having received from one a bite decidedly more severe than the sting of the wasp. The bite of the viper, as far as we are aware, proves mortal, not merely to children and debilitated persons, but to about 20 per cent of the sufferers. Nor, in presence of the facts recorded by Dr. Coues, can we pronounce the skunk, "when unmolested, perfectly harmless." Passing from facts to matters of opinion, we must protest against the exaggerated view taken by the author of the interval between man and the anthropoid apes. Not satisfied with giving to the former, as did Cuvier, the rank of a separate order, Dr. Nicholson even questions whether the human species "should not have the value of a distinct sub-kingdom, whilst there can be little hesitation in giving Man, at any rate, a class to himself." As a deficiency, we may point out that the information given concerning the sense-organs of the vertebrate animals is somewhat meagre.

But passing over what to us, at least, appear as shortcomings, we hold that it would be difficult indeed to find a work which gives, in so brief a compass, so luminous and philosophical a view of the whole animal kingdom. To any earnest student entering upon the science of Biology the "General Introduction" alone must be a boon of the highest order.

A Key to Organic Materia Medica, &c. By Dr. John Muter, M.A., F.C.S., Director of the South London School of Pharmacy. Second Edition. London: Simpkin and Marshall. 1878.

The book before us is another proof that the training of pharmacists is becoming daily more thoroughly scientific. The first edition of this work appeared about four years ago, and was intended principally as a guide for the students frequenting Dr. Muter's School of Pharmacy, at Kennington Cross. The present issue, however, has been so enlarged and improved that it has become available for study to all classes of pharmaceutical students, whether under Dr. Muter's tuition or not. The products described are given in their botanical and zoological order, beginning with Aconite and ending with Castoreum; the source, method of gathering, description, uses, chemistry, and pharmacology of each being described in succinct but sufficiently detailed terms. Dr. Muter is evidently alive to the fact that most pharmaceutical students of necessity consume large quantities of midnight oil, for his book is printed in large clear type. In the Appendix is given an account of Dr. Muter's method of mounting, observing, measuring, and classifying the starches, which will be found equally useful to the vegetable physiologist and the pharmacist. The Index is a model of its kind, and is due to the pen of Mr. Joseph Ince, who deserves for it a vote of thanks from the Index Society.

Memoirs of the Geological Survey of India. Palæontologia Indica, being Figures and Descriptions of the Organic Remains procured during the progress of the Geological Survey of India. Indian Tertiary and Post Tertiary Vertebrata. Vol. i., 2. Ser. x., 2.—Molar Teeth and other remains of Mammalia, by R. LYDEKKER, B.A. Calcutta: Geological Survey Office. London: Trübner and Co.

THE remains here described and figured belong to *Rhinoceros palæindicus*, *R. sivalensis*, *R. platyrhinus*, *R. iravadicus* (a new species), *R. planidens*, and *Acerotherium Perimense*. If we include both extinct and living forms the total number of species of Rhinocerotes found in South-Eastern Asia is fifteen, whilst there is evidence of three further species from Burma, Attock, and Sind respectively. Of ruminants we find a description of *Vishnutherium iravadicum*, or at least of its teeth and a fragment of its jaw. Till the entire skull is obtained it will be difficult to say whether this extinct form was most nearly allied to *Sivatherium* or to *Camelopardalis*. Of this latter genus there is a figure and description of the remains of *C. sivalensis*. Next follow *Bramatherium Perimense*, *Camelus sivalensis* (a specific name which we fear occurs too often), and *Dorcatherium majus* and *minus* (two new species). *Cervus latidens* was the largest of the Cervidæ of Siwalik, its teeth equalling in size those of the Irish elk. The other Cervidæ are *C. triplidens* and *C. simplicidens*. The teeth of *Listriodon pentapotamiæ* might be mistaken for the lower molars of *Tapirus* were it not for their square form. *Dinotherium pentapotamiæ* is distinguished from the European species by its much smaller size. The teeth of *Sanitherium Schlagintweitii* are distinguished from those of *Sus* by the greater simplicity and distinctness of the main tubercles; by the hinder lobe being relatively larger and taller; and by the plane of wear being more oblique. *Tetraconodon magnum*, a hippopotamoid animal, is characterised by the abnormal development of its pre-molar teeth, which are considerably larger than the two molars.

The edentate animal, *Manis Sindiensis*, had the same general organisation as the living species. *Amphycyon palæindicus*, a carnivorous animal approximating to the ursine group, was nearly as large as the polar bear.

Ser. ii., 2, contains descriptions and illustrations of a number of fossil plants discovered in the Jurassic (Lias) formation of the Rajmahal Hills. This flora has a mesozoic character; it is much more numerous than that from Kach, both in species and specimens. Its chief types of the Rajmahal flora are in the class of Cycadeaceæ and some genera of ferns.

Descriptive Catalogue of Photographs of North-American Indians. By W. H. Jackson. Washington: Government Printing-Office.

The collection of photographs of members of different North-American tribes, formed under the auspices of the United States Geological Survey of the Territories, is an ethnological monument of peculiar value. It embraces over one thousand negatives, representing no fewer than twenty-five distinct tribes. Of these not a few are in process of extinction, whilst the rest are undergoing a process of intermixture with each other, and with the different nationalities of European or African origin who have overspread the western hemisphere. Hence a collection like the one in question is necessarily unique, and if it should be allowed to perish could never be reproduced. The catalogue is arranged ethnologically, and includes a short history of each tribe.

The Scientific Basis of Music. By W. H. Stone, M.A., M.B. Oxon, F.R.C.P., Lecturer on Physics at St. Thomas's Hospital, Vice-President of the Physical Society. 71 pp. London: Novello, Ewer, and Co.

This little book is one of a series of "Music Primers" issued by the above enterprising publishers. Although intended as manuals of instruction for beginners, they are so carefully written, and so much above the ordinary run of books—good, bad, and indifferent—which we meet with as tutors and instructors in various branches of music, that they can be read with interest even by advanced students, and, as far as they go, are worthy to take rank with some of the admirable works published under the auspices of the Paris Conservatoire. Dr. Stone's work treats on a department hitherto much neglected by the musical student: the author—himself a good practical musician, as well as a physicist—is admirably qualified for the task he has undertaken.

In the first chapter the sources of sound are fully and clearly explained. At page 21 the author mentions the class of *Membranous Reeds*, such as the larynx and the human lip acting on the cupped mouthpiece of a brass instrument: the author defers the consideration of the subject to a future chapter, but careful reading fails to find any further mention of this interesting class of sound-producers; it is to be hoped that this defect may be remedied in a future edition.

The following chapters explain the subjects of Velocity, Reflection, Refraction, Interference, Tonometry; Musical Tone, Harmonics, Consonance, Quality; Concord and Discord, and Resultant Tones.

The last chapter, on Scale and Temperament, gives an account of several contrivances, all more or less elaborate, having for their object the perfect tuning of keyed instruments. It is to be feared that these complex key-boards offer such obstacles to freedom of execution as to be useless for all practical purposes. Dr. Stone's remarks on tuning an orchestra are admirable, and will be read to advantage by every instrumentalist, many of whom, as conductors know too well, are extremely loose in their ideas of accuracy.

A page at the end of the little book is devoted to the bibliography of the subject, and will greatly aid the student in extending his researches.

Studies in Spectrum Analysis. By J. NORMAN LOCKYER, F.R.S. Second Edition. London: C. Kegan Paul and Co., 1, Paternoster Row. 1878.

MR. NORMAN LOCKYER's present volume forms part of Messrs. Paul's excellent International Scientific Series, and is written for the perusal of the more serious portion of the reading public, as well as for the more purely scientific student. Beginning with the vibrations of a jerked rope, the author explains, in a singularly clear and homely manner, the phenomena connected with wave-motion, the illustrations employed by him being of a nature which will be understood by every observant person. He shows that the principles upon which all undulations are produced —whether they are caused by muscular force, sound, heat, light or electricity—are similar, if not identical, a point which is only too frequently lost sight of by writers and lecturers on popular science. If, by setting forth the relationship between a note in music and a bright line in the spectrum, a thoughtful reader or lecturer can by analogy be made to see how the effects of a change in the rate of the wave-motion of the luminiferous ether as affecting colours are brought about, a great point is gained. We believe Prof. Barrett, in his lectures on the Connection between Light and Sound, was one of the first to break ground in this direction: and the fact that light is nothing more than an excessively fine kind of sound, the effects of which were perceived by the eye instead of the ear, was a revelation to the majority of his hearers. Mr. Lockyer treats of sound, light, heat, and electricity concurrently as being parts of one grand system of wave-motion, and we should be pleased to see his example followed by writers on elementary science who only are too fond of separating these subjects by hard-and-fast lines instead of treating them correlatively.

The present work may be said to be in some sort the complement of Mr. Lockyer's former work on "The Spectroscope and its Variations," seeing that it goes much more deeply into the theoretical part of this subject. The subject of wave-motion having been explained, the author next describes the principal methods of demonstrating spectral phenomena, and the value and use of the photographic camera in their registration.

The fourth chapter, in which atoms and molecules are treated of in relation to the spectroscope, contains a large amount of theoretical speculation of a very interesting character; and the same may be said of the chapter on Dissociation, in which Mr. Lockyer boldly attacks the integrity of certain of the elements, such as calcium and hydrogen. Mr. Lockyer's speculations in this direction are of a most suggestive kind, and ought to form the starting-points for a large amount of investigation. If our present elements are ever to be split up, either theoretically or practically, the spectroscope will have undoubtedly commenced the work.

The account given of Messrs. Lockyer and Roberts's experiments on the quantitative analysis of gold and other alloys show that one of these days the spectroscope will play an important part in the assay of the precious metals. The results given by these experiments are of a very promising nature, and it is only the close attention which Mr. Lockyer has to give to the more important branches of Spectroscopy that prevents him and his colleague from bringing their researches to a thoroughly practical termination. By the present method an assay takes at least two hours, whereas by the use of the spectroscope it might be performed in a few minutes.

The book is well illustrated by eight photo-lithographic plates of spectra and fifty well-executed woodcuts.

Industrial Chemistry. A Manual for Use in Technical Colleges and Schools, and for Manufacturers, &c. Based upon a Translation, partly by Dr. T. D. Barry, of Stohmann and Engler's German Edition of Payen's "Précis de Chimie Industrielle." Edited throughout, and supplemented with Chapters on the Chemistry of the Metals, &c., by B. H. Paul, Ph.D. Illustrated with 668 Engravings on Wood. London: Longmans and Co. 1878.

After a very attentive examination of this, the most recent contribution to the literature of Industrial Chemistry, we must confess that we are utterly puzzled to discover for whose benefit this book has been compiled. Judging from the title-page and preface, it is not only intended for manufacturers and technological students, but for schoolboys and the general public as well. Now, if it is intended for the first-mentioned category of readers, it is at once too copious and too meagre, teaching those things it ought not to teach, and leaving untaught those things it ought to teach; if for the latter, it is again far too detailed in some parts and too superficial in others. The great mistake committed by the whole of the five writers connected with the work has been in endeavouring to teach too many things at once, the consequence being that the book is overloaded with instructions in the rudiments of chemical science, and descriptions of elements and compounds that have nothing to do with Industrial Chemistry. The natural result of this waste of space is that hosts of important compounds, whose names have long been household words even amongst the general public, are either passed over in silence or dismissed in a few lines. Will it be credited that—in a work published in the last quarter of the nineteenth century, and written "for manufacturers, &c."—while page after page is devoted to lengthy descriptions of such rarely seen elements as tellurium, gallium, yttrium, and a dozen others, half a page is given to platinum as an industrial metal, and such everyday products as dynamite and carbolic acid are barely mentioned. Had the space occupied by so much useless and extraneous matter, all of which may be found in any half-crown Manual of Chemistry, been devoted to the real object of the work, we might possibly have had a few pages given to such minor matters as calico-printing, aniline, anthracen and naphthalin dyes, collodion, Esparto grass, pebble powder, oxalic acid and its salts, tannic and pyrogallic acid, picric acid, and other chemical products which are constantly mentioned in the columns of every newspaper.

The way in which the translation is executed and edited leaves much to be desired; but the portions added to Messrs. Stohmann and Engler's adaptation of Payen's "Précis," which are evidently from the practised pen of Dr. Paul, form a strong contrast, in the ease and completeness with which they are written, to the disjointed style of the remainder of the work. The additions, in fact, are the best part of the book, which is more than half again as large as Stohmann and Engler's translation.

The work is ostensibly intended for English use, but in many parts too much favour has been shown to foreign processes to the entire exclusion of the methods of working usually adopted in this country. For instance, although Weldon's chlorine process is fairly described, Spence's improvements in the manufacture of alum and Mactear's method of sulphur recovery are entirely ignored. These are only two out of the many English processes which have been passed over in silence.

Another singular defect, which ought not to exist in a book of this sort, is the almost entire absence of references to fuller accounts of processes than the space at the editors' disposal would allow them to give—a fault that is also to be found in the German and French editions.

It is with great regret that we feel called upon to be so severe upon a work which has appeared with so many well and favourably known names attached to it. Such a book—we mean a manual of technological chemistry in the fullest sense of the term—is much wanted, but it must consist of many volumes, and be written and edited by more than one man. A book on industrial chemistry in a single volume is of but little use to the manufacturer or the technological student. A popular account of the more striking improvements which have lately taken place in chemical manufactures, treated in a lively style, and a plainly written manual of chemistry applied to the arts, for the use of schoolboys who have already gained some knowledge of the science, are both loudly called for, but Drs. Paul and Barry's compilation satisfies none of these requirements. It is also to be regretted that the beautiful copper plate engravings of the French and German editions have been replaced by small woodcuts.

Metals and their Chief Industrial Applications. By CHARLES R. ALDER WRIGHT, D.Sc., &c. London: Macmillan and Co. 1878.

THIS book contains the substance, with several valuable additions, of the course of lectures on this subject delivered by Dr. Wright, last year, at the Royal Institution. The author has the gift of conveying a large amount of practical information in a few words, without, however, falling into the sin of dryness. The lectures are avowedly popular; theory, therefore, receives comparatively little attention. The principles, however, upon which different ores are made to yield up their metals are clearly and succinctly explained. Dr. Wright has brought his information down to the very latest date, and the processes of Siemens, Bessemer, and other modern workers in the field of metallurgy are fully described. As an instance of the author's conscientiousness in this respect it may be mentioned that Allen's nickel process, described in the "Journal of the Society of Arts" of so late a date as last February, is to be found in its proper place.

For a young student desirous of gaining a general insight into metallurgical processes there could hardly be a better manual. The book is well illustrated; and although it seems ungracious to find fault where there is so much to praise, we must remind Dr. Wright that an Index is an essential part of such a work.

Transactions of the Edinburgh Geological Society. Vol. iii., Part I. Edinburgh. Printed for the Society. 1877.

This issue contains a large amount of interesting and valuable matter. There is an Inaugural Address on the "Palæontological Signification of the Migrations of Animals," delivered by Prof. H. A. Nicholson. The speaker treats almost exclusively of Marine Invertebrata, and the following are some of his general conclusions:—Oceanic currents have a great influence on the distribution of marine animals, acting not only upon pelagic species, but also upon the locomotive young of littoral species. Under any circumstances there is a probability that pelagic species will have a wider range than those of littoral habits. Littoral species are generally limited in their distribution by the depth of water near land, by the trend of the land itself, and by the extension of the shore. A species requiring a definite high or low temperature for its existence will spread much farther along a shore running east and west than it can along one having a north and south direction. The conditions which limit the range of littoral species are not stable and permanent, but vary much at different periods in consequence of subsidence or elevation of the land. The distribution of pelagic species is mainly influenced by the surface temperature of the water, but it may be temporarily or permanently affected by winds or currents. Deep-sea forms are usually widely diffused, their range depending chiefly on temperature, and being especially influenced by oceanic currents. Fresh-water Invertebrates are, in the main, governed in their migrations by the same laws as those which influence marine forms. Some marine Invertebrates are probably capable of adapting themselves to a gradual change of the sea in which they live to fresh water.

In a paper by Mr. R. Richardson, on "Phenomena of Weather-Action and Glaciation," we find some interesting observations on that process of erosion which certain so-called sceptics in geology are endeavouring to deny. But the man who can shut his eyes to the phenomena which on a larger or smaller scale are to be witnessed in every mountain-chain will certainly not be convinced by any human testimony. That the glaciation of Switzerland has decreased, and is still decreasing, the author finds abundant proof.

Dr. D. Hahn, in a humorous paper, advocates the introduction of a dual nomenclature in mineralogy, similar to that established in zoölogy and botany. We regret to notice that certain German names of minerals mentioned are greatly obscured by typographical errors. Dr. Hahn also communicates a paper on the "Phosphorescence of Minerals."

Mr. A. Somervail's communication, "On the Glacial Phenomena of Scotland, with especial reference to the recent works of

Dr. Croll and Mr. James Geikie," agrees in many points with the views put forward by Mr. Mattieu Williams in his "Through Norway with Ladies" (see "Quarterly Journal of Science," vol. vii., p. 537). Mr. Somervail, though by no means questioning the fact of glaciation or of a glacial epoch, entertains doubts concerning the so-called "Interglacial periods," and holds that "the conditions under which the lower drift deposits have been formed were not of that severe and rigorous kind that has of late been so vigorously advocated." He does not find evidence for a vast ice-sheet coming from Scandinavia, and coalescing, upon what is now the bed of the North Sea, with another sheet descending from the Highlands. He remarks that many of the supposed interglacial beds, which, according to Dr. Croll and Mr. Geikie, should have been formed under a subtropical temperature, "have yielded abundance of marine shells which show a much colder set of conditions than those actually inhabiting our present seas." The climatic conditions of the Glacial period may, he thinks, when more fully understood, be explained on geographical grounds without having recourse to the astronomical theory as elaborated by Dr. Croll.

Memoirs of the Geological Survey of India. Vol. xiii., Parts 1 and 2. Calcutta: Government Survey Office. London: Trübner and Co.

This volume consists of an account of the Wardha Valley Coalfield. The first notice of the existence of coal in the locality dates as far back as 1831. Twenty-three years later, Mr. Hislup, one of the pioneers of Indian geology, examined the valley. The coal-field, "as limited by an arbitrary line to the south, and by its natural geological boundaries to the east, west, and north, covers an area of about 1600 square miles, and is included between latitudes 19° 28′ and 20° 27′ N. and longitudes 78° 50′ and 79° 45′ E. The district, as suspected by Mr. Blanford and subsequently fully proved by Mr. Fedden, bears evident marks of glaciation. "A boulder-bed, containing some beautifully polished and scored boulders, rests upon a floor of compact Vindhyan limestone, which, when freshly exposed, is found to be striated and grooved in long parallel lines, in the manner so familiar to glacialists." This observation was made "near the little village of Irai, on the right bank of the Pém River, not quite a mile above its confluence with the Wardha, and 10 miles W.S.W. of Chánda." In the Wardha Valley numerous beds of coal occur, one of them 60 feet in thickness. Some of the best, however, are very limited in area. Still the amount of coal is vastly in excess not merely of the present, but even of the probable future wants of the country, both for manufactures and domestic and naval purposes.

Part 2 gives the geology of the Rajmehal Hills, a country bounded on the north and partially on the east by the Ganges, on the south by the Dwarka River and the district of Birbhum, and on the west by the hilly country and the plains of Birbhum and Bhagulpur. One of the most singular geological features of the district is the radiating columnar trap figured in Plate IV., which presents the appearance of pillars radiating out in all directions from a common centre. The region, calmly considered, is pronounced " one of many in India where properly organised commercial enterprise may fairly expect to achieve a reasonable amount of success. There is a total area of about 1200 square miles of coal-field, containing on the lowest estimate 210 million tons of available coal, not of the best quality, but easily accessible. The basaltic trap yields agates, chalcedony, common opal, and various kinds of rock crystal in abundance, but no attempts have yet been made to collect them for commercial purposes.

The Physical System of the Universe, an Outline of Physiography. By SYDNEY B. J. SKERTCHLY, F.G.S., H.M. Geological Survey. London: Dalby, Isbister, and Co. 1878.

THIS is one of the most comprehensive works upon Physiography that has yet appeared. The excellent treatises of Mrs. Somerville and Prof. Huxley are " too much of the earth, earthy," and treat our tiny globe with an excessive amount of consideration. Mr. Skertchly, on the other hand, takes a much broader view of the subject, and refuses to look upon our earth as being the central point of the Universe, regarding it as nothing more than an infinitesimal portion of one vast whole. As might be expected, therefore, the larger half of the book deals with facts and principles which affect the whole of creation, the rest being devoted to the description of the earth, which is looked upon more in the light of a unit in the solar system than as a habitation for man. Mr. Skertchly travels far and wide, taking his reader with him from atoms and molecules to suns and systems.

The author is evidently possessed of the broadest of views in scientific matters, and seems ever ready to seize hold of the newest facts and theories, and lay them before his readers in a digestible form. The latest discoveries relative to the otheoscope and radiometer, for instance, are described at length, and the important light thrown by them upon the modern theory that light, heat, and chemical action are different effects of the same kind of wave-motion, is well brought out. In connection with this theory Mr. Skertchly proposes that we should get rid of all such terms as undulations, vibrations, and waves, and use the neutral term *undæ* for waves of all periods, no matter whether they manifest their existence by giving rise to luminous, heating, or chemical effects. The adoption of some such term would clear away many of the paradoxical expressions which are now used by authors and lecturers, such as radiant heat,—which is not heat until it falls on some particular kind of matter,—dark heat, actinic rays, and so on.

The sun, as may be imagined, receives a large amount of consideration. The latest researches into the constitution of comets are well described; and the way in which recent experiments with the radiometer and otheoscope, and Tyndall on "Amyl Nitrite Clouds," bear on this interesting but mysterious subject is lucidly explained.

Mr. Skertchly writes in a plain lucid style, and never disdains to use a commonplace word or illustration when it will serve to make his explanation plainer. He is an admirable expositor, and irresistibly carries his reader along with him by the charm of his style and the interest which he excites. The book may be read by all classes, from the philosopher and student to the intelligent man of culture who desires to gain some insight into the latest facts and theories relating to the physical constitution of the Universe.

Ure's Dictionary of Arts, Manufactures, and Mining. Vol. iv. Supplement, by Robert Hunt, F.R.S. London: Longmans and Co. 1878.

The editor of a technological work like Ure's well-known Dictionary must have a thankless task, and may be compared to an unsuccessful Hercules trying to stay a perennial hydra. In the present instance Mr. Hunt and his able assistants may be congratulated on the large amount of success with which they have been able to lay before their readers a succinct description of the progress made by modern manufacturers up to the end of last year, and even beyond it. The anxiety shown by the editor to give the latest possible information on every subject is evinced by the fact of his having deemed it necessary to publish an Addendum to his Supplement, for the sake of giving us the discoveries of MM. Pictet and Cailletet, and the newest forms of telephone. It must have been with a slight feeling of mortification that Mr. Hunt must have welcomed the discovery of the microphone, just too late to be included in the new volume.

The system of publishing periodical supplements to such works is an excellent one, and has already been adopted with great success in the case of the sister book, "Watts's Dictionary of Chemistry."

The merits of "Ure's Dictionary" are so well known that we need do no more in the way of praise than to say that the present volume is not only on a par with the preceding ones, but is even superior to them, on account of the very numerous bibliographical references scattered through the book.

Principles of Machine Construction, &c. By the late EDWARD TOMKINS. Edited by HENRY EVERS, LL.D. Vol. i., text. Vol. ii., plates. London: W. Collins, Sons, and Co. 1878.

THIS work, which forms part of Collins's Advanced Science Series, was left partly unfinished by the author, who died before its completion. His original design has, however, been fully carried out by Dr. Evers, who found he could not improve upon it. The first four chapters are devoted to a course of instruction in geometrical and mechanical drawing as applied to machinery, and are fully illustrated by nearly a hundred cuts and six quarto lithographic plates. The different kinds of motion and its transmission, the various forces, the materials of construction and their treatment, occupy the student's attention during the next two chapters, the remainder being devoted to machines in general and their parts; this portion of the book being illustrated by over two hundred and fifty woodcuts and more than forty plates. The plates in the accompanying Atlas, drawn by the late Mr. Tomkins, are well and clearly executed, and are sufficiently large and detailed to be used as drawing copies for advanced students in Science and Art Classes, to whose attention we cordially recommend these volumes.

Free Evening Lectures, delivered in connection with the Special Loan Collection of Scientific Apparatus, 1876. Published for the Lords of the Committee of Council on Education, by Chapman and Hall, 193, Piccadilly.

THE lectures here published were of necessity somewhat popular in their character. As now presented to the reader they labour under the disadvantage of being unaccompanied by the experiments and objects by which they were illustrated when originally delivered. Still they may be read by the general public with profit, and we trust not without pleasure. We must particularly call attention to the note appended to Mr. G. Carey Foster's lecture on "Electricity as a Motive Power," in which the learned author clearly and conclusively demolishes the vague hopes entertained by many that when our coal-fields are exhausted electricity will, in some way or other yet to be discovered, come to our rescue.

Mr. W. J. Harrison's lecture on "Local Geology, with special reference to that of Leicestershire," is interesting from the lucid manner in which the author describes the gradual changes from *Castanea atavia* to *C. vesca*, as traced by Baron von Ettingshausen, in proceeding from more ancient to more modern formations. Mr. Harrison adds—"If we grant that even one species can change in this way, we shall do all that Mr. Darwin desires."

We regret that the work has not appeared earlier, and that it is unaccompanied by an index.

Proceedings of the Literary and Philosophical Society of Liverpool, during the Sixty-sixth Session, 1876-77. London: Longmans and Co. Liverpool: D. Marples and Co., Limited.

This volume contains an account of the papers read and the discussions held at the meetings of the Society. From these we are happy to learn that good work is being done. The Rev. H. H. Higgins, a zealous and able naturalist, has taken a cruise in the West Indian Seas on board the *Argo*, and has brought back and described a large and interesting assortment of sponges, Mollusca, fishes, and cryptogamic plants, which are now lodged in the Liverpool Museum. Several Associates of the Society—among whom we may mention Capt. Perry, Capt. Cawne Warren, Capt. Slack, and Mr. E. Dukinfield Jones—have also contributed observations and collections in various departments of Natural History. A letter from Mr. J. Adams, of Pitcairn Island, read by Mr. J. L. Palmer, R.N., gives an account of the appearance of a sea-serpent on October 15th, 1870. It is represented as from 30 to 40 feet in length, and about a foot or 18 inches in diameter. No improbable or sensational features are ascribed to the animal.

Among the papers read before the Society, and here inserted in full, very few indeed can call for our notice. The Rev. T. P. Kirkman laments to see "crowds of good heads busy with mere observations obtained by rifling sea and land and sky, by dissection of the dead, and vivisection of the tortured living," and wishes that, like himself, they would devote themselves to the discussion of questions which Milton happily represents as engaging the attention of a Literary and Philosophical Society in Pandemonium.

Mr. E. Davies, F.C.S., contributes an interesting paper entitled

"Popular Errors about Poisons." It may, however, be questioned whether he does not go too far in denying the existence of slow poisons whose effects are not perceived until a considerable time after their administration. That death does not necessarily follow immediately after the introduction of a lethal substance into the system is often shown in cases of hydrophobia. Whilst, therefore, we fully join Mr. Davies in repudiating the notion that a poisoner can so regulate the dose given as to ensure the death of his victim at some specific and remote time, we do not think it impossible that a man after having taken a poison may remain perhaps for months in apparent health. Of this we have heard of three painful cases, on what seems to be unimpeachable authority.

Mr. A. J. Mott points out some of the shortcomings of Haeckel's "History of Creation," and contends—erroneously in our opinion—that the disciples of Haeckel are the only logically-consistent Evolutionists. The paper shows extensive reading and acute thought, but to our mind it conveys the impression that Mr. Mott is not a working biologist.

Conferences held in Connection with the Special Loan Collection of Scientific Apparatus, 1876. Chemistry, Biology, Physical Geography, Geology, Mineralogy, and Meteorology. Published for the Lords of the Committee of Council on Education. London: Chapman and Hall, 193, Piccadilly.

This work has been tardy in making its appearance. So much has happened to call public attention in other directions that the Exhibition of Scientific Apparatus of 1876 is now almost forgotten. This book consists of a series of papers or addresses read or delivered by certain eminent men, and followed by some slight discussion, the whole being supposed to be connected with or inspired by the articles then and there exhibited. Though sadly in need of a good index, it contains, as might be expected, no small store of interesting facts and of weighty opinions. Thus Prof. Frankland points out, as an important deficiency in our national appliances for the furtherance of scientific culture, the absence of a museum of chemical products. Every chemist must admit that a collection of this nature, kept well up to the level of the day, would be "of the highest interest both to the student and the investigator." The expense of forming and enlarging such a collection would be smaller than might at first sight be imagined, since there are few discoverers of new compounds who would not think it a privilege rather than a tax to deposit a specimen of their products in a great national collection. Why, then, is Prof. Frankland's suggestion still not acted upon?

That other capitals are also deficient in collections of this kind is a poor apology for London.

Still more important is the paper, read by Prof. Frankland on behalf of Prof. Fremy, on the Endowment of Scientific Research. Our own opinions on this subject have been repeatedly declared; but it is with great satisfaction that we find them essentially shared by a man of science of the standing, authority, and experience of M. Fremy, who has for years been earnestly striving to resuscitate original research in France, and who has even instituted a laboratory in which students who have completed their chemical education are received and allowed to work gratis. We are very happy to find Prof. Fremy's paper thus laid before the British public under the authority of the Educational Committee of the Privy Council.

An interesting fact, "not generally known," occurs in Dr. Gilbert's able paper on "Some Points in the Nutrition of Animals." We learn that on comparing the ox, the sheep, the pig, and man, the proportion of stomach by weight is approximately in oxen 3·2 per cent, in sheep 2·44, in pigs 0·88, and in man only 0·38. The author draws hence the very natural inference that the pig requires a more concentrated and digestible food than sheep and oxen, whilst man, on the other hand, requires a still more concentrated food than the pig, the practical conclusion—unwelcome to vegetarians—being that "man was not made to consume potatoes and cabbages by the bushel."

Some remarks made by Prof. Duncan must be very interesting to all inquirers into the distribution of organic life upon the surface of the globe. He declares that "there is a remarkable isolation, so to speak, of the flora of South-Western Australia. It is isolated from that of South-Eastern Australia, but it is most remarkable in its African affinities, necessitating a belief in the former existence of some land connection between Africa and Western Australia. Again, we find the flora of New Zealand connected with South America and with Eastern Australia." These conclusions, and indeed the facts upon which they repose, seem scarcely to harmonise with the views of Mr. A. R. Wallace and other authorities, based upon a consideration of the distribution of animal life.

There is much other matter in this useful and instructive volume which we should be glad to particularise did space permit. We merely mention the interesting paper by Dr. Cornelius B. Fox, on the Use of Aspirators in Atmospheric Ozonometry; Prof. Donder's memoir on the Velocity of Thought, which when brought to the test is found to be incalculably smaller than was supposed by poets and moralists; and Dr. Royston Pigott's notice of the Microscopic Researches of Dr. Drysdale and the Rev. E. Dallinger. The author remarks that "all conclusions founded upon the non-appearance of animalcules in a given fluid, as seen by microscopes of very great cost and power, fall to the ground unless these peculiar methods are adopted which have now been invented." The bearing of this discovery upon the question of spontaneous generation will at once be recognised.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iv., No. 2. Washington: Government Printing-Office.

This issue includes an Essay which in point of importance, if not of extent, is well worthy to figure as an independent work. Under the tirle "The Geographical Distribution of the Mammalia considered in relation to the Principal Ontological Regions of the Earth, and the Laws that Govern the Distribution of Animal Life," Mr. J. A. Allen criticises the views of Dr. Sclater and Mr. A. R. Wallace,* and expounds in detail a system of his own, of which he published a preliminary notice in 1871, and to which Mr. Wallace refers in his great work. His fundamental principle is that "life is distributed in circumpolar zones under the controlling influence of climate, and mainly of temperature." Quoting from Mr. Wallace the declaration that "the continents, resembling a huge creeping plant with roots at the North Pole, and the matted stems and branches of which cover a large part of the northern hemisphere and send three great offshoots toward the South Pole," he likens the distribution of animal life to that of the land. Near the North Pole the land is comparatively little interrupted, the Eastern and Western Worlds being sundered merely by Behring's Straits. In harmony with this geographical fact we have an Arctic or North Circumpolar realm of animal life whose essential forms are common to the two continents. The further we proceed from the Pole the more we find differentiation increase. Thus Mr. Allen obtains eight primary divisions or "realms"—the Arctic, the North Temperate, the American Tropical, the Indo-African, the South American Temperate, the Australian, the Lemurian, and the Antarctic. Most of these realms are subdivided into "regions," and again into "provinces." The resulting arrangement differs, however, from that of Mr. Wallace less perhaps than might have been at first sight expected, and the same boundary lines are observed in both. Thus, setting the Circumpolar realm aside, Mr. Allen's North Temperate —with its two secondary divisions—coincides with Mr. Wallace's Nearctic and Palæarctic regions. The American Tropical of the one author and the Neotropical of the other differ merely in the circumstance that Mr. Allen has raised its extreme southern portion to the rank of a distinct primary division. The Ethiopian

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* See Geographical Distribution of Animals, by A. R. Wallace.

and the Oriental regions of Mr. Wallace, thrown together, form the Indo-African realm of Mr. Allen; or rather, we may say, that he takes the two primary divisions of Mr. Wallace, and reduces them to a secondary rank. Madagascar and the Mascarenes, instead of viewing as a zoological province of Africa, he elevates to the rank of a primary realm—a view with which we feel disposed to agree. Concerning the Australian region the two authorities agree almost entirely as far as boundaries are concerned, though Mr. Allen decidedly refers to his Indo-African realm the Philippine group, which Mr. Wallace leaves doubtful, and also Celebes. As regards the subdivisions of the Australian realm, Mr. Allen includes in his Papuan province not merely New Guinea, with the smaller islands to the west and the south-east, but Australia proper north of 20° S. lat., which he considers here as approximately representing the isotherm of 70° F.

Into the evidence which the author adduces in support of his classification, and the objections which may be urged against it, space does not at present allow us to enter. He appears to have abandoned his formerly proposed separation of Temperate South Africa as a primary realm because it lies "wholly within the warm-temperate belt, and widens rapidly northward to abut very broadly against the torrid zone." Yet this latter feature belongs no less decidedly to Temperate South America, for which the author still claims the rank of a primary realm. A corollary which can scarcely have escaped the reader is formally stated towards the close of this most interesting treatise:—"The northern circumpolar lands may be looked upon as the base or centre from which have spread all the more recently developed forms of mammalian life, as it is still the bond which unites the whole. Of the few cosmopolitan types that in a manner bind together and connect the whole mammalian fauna of the globe (the Lemurian and Australian Realms in part excepted) nearly all have either their true home or belong to groups that are mainly developed in the northern lands. But if this be true of the mammalia must it not hold good of all the great animal groups, and indeed of plants also? If not, the value of the mammalia as a guide to the determination of zoological regions becomes exceedingly doubtful. But if so, we are led to the conclusion that the dawn of life must have been not in the equatorial, but the arctic regions, and that not merely the first animal forms altogether, but the first of each, at least of the grand divisions of the animal kingdom, must have been there developed.

It is obviously impossible in a brief notice like the present to do justice to a work which will doubtless receive from biologists wide-spread and serious attention. If Mr. Allen's time and duties permit it, he could scarcely confer a more valuable boon upon science than the working out of his hypothesis for the animal world at large.

Records of the Geological Survey of India.

Vol. x., Part 4, contains an account of the Geology of the Mahanadi Basin and its vicinity, by Mr. V. Ball, with a notice of the diamonds, gold, and lead-ores of the Sambalpur district, by the same author. The diamonds appear to be decidedly a thing of the past. About 1856 the right of seeking for diamonds in this district was leased by the Government at the low rate of 200 rupees per annum, but the lessee abandoned the undertaking as unremunerative. The author, however, thinks that the southern channel of the Mahanadi might prove more productive than the northern, which has been the scene of all recorded and traditional explorations. The matrix of the diamonds is probably situate in the sandstones and shales of the Barapahar Hills. Beryl, topaz, carbuncle, amethyst, cornelian, and clear quartz were formerly collected in the Mahanadi, probably derived from the metamorphic rocks.

Gold-washing is chiefly confined to the small jungle streams, but the quantity collected does not appear considerable.

Galena, containing about 12 ozs. of silver to the ton of lead, has been found near Sambalpur, but the investigations made were not on a sufficient scale to decide whether it occurs in remunerative quantities.

The principal papers in vol. xi., part 1, are Mr. Lydekker's Notices of the Fossil Mammals of the Siwalik, and Mr. Blandford's Reply to Dr. Feistmantel on the Palæontological Relations of the Gondwana System.

Report of the United States Geological Survey of the Territories. By F. V. Hayden. Vol. vii. Washington: Government Printing Office.

This volume contains a description of the tertiary flora of the western territories. The descriptions are full, and the illustrations numerous and well executed.

Reports of the United States Geological Survey of the Territories. Vol. xi. Monograph of North-American Rodentia. By Elliott Coues and J. A, Allen. Washington: Government Printing Office.

We have again good cause to congratulate the Staff of the United States Geological Survey on the work they are accomplishing. We have here a most elaborate account of the rodents of North America, arranged in their eleven families. Under each species we find a full account of its specific characters and of its geographical distribution. Tables of measurements, generally made upon a great number of specimens, are appended; the synonymy is given in full; and the characteristics of varieties, if such exist, are pointed out. In addition, the generic and sub-generic peculiarities are explained in detail. The only deficiency which we find is in the habits of the various species, concerning which the information supplied is meagre. The carnivorous nature of some of the Spermophile squirrels is, however, clearly shown—a fact of great importance, and one which proves the necessity of great caution in arguing from the structure of an animal to its habits, and especially its diet.

There is also a synoptical list of the extinct Rodentia of North America, and a most elaborate and valuable bibliography of North-American Mammalia.

Each section of the work is accompanied with engravings showing the skulls of the principal species described. The index is scarcely as full as might reasonably be expected from a work of this character.

Heaven and Hell, or the Divine Justice Vindicated in the Plurality of Existences. Containing a Comparative Examination of the Various Doctrines concerning the Passage from the Earthly Life to Spirit Life; Future Rewards and Punishments; Angels, Devils, &c.: followed by numerous examples of the State of the Soul, during and after Death; being the practical confirmation of the Spirits' Book. By ALLAN KARDEC. Translated from the Sixtieth Thousand by ANNA BLACKWELL. 1878. London: Trübner and Co., Ludgate Hill.

THE volume before us is one of a series explaining and defending the doctrines held by Spiritualists. The first principles of Spiritualism are dealt with in previous works; the one under consideration treating, as its title denotes, of the state of the Spirit after its release from corporeal bondage. Basing his arguments upon the plurality of existences or successive terrestrial incarnations, the author proceeds to consider Heaven and Hell from Christian, Pagan, and Spiritualist standpoints, and endeavours to prove the triumph of Spiritualistic doctrine over those preceding its revelation. The subject is most exhaustively considered, embracing the actual situation of Heaven and Hell, the duration of happiness and misery, the existence of Angels and Demons, Purgatory, &c. Throughout the work is remarkable for its sound argument and practical common sense, and those who are most disposed to disagree with the principles enunciated cannot fail to admire the research and force of reasoning that is displayed. The second portion of the work is devoted to examples of statements made in the earlier chapters, being the revelations of released Spirits, and is chiefly of interest as relating to the future of the suicide and to terrestrial expiations. Suicide, under any circumstances, is denounced as crime, and the fate of the unhappy beings who yield to the temptation is vividly depicted. The translator's commentaries form a valuable addition to the work, which will be appreciated as a scholarly adjunct to Spiritualistic literature.

SCIENTIFIC NOTES.

By far the most prominent event of the quarter has been the discovery of the Microphone. The scientific world had scarcely recovered from the surprise excited by the wonders of the Telephone and Phonograph, when Prof. Hughes, the well-known electrician, announced that the electrical resistance of certain bodies was influenced by sonorous vibrations propagated in their vicinity, just as selenium is influenced by light, and that in so supreme a degree as to magnify sounds of the most delicate nature to such a pitch that the tramp of a fly on a wooden box could be distinctly heard. The way in which Prof. Hughes discovered this important fact was the following:—He was trying some experiments on the changes which take place in the electrical resistance of a strained wire, using for the purpose a closed circuit, including a small battery and a Bell Telephone. He noticed that, although no change was apparent when the stretched wire was spoken to, when it broke a rush of sound was heard in the receiving Telephone, the noise being repeated when the wires were re-united. By simply crossing the broken wires, and placing a small weight on the top of them, it was found that sounds uttered close to them were repeated in the Telephone. Other conducting materials were tried with similar effect, some of them—such as gas carbon and pine charcoal—being more sensitive than others. The problem solved by Prof. Hughes's discovery is to introduce into an electrical circuit an electrical resistance which shall vary in exact accord with sonorous vibrations, which are thus transformed into electrical vibrations, to appear once more as sound in the ear-piece of the Telephone; in other words, the molecules of conducting bodies in molar contact and under pressure are so regulated in their motions by the waves of sound in their neighbourhood that they decrease and increase the resistance of a circuit to a remarkable degree. A great variety of Microphones were constructed by Prof. Hughes, of all kinds of materials, each having a special range of variation of resistance. Thus a simple stick of charcoal resting on another is so sensitive that a fly's tramp may be distinctly heard; but it would be too delicate for the human voice, the vibrations caused by this being too powerful. In transmitting articulate sounds the area and the number of the points of contact must be increased so as to reduce the extreme sensibility of the instrument. For magnifying the human voice Prof. Hughes uses a Microphone consisting of a glass tube containing several cylinders of pine or willow charcoal in close contact, the two end pieces being in circuit. Mr. Blyth, F.R.S.E., has succeeded in making a most efficient articulating Microphone out of a jam-pot, or a wooden box containing gas-cinders: electrical continuity is insured by the ends of the circuit being connected with two tin plates thrust between the cinders and the side of the vessel. In another experiment he found that by wetting the cinders he could do away with the battery, while, on the other hand, by using a stronger battery he found that the cinder boxes would act both as receivers and transmitters, thus doing away with the necessity for using a magnetic Telephone. The sounds in the latter case were very faint, and not easily distinguished; but the fact remains that they were transmitted, nothing being in circuit but a couple of cinder boxes and two Grove's cells. Hughes's Microphone has already been applied most successfully as a test for the presence of stone in the bladder and foreign solid bodies in the animal tissues. Experiments of a very promising nature have also been made with the Microphone as a substitute for the Stethoscope in lung and heart disease. Mr. W. J. Lancaster, F.C.S., has rendered the Microphone still less complicated, by doing away with the subsidiary battery, and using the negative plate of a simple carbon and zinc pair as the resonator. Mr. Lancaster's Pile Microphone, as he calls it, consists of a thin wooden box, 6 ins. × 4 ins. × 1 in., upon the top of which is screwed a plate of zinc, 4 ins. × 2 ins., and about one-tenth of an inch thick, somewhat smaller than the top of the box. On this is placed a layer of blotting-paper soaked in dilute sulphuric acid, and on this again a gas carbon-plate, ½ an inch thick; a second, but smaller and thinner, plate of carbon is suspended over the first, at about 2 inches distance, by being screwed to an upright which passes through the plates of the pile into the box below. Against the suspended plate rests a thin upright rod or plate of carbon, the lower end of which is pointed, and rests in a depression scooped out of the lower plate and filled with mercury. Mr. Edison, the inventor of the Phonograph, lays claim to the priority of discovery, on account of his having discovered that the compression of a piece of black-lead interposed in a circuit proportionately altered its electrical resistance, but any discussion as to the merits of the rival discoverers would at present be premature and out of place.

At the March meeting of the Physical Society a new battery of extraordinary capabilities was exhibited and described by Mr. W. H. Preece. It was specially devised by Dr. Byrne to be used by medical men in certain cauterising operations. It consists of a simple cell of platinum and zinc, excited by a mixture of potassium bichromate, sulphuric acid, and water. The negative plate is compound, being backed with lead to which is soldered a sheet of copper, which is again covered with lead. The lead back is covered with a non-conducting varnish, the platinum surface being of course left free. While in use a stream of air is forced through the liquid by a small air-pump, stirring it up actively, the heating power being thereby largely increased. The action of the air is proved to be entirely mechanical, and seems to work by exposing the zinc plate to successive layers of fresh acid. With an 18-inch inductorium sparks of over 17 inches could be obtained from a large 10-cell battery, but immediately the current of air was stopped the spark fell to 8 inches. With a battery of four cells, measuring 4 inches by 2 inches, a length of 6 inches of platinum wire, No. 18 (0·05 in.), was heated to bright redness. With the larger 10-celled battery no less than 30 inches of No. 14 were heated.

At another meeting of the same Society Prof. S. P. Thompson exhibited and described a cheap and efficient form of optical bench. Two straight oak bars, about 2 metres in length, and clamped together as in a lathe-bed, and a number of slides carrying various appliances slide easily without shake, and can be fixed in any position by wedges. The several frames carrying the diffraction grating or edges, the eye-piece (with an engraved glass micrometer), &c., are so made, in wood, as to be capable of adjustment in any plane, and the instrument can also be employed for making photometric and other similar measurements. The mean of two determinations for the wave-length of certain red light gave 0·000629 as compared with Fresnel's figure, 0·000640, while the total cost did not exceed £3.

The following method of precipitating the gold contained in old toning-baths is proposed by F. Haugh. The baths when no longer fit for use are filtered into a white glass flask, rendered alkaline with a little bicarbonate of soda, and a concentrated alcoholic solution of magenta is added drop by drop until the liquid has taken the deep red hue of syrup of raspberries. The flask is then exposed for six to eight hours to the light of a bright window. At the end of this time the gold is found to be deposited as a violet powder, whilst the supernatant liquid has become colourless. It is carefully decanted, so as to preserve merely the deposit. When a sufficient quantity of protoxide of gold has thus been collected, it is carefully washed upon a filter, dried, and the filter is burnt. The dry residue and the ash of the filter are then dissolved at a gentle heat in an excess of *aqua regia*, and the solution—diluted with distilled water—is separated from the insoluble substance by filtration.

For the restoration of writing effaced by time M. E. von Bibra proposes to moisten the writing with a moderately concentrated solution of tannin, the excess of which is then removed by the application of the washing-bottle, and the paper dried at 65°.

The same gentleman has also given, in the *Bulletin de la Societe Chimique de Paris*, a process for seasoning new casks. He proposes to eliminate all soluble matter from the interior of the staves by the use of crystals of soda, of which 1 kilo. is used per hectolitre of the contents. The cask is first filled two-thirds full of clean water, the proper quantity of solution of soda is added, and after the liquid is mixed the cask is filled to the bung. After standing for ten or twelve days the alkaline liquid is run out, and the cask repeatedly rinsed with clean water.

Dr. J. H. Gladstone has examined some candles which are stated to have been recovered from the wreck of a Dutch vessel sunk in Vigo Bay during the war in the year 1702. As the vessel was supposed to be a treasure ship many attempts have been made to recover its contents, and this was successfully accomplished in 1875, when these candles among other things were obtained. They have therefore been submerged for 173 years. The wick has rotted away, leaving scarcely any trace of its existence, while the fatty portion has become a friable heavy substance, of a dull white colour. The candles bore evidence of having been made by dipping, for the concentric layers were easily separated from one another, and this facilitated the examination of the outer and inner portions of the same piece. Both the outer and inner portions still contain some of the fat apparently unchanged; they are unctuous to the touch, and have a fatty odour, and when heated to a little below 110° C. they began to change colour; at 140° they softened, and at 200° small portions were melted out. The most interesting point is, that whereas the fats have been in contact with a practically unlimited amount of sea-water for 173 years, and a chemical change between them has been possible, the double decomposition has proceeded so extremely slowly that the reaction is only about half completed at the present time.

Mr. T. A. Readwin records a curious instance of spontaneous metal-growth. About twelve years ago he put about an ounce of rolled metallic cadmium (coiled six times) into a clear flint-glass phial, corked it tightly, cut the cork level with the mouth of the phial, and sealed it very carefully. At the time the metal did not quite reach up to the neck of the phial. Since that time it has elongated more than a quarter of an inch, and, during the present year, the sealing wax has been broken all round and the cork forced outward more than one-eighth of an inch. The cork is even now fairly tight, but the metal follows after it to touching forcibly, and probably the cork will be presently completely forced out. The cadmium is fast oxidising and endeavouring to uncoil itself, and the cut edges at the bottom of the phial are becoming foliated.

We have received a number of biscuits and other preparations containing preserved solid and liquid food, both animal and vegetable, which are the practical results of a new process lately patented by Dr. Campbell Morfit. They include substances of the most diverse nature, such as milk, cream, cheese, beef, garden rhubarb, cabbage, tomato, pork sausage, and a variety of other alimentary products, all of which are perfectly savoury and toothsome, in spite of their being more than a year old It is, however, more with Dr. Morfit's process than with its present results that we have now to deal, for we must look upon his discovery as being yet in its infancy. Dr. Morfit's experiments, which he has prosecuted uninterruptedly for the last two years, seem to prove that ordinary gelatin, when it is once thoroughly diffused through a vegetable or animal substance, and dried in and with it, will protect it from decomposition or other alteration for a prolonged period, in spite of atmospheric or climatic changes. This is clearly proved by the samples submitted to us, which—although they have been exposed to the constant changes of temperature and moisture consequent on their having been kept for more than a year in the store-room of an ordinary dwelling-house—are still perfectly good and sweet, their natural characteristic flavours being well preserved. Some lime-fruit juice biscuits, for instance, which are more than a year old, have preserved, in a very perfect manner, the peculiar flavour by which the juice of the lime can always be distinguished from that of the lemon. The primary principle of Dr. Morfit's process is the getting rid of nearly the whole of the natural water contained in the substance to be preserved, by submitting it to a certain degree of heat, the place of the water being supplied by gelatin. The compound is then dried, and in this state may be kept for any length of time, or else it may be made up into biscuits by incorporating it with biscuit-powder. Let us take Dr. Morfit's method of preserving beef as an example. The beef must be as free from fat and bone as possible, and should be first stewed in its own liquor, or with the least possible quantity of water, and seasoned or not according to taste. The whole is then reduced, by any available mechanical means, to a state of smooth and fine pulp, and triturated with a solution of gelatin in water. One pound of gelatin is enough for 15 pounds of meat, fowl, or fish, the gelatin being dise solved either in a sufficiency of water or in the natural juice of the substanc- itself. In the case of fruit—such as gooseberries, currants, or plums—they are stoned or skinned when necessary, and cooked or not, as the case may be. They are then made into a pulp and mixed with gelatin dissolved in water or their own juice, heated so as to insure a thorough mixture of the ingredients, and then poured into coolers. In certain cases the gelatin may be replaced by mucilage of Irish moss, but the result, although cheaper, is not so good. Dr. Morfit's method of condensing milk without the use of sugar is of great interest, seeing that the Swiss and other descriptions of condensed milk, which are now so largely sold, cannot be taken by delicate infants or by persons of weak digestion, owing to the large amount of sugar in them. One pound of gelatin is dissolved in one gallon of fresh milk at a temperature of from 130° to 140° F., the whole being allowed to set into a jelly, which is dried. The dried jelly is then dissolved in another gallon of fresh milk, and allowed to set and dry as before, the operation being repeated with fresh milk until the original pound of gelatin has taken up eight gallons of milk or more. Consommé of meat may in like manner be condensed until one pound solid shall represent thirty times its weight of fresh beef. As may be readily guessed, the process may be carried on without any of the expensive plant and troublesome manipulation involved in the usual modes of condensing milk and making Liebig's extract, besides which, in the latter case, the whole of the nitrogenous parts of the meat are preserved intact. From a hygienic point of view, the lime-fruit juice biscuits ought to be admirably suited for use in the Navy. Without entering into the question as to whether it is the citric acid, or the phosphatic salts, or the potash, contained in the lime-juice that is the real anti-scorbutic agent, it is sufficient to say that the 40 per cent. of Montserrat lime-fruit juice preserved by Dr. Morfit's process, and incorporated with the biscuits, has preserved all its properties without any change for more than a year, and, *à priori*, there is no reason to suppose that it would not keep good for ten or twenty times that period. It may be mentioned, in conclusion, that the different jellies may be dried into hard tablets or flakes at a uniform temperature of from 38° to 40° C., and sent into the market in this convenient form, as well as under the more bulky guise of biscuits. A few cases of lime-fruit juice tablets, prepared according to Dr. Morfit's method, would probably have saved the lives of several brave men during the late expedition to the Polar regions. Speaking from a purely scientific point of view, and judging by the results we have already described, the principle of Dr. Morfit's invention seems to be theoretically a sound one. These results we must regard at present as tentative, and it only remains to the inventor of the process to confer a large benefit on the community by extending its application, thereby notably increasing our not too abundant stock of hygienic and alimentary products.

The Ross microscope, as remodelled by Mr. F. H. Wenham, and described in the "Quarterly Journal of Science" for 1873 (p. 422), has again been greatly improved by the same ingenious gentleman. The general form of the stand resembles the former one, but the weight has been diminished without in any way interfering with its stability. The stage is also somewhat lower, rendering manipulation more convenient. The fine adjustment no longer acts upon an adapter in the nozzle, but bears directly upon a slide carrying the whole body of the instrument. This contrivance gives great accuracy and steadiness of movement, and prevents the alteration of magnifying power inseparable from the older contrivance, and which gives great trouble when micrometers are in use. The stage has been simplified and rendered extremely thin, without sacrificing any useful movement. Below the stage the American swinging bar has been adopted, allowing the mirror, with the whole of the very simple yet effective sub-stage apparatus, to be moved out of centre, even to the extent of being placed *above* the stage, while its accurate placing in the axes of the microscope when required is secured by means of a clamping-screw. The scope offered for the employment of great variety of oblique illumination by simple contrivances, such as spare objectives and eye-pieces used as condensers, will suggest itself, while the microscope will still carry every other kind of illuminator. Notwithstanding these advantages, the simplification in construction has enabled the stand to be made at 25 per cent less cost than its predecessor.

The adjustment collar, a source of much trouble to the less expert observers with high powers of large angular aperture, has been dispensed with in an objective constructed by Herr Zeiss, of Jena, at the suggestion of Mr. J. W. Stephenson, F.R.A.S., from the formula of Prof. Abbe. The objective is on the immersion principle, and depends for its properties upon a fluid being employed having the same refraction and dispersion as crown glass. After many trials it was found that oil of cedar-wood gave perfect definition with oblique light, but for central illumination was greatly improved by the addition of one-fourth or one-fifth of oil of fennel seed (*Ol. Fœniculi*). The objective has a balsam angle of 113° = 1·25 numerical aperture.* It has a large working distance. The space between the front lens and the object is 0·02, which gives a working distance 0·012 for 0·008 cover-glass, 0·016 for 0·004, and so on. Its power is rather more than one-ninth, and, having component lenses throughout the combination larger than in other objectives of the same power, it transmits more light.

Two numbers of the "Journal of the Royal Microscopical Society" have been issued. Although the amount of printed matter is less than under the former arrangement, and the issue is only bi-monthly instead of monthly, the Fellows of the Society are decidedly gainers by the change, as—the Council now having entire control over the publication of their Transactions—a quantity of controversial matter, which detracted greatly from the status of the former journal, is rigidly excluded. Besides papers read before the Society, the Journal contains a well-compiled abstract of Microscopy from foreign and other sources. It also contains a list of articles on microscopical subjects, published in various British and foreign periodicals.

The Atlas of Colorado, issued by the United States Geological Survey of the Territories under Prof. F. V. Hayden, embodies the results of the geological and geographical work of the Survey during the years from 1873 to 1876 inclusive. This atlas will contain the following maps:—1st. A general drainage map of Colorado on a scale of twelve miles to the inch. 2nd. An economic map of the same region, having as its basis the above-mentioned drainage map. This map will indicate the areas of arable, pasture, timber, coal, mineral, and desert land in as great detail as possible on the scale. 3rd. A general geological map on which the areas covered by the principal formations will be shown. The drainage map will form the basis for this also. 4th. A map showing the scheme of the primary triangulation in the State. Scale twelve miles to the inch. 5th. Six topographical sheets, showing the same area as that covered by the general drainage map, but in much more

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* Numerical aperture is product of the sine of the semi-aperture with the refractive index of the medium in which the observation is made (*vide* "Description of Professor Abbe's Apertometer," Trans. Roy. Micr. Soc., Dec. 5, 1877, vol. i., p. 19).

detail. The scale of these sheets is four miles to an inch. The relief of the country is indicated by contour lines, at vertical intervals of 200 feet. The area covered by each of these sheets is 11,500 square miles. 6th. Six geological sheets, of which the basis are the six topographical sheets just mentioned. On these the detailed geology is expressed by colours.

The "Mineralogical Magazine," which contains the proceedings of the Mineralogical Society, in addition to other interesting items about minerals, has now attained its second year, and, judging by its contents, seems likely to have many years of prosperity before it. Chemists, geologists, and mineralogists alike will find valuable matter in its pages. Judging by the balance-sheet, the Mineralogical Society appears to be growing in numbers and worldly possessions.

Captain Burton's expedition to the Land of Midian seems to have done a large amount of good work in a short time. After four months' absence, they brought back an immense number of entomological and other specimens with them, 25 tons of which were illustrative of the geology and mineralogy of the districts through which they passed. The country seems a rich one. Several gold deposits of an easily workable nature have been discovered in the south of Midian, while in the north there are silver and copper deposits of great value. Three turquoise mines were found, of which one is being roughly worked, besides sulphur beds, rock salt, two salt lakes, gypsum mines, and alabaster quarries. Part of the ores will be retained in Cairo for analysis, and the rest sent to London and Paris. Colonel Gordon's expedition through the Khedive's newly acquired territory in Southern Egypt has also come across several auriferous and argentiferous deposits, specimens of which have been sent to M. Daubrée, the director of the Paris School of Mines, for analysis.

The Hygienic Society of Paris has arranged with Captain M. Giffard for the performance of a number of scientific experiments on the effects of diminished barometric pressure on respiration and other vital processes by the aid of his giant captive balloon. We are glad to hear that this magnificent machine is to be put to a scientific use.

The French Chamber of Deputies has voted a sum of 690,000 francs for the construction of a new astronomical observatory at Meudon, near Paris, on the site of the old chateau which was destroyed by the Germans in 1871. Of this munificent sum, 390,000 francs is to be spent on a refractor, 250,000 francs on the building, and the rest in extra salaries and incidental expenses. The whole is expected to be completed within two years. The separation of the staff of the astronomical and meteorological departments has been decided upon, and, taking advantage of the good example set them, the Swedish Diet has granted the sum necessary for building a separate meteorological observatory at Upsala, where so much good work has been done.

We regret to have to announce the death of the secretary and director of the Smithsonian Institution, Washington, D.C., Joseph Henry, LL.D., which occurred on Monday, May 13th. Professor Henry was born in Albany, in the State of New York, December 17th, 1799. He became Professor of Mathematics in the Albany Academy in 1826; Professor of Natural Philosophy in the College of New Jersey, at Princeton, in 1832; and was elected the first Secretary and Director of the Smithsonian Institution in 1846. He received the honorary degree of Doctor of Laws, from Union College, in 1829; and from Harvard University in 1851. He was President of the American Association for the Advancement of Science in 1849; was chosen President of the United States National Academy of Sciences in 1868; President of the Philosophical Society of Washington in 1871; and Chairman of the Light-House Board of the United States in the same year; the last three positions he continued to fill until his death. Professor Henry made contributions to science in electricity, electro-magnetism, meteorology, capillarity, acoustics, and in other branches of physics; he published valuable memoirs in the transactions of various learned societies of which he was a member; and devoted thirty-two years of his life to making the Smithsonian Institution what its founder intended it to be, an efficient instrument for the "increase and diffusion of knowledge among men."

At a Special Meeting of the Board of Regents of the Smithsonian Institution, held on May 17, 1878, Professor Spencer Fullerton Baird, for many years the assistant secretary of the Institution, was duly elected as the Secretary of the Smithsonian Institution, to succeed the late Professor Joseph Henry.

We are indebted to Prof. F. V. Hayden for an account of the field-work of the United States Geological and Geographical Survey of the Territories, under his direction, for the season of 1877. From it we learn that in 1874 the photographic division of the United States Geological Survey was instructed, in connection with its regular work, to visit and report upon ruins of an extensive and interesting character, which were known to exist throughout New Mexico and Arizona, and in pursuance of this object made a hasty tour of the region about the Mesa Virde and the Sierra el Late, in South-western Colorado, the results of which trip, as expressed by Bancroft, in the "Native Racesof the Pacific Coast," "although made known to the world only through a three or four days' exploration by a party of three men, are of the greatest importance." The following year the same region was visited by Mr. W. H. Holmes, one of the geologists of the Survey, and a careful investigation made of all the ruins. Mr. Jackson, who had made the report the previous year, also revisited this locality, but extended his explorations down the San Juan to the mouth of the De Chelly, and thence to the Moqui villages in North-eastern Arizona. Returning, the country between the Sierra Abajo and La Sal and La Plata was transversed, and an immense number of very interesting ruins were first brought to the attention of the outside world by the report which was published the following winter by Messrs. Holmes and Jackson, in the survey, vol. ii., No. 1. The occasion of the Centennial Exhibition at Philadelphia led to the idea of preparing models of these ruins for the clearer illustration of their peculiarities, four of which were completed in season for the opening of the Exhibition. A study of the models give a very excellent idea of the ruined dwellings themselves. The first of these models, executed by Mr. Holmes, with whom the idea originated, represents the cliff house of the Mancos Canon, the exterior dimensions of which are 28 inches in breadth by 46 inches in height, and on a scale of 1·24, or 2 feet to the inch. This is a two-story building, constructed of stone, occupying a narrow ledge in the vertical face of the bluff 700 feet above the valley, and 200 feet from the top. It is 24 feet in length and 14 feet in depth, and divided into four rooms on the ground-floor. The beams supporting the second floor are all destroyed. The doorways, serving also as windows, were quite small, only one small aperture in the outer wall facing the valley. The exposed walls were lightly plastered over with clay, and so closely resembled the general surface of the bluff that it becomes exceedingly difficult to distinguish them at a little distance from their surroundings. The second model of this series was constructed by Mr. Jackson, and represents the large "cave town," in the valley of Rio de Chelly, near its junction with the San Juan. This town is located upon a narrow bench, occurring about 80 feet above the base of a perpendicular bluff some 300 feet in height. It is 545 feet in length, about 40 feet at its greatest depth, and shows about 75 apartments on its ground-plan. The left-hand third of the town, as we face it, is overhung some distance by the bluff, protecting the buildings beneath much more perfectly than the others. This is the portion represented by the model. A three-story tower forms the central feature; upon either side are rows of lesser buildings, built one above another upon the sloping floor of rock. Nearly all these buildings are in a fair state of preservation. This model is 37 by 47 inches, outside measurements, and the scale 1·72, or 6 feet to the inch. A "restoration" of the above forms the third in the series, of the same size and scale, and is intended, as its name applies, to represent as nearly as possible the original condition of the ruin. In this the approaches were made by ladders and steps hewn in the rock, and the roofs of one tier of rooms served as a terrace for those back of them, showing a similarity, at least, in their construction to the works of the Pueblos in New Mexico and Arizona. Scattered about over the buildings are miniature representations of the people at their various occupations, with pottery and other domestic utensils. The "triple-walled tower," at the head of the McElmo, is the subject of the fourth model. It was constructed by Mr. Holmes, and represents, as indicated by its title, a triple-walled tower, situated in the midst of a considerable extent of lesser ruins, probably of dwellings, occupying a low bench bordering the dry wash of the McElmo. The tower is 42 feet in diameter, the wall 2 feet thick, and now standing some 12 feet high. The two outer walls inclose a space of about 6 feet in width, which is divided into 14 equally-sized rooms, communicating with one another by small window-like doorways. The next is a "cliff-house" in the valley of the Rio de Chelly. It is about 20 miles above the cave town already spoken of. This is a two-storey house, about 20 feet square, occupying a ledge some 75 feet above the valley, and overhung by the bluff. The approach from the valley is by a series of steps hewn in the steep face of the rock; and this method was the one most used by the occupants, although there is a way out to the top of the bluff. This model is 42 inches in height by 24 broad, and is built upon a scale of 1·36. Téwa, one of the seven Moqui towns in North-eastern Arizona, is a very interesting and instructive model, representing, as it does, one of the most ancient and best authenticated of the dwellings of a people who are supposed to be the descendants of the cliff-dwellers. Téwa is the first of the seven villages forming the province as we approach them from the east, and occupies the summit of a narrow mesa some 600 feet in height and 1200 yards in length, upon which are also two other somewhat similar villages. The approach is by a circuitous road-way hewn in the perpendicular face of the bluff which surrounds the mesa upon all sides. It is the only approach accessible for animals to the three villages. Other ladder-like stairways are cut in the rock, which are used principally by the water-carriers, for all their springs and reservoirs are at the bottom of the mesa. This village is represented upon a scale of 1 inch to 8 feet, or 1·96. The dimensions of the model are 36 inches in length, 29 inches in width, and 14 inches in height. In the spring of 1877 Mr. Jackson made a tour over much of the northern part of New Mexico, and westward to the Moqui towns in Arizona, and secured materials for a number of very interesting models, illustrating the methods of the Pueblos or town-builders in the construction of their dwellings. Two villages have been selected for immediate construction, as showing the most ancient and best known examples of their peculiar architecture, viz: Taos and Acoma; the one of many-storied, terraced houses, and the other built high up on an impregnable rock. The model of Taos is now completed, the dimensions of which are 42 by 39 inches, and the scale one inch to twenty feet, 1 : 240. Of this town Mr. Davis says: It is the best sample of the ancient mode of building. Here are two large houses three or four hundred feet in length, and about one hundred and fifty feet wide at the base. They are situated upon opposite sides of a small creek, and in ancient times are said to have been connected with a bridge. They are five and six storeys high, each story receding from the one below it, and thus forming a structure terraced from the top to bottom. Each storey is divided into numerous little compartments, the outer tier of rooms being lighted by small windows in the sides, while those in the interior of the building are dark, and are principally used as store-rooms. * * * The only means of entrance is through a trap-door in the roof, and you ascend from storey to storey by means of ladders on the outside, which are drawn up at night. Their contact with Europeans has modified somewhat their ancient style of buildings, principally in substituting doorways in the walls of their houses for those in the roof. Their modern buildings are rarely over two storeys in height, and are not distinguishable from those of their Mexican neighbours. The village is surrounded by an abode wall, which is first included within the limits of the model, and incloses an area of eleven or twelve acres in the extent. Within this limit are four of their *estufas* or secret council-houses. These are circular underground apartments, with a narrow opening in the roof, surrounded by a palisade, ladders being used to go in and out. These models are first carefully built up in clay, in which material all the detail is readily secured, and are then cast in plaster, a mould being secured by which they are readily multiplied to any extent. They are then put in the hands of the artists and carefully coloured in solid oil paints to accurately resemble their appearance in nature, and in case of restorations or modern buildings, all the little additions are made which will give them the appearance of occupation. The Survey is in possession of the data for the construction of many more models, and they will be brought out as opportunity is given. They have also, in connection with the views, multiplied many of the curious pieces of pottery which have been brought back from that region by the various parties connected with the survey.

Mr. J. J. Maclaren writes to ask us to correct an error which occurred in the review of his work on the "Chemical Difficulties of Evolution," which appeared in the April number of this journal. He says:—"I am *not* the author of the work on Darwin by James Maclaren (published in 1876), to which your reviewer refers in his article, and, in so far as he has censured the author of that work, he has been under a mistake, and has done that gentleman an injustice. If your reviewer will look at the title-pages and the prefaces of the two books, he will at once see the mistake which he has made. As it is, the similarity of name, and the accident that the excellent printing and paper of the book to which your reviewer refers led me to employ the same publisher, have led him into an error of which I am bound to take notice, and which I am confident you will wish to correct at once."

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I. FAMINES IN INDIA.

By Fred. Chas. Danvers.

FEW, if any, countries of the world have escaped the scourge of famine at one time or another of the period of their national existence. The earliest calamities of this kind, of which any records exist, occurred in Palestine and the surrounding countries, in the days of Abraham, Isaac, and Jacob; but little is known of the numbers and extent of the famines that occurred in the world before the commencement of the Christian era, those historians who have recorded such events being few and far between, and the information they have furnished concerning them is but scanty. It is not intended, on the present occasion, to attempt to trace the famines of the world, so far as the materials for that purpose might be available, but to confine our observations and remarks to those that have, in times past, desolated our Empire in the East. Famines in Europe are not now of such frequent occurrence as in bygone centuries,—indeed they may be said to have altogether ceased; but in India they appear to occur more frequently than of old, and in greater severity than formerly. In making this remark, however, it must be observed that there is no sure evidence that such is the case absolutely, but there certainly is tolerably reliable proof that—in the East Indies, at least—they have increased in severity within the last hundred years; but previously to 1770 the records of Indian famines are scanty, and probably not wholly reliable as to details. A more important question for consideration than the mere frequency of famines is, however, the cause of famines, and, consequent upon that, the best means to be employed for meeting them, or for counteracting, to a greater or lesser degree, their effects. Whatever may be the primary cause, or causes, of the meteorological disturbances which interfere with the usual amount of rain falling in any year, it seems very questionable whether human ingenuity will be able to devise means for regulating atmospheric phenomena ; nevertheless it is certain that the advancement of civilisation, with its attendant circumstances, does, in some way or another, not only render a land less liable to the scourge of famine, but it also provides the means for mitigating its effects on the occurrence of a visitation of this nature. The events of recent years lead, however, to the painful conclusion that—so far as India is concerned—practical civilisation has gone a very little way, when we find that hardly more than a first step has yet been taken in the successful application of our knowledge and material resources to warding off calamities such as these. In India the amount of annual rainfall varies considerably in different localities ; for whilst Sind and parts of Rajputana and the Punjab are comprised within an arid zone, and have an average rainfall of less than 10 inches, the central table land of the Peninsula, known as the Deccan, and the greater part of Mysore, have a fall of less than 30 inches, in the extreme east the amount of rain reaches from 80 to upwards of 100 inches. The absolute insufficiency of the rainfall, within the arid zone, to support cultivation, has led the inhabitants of those parts, from the very earliest date, to make up for the deficiency by turning the waters of the Indus and its feeder rivers over the land during the season of agriculture. These parts may therefore be fairly said to be placed beyond serious risk of famine through drought, the rivers whence their means of irrigation are obtained being fed by the perpetual snows of the Himalayas ; and even though their waters may occasionally partially fail, it appears that, when this is the case, the inconvenience caused is very limited and local, and of no serious moment. In Southern India the enormous numbers of tanks, which owe their existence to former dynasties, testify to the necessity that has, for ages past, existed for the careful husbanding of water for the purposes of irrigation.

The precautions taken in those parts where the rainfall is but limited, to provide water for the soil, being, as they were, unquestionably undertaken to provide against loss of crops, and therefore against famine, it is not unreasonable to suppose that the necessity of these works of artificial irrigation was impressed upon the rulers, by whom they were constructed, by the hard lesson of experience through drought and famine. Although it is certain that India has been subjected to famines from the very earliest ages, very little information now exists regarding those that occurred more than 108 years ago, but from that date there are to be found tolerably accurate particulars of all that have since taken place. Ferishta, the Persian historian, mentions a famine in India during the reign of one Jei-chund, who was Emperor of India between the years 503 and 443 B.C. This is probably the earliest of which any record exists, and after it there occurs a lapse of nearly 1500 years before the next famine mentioned, which, it is stated, took place in 1022 A.D., a year remarkable for a great drought and famine in Hindustan, as well as in many other parts of the world. This was followed, after an interval of about thirty years, by a seven years' drought in "Ghor," which was accompanied by a considerable loss of life, both amongst men and animals. The next famine mentioned occurred in 1291, in the neighbourhood of Delhi, where no rain fell for a whole year, and the same part appears to have been again visited with a similar calamity in the year 1342. Two years later there was a famine in the Deccan. In 1413 Delhi was once more subjected to scarcity through drought, which appears to have been principally felt in the country between the Ganges and Jumna Rivers. The next famine recorded is said to have occurred in Hindustan about the year 1495, but neither its extent nor the particular localities affected are more specially described. After this a considerable interval occurs, no subsequent dearth being mentioned until the year 1661. This famine is said to have prevailed in different parts of India, and the Emperor Aurungzebe took such measures as were within his power to alleviate the distress of his subjects. He remitted the taxes that were due; he employed those already collected in the purchase of corn, which was distributed among the poorer classes; and he expended immense sums out of the treasury in conveying grain by land, as well as by water, into the interior provinces, from Bengal, and from the countries which lie on the five branches of the Indus, which suffered less on account of the great rivers by which they are watered. The grain so conveyed was purchased, at any price, with the public money, and it was re-sold at a very moderate rate, whilst the poorer people were supplied, at fixed places, with a certain quantity without any consideration whatever. By these measures, it is stated, whole provinces were delivered from impending destruction, and many millions of lives were saved.

This brings us to the end of the famines regarding which information is obtainable from Mahommedan sources. The Hindoos, with their well-known contempt for history, have recorded nothing of a similar nature on the subject of famines in the peninsula of India, and no light can therefore now be thrown upon the question as to how far that part of the Indian Empire was, in early times, subject to such visitations.

With the great famine of 1769-70 we enter upon the commencement of a period from which the principal famines of India have been chronicled by European historians; but it cannot be said with certainty that a full account of these calamities, for the whole of India, began to be recorded previously to 1860-61, in which year parts of the North-western Provinces and the Punjab suffered severely from drought, owing to the failure of the usual rains. For this reason there can be little doubt that famines of greater or less severity have occurred, within the past hundred years, especially in the more remote parts of the Empire, of which no particulars now are to be found; and there are strong reasons for believing that this has been the case, as legends exist, amongst some of the older native inhabitants, of great scarcity in certain years, and of its attendant calamities to the people, of which, however, no more reliable information can now be obtained. Without attempting to enter into detail regarding the famines of the past century—which would occupy far more space than could fairly be claimed for that purpose—a brief account of some of the most noted of these visitations will form an appropriate introduction to a consideration of their causes, so far as this has as yet been ascertained by scientific research, and of the best means to be adopted with a view to relieve the districts affected from their attendant consequences.

Subsequently to that in Northern India, of 1769-70, the next severe famine visited the Carnatic, and the Settlement of Madras in the years 1781 to 1783, which was, to a great extent, brought about by the devastations caused by Hyder Ali's armies. The Settlement of Madras was reduced to such straits for food that Government found it necessary to take steps for regulating the supplies of grain, and in January, 1782, a public subscription was raised for the relief of the poor, from which originated the institution for the relief of the Native poor, known as the Monegar Choultry. In 1783-84 certain parts of Upper India are said to have been seriously affected by drought, but, as the countries which suffered most were not then subject to British rule, very little information is obtainable relative to that famine. In the years 1790 to 1792 there was a very serious drought in the Madras Presidency, and at an early period Government suspended the import and transit duties on all kinds of grain and provisions, and themselves imported grain from Bengal. Rice was distributed gratuitously by Government, and relief was afforded by employing the poor on public works. In 1824-25 Madras was again visited with famine, which was caused by failure of rain in most of the provinces of that Presidency, and more particularly in the Carnatic and Western Districts; numbers of starving poor flocked into Madras, and relief establishments were formed for them partly by private charity and partly at the expense of Government. The year 1837-38 was long afterwards remembered in consequence of the failure of rain, which brought scarcity and famine to the North-western Provinces and to Rajputana. No sooner had the serious pressure of famine begun to be felt than the ordinary bonds of society seemed to be broken by it. Beginning at Rohilkhund, the population gathered into bands for plunder, and attacked the grain stores in the larger villages and towns. This calamity was of such intensity and magnitude as to have been almost unmanageable, and the deaths from starvation alone are believed to have exceeded 800,000. Relief Committees were formed for the distribution of private charity, and Government expended very large sums in the employment of the population on relief works, in addition to which, before February, 1838, no less a sum than £600,000, on account of revenue, had been remitted.

The next serious famine seems to have occurred in 1860-61, the effects of which extended over the Punjab and parts of the North-western Provinces, affecting districts having a population of about 13 millions, and a cultivated area, in ordinary seasons, of 12,293,000 acres, of which 4,457,000 acres were thrown out of cultivation during the famine period. Up to the 30th April, 1861, as many as 26 central and 75 district relief-houses had been established in the famine districts, at which, on the average, 80,000 people were relieved daily, and, besides this, a series of special relief works was organised for the relief of the able-bodied poor, upon which 143,500 people were, on the average, daily employed. Owing to the better means of communication, greater promptness in anticipating the effects of the drought, and improved organisation for dealing with the distress, the famine of 1860-61 was a much less awful calamity than that of 1837-38. Four years later—viz., in 1865-66—Orissa, Bengal, and Behar were visited by famine, owing to the premature cessation of the rains throughout the Lower Provinces of Bengal, in the middle of September, 1865. In Behar this had been preceded by two years of bad crops. The general distress amongst the lower classes was manifested by a large increase in crime; village granaries were burnt and plundered, and extensive grain robberies—committed solely for the purpose of obtaining food—were of constant occurrence. In June committees were formed, funds raised for the relief of those incapable of work, and employment was provided for the able-bodied on public works.

Historical records show that Orissa has at various times suffered from terrible famines. Great famines are said to have occurred in the fourteenth, fifteenth, and sixteenth centuries of our era. The great famine of Bengal of 1770 was felt grievously in Orissa, and a few years later, in 1774-75, another great scarcity is said to have occurred. The last great famine, of the traditions of which the old men speak, was in 1792-93, in the time of the Mahrattas; but none of a general character happened in the present century before the year 1865-66. For the two preceding years the crops had been below the average, and the failure of rain in this year was immediately followed by serious famine. It was very soon apparent that the consequent distress could not be effectually met by local private charity; public works on a large scale were shortly put in hand to give employment to the starving population, who were paid the ordinary rate of money wages. Notwithstanding repeated applications for permission to import rice, on the part of Government, owing to the failure of private enterprise to supply the necessary food to the affected districts, they were persistently negatived, and the consequence was that the works undertaken to relieve the distressed people were to a very great degree inoperative, for want of rice to feed the labourers. As the famine increased in intensity Relief Committees were formed, by whom subscriptions were raised and food distributed. At length public works and relief works were stopped for want of food, and Government then sanctioned the importation of rice for the relief of the sufferers, and in July centres were established for the distribution of cooked food.

In 1866 the Madras Presidency was also visited by drought. The two years preceding had been generally unfavourable to agriculture, but it was not till October, 1865, that symptoms of distress began to show themselves, and in the following month Government sanctioned the employment of the poorer classes on public works. By March, 1866, the distress had increased, and many relief depôts were opened by private subscription at several places in the Ganjam district. In June the Bengal Government sanctioned a grant of Rs. 20,000 from the balance of the North-west Provinces Famine Relief Fund, which was chiefly expended in grain, on the public account, and despatched to Ganjam. Considerable funds were also raised locally, and in Madras, for the relief of the distress of that district, and the Madras Government sanctioned an expenditure of Rs. 3000 per mensem, from the State Funds, for the same purpose. In July a General Famine Relief Committee was formed to collect subscriptions to alleviate the distress, and Government promised to make contributions to the Relief Fund equivalent in amount to the private subscriptions which might be raised for the purpose, and sanction was, at the same time, given for the employment of the able-bodied among the sufferers on public works. The years 1868 to 1870 were noted for an extensive famine which ranged from Behar, over great parts of the North-west Provinces, the Punjab, and Rajputana, and was caused by the failure of the harvest of 1868 following upon an unfavourable season in the preceding year. An untimely fall of rain in the autumn of 1869 caused great anxiety, and the crops proved very short. Generally speaking the famine had disappeared in the North-west Provinces by October, 1869; but in Ajmir and Mhairwarra a plague of locusts succeeded the drought, and prolonged the sufferings in those parts until the close of March, 1870, or even later.

The next famine visited Bengal and Behar, in the years 1873-74. In 1872 the rain was somewhat deficient, and the following year was also a dry one, almost beyond precedent, and what rain did fall was unfortunately distributed; south of the Ganges it was excessive, but in North Behar, and almost the whole of Bengal, the rain was far below the average. Coupled with deficient rainfall, the monsoon of 1873 was abnormally hot. The famine reached its culminating point in April and May, 1874, and it was completely over by about October, 1874. The experience gained in dealing with previous famines was of great value on this occasion, and enabled Government successfully to meet the calamity and organise systems of relief. Tolls were removed from ferries and roads along the chief routes for grain transport; assistance was given to *bonâ fide* importers of grain, with loans of money without interest; a number of relief works were commenced, on which wages to labourers were authorised to be paid in grain whenever the local price reached famine rates; storehouses for Government grain were constructed on all the chief works; and other facilities were provided for the transport and distribution of food through the ordinary channels of trade, or through the agency of Relief Committees.

Little need be said here of the recent great famine in Madras and Bombay. In 1875 the season was in many places unpropitious, and in the following year the south-west monsoon was deficient throughout the greater part of the Madras Presidency and in portions of Bombay. In Madras, famine first threatened in July, 1876, whilst in October the whole of the nine districts of the Bombay Deccan were seriously threatened, nearly all the monsoon crops having perished, and there having been no later rains to admit of sowing the rabi. The spring and summer rains again failed in 1877, and the distress became still greater generally throughout the affected districts. The relief measures adopted by Government, and through private agency, have so recently formed the subject of report and discussion in the daily papers that any repetition on that point seems unnecessary here.

We now turn from the foregoing brief historical summary to a consideration of the scientific side of the question. This divides itself into two heads, viz. (1), the causes of famines; and (2), the means to be adopted for averting them, on the one hand, or of mitigating their effects, on the other.

A close examination into all the circumstances of past famines shows that they have not been caused by the failure of rains in one season only. The stocks of grain ordinarily reserved in the country would appear to be sufficient, generally, to carry the people over a single season of drought, and consequently of deficient harvest; but when a famine occurs it has been the result of one or two consecutive bad seasons, followed by one of unusual drought; and experience thus teaches that the probable advent of a famine may be anticipated, with at any rate sufficient exactness to enable precautionary measures to be adopted in anticipation, with a view to meeting the emergency should it arise. Fortunately, India is capable of supplying far more food than is required for the maintenance of her populations, even in ordinary seasons, and as droughts occur only locally, and have never hitherto been known to affect the entire Empire at one and the same time, the surplus produce of one district is always available to supply the wants of others when famine occurs in them. It is a point worthy of remark that severe droughts in Northern India have, on several occasions, followed closely upon distress similarly caused in the Peninsula of India: thus, the Madras famine of 1781 to 1783 was followed by one which affected Bengal, the North-west Provinces, and the Punjab, in 1783-84; the failure of rains which resulted in scarcity in many of the provinces of the Madras Presidency, in 1824-25, was followed by a similar calamity in the North-west Provinces in the succeeding year. The "Guntoor" famine of 1833 preceded, only by a few years, one which affected the North-west and Lower Provinces of Bengal in 1837-38, and the Madras famine of 1866 was very closely followed by one in the North-west Provinces and the Punjab in 1868 to 1870.

Although no absolute famine has as yet threatened in Northern India, following upon the recent disastrous and long-continued calamity in the Peninsula, there are not wanting signs which are causing no small amount of anxiety with regard to crop prospects in other parts of the Empire.

An attempt has been made to connect the occurrence of severe famines in India with the periods of maximum sun spots, the reason for which has, however, never been very clearly enunciated; neither has it been shown why such solar phenomena should affect one part of the world and not others, or why the influences—whatever they may be—should fall sometimes upon one part of India and sometimes upon another; indeed, the more this theory is investigated the more untenable it appears to be; but, even if it could be established, it is to be feared that, with our present limited knowledge of meteorological phenomena, but little benefit could be derived from a knowledge of the fact that certain solar disturbances did influence terrestrial meteorology.

Famines in India, excepting those of purely local origin and extent, may be said to be caused by failure of the usual rains; failure, that is, not always in regard to quantity only, but often also to distribution, for sometimes the rain will fall so heavily at the commencement of the monsoon season as to render agricultural operations impossible, and fail altogether when the seed is at last sown and moisture is required to bring the plant to maturity. Also, when there has been a light fall only at the sowing season, excessive storms will sometimes destroy the ripening crops before they can be gathered in. These are, however, rather exceptional cases, and, as a rule, failure in the quantity of rainfall, rather than its unequal distribution, is the primary cause of famine in India. Local famines have been caused by excessive floods in rivers, and by blight. The former, as we shall presently see, is in a great measure capable of being remedied, but for the latter we are unable at present to suggest any practical cure.

The question as to how famines may be absolutely averted is one upon which speculation may be allowed to indulge itself, but absolute proof is wanting. England, Ireland, and most parts of Europe were, in centuries past, visited by many and severe famines, of much greater frequency than at the present day; but, with the advancement of civilisation, their tendency to occur has most sensibly diminished. In dwelling upon this point it is of importance to distinguish between the less frequency of failures of crops and the altered conditions of European countries, in consequence of which failure of crops in any one country would not necessarily mean famine, in the same sense as it once would have done; but it may be observed that, with the improvement of Western countries in this respect, the Eastern hemisphere appears to have become more subject to severe droughts than heretofore, and not only is this the case absolutely, but these visitations—so far as the East Indies are concerned—have of late years shown an evident tendency to increase in severity and frequency, the causes of which are at present unappreciated, but they are certainly deserving of the most serious consideration and investigation by scientific men, no less than by Government. A theory has of late gained ground amongst certain enquirers into this subject that the amount of rainfall is influenced by forests, and that, in countries subject to drought, all that is necessary is to plant trees in order to restore the proper seasons of rainfall. How far this is supported by facts is, however, at present uncertain. We have instances of barren wastes in Africa, Sind, and elsewhere, upon which rain hardly ever falls; whilst, on the other hand, there are the forest-clad mountains of Northern and Western India, upon which the amount of precipitation is excessively heavy. These may be taken as two extremes; but although illustrating the facts that in certain places where there is no foliage rain is almost unknown, and that an abundance of rain and dense forests occur together, they do not help to solve the problem to what extent denudation of forest tracts may be permitted without interfering with the average annual rainfall. From meteorological observations taken in the Madras Presidency it does not appear that this point has yet been reached, but it is nevertheless contended by some that the destruction of trees in the forests of Southern India has already been carried to an extent absolutely injurious to the interests of cultivation. If this be the case it follows that, although the quantity of rainfall may not have been absolutely affected, its periodic distribution may nevertheless be rendered less certain and constant, the consequences of which would be only in a small degree less disastrous to agriculture than absolute drought; but on this point, also, careful observations over extensive areas, and for a succession of years, are necessary before any positive decision can be pronounced on the subject. In one respect the influence of trees upon rain, after it has fallen, has been well ascertained, and their importance to the country in that respect most fully established. In the absence of trees upon slopes and hill-sides the rainfall rapidly rushes over the surface of the land to the nearest drainage lines, in the steeper places carrying with it the soil from the surface, destroying at one and the same time the fertility of the land, and choking up the drainage channels of the country; also, in the absence of the check to this rapid surface drainage, the rain-water over large areas is rapidly precipitated into the nearest rivers, filling them beyond the capacities of their channels, and so causing floods in their lower reaches, especially where, emerging from the hills, they enter upon the alluvial plains. By the presence of trees these effects are prevented; the rainfall is arrested in its flow over the soil, and being retained by lower growth, and the roots of trees, finds its way into the soil, and thence by degrees, through underground channels, into the rivers, whilst a considerable portion is retained in the substrata, replenishing springs and filling wells.

So far as experience at present teaches there do not appear to exist any known methods by the adoption of which famines can be actually averted, but they appear to die out before the advance of civilisation, the effects of which seemingly react in numberless ways upon the physical conditions of a country, which, in turn, exercise no inconsiderable change in the meteorological state of the atmosphere. Many of these effects of civilisation are sufficiently known to enable them to be defined, and although they may doubtless be put into operation, upon a small scale, and in detached localities throughout India, by the agency of

Government, it would be hopeless to expect the full benefits of the necessary changes to be felt, as they have been in Europe, in practically rendering famines impossible, until the populations themselves are sufficiently advanced in moral and intellectual civilisation to appreciate the truths involved in these measures, and endowed with sufficient energy to carry them thoroughly into effect. It is only when all the known means for mitigating the effects of famines have been well established throughout the country —or, in other words, when civilisation, as it exists in the Western Hemisphere, is finally and fully established in the East—that there can be any serious hope entertained that the calamitous visitations of famine, now so common in India, will cease to occur.

The three principal measures to be adopted with a view to mitigate the effects of famines are—(1.) The provision of a complete system of communications throughout the country. (2.) Improvements in the system of agriculture generally. (3.) The construction of artificial irrigation works wherever they may be practicable. With regard to the first, viz., communications, there exist two rival plans, each of which has its advocates; the one plan comprising railways as the chief arterial system of the country; and the other, lines of water communication. It can hardly be necessary to say much upon this question. It is a fact that the whole of India has never yet, within historic periods, been subjected to drought and famine throughout its length and breadth at one and the same time; it already grows more food grain than is required for the supply of its populations under ordinary circumstances, and it exports wheat largely to this country, as well as rice. At no time, it is believed, has there been an actual scarcity of food in the country, and it necessarily follows that suitable means of communication alone are necessary to enable the surplus produce of one district to be transported to another, where, by reason of drought or other causes, the supply may be deficient. In certain places, and under favourable conditions, no doubt grain—in common with all other bulky articles—may be more cheaply conveyed by water carriage than by railways; and, wherever a never-failing supply of water can be relied upon, every encouragement should undoubtedly be given to improve what may be styled the *natural* highways of the country, viz., its rivers, and in certain circumstances it may even be desirable to render irrigation canals suitable for navigation. Experience on this latter point, in India, does not support the supposition that expenditure incurred for the purpose will be directly remunerative; but then, also, neither are roads; and the arguments that are used in favour of spending money on the latter class of works apply also to the former. Means of communication in a country is the first necessity for the introduction of civilisation, which almost naturally follows upon the free communication and interchange of goods and ideas between people. As civilisation becomes more advanced, improved means of communication are demanded, and have to be supplied. In India, Western civilisation and Indian semi-barbarism meet, and the requirements of the latter are being supplied upon a scale calculated by the experiences of the former; and an advanced form of civilisation, in the shape of communications, is thus being forced upon a people not yet educated up to the extent of their country's requirements. Nevertheless, railways may be said to have proved not only a decided success in India, but an absolute necessity in times of famine. No other means of transport could have possibly met the recent requirements of Southern India; and the experience gained during the past two years, as to the value of the iron road, may be expected to lead, in the immediate future, to a large extension of the railway systems in that country. What neither navigable channels nor country roads could have accomplished has been done by railways, and the immense amount of food which they have been instrumental in conveying into the famine-stricken districts has proved the means of saving, probably, some millions of people from starvation. It is, therefore, by the judicious extension of railways, primarily, that the principal movements of food will for the future be conducted, and by maintaining an even balance—as might be done—between supply and demand, the onerous pressure of famine prices, as they have heretofore been experienced in seasons of drought, should be greatly mitigated, and the occurrence of actual local famine be rendered impossible.

But although a perfect system of communications would thus render absolute famine—such as has been hitherto common in India—impossible, it would contribute nothing towards the avoidance of drought, and the consequent failure of crops. There can, however, be no doubt that, by an improvement in the present system of cultivation, much might be done towards rendering the crops less liable to total destruction in seasons of deficient rainfall, and enabling them better to withstand the effects of drought. Under existing circumstances the contrary appears at present to be the case; and instead of improvements in this direction, an opinion is prevalent that the land generally is becoming exhausted, and consequently less productive than it formerly was. One reason for this may probably be that, with an increase of population, fresh lands are broken up for cultivation which are generally of only inferior quality, and, with the existing rude state of agriculture, often yield barely sufficient to do more than cover the cost incurred; whilst, with regard to the better soils, the exhausting nature of the tillage is gradually lessening their productive powers. The use of manure is practised in India to a very limited extent, dry crops being generally left entirely without it, and it necessarily follows that the plant food in the soil must be gradually becoming less and less, where everything is taken from it and nothing returned. When a field becomes barren it is thrown out of cultivation and left fallow for two or three years, when the previous system of exhaustive cultivation is again repeated as long as the land will produce sufficient to pay for seed and labour. The general scarcity of manure in India is, in a great measure, attributable to the want of fire-wood, in consequence of which the dung of cattle is used as fuel, instead of being ploughed into the land as manure. The use of manure, especially in a hot and dry country, is not limited to its value as a means of plant food, when it contains organic matter. Mineral manures are, no doubt, limited in their value to this purpose; but it has been ascertained, by careful experiment, that the addition of organic matter to the soil not only increases its power of absorbing moisture from the atmosphere, but makes it more retentive of moisture when once absorbed, and therefore less liable to be desiccated by dry winds and solar heat.

Next after manure, improved ploughing presents a means of greatly benefitting the crops and the fertile properties of the soil. By breaking up the land to a greater depth than is now done, an unexhausted soil will often be disturbed, possessing greater elements of fertility than the worn-out upper stratum on which crops have been grown, perhaps continuously, for hundreds of years past. Besides this, by deepening the loosened surface, the land is rendered more capable of absorbing rainfall, and permitting it to penetrate to the lower strata, to which also the roots of plants are enabled to reach, owing to the breaking up of the hard "pan" which usually underlies the top three or four inches of soil, which now, under the native system of cultivation, alone are disturbed in the so-called process of ploughing.

Subsoil drainage, also, has been proved in India, as in England, to be greatly conducive to good cultivation, and its benefits have been found to be particularly marked in seasons of drought. Dry soil, according to Prof. Liebig, derives its moisture partly from the vapours of water in the air, and partly by absorption from the deeper-lying moist strata, from which a constant distillation of water is taking place towards the surface. By drainage, the water, which rises by capillary attraction, being placed at a greater depth, the dry soil now derives from the lower strata a quantity of moisture in the form of vapour which supplies the wants of plants, and hence it appears that subsoil drainage renders the land less liable to be seriously affected by drought; and so far it is calculated, under certain conditions, to mitigate the effects which, in India, now invariably follow upon a failure of the usual rains, or even their unequal distribution during the monsoon season.

Another benefit to agriculture would also arise from the more general presence of trees over cultivated areas. In the first place their leaves would furnish material for manure, whilst the dead wood and branches, by supplying means for fuel, would enable the dung of the cattle to be appropriated to its more legitimate use as manure. The influence of trees over climate is also very important. By the process of distillation which is ever taking place in the surface of their leaves the temperature of the surrounding atmosphere is lowered, whilst they also communicate moisture to it owing to the process of evaporation which is constantly taking place through their means. By thus rendering the climate in their immediate vicinity both cooler and moister they contribute greatly to the interests of agriculture, and in only a somewhat lesser degree by the protection which they afford to the crops against wind and storms, and to the soil from evaporation and drought against dry winds and heat.

From whatever point it is viewed, improved agriculture appears well calculated to contribute, in no small degree, towards protection against famines in India, and it will undoubtedly do much to save crops from destruction in seasons of drought, where artificial irrigation is not available. When—from the vicinity of tanks, rivers, or canals—water can be obtained to supply the deficiency of rain, every advantage should undoubtedly be taken of its proximity to protect crops during drought, and to increase the yield in ordinary seasons; but, under any circumstances, the area of land which can be so watered must be but small compared with the total extent of cultivation throughout the country. Some classes of irrigation works, too, are liable to fail from want of water, at the very time when it is most in demand; and it is only those irrigation works which derive their water from perennial rivers, whose supplies are not wholly dependent upon fluctuating rainfall, which can be relied upon as unfailing sources in times of greatest emergency. In ordinary seasons irrigation works must always prove of great advantage, for by enabling larger crops to be grown than could otherwise be the case they must largely contribute towards the well-being of the cultivators, and, by rendering it possible for them to accumulate wealth, either in the shape of grain or money, they will thereby be enabled the better to endure the strain to which they are subjected in years of famine.

The improvement and extension of communications, whether they be roads, railways, or water navigation channels, and the multiplication of irrigation works wherever they can be constructed, are means within the power of Government to adopt for the protection of the people from the effects of famine, and for the advancement of the general well-being of the country. The multiplication of these, or other great public works, is a matter with which the advancement of the people in intelligence has at present no immediate connection, but as regards improved agriculture the case is far different, and this will, and can, never be introduced generally into the country until the people themselves shall have been raised by education, both moral and intellectual, from their present state to one of more advanced civilisation. Towards this there can be little doubt that railways will contribute more than any other means at present available. The conclusion to be drawn from a careful consideration of all the circumstances of the case is, that there is no immediate remedy against famines possible in India; their effects may be mitigated by judicious treatment locally, but their real cause, being like poison in the human blood, must be dealt with in a patient and scientific manner, and time allowed for the full and free action of western civilisation throughout the entire system, before it can be expected that the medicine will effect a perfect cure, and the East, like the West, be delivered from fear of those visitations which now destroy her children, cripple her resources, and check that advancement in material prosperity which it might be hoped she would otherwise enjoy.

II. THE PROGRESS OF THE DOCTRINE OF DEVELOPMENT.*

AMONG the general public, and even to some extent among men of Science who are not biological specialists, it is too commonly supposed that Evolutionism sprang at once into full maturity from the brain of Mr. Darwin; that Darwinism and Evolutionism are convertible terms; and that the doctrine is accepted with but little scrutiny by the majority of modern naturalists more as an article of faith than a scientific truth. Nothing could be farther from the real state of the case. The theories of Mr. Darwin are regarded as a mere tentative sketch, to be revised, emended, filled in, or even cancelled, as future observation and experiment may dictate. To this process they are being constantly submitted, and the general result may be stated to be that while the belief, or rather the conviction, of the truth of Evolution as "God's mode of Creation" is gaining ground, the precise agencies by which Mr. Darwin supposed such Evolution to be mainly effected are looked upon by many with doubt, or are at least relegated to a more subordinate position.

It is well known that Mr. Darwin, along with many of his more immediate followers, ascribes the development of species as we actually find them to two causes—both slow, gradual, and uniform in their action. These are Natural Selection and Sexual Selection, the former an utterly unconscious, but the latter a conscious, agency. To Sexual—or, as it has been not unhappily styled, Female—Selection he attributes not indeed the origin of any new form of life, but the ornamentation, and especially the colours, of the higher animals, such as the vertebrates and insects, and especially the generally brighter hues and the decorative appendages which characterise the male sex. He argues that the females having for ages past given the preference to the most beautiful males of their respective species, these have had a better chance of leaving a numerous offspring than their less brilliant rivals, and have transmitted their

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* Tropical Nature and other Essays. By ALFRED R. WALLACE. (London: Macmillan and Co.)

All the Articles of the Darwin Faith. By the Rev. F. O. MORRIS, B.A. (London: Moffatt, Page, and Co.)

Zur Entwickelungs-Geschichte der Menscheit. By L. GEIGER. (Stuttgart.)

attractions to their male posterity. Thus, by the secular action of this principle, he contends that the gorgeous hues and exquisite designs of the wings of butterflies, the train of the peacock, the secondary wing-feathers of the Argus pheasant, the gorgets and crests of humming-birds, have been elaborated. This hypothesis certainly harmonised with a considerable number of striking facts previously unexplained, and seemed at first glance to agree with many more. Mr. Wallace, however, has long held this portion of Mr. Darwin's views to be erroneous, and brings forward against it certain exceedingly weighty arguments. In an earlier work, with which every naturalist ought to be familiar,* he has shown that in female birds the need of protection, especially during the season of incubation, has repressed those bright colours which would otherwise be produced by general laws in both sexes alike. He now further argues that high colouration, if not directly due to, is yet correlated with, vital intensity. "The very frequent superiority of the male bird or insect in brightness or intensity of colour, even when the general colouration is the same in both sexes, seems to me to be primarily due to the greater vigour and activity and the higher vitality of the male. The colours of an animal usually fade during disease or weakness, while robust health and vigour add to their intensity. This is a most important and suggestive fact, and one that appears to hold universally. In all quadrupeds a dull coat is indicative of ill health or low condition, while a glossy coat and sparkling eye are the invariable accompaniments of health and energy. The same rule applies to the feathers of birds, whose colours are only seen in their purity during perfect health; and a similar phenomenon occurs even among insects, for the bright hues of caterpillars begin to fade as soon as they become inactive preparatory to undergoing their transformation." Whenever there is a difference of colour between the sexes the male is the darker or more strongly marked, and the difference of intensity is most visible during the breeding season, when vitality is at its maximum. It is undoubtedly true that female birds do exercise a choice, but the bulk of the evidence on this point, as collected by Mr. Darwin himself, far from proving that such choice is determined by colour, points in a directly opposite direction. The "most vigorous, defiant, and mettlesome male" seems to be preferred. These attributes may be, and in a majority

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* Contributions to the Theory of Natural Selection.

of cases doubtless are, correlated with intensity of colour. But, if so, it is persistency and energy, rather than mere beauty, to which success is due. Three eminent breeders of poultry—Messrs. Hewitt, Tegetmeier, and Brent—informed Mr. Darwin that they "did not believe that the females prefer certain males on account of the beauty of their plumage." Mr. Tegetmeier is convinced that "a game cock, though disfigured by being dubbed and with his hackles trimmed, would be accepted as readily as a male retaining all his natural ornaments." Old hens, and those of a pugnacious disposition, as Mr. Darwin states, quoting Mr. Brent, "dislike strange males, and will not yield until well beaten into compliance"—certainly a curious kind of "Female Selection." Mr. Darwin himself admits that, "as a general rule, colour appears to have little influence upon the pairing of pigeons." The case of the hen canary "who chose for her mate a greenfinch, in preference to either chaffinch or goldfinch," also tells against Mr. Darwin's hypothesis. Nor is the instance of Sir R. Heron's peahens more fortunate. If these birds preferred a pied cock to one normally coloured their conduct was a strange anomaly, because, as Mr. Wallace remarks, "pied birds are just those that are not favoured in a state of Nature, or the breeds of wild animals would become as varied and mottled as our domestic varieties." But if there is no sufficient evidence that female birds in the choice of mates are influenced by the beauty of the opposite sex, the case is still more decided as regards butterflies. Here the males surpass the most splendid male birds at once in brilliance of colouration and in elegance of pattern, whilst the females in a multitude of cases are comparatively plain and obscure. Yet there is no evidence to prove that the female is at all influenced by this beauty, "or even that she has any power of choice." Mr. Darwin himself can find no more satisfactory argument than the following:—"Unless the female prefer one male to another the pairing must be left to mere chance, and this does not appear probable." Yet we observe the males fight and jostle each other in pursuit of a female, who submits herself with indifference to the victor. Mr. Darwin admits that in the case of the silk-moths "the females appear not to evince the least choice in regard to their partners." Would not the same rule be found to hold good with any other Lepidopterous insect, if only observed as extensively? Here, as among birds, "the most vigorous and energetic, the strongest winged, or the most persevering, wins the object of his pursuit." Mr. Wallace adds that "Natural

Selection would here act, as in birds, in perpetuating the strongest and most vigorous males, and, as these would usually be the most highly coloured of their race, the same results would be produced as regards the intensification and variation of colour in the one case as in the other."

But now comes the question, why, if the females are not attracted by the beauty of their mates, do the males make such a striking display of the brilliance of their plumage? Of the fact of such display there can be no doubt whatever. But the main point—the question whether the choice of the females is at all influenced by shades of colour or slight differences in design—is totally unproven. There is no evidence that the females admire, or even notice, the display. "The hen, the turkey, and the pea-fowl go on feeding while the male is displaying his finery." The flutterings and dancings, the erection of tails and crests, are probably a mere result of the exuberant energy with which the male at this season is overcharged.

Mr. Wallace, however, founds his strongest argument on the interference and opposition of Natural and Sexual Selection. He says—"Natural Selection, or the survival of the fittest, acts perpetually, and on an enormous scale. Taking the offspring of each pair of birds as on the average only six annually, one-third of these at most will be preserved, whilst the two-thirds which are least fitted will die. At intervals of a few years, whenever unfavourable conditions occur, five-sixths, nine-tenths, or even a greater proportion of the whole yearly production, are weeded out, leaving only the most perfect and best adapted to survive. Now, unless these survivors are on the whole the most ornamental, this rigid Natural Selection must neutralise and destroy any influence that may be exerted by Female Selection. The utmost that can be claimed for the latter is that a small fraction of the least ornamented do not obtain mates, while a few of the most ornamented may leave more than the average number of offspring. Unless, therefore, there is the strictest correlation between ornament and general perfection, the more brightly coloured or ornamented varieties can obtain no permanent advantage; and if there is (as I maintain) such a correlation, then the sexual selection of colour or ornament, for which there is little or no evidence, becomes needless, because Natural Selection—which is an admitted *vera causa*—will itself produce all the results. In the case of butterflies the argument becomes even stronger, because the fertility is so much greater than in birds, and *the weeding out of the unfit takes place, to a great* *extent, in the egg and larva state.* Unless the eggs and larvæ which escaped to produce the next generation were those which would produce the more highly-coloured butterflies, it is difficult to perceive how the slight preponderance of colour sometimes selected by the females should not be wholly neutralised by the extremely rigid selection for other qualities to which the offspring in every stage are exposed."

The above considerations, we submit, fully warrant naturalists, if not in the utter rejection of conscious Sexual Selection, at any rate in placing it in a kind of suspected position, to be condemned except some unexpected piece of evidence should be brought to light in its favour.

But we may venture farther, calling especial attention to the words we have italicised. No one who has made observations with even moderate care, upon any department of the animal kingdom, can doubt the sharpness of the struggle for existence, or can deny that of the eggs deposited by a female butterfly but a very small fraction ever come to maturity. Many no doubt, as Mr. Wallace states, perish as such without ever seeing the light at all. But how is this effected? Every egg of the whole brood is equally and similarly helpless in case of the approach of a devourer or a parasite. None of them can escape by dint of any strength, swiftness, or cunning which it may possess in excess of the rest. Without absolutely saying that no variation can ever be traced among the eggs laid by one mother, we are warranted in declaring that any difference, either in colour, shape, odour, or other properties, which may cause egg *a* to be less easily perceived, or when perceived by an enemy to be more readily rejected, than eggs *b*, *c*, and *d* must be exceedingly trifling, and that the immunity thus gained must be regarded as a mere vanishing quantity. For one that escapes in virtue of such properties ten will owe their survival to what—humanly speaking—must be pronounced mere chance. One egg, without possessing any attribute of superiority or greater fitness, may have been deposited by its mother in a less conspicuous place than the rest. One egg may have perished, not from any comparative imperfection or want of fitness on its part, but because some ovivorous or parasitical insect chanced to pass over the particular leaf to which it was attached. Numbers of other causes might be mentioned—as far as we can judge perfectly accidental—upon which the quickening or the death of an egg may depend. Here, therefore, is no selection, no weeding out, but a destruction of one portion and a preservation of the rest with as little reference to any properties they possess as if the momentous question had been decided by lot.

From the egg we pass to the larva. Here there are undoubtedly greater individual differences. We can well admit that one caterpillar may have keener senses to perceive the approach of danger, greater agility in escaping, more cunning in concealment, an odour less attractive to enemies than have others, and may thus derive an advantage over them in the struggle for existence, and may thus fairly be pronounced more fitted for the conditions under which it must exist, and better adapted to survive. But here also a vast number of cases must occur in which chance alone can decide. The totally accidental matter of position at some momentous time may be of far greater consequence for the life of a larva than a slight variation in any of the points just enumerated. Thus an ichneumon may oviposit in the bodies of caterpillars *a*, *b*, *c*, &c., whilst caterpillar *x* may escape from the simple fact that the enemy's stock of eggs ready to be deposited was exhausted before she reached it. Or two larvæ upon two different plants may each be threatened by the approach of an ichneumon. But the one invader may become entangled in the web of a spider or be snapped up by a bird, whilst the other meets with no hindrance and effects her purpose. In the pupa state, again, no small portion of the deaths take place; and here we have a reversion almost to the conditions of the egg. Without any reference to attributes of their own, some pupæ may have been discovered by birds, by field-mice, by hedgehogs, and by other of the numerous birds, beasts, or insects who consume such prey with readiness, whilst others by pure accident may have escaped. Whatever effect the first small steps of variation may have had in determining the survival of any given individual, it seems insignificant compared with the effects of chance. The condition of a Lepidopterous insect, from the egg to its emergence as imago, seems very much like that of the inmates of a town under the infliction of a heavy bombardment. It may perish or it may survive, neither alternative being so much determined by its own peculiar attributes as by the position which it occupies at some given moment. With the mature butterfly the case is different. We can well conceive that variations in point of speed, not relatively greater than such as are well known to occur between individuals of one species, may turn the scale for life or for death, and can thus imagine the gradual elimination of the slower and the preservation of the swifter forms.

From butterflies we pass to birds. In a work containing much with which we are unable to agree,* the author, contending that over-preserving and the extirpation of hawks have not led to the multiplication of weak and sickly grouse, which formerly would have been improved away, and have left more scope for their stronger and healthier fellows, argues that it is not the weaker and slower birds which fall victims to the falcon. The celerity of this destroyer is so tremendously in excess of that of the fleetest grouse that all differences in speed among the latter birds utterly vanish. The strongest-winged and most vigorous moorcock, if once espied in the air by the enemy, has practically no greater chance of escape than a feeble and sickly bird. On the contrary, the boldest and most energetic grouse, who may fairly be assumed to be, as a rule, the healthiest, will fall victims more frequently than their weaker brethren, from the mere fact that they are more active and venturesome, and hence more likely to be on the wing. The effects ot the co-existence of falcons and grouse in any country will be, therefore, not the development of a form of the latter better adapted for rapid flight, and ultimately, in the course of many generations, endowed with longer and more pointed wings, but merely a thinning of numbers, which will tell equally upon the strong and the weak, and which in some cases may even give an advantage to the latter.

This argument of Mr. Morant's concerning the influence of the falcon upon the development of the grouse appears to us applicable not merely to this individual instance, but to every case where a bird or a beast has to struggle for existence against enemies greatly its superiors in speed, in strength, or in general resources. Slight increments of swiftness or force, trifling improvements in offensive or defensive arms, would be absolutely thrown away under such circumstances, however valuable they might be as against an enemy but slightly superior to the original form. Hence there are numbers of cases where it must become questionable how, on the principle of Natural Selection, advances in these important directions are to be effected. If variation proceeds not at one uniform rate and by gradations almost imperceptible, but occasionally by more rapid movements, the matter is entirely different. Nor are considerations of

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* "Game-Preservers and Bird-Preservers." See Quarterly Journal of Science, vii., 145.

speed and strength isolated in this respect. Something very similar will prevail concerning the advantage which animals gain by their so-called "protective" resemblances, either to other species or to their inanimate surroundings. Let us suppose a creature ill-adapted to escape from its enemies by speed or strength; conspicuous in its form and colouration, and therefore unable to conceal itself; and, lastly, attractive to the smell and taste of rapacious animals, and consequently eagerly sought for by them as food. If, now, one individual of the species varies in colour from the normal standard in a direction slightly verging towards a protective hue, the advantage that it will hence derive in the struggle for existence will be equally trifling, even although a multiplicity of steps such as it has just taken might finally render the modified form scarcely perceptible to its enemies. Or we may suppose that one individual of the persecuted species takes the first step towards the development of a repulsive odour. Here, also, its chances of existence will not be perceptibly increased, though its devourer, if able to reflect so far, may perhaps think that the morsel was not quite so good as usual.

We submit, therefore, that under a multitude of circumstances, if variations of colour or odour, or augmentations of speed, are to give the individual thus modified a greater chance of survival, they must either occur simultaneously in a considerable number of specimens, or they must be advances in the required direction, not slight and scarcely perceptible, but well-marked.

There is another and a different consideration which in our opinion must not be overlooked, as powerfully tending to modify the influence of Natural Selection. It has been argued that individuals favourably modified in any way, but especially as regards strength or swiftness, will stand a much better chance, not merely of escaping their enemies or securing their prey, but also of obtaining mates and leaving offspring. Yet, so far as birds are concerned, this advantage, be it great or small, appears to be neutralised. In Mr. Wallace's work we find the following passage:—"Again, the evidence collected by Mr. Darwin himself proves that each bird finds a mate under any circumstances. He gives a number of cases of one of a pair of birds being shot, and the survivor being always found paired again almost immediately. This is sufficiently explained on the assumption that the destruction of birds by various causes is continually leaving widows and widowers in nearly equal proportions, and thus each one finds a fresh mate, and it leads to the conclusion that permanently unpaired birds are very scarce; so that, speaking broadly, every bird finds a mate and breeds."

Mr. Morant also remarks that there must exist somewhere "an establishment for unmarried female falcons."

Mr. Wallace very justly argues that this fact must counteract the effects, if any, of Sexual Selection. But it is scarcely less hostile to the action of Natural Selection. Granting that the pairs, as first formed, are composed of the strongest and most vigorous males and of the finest and healthiest females. But after a short time of the non-selective slaughter carried on, if not by man, yet by hawks, ravens, wild cats, weasels, snakes, and other bird-destroyers, the rejected of either sex find themselves mated, and of course become parents, substantially to as great a degree as their more favoured rivals. It may of course be contended that this indiscriminate slaughter falls equally upon the mated and the unmated. We doubt the correctness of this supposition: birds in the various operations connected with nest-building, hatching, and feeding their young, have to expose themselves necessarily more to danger than their bachelor and spinster neighbours. Among the lower animals, as well as among mankind, the pleasures and advantages of married life have, it seems, to be paid for.

Hence, without at all seeking to deny the existence and working of Natural Selection as a force effecting modifications in organic life, which may often extend to the formation of what we call species, we feel bound to admit that its influence is checked and modified in a variety of manners.

In Mr. Wallace's work another interesting question is discussed with results which further strengthen us in the belief that Evolution must have other—and probably more powerful—causes, and has at all events not always been effected by uniform and imperceptible gradations.

We are here reminded that the progressive development of the senses—a point scarcely as yet sufficiently investigated—is one of the most efficient ways in which animals may become modified in harmony with varying circumstances. An individual bird or beast, if possessing sharper sight, more delicate hearing or scent, than the bulk of its fellows, must plainly have a great advantage in the struggle for existence. It will be sooner warned of the approach of an enemy; it will more readily detect the presence of its prey, and will escape a number of subtle dangers to which it might otherwise succumb. Thus most of our readers will be familiar with the curious case of the pigs in Virginia mentioned by Mr. Darwin. All white pigs, it appears, were destroyed by feeding upon a certain root which took no effect upon black pigs. This remarkable phenomenon is ascribed by Mr. Darwin to a constitutional peculiarity connected with the dark colour, the black animals enjoying a perfect immunity from the effects of a poison which was fatal to all of the white variety. Dr. Ogle, however, gives a different and more probable explanation. He remarks that we have no evidence that the black pigs partook of the root at all. He considers that it possessed an odour or a flavour offensive to their senses, while the white pigs—endowed with less acute and discriminating smell and taste—ate it and perished. This fact is an admirable instance of the importance of acute senses to the preservation and multiplication of a species. Yet at the same time an advance in this respect can rarely be assumed to modify the structure of an animal or cause it to develope into a new species, even though acuteness or dulness of the senses may be respectively correlated with certain colours.

With the acuteness of the senses and its progressive development is naturally connected the history of colour, odour, and flavour in the world. Have the faculties and their objects been evolved in mutual harmony? Especially was colour existent before the colour-sense of animals had become able to recognise it—a process which, as we learn from the existence of colour-blindness, is even yet not complete. Are we to expect further advances as in the faculty, so in what it perceives?

Mr. Wallace considers that "when the sense of sight was first developed in the animal kingdom, we can hardly doubt that what was perceived was light only, and its more or less complete withdrawal. As the sense became perfected, more delicate gradations of light and shade would be perceived. At what grade in animal development the new and more complex sense—which takes cognizance not merely of the quantity of light, but also of its quality—first began to appear we have no means of determining." It was a somewhat prominent tenet of the old Natural History that the phenomena of colour, and indeed of ornamentation, in Nature, existed mainly in reference to man and with a view to his delectation. Mr. Wallace by no means agrees with many leading modern naturalists in the complete rejection of this assumption. He asks—"And even now, with all our recently acquired knowledge of this subject, who shall say that these Old-World views were not intrinsically and fundamentally sound; and that, although we now know that colour has uses in Nature that they little dreamt of, yet the relation of those colours—or rather of the various rays of light—to our senses and emotions may not be another, and perhaps more important, use which they subserve in the great system of the Universe." Elsewhere he remarks that "the extreme diversities and exquisite beauties of colour seem out of proportion to the causes that are supposed to have produced them, or to the physical needs to which they minister." And again:—"It is hardly conceivable that the material uses of colour to animals and to ourselves required such very distinct and powerfully contrasted sensations; and it is still less conceivable that a sense of delight in colur, *per se*, should have been necessary for our utilisation of it. The emotions excited by colour and by music alike seem to rise above the level of a world constructed on purely utilitarian principles." Yet at the same time he declares, and truly, that he has shown reasons for believing that the presence of colour in some of its infinitely-varied modifications is more probable than its absence, and that variation of colour is an almost necessary concomitant of variation in structure, development, and growth. On the colour-sense in animals he remarks "that the higher vertebrates, and even some insects, distinguish what are to us diversities of colour, but this by no means proves that their sensations of colour bear any resemblance to our own. An insect's capacity to distinguish red from blue or yellow may be (and probably is) due to perceptions of a totally distinct nature, and quite unaccompanied by any of that sense of enjoyment, or even of radical distinctness, which pure colours excite in us. Mammalia and birds, whose structure and emotions are so similar to our own, do probably receive somewhat similar impressions of colour, but we have no evidence to show that they experience pleasurable emotions from colour itself when not associated with the satisfaction of their wants or the gratification of their passions."

There is here, it appears to us, some little assumption. We have certainly no evidence that birds and beasts experience pleasurable emotions from colour alone. But what evidence have we to the contrary? The capacity of an insect to distinguish colours *may* be accompanied by any of that sense of enjoyment which pure colours excite in us. But why should we pronounce this probable? Still more, why should its power to *distinguish* colours be unaccompanied by a sense of radical *distinctness*? Quite admitting that the sight-organs of insects may differ from our own no less in mode of action than they do in structure, we should be prepared to expect that their perceptions may in nicety and accuracy surpass our own. Mr. Wallace himself, in his "Contributions to the Theory of Natural Selection," says "their [insect's] sight may far exceed ours both in *delicacy* and in range." In the present work, also, Mr. Wallace treats of colour as affording a means of mutual recognition, of especial value to insects, though he adds that "in birds such marked differences of colour are not required, owing to their higher organisation and *more perfect* senses." Now we have certainly no facts to prove that the sense of smell in birds ever attains anything like the delicacy and accuracy which are evinced in the case of certain insects—those, for instance, who are caught by the stratagem of "sembling."*

The brilliant and striking colouration of many berries Mr. Wallace considers may subserve the dissemination of the species. Birds attracted by the colour swallow the berry, and void the seeds in localities where they may take root. The same brilliant hues occur also, however, in larger fruits, where the seeds are never swallowed. Both birds and insects show that they are perfectly able to distinguish a ripe cherry, plum, or peach from one that is still green, and generally confine their attentions to the more highly coloured sunny side ; but the stone is left hanging on its stalk. Consequently the possession of striking colours by the fruit, and the recognition of such colours by birds, wasps, butterflies, &c., does not aid in the multiplication of the tree.

Mr. Darwin and Mr. Wallace both seem to agree that the highly-coloured spots on the wings of butterflies, being generally placed remote from any vital organ, may have a protective effect, causing birds to strike at these parts rather than at the head or body. If, however, we carefully consider the flight of a butterfly, we shall be inclined to doubt whether a blow aimed at the tips of the wings might not be quite as likely to fall upon the body.

Public attention has lately been drawn to a point in the history of colour-perception in our own species, which at first sight seems to have an important bearing upon the antiquity of man and the rate of his intellectual development, as well as to throw a useful side-light upon sexual selection, upon mimetism, and other phenomena among the lower animals. It is well known that a large proportion of

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* Quarterly Journal of Science, vol. viii., p. 304 (July, 1878).

living men and women in modern civilised nations (according to some authorities about 5 or 6 per cent), whilst perfectly able to distinguish by the eye the outline and texture of any object placed before them, its apparent distance, and its degree of illumination, fail more or less completely to recognise colours. To such persons scarlet and green are respectively undistinguishable, and are both liable to be confounded with grey. In other cases the eye perceives no difference between blue and yellow, and in some extreme instances the solar spectrum appears merely as a band lighter in some portions and darker in others, and all objects are viewed as if by a monochromatic light.

But imperfect as is the human colour-sense at the present day, there is, in the opinion of some, evidence that it has distinctly advanced within the brief span known as "historical time." Philologists have been struck with the fact that in the most ancient writings extant, such as the Bible, the Vedas, the Zendavesta, and the poems of Homer, no definite nomenclature for colours can be traced.

The phenomena of colour seem to have attracted less attention at the times when the above writings were produced than at the present day. One and the same term is applied to blue, to green, and to black objects. Iron is called by Homer "violet-coloured." In the autumn of 1877 an article by Mr. Gladstone on the colour-sense, as exhibited in the poems of Homer, appeared in the "Nineteenth Century," and has since been reproduced in the "Revue Internationale des Sciences." The writer there formally undertakes to show that the few colour-terms used by Homer are applied to objects so different among themselves "that they cannot denote colours as we perceive and differentiate them, but seem more applicable to different intensities of light and shade. Thus, to give one example, the word *porphureos* (ordinarily rendered purple) is applied to clothing, to the rainbow, to blood, to a cloud, to the sea, and to death, and no one meaning will suit all these applications except comparative darkness." In other cases the same object has varying colour-terms applied to it, the meaning of these being indicated merely by a reference to other objects fluctuating in themselves, so that the difficulty of determining what hue the writer meant in any particular case is insuperable. "Mr. Gladstone concludes that archaic man had a positive perception only of degrees of light and darkness, and that in Homer's time he had advanced to the discrimination of red and yellow, but no further, the green of grass and foliage and the blue of the sky being never once referred to."

But the very same want of reference to definite colours and the same poverty of colour-terms may be traced in literature very much later than the epoch of Homer. Thus Latin authors who flourished as late as the first century of the Christian era apply the word *cæruleus* to sky-blue, to steel-blue, to the colour of the human eyes, to the olive tree, and to dark grey and black objects; *viridis*, commonly rendered green, is used by Virgil for the colour of the human face when turning pale, and by Pliny for the hue of the clear heavens; *purpureus* is applied to the poppy, to the rainbow, to the violet, the rose, the willow, the human hair, the sea, and to the face when blushing. The colour of the sky is never mentioned in the Koran, and, according to Geiger, is first clearly alluded to in an Arabic work of the ninth century.

Now, that the vision of man, and indeed of all animals, was at one time monochromatic, and has gradually reached its present stage of development by a passage through some of the phases of what we now call colour-blindness,—which must be regarded as a reversion to an earlier condition,—we feel no difficulty in admitting. But that the human colour-sense should remain in a condition so rudimentary down to the days of Homer, and even of Aristotle, Pliny, and Vitruvius, and should then advance by "leaps and bounds" to its present condition, is an assumption difficult to realise, and scarcely compatible with our modern evidence concerning the antiquity of our race.

Mr. Wallace, with his usual acute insight, detects an error in the conclusion to which Mr. Gladstone has been led. He remarks:—"These curious facts, however, cannot be held to prove so recent an origin for colour-sensations as they would at first sight appear to do, because we have seen that both flowers and fruit have become diversely coloured in adaptation to the visual powers of insects, birds, and mammals. Red being a very common colour of ripe fruits which attract birds to devour them, and thus distribute their seeds, we may be sure that the contrast of red and green is to them very well marked. It is indeed just possible that birds may have a more advanced development of the colour-sense than mammals, because the teeth of the latter commonly grind up and destroy the seeds of the larger fruits and nuts which they devour, and which are not usually coloured; but the irritating effect of bright colours on some of them does not support this view. It seems most probable, therefore, that man's *perception* of colour in the time of Homer was little, if any, inferior to what it is now, but that, owing to a variety of causes, no precise *nomenclature* of colours had become established. One of these causes probably was that the colours of the objects of most importance, and those which were most frequently mentioned in songs and poems, were uncertain and subject to variation. Blood was light or dark red, or when dry blackish; iron was grey or dark, or rusty; bronze was shining or dull; foliage was of all shades of yellow, green, or brown; and horses or cattle had no one distinctive colour. Other objects—as the sea, the sky, and wine—changed in tint according to the light, the time of day, and the mode of viewing them; and thus colour, indicated at first by reference to certain coloured objects, had no fixity. Things which had more definite and purer colours—as certain species of flowers, birds, and insects—were probably too insignificant or too much despised to serve as colour-terms; and even these often vary, either in the same or in allied species, in a manner which would render their use unsuitable."

Mr. Wallace might here have added that the attention of the Oriental and Mediterranean nations was always turned towards man rather than to external nature. Hence their comparative indifference to beautiful scenery, their neglect of landscape painting, their failure in physical science, and their contempt for the industrial arts—so remarkable if we consider their degree of civilisation and the high intellectual development to which some of them had attained. That such nations should have no very precise nomenclature for colours—a nomenclature chiefly required in the pursuit of Natural Science and in certain manufactures—affords no proof that their colour-sense was not as perfect as our own. Hence it cannot be contended that the facts signalised by L. Geiger and by Mr. Gladstone enable us to draw any trustworthy inference as to the antiquity of the human race.

This brings us in contact with the subject which we are only just learning to discuss with scientific calmness and candour. The day is scarcely over since the dreams of Archbishop Usher and his coadjutors were supposed to be founded upon the direct testimony of Revelation. The notion that the world was "created in autumn 4008 years before the vulgar Christian era," and that our species had consequently not existed for quite 6000 years, was accepted as a main point of faith. Facts and arguments which pointed to a longer date raised gratuitous alarm among Christians, and exultation no less gratuitous among atheists. These mists and clouds are now clearing away, and thinkers of unimpeachable orthodoxy now admit that there are no theological grounds for a denial either of the antiquity of man or of the doctrine of Evolution, and that the Church may watch the contest between the Old and the New Schools of Biology as calmly as she did that between the Phlogistian and the Lavoisierian Schools of Chemistry. But Mr. Wallace puts in a word of caution which cannot be deemed useless. He reminds us that the hypothesis now dominant in scientific circles, that man has been gradually developed from some lower animal form, and that he has existed upon the earth from the Miocene epoch, possibly even from the Eocene, is not unbeset with difficulties. In the interests of Science these should receive full and fair consideration, and not be ignored, as were till recently the facts incompatible with the chronology of Usher. It is recognised as a curious circumstance that, notwithstanding the care with which pre-historical human remains have been sought for in all civilised countries,—notwithstanding the incidental facilities for research afforded by railway excavations, mines, and other engineering operations,—little if any light has recently been thrown upon the time or the mode of man's origin. "Amid the countless relics of a former world that have been brought to light, no evidence of any one of the links that must have connected man with the lower animals has yet appeared." Professor Mivart, in his well-known work "Man and Apes," has shown, by a most careful structural analysis, that man is related not exclusively and specially to any one of the anthropoid apes now existing, but almost equally to the orang, the chimpanzee, the gorilla, and the gibbon. Hence, on the evolutionist hypothesis, he is descended not from any one of these, but from an extinct and as yet unknown form which must have branched off at an exceedingly early date from the common stock. "As far back as the Miocene deposits of Europe we find the remains of apes allied to these various forms, so that in all probability the special line of variation which led up to man branched off at a still earlier period. And these early forms, being the initiation of a far higher type, and having to develope by natural selection into so specialised and altogether distinct a creature as man, must have risen at a very early period into the position of a dominant race, and spread in dense waves of population over all suitable portions of the great continent—for this, on Mr. Darwin's hypothesis, is essential to developmental progress through the agency of natural selection."

Such being the case, it is asked why we find no relics of earlier forms of man in company with those of other animals which were, *ex hypothesi*, less abundant? We reply that not one-hundredth part of what is now dry land has hitherto been satisfactorily explored. Possibly the extinct anthropoids may have mainly inhabited some of the regions which existed where now there roll wide, though shallow, seas. Living, as we might expect, among low-land tropical forests, their bodies, when dead, would be fully exposed to all the destructive agencies of Nature. Perhaps their habits were specially unfavourable to the preservation and fossilisation of their remains. Perhaps cannibalism was widely prevalent in those days. The order Primates is hitherto but sparingly represented among the fossil Mammalia. Nay, leaving the geological ages out of the question, and coming down the stream of time to within historical days, let us take some country which we know to have been densely peopled from four thousand to three thousand years ago, and ask how many human remains of such dates could be there discovered? We naturally except Egypt, and any other country where it was customary to embalm the dead. Is there some cause why the skeletons of the anthropoids and of man are more perishable than those of the lower forms of vertebrate life? Some writers have suggested that as the Quadrumana are now almost exclusively tropical, and the anthropoid species even equatorial, we should look for the earliest ancestors of man in such regions as the Malay Islands or Western Africa. To this Mr. Wallace replies that existing anthropoid apes are confined to equatorial regions because there only can a perennial supply of fruits suitable for their nourishment be found. But as in the Miocene epoch Southern Europe possessed an almost tropical climate, this restriction as to locality might then not have existed. Still experience shows us that a species is not necessarily found wherever conditions suitable for its existence are present.

We must, however, admit that if further geological exploration fails to place in our hands a greater number of human remains from the pre-historic ages, and especially anthropoid forms lower than the existing races of man, though higher than any existing apes, the views now dominant in scientific circles concerning the origin and early history of our race will stand in need of a careful revision. We shall apparently have to admit that man, however ancient, can scarcely have been formed by that slow and uniform process of development which must result from the operation of Natural Selection. It will be, as Mr. Wallace declares, " at least a presumption that he came into existence at a much later date and by a much more rapid process of development. In that case it will be a fair argument that just as he is in his mental and moral nature, his capacities and aspirations, so infinitely raised above the brutes, so his origin is due in part to distinct and higher agencies than such as have affected their development."

But is it necessary that the process of Evolution, by whatsoever agencies effected, must always have maintained a uniform degree of speed ? We have no desire to recur to "catastrophism," geological or biological, or to represent unknown and immeasurable forces as being arbitrarily introduced into action and again as arbitrarily withdrawn. But we find in phenomena governed by forces strictly natural, and even measurable, changes occurring more rapidly at certain stages than at others. To take a simple and familiar instance, the progressive increase of the length of the day in spring and its corresponding decrease in autumn is much more rapid at the equinox than at any other time. Or, turning to a region much more closely connected with the subject in hand, if we observe the development of an individual man—or indeed of any other animal—from birth to maturity, we do not find equal amounts of progress effected in equal successive portions of time. We know that in the life of a youth there is a period when, in stature and in the development of his mental and bodily powers, he appears almost at a standstill for two or three years, this lull being followed by a period of intensified growth, in which he shoots up at once into manhood. Is it not at least possible that a similar want of uniformity may be traceable in the evolution of species ? Prof. Leconte argues that every organism will oppose a certain amount of resistance to agencies calculated to effect a change. This resistance being once overcome, change will be for a time rapid, until a state of approximate equilibrium is again reached. Hence we may expect that at certain points where a great change has taken place certain " links "—the intermediate forms—will be missing. Their career is likely to be exceedingly short, not running to many generations, and for the same reason the number of individuals must be limited. Hence the probability of the fossil remains of such " links " being preserved for our inspection is infinitesimal indeed. When, on the other hand, the equilibrium is re-established, species exist with little change for centuries, possibly for thousands of years ; they spread over every accessible land suitable to their requirements, and increase in numbers as far as the supply of food and the other conditions of existence will allow. The probability is, then, that of the multitudes of individuals who successively flourish some will die under circumstances favourable to the fossilisation of their remains.

The differences of opinion we have been considering on the mode in which Evolution is effected, its main causes, and the laws of its action, are not surprising in view of the extent, the complexity, and the difficulty of the subject. Mr. Darwin has not so much solved the great problem of organic life as shown the way in which its successful study and its ultimate solution are possible. But whilst the greatest naturalists of the day are eagerly and patiently devoting themselves to this task, there are others who still feel free to introduce into the question extraneous difficulties, and to appeal to the passions and prejudices of an unscientific public.

Whilst waiting for an unpunctual train, at Dartford Station, our eye was caught by Mr. Morris's pamphlet. We read it through with equal feelings of surprise and regret. It is a work which might have been pardoned if it had appeared ten years before its actual date (1875), and if it had been from the pen of some journalist, novelist, barrister, &c., who could scarcely distinguish a humming-bird from a Sphinx-moth; but the Rev. F. O. Morris is himself a naturalist of merit, and, had he been so minded, might surely have criticised Mr. Darwin's theories, if unfavorably, still in a manner more useful to Science and more creditable to himself. As it is he sins equally against good taste, logic, and facts. Here is a specimen taken at random :—" I believe that such persons, in former times, as Sir Isaac Newton, Herschell, Lord Bacon, Dr. Johnson, Milton, Locke, Sir Matthew Hale, &c., who were believers in the Bible, were far behind me in intellect and knowledge. I believe, in like manner, that others in the present time, who are believers also as they were, such as Sir Roundell Palmer, Lord Hatherley, Lord Shaftesbury, Faraday, Sir David Brewster, &c., and others who like them have taken the highest honours in the Universities, and distinguished themselves in the highest departments of art, science, and politics, are quite beneath me in mind and attainments, for if I am right —as I must be, and therefore am—they of course must be wrong." *

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* It might be asked in what University Faraday graduated, till the day when he conferred rather than received honour by accepting degrees ? We might also inquire in what " highest departments of art" Sir Roundell Palmer Lord Hatherley, or Lord Shaftesbury has distinguished himself ?

What is all this but the old stale sneer which for ages has been levelled against every inventor and discoverer, who is taunted with setting himself up to be wiser than all the eminent men of the past! But Mr. Morris "double-banks" the fallacy. None of the distinguished men he mentions are biologists at all, and therefore, as far as the subject is concerned, they are all immeasurably inferior in knowledge and attainments to Mr. Darwin. Further, there is the gratuitous assumption that Mr. Darwin, as an Evolutionist, must necessarily reject the Bible. We know many Evolutionists who unhesitatingly accept the Bible as a moral and spiritual revelation, though they do not manipulate it into a geological text-book, or believe in the human traditions—chronological especially—which have sprung up around it. But Mr. Morris not merely accuses Mr. Darwin of Infidelity, but, if we do not misunderstand him, of a formal and conscious Infidel propagandism. "I have done all I could to make others as wretched as I am myself." "I do my little best or worst to shake their faith," &c. Need we put on record our solemn conviction that the aims of Mr. Darwin, Mr. Wallace, and of the majority of the naturalists of the new school, have been purely biological, and that to furnish arguments to the Infidel was no part of their plans? Need we remind Mr. Morris that charges closely analogous to those which he insinuates against Mr. Darwin were brought against Sir Isaac Newton, and with quite as much plausibility? Need we repeat that he who thinks to decide a scientific controversy by such foul play forfeits, *ipso facto*, all claim to the treatment of a gentleman and a scholar, and should at once be handed over to a very different court than that of the reviewer?

As a "supplement" to his curious collection of imputations and travesties, Mr. Morris gives certain extracts from the daily papers! We should have hoped that every man of science in England, or rather in Europe, must be fully aware of the gross blunders made by political and literary journals whenever they condescend to discuss a scientific question. One daily paper not long ago informed the world that "all gases explode far below redness, leaving nothing but a few particles of dust." A journal that displayed such ignorance on a question of history, of law, or of theology, would be well-nigh laughed out of existence. But an error in physics, or chemistry, or biology is detected by few, and therefore the proprietors of political papers do not think it worth their trouble to refer the criticism of a scientific treatise or a presidential address before the British Association to an expert. If Mr. Morris finds it necessary to call in the aid of "Punch," the "John Bull," or the "Globe," he only betrays his own "plentiful lack" of sound argument. But there is yet a final court of appeal: the authority is invoked of one who, we suppose, is no less distinguished by his candour, his courtesy, and his strict regard for truth, than by his vaunted "thinness of skin," his freedom from egotism, and the typographical eccentricities of his works, where italics and small capitals cover a multitude of sins. The pamphlet is, it seems, dedicated to "The Right Honourable the Common Sense of the People of England." We have more than once been compelled to point out that "common sense" is the name under which many worship their own ignorance. We were partly in the wrong; it is the name they invoke when they seek to utilise the ignorance of others.

III. THE "WOMAN'S-RIGHTS'" QUESTION CONSIDERED FROM A BIOLOGICAL POINT OF VIEW.

SINCE Natural History was remodelled by Mr. Darwin it has been found capable of throwing valuable lights, previously little anticipated, upon topics quite unconnected with the origin and attributes of zoological or botanical species. Of this solidarity of the sciences—one supplying another with methods of inquiry—a striking instance is afforded by a recent work,[*] in which the doctrine of Natural Selection is successfully utilised in the study of certain political subjects. That futher applications more or less analogous are still possible will scarcely be doubted. There is in particular one question now agitating human society which seems particularly to require such treatment. Everyone knows that of late years a movement has sprung up to secure for women, as contra-distinguished from men, certain rights, liberties, and powers of which it is contended they have been arbitrarily and wrongfully deprived. To

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[*] Physics and Politics, by WALTER BAGEHOT.

define this movement, and to formulate distinctly the demands of its supporters, is a scarcely possible task. Innovators and agitators of all kinds enjoy the advantage that they cannot be tied down to any fixed set of propositions by which and by whose logical consequences they are prepared to stand or fall. On the contrary, if one ground is found untenable another is instantly taken up; what satisfies one champion of the cause is rejected by another, and what today is accepted as final—as in the case of the anti-vivisection movement—is to-morrow proclaimed a mere instalment, and made the basis of fresh demands.

Perhaps we may best describe the movement as an attempt to obliterate all—save the purely structural—distinctions between man and woman, and to establish between them a complete identity of duties and functions in place of that separation which has more or less hitherto always existed. That certain speakers and writers, not content with mere identification, go on to inversion, and would assign to men the particular tasks now allotted to women, though a significant fact, need not detain our attention.

It is of no use laughing at this agitation as the outcome of a mere "crotchet." In certain states of the moral atmosphere crotchets spread just as do epidemics—which they closely resemble—in certain conditions of the physical atmosphere and other surroundings of man. Who would attempt to deal with the cholera or the small-pox by ridicule, how pungent and incisive soever?

We purpose therefore to examine this movement in the light of the principles of Natural Selection, of Differentiation, and Specialisation, and to enquire whether the relations of the sexes in the human species and the distribution of their respective functions are or are not in general harmony with what is observed in that portion of the Animal Kingdom which lies nearest to man—to wit, in the Mammalia. With the origin and history of the agitation, with the hopes and motives of its supporters, and with the ethical, sentimental, economical, and political arguments used on either side we have no direct concern.

Even a very superficial and popular survey of the class Mammalia will satisfy us that the structural differences between the males and the females of each species are by no means confined to the reproductive organs. The male ruminant, whale, bat, elephant, rodent, carnivore, or ape, is on the average a larger and heavier animal than his mate. The tiger, for instance, exceeds the tigress in size by a proportion of from 10 to 20 per cent. In few, if any, species is the superior stature of the male more striking than in the one which approaches man most nearly in its physical development—the gorilla.

But the mere difference of size is not all; the female is scarcely in any normal case a mere miniature copy of the male. Her proportions differ; the head and the thorax are relatively smaller, the pelvis broader, the bones slighter, and the muscles less powerful. The male in many cases possesses offensive weapons which in the female are wanting. In illustration we need only refer to the tusks of the elephant and the boar, and the horns of many species of deer. On the contrary, there is no instance of a female mammal possessing any weapon which is not also found, to at least an equal degree, in the male.

Further, the superior size of the head in the male, is not merely due to the more massive osseous growth needful for the support of tusks, horns, &c., but to a proportionately larger development of brain. Thus, according to the recent investigations of M. le Bon* "taking the mean weight of seventeen brains of human males, of 154 to 164 centimetres in height, and comparing them with the brains of seventeen women of the same stature, we find between the two a difference of 172 grms. (nearly 6 ounces) in favour of the male."

Summing up these facts, commonplace but not the less important, we see that in the whole mammalian class, man included, the males are distinguished from the females, not merely by larger size, but by superior cerebral and thoracic development, and by the more general possession of offensive weapons. On the other hand, trite as the remark may seem, the organs for the nutrition of the young are exclusively confined to the female. Are we to suppose that these sexual differences are devoid of meaning, merely accidental, or artificial in their origin?

We must next inquire to what functional distinctions these structural differences correspond, and what is their signification? It is generally admitted that among animals of one and the same species the larger will be found to be the stronger, and generally speaking physically the superior. Exceptions doubtless occur, but if we were to take one hundred men in normal health whose "fighting weight" ranged from 11 to 12 stone each, and compare them with another hundred averaging a stone less, we should find the former set able to lift greater weights, strike harder blows,

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* Comptes Rendus, lxxxvii., No. 2, p. 80.

and in every way excel the second lot in athletic performances.

Again, it is found that the size of the chest, and consequent volume of the lungs, affords a very good standard by which the general vigour, the vital energy of either man or beast may be gauged. The more a man, free from corpulence, measures round the chest, the better are his stamina, and the greater his power to support fatigue and hardships. Of this fact the military and the sporting world are perfectly aware, and never fail to take it into account in estimating the eligibility of a recruit or the probable performances of an athlete.

Having seen, then, that male animals are not merely actually larger than their respective females, but surpass them proportionally in the size of the thorax, we naturally expect the former to be decidedly the stronger, gifted with a more intense and exuberant vitality. Nor are our expectations disappointed. The bodily strength of a cow is trifling indeed compared with that of a bull of the same breed. In races a filly is very frequently—merely as such—allowed to carry less weight than a horse. A lady gorilla would be in evil case indeed if her husband did not treat her with a gentleness and kindness which many of our own species would do well to imitate. And as to mankind—Is not, perhaps, the most legitimate source of the very movement we are criticising an attempt to secure women against the superior strength of men? Yet at a meeting at Manchester a male agitator actually sought to deny the superior physical power of man, because it would be easy to find a fish-wife stronger than a cotton-weaver. The argument being intensely illogical was frantically applauded.

Persons are not, however, wanting, who—whilst admitting the general inferiority of women to men in physical strength—contend that this weakness is the result of continued and systematic repression. Woman, they say, has been forcibly debarred from invigorating pursuits, and comparative feebleness is the natural result. We would ask such advocates whether this systematic repression has been also carried out among the lower mammals, and, if not, what is the origin of the weakness of the female sex in their case, which is at least as well marked as among mankind? Has the "subjugation" of woman had its parallel in the "subjugation" of the cow, the mare, the ewe, the lioness?

That the women of the middle class in all civilised countries, and of the higher in some, would be much healthier and stronger if they took more exercise in the open air and swallowed less tea, we admit. But in that case we contend that their increased vigour would descend not to their daughters exclusively or specially, but to all their children.

Further, in some countries and among certain classes, a great amount of physical labour falls to the lot of the women, without their being thereby rendered equal in strength to the men. Among the North-American aborigines the squaw has the monopoly of hard work, whilst her husband—save when on the chase or on the war-path—indulges in idleness. Yet he runs no risk on that account of being surpassed in strength by his wife, and ultimately finding himself in consequence "subjugated."

No less is the superior cerebral development of the male sex in the human species, to which we have already referred as an indisputable fact, devoid of functional importance. It has, indeed, been contended that the difference in weight between the brain of the two sexes is a mere "survival" from some lower state of civilisation, or of existence which we may expect to see ultimately disappear. Such hopes, if they anywhere exist, must be abandoned in view of the results of M. le Bon, already quoted. This biologist finds that "the difference between the respective weight of the brain in man and woman constantly goes on increasing as we rise in the scale of civilisation, so that as regards the mass of the brain, and consequently in intelligence, woman becomes more and more differentiated from man. The difference which exists between the mean of the crania of contemporary Parisian men and that of contemporary Parisian women is almost double of the difference which existed in Ancient Egypt."

Taking hold of this simple fact, that the brain in the male is not merely larger, but increasingly larger, than in the female, we need not long search for its meaning. As the same writer to whom we have referred declares, "On examining series of crania sufficiently numerous we find that in the human species the largest brains belong to the races highest endowed intellectually, and in each race to its most intelligent members. Just, therefore, as higher civilisation is heralded, or at least evidenced, by increasing bulk of brain; just as the most intelligent and the dominant races surpass their rivals in cranial capacity; and just as in those races the leaders, whether in the sphere of thought or of action, are eminently large-brained,—so we must naturally expect that man, surpassing woman in volume of brain, must surpass her in at least a proportionate degree in intellectual power. We are sorry to be compelled here to own that while we know that in most, if not all, mammalian species the brain of the male exceeds in size that of the female, we have no observations as to any corresponding difference in mental power. That such difference, on careful examination, will be found to exist is highly probable; but we must likewise expect that it will be found less distinctly marked the lower the rank of the species.

To return: the intellectual superiority thus claimed for the male sex, in virtue of a higher cerebral development, is fully manifested in the history of the various arts and sciences. In every department the first, the leading, minds have belonged to the male sex. Homer, Shakspeare, Phidias, Beethoven, no less than Newton, Liebig, and Darwin, are men.

In reply to this historical confirmation of what biology foretells, the advocates of the movement adduce three arguments, all, in our opinion, singularly inconclusive.

Admitting the superiority of the male brain in bulk and weight to that of the female, they maintain the existence of a qualitative difference which renders the two incommensurable. This hypothesis, however, is a pure assumption. We should have an equal right to maintain that the brains of different races of men, especially as existing in ages widely remote from each other, were incapable of mutual comparison. Or, in the same spirit, it might even be urged that the smaller size of the muscles in woman was no proof of any inferiority in physical strength.

Secondly, it is contended by those who seek to identify the duties, functions, and spheres of action of the two sexes, that many women have distinguished themselves in the arts and sciences. Admitting to the full this fact, we can only place it on a level with the kindred phenomenon that not a few women have, in disguise, entered the army or navy, and have acquitted themselves as creditably as their male comrades; or that others have worked long and undetected as excavators, in the construction of railways, &c. The *savante*,—the woman of science,—like the female athlete, is simply an anomaly, an exceptional being, holding a position more or less intermediate between the two sexes. In the one case the brain, as in the other the muscular system, has undergone an abnormal development. That such cases should occur need no more surprise us than does the converse phenomenon, the existence of womanish man. We meet with subjects, otherwise of the male sex, in whom the beard is scanty or wanting, the limbs slight and rounded, the voice high, the chest narrow, and the pelvis broad, or who, if they do not structurally approximate to the female sex, betray a preference for feminine occupations, which wins for them such epithets as "molly-cots," "cot-queans," &c. At the risk of somewhat anticipating ourselves we cannot suppress the remark that no one demands especial laws and institutions for the benefit of such womanish men, or proposes their exemption from the customary duties of the male sex, how burdensome soever these may be felt.

The third and last plea put forward to explain, if possible, the cerebral inferiority of woman and her concomitant intellectual inferiority, is an adaptation of the one already proposed to account for her smaller physical strength. It is gravely asserted that mental activity in art or science has been systematically repressed among women, and that in consequence their cerebral development has been injuriously interfered with. To this contention it would be a sufficient reply were we to simply point to the fact already mentioned, that the relative inferiority in the size of the brain of women, instead of diminishing as their social status has improved, has, on the contrary, been increasing. We may hence fairly argue that it exists not in virtue of any artificial interference, but of a law of Nature. We can, however, adduce other considerations. In the pursuit of the fine arts, woman, instead of being checked and hindered, whether by law or by social conventions, has been encouraged. An acquaintance with music has been literally forced upon every girl of the upper and middle classes. Yet, leaving composers out of the question, how many of the million female performers on the pianoforte, now to be be found in Europe and America, can take rank with Liszt and Thalberg? In the highest development of literature, poetry, sex has been no obstacle to the recognition of merit. Yet neither Sappho in the past nor Mrs. Hemans and Mrs. Browning in our own day can be placed even in the same class with the leading poets of Greece, England, and Germany.

Women have certainly till of late met with few direct facilities for the pursuit of science. But, in England at least, neither have men. Our great scientific discoverers, until quite recent days, have been substantially self-taught, and even if in their youth they enjoyed a university education their subsequent researches, though *post hoc*, have assuredly not been *propter hoc*. Scientific books and apparatus have been as accessible to one sex as to the other; and these have generally been the only opportunities that our discoverers have had at their command. How to use such appliances they had to discover for themselves. We deny, therefore, that the exclusion of young women from universities, *in which modern sciences were not taught*, can have hindered them from entering upon a scientific career. Equally do we deny that public opinion forbade for them study and research. Had Miss Herschell been a man her astronomical discoveries could not have been more highly or more deservedly appreciated. Not a dog barked at her for preferring determining the orbits of comets to ordinary feminine avocations. In like manner, if any woman had possessed the necessary faculties and turn of mind, there was nothing in the way of public prejudices or established customs to prevent her from having anticipated Dalton in discovering the laws of definite chemical combination. Nor if thus discovered would the "atomic theory" have met with a less favourable reception. We then entirely deny the existence of any supposed conspiracy to repress scientific talent in the female sex, and we hold that the three arguments adduced to explain its comparative rarity among women to be utterly inconclusive.

A further distinction between the sexes, common to mankind and to all the mammalian class, must be sought in the moral faculties. Take what species we like we find the males bolder, more pugnacious and quarrelsome, more adventurous and restless, and less tractable and docile. The females, on the other hand, save in protection of their young from real or supposed danger, are mild, gentle, and inoffensive. Of this no more indisputable instance could be found than the case of domestic cattle, the cow—with the exception of certain "strong-minded" individuals—being perfectly harmless, whilst the bull, when above four years old, is one of the most dangerous animals known, attacking and killing human beings, not for food, like the lion or the tiger, but out of pure "superfluity of naughtiness." Very similar is the distinction between the character of the sexes among the Quadrumana. No animal is more wantonly and gratuitously mischievous than an adult male baboon, and we are unable to find an instance of one having been tamed so far that he could be allowed his liberty. The females, on the other hand, are capable of domestication. Were there any necessity to multiply instances a fair-sized volume might be filled with accounts of the intractability of male Mammalia of different species, as contrasted with the mildness and docility of their females, whilst in no animal is the case reversed.

That the sexual distinction of character in our own species is precisely analogous in its nature will, we trust, be admitted without argument.

We find, therefore, summing up the foregoing facts, that throughout the mammalian community the males are larger and heavier than the females, whom they moreover especially exceed in thoracic and cerebral development; that they are consequently stronger, more intensely animated, and in disposition bolder and fiercer. The very same differences are found in average men as compared with average women, with the additional peculiarity that here the superior size of brain expresses itself in higher intellectual power.

It would be ridiculous to suppose that all these diversities, structural and functional, are objectless, and do not imply a corresponding diversity of duties. This accordingly we find to be the case:—The male, at least in all species which form unions of any degree of permanence,—whether monogamous or polygamous,—defends and protects the female and her young ones. Thus if a herd of elephants is menaced the most powerful tuskers take their station on the side where danger appears, whilst the females and the young are placed as far as possible out of harm's way. If bisons are attacked by wolves the bulls form a circle enclosing the cows and calves. A similar order is adopted by wild horses. A gorilla will encounter any danger in defence of his mate, and even among baboons the old males will face an approaching enemy while the weaker members of the troop make good their escape. A lion has been seen in the same manner covering the retreat of his lioness and her cubs.

Other examples might be given were it at all needful, but those already stated are surely sufficient to establish the principle. Among herbivorous and omnivorous species, where food is plentiful, there is no occasion for the male to take upon him the duties of provider, but among the Carnivora he frequently supports as well as defends his family. The lion is in this respect a well-known instance.

We find, therefore, that throughout the class Mammalia the respective tasks of the two sexes are precisely such as we find in our own species; the male is the defender and provider, wherever such defence and provision are necessary; the female is the nurse. The man who brings home to his wife his weekly earnings, his professional fees, or his share of the profits of a business, merely repeats on a higher scale the action of the lion who carries a deer or an antelope to his den. Each sex fulfils the tasks for which it is specially adapted by Nature, and anything like "subjugation" is utterly out of the question. Were the duties of the two sexes confounded together, or, still more, were they inverted, —the female, for instance, going forth to face danger or to hunt for prey, while the male was left to nurse the young,—the position of the species in the great and constant struggle for existence would be very decidedly altered for the worse. We must conclude, therefore, that the attempt to alter the present relations of the sexes is not a rebellion against some arbitrary law instituted by a despot or a majority,—not an attempt to break the yoke of a mere convention; it is a struggle against Nature; a war undertaken to reverse the very conditions under which not man alone, but all mammalian species have reached their present development. Sentimental speakers and writers have commented on the well-known fact that even a very young boy will, to his utmost ability, defend his sister or female playmate, and have expressed a hope that this habit—the result of early training—would wear out, the female no longer needing and the male no longer offering protection. Alas! is the very same habit in the ape, the lion, or the bison the result of a mistaken training, or of an Old-World convention, to be laid aside in these enlightened days? What would be the position of a family of young lions if both their parents went forth to hunt? Yet very similar will be that of children if their mother, as well as their father, goes out to the daily toils of a profession, leaving them perhaps to themselves,—perhaps to the care of ignorant and unprincipled hirelings. The results of mothers withdrawn from domestic duties, and spending their days in industrial pursuits, have been sufficiently exemplified in our manufacturing towns. Here, in the very highest interests of the race, it has been found necessary to check and limit female labour, which ought never to have been introduced. Had this precaution been taken a man would have been able to earn as much as he and his wife jointly have been able to realise under the factory system. But what reason have we to expect that the introduction of female labour into professional spheres will prove a greater boon either to the aspirants themselves or to the nation than it has been in the factory and the workshop? A friend, of original habits of thought, points out[*] that upon man alone was laid the penalty of labour as upon woman the sorrow of child-bearing. This is in fact the very same lesson, clothed in theological language, which we learn from biology. Among the lower animals, who, as compared with man, may be called the proletariate[†] of creation, both sexes indeed seem merely or mainly to exist in order to perpetuate

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[*] Genesis iii., 16, 17.

[†] As applied to the human species we consider this term eminently foolish. The man who benefits his race in no other way will probably injure it by leaving posterity like himself.

their species. Still, even here, the female is more exclusively constructed for and more totally absorbed in the task of reproduction than the male. The share of the latter in this function is, strictly speaking, momentary, whilst during the stage of maturity the energies of the normal female are more or less completely devoted to the nurture, intra- and extra-uterine, of her offspring. Even when she never becomes a mother the generative system exercises a modifying influence upon her whole career. This consideration throws a strong light upon the ground taken by certain of the more "advanced" female advocates of the movement. The *femme libre* of the new social order may, indeed, escape the charge of neglecting her family and her household by contending that it is "not her vocation to become a wife and a mother." Why then, we ask, is she constituted a woman at all? Merely that she should become a sort of second-rate man? We have already declared, and we repeat, that we wish a free career for every talent. If an abnormal woman possesses a man's muscular strength and adaptation for toil, we would not, either by law or by social influences, seek to debar her from working at the oar, or the forge, or even from wielding the policeman's truncheon or the soldier's rifle. But we would not calculate on such anomalies; we would not legislate for their special protection, or seek to increase their number. In a manner perfectly analogous, if a woman possesses the taste and the power for scientific research usually confined to men,—and far from common even among them,—we would not wish to restrain her from the cultivation of her peculiar faculties; but we would not foster the growth of such a class of females. We would not seek to entice women into the observatory, the laboratory, or, above all, into the dissecting-room, nor erect colleges for the training of *savantes*, any more than we would organise female regiments and open institutions where muscular young ladies might perfect themselves in the management of heavy artillery.

It is generally—too generally—assumed that every novelty, every change from what has hitherto been customary and recognised, commends itself, on the mere ground of its novelty, to men of science, as indeed to all unfettered inquirers, and will be resisted merely by those whose guiding principle is an unreasoning attachment to what is established. Never, perhaps, was it shown more clearly than with reference to the present question that innovation may be retrograde,—that a proposed change, if carried out, may involve a return to a lower stage of development. What is the very essence of all advance to a higher stage of being, save differentiation? We see what was at first homogeneous, uniform in structure, become resolved into distinct tissues and members. We see functions, which in some rudimentary state were jointly exercised by the whole body of an animal, gradually allotted out to special organs, and during and in consequence of this very specialisation acquiring a far higher degree of perfection than they heretofore possessed. Look at the first rudimentary state—germ, seed, or ovum—of the plant or animal, and compare it with the mature organism to which it ultimately gives rise. What was one has become manifold; what was simple is now highly complex. The globule of albuminoid matter has developed into distinct members — sense-apparatus, organs respiratory, digestive, circulatory, locomotive, &c.—each of which has a separate task to fulfil, and is distinctly organised for that very purpose. It is no departure from our subject to remark that though in the organic body one organ may, under certain circumstances, undertake the duties of another, such vicarious action involves grave peril to the organ concerned and to the entire animal. Perhaps the world may yet find that the analogy between the individual and mankind holds good in this respect, and that a social congestion may follow from the movement we are examining.

To return: the increase of size which distinguishes the butterfly from the egg, or the oak from the acorn, is a trifling feature compared with the accompanying differentiation—chemical, morphological, and functional—which has taken place.

If we pass from a consideration of the individual plant or animal to a survey of the entire organic realms we find, as we advance from the humblest and meanest beings to the highest, merely a repetition of the same great fact. At the one extremity of the scale—if this expression may still be used—we find beings whose senses, such as they are, must be exercised by the whole external surface of the body, those more special functions which we know as sight, hearing, &c., being still identical with feeling. No distinct nervous system, still less definite nerve-centres, can be traced. Nor are there any organs specially devoted to the processes of respiration, circulation, digestion, &c. A common internal cavity takes the place, and in a crude way fulfils the duties of all these parts. Externally the same uniformity prevails; there are no limbs, no members exclusively constructed for locomotion in any of its modes, or for prehension. The animal moves by elongating and contracting its whole body, or by rolling over. In many of the lower forms of animal life the sexes are not separated, the functions of the male and the female being exercised by one and the same individual. It is a fact long familiar to the world that the polype may be cut into without injury, each part soon becoming a complete animal.

To such simplicity of structure the completest contrast is afforded by the higher animals. Throughout their bodies we find a "division of labour," each function having its organ and each organ its distinct function. To trace how this differentiation is carried out would be wearisome, and, being admitted, is fortunately needless.

It may be useful, however, to call to mind the fact that animals which when mature are broadly and easily distinguished from each other, are more and more alike the earlier the stage of growth at which we institute a comparison. The differences between a baby chimpanzee and a human infant are much slighter than those between the adults of the respective species. If we extend our researches to the embryonic state we find that the rudimentary man can scarcely be distinguished from many of the other vertebrates. It is only, as Prof. Huxley points out, in the later stages of pre-natal growth that the human fœtus differs from that of an ape. In the former the convolutions of the brain, according to Prof. Bischoff, reach about the same stage of development as in an adult baboon. The great toe, in man, is considered by Prof. Owen the most characteristic feature of the human skeleton; but in an embryo about an inch in length Prof. Wyman found this member not lying parallel with the other toes, but projecting out from the side of the foot as it does permanently in the so-called Quadrumana in their mature condition. Thus plainly does it appear that differentiation is the way to perfection, each animal as it approaches maturity diverging more and more from other forms, from which in its earlier stages it was scarcely distinguishable.

Yet again, we may turn from a survey of the growth of the individual, and from a comparison of the highest and lowest forms of contemporary organic life, to the consideration of the successive phases of being that have peopled our earth. Here, too, we find the same great law prevail. In the remote past we find what are called "generalised forms," —animals which seem to have combined in themselves the rough outlines of what we now find developed into perfectly distinct beings.

Suppose it were now proposed as an improvement in the structure of man, or of any other mammalian species, that the functions now exercised by two distinct organs—such as, *e.g.*, the eye and the ear, or the nerves of motion and of sensation—should be "lumped" together, committed to one only set of organs; would such a change, if we for the moment suppose it practicable, be an advance or a retreat? Would it raise or lower the species in the scale of existence? It might seem a convenience if, instead of seeing with our eyes alone, we could also see and hear with our ears; but would either the seeing or the hearing be done as well as now, when each is the sole function of an express organ? On the principles of the old Natural History, as well as of the New, we may safely reply in the negative. The change we have supposed is fortunately incapable of being effected, otherwise the attempt would doubtless be made in the name of "progress."

But we may follow the principle of Differentiation, and trace its workings over the boundaries of Biology into those of Sociology, if such a science can be said to exist. Where differences of structure can no longer be traced we still find differences of function. In man we find no variation in the number and position of bodily organs; yet identical organs in different individuals are trained to special tasks which to other men would be impossible, and which might seem to necessitate a structural difference. We come, that is to say, upon the division of labour, which is one of the most characteristic and essential features of civilisation. We have seen that in the lowest forms of animal life the entire body seemed to subserve every vital process; just so in the lowest stages of human society, every individual is at once warrior, hunter, builder, maker of arms and other utensils, and—in as far as agriculture is practised at all—tiller of the soil. Every man is perforce, in the words of the adage, "Jack of all trades," with the inevitable consequence that he is "master of none." In a civilised nation, just as in the higher animals, all this is reversed. Every function has its special organ, or, in other words, every task is committed to a separate man or body of men. In all this we can trace out nothing that speaks of arbitrary interference or compulsion. In the animal or human body each function is committed to organs fitted for that function. The stomach does not protest because it is not the seat of respiration, nor does the heart crave to undertake the task of digestion, either instead of or along with its own duties. In human society—complain as we may about "square pegs" being placed in "round holes"—the different tasks are in the main assigned to the men most competent for their performance. In a savage tribe the strongest and bravest naturally leads in war; the man keenest of eye and ear becomes the scout, either as regards hostile tribes or beasts of the chase. The wisest and most eloquent—attributes which if necessarily connected in primitive times are now so no longer—took the foremost place in council. The man of greatest manual dexterity would be chief bow-maker to the tribe. The process in operation was, in fact, Natural Selection. The man who undertook a task for which he was unfitted, or less fitted than others, was gradually eliminated, as far as that particular task was concerned. In proportion as new wants sprung up and new means of gratifying them were devised, social functions were multiplied, and the division of labour became more minute. Yet even in the very rudest state, as far at least as anthropologists have been able to trace, there never was a time when the duties of all persons were absolutely identical. To men and to women different duties were assigned on the same principle of Natural Selection. Changes have, indeed, taken place in the distribution of the tasks respectively allotted to the two sexes. But these changes, it is important to note, till the "woman's-rights' movement sprung up, have all been in one direction—the direction of increasing differentiation. The distinction between men's work and women's work has been increased, not diminished. The barbarian and the semi-civilised nation allowed women to carry heavy burdens, to tug at the oar, to wield the spade, the hoe, the mattock in the fields, and even to labour in mines. In our higher civilisation such tasks are limited to man, and, as we have already remarked, to abnormal "mannish" women. The movement we are considering, in so far as it aims at breaking down the natural barriers between the duties of the two sexes, is palpably retrograde. If advancement towards perfection is reached by differentiation, *anti-differentiation*,—if we may use the expression,—whether structural or functional, must be a return to a lower condition. If the first and plainest step in the division of labour is to be abandoned, how can others be maintained ?

It has been already pointed out in the "Quarterly Journal of Science" that among vertebrate animals the social unit of which nations are put together is the family, whether that be monogamous or polygamous. A community of rooks is made up of an assemblage of married couples. A tribe of baboons consists of a number of males, each one having his wives and offspring. Now the "woman's-rights' movement" not merely runs counter to Nature in the respects we have already shown, but it is open to the charge of seeking to destroy family life and to constitute society of individuals—of atoms instead of molecules. In so doing it tends towards the condition of things prevalent in certain insect-communities. But there the mass of the nation, and especially its working and fighting members, is composed of what are commonly called neuters. Of such an arrangement no trace prevails among vertebrate animals, and we do not therefore see how their example can afford us any practical precedent.

We have therefore, in fine, full ground for maintaining that the "woman's-rights' movement" is an attempt to rear, by a process of "unnatural selection," a race of monstrosities—hostile alike to men, to normal women, to human society, and to the future development of our race. We know that the modern "honorary secretary" is always ready to exclaim "Let heaven and earth perish, so my crotchet may be realised." But we would bid him ask himself whether the end is worth the means?

IV. WHAT IS A FLOWER?

By F. T. Mott, F.R.G.S.

It is admitted that a complete flower consists of four parts or whorls, viz., the pistil, the stamens, the corolla, and the calyx; but it is maintained that the *essential* parts are the pistil and stamens only. The Monochlamydeæ, in which the corolla is absent, and even the Achlamydeæ, which have neither corolla nor calyx, are still included among Flowering Plants.

The technical rules by which this conclusion is arrived at are, first, that in classification organs which are universal have more value than those which are sometimes wanting; and, secondly, that fertilisation by pollen is more complex, and therefore of higher value than fertilisation by antherozoids.

For the mere purpose of classification these rules are probably sound, but at the same time they hide from us one view of Nature which is a truer and more interesting view than that which they present to us. They help to make us familiar with the *results* at present attained, but they blind us to the *processes*, to the laws of perpetually changing life which have given to the vegetation of the world a different aspect from age to age.

In order to classify results we give the highest value to characters which are most fixed and permanent, and these are to be found in the organs which are earliest developed and most universal. But in order to see what Nature is doing, and what she promises to do,—what point she has reached, and what is likely to be her next step forward,—we must look not to the old and permanent, but to the new and the changing.

There is scarcely anything more permanent in a plant than its organs of reproduction: the forms which these assume are therefore of high value in the distinction of genera and species. It is true, also, that when these organs take the form of pistil and stamens they are more complex than, and show a decided advance in development over, the lower and simpler form of archegonium and antheridium. On these grounds it is correct to distinguish pollen-bearing from antherozoid-bearing plants, and convenient to use the variations in pistils and stamens as permanent marks of genera and species.

But is it correct to make "pollen-bearing" synonymous with "flowering," and to regard pistil and stamens as the essentials of a flower, calyx and corolla as only accessories?

There is an immense difference in appearance between a rose and a catkin, between the "flower" of an iris and the "flower" of a grass. A large part of the beauty of the world and the poetry of life would be blotted out if plants had no corolla. Can it be true that this last and loveliest outcome of vegetative force is a mere superfluity? a bit of meretricious tinsel just put on by a few clever plants to catch bees and butterflies, as a girl uses a bright ribbon to catch wandering eyes? But the girl's best and most attractive ornament, after all, is her face, which was made by no art of hers. Like the flower, it is the inevitable product of internal forces, ordained from the hour of birth—the loveliest thing in all the world. And is the beauty of womanhood only a lure for men?

Let the following arguments be fairly weighed:—

1. Reproductive organs are common to all plants of every grade, and the process of fertilisation is essentially the same whether it be by antherozoids or by pollen. Reproductive organs do not, therefore, in themselves constitute a flower

unless *all* plants have flowers. But if the archegonium of a moss and the catkin of the hazel are flowers, we surely want another name for the rose and the lily. Yet it is these which have the prescriptive right to that name. The rose is a flower to all people and in all tongues. The name has been appropriated by botanists to the reproductive organs of other plants on the assumption that these were the essential objects which it represented, but the popular voice has never confirmed the act. The child who gathers buttercups and daisies with delight repudiates docks and sedges as "weeds," not "flowers." This popular nomenclature is not necessarily right, but it shows that there is a marked distinction which botanists have perhaps not sufficiently regarded.

2. Plants do not exist for the *purpose* of reproduction. It seems to be very generally assumed that when a plant has perfected seed it has accomplished the object of its life: this is surely a mistake. Reproduction is a means, not an end; the means by which the continuation of the race is secured through generations of perishing individuals. The life of the individual is maintained by food, the life of the species by reproduction, which means the carrying forward of potential energy, from generation to generation, with gradual accumulation during the development of a species and gradual loss during the period of dying out. But as the individual does not live for the mere purpose of feeding, so the species does not live for the purpose of being reproduced. It may be said, rather, that both the individual and the species exist because the condition of the ever-moving and ever-changing Force—whose movements and changes are precisely recorded and represented by the movements and changes of Matter—produces, at this epoch, effects of that particular kind. Whether that Force be voluntary or involuntary, this is the "final cause" of all things as far as human reason can discover.

The motions of Force being wave-motions, the life of both individual and species is of the nature of a wave, and has its gradual accumulation, concentration, and climax, and then its gradual decline, dissipation, and extinction.

Assimilation of food and reproduction are necessary incidents in this process, but the cause is the existence of the Force-wave, and the ultimate end of that wave is the attainment of its climax, its maximum of concentration and unification.

The climacteric of individuals is attained at various stages, according to the position of the individual wave in the great specific or ordinal wave. There are Fungi and Algæ which never attain to any development beyond the embryonic cellular tissue; ferns which develop early forms of vascular tissue; Conifers reaching the higher stage of wood-formation; oaks and alders, poplars and willows attaining to the leaf stage;—and beyond these, what? The true flower-bearers, the dichlamydeous phanerogams, whose life-wave—passing through all the earlier stages of cell, wood, and leaf—attains to the development of corolla, and floods the world with a glory of colour previously unknown.

3. The corolla of nearly all flowers, when first formed in the bud, is of a pale greenish tint. The cells are filled with protoplasm, partly fluid, and partly granular in the form of chlorophyll. As the bud swells, the cells of the corolla are rapidly enlarged, at the expense of their contained protoplasm. If the protoplasm is entirely exhausted in this process, the cells, when the flower opens, are empty or filled with a thin transparent fluid, and the corolla is *white*, from the total reflection and transmission of all the light which falls upon it, the intensity of the whiteness depending upon the smallness of the cells. If there is still protoplasm to spare when the corolla cells are developed, this surplus is differentiated into substances either fluid or granular, which give colour to the cells they occupy. In proportion to the quantity of this surplus will be the quantity and intensity of the colour.

Colour is of course produced by the absorption of certain constituent rays in the white sunlight, and the reflection of the remainder. The colours of flowers are nearly all *secondary* colours, combinations of two out of the three primaries, red, green, and violet. It is evident, therefore, that the colouring-matter in the cells is monochromatic—that it absorbs only one of the three primary colours, or at least that it absorbs one in much greater proportion than any other: and since the colours of flowers are mostly very bright, the quantity of any other colour absorbed must be small. The brightness of colour is in proportion to the absolute amount of light reflected to the eye. White light, which is a ternary compound, will always be brighter than any combination of portions of its constituents.

The secondary colours are brighter than the primaries, because they are composed of the combined light of two constituents instead of the single light of one.

Grey is a feeble white—a mixture of the three primaries, but not in quantities sufficient to give the effect of white.

Brown is a feeble yellow—a mixture of red and green in small quantities. The green of summer foliage is the primary green of the spectrum with small additions of red and violet, producing yellow or blue greens.

The colours of the stems and branches of trees and shrubs are generally *browns* and *greys*; the foliage is *green*, and the flowers are *yellows*, *pinks*, and *blues*. In terms of the absorption of light these facts mean that in the stem and branches all the rays are absorbed, very little being reflected, violet least of any; that in the leaves *two* of the primaries are absorbed, while one is reflected; and that in the flowers one primary only is absorbed and two are reflected.

This change of condition in the colouring-matter at the three stages of development is in each case in the direction of greater concentration and unification. The polychromatic colouring-matter of the stem becomes dichromatic in the foliage and monochromatic in the flower.

The phenomena of light absorption are supposed to depend upon the molecular condition of the absorbing substance. Molecules appropriate the energy of those light-rays whose wave-lengths coincide with their own normal vibrations. In the stems of trees vibrations of all lengths are mixed together, and all the light-waves are absorbed. In the foliage the vital energy is concentrated in two forms of vibration only. In the flower concentration has been carried to its extreme limit. White flowers, which in the present era are as numerous as all the coloured flowers put together, are nevertheless, as flowers, in an *embryonic* stage. The protoplasm, the vital substance, has been exhausted in producing the petalous structure. There is not energy enough to fill the cells with living matter. These white-flowered species are in arrear of their coloured congeners, but some of them will probably in a future epoch attain the higher grade.

It is the special character of the centripetal wave of Vital Force to simplify relationships at each successive stage of development. This is seen in the external elements of a flower as well as in its internal structure. The arrangement of vascular bundles in the stem is not distinctly symmetrical. In the arrangement of leaves, however, there is a symmetry more or less evident, while in the flower there is a still greater concentration of parts and simplification of arrangement, so that symmetrical relationship is a striking feature and one of the main elements of floral beauty.

4. Flowers may exist, and do frequently exist, without

the reproductive capacity. Some species of violets produce their coloured and fragrant flowers in the spring, but these are barren, while the seed is produced in late summer from blossoms which contain the perfect stamens and pistils, but have no corolla. The Hydrangea and the cultivated Guelder-rose, with the majority of double-blossomed plants, carnations, roses, hawthorns, cherries, gorse, &c., must be excluded from the category of flowers if the reproductive function is taken as the distinguishing character. These plants, though luxuriant in growth and supremely beautiful, bear no seed when in their finest condition; they do not reproduce the species; the vital wave has reached in them its ultimate climax; the destiny of the species is fulfilled; there is no longer any necessity to carry forward the wave of Force from generation to generation. They have attained the beauty which marks maturity; reaction is about to set in; the wave would enter upon its descending phase, and the species would rapidly die out and disappear if it were not that man stays the dissipation of the wave by lateral propagation—a process analogous, perhaps, to that of preventing the dissipation of any given sound by inclosing it in a tube.

It is no answer to this argument to assert that the doubling of the flower, or the enlargement of the corolla at the expense of the stamens and pistil, or the conversion of the calyx into a coloured corolla, is an abnormal condition. Variation is universal. Abnormal variation is only such as exceeds the average limit. Alter the surroundings, or increase the energy of the wave, and the average changes; what was before abnormal is now normal. Abnormal variations, when they are due to accumulating energy, are prophecies of future development. A wild stock will just exist and perpetuate its species in poor soil crowded with competitors. Give it more air and food, and its growth will be more luxuriant, and its flowers larger and brighter. Remove it to the garden, supply it with the most favourable conditions for accumulating energy, and some of its flowers will produce a fifth petal. Select the seed which these flowers leave behind them, and you will at last get the beautiful but barren double stocks. Each change has been due to the same cause, augmented energy, and no one is more abnormal than the others.

The double flower is not the result of disease, but of accumulated energy. Diminish the energy, and you get palpable degradation in size or colour, or both. When Aphides attack the heads of wild honeysuckle the flowers are small and *green*. The change of the protoplasm from a dichromatic to a monochromatic condition has not been effected, for want of vital energy. The phenomenon of increased blossom arising from checked growth is merely the result of a transfer of energy from one of the great secondary waves to another. The splendid flowers of many monocotyledons have probably been hastened towards maturity by the almost total suppression of the osseous secondary wave. The energy not used in the formation of stem and branches has been concentrated upon the blossom. Dr. Masters records that when *Kerria Japonica* was first introduced into Europe it bore single flowers, but in a few years every existing plant had become double-blossomed. Doubtless the change of climate hastened its development, brought the wave rapidly to its climax, and, of course, the species would have died out in Europe but for artificial propagation.

The average energy in those wild plants which bear coloured flowers is, at this epoch, sufficient to produce monochromatic protoplasm in one whorl only. An increase of energy brings the other whorls into the same condition, and the flower is doubled.

5. Flowers, then, are the ultimate term in that wonderful series of biological phenomena exhibited in the Vegetable Kingdom. Their essential characters are—concentration of external parts producing visible symmetry, and of internal energy producing monochromatic protoplasm, or, as in white flowers, the absence of protoplasm, from defect of energy at the moment of consummation. The reproductive organs associated with them are imperfectly developed petals, whose office is to carry on the specific wave until it reaches that point of concentration at which these organs also assume their ultimate character, when the doubled flower marks the fact that—in at least that place and time —the last of the four great secondary waves has reached its climax. When the species everywhere attains the same condition, that specific wave is about to enter upon its descending phase, and the species will shortly disappear.

It is probable that we can as yet form no conception of the splendour which flowers are destined to bestow upon this world in some far-off future age. We are yet in the epoch of foliage rather than of blossom; and beautiful as this is in its varied forms and tints, from tender spring to gorgeous autumn, when the flowers open and the brilliant secondary colours flash out amidst the primary green, we own that a new glory has been conferred upon the beautiful earth.

We collect in our gardens and conservatories the richest flowers that are scattered here and there through northern plains and tropic forests, but the "coming race"—whether of man or of some higher type of being—shall behold the earth one magnificent conservatory, one universal garden, and the music of colour shall become a familiar art.

V. GENESIS OF MATTER.

ASTRONOMERS have played with world-making, and mechanicians and mathematicians have laboured to do and to undo the movements of heavenly bodies, and in some departments probably the conclusions are final, but there is much left undone, and to chemists belongs a great portion both of work and imagination. To make a world chemically we must begin with chaos. I shall not attempt to go farther back to-day, but who knows where the imagination will venture? We must begin with disorderly masses. There must be no gravitation; that introduces order at once, and commands the attention of all that part of creation which exists in the form of that which we call Matter. It gives the commands "Stand still" or "Move on," to the greatest and smallest of the bodies which we know, with a calmness and inflexibility that no one attempts to resist. There must be no cohesion, or a large amount of order would exist; but, in small and numerous communities, a collection of villages like a region inhabited by savages; and this may have been a stage—who can tell? There must be none of our elements, because they all gravitate; and this wild condition of things we can only imagine by analysing the present.

Of course we begin with hydrogen, as apparently least composite. The belief is that this as a gaseous body is at least a compound of its own particles; in other words, that the ultimate parts of hydrogen do not keep isolated, but show their love of combination by forming molecules. Afterwards there is very little inclination visible to unite under ordinary temperatures. Let us suppose a molecule—or, if it pleases better, an atom—of hydrogen put into a jar containing nothing beyond that which fills a vacuum. No "wild bull with fireworks on its horns and eagle gnawing at its back" will ever rage so violently; it will strike the side thousands of times in a second, and according to some it will continue to do this for ever and ever. Nay, is it not true that Force is immortal, and this motion must go on for ever. An eminent thinker has put this forward as a fundamental characteristic of atoms, that each, although formed out of the original material, has impressed upon it a peculiar motion, which is its personality, and constitutes its individuality and peculiarity as an element. That may be so in a sense, but it is not the gaseous motion which is the original creation, for that we know ceases, unless we can prove the same quality of motion to be existing even when the gaseous state ceases. And, after all, what is an atom separate from its motion? Is it not true that Force and Matter are one—that Matter moves by its own intrinsic and eternal quality of motion? The exact motion of gases is not eternal; the same frantic atom of hydrogen will stop its violent movements, give them up with one mighty gasp, —and so far as we know will never resume them,—and apparently on slight provocation, by a touch of chlorine, of oxygen, or of cold. Hydrogen, then, rages only under certain circumstances, dances only when motion is put into it; its motion is not eternal and unchangeable; it may be frozen into a liquid; it may be made into a solid, for ever inactive, and, like any poor piece of stolid matter, it may lie for ever dead. True you may give it life again, and it will dance on as merry as before; but the life must come to it.

Some people think that this gaseous motion is a perpetual one, and that they have at last discovered it in gases; but no—the fire seems to be required to keep up the motion of the hydrogen, as it keeps up the motion of the steam-engine, and when that goes out both cease. Matter is very dead; it may, however, be eternal: this hydrogen may move about as long as it is supplied with heat, and there may be no change in it for ever. We cannot suppose it to wear on, to weary in its course, unless we find an analogy in the loss of heat, when it calms down, and by loss of heat finds rest. It is clear, then, that it is not a primitive quality of hydrogen to move for ever as a gas, only if it is set in motion it will move in a certain way. But when the motion ceases as gaseous motion, does it still go on in a liquid or solid? It may be so, but we have no real proof; and at any rate the motion, although of a fundamentally similar kind, cannot be identically the same. Nobody can imagine an atom doing anything but move, and so to keep up its characteristics we are obliged to suppose some action. Let us imagine the contrary; must it not at least have a tendency to action such as we find in attraction? We may imagine this, but I fear it cannot have this inclination without some remaining heat; and if we take all the life—that is, the motion—we know of, and leave nothing to give a tendency till heat comes again, we imagine the motion to exist in the atom itself in one case, and only in the atom when heated up—that is, set in motion—in the other case. There must be a latent capacity in the atom, and that must be represented in some way. Is it in a form or in another motion?

Here we come to a very curious point. When the heat comes to the hydrogen, and motion begins, the heat may be inclined to say, "I only am the power; the hydrogen is nothing; matter is a dead thing, with which I play as I choose." But we may answer the heat thus:—"Cause oxygen to move in exactly the same way, and I will believe you; or cause any other element to do the same." The heat cannot, because each has its own peculiar movement; and so we learn that hydrogen, after all, did play its part in the dance, and played it too with a power that seemed to be eternal. Although it cannot act alone, it can wait for endless ages for a companion with whom to rejoice—that is, take the heat away, freeze it to the utmost, and the particles will cease. At least I assume this; if indeed the existence of solid hydrogen, even for an instant, is not a sufficient proof. If any one says that heat is held too much as a material here, the reply is, "No, it is treated as an external agent." The hydrogen deprived of heat will be quite still so far as we know, and it is interesting to know in what condition it rests. It is not dead, but sleeps; its particles are closely associated, and the colder it is the more firmly do the particles unite—up to a certain point, at least, I for one do not know how far. After all, this is not death; in death there are no bonds of brotherly love. Can any one measure the strength of union of such particles, and find if there is more force expended by this hydrogen when it is active than when it is passive or void of heat? This to some extent we can do, although we cannot do it perfectly till we know the cohesive force, and a few more points, more clearly. But if, by the want of heat, there is this enormous power of compression—or, in other words, of attraction for itself—developed, then it would appear as if the original power of the hydrogen were enormous, and the heat did not increase it, but merely altered its direction. We must, however, remember that there is a difference in the forces; the one is energetic—the gas will move of itself; the solid will not. Yet here we come to a stand—we have abundant force in the dead hydrogen. Can nothing else bring it out but heat? Can nothing cause it to burst forth of itself, and convert its terrible grip into activity? Who knows but the cold may overdo its work, and break up the hydrogen itself, when the earlier elements may spring out and convert that deadly grip into the gay dance of life and a new creation? If any one says that this is only a mode of manufacturing heat out of cold, I will say I know the objection, and I will stop this direction for the present and go elsewhere, assisted by a little imagination.

If we desire to rob hydrogen of more of its qualities we must break it up. Do we not see that the more it unites the more varied is its activity, and the more many-sided is its life? Let us break it into more simplicity, and remove its powers; let us say "Hydrogen become simple, whatever that be,"—either the one matter, the *vera substantia* of all things, or at least something even more ready to escape than hydrogen is in a state of gas. For this we need a new power; we must pass the bounds of the known; we must find a force and split the hydrogen, tearing it asunder by aphairesis, and leaving a something or two somethings which have lost at least one of their powers, that of remaining together until some equal force unites them again. That which they have lost is, let us say first, affinity. If affinity is lost, why not gravitation also, of which it is but a branch? But by what right have you gone beyond the bounds of our matter? It is only a leap, and I shall return; but in my leap I saw that there was a something different from this earth, and the matter there did not gravitate. Indeed! and how can it be matter if it did not gravitate? I am not aware that gravitation is any necessary quality of matter. We can suppose matter without. Indeed there are various kinds of gravitation of one body to another, and much more powerful than that which we designate especially by the name of Gravity. But this I said in a previous essay, in part. A magnet has another gravitation in it, which may go out of it; why may not our common gravitation be put out also? It is a mere fancy that it is necessary. But how did hydrogen obtain its gravitation? How does any body obtain a new property? It is by intrusion of other things, arts, and conditions, as new races are formed; and hydrogen most probably obtained its gravitation either by the combination which first made it hydrogen or in some of the stages. Probably there were more than one stage; by going back we may suppose the one above spoken of, whereby it lost its cohesion in the greatest cold, its tendency to itself; and by another, its tendency to any other matter or its gravity; and thus we have its free parts in the wide Universe—and what are these parts of hydrogen like?

Now, do not wonder that hydrogen should have parts. Do you suppose that such a complex substance—yes, complex, it has many properties—was made at once? Nature makes the complex out of the simple. Hydrogen must be supposed capable of being broken up. The parts are not to be explained by experiment; we have no experience of the chemistry of the lower stage of matter. In the present known hydrogen we have atom striking against atom, and interrupted in its motion; we have also atom combining with atom, and more interrupted; and we have atom gravitating towards atom, seeking a fresh stage of interruption, and a complex social system that cannot be kept up by the simple savage atoms of the early stages of existence. These earlier atoms may have no community of feeling. We suppose the great gravitation of the Universe as nothing to them, and that they live unconscious by their acts of any existence outside of them. Of course some people will say that in any case they cannot pass through our matter: that is quite an assumption. Glass allows something to pass, and why not our more simple bodies? We are not quite sure whether the same does not in some way take place with all matter which is transparent to several agencies.

And now that you have your free and independent matter, what will you do with it? I cannot catch it and curb it; it will not yield to my entreaties, or be kept down by my chains. Hydrogen would blow against me; this will not. Hydrogen would burn, and even sing in doing so; but this will not. And yet I cannot consider it without qualities as to hydrogen itself. The world sought it thousands of years, but could not catch it. By itself it will do nothing but escape from us. After a long time it was found that by combining with oxygen it burnt, and greater firmness was given to both. By analogous means may one element receive new qualities, such as weight and resistance. But we do not suppose that the removal of gravitating power removes all resistance. That is not necessary, as cohesion is much more powerful than gravitation; but we have removed cohesion also from our new elements: they are free.

The difficulty now is to know in what relation this new, or rather primitive substance really stands to matter at all. We might say, in one aspect, as an iron magnet stands to a piece of iron. As a magnet it is powerful, and if nothing but iron were about it we should see greater influence; it lifts iron in spite of gravitation, and a few touches make it dead iron again. The space round the magnet is to iron as if it were solid and resisting. Is it irrational to suppose matter which fills space, and by a reversed relation turns all the space into a solid. It is no more wonderful; in wandering into past or distant nature the eye is unable to see clearly.

But, after all, is this not a mere repetition of Helmholz's idea. It may be like it in some respects, but not in all, and in this it is very different—he seems to understand the condition to which he reduces matter. I do not understand the state in which mine is; strange new conditions arise, and I need not say that I cannot see how his will work. I look only to certain analogies very safe for easy reasoning and pioneering.

Another objection seems to be that if you have brought matter to nothing your reasoning is of no use. I have brought it to nothing, or at least to a something which at first view seems next to it, that it may be seen how little it is without the breath of life put into it. Even the life called resistance is almost gone,—certainly all the life that we call usually material,—but give it combination and it may fall to the sun as hydrogen, and it may rave amongst oxygen to keep up our constant fires. Certainly it is a limited view that gives to our elements only primitive life, and that conceives them to be simple. Of course I use life analogically. Neptune never went to Greenland, so far as we know. Imagine him riding in his car, splashing through the ocean, and finding suddenly that he had got into a prison; the very water is held above him, his horse's hoofs are getting too cold to move, and to his astonishment the whole team become statues, and even he, the king, cannot control this new element, ice. Imagine him coming to Olympus and telling this wonderful tale, whilst Mercury said "You are confined in your movements; indeed I never heard before of your going outside the Pillars of Hercules. I am accustomed to go far, and in my movements among the stars I have come among similar stiffening of far finer matter than water, namely, that which flows in universal space. The messengers of the spheres have moved in the space as unconscious of the existence of anything present as men of the ignorant class are of air, or as fish are of water; and they have been astonished when—as if at a signal—great darts of white have shot through the empyrean, each sending out other darts, until the whole became as solid as the seas of the Hyperboreans." "And why not?" said Vulcan; "most things melt by heat, but some things become lighter by becoming solid, and water, I think, is one of these, as I sometimes have occasion to observe when I seek the cooling snows of Olympus, or its solid ice, warming it to wash the soot off me before I come to supper. I have seen such things without going so far: have I not seen some men rejoicing in the soft winds of heaven, or the hills warmed and moistened by the sun and sea breezes, when suddenly Jove's thunders rolled, and the moisture fell down in heavy hail that struck the frightened people and their flocks, and endangered, if not destroyed, their lives? Why may not a touch of Jove's fire harden the unfathomed space itself as readily as a tender soul, by a change of thought, turns to rage and cruelty? or why may it not become so cold that even that may stiffen as the air itself, or something in it, which makes white the forests below us in a few hours?"

These Olympians were not sound chemists, and forgot that the total vapour was as heavy as the hail to which it turned; but your free matter, by your own account, must be very light, or rather bearing no weight it must be very thin at best. Its weight and thinness are not dependent on each other; its very power of dispersion shows that there is some force in it, and it is not yet reduced to nothing, and you will at least allow that there is plenty to draw from. And yet have I not sat, at a little distance, at a large louvre window, and looked on in the dim evening; the light seemed abundant, and nothing was observed to interrupt my view: the edges of the glass louvre strips were towards me, and after a while I began to see that there were thin lines; suddenly the space was obscured: there were no window-shutters, no gradual clouding. Had my eyes lost their power of seeing? A veil comes gradually: could it be a veil? No, it was a sudden reversal of the louvre. How much may be done by this, and how many analogies it leads to!

I suppose we all allow anything as a temporary fancy, and this I think may be considered, that the weight of a body is not a necessary quality, but entirely external to it. 2. That if so a time may have been when weight came into creation. 3. A condition may still exist of substance having no weight. 4. We may imagine a place to exist where matter has no weight, but this is less easily conceived of common matter. 5. We may imagine weight to be taken out and in; we have an analogy in a magnet lying on the ground, attracting, and in a sense making heavier, the iron over it. 6. That something which gives weight may exist without our matter, although we cannot prove it. 7. If it is outside of our matter it may be an existence of a kind to be passing through our matter. 8. Possibly an existence polarising the vibrations, or rather movements, of molecules, and giving them a direction. 9. If so, it probably has a motion of its own. 10. If so, the parts of which it is composed are necessarily smaller than the atoms of our chemistry: it is as difficult to conceive it non-atomic as to conceive our matter to be so, and we do not render it more intelligible by calling it non-atomic. 11. The state of Physics and Chemistry demands a finer atom than ours, and we can get it best by breaking ours up. 12. If our atoms were as small as these supposed ones they would go through glass, and perhaps through all our common matter, as these finer are supposed to do. 13. If our so-called atoms are complex, they are probably compressible to a certain extent, and changeable in shape. 14. Atoms of more primitive form will be less so, and probably infinitely hard, as we have supposed ours to be, and as ours would be if they were primordial. 15. If there are any which by pressing forward cause common matter to gravitate, they themselves must, if they do not gravitate, have a power of direction. It is very hard to imagine this, although it has been supposed to have been explained. 16. If so, they cannot act merely on the surface, as the weight of a body does not depend on its surface; they may act on every atom, and when they penetrate our matter they can so act, and by this means we obtain a force immeasurably multiplied, *i.e.*, multiplied according to the number of atoms in a body. 17. These ideas connect the finer substance with our coarser matter; it is not fair quite to sever them; they do not, however, convert the finer matter into forces, because even that we can separate from force, and are compelled to do so by our reason.

We must some day think more of this.

VI. THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE British Association held its 1878 meeting at Dublin, in August last. This, the forty-eighth meeting of the Association, and the third visit it has paid to Dublin, was one of the most successful meetings ever held, the number of Members and Associates present being 2756. Mr. Spottiswoode, President, in his Address first made some remarks upon the purposes, operations, and prospects of the Association, of which he has been for many years the Treasurer. He then passed to the consideration of the external aspects and tendencies of Mathematical Science.

Viewed from a mathematician's own point of view, mathematics offer so few points of contact with the ordinary experiences of life or modes of thought, that any account of its actual progress must, he said, fail in the first requisite of an Address—namely, that of being intelligible. But although in its technical character mathematical science suffered the inconveniences, while it enjoyed the dignity, of its Olympian position, still in a less formal garb, or in disguise, it is found present at many an unexpected turn; and although some may never have learnt its special language, not a few have, all through their scientific life, and even in almost every accurate utterance, like Molière's well-known character, been talking mathematics without knowing it. It is, moreover, a fact not to be overlooked that the appearance of isolation, so conspicuous in mathematics, appertains in a greater or less degree to all other sciences, and perhaps also to all pursuits in life. In its highest flight each soars to a distance from its fellows. Each is pursued alone for its own sake, and without reference to its connection with, or its application to, any other subject. The pioneer and the advanced guard are of necessity separated from the main body, and in this respect mathematics does not materially differ from its neighbours. In his preface to the "Principia," Newton gives expression to some general ideas which may well serve as the key-note for all future utterances on the relation of mathematics to natural, including also therein what are commonly called artificial, phenomena:—"The ancients divided mechanics into two parts, rational and practical; and since artizans often work inaccurately, it came to pass that mechanics and geometry were distinguished in this way, that everything accurate was referred to geometry, and everything inaccurate to mechanics. But the inaccuracies appertain to the artizan and not to the art, and geometry itself has its foundation in mechanical practice, and is in fact nothing else than that part of universal mechanics which accurately lays down and demonstrates the art of measuring." He next explains that rational mechanics is the science of motion resulting from forces, and adds,—"The whole difficulty of philosophy seems to me to lie in investigating the forces of nature from the phenomena of motion, and in demonstrating that from these forces other phenomena will ensue." Then, after stating the problems of which he has treated in the work itself, he says—"I would that all other natural phenomena might similarly be deduced from mechanical principles. For many things move me to suspect that everything depends upon certain forces in virtue of which the particles of bodies, through forces not yet understood, are either impelled together so as to cohere in regular figures, or are repelled and recede from one another."

Every subject, whether in its usual acceptation scientific or otherwise, may have a mathematical aspect; as soon, in fact, as it becomes a matter of strict measurement, or of numerical statement, so soon does it enter upon a mathematical phase. It is not so much elaborate calculations or abstruse processes which characterise this phase as the principles of precision, of exactness, and of proportion. These are principles with which no true knowledge can entirely dispense. If it be the general scientific spirit which at the outset moves upon the face of the waters, and out of the unknown depth brings forth light and living forms, it is no less the mathematical spirit which breathes the breath of life into what would otherwise have ever remained mere dry bones of fact, which reunites the scattered limbs and re-creates from them a new and organic whole.

Taking precision and exactness as the characteristics which distinguish the mathematical phase of a subject, we are naturally led to expect that the approach to such a phase will be indicated by increasing application of the principle of measurement, and by the importance which is attached to numerical results. And this very necessary condition for progress may be fairly described as one of the main features of scientific advance in the present day.

If, continued the President, it were my purpose, by descending into the arena of special science, to show how the most various investigations alike tend to issue in measurement, and to that extent to assume a mathematical phase, I should be embarrassed by the abundance of instances which might be adduced. I will therefore confine myself to a passing notice of a very few, selecting those which exemplify not only the general tendency, but also the special character of the measurements now particularly required—viz., that of minuteness, and the indirect method by which alone we can at present hope to approach them. An object having a diameter of an 80,000th of an inch is perhaps the smallest of which the microscope could give any well-defined representation; and it is improbable that one of 120,000th of an inch could be singly discerned with the highest powers at our command. But the solar beams and the electric light reveal to us the presence of bodies far smaller than these. And, in the absence of any means of observing them singly, Professor Tyndall has suggested a scale of these minute objects in terms of the lengths of luminiferous waves. To this he was led, not by any attempt at individual measurement, but by taking account of them in the aggregate, and observing the tints which they scatter laterally when clustered in the form of actinic clouds. The small bodies with which experimental science has recently come into contact are not confined to gaseous molecules, but comprise also complete organisms; and the same philosopher has made a profound study of the momentous influence exerted by these minute organisms in the economy of life. And if, in view of their specific effects, whether deleterious or other, on human life, any qualitative classification, or quantitative estimate, be ever possible, it seems that it must be effected by some such method as that indicated above. Again, to enumerate a few more instances of the measurement of minute quantities, there are the average distances of molecules from one another in various gases and at various pressures; the length of their free path, or range open for their motion without coming into collision; there are movements causing the pressures and differences of pressure under which Mr. Crookes's radiometers execute their wonderful revolutions. There are the excursions of the air while transmitting notes of high pitch, which through the researches of Lord Rayleigh appear to be of a diminutiveness altogether unexpected. There are the molecular actions brought into play in the remarkable experiments of Dr. Kerr, who has succeeded, where even Faraday failed, in effecting a visible rotation of the plane of polarisation of light in its passage through electrified dielectrics, and on its reflection at the surface of a magnet. To take one more instance, there are the infinitesimal ripples of the vibrating plate in Mr. Graham Bell's most marvellous invention. Of the nodes and ventral segments in the plate of the telephone which actually converts sound into electricity and electricity into sound, we can at present form no conception. All that can now be said is that the most perfect specimens of Chladni's sand figures on a vibrating plate, or of Kundt's lycopodium heaps in a musical tube, or even Mr. Sedley Taylor's more delicate vortices in the films of the phoneidoscope, are rough and sketchy compared with these. For notwithstanding the fact that in the movements of the telephone-plate we have actually in our hand the solution of that Old World problem, the construction of a speaking-machine, yet the characters in which that solution is expressed are too small for our powers of decipherment. In movements such as these we seem to lose sight of the distinction, or perhaps we have unconsciously passed the boundary between massive and molecular motion. Through the phonograph we have not only a transformation, but a permanent and tangible record of the mechanism of speech. But the differences upon which articulation (apart from loudness, pitch, and quality) depends appear from the experiments of Fleeming Jenkin, and of others, to be of microscopic size. The microphone affords another instance of the unexpected value of minute variations—in this case of electric currents; and it is remarkable that the gist of the instrument seems to lie in obtaining and perfecting that which electricians have hitherto most scrupulously avoided, viz., loose contact. Once more, Mr. De la Rue has brought forward, as one of the results derived from his stupendous battery of 10,000 cells, strong evidence for supposing that a voltaic discharge, even when apparently continuous, may still be an intermittent phenomenon; but all that is known of the period of such intermittence is, that it must recur at exceedingly short intervals. And in connection with this subject it may be added that, whatever may be the ultimate explanation of the strange stratification which the voltaic discharge undergoes in rarefied gases, it is clear that the alternate disposition of light and darkness must be dependent on some periodic distribution in space or sequence in time which can at present be dealt with only in a very general way. In the exhausted column we have a vehicle for electricity not constant like an ordinary conductor, but itself modified by the passage of the discharge, and perhaps subject to laws differing materially from those which it obeys at atmospheric pressure. It may also be that some of the features accompanying stratification form a magnified image of phenomena belonging to disruptive discharges in general; and that consequently, so far from expecting among the known facts of the latter any clue to an explanation of the former, we must hope ultimately to find in the former an elucidation of what is at present obscure in the latter. A prudent philosopher usually avoids hazarding any forecast of the practical application of a purely scientific research. But it would seem that the configuration of these striæ might some day prove a very delicate means of estimating low pressures, and perhaps also for effecting some electrical measurements. Now, it is a curious fact that almost the only small quantities of which we have as yet any actual measurements are the wave-lengths of light; and that all others, excepting so far as they can be deduced from these, await further determination. In the meantime, when unable to approach those small quantities individually, the method to which we are obliged to have recourse is, as indicated above, that of averages, whereby, disregarding the circumstances of each particular case, we calculate the average size, the average velocity, the average direction, &c., of a large number of instances. But although this method is based upon experience, and leads to results which may be accepted as substantially true; although it may be applicable to any finite interval of time, or even any finite area of space (that is, for all practical purposes of life), there is no evidence to show that it is so when the dimensions of interval or of area are indefinitely diminished. The truth is that the simplicity of Nature which we at present grasp is really the result of infinite complexity; and that below the uniformity there underlies a diversity whose depths we have not yet probed and whose secret places are still beyond our reach.

The President then proceeded to make special remark on some processes peculiar to modern mathematics: he selected for examination three methods, in respect of which mathematicians are often thought to have exceeded all reasonable limits of speculation, and to have adopted for unknown purposes an unknown tongue, with the view of showing that not only in these very cases mathematical science has not outstepped its own legitimate range, but that even art and literature have unconsciously employed methods similar in principle. The three methods in question are—first, that of Imaginary Quantities; secondly, that of Manifold Space;

and thirdly, that of Geometry not according to Euclid. He explained Imaginary Quantities as follows ;—

To fix our ideas, consider the measurement of a line, or the reckoning of time, or the performance of any mathematical operation. A line may be measured in one direction or in the opposite ; time may be reckoned forward or backward ; an operation may be performed or be reversed, it may be done or it may be undone ; and if having once reversed any of these processes we reverse it a second time, we shall find that we have come back to the original direction of measurement or of reckoning, or to the original kind of operation. Suppose, however, that at some stage of a calculation our formulæ indicate an alteration in the mode of measurement such that, if the alteration be repeated, a condition of things not the same as, but the reverse of, the original will be produced. Or suppose that, at a certain stage, our transformations indicate that time is to be reckoned in some manner different from future or past, but still in a way having definite algebraical connection with time which is gone and time which is to come. It is clear that in actual experience there is no process to which such measurements correspond. Time has no meaning except as future or past ; and the present is but the meeting point of the two. Or, once more, suppose that we are gravely told that all circles pass through the same two imaginary points at an infinite distance, and that every line drawn through one of these points is perpendicular to itself. On hearing this statement we shall probably whisper, with a smile or a sigh, that we hope it is not true ; but that in any case it is a long way off, and perhaps, after all, it does not very much signify. If, however, as mathematicians we are not satisfied to dismiss the question on these terms, we ourselves must admit that we have here reached a definite point of issue. Our science must either give a rational account of the dilemma or yield the position as no longer tenable. Special modes of explaining this anomalous state of things have occurred to mathematicians. But, omitting details as unsuited to the present occasion, it will, I think, be sufficient to point out in general terms that a solution of the difficulty is to be found in the fact that the formulæ which give rise to these results are more comprehensive than the signification assigned to them ; and when we pass out of the condition of things first contemplated they cannot (as it is obvious they ought not) give us any results intelligible on that basis. But it does not therefore by any means follow that upon a more enlarged basis the formulæ are incapable of interpretation; on the contrary, the difficulty at which we have arrived indicates that there must be some more comprehensive statement of the problem which will include cases impossible in the more limited, but possible in the wider, view of the subject.

If, both in geometry and in algebra, we occasionally make use of points or of quantities which from our present outlook have no real existence, which can neither be delineated in space of which we have experience, nor measured by scale as we count measurement; if these imaginaries, as they are termed, are called up by legitimate processes of our science; if they serve the purpose not merely of suggesting ideas, but of actually conducting us to practical conclusions; if all this be true in abstract science, I may, perhaps, be allowed to point out, in illustration of my argument, that in art unreal forms are frequently used for suggesting ideas, for conveying a meaning for which no others seem to be suitable or adequate. Are not forms unknown to biology, situations incompatible with gravitation, positions which challenge not merely the stability but even the possibility of equilibrium—are not these the very means to which the artist often has recourse in order to convey his meaning and to fulfil his mission? Again, if we turn from art to letters, truth to nature and to fact is undoubtedly a characteristic of sterling literature; and yet in the delineation of outward nature itself, still more in that of feelings and affections, of the secret parts of character and motives of conduct, it frequently happens that the writer is driven to imagery, to an analogy, or even to a paradox, in order to give utterance to that of which there is no direct counterpart in recognised speech.

Passing to the second of the three methods—viz., that of manifold space—Mr. Spottiswoode remarked that our whole experience of space is in three dimensions, viz., of that which has length, breadth, and thickness; there is, however, another aspect under which even ordinary space presents to us a fourfold, or indeed a manifold, character. In modern physics space is regarded not as a vacuum in which bodies are placed and forces have play, but rather as a plenum with which matter is co-extensive. And from a physical point of view the properties of space are the properties of matter, or of the medium which fills it. Similarly, from a mathematical point of view, space may be regarded as a *locus in quo*, as a plenum, filled with those elements of geometrical magnitude which we take as fundamental. These elements need not always be the same. For different purposes different elements may be chosen; and upon the degree of complexity of the subject of our choice will depend the internal structure or manifoldness of space. Thus, beginning with the simplest case, a point may have any singly infinite multitude of positions in a line, which gives a onefold system of points in a line. The line may revolve in a plane about any one of its points, giving a twofold system of points in a plane; and the plane may revolve about any one of the lines, giving a threefold system of points in space. Suppose, however, that we take a straight line as our element, and conceive space as filled with such lines. This will be the case if we take two planes,—*e.g.*, two parallel planes,—and join every point in one with every point in the other. Now the points in a plane form a twofold system, and it therefore follows that the system of lines is fourfold; in other words, space regarded as a plenum of lines is fourfold. The same result follows from the consideration that the lines in a plane, and the planes through a point, are each twofold. Again, if we take a sphere as our element we can through any point as a centre draw a singly infinite number of spheres, but the number of such centres is triply infinite; hence space as a plenum of spheres is fourfold. And, generally, space as a plenum of surfaces has a manifoldness equal to the number of constants required to determine the surface. Although it would be beyond our present purpose to attempt to pursue the subject further, it should not pass unnoticed that the identity in the fourfold character of space, as derived on the one hand from a system of straight lines, and on the other from a system of spheres, is intimately connected with the principles established by Sophus Lie in his researches on the correlation of these figures. If we take a circle as our element we can around any point in a plane as a centre draw a singly infinite number of circles; but the number of such centres in a plane is doubly infinite; hence the circles in a plane form a threefold system, and as the planes in space form a threefold system it follows that space as a plenum of circles is sixfold. Again, if we take a circle as our element, we may regard it as a section either of a sphere or of a right cone (given except in position) by a plane perpendicular to the axis. In the former case the position of the centre is threefold; the directions of the plane, like that of a pencil of lines perpendicular thereto, twofold: and the radius of the sphere onefold; sixfold in all. In the latter case the position of the vertex is threefold; the direction of the axis twofold; and the distance of the plane of section onefold; sixfold in all, as before. Hence space as a plenum of circles is sixfold. Similarly, if we take a conic as our element, we may regard it as a section of a right cone (given except in position) by a plane. If the nature of the conic be defined, the plane of section will be inclined at a fixed angle to the axis; otherwise it will be free to take any inclination whatever. This being so, the position of the vertex will be threefold; the direction of the axis twofold; the distance of the plane of section from the vertex onefold; and the direction of that plane onefold if the conic be defined, twofold if it be not defined. Hence, space as a plenum of definite conics will be sevenfold, as a plenum of conics in general eightfold. And so on for curves of higher degrees. This is, in fact, the whole story and mystery of manifold space. It is not seriously regarded as a reality in the same sense as ordinary space; it is a mode of representation, or a method which, having served its purpose, vanishes from the scene. Like a rainbow, if we try to grasp it, it eludes our very touch; but, like a rainbow, it arises out of real conditions of known and tangible quantities, and if rightly apprehended it is a true and valuable expression of natural laws, and serves a definite purpose in the science of which it forms part. Again, if we seek a counterpart of this in common life, I might remind you that perspective in drawing is itself a method not altogether dissimilar to that of which I have been speaking; and that the third dimension of space, as represented in a picture, has its origin in the painter's mind, and is due to his skill, but has no real existence upon the canvas which is the groundwork of his art. Or, again, turning to literature, when in legendary tales, or in works of fiction, things past and future are pictured as present, has not the poetic fancy correlated time with the three dimensions of space, and brought all alike to a common focus? Or, once more, when space already filled with material substances is mentally peopled with immaterial beings, may not the imagination be regarded as having added a new element to the capacity of space, a fourth dimension of which there is no evidence in experimental fact?

The third method proposed for special remark is that which has been termed Non-Euclidean Geometry; and the train of reasoning which has led to it may be described in general terms as follows:—Some of the properties of space which on account of their simplicity, theoretical as well as practical, have, in constructing the ordinary system of geometry, been considered as fundamental, are now seen to be particular cases of more general properties. Thus a plane surface and a straight line may be regarded as special instances of surfaces and lines whose curvature is everywhere uniform or constant. And it is perhaps not difficult to see that, when the special notions of flatness and straightness are abandoned, many properties of geometrical figures which we are in the habit of regarding as fundamental will undergo profound modification. Thus a plane may be considered as a special case of the sphere,—viz., the limit to which a sphere approaches when its radius is increased without limit. But even this consideration trenches upon an elementary proposition relating to one of the simplest of geometrical figures. In plane triangles the interior angles are together equal to two right angles; but in triangles traced on the surface of a sphere this proposition does not hold good. To this other instances might be added. The principle of representing space of one kind by that of another, and figures belonging to one by their analogues in the other, is not only recognised as legitimate in pure mathematics, but has long ago found its application in cartography. In maps or charts, geographical positions, the contour of coasts, and other features, belonging in reality to the earth's surface, are represented on the flat; and to each mode of representation, or projection as it is called, there corresponds a special correlation between the spheroid and the plane. To this might perhaps be added the method of descriptive geometry, and all similar processes in use by engineers, both military and civil.

With regard to pure and applied mechanics, it has often, said Mr. Spottiswoode, been asked whether modern research in the field of pure mathematics has not so completely outstripped its physical applications as to be practically useless; whether the analyst and the geometer might not now, and for a long time to come, fairly say, "Hic artem remumque repono," and turn his attention to mechanics and physics. That the Pure has outstripped the Applied is largely true; but that the former is on that account useless is far from true. Its utility often crops up at unexpected points; witness the aids to classification of physical quantities, furnished by the ideas (of Scalar and Vector) involved in the Calculus of Quaternions; or the advantages which have accrued to physical astronomy from Lagrange's Equations, and from Hamilton's Principle of Varying Action; on the value of Complex Quantities, and the properties of general Integrals, and of general theorems on integration for the Theories of Electricity and Magnetism.

These extensions of mathematical ideas would, however, be overwhelming if they were not compensated by some simplifications in the processes actually employed. Of these aids to calculation I will mention only two, viz., symmetry of form and mechanical appliances; or, say, Mathematics as a Fine Art, and Mathematics as a Handicraft. And first, as to symmetry of form. There are many passages of algebra in which long processes of calculation at the outset seem unavoidable. Results are often obtained in the first instance through a tangled mass of formulæ. But almost within our own generation a new method has been devised to clear this entanglement. By Lagrange, and to some extent also by Gauss, among the older writers, the method of which I am speaking was recognised as a principle; but beside these perhaps no others can be named until a period within our own recollection. The method consists in symmetry of expression. In algebraical formulæ combinations of the quantities entering therein occur and recur; and by a suitable choice of these quantities the various combinations may be rendered symmetrical and reduced to a few well-known types. This having been done, and one such combination having been calculated, the remainder, together with many of their results, can often be written down at once, without further calculations, by simple permutations of the letters. Symmetrical expressions, moreover, save as much time and trouble in reading as in writing.

With regard to mechanical appliances, Mr. Babbage, when speaking of the difficulty of ensuring accuracy in the long numerical calculations of theoretical astronomy, remarked that the science which in itself is the most accurate and certain of all, had, through these difficulties, become inaccurate and uncertain in some of its results. And it was doubtless some such consideration as this, coupled with his dislike of employing skilled labour where unskilled would suffice, which led him to the invention of his calculating machines. Prof. James Thomson has recently constructed a machine which, by means of the mere friction of a disk, a cylinder, and a ball, is capable of effecting a variety of complicated calculations, which occur in the highest application of mathematics to physical problems. By its aid it seems that an unskilled labourer may, in a given time, perform the work of ten skilled arithmeticians. The machine is applicable alike to the calculation of tidal, of magnetic, of meteorological, and perhaps also of all other periodic phenomena. It will solve differential equations of the second, and perhaps of even higher orders. And through the same invention the problem of finding the free motion of any number of mutually attracting particles, unrestricted by any of the approximate suppositions required in the treatment of the lunar and planetary theories, is reduced to the simple process of turning a handle. When Faraday had completed the experimental part of a physical problem, and desired that it should thenceforward be treated mathematically, he used irreverently to say—"Hand it over to the calculators." But truth is ever stranger than fiction; and if he had lived until our day he might, with perfect propriety, have said—"Hand it over to the machine."

After referring to the origin of mathematical ideas and their expression, the President passed to the attitude of Literature and Art towards Science; and here he remarked that, considering the severance which still subsists in education and during our early years between Literature and Science, we can hardly wonder if when thrown together in the afterwork of life they should meet as strangers, or if the severe garb, the curious implements, and the strange wares of the latter should seem little attractive when contrasted with the light companionship of the former. The day is yet young, and in the early dawn many things look weird and fantastic which in fuller light prove to be familiar and useful. The outcomings of Science, which at one time have been deemed to be but stumbling-blocks scattered in the way, may ultimately prove stepping-stones which have been carefully laid to form a pathway over difficult places for the children of "sweetness and of light." The instances on which we have dwelt are only a few out of many in which mathematics may be found ruling and governing a variety of subjects. It is as the supreme result of all experience, the framework in which all the varied manifestations of Nature have been set, that our Science has laid claim to be the arbiter of all knowledge. She does not indeed contribute elements of fact, which must be sought elsewhere; but she sifts and regulates them; she proclaims the laws to which they must conform if those elements are to issue in precise results. From the data of a problem she can infallibly extract all possible consequences, whether they be those first sought or others not anticipated; but she can introduce nothing which was not latent in the original statement. Mathematics cannot tell us whether there be or be not limits to time or space; but to her they are both of indefinite extent, and this in a sense which neither affirms nor denies that they are either infinite or finite. Mathematics cannot tell us whether matter be continuous or discrete in its structure; but to her it is indifferent whether it be one or the other, and her conclusions are independent of either particular hypothesis. Mathematics can tell us nothing of the origin of matter, of its creation or its annihilation; she deals only with it in a state of existence; but within that state its modes of existence may vary from our most elementary conception to our most complex experience. Mathematics can tell us nothing beyond the problems which she specifically undertakes; she will carry them to their limit, but there she stops, and upon the great region beyond she is imperturbably silent.

Conterminous with space and coeval with time is the kingdom of mathematics; within this range her dominion is supreme; otherwise than according to her order nothing can exist; in contradiction to her laws nothing takes place. On her mysterious scroll is to be found, written for those who can read it, that which has been, that which is, and that which is to come. Everything material which is the subject of knowledge has number, order, or position; and these are her first outlines for a sketch of the Universe. If our more feeble hands cannot follow out the details, still her part has been drawn with an unerring pen, and her work cannot be gainsaid. So wide is the range of mathematical science, so indefinitely may it extend beyond our actual powers of manipulation, that at some moments we are inclined to fall down with even more than reverence before her majestic presence. But so strictly limited are her promises and powers, about so much that we might wish to know does she offer no information whatever, that at other moments we are fain to call her results but a vain thing, and to reject them as a stone when we had asked for bread. If one aspect of the subject encourages our hopes, so does the other tend to chasten our desires; and he is perhaps the wisest, and in the long run the happiest among his fellows, who has learnt not only this science, but also the larger lesson which it indirectly teaches—namely, to temper our aspirations to that which is possible, to moderate our desires to that which is attainable, to restrict our hopes to that of which accomplishment, if not immediately practicable, is at least distinctly within the range of conception. That which is at present beyond our ken may, at some period and in some manner as yet unknown to us, fall within our grasp; but our Science teaches us, while ever yearning with Goethe for "Light, more light," to concentrate our attention upon that of which our powers are capable, and contentedly to leave for future experience the solution of problems to which we can at present say neither yea nor nay. It is within the region thus indicated that knowledge in the true sense of the word is to be sought. Other modes of influence there are in society and in individual life, other forms of energy beside that of intellect. There is the potential energy of sympathy, the actual energy of work; there are the vicissitudes of life, the diversity of circumstance, health, and disease, and all the perplexing issues, whether for good or for evil, of impulse and of passion. But although the book of life cannot at present be read by the light of Science alone, nor the wayfarers be satisfied by the few loaves of knowledge now in our hands, yet it would be difficult to overstate the almost miraculous increase which may be produced by a liberal distribution of what we already have, and by a restriction of our cravings within the limits of possibility. In proportion as method is better than impulse, deliberate purpose than erratic action, the clear glow of sunshine than irregular reflection, and definite utterances than an uncertain sound,—in proportion as knowledge is better than surmise, proof than opinion,—in that proportion will the mathematician value a discrimination between the certain and the uncertain, and a just estimate of the issues which depend upon one motive power or the other. While on the one hand he accords to his neighbours full liberty to regard the unknown in whatever way they are led by the noblest powers that they possess, so on the other he claims an equal right to draw a clear line of demarcation between that which is a matter of knowledge and that which is at all events something else, and to treat the one category as fairly claiming our assent, the other as open to further evidence. And yet, when he sees around him those whose aspirations are so fair, whose impulses so strong, whose receptive faculties so sensitive, as to give objective reality to what is often but a reflex from themselves, or a projected image of their own experience, he will be willing to admit that there are influences which he cannot as yet either fathom or measure, but whose operation he must recognise among the facts of our existence.

MATHEMATICAL AND PHYSICAL SCIENCE. (Section A.)

In the absence of the President (the Rev. Professor Salmon) the Rev. Professor Haughton opened the business of this Section.

Prof. J. D. Everett read the "Report of the Committee on Underground Temperatures." The principal novelty was the proposal to make observations in filled-up bores by a thermo-electric method. Two wires, one of iron and the other of copper, each covered with gutta-percha, were to be joined at both ends, where a portion would be left uncovered. One junction would be buried in the bore, while the other would remain above ground, available for observation. A current would flow through the circuit composed of these two wires whenever the two junctions were at unequal temperatures, and the observer would immerse the accessible junction in a basin of water containing a thermometer, and would regulate the temperature of the water until he found by a galvanometer that no current passed. He would then know that the temperature of the water, as indicated by the thermometer, was the same as that of the buried junction.

The "Report of the Committee for the Determination of the Mechanical Equivalent of Heat" was then read. The Committee are now making a more accurate investigation of the true position of the freezing- and boiling-points of the thermometer when cleared from the effects of the imperfect elasticity of the glass of which they are constructed.

Papers were read by Mr. J. E. H. Gordon, "On some Experiments on Specific Inductive Capacity," and "On the Effect of Variations of Pressure on the Length of Disruptive Discharge in Air."

Mr. G. Johnstone Stoney communicated the results of long investigation, by himself and Prof. J. Emerson Reynolds, "On the Spectrum of Chloro-chromic Anhydride." He described the kinetic theory of gas molecules darting about and continually striking against each other; but, besides these, he said there were internal motions within the molecules, which in many cases were either periodic or quasi-periodic. The evidence of this was obtained from the spectra of gases. For many years he had been engaged in searching for cases of harmonic motion in gas, and, with the assistance of Prof. Reynolds, he had obtained the positions of 105 lines in the spectrum of chloro-chromic anhydride, which proved to be harmonics of one particular motion. The time of one of the oscillations had been measured to the 810,000,000,000th part of a second.

Prof. James Thomson next read a paper "On the Flow of Water in Uniform Régime in Rivers and in Open Channels generally." He described the various theories which had previously been put forward to account for the well-known fact that the flow of water is not always greatest at the surface, and advanced a new theory, which he illustrated by various diagrams.

This was followed by a paper "On the Pedetic Action of Soap," by Prof. W. Stanley Jevons, which we give *in extenso*:—

Since the publication, in the "Quarterly Journal of Science" for April, 1878, of my paper on Pedesis, or the so-called Brownian movement of microscopic particles, it has been suggested to me that soap would form a good critical substance for experiment in relation to this phenomenon. It is the opinion of Prof. Barrett, and some other physicists, that the movement is due to surface tension, whereas I believe that chemical and electromotive actions can alone explain the long-continued and extraordinary motions exhibited by minute particles of almost all substances under proper conditions. Soap considerably reduces the tension of water in which it is dissolved, without much affecting (as is said) its electric conductibility. If, then, pedesis be due to surface-tension, we should expect the motion to be killed or much lessened when soap is added to water.

Having tried the experiment, I find that the result is of the opposite character to what Prof. Barrett anticipated. With a solution of common soap the pedetic motion becomes considerably more marked than before. I have observed this result not only with china clay and some other silicates, but also with such comparatively inert substances as the red oxide of iron, chalk, and even the heavy powder of barium carbonate. The last-named substance—one of those we should least expect to dance about of its own accord—gave a beautiful exhibition of the movement when mixed with a solution of about 1 per cent of soap, and viewed with a magnifying power of 500 or 1000 diameters.

The correctness of this result was also tested by observing the suspending power of solutions of soap-solution compared with water. If a little china clay be diffused through common impure water—that, for instance, of the London Water Companies—the greater part of the clay will soon be seen to collect together in small flocks, and fall to the bottom in two or three hours, the water being almost clear. However, if a little soap be dissolved in the water, the behaviour of the clay is quite different. The larger particles soon subside, but the smaller ones remain diffused through the liquid for a long time, giving it a milky appearance, quite different from the flocky and grainy appearance of the common water: if 1 per cent of sodium carbonate be dissolved in common water, and china clay be mixed therewith, the subsidence of the clay is still more rapid, owing, as I have explained, to the increase in the electric conductivity of the fluid, and the consequent decrease of pedesis. But I now find that if soap be added at the same time pedesis is not destroyed, but considerably increased, and the clay remains a long time in suspension, two or three days at least.

These facts give a complete explanation of the detergent power of soap. It has long seemed to me unaccountable that for cleansing purposes the comparatively neutral soap should be better than the alkaline carbonate by itself. We are told that the alkali is but feebly combined with the stearic or other fatty acids. But why combine it at all if we need only the alkaline power of the base? The fact is that the detergent action of soap is due to pedesis, by which minute particles are loosened and diffused through the water, so as to be readily carried off. Pure rain or distilled water has a high cleansing power, because it produces pedesis in a high degree. The hardness of impure water arises from the vast decrease of pedesis due to the salts in solution. Hence the inferior cleansing power of such water. If alkaline salts be dissolved in the water, it becomes capable of acting upon oleaginous matter, but the pedetic power is lessened, not increased. But if soap be added also, we have the advantage both of the alkaline dissolving power and of the pedetic cleansing power. At the same time we have a clear explanation why silicate of soda is now largely used in making soap; for I have shown, in the paper referred to, that silicated soda is one of the few mineral substances which increase the pedetic and suspensive power of water.

I believe that the detergent power of soap and water is one of the many important phenomena which may be explained by the study of pedesis, and I propose to follow up the investigation of this movement in regard to the several substances which tend to increase it.

Mr. Robert Sabine read a paper "On Motions produced by Dilute Acids on some Amalgam Surfaces." These motions result from an alternate play of deoxidation of the mercury underneath the acid by electrolysis, due to the currents of small floating particles of the positive metal causing the drop to contract, and of oxidation of the surface outside the acid drop causing it to re-expand.

Mr. J. R. Wigham read a paper on "New Applications of Gas for Lighthouses." The first part of his paper referred to the Quadriform Gaslight. Galley Head is a promontory on the coast of Cork, in the neighbourhood of which there had been several shipwrecks. The Commissioners of Irish Lights therefore determined to place upon it the most distinctive and powerful light which they could obtain. With this view, and acting upon the advice of Dr. Tyndall, they adopted the Quadriform Group Flashing Gas Light. The power of the burner was obtained by a peculiar arrangement of numerous fishtail jets, and by suspending over the flame an oxidiser of talc, or some other material, by means of which the current of air was brought in contact with the most smoky part of the flame, rendering it not only smokeless, but exceedingly white. The oxygen of the air was twice availed of; first through the bottom of the flame, and secondly at the top, where its action raised to a white-heat a large quantity of solid carbon found there. The burner requires no chimney-glass. It was so constructed that the lightkeeper could increase the power of the light by five steps, accordingly as the state of the weather might seem to require. In clear weather 28 jets were used, and the number might be enlarged to 48, 68, 88, or 108. The changes from one to another could be very quickly effected by the use of mercurial joints. [The burner which was exhibited on the table was then lighted, and the five different degrees of illuminating power were illustrated, the effect of such an immense volume of flame being most startling in such a confined space.] It was well known that some "fogs" are so dense that even the rays of the sun could but feebly penetrate through them; but when the weather was merely what was called "thick," then it was that the mariner derived great benefit from such differentiating lights. The power of the burner would, of course, be very much increased by the use of lenticular apparatus, a specimen of which was before the meeting. [It may be here parenthetically remarked that early in the day some amusement was caused by the fact that the rays of the sun shining through the lens, which faced the window, set the green baize table-cover smoking, and it had to be placed under cover.] If instead of using this ordinary lighthouse lens, which was designed and calculated to transmit the light of an oil-lamp 4 inches in diameter and 3½ inches high, a lens made to suit his large gas-flame, which was 12 inches in diameter and

8½ inches high, was used, of course the illuminating power would be enormously increased. He trusted that before long lighthouse authorities would sanction the construction of such lenses, and so gain the full benefit of the large flames which gas only could produce. Being fully convinced of the importance of using large lights in illuminating fogs, he determined to use lenses to the utmost limit which was now permitted, and he designed a means by which he was able to quadruple the power of the largest gas-light. This plan consisted in placing two and three and four burners vertically over one another, and making an arrangement by which the products of the combustion of the lower burners were turned outwards, so as not to interfere with the upper burners, while the supply of fresh air was given through cylindrical openings, having no contact with the flues. The illuminating power was thus materially increased. The quadriform apparatus erected at Galley Head contains 32 lenses. As the lenses touch each other the rays of light blend at a short distance, and throw out one bright light. It is calculated that its illuminating power coupled with this large gas-burner was about equal to one million sperm candles. The light at Galley Head is not only quadriform, but is also a group-flashing light—that is, the flashes from the lenses, instead of being simple flashes, were each of them, by the continual extinction and re-ignition of the gas, broken up into four or five beams, which constituted a group of flashes, recurring at regular intervals. This is accomplished by the same clockwork machinery as that by which the lenses were caused to revolve. The interval between the groups and flashes is one minute, and the interval between the flashes two seconds, during which the gas is shut off, and thus there is a great economy of gas.

The second portion of the paper described the combined gas and electric light for lighthouses. For use during fogs Mr. Wigham added an intense light to his large gas-burner, which consists of what he terms a core for his burners. He preferred the electric light because of its greater intensity and the facility with which it could be applied.

The third portion of the paper dealt with a mode of lighting sea beacons from a position on shore. When it is desired to maintain lights on beacons to which access by boats is difficult or expensive, Mr. Wigham advises the application of gas properly dried by chloride of calcium as the means of illumination. The gas station on shore may command any number of beacons, which may be simultaneously lighted. During the daytime the gas was supplied at a pressure equal to a column of water 6 inches high, to maintain a small jet in the lantern on each beacon. The high pressure prevented the jet from being blown out by the wind, and the arrangement which he had devised enabled the operator to turn on the flame or diminish it to a small jet as easily as the same result could be accomplished in the case of an ordinary light in a dwelling-house, but by using exactly opposite means, *i.e.*, the gas was turned *off* to be lighted and turned *on* to be extinguished.

Mr. Wigham also read a short paper on "Fog Signals." He proposes to use the gas available in lighthouses for gas guns. The report is caused by the explosion of the mixture of oxygen and coal-gas. The gun may be fixed on a rock in the sea at a considerable distance from a lighthouse or a fog-signal station, and will be lighted and fired as often as required from the station, without the keepers leaving their post.

A third paper was read by Mr. Wigham "On a New Atmospheric Gas Machine," which the inventor claimed was admirably adapted for places where coal-gas is difficult of application, as in country houses at a considerable distance from towns. When two burners of the same size were lighted, the one with ordinary gas and the other with atmospheric gas, it was difficult to tell which was the brighter or clearer. Mr. Wigham said it required neither fire nor retorts nor gas-holders; no special knowledge was necessary on the part of the person who attended to it, and the products of its combustion were as harmless as those evolved from the purest wax. It would not tarnish silver or injure the decorations of any furniture.

Mr. G. J. Stoney read a "Report on the Oscillation-Frequencies of the Rays of the Solar Spectrum." The report shows that so long as light is propagated through a vacuum the undulator, however complex, maintains its form unaltered at all distances from the source of light; for in vacuous spaces waves of different periods advance at the same rate and directly forward, and therefore the simple component undulations which are represented by the several terms of a sinusoid series accurately accompany each other throughout their whole journey. But the event is different if the light encountered an optical agent which acted differently on waves of different periods. Of this kind were the prisms and diffraction-gratings of our spectroscopes. Waves of different periods are compelled to travel in different directions, and thus the several terms of the sinusoid series appear under the form of lines in the spectrum. The wave-lengths corresponding to each position in the spectrum had been determined with great care, and these, when corrected for the dispersion of the air, were proportional to the corresponding periodic times, which thus become known. By a discussion of the observations it might be expected that much will be learned with regard to the original disturbance caused by the source of light. In the present state of science it is of importance to facilitate this inquiry as much as possible, and it is hoped that aid will be given to the student of Nature by the table published in connection with the Report, in which the oscillation-frequencies of the principal rays of the visible part of the solar spectrum have been computed from Angstrom's determinations of their wave-lengths in air, combined with Ketteler's observations on the despersion of air.

Mr. Stoney also read three other papers:—"On a Spectroscope of Unusually Large Aperture," "On the Support of Spheroidal Drops and Allied Phenomena," and "On the Cause of the Travelling of Spheroidal Drops."

Captain Douglas Galton then read a paper "On some Recent Experiments upon the Coefficient of Friction between Surfaces Moving at High Velocities." The author has recently been engaged in making some experiments in connection with the action of brakes in use on railways. A number of diagrams and tables illustrated the means by which the results were obtained, and the principal results shown by them may be summed up as follows:—(1.) The application of brakes to the wheels, when skidding is not produced, does not appear to retard the rapidity of rotation of the wheels. (2.) When the rotation of the wheels falls below that due to the speed at which the train is moving, skidding appears to follow immediately. (3.) The resistance which results from the application of brakes without skidding is greater than that caused by skidded wheels. (4.) During the movement of skidding the retarding force increases to an amount beyond that which prevailed before the skidding took place; but immediately after the act of skidding is complete, this pressure falls down again to much below what it was before the skidding. (5.) The pressure required to skid the wheels is much higher than that required to hold them skidded, and appears to bear a relation to the weight on the wheels themselves, as well as to their adhesion and velocity. The general conclusion which would appear to follow from the results of the preliminary experiments is that none of the hand brakes, and only some of the continuous brakes now in use, have been designed with a clear knowledge of the most essential conditions required in a perfect brake.

The Rev. Robert Harley, F.R.S., read a paper "On the Stanhope 'Demonstrator,' or Logical Machine." The author stated that towards the close of the last century a logical instrument was constructed by Charles, third Earl Stanhope. The present Earl found the instrument and some fragmentary papers on Logic among the relics of his scientific ancestors, and, at the suggestion of Mr. Spottiswoode, placed them in the hands of Mr. Harley, who has made a careful study of them. Earl Stanhope (born 1753, died 1815) is known to Science chiefly by his printing-press, microscopic lens, arithmetical machine, monochord, and steamboat; but of his logical speculations, which occupied his thoughts for thirty years, and of his curious contrivance for working logical problems, called by him the "Demonstrator," nothing has been known. Mr. Harley noticed that Stanhope anticipated George Bentham, Sir William Hamilton, George Boole, and others, in his quantification of the predicate, and notably De Morgan's rule for the numerically definite syllogism. Stanhope stated the rule as applicable to all syllogistic reasoning, and he constructed his "Demonstrator" for the mechanical working of the rule. It does not seem equal to very difficult and complicated questions, nor nearly so powerful as Prof. Jevons's logical machine, which before the discovery of the "Demonstrator" was supposed to be the first invention in this direction.

Dr. Janssen described his method of Solar Photography, and read a paper "On Total Eclipses."

Mr. E. J. Hardman read a paper "On Lead and Platinised Lead as a Substitute for Carbon and Platinised Silver in Leclanché, Bichromate, and Smee's Batteries."

Mr. W. J. Millar described a new Receiver for Microphone.

Mr. Mattieu Williams read a paper "On an Experimental Verification of the Velocity of Transmission of Radiant Heat."

The "Report of the Committee on Atmospheric Electricity" was read by Prof. Forbes. Three electrometers have been given: one to Surgeon-Major Johnson, in India; the second to Mr. Michie Smith, in India; and the third to Dr. Grabham, in Madeira. Surgeon-Major Johnson was engaged in the frontier war in India; and Dr. Grabham has hitherto been too much occupied to make observations; while Mr. Michie Smith has not yet had time to furnish any;

so that up to the present time no observations have been received.

Mr. W. Ladd read a paper "On Edmunds's Phonoscope." This instrument is for producing figures of light from vibrations of sound. It consists essentially of three parts—an induction coil, an interrupter, and a rotary vacuum tube. The action of the instrument is as follows:—Sounds from the voice or other sources produce vibrations on the diaphragm of the interrupter, which, being in the primary circuit of the induction coil, induce at each interruption a current in the secondary coil, similar to the action of a contact-breaker or rheotome; therefore each vibration is made visible as a flash in the vacuum tube. The tube revolving all the time at a constant speed, the flashes produce a symmetrical figure like the spokes of a wheel, as in the Gassiot star. The number of spokes or radii is according to the number of vibrations in the interrupter during a revolution of the tube, and the number of vibrations being varied to any extent according to the sounds produced, the figures in the revolving tube will be varied accordingly. The same sounds always produce the same figures, provided the revolutions be constant. In case of rhythmical interruption being produced in a given sound, as in a trill, most beautiful effects are noticeable, owing to the omission of certain radii in regular positions in the figure. The uses of this instrument are the rendering visible of sounds, and showing the vibrations required in their production, and it forms a mode of confirming by sight an appeal to the ear.

The next paper was by the same author, "On Byrne's Compound Plate Battery," described in the last number of the "Quarterly Journal of Science."

Prof. G. Forbes described "A Clock with a Detached Train." In the course of some experiments still progressing he used a clock to give electric signals every second. It has a train of only one wheel working in a pinion. It is driven by a weight which falls through 5 feet in an hour. This clock only goes one hour, but serves the purpose for which it was made. He wished lately, however, to make it go for a longer time, so he drove it by a weight hung by a pulley on an endless chain in the usual way, and he attached a common five shilling Swiss alarm clock to the chain to wind it up continuously. This answers so well that he would suggest a similar construction as not only the cheapest, but also the best form of a clock with escapement. It consists of a pendulum and escapement with no train whatever, with an endless chain or thread passing over a pulley on the axles of the scape-wheel, and over the minute wheel of a secondary clock, hanging between them in a festoon which supports the weight by a pulley. The secondary clock gives the hours and minutes, and the clock without train shows the seconds. We thus have a clock without the errors introduced by a train. It is a gravity escapement without the locking friction.

Prof. G. Forbes also described "An Instrument for Determining the Quantity of Fire-Damp in Mines," which consists of a resonator of various dimensions, and a tuning-fork of definite pitch. The resonator is a metal tube 1 inch in diameter and 15 inches long, in which a piston slides so as to regulate the length of the tube. This tube is fixed in a block of wood, to which is attached a tuning-fork, whose points are just above the open end of the tube. The tuning-fork is sounded in every convenient way, and the piston is moved out and in until the proper length is found, which is indicated by the resonator intensifying the sound of the tuning-fork. With practice the length can thus be determined with an accuracy of at least 1 in 250. But the length depends on the density of the gas, a light gas requiring a longer resonator, and, by reading off on a scale the position of the piston, a person can judge of its density. In this manner 1 or 2 per cent of fire-damp, mixed with common air, can be detected. Barometric pressure produces no difference on the instrument. The temperature correction is made by reading off a thermometer of the proper dimensions, instead of reading off a fixed mark on the piston.

Prof. Silvanus P. Thompson narrated several instances of rainbows, chiefly seen in Switzerland, when radial streaks of light, devoid of colour, were observed within the primary bow, and without the secondary bow.

Mr. C. Meldrum read a paper "On Sun-Spots and Rainfall." The conclusion at which the author arrived, from a great number of observations in all parts of the world, was that the maximum and minimum rainfall apparently coincided with the maximum and minimum sun-spots respectively.

Mr. W. Morris read a paper "On the Temperature of the Earth Within." He held that the present method of determining underground temperatures with the thermometer as used in air or water was quite unsatisfactory. He suggested the employment of pairs of chronometers, one of each pair to be placed at the bottom of a bore, and the other at the surface.

Mr. James Glaisher read the "Report of the Committee on Luminous Meteors." It contained an account of meteors doubly observed, with a table showing their real paths, velocities, and radiant points; a detailed account of large meteors; general directions to observers for recording meteors and aërolites; the discussion of a meteor of short period, and an analysis of the constituents of masses of meteoric iron and stone-falls.

Lord Rosse gave a short explanation of the peculiarities of an equatorial mounting recently erected at Parsonstown, showing the points in which it differed from the ordinary type of mounting. The optical arrangements were exactly the same as in ordinary cases. The old mounting at Parsonstown, being made of wood, fell into decay, and he resolved to replace it by metal, and mount the instrument equatorially. The leading peculiarities of the mounting were, that the points of reversal were situated at the east and west, instead of at the north and south. The bearings on which the instrument turned in right ascension were smaller than in the ordinary mountings. The motions in declination and in right ascension were effected by means of screws, so that on a windy night the instrument could not run away with the observer. The tube was square. The clock was connected by means of a stretched strap of brass, and the gallery was quite unique. The counterpoise was less than usual. The only reflector of a similar size mounted equatorially was that constructed by Mr. Grubb for the Melbourne Government; but its drawback was that just as the observer had moved it to the very best position for observation—namely, the south—it had to be reversed and every connection altered. The gallery was also worked by means of screws, so that there was no danger of the observer coming down quicker than he wished. The tube was square in section. The cage was independent of the mounting, moving on a circular rail, and with a second motion like that of a derrick crane. The mounting was only suitable for a reflecting telescope.

Prof. R. S. Ball read a paper "On the Annual Researches made at Dunsink on Parallax of Stars." Before commencing the observations described and tabulated in the paper a working list was formed, containing red stars, variable stars, stars with large proper motions, and several other stars which were chosen on different grounds. The observations had the special object of seeing whether any of them had a large parallax. Forty-two different objects had been selected from this working list, but in almost every case the observations convinced him that the parallax was constantly less than one second, and most probably did not exceed half a second. It would therefore be understood that the results were purely negative so far as the immediate object in view was concerned, as they did not suggest the existence of any parallax worth following up. The principle upon which the reconnoitring observations were conducted was this—the effect of annual parallax upon a star was to make the apparent place of the star describe a minute ellipse, of which the mean place of the star occupied the centre. The star was observed twice. At the first observation the star was at or near one of the extremities of the major axis of the ellipse; at the second observation it was at the other extremity—so that the observations were so arranged that in each case parallax would have the greatest effect it was capable of producing.

Prof. H. Hennessy read a paper "On the Climate of the British Islands." When he first made his investigations on this subject he was led to the conclusion that the distribution would be represented by isothermal lines having a certain parallelism to the coast line of these islands. These isothermal lines had been laid down from actual observation, because he had found that the law of increase and decrease of temperature, in going inland over a table land or flat country, was so extremely slow that it was perfectly absurd to use the co-efficient of one degree to 300 feet, which had been obtained by balloons. The actual results confirmed in the minutest particulars the theory of isothermal lines which he propounded years ago, and he believed that the more observations were multiplied not only in these islands, but in New Zealand, Tasmania, and similar places, the more would it be found that his theory was correct. The islands, however, must have their coasts bathed by oceanic currents of a high temperature. The isothermal lines for Ireland showed that the distribution of temperature was more influenced by the sea than by latitude.

Other papers read were "On a Diagonal Eyepiece, required in certain Optical Experiments," by Prof. G. Forbes; "On New Magnetic Figures," by Dr. S. P. Thompson; "On Dimensional Equations," by Prof. James Thomson; "On a New Form of Electro-Registering Apparatus," and "On an Isochronous Pendulum," by Mr. Denny Lane; "On the Variability of Standard of Height," by Mr. J. E. Hilgard; and "On Lightning Conductors," by Mr. R. Anderson.

The Committee's "Report on Babbage's Analytical Engine" was read in the department of Mathematics. After referring to the general principles of calculating engines, and the special characteristics and capabilities of Mr. Babbage's, the Report dealt with the advisability of constructing an analytical machine. On the question of cost the Committee said—"It has not been possible for us to form any exact conclusion as to the cost. Nevertheless there are some data in existence which appear to fix a lower limit to the cost. Mr. Babbage, in his published papers, talks of having 1000 columns of wheels, each containing fifty distinct wheels: this apparently refers to his store. Besides the many thousand moulded pewter wheels for these, and the axes on which they are mounted, there is the mill, also consisting of a series of columns of wheels and of a vast machinery of cams, clutches, and cranks, for their control and connection, so as to bring them within the directing power of the Jacquard systems of variable cards and operation cards. Without attempting any exact estimate, we may say that it would surprise us very much if it were found possible to obtain tenders for less than £10,000, while it would pretty certainly cost a considerable sum to put the design in a fit state for obtaining tenders. On the other hand, it would not surprise us if the cost were to reach three or four times the amount above suggested. It is understood that towards the close of his life Mr. Babbage had contemplated carrying out the manufacture of the engine on a smaller scale, confining himself to twenty-five figures instead of fifty, and to two hundred columns instead of a thousand or more. This would, of course, reduce the expense of the metal work proportionately, but we do not think that it would materially reduce the charge which we anticipate for bringing the design into working order." The conclusion at which the Committee arrived was that they could not advise the British Association to take any steps to procure the construction of Mr. Babbage's analytical engine.

In the department of Physical Science Mr. J. T. Bottomley read the "Report of the Committee for commencing Secular Experiments on the Elasticity of Wires." The arrangements for suspending the wires are complete, and two wires, one of palladium and the other of platinum, have been suspended in their places.

Prof. Adams exhibited and described a new form of polariscope which enabled measurements to be made of crystals to the diameter of rings, and also the angles between the optic axes.

A telegram and letter from Prof. W. C. Ayrton, of Japan, to Sir Wm. Thomson, "On a New Determination of the Number of Electro-static Units in the Electro-magnetic Unit," were read. The letter gave an account of very interesting and delicate experiments, which resulted in an answer of 298,000,000 metres per second, corresponding with the velocity of light as given by Foucault.

Mr. R. J. Moss gave a description of an instrument of research for the investigation of Crookes's stress and experiments on spheroidal drops.

Prof. Barrett described a new form of trap-door electrometer.

Mr. J. E. Gordon described some experiments on specific inductive capacity. His observations were illustrated by formulæ and coloured diagrams, without which they cannot be well understood.

At the request of the Chairman, Prof. W. E. Ayrton, who had just arrived from Japan, said—The value of Mr. Gordon's experiments arises from his belief that the specific inductive capacity of a dialectric rapidly increases with time. As Mr. Gordon has remarked, Prof. Perry and myself were theoretically led to this idea several years ago, as we described in our paper on the "Viscosity of Dielectrics" some time back, and which appears in a recent number of the "Proceedings of the Royal Society of London." The idea that all dialectrics behave more or less like strained viscous substances has been the leading principle that has guided Prof. Perry and myself in our electrical observations—an idea to which our attention has been especially directed in the following way:—When Mr. Perry was examining certain curves obtained for the time increase of strain in a substance subjected to a constant stress simultaneously with some that we had obtained for the soaking out of the charge in an excellently well-insulated Leyden jar, the coatings of which had been maintained for days at a constant difference of potentials, then suddenly discharged, and finally insulated the one from the other, he observed a remarkable analogy between the two classes of curves, and careful examination of all our results up to the present time bears so close an analogy with the stress and strain phenomena in various substances, that we feel that this analogy means a physical connection. Reasoning by analogy, we may conclude that as the rate of production of electric strain grows less and less as the interval elapsing since charging increases, therefore the rate of conversion of electric energy into heat, that is, conduction, also grows less and less, and therefore it is correct to say that the resistance of a dielectric does really increase by electrification. Besides this there is, of course, some of the absorbed energy which is recoverable, and which, therefore, must not be confounded with conduction; just as the energy recoverable from a deflected beam must be distinguished from that lost through conversion into heat, on account of internal friction. From the curves we have obtained of the charging of condensers, and assuming that there is no discontinuity, we must assume that even the first charging is itself a very rapid absorption, and since there is viscosity, even the very first charging must be accompanied with a generation of heat—that is, true conduction. Also, since it is known that gases, like all other substances, are to a certain extent viscous, we cannot believe that air and other gaseous condensers show absolutely no absorptive phenomena; in fact, sufficiently accurate experiments have not yet been made on the subject. We conclude, therefore, that the less the specific resistance the greater is its molecular plasticity, and the more plastic the substance is the greater will be the first charge; therefore from the stress and strain analogy it follows that the less the specific resistance of a substance the greater will generally be the specific inductive capacity. Influenced by these ideas we examined experimentally the specific resistance ofseveral dielectrics, and were eventually able to compare the specific inductive capacities and specific resistances of dilute sulphuric acid, mica, gutta-percha, shellac, Hooper's material, ebonite, paraffin-wax, glass, air, and we found that if the above substances were arranged in descending order of specific inductive capacity they were found arranged in ascending order of specific resistance. Again, since the resistance of air to electric discharge is less than that of a Sprengel vacuum, we were theoretically led to expect (in contradiction to the results of Faraday, who concluded that the specific inductive capacities of all gases at all temperatures and pressures were identical) that very accurate experiments would show that different gases had different specific inductive capacities. This result we were experimentally able to verify after months of investigation, and we found that the denser the gas the higher the specific inductive capacity, a vacuum having the least capacity of the substances we experimented on. It was our intention at that time to determine accurately the electric capacity of a Crookes's vacuum, an investigation I hope to complete this winter, and I anticipate finding this capacity extremely small; for although we found that the specific inductive capacity of a moderately good vacuum made in a space originally containing air was as much as nine-tenths of that of air at ordinary pressure, still Mr. Crookes's recent experiments connected with his radiometer on the logarithmic decrement of a torsion pendulum vibrating in gaseous or vacuous spaces show that the viscosity of the gas does not much diminish until the vacuum has become very perfect. Consequently, it may be anticipated that it will be at the completion of the exhaustion (with a most perfect Sprengel pump) of a gas condenser that the great diminution in the specific inductive capacity of the vacuous dielectric will be experienced. If this result be really arrived at, then it will follow that if it were possible to insulate a conducting wire in a vacuum tube under the sea, the speed of telegraph messages through a long submarine cable, such as crosses the Atlantic or Indian Oceans, could be raised from the present low speed of seventeen words a minute to that of one thousand, which has already been obtained on land lines with Bain's automatic instruments. I may also mention that the mathematical reduction of the curves of viscores yielding, soaking out of charge in dielectrics, &c., will form the subject of a paper Mr. Perry and I propose publishing very shortly.

Chemical Science. (Section B.)

The President, Professor Maxwell Simpson, F.R.S., in delivering the opening address, brought before the section the claims of chemical science to a place in general education, and the claims of original research to a place in the curriculum for higher degrees in our universities.

Without chemistry, he said, we can know nothing of the air we breathe, the water we drink, or the food we eat; we cannot understand the process of combustion, respiration, fermentation, putrefaction, or the endless chemical changes which are continually in operation around us, and which affect our lives for good or for evil. The whole of the phenomena of nature must for ever remain to us, more or less, an inscrutable mystery. Was it not also desirable that we should have some acquaintance with the chemical arts from which we derive so many of our comforts and luxuries; Should we not know something of the arts of photography, dyeing, metallurgy—something of the manufacture of glass and china, and of the thousand beautiful things that are constantly in our hands? Chemistry, too, furnishes us with a key which enables us to unlock vast stores of knowledge contained in physics, geology, mineralogy, physiology, and astronomy.

With regard to the mental discipline, the mind of the student is, continued Professor Simpson, exercised in both the inductive and deductive methods of reasoning. His original faculties are stimulated by the consciousness that he can in many cases readily test the worth of his ideas by experiment. With inexpensive apparatus and a good balance, the intelligent student can make out for himself some of the laws and many of the facts of science, and, it may be, also add to them. He glides insensibly from the known to the unknown. The student of chemistry can reach the field of original work sooner than the student of most other sciences: Once he commences original research the development of his intellectual faculties rapidly progresses. His imagination is daily exercised in propounding new theories, and devising experiments in order to ascertain their truth or falsehood. Laboratory work teaches us to use our senses aright, sharpens our powers of observation, and prevents us from reasoning rashly from appearances. It also promotes manual dexterity, and trains the hands to work in subordination to the head. With regard to the effect of original work on the character, many virtues were necessary to the chemist—courage, resolution, truthfulness, and patience. He is often obliged to perform experiments which are attended with great danger. But the chemist must not be discouraged by fear of accident, neither must he be disheartened by the temporary failure of his experiments, nor at the slowness of his processes. Bunsen was obliged to evaporate 44 tons of the waters of the Durcheim springs in order to obtain 200 grains of his new metal cæsium. It took Berthelot several months to form, by a series of synthetical operations, an appreciable quantity of alcohol from water and carbon, derived from carbonate of baryta. Many years ago, in the laboratory of Wurtz, a poor student was carrying from one room to another a glass globe which contained the product of a month's continuous labour, when the bottom of the globe fell out, and the contents were lost. Nothing daunted, he recommenced his month's work, and brought his research to a successful issue. Above all things the chemist must be true. He must not allow his wishes to bias his judgment or prevent him from seeing his researches in their true light.

He was glad that the importance of original research, as a part of higher education, is at last beginning to be recognised in this country. The Royal University Commission at Oxford has recently recommended that candidates for the higher degrees in science shall, in that University, be required in future to work out an original investigation. In Germany, where education has been so long and so well understood, original work has been, for at least the last half century, a *sine quâ non* for a degree. Another admirable rule exists in that country, the adoption of which in Great Britain might go far to wash out the stain from our islands, of not having contributed our fair quota to the advancement of human knowledge. It is this—the Germans make a point of securing invariably that their scientific chairs shall be filled by men who have already distinguished themselves by their discoveries. The professor, on his appointment, naturally desires to continue his investigations, and endeavours to secure, and usually succeeds in securing, the assistance of his pupils. This is a mutual advantage. The professor is able to do more work for science, and the student, on his part, learns to conduct for himself an original investigation. Hence there is always a rising generation of original workers in Germany who turn out papers more or less meritorious with the rapidity of a Walter's press. They are stimulated by the hope of one day arriving themselves at a professor's chair, the path to which they are well assured is only through the toilsome field of original investigation. The labour is also one of love, and the student's ambition, for the time at least, is bounded by the desire to do something for science. And from a multitude of such enthusiasts the great professors come.

Speaking of the encouragement of research in this country the President said, that to promote original work in this country, he believed it was indispensable that our professors should be well paid. It would save them from the necessity of supplementing their incomes by commercial analyses, and thus enable them to devote their spare time to original work. And to secure that they shall have spare time, he would like to see in every laboratory a competent assistant, who would be able occasionally to take up the professor's lectures, should he be engaged in important work. He was glad to see that the Oxford Commission also recommends the appointment of well-paid assistants. Well-paid professorships and well-paid assistantships would be attractive prizes for our students to work up to; and if it were clearly understood that the only way to these prizes was through original investigation, we should very soon have an army of zealous and competent workers. The plan of appointing a staff of original workers unconnected with teaching had been proposed; but he did not approve of it. The original worker is, as a rule, the best teacher, and the rising generation of students should not be deprived of the advantage of this instruction. No doubt the Government Grant Fund did a good deal for science, but the field of its operations is, under present conditions, limited. Professors, as a rule, are so occupied with teaching that they cannot avail themselves of the fund; and of those students who might be competent and willing, very few can afford to do so. Instead of trusting to the precarious and insufficient support of the fund, they must endeavour to settle themselves permanently in life. It was much to be regretted that the Universities of Oxford and Cambridge, with such splendid revenues at their disposal, should contribute so little to the advancement of physical science. He hoped the day was not far distant when the fellowships—or at least a few of them—which now go to reward young men for merely passing a good examination, shall be given *without examination* to men who shall have advanced human knowledge in any department. At present a fellowship of £250 or £300 a-year, lasting ten or twelve years, and in some cases for life, may be obtained on showing proof of a good memory—or, at most, a capacity for assimilating other men's ideas. To make discoveries—to follow out a new train of thought, and establish it by experiments specially devised to that end, has been left not only without reward, but almost without recognition in our two principal seats of learning. The world at large, ignorant as it is, has a sounder instinct on this subject, and the man who makes the humblest addition to the stock of knowledge in the world rarely fails to receive the world's respect and honour. Professor Simpson concluded by saying that these suggestions could not be well carried out unless the Government takes into its own hands the appointment to all scientific chairs. Of this he thought he saw indications. He believed that sooner or later the Government will assume the supreme direction of education in this country. It has already taken primary education under its control, and quite recently, in Ireland, intermediate education to a great extent. And did the appointment of so many University Commissions not show a disposition on the part of the Government to assume the direction of higher education also?

GEOLOGY. (Section C.)

In opening the proceedings of this section the President (Dr. Evans) called attention to the fact that the present was the third occasion on which the British Association had met in this city; its first meeting was held in Dublin in the year 1835. The President of the Geological Section being a man of whom Irish science might well be proud, and who, he was thankful to say, was still living to enjoy his well-deserved honours—the veteran geologist, Sir Richard John Griffith, the author of the first Geological Map of Ireland. It seemed hardly credible that the construction of this map was commenced in the summer of 1812, or sixty-six years ago; but the records of the Geological Society of London testify to the still more remarkable fact that Sir Richard Griffith was elected a Fellow of that society in 1808—seventy years ago. Indeed, in 1854, when the Wollaston medal was awarded to the then Dr. Griffith, the president, the late Professor Edward Forbes, spoke, as he said, reverentially to one of the earliest members of the society, and to a geologist who appeared in print before he, the president, was born. It was well said on that occasion that the map lately mentioned was one of the most remarkable geological maps ever produced by a single geologist.

(Our readers will have heard with regret of the death of Sir Richard Griffith only about a month after the delivery of Dr. Evans's address. He died on the 22nd of September at the advanced age of 94.) Dr. Evans also paid a tribute of respect to the memory of one who was originally a student in Trinity College, and who subsequently occupied posts of the highest importance in connection with the Geological Society of Dublin and the Geological Survey of Ireland, besides filling the professorial chair of geology in the Dublin University, to Dr. Thomas Oldham, the late director of the Geological Survey of India.

The president then referred to the geology of Ireland, and passed on to the consideration of the date which is to be assigned to the implement-bearing beds of Palæolithic age in England. Dr. James Geikie has, he said, held that, for the most part, they belong to an interglacial episode towards the close of the glacial period, and regards it as certain that no palæolithic bed can be shown to belong to a more recent date than the mild era that preceded the last great submergence.

His follower, Mr. Skertchley, records the finding of palæolithic implements in no less than three interglacial beds, each underlying boulder clays of different ages and somewhat different characters, the Hessle, the purple, and chalky boulder clay. This raises two main questions; first, as to how far Dr. Croll's theory of the great alternations of climate during the glacial period can be safely maintained; and secondly, how far the observations as to the discovery of implements in the so-called Brandon beds underlying the chalky boulder clay can be substantiated. Another question is how far the palæolithic deposits can be divided into those of modern and ancient valleys, separated from each other by the purple boulder clay, and the later of the two older than the Hessle beds. He would only observe, that in a considerable number of cases the gravels containing the implements can be distinctly shown to be of much later date than the chalky boulder clay, and that if the implements occur in successive beds in the same district, each separated from the other by an enormous lapse of time, during which the whole country was buried beneath incredibly large masses of invading ice, and the whole mammalian fauna was driven away, it was a very remarkable circumstance. It was not the less remarkable because this succession of different palæolithic ages seemed to be observable in one small district only, and there was as close a resemblance between the instruments of the presumedly different ages as there was between those of admittedly the same date. He had always maintained the probability of evidence being found of the existence of man at an earlier period than that of the post-glacial or quaternary river gravels, but, as in all other cases, it appeared to him desirable that the evidence brought forward should be thoroughly sifted and all probability of misapprehension removed before it was finally accepted. In the present state of our knowledge, he did not feel confident that the evidence as to these three successive palæolithic deposits had arrived at this satisfactory stage. At the same time it must be borne in mind that if we make the palæolithic period to embrace not only the river gravels but the cave deposits of which the south of France furnishes such typical examples, its duration must have been of vast extent.

In connection with the question of glacial and inter-glacial periods, Dr. Evans mentioned that of climatal changes in general. The return of the Arctic Expedition, and the reports of the geological observations made during its progress, which have been published by Captain Fielden, one of the naturalists to the expedition, in conjunction with Mr. De Rance and Professor Heer, have conferred additional interest on the question of possible changes in the position of the poles of the earth, and on other kindred speculations. Near Discovery Harbour, about latitude 81° 40′, Miocene beds were found containing a flora somewhat differing from that which was already known to exist within the Arctic regions. "The Grinnell Land lignite," say the authors of the report, "indicates a thick peat moss, with probably a small lake, with water lilies on the surface of the water and reeds on the edges, with birches, poplars, and taxodiums on the banks, and with pines, firs, spruce, elms, and hazel-bushes on the neighbouring hills." When we consider, continued Dr. Evans, that all of the genera here represented have their present limits at least from twelve to fifteen degrees farther south, while the taxodium is now confined to Mexico and the south of the United States, such a sylvan landscape as that described seems entirely out of place in a district within six hundred miles of the pole, to which indeed, if land then extended so far, these Arctic forests must have also extended in Miocene times. Making all allowance for the possibility of the habits of such plants being so changed that they could subsist without sunlight during six months of a winter of even longer duration, he could not see how so high a temperature as that which appears necessary, especially for the evergreen varieties, could have been maintained, assuming that Grinnell Land was then as close to the North Pole as it is at the present day. Nor is this difficulty decreased when we look back to formations earlier than the Miocene, for the flora of the secondary and palæozoic rocks of the Arctic regions is identical in character with that of the same rocks when occurring twenty or thirty degrees farther south, while the corals, encrinites, and cephalopods of the carboniferous limestone are such as, from all analogy, might be supposed to indicate a warm climate.

The general opinion of physicists as to the possibility of a change in the position of the earth's axis has, continued the president, recently undergone modifications somewhat analogous in character to those which, in the opinion of some geologists, the position of the axis has itself undergone. Instead of a fixed dogma as to the impossibility of change, we find a divergence of mathematical opinion and variations of the pole differing in extent, allowed by different mathematicians who have of late gone into the question. All agree in the theoretical possibility of a change in the geographical positian of the earth's axis of rotation being affected by a redistribution of matter on the surface, but they do not appear to be all in accord as to the extent of such changes. Mr. Twisden, for instance, arrives at the conclusion that the elevation of a belt twenty degrees in width, such as that which he (the president) suggested in his presidential address to the Geological Society in 1876, would displace the axis by about ten miles only, while Professor Haughton maintains that the elevation of two such continents as Europe and Asia would displace it by about sixty-nine miles, and Sir W. Thomson has not only admitted, but asserted as highly probable, that the poles may have been in ancient times "very far from their present geographical position, and may have gradually shifted through ten, twenty, thirty, forty, or more degrees without at any time any perceptible sudden disturbance of either land or water."

The President was glad to think that this question, to which he had to some extent assisted to direct attention, had been so fully discussed, but he could hardly regard its discussion as being now finally closed. It appeared to him doubtful whether eventually it would be found possible to concede to this globe that amount of solidity and rigidity which at present it is held to possess, and which to his mind at all events seemed to be in entire disaccordance with many geological phenomena. Yet this, as the Rev. O. Fisher has remarked, is presupposed in all the numerical calculations which have been made. He was also doubtful whether in the calculations which have been made, sufficient regard has been shown to the fact that a great part of the exterior of our spheroidal globe consists of fluid which, though of course connected with the more solid part of the globe by gravity, is readily capable of readjusting itself upon its surface, and may, to a great extent, be left out of the account in considering what changes might arise from the disturbance of the equilibrium of the irregular spherical or spheroidal body which it partially covers. It appeared to him also possible that some disturbances of equilibrium may take place in a mysterious manner by the redistribution of matter or otherwise in the interior of the globe. Captain F. J. Evans, arguing from the changes now going on in terrestrial magnetism, has suggested the possibility of some secular changes being due to internal, and not to external causes; and if it be really true that there is a difference between the longest and shortest equatorial radii of the earth, amounting to six thousand three hundred and seventy-eight feet, such a fact would appear to point to a great want of homogeneity in the interior of our planet, and might suggest a possible cause for some disturbance of equilibrium.

Professor Haughton had been mentioned among those who, from mathematical considerations, have arrived at the conclusion that a geographical change in the position of the axis of rotation of the earth is not only possible but probable. In a recent paper, however, he has maintained that, notwithstanding this possibility or probability, we can demonstrate that the pole has not sensibly changed its position during geological periods. He arrives at this conclusion by pointing out that in the Parry Islands, Alaska and Spitzbergen, there are triassic and jurassic deposits of much the same tropical character, and then by a geometrical method fixing the North Pole somewhere near Pekin, and the South Pole in Patagonia, within seven hundred miles of a spot where jurassic ammonites occur, shows that such a theory is untenable. In the same way he fixes the pole in Miocene times near Yakutsk, within eight hundred miles of certain Miocene coal beds of the Japanese islands. These objections were at first sight startling, but he (Dr. Evans) thought it would be found that if, instead of drawing great circles through certain points, we regard those points as merely isolated localities in a belt of considerable width, there is no need of fixing the pole of either the jurassic or the miocene period with that amount of nicety with which Professor Haughton has ascertained its position. The belt may indeed be made to contain the very places on which the objection is founded. One other consideration to be urged was as to the safety of regarding all deposits of one geological period as contemporaneous in time. Although an almost identical flora may be discovered in two widely-separated beds, it appeared to him that chronologically they are more probably of different ages than absolutely contemporaneous; and, inasmuch as the duration of the miocene period must have been enormous, there would be time—if once we assume a wandering of the poles—for such wandering to have been considerable between the beginning and end of the period.

The president then adverted to the discovery of palæozoic rocks under London. So long ago as 1856 the Kentish Town boring had shown that immediately below the Gault red and variegated sandstones and clays occurred, which Professor Prestwich regarded as probably of old red or Devonian age. The boring of Messrs. Meux and Co. has now shown that under Tottenham Court Road, at a depth of little more than nine hundred feet from the surface, there are true Devonian beds, with characteristic fossils, and that Mr. Godwin Austen's prophecy of the existence of palæozoic rocks at an accessible depth under London has proved true. Professor Prestwich, from a consideration of the French and Belgian coal fields, inclines to the belief that in the district north of London carboniferous strata may be found.

In the department of theoretical geology, the president called attention to some experiments by M. Daubrée, in which he has attempted to reproduce on a small scale various geological phenomena, such as faulting, cleavage, jointing, and the elevation of mountain chains.

With regard to recent progress in palæontology, he referred to the magnificent discoveries in North America, which are principally due to the researches of Professors Marsh, Leidy, and Cope. The *diceratherium*, a rhinoceros with two horns placed transversely, and the *dinoceras*, somewhat allied to the elephant, but with six horns, arranged in pairs, were, he said, as marvellous as some of the beasts seen by Sir John Maundeville on his travels, or heard of by Pliny. But perhaps the most remarkable series of remains ever discovered were those which so completely link the existing horse with the *eohippus* and *orohippus*, and still farther extend the pedigree of the genus *equus*, which had already been some years ago so ably traced by Professor Huxley.

Of these American discoveries, as well as those made in the tertiary beds of Europe, M. Albert Gaudry has largely availed himself in his recent beautiful volume on the links in the animal world in geological times, a work which will long be a text-book on the inter-relation of different orders, genera, and species.

The president concluded by remarking that Professor Marsh has largely added to our knowledge of early forms of birds with teeth. The tertiary *Odontopteryx toliapicus* from Sheppey, described by Professor Owen, seems rather to be endowed with bony tooth-like processes in the jaw, than actual teeth, and the head of the *argillornis* from the same locality is at present unknown. But the *hesperornis* and *ichthyornis* from the cretaceous beds of America possess veritable teeth, in the one case set in a long groove in the jaw, and in the other in actual sockets. Such intermediate, or, as Professor Huxley would term them, intercalary forms, tend materially to bridge over the gap which at first sight appears to exist between reptiles and birds, but which to many palæontologists was far from being impassable, long before the discoveries just mentioned. The amphicœlous character of the vertebræ of *ichthyornis* presents another most remarkable peculiarity, which is also of high significance. There were rumours of the discovery of another *archæopteryx* in the Solenhofen Slates, which is said to present the head in a much more complete condition than that in which it occurs on the magnificent slab now in the British Museum. As yet, the jaws have not had the matrix removed from them; but should they prove to be armed with teeth, it would to him be a cause of satisfaction rather than surprise, as an opinion he expressed some fifteen years ago, would be confirmed, viz.: that this remarkable creature may have been endowed with teeth, either in lieu of or combined with the beak.

Professor Hull, F.R.S., then proceeded to give a sketch of the geology of the environs of Dublin.

Mr. J. Nolan, M.R.I.A., read a paper "On the Ancient Volcanic Districts of Slieve Gullion, between Dundalk and Carlingford Bays."

Mr. Mattieu Williams, F.R.A.S., F.C.S., read some valuable notes "On the Glaciation of Ireland and the Tradition of Lough Lurgan." The first part of this paper discussed the origin of the ridges of drift which cover so large an area of Ireland, and the fact that this, like all the other varieties of till or boulder clay, are unrepresented by the moraines of existing Alpine glaciers, which have no such clayey matrix; while, on the other hand, true moraines, such as we now see in the course of formation, are the rarest of all the vestiges of ancient glaciation. The author's explanation of this is that the ancient glaciers of Ireland, Scotland, Norway, &c., for the most part terminated in the sea, and deposited the *whole* of their contents in still water as their terminal portions thinned and floated upwards; these deposits thus including the stony material of modern alpine moraines *plus* the fine particles that are washed out by the glacier torrent and now deposited lower down as alluvium. The heaping into ridges he explained by the fluctuations of advance and recession of the glaciers at a later period. (These views are also described in the "Quarterly Journal of Science," April, 1877.)

The second part of the paper refers to an ancient legend, which describes a great fresh water lake occupying the site of Galway Bay, and the formation of the present bay by irruption of the sea through the ancient barriers of the lake. The author met with this legend very recently, and his recollections of the great drift ridges running towards the bay suggested the probability of their former extension across. To verify this he revisited the bay, re-examined these ridges of drift, and found that several now stand in the Bay, forming long islands and promontories projecting from various parts of its shores, and that some have been cut off by the sea forming cliffs of light coloured boulder clay richly studded with finely striated boulders (specimens and heel-ball rubbings of which were shown). The details of the position of these ridges, and relations of the cliffs (some of which are 50 feet high) to those on the opposite shores are explained.

The soundings between these are also referred to, as they show the existence of great bars connecting some of the most remarkable of these cliff sections of drift.

The author concludes that there is evidence of the former existence of several ridges of drift extending obliquely into the bay, and that some of these may have so nearly crossed it as to leave only a sufficient channel for the outlet of the river water; but that the magnitude of the lake thus formed would not be so great as the tradition indicates, unless the waters were backed considerably over the plains around Galway, which would have been the case had its level been only slightly raised.

He found a popular opinion prevailing that the Aian Islands are the remains of a great barrier, but there is no present evidence supporting this.

Mr. W. H. Baily, M.R.I.A., F.G.S., read a paper on some Additional Remains of Labyrinthodont Amphibia and Fish found in the Jarrow Colliery, near Castlecomer, County Kilkenny. He illustrated his remarks with well drawn diagrams, and exhibited some of the specimens clearly defined in the coal, showing the vertebræ, head, fins, &c. Some of the reptiles were 3 feet 7 inches in length, and the fish specimens were beautifully marked in the matrix.

Mr. W. Pengelly, F.R.S. read the fourteenth report on the Exploration of Kent's Cave. He mentioned that the cave was situated a distance of one mile eastward of Torquay, and half a mile distant from the bay. It was beneath a hill 250 feet high. The deposits from time to time were black mould, in which were found sheep remains; under that stalagmite, and then cave earth, in which the bones of hyænine were discovered; then crystalline stalagmite and breccia, as yet the most ancient deposit found in the cavern. In the two latter the bones of bears only were found. The hyænine bones were the most numerous. Only one example of cave lion and one of fox were found here. Mr. Pengelly exhibited a number of specimens of jaw-bones and teeth of the hyena, of the bear, some flint instruments, and a remarkable quartzite discovered in the cave during last year. The report contained an account of the explorations carried during the year. At the end of July, 1878, the two reaches only had been explored. Its floor was found to be a complete pavement of blocks of limestone, some of considerable size. The chamber measured about 30 feet from north to south; from 7 to 13 feet from east to west, and from 8 to 13 feet from the roof to the bottom of the cavern. The only objects of interest found in the chamber were four pieces of bone, which occurred at depths exceeding a foot, and a lump of oxide of manganese. The recess near the junction of the two reaches was, in proportion to its capacity, much more productive.

Mr. Isaac Roberts, F.G.S., read a very instructive paper "On Experiments on Filtration of Sea Water through Triassic Sandstone."

Mr. W. Pengelly, F.R.S., read a paper "On the Relative Ages of the Raised Beaches and Submerged Forests of Torbay."

Mr. V. Ball, M.A., gave a description of a new Geological Map of India which is soon to be published, with a manual of the geology of that country. Mr. Ball stated that his investigations in India had converted him to the glacial theory as exemplified in the boulders he had seen.

Professor Williamson, Owens College, Manchester, read a paper with regard to coal measures.

The President gave a short account of some fossils from the Northampton Sands, which, he said, formed an important part of the oolites, which had been described in papers published by the Geographical Society of London.

Mr. J. Nolan, M.R.I.A., read a paper "On the Metamorphic and Intrusive Rocks of Tyrone."

Mr. Sterry Hunt, LL.D., F.R.S., read a paper "On the Origin and Succession of Crystalline Rocks." The author's researches into the composition and structure of the crystalline rocks, conjoined with his studies of the chemistry of natural waters, led him, in 1860, to reject the hitherto received view of the epigenic or metasomatic origin of serpentine, steatite, chlorite, and similar rocks, and to maintain their derivation from silicates formed by chemical processes and deposited in the water of lakes or seas. This view he soon after extended to the various other exceptional rocks found in crystalline formations which it was, in 1864, asserted had been "formed by a crystalline molecular rearrangement of silicates generated by chemical process in waters at the earth's surface."

In continuation of the same subject, a paper was read by Dr. Hicks on some new pre-Cambrian areas in Wales.

Mr. E. T. Hardman, F.C.S., read a paper on "Hullite," a hitherto undescribed mineral. He said this mineral occurs in abundance at Carnmoney Hill, near Belfast, in the basalt forming the neck of a miocene volcano. It has never been described as analysed, and has been referred to in the Survey maps and labelled on the Survey collections as obsidian, doubtless from its black colour and waxy lustre. In physical character it somewhat agrees with the chlorophaite of Maculloch, but is entirely different in composition, which more resembles that of delessite: from this, however, it differs essentially in colour, hardness, streak, and specific gravity, but it appears on the whole to belong to the ferruginous chlorite group. Physical characters—Colour, velvet black; hardness, 2; brittle; lustre, waxy to dull; very slightly affected by acids: occurs at Carnmoney and Shane's Castle, near Lough Neagh. Its most remarkable characteristics are its low specific gravity, and its resistance to the blowpipe—both curious points, considering the large quantity of iron it contains. The author proposed to call the mineral Hullite, after Professor Hull, in commemoration of the valuable work he has done in elucidating the microscopic mineralogy of the basalts of Ireland. Professor Hull has examined the microscopic structure of the mineral, and of the rock in which it occurs. Under the microscope it is of an amber brown colour, nearly opaque. It permeates the whole rock, filling the interstices, and enclosing the other minerals. It appears very much to assume the character of chlorite, and is undoubtedly a distinct mineral, and not a product of alteration.

Prof. E. Hull, M.A., F.R.S., Director of the Geological Survey in this country, gave a short account of the progress of the Geological Survey of Ireland from its commencement in 1832, under the late General Portlock, R.E., down to the present day, stating that the whole country south of a line drawn roughly from Larne on the coast of Antrim to Sligo had been surveyed, while 160 sheets of the geological map, on a scale of 1 inch to the statute mile, had been published. Along with these had also been issued 78 separate explanatory memoirs describing the structure and palæontology of

126 sheets. It had been found necessary to revise the geology of the Leinster and Tipperary coal-fields, the carboniferous trap-rock of the County Limerick, and the south-east portion of the country, including parts of Wicklow and Wexford. The coal-fields of the North of Ireland had also been surveyed and published in maps both on the 6-inch and 1-inch scales; and it was also intended that the districts of the County Antrim containing pyrolitic iron ores should be illustrated by maps on both scales. The district still remaining to be examined included the greater portions of Donegal, Tyrone, Fermanagh, Sligo, and Antrim.

The President said the object of these Surveys was to make the public at large thoroughly acquainted with the geology of the country in which they resided. Maps were carefully drawn, and memoirs published from time to time in illustration of the maps; but unfortunately, so far as the diffusion of knowledge was concerned at the present time,—not owing to any prohibition of the Geological Survey, but owing to some mistaken view on the part of the Treasury,—prohibitive prices were placed upon the geological memoirs. He had seen small pamphlets priced at 16s. or 17s., though these pamphlets were printed and published at the public expense for the benefit of the public. He held in his hand a very small pamphlet, which was published at 9s. He did not think a false economy of this kind ought to be suffered to go on without a protest on behalf of those who were interested in geological progress. He therefore felt it right to make these remarks, in the hope that steps might be taken to bring this matter under the consideration of the Treasury, and point out how with the one hand they were lavishly spending money for the advancement of geological knowledge, and with the other withholding it from the public.

Dr. Sterry Hunt read a paper "On the Geological Relations of the Atmosphere." The author began by noticing the inquiries of Ebelmen into the decomposition of rocks through the influence of the atmosphere, resulting in the fixation of carbonic acid and oxygen, and discussed the question at length with arithmetical data. He inquired farther into the fixing of carbon from the air by vegetation, with liberation at the same time of oxygen, both from carbonic acid and from the decomposed water, the hydrogen of which, with carbon, forms the bituminous coals and petroleums. It was shown that the carbonic acid absorbed in the process of rock decay during the long geologic ages, and now represented in the form of carbonates in the earth's crust, must have equalled probably two hundred times the entire volume of the present atmosphere of our earth. This amount could not, of course, exist at any one time in the air; it would at ordinary temperatures be liquefied at the earth's surface. Whence came this vast quantity of carbonic acid which must have been supplied through the ages? The hypothesis of Elie de Beaumont, who supposed a reservoir of carbonic acid stored up in the liquid interior of the planet, was discussed and dismissed. The gas now evolved from the earth's crust from volcanic and other vents was probably of secondary origin, and due to carbonates previously formed at the surface. The solution of the problem offered by the author is based upon the conception that our atmosphere is not terrestrial, but cosmical, being a universal medium diffused throughout all space, but condensed around the various centres of attraction in amounts proportioned to their mass and temperature, the waters of ocean themselves belonging to this universal atmosphere. Such being the case, any change in the atmospheric envelope of any globe, whether by the absorption or the disengagement of any gas or vapour, would by the laws of diffusion and static equilibrium be felt everywhere throughout the universe, and the fixation of carbon at the surface of our planet would not only bring in a supply of this gas from the worlds beyond, but, by reducing the total amount of it in the universal atmosphere, diminish the barometric pressure at the surface of our own and of all other worlds.

Prof. W. King, D.S.L., contributed a paper "On the Age of the Crystalline Rocks of the County Donegal." He had succeeded in obtaining some true fossils in portions of the Innes Lower Limestone that have scarcely undergone any change. This was the first example, as far as he could ascertain, of an undoubted fossil having been detected in these limestones. The fact may be taken as evidence that their deposits and their associated argillaceous and siliceous masses are of the Lower Silurian Age, and it seemed highly probable that the more intensely metamorphosed rocks in the north-west division of Donegal belonged to the same geological period.

BIOLOGY. (Section D.)

The President of this Section was Prof. Flower, F.R.S.; Dr. Robert M'Donnell, F.R.S., presiding over the Department of Anatomy and Physiology, and Prof. Huxley over that of Anthropology. Our space will not permit us to give abstracts of the excellent addresses of these gentlemen, or of the papers brought before the Section.

Prof. Sir Wyville Thomson presided over Section E (Geography): his address was listened to by a crowded audience. Prof. Ingram, LL.D., presided over Section F (Economic Science and Statistics); and Mr. E. Easton, C.E., over Section G (Mechanical Science).

At one of the meetings of the Geographical Section Sir Wyville Thomson read a paper "On the Progress in the Official Report of the *Challenger* Expedition." While the ship was at sea their time was entirely devoted to the registering of observations, and cataloguing, labelling, and storing specimens. With regard to the destination of the collection he proposed that in the first place each specialist should be required to set aside all unique specimens, and the most complete series possible of all species of which there were duplicates, which should be sent to the British Museum, and that afterwards duplicates should be arranged in sets and distributed to museums at home and abroad. From what he saw at present this difficult Report of the Voyage of H.M.S. *Challenger* would extend to from fourteen to sixteen quarto volumes of 500 or 600 pages. The whole would be illustrated by about 1200 plates and many woodcuts and photographs. The map of the first volume was nearly completed, and the charts or ship's course and the sections showing the natural distribution of ocean temperature. The second volume will consist chiefly of tables, and will include a report on the magnetic observations met during the voyage, drawn up under the superintendence of the hydrographer of the Navy, and a detailed report on the meteorology prepared by Capt. Tizard. Another volume would contain the discussion of the nature of the bottom, the composition and specific gravity of sea-water and the composition of its gases, and a number of other general matters; and the remainder of the book will be occupied by a series of memoirs by different authors on the various groups of animals which constitute the deep-sea fauna.

NOTICES OF BOOKS.

Conferences held in Connection with the Special Loan Collection of Scientific Apparatus at South Kensington in 1876. London: Chapman and Hall.

These Conferences originated in a letter addressed by Lord Sandon to the Presidents of the various Learned Societies, in which it was suggested that the value of the Loan Collection of Scientific Apparatus then gathered together at South Kensington would be greatly enhanced by demonstrations of the mode of working various instruments, and by papers on different subjects connected therewith. The Conferences commenced on May 16th and were continued until the 2nd of June. Five Sections were formed, each possessing a President and several Vice-Presidents. The Sections were respectively devoted to—1. Physics. 2. Mechanics. 3. Chemistry. 4. Biology. 5. Physical Geography, Geology, Mining, and Meteorology. The volume now before us relates to the two first in the list.

The Physical Section was presided over by Mr. W. Spottiswoode, and it numbered among its Vice-Presidents eminent Professors from Germany, Italy, France, and Holland. In the opening address the President enumerated some of the greater curiosities of the Collection:—A quadrant of Tycho Brahe, telescopes of Galileo, lenses constructed by Huyghens, a telescope of Newton, Sir W. Herschel's grinding-machines for specula, and a telescope which he constructed in his earlier days; also the original siderostat of Foucault, a compound speculum by the late Lord Rosse, the Kew photoheliograph, and the "compound microscope" of Zacharias Gaussen constructed in 1590. The original air-pump and Magdeburgh hemispheres of Otto von Guericke were shown; also an air-pump of Boyle, the compressor of Papin, Regnault's apparatus for determining the specific heat of gases, Fizeau's and Foucault's original revolving mirrors, Daguerre's first photograph on glass, and original electrical apparatus of Faraday, and Ampère.

The first address was delivered by Mr. William Huggins, "On Spectroscopy applied to the Heavenly Bodies other than the Sun." The spectrum of the nebula in Orion was compared with those of hydrogen and nitrogen; the spectrum of Sirius with that of hydrogen in a vacuum tube; and the spectrum of Arcturus with those of hydrogen, magnesium, and sodium.

Mr. Lockyer afterwards gave an account of the present state of spectroscopic research, specially in reference to the mapping of spectra. Maps are now drawn to twelve times the scale of Ångström's map, and of course the mapping has become much more minute. Thus in Ångström's map only three lines are shown between H and H_2, while in the new maps no less than *ninety-nine* are inserted.

Mr. H. C. Sorby followed with an account of "Spectrum Microscopes and the Measuring Apparatus used with them."

Other papers were read—by M. Pictet, on "Ice-making Machines;" on "Compass Correction in Iron Ships," by Sir Wm. Thomson; "The Radiometer," by Prof. Wartmann, of Geneva; and "The Anemometer," by Mr. Fletcher.

At the second meeting of the Conference Dr. Tyndall described some of his remarkable experiments on the reflection of sound by layers of air of different densities, and on sensitive flames. Dr. Stone read a paper on "Just Intonation" in music, and on the limits of audible sound; and Mr. Bosanquet gave an account of his instruments for the attainment of just intonation. Prof. W. G. Adams then read a paper on Sir Charles Wheatstone's acoustical discoveries.

In a very able and detail article on the "Instruments contributed by Italy," Prof. Eccher discussed the various inventions and discoveries of Galileo, and the instruments of the Accademia del Cimento, so liberally contributed to the Loan Collection by the city of Florence. The position of Galileo in the history of the Sciences is too well known to need more than passing notice, but the work of men like Redi, Viviani, Marsili, Dati, and Borelli is often overlooked or forgotten. Let us glance at the labours of the last-mentioned of these men—the Neapolitan Borelli. "A mathematician, a physician, an astronomer, he occupied himself with all subjects, and thus supplied himself with ample materials for future discoveries. He studied the reciprocal actions of floating bodies (*galleggianti*), and discovered its theory; he was the first to observe the variations of the barometer with the changes in the atmosphere; he considered the question of the freezing of water; he planned the experiments which were decisive against the idea of the positive lightness (*leggerezza positiva*); he measured the greatest expansion of air freed from surrounding pressure; he determined the weight of air compared with water; he suggested the famous experiment with the silver ball, to test the compressibility of water; and another experiment on the propagation of sound (*in vacuo*); he studied the contraction of various liquids in cooling; and in the researches on the velocity of propagation of light he was the first to construct the heliostat (*eliostata*);" . . . he also made various astronomical discoveries; he was the first to observe the *vena contracta*, and he devised new experiments to illustrate Galileo's idea of the fall of bodies in a vacuum.

On the occasion of the next Conference Mr. Dewar described his *Charcoal Vacuum*, which he produces by placing charcoal in a tube, and exhausting by means of a mercurial air-pump at a red-heat. When this has been completed the tube is sealed up, and the last trace of mercury vapour, gas, sulphuric acid, and other impurities are absorbed by the charcoal, and in a few minutes the electric spark refuses to pass through the vacuum. A very good vacuum may also be made by filling a tube with bromine vapour, and absorbing it by means of charcoal. Prof. Rijke gave a very interesting account of the historical instruments from Leyden, which included the first compound microscope made at Middleburg, by Hans and Zacharias Gaussen, before 1610; various mechanical contrivances of S'Gravesande for illustrating the velocities of falling bodies; and lenses constructed by Huyghens.

The Mechanical Section of the Conference was presided over by Mr. C. W. Siemens, and among the Vice-Presidents were General Morin, Director of the Conservatoire des Arts et Metiers, and M. Tresca, the Sub-Director. The President's Address and several of the earlier papers relate to the subject of measurements—linear, superficial, solid, electrical—and of time. M. Tresca communicated a very important paper "On the Flow of Solid Bodies," embodying the results of many years of study and research; Mr. Barnaby, Chief Constructor of the Navy, a lengthy and valuable paper "On Naval Architecture;" and Mr. Thomas Stevenson described the more important improvements which have lately been made in lighthouses.

The subject-matter of this volume is most admirable, and the works cannot fail to be frequently consulted by men of Science.

A Handbook of Practical Telegraphy. By R. S. Culley. Seventh Edition. London: Longmans and Co. 1878.

This well-known work, which has been revised and considerably enlarged in this edition, contains an account of electric telegraphy in all its forms. Commencing with the sources of electricity, the author passes on to a consideration of the laws of the current, static and dynamic induction, atmospheric electricity, and earth-currents. The construction of telegraph lines, the management of circuits, and the apparatus used for signalling are then discussed; a long section is given to submarine telegraphy; and recent inventions, including the telephone, are finally discussed.

This work will undoubtedly continue to maintain the high position which it has so long enjoyed.

Natural Philosophy for General Readers and Young Persons. Translated from the French of Ganot, by E. Atkinson. Third Edition. London: Longmans and Co.

This work has not been much enlarged, but it has received the very useful additional feature of a series of questions systematically arranged in reference to the corresponding parts of the book, which must greatly enhance its value, both to teachers of Science and to self-taught students.

Electric Lighting. A Practical Treatise, by Hippolyte Fontaine. Translated from the French, by Paget Higgs, LL.D., Assoc. Inst. C.E. London: E. and F. N. Spon. 1878.

This work is divided into twelve chapters, which treat of the Voltaic arc, electric lamps, magneto-electric machines, the motive power they absorb; the industrial applications of the light to manufactories, lighthouses, ships, and forts; the cost of electric lighting; lighting by incandescent tubes; and, finally, of the divisibility of the electric light, The light was first applied to lighthouses in 1863, at the lighthouse of Le Hène, near Havre, and the light (produced by an "alliance" electro-magnetic machine) was found to penetrate 8 kilometres further than an oil-light, while in foggy weather the range of the light was twice as great. There now exist electric lighthouses in France, England, Russia, Austria, Sweden, and Egypt, and the French Government are about to try the effect of a Gramme machine, giving a light equal to that of 2000 Carcel burners. Formerly the light of 100 Carcel burners required the expenditure of 3horse-power, but M. Gramme has succeeded in producing a light of 450 burners with an engine of 2 horse-power. As to the cost, the lighting of the Rue Impériale, in Lyons, with 60 Bunsen elements, was found to cost 3·55 francs per hour. An interesting table will be found (p. 164) giving the cost of various sources of light, from which we learn that the cost for 4000 burners per hour is estimated as follows:—

Wax candles	132·00	francs.
Stearine	98·40	,,
Tallow	56·40	,,
Colza oil	28·00	,,
Voltaic battery	24·00	,,
Petroleum oil	21·60	,,
Shale oil	18·72	,,
Oil gas	10·00	,,
Gramme machine with steam motor ...	1·78	,,
,, ,, ,, hydraulic power	0·44	,,

Thus under the most favourable conditions the Gramme machine light is 300 times less expensive than the light of wax candles. It must be borne in mind, however, that interest and wear and tear ought to be charged upon the machinery.

An exact comparison of the relative cost of gas and electric lighting is given in the following tables for a spinning-mill of 800 looms.

First, in the case of the gas-lighting :—

Number of burners	415	
Number of hours of night-work per year	500	
Cost of introducing gas	12,000	francs.
Price of cubic metre of gas	0·25	franc.
Cost of gas per year	5486·85	francs.
Interest 12,000 francs, at 10 per cent	1200·00	,,
Maintenance of apparatus and contingencies	263·00	,,
Total cost of gas-lighting per year	6950·10	,,

The corresponding electric lighting requires :—

Six Gramme machines, at 1500 francs	9000	francs.
Six regulators of 300 burners, at 450 frs.	2700	,,
Wires and mounting	1300	,,
Twelve horse-power steam motor	8000	,,
Total cost of apparatus	21,000	,,

Working expenses :—

Consumption of electric carbons, 240 metres, at 2·5 francs	600	francs.
Consumption of coal, 10 tonnes, at 30 frs.	300	,,
Interest on 21,000 francs, at 10 per cent	2100	,,
Annual maintenance and contingencies	1600	,,
Total cost of electric lighting per year	4600	,,

"The electric light in this case costs annually 33 per cent less than that by gas, gives six times more light, and suppresses all danger of fire."

The light emitted by Geissler's tubes has been found to be of such slight intensity that they are practically useless even for powder-mills and gunpowder-works. The divisibility of the electric light, as employed by Jablochkoff in 1877, will apparently lead to a greater introductiion of the use of this kind of light. "By the term 'divisibility of the electric light' we do not mean the production of several intense lights by means of one machine or battery, but simply the maintaining of a few small luminous centres, each equal to 1 to 15 Carcel burners."

M. Fontaine's work will be read with interest by all who are concerned in the application of electricity to lighting purposes. The book is well illustrated, and the translation has been carefully made.

The Human Eye; its Optical Construction Popularly Explained. By R. E. Dudgeon, M.D. London: Hardwicke and Bogue. 1878.

This work commences with an account of various instruments which help to illustrate the mechanism of the eye; lenses are discussed, and the refraction of light by lenses of different form and curvature. The eye is compared to a photographic camera, and various sectional diagrams are given to illustrate the views expressed. It is asserted that Külme has found that the image of an object thrown upon the retina during life remains visibly impressed on that membrane after death. The minute structure of the crystalline lens receives illustration; also the conditions of long- and short-sightedness. Clossat long ago pointed out the fact that the curvature of the crystalline lens is not that of a sphere, but of an oblate spheroid. Dr. Dudgeon proposes the use of *air* lenses for the use of divers under water. Of course an air-lens ceases to be a lens as soon as it is out of water. An interesting account is given of subaqueous optics and effects—among them the effects observed by a diver furnished with air-lens spectacles, who is resting on the bottom of a bath, full of clear water, with a perfectly unruffled surface. The eyes of fishes are discussed, and diagrams are given of the lenses of the skate and turtle. Dr. Dudgeon believes that the lens possesses a "rotation movement" in accommodation for near vision. "If the transition from near to distant accommodation takes place in the dark, there occurs, as Czernak has remarked, a sudden flash of light in the eye, which he calls a *phosphine*. This phenomenon is, I believe, caused by the sudden springing back of the lens to its normal unaccommodated position, communicating a shock to the retina whereby the sensation of light is evoked,—just as a slight blow on the eye will produce the sensation of a flash of ordinary light. At the same time the sudden relaxation of the ciliary muscle, or tensor chloroideæ, permitting the striated chloroid membrane to resume its position, will intensify the shock that produces the flash."

The work is clearly written throughout, and embodies the results of the most recent German researches on the subject, and it will be welcomed alike by the old and the young physiologist.

Light. A Series of Simple, Entertaining, and Inexpensive Experiments in the Phenomena of Light, for the Use of Students of every Age. By ALFRED M. MAYER and CHARLES BARNARD. "Nature" Series. London: Macmillan and Co. 1878.

THIS work describes, in simple language, a number of experiments illustrating the principal properties of light, by means of a beam of sunlight admitted into a dark room, and various contrivances. The experiments are highly ingenious, and, although in many cases insufficiently explained, the young student cannot fail to learn a good deal from the book. As an example of the effective experimental method employed, we may specially mention the device for illustrating the refraction of light, figured and described on page 46.

The book is specially designed "to give to every teacher and scholar the knowledge of the art of experimenting."

The Devil Demonstrated. By A PHYSIOLOGIST. London: Printed for the Author, by G. J. Ogden and Co.

WE have here a curious and highly original work, written ten years ago, and now published anonymously for fear of the tea-tables. The author, it should be premised, is not a materialist: he distinctly admits the existence of the Deity, and of an immaterial, supra-vital, responsible principle in man, not perishing with what is commonly called death. He recognises, moreover, the authority of the Christian Revelation, and in seeking to establish his doctrine he appeals to St. Paul against the authority of Milton, a writer whose glosses on the Scriptures have—in England at least—been popularly identified with the Sacred text, and are, we believe, to no small extent responsible for the antagonism which in some quarters still prevails between Religion and Science.

The author contends, in brief, that the Devil is neither a person nor a spirit, but an evil principle connected with the flesh, which lives and dies with that flesh, not found in man alone, but in every animal or vegetable form of matter. "Where there is force acting upon matter, there is the Devil"—a formula which would of course comprehend inorganic substances as well as plants and animals. "Wherever there is any evil principle, anything which works injury, the Devil, or evil, is there present, as much as in man."

This view is expounded in the form of a dialogue between a Doctor, a Friend, and a Patient, introduced by the latter, and who here is supposed to represent the unscientific British public.

This typical hearer receives the novel doctrine very complacently, considering that he has just declared that he "knows nothing of modern science, and looks upon it with horror," that he "cannot bear to meet a man of science," and that he has "read some of the pestilent and heretical works lately issued."

In the argument which the author brings forward we find, as one step, an account of atoms in which it is asserted "we know that they are formed of two distinct parts; that they are spheroids, having a dense axis and a less dense equator. For instance, the axis of the atom of oxygen is formed of ozone and the equator of antozone; the atom of water has oxygen for its axis and hydrogen for its equator." How an atom, which is *ex hypothesi* indivisible, can yet be formed of two distinct parts, more or less dense, it is difficult to conceive. Allowing, however, the author to substitute the term "molecule" for "atom," we are still not satisfied. The very existence of antozone is too problematical for us to accept this theory of the constitution of the molecule of oxygen. But even if we accept the whole of the author's interesting demonstration, we utterly fail to see that it leads necessarily to the point at issue. Supposing that there are sources of evil existing "within man, and concomitants of his flesh," this by no means disproves the existence of an external author of evil, a parastatic spirit.

Further, the author's views are deficient in clearness: from some passages we should judge that he pronounces all Nature—everything consisting of matter acted on by force Devilish—evil. But from others we should gather that he would consider these attributes as restricted to matter and force when exerting abnormal, morbid, and injurious influences. Accepting this as the more rational view, we have still to ask where is the boundary between good and evil—especially in a physical view—to be drawn, and who is to decide? A storm that ravages our crops and sinks our shipping may at the same time purify the air and remove an epidemic. Is it, then, diabolical or non-diabolical, or a happy mixture of both? The author seems to us not to distinguish clearly enough between physical and moral evil, which may be perfectly independent of each other. For granting to the fullest extent Dr. Johnson's dictum that "every sick man is a villain,"—granting that a diseased liver, indigestion in any form, and no less the mediæval religious exercise of fasting, are the teeming sources of envy, "hatred, malice, and all uncharitableness," we must never forget that there is at any rate one shortcoming to which man is prone, almost in the exact ratio of his health and vigour. It is curious how completely modern moralists forget the distinction between "appetites" and "passions."

Should the author's views meet with acceptance they would not, we believe, prove necessarily antagonistic to a belief in man's responsibility, but they might favour the recrudescence of mediæval asceticism, and of that contempt for matter and "the flesh" which has been so formidable a hindrance to man's intellectual and physical progress.

Elements of Chemistry, Theoretical and Practical. By W. A. MILLER, M.D., &c. Revised by CHARLES E. GROVES, Sec. F.I.C., F.C.S., &c. Part II.—Inorganic Chemistry. Sixth Edition, with Additions. London: Longmans and Co. 1878.

STUDENTS and teachers, young and old, will welcome the sixth edition of Part II. of Dr. Miller's well-known Manual, which this time is entirely revised by Mr. Groves, who assisted Prof. McLeod in the preparation of the fifth edition, published in 1874. During the last four years inorganic chemistry has made but little progress, both professors and students preferring to employ their talents and occupy their time with trying to cram an additional atom of oxygen or hydrogen into some unhappy organic radical that no one but themselves has ever seen, and that strongly objects to give hospitality to the intruder, rather than with one or other of the endless problems with regard to the commonest reactions that are hourly staring us in the face.

Although there are but few new facts to chronicle during the period mentioned, one of them at least is of very great importance. We of course allude to the discovery of gallium by M. Lecoq de Boisbaudran, and its very unexpected confirmation of Newland's and Mendelejeff's periodic law. The influence of this discovery on the theory of the relationship between the elements, first glimpsed by Prout, is a very important one, and it might, we think, have been more enlarged upon in the fifty pages which Mr. Groves has added to the sixth edition. An account of Davyum, too, is found in its proper place. Short accounts are given of Deacon's and Weldon's chlorine processes, but we have searched in vain for any notice of Mactear's sulphur regeneration process. The ammonia process for making sodic carbonate is briefly described, but we are not informed whether it is successful or not. If, as has been stated, the Island of Cyprus contains innumerable salt-springs, this process may play an important part in the regeneration of our latest possession.

Under Iron, the description of Danks's method of puddling by machinery; and short accounts are given of Crampton's and Henderson's processes. The account of Siemens's process is also enlarged—a remark that equally applies to the Bessemer process.

As in the fifth edition, Dr. Frankland's constitutional formulæ are introduced between brackets, to show that they are interpolations into the original work. We are at a loss to see the necessity for this. Dr. Frankland's formulæ are far from being universally used by teachers, who find them cumbersome in use, and more apt to lead to blunders on the part of stupid students than the ordinary empirical formulæ. At present we really know little or nothing about the constitution of bodies, and in our opinion it would be better humbly to acknowledge our ignorance by giving up the use of more or less improbable constitutional formulæ in our text-books.

Flowers; their Origin, Shapes, Perfumes, and Colours. By J. E. Taylor, Ph.D., F.L.S., F.G.S., &c. London: Hardwicke and Bogue.

We have here not indeed a record of original research, or a formal treatise on phytology intended for the professed student of vegetable life, but an attempt to "place before that portion of the intelligent public who have the desire, but neither the time nor opportunity to make themselves acquainted with natural science, the charming and suggestive results of modern botanical investigation." To such a task the author brings unusual qualifications. An able and enthusiastic botanist, and fully acquainted with all the recent discoveries in natural history, we might well expect from his pen a work at once readable, thoughtful, and suggestive. Nor shall we be disappointed. The volume before us teems with interesting facts, described for the most part in clear language, and made more telling by the aid of numerous and well-selected illustrations. The reader who may feel disinclined to wade through original memoirs will find here, for instance, an account of the newly-discovered carnivorous plants, the wondrous story of the mutual relations between insects and flowers, and of the uses of colour and perfume to the latter. He will learn here something of the wonderful contrivance to prevent exclusive in-breeding, and secure occasional, if not frequent, crosses. He will find for what purpose and in what manner plants climb. Passing from individual phenomena to general truths, he will learn to take a broader and healthier view of the vegetable—as indeed of the whole organic—world than to consider it existing with exclusive reference to man.

Still we must own to a certain amount of disappointment in the perusal of Dr. Taylor's book. There is, in the first place, a considerable amount of needless and wearisome repetition. Thus the distinctive attributes of the two great groups of flowers, the anemophilous and entomophilous, are explained twice over, on pages 15 and 149. The relations between flowers and insects are touched upon in almost the same words on pages 29 and 148. On page 15 we read—"Every cottager who has hung the gaudy-coloured paper fly-cages in his room, to prevent his clean white-washed roofs and walls from being dirtied by the common house-fly, has practically availed himself of the attraction which bright colours have for even these non-flower-loving insects." On page 174 the same idea recurs in almost the same words:—"In the brilliantly coloured paper fly-cages which he has been in the habit of hanging from the roof of his cottage, to save its clean white-wash from soiling, the cottager has practically taken advantage of the attractions which colour has even for the Diptera."

There are also certain peculiarities of expression which a little care in the revision of the press might have prevented. Thus in one passage we are told that "spectroscopic analysis has made it (the nebular hypothesis) more probably truthful than ever." Elsewhere there is reference to "a paragraph which Brongniart *made* in 1849;" and on page 184 we read—"Both them and the jonquils are remarkable for having a corona or tube within the flower."

As regards the subject-matter of the work before us, there are also certain points on which issue might be joined. Thus the author (p. 299) ranks humming-birds "among the latest products of Evolution." Mr. Wallace, on the other hand,* holds that "their extreme isolation from other forms, no less than the abundance and variety of their generic and specific forms, clearly point to a very high antiquity."

On page 19 we read that the *Sphinxes* (Sphinges) or hawk-moths "have not only long probosces, but slender bodies and hawk-like wings." To us the bodies of the hawk-moths appear thick in comparison with those of the generality of Lepidoptera.

The author, with many naturalists, considers that the bright colours of certain berries, &c., are designed to induce birds to partake of them. "It may be found that the colours of our wild berries are in this way as serviceable to the plants in obtaining for them distribution of bird-agency as those of their flowers have been in attracting those insects by whose aid these very seeds have been produced. . . . It may be that the very reason why the coloured succulent berries of some plants are uneatable by or poisonous to other animals than birds is a gain to them, in preventing their being swallowed by creatures in whose stomachs the seeds would be digested and assimilated. Singularly enough, with the increased size of our garden fruits produced through cultivation, there has often been developed a

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* Tropical Nature, p. 148.

different habit on the part of our wild birds. They no longer swallow the cherries and plums whole, and so they content themselves by eating away all the juicy pericarp, leaving the stones hanging by the footstalks." This we may, indeed, see on every cherry-tree, but we may see it also on the wild cherry. It is not swallowed whole, but its small layer of pulp is carefully picked away from the stone. We must also remember that many Mammalia void the seeds of fruits undigested.

It is a curious fact that brilliant colouration in caterpillars and other insects should so often be a warning to birds that the species is not edible, whilst in fruits it is an invitation to a banquet. There is only one further passage to which we have space to advert. "The Romish Church," says our author, "authoritatively denounced the Copernical theory of the Universe, just as she has recently denounced Darwinism." Now that many clergymen of the Catholic Church have expressed themselves hostile to the doctrine of Evolution is indisputable. In this respect they have not been singular. Clergymen of the Church of England, ministers of the Scottish Church and of dissenting communities, have done the same. But that any formal and authoritative condemnation of "Darwinism" has been pronounced by the Holy See we have yet to learn.

Dr. Taylor's work is provided with an elaborate table of contents and an excellent index.

Annual Record of Science and Industry for 1877. Edited by SPENCER F. BAIRD. New York: Harper Brothers. London: Trübner and Co.

THIS useful work has undergone a change of plan. The abstracts from scientific journals and from the Transactions of Societies, which formerly constituted its main feature, has been omitted, in consequence of the rapidly increasing quantity of scientific work done in the world, and in its place the summaries of progress of each science have been extended.

In biology no very striking advance during the past year is here recorded. Mr. Scudder, though from a different point of view, joins Mr. A. R. Wallace in objecting to the doctrine of Sexual Selection." Whilst admitting with Mr. Darwin that where the sexes differ in colouration the male is most beautiful, he contends that it is the female who "first departs from the normal type of colouring of the group to which the species belongs." He recalls no case where such a departure can be traced in the male alone. He points out that the males of certain butterflies possess peculiar cells, to which he gives the name of *androconia*. These are of great beauty and delicacy, but are hidden among the others. Here, the compiler remarks, "the theory of Sexual Selection proposed by Darwin appears to fail just where it should aid us most." We certainly cannot contend that Sexual Selection can account for ornamentation in an invisible part. But as little will the doctrine of the old school of natural history—that beauty in plants and animals exists merely for the delectation of man—serve our purpose here.

Mr. Packard deprived a number of insects of all orders of their antennæ. He found that the hive-bee was more affected than any of the others operated upon. "The removal of the antennæ of this insect seemed to show that the sense of hearing may reside in the antennæ, while that of smell has its seat in the palpi (and perhaps the tongue) alone. It would also seem as if the antennal nerves were so continuous with the brain (supra-œsophagical ganglia) that they form as it were a part of it, their removal at a little distance from their origin producing such a shock to the ganglionic nervous system that the insect acts somewhat like a bird when deprived of its central hemispheres. In an ichneumon the sense of taste appears to be situated in the ends of the palpi. In the butterflies the sense of taste, as well as touch, is situated in the spiral tongue. Spiders, on losing their maxillary palpi, seemed to be affected much as insects on the loss of their antennæ."

In view of the great shock to the nervous system here admitted we should feel somewhat sceptical as to the value of the indications afforded by such a method of experimentation. Our own observations—made on presenting to insects a great variety of bodies, attractive and repulsive—lead us to the conclusion that the antennæ are the organs of smell. The following experiments, performed by Père Montrousier, of New Caledonia, supports the same view:—He coated a weevil, *Octhorinus cruciatus*, over with wax, save the tips of the antennæ. On presenting to it oil of turpentine it was much agitated, and endeavoured to escape. Another specimen, on the contrary, had merely the tips of its antennæ coated with wax, and it remained perfectly indifferent to all strong-smelling substances.

Dr. Hartlaub's researches on the birds of Madagascar strengthen the case of those who consider that island too distinct in its fauna from the adjoining mainland to be regarded merely as a sub-region of the Ethiopian region. In Madagascar there occurs not one of the forms most characteristic of Africa.

Sir John Lubbock, in his latest experiments, finds that ants recognise old companions, and receive them amicably, even after a year's separation, while strangers are almost invariably attacked.

Prof. A. Gray thus states "Nature's golden rule for flowers: get fertilised; cross-fertilised if you can; self-fertilised if you must."

A Science Primer. On the Nature of Things. By J. G. Macvicar, A.M., LL.D., D.D. Edinburgh and London: W. Blackwood and Sons.

Dr. Macvicar uses the word "Primer" in a somewhat peculiar sense. It is generally taken to signify a plain, simple, elementary treatise which a mere tyro may comprehend, and which, passing over controversial matters, deals merely with what is fully established and generally recognised. From all this the "Primer" before us differs widely. It contains much which we, after a life devoted to science, find ourselves unable to understand clearly; much that is, to say the least, doubtful, and much that sadly lacks demonstration. The very methods of science are, in our opinion, completely ignored, and the results seem to us indefinite and intangible. The author tells us that his work is "grounded on the belief of [in ?] an Almighty Being possessing unity, omnipresence, and ever-blessedness, and awarding existence to a creation for the sake of manifesting Himself and extending blessedness beyond Himself, and, in a word, to be a mirror of Himself so far as the finite can bear a likeness to the Infinite. After setting out with this cosmical law of *assimilation*, by its aid alone bearing on only one kind of created substance or energy ('mind-stuff'), he deduces the creation of the world of Spirits, and as their home the Universal Ether or medium of light. Then, as a beautiful cloud-work in the azure of the Spirit World, he gives the genesis of Matter and the molecular system, culminating in this planet in the construction of the myo-cerebral organism, whose characteristic function is to construct a powerfnl tissue of organised ether or the matter of light, which being unified in its focus of vital action into an element of energy so powerful as to have recovered the primal attribute of energy—namely, mental power—is a spirit." In consideration for scientific men he advises them to turn first to the latter part of the work, where he thinks that he has verified his theory "by a detailed appeal to natural phenomena, and experiments in physics and chemistry." Thither accordingly we proceed, in the hope of finding a key to what has gone before. But on arriving at our destination we find experimental evidence is wanting. There are assertions, but they lack demonstration. We read, p. 65, "Thus with regard to silica itself, its molecular structure, as obtained by our method, shows that it is not destined to remain for ever dead, giving to nature nothing but barren rock and sand. Every atom of it is capable of development into coupled atoms of oxygen and carbon, and a tetratom of hydrogen. This the actual chemistry must assent to, thus far at least as to admit that their atomic weights are the same.

$$\text{“ Silica, at weight } 60=\left\{\begin{matrix}2O=32\\2C=24\\4H=\ \ 4\end{matrix}\right\}=60.\text{”}$$

We will not here object that the atomic weight of oxygen is considered to be not 16, but 15·96, and that of carbon not 12, but 11·97. But we must certainly enquire whether Dr. Macvicar assumes it as a general truth that if the atomic weight of an element equals the sum of the atomic weights of certain other elements, it is to be considered as capable of resolution into the latter? We shall be ready to accept the author's view of the composition of silica so soon as he shall have succeeded in resolving it into oxygen, carbon, and hydrogen, or in forming it synthetically by the union of these elements.

Concerning ammonia the author entertains also peculiar views. He writes:—"The experimental chemist experiences no greater difficulty in his pursuit of exact analysis than to obtain possession of a considerable quantity of mere or pure water. Let him distil water over and over again, if only the distillate have stood for a time he finds ammonia in it. This phenomenon the popular chemistry which maintains the solidity, simplicity, and non-developable character of the sixty-three chemical atoms, is obliged to ascribe to the previous existence of organic matter in the water." This state of things, which he thinks "cannot but be provoking to the chemist," he accounts for by pronouncing ammonia "a product of pure water itself, and immediately after common vapour itself a primeval substance." Before Dr. Macvicar exults over the "popular chemistry" let him remember that ammonia is not difficult to detect in the atmosphere, and cannot well escape being absorbed by water on prolonged standing, even if preserved in bottles with the best-fitting stoppers. Can he bring forward a case where a competent experimentalist has obtained water free from ammonia and from organic matter, has then sealed it up before the lamp, and has then on opening it—after the lapse, say, of a year—found it ammoniacal? If the author can make ammonia out of water, not merely honour will be his, but wealth such as the world has not yet dreamt of. Will he neither tell us how it is to be done, nor, if that is impossible, point out where Nature is doing it in "a considerable quantity"?

It will have been perceived that Dr. Macvicar does not recognise the ordinary elements. He says—"There are between sixty and seventy (kinds of) particles of matter which chemists down to the present day have not succeeded in decomposing, and which, strange to say, on this negative evidence they have concluded to differ from all the thousands of other (kinds of) particles which in the progress of analytical chemistry they have succeeded in decomposing, and hold to be simple and solid unities or true natural atoms of matter."

The author must surely be aware that the definition of an element, in chemical language, is merely a body which we have neither been able to compose nor to decompose. "Strange to say" he goes on to show why these elements should "differ from all the thousands of other particles" which have been decomposed. "Surely," he exclaims, "it is most improbable that human creatures in this small planet should be able to accomplish in their chambers the destruction of particles on which the economy of Nature out in the Universe has mainly been devolved." Here, then, is a formal admission that these elements are after all naturally distinguished from their compounds.

Our chemical readers, however, will not thank us for further pursuing the examination of a book which is rarely tangible, and which formally discourages the appeal to experiment, holding up the early physicists of Greece as examples, and recommending us to examine facts with the mental eye rather than with that of the body. Such works appear from time to time, written generally by men who have little practical acquaintance with their subject, and pass away without having contributed anything to the stock of human knowledge or to our means of research.

A Practical Treatise on the Steam-Engine. By ARTHUR RIGG. London: E. and F. N. Spon. 1878.

THIS work is exactly what is described by its title. Written in an easy style, and with an avoidance of too much technicality, its contents may be readily understood by the young student or the mechanic, to whom it appears to be addressed rather than to the more scientific investigator of theories, and yet its pages may with advantage be consulted by the latter, for it is the result of careful and conscientious practice based upon scientific reasoning and knowledge. In the commencement the different measurements required in the constructive dimensions of the steam-engine are considered, and it is shown how, for this purpose, the English units of the foot and inch are better adapted than the French metre. Following this, the laws which govern matter in its various forms and motions are investigated under the different headings of Velocity, Momentum, Work, Power, and Centrifugal Force, each of which is carefully but clearly explained.

Having thus disposed of the definitions connected with the main subject, we are next introduced to the horizontal steam-engine, with which, Mr. Rigg truly says, for compactness and easy accessibility to all parts no other type of steam-engine can compare. There are, however, different classes of these engines made, some for sale, others for work, and it is to the latter only —in which the object of the designer has been to apply the discoveries of Science, and practice of experience, to means for the most economical production of power—that the attention of the reader is directed. In this chapter the details of all the principal parts are explained, and freely illustrated by numerous plates. Then follow particulars of the construction of the more important parts, including pattern-making, moulding, and casting, for the cylinder and cylinder-cover, in which the proper allowance for contraction on cooling is considered. The piston and piston-rod, and the proper method of their construction, are also explained, and the slide valve with its connected parts receive a share of attention commensurate with their importance. Then follow explanations regarding the other parts of the engine, which need not be detailed here; and indicators and indicator-diagrams are next discussed and explained, and after this the influence of the velocity of reciprocating parts of steam-engines.

The concluding chapter consists of a brief *résumé* of the principal theoretical considerations in the modern science of thermodynamics which have a direct bearing on the many points, referred to in early chapters, in the construction of steam-engines. Here the mechanical equivalent of heat is considered but briefly, and we cannot avoid an expression of opinion that this subject might with advantage have been more fully dwelt upon; it is, however, dismissed in two pages, and, in fact, the most important questions connected with it are not even touched upon. It is true that of this chapter it is said, in the Preface, that it may almost be considered as an Appendix, for that it is merely intended as an outline of the views generally received among Engineers at the present time on the relationship which exists between heat and work; but we should have preferred this portion of the Appendix being more fully explained. A greater amount of space is given to the explanation of Isothermal and Adiabatic Lines; and a few brief remarks on the amount of expansion that can be made practically useful bring the book to an end.

That important addition to all works likely to be of use for purposes of reference, viz., a good index, contributes in no small degree to the value of the present work. The illustrations, of which there are ninety-six plates, besides numerous woodcuts interspersed with the text, are well drawn and carefully printed. Indeed the whole volume, from beginning to end, is got up in a style quite in keeping with the character of the work itself, which leaves little to be desired; and on the whole it may be safely stated that it embodies a fair and carefully drawn record of the best practice as it exists at the present day, so far as fixed engines are concerned. The subject of locomotives and marine engines has, however, been purposely omitted, for the reason, as is explained, that they have already been pretty fully discussed in other works.

Sanitary Engineering. A Guide to the Construction of Works of Sewerage and House Drainage, with Tables for facilitating the Calculations of the Engineer. By Baldwin Latham, C.E., F.G.S., F.M.S., &c. Second Edition. London and New York: E. and F. N. Spon.

Perhaps the greatest difficulty in the way of the sanitary regeneration of a city, a district, or an entire country, lies in the fact that it requires the co-operation of a number of distinct professions, none of which can be safely dispensed with. The chemist has to investigate the composition of polluted waters, to point out in what respect and in how far they differ from a natural standard, and to adjudicate on the efficiency of the means or processes proposed for their purification. The medical practitioner is required to trace out the effects upon public health of impure air and water, whether the latter be used in diet or merely allowed to exist beneath and near human habitations. The practical agriculturist must pronounce on value of the schemes put forward for the utilisation of sewage. Passing over the functions of the jurist and the statistician, we come to the duties of the engineer, which though last are assuredly not least. He has to devise and carry out methods both for introducing pure water into our cities and villages in the most efficient manner, and without becoming contaminated on the way, and, on the other hand, to remove from the vicinity of our dwellings all polluted liquids, so that they may have the least opportunity of diffusing contamination. This involves the construction of water-works, the laying down sewers and drains, with all their accessories, such as ventilators, traps, &c. There is, curiously enough, one engineering problem in connection with this subject the solution of which we do not remember to have seen even attempted. This is—Suppose a given flow of sewage is treated by any precipitation process, to find the shape and size of tanks in which the precipitated matter shall be most quickly and completely separated from the effluent water.

Mr. Baldwin Latham's work contains some very curious information on sanitary laws and customs in antiquity. It is salutary, though perhaps humiliating, to learn that in this boastful nineteenth century we are scarcely more advanced in practical hygiene than were our predecessors some three thousand years ago. Rome was probably better supplied with water than any city of the present day, and its citizens would scarcely have erected statues in honour of the inventor of monopolist water-companies. The author ascribes the worship paid by the Ancient Egyptians to the Scarabæus, or "Sacred dung-beetle" (*Ateuchus sacer*), to their acquaintance with its sanitary services.* On the same principle it might perhaps be argued that

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* See Quarterly Journal of Science, vol. vi., p. 163.

the Ancient Hebrews, in dedicating flies to the Evil One, were influenced by a knowledge of their function as disseminators of disease and death. We learn that sewage-irrigation was practised in Jerusalem. The blood of the sacrifices offered in the Temple, and doubtless the other nuisances of the city, were flushed down with abundant water into the Valley of the Kedron, and were there received in subsidence-tanks. The solid matter was collected and sold for manure, and the effluent used for irrigation. It must be remembered that in a climate like that of Judæa, where the rainfall is scanty and the evaporation excessive, a supply of water is often the one thing needed to convert a desert into a garden. The water-closet, which is often considered as a modern and an English invention, and is often spoken of in France as "cabinet Anglais," is traced back to Asia. "They were introduced into Rome during the Republic, and are noticed by several ancient writers. Those constructed in the palace of the Cæsars were adorned with marbles, arabesques, and mosaics. At the back of one still extant there is a cistern the water of which is distributed by cocks to different seats." In Ogilby's "Africa," a work published in 1670, these are described as being numerous in the city of Fez. Sir John Harrington introduced them into this country in the reign of Elizabeth, but, according to M. Roubo, they had been long used in France before becoming known in England. As a contrast we find that in our own days, in the sixteen years ending in 1862, fifty-one cesspools in Windsor Castle were abolished. These *foci* of disease and nuisance within the ancient palace of our monarchs varied in size from 12 feet by 8, and 9 feet in depth, to 3 feet in diameter by 6 feet in depth.

But indeed we are only beginning to throw off the heritage of filth handed down to us from the Dark Ages. Mr. Latham may perhaps exaggerate when he tells us that "for a thousand years there was not a man or a woman in Europe that ever took a bath." But it is certain that for a long time cleanliness and heresy were deemed to be closely connected. Even in our own days, in the south-eastern and south-western peninsulas of Europe, dirt is regarded as the characteristic privilege of the Christian as compared with the Turk or the Moor.

Among other subjects Mr. Latham discusses the vexed question of a single or a double system of drainage. He points out that the surface-water of our cities is almost, if not quite, as impure as sewage properly so-called. He shows that if the rainfall is entirely excluded from the sewers, the latter will require flushing. But, as the best arrangement, he seems to recommend "a system of sewerage combining the admission of small amounts of rainfall into the intercepting sewers of a town, while in time of heavy rainfall the comparatively pure surface-water should flow on to its natural outlet." The great arguments in favour of this plan are that it restores to the streams water that is much needed, and greatly facilitates every process for the treatment of the sewage. One of the most telling arguments against irrigation, as ordinarily practised, is that in seasons of heavy rain, when the soil is least able to bear any additional quantity of water, the most is forced upon it by the swollen sewers. Where Mr. Latham's plan, or any similar scheme, is adopted, this objection falls away. Sewage-precipitation works are also much embarrassed by floods when the deposit is in danger of being swept out of the tanks and carried into the rivers.

The chapter on the sanitary appliances of a district, as affecting the quality and quantity of the sewage, contains some statements with which we cannot agree. That "in the sewage of a midden-stead town there is a larger proportion of urine present in a given volume of sewage than is found in a water-closet town" is simply impossible, and the "excess of chlorine," upon which so hazardous an assertion is built, is therefore no sure guide.

A large proportion of the matter in this book, though of little interest to the general reader, is certain to be most valuable to the engineering profession and to municipal authorities, who will find it a priceless manual of reference.

The Steam-Engine considered as a Heat-Engine. By JAMES H. COTTERILL, M.A., Professor of Applied Mechanics in the Royal Naval College. London: E. and F. N. Spon. 1878.

THIS work is represented, in the Preface, as a second edition of some Notes on the Theory of the Steam-Engine, published in 1871. It has, however, been so supplemented and added to as most considerably to enhance its value. The object of the present book is stated to be to study the process of the conversion of heat into work in steam-engines; and the experiments of Regnault, Raukme, and others, are not only repeatedly verified, but also worked out to their natural conclusions, aided by the further experience which has, since their days, been obtained on the subject.

In a chapter on the physical properties of steam it is explained that the "total heat of evaporation of water is the quantity of heat requisite to raise a pound of water from 32° to a particular temperature, and evaporate it at that temperature, while the latent heat of evaporation of water is the quantity of heat requisite to evaporate a pound of water at a given temperature." This latent heat diminishes as the temperature increases, approximately by rather more than seven-tenths of a thermal unit for each degree of Fahrenheit; this rate of diminution is, however, not exactly constant, but increases with the temperature. The density of steam is not at present exactly ascertained, but the general results of experiments show that the weight of a cubic foot increases nearly in proportion to the pressure, but at a somewhat slower rate.

In the next chapter, on the convertibility of heat and work, it is asserted that "energy when exerted is not destroyed, but simply transferred from one body to another." It is shown, however, what a very small proportion of the heat expended in the process of evaporation produces an equivalent of work,—hardly one-twelfth,—the greater part of the heat having been employed in producing changes within the water itself, in overcoming the molecular cohesion of its particles, which resists its conversion into steam. The heat expended is thus converted into two classes of work, viz., external work, or that which is appreciable to the senses, and internal work. In applying the foregoing reasonings to the steam-engine it is demonstrated that, in a non-expansive engine, only from 5½ to 7½ per cent of the heat expended is converted into useful work, but that the efficiency may be more than doubled by expanding the steam five times in the cylinder, and still further increased, though only to a small extent, by a further increase in the expansion.

Chapter IV. deals with physical properties of the permanent gases, in which the theory of a heat-engine worked with a *perfect gas*, under conditions of maximum efficiency, is considered, and it is explained that under certain conditions 56 per cent of the heat expended may be transformed into mechanical energy. We have next a consideration of the circumstances of "a perfect heat-engine," and here we find that "in the best possible steam-engine, unless the steam be superheated considerably, at least two-thirds of the whole heat expended is wasted, the waste arising from no fault in the construction or nature of the engine, but solely from the narrow limits of temperature within which we are restricted to work. Again, the consumption of steam in a perfect steam-engine is much less than that of an actual engine under the same circumstances, showing that faults in the construction of the engine or the treatment of the steam must exist, which, at least theoretically, are remediable."

The expansion of steam, the effect on engines receiving heat at varying temperature, the consequences of "clearance" and "wire-drawing," and the action of the sides of the cylinder and of water remaining after exhaust, are dealt with at some length in the succeeding chapters, whilst the last one is devoted to the application of the theory of the steam-engine, already discussed, in further detail to the working of steam-engines in practice. Here we learn that "Experience appears to show that 18 lbs. of steam per I.H.P. per hour is about the minimum consumption of steam commonly reached in actual engines; and the preceding calculations indicate that, in condensing engines, this is equivalent to saying that about 13 per cent of the whole heat expended may be turned into mechanical work, or about 50 per cent of the heat which would be turned into work in a theoretically perfect engine working between the same limits of temperature." The average result, however, even in economically worked engines, is much less,

We have now, it is hoped, given sufficient particulars of the contents of this book to show its great value as a work calculated to lead the thinking mind to a study of the more important theories and principles connected with the steam-engine. Much more might have been said here, but the space available for a review is necessarily limited, and we can only now conclude this brief notice by strongly recommending the work before us to all more advanced students of steam and of the steam-engine.

Improvements in certain Iron-work. By Messrs. HOOPES and TOWNSEND.

THIS little work not being provided with a title, we have been obliged to provide it with one of our own invention. It is a notice of the trade specialities of the firm from whom it proceeds—such as "key-stone" boiler-rivets, cold punched nuts, &c. It contains a Report, by Prof. R. H. Thurston, on the results of experiments performed at the Stevens Institute of Technology, in order to decide the comparative resisting power of "cold-punched" and "hot-pressed" nuts. The former, according to the figures quoted, showed a marked superiority.

The Speaking Telephone and Talking Phonograph, and other Novelties. By GEORGE B. PRESCOTT. Fully illustrated. New York: D. Appleton and Co. 1878.

MR. PRESCOTT is already favourably known, both in this country and America, as the author of an excellent Manual of Electricity and Telegraphy: but we are sorry to say that the present work, although valuable in its way as a record of facts, is not calculated to add to his reputation as a scientific writer. It consists, for the most part, of verbatim reprints of most of the papers on telephony and kindred subjects which have been read before the different scientific societies of England and America during the last two or three years, loosely strung together, and interspersed with a few remarks from the author. These papers are all printed in the same type as the original matter, and as inverted commas are placed only at the beginning and end of each of them, it is often difficult to know whether it is Mr. Prescott or some one else that is speaking in the first person without referring back several pages. The very Introduction itself is confused by the reprint of a speech delivered by one of the counsel at a recent telegraphic trial in New York. It is a very meagre account of the history of electrical discovery, which we cannot agree with Mr. Prescott in thinking either "interesting" or "valuable," seeing that exactly fourteen words are devoted to Faraday's discoveries in magnetic induction.

The first chapter is original, and gives us a clear and succinct account of the principles of sound; Reiss's musical telephone, and Gray's improvements on it; Gray's, Bell's, and Dolbear's articulating telephones; and Phelps's duplex telephone—the latter a very beautiful piece of apparatus, as far beyond Bell's trumpet-shaped instrument as a modern Westley Richard's central fire breech-loader is beyond a Joe Manton. Edison's carbon disk telephone is also described, but the book appears to have been published too early in the year to have included any account of the microphone.

Chapter II. consists of the paper read by Prof. Bell, in October last, before the Society of Telegraphic Engineers, with a few notes by Mr. Prescott. Chapter III. is made up of articles from the "Westminster Review," "Engineering," and "Chambers's Journal. Chapter IV. is a hotch-potch from various sources—such as "Silliman's Journal," "Poggendorf's Annalen," De la Rive's "Electricité," &c.—on the production of sounds by electricity. Chapter V. consists of Mr. Elisha Gray's papers on Telephonic Researches, read before the American Electrical Society or contributed to their Transactions. At the end we find both Mr. Gray's and Prof. Bell's original specifications, as filed by them at the New York Patent Office on the same day, *i.e.*, February 16, 1876.

Chapter VI. contains an account of Mr. T. A. Edison's telephonic researches, from his own pen, giving a most interesting account of his various discoveries and inventions, apparently written expressly for Mr. Prescott's work. Chapter VII. is a history of electro-harmonic telegraphy, read by Mr. F. L. Pope before the American Electrical Society in December last. Chapter VIII. consists of an abstract from a paper by Professor Dolbear, entitled "Researches in Telephony," in which he definitely claims to have been the first person to use a permanent magnet for vibrating the disk of a telephone.

In Chapter IX. Mr. Prescott once more resumes his task, and lucidly describes the improvements made in telephonic instruments by Messrs. Blake, Peirce, Channing, and others, in which he claims for the latter physicist the invention of the first portable telephone. Chapter X. is on the Talking Phonograph, and is almost entirely written by Mr. Prescott. The prophecies as to what Mr. Edison's invention will accomplish in the future, and the extract from a particularly popular article in "Scribner's Monthly," are hardly worthy of a place in a serious scientific book.

As a piece of scientific exposition Chapter XI., on Quadruplex Telegraphy, is undoubtedly the best in the book, and is a tantalising proof of Mr. Prescott's ability to treat of such subjects. It is certainly the most thorough description of the wonderful improvements which have lately been made in multiplex telegraphy which we have yet met with.

Electric call-bells and the electric light receive attention in the two last chapters of the book. The last subject might, however, have been treated of at greater length, seeing the attention it is attracting in London and Paris. We should have been glad, for instance, to have heard more of Mr. Farmer's method of obtaining a number of lights from a single source, by using thin strips of platinum or indium, and raising their temperatures to a point slightly below that of melting.

The Index is far too small for a work containing so much matter.

In the chapters which are due to Mr. Prescott's pen alone, he proves to us with what lucidity he can treat a scientific subject.

The work contains over two hundred woodcuts, most of them of great beauty, and many of them far beyond anything that has yet been accomplished by our English wood-engravers in the way of scientific illustrations. Mr. Prescott also adopts the excellent plan of repeating a cut whenever it is necessary, instead of worrying the reader by frequent cross references. The general "get up" of the book is fully worthy the reputation of the firm by which it is issued.

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