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THURSDAY, SEPTEMBER 22, 1870

THE GOVERNMENT AND THE ECLIPSE EXPEDITION

SEVERAL of the correspondents who have been lately so busily engaged in chronicling the scenes of the present terrible war have wound up their descriptions by the remark that they could scarcely get rid of the impression that they have been all the time the sport of horrid dreams—that the carnage has existed only in their imagination, and that civilised Europe has been uninterruptedly pursuing the arts of peace.

Similarly it is difficult for an English man of science, proud of his nation's past achievements in many of the most noble problems, and among them those which have necessitated voyages to various parts of the world—now to witness a transit of Venus—now to make magnetic observations—now to survey the Polar Regions—and now to observe an eclipse of the sun ; to realise the fact that in this present year of grace, 1870, there exists a British Government which in the curtest possible manner has put a stop to an expedition which promised to add as much to our renown in the present as the expeditions to which we have referred, and many others that we might have mentioned, have done in the past.

The proposed expedition now in question had for its object the observation of the approaching Eclipse of the Sun. A brief statement will suffice to show its importance, and how naturally it followed other similar expeditions.

It is now more than a century and a half ago since certain things called *red flames* were seen round the edge of the eclipsed sun ; another appearance called the *corona*, sometimes of great irregularity and magnitude, having been noticed from the highest antiquity. Time went on, however, till only as lately as ten years ago a British expedition to Spain determined, to the satisfaction of everybody, that these red flames belonged to the sun, and not to the moon, as had been suggested. Here, then, was one great point gained by the help of the British Government—other European Governments, let us add, assisting. This was in 1860. In 1868 there was another eclipse, this time in India. The Indian side of the Government nobly assisted again by telling off officers, granting money, and affording the use of a ship to a distinguished Frenchman who chose India for his standpoint of observation, the French and German Government expeditions preferring other stations. It is seen, therefore, that the work done at the eclipse of 1868 was Government work, and it was admirable. The nature of the red flames, which had been localised in 1860, was now roughly determined.

There was still another eclipse in 1869, visible in America. Here Government help was not needed at all, as the eclipse swept right over the land ; *still it was nobly given*, and one of the most important recent contributions to astronomy is the American Government Report, of the observations made by its officers. The American observers fairly broke ground in another branch of the research, the nature—not of the red flames this time but—of the corona ; but they did not settle the question, and it now remained for astronomers to "crown the edifice" of all our eclipse work by settling it at the eclipse next December.

Thanks to the Janssen-Lockyer discovery, we can now study the red flames day by day ; and, although the corona resists the new method, the discovery, taken in connection with Frankland and Lockyer's observations of the hydrogen spectrum, has yet thrown much light on its possible nature, so that the new method makes observations next December more precious.

Having shown, then, both the importance of the observations of the next eclipse, and the fact that they are the natural sequel of the work which has been done by Government aid, and which, it is not too much to say, could not have been done without it, let us next state some facts with regard to the proposed Expedition. It was first proposed by the council of the Royal Astronomical Society, who appointed a committee of council to take the necessary steps. The Royal Society followed suit, and next a joint committee was formed. To this joint committee were committed the whole of the arrangements. Here, shortly, is the programme agreed upon :—

1. At least two expeditions would be necessary—one to Spain, the other to Sicily.
2. At least 1,000*l*. would be required for instruments and observatory-building expenses, &c.
3. The presidents of the Royal and Royal Astronomical Societies and the Astronomer Royal were to form a deputation to the Government to ask for two ships and 500*l*., the remainder being subscribed by the societies themselves.
4. The observers were to be carefully organised, and competent persons placed in charge of each branch of the research in each expedition.

It is clear from this that our scientific bodies have done all in their power.

Unofficial *pourparlers* having led to the belief that the Government would not be found wanting on its part, those interested in the work spared no pains to organise the expedition as minutely as possible, and all the best observers came forward to make it second to none of its predecessors.

Here is the official list of those who were prepared to take part in the observations :—

Spectroscopy.—Mr. Lockyer, Dr. Gladstone, Mr. Buckingham, Lieut. Brown, Mr. T. W. Backhouse, Lieut. Collins, Rev. A. W. Deey, Lieut. Davies, Mr. G. Griffith, Mr. W. B. Gibbs, Rev. H. A. Goodwin, Mr. S. Hunter, Mr. W. A. Harris, Rev. F. Howlett, Mr. W. Ladd, Capt. J. P. Maclear, Rev. S. J. Perry, Mr. A. C. Ranyard, Mr. G. M. Seabroke, Mr. H. Tomlinson, Mr. Pedler.

Polarisation.—Prof. Pritchard, Mr. R. Abbatt, Mr. Bushell, Mr. Chambers, Mr. G. Griffith, Mr. W. B. Gibbs, Rev. F. Howlett, Mr. W. Ladd.

Photography.—Mr. Browning, Mr. Brothers, Lieut. Abney, Mr. Buckingham, Lieut. Brown, Mr. Chambers, Mr. Clodd, Mr. R. Sedgfield.

General Objects.—Mr. Lassell, Col. Strange, Mr. Dallmeyer, Mr. J. Bonomi, Mr. E. J. Lowe, Prof. J. Phillips, Prof. H. E. Roscoe, Mr. G. J. Stoney, Mr. C. G. Talmage, Mr. C. B. Vignoles.

General Assistance.—Mr. R. Abbay, Mr. R. Inwards, Lord Lindsay, Mr. W. J. Lewis, Rev. W. Monk, Mr. R.

S. Newall, Mr. L. B. Phillips, Mr. W. Pole, Mr. F. C. Penrose, Prof. Tyndall, Mr. R. Webster, Mr. J. E. Backhouse, Mr. E. E. Bowen, Col. Drayson, Admiral Ommanney, Mr. Thos. Slater, Mr. P. E. Sewell, Mr. W. Rossiter, Capt. Noble, Mr. W. K. Clifford, Mr. W. H. H. Hudson.

Here was a rich promise of a victorious campaign, and the scientific world already congratulated itself on being able at last to "settle the corona," when suddenly, as a bolt out of the blue, came a letter from the Admiralty *declining even a single ship*, on the ground that such a purpose was entirely foreign to the purpose for which Parliament places funds at the disposal of the Naval Department.

We think we had better leave this astounding statement as it stands. It seems really as if the present Admiralty authorities are in absolute ignorance as to the real facts of the case; as to what England has done before; as to what precedents exist to which men of science can point.

Under these circumstances we trust that an appeal will be made to Mr. Gladstone, whose culture, wider than that of his more prominent colleagues, will at once grasp the huge Philistinism of this proceeding. Should he reverse their decision, which he may fairly do, on the mere ground that it is against all precedent, assuredly the scientific men of Britain will hail it as a happy omen—an indication that the hope experienced by Prof. Huxley at Liverpool the other day will, in time, be realised. If, on the other hand, the decision is to stand, it must be distinctly understood that, both in the judgment of our contemporaries and of posterity, it will, as has been already been pointed out in the daily press, bring shame upon the scientific repute of England, who now, with her forces all ready to achieve another victory over nature, is held back by "My Lords" for the sake of a few pounds sterling. Surely there is little hope for us if in such a campaign as this we are to succumb to a

> Lust of gold
> And love of a peace that is full of wrongs and shames;
> Horrible, monstrous! not to be told.

REPLY TO PROFESSOR HUXLEY'S INAUGURAL ADDRESS AT LIVERPOOL ON THE QUESTION OF THE ORIGIN OF LIFE

I.

SPEAKING with all the authority which years of earnest and successful labour have conferred, and, moreover, "from the elevation upon which the suffrages of his colleagues had for the time placed him," Prof. Huxley has just given us in his Inaugural Address, as President of the British Association for the Advancement of Science, a "history of the rise and progress of a single biological doctrine"—that first proclaimed by Francesco Redi, and to the effect that *Every living thing proceeds from a pre-existing living thing.*

However reluctant to enter a protest against what has been said by an eminent scientific man, for whom I have always entertained the greatest respect and esteem, I feel so strongly that the representations which have been made concerning a subject to which I have directed the most earnest attention for the last eighteen months, are not only inadequate, but altogether incapable of being regarded as an impartial statement of the main points at issue, that I cannot hesitate as to the propriety of publicly expressing this opinion.

Fearful, therefore, lest harm should be done to the cause of science by this address, through the great influence of the speaker, and mindful of the momentous issues which turn upon the proper solution of the question under discussion, I—sinking all personal feelings, risking all imputations, anxious only that the truth should be known—will venture to state what really seems to me to be the true aspect of the problem, and how far the remarks of Prof. Huxley really bear upon this, or have been, in other respects, not sufficiently explicit.

The doctrine, whose history Prof. Huxley professes to trace, and whose probable truth he thinks remains unshaken, has reference to a question which is of more fundamental importance than any other throughout the whole range of Biological science. It is either true that *all* living matter, without exception, comes into being in connection with pre-existing living matter, or else it is true that *some* living matter can arise from non-living materials free from all connection with pre-existing living matter. This alternative is one the full meaning of which may, perhaps, be realised better by putting another, which, though strictly analogous, is somewhat freer from mystery. It may, then, similarly be said, it is either true that *all* crystalline matter, without exception, comes into being in connection with pre-existing crystalline matter, or else it is true that *some* crystalline matter can arise from non-crystalline materials, free from all connection with pre-existing crystalline matter. Matter when it passes into the *crystalline* condition exhibits properties of a certain kind, and when it passes into the *living* condition it exhibits properties of another kind, to which we commonly apply the term "vital." Now the question in each case is, whether by mere concurrence of certain physical conditions, aiding and abetting the inherent properties of the matter itself, some kinds of matter can fall into modes of combination called *crystalline*, whilst other kinds are capable of falling into modes of combination called *living*; or whether, in each case, a pre-existing "germ" of the particular kind of matter is necessary, in order to determine, in suitable media, either of these modes of combination. Are we to believe that crystals can appear in no solution whatsoever without the pre-existence in that solution of certain crystalline germs,* and similarly that living things can arise in no solution whatsoever without the pre-existence in such solution of living germs? To many persons it may at first sight seem that there is no analogy between the two cases; such, however, is not the opinion of very many who are best entitled to speak on the subject.|| It is admitted by them that the analogy is of the closest description; and it is interesting to note that although the actual evidence which can be brought to bear upon these two questions is very similar in kind, and alike conflicting in nature, the generally received opinions as regards the proper answers to be given to these two questions have inclined to the view that, whilst it is possible for crystals to originate *de novo*, it is at present impossible for living things to originate after this fashion.

* It must not be supposed that this is a mere hypothetical case. On the subject of crystallisation generally in supersaturated solutions, I will quote the following passage from Watts' *Dictionary of Chemistry*, Vol. v., p. 349. :—"This sudden crystallisation, if not produced by cold, appears to depend essentially on contact of the solution with small solid, perhaps crystalline particles; for it is not produced by passing air previously purified by oil of vitriol through the solution, or by agitation with a glass rod previously purified from dust by ignition. *According to Violette and De Gernez, the sudden crystallisation is in all cases induced only by contact with a crystal of the same salt*, possessing the same form and degree of hydration as the crystals, which separate out; and in the case of those supersaturated solutions which crystallise suddenly on exposure to the air, it is due to the presence of minute particles of that salt floating in the air. From an experiment of De Gernez it appears that microscopic crystals of sodic sulphate may be obtained by passing air, even in the open country, through pure water, and evaporating the water on a glass plate. Jeannel, however, denies the necessity of contact with the salt actually contained in the solution. He finds, indeed, that a supersaturated solution of sodic acetate may be made to crystallise by contact with any solid substance (a piece of paper for example), and a solution of sodic tartrate by contact with a clean, dry, glass rod" Here, then, we have also a veritable "germ" controversy. I was informed, however, a few weeks ago by Prof. Frankland that even in the case of sodic sulphate it had lately been shown that, *under certain conditions*, crystallisation can certainly take place where no crystalline germ could possibly have existed. The "germ" theory of the origin of crystals in supersaturated solution, has, therefore, been overthrown. This has been possible, however, only because it has been more easy to show that a given set of conditions are inimical to the existence of a crystal, than it has yet been to induce people to believe that any given set of conditions are incompatible with the existence of living matter.

It is worthy of remark, however, that the germ controversy concerning crystals can only be settled in the minds of those who are content to accept the high probability that the properties of any *invisible* portions of crystalline matter would correspond with the properties which similar visible crystalline matter is known to display. It is this reluctance to admit an equally high probability in the case of living matter, which alone causes the sister controversy to continue. Otherwise the question would have been settled long ago.

† The analogy between the supposed possible origins of crystals and organisms in solutions has been rendered much more obvious since the discovery by the late Professor Graham, that when dissolved the saline substance does not remain as such in solution, but that the acid and the base exist separately, and are separable by a process of dialysis. When crystallisation takes place, therefore, we have a combination of materials taking place similar to, though simpler than, what may be presumed to take place in the genesis of a Living thing.

The question is one of much interest, and it may therefore well be asked why such a totally different verdict should have been given in two cases, the analogy of which is so remarkable. The reason is, however, not difficult to find. Mere theoretical considerations have been all-powerful in influencing the verdict, and in inducing those who are informed upon the subject to read the evidence in different ways. Living things manifest such complex properties that the whole notion of Life has been shrouded in mystery. Biologists at first could not bring themselves to believe—some cannot do so now—that the phenomena which living things manifest are absolutely dependent upon the properties of the variously organised matter entering into their composition. They were obliged to have recourse to some metaphysical entity—some "anima," "archæus," or "vital principle"—under whose directing influence the living form was supposed to be built up, and upon whose persisting influence many of the phenomena of Life were supposed to depend. The aid of no similar metaphysical "principle" has, however, been deemed necessary in order to account for crystalline structures and properties. It was in the main conceded by most physicists, and the doctrine remained unquestioned by biologists, that matter of certain kinds might, by virtue of its own inherent properties, aided by certain favouring circumstances—and quite independently of all pre-existing germs—fall into such modes of collocation as to give rise to crystals. But, owing to the influence of the theoretical considerations already mentioned concerning the nature of Life, a similar possibility could not easily be granted in reference to the origin of Living things. Was it not held that the living thing owed its structure or organisation to the active influence of a special and peculiar principle? This "vital principle" was neither ordinary matter nor ordinary force, neither was it in any way derivable from either of these; how then could it be supposed that the coming together of matter of any kind could give rise to a living thing?* The aggregate of properties, which we designate by the word "Life," were not supposed to be dependent upon, to be, in fact, properties of the material aggregate which constituted the Living thing. Life was presumed to be due to the manifestations of a something altogether peculiar—of a "vital principle," which was inseparable from living matter. Doctrines akin to these having been already proclaimed and disseminated by the influential teachings of Paracelsus, Van Helmont, and others, it cannot be a matter for surprise that the brilliant demonstrations of Redi should have had a great influence in their time. Observation after observation appeared now to confirm the existence of a seemingly universal mode of origin of Living things—a mode too which was more in harmony with the philosophical views of the day than that which had hitherto been deemed possible. Doubts, however, soon sprang up. New means of observation opened up new questions for solution. And what has been the result? Many battles have been fought, many victories have been won, and now the biological doctrines of the day have assumed an entirely new form. The ever-increasing strides of Science have wrought the most fundamental changes in our notions concerning Life. Under the influence of the well-established doctrine concerning Persistence of Force—and more especially since the clear recognition of the subordinate doctrine as to the Correlation existing between the Physical and Vital forces—physiologists have now begun to recognise, and most unhesitatingly to express the opinion, that the phenomena manifested by living things are to be ascribed simply to the properties of the matter as it exists in such living things. No one has expressed himself more decidedly on this subject than Prof. Huxley himself, and he may fairly be taken as an exponent of the modern doctrines on this question. He says:†—"Carbon, hydrogen, oxygen, and nitrogen are all lifeless bodies. Of these, carbon and oxygen unite in certain proportions and under certain conditions to give rise to carbonic acid; hydrogen and oxygen produce water; nitrogen and hydrogen give rise to ammonia. These new compounds, like the elementary bodies of which they are composed, are lifeless. But when they are brought together under certain conditions they give rise to the still more complex body, protoplasm; and this protoplasm exhibits the phenomena of life. I see no break in this series of steps in molecular complication, and I am unable to understand why the language, which is applicable to any one term of the series, may not be used to any of the others. We think fit to call different kinds of matter carbon, oxygen, hydrogen, and nitrogen; and to speak of the various powers and activities of these substances as the properties of the matter of which they are composed. . . . Is the case in any way changed when carbonic acid water and ammonia disappear, and in their place, under the influence of pre-existing protoplasm, an equivalent weight of the matter of Life makes its appearance? . . . What justification is there then for the assumption of the existence in the Living matter of a something which has no representative or correlative in the not-living matter which gave rise to it?"

* Buffon, it is true, as Professor Huxley has pointed out, did make an attempt to reconcile two incompatible theories.

† *Fortnightly Review*, Feb. 1869.

For Professor Huxley, then, and for all who hold similar opinions on this subject, the constitution and properties of living things are so far comparable with the constitution and properties of crystals, that both, in each case, are alike supposed to be the products of the combination of ordinary matter of different kinds. And, as might have been expected, nearly all the biologists and physicists who hold these opinions, are now inclined to admit their belief in the *possibility* of the origination of living matter free from the influence, and independently, of all pre-existing living matter. They are quite content to admit that Redi's doctrine may be wrong. Prof. Huxley, indeed, in his recent address, desires us to understand that this is an opinion to which he still adheres; he says:—"I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call 'vital,' may not some day be artificially brought together."

Having reached this stage, having got rid of the supposed necessity for the intervention of a special "vital principle" before living matter can come into existence,* I think it will be seen by all how very important it has become to look into the truth of Redi's doctrine, which has found its best modern expression in the phrase *omne vivum ex vivo*, seeing that that doctrine was born and nourished under the influence of the old, and now well-nigh effete, metaphysical notions concerning Life. Certainly, now that this theoretical barrier has been removed, we ought to inquire more carefully than ever whether there is still a sufficient warrant for the different verdicts which have been given in answer to the questions as to whether crystals on the one hand, or living things on the other, do or do not originate *de novo* in this particular stage of the Earth's history.

Now, at all events, theory inclines no more to the one side than it does to the other; it is quite possible to reconcile this with either view.

Seeing, therefore, that we may now act without fear as impartial judges, let us inquire into the nature of the evidence which alone can be relied upon for the solution of these two questions.

If living things are to come into being *de novo*, they could, or, at all events, are only supposed to originate from the rearrangement of matter which previously existed in a state of solution. And although it is known to be possible for certain kinds of pre-existing solid matter to assume a crystalline form, we will, for the present, confine our attention to the origin of crystals in an apparently homogeneous fluid. Each of these material forms, therefore, would have to commence as a smallest conceivable speck, and each would grow, though differently, by the formation of matter of like kind, under influences generally similar to those which were influential in bringing about the primordial collocation. These primordial collocations, however, are hidden from our view, and will, perhaps for ever, remain so. As a matter of observation, all that we actually know concerning the origin of crystals or of certain living things in solutions is this. In previously homogeneous solutions of crystallisable matter, or in certain apparently homogeneous colloidal solutions, we may, under certain conditions, see the minutest crystals† or living things, respectively, make their appearance. In both cases these are, at first, mere motionless specks, whose *minimum visible* stage may be less than $\frac{1}{100000}$th of an inch in diameter. It must either be presumed, in the case of such embryo living things (as most people do presume in the case of crystals), that these, even then, and however minute, represent stages in the growth of later material collocations which had been initiated under the combined influence of existing matter and "conditions" at a point far beyond the reach of our most aided vision; or, on the other hand, it is equally

* It may perhaps be as well to state here that I have not much expectation of influencing those whose belief in the existence of a special "vital principle" remains still unshaken.

† The appearance of the crystals is best watched in the viscid solutions described by Mr. Rainey; since the rapidity of the process is thereby very much diminished, and the forms themselves are also more akin to those of living things. See his work *On the Modes of Formation of the Shell of Animals*, &c., 1858, p. 9.

open for us to suppose that such minutest visible living things had proceeded from the growth of pre-existing germs which were themselves invisible.

This being, as I conceive, the real state of the case, and Professor Huxley being in the position of a person, admitting* that a crystal can be produced *de novo*, admitting also the *possibility* that a living thing may so arise, but denying that there is any evidence worthy of serious consideration to show that a living thing can at the present time originate *de novo*, let us see on what evidence he has come to this conclusion, and what other evidence he has practically ignored.

In the first place, he does not attempt to deny—he does not even allude to the fact—that *Living things may and do arise as minutest visible specks, in solutions in which, but a few hours before, no such specks were to be seen.* And this is in itself a very remarkable omission. The statement must be true or false, and if true, as I and others affirm, the question, which Professor Huxley has set himself to discuss, is no longer one of such a simple nature as he represents it to be. It is henceforth settled, so far as *visible* germs are concerned, that living things *can* come into being without them. It can now, at all events, be said that *some* Living things do not come from *visible* germs. Who, therefore, in the face of this fact will say that the doctrine *omne vivum ex vivo* remains unshaken? Perhaps, however, this particular case where an exception to the rule is possible, was not known to Professor Huxley. I wish I could bring myself to believe that this was really the case. Certain it is that had he recognised the existence of this apparent exception to the general rule he would then have had to discuss a much more difficult question, and he would have been compelled seriously to inquire into the value of experiments whose existence he has now almost ignored. Again then I affirm that multitudes of minute living things may and do gradually appear in fluids, beneath the microscope, where no *visible* germs previously existed. Here the hypothesis that *every* living thing proceeds from a pre-existing living thing may break down, and those who wish to establish the continuity of this rule are bound to discuss the nature of the existing evidence which is in favour of the notion of the living things in question originating from pre-existing *invisible* germs, as against the opposite *possibility* of their having originated *de novo.* The burden of proof rests as much on the one side as it rests on the other. We cannot safely continue to affirm a rule until the cases in which it seems doubtful have been thoroughly discussed. Analogy is often but a treacherous guide.

And, when we come to the discussion of this hypothesis as to the origin of living things from germs which are *invisible*, all alike are rendered, to a certain extent, helpless. No one, then, can come forward, as Redi is said to have done, "strong with the sense of demonstrable fact," and any one who wishes or calls upon his opponent to demonstrate the truth of his views, when the question is one concerning the presence or absence of *invisible germs*, shows himself to be ignorant as to how the matter in dispute can alone be settled. The subject is one in which direct demonstration must give place to reasoning, although experiment and observation may and must be brought forward in support of this. Let those, however, who wish to proclaim the universality of the rule *omne vivum ex vivo*, recollect that, if they expect to influence reasonable people who are themselves competent to form an opinion on the subject, they are bound to consider the *possible* exception to which their attention is directed, and to weigh the evidence for and against the origin of these minutest visible living specks from germs which are supposed to exist, but which are *invisible.*

The reason, indeed, which seems to induce most people to believe that living things cannot arise *de novo*, is because in 999 cases out of a thousand which come under their actual notice, there cannot be a question that a living thing originates from a pre-existing living thing. A rule, which is of such apparently universal application, they say, is most likely to be the rule which applies to any doubtful case. Much is made out of this argument, which is, of course, a very valid one so far as it goes. But, on the other hand, knowing, as I have pointed out, *that any living things which arise, de novo, from non-living matter, must appear in solutions as minutest visible specks*, it need not be a matter of much surprise that this mode of generation is one which is unfamiliar to the world at large. Have we not seen, indeed, that the most accomplished biologist, provided with the very best microscope hitherto made, though he gets down to a *minimum visible* stage of less than $\frac{1}{100000}$" in diameter is just as powerless in face of the hypothesis of *invisible* germs as those who worked with the rude microscopes which alone were in vogue two centuries ago? And, more especially is this consideration one which presses for earnest attention, when we further consider that some of the minute living things which first appear as tiniest specks in homogeneous solutions grow into *Bacteria*, and that concerning the real origin of these, *in such cases*, we are as ignorant as we were concerning the real origin of crystals, when they appeared in previously homogeneous solutions. The probability that these latter have originated *de novo* has, of late years, had to be established by a process of reasoning similar to what we are obliged to have recourse to, if we wish to throw light on the question of the origin of these specks of Living matter. *Bacteria* grow, and after a time aggregations of them may be converted under our very eyes into *Fungus*-spores* capable of throwing out a filaments and of developing into perfect plants. Nobody pretends to know, however, how, or whether, the *Bacteria* which make their appearance in a homogeneous solution have originated from invisible *Fungus*-emanations: all that we know is, that in suitable solutions, appearing homogeneous to high microscopic powers, in the course of a very short time, a multitude of perfectly motionless specks appear, in situations where previously no specks had existed. Being motionless and diffused their number cannot be accounted for by any supposed rapidity of multiplication—the only possible explanations seem to be, either that the specks have originated from as many pre-existing germs which were invisible, or else that they have proceeded from material collocations, which have been initiated in the fluid itself by virtue of the molecular properties of the substances in solution, and the physical forces or sum total of "conditions," acting thereupon.

And this is really the question which has to be considered. When it is supposed that Living things do appear independently of pre-existing living matter, in certain solutions nothing more than this is supposed to have taken place. New Living matter is presumed to have appeared—independently of germs—in the solutions within these flasks, and to have made *its* appearance as living matter may, in certain other fluids, under our very eyes, in the form of minutest visible specks, which have been exposed to great and long-continued heat in hermetically sealed masses. And similarly such specks, are the only forms of Living matter which are supposed to be capable of arising *de novo.* Once formed, it is true, one of these living specks may develop into a *Bacterium*, and this may develop into a *Vibrio* or a *Leptothrix* filament, whilst another of the living specks may develope at once into a *Fungus*-spore.† It should be clearly understood, however, that *all the Living things which are supposed to arise out of non-living materials, are presumed to appear in fluids, and gradually to emerge from the region of the invisible into that of the visible;* at which latter point they, for us, constitute specks less than $\frac{1}{100000}$" in diameter.

Making no statements whatever upon this subject, however, in support of the doctrine which he considers to remain unshaken, let us see what line of argument Professor Huxley has taken, in order to establish the validity of this belief to the members of the British Association for the Advancement of Science.

The "long chain of evidence" which he considers sufficient to allow us still to place faith in the rule *omne vivum ex vivo*, seems to me, to be, in reality, utterly inadequate for this purpose, and incapable of affecting the real question at issue. Nothing that has been said bears at all upon the problem as to whether it is possible that the minute living specks to which I have referred do or do not originate *de novo*, though, as I have already said, it is these, and such as these only, which are presumed to originate after this fashion. If he had really wished to influence those who are conversant with the subject, it would have been absolutely imperative for Prof. Huxley to have entered fully into the consideration of a subject which I will presently mention, but to which he makes only the most casual allusion. All the facts which he has brought forward—all the references to the investigations of Spallanzani, Schultze and Schwann, Cagniard de la Tour, Helmholtz, Schrœeder and Deutsch, Tyndall and Pasteur—are simply contributions to the "Atmospheric Germ Theory," tending to show that there are germs of living things in the air, and that the living things found in *some* solutions may have been developed therefrom. But although differing

* I suppose this may fairly enough be presumed even in the absence of any specific statement as to his belief on the subject. This is, however, an assumption on my part.

* See NATURE, No. 35, p. 173, Fig. 3.
† See NATURE, No. 37, pp. 221, 223.

from him in my interpretation of the results of some of these investigations,* I am quite content to accept the conclusion which is alone derivable from this long chain of evidence. I am even prepared to grant to Professor Huxley, for the sake of argument, that *Bacteria* may be "suspended in the atmosphere in myriads." The evidence thus referred to, if true in all respects, would have been very valuable if it had been brought against the doctrine that none of the minute living things of infusions derived their origin from atmospheric germs, though it may and does fall utterly powerless before the doctrine which is alone urged, that *some* of the Living things met with in infusions appear to be produced independently of pre-existing living matter. If it could be proved that the air contained five hundred times as many germs as can now be shown to exist therein, this discovery would still be quite compatible with the truth of the other doctrine that under the influence of certain conditions some Living things, appearing as minutest visible specks, do arise *de novo* in solutions.

Whether such an occurrence can or cannot now take place is a question which is not at all dependent upon the prevalence or paucity of germs in the atmosphere. I may also remind Prof. Huxley of a fact which he seems to have forgotten, and that is, that the atmosphere is not the only source of germs. These may be present in the water or in the materials dissolved therein. Seeing, therefore, that in certain experiments which constitute the corner-stones of his edifice of proof, and which are brought forward, I suppose, as being capable of influencing our judgment upon this great question, the materials which were dissolved and the water employed were merely boiled for fifteen minutes, we must look upon this as an admission by Prof. Huxley that in his opinion the exposure of the *solution for such a time to a temperature of* 100° *C. was an adequate precaution to ensure the destruction of all pre-existing living things that may have been contained therein.* This is a most important admission—tacit though it be—in the face of other evidence which can be mentioned, and if Prof. Huxley does not really believe this, how is it possible for us to understand what either his argument or science gains from the citation of the following experiments?

Having boiled portions of "Pasteur's solution" for fifteen minutes, in three separate flasks, he placed in the neck of one of them, whilst ebullition was continuing, a large plug of cotton wool, left another with the mouth of the flask open, whilst into the third, when cool, he placed some *Bacteria* taken from a solution of hay. "In a couple of days of ordinary warm weather," he says, "the contents of this [latter] flask will be milky from the enormous multiplication of *Bacteria.* The other flask open and exposed to the air will, sooner or later, become milky with *Bacteria*, and patches of mould may appear in it; while the liquid in the flask, the neck of which is plugged with cotton wool, will remain clear for an indefinite time." And then Prof. Huxley adds:—"I have sought in vain for any explanation of these facts, except the obvious one, that the *air contains germs* competent to give rise to *Bacteria*," and similar to those with which one of the solutions was purposely inoculated. Now, with reference to these statements, the possibility at once suggests itself, that had a different solution been used in the case where the neck of the flask was plugged with cotton-wool a very different result might have been obtained. In order to throw light upon this subject I have performed the following experiments:—

Immediately after reading Prof. Huxley's address, I procured a piece of cooked meat, made an effusion of the same, and after filtration put it into a flask. It was then boiled for fifteen minutes, after a large plug of cotton wool, 1¼" in length, had been pushed into its neck. After this time the plug was rendered tighter by pushing in more wool. Another flask was prepared in a similar way, only in this, a strong filtered infusion made from undressed meat was placed. At the expiration of the fourth day (Monday morning, Sept. 17th) the weaker solution, still quite *clear*, was opened, and on microscopical examination of two or three drops of the fluid a multitude of minute motionless particles of various sizes were seen, others in active movement, and two or three *Bacteria* about $\frac{1}{8000}$" in diameter. The flask containing the stronger solution was opened at the expiration of forty-two hours. The fluid still appeared quite clear, and on microscopical examination of a few drops of the fluid *many* tolerably active Bacteria were found varying between $\frac{1}{6000}$"—$\frac{1}{1000}$" in length, besides a multitude of particles, some moving and others motionless.

These results seem to me what might have been expected after what I have made known concerning putrefaction *in vacuo.* It could scarcely be expected that mere *filtration* of air should be able to prevent putrefaction when it has been already shown that this will take place in the absence of air.

What conclusion, then, is now deducible from Prof. Huxley's three comparative experiments? Certainly nothing that has any value for the support of his argument.

[A strong point made by Prof. Huxley is the supposed fact that the possibility of preserving meat is a fatal reply to the experiments of myself and others. I shall show next week, that the actual facts strengthen my point of view, and that "perfectly good" cases of meat which I have examined have contained *Bacteria* and *Leptothrix* filaments.]

H. CHARLTON BASTIAN

* Neither time nor space will permit of my mentioning these various points on which I am inclined to differ from him. When Prof. Huxley says, however, after a tragical metaphor, "It must be admitted that the experiments and arguments of Spallanzani furnish a complete and a crushing reply to those of Needham," I will only say that I cannot agree with him, and will remind him that, in this case at least, he is not supported by Pasteur, whose logic is so invincible. Pasteur says (*Ann. de Chim. et de Phys.*, 1861, p. 9):—"Un examen impartiel des observations contradictoires de Spallanzani et de Needham sur le point le plus delicat du sujet, va nous montrer en effet, *contrairement à l'opinion généralément admise* que Needham ne pouvait en toute justice abandonner sa doctrine en presence des travaux de Spallanzani."

I would also call Prof. Huxley's attention, as an impartial historian, to some communications made by M. Victor Meunier to the French Academy (*Comp. Rend.* 1865), from which he will see with reference to the vessels with bent necks, that it is possible to perform these experiments with an "entire success" of a different kind from that to which he alludes. Others besides myself have also performed such experiments with results similar to those of M. Meunier. Much seems to depend upon the nature of the solution employed.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

English Physiology

THE present state of physiology in this country ought to be a matter of regret. Though foremost in many things, Britain is far behind Continental countries in the field of physiological science. We can boast of a few distinguished physiologists, as John Hunter, Sir Charles Bell, and Dr. John Reid; and famous microscopists, as Carpenter and Beale; but a very small number of English names can be cited compared with the host of Continental physiologists, past and present, as Magendie, Müller, Von Beyold, Von Baer, Beclard, Bernard, Brown-Sequard, Du Bois Reymond, Helmholtz, &c. This discrepancy arises not from want of talent, but from lack of opportunity. The mental qualities required by a physiologist, as observation and memory, are developed separately at different periods of life. Hence there are only a limited number of years during which any such branch of learning can be cultivated with fresh ardour, and during which the power termed originality can be brought into play. The Continental schools make use of these precious years by affording those who are naturally inclined to cultivate any one branch of science, full scope for repeating the observations of their predecessors, and for endeavouring to add to the existing stock of knowledge. By having various laboratories and certain paid appointments connected with their universities, they allow young men to devote their whole time and energy to the study of individual subjects, as physiology. Those who set themselves to work of this kind do not look forward to the practice of the medical profession, but purpose to live and work as physiologists. These young men are known by their labours to be specialists, and are proposed by the senatus with which they are connected for a vacant professorship when it occurs. This is the only method of securing original and extensive work in any one scientific branch, as Physiology. It would be well, therefore, if the approaching Royal Commission of Inquiry into the State of Science in this country would not overlook Physiology, but would make some arrangements whereby Great Britain might no longer be stigmatised by her Continental neighbours as "having no Physiologist."

Dr. Stricker, in the two articles he has already communicated to NATURE on "The Medical Schools of England and Germany," has not referred to the Edinburgh University, which possesses the best furnished Physiological Laboratory to be found in Great Britain, and one equal to most of those met with in Germany. The plan recently introduced into this Scottish University of having salaried assistants to the professors resembles that which exists in the German Universities. This arrangement not only allows opportunity for carrying on original research, but also enables the professors to impart a most beneficial impulse to younger men.

In this country it has hitherto succeeded best, and appears to be most in consistence with our national constitution, to place the pecuniary assistance afforded to scientific men under the control of learned bodies as the Universities and the Royal Society. It is to be hoped, however, that our Government will, ere long, recognise the duty of advancing physiologists by aiding them with grants of money. Every medical school in Britain ought to have a physiological laboratory, well furnished with instruments—electrical, chemical, and physical; and with the view of instituting and supporting these it will be necessary to supply certain sums of money annually from the public exchequer. In addition to money given to Medical Schools for the purpose of buying instruments, two or more grants might be set aside as prizes to be bestowed annually on the schools furnishing the best physiological work, and to be distributed by the governing bodies of the said schools among the workers. The awarding of such prizes might very well be entrusted to the Royal Society.

I trust that this plea on behalf of physiology will not pass unnoticed by you.

Birkenhead P. M. BRAIDWOOD

Mirage

MIRAGE is not, in my experience, an uncommon phenomenon. I saw it this summer on the flats at the mouth of the Dee (Wirral side). It may often be seen, with a bright sun and still air, after heavy rain, on Hartford Bridge Flats, and other level and gravelly heaths in the Bagshot Sand district, where the fir trees may be seen floating in water, or forming promontories jutting out into a lake—a phenomenon similar to, though far less striking than, what I have seen on the Plain of Aan, in Provence.

The most curious mirage-effects I ever saw were on the Wash during hot summer weather. The mirage is there known as the "looming of the land," and when it is about it is impossible at moments to distinguish the sand and weed-banks from the sea, while the distortion, both perpendicular and horizontal, of ship's masts, &c., is ludicrous. In one case I saw a herd of seals on a sand-bank transformed into a row of long-legged monsters, wading in water, or rather rooted by their long legs to the legs of a similar row of monsters below them, which was their distorted reflection in wet mud. I had some difficulty, at first, in making out what they were.

Eversley Vicarage, Sept. 16 C. KINGSLEY

Astronomical Science

IN your number for August 5th, a letter referring to Astrology, signed "C. J. Robinson" ends as follows:—"Astronomical Science is hardly likely for the sake of sentiment to treasure up the discarded swaddling clothes which for so many centuries impeded its onward progress." Surely such language indicates a sad confusion of ideas on the subject, since it is most unquestionable that the belief of antiquity in Astrology—far from retarding—greatly promoted the study of Astronomy. In fact, the names of Ptolemy and Kepler show that the greatest of ancient and the greatest of modern astrologers were at the same time the greatest Astronomers of their era, and the brilliant discoveries of the latter in both sciences suffice to dispose of the "swaddling clothes" theory without citing the instance of Napier, who, it is well known, invented that most admirable scientific expedient and indispensable handmaid to Astronomy, Logarithms, to shorten and facilitate his astrological calculations.

I have not seen Moore's Almanac referred to by Mr. Robinson, but any one by consulting an Ephemeris may verify the following curious facts. War against Prussia was declared by the French Emperor on the 15th July. The preceding lunar change was a total eclipse of the Moon on the 12th, in 20° 15′ of Capricorn, when the Sun and Moon had (substantially) the same declination as Herschel, Saturn, and Jupiter. Between noon on the 14th and noon on the 15th, Mars came to the opposition of Saturn retrogade. On the 15th, Herschel was in conjunction with the Sun, the planet having at the same time the exact parallel of declination of Saturn and Jupiter. So exceptional and extraordinary did these planetary positions and relationships appear to me that more than two years ago I made two crosses at the middle of July in my Ephemeris, and outlined a hand in the margin that I might not omit to note when the time came whether anything unusual occurred. Now the eclipse on the 12th took place on the Ascendant in the Revolutionary Figure of the Emperor Napoleon in square to Mars and opposition to Herschel, and according to the old astrologers "an eclipse of the Moon in Capricorn in evil aspect to Mars causes military disasters," whilst modern astrologers credit aspects of Herschel with producing events of a strange and unexpected character. Again the same figure presents the Moon in conjunction with Saturn retrograde on the place arrived at by Herschel by direction, whilst the Ascendant falls on the place attained by Saturn, the whole presenting, according to the canons of Astrology, a rare combination of evil portents. Probably it is the preceding data taken in conjunction with the primary directions (also of evil import) which have furnished the ground for the predictions of misfortune to the French Emperor to which Mr. Robinson alludes. T. S. PRIDEAUX

7, Eardley Crescent, West Brompton

[We insert this as a specimen of a kind of letter which it should be impossible to write in the nineteenth century.—ED.]

Insects upon a Swallow

DURING the month of August, at Meran, in the Tyrol, a swallow sitting upon a stone at the side of a public thoroughfare let me take it up without showing the least fear, or even moving. The cause of its indifference was immediately apparent; two large insects of a dark slate colour were running about the bird upon the outside of its feathers, their power of adhesion being considerable. While trying to remove them, one got upon my hand and was lost, being thrown some distance by the second of two hasty but vigorous shakes. The other fell to the ground after hanging by a thread, similar to, but much stronger than, a spider's single thread. The form of the insects was quadilateral, the head being at one of the angles, the measurement between the opposite angles being about ¼ inch; the strength of the skin was so great that the insect required three crushing rubs by a lady's foot against the road before its activity was destroyed. The bird seemed conscious of release from its parasites, and struggled to get away, and then was only just able to flutter languidly to a tree about forty yards distant. The toughness of the insect, its activity and power of clinging, fully account for the inability of the bird to free itself.

I have seen an account *somewhere* of a bird, whether a swallow or lark I forget, similarly troubled, and showing the same fearlessness of capture. G. H. H.

Birkenhead, Sept. 8

NOTES

PROFESSOR HUXLEY's presidential address is not his only outcome at Liverpool which it is our duty to chronicle—a duty which we perform with gratitude to him for his plain speaking. At the unveiling of Mr. Gladstone's statue on the 14th inst., Mr. Huxley, after referring to the Compulsory Education measure, which promises in time to rid us of our worse than Eastern degradation, as one of Mr. Gladstone's greatest achievements, added that if he might presume to give advice to a man so eminent as Mr. Gladstone—if he might ask him to raise to a still higher point the lustre which would hereafter surround his name in the annals of the country, it was that he should recollect there was more than one sort of learning, and that the one sort which was more particularly competent to cause the development of the great interests of the country, was that learning which we were in the habit of calling Science. That Mr. Gladstone was profoundly acquainted with literature, that he was an acute and elegant scholar, they all knew, but he suspected that the full importance for the practical interests of the country of developing what was known as Science was not quite so clear to the Prime Minister as it might be But, seeing the great faculty of development which his past career had shown, he had no doubt that such a man would by-and-by see that if this great country was to become what it should be, he must not only put the means of education within the reach of every person in the land, but must take care that the education was of such a nature as to provide those persons with the knowledge which they could apply to their pursuits, and which would tend to make them understand best those laws under which the human family existed.

THERE was still another outcome equally noticeable for its plain speaking, for, addressing the working men of Liverpool, the President of the British Association remarked, and his remarks were responded to by three cheers, that it had been a shock to him, walking through the streets of Liverpool, to see unwashed, unkempt, brutal people side by side with indications of the greatest refinement and the greatest luxury. He remarked upon this to working men because he believed it was the secret of the uneasiness and unrest which betrayed themselves in their political movements. The people who formed what are called the upper strata of society talked of political questions as if they were questions of Whig and Tory, of Conservative and Heaven knows what; but beneath there was the greater question whether that prodigious misery which dogs the footsteps of modern civilisation should be allowed to exist, and whether there should be in those nations which prided themselves most upon being Christian that predominant and increasing savagery of which such abundant specimens were seen in Liverpool. If in the course of history the savagery of nations has been gradually put down by one process only—by learning the laws of nature and the laws of social life, and obeying those laws—he urged upon them that history in this matter did not lie; and if they were to succeed in their great aspiration, they had only one course, and that was to learn the laws of nature and do their best to obey them. To that end all their efforts should be directed; to that end the great educational movement must be directed; and if their efforts were directed wisely and well, he could not doubt they would meet eventually with the most perfect measure of success.

THE war has, unfortunately, much curtailed the Reports of the British Association in the London Press, but this is not all, it is surely a matter for regret that the space given has been devoted to the least important papers. The Liverpool Press, too, has disappointed us, and has fallen far short of what has been done in smaller towns at recent meetings of the Association. We do not blame the editors, they know their public, and this is the real source of the evil: the British public, there is no concealing the fact, have as yet not the least idea of the importance of Science; they do not know what it means, or how it may help them in the daily affairs of life. One of the Liverpool reports has particularly struck us on account of its charming *naiveté*. Speaking of Professor Clerk Maxwell's paper on Hills and Dales, after remarking, with a tone of regret, that in Section A there was only a very small attendance which "consisted exclusively of mathematicians," the report adds, "The whole subject was treated in a very scientific manner, quite unlike what might have been supposed from the plain English title of the paper."

THE five classes of the Institute of France have unanimously resolved to draw up a protest in view of the eventual bombardment of the monuments, libraries, and museums of Paris. The protest will be addressed to every academy in the world, inviting them at the same time to give their adhesion to it. Meanwhile, the Minister of Public Instruction has been given a credit of 50,000f. to enable immediate precautions to be taken.

THE foundation stone of the new building about to be erected for Owens College, Manchester, is to be laid next Friday. His Grace the Duke of Devonshire, K.G., F.R.S., President of the Extension College, the Bishop of Manchester, and the President, and other distinguished members of the British Association are expected to be present and take part in the proceedings.

IT is said to be the intention of the Government to greatly increase the number of medical officers who are employed for the purposes of sanitary supervision under the Privy Council; the whole of the kingdom will be divided into sanitary districts, each under a medical officer with a salary of not less than 600*l*. per annum.

ANOTHER Dutch contribution to Natural History lies on our table in the form of a monograph of the Squirrel, "The Osteology and Myology of *Sciurus vulgaris* L., compared with the Anatomy of the Lemuridæ and the Chiromys, and on the Position of the latter in the Natural System," by Dr. C. K. Hoffmann and H. Weyenbergh, jun. It is a prize essay for the Dutch Academy of Sciences at Haarlem, but is written in German.

PROF. RAULIN'S "Description physique et naturelle de l'île de Crête," is published under the authority of the French Minister of Public Instruction in two thick volumes and an atlas; the first volume including the history, geography, and statistics of the island; the second, its meteorology, geology, botany, and zoology.

THE first annual part of Dr. G. Radde's Report of biological and geographical researches in the Caucasus country includes an account of journeys in the Mingrelian mountains, and the three mountain-valleys Rion, Tskenis-Tsqabi, and Ingur, with maps and plates, the latter beautifully executed and coloured.

A SERIES of experiments has been made at the Government farm in Madras on the applicability of the "gram" plant as fodder for cattle and horses in those districts instead of the seed; it succeeds well on poor soil, and gives four crops in the year. It has been found superior to grass, and can be made into hay.

THERE is good report of the progress of ipecacuanha cultivation in India, where it is found so valuable in that prevalent disease, dysentery. Since Dr. John Murray obtained for it the notice of the Indian Government, it has been successfully planted in the Neilgherries and other of our hill settlements, and in the plains. It has done well even at Calcutta.

A SOLUTION of tannin has been used in the treatment of cotton fabrics, as are hides in the manufacture of leather, and, according to *Cosmos*, the cotton thereby acquires greater strength, and resists moisture and disintegrating effects better. No attempt is made to explain the chemical reaction which produces this important change, but it is believed that the change cannot be great, since it has escaped the notice of practical tanners.

THE Rev. Cyrus Byington's "Grammar of the Choctaw Language," the manuscript of which is in the possession of the American Philosophical Society of Philadelphia, has just been published under the editorial supervision of Dr. D. G. Brinton. Mr. Byington was a native of Stockbridge, Massachusetts, and from 1819 to the close of his life, in 1868, was a missionary among the Choctaws, whose language he studied with so much thoroughness, that when he died he was engaged in revising his grammar for the seventh time; and his family still have a Choctaw dictionary, embracing 15,000 words, which was left, like the grammar, in manuscript. Another work of interest to philologists has been printed at Bogota, in New Granada: a Grammar of the Chibcha Language, by Dr. Ezequiel Uricoechea. The title is:—"Grammatica. Frases, Oraciones, Cathezisms, Confessonario y Boca Bulario de la Lengua Chibcha, 1620. Copiada del Manuscrito Orijinal por E. Uricoechea." The volume fills 64 pages in 8vo.

SALMON, it appears, are found in great abundance on the Pacific coast. The *San Francisco Bulletin* says, "From Mexico to Alaska every clear stream running into the ocean is frequented by salmon. These fish even ascend small streams which one can jump across, and the number which frequent large streams is wonderful. The size, quality, and shape vary considerably in the different streams, the largest being caught in the Sacramento river. While the salmon theoretically must have clear water, it is remarkable that it seems to thrive in the muddy waters of the Sacramento." Here is possibly some news for Mr. Frank Buckland.

THE Government of Honduras, in Central America, has granted to M. de la Roche for ten years the exclusive privilege of planting and exporting Corozo nut, paying two reals each hundred weight as royalty. What Corozo nut may mean, it is not easy to say.

MR. E. G. SQUIER, late commissioner of the United States in Peru, reprints a paper read before the American Geographical Society, containing Observations on the Geography and Archæology of Peru.

PROFESSORS JOHN TORREY and Asa Gray reprint from the Proceedings of the American Academy of Arts and Sciences their "Revision of the Eriogoneæ," a tribe of the order *Polygonaceæ*, first instituted by Mr. Bentham, wholly American, and especially characteristic of the drier western regions of the northern continent. They recognise seven genera, the same number as Mr. Bentham, the number of species being increased from 105 to 115.

THE Quekett Microscopical Club has just issued its fifth Report. The number of members has increased during the last five years from eleven to over 500, "all imbued with a strong desire to seek out the unfathomable stores of interest revealed by the microscope, and all influenced by that insatiable thirst for the observation of the minute and the beautiful that only the microscope can open to view." The club now meets twice a month throughout the year, at University College, Gower Street, and excursions are made during the summer season for the purpose of providing microscopical research.

WE have just received the prospectus of the Ladies' Educational Association, and we are glad to see that the lectures will include two subjects in Science, a chemical course by Professor Williamson, and a course of eighteen lectures on experimental physics, by Professor G. Carey Foster. Both courses will be delivered at University College. By applying to the secretary, a free ticket can be procured for the first lecture of each course; those requiring class tickets, free tickets for opening lectures, prospectuses, and information, are requested to send to the hon. secretary, J. E. Milne, Esq., 27, Oxford-square, Hyde-park, W.

THE two volumes now published of Willkomm and Lange's "Prodromus Floræ Hispanicæ," include the Ferns, Gymnospermæ, Monocotyledones, Apetalæ, and Gamopetalæ.

EXPERIENCE only too clearly shows that familiarity breeds contempt. Even earthquakes are now quite appreciated in some parts of the world. Thus the *San Francisco Bulletin* of the 11th August says, "Popular prejudice is rather in favour of these lighter demonstrations of subterranean force, as they seem to stave off the heavier shocks." What next?

THE Smithsonian Institute of Washington has appointed a committee of scientific men to make a series of experiments to ascertain the temperature of the earth's crust at a considerable depth below the surface. For this purpose an artesian well at St. Louis is to be utilised, and as this is 3,843 feet deep some interesting results may be looked for.

THE decree of the Committee of the National Defence of Paris announcing that all woods and forests which might endanger the defence of the country will be set on fire on the approach of the enemy, has already been acted upon to a large extent. Independently of the loss in an artistic and æsthetic point of view, we can hardly be aware, probably the Parisians are hardly aware themselves of the amount of self-sacrifice this resolution will entail on themselves, and on their descendants. For some years past the climate of the central regions of France has been rapidly becoming drier, to the serious injury of many of the crops, a result attributed in part to the extensive cutting down of forests. The destruction of the world-famous forests of Fontainebleau, St. Cloud, St. Germain, and the Bois de Boulogne, will involve a material loss to the country, possibly hardly exceeded by the actual expenses of the war itself.

THE BRITISH ASSOCIATION

LIVERPOOL, *Tuesday Morning*

THE Liverpool meeting of the British Association is a great success. Distinguished visitors, a large company, interesting papers, and splendid weather, have all combined in its favour. The hotels are all full to overflowing, and accommodation extremely difficult to get. Almost all our well-known *habitués* are here; and among foreigners, Henry, Van Beneden, Stricker, Bolzani, and a number of others, lend lustre to the meetings. Professor Huxley's address on Wednesday was listened to by a large and attentive audience, who appeared thoroughly to follow his train of argument. At the general committee some new regulations of considerable importance were proposed, particulars of which will be found in another column. On Thursday a casual visitor to Liverpool would see at once, on emerging from the railway station at Lime-street, that something unusual was stirring. The centre of operations was the space that includes St. George's Hall, where several of the sections are located, and the closely adjoining Derby Museum, where the Biological Section is to be found, and the reception-room, reading-room, and post-office. In this space and the neighbouring streets, the members of the Association may be recognised by the little blue or buff map-cards they carry, as symbolical as Murray in a Continental tour. Taking the various sections in turn, we find three located in St. George's Hall, A, E, and G. The Geographical Section is always a popular one, and attracted the largest audience of any to listen to the well-known and popular president, Sir Roderick Murchison, deliver his opening address. The room selected was the small concert-room—not a very small one, by-the-bye—which was well filled, a large proportion of the audience being ladies, though I am afraid Sir Roderick's failing voice hardly reached the whole of the company. How large a proportion stayed to hear Sir H. Rawlinson's report on the Site of Paradise I did not wait to see. The two other sections in St. George's Hall, the Mathematical and Mechanical, had to be reached by long winding passages, past rooms redolent of the law; and were, of course, much more thinly attended, but very few ladies being met with here. Crossing over to the Derby Museum, and passing through the Reception Room, we reach the Free Public Library, where Professor Rolleston enchained a large audience by an address of upwards of an hour, which was generally admitted to be *the* address of the day. His commanding presence, measured diction, and his happy hits and classical allusions, exercised a great charm over the meeting, and many were the inquiries where a report of the speech was to be obtained. This section then divided into three sub-sections, under the presidency of Prof. Rolleston, Prof. Michael Foster, and Mr. John Evans, each of which attracted a goodly number of visitors. A very large audience listened to Dr. Brown-Séquard's long but very interesting account of his researches on the nervous system of guinea-pigs, of which little animal it is said, that he has left two thousand behind him in Paris, which, he fears, may fall a prey to the Prussians or to the exigencies of the siege. Those sections which are held at comparatively remote places are at a certain disadvantage, though many of the visitors appeared to avail themselves of cabs or of the luxury of the tramway omnibuses, which London does not yet possess, to visit them. The Geological Section is held in a small and not very convenient room, the Concert Hall, in Lord Nelson Street. Here there was no opening address, and the attendance was small. The most remote of all is the Chemical Section at the Royal Institution in Colquitt Street. At the Section for Economic Science, in the Town Hall, I arrived just in time to hear the venerable Sir John Bowring propose a vote of thanks to the president, Prof. Jevons, for his opening address. The "economic" rule was there laid down, which might have been adopted with advantage in some of the other sections, that, during a discussion, no one was to speak for more than ten minutes. Considering the distance from the central buildings, the attendance here was not small; and the committee of this section appears to contain the largest infusion of the aristocratic element—Lord Derby, Lord Houghton, Sir J. K. Shuttleworth, and Lord Neaves. The first day was closed by a *soirée* given by the Mayor in the Town Hall, where about 1,400 visitors were collected, consisting of members of the Association and the principal visitors in Liverpool. A *soirée* was also held in the Free Public Library, where Mr. Moore and the Rev. H. H. Higgins had got together a very large and interesting collection of scientific objects and works of art, including the well-known and valuable "Mayer collection."

On Friday and Saturday I confined my visits to the several sections comprised under the head of Natural Science. In the Geological Section there have been several papers of great interest, Mr. Judd's having attracted particular attention, and the discussion which followed was thought very interesting. The chemists do not appear to have mustered in force; at all events, that section took a holiday on Saturday, and a good many interested in it have seized the opportunity of visiting some of the numerous chemical works in the neighbourhood. Section D, on the other hand, has suffered from a plethora of papers, notwithstanding its division into three sub-sections. The Geographical Section is always popular. Great disappointment was expressed at the withdrawal of Mr. Hepworth Dixon's paper, which was, however, to some extent compensated by the part he took in the discussion which followed Governor Gilpin's paper on Colorado. [But the greatest crush on Friday was in Prof. Rolleston's sub-section; and the interest excited on the subject of Spontaneous Generation, by the president's opening address, was shown by the attention with which the crowded audience listened to a two-hours' discussion, for which purpose the three sub-sections were for a time united. The campaign was opened by a description of some recent experiments by Dr. Child, the well-known advocate of spontaneous generation, on Abiogenesis. This was followed up by an elaborate paper, on the other side, from Mr. Samuelson, which was succeeded by some remarks from Dr. Crace Calvert, who spoke of the extreme difficulty with which the atmosphere can be entirely freed from organic germs. The discussion which followed was sustained by Dr. Hooker, Mr. Bentham, and other distinguished naturalists, entirely on the orthodox side of Biogenesis. In the remarks with which he summed up the debate, Prof. Rolleston complimented Dr. Child on the gallantry with which he had stood in the breach alone against such a consensus of opposition; and expressed his regret at the absence of the great champion of Abiogenesis, Dr. Charlton Bastian—the report of whose experiments in NATURE, he said, it was the bounden duty of every one who had now heard the other side of the question to read. Prof. Huxley was not present during the discussion; and though Mr. Herbert Spencer was, he took no part in it. It is rumoured that the combat is to be renewed to-morrow by the chiefs themselves: if this is the case, probably the whole Association will be there to hear them.] In a discussion which took place in the same section on Thursday, Mr. Gwyn Jeffreys spoke of the hindrance which the *Porcupine* dredging expedition in the Mediterranean had experienced from the unfavourable weather. He mentioned the fact, which is but little known to naturalists, and which is not without importance in reference to the theory of glacial epochs, that the species of mollusca dredged up from great depths in the Mediterranean in previous expeditions are identical with Arctic species. It is very gratifying that Prof. Wyville Thomson, who was prevented from taking his share in that expedition through illness, is now completely restored to health. Prof. Tyndall's eloquent discourse on Friday on the scientific uses of the imagination was just of the kind which pleases the audience for whom it was intended, and was rapturously applauded.

At a meeting of the General Committee yesterday afternoon, Prof. Huxley in the chair, invitations were read for meetings of the Association to be held at Edinburgh, Brighton, Bradford, and Belfast. The Scotch metropolis was represented by Prof. Balfour and Sir Walter Elliot, the southern watering-place by Mr. Hallett and Mr. Mayall the photographer, the Yorkshire manufacturing town by the Mayor of Bradford and Mr. Alderman Law, and the Irish city by Dr. M'Gee and Mr. Patterson. In reference to Brighton, it was mentioned that the three south-eastern counties of England have together only enjoyed one meeting of the Association during the last twenty-five years, the Thames appearing to be a kind of Rubicon which the Association has found a difficulty in crossing. It was moved by Sir Roderick Murchison, seconded by Mr. Cowan (who stated that the neighbourhood of Edinburgh manufactured 200 miles of paper per diem), that the next meeting of the Association be held at Edinburgh. Lord Houghton then moved, in accordance with the new resolution passed by the General Committee, that the meeting for 1872 be held at Brighton. This was seconded by Mr. Gassiot, and also carried unanimously. The motion of Prof. Stokes, seconded by Mr. Spottiswoode, that Sir William Thomson be the president-elect, was received with great enthusiasm. Sir Walter Elliot proposed, and Prof. Rolleston seconded, the appointment of the vice-presidents for 1871, viz, the Duke of Buccleuch, the Lord Provost of Edinburgh, the Right Hon. J. Inglis, Sir Alexander Grant, Sir Roderick Murchison, Sir Chas. Lyell, Dr. Lyon Playfair, and Dr. Christison. Prof. Crum Brown, Mr. Ed. Sang, and Mr. T. B. Margaret are to be the local secretaries; and the time fixed was the middle of August, the day to be settled by the Council. The committee then resumed the subject of Vivisection, which had been adjourned from the last meeting. Mr. Johnstone Stoney proposed the resolution of which he had given notice, that "Having regard to the well-known character of the British Association, and to the circumstance that the business of the General Committee is necessarily transacted under pressure of time, it is not expedient, under ordinary circumstances, that it be recommended to this committee to appoint committees or pass votes for investigations to be carried on by the method of vivisection." This resolution Mr. Stoney was anxious to withdraw in favour of two others, but the chairman decided that these could only be brought forward in the form of amendments. The original resolution was therefore seconded, *pro formâ*, by Prof. Stokes. Mr. Samuelson then proposed as an amendment, "That the committee of Section D be requested to draw up a statement of their views upon physiological experiments in their various bearings, and that this document be circulated among the members of the Association, and that the said committee be further requested to consider from time to time whether any steps can be taken by them or by the Association which will tend to reduce to its minimum the sufferings entailed by legitimate physiological inquiries, or which will have the effect of employing the influence of the Association in discouragement of experiments which are not clearly legitimate of living animals." The amendment was seconded by Professor Rolleston, and carried by a large majority. The following appointments were then made :—Council : The President and President elect ; Vice-president and Vice-presidents elect ; General Secretaries and Assistant-secretary ; General Treasurer ; trustees, presidents of former years, and the following gentlemen :—Mr. Bateman, Dr. Beddoe, Mr. G. Busk, Dr. Debus, Mr. Warren Delarue, Mr. J. Evans, Captain Galton, Mr. F. Galton, Mr. Gassiot, Mr. Godwin-Austen, Lord Houghton, Mr. W. Huggins, Sir John Lubbock, Prof. W. A. Miller, Mr. Newmarch, Sir S. Northcote, Prof. Ramsay, Prof. Rankine, Dr. J. Simon, Lieut.-Col. Strange, Col. Sykes, Sir W. Tite, Prof. Tyndall, Mr. A. R. Wallace, Prof. Wheatstone, Prof. A. W. Williamson. General Secretaries, Prof. Hirst and Dr. Thomas Thomson. Assistant Secretary, Mr. Griffiths. Treasurer, Mr. Spottiswoode. Auditors, Mr. G. Busk, Dr. M. Foster, Mr. Gwyn Jeffreys. Mr. J. Evans and Dr. M. Foster were added to the Committee of Recommendations. B.

REPORT OF THE COUNCIL

"The Council have received the usual reports from the General Treasurer and from the Kew Committee. Their reports for the past year will be laid before the General Committee this day.

The Council have to report upon the action they have taken relative to each of the four resolutions referred to them by the General Committee at Exeter.

The first of these resolutions was—

'That the Council be requested to take into their consideration the existing relations between the Kew Committee and the British Association.'

The Council accordingly appointed a Committee of their own body to examine into these relations. This Committee had before them a special report drawn up by the Kew Committee, and, after due deliberation, they recommended—

'That the existing relations between the Kew Observatory and the British Association be continued unaltered until the completion, in 1872, of the magnetic and solar decennial period; but that after that date all connexion between them shall cease.'

The Council adopted this recommendation, and now offer it, as their own, to the General Committee.

The second resolution referred to the Council was as follows :—

'That the full influence of the British Association for the Advancement of Science should at once be exerted to obtain the appointment of a Royal Commission to consider—

First, the character and value of existing institutions and facilities for scientific investigation, and the amount of time and money devoted to such purposes.

Secondly, what modifications or augmentations of the means and facilities that are at present available for the maintenance and extension of science are requisite ; and,

Thirdly, in what manner these can be best supplied.'

By a third resolution the Council was 'requested to ascertain whether the action of Government in relation to the higher scientific education has been in accordance with the principles of impartiality which were understood to guide them in this matter ; and to consider whether that action has been well calculated to utilise and develop the resources of the country for this end, and to favour the free development of the higher scientific education. That the Council be requested to take such measures as may appear to them best calculated to carry out the conclusions to which they may be led by these inquiries and deliberations.'

The Committee of the Council appointed to consider these two resolutions reported their opinion to be favourable to the appointment of a Royal Commission to inquire into the relations of the State to scientific instruction and investigation ; and they added that no such inquiry would, in their opinion, be complete which did not extend itself to the action of the State in relation to scientific education, and the effect of that action upon independent educational institutions.

Your President and Council, acting on the advice of this Committee, constituted themselves a Deputation and waited upon the Lord President of the Council. They are glad to be able to report that their efforts to bring this important subject before Her Majesty's Government have been attended with success. On the 18th of May, Her Majesty issued a Commission "to make inquiry with regard to Scientific Instruction and the Advancement of Science, and to inquire what aid thereto is derived from grants voted by Parliament or from endowments belonging to the several universities in Great Britain and Ireland and the colleges thereof, and whether such aid could be rendered in a manner more effectual for the purpose." The Commissioners appointed by Her Majesty are the Duke of Devonshire, the Marquis of Lansdowne, Sir John Lubbock, Bart., Sir James Phillips Kay Shuttleworth, Bart., Bernhard Samuelson, Esq., M.P., Dr. Sharpey, Professor Huxley, Dr. W. A. Miller, and Professor Stokes. J. Norman Lockyer, Esq., F.R.S., has been appointed Secretary to the Commissioners, who, up to last July, were engaged taking evidence with great assiduity, and have now adjourned their meetings until November. There is every reason to hope that valuable results will follow from their deliberations.

The fourth resolution which the General Committee referred to the Council was—

'That the rules under which members are admitted to the General Committee be reconsidered.'

A Committee of the Council devoted considerable care to a revision of the existing rules. The modified rules approved by the Council are now submitted for adoption to the present General Committee, whose constitution is, of course, not affected thereby. The most important of the proposed changes are that henceforth new claims to membership of the General Committee shall be forwarded to the Assistant General Secretary at least one month before the next ensuing Annual Meeting of the Association ; that these claims shall be submitted to the Council, whose decision upon them is to be final ; and that henceforth it is not the authorship of a paper in the Transactions of a scientific society which is alone to constitute a claim to membership of the General Committee, but the publication of any works or papers which have furthered the advancement of any of the subjects taken into consideration at the Sectional meetings of the Society.

Your Council has, also, had under its consideration the desirability of removing certain administrative inconveniences which arise from the circumstance that the next place of meeting is never decided upon by the General Committee until near the close of the actual meeting. They are of opinion that the arrangements of the General Officers would be greatly facilitated, and at the same time the convenience of those who invite the Association consulted, if the General Committee were to decide upon each place of meeting a year earlier than they do at present. In order to make the transition from the existing practice to the proposed one, your Council recommend that two of the invitations which will be received at the present meetings be accepted, one for 1871, and another for 1872.

It has often been urged that the Association labours under disadvantages in consequence of its not possessing central offices in London, where its Council and numerous committees could hold their meetings, where the books and memoirs which have been accumulating for years could be rendered accessible to Members, and where information concerning the Association's proceedings could be promptly obtained during the interval between annual meetings. The Council have had the subject under consideration, and in the event of the establishment at Kew being discontinued, they are prepared to recommend that suitable rooms, in a central situation, should be procured. The additional annual expenditure which this would involve would probably not exceed 150*l*.

The Council have added the names of Professor H. A. Newton and Professor C. S. Lyman, who were present at the Exeter meeting, to the list of corresponding members."

We append the new rules referred to in the Council's Report.

"*New Rules for Admission to the General Committee.*

The General Committee will in future consist of the following classes of members :—

CLASS A.—PERMANENT MEMBERS

1. Members of the Council, presidents of the Association, and presidents of sections for the present and preceding years, with authors of reports in the Transactions of the Association.

2. Members who, by the publication of works or papers, have furthered the advancement of those subjects which are taken into consideration at the sectional meetings of the Association. With a view of submitting new claims under this rule to the decision

of the Council, they must be sent to the assistant general secretary at least one month before the meeting of the Association. The decision of the Council on the claims of any member of the Association to be placed on the list of the General Committee to be final.

CLASS B.—TEMPORARY MEMBERS

3. The president for the time being, or, in his absence, one delegate representing him, of any scientific society publishing transactions. Claims under this rule to be sent to the assistant general secretary before the opening of the meeting.

4. Office-bearers for the time being, or delegates, altogether not exceeding three, from scientific institutions established in the place of meeting. Claims under this rule to be approved by the local secretaries before the opening of the meeting.

5. Foreigners and other individuals whose assistance is desired, and who are specially nominated in writing, for the meeting of the year, by the president and general secretaries.

6. Vice-presidents and secretaries of sections."

SECTIONAL PROCEEDINGS

SECTION A.—*Mathematical and Physical Science.*—President, Prof. J. Clerk Maxwell, F.R.S.

The president delivered the following address:—

AT several of the recent meetings of the British Association the varied and important business of the Mathematical and Physical Section has been introduced by an Address, the subject of which has been left to the selection of the president for the time being. The perplexing duty of choosing a subject has not, however, fallen to me. Professor Sylvester, the president of Section A at the Exeter meeting, gave us a noble vindication of pure mathematics by laying bare, as it were, the very working of the mathematical mind, and setting before us, not the array of symbols and brackets which form the armoury of the mathematician, or the dry results which are only the monuments of his conquests, but the mathematician himself, with all his human faculties directed by his professional sagacity to the pursuit, apprehension, and exhibition of that ideal harmony which he feels to be the root of all knowledge, the fountain of all pleasure, and the condition of all action. The mathematician has, above all things, an eye for symmetry; and Professor Sylvester has not only recognised the symmetry formed by the combination of his own subject with those of the former presidents, but has pointed out the duties of his successor in the following characteristic note:—

"Mr. Spottiswoode favoured the Section, in his opening address, with a combined history of the progress of mathematics and physics; Dr. Tyndall's address was virtually on the limits of physical philosophy; the one here in print," says Professor Sylvester, "is an attempted faint adumbration of the nature of mathematical science in the abstract. What is wanting (like a fourth sphere resting on three others in contact) to build up the ideal pyramid is a discourse on the relation of the two branches (mathematics and physics) to, and their action and reaction upon, one another—a magnificent theme, with which it is to be hoped that some future president of Section A will crown the edifice, and make the tetralogy (symbolisable by $A + A'$, $A \cdot A'$, AA') complete."

The theme thus distinctly laid down for his successor by our late President is indeed a magnificent one, far too magnificent for any efforts of mine to realise. I have endeavoured to follow Mr. Spottiswoode, as with far-reaching vision he distinguishes the systems of science into which phenomena, our knowledge of which is still in the nebulous stage, are growing. I have been carried by the penetrating insight and forcible expression of Dr. Tyndall into that sanctuary of minuteness and of power where molecules obey the laws of their existence, clash together in fierce collision, or grapple in yet more fierce embrace, building up in secret the forms of visible things. I have been guided by Professor Sylvester towards those serene heights

"Where never creeps a cloud, or moves a wind,
Nor ever falls the least white star of snow,
Nor ever lowest roll of thunder moans,
Nor sound of human sorrow mounts, to mar
Their sacred everlasting calm."

But who will lead me into that still more hidden and dimmer region where Thought weds Fact; where the mental operation of the mathematician and the physical action of the molecules are seen in their true relation? Does not the way to it pass through the very den of the metaphysician, strewed with the remains of former explorers, and abhorred by every man of science? It would indeed be a foolhardy adventure for me to take up the valuable time of the section by leading you into those speculations which require, as we know, thousands of years even to shape themselves intelligibly.

But we are met as cultivators of mathematics and physics. In our daily work we are led up to questions the same in kind with those of metaphysics; and we approach them, not trusting to the native penetrating power of our own minds, but trained by a long-continued adjustment of our modes of thought to the facts of external nature. As mathematicians, we perform certain mental operations on the symbols of number or of quantity, and, by proceeding step by step from more simple to more complex operations, we are enabled to express the same thing in many different forms. The equivalence of these different forms, though a necessary consequence of self-evident axioms, is not always, to our minds, self-evident; but the mathematician, who, by long practice, has acquired a familiarity with many of these forms, and has become expert in the processes which lead from one to another, can often transform a perplexing expression into another which explains its meaning in more intelligible language.

As students of physics, we observe phenomena under varied circumstances, and endeavour to deduce the laws of their relations. Every natural phenomenon is, to our minds, the result of an infinitely complex system of conditions. What we set ourselves to do is to unravel these conditions, and by viewing the phenomenon in a way which is in itself partial and imperfect, to piece out its features one by one, beginning with that which strikes us first, and thus gradually learning how to look at the whole phenomenon so as to obtain a continually greater degree of clearness and distinctness. In this process, the feature which presents itself most forcibly to the untrained inquirer may not be that which is considered most fundamental by the experienced man of science; for the success of any physical investigation depends on the judicious selection of what is to be observed as of primary importance, combined with a voluntary abstraction of the mind from those features which, however attractive they appear, we are not yet sufficiently advanced in science to investigate with profit.

Intellectual processes of this kind have been going on since the first formation of language, and are going on still. No doubt the feature which strikes us first and most forcibly in any phenomenon, is the pleasure or the pain which accompanies it, and the agreeable or disagreeable results which follow after it. A theory of nature from this point of view is embodied in many of our words and phrases, and is by no means extinct even in our deliberate opinions. It was a great step in science when men became convinced that, in order to understand the nature of things, they must begin by asking, not whether a thing is good or bad, noxious or beneficial, but of what kind is it? and how much is there of it? Quality and quantity were then first recognised as the primary features to be observed in scientific inquiry. As science has been developed, the domain of quantity has everywhere encroached on that of quality, till the process of scientific inquiry seems to have become simply the measurement and registration of quantities, combined with a mathematical discussion of the numbers thus obtained. It is this scientific method of directing our attention to those features of phenomena which may be regarded as quantities which brings physical research under the influence of mathematical reasoning. In the work of the section we shall have abundant examples of the successful application of this method to the most recent conquests of science; but I wish at present to direct your attention to some of the reciprocal effects of the progress of science on those elementary conceptions which are sometimes thought to be beyond the reach of change.

If the skill of the mathematician has enabled the experimentalist to see that the quantities which he has measured are connected by necessary relations, the discoveries of physics have revealed to the mathematician new forms of quantities which he could never have imagined for himself. Of the methods by which the mathematician may make his labours most useful to the student of nature, that which I think is at present most important is the systematic classification of quantities. The quantities which we study in mathematics and physics may be classified in two different ways. The student who wishes to master any particular science must make himself familiar with the various kinds of quantities which belong to that science. When he understands all the relations between these quantities, he regards them as forming a connected system, and he classes the whole system of quantities together as belonging to that particular science. This classification is the most natural from a physical point of view, and it is generally the first in order of time. But when the student has become acquainted with several different sciences, he finds that the mathematical processes and trains of reasoning in one science resemble those in another so much that his knowledge of the one science may be made a most useful help in the study of the other. When he examines into the reason of this, he finds that in the two sciences he has been dealing with systems of quantities, in which the mathematical forms of the relations of the quantities are the same in both systems, though the physical nature of the quantities may be utterly different. He is thus led to recognise a classification of quantities on a new principle, according to which the physical nature of the quantity is subordinated to its mathematical form. This is the point of view which is characteristic of the mathematician; but it stands second to the physical aspect in order of time, because the human mind, in order to conceive of different kinds of quantities, must have them presented to it by nature. I do not here refer to the fact that all quantities, as such, are subject to the rules of arithmetic and algebra, and are therefore capable of being submitted to those dry calculations which represent, to so many minds, their only idea of mathematics. The human mind is seldom satisfied, and is certainly never exercising its highest functions, when it is doing the work of a calculating machine. What the man of science, whether he be a mathematician or a physical inquirer, aims at is, to acquire and develop clear ideas of the things he deals with. For this purpose he is willing to enter on long calculations, and to be for a season a calculating machine, if he can only at last make his ideas clearer. But if he finds that clear ideas are not to be obtained by means of processes, the steps of which he is sure to forget before he has reached the conclusion, it is much better that he should turn to another method, and try to understand the subject by means of well-chosen illustrations derived from subjects with which he is more familiar. We all know how much more popular the illustrative method of exposition is found, than that in which bare processes of reasoning and calculation form the principal subject of discourse. Now a truly scientific illustration is a method to enable the mind to grasp some conception or law in one branch of science, by placing before it a conception or a law in a different branch of science, and directing the mind to lay hold of that mathematical form which is common to the corresponding ideas in the two sciences, leaving out of account for the present the difference between the physical nature of the real phenomena. The correctness of such an illustration depends on whether the two systems of ideas which are compared together are really analogous in form, or whether, in other words, the corresponding physical quantities really belong to the same mathematical class. When this condition is fulfilled, the illustration is not only convenient for teaching science in a pleasant and easy manner, but the recognition of the mathematical analogy between the two systems of ideas leads to a knowledge of both, more profound than could be obtained by studying each system separately.

There are men who, when any relation or law, however complex, is put before them in a symbolical form, can grasp its full meaning as a relation among abstract quantities. Such men sometimes treat with indifference the further statement that quantities actually exist in nature which fulfil this relation. The mental image of the concrete reality seems rather to disturb than to assist their contemplations. But the great majority of mankind are utterly unable, without long training, to retain in their minds the unembodied symbols of the pure mathematician; so that if science is ever to become popular and yet remain scientific, it must be by a profound study and a copious application of those principles of truly scientific illustration which, as we have seen, depend on the mathematical classification of quantities. There are, as I have said, some minds which can go on contemplating with satisfaction pure quantities presented to the eye by symbols, and to the mind in a form which none but mathematicians can conceive. There are others who feel more enjoyment in following geometrical forms, which they draw on paper, or build up in the empty space before them. Others, again, are not content unless they can project their whole physical energies into the scene which they conjure up. They learn at what a rate the planets rush through space, and they experience a delightful feeling of exhilaration. They calculate the forces with which the heavenly bodies pull at one another, and they feel their own muscles straining with the effort.

To such men impetus, energy, mass, are not mere abstract expressions of the results of scientific inquiry. They are words of power which stir their souls like the memories of childhood. For the sake of persons of these different types, scientific truths should be presented in different forms, and should be regarded as equally scientific, whether it appears in the robust form and the vivid colouring of a physical illustration, or in the tenuity and paleness of a symbolical expression. Time would fail me if I were to attempt to illustrate by examples the scientific value of the classification of quantities. I shall only mention the name of that important class of magnitudes having direction in space which Hamilton has called Vectors, and which form the subject-matter of the Calculus of Quaternions—a branch of mathematics which, when it shall have been thoroughly understood by men of the illustrative type, and clothed by them with physical imagery, will become, perhaps under some new name, a most powerful method of communicating truly scientific knowledge to persons apparently devoid of the calculating spirit. The mutual action and reaction between the different departments of human thought is so interesting to the student of scientific progress, that, at the risk of still further encroaching on the valuable time of the Section, I shall say a few words on a branch of science which not very long ago would have been considered rather a branch of metaphysics: I mean the atomic theory, or, as it is now called, the molecular theory of the constitution of bodies. Not many years ago, if we had been asked in what regions of physical science the advance of discovery was least apparent, we should have pointed to the hopelessly distant fixed stars on the one hand, and to the inscrutable delicacy of the texture of material bodies on the other. Indeed, if we are to regard Comte as in any degree representing the scientific opinion of his time, the research into what takes place beyond our own solar system seemed then to be exceedingly unpromising, if not altogether illusory. The opinion that the bodies which we see and handle, which we can set in motion or leave at rest, which we can break in pieces and destroy, are composed of smaller bodies which we cannot see or handle, which are always in motion, and which can neither be stopped nor broken in pieces, nor in any way destroyed or deprived of the least of their properties, was known by the name of the Atomic Theory. It was associated with the names of Democritus and Lucretius, and was commonly supposed to admit the existence only of atoms and void, to the exclusion of any other basis of things from the universe.

In many physical reasonings and mathematical calculations we are accustomed to argue as if such substances as air, water, or metal, which appear to our senses uniform and continuous, were strictly and mathematically uniform and continuous. We know that we can divide a pint of water into many millions of portions, each of which is as fully endowed with all the properties of water as the whole pint was, and it seems only natural to conclude that we might go on subdividing the water for ever, just as we can never come to a limit in subdividing the space in which it is contained. We have heard how Faraday divided a grain of gold into an inconceivable number of separate particles, and we may see Dr. Tyndall produce from a mere suspicion of nitrite of butyle an immense cloud, the minute visible portion of which is still cloud, and therefore must contain many molecules of nitrite of butyle. But evidence from different and independent sources is now crowding in upon us which compels us to admit that if we could push the process of subdivision still further we should come to a limit, because each portion would then contain only one molecule, an individual body, one and indivisible, unalterable by any power in nature. Even in our ordinary experiments on very finely divided matter we find that the substance is beginning to lose the properties which it exhibits when in a large mass, and that effects depending on the individual action of molecules are beginning to become prominent. The study of these phenomena is at present the path which leads to the development of molecular science. That superficial tension of liquids which is called capillary attraction is one of these phenomena. Another important class of phenomena are those which are due to that motion of agitation by which the molecules of a liquid or gas are continually working their way from one place to another, and continually changing their course, like people hustled in a crowd. On this depends the rate of diffusion of gases and liquids through each other, to the study of which, as one of the keys of molecular science, that unwearied inquirer into nature's secrets, the late Prof. Graham, devoted such arduous labour.

The rate of electrolytic conduction is, according to Wiedemann's theory, influenced by the same cause; and the conduction of heat in fluids depends probably on the same kind of action.

In the case of gases, a molecular theory has been developed by Clausius and others, capable of mathematical treatment, and subjected to experimental investigation; and by this theory nearly every known mechanical property of gases has been explained on dynamical principles, so that the properties of individual gaseous molecules are in the fair way to become objects of scientific research. Now Sir William Thomson has shown by several independent lines of argument, drawn from phenomena so different in themselves as the electrification of metals by contact, the tension of soap-bubbles, and the friction of air, that in ordinary solids and liquids the average distance between contiguous molecules is less than the hundred-millionth, and greater than the two-thousand-millionth of a centimètre. This of course is an exceedingly rough estimate, for it is derived from measurements, some of which are still confessedly very rough; but if, at the present time, we can form even a rough plan for arriving at a result of this kind, we may hope that as our means of experimental inquiry become more accurate and more varied, our conception of a molecule will become more definite, so that we may be able at no distant period to estimate its weight. A theory which Sir W. Thomson has founded on Helmholtz's splendid hydrodynamical theorems, seeks for the properties of molecules in the ring-vortices of a uniform, frictionless, incompressible fluid. Such whirling rings may be seen when an experienced smoker sends out a dexterous puff of smoke into the still air, but a more evanescent phenomenon it is difficult to conceive. This evanescence is owing to the viscosity of the air; but Helmholtz has shown that in a perfect fluid such a whirling ring, if once generated, would go on whirling for ever, would always consist of the very same portion of the fluid which was first set whirling, and could never be cut in two by any natural cause. The generation of a ring-vortex is of course equally beyond the power of natural causes, but once generated, it has the properties of individuality, permanence in quantity, and indestructibility. It is also the recipient of impulse and of energy, which is all we can affirm of matter: and these ring-vortices are capable of such varied connections, and knotted self-involutions, that the properties of differently knotted vortices must be as different as those of different kinds of molecules can be.

If a theory of this kind should be found, after conquering the enormous mathematical difficulties of the subject, to represent in any degree the actual properties of molecules, it will stand in a very different scientific position from those theories of molecular action which are formed by investing the molecule with an arbitrary system of central forces invented expressly to account for the observed phenomena. In the vortex theory we have nothing arbitrary, no central forces or occult properties of any other kind. We have nothing but matter and motion, and when the vortex is once started its properties are all determined from the original impetus, and no further assumptions are possible. Even in the present undeveloped state of the theory, the contemplation of the individuality and indestructibility of a ring vortex in a perfect fluid cannot fail to disturb the commonly received opinion that a molecule, in order to be permanent, must be a very hard body. In fact one of the first conditions which a molecule must fulfil is, apparently, inconsistent with its being a single hard body. We know from those spectroscopic researches which have thrown so much light on different branches of science, that a molecule can be set into a state of internal vibration, in which it gives off to the surrounding medium light of definite refrangibility—light, that is, of definite wave-length and definite period of vibration. The fact that all the molecules, say of hydrogen, which we can procure for our experiments, when agitated by heat or by the passage of an electric spark, vibrate precisely in the same periodic time, or, to speak more accurately, that their vibrations are composed of a system of simple vibrations having always the same periods, is a very remarkable fact. I must leave it to others to describe the progress of that splendid series of spectroscopic discoveries by which the chemistry of the heavenly bodies has been brought within the range of human inquiry. I wish rather to direct your attention to the fact that not only has every molecule of terrestrial hydrogen the same system of periods of free vibration, but that the spectroscope examination of the light of the sun and stars shows that in regions the distance of which we can only feebly imagine there are molecules vibrating in as exact unison with the molecules of terrestrial hydrogen as two tuning forks tuned to correct pitch, or two watches regulated to solar time. Now this absolute equality in the magnitude of quantities, occurring in all parts of the universe, is worth our consideration. The dimensions of individual natural bodies are either quite indeterminate, as in the case of planets, stones, trees, &c., or they vary within moderate limit, as in the case of seeds, eggs, &c.; but, even in these cases, small quantitive differences are met with which do not interfere with the essential properties of the body. Even crystals, which are so definite in geometrical form, are variable with respect to their absolute dimensions. Among the works of man we sometimes find a certain degree of uniformity. There is a uniformity among the different bullets which are cast in the same mould, and the different copies of a book printed from the same type. If we examine the coins, or the weights and measures, of a civilised country, we find a uniformity, which is produced by careful adjustment to standards made and provided by the State. The degree of uniformity of these national standards is a measure of that spirit of justice in the nation which has enacted laws to regulate them and appointed officers to test them. This subject is one in which we, as a scientific body, take a warm interest, and you are all aware of the vast amount of scientific work which has been expended, and profitably expended, in providing weights and measures for commercial and scientific purposes. The earth has been measured as a basis for a permanent standard of length, and every property of metals has been investigated to guard against any alteration of the material standards when made. To weigh or measure anything with modern accuracy, requires a course of experiment and calculation in which almost every branch of physics and mathematics is brought into requisition.

Yet, after all, the dimensions of our earth and its time of rotation, though, relatively to our present means of comparison, very permanent, are not so by any physical necessity. The earth might contract by cooling, or it might be enlarged by a layer of meteorites falling on it, or its rate of revolution might slowly slacken, and yet it would continue to be as much a planet as before. But a molecule, say of hydrogen, if either its mass or its time of vibration were to be altered in the least, would no longer be a molecule of hydrogen. If, then, we wish to obtain standards of length, time, and mass which shall be absolutely permanent, we must seek them not in the dimensions, or the motion, or the mass of our planet, but in the wave-length, the period of vibration, and the absolute mass of these imperishable and unalterable and perfectly similar molecules. When we find that here, and in the starry heavens, there are innumerable multitudes of little bodies of exactly the same mass, so many, and no more, to the grain, and vibrating in exactly the same time, so many times, and no more, in a second, and when we reflect that no power in nature can now alter in the least either the mass or the period of any one of them, we seem to have advanced along the path of natural knowledge to one of those points at which we must accept the guidance of that faith by which we understand that "that which is seen was not made of things which do appear." One of the most remarkable results of the progress of molecular science is the light it has thrown on the nature of irreversible processes,—processes, that is, which always tend towards, and never away from, a certain limiting state. Thus if two gases be put into the same vessel they become mixed, and the mixture tends continually to become more uniform. If two unequally heated portions of the same gas are put into the vessel, something of the kind takes place, and the whole tends to become of the same temperature. If two unequally heated solid bodies be placed in contact, a continual approximation of both to an intermediate temperature takes place. In the case of the two gases, a separation may be effected by chemical means; but in the other two cases the former state of things cannot be restored by any natural process. In the case of the conduction or diffusion of heat the process is not only irreversible, but it involves the irreversible diminution of that part of the whole stock of thermal energy which is capable of being converted into mechanical work. This is Thomson's theory of the irreversible dissipation of energy, and it is equivalent to the doctrine of Clausius concerning the growth of what he calls Entropy. The irreversible character of this process is strikingly embodied in Fourier's theory of the conduction of heat, where the formulæ themselves indicate a possible solution of all positive values of the time which continually tends to a uniform diffusion of heat. But if we attempt to ascend the stream of time by giving to its symbol continually diminishing values, we are led up to a state of things in which the formula has what is called a critical value; and if we inquire into the state of things the instant before, we find that the formula becomes absurd. We thus arrive at the conception of a state of things which cannot be conceived as the physical result of a previous state of things, and we find that this critical condition actually existed at an epoch not in the utmost depths of a past eternity, but separated from the present time by a finite interval. This idea of a beginning is one which the physical researches of recent times have brought home to us, more than any observer of the course of scientific thought in former times would have had reason to expect. But the mind of man is not like Fourier's heated body, continually settling down into an ultimate state of quiet uniformity, the character of which we can already predict; it is rather like a tree shooting out branches which adapt themselves to the new aspects of the sky towards which they climb, and roots which contort themselves among the strange strata of the earth into which they delve. To us who breathe only the spirit of our own age, and know only the characteristics of contemporary thought, it is as impossible to predict the general tone of the science of the future as it is to anticipate the particular discoveries which it will make. Physical research is continually revealing to us new features of natural processes, and we are thus compelled to search for new forms of thought appropriate to these features. Hence the importance of a careful study of those relations between mathematics and physics which determine the conditions under which the ideas derived from one department of physics may be safely used in forming ideas to be employed in a new department. The figure of speech or of thought by which we transfer the language and ideas of a familiar science to one with which we are less acquainted may be called scientific metaphor. Thus the words velocity, momentum, force, &c., have acquired certain precise meanings in elementary dynamics. They are also employed in the dynamics of a connected system in a sense which, though perfectly analogous to the elementary sense, is wider and more general. These generalised forms of elementary ideas may be called metaphorical terms in the sense in which every abstract term is metaphorical. The characteristic of a truly scientific system of metaphors is that each term in its metaphorical use retains all the formal relations to the other terms of the system which it had in its original use. The method is then truly scientific, that is, not only a legitimate product of science, but capable of generating science in its turn. There are certain electrical phenomena, again, which are connected together by relations of the same form as those which connect dynamical phenomena. To apply to these the phrases of dynamics with proper distinctions and provisional reservations is an example of a metaphor of a bolder kind; but it is a legitimate metaphor if it conveys a true idea of the electrical relations to those who have been already trained in dynamics. Suppose, then, that we have successfully introduced certain ideas belonging to an elementary science by applying them metaphorically to some new class of phenomena. It becomes an important philosophical question to determine in what degree the applicability of the old ideas to the new subject may be taken as evidence that the new phenomena are physically similar to the old. The best instances for the determination of this question are those in which two different explanations have been given of the same thing. The most celebrated case of this kind is that of the corpuscular and the undulatory theories of light. Up to a certain point the phenomena of light are equally well explained by both; beyond this point one of them fails. To understand the true relation of these theories in that part of the field where they seem equally applicable we must look at them in the light which Hamilton has thrown upon them by his discovery that to every brachystochrone problem there corresponds a problem of free motion, involving different velocities and times, but resulting in the same geometrical path. Professor Tait has written a very interesting paper on this subject. According to a theory of electricity which is making great progress in Germany two electrical particles act on one another directly at a distance, but with a force which, according to Weber, depends on their relative velocity, and according to a theory hinted at by Gauss, and developed by Riemann, Lorenz, and Neumann, acts not instantaneously, but after a time depending on the distance. The power with which this theory, in the hands of these eminent men, explains every kind of electrical phenomena must be studied in order to be appreciated. Another theory of electricity which I prefer denies action at a distance and attributes electric action to tensions and pressures in an all-pervading medium, these stresses being the same in kind with those familiar to engineers, and the medium being identical with that in which light is supposed to be propagated. Both these theories are found to explain not only the phenomena by the aid of which they were originally constructed, but other phenomena which were not thought of, or perhaps not known at the time, and both have independently arrived at the same numerical result which gives the absolute velocity of light in terms of electrical quantities. That theories, apparently so fundamentally opposed, should have so large a field of truth common to both is a fact the philosophical importance of which we cannot fully appreciate till we have reached a scientific altitude from which the true relation between hypotheses so different can be seen.

I shall only make one more remark on the relation between mathematics and physics. In themselves, one is an operation of the mind, the other is a dance of molecules. The molecules have laws of their own, some of which we select as most intelligible to us and most amenable to our calculation. We form a theory from these partial data, and we ascribe any deviation of the actual phenomena from this theory to disturbing causes. At the same time, we confess that what we call disturbing causes are simply those parts of the true circumstances which we do not know or have neglected, and we endeavour in future to take account of them. We thus acknowledge that the so-called disturbance is a mere figment of the mind, not a fact of nature, and that in natural action there is no disturbance. But this is not the only way in which the harmony of the material with the mental operation may be disturbed. The mind of the mathematician is subject to many disturbing causes, such as fatigue, loss of memory, and hasty conclusions; and it is found that from these and other causes mathematicians make mistakes. I am not prepared to deny that, to some mind of a higher order than ours, each of these errors might be traced to the regular operation of the laws of actual thinking; in fact we ourselves often do detect, not only errors of calculation, but the causes of these errors. This, however, by no means alters our conviction that they are errors, and that one process of thought is right and another process wrong. One of the most profound mathematicians and thinkers of our time, the late George Boole, when reflecting on the precise and almost mathematical character of the laws of right thinking as compared with the exceedingly perplexing, though perhaps equally determinate, laws of actual and fallible thinking, was led to another of those points of view from which science seems to look out into a region beyond her own domain. "We must admit," he says, "that there exist laws" (of thought) "which even the rigour of their mathematical forms does not preserve from violation. We must ascribe to them an authority, the essence of which does not consist in power, a supremacy which the analogy of the inviolable order of the natural world in no way assists us to comprehend."

SECTION B.—*Chemical Science.*—President, Professor Roscoe, F.R.S.

Report of the Committee on the Chemical Nature of Cast Iron. — Mr. David Forbes, F.R.S., reported on behalf of Professor Abel, Dr. Matthiessen and himself, that it had not been in their power, as a Committee, to make any important progress in the investigation of the chemical nature of cast iron during the past year. This was partly owing to the dismantled condition of the required apparatus. The Committee asked to be reappointed, so that the experiments might be resumed without much further delay.

On a New Chlorine Process without Manganese.—Mr. Henry Deacon, of Widnes. As the closing paragraph of Professor Roscoe's address briefly and clearly described the essential nature of Mr. Deacon's process, it is not necessary further to refer to it, except to state that hitherto Mr. Deacon has not succeeded in making the process a commercial success, and he prefers in the meantime to employ Mr. Weldon's process, though he is satisfied that his own will yet become practically applicable in the production of chlorine for the manufacture of bleaching powder.

SECTION C.—*Geology.*—President, Sir Philip de Malpas Grey Egerton, Bart., M.P., F.R.S.

The President departed from the practice of giving an introductory address, inasmuch as the time of the Section would be fully occupied with the reading and discussion of the papers to be submitted to it.

On the Glaciated Surface of Triassic Rocks near Liverpool.—Mr. G. H. Morton. The grooves on the rocks were of a uniform direction, 35° W. of N., and were due to the action of land ice.

Mr. Boyd Dawkins and Rev. Mr. Crosskey distinguished between the deposits produced by land ice and the later deposits containing stones dropped from floating ice. Professor Hawkins insisted that the granite boulders of Lancashire and Cheshire were derived from Ravenglass and not from Shapfell as stated; but Professor Williamson and Mr. Dawkins asserted that they had found Shap boulders in the clay.

On Sections of Strata between Huyton and St. Helens, exposed in cuttings in the railway now making between these places.—Dr. Ricketts

Report on Slicing and Photographing of Fossil Corals.—Mr. James Thompson. The continued investigation of these fossils had added greatly to the number of species, and the prepared sections had exhibited indications which supplied characters by which recognised forms could be better distinguished and false species eliminated. The report was accompanied with a large series of slices and exquisite photographs, full of promise for the future students of the group.

Report on the Fossils of Kiltorcan.—Mr. W. H. Bailey. These consisted of specimens of *Cyclopteris hibernica*, the remains of *Sagenaria bayleana*, a freshwater shell, *Anodonta jukesii* a crustacean *Protocaris mchenrici*, and scales of *Coccosteus* and *Glyptolepis*. Mr. R. H. Scott stated that Professor Heer had determined the specific identity of fossils from Bear Island collected by the Swedish Polar expedition in 1869, with those from Kiltorcan. Mr. Carruthers had recently examined the extensive series of Kiltorcan Fossils at Dublin, made by Mr. Bailey. They supplied information regarding all the parts of the *Sagenaria* and *Cyclopteris*, enabling investigators to deal with them almost as satisfactorily as with recent plants.

Fourth Report on the Leaf-beds of the Bagshot Series of Hampshire.—Mr. W. S. Mitchell.

On the Evidences of Recent Changes of Level on the Mediterranean Coast.—Mr. G. Maw. The coast structure, the general absence of sea-cliffs within the straits of Gibraltar, due to the shelving of the contour of the land under the sea were described. The inset current from the Atlantic was considered as indicating a general subsidence of the whole Mediterranean basin. The submarine springs passing through channels of sub-aerial origin occurring in the coast caverns implied a submergence of the coast line. Evidences of upheaval were to be found in the lagoons and flats which abound on the coast of Corsica, and which are covered with long ridges of shingle deposited, it was believed, by streams which debouched on the marsh when it was submerged. Further at Gibraltar a great deposit of stratified sand in Catalan Bay, showing a submergence of 700 feet; and at Cadiz, as well as at Tangiers on the opposite side of the basin, raised sea beaches were found. Sir Charles Lyell thought the out-set current balanced the inset one and destroyed the value of that current as supporting the notion of submergence. Prof. Busk referred to the memoirs of the late Mr. Smith, of Jordanhill, on the Mediterranean basin, and at length gave his own observations on the rock terraces and caverns of the rock of Gibraltar. There were three successive terraces exhibited only on the eastern side of the rock, showing that a barrier extended in recent geological times across the straits confining the Mediterranean to a higher level than the Atlantic. The changes described by Mr. Maw were really the last of a series indicated by the terraces at Gibraltar. Prof. Ansted thought the great power of enormously swollen streams so common in Corsica and neighbouring regions might account for some of the phenomena referred to by the author.

On the Organisation and Affinities of the Calamites of the Coal Measures.—Prof. Williamson. Numerous well preserved and novel forms of stems of *Calamites* were described with great minuteness, and estimates of their systematic position, based on these structures, were suggested. The author described two forms of nodullary rays, and a third set of radiating cellular structure connected with nodes. He considered the phragmas seen on Calamite stems to indicate the attachment of the roots. He suggested that this group of fossils must be separated from the *Equisetaceæ* and placed in an order to which he proposed to join the name *Calamitaceæ*. Mr. Cann thus doubted whether systematic determinations based on stem structure were of value. He preferred the evidence derived from the fruit, and that had been determined to differ very little from the fruit of the living *Equisetum*. The various points of difference pointed out by the author he considered to depend upon the more highly organised vegetative portions of the fossils, and exogenous growth of the stems. Mr. Bentham insisted on the value of fruit characters for determining systematic position, and urged the desirability of employing subgeneric names for imperfectly determined forms.

SECTION D.—*Biological Science*—President, Prof. Rolleston, M.D., F.R.S.

The President delivered the following address:—

AMONGST the duties of the President of a Section the delivery of an Address has in these latter days somehow come to be reckoned, and that I may interpose myself for but as short a time as possible between your attention and the papers announced to you for reading upon your list, I will begin what I have to say without any further preface.

I wish first to make a few observations as to the kind of preparation which is indispensable, as it seems to me as a preliminary to an adequate and intelligent comprehension of the problems of biology; or, in other words, to an adequate and intelligent comprehension of the discussions which will take place in this room and in the two other rooms which will be assigned to, and occupied by, the departments of Ethnology and Anthropology, and that of Physiology Pure and Proper, and Anatomy.

Having made these observations, I propose, in the second place, to enumerate the subjects which appear likely to occupy prominent places in our forthcoming discussions; and thirdly, I will, if your patience allows me, conclude with some remarks as to certain of the benefits which may be expected, as having been constantly observed to flow from a due and full devotion to biological study.

In the first place, then, I wish to say that though the problems of biology have much of what is called general interest; that is to say, of interest for all persons, attaching to them, as indeed how could they fail to have, including as they do the natural history of our own and of all other species of living organisms, whether animal or vegetable; some special preparation must be gone through if that general interest is to be thoroughly and intelligently gratified. I would compare the realm of biology to a vast landscape in a cultivated country of which extensive views may be obtained from an eminence; but for the full and thorough appreciation of which, if necessary, the gazer should himself have cultivated some portion, however small, of the expanse at his feet. It is, of course, a matter of regret to think that persons can be found who look upon an actual landscape without any thought or knowledge as to how the various factors which make up its complex beauty have come to co operate; how the hand of man is recognisable here; how the dip of the strata is visible there; and how this alternation is detectable in another place as the potent agency in giving its distinctive features; but I take it that real and permanent, however imperfect, pleasure may be drawn from the contemplation of scenery by persons who are ignorant of all these things. I do not think this is the case when we here deal with *coup d'œil* views of biology. The amount of the special knowledge, the extent of the special training need not necessarily be great, but some such special knowledge and training there must be if the problems and argumentations familiar to the professed biologist are to be understood and grasped by persons whose whole lives are not devoted to the subject, so as to form for them acquisitions of real and vital knowledge.

The microscope has done very much, indeed I may say it has done almost all that is necessary for enabling all persons to obtain the necessary minimum of practical and personal acquaintance with the arrangements of the natural world of which I am speaking. The Glass trough used in Edinburgh, the invention of John Goodsir, whose genius showed itself, as genius often does show itself, in simple inventions, can be made into a miniature aquarium. I purposely use a word which calls up the idea of an indoors apparatus, wishing thereby to show how the means I recommend are within the reach of all persons; and in it, lying as it does horizontally and underlaid as it is by a condenser, animal and vegetable organisms can be observed at any and at all hours, and continuously, and with tolerably high magnifying powers even whilst undisturbed. Thus is gained an admirable field for the self-discipline in question. The microscope which should be used by preference for exploring and watching such an aquarium should be such an one as is figured in Gaskell's work on the Microscope (p. 58, fig. 36), as consisting of a stem with a stout steadying base, and of a horizontal arm some nine inches long, which can carry indifferently simple lenses or a compound body. I think of the two it is better that the aquarium should be horizontal rather than the microscope; and those who think with me in this matter can nevertheless combine for themselves the advantages of the horizontal position of the instrument with those of the horizontal position of the objects observed by modifying the eye-piece in the way figured by Quekett (p. 381, fig. 266.) It would be a long task to enumerate fully all the scientific lessons which may be gathered, firstly, and all the educational agencies, secondly, which may be set and kept in movement by a person who possesses himself of this simple apparatus. The mutual interdependence of the animal and vegetable kingdoms, their *solidarité* as the French have called it, and as the Germans have called it too, copying herein the French, is one of the first lessons the observer has forced upon him; the influence of physical and chemical agencies upon the growth and development of living beings he soon finds strikingly illustrated; the mysterious process of development itself is readily observable in the eggs of the common water-snails and in those of freshwater fish, so that the way in which the various organs and systems of organs are chiselled out, built up, and finally packed together and stratified can be taken note of in these yet transparent representatives of these great sub-kingdoms which all the while are undisturbed and at peace; and all these points of large interest are but a few of many which these small means will enable anyone to master for himself in the concrete actuality, and thoroughly. The necessity for carefulness and truthfulness in recording what is seen, the necessity for keeping in such records what one observes quite distinct from what one infers, the necessity for patience and punctuality, are lessons which, from having a moral factor as well as a scientific one in their composition, I may specify as belonging to the educational lessons which may be gathered from such a course of study.

I have been speaking of the microscope as an instrument of education, and I wish before leaving the subject to utter one caution as to its use when this particular object of education is in view. If a subject is to act educationally, it must be understood thoroughly; and if a subject is to be understood thoroughly, it must form one segment or stretch in a continuous chain of known facts. Ἀρκτέον ἀπὸ τῶν γνωρίμων, said one of the greatest of educators; you must start from some previously existing basis of knowledge, and keep your communications with it uninterrupted if your knowledge is not to be unreal. And my concrete application of these generalities is contained in the advice that no sudden jump be made from observations carried on with the naked eye to observations carried on with the highest powers of the microscope. I am speaking of the course to be pursued by beginners, and beginners we all were once, and if our places are to be filled, and filled they will be, by better men as we hope than ourselves, they will have to be filled, we also hope, by men who have yet to become beginners. It is in their interest I have been speaking, and I say that a beginner does not ordinarily get an intelligent conception of the revelations of the microscope except in Bacon's words, *Ascendendo continenter et gradatium*, by progressing gradually from observations with the naked eye through observations dependent upon dissecting lenses, doublets and triplets, and the lower powers of the compound microscope, up to observations to be made with the higher and highest magnifying powers.

Unless he ascend by gradations from organs and systems, structures and tissues and cells, his wonder and admiration at the results of the ultimate microscope analysis, of what he had but a moment before knowledge of only in the concrete and by the naked eye is likely to be but unintelligent.

There are three other agencies which can be set into activity with nearly as little trouble and difficulty as the simple apparatus of which I have just been speaking, and which will, like it, secure as a necessary preliminary discipline "*propädeutik*" for their rational comprehension of Biology. These are Local Museums, Local Field Clubs, and Local Natural Histories. Local authorities, persons of local influence, should engage and interest themselves in the starting into life of the two former of these agencies, and if some such person as Gilbert White could be found in each county to write the Natural History of its Selborne, I know not at what cost it could not be well to retain his services. As the world is governed upon each particular area of its surface, there is to be found a certain percentage of the population occupying it who have special calls for particular lines of study. It is the interest of each county to have such means and such institutions in being as will render it possible to detect the existence of persons gifted with such special vocations, to give the talent thus entrusted to them fair scope for development, and to render smaller the risk of their dying mute and inglorious. A young man who is possessed of a talent for Natural Science and Physical Inquiry generally, may have the knowledge of this predisposition made known to himself and to others, for the first time, by his introduction to a well-arranged Loca Museum. In such an institution, either all at once, or gradually, the conviction may spring up within him that this investigation of physical emblems is the line of investigations to which he should be content to devote himself, relinquishing the pursuit of other things; and then, if the museum in question is really a well-arranged one, a recruit may be thereby won for the first and growing army of physical investigators, and one more man saved from the misery of finding, when he has been taken into some other career, that he has somehow or other mistaken his profession, and made of his career one life-long mistake. Here comes the question. What is a well-arranged museum? The answer is, a well-arranged museum for the particular purpose of which we are speaking, is one in which the natural objects which belong to the locality, and which have already struck upon the eye of such a person as the one contemplated, are clearly explained in a well-arranged catalogue. The curiosity which is the mother of science is not awakened for the first time in the museum, but out of doors, in the wood, by the side of the brook, on the hillside, by scarped cliff and quarried stone; it is the function of the museum, by rendering possible the intellectual pleasure, which grows out of the surprise with which a novice first notes the working of his faculty of inspiration, to prevent this curiosity from degenerating into the mere woodman's craft of the gamekeeper, or the rough empiricism of the farmer. The first step to be taken in a course of natural instruction, is the providing of means whereby the faculties of observation and of verification may be called into activity; and the first exercise the student should be set down to is that of recognising in the actual thing itself, the various properties and peculiarities which some good book or some good catalogue tells him are observable in it. This is the first step, and, as in some other matters, *ce n'est que le premier pas qui coute*. And it need not cost much. There is a name familiar to Section D, and indeed not likely for a long while to be forgotten by members of the British Association generally, extrinsic means as well as the intrinsic merits of the well-loved man conspiring to keep his memory fresh among us, and the bearer of that name, Edward Forbes, has left it as his opinion that "It is to the development of the provincial museums that, I believe, we must look in future for the extension of intellectual pursuits throughout the land." (Lecture "On the Educational Uses of Museums," delivered at the Museum of Practical Geology and published in 1853. Cited by Toynbee, "Hints on the Formation of Local Museums," 1863, p. 46.) With the words of Edward Forbes I might do well to end what I have to say, but I should like to say a word as to the policy of confining the contents of a local museum to the natural-history specimens of the particular locality. No doubt the first thing to be done is the collection of the local specimens, and this alike in the interest of the potential Cuviers and Hugh Millers, who may be born in the district, and in the interest of the man of science who may visit the place when on his travels. But so long as a specimen from the antipodes or from whatever corner of our world be really valuable, and be duly catalogued before it is admitted into the museum, so that the lesson it has to teach may be learnable, I do not see my way towards advising that foreign specimens be excluded. It is to my mind more important that all specimens should be catalogued as soon as received, than that any should be rejected when offered.

I must not occupy your time further with this portion of my address. Let me first say that a person who wishes to know what a Field Club can do for its members, and not for them only, but for the world at large, will do well to purchase one, or any number more than one, of the Transactions of the Tyneside Naturalist's Field Club; and that if there be any person who thinks that White's Selborne relates to a time and place so far off that there can be no truth in the book, and who yet would like to try upon himself the working of the fourth disciplinary agencies of which I have spoken; that, namely, of sending some Local Natural History on the spot of which it treats, and comparing it with the things themselves *in situ*, let him repair to Weymouth, and work and walk up and down its cliffs and valleys with Mr. Darwin's book in his hands.

I shall not be suspected in this place and upon this occasion, nor, as I hope, upon any other, of a wish to depreciate the value of scientific instruction as an engine for training the mind. But neither, on the other hand, should I wish to depreciate the value of literary culture, my view of the relations of these two gymnastics of the mind being the very simple, obvious, and natural one that they should be harmoniously combined—

Alterius sic
Altera sic poscit opem vis, et conjuvat amice.

I know it may be said that there are difficulties in the way, and especially practical difficulties, but I have always observed that people who are good at finding out difficulties, and especially practical difficulties, are like people who are good at finding out excuses,—good at finding out very little else. The various ways of getting over these difficulties are obvious enough, and have been hinted at, or fully expressed by several writers of greater or less authority on many occasions. It is, however, of some consequence that I should here say what I believe has not been said before, namely, that a purely and exclusively literary education imperfect and one sided, as it is, is still a better thing than a system of scientific instruction (to abuse the use of the word for a moment) in which there should be no courses of practical familiarising with natural objects, verification, and experimentation. A purely literary training, say, in dialectics, or what we are pleased to call logic, to take a flagrant and glaring instance first, does confer certain lower advantages upon the person who goes through it without any discipline in the practical investigation of actual problems. By going through such a training attentively, a man with a good memory and a little freedom from over-scrupulousness, can convert his mind into an arsenal of quips, quirks, retorts, and epigrams, out of which he can, at his own pleasure, discharge a mitraille of chopped straw and chaff-like arguments, against which no man of ordinary fairness of mind can, for the moment, make head. It is true that such sophists gain this dexterity at the cost of losing, in every case, the power of fairly and fully appreciating or investigating truth; of losing in many cases the faculty of sustaining and maintaining serious attention to any subject; and of losing in some cases even the power of writing. A well-known character in an age happily, though only recently, gone by, who may be taken as a Cæsar worthy of such Antonies, used to speak of a pen as his torpedo. Still they have their reward, they succeed now and then in convincing juries, and they are formidable at dinner tables. It would not be fair, however, not to say that a purely literary training can do much better things than this. By a purely Classical Education a man, from being forced into seeing and feeling that other men could look upon the world, moral, social and physical, with other (even if not with larger) eyes than ours, attains a certain flexibility of mind which enables him to enter into the thoughts of other and living men, and this is a very desirable attainment. And, finally, though I should be sorry to hold with a French writer that the style makes the man, the benefit of being early familiarised with writings which the peculiar social condition of the classical times, so well pointed out by De Tocqueville (*De la Démocratie en Amérique*), conspired and contributed not a little to make models of style, is not to be despised. Such a familiarity may not confer the power of imitating or rivalling such compositions, but it may confer the power of appreciating their excellences, the one power appearing to us to be analogous to the power of the experimenter, and the other to that of the pure observer in Natural Science; and we should undervalue neither.

Masters of Science, it must be confessed, are not always masters of style; let not the single instance of last night tempt you to generalise, it was but a single instance, the writings of the man whom we in this Section are most of us likely to look upon as our master in Science have been spoken of by our President in his recently published volume as "intellectual pemmican;" and if scientific reading and teaching is to be divorced from scientific observation of natural objects and processes, it is better that a man, young or old, should have in his memory something which is perfect of its kind, entire and unmutilated, such as the opening of sentences of the *Brutus* of Cicero, which Tacitus, I think, must have had in his memory when he wrote his obituary of Agricola, or as the opening sentences of the *Republic* of Plato, or the conclusion of the *Ajax* or Sophocles, than that he should have his memory laden with a consignment of scientific phrases which *ex hypothesi* have for him no virtual reality. I have already said that I am strongly of opinion that literary should always be combined with scientific instruction in a perfect educational course; these somewhat lengthy remarks refer therefore only to systems in which it is proposed that we should have not only a bifurcation but a radical separation of studies and students, and the moral of this may be summed up by saying that a purely scientific education must be a thoroughly practical one, familiarising the student with actual things as well as with words and symbols. It was upon the solid ground that Antæus learnt the art of wrestling, it was only when he allowed himself to be lifted from it that he was strangled by Hercules.

Coming now to the second part of my address, I beg to say that the word Biology is at present used in two senses, one wider, the other more restricted. In this latter sense the word becomes equivalent to the older, and till recently more currently used word "Physiology;" it is in the wider sense that the word is used when we speak of this as being the section of Biology; and this wider sense is a very wide one, for it comprehends animal and vegetable Physiology and Anatomy, firstly; Ethnology and Anthropology, secondly; and thirdly, Scientific Zoology and Classificatory Botany, inclusively of the Distribution of Species. It may have been possible in former times for a single individual of great powers of assimilation to keep himself abreast of, and on a level with, the advance of knowledge along all these various lines of investigation; but in those times knowledge was not, and could not, owing to difficulties of intercommunication, the dearness of books, the costliness or the non-existence of instruments, have been increased at the rate at which it is now being, year by year, increased; and the entire mass of actually existing and acquired knowledge was of course much smaller, though man's power of mastering it was no smaller than at present. It would now be an indication of very great ignorance in anybody which should pretend that his own stock of information could furnish him with something in each one of the several departments of knowledge I have just mentioned, which should be worthy of being laid before such an assembly as this. As will have been expected, I shall not presume to do more than glance at the vegetable kingdom, large as is the space in the landscape of life which it makes. What I do propose to do is merely to draw your attention to a very few of the topics of leading interest, which are at the present moment being, or rather will shortly begin to be, discussed by experts in the Department of Physiology and Anatomy; in the Department of Ethnology and Anthropology; and thirdly, in the Department of Scientific Zoology.

Under the head and in the Department of Physiology Proper and Anatomy, our list of papers and, I am happy to add, the circle of faces around us suggests to us the following subjects as being the topics of main interest for the present year: the questions of Spontaneous Generation; that of the influence of organised particles in the production of disease; that of the influence of particular nervous and chemical agencies upon functions; that of the localisation of cerebral functions; that of the production and indeed of the entire *role* in the economy of creation of such substances as fat and albumen; and, finally, that of the cost at which the work of the animal machine is carried on.

The question of Spontaneous Generation touches upon certain susceptibilities which lie outside the realm of science. In this place, however, we have to do only with scientific arguments, and I trust that the Section will support the Committee in their wish to exclude from our discussions all extraneous considerations. Truth is one; all roads which really lead to it will assuredly converge sooner or later; our business is to see that the one we are ourselves concerned with is properly laid out and metalled.

Upon this matter I am glad to be able to fortify myself by two authorities; and first of these I will place an utterance of Archbishop Whately, which may be found in the second volume of his Life, pp. 66–68, æt. 57, an. 1844. "A person possessing real faith will be fully convinced that whatever suppressed physical fact appears to militate against his religion will be proved by physical investigation either to be unreal or else reconcilable with his religion. If I were to found a church, one of my articles would be that it is not allowable to bring forward Scripture or any religious considerations at all to prove or disprove any physical theory or any but religious and moral considerations." My second quotation shall be taken from the great work of one of the first, as I apprehend, of living theologians, John Macleod Campbell, "The Nature of the Atonement," pp. xxxii.–xxxiii. Introd., and it runs thus:—There are "other minds whose habits of pure scientific investigation are to them a temptation to approach the claim of the Kingdom of God on our faith by a wrong path, causing them to ask for a kind of evidence not proper to the subject, and so hindering their weighing fairly what belongs to it. No scientific study of the phenomena which imply a reign of law could ever have issued in the discovery of the kingdom of God. But neither can it issue in any discovery which contradicts the existence of that kingdom ; nor can any mind in the light of the kingdom of God hesitate to conclude that if such seeming contradictions arise there is implied the presence of error either as to the facts or as to conclusions from the facts." These are valuable words and weighty testimonies. But in a matter of this importance one must not forbear to point out what may seem to be wanting even in the dicta of such men as the two I have quoted. Neither of them have allowed the possibility of error attaching itself to the utterances of more than one of the two parties in such issues as those contemplated. Neither appears to have thought of the cases in which religious men, if not theologians, have brought war on the world because of the offences they have with ill-considered enunciations created. And, whilst fully sympathising with all that the Archbishop and Mr. Campbell have said, I must say that they appear to me to have left something unsaid, and this something may be wrapped up in the caution that there may be faults on both sides. But at any rate this Section cannot be considered a fit place for the correction of errors save of the physical kind ; and all other considerations are for this week and in this place extraneous. In some other week or in some other place it will be, if it has not already been, our duty to give them our best attention.

To come now to the kind of considerations which are the proper business of Section D, let me say that for the discussion of the question of Spontaneous Generation very refined means of observation, and, besides these, very refined means of experimentation, are necessary. And I shall act in the spirit of the advice I have already alluded to as given to the world by one of her greatest teachers, if I put before you a simple but a yet undecided question for the solution of which analogous means of a far less delicate character would appear to be, but as yet have not proved themselves to be, sufficient. Thus shall we come to see very plainly some of the bearings, and a few of the difficulties, of the more difficult of the two questions. What an uneducated person might acquiesce in hearing spoken of as Spontaneous Generation, takes place very constantly under our very eyes, when a plot of ground which has for many years, or even generations, been devoted to carrying some particular vegetable growth, whether grass or trees, has that particular growth removed from it. When such a clearing is effected, we often see a rich or even a rank vegetation of a kind previously not growing on the spot spring up upon it. The like phenomenon is often to be noted on other surfaces newly exposed, as in railway cuttings and other escarpments, and along the beds of canals or streams, which are laid bare by the turning of the water out of its channel. Fumitory, rocket, knotgrass, cowgrass, *Polygonum aviculare*, and other such weeds, must often have been noted by every one of us here in England as coming into and occupying such recently disturbed territories in force ; whilst in America the destruction of a forest of one kind of wood, such as the oak or the chestnut, have often been observed to be followed by an upgrowth of young forest trees of quite another kind, such as the white pine, albeit no such tree had been seen for generations growing near enough to the spot to make the transport of its seeds to the spot seem a likely thing. In one case referred to by Mr. Marsh, the hickory, *Carya porcena*, a kind of walnut, was remarked as succeeding a displaced and destroyed plantation of the white pine. Now the advocates of Spontaneous Generation must not suspect me of hinting that there is any question, except in the minds of the grossly ignorant, of the operation of any such agency as spontaneous generation here; no one would suggest that the seeds of the *Polygonum aviculare*, to say nothing of those of the Hickory, were produced spontaneously ; but what I do say is, that the question of how those seeds came there is just the very analogue of the one which they and their opponents have to deal with. And it is not definitely settled at this very moment. Let us glance at the instructive historical parallel it offers. For the very gross and palpable facts of which I have just spoken there are two explanations offered in works of considerable authority. The one which has perhaps the greatest currency and commands the largest amount of acceptance is the one which, in the words of De Candolle, regards *la couche de terre végétale d'un pays comme un magazin de graines*, and supposes that in hot summers and autumns, such as the present, the fissures in the ground, which have proved so fatal this year to the young partridges, swallow up a multitude of seeds, which are restored again to life when the deep strata into which they are thus introduced, and in which they are sealed up as the chasms close up, come in any way to be laid open to the unimpeded action of the sun and moisture. Squirrels, again, and some birds resembling herein the rodent mammalia, bury seeds and forget to dig them up again ; and it is supposed that they may bury them so deep as to be protected from the two physical agencies just mentioned. Now Germination cannot take place in the absence of oxygen, and I would add that well-sinkers know to their cost how often the superficial strata of the earth are surcharged with carbonic acid. The rival explanation and the less popular—I do not say the less scientific—looks to the agency of transportation as occurring constantly, and sufficing to explain the facts. By accepting this explanation, we save ourselves from running counter to certain experiments, some of which were carried out, if I mistake not, under the auspices of this Section (see British Assoc. Reports), and which appear to curtail considerably the time during which seeds retain their vitality, and to multiply considerably the number of conditions which must be in force to allow of such retention for periods far shorter than those which have to be accounted for. A better instance of the expediency of checking the interpretations based merely upon observations however accurately made by putting into action experiments, cannot be furnished than by recording the fact put on record by Mr. Bentham, when discussing this question in his last year's address to the Linnæan Society.

"Hitherto direct observation has, as far as I am aware, only produced negative results, of which a strong instance has been communicated to me by Dr. Hooker. In deepening the lake in Kew Gardens, they uncovered the bed of an old piece of water, upon which there came up a plentiful crop of *Typha*, a plant not observed in the immediate vicinity ; and it was therefore concluded that the seed must have been in the soil. To try the question, Dr. Hooker had six Ward's cases filled with some of the soil remaining uncovered close to that which had produced the *Typha*, and carefully watched ; but not a single *Typha* came up in any one of them." (Note in President's address May 24th, 1869, page 72 of Linnæan Society's Proceedings.)

To this I would add that experiments with a positive result, and that positive result in favour of the second hypothesis, if hypothesis it can be called, are being constantly tried in our colonies for us, and on a large scale. I had taken and written here of the *Polygonum aviculare*, the "knot" or "cowgrass"—having learnt on the authority of Dr. Hooker and Mr. Travers (see Natural History Review, January 1864, p. 124, Oct. 1864, p. 619), that it abounds in New Zealand, along the roadside, just as it does in England—as a glaring instance, and one which would illustrate the real value of the second explanation even to an unscientific man and to an unassisted eye. But on Saturday last I received by post one of those evidences which make an Englishman proud in thinking that whithersoever ships can float thither shall the English language, English manners, and English Science be carried, in the shape of the second volume of the Transactions of the New Zealand Institute, full like the first, from beginning to the last page with thoroughly good matter. In that volume, having looked at its table of contents, I turned to a paper by Mr. T. Kirk on the Naturalized Plants of New Zealand, and in this, at p. 142, I find that Mr. T. Kirk prefers to regard the *Polygonum aviculare* of New Zealand as indigenous in New Zealand. Hence that illustration which would have been a good one falls from my hands. And I must in fairness add, that because one agency is proved to be a *vera causa*, it is not thereby proved that no other can by any possibility be competent simultaneously to produce the same effect, whatever the Schoolmen with the law of Parsimony ringing in their ears may have said to the contrary. I have dwelt upon this subject at this length with the purpose of showing how much difficulty may beset the settlement of even a comparatively simple question which involves only the use of the unassisted eye, or at most of a simple lens. The *a fortiori* argument, I leave you to draw for yourselves with the simple remark, that the question of Spontaneous Generation is now at least one to be decided by the microscope, and by the employment of its highest powers in alliance with other apparatus of all but equal complexity.

We come, in the second place, to say a word as to the extent of the influence which organic and living particles, of microscopic minuteness but solid for all that, have been supposed, and in some instances at least have been proved, to exercise upon the genesis and genesiology of disease, and so upon the fortunes of our race, and our means for bettering our condition, and that of our fellows. I need not refer to Dr. Sanderson's valuable Report (just published in the Privy Council's Medical Officer's Blue Book, Twelfth Report, 1870, p. 229, upon those contagion particles which he proposes to call by the convenient name, slightly modified from one invented by Professor Bichamp, of Microzymes ; for Dr. Sanderson is here to refer to the matter for himself and for us ; and when this meeting is over we shall all do well to lay to heart what he may tell us here and now, and besides this, to study his already printed views upon the matter. It may be perhaps my business to remind you that these views, so far as they are identical with Professor Halliers' as to the importance of those most minute of living organisms, the micrococcus of his nomenclature, the microzymes of Mr. Simon's Blue Book, were passed in review as to their botanical correctness by a predecessor of mine in this honourable office — namely, by the Rev. J. M. Berkeley, at the meeting held two years ago at Norwich ; and that some of the bearings of the theory and of the facts, howsoever interpreted, upon the Theory of Evolution, were touched upon by Dr. Child in his interesting volume of Physiological Essays, p. 148, published last year. It would not perhaps be exactly my business to express my dissent from any of these results or views put forward by any of these investigators I have mentioned ; but I wish to point out to the general public that none of these inquirers would affirm that the agencies shown by them to be potent in the causation of certain diseases were types and models of the agencies which are, did we but know it, could we but detect them, potent in the causation of all diseases. Many diseases, though possibly enough not the majority of the strictly infectious diseases, are due to material agents quite distinct in nature from any self-multiplying bodies, cytoid or colloid. To say nothing of the effects of certain elements—and elements, it will be recollected, in their singleness and simple atomicity have, as the world happens to be constituted and governed, never been honoured with the office of harbouring life—which when volatised, as mercury, arsenic, and phosphorus may be, or indeed which, when simply dissolved, may be most ruinous to life, there are, I make no doubt, animal poisons produced in and by animals, and acting upon animal bodies which are neither organised nor living, neither cytoid nor colloid. Dr. Charlton Bastian is not likely to underrate the importance of such agents, howsoever produced, in the economy, or rather in the waste, of Nature, yet from his very careful record of his own very closely observed and personal experience we can gather that he would not demur to conceding that non-vitalised, however much animalised, exhalations may be only too powerful in producing attacks, and those sudden and violent and fever-like attacks, of disease. Dr. Bastian tells us (Phil. Trans. for 1866, vol. 196, pt. ii. pp. 583-584) that whensoever he employed himself in the dissection of a particular nematoid worm, the *Ascaris megalociphala*, he found occasion to observe, and that in himself, and very closely, the genesiology of a spasmodic and catarrhal affection, not unlike hay-fever as it seems to me, but under circumstances which appear to preclude the possibility of any living organisms being the cause of it as they have been supposed, and by no less an authority than Helmholtz, to be of the malady just mentioned. For in Dr. Bastian's case this affection was produced, not only when the *Ascaris megalocephala* was dissected when fresh, but "after it had been preserved *in methylated spirit for two years, and even then macerated in a solution of chloride of lime for several hours before it was submitted to examination.*" Could any microzyme or megalozyme have survived such an amount of antizymotic treatment—such a pickling as this ? This is not exactly a medical association, and I have entered upon this discussion not altogether without a wish to show how subjects of apparently the most purely scientific and special interest, as Mycology and Helminthology, the natural history, that is to say, and the morphology of the lowest plants and of the lowest vermes, may come when we least expect it, come or be brought to bear upon matters of the most immediate and pressing practical importance. And in this spirit I must say a word upon the way in which the pathology of snake-bites bears upon the matters I have been speaking of, and the extent of the debt which practical men owe to such societies as our Ray Society, and to such publications as their colossal volume on the snakes of India, in which Dr. Günther's views as to the real history of the striking and terrible yet instructive phenomena alluded to, are combined ("Snakes of India," Ray Society, 1864, p. 167). That the snake-poison is an animal poison is plain enough; that it is fatal to men and animals everybody knows; but I rather think that these two facts relative to it are not equally notorious, rich in light though they be, viz., that the potency of this particular animal poison varies in direct ratio to the quantity imbibed or infused, just as though it were so much alcohol, or so much alcoholic tincture of musk or cantharides ; or secondly, that its potency varies in direct ratio to another varying standard, viz., the size of the animal producing it. Now, the vaccine matter from the arm of a child is as potent as the vaccine matter from the arm of any giant might be, if such a large creature could in these days escape the operation of the vaccination laws ; and whether a grain or a gramme of it be used, will make no difference, so long as it be used rightly. There is a contrast indeed between the *modus operandi* of these two animal poisons. I would add that in the Edinburgh *Monthly Medical Journal* for the present month there is a very valuable paper, one of a series of papers, indeed, of the like character, by Dr. Fayrer, where at page 247, among much of anatomical and other interest, I find the following important statement:—"This poison may be diluted with water, or even ammonia or alcohol, without destroying its deadly properties. It may be kept for months or years, dried between slips of glass, and still retain its virulence. It is capable of absorption through delicate membranes, and therefore it cannot be applied to any mucous surfaces, though no doubt its virulence is much diminished by endosmosis.* It appears to act by a catalytic form ; that is it kills by some occult influence on the nerve centres." There is such a thing as an ignorance which is wiser than knowledge, *for the time*, of course, only ; such an ignorance is wisely confessed to in these words of Dr. Fayrer's :—An explanation may be true for some, yet not thereby necessarily for all, the facts within even a single sphere of study, even a true explanation may have but a very limited application, as a tangent cannot touch a circle at more than a single point. The memoirs, published in our own reports by Dr. R. W. Richardson, on the action of the nitrites, and those published by Dr. A. Crum Brown and Dr. Fraser, there and elsewhere, on the connection between chemical constitution and physiological action, deserve especial study as bearing on the other side of this discussion ; whilst Prof. Lister's papers show how the reference of certain diseases to inhalistic agencies may become o most useful importance in practice. There exists, as is wel known, a tendency to involve all Physiological into Physico-chemical phenomena ; undoubtedly many have been, and some more may still remain, to be so ordered, but the public may rest assured that in the kingdom of Biology no desire for a rectification of frontiers will ever be called out by any such attempts at, or successes in the way of, encroachment ; and that where physics and chemistry can show that physico-chemical agencies are sufficient to account for the phenomena, there their claim upon the territory will be acceded to, as in the cases we have been glancing at ; and where such claims cannot be established and fail to come up to the quantitative requirements of strict science, as in the cases of continuous and of discontinuous development or self-multiplication of a contagious germ, and in some others, they will be disallowed.

(*To be Continued.*)

SECTION E.—*Geographical Science.*—Sir Roderick Murchison, K.C.B., F.R.S., President.

In opening his address, after alluding to the more recent geographical discoveries which had been made, and to the geographical expeditions now in progress in Asia and Africa, and of which a much fuller account is to be found in his recent address to the Geographical Society, the President passed on to the subject of Deep Sea Soundings and their relation to Geology. Here he said he dissented most strongly from the views held by Dr. Carpenter and others, that in a broad sense we may be said to be in the Cretaceous epoch, since so many of the marine forms met with were similar to, if not identical with, those which lived at that time. Thus he says, "May we not indeed by a similar bold hypothesis affirm that we still live in the older Silurian period? for albeit no bony fishes then existed, many globigerinæ and creatures of the lowest organisation have been found in these old rocks associated with terebratulidæ and dingulæ, the *generic* forms of which still live." Surely we need not point out to Sir Roderick Murchison that *generic* forms are one thing and *identical species* another, and that whilst the former are evidence of *similarity of condition*, the latter are evidence of *persistency of conditions.* From this Sir Roderick passed to the subject of the Physical Geography of the Ocean, and paid a great compliment to the valuable work lately published by Mr. J. K. Laughton, on "Physical Geography in its Relation to the Prevailing Winds and

* Diapedesis may account for what virulence remains, and the poison may therefore possibly be a cytoid.

Currents;" and after referring to the various papers about to be read to the Section, he concluded his address by expressing his conviction, founded on all recent information, that Dr. Livingstone was alive and amongst friendly natives, and that Sir Samuel Baker would shortly meet him with fresh stores of provisions, &c.

SECTION F.—*Economic Science.*—President, Prof. W. Stanley Jevons.

In the President's address, which was of some length, after some introductory remarks, tending to show that the Economic Section has to deal with a class of subjects capable of strictly scientific treatment, and that the social are the necessary complement to the physical sciences, Professor Jevons proceeded to state that in this kingdom during the last thirty or forty years we had tried a mighty experiment, and to a great extent had failed. The growth of the arts and manufactures and the establishment of free trade had opened the widest means of employment, and brought an accession of wealth previously unknown; the frequent remission of taxes had left the working classes in fuller enjoyment of their wages; the poor laws had been reformed and administered with care, and the emigration of millions might well have been expected to leave room for those that remained. Nevertheless within the last few years we had seen pauperism almost as prevalent as ever, and the slightest relapse of trade threw whole towns and classes of people into a state of destitution little short of famine. This state of things was exactly what Malthus would have predicted of a population which, while supplied with easily earned wealth, is deprived of education and bribed by the mistaken benevolence of the richer classes into a neglect of the future. We now had an Education Act, but this ought not to withdraw attention from many other causes of evil still existing in full force. Amongst these was the mistaken humanity of charitable people. The amount extended by the upper to the lower classes was almost incredible; but it did more harm than good. The helpless poor were most numerous precisely in those towns where charitable people and institutions most abounded. But far worse than private charity were the innumerable small charities established by the bequests of mistaken testators. It would be well worthy of Mr. Goschen's attention whether all such charities might not be transferred to the care of the guardians of the poor, so as to be brought under the supervision of the Poor Law Board, and distributed in accordance with sound principles. The State, which undertook the ultimate support of the poor, was bound to prevent its own efforts to reduce pauperism from being frustrated, as they are at present. As regards medical charities, Professor Jevons said no one could for a moment propose to abolish hospitals and numerous institutions absolutely necessary for the relief of accidental suffering; but no working man was solvent who did not lay aside so much of his wages as would meet the average amount of sickness falling to the lot of the man and his family. So it was not easy to determine this amount. There were or might be sick clubs which would average the inequalities of life. Hospitals need not be self-supporting, and in cases of severe and unforeseen suffering they might give the most lavish aid; but they ought not to relieve slight and ordinary disease without a contribution from those benefited. With respect to the Poor Law medical service, every one admitted that where medical aid is given it ought to be good and sufficient; but, on the other hand, the better we make that service the more do we tend to increase and perpetuate that want of self-reliance and providence which is the crowning defect of the poorer classes. In this and many other cases we ought to regulate our humane impulses by a stern regard to the real results of our actions. Referring to the financial policy of the kingdom, Professor Jevons pointed out that in Cobden's sense free trade is now actually achieved. For the future the remission of customs duties would be grounded on other motives than it has often been in the past. It was a mistake to suppose that foreign trade ought to be encouraged before everything else. The internal trade and industry of the country were at least equally deserving of attention, and it might be that there were stamp duties, licence duties, rates, and other taxes which, in proportion to the revenue they returned, did far more injury than any customs duties now remaining. The question of local taxation was one which especially required attention. The amount raised by local rates was more than equal to the whole of the customs duties; nevertheless they continued to be levied substantially according to an Act passed in the reign of Queen Elizabeth. There was sure to be a continuous increase of local taxation. Turning to the question of surplus revenue, Professor Jevons said that there probably now existed no grievous pressure of taxation, and no considerable inequality as regards the several classes of the people. He calculated that average families spending 40*l.*, 85*l.*, and 500*l.* a year, consuming moderate quantities of tobacco and spirituous liquors, all paid about 10 per cent. of their income in general or local taxation. Only the taxation of the middle classes was mostly unavoidable, whereas at least half the taxation of the poorer classes depended upon the amount of tobacco and spirituous liquors consumed. The present incidence of taxation, therefore, was such that it seemed inexpedient to proceed further in the reduction of the customs and excise duties. To do so would be to throw the whole cost of Government upon the wealthier classes, and especially those who have tangible property. Besides, when really hurtful taxes were removed, the working classes were not sufficiently temperate and educated to render it certain that the further remission of taxes would lead to the profitable expenditure of income. The true channel for surplus revenue was the reduction of the national debt. The wars at the commencement of this century had secured for us fifty years or more of nearly unbroken peace, and yet at the end of this period of ever-advancing wealth the great debt stood almost at the same figure as at the beginning. We enjoyed the peace and left our descendants to pay its cost. If it was said that this country is now far wealthier and better able to endure the annual charge of the debt than ever before, it must be remembered that the expense of war is also grtealy increased. In a great war we should now have to incur an expenditure of hundreds of millions, or else relinquish our prominent position. In dealing with the subject of the excessive mortality in great towns, Prof. Jevons expressed his surprise that more attention had not been drawn to the probable influence of a poor Irish population in raising the death-rate. According to the census returns of 1861, the unhealthy towns of Liverpool, Manchester, Salop, Glasgow, Dundee, &c., were all distinguished by possessing a large Irish population, whereas the healthy towns of London, Birmingham, Bristol, Hull, Aberdeen, &c., had less than 7½ per cent. of adult Irish residents. Sheffield was the only remarkable exception to this indicated. Prof. Jevons referred to the approaching census of 1871, as likely to afford many data for the investigations of economists, and insisted that it ought to be taken in as nearly as possible a uniform manner in all the three parts of the United Kingdom. He also directed attention to the copious and excellent statistical publications now provided by Government; referred to the efforts which were being made, previous to the present war, to facilitate the adoption of an international currency; and concluded with some remarks on the transference of the telegraphs to Government control. Many people looked forward to the time when the uniform cost of a telegram would be 6*d.*; but such a reduction of the rate, by bringing an increase of work, would greatly augment the expenses of the department, and inflict a loss upon the nation.

CONTENTS