THURSDAY, SEPTEMBER 9, 1875

*THE SCIENCE COMMISSION REPORT ON THE ADVANCEMENT OF SCIENCE**

UNDER head III. is brought forward the "Evidence relating to the Establishment of Physical Observatories."

On the general question of the establishment and maintenance of Physical Observatories, Lord Salisbury agrees that—

". . . Some of these institutions which have been alluded to in your grace's question, especially observatories, clearly fall within the duties of the Government; and certainly, from all that one hears, it is probable that their duty in that respect is inadequately performed, and that observatories for a much larger range of observations might with great advantage be multiplied." . . . .

Sir George Airy, Astronomer Royal, thus states his view on the subject:—

"When I began to be an astronomer, such questions as those of the constitution of the sun and the like were not entertained." . . . .

"Are you prepared to express an opinion as to whether it is an object which would be a proper one for the Government to take up as a State Establishment?—The Government are already pushed very hard in their estimates. The screw is always put upon them, 'Cannot you reduce the estimates a little more?' And then it would always come to a question of extensive feeling in the House of Commons, and of popular feeling out of the House of Commons; and I am confident from what I have seen that those two bodies would not in every case support an extension." . . . .

"Should you say that it is an object which is not very likely to be prosecuted with sufficient vigour unless taken up by the Government?—I do not see how it could go on except it were taken up by the Government. I do not believe that it could go on in any other way."

"It is not likely, you think, to be prosecuted by private individuals, or by other public bodies such as the Universities?—No, I think that their funds are almost all required for other objects, and the difficulty even of getting the business into shape is extremely great." . . . .

"Then such observations, in all probability, will either not be made at all or must be taken up by the Government?—That is my view." . . . .

Mr. De la Rue's opinion is thus given in reply to question 13,066:—

"I think that the time for the State providing means for reducing observations has now come: when the State should take up, besides mathematical astronomy (which deals with the places of the stars and planets, and the moon especially), physical observations, more particularly observations of the sun, which appear to me to bear directly upon meteorological phenomena." . . . .

Sir W. Thomson points out the importance of multiplying such Observatories:—

". . . In respect to the observatories, it might be necessary to have several observatories for astronomical physics in this country, if it were only to secure observations of interesting conjunctures, notwithstanding the varieties of the weather, that there may be in different parts of the country; and, again, observatories for astronomical physics ought most certainly to be founded in other parts of the British dominions than England, Ireland, and Scotland; in other latitudes and on the other side of the world."

* Continued from p. 364.

Dr. Siemens expresses the same view in the following evidence:—

". . . An observatory or several observatories should be established for carrying on physical research, research to obtain information on general subjects, such as solar observations, magnetic observations, and other subjects that might be thought desirable to obtain continually information upon." . . .

"I think that almost the only new establishments which you recommend are certain physical observatories?—Yes."

"What would be the principal object of such observatories?—For the purpose of magnetic observations, solar observations, and other general inquiries into physical phenomena."

"Do you contemplate the establishment of more than one such observatory?—Probably more than one would be desirable."

"Do you contemplate the establishment of any such observatories in any of the colonial possessions of the country?—Yes, I think so."

"Speaking generally, would they be costly establishments to found?—Not very costly, not so costly as astronomical observatories."

Dr. Frankland has also given evidence on the importance of promoting the study of Astronomical Physics, pointing out that "it would be necessary, in connection with the Physico-Astronomical Observatory, to have the means of performing various chemical experiments and making physical observations. Of course the chemical operations would be quite subsidiary to the cosmical observations there."

Mr. De la Rue, in reference to locality and organisation, in answer to the question whether provision for carrying out observations of this character should be in connection with the Greenwich Observatory, says:—

"In connection with the Greenwich Observatory, yes, but at the Greenwich Observatory, I should say not. I do not think, in the first place, that there is space enough at Greenwich, and the duties of the staff are already so very onerous that it would require a separate establishment for such special work; besides other new buildings it would entail a chemical laboratory, and there is hardly space for those at Greenwich. I believe also that it would cause too divided attention on the part of the Astronomer Royal, if he were called upon to personally superintend investigations in the physics of astronomy, although I think it would be very desirable that any new establishments, if they are to exist, should be affiliated to Greenwich."

Admiral Richards, late Hydrographer to the Admiralty, and a Visitor of the Royal Observatory, Greenwich, says:—

"If you are going permanently to establish physical observatories, I should prefer to see separate ones. I think that the physical work probably would be better separated from the Royal Observatory."

"You think that the two classes of observations are so distinct in character as to render that desirable?—Of course there is a certain amount of meteorology that must be observed at the astronomical observatory; but it need not be of any extended character."

A resolution in general accordance with the views expressed by Sir George Airy was transmitted to the Commission in July 1872, by the President and Council of the Royal Astronomical Society. This resolution is in favour of the extension of the Royal Observatory at Greenwich and other existing Astronomical Observatories, and does not recommend the establishment of an independent Government observatory for the cultivation of astronomical physics in England.

In connection with some points on which differences of opinion have been expressed in this evidence, a paper was handed in by Col. Strange, consisting of questions addressed by him to Prof. Sir W. Thomson, Prof. Hilgard, the Secretary of the American National Academy of Sciences, and Prof. Balfour Stewart, and to M. Faye, the President of the French Academy of Science.

Col. Strange's questions were as follows :—

"1. Is the systematic study of the solar constitution likely to throw light on subjects of terrestrial physics, such as meteorology and magnetism?

"2. What means, at present known to science, are available for studying the sun?

"3. Do you consider that photography (one of the assumed means) will suffice for the purpose?

"4. Do you consider that the class of observations (defined in your answer to my question 2) are such as can be efficiently made in an observatory maintained by the State, or that any of them would be better left to the zeal of volunteer astronomers?

"5. Do you consider that it would be advantageous to carry on physico-astronomical researches on an extensive scale, and meridional observations, in one and the same observatory, under a single director?"

We regret that our space will not permit us to give the replies of these eminent men to Col. Strange's questions. They were, however, strongly in favour of the establishment of physical observatories on a footing quite distinct from existing meridional observatories, and equipped with the laboratories and workshops without which such institutions would be useless. We commend to all who are interested in this question the perusal of this correspondence, which is to be found as Appendix vii. to vol. ii. pp. 27-31. Its value is enhanced by the fact that two of the writers, Prof. Hilgard and M. Faye, are distinguished foreign men of science.

Evidence relating to Meteorology.

Under this head a considerable amount of evidence was taken, particularly as to the constitution, objects, and results of the Meteorological Office.

This Office is under the management of the Meteorological Committee of the Royal Society, the functions of which are thus described in the report annually presented to Parliament :—

"The Meteorological Committee consists of Fellows of the Royal Society who were nominated by its President and Council, at the request of the Board of Trade, for the purpose of superintending the meteorological duties formerly undertaken by a Government Department, under the charge of Admiral Fitzroy.

"The Committee are credited with a sum of 10,000*l.* voted annually in the Estimates, for the administration of which they are wholly responsible, and over which they are given the entire control.

"The meetings of the Committee are held once a fortnight, or oftener when necessary, when every subject on which action has to be taken by their executive officers receives their careful consideration. The duties of the Committee are onerous and *entirely gratuitous;* they were accepted and are very willingly performed by the members, on account of the earnest desire they severally feel for the improvement of meteorological science."

The position of the Committee is anomalous. In the words of the director of the Meteorological Office—

"The Government distinctly disclaims all connection with us, whilst the Royal Society equally disclaims all control over us, except merely the nomination of the members of the Committee."

"As a matter of fact, all that the Royal Society does is to nominate the members of the Committee?—That is all."

"Having so done, it ceases to have any control whatever, does it not?—Entirely."

"What is the precise relation between the Office and the Government?—That the Government gives a vote of 10,000*l.* every year, and that it calls for no account of this money excepting the account annually presented to Parliament."

"Who audits the accounts?—The members of the Committee. There is no formal audit, because, as the Government would not recognise any audit excepting its own, the Committee considered that it was not worth while paying an auditor if such audit would not be recognised, and, as a matter of fact, two of the members take the trouble of auditing the accounts every year."

"What, in your opinion, are the chief advantages and disadvantages of such an arrangement as compared with those of the direct management of the Office by the Government?—The chief advantage is the perfect freedom from political management. The risk in being connected with the Government is that if a new President of the Board of Trade comes, he may reverse the action of the preceding one. The existence of a scientific supervision for the Office is exceedingly important; it acts as an intermediate party between the public and the Office. I may mention a decided disadvantage which results from the Office not being connected with the Government, namely, the loss of prestige. The difficulty is, that if we are sending instruments by sea or by railroad, if we do not call them Government instruments we cannot get as much attention paid to them; and it is my opinion that we should get more co-operation from the merchant navy if we were an office of the Board of Trade. We should have more prestige as acting directly from the Government."

A very clear account of the objects which the Meteorological Committee propose to themselves is given in the evidence of Major-General Strachey, one of the members :—

The Commissioners remark that it is admitted that the objects thus described do not exhaust the whole of meteorology, and that the Committee in their selection of these objects have been, to a great extent, guided by the proceedings of the Meteorological Department of the Board of Trade, which existed prior to, and which has been superseded by the Committee. Thus Major-General Strachey says :—

"The Committee is now in reality doing no more than continuing the exercise of certain functions which had, in the course of time, been thrown upon the Board of Trade by the position which that department occupies in connection with the public administration."

"Has the consequence been that the action of the Committee has been from the outset rather in a practical direction than in one of original research or scientific observation, properly so called?—I think distinctly that such is the case, and that it has necessarily followed from the position in which the Committee was placed. If a reference is made to the earlier papers, and to the report of the gentlemen on whose suggestions the present arrangements originated, there perhaps is an indication that they anticipated something more in the way of scientific research than has actually occurred; but the turn that things have taken seems to me the necessary result of the sort of duties that were put upon the Committee under the essential condition that it had but a limited sum of money to spend."

"Have any results of scientific importance in your opinion been obtained by the action of the Committee?—In the direction of what one may call investigation of an absolutely scientific character, I should say none at all. Of course the observations that are made at the special observatories are valuable scientific information, and so far one has no right to say that scientific results have not been produced; but I do not think that these can properly be referred to as specific results of anything that the Committee has done. To the best of my belief there has been nothing undertaken in the way of original investigation into the specific physical causes of any of the phenomena which are recorded, nor any original research, properly so called, in relation to any of the several branches of meteorology. The Committee hardly has appliances at its command for any such investigations, and, the funds at its disposal being limited, it was hardly possible that it should attempt them. It is also no doubt quite true that the observations which are made at the seven observatories do not include any matters which are of great importance in physical science, and which would properly come within the range of meteorology."

"Are the funds at the disposal of the Committee in your opinion insufficient for doing anything more than has been actually done at present?—I should say distinctly that this is the case. The Committee has always considered that it is bound to attend primarily to the special objects before referred to, which were in a specific manner made over to it, and it finds that after this has been done there is no money left for other things."

Again, the same witness expressed a decided opinion that the State should do more for the promotion of meteorological science than it does at present, but entertains some doubt whether any increased duties could advantageously be allowed to devolve upon a body such as the Meteorological Committee.

The same view is expressed by Professor Balfour Stewart:—

"Would you organise the Meteorological Committee in any really different form to that which at present obtains?—I should be inclined to dispense with the Meteorological Committee altogether, and substitute a Meteorologist Royal, or whatever his appellation might be, a single official who should be responsible to the Government in the same way as the Astronomer Royal is responsible for his department. I do not see why the one department should be on one footing and the other department on a different footing. I think that there are grave disadvantages with a department administered by an unpaid committee."

"Would you appoint a Meteorologist Royal corresponding with the Astronomer Royal?—Yes, whatever the name might be; I should appoint an official very much corresponding to the Astronomer Royal, and responsible to the same extent. A board of visitors would not be objectionable, but the direction of an unpaid committee appears to me to be very objectionable."*

Evidence relating to Tidal Observations.

Evidence in reference to tidal observations has been placed before the Commission by Dr. Joule and Prof. Sir W. Thomson.

Dr. Joule is of opinion that—

"With regard to the sea level and the tides, although the laws with regard to the tides are pretty well known, they ought to be continuously observed, if only for the purpose of registering the changes arising from the alteration of banks, depth of channels, &c. Also with regard to the sea level, there have been reports from time to time with regard to the inroads of the sea on our coasts, but sufficient steps do not appear to have been taken to ascertain the facts in those cases. It seems to me very important to be acquainted with any alterations in the configuration of the earth which may be taking place, however minute those alterations may be."

Sir W. Thomson gives the following evidence on this point:—

"In addition to those institutions which you have recommended, you consider, do you not, that it would be advisable that the Government should undertake secular observations of the tides?—Yes, certainly, secular observations of the tides with accurate self-registering tide gauges, with the triple object of investigating the science of the tides, of perfecting our knowledge of the actual phenomena of the tides, both in respect to navigation and as a branch of natural history, and, thirdly, with a view to ascertaining the changes of the sea level from century to century."

"Is anything of the kind done at present?—There are several tide gauges, some of which have been carried on with great care, others with not sufficient care, and none with any security of permanence."

"Was not it in connection with the Ordnance Survey of Great Britain?—No sufficient steps have been taken to ascertain whether the sea level is changing relatively to the land in any part of this country."

The Commissioners state that the accurate reduction of tidal observations, without which, of course, they are useless, has not hitherto been undertaken by any department of the State, and we are indebted to the zeal of individuals for the results which have been obtained. The reductions are laborious, and require the employment of paid computers. A memorial from the British Association for the Advancement of Science to the Lords Commissioners of the Treasury, put in evidence by Sir William Thomson, shows the difficulty that has been felt in procuring the moderate sum required for the reductions, the amount asked for being only 150*l*.

The Lords Commissioners of the Treasury did not accede to the prayer of the memorial, so that at present there is no guarantee that the observations which have already been accumulated, and those which are still in progress, will ever be adequately discussed and utilised.

Evidence relating to the Extension of the Government Grant administered by the Royal Society.

The Commissioners remark: "The strong and concurrent evidence which we have received as to the usefulness of the Government grant, as at present administered by a Committee of the Royal Society, has led us to inquire whether this grant might not be advantageously extended; and the witnesses whom we have examined on this point are unanimous in expressing the opinion that great benefits might be expected from such an extension."

Prof. Owen, Mr. Spottiswoode, Prof. Grant, Mr. De la Rue, and Col. Strange are amongst those who gave evidence to the above effect. Lord Salisbury is also of opinion that the Government grant might be increased, in order to afford liberal assistance to "first-rate workers."

Evidence as to the Payment of Scientific Workers.

The Commissioners remark:—

"On this branch of our inquiry the evidence laid before

* The whole of the evidence, of which the above are curtailed extracts, coincides with the trenchant remark of the Astronomer Royal in his last Report to the Board of Visitors that "The subject of Meteorology hardly deserves the name of a science."

us, both by statesmen and men of science, is to the same effect, and in favour of increased State aid. It has also especially been urged upon us, that to afford, by direct pecuniary aid, the means of livelihood to men of distinction in pure investigation would be a great advantage to science, as competent investigators would thus be enabled and encouraged to pursue a strictly scientific career."

Lord Salisbury is of opinion that the cause of science is hindered by the want of a sufficient career for scientific men, giving the following statement of his reasons :—

"I am induced to think so, by noticing how very much more rapid the progress of research is where there is a commercial value attached to the results of it, than in other cases. The peculiar stimulus which has been given to electrical research, in the particular direction of those parts of it which concern the telegraph, is a very good instance in point, and the extent to which researches into organic chemistry have almost clustered themselves round the production of coal tar colours is another instance in point. And therefore it is difficult to avoid the conclusion that research is really hindered by the necessity under which those who are most competent to conduct it feel themselves, of providing for their own support by means of the talent and the knowledge which they possess."

With regard to the scale on which such remuneration or payments for maintenance should be made, Lord Salisbury observes :—

"I should say, taking the parallel [that of certain offices in the Church], to which I have already alluded, that an income of about 1,000*l.* or 1,500*l.* a year would be the kind of income which would suffice for the purpose that I have in view."

And he would also add provision for retirement.

With reference to the safeguards against abuse which would be necessary, Lord Salisbury continues :—

". . . It would, for their [the investigators'] own interest, and to save them from invidious comments, be desirable to impose upon them the necessity of publishing, either in the form of books or in the form of lectures (but not sufficient in number really to impede their work), an account of the result of their labours during each successive year. Perhaps one or two stated lectures in the course of a year, to be delivered to University students, would be the best means of imposing upon them that test of industry."

Lord Derby takes the same view :—

"I think that, in one way or another, where you have a man of very great eminence as a scientific discoverer, it is unquestionably the duty of the State to provide him with means and leisure to carry on his work. Whether that is to be done by giving him an office under the British Museum, or in any similar institution, or whether it is to be done by simply granting him a pension in recognition of eminent scientific service, or in whatever other way it is done, it seems to me to be immaterial, but I certainly consider that it is a very important part of the public duty, to relieve men who have shown an eminent capacity for original discovery and research from the necessity of engaging in a lower kind of work as a means of livelihood." . . .

Sir W. Thomson, in a reply to which we have already referred, stated his opinion on this point as follows :—

"That men should be enabled to live on scientific research is a matter of most immediate consequence to the honour and welfare of this country. At present a man cannot live on scientific research. If he aspires to devote himself to it he must cast about for a means of supporting himself, and the only generally accepted possibility of being able to support himself is by teaching, and to secure even a very small income, barely sufficient to live upon, by teaching, involves the expenditure of almost his whole time upon it in most situations, so that at present it is really only in intervals of hard work in professions that men not of independent means in this country can apply themselves at all to scientific research." . . .

Prof. Henry, the distinguished director of the Smithsonian Institution in the United States, who was good enough to appear before the Commission when he was in this country, gave the following emphatic evidence in the same direction :—

"My idea would be that if the funds were sufficient, and men could be found capable of advancing science, they should be consecrated to science, and be provided with the means of living above all care for physical wants, and supplied with all the implements necessary to investigation."

Prof. Balfour Stewart, after referring to the instances of wealthy persons who undertake scientific research in this country, points out that the number of those so circumstanced is very small in comparison with the number of able men who are willing to give their time and capacities to observations and research. He goes on to say that able men, and men competent to conduct research, suffer in this country from not having sufficient means at their disposal to proceed as they would like to do.

"Do you anticipate, then, that if there were any intelligent centre for the distribution of a sufficient fund to persons having the requisite capacities for observation and research, but not having the means, the distribution of such a fund would have any benumbing influence upon original observation and research?—No, I should think quite the contrary ; it would encourage it very much."

Mr. Gore also advocates the enlargement of the present system.

". . . . I should strongly advocate that the present system should be enlarged, so that the investigators should not merely be reimbursed for all that they have expended, but also paid in some measure for their time and labour, because each investigator has to give up a profitable employment in order to find the time."

He then gives his own personal experience, which probably resembles that of many of those who, without private fortune, engage in pure research.

"I refuse a great many engagements in analyses and other scientific matters for the manufacturers who come to me. . . . I gave up some pupils a short time ago to enable me to have more time for original investigation."

Dr. Joule, Dr. Siemens, Mr. De la Rue, and other scientific authorities testify to the same effect, and urge the adoption of some form of remuneration for valuable work done, as a measure not merely just to the individual, but serviceable to the State by the encouragement it would afford to those able men of small means, who abound in this country, to engage in original researches of great importance to the community.

(*To be continued.*)

THE IRISH FISHERIES

Report of the Inspectors of Irish Fisheries on the Sea and Inland Fisheries of Ireland for 1874. Presented to both Houses of Parliament. (Dublin: Alex. Thom, 1875.)

DURING the last few years increased attention has been paid to the vast stores of food, which this country possesses, in the fish frequenting its inland waters and territorial seas. Legislation, attended on the whole with marked success, has led to the development of the salmon fisheries of the United Kingdom; a much less successful attempt has been made to increase the produce of our exhausted oyster fisheries; and a very able Commission, which enjoyed the advantage of Prof. Huxley's assistance, has investigated and authoritatively disproved the allegation that our sea-fish were decreasing. In England and Scotland, at any rate, satisfaction is usually expressed at this state of things. With the single exception of the oyster, the harvest of the sea proves annually as productive, or even more productive, than ever, while the increasing consumption of a growing population and the greater destructiveness of modern implements of fishing, are not apparently unduly diminishing the numbers of our sea-fish. Ireland, however, to judge from the language of her representatives in Parliament, is less satisfied with her position. The very fish, if we may credit some authorities, are deserting the coasts of this unhappy country; and Irish fishermen, with their old tackle worn out, and with no money to purchase new, are emigrating to other fishing grounds on the other side of the Atlantic. The picture annually presented to us of the miserable condition of Irish fishermen was so deplorable, that Parliament, last year, was induced to interfere. The Ministry was surprised by a hurried division, and unexpectedly defeated by a narrow majority. Its defeat compelled it to place a portion of the Irish Reproductive Fund at the disposal of the Irish Inspectors of Fisheries; and the Inspectors are now enabled to lend small sums of money to needy fishermen on their personal security. No such loan has yet been made. But, on the eve of adopting a new policy, it is occasionally desirable to review the circumstances which have led to it; and we turn, for this reason, to the recently published report of the Inspectors of Irish Fisheries.

The Report is divisible into two portions. The first and shorter portion refers to Sea Fisheries, Oyster Fisheries, and Harbours; the second and longer portion to the Inland or Salmon Fisheries. The salmon fisheries of Ireland are fairly prosperous. The amendments which are required in the law are not numerous or important; and we do not therefore propose to follow the Inspectors into their review of them. But the ten pages of the Report which are devoted to the sea fisheries and oyster fisheries of Ireland, deserve for every reason most attentive consideration. The oyster fisheries occupy a very short space in the Report, and may be dealt with in the first instance. The Inspectors have exercised almost absolute powers in dealing with this question. They are authorised to appropriate to any individual who applies to them, large portions of the fore shore of Ireland, and 130 licensed beds, occupying 18,825 acres of fore shore and sea-bottom, have thus been appropriated. The result of this wholesale appropriation of public dredging ground might well have justified Parliamentary interference. "The chief object," say the Inspectors, "in granting licenses (cultivation) has not been fulfilled. In the majority of cases we believe there has not been anything deserving to be called an attempt to cultivate the ground granted. The proprietors in numerous instances content themselves with getting as much as they can for their private use, and do nothing to replenish. We would be fully justified in cancelling the majority of the licenses." We quite agree with the Inspectors in this view; but we should like to know why some of the licenses have not already been cancelled. Two years ago the Inspectors assured us that they had "warned some licensees that their licenses will be withdrawn unless within twelve months they proceed to cultivate." The public have a right to inquire whether the warning has been attended to, and if not, why the threat of the Inspectors has not been carried out. The Inspectors, indeed, say that they have so "many pressing duties to perform," that they have been compelled to postpone attending to the oysters. But can any duty be more pressing than the restoration to the public of ground really taken from them under false pretences? The Inspectors have found time to grant five new licenses; they would have done much more to promote oyster culture if they had cancelled five old ones. "Overdredging and a succession of bad spatting years" are of course given as the cause of the growing scarcity. But it is worth while remarking that we had nothing of bad spatting years till overdredging had decreased the stock of oysters. If an oyster bed be scraped clean of all the adult oysters, no spatting season, however favourable, can be a good one.

But the most important portion of the present report is undoubtedly that which relates to the Irish Sea Fisheries. There can be no question about the decrease of Irish fishermen. In 1846, or before the famine, 113,073 men and boys were employed in 19,883 vessels and boats on this industry. In 1874 the number of vessels was reduced to 7,246! the number of hands to 26,924! The decline both in boats and men has been continuous throughout the period. But, with due deference to the Inspectors, it is easy to account for it. The "melancholy ocean" which surrounds Ireland is subject to very severe storms: and no fishing-boat can prosecute its industry consecutively throughout the year. Under such circumstances one of two things must happen—either the Irish Seas must be fished by men who, in strong weather, may resort to quieter fishing-grounds, or the Irish fisherman must combine other operations with his fishing. Before the famine the last of these things occurred. Every Irishman was a cottier. He tended his potatoes and his pig in bad weather: and he went a-fishing in calm weather. But, since the famine, the cottiers have gradually been worked out. Large farms have swallowed up small ones: and the occupiers of large farms, and their servants have no time to go out fishing. The class from which the mass of Irish fishermen were drawn had ceased, or is ceasing, to exist; and Irish fishermen are consequently decreasing in numbers. But, though Irish fishermen are decreasing, the Irish fisheries are not decaying. What do the Inspectors tell us? There were only 187 Irish boats engaged last year in the herring fishery off Howth. But there were 343 English, Scotch, and Manx boats. There were only 61 Irish boats in the mackerel fishery off Kinsale. But there were 226 English, Scotch, and Manx boats. The Englishmen, Scotchmen, and Manxmen, following the fish round the whole coasts of England, Scotland, and Ireland, beat the Irishmen, who never follow them at all. Every one has seen Cornish boats fishing for herrings in the North Sea; or Scotch boats beating the English in their own waters. But no one ever saw an Irish fishing-boat in either a Scotch or English sea. The Englishmen and Scotchmen, with their capital continuously employed throughout the year, beat, of course, the Irishman who leaves it idle and unemployed for three-fourths of it.

The view which we have thus expressed is not, however, shared either by the friends of Ireland or the Irish Inspectors. In their eyes the decrease in the number of Irish fishermen is equivalent to the decay of the Irish fisheries; and both of these are due to the unsympathetic attitude of this country. Last year nothing would do any good but loans. Now that the Reproductive Loan Fund has been utilised for this purpose with effects which we shall immediately notice, nothing will do any good but a safe and commodious harbour at Arklow. Such a harbour "is most necessary for the successful prosecution of both herring and oyster fisheries," and "unless something be done, there is little hope of any substantial improvement." We have no desire to discourage the construction of safe and commodious harbours, but we should like to ask the Irish Inspectors whether they ever heard of a place in England called Yarmouth. It is as important a fishing station as Arklow, it is on as stormy a shore; but when a storm is raging, the Yarmouth fishermen have to stand out to sea to avoid being driven on to the coast. We never heard that the want of a harbour at Yarmouth had destroyed the Yarmouth fishery; and we think that Yarmouth has at least as good a claim as Arklow for the construction of such a harbour. The new system of loans to fishermen remains for consideration. There has, of course, been no want of applicants for the loans. 2,800 individuals have already applied for the money, and we have no doubt there are a good many more quite prepared to follow their example. 1,300 of the 2,800 applications emanate from County Galway, and 160 of these applicants live in one parish. No more than six of the 160 "fulfil the conditions which should entitle them to obtain a loan!" We presume that as the Inspectors pointedly refer to the 160 applicants, they may be regarded as fair examples of the 2,800 who have applied. In that case only 105 persons throughout Ireland will, in the lenient judgment of the promoters of the policy, be entitled to participate in the loan. Is it possible to conceive a more striking illustration of the consequences of the policy?

MAGNUS'S "ELEMENTARY MECHANICS"

Lessons in Elementary Mechanics introductory to the Study of Physical Science, with numerous Exercises. By Philip Magnus, B.Sc., B.A. (Longmans, 1875.)

IN order to assign any work to its proper place it is necessary that we should try to ascertain what is the author's aim in writing it, and also to see if that aim be to any fair extent attained; further, we should take into our account the consideration of the question whether if the author's end be attained it is one worth arriving at. If the verdict on all these issues be favourable, then we may say that the *raison d'être* of the work is justified. For the aim of the present volume the title will suggest at once that the author does not attempt to produce a treatise which shall enter into comparison with such works as those produced by Thomson and Tait. Let us hear his own statement: "The lessons are intended for the use of those who have had no previous acquaintance with the subject;" and so he has endeavoured to bring into prominence the leading principles of Mechanics, and to exemplify them by simple illustrations. Here we may observe that the term mechanics is used in the ordinary acceptation of that word now-a-days, *i.e.*, as the science of the motion and equilibrium of bodies, and not in the Newtonian sense to which Messrs. Thomson and Tait seek again to restrict it. Starting on the hypothesis that the idea of Motion is more elementary than that of Force, since it is only from a combination of forces that equilibrium can result, the author makes the subject of Statics depend upon the laws of Dynamics. Hence the proposition, which is generally cited as that of the Parallelogram of Forces, Mr. Magnus derives at once from Newton's second Law.

After a short preliminary introduction we have "Kinematics—Motion" treated under the heads of Measurement of Motion and Falling Bodies; then "Dynamics—Force," under which heading we have Measurement of Force, the Laws of Motion, Energy, Machines.

The second part of the book discusses "Statics—Rest," under the following heads: Theory of Equilibrium, Centre of Gravity.

The style is lucid, the solved exercises carefully chosen, the work compact. With the exception above mentioned, of Statics being made dependent on Dynamics, the arrangement and matter are much the same as we find in English treatises. An intelligent boy ought in a few months to be able to make himself master of the greater portion of this small book, which Mr. Magnus has aimed at making sufficiently elementary to be placed in the hands of a beginner. What we consider to be higher praise is that we believe it to contain nothing that the student will have to unlearn in a subsequent portion of his career. We can recommend it as a trustworthy introduction to more advanced text-books.

We have endeavoured to test its accuracy as regards the answers to the numerous questions scattered over its pages. Of these there are 279 in the Dynamical portion, 192 in the Statical portion, besides 79 questions in an appendix composed of papers from the Matriculation, South Kensington, College of Preceptors, Oxford Local, Cambridge Local, and other Examinations. These answers seem to us to be exceptionally correct, as, though we have tried them all, we differ from Mr. Magnus's results in only a dozen cases; some of these cases are apparently clerical errors. We make this statement, taking into account two or three slips of errata which have been subsequently distributed by the author.

In Ex. 23, p. 86, 1·368th should be 1-368th, *i.e.*, $\frac{1}{368}$; § 199, we think, would not be easy for the pupil unless he had some aid from a tutor. Some of the questions given to the Matriculation candidates of the University of London seem to us hardly suitable for them; we shall select one, because even so experienced a teacher as the writer of the work we have noticed at first fell into an error. The question is: "Suppose that at the equator a straight, hollow tube were thrust vertically down towards the centre of the earth, and that a heavy body were dropped through the centre of such a tube. It would soon strike one side; find which, giving a reason for your reply." The author gives an answer which we have heard one or two "coaches" give also, but on a slip he has corrected his printed answer.

Again, in Ex. 27, p. 55: "A balloon has been ascending vertically at a uniform rate for 4·5 secs., and a stone let fall from it reaches the ground in 7 secs.; find the velocity of the balloon and the height from which the stone is let fall." Both Mr. Magnus and Dr. Wormell ("Natural Philosophy," p. 129, Ex. 45) work this question as if the balloon were at rest when the stone is let fall; we see no reason for their doing so in the wording of the question. They give the same height for the balloon, but differ in the velocity.

OUR BOOK SHELF

Game Preservers and Bird Preservers. By Capt. J. F. Morant. (Longmans, Green, and Co., 1875.)

To increase the annual rental of Scotch moorland, and to feel certain that at least thirty brace of grouse will fall to each gun after a whole day's sport, are the greatest delights of a certain few, according to whom every other consideration must be put in abeyance. Capt. Morant is one of these. "The red grouse is about the best game bird in the whole world, and deserves all the care we can bestow upon him." This care involves the annihilation of every creature that shows the least disposition to destroy and feed upon the eggs, young, or adult of *Lagopus scoticus;* and the death-list is no small one, including eagles, buzzards, hen harriers, all other Raptores, ravens, crows, magpies, wild foxes, polecats, stoats, and weasels. The stomachs of hawks are often found to contain the remains of weasels and rats; why kill them if they destroy those vermin? "If an alderman were shipwrecked on an uninhabited island, he would probably live upon the contents of a cask of biscuits which might be washed ashore. But the scientific gentleman among a party of savages who might examine him after his friends who happened to land on that island had killed him for their supper, would, we know, arrive at an erroneous conclusion if he entered it in his note-book as a fact that the animal *alderman* lived entirely on dry biscuit." This running analogy is the argument employed throughout the book, and it is this which makes it a particularly amusing one to glance through; whether it carries conviction with it is a different thing. The grouse disease is explained as depending on the fact that these birds, unlike others, eat only one food, heather, and when this is injured by cold or otherwise, they have no other to fall back on. That many shot-damaged birds survive and afterwards produce unhealthy offspring is considered unlikely. "Can we fancy a grouse telling his mate on a spring morning, My dear, I feel very poorly to-day; that No. 5 in my spine is troubling me dreadfully?" The author's raid against all the Raptores is very severe; he in this, as in other points, being much opposed to the general tenour of the report of the evidence given before the Parliamentary Select Committee appointed in 1873. His considerable experience adds great weight to the aspect of the question which he espouses.

The Handy-Book of Bees, being a Practical Treatise on their Profitable Management. By A. Pettigrew. Second Edition, revised and improved. (Edinburgh and London: Blackwood and Sons, 1875.)

A Manual of Bee-keeping. By John Hunter, Honorary Secretary of the British Bee-keepers' Association. (London: Hardwicke, 1875.)

THESE two volumes have different objects and will serve different purposes. The first edition of Mr. Pettigrew's book was favourably noticed in our columns five years ago (NATURE, vol. ii. p. 82), and we are glad to see that a second edition has been called for. Still more pleased are we to find that the author is open to conviction, and that he has acknowledged and corrected a few theoretical errors in the first edition. For the economical management of bees with a view to profit, there is no better guide than Mr. Pettigrew.

Mr. Hunter's volume, on the other hand, is essentially a book for the amateur, to whom profit is of less importance than the amusement and interest of bee-keeping. He gives an account of all the appliances of the modern apiarian, and of the most recent improvements in the treatment and study of bees. The various kinds of honey-extractors, feeders, guide-combs, and queen-cages; the methods of artificial swarming, queen-breeding, and ligurianising; the diseases and enemies of bees; and the various methods of preparing and preserving the honey and wax, are all briefly discussed. Some of the most recent observations on the habits and instincts of bees are given, including Sir John Lubbock's interesting proof that they distinguish colours. The book is illustrated with a number of useful woodcuts, chiefly of hives and apparatus; and it will be indispensable to amateurs who wish to acquaint themselves with the most recent improvements in the art of bee-keeping, and the latest discoveries as to the habits, instincts, and general natural history of the honey-bee. A. R. W.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Personal Equation in the Tabulation of Thermograms, &c.

IN a late number of NATURE (vol. xii. p. 101) you have commented upon the work performed by the Meteorological Office. Although in no way interested in the defence of that department, I think objection may fairly be taken to the style of criticism adopted. Not only would it, in most cases, be necessary to refer to the original thermograms satisfactorily to detect the many small errors pointed out, but it is well known to practical men that owing to certain idiosyncrasies of individuals some of the numbers 1, 2, 3 . . . 8, 9, 0 do occur in estimations more often than others, and of course more often than they should do theoretically. In no case are such personal peculiarities likely to show themselves more than in the determination of the position of a hazy photographic trace of sensible breadth, as between two sharply defined lines. As an example of my meaning, I may refer to somewhat similar estimations of tenths of seconds, as tabulated by the highly-trained and experienced observers of Greenwich, only premising for the information of the uninitiated, that the tenth part of a second is far too large a measure of time to be trifled with by astronomers, and that practically the estimation is simply that of the position of one sharply marked puncture or dot as referred to two others equally well defined on either side of it, indicating the beginning and end of the second, and separated by about one-third of an inch. Referring to the Greenwich Observations of 1864 (the only volume I have at hand), and taking three days' observations at random for the experiment, I have determined the percentage of times that each of the numbers 1, 2, 3...8, 9, 0 occur as the tenth at which transits of stars took place. As there is no theoretical reason why one number should predominate over another, we may expect that the percentage for each figure will be accurately 10, or each a tenth of the entire number.

The following are the percentages founded upon 511 estimations on April 21, upon 379 on April 19, and upon 393 on Nov. 5, 1864, respectively:—

	1.	2.	3.	4.	5.	6.	7.	8.	9.	0.
Percentages	5·7	6·5	9·0	21·1	11·7	11·0	6·3	8·4	5·9	14·3
	6·9	9·2	10·0	13·7	10·8	12·4	7·4	8·7	5·3	15·6
	8·4	8·1	7·6	13·7	10·9	9·4	8·1	9·7	8·9	15·0
Mean of 3 days	7·0	7·9	8·9	16·2	11·1	10·9	7·3	8·9	6·7	15.0

Although no one acquainted with the care bestowed upon this description of work at Greenwich would for one moment think of impugning the accuracy of these estimations, they show precisely the excess of whole seconds that is taken in the before-mentioned article as indisputably proving the carelessness of the tabulations at the Kew Observatory.

As regards these averages, it is to be remarked that with one slight exception all the numbers that are above or below the theoretical average in one example are above or below in all, and that there is only one case in which the range of difference exceeds 3 per cent. The partiality shown for the figures 0 and 4 is also most marked, and of itself would be enough to show that the same person had made all the estimations.

There is another light in which we may regard these results, which still more plainly indicates my meaning. The decimals ·1, ·2, &c., ought to include all possible positions of the puncture between ·05 and ·15, between ·15 and ·25, and so on; but according to the reader of the chronographic sheets, ·1 includes only those positions of the puncture between ·081 and ·151; ·2 includes those between ·151 and ·230; ·3 those between ·230 and ·319; ·4 those between ·319 and ·481, and so on. Thus the error of any single determination is very small indeed, a remark that will apply equally to the tabulations Meteorological Office.

To show that different observers have very different idiosyncrasies, I may append the following averages similarly determined, this time from the purely astronomical estimations of the time of transit of stars across the well-defined spider lines of the telescope by the method known as eye and ear observation, these estimations being made on a precisely similar principle. From the Greenwich observations of 1864 I find 206 such estimations by Mr. Dunkin, the standard observer at that time; 259 by Mr. Ellis; and lastly, 500 by myself in the present year, made at this observatory, yield the following:—

	1.	2.	3.	4.	5.	6.	7.	8.	9.	0.
D., 1864 ...	7·8	16·5	11·7	12·1	13·6	7·8	9·2	13·6	6·8	1·0
E., 1864 ...	5·4	8·5	7·7	9·7	8·5	11·2	12·4	13·5	12·4	10·8
P., 1875 ...	13·4	13·0	10·6	10·8	7·8	8·6	8·8	13·6	4·8	8·4

Although founded on rather too few estimations, there is little doubt that the salient features would be preserved in a more extended discussion. Thus D's avoidance of whole seconds and the adjacent numbers 1 and 9, E's avoidance of the former of these, and my own of the latter, may be expected confidently, however large a number of estimations are taken into account. The universal fondness for 8 is also noteworthy.

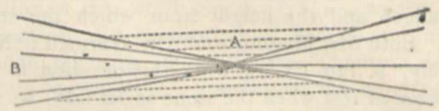
Orwell Park Observatory, near Ipswich — JOHN J. PLUMMER

Source of Volcanic Energy

IN your report of the meeting of the Geological Society in NATURE, vol. xii. p. 79, I find notes of a communication submitted by the Rev. O. Fisher, F.G.S., on Mr. Mallet's theory of volcanic energy, and as I consider Mr. Mallet's paper to be one of surpassing value, I wish to make a few remarks on the criticism of it by Mr. Fisher. Mr. Fisher objects to the possibility of assuming high *local* temperatures to be produced by the transformation of tangential forces into heat, within the earth's crust.

If the strata of which the earth's crust is composed could be represented in a diagram by so many concentric circles of perfect regularity, the crushing force resulting from tangential pressures caused by the regular contraction of the mass would of course be equal all through the mass; but, as a matter of fact, such a diagram would not be a faithful representation of the lie of strata in the earth's crust. These strata occur at all sorts of angles, and are broken in upon by faults of great extent; so the pressures produced upon various parts of the earth's crust are far from equal. These inequalities are also increased by the differences in density of strata as also by the thinning out of strata of the same density.

For instance, a strain may occur somewhat in the manner of the annexed diagram. A set of strata may bear upon a point A, considering the forces to act in the direction BA, CA, and so cause the pressure upon a square foot at A to be a hundredfold greater than on a square foot at B. The work done, therefore, may not be equally distributed over certain areas; but forces may *converge* upon various points, and if the work is thus intensified in certain points, the heat developed in such points must be greater than where the forces are not so concentrated. It seems to me, then, that the rocks at A may be crushed to *fusing-point* by converging forces, while at the same time the rocks of the same set of strata at B may be at a much lower temperature.



If what I have attempted to point out contains no "untenable assumption," the possibility of the developed heat being localised remains intact; and this is certainly the main feature of Mr. Mallet's theory.

Mr. Fisher's objection to the primeval formation of our present existing ocean beds and continents seems a fair one, notwithstanding the fact of the remarkable steepness of the western coasts of all continents remarked upon by Mr. Mallet, but this remarkable similarity of formation may be no more remarkable than the fact of all the great promontories of the world pointing to the south and none to the north. Still, however, Mr. Mallet's paper may help us, for if the tangential pressures produced in the earth's crust be sufficient in some cases to produce long lines of volcanic activity, may they not in other cases be resolved into motions acting in various directions and causing the upheaval of continents and depression of ocean beds?

In conclusion I may remark that if *mere* cooling is not considered sufficient to account for the development of such forces, may not forces produced by gravitation acting in the very same direction be well acknowledged? Not mere gravitation of the surface upon a retreating nucleus, which of course is part of Mr. Mallet's theory, but gravitation of the *whole* mass to itself, which enormous source of energy must also express itself in tangential pressures in the more resisting crust of the earth?

Kenmare — W. S. GREEN

Sanitary State of Bristol and Portsmouth

IN reference to the peculiar low mortality of some large towns in Great Britain, stated in the abstract of a communication to the Scottish Meteorological Society in NATURE, vol. xii. p. 281, as Portsmouth and Bristol, in contradistinction to others apparently in similar circumstances, having a high death-rate, I beg leave to point out that each of these towns is differentiated from the others mentioned in the paper in a *social* point of view more than in physical conditions. There is a large district in each of them, inhabited chiefly by visitors, tourists, retired professionals, and mercantile people, who take up their quarters in Southsea and Clifton, for the period of the regular seasons in each, or for limited tenure of occupation, either with reference to health, pleasure, or education of their families.

These divisions or quarters of Portsmouth and Bristol are under different physical conditions from the parent cities they are attached to, in that they are of separate growth, of later date of construction, better built, and inhabited by a wealthier class of people.

They might be compared to the apple-grafting on a crab-tree, on the old stem of which they flourish, but bear more showy flowers and more luxuriant fruit, and they thus tend to ameliorate the inherent deficiencies of the original tree by adding a higher and more cultivated life.

Topographically speaking, again, these two districts are entirely different from each other, though equally healthy, as above stated, Southsea being built upon a plain near the sea, and Clifton being built upon a hill above a river: the one lies on gravel and the other on limestone, so that these and other material circumstances, oddly enough, can scarcely be thought likely to produce a common result on their sanitary state.

The original towns of Portsmouth and Bristol, however, are nearly alike in some points, but not in others. Both are shipping ports, both are on tidal harbours, both are built along the banks on each side, and are therefore low in altitude above the sea; but the former lies on gravel, while the latter is built on alluvium and red sandstone. Most other large towns are of a homogeneous constitution, as Manchester in manufactures, Liverpool in shipping, Scarborough as a seaside resort, and Cheltenham as an inland watering-place; but Portsmouth and Bristol are peculiar in having this double social composition of a shipping port and a health-resort in one borough, and which, therefore, might be taken into account in any deductions from statistics of health or mortality of their united populations.

British Association, Bristol W. J. BLACK

A Lunar Rainbow?

THERE can be little doubt that your Australian correspondent, Mr. Lefroy (vol. xii. p. 329), has seen one of the phases of an Aurora Australis. Similar appearances have been observed by me in Scotland, passing south of the zenith (and nearly through the anti-dip, as at Fremantle). Their sudden occurrence and temporary persistence are perplexing to those who have not seen this particular display before. The first seen by myself (in 1844, I think) was a single beam which remained in the same position during some hours; it was described by me next day in a local paper, while a well-known observer in a communication to an Edinburgh journal had taken it for a comet.

It is pleasant to see accounts of such phenomena sent to NATURE from all parts of the world, even when the true cause has not always been apparent. It is not improbable that the magnets at Melbourne will have shown some slight disturbance about 8h. 30m. P.M. of May 16.

JOHN ALLEN BROUN

I DO not see any reason to doubt that the phenomenon seen by "J. W. N. L." in Australia, and described by him in vol. xii. p. 329, was an aurora. I never saw one with so many arches as he mentions (eighteen or twenty), but there can be no reason for supposing so large a number to be impossible. In almost every other respect his description agrees exactly with auroras such as may occasionally be seen. T. W. BACKHOUSE

West Hendon House, Sunderland, Sept. 4

The House-Fly

I WAS somewhat interested in Mr. Cole's remarks on the house-fly in NATURE (vol. xii. p. 187), and recently had an example of another of its enemies. On touching a rather small decrepit house-fly which was making its way across a sheet of paper, three minute, active animals, apparently beetles, tumbled out of it; they were light brown in colour, and very much the shape of *aphides*, and about the size of the hole a medium sized pin would make when pushed through paper. F. P.

OUR ASTRONOMICAL COLUMN

M. LEVERRIER'S THEORY OF SATURN.—Early in the year 1874, M. Leverrier presented to the Paris Academy of Sciences the conclusions he had drawn from the comparison of his analytical theory of the planet Jupiter with the meridian observations made at Greenwich and Paris during the long period of 120 years, which he found to be represented thereby with all desirable precision; thus proving that the motion of Jupiter is not subject to any sensible action beyond the effects of the known planets.

The comparison of the theory of Saturn with a similar extended course of normal positions, each one based upon a great number of observations, has not run quite so smoothly, but, on the contrary, has presented some slight difficulties, upon which M. Leverrier makes known his opinion, in a communication to the Paris Academy on the 23rd of last month. During the thirty-two years of modern observations, 1837-69, the differences between theory and calculation, except in two instances, remain below 0·2s. in the times of passage observed on the meridian; for the older observations of Maskelyne and Bradley, somewhat larger discordances are shown. The residuals are, however, upon the whole, very small, and a question arises, whether such quantities can be legitimately neglected, or, if not, whether their cause is to be sought in incompleteness of the analysis or in errors of the observations themselves. M. Leverrier has not been content to rest upon the first supposition, but states that he has used every effort to elucidate the source of the remaining differences. To satisfy himself and astronomers generally that there is no defect or inaccuracy of theory, M. Leverrier has taken extraordinary pains to guard against error or omission. When he found in his earlier researches a discordance between theory and observation in the case of Mercury, he was able to explain the whole by admitting an increase in the motion of the perihelion, which might be attributed to the existence of cosmical matter or the action of small bodies nearer to the sun than the planet; and again, when the comparison of theory with the observations of Mars showed differences, they were explainable by a similar assumption of increased motion of the perihelion, necessitating an increase in the mass of the earth, and consequently of the solar parallax. In the case of Saturn, the smallness of the residuals has rendered it a much more difficult matter to pronounce with confidence upon their cause. Having reviewed the whole of his analytical theory, M. Leverrier, with the view to further verify it, considering this theory as a first, though exact approximation, proceeded by methods of interpolation to reconstruct it, taking account at once of the terms of all orders. Every possible verification having been thus accumulated, he concluded that no error was to be apprehended in this direction. The comparison with the normal positions having been certified with equal care, he ascertained the effect of small changes in the masses of Jupiter and Uranus, the errors being exhibited in functions of the corrections to these masses, and the results prove that no alteration in the adopted value of either mass will destroy the residuals as a whole; if they are somewhat diminished thereby in one part of the series, it is only at the expense of increasing them in other parts. Indeed, M. Leverrier establishes one point, and a very remarkable one it will no doubt be considered, viz., that the 120 years of meridian observations of Saturn are insufficient to afford a reliable value of the mass of Jupiter; or, in other words, that the mass of Jupiter which has so great an importance in the elements of the solar system, is not yet determinable from the comparison of the theory of Saturn with observations. This was not the case as regards the mass of Saturn, which M. Leverrier found from his researches upon the motion of Jupiter to be $\frac{1}{3529{\cdot}56}$ a somewhat smaller value than that resulting from Bessel's measures of the Huygenian satellite.

Under the above circumstances, the probability that errors of observation are the cause of the remaining differences from theory is much increased, and M. Leverrier appears inclined to attribute these errors to the interference of the rings under their various phases, an explanation which practical men will assuredly regard with favour. Considering that at certain times the rings disappear entirely, when the planet's centre may be well observed, while at others intervening in an elliptical form, projecting shadows and occasionally rendering impossible the observation of one of the limbs, there is nothing unlikely, as M. Leverrier remarks, in an uncertainty of some tenths of a second in R.A., which would sufficiently explain all. At any rate, whatever influence the interference of the rings may have upon the observations, it is doubtless of a variable character, as well on account of the physical fact itself, as from the effect it may exercise on personal equations.

MR. DE LA RUE'S TABLES FOR REDUCTION OF SOLAR OBSERVATIONS.—"Auxiliary Tables for determining the angles of position of the Sun's Axis and the Latitude and Longitude of the Earth referred to the Sun's equator," which have been employed in the reduction of the ten-year series of solar photograms taken at the Kew Observatory, have just been printed by Mr. De la Rue, professedly for private circulation, though, as they have been imposed in the size and type of the "Philosophical Transactions," it may possibly be the author's intention to append them to a future communication to the Royal Society, in continuation of other important papers already published in the "Transactions,"—a place which the Tables will advantageously occupy. They give with sun's longitude as argument, the inclination of the solar axis to the circle of declination, reckoned positive when the axis is west of the north point of the sun's disc, and assuming the inclination of his equator to the ecliptic to be 7° 15'·0, and the longitude of its ascending node $74° - \nu$; and with argument, sun's longitude $+ \nu$, the "Heliographical latitude of the earth" and "Reduction of longitude." The obliquity of the ecliptic is taken, 23° 27'·5, but to correct the angle between the circle of declination and the sun's axis, for difference of true and assumed obliquity, a supplementary table is provided.

The Tables have been calculated by Mr. Marth, and it will be obvious to anyone initiated in such work, that considerable trouble has been taken to ensure their accurate production.

MIRA CETI.—A *minimum* of this variable star is set down in Schönfeld's ephemeris for September 30. The minima have not been properly observed nearly so often as the maxima, though equally important in the investigation of the laws which regulate the fluctuations of light, and which, according to Argelander's researches, involve a more complicated formula than has yet been deduced for any other variable. The circumstances of the approaching minimum are very favourable for observation.

SCIENCE IN GERMANY

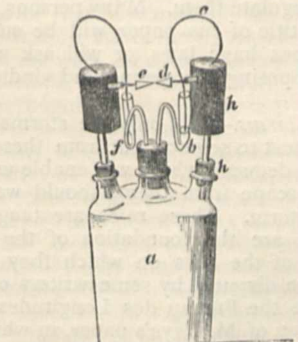
(*From a German Correspondent.*)

ONLY for a small number of elements and their compounds is the relatively low temperature of the non-luminous gas flame sufficient to produce spectra which can be of use in analytical researches; by far the larger number turn into vapour at such degrees of temperature as we can obtain solely by the electric spark. We are therefore confined to spark spectra for such bodies which do not give spectra in the flame, and these spark spectra can all the less be dispensed with in those cases where *new* elements are sought for, or where it is a question of proving beyond all doubt the presence of certain bodies, which in their chemical properties are so much alike that ordinary reagents do not suffice for their discovery or separation.

But there are difficulties in the way of practically using spark spectra, which have been the reason why these important means of reaction have not yet found their entry into all chemical laboratories. First of all, a simple method has been wanting by which spark spectra can be obtained at any time. Whoever has been obliged to use currents of great intensity with temporary interruptions of days, weeks, or months, knows how much unpleasantness is caused by fitting, taking to pieces, and cleaning the ordinary constant batteries used hitherto. Another difficulty lies in the fact that spectrum tables are still wanting which would be of sufficient service for all practical purposes. It is true that a large quantity of measurements have been published, and doubtless some of them are extremely accurate, but with the greatest part of them the purity of the substances experimented with is not in the least guaranteed, and very often it can be proved not to have been attended to at all. If it is attempted to reduce to a universal scale all the spectrum drawings at hand which have been obtained by different observers, with different refractive media, with different widths of the slit, some at a higher, and some at a lower temperature, tables are obtained which are completely and utterly useless in the laboratory.

Lately Prof. Bunsen, of Heidelberg, has tried to remove all these difficulties. In a very important treatise, the first part of which has just been published, he first describes a new battery and a new spark apparatus, by means of which spark spectra can at any time be obtained with the same ease and facility as ordinary flame spectra. The battery is the charcoal-zinc battery without clay cells. The exciting liquid is a mixture of bichromate of potash and sulphuric acid. In order to prepare 10 litres of this liquid, Prof. Bunsen gives the following instructions:—0·765 kilogrammes of commercial powdered bichromate of potash, which as a rule contains about 3 per cent. of impurities, are mixed with 0·832 litres of sulphuric acid in a stone jar while the mass is being constantly stirred; when the salt is changed to sulphate of potash and chromic acid, 9·2 litres of water are added, the stirring being kept up and the water allowed to flow from a spout about ½ inch wide; the crystal meal, which already is very warm, thus gets warmer and warmer and eventually dissolves completely. The exciters for this liquid are: a rod of the *densest* gas coal, 4 cm. broad, 1·3 cm. thick, and immersed 12 cm. deep into the liquid, and a rolled plate of zinc 4 cm. broad, 0·5 cm. thick, and immersed to the same depth as the coal; the zinc plate is entirely coated with a layer of wax (which is put on whilst hot), except that plane which is turned towards the coal and which is amalgamated. The distance between coal and zinc is entirely optional; in the spectral and analytical researches of Prof. Bunsen it varied according to circumstances between 3 and 10 millimetres. The results with this battery are, however, not very satisfactory with regard to duration and constancy of current, if the cell containing the exciting liquid is made of the same size and shape as those in the ordinary Grove or Bunsen battery. The reason of this lies in the circumstance that in the nitric acid of those batteries there is far more oxygen contained, which is employed for depolarisation, than in an equal weight of the chromate liquid, and that therefore a comparatively much larger quantity of the latter is used up to obtain the same effect. The chromic acid battery therefore, compared to Grove's battery, requires cells of at least three to four times more capacity. The best shape for these cells is that of narrow, high cylinders. The column of liquid, of about 1·6 litres, has a diameter of about 0·088 metres, and stands 0·28 metres high in the cylinder, which bears a mark at that height. The zinc-coal pair is only immersed up to half its height into the liquid column, and has an active zinc surface of about forty-eight square cm.

With regard to the constants of this chromic acid battery *without* clay cells, it considerably surpasses in electromotive force all other apparatus *with* clay cells hitherto used. It possesses an electromotive force which is about 13 per cent. larger than the ordinary charcoal-zinc or Grove battery. Its essential conduction resistance is about 12 per cent. smaller than that of Grove's battery with clay cells. In order to be able to judge the economical effect of the chromic acid battery, we will consider a little more in detail the chemical processes taking place in this battery. In unconnected freshly filled Grove batteries the consumption of zinc is very small, only when after prolonged use an electrolytic and endosmotic exchange has taken place between the two exciting liquids, a consumption of zinc, independent of the generation of the current, becomes apparent. In the unconnected chromic acid battery, however, the consumption of zinc at the very beginning is entirely the same as that which is observed in connected batteries during the generation of the current. This circumstance makes it indispensable to arrange the chromic acid battery in such a manner as to make it easy, at every interruption of the current, to bring the exciting plates out of contact with the liquid. This is attained by a simple hand lever arrangement by which the plates can be dipped into or raised out of the liquid. It is of particular interest, not only for practical purposes, but also from a theoretic point of view, to compare the consumption of zinc during the generation of the current with that in the unconnected battery, as theory alone gives no basis on which to decide the question whether the zinc disssolved in the unconnected battery is entirely, partly, or not at all used in the connected battery for the generation of the current. Investigation showed that the quantity of zinc dissolved in the disconnected battery is a little under half of the consumption of zinc necessary acording to theory to generate the current in the connected battery, and that only a part of the metal dissolved in the disconnected battery without current-generation is used up in the connected one for the generation of the current. This fact entirely corresponds with the view that the dissolution of the zinc must not be looked upon as the cause of the current, but as a necessary condition of the same. Investigation further showed that while in the chromic acid battery above described, on the average only 22 per cent. of zinc was lost, the loss in the nitric acid battery experimented with was 48 per cent. on the average. The chromic acid battery without clay cells is the least constant one amongst the ordinary constant batteries. But if used in a proper manner it may serve for a very long time. Prof. Bunsen possesses a battery of this kind, of forty pairs, with an active zinc surface on each plate of only forty square cm. For the last eight lecture-terms it has served for all experiments without its having been necessary during this long time to renew the zinc plates, or their coatings of wax, or the original exciting liquid, nor to clean the conducting connection parts; it has been merely necessary to renew now and then the amalgamation of the zinc plates (an operation which only takes a few minutes of time) and to replace that part of the liquid which was lost by evaporation in the air, by simply filling the cylinders with water up to the marks on their sides. The apparatus to this day still gives an electric arc between carbon points which amply suffices for the photo-chemical lecture experiments. The currents obtained by this battery, which has now been in use for already more than four years, are still powerful enough for demonstrations in electrolysis, spark spectra, decomposition of gases by induction sparks, &c., and will doubtless continue to suffice for all these purposes for some time to come. But we must again repeat that effects of such magnitude can only be expected if the precaution is used (and it is very easy to do so) not to leave the pairs in contact with the liquid for one moment longer than the duration of the current necessary for the experiments requires it.



The battery used for the production of spark spectra consists of four of the pairs above described. The pole wires conduct the primary current, of which a branch puts the current interruptor into action, to a Ruhmkorff apparatus, the induction coil of which has a diameter of nearly 0·2 metres and a length of 0·5 metres. The current induced in the same is conducted to the spark apparatus, standing in front of the slit of the spectroscope: *a*, a bottle with three necks, serves as a stand for the spark apparatus. The induced current goes from the mercury cup *b*, through the fine wire *c* to the carbon point *d*, which is fastened on a pointed platinum wire; thence it passes as a spark to the other carbon point *e*, and from this it reaches the second mercury cup *f*, which is connected with the other end of the induction coil. The platinum wires, which are surrounded by glass tubes sealed firmly upon them, can be moved upwards or downwards by the corks *h*, and this allows of a quick and exact fixing of the carbon points before the slit of the spectroscope.

The carbon points destined to receive the little quantities of liquids under examination are best prepared from the ordinary and not too light drawing charcoal, which is easily procurable. In order first to impart conducting power to the charcoal, a great number of the sticks are exposed to the most intense white heat for some time in a covered porcelain crucible, which stands in a larger clay crucible, and is on all sides surrounded by charcoal powder. Then the sticks are cut to points at one end, and the little charcoal cone thus obtained is cut off with a fine watchmaker's saw. In order to remove the silica, magnesia, manganese, iron, potash, soda, and lithia which the charcoal contains, about a thous and of the points are boiled in a platinum dish, first with hydrofluoric acid, then with concentrated sulphuric acid, then with concentrated nitric acid, and finally with hydrochloric acid, repeating each process several times, while between each manipulation each of the acids is removed by washing and boiling with water. After this treatment the carbon points are ready for use. A carbon cone of this description weighs about 0·015 grammes, and can absorb more than its own weight of liquid. The spark spectra obtained by aid of them are of very long duration.

We will report on the second part of Prof. Bunsen's treatise as soon as it has been published. W.

*HISTORICAL NOTE ON THE OBSERVATION OF THE CORONA AND RED PROMINENCES OF THE SUN**

SO much interest attaches to the phenomena of the corona and red prominences, as observed during total solar eclipses, and correct views of their nature and of the proper means of observing them are so recent, that I feel it proper to give here a brief account of what I believe to be the first attempt to see these, under ordinary conditions, with an uneclipsed sun.† This account is contained in the private diary of the late G. P. Bond, formerly director of the Observatory of Harvard College, which has become known to me through the kindness of his daughters.

Bond observed the total solar eclipse of July 28, 1851, at Lilla Edet in Sweden, and his report is published in the Memoirs of the Royal Astronomical Society, vol. xxi., p. 97.

From Sweden, Bond went to Geneva, where he arrived in September 1851, and from this point I may transcribe from his diary, making no changes except the occasional insertion or omission of unimportant words.

"*Geneva, Sunday, Sept.* 14, 1851.—I think I must go to Chamounix to try whether it may be possible to discern the red flames on the sun's disc by occulting all but the very edge, upon one of the lofty peaks. It seems to me not altogether impossible. Certainly an experiment worth trying and a new application of the '*Aiguilles.*' . . .

"*Geneva, Sept.* 15, 1851.— . . . The weather looks dark and lowering, with an uncomfortable north-east

* By Edward S. Holden. Reprinted from the August number of the *American Journal of Science.*

† Airy, Nasmyth, Baden-Powell, Piazzi-Smyth, and others experimented in this direction, about this time, with various results. See Edinburgh Ast. Obs., vol. xi. p. 279; Mem. R. A. S., vol. xvi. p. 301, &c.

wind, but M. Plantamour thinks it is likely to be fine weather, and on this recommendation I took a place in the diligence for Chamounix. . . .

"*Chamounix, Sept.* 18, 1851.—Last evening the stars were shining through the opening clouds, giving promise of improving weather, but a glance out of the window this morning dispels all such anticipations. . . .

"*Chamounix, Sept.* 19, 1851.—I woke this morning at five, and my first impulse was to go to the window to see the signs of the weather. Last night I had hopes of an improvement. But I was surprised to find a clear sky; some clouds were resting round the *aiguille*, but the summit of Mont Blanc was clear. Started for Montanvert at 7.15 with a guide. . . .

"*Mer de Glace.*— . . . Attempted two or three times to hide the sun's disc by projecting rocks to try to see the red prominences, but could not get a station far enough off. . . .

"*Chamounix, Sept.* 20, 1851.—Snowing fast in morning. Weather desperately bad. But before going to bed it was quite clear. . . .

"*Chamounix, Sept.* 21, 1851.— . . . The fine prospects of last night were effectually put aside by another snowstorm. . . .

Chamounix, Sept. 22, 1851.—The morning bad as usual. . . .

Chamounix, Sept. 23, 1851.—This morning still cloudy, yet the prospect for an improvement was encouraging. Soon after breakfast the sun appeared struggling in the clouds, and I hurried off with a spy-glass not to lose the slightest chance of seeing the phenomena I wished to. . . . I spent two or three hours in the wet fields to no purpose. In the afternoon there was an effort at clearing again.

"*Chamounix to Martigny, Sept.* 24, 1851.—The clouds this morning still hung on the mountains, but overhead there seemed some signs of clear sky. To make sure of losing no chance I took an early breakfast and left for the fields with the ordinary spy-glass belonging to the hotel under my arm. Sometimes it would be almost clear, and then again it began to rain, and I was undecided whether to give up and start for Martigny or to stay another day. At last I saw the sun's disc and took up my station on the edge of the shadow of the *Aiguille de Blettière*. It was still cloudy, but I was satisfied from the nature of the experiment—

"1st. That a very clear air is necessary.

"2nd. Plenty of time to choose projections, affording views of as large a portion of the circumference of the disc as possible while the rest is hidden.

"And lastly, a good achromatic telescope easily moved.

"I did not expect to find it so easy an experiment, nor to find a mass so well fitted for the purpose as the *Aiguille de Blettière*, which has a smooth edge, inclined, so as to allow the sun to disappear slowly behind it,

"The naked eye easily bears a small portion of the sunlight. From 7 to 9½ I followed the shadow over the valley. It was nearly clear for a few moments before it reached the woods on the side of the mountain, but there were still some light clouds over the sun, and nothing could be seen certainly of the corona; the clouds and mist would account for what I did see, and on the other hand the colour of the telescope supplied too much red just at the edge for one to be able to see any of the red flames, if they existed there.

"On the whole, I am more than ever sure that the experiment can be made, and I think will be by some one more fortunate than I."

SOLAR OBSERVATION IN INDIA

NOW that the subject of solar observation in India is likely to occupy the attention of the scientific public, he following details of the Solar Observatory now in progress of construction at Calcutta may be of interest to readers of NATURE.

The suggestion emanated in the first place from the well-known Italian astronomer and spectroscopist, Prof. Tacchini, who was sent to India by the Italian Government as director of the Transit of Venus Expedition. The idea thus put forth was at once taken up by Père Lafont, the principal of St. Xavier's College. A subscription was opened to enable the work to be carried on, and in a short time the collections had amounted to 10,000 rupees, to which the Indian Government added 5,000 rupees. So warmly does the idea seem to have been taken up, that a theatrical benefit was given, at the suggestion of Col. Wyndham, in aid of the Observatory fund.

The observations proposed to be carried out are to supplement those made in Italy, where from November to March (inclusive) the sky is often unfavourable for observation. A complete annual record of changes in the sun's chromosphere, &c., will thus be kept up. With regard to instruments, an equatorial of 7-inch aperture is now being constructed by Merz, but more funds are needed to complete the instrumental "plant" of the Observatory. In course of time it is to be hoped that a transit instrument and a complete set of meteorological apparatus will be added.

The Italian Transit of Venus Expedition has thus been the means of sowing seeds which, finding themselves in a soil most favourable for development, are calculated at no very distant period to bear fruit of the greatest value to science. When in Calcutta with the Royal Society's Eclipse Expedition, last April, I visited the Observatory in company with Prof. Tacchini, and the work of construction was then in a very advanced state. Prof. Tacchini has recently written to say that the building is now almost completed.

The energy which has been displayed in connection with the Calcutta Observatory * redounds greatly to the credit of our Indian colleagues. It is only by systematic observations of this kind, carried on by public enterprise, that we can ever hope to detect cyclical changes in the sun's composition and constitution—changes which, taking enormous periods for their completion, may demand continuous records to be carried on even through many generations. R. MELDOLA

THE LAWS OF STORMS †

M. FAYE, in the article referred to below, and of which we propose to give an abstract at considerable length, begins by referring to the stupendous force of tropical tempests as contrasted with those of Europe, and to the practical importance of knowing the laws which regulate them. Many persons, he believes, on reading the title of his paper, will be surprised to learn that hurricanes have laws, or will ask what an author means by proposing to expound and vindicate the Law of Storms.

Laws of Storms.—Not only are storms subject to laws of great interest to science, but from these laws practical rules may be deduced which will enable us to avoid these dangers, or escape from them, should we happen to be caught in a storm. These rules are taught in all naval schools, and are the foundation of the sailor's safety. The validity of the laws on which they are based has, however, been disputed by some writers on Meteorology, and therefore the Bureau des Longitudes has authorised the publication of M. Faye's paper, in which he attempts clearly to expound and to defend the disputed laws.

Referring to the valuable labours of Piddington in India and Redfield in the United States, and of Reid, M. Faye says that the only premises they had to start

* The Observatory is situated in St. Xavier's College, Park Street, Calcutta.

† Abstract of a paper, "Défense de la Loi des Tempêtes, par M. Faye, Membre de l'Institut," in the *Annuaire* of the Bureau des Longitudes for 1875.

from were the idea that there ought to be something regular in the progress of hurricanes, and the observed fact that in every disastrous storm the wind appeared to move in a circle. They said to themselves: "We do not seek to know how storms are formed, but how they progress." Instead of speculating, as did former meteorologists, on storms of aspiration, on the rôle of electricity, on the conflict of opposing currents, &c., they collected for each tempest extracts from the log-books of all the ships which had been involved in it. After having abstracted and arranged this immense quantity of material, they marked upon a chart, at certain dates, the positions of these ships and the direction of the winds observed. Then, by placing on this chart, after several trials, a series of tissue-papers on which had been drawn concentric circles, they made sure that the wind-arrows at the same instant closely coincided with these circles, so that at that very instant, over all the region subjected to the storm, the mass of air resting on the ground or on the sea must have been acted on by a vast gyrating movement around a centre. Some idea of the nature of these researches will be obtained from Fig. 1, which shows a very small part of the chart of the hurricane which ravaged the island of Cuba in 1844. Redfield collected sufficient information to determine the figure of the hurricane at twenty-five different times, between Oct. 4 and 7; the figure shows two of these. The same phenomenon was reproduced at all the other times; everywhere the hurricane assumed this strikingly circular form.

All tornadoes, typhoons, hurricanes, present the same character wherever they occur, and they preserve it throughout the entire duration, and over all their area, which often extends to more than 600 leagues. The conclusion is evident; there is evidence here of a vast rotatory movement, definitively confined to one portion of our atmosphere, which is at the same time subjected to a movement of translation.

It is remarkable that when all the separate results obtained over the whole of the northern hemisphere are compared, it is seen that the gyration takes place always and everywhere from right to left, in a direction opposite to that of the hands of a watch (see Fig. 1). Still more remarkable is it that over all the southern hemisphere the same law, the same gyration is found, but in a direction opposite to that of the preceding, from left to right, *i.e.*, the same direction as that of the hands of a watch. There is here evidently one law, and that a law without exception; these terrible gyratory movements turn constantly to the left in the northern hemisphere, to the right in the southern hemisphere.

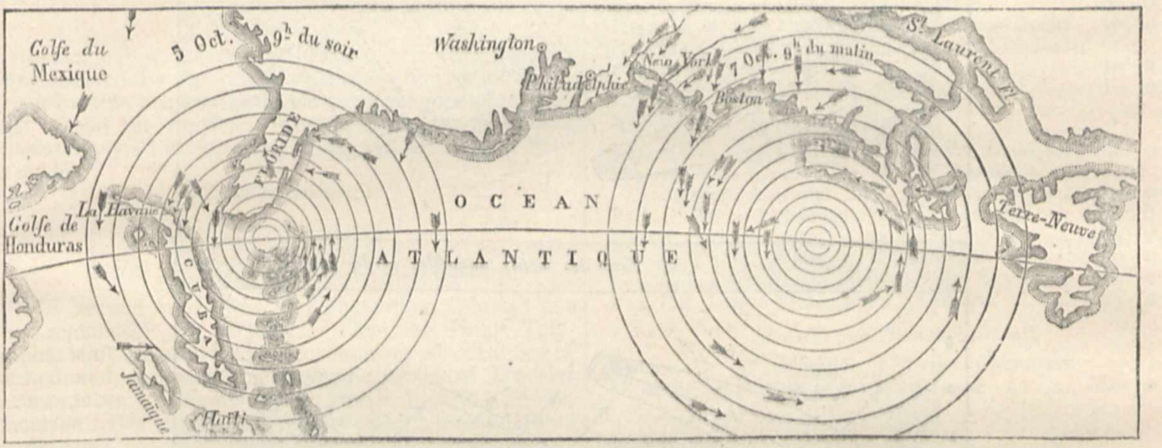


FIG. 1.—Hurricane at Cuba from Oct. 5 to 7, 1844.

Finally, the trajectories present some very striking common characteristics in each hemisphere, and in both hemispheres a remarkable symmetry. The lines tracked by the centres of these cyclones do not descend directly from the equator to either pole; on the contrary, they incline first to the west, then, after having passed the limit of the trade-winds, they bend towards the east, in a final direction roughly perpendicular to the former. Fig. 2 will enable the reader to follow in the two hemispheres the development of cyclones. Originating not far from the zone of calms or of variable winds, on both sides of the equatorial zone, they measure scarcely more than two or three degrees at the outset, but as they proceed towards higher latitudes their area gradually enlarges. In the two temperate zones they attain a diameter of more than ten degrees, and frequently occupy upon the terrestrial globe a space considerably larger than that of France.

Thus all is symmetrical on each side of the equator, or rather of the zone of calms, which oscillates a little each year with the course of the sun. There is symmetry in the direction of rotation, symmetry in the direction of progressive motion, general symmetry in the figure of all these trajectories; and this holds good all over the globe.

Such are the storm laws, the discovery of which is mainly due to England and the United States, "the two greatest maritime powers of the world." The product purely of observation, of empiricism, to use that word in its highest sense, they have not yet reached the stage of theory. On the contrary, in order to discover these laws, it has been necessary to cast aside contemporary prejudices and doctrines, the deadening influence of which we have hourly opportunities of witnessing.

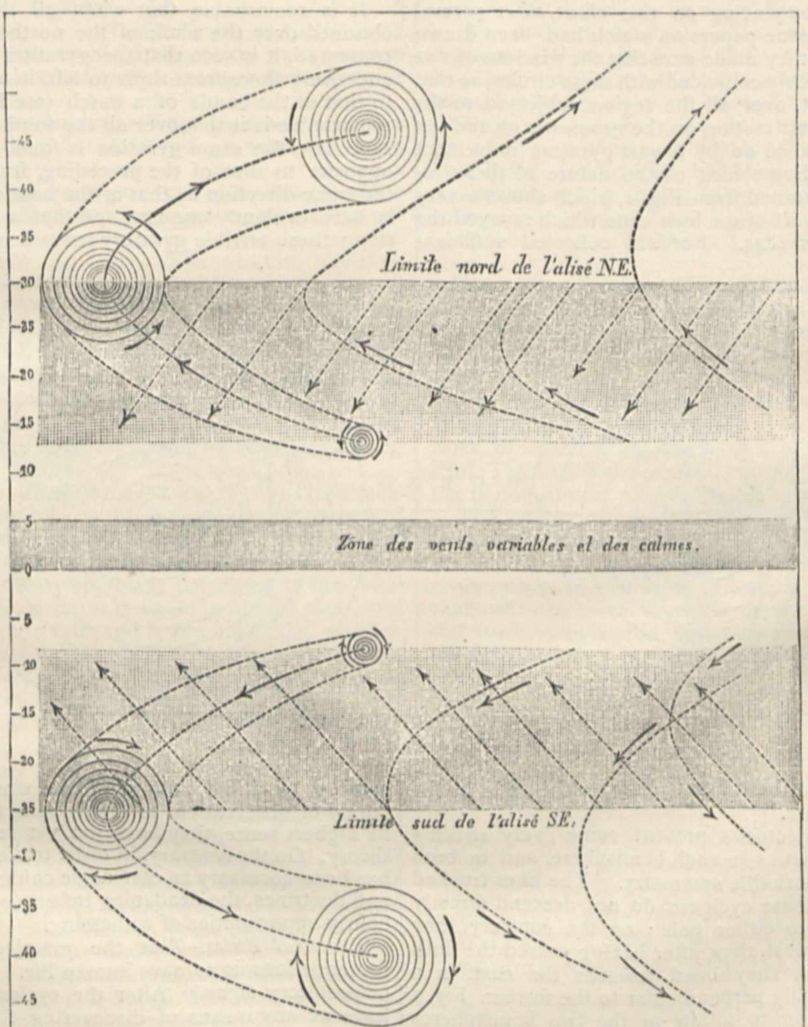
Practical Rules.—But the practical object of these investigations is to save human life. Do we know of no premonitory signs? After the cyclone has commenced, have we any means of discovering the direction of the centre where the rotation is accelerated, where all the sources of danger are accumulated? How can we find out the direction of its march? How learn whether a ship is caught in the dangerous region, where the rate of the wind is the sum of the rates of rotation and of progress; or in the moderate region, where the rate of the wind is only the difference? Finally, what manœuvres are necessary in order to avoid the tornado or to escape from it if by mischance we should be caught in it?

To all these questions there are answers, some exact, imperative as are the exigencies of the danger; others more elastic, leaving room for tact and ability on the par of a commander.

By a fall continuous and prolonged, the barometer, which is never at fault in the tropics, announces that a cyclone is at a distance. As soon as the wind blows with a certain force, it is easy to determine the direction in which the centre of the cyclone will be found. The following is Piddington's rule:—Turn the face to the wind and stretch out the right arm; the centre is in this direction. The left arm must be used when a ship is in the southern seas. Soon the wind increases, and the fall of the barometer becomes more rapid; the centre is getting nearer, for the cyclone has an onward motion. If the wind continues to increase without changing direction, you are in the very path of the centre, and soon you will be in the very heart of the tempest. Then suddenly a calm ensues; at the centre of the cyclone exists a circular space where a relative calm prevails. There the sky reassuming its serenity, the sailor might be led to believe himself safe; but this space is soon passed, and immediately the tempest recommences. Only the wind has suddenly jumped round 180 degrees; it blows now in the direction opposite to the previous one, at right angles to the trajectory of the centre of the cyclone.

The situation which we have just supposed is a peculiar case; in general the vessel will be found to the right or the left of this trajectory, whose direction, moreover, an

FIG. 2.—Hurricanes of the northern hemisphere (July to October).



Hurricanes of the southern hemisphere (January to April).

attempt must be made to determine.* The alternative is far from being a matter of indifference; it is a question of life or death, for the one corresponds to the favourable semicircle, the other to the dangerous. The following is Reid's rule, which eliminates all uncertainty:—In whatever hemisphere, if the wind changes direction successively by turning in the same direction as the cyclone itself, the favourable semicircle is indicated; if the wind changes by turning in the direction opposite to that of the proper rotation of the cyclone, the dangerous semicircle is indicated.

This may be accounted for by examining Fig. 3. The observer, supposed to be immovable, has his face turned towards the series of winds which will strike him successively as the cyclone passes over him.

In the favourable semicircle (southern hemisphere), if the ship behaves well in a rough sea, it is possible to avoid the centre and the cyclone itself by the shortest way, perpendicularly to its trajectory. The storm is

* We do not dwell on this last point, which can only be solved by skilfully comparing the indications of the barometer with those of the direction and force of the winds.

always formidable, but it is manageable. If, however, the violence of the wind, the state of the sea, and the weakness of the ship should make flight impossible, there should be no hesitation in putting about ship and bringing to on the starboard tack (the wind on the right side). The vessel appears then to make for the centre of the

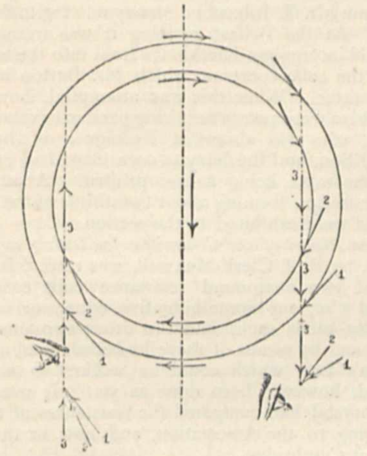


Fig. 3.

hurricane, but it makes no headway; it thus escapes being covered by the wind, and there is no risk of being struck by seas behind, inevitable consequences of a port tack. Soon the hurricane disappears by its motion of translation, good weather reappears, and at last sail may be made.

(*To be continued.*)

THE BRITISH ASSOCIATION

THE second *soirée* was very interesting, although not remarkable for novelties. The Post Office Telegraphic staff appeared in force, showing all varieties of method and apparatus. A splendid series of Geissler vacuum tubes was exhibited by Mr. F. J. Fry. Sir W. Thomson's tide-gauge and tide-calculator, the apparatus for deep-sea sounding, models of railway signals, means of communication between passengers and guard, and Dr. Leitner's collections from Dardistan were among the most attractive objects.

The concluding general meeting presented no remarkable feature, and called forth no very notable speeches. Among the papers to be printed in full in the Report is that of Prof. Cayley, on the application of mathematical trees to chemical theory. The local committee and officials were thanked most heartily and deservedly. They have had the best intentions, adequate means, and good plans, and have employed the energy needed for the fruition of their ideas. The actual number of members, associates, and ladies present during the meeting was 2,249, the number having been somewhat swelled by late arrivals.

The vote of thanks to the President, moved by Sir W. Thomson and seconded by Dr. Carpenter, was not merely formal. Sir W. Thomson eulogised Sir John Hawkshaw as a man who believed that good practice proceeded from good theory. Certainly the President's tone of mind seems to have influenced the work and proceedings of the meeting, for it has been on the whole quiet and genial, yet busy and important in useful results obtained by the scientific employment of common sense, if not of imagination. Thus ended the formal proceedings of a meeting in which three Sections had to sit up to the latest moment in order to get through their work.

The following is the list of grants of money appropriated to scientific purposes. The names of the members who would be entitled to call on the general treasurer for the respective grants are prefixed:—

	£	s.	d.
Mathematics and Physics.			
*Cayley, Prof.—Printing Mathematical Table ...	159	4	9
*Brooke, Mr.—British Rainfall	100	0	0
*Glaisher, Mr. J.—Luminous Meteors (25*l.* renewed)	30	0	0
*Maxwell, Prof. C.—Testing the exactness of Ohm's Law (renewed)	50	0	0
*Stokes, Prof.—Reflective Power of Silver and other Substances (renewed)...	20	0	0
*Tait, Prof.—Thermo-Electricity (renewed)	50	0	0
Thomson, Sir W.—Tide Calculating Machine ...	200	0	0
Chemistry.			
*Roscoe, Prof.—Specific Volume of Liquids... ...	25	0	0
*Armstrong, Dr.—Isomeric Cresols and the Law of Substitution in the Phenol Series	10	0	0
Clowes, Mr. F.—Action of Ethylbromobutyrate on Ethyl Sod-aceto-acetate	10	0	0
*Allen, Mr.—Estimation of Potash and Phosphoric Acid	20	0	0
Geology.			
*Lubbock, Sir J., Bart.—Exploration of Victoria Cave, Settle...	100	0	0
*Evans, Mr. J.—Record of the Progress of Geology	100	0	0
*Evans, Mr. J.—Kent's Cavern Exploration... ...	100	0	0
*Herschel, Prof.—Thermal Conductivities of Rocks	10	0	0
*Hull, Prof.—Underground Waters in the New Red Sandstone and Permian	10	0	0
*Bryce, Dr.—Earthquakes in Scotland	20	0	0
Biology.			
*Sclater, Mr.—Record of the Progress of Zoology..	100	0	0
*Dresser, Mr.—Close Time for the Protection of Indigenous Animals	5	0	0
Balfour, Prof.—Physiological Action of Sound ...	25	0	0
Huxley, Prof.—Zoological Station at Naples ...	75	0	0
*Brunton, Dr. L.—Nature of Intestinal Secretion...	20	0	0
Fox, Col. Lane—Instructions for Use of Travellers	25	0	0
Fox, Col. Lane—Prehistoric Explorations	25	0	0
Statistics and Economic Science.			
Beddoe, Dr.—Examination of Physical Characters of the Inhabitants of the British Isles	100	0	0
Mechanics.			
*Froude, Mr. W.—Instruments for Measuring the Speed of Ships (renewed)...	50	0	0
Napier, Mr. J.—Effect of the Propeller on the Turning of Steam Vessels	50	0	0
	£1489	4	9

* Re-appointed.

I was fortunate enough to get a ticket for the Salisbury and Stonehenge excursion, for which the applications were very numerous. Mr. Blackmore's magnificent museum illustrating the Stone Age was a delight to all scientific minds; and the presence of the founder, his brother, and his brother-in-law, Mr. E. T. Stevens, enhanced the pleasure of the visit. The Cathedral and Stonehenge, in addition, made up a very full day's round. The Mayor of Bristol took a party to Bowood and Avebury. How the Rev. Bryan King obtained his data for estimating that Avebury was about seven centuries older than Stonehenge I cannot conceive. The Silbury tumulus afforded a splendid view to the visitors, if very little science could be got out of it. A third party, that drove through the Cheddar valley, saw at Stanton Drew yet a third of the famous stone erections so conveniently placed around Bristol. The Tortworth excursion was a really hard day's work among many varieties of rock, especially palæozoic, but it was as profitable as it was hard, for the geologist. The Bristol waterworks were of high interest for engineers; and the attractions of Bath, Wells, and Tintern were displayed to every advantage by reason of beautiful weather and hearty welcomes.

REPORTS.

Report of the Committee on Mathematical Tables.—The portion of the report that had been prepared by Prof. Cayley during the year contained a *résumé* of works and memoirs on the theory of numbers. The publication of the elliptic function tables had, under the direction of Mr. J. W. L. Glaisher, proceeded during the year, and the first sixty-four pages of the table, printed from the stereotype plates, were exhibited to the Section. It was expected that the whole table would be printed by the next meeting. Mr. Glaisher stated that considerable additions had been received from mathematicians relating to the report on general tables, and that it was probable a supplementary report on this subject might be presented at the next meeting of the Association.

Hyperelliptic Functions.—Mr. W. H. L. Russell stated the contents of the portion of his report that he had written in the year, and which related chiefly to memoirs of Weierstrass. His report would be completed in two more parts.

Report of the Committee on Mathematical Printing, by Mr. W. Spottiswoode.—At the Belfast meeting the committee, consisting of Mr. Spottiswoode, Professors Stokes, Cayley, Clifford, and Mr. J. W. L. Glaisher, was appointed to report on mathematical notation and printing, with the view of leading mathematicians to prefer in optional cases such forms as are more easily put into type, and of promoting uniformity of notation. The report related wholly to printing, and contained a list of forms having the same signification, the one requiring "justification," and the other not (such as *ex. gr.* $\sqrt{a + x}$, and $\sqrt{(a + x)}$.) There were also attached diagrams showing the mechanical operation of setting up mathematical expressions in type, so that when there were two forms equally satisfactory from the mathematical point of view, writers might choose the one that would give the printer less trouble; as everything that tended to cheapen mathematical printing tended to the spread of the science. With regard to notation, the committee had thought it better not to report, feeling that in presence of the differences of opinion that must exist, it would be desirable that the matter should be discussed by a larger committee. The committee was reappointed to report on mathematical notation, with the addition of Sir Wm. Thomson, Professors H. J. S. Smith and Henrici, and Lord Rayleigh.

Report of the Committee on Tides, by Sir William Thomson.—He remarked that tides rise and fall in a series of harmonic vibrations, like the various tones in music, some tidal waves being due to the moon, others to the sun, others to meteorological causes. Even the overtones in music—so thoroughly studied by Helmholtz—were represented in the tidal waves. The committee had been engaged upon tides for a long time, and had shown the Government, harbour authorities, and others interested, the way to continue the work, but it could do so itself no longer, for he believed that day to be the last of the existence of the committee. The calculations connected with tidal observations were of a laborious nature. Col. Walker, of the Trigonometrical Survey of India, had helped the committee very much by printing the forms required for the calculations. Col. Walker had also had a series of tidal observations made in the Indian seas, and might possibly send the results home to have the calculations made from them. The Indian Government would probably have further observations made, especially in an important new harbour they were constructing at Madras. A great mass of other observations was accumulating. Mr. H. C. Russell, the Government astronomer at Sydney, had made several years' tidal observations, but had been obliged to stop them on account of the cessation of the grant for the work, but he hoped that the duty would be undertaken once more; as yet, the committee had no reductions whatever of tidal observations in the southern hemisphere, and knew nothing about the tides there. He had been promised a long series of observations, extending over eighteen years, from Brest, and he had applied for a series of eighteen years' observations from Toulon; so that he expected to obtain some information about tides on the French coast. The Tidal Committee had had some assistance from the Royal Society, which had given it a grant of 100*l.* to carry on tidal calculations. It had thus ascertained that the tides in Erebus Bay were connected with the Atlantic and not with the Pacific. Sir William Thomson then exhibited to the meeting and described his tide-gauge and tide-calculating machine, the latter being an improvement on that first described at Brighton and shown at Bradford two years ago. Although the old committee on tides ceased to exist at this meeting, a new one was appointed, consisting of Sir W. Thomson, Prof. J. C. Adams, Rear-Admiral Richards, General Strachey, [Mr. W. Parkes, Col. Walker, Prof. Guthrie, Mr. J. W. L. Glaisher, Mr. John Exley, Mr. J. N. Shoolbred, and Mr. J. R. Napier, and the sum of 200*l.* was granted to them for completing and setting up in London, where it may be available for use, Sir William Thomson's tide-calculating machine. It was suggested that perhaps the machine might be placed at South Kensington.

Report of the Committee on Wave Numbers.—Portions of a letter were read from Mr. G. Johnstone Stoney relating to the work done in the year. At the Belfast meeting it was arranged that Mr. Stoney should interpolate Kirchhoff's lines into the table of wave numbers of the solar spectrum which Mr. Burton had prepared for the committee. When this was attempted, however, it was found that there were points requiring personal explanations from Mr. Burton, who was absent at Rodriguez on the Transit of Venus expedition, and the delay so occasioned had prevented any portion of the table being as yet printed. About thirty-four folios in manuscript, forming about two-thirds of the whole, were complete and were exhibited to the section.

An *interim Report of the Committee for testing experimentally Ohm's Law*, by Prof. Clerk-Maxwell, was read. It stated that he had had two compound resistance coils constructed by Warden and Co., one containing five equal, or nearly equal, coils of thirty ohms each, and the other two similar coils of thirty ohms, and by means of these he had devised a satisfactory test of Ohm's Law which could be worked to about $\frac{1}{40000}$. Nothing had, however, been done as yet. It was mentioned that Mr. Chrystal had compared the resistances of the standard coils belonging to the Association, and now in the Cavendish Laboratory at Cambridge.

Prof. Thorpe presented a preliminary *Report of the Committee appointed for the purpose of determining the specific volumes of liquids.* It gave a *résumé* of experiments upon certain liquids and gases, experiments made with a view of following up the work of Hermann Kopp, to whom almost all our knowledge of the subject is due, and further to arrive at definite conclusions with reference to the laws laid down by Kopp.

Prof. Corfield, on reading the *Report of the Sewage Committee*, observed that want of funds during the past year had prevented them from employing a sufficient amount of labour to obtain many useful results, but he was happy to state that the prospect for the ensuing year was brighter, as they had had a very liberal offer of pecuniary assistance. The observations which have been made on the Sewage Farm (situated near Romford) go to show that the weight of the crops removed from the land has increased each year. The great thing required is to make a comparison between the nitrogen taken up by the crops and the effluent nitrogen, and in order to accomplish this with accuracy it was necessary that the experiments should be constantly repeated, and should extend over a considerable number of years.

Report of Committee for considering the desirability of establishing a close time for the Protection of Indigenous Animals.—This report expressed regret that it had been found impossible to introduce the desired measure into Parliament this year in time to allow of its being carried; but Mr. Henry Chaplin, M.P. for that part of the Atlantic Doldrums which lies in the track of Mid-Lincolnshire, holds out the hope that he will bring forward such a measure early next session. The committee continue to receive assurances of the efficient working of the Sea Birds' Perservation Act of 1869.

The report (unfortunately the last) of the Sub-Wealden Exploration states that the new bore-hole has failed to penetrate to the Palæozoic rocks. The small diameter prevents tubing, and the sides now appear to be too friable to preserve verticality. Cessation of the work is hourly expected. The most noteworthy result of this heroic but unsuccessful investigation is the great thickness of the Kimmeridge clay, which, as was predicted by Mr. Searles Wood, considerably exceeds the estimate of the Sub-Wealden Boring Committee.

SECTIONAL PROCEEDINGS

SECTION A—Mathematics and Physics

Dr. J. Janssen made four communications to the Section, the first of which related to the eclipse of April 1875, as observed at Bangchallô (Siam). He used a special telescope for the study of the corona. The results were—1. The establishing that the line 1474 is infinitely more pronounced in the corona than in the protuberances. This line seems even to stop abruptly at the edge of the protuberances without penetrating them. The light, then, which gives the line 1474 belongs entirely to the corona. This observation is one of the strongest proofs which can be adduced to prove that the corona is a real object, a matter radiating by itself. The existence of a solar atmosphere situated beyond the chromosphere—an atmosphere which M. Janssen had recognised in 1871, and proposed to call the coronal atmosphere—thus receives confirmation. 2. *Height of the coronal atmosphere.* In 1871 Dr. Janssen announced that the coronal atmosphere extended from half the sun's radius to a whole radius at certain points. This assertion has been confirmed not only by the direct views of the phenomenon, but also by photography. At Dr. Janssen's request Dr. Schuster took photographs of the corona with exposures of one, two, four, and eight seconds. In this series of photographs the height of the corona increases with the time of exposure. The height of the corona in the eight-seconds' photograph exceeds at some points a solar radius. (It is true that we ought to take account of the influence of the terrestrial atmosphere.) 3. As the sky was not of perfect clearness at Bangchallô, Dr. Janssen observed phenomena that explain previous observations of eclipses which seemed to invalidate the existence of the corona as a gaseous incandescent medium. On the whole, the observations of the 5th of April, 1875, have advanced us a fresh step in the knowledge of the corona by bringing forward new proofs of the existence of an atmosphere round the sun, principally gaseous, incandescent, and very extended.

In his second paper Dr. Janssen stated the results obtained by the expedition to Japan to observe the Transit of Venus. The expedition, which was under Dr. Janssen's direction, divided into two parts, the one taking up its station at Nagasaki and the other at Kobí. At Nagasaki Dr. Janssen observed the transit with an equatoreal of 8 inches aperture. (1) He obtained the two interior contacts. (2) He saw none of the phenomena of the drop or of the ligament; all the appearances were geometrical. (3) He observed facts which establish the existence of an atmosphere to Venus. (4) He saw the planet Venus before her entry on the sun, with suitable coloured glasses. This important observation proves the existence of the coronal atmosphere. (5) There was taken at Nagasaki a plate of the revolver for the first interior contact. (6) M. Tisserand observed the two interior contacts with a 6-inch equatorial; the contacts were sensibly geometrical. (7) Sixty photographs of the transit on silvered plates were obtained; and (8) also some other photographs (wet collodion and albumenised glass). At Kobí (weather magnificent) fifteen good photographs of the transit (wet collodion and albumenised glass) were obtained of about 4 inches in size; they will admit of being combined with the English photographs at the southern stations. The astronomical observation of the transit was successfully made by M. De la Croix, who was provided with a 6-inch telescope. His observations attest the existence of an atmosphere round Venus.

Dr. Janssen's third communication related to his magnetic observations in the Gulf of Siam and the Gulf of Bengal. He made observations at Bangkok, Bangchallô, Ligor, Singora, and Singapore, and concluded that the magnetic equator passes actually between Ligor and Singora, about 7° 43′ N. latitude. The *line without declination* passes very near to Singapore. In the Gulf of Bengal the equator passes through the north of Ceylon (the precise position will be given). The position of Ligor has been rectified. It is erroneously placed on the maps lat. 8° 24′ 30″.

Dr. Janssen had also made some observations which relate to mirage at sea. He had paid great attention to the phenomena in all his journeys since 1868, and had observed some very curious facts relating to mirage chiefly at sunrise and sunset. He found that (1) the mirage was almost constant at the surface of the sea; (2) that the appearances were explained by admitting the existence of a plane of total reflection at a certain height above the sea; (3) that the phenomena are due to a thermic and hygrometric action of the sea on the neighbouring atmospheric strata; (4) that there exist at sea direct, inverse, lateral, and other mirages; (5) that the phenomena have a very general influence on the apparent height of the sea horizon, which is sometimes diminished, sometimes increased. This variation of the apparent horizon it is very important to take into account, if we remember the use made of the horizon in nautical astronomy.

Prof. Hennessy, of Dublin, read two papers, one *On the influence of the physical properties of water on climate,* and the other *On the possible influence on climate of the substitution of water for land in Central and Northern Africa.* In the former the author referred to his earlier writings, in which he had taken an opposite view to Sir John Herschel, who stated that the effect of land under sunshine was to throw heat into the general atmosphere, and to distribute it by the carrying power of the air over the whole earth, and that water was much less effective in this respect, the heat penetrating its depths and being there absorbed, so that the surface never acquires a very elevated temperature even under the equator. Prof. Hennessy had arrived at the conclusion that of all substances largely existing in nature, water was that which was the most favourable to the absorption and distribution of solar heat throughout the external coating of the earth.

In his second paper, the author referred to the fact that more than six years since he had put forward proofs of the connection between some of the hot winds that blow from the south-west in Central and Southern Europe with the currents of the Atlantic, and not with the Desert of Sahara, as has been usually supposed. Similar views had been enunciated by Prof. Wild, director of the Physical Observatory of Russia, and others. The attention excited by the great midday heat of Central Africa caused many to overlook the remarkably low nocturnal temperature, and thus to ascribe to the desert a thermal influence that it does not possess. The author's views with regard to the physical properties of water in connection with climate, indicate that the substitution of an area of water over the Sahara for the existing dry land would be followed by the storing up of the heat received so largely in that region from the sun's rays which is now partly dissipated by nocturnal radiation. A great mediterranean sea in Africa would become a source of positive thermal influence on distant places. In the Red Sea the temperature is high by night as well as by day, and this would also occur in the hypothetical mediterranean of the Sahara. The climatal effect of this sea would upon the whole result in a higher mean temperature for these parts of the globe, and it would undoubtedly not operate in producing a lower temperature in Europe so as to cause a descent of the snow line. Its operation would probably be the reverse.

Prof. Osborne Reynolds read a paper *On the Force caused by the communication of Heat between a Surface and a Gas.*—This paper dealt chiefly with the remarkable discovery recently made by Mr. Crookes, that, under certain conditions, discs of pith suspended in a very perfect vacuum, and at the end of arms free to rotate, are made to spin round when light or radiant heat falls upon them. Prof. Reynolds said that he believed that Mr. Crookes asserted that radiant heat was attended by a force which produced this effect, but no such assumption would, he thought, explain the results. When a candle was presented the disc would tend to run away, and when a piece of ice was presented it would tend to follow; this showed that the force was not a radiative one, and he thought that, except as regarded the raising of the temperature of the body, radiant heat had nothing to do with the motions. The suspended body might give up its heat to the ether or to the surrounding gas, and thus propel itself, for the communication of this heat to the surrounding medium must be accompanied by a reaction. It had been said that Mr. Crookes used a perfect vacuum, so that there could be no gaseous reaction; but it remained to be proved that he used a vacuum so absolutely perfect. The greater the perfection of the vacuum the less was the resistance, and that was why the body appeared under such circumstances to be driven by a greater force. He had not witnessed the experiments with light, made by Mr. Crookes, but he thought that the results were probably due to the conversion of light into heat.—The discussion on this paper was adjourned, as it was hoped that Mr. Crookes would be able to be present; unfortunately, however, he was not able to arrive in time, and Prof. Balfour Stewart, the president, remarked that, as had been said by Prof. Stokes, it was doubtful whether Prof. Reynolds's explanation covered the whole ground. There was something else besides residual gas in the bulbs, viz., ether, and the particles of the radiometer might communicate more force to the ether when moving in one direction than when falling back again; consequently, motion might be given to the whole body to restore the balance. At all events Mr. Crookes's experiments were among the most interesting in the range of physical science.

Capt. H. Toynbee read a paper *On the physical geography of that part of the Atlantic Doldrums which lies in the track of ships crossing the Equator.* The paper was accompanied by diagrams, which showed the isobaric lines of mean pressure for each ·05 of an inch, together with arrows showing the prevailing winds and their force, also the isothermal lines for every second degree of air temperature, and further the isothermal lines for every second degree of sea temperature, together with arrows showing the prevailing currents and their speed in twenty-four hours. The paper called attention to important facts relating to atmospheric pressure, temperature, wind, currents, weather, sea-charts, natural history, earthquakes, &c. The diagrams gave monthly pictures of the Doldrums, showing how in some months they are wedge-shaped, as the late Commodore Maury remarked. The whole paper was a *résumé* of a work about to be published by the Meteorological Office.

Sir W. Thomson gave an account of the graphical process employed by him and Mr. J. Perry (now professor in Japan) for determining the form of a hanging drop, and other cases of the capillary surface of revolution.

On account of the interest attaching to the address of the President of the Mechanical Section *On Stream Lines*, and to the fact that as it was being delivered simultaneously with Prof. Balfour Stewart's address only a few members of this Section were able to hear it, Mr. Froude repeated it and the experiments with which it was accompanied again in Section A on the Tuesday morning. One experiment in particular was very interesting. A wooden wheel was fixed at a height of about 14 feet, and an endless chain hanging loosely over the wheel in a loop drooped to within 4 feet of the ground. When the wheel with its suspended chain was made to rotate rapidly by means of multiplying gear, the links of the chain symbolised the particles of a running stream of water. When the chain was struck, while it was rotating, with a wooden mallet, the curved forms into which it was thus beaten were to some extent persistent, as if it were a stiff, fixed wire rope, instead of being a loose chain in motion. Mr. Froude said that this experiment illustrated how water in flowing through pipes did not tend to push them straight, but rather adapted its motions to their curvatures.

In a letter from Mr. Meldrum, of Mauritius Observatory, written to accompany forty-nine tables (which, however, had not arrived), he expressed an opinion that the evidence adduced in favour of a rainfall periodicity was so strong that he believed we should by and by be able to predict the general character of the seasons.

Communications were made to the Section by Mr. H. A. Rowland, of John Hopkins University, Baltimore, *On the Magnetising Function of Iron, Nickel, and Cobalt*, and *On Magnetic Distribution*; and Mr. A. Malloch explained a method he had found accurate and convenient for producing a sharp meridian shadow.

On the whole, the physical papers read before the Section were not equal to the average of recent years, either in number or importance; but, as a compensation, the number of mathematical papers was unprecedented, and the Bristol meeting will be remembered both on this account and for the numerous attendance of mathematicians. On the Saturday, which has by custom long been set apart for mathematics, no less than twenty-four papers (including the three reports noticed in another column) on pure mathematics were read. Prof. Cayley explained the theory of the analytical functions which he had termed factions. Sir W. Thomson had three papers all relating to the mathematical treatment of the differential equations that occur in Laplace's theory of the tides. Prof. H. J. S. Smith explained the effect of the quadric transformation on the singular points of a curve, showing how singularities lying upon one side of the triangle of reference became transformed into singularities of a higher order at the opposite angle; and in another paper of great interest he pointed out the connection between continued fractions and points in a line (for example, between $\frac{24}{7}$ expressed as a continued fraction, and the order in which the points of section occur if a given line be divided into twenty-four and also into seven parts). Prof. Smith also spoke on the subject of singular solutions. Prof. Clifford's communications related to the theory of linear transformations, and one contained a graphical representation of invariants. Mr. J. W. L. Glaisher gave some theorems on the nth roots of unity, and explained a formula of verification in partitions, which was founded on and is complementary to one communicated by Sylvester to the Edinburgh meeting in 1871, viz., that

$$\Sigma(1 - x + xy - xyz + \ldots) = 0,$$

while the theorem in the paper was that

$$\Sigma(1 + x + xy + xyz + \ldots) = \Sigma 2^r,$$

r being the number of different elements employed in any partition. Mr. H. M. Jeffery's papers related to cubic spherical curves with triple cyclic arcs and triple foci, and to the shadows of plane curves on spheres. Mr. H. M. Taylor's paper contained a contribution to the mathematics of the chess-board, and his process enabled him to determine by a mathematical procedure the relative values of the pieces at chess probably as accurately as they admit of being found. Prof. R. S. Ball's communication related to a screw-complex of the second order, and Prof. Everett spoke on motors. Prof. Paul Mansion, of Ghent, had sent two papers, one containing an elementary solution of Huyghens's problem on the impact of elastic balls, and the other relating to singular solutions. Mr. W. Hayden contributed some geometrical theorems.

SECTION C—Geology

After the President's address, a lengthy and elaborate paper on the Northern End of the Bristol Coalfield was read by Messrs. Handel Cossham, E. Wethered, and Walter Saise. The paper was illustrated by many maps and sections. This was followed by a paper by Mr. J. M'Murtrie on mountain limestone lying in isolated patches at Luckington and Vobster. The singularity of this case will be realised when it is mentioned that the mountain limestone lies above the coal-measures, which, when originally deposited, overlaid the limestone. The Geological Survey examined the ground many years ago, and came, not unnaturally, to the conclusion that the limestone areas were bounded on all sides by faults. Mr. M'Murtrie has been able to show that the coal-measures are continued without disturbance beneath the limestone. The whole thing is inverted, and much interesting talk arose as to the possible movements which could have produced so great a displacement. Mr. Moore, of Bath, followed with an account of the deposits of Durdham Down yielding *Thecodontosaurus*. The age of the deposit in which this most remarkable Dinosaurian occurs was discussed at some length, but no definite result was arrived at, and the discussion was deferred till Monday.

Mr. Stoddart described an auriferous limestone found at Walton. The metal was distributed through the mass in extremely minute quantity, and the difficulty of obtaining recognisable samples was very great.

Prof. Hughes's paper, *On the Classification of the Sedimentary Rocks*, began by pointing out that the great divisions are not now drawn where the greatest breaks, all evidence considered, occur in nature. The sequence may be shortly given in these terms. Laurentian—Gap—Labrador Series—Gap [? Huronian—Gap] — Cambrian (from red conglomerates of St. David's up to base of May Hill Sandstone)—Gap—Silurian (from May Hill Sandstone = Upper and Lower Llandovery, to top of Red Marls of Sawdde and Horeb Chapel)—Gap—Carboniferous (from bottom of Devonian and Upper Old Red to top of Upper Coal Measures)—Gap—Jurassic (from bottom of breccia and conglomerates of so-called Permian and New Red to top of fluviatile and estuarine deposits of Weald.) The author deferred the full consideration of the rocks above this horizon to a future time, merely commenting on some of the points which seemed to him more especially to call for change.

In supporting this classification he criticised the division of the May Hill Sandstone into Upper and Lower Llandovery, and commented severely upon the re-naming of these beds, which had been previously correctly described by Prof. Sedgwick under the title May Hill Sandstone. He went into the Cambrian and Silurian controversy at some length, and pointed out that not only was Sedgwick's classification found to be the best in the present state of our knowledge, but that Murchison's had not correctly placed any one of the beds about which he came in collision with Sedgwick. What Murchison then called Caradoc overlapping Llandeilo at Llandeilo, has turned out to be May Hill Sandstone; what Murchison then called Cambrian underlying Llandeilo Flags, has turned out to be Caradoc resting on them, and part of the Llandeilo has had to be turned the other way up. The Survey corrected this, and it has appeared corrected in Murchison's later works, but he has never allowed that Sedgwick was right and he was wrong in 1839. Prof. Hughes thought it was too bad that some should still claim for Murchison the credit of having correctly placed the Ludlow and Wenlock, Caradoc and Llandeilo, but say nothing of the names having at that time been applied to totally different rocks.

He considered the Devonian and Upper Old Red to have been deposited over a continental area which sunk first on the south: hence the earlier character of the Devonian fauna in the south, and the greater denudation of the pre-Devonian land of the north. The Permian he wished to abolish as a separate formation, as it was a group made up of some stained carboniferous rocks and some of Sedgwick's previously described Magnesian Limestone and New Red. He thought that the continental area on whose submerged surface the New Red was deposited sunk unequally, and that conglomerates, where there was material to furnish them, were formed along the receding shore line, but at different dates as different parts of the land got down below the waves. He challenged anyone to show a section in which a greater break could be seen between the Trias and so-called Permian than several which occur amongst various members of the Upper New Red itself—and commented upon the unsatisfactory character of the palæontological evidence and of the stratigraphical evidence derived from tracing lines through a district where the rock was seldom seen.

Prof. Hull commented upon the difficulty of introducing any material changes in a nomenclature now so widely accepted. Prof. Harkness stated that he was in favour of adopting the classification of Silurian rocks given in Lyell's "Student's Manual." In reply, Prof. Hughes maintained his original claims with much humour and energy.

Prof. Hébert's very interesting communication on Undulations in the chalk of the North of France had special reference to the strata likely to be encountered in the drift-way of the Channel Tunnel. The Professor considered that observations of dips established the existence of two series of folds, one transverse to the other, which by their intersection produce bosses, or geological hills. The lower rocks, and notably the Greensand, may thus come to the surface in the Channel, and admit the sea-water through their porous substance. Sir John Hawkshaw was present, and combated the geological difficulties with great success. A course of no fewer than five hundred borings, made by a plunger from the side of a vessel, had satisfied him of the substantial accuracy of the geological map of the Straits constructed from shore observations, and the information yielded by these borings was in his opinion adequate to prove that the tunnel will run through Lower Chalk in its whole extent. A small irregularity, bringing in some less compact rock, may be successfully and easily encountered by the engineer. In answer to a suggestion that the shallow holes made by the plunger might be deceptive, owing to a superficial detritus along the floor of the Straits, Sir John Hawkshaw explained that the strong wash of the Channel produced a perfectly clean floor. All along the Straits the instrument had brought up chalk where chalk was expected, and gault where gault was expected, and these formations had a perfectly definite boundary upon the floor of the sea.

A paper by Mr. Sanders described some large bones from the Rhætic beds of Aust Cliff. The dimensions of these fragments are so great as to suggest a large Dinosaurian, but the absence of any medullary cavity seems to imply that the body was habitually submerged. The articular ends, which might be expected to yield useful characters, are not preserved. A communication from Mr. Brodie opened the question of the extent and classification of the Rhætic beds. The interesting discovery of these deposits at Leicester formed the chief and most novel feature of the discussion. Confident statement was on the whole more conspicuous than matured reasoning in this part of the proceedings of the Section, and much evidently remains to be done to elucidate the palæontological and physical relations of the deposits in question. For the moment the preponderance, at least of authority, rests with those who affirm the universal spread of a Rhætic age, and look in every quarter of the globe for a bone-bed with Ceratodus and an *Avicula-contorta* zone.

A large audience assembled to hear Dr. Carpenter's paper on the red clay found by the *Challenger*. The substance of his remarks has already appeared in the Proceedings of the Royal Society.

The greater part of Tuesday's sitting was occupied by papers and discussion upon the Glacial Period. By this time the easily observable glacial phenomena have been co-ordinated, and there is not quite so much room as formerly for supposition and unconnected facts. The discussion elicited a few curious points, and was interesting, if not particularly instructive. Most readers of such modern summaries as are given in Lyell's "Principles" or Geikie's "Ice Age" would demur to the too sweeping language in which the Chairman summed up the argument. Dr. Wright's opinion that no man living knows anything of the Glacial Period may possibly be just, but it is not sufficiently incontestable to be enunciated *ex cathedrâ*. The most novel points of Dr. Carpenter's communication upon the "Sea Bottom of the North Pacific" were the low temperature of the water at great depths, and the supposed existence of coral reefs, drowned by too rapid submergence, upon all the submarine summits. The species are believed to be recent, and the submergence comparatively modern. Some notice was taken of the results obtained by the *Valorous*, and of Mr. Gwyn Jeffreys' view that the Arctic shells of the Sicilian Tertiaries were derived from polar areas by migration through a marine gap not far distant from the present canal of Languedoc. Mr. Thomson's views as to some new genera of fossil corals, which met heavy criticism at the Geological Society, were brought up once more here, but gained no support of consequence. The method of investigation is curious, but it has hitherto proved somewhat barren of results.

Among other good papers may be cited Prof. A. H. Green's account of the Millstone Grit of North Derbyshire and South Yorkshire. This was a highly-condensed statement of the stratigraphical relations of an extensive group of very interesting rocks. The variations in thickness of the different grits were referred to inequalities of the old sea-floor upon which they were accumulated, hollows permitting a greater thickness to form. Had discussion been allowed, it would have been interesting to notice the remarks thrown out by those classifiers of strata who regard the formation of every rock as a definite and almost universal event in the earth's history. Rarely has a better example been given than this of the local conditions, often quite trivial in themselves, which regulate the extent, divisions, and thickness, as well as the mineral and fossil characters of a large formation.

SECTION D.

Biology.

Opening Address by Dr. P. L. Sclater, M.A., F.R.S., F.L.S., President.*

V.—NEOTROPICAL REGION.

The Neotropical Region is, I suppose, on the whole the richest in animal life of any of the principal divisions of the earth's surface. Much work has been done in it as regards every branch of zoology of late years, and I must confine myself to noticing the most recent and most important of the contributions to this branch of knowledge.

I believe the following † to be altogether the most natural sub-divisions of the Neotropical Region, which are nearly as they are set forth in Hr. v. Pelzeln's "Ornithology of Brazil."

1. *Central American Sub-region*, from Southern Mexico to Panama.
2. *Andean* or *Columbian Sub-region*, from Trinidad and Venezuela, along the chain of the Andes, through Columbia, Equador, and Peru, down to Bolivia.
3. *Amazonian Sub-region*, embracing the whole watershed of the Orinoco and Amazons up to the hills, and including also the highlands of Guiana.
4. *The South Brazilian Sub-region*, containing the wood-region of S.E. Brazil and Paraguay and adjoining districts.
5. *The Patagonian Sub-region*, containing Chili, La Plata, Patagonia, and the Falklands.

Besides these we have:—

6. *The Galapagos*, which, whether or not they can be assigned to any other sub-region, must be spoken of separately.

I. The Central American Sub-region

was, up to twenty years ago, very little known, but has recently been explored in nearly every part, and is perhaps now more *nearly* worked out than any other of the above-mentioned sub-regions. There is as yet no complete work on the zoology of any portion of it, and the discoveries of Sallé, Boucard, de Saussure, and Sumichrast in Mexico, of Salvin in Guatemala, of v. Frantzius and Hoffman in Costa Rica, of Bridges and Arcé and Veragua, and of McLeannan in Panama, together with those of numerous other collectors, are spread abroad among the scientific periodicals of Europe and America. Even of Mexican zoology, long as it has been worked, we have no general account. To mention all these memoirs in detail would be impossible within the limits of this address; but I will say a few words about the more important of them that have lately appeared.

* Continued from p. 382.

† A general sketch of the Mammal-life of this region is given in my article on the Mammals of South America in the Quar. Journ. of Science for 1865, and a Summary of the Birds in Sclater and Salvin's "Nomenclator Avium Neotropicalium."

The French are now publishing a work on the results of their scientific expedition to Mexico during the short-lived Empire. Three parts on the Reptiles by Duméril and Bocourt were issued in 1870, and a part on the Fishes, by L. Vaillant, has recently appeared.

A paper on the Mammals of Costa Rica has lately been published by v. Frantzius in Wiegmann's Archiv. Unfortunately, it seems to have been drawn up mainly from notes without reference to the specimens in the Berlin Museum, but nevertheless contains much that is useful and of interest.

Dr. Günther's admirable memoir of the fishes of Central America, published in the Zoological Society's "Transactions" in 1869, is based upon the collections made by Capt. Dow in various parts of the coast, and by Messrs. Salvin and Godman in the freshwater lakes of the highlands of Guatemala and in other localities.

Its value in relation to our general knowledge of the fishes of this portion of America, heretofore so imperfectly known, can hardly be over-estimated. As regards the birds of Central America, it is much to be regretted that we have at present no one authority to refer to. The collection of Messrs. Salvin and Godman embraces very large series from different parts of this region, and together with those of my own collection, wherein are the types of the species described in my own papers, would afford abundant materials for such a task. Mr. Salvin and I have often formed plans for a joint work on this subject, and I trust we may before long see our way to its accomplishment. A similar memoir on the Mammals of Central America is likewise of pressing necessity for the better understanding of the Neotropical Mammal Fauna. There are considerable materials available for this purpose in the collections of Salvin and Arcé in the British Museum, and I trust that some naturalist may shortly be induced to take up this subject.

2. The Andean or Columbian Sub-region.

Of this extensive sub-region, which traverses six or seven different States, there is likewise no one zoological account; but I may mention some of the principal works lately issued that bear upon the subject. Léotaud's "Birds of Trinidad" gives us an account of the ornithology of that island, which forms a kind of appendage to this sub-region, and Dr. Finsch has more recently published a supplementary notice of them. Of Venezuela, Columbia, and Ecuador there are only scattered memoirs in various periodicals on the numerous collections that have of late years been made in those countries to be referred to. Several excellent collectors are now, or lately have been, resident in these republics, Herr Georing and Mr. Spence in Venezuela, Mr. Salmon in Antioquia, Professor Jameson and Mr. Fraser in Ecuador, whose labours have vastly added to our knowledge of the zoology of these districts. When we come to Peru, we have Tschudi's "Fauna Péruana" to refer to, which, though unsatisfactory in execution, contains much of value. How far from being exhausted is the rich fauna of the Peruvian Andes, is sufficiently manifest from the wonderful discoveries lately made by Jelski in the district east of Lima, which was in fact that principally investigated by Tschudi. Of these, M. Taczanowski has lately given an account as regards the birds in the Zoological Society's "Proceedings"; and Dr. Peters has published several notices of the more remarkable Mammals and Reptiles.

Further south, in Bolivia, our leading authority is still the zoological portion of D'Orbigny's "Voyage dans l'Amérique Méridionale." This rich and most interesting district has, it is true, been visited by several collectors since D'Orbigny's time; but the results of their journeys have never been published in a connected form, though many of their novelties have been described. Bolivia, I do not doubt, still contains many new and extraordinary creatures hid in the recesses of its mountain valleys; and there is no part of South America which I should sooner suggest as a promising locality for the zoological collector.

3. The Amazonian Sub-region.

On Guiana, where the Amazonian fauna seems to have had its origin, we have a standard work in Schomburgk's "Reise," the third volume of which, containing the Fauna, was drawn up by the Naturalist of the Berlin Museum. For the valley of the Amazons itself, the volumes of Spix and Martius, though not very accurate, and rather out of date, must still be referred to, as likewise the zoology of Castelnau's "Expédition dans l'Amérique du Sud," for the natural history of the Peruvian confluents. As regards the birds, however, we have several more recent authorities. In 1873 Mr. Salvin and I published in the Zoological Society's "Proceedings" a *résumé* of the papers treating of Mr. E. Bartlett's and Mr. John Hauxwell's rich ornithological collections on the Huallaga, Ucayali, and other localities in Eastern Peru. Subsequently we communicated to the same Society an account of Mr. E. L. Layard's collection of birds made near Para, and took occasion to deduce therefrom some general ideas as to the relations of the Avifauna of the Lower Amazons.

As regards the two lower great confluents of the Amazons, Rio Madeira on the right bank, and the Rio Negro on the left bank of the mighty river, our knowledge of their avifaunas is mainly due to the researches of Johann Natterer—one of the most successful and energetic zoological collectors that ever lived—of whose discoveries in ornithology a complete account has lately been first published by Mr. A. v. Pelzeln, of Vienna. It is much to be wished that a similar *résumé* of Natterer's discoveries and collections of Mammals, in which order his investigations were of hardly less importance, should be given to the world; and I trust Herr v. Pelzeln will forgive me if I press this subject on his attention.

The fishes of the Amazons and its confluents are many and various, and fully deserve a special monograph. The late Professor Agassiz made his well-known expedition up the Amazons in 1865 with the particular view of studying its fishes, and amassed enormous collections of specimens for the purpose.* Whether (as other naturalists have hinted) Professor Agassiz's estimate of the number of new and undescribed species contained in their collection was exaggerated or not is at present uncertain, as the specimens unfortunately lie unstudied in the Museum of Comparative Zoology at Cambridge, Mass. It is a thousand pities this state of things should continue; and I venture to suggest to the great Professor's numerous friends and admirers in the U. S. that no more appropriate tribute to his memory could be raised than the publication of a monograph of Amazonian fishes based on their collections.

4. The South-Brazilian Sub-region.

This sub-region, which embraces the wood region of S.E. Brazil and adjoining districts, and contains in nearly every branch of zoology a set of species and genera allied to but separable from those of the Amazonian Sub-region, has been much frequented by European naturalists. Its productions are consequently tolerably well known, though there is even here still very much to be done. Burmeister's "Systematische Ubersicht" and "Erläuterungen" may be referred to for information on its Mammals and Birds; likewise Prince Max. of New Wied, "Beiträge," which, although of old standing in point of date, is still of great value. The late Dr. Otto Wucherer, a German physician resident at Bahia, paid much attention to the Reptiles of that district, and has written an account of its Ophidians which will be found in the Zoological Society's "Proceedings."

Hr. Hensel has also recently published in Wiegman's "Archiv" a valuable memoir on Mammals collected in South Brazil, which should be referred to. Prof. Reinhardt has recently completed an excellent account of the avifauna of the Campos of Brazil, based on his own collections and those of Dr. D. W. Lund; and Hr. v. Berlepsch has treated of the birds of Santa Catharina. These are all three most useful contributions to our knowledge of this sub-region. But it is melancholy to think that although a (*soi-disant*) highly civilised European race has resided in the Brazilian Empire so long, and has introduced railways, steamboats, and many other of the appliances of modern Europe, there has never, so far as I know, been produced by them any one single memoir worthy of mention on the teeming variety of zoological life that everywhere surrounds them.

For information on the animals of Paraguay we must still refer to the writings of Don Felix d'Azara, and to Dr. Hartlaub's reduction of his Spanish terms to scientific nomenclature. But modern information about this part of the South-Brazilian Sub-region would be very desirable.

5. The Patagonian Sub-region.

For the zoology of the Argentine Republic, which forms the northern portion of this sub-region, the best work of reference is the second volume of Dr. Burmeister's "La-Plata Reise," which contains a synopsis of the Vertebrates of the Republic. Dr. Burmeister, who is now resident at Buenos Ayres as director

* See "Travels in Brazil," by Prof. and Mrs. Louis Agassiz, Boston, 868.

of the public museum of that city, has lately devoted himself to the study of the extinct Mammal-fauna, and specially to that of the Glyptodont Armadillos, of which he has lately completed a splendidly illustrated monograph. He has likewise been the chief adviser of the Government in their plans for recognising the University of Cordova, which will ultimately no doubt do much for the cause of natural science in the Argentine Republic. Mr. W. H. Hudson, of Buenos Ayres, has long studied the birds and other animals of that country, and deserves honourable mention in a country where so few of the native-born citizens pursue science. His bird-collections have been worked out by Mr. Salvin and myself, and Mr. Hudson has likewise published a series of interesting notices on the habits of the species.

The "Zoology of the Voyage of the *Beagle*" contains much information concerning the animals of La Plata, Patagonia, and Chili. The "Mammals" by Waterhouse, the "Birds" by Gould and G. R. Gray, the "Fishes" by Jenyns, and the "Reptiles" by Bell, illustrated with notes and observations of Mr. Darwin, will ever remain among the leading authorities on the animals of this part of America. On the Rio Negro of Patagonia, where Mr. Darwin made considerable collections, we have a more recent authority in Mr. W. H. Hudson, whose series of birds from this district was examined by myself in 1872.

Dr. R. O. Cunningham has recently followed on the footsteps of Mr. Darwin in Patagonia, and besides his journal of travels has published notes on the animals met with, in the Linnean Society's Transactions. Mr. Salvin and I have given an account of his ornithological collections in several papers in the "Ibis."

As regards the Falkland Islands, two excellent collectors and observers have of late years been stationed there, and have provided the means of our becoming well acquainted with the native birds. Capt. Packe's collections have been examined by Mr. Gould and myself, and Capt. Abbott's by myself in a paper to which he has added many valuable notes.

Lastly, as regards Chili, we have Gay's somewhat pretentious "Fauna Chileña," forming the zoological portion of his "Historia Fisica y Politica de Chile." The volume on the Mammals and Birds was compiled at Paris by Desmurs, and that on the Reptiles and Fishes by Guichenot, but they are not very reliable. The naturalists of the National Museum of Santiago, Philippi and Landbeck, have of late years published in Wiegman's "Archiv" many memoirs on the zoology of the Chilian Republic, of which I have given a list in a paper on the Birds of Chili in the Zoological Society's "Proceedings" for 1867. More recently Messrs. Philippi and Landbeck have published a catalogue of Chilian birds in the "Anales de la Universidad de Chile." But Mr. E. C. Reed, F.Z.S., who is likewise attached to the museum of Santiago, writes me word that he is now engaged in preparing for publication a complete revision of the Vertebrates of the Republic, which will no doubt give us still better information on this subject.

6. Galapagos.

Until recently our knowledge of the very singular fauna of the Galapagos was mainy based upon Mr. Darwin's researches, as published in the "Zoology of the *Beagle*," above referred to. Recently, however, Mr. Salvin and I have described some new species of birds from these islands from Dr. Habel's collection, and Prof. Sundevall has published an account of the birds collected there during the voyage of the Swedish frigate *Eugénie* in 1852. Mr. Salvin has likewise prepared and read before the Zoological Society a complete memoir on the Ornithology of the Galapagoan Archipelago, which will shortly be printed in the Society's "Transactions." Much interest has likewise been recently manifested concerning the gigantic Tortoises of the Galapagos, which, Dr. Günther has reason to believe, belong to several species each restricted to a separate island.* Indeed, I am much pleased to hear that the Lords of the Admiralty, incited by Dr. Günther's requests, have despatched H.M.S. *Tenedos* for the Pacific squadron at Panama to the Galapagos, for the express purpose of capturing and bringing to England specimens of the tortoises of each of the islands. We may, therefore, hope to be shortly more accurately informed upon this most interesting subject.

Va. The Antillean Sub-region.

The study of the fauna of the West India Islands presents problems to us of the greatest interest: first, on account of the relics of an ancient and primitive fauna which are found there, as indicated by the presence of such types as *Solenodon*, *Dulus*, and *Starnœnas*; and, secondly, from the many instances of representative species replacing each other in the different islands. Much, it is true, has been done towards the working out of Antillean Faunas of late years, but much more remains to be done; and it is indeed scandalous that there should be many islands under the British rule, of the zoology of which we are altogether unacquainted. The greater activity of our botanical fellow-labourers has supplied us with a handy volume of the Botany of these islands;* and it is by no means creditable to the zoologists to remain so far behind in this as in other cases already alluded to. Within the compass of the present address it would not be possible for me to enumerate all our authorities upon Antillean zoology, but I will mention some of the principal works of reference under the following heads:—

1. *The Bahamas.*	3. *Jamaica.*	5. *Porto Rico.*
2. *Cuba.*	4. *Haiti.*	6. *The Lesser Antilles.*

1. *The Bahamas.*

The late Dr. Bryant has published in the *Boston Journal of Natural History* several articles upon the birds of the Bahamas, where he passed more than one winter. These islands, however, merit much more minute investigation than has as yet been bestowed upon them.

2. *Cuba.*

Ramon de la Sagra's "Historia Fisica y Politica de Cuba" and Lenbeye's "Aves de la Isla de Cuba," were up to a recent period our chief authorities upon Cuban zoology. But Cuba has long had the advantage of the residence within it of an excellent naturalist—Don Juan Gundlach—who has laboured hard towards the more complete investigation of its remarkable zoology. We are indebted to him for collecting the specimens upon which Dr. Cabanis based his revision of Cuban ornithology, published in Wiegmann's "Archiv," as also for a tabular list of Cuban birds, published in the same journal for 1861, and for several supplements thereto, for the more recent reviews of the mammals and birds of the island, published in the first volume of Poey's "Repertorio," and for many other contributions to the natural history of Cuba. This last-named work, as also the previous "Memorias sobre la historia natural de la Isla de Cuba" of the same author, contains a number of valuable contributions to our knowledge of the rich fauna of this island, and should be carefully studied by those who are anxious to become acquainted with the peculiarities of the Cuban fauna.

3. *Jamaica.*

Mr. Gosse's meritorious work on the Birds of Jamaica, and his "Naturalists' Rambles," are still the main source of our information on the fine island of Jamaica, and very little has been done since his time. A young English naturalist, Mr. W. Osburn, made some good collections in Jamaica in 1860, of which the Mammals were worked out by Mr. Tomes and the Birds by myself. Mr. W. T. March has also more recently sent good series of the birds of the island to America, and Prof. Baird has edited his excellent notes on them. I must not lose the opportunity of calling special attention to the Seals of the Antilles (*Monachus tropicalis* and *Cystophora antillarum* of Gray), of which, so far as I know, the only specimens existing are the imperfect remains in the British Museum brought home by Mr. Gosse. More knowledge about these animals (if there be really two of them) would be very desirable.

4. *Haiti.*

Of this large island very little more is known as regards its zoology than was the case in the days of Buffon and Vieillot. Of its birds alone we have a recent account in a paper which I wrote upon M. Sallé's collection, and in a more recent memoir drawn up by the late Dr. Bryant, and published in the "Proceedings" of the Boston Society of Natural History for 1863.

5. *Porto Rico.*

Nearly the same story holds good of this Spanish island, of which our only recent news relates to the birds, and consists of two papers—one by Mr. E. C. Taylor in the "Ibis," and the other by the late Dr. Bryant, in the journal above mentioned.

6. *The Lesser Antilles.*

As I remarked above, every one of the numerous islands, from Porto Rico down to Trinidad, requires thorough examination.

* See Nature, vol. xii. p 238 (1875).

* Griesbach's "Flora of the West Indies."

It is remarkable that no one has yet been found to attack this interesting subject, which might easily be performed by excursions during the winter months of a few succeeding years.

As regards the ornithology of these islands, the subjoined summary of what we really know and do not know is mainly taken from a paper on the Birds of St. Lucia, which I read before the Zoological Society of London in 1871.

1. *The Virgin Islands.*—Of these islands we may, I think, assume that we have a fair acquaintance with the birds of St. Thomas, the most frequently visited of the group, and the halting place of the West Indian mail steamers. Mr. Riise, who was long resident here, collected and forwarded to Europe many specimens, some of which were described by myself,* and others are spoken of by Prof. Newton in a letter published in the "Ibis" for 1860, p. 307. Mr. Riise's series of skins is now, I believe, at Copenhagen. Frequent allusions to the birds of St. Thomas are also made by Messrs. Newton in their memoir of the birds of St. Croix, mentioned below. In the "Proceedings" of the Academy of Natural Sciences of Philadelphia for 1860, Mr. Cassin has given an account of a collection of birds made in St. Thomas by Mr. Robert Swift, and presented to the Academy; twenty-seven species are enumerated.

Quite at the extreme east of the Virgin Islands, and lying between them and the St. Bartholomew group, is the little islet of Sombrero, "a naked rock about seven-eighths of a mile long, twenty to forty feet above the level of the sea, and from a few rods to about one-third of a mile in width." Although "there is no vegetation whatever in the island over two feet high," and it would seem a most unlikely place for birds, Mr. A. A. Julien, a correspondent of Mr. Lawrence of New York, succeeded in collecting on it specimens of no less than thirty-five species, the names of which, together with Mr. Julien's notes thereupon, are recorded by Mr. Lawrence in the eighth volume of the "Annals of the Lyceum of Natural History of New York."

The remaining islands of the Virgin group are, I believe, most strictly entitled to their name so far as ornithology is concerned, for no collector on record has ever polluted their virgin soil. Prof. Newton ("Ibis," 1860, p. 307) just alludes to some birds from St. John in the possession of Mr. Riise.

2. *St. Croix.*—On the birds of this island we have an excellent article by Messrs. A. and E. Newton, published in the first volume of the "Ibis."† This memoir, being founded on the collections and personal observations of the distinguished authors themselves, and having been worked up after a careful examination of their specimens in England, and with minute attention to preceding authorities, forms by far the most complete account we possess of the ornithology of any one of the Lesser Antilles. It, however, of course requires to be supplemented by additional observations, many points having been necessarily left undetermined; and it is much to be regretted that no one seems to have since paid the slightest attention to the subject.

3. *Anguilla, St. Martin, and St. Bartholomew.*—Of this group of islands St. Bartholomew alone has, as far as I know, been explored ornithologically, and that within a very recent period. In the Royal Swedish Academy's "Proceedings" for 1869 will be found an excellent article by the veteran ornithologist Prof. Sundevall, on the birds of this island, founded on a collection made by Dr. A. Von Göes. The species enumerated are forty-seven in number.

4. *Barbuda.*—Of this British island I believe I am correct in saying that nothing whatever is known of its ornithology, or of any other branch of its natural history.

5. *St. Christopher and Nevis,* to which may be added the adjacent smaller islands *St. Eustathius and Saba.*—Of these islands also our ornithological knowledge is of the most fragmentary description. Mr. T. J. Cottle was, I believe, formerly resident in Nevis, and sent a few birds thence to the British Museum in 1839. Amongst these were the specimens of the Humming-birds of that island, which are mentioned by Mr. Gould in his well-known work. Of the remainder of this group of islands we know absolutely nothing.

6. *Antigua.*—Of this fine British island, I regret to say, nothing whatever is known as regards its ornithology. Amongst the many thousands of American birds that have come under my notice during the past twenty years, I have never seen a single skin from Antigua.

* Ann. N.H. ser. 3, vol. iv. p. 225; and P.Z.S. 1860, p. 314.
† "Ibis," 1859, pp. 59, 138, 252, and 365.

7. *Montserrat.*—Exactly the same as the foregoing is the case with the British island of Montserrat.

8. *Guadeloupe, Deseadea, and Marie-galante.*—An excellent French naturalist, Dr. l'Herminier, was for many years resident as physician in the island of Guadeloupe. Unfortunately, Dr. l'Herminier never carried into execution the plan which I believe he contemplated, of publishing an account of the birds of that island. He sent, however, a certain number of specimens to Paris and to the late Baron de la Fresnaye, to whom we are indebted for the only article ever published on the birds of Guadeloupe or of the adjacent islands.

9. *Dominica.*—Dominica is one of the few of the Caribbean islands that has had the advantage of a visit from an active English ornithologist. Although Mr. C. E. Taylor only passed a fortnight in this island in 1863, and had many other matters to attend to, he nevertheless contrived to preserve specimens of many birds of very great interest, of which he has given us an account in one of his articles on the birds of the West Indies, published in the "Ibis" for 1864. It cannot be supposed, however, that the birds of this wild and beautiful island can have been exhausted in so short a space of time, even by the energetic efforts of our well-known fellow-labourer.

10. *Martinique.*—This island is one of the few belonging to the Lesser Antilles in which birdskins are occasionally collected by the residents, and find their way into the hands of the Parisian dealers. There are also a certain number of specimens from Martinique in the Musée d'Histoire Naturelle in the Jardin des Plantes, which I have had an opportunity of examining; but, beyond the vague notices given by Vieillot in his "Oiseaux de l'Amérique du Nord," I am not aware of any publications relating specially to the ornithology of this island. Mr. E. C. Taylor passed a fortnight in Martinique in 1863, and has recorded his notes upon the species of birds which he met with in the excellent article which I have mentioned above; but these were only few in number. The International Exhibition in 1862 contained, in the department devoted to the products of the French colonies, a small series of the birds of Martinique, exhibited by M. Bélanger, director of the Botanical Garden of St. Pierre in that island.* This is all the published information I have been able to find concerning the birds of Martinique.†

11. *St. Lucia.*—Of this island I gave an account of what is known of the birds in a paper published in the Zoological Society's "Proceedings" for 1871, based upon a collection kindly forwarded to me by the Rev. J. E. Semper. Mr. Semper subsequently communicated some interesting notes on the habits of the species.

12. *St. Vincent.*—St. Vincent was formerly the residence of an energetic and most observant naturalist, the Rev. Lansdowne Guilding, F.L.S., well known to the first founders of the Zoological Society of London, who, however, unfortunately died at an early age in this island without having carried out his plans for a fauna of the West Indies.

Mr. Guilding paid most attention to the invertebrate animals; but his collections contained a certain number of birds, amongst which was a new Parrot, described after his decease by Mr. Vigors as *Psittacus Guildingii,* and probably a native of St. Vincent.

13. *Grenada and the Grenadines.*—Of the special ornithology of this group nothing is known.

14. *Barbados.*—The sole authority upon the birds of Barbados is Sir R. Schomburgk's well-known work on that island. This contains (p. 681) a list of the birds met with, accompanied by some few remarks. It does not, however, appear that birds attracted much of the author's attention; and more copious notes would be highly desirable.

15. *Tobago,* I believe, belongs zoologically to Trinidad. Sir W. Jardine has given us an account of its ornithology from Mr. Kirk's collections.

VI.—THE AUSTRALIAN REGION.

Of the Australian Region I will speak in the following subdivisions:—

1. *Australia and Tasmania.*
2. *Papua and the Papuan Islands.*
3. *The Solomon Islands.*

* See an article on Ornithology in the International Exhibition, "Ibis," 1862, p. 288.
† On animals formerly living in Martinique but now extinct, see Guyon, "Comp. Rend." lxiii. p. 589 (1866).

That we know more of the fauna of Australia than of other English colonies in different parts of the world is certain, but no thanks are due from us for this knowledge either to the Imperial or to any of the Colonial Governments. The unassisted enterprise of a private individual has produced the two splendid works upon the Mammals and Birds of Australia, which we all turn to with pleasure whenever reference is required to a member of these two classes of Australian animals. Mr. Gould's "Mammals of Australia" was completed in 1863. Since that period the little additional information received respecting the terrestrial Mammals of Australia has been chiefly furnished by Mr. Krefft, of the Australian Museum, Sydney, in various papers and memoirs. Mr. Krefft has also written the letterpress to some large illustrations of the "Mammals of Australia," by Miss H. Scott and Mrs. H. Forde, in which a short account of all the described species is given. On the Marine Mammals, however, which were scarcely touched upon by Mr. Gould, we have a treatise by Mr. A. W. Scott published at Sydney in 1873, which contains a good deal of useful information concerning the seals and whales of the Southern Hemisphere.

The magnificent series of seven volumes of Mr. Gould's "Birds of Australia" was finished in 1848. In 1869 a supplementary volume was issued, containing similar full-sized illustrations of about 80 species. In 1863 Mr. Gould reprinted in a quarto form, with additions and corrections, the letterpress of his great work, and published it under the title of a "Handbook to the Birds of Australia." This makes a convenient work for general reference. Of two colonial attempts to rival Mr. Gould's series I cannot speak with much praise. Neither Mr. Diggle's "Ornithology of Australia" nor Mr. Halley's proposed "Monograph of the Australian Parrots" are far advanced towards conclusion—indeed, of the last-mentioned work I have seen but one number.

Several large collections of birds have been made in the peninsula of Cape York and adjoining districts of Northern Queensland of late years, and it is a misfortune for science that we have had no complete account of them. One of the largest of these, however, made by Mr. J. T. Cockerell, has luckily fallen into the hands of Messrs. Salvin and Godman, and will, I trust, be turned to better uses than the filling of glass cases and the ornamentation of ladies' hats.

It seems to me that there is still much to be done even in birds in Northern Australia, and I cannot help thinking that Port Darwin, the northern extremity of the trans-continental telegraph, would be an excellent station for a collecting naturalist, and one where many novelties, both zoological and botanical, would certainly be met with.

On the Snakes of Australia we have an excellent work published in 1869 by Mr. Gerard Krefft—one of the few really working Australian naturalists, who, it appears, is not appreciated in Sydney as he fully deserves to be. Mr. Krefft, during his long residence in Sydney, has become well acquainted with the Ophidians of the colony and has devoted special attention to them, so that he has the advantage of practical as well as scientific acquaintance with his subject. The late Dr. Gray has written many papers on the Tortoises and Lizards of Australia. Of the latter we have to thank Dr. Günther for a complete monographic list just published in one of the newly issued numbers of the "Voyage of the *Erebus* and *Terror*." Most of the plates of this work were also issued in 1867 by Dr. Gray in his "Fasciculus of the Lizards of Australia and New Zealand."

For information on the fishes of Australia reference must be made to the ichthyological portion of the "Zoology of the *Erebus* and *Terror*," by Sir John Richardson, and to the same author's numerous papers on Australian fishes in the "Annals of Nat. Hist." and "Transanctions" and "Proceedings" of the Zoological Society of London. The Count F. de Castelnau, who seems to be almost the only working ichthyologist in Australia, has recently published in the "Proceedings of the Zoological and Acclimatisation Society of Victoria," several papers on the fishes of the Melbourne fish-market and of other parts of Australia, which include a complete synopsis of the known Australian species.

2. Papua and its Islands.

I believe that my paper upon the Mammals and Birds of New Guinea, published by the Linnean Society in 1858, was the first attempt to put together the scattered fragments of our knowledge of this subject. In 1859 a British Museum Catalogue by Dr. J. E. and Mr. G. R. Gray, gave *r ésumé* of the then known members of the same two classes belonging to New Guinea and the Aru Islands, and included notices of all Mr. Wallace's discoveries. In 1862 Mr. Wallace gave descriptions of the new species discovered subsequently to his return by his assistant, Mr. Allen. In 1863 Dr. Finsch published at Bremen an excellent little essay called "Neu-Guinea und seine Bewohner," in which is given a complete account of our then state of knowledge of the subject. But within these last ten years still more serious efforts have been made by naturalists of several nations to penetrate this *terra incognita*. Two emissaries of the Leyden Museum—Bernstein and V. Rosenberg—have sent home full series of zoological spoils to that establishment, and have discovered a host of novelties. Of these the birds have been described by Prof. Schlegel in his "Observations Zoologiques." An intrepid Italian traveller, Signor L. M. d'Albertis, made a still further advance, when in September 1872 he accomplished the first ascent of the Arfak Mountains,* and discovered the splendid Bird of Paradise and other new species which I described in 1873. Quickly following on his footsteps, Dr. A. B. Meyer penetrated still further into the unknown interior, and reaped the abundant harvest of which he has given us an account in six papers lately published at Vienna. Dr. Meyer has now become director of the Museum of Dresden, and is no doubt occupied in the further elaboration of his rich materials. In the meanwhile some accomplished Italian naturalists are engaged on the collections of D'Albertis and his quondam companion Beccari. Count Salvadori, who is at work on the birds, will take the opportunity of preparing a complete account of the ornithology of Papua and its islands, similar to that in Borneo, of which I have already spoken. The Marquis Giacomo Doria has already published one excellent paper on "The Reptiles of Amboina and the Ké Islands," collected by his compatriot Beccari, and is preparing other memoirs on the Mammals and Reptiles of New Guinea and the Aru Islands obtained by D'Albertis.

Dr. Meyer has lately given an account of his herpetological discoveries in New Guinea, which comprehend several new and most interesting forms, in a memoir read before the Academy of Berlin; and Dr. Bleeker some years ago gave a list of the reptiles obtained by V. Rosenberg in that island, and enumerated the Papuan reptiles then known to him.

All these expeditions, however, have been directed towards the western peninsula of New Guinea, which alone is yet in any way explored by naturalists. Of the greater south-eastern portion of the island (unless we are inclined to give credit to Capt. Lawson's wonderful exploits) we have as yet very little information. A cassowary † and a kangaroo, ‡ brought away by the *Basilisk* from the southern coast, both proved to be new to science, as did likewise a Paradise Bird obtained in the same district by Mr. D'Albertis. § This is sufficient to give us an idea of what we may expect to find when the interior of this part of New Guinea is explored. And I may take this opportunity of mentioning that a most active and energetic traveller is perhaps at this very moment at work there. M. L. M. d'Albertis, of whose previous labours I have just spoken, returned to the East last autumn. Letters received from him by his Italian friends in June last state that he had at the time of writing already succeeded in reaching Yule Island near Mously Bay, on the south-east coast of New Guinea, and proposes to establish his head-quarters there for expeditions into the interior.

3. New Ireland, New Britain, and the Solomon Islands.

I devote a few words specially to these islands because they are easy of access from Sydney, and because their productions are of particular interest, belonging, as they do, to the Papuan and not to the Polynesian fauna. I have put together what is known of the birds of the Solomon's group in a paper read before the Zoological Society in 1869. Seeing the interesting results obtained from the examination of one small jar of birds collected by an unscientific person, there can be little doubt of the value of what would be discovered on the more complete investigation of the group. As regards New Ireland and New Britain, we have but scattered notices to refer to. The last-named island is, we know, the home of a peculiar cassowary (*Casuarius bennetti*).

* See Nature, vol. viii. p. 501 (29).
† *Cazuarius picticollis*, Scl., P.Z.S. 1875, p. 85.
‡ *Dorcopsis luctuosa* (D'Albertis), *v.* Garrod, P.Z.S. 1875, p. 48.
§ *Paradisea raggiana*, Sclater, P.Z.S. 1873, p. 559.

A list of the fishes of the Solomon Islands is given by Dr. Günther in Mr. Brenchley's "Cruise of the *Curaçoa*," which I shall allude to presently.

VII.—PACIFIC REGION.

Of this region, where Mammals (except a few bats) are altogether absent, and birds are the predominant form of vertebrate life, I will say a few final words under three heads:—
1. *New Zealand.* 2. *Polynesia.* 3. *The Sandwich Islands.*

1. *New Zealand.*

In New Zealand, of all our Colonies, most attention has lately been devoted to natural history, and several excellent naturalists are labouring hard and well—I need only mention the names of Dr. Hector, Dr. Haast, Capt. F. W. Hutton, and Dr. Buller. The commendable plan of affiliating the various local societies together to one institute has resulted in the production of an excellent scientific journal, already in its sixth volume, which contains a mass of most interesting papers on the fauna and flora of the colony. To refer to these memoirs in detail is quite unnecessary; but it is obvious, on turning over the pages of the volumes of the Transactions of the New Zealand Institute, how great are the exertions now being made to perfect our knowledge of the natural products, both recent and extinct, of our antipodean colony.

Dr. W. L. Buller's beautiful volume on the ornithology of New Zealand, finished in 1873, is likewise a most creditable production both to the author and to those who have supported and promoted his undertaking. Few, indeed, are the colonies that can boast of a similar piece of work!

In 1843 the late Sir John Richardson presented to this association a special report on the Ichthyology of New Zealand; but much advance has, of course, been made since that period.

The lizards of New Zealand have been recently enumerated along with those of Australia in Dr. Günther's memoir above referred to.

2. POLYNESIA.

Great additions have recently been made to our knowledge of the natural productions of the Polynesian Islands by the travellers and naturalists employed by the brothers Godeffroy of Hamburgh. These gentlemen not only have extensive collections *made*, but also trouble themselves to get them properly *worked out*. The excellent volume on the ornithology of the Fiji, Samoa, and Tonga Islands, published in 1867 by Drs. Finsch and Hartlaub, is based entirely upon materials thus obtained, as are likewise the many capital memoirs which fill the parts of the illustrated quarto *Journal der Museum Godeffroy*—a journal replete with information upon the geography, ethnography, and natural history of Polynesia. Amongst these memoirs I must call special attention to Dr. Günther's "Fische der Sudsee," founded upon Mr. Andrew Garrett's splendid collection of fishes and of drawings of them, coloured from life, of which three parts are already issued. We have now almost for the first time the after opportunity of becoming acquainted with the exceeding beauty of the tropical fishes in life.

The late Mr. Julius Brenchley's account of his cruise in H.M.S. *Curaçoa* among the South Sea Islands, and published in 1873, contains an appendix of "Natural History Notices," illustrated by figures of remarkable specimens obtained on the occasion. Of these the part relating to the birds is by the late Mr. G. R. Gray, and those concerning the reptiles and fishes by Dr. Günther.

3. THE SANDWICH ISLANDS.

The Sandwich Islands stand apart zoologically as geographically from the rest of Polynesia, and merit more special attention than has yet been bestowed upon them. Of their birds, which form the most prominent part of their vertebrate fauna, Mr. Dole has given a synopsis in the "Proceedings of the Boston Society of Natural History." In noticing this paper in the "Ibis" for 1871, I have introduced some supplementary remarks upon the general facies of the Avifauna.

CONCLUSION.

In concluding this address, which has extended, I regret to say, to a much greater length than I anticipated when I selected the subject of it, I wish to endeavour to impress upon naturalists the paramount importance of locality.

In the study of distribution more probably than in any other direction, if perhaps we except embryology, will be ultimately found the key to the now much vexed question of the origin of species. The past generation of naturalists could not understand the value of locality. A museum was regarded as a collection of curiosities, and so long as the objects were there it little mattered in their eyes whence they came. The consequence is that all our older collections, and even, I regret to say, our national collection itself, are filled with specimens utterly without a history attached to them, unless it be that they were purchased of a certain dealer in a certain year. Even in the present generation it is only the more advanced and enlightened thinkers that really understand the importance of locality. It is with the hope of impressing the value of locality and distribution more firmly upon you that I have devoted my address not to the general progress of biology, but to the present state of, and recent additions made to, our knowledge of the geographical distribution of the Vertebrata.

Dr. Carpenter, in moving a vote of thanks to the President for his address, said its value would only be fully appreciated by the working naturalist studying and consulting it in the prosecution of his researches. Such a stock-taking was of the highest value in guiding to the right study of what was known, and in laying bare deficiencies. Within a few years the subject of geographical distribution had arisen to great dimensions, both in relation to the origin of species and to the changes in the earth's surface since the present distribution of life had been approximately attained. Any single fact with regard to distribution had its value, but accuracy was vital; as he proceeded to show. The different species of fresh-water fish in Swiss lakes were now regarded as modifications due to differences of food, temperature, bottom, &c., having their slow effect in developing races since the time when the various waters were in communication, and if changes were admitted to such an extent in our existing fauna as the result of plain causes, it was legitimate to argue that much greater changes might have taken place in the ages of geological time.—Professor Allman spoke of the increased importance of all the results of exploration since the promulgation of the doctrine of descent, which was now almost universally accepted in one form or another.—Professor Rolleston said that Dr. Sclater's paper on Geographical Distribution had come out in 1858, before Messrs. Darwin's and Wallace's papers had been published; and yet what he had laid down in 1858, he had in no important points had to modify. He did not know of any biological doctrines that had undergone so little change since that period.—Dr. Sclater announced that he proposed to add an appendix to his address, containing the full titles of all the works he had referred to.

Department of Zoology and Botany.

Professor Newton read a paper "On certain neglected subjects of ornithological investigation." He said that it seemed to him that ornithologists had been getting into certain well-worn ruts, to the abandonment of other tracks which were well worth travelling upon. He had recently had occasion to take stock of our present ornithological knowledge, and on the whole the result was gratifying. Some departments had received an enormous impetus from the doctrines of evolution, and that impetus would continue and would probably be increased. Some years ago there was a very general disposition to cry down species-mongers, as they were called in opprobrium; but it was a very short-sighted view; and in his opinion they were having their revenge, for their work had now a value far above that which it had in the Pre-Darwinian days. The result of labours on geographical distribution was good, and was gradually helping to build the edifice of evolution; not that the edifice was erected yet; its walls were still far from complete. Yet he thought its completion was about as sure as anything well could be. The subject of what he might call developmental osteology, in which the illustrious name of Parker stood practically alone, was one in which it might truly be said that the harvest was plenteous and the reapers few. There was room for a score of Parkers; yet it was no more likely that they would get them than that they would get a score of Shakespeares. Fossil ornithology had not as yet produced very great results, but descriptive anatomy was in a fairly good condition, although he was afraid that a great many skilled observers of the outsides of birds knew very little about it. As to pterylography, he feared it was not very much thought of, and that a vast majority of ornithologists did not know the meaning of the word. He recommended all to read the translation of Nitzsch's great work on the subject in the Ray Society's publications. He noticed the greatest falling-off in observational ornithology. They had outdoor ornithologists by dozens, all going on in exactly the same way as their predecessors, each trying to find out the same facts for himself; so that they were almost at a standstill, especially on the subject of the migration of birds. Observers were content not to do anything more than had been done by Gilbert White, forgetting that he had had to prove or disprove the fact of migration, about which there was no question now. We wanted to know something of the causes of migration and of the faculties by which it was performed. Hundreds of records of dates of arrival of birds would bring us no nearer to these discoveries. He thought a digestion and collation of the immense mass of facts on these subjects already existing in Great Britain was wanted, such as had already been prepared for Germany; but one thing that would *not* come of it, he was persuaded, was an answer to the questions he had indicated. There was great want of information as to the routes taken in migration, and also as to the facts of partial migration. He thought they must look in this direction for the solution of the larger question. It would be very enlightening if they could know something of the reasons which induced the migration of the majority of individuals of a species, leaving some behind. It had been suggested by Dr. von Mittendorff that probably birds in their migrations were guided by a knowledge of the situation of the magnetic pole; and however much they might disbelieve that, they had really no facts which could controvert that or any other wild theory on the subject. As to birds learning the way by experience, and by the teaching of those who had traversed the route before, that would not explain migrations which took place by night, or over a thousand miles of sea. The laws of plumage and of moulting were little known, and might with advantage be studied by those who had constant access to zoological gardens, such as those at Clifton. The duration of the periods of incubation of birds was almost unknown, as well as the reasons for the variations. Nothing was known for certain as to the effect of variations of atmospheric temperature or other conditions in shortening or lengthening the period. Out of more than 200 species of British birds, the duration of incubation was known in only about twenty; and of foreign birds even less was known. He could mention other branches in which knowledge was deficient, but perhaps what he had said would be sufficient to induce some of those who had not adopted any special branch of study to prosecute some of the inquiries he had recommended. The good workers at present labouring were fully occupied with important subjects. He could not expect that they would be able to divert their attention from their chosen departments.—In the discussion which followed, Canon Tristram remarked on the ease with which many who go abroad for the winter or summer might make valuable records of the time of arrival or the latest time of seeing migratory birds. Mr. Elwes urged on country clergymen the valuable service they might easily render by taking ornithology as a recreation; much was lacking in regard to osteology and nidification; skins were too much attended to. Mr. Bettany urged the study of Mr. Parker's papers on all ornithologists who could make themselves capable of comprehending them, in order to prepare the way for a better understanding of the genetic affinities of birds in the future; and also mentioned the service that might be done to such men as Mr. Parker by any naturalist who would collect a series of specimens from the earliest to the adult stage of any single species, and preserve them for study by such an authority.

On the reading of the report on the Zoological Station at Naples, which we have abstracted elsewhere, Mr. Spence Bate said it was greatly to be desired that such schools of study should be established in Great Britain. He did not think they should have to go to Naples for one. They should be attached to the various aquariums now being established.

Dr. C. T. Hudson read an able paper, the result of many years' study, on the classification and affinities of the Rotifera. It was illustrated by a large number of beautiful magnified drawings of their anatomy, bearing testimony to the industry and ability of the author. He commenced by discussing Ehrenberg's classification, and showed that its fundamental principles were erroneous, for it was based on a supposed structure of the trochal disc which did not really exist, on a forced interpretation of the term lorica, and on the presence, absence, and number of certain red spots, which Ehrenberg took for granted as eyes, but which were not always so. Moreover, those that really were eyes were often present in the young animal, but invisible in the adult. Ehrenberg's symmetrical system brought together widely dissimilar forms, and separated those that were intimately connected. Not a single Rotifer, as far as the author could find, properly came under Ehrenberg's Monotrocha. A new Melicerta had been found, that did not make a tube; and his Sorotrocha included every form of head. There was no such thing really as a Holotrochous form. The systems of Leydig and Dujardin were then examined and shown to be inferior to Ehrenberg's, though it was pointed out that each naturalist had contributed a happy idea, the former having brought into prominence the great value of the foot as a characteristic for classification, the latter having the thought of classifying the Rotifers by their mode of locomotion. Dr. Hudson then proceeded to offer a natural classification, using the best results of preceding observers, based on the habits, teeth, water, vascular and nervous systems. There were four great groups, subdivided into families. (1) *Rhizota*, the permanently attached forms, all having teeth of the same pattern, including the Floscularina and Melicertidæ; (2) *Bdelloida*, those that swim and creep like a leech, including the Philodinidæ, the lowest and most worm-like forms; (3) *Ploïma*, or free-swimmers, including Brachionidæ, Pterodinidæ (a new genus and species of his own), Euchlanidæ, and Notommatina; (4) *Scirtopoda*, or jumpers, including Pedalionidæ, and Synchætidæ. As to the affinities of the Rotifers, while giving up Philodinidæ to the Vermes, he advanced numerous reasons for believing that the other Rotifers were allied to Entomostracans, and ought to be classed with them. He claimed to have destroyed some of the arguments of Professor Huxley on this point, by finding male forms which had been previously unknown, and among them the male of *Lacinularia socialis*, the very species taken as the text of Professor Huxley's remarks on the whole class. The resemblance of Pedalion to some Entomostracous larvæ was insisted on, as also its connection by other aberrant rotifers with those of typical form.—Mr. Spence Bate spoke highly of the labour and skill which Dr. Hudson had spent upon this class, but he must say that in regard to the affinities of Rotifera the evidence brought forward had been such as to convince him most conclusively that they were *not* related to the Crustacea.

On the Primary Divisions of the Chitonidæ, by P. P. Carpenter, B.A., Ph.D., Montreal.—He divided them into articulated or perfect, and non-articulated or imperfect; each of these were naturally divided into regular and irregular. The Palæozoic Chitons were all imperfect, and culminated in the Carboniferous period; very few are now living. The Neozoic epochs gradually developed perfect Chitons which culminate at the present time. The writer sought information as to unusual forms, recent or fossil, to be posted to 508, Guy Street, Montreal.

Department of Anatomy and Physiology.

ADDRESS BY PROFESSOR CLELAND, M.D., F.R.S., VICE-PRESIDENT.

I shall not venture to occupy the time of the Section with any *résumé* of the work done in anatomy and physiology during the past year, as such information is readily accessible in the pages of journals and year-books. I shall content myself with making some comments on the condition of anatomy at the present time in a few important particulars.

I had intended to speak also of some subjects connected with physiology; but I find that I cannot do so without lengthening my remarks to a greater extent than might be desirable. I shall be content, therefore, so far as that science is concerned, to mention that, although experimental physiology is probably less cultivated in this country than in any other in which biology is studied, it has been practically decided by Parliament that it is quite time to put some check on investigation in that direction; for, as everyone knows, a Royal Commission has been appointed to inquire into vivisection. In the scientific world all are agreed, whatever opinions may prevail in other sections of the community, that the man who would wantonly inflict pain on a brute beast is himself a brute, and deserving to be roughly handled; and because there is no difference of opinion on that subject, and because no experimental science can well prosper if one man is to judge for another what experiments are justifiable to institute or to repeat, or are likely to give important results, I do deplore the clamour which well-meaning persons have raised, and regret that it has been so far yielded to.

In anatomy the most important progress in recent years has been made in those departments which abut most closely on physiology, namely, the microscopy of the tissues and development. The whole conception of the nutrition of the body has become altered in comparatively recent years by the additions to our knowledge of the nucleated corpuscles, which are the living elements of which it is composed; and principally by the recognition of the secondary nature of cell-walls, the close connection or even continuity of the nerves with other textures, and the identity of the white corpuscles of the blood with amœboid or undifferentiated corpuscles outside the vessels. The origin of every living corpuscle from corpuscles pre-existing is no longer difficult to imagine, but may, I incline to think, be almost looked on as proved. The history of each may be traced back through conjugated germs to the corpuscles of preceding generations in uninterrupted succession, and the pedigree of the structural elements is seen to differ in no way from that of individual plants or animals. It is true, indeed, that no absolute proof exists that new living corpuscles originating by mere deposit are not added to the others; but the evidence against such a thing taking place is exactly of the same description as that which exists against spontaneous generation of independent organisms, namely, that things previously unexplained by the theory of parentage are explained now, while, on the other hand, there is no sufficient evidence of the origin of life by any other mode.

Leaving histology (he said), I shall devote the rest of my remarks to the morphology of the *Vertebrata*. Here I am less disposed to indulge a gratulatory vein. No doubt within the last dozen years we have had work to be grateful for. Worthy of a prominent place in this, as in other departments of anatomy, is the encyclopædic work, the "Leçons," of Milne-Edwards, invaluable as a treasury of reference to all future observers; while the memoirs of Gegenbaur on the carpus, on the shoulder-girdle, and on the skulls of Selachian fishes, and Kitchen Parker's memoirs devoted to mature forms, may be taken as examples that morphological problems suggested by adult comparative anatomy have not lost their attraction to men capable of elaborate original research. And I the more willingly select the names of these two writers, because on one subject on which they have written, the shoulder-girdle, I am compelled to differ from their conclusions and to adhere rather to those of Owen, so far as the determination of the different elements in fishes is concerned; and by stating this (although the subject cannot be now discussed) I am enabled to illustrate that the appreciation of the value of elaborate and painstaking work is a matter totally distinct from agreement with the conclusions which may be arrived at in the investigation of complicated problems, although wisdom and penetration as to these must ever command admiration.

But when one looks back on the times of Meckel and Cuvier, and on the activity inspired by the speculations of the much-abused Oken, the writings of Geoffroy St.-Hilaire, the less abstrusely speculative part of the works of G. C. Carus, and the careful monographs of many minor writers; when one reflects on the splendid grasp of Johannes Müller, and thinks of the healthy enthusiasm created in this country for a number of years by Owen's "Archetype and Homologies of the Vertebrate Skeleton," and then contemplates the state of vertebrate morphology at the present moment, it seems to me that its homological problems and questions of theoretical interest do not attract so much attention as they did, or as they deserve.

The Origin of Species by Natural Selection.

There can be no doubt that a great and curious influence has been exercised on morphology by the rise of the doctrine of the origin of species by natural selection. Attention has been thereby directed strongly for a number of years to varieties, and probably it is to this doctrine that we owe the larger number of observations made on variations of muscles, nerves, and other structures. Particularly elaborate have been the records of muscular variations, very praiseworthy, interesting to the recorders, very dry to most other people, and hitherto, so far as I know, barren enough of any general conclusions. So much the more credit is due to those who have worked steadily in faith that beauty will emerge to gild these results some day.

But the doctrine of natural selection has had a further effect in anatomical study, aiding the reaction against the search for internal laws or plans regulating the evolution of structures, and directing attention to the modifying influences of external agencies. This effect has happened naturally enough, but it has been far from just; rather is it a pendulum-like swing to another extreme from what had previously been indulged in. The doctrine of natural selection starts with the recognition of an internal formative force which is hereditary; and in the development of the doctrine, the limits of hereditary resemblance have een greatly studied; and further, it will be observed that one of the fundamentals of the doctrine is, that the formative force alters its character gradually and permanently when traced from generation to generation in great tracts of time. Now I am not going to enter on a threadbare discussion of the origin of species in this company; suffice it to say that, while the existence and extensive operation of such a thing as natural selection seems to have been convincingly proved, it is a very different thing to allege that it has been the sole, or even the principal agent in producing the evolutions of living forms on the face of the earth. So far as anatomy is concerned, it is a secondary matter whether the link between the members of the evolving hosts of life have been genetic or not. But I wish to point out that, even pushing the Darwinian theory to the utmost possible extreme, the action of external agents infers the existence of something acted on; and the less directly they act, the more importance must be given to the hereditary or internal element. We are therefore presented with a formative force, which exhibited itself in very simple trains of phenomena in the first beginnings of life, and now is manifested in governing the complex growth of the highest forms. We are set face to face with that formative force, and are obliged to admit its inherent capability of changing its action; and that being the case, is it more of an assumption to declare that the changes are all accidental and made permanent by accident of external circumstances, or to consider that it has been the law proper to this force to have been adequate to raise forms, however liable to modification by external circumstances—to raise them, I say, from the simple to the complex, acting through generations on the face of the earth, precisely as it acts in the evolution of a single egg into an adult individual? This is that formative force which has been elaborately shown by Mr. Darwin, in launching his theory of "pangenesis," not only to be conveyed through whole organisms and their seed, but to pervade at all times the minutest particles of each; and I merely direct attention to the fact that its extension over the whole history of life on the globe must be granted, and ask if, in the range of forms which furnish at the present day an imperfect key to the ages which are past, there is not exhibited a development comparable, in its progression to definite goals, with what is shown in the life of a single plant or animal. For my own part, I am fully convinced of a unity of plan running through animal forms, and reaching, so far as the main line is concerned, its completion in the human body. I confess that I think that there is evidence that animal life has reached its pre-ordained climax in humanity; and I cannot think it likely that, as myriads of years roll on, descendants differing *in toto* from man will be developed. To argue the subject would be to enter on the largest subjects of morphological anatomy, and on speculations on which agreement could not be expected. Even, however, in the nature of the variations in the human race there seems to be some evidence that the progress of evolution is to be traced from man, not to other animal forms yet to appear, but, through his physical nature, into the land of the unseen. Those variations, keeping out of view differences of bulk and stature, which appear to have some relation to geographical position, are principally to be found in the head, the part of the body most closely connected with the development and expression of the mental character; and I may mention that when, some years ago, my attention was directed to the variations of the skull, the only part whose variations in different races I have had opportunity of studying with any degree of minuteness, I became satisfied that in uncivilised races there might be distinguished skulls which had undergone hereditary degeneration, others which had reached the most advanced development possible for them, and a third set, notably the Kaffirs, with large capabilities for improvement in the future. Indeed it is beyond doubt that there is a limit for each type of humanity beyond which it cannot pass in the improvement of the physical organisation necessary for mental action.

There are also some curious indications in human structure of the formative force nearing the end of its journey. In the details of the skeletons of other animals one sees the greatest precision of form; but there are various exceptions to this neatness of finish in the skeleton of man, and they are found in parts specially modified in connection with the peculiarities of his development, and not requiring exactness of shape for physiological purposes; while, on the other hand, physiognomical mould and nicety of various physiological adaptations are found in perfection. Look at the variations in the breast-bone, especially at its lower extremity, which is never shapely, as it is in the lower animals. Look at the coccygeal vertebræ; they are the most irregular structures imaginable. Even in the sacrum and in the rest of the column the amount of variation finds no parallel in other animals. In the skull, except in some of the lowest forms of humanity, the *dorsum sellæ* is a ragged, warty, deformed, and irregular structure, and it never exhibits the elegance and finish seen in other animals. The curvature of the skull and shortening of its base, which have gradually increased in the ascending series of forms, have reached a degree which cannot be exceeded; and the nasal cavity is so elongated vertically, that in the higher races nature seems scarcely able to bridge the gap from the cribriform plate to the palate, and produces such a set of unsymmetrical and rugged performances as is quite peculiar to man; and to the human anatomist many other examples of similar phenomena will occur.

Questions of homology are matters which must be ever present in the study of structure, as distinct from function—both the correspondence of parts in one species to those in others, and the relations of one part to another in the same animal; and perhaps I shall best direct attention to the changes of opinion on morphological subjects in this country during the last twenty-five years by referring shortly to the homological writings of three eminent anatomists—Professors Owen, Goodsir, and Huxley.

Changes of Opinion on Morphological Subjects.

For the first time in English literature the great problems of this description were dealt with in Prof. Owen's work already referred to, published in 1848; and it is unnecessary to say that, notwithstanding the presence of unquestionable errors of theory, that work was a most valuable and important contribution to science. The faults in its general scope were justly and quietly corrected by Goodsir at the meeting of this Association in 1856 in three papers, one of them highly elaborate; and in these he showed that the morphology of vertebrate animals could not be correctly studied while reference was made exclusively to the skeleton. He showed the necessity of attending to all the evidence in trying to exhibit the underlying laws of structure, and especially of having constant regard to the teachings of embryology. Among the matters of detail which he set right it may be mentioned that he exposed the untenability of Prof. Owen's theory of the connection of the shoulder-girdle with the occipital bone, and pointed out that the limbs were not appendages of single segments corresponding with individual vertebræ. Referring to the development of the hand and foot, he showed the importance of observing the plane in which they first appear, and that the thumb and great toe are originally turned towards the head, the little finger and little toe toward the caudal end of the vertebral column. But he probably went too far in trying to make out an exact correspondence of individual digits with individual vertebral segments, failing to appreciate that the segmentation originally so distinct in the primordial vertebræ becomes altered as the surface of the body is approached—a truth illustrated in the vertebral columns of the plagiostomatous fishes, in the muscle-segments over the head in the pleuronectids, and in the interspinal bones bearing the dorsal and anal fin-rays of numbers of fishes, but, so far as I know, not hitherto sufficiently appreciated by any anatomist.

In 1858 Prof. Huxley delivered his Croonian Lecture on the vertebrate skull, and in 1863 his lectures at the Royal College of Surgeons on the same subject. He profited by the wisdom of Goodsir, and studied the works of Rathke, Reichert, and other embryologists. But, rightly or wrongly, he took a step further than Goodsir. He assumed from the first that the homologies of adult structures could be determined by development, and that by that study alone could they be finally demonstrated. As regards the skull, the constitution of which always remains the central study of the vertebrate skeleton, his writings marked the introduction of a period of revulsion against not only the systems of serial homologies previously suggested, but even against any attempt by the study of the varieties of adult forms to set them right. Mr. Huxley has added materially to the previously existing number of interpretations as to what elements correspond in different animals, and in doing so has found it necessary to make various additions to the already troubled nomenclature. Those who consider these changes correct will of course see in them a prospect of simplicity to future students; but to those who, like myself, have never been able to agree with them, they are naturally a source of sorrow. Among the changes referred to may be mentioned the theory of the "*periotic bones.*" That theory, I venture to think, a very unfortunate one, introducing a derangement of relations as widespread as did Goodsir's theory of the frontal bone. And do not think me presumptuous in saying so, seeing that this theory is in antagonism with the identifications of every anatomist preceding its distinguished originator, not excepting Cuvier and Owen; nor is it easy to discover what evidence it has to support it against the previously received decision of Cuvier as to the *external occipital* and *mastoid* of fishes. Without entering into the full evidence of the subject, it may be stated that, so far as this theory affects the *alisphenoid* in the skull of the fish, it must be given up, and the determination of Prof. Owen must be reverted to, when it is considered that in the carp the third and fourth nerve pierce what that anatomist terms the *orbitosphenoid*, the bone which is *alisphenoid* according to the theory which terms the *alisphenoid* of Owen the *prootic*. A proof still more striking is furnished by *Malapterurus* and other Silurids, in which the bone in question is pierced by the optic nerve. That being the case, the prootic theory will be seen to have arisen partly from giving too much importance to centres of ossification, and partly from considering the nerve-passage in front of the main bar of the *alisphenoid* of Owen as corresponding with the *foramen ovale* of man rather than with the *foramen rotundum* and sphenoidal fissure. A spiculum, however, separating the second from the third division of the fifth nerve, and having therefore the precise relations of the mammalian *alisphenoid*, does exist in the carp and other fishes. But in reptiles Prof. Huxley's determination of the *alisphenoid* is right, and Prof. Owen's clearly wrong; for in the crocodile the *alisphenoid* of Huxley and others is perforated by the sixth nerve, so that it cannot have any claim to be called *orbitosphenoid.* I must, however, maintain, against Prof. Huxley's view, Prof. Owen's determination of the *nasal* in fishes, notwithstanding that Prof. Owen has failed to appreciate the exact relation of that bone to the nasals of mammals, and has thereby laid his position open to attack. The arguments on that point Prof. Huxley was good enough to lay before the public fourteen years ago, by kindly reading for me before the Royal Society a paper which subsequently appeared in its "Transactions;" and I am not aware that anyone has since attempted to controvert them.

I shall not trouble you further with such matters of detail; but it will be clear from what has been said that the beginner in comparative anatomy must at the present day find himself at the outset, in the most important part of his osteological studies, faced with a diversity of opinion and confusion of nomenclature sufficient to produce much difficulty and to have a repelling effect on many minds. Such difficulties might well be encountered with enthusiasm where a belief existed that behind them lay a scheme of order and beauty; but not many will spend time in investigating such intricate details if they doubt the interest of the general conclusions likely to be reached by mastering them. On this account it is a great pity that the scepticism generated partly by the difficulties of the subject, and partly by reaction from the dogmatism of the admirers of Oken, does too frequently discourage the investigation of the serial homologies of the parts entering into the segments of the skull, and the determination of the nature and number of those segments. It is a pity that so much clamour has been made for a number of years against the expression "vertebral theory of the skull," because fighting against words is but stupid warfare at the best, and because all that was really meant, and could be justly stated, could have been brought into prominence without objecting to a time-honoured phrase. It is questionable if anyone who ever used the convenient term "vertebral theory" meant to indicate more than a certain community of plan on which were built the segments of the skull as well as those of the spinal column; that, in fact, the two constituted one complete chain, of which the first few segments were so different from the rest, that till Oken pointed the fact out, it was not recognised that they were segments lying in lineal continuity with the rest. But the matter has recently stood thus:—that to some minds, in the imperfect state of our knowledge, one thing seemed essential to a segment comparable to the rest, and to others something else seemed requisite; and the oddity of the position of affairs is this, that the objectors to the phrase "vertebral theory" have been as crotchety in setting up imaginary essentials to a segment as their neighbours. On the one side we were taught to expect certain definite osseous elements in each segment, to which definite names were given; while, on the other, in opposition schemes, centres of ossification have been built on as matters of primary consequence, although a glance at the modifications in the vertebral column proper might convince anyone that they are things of the very slightest importance morphologically. Also those who have objected to speaking of cranial vertebræ have put great importance on the point at which the *chorda dorsalis* terminates, although it has been long known that in one animal the *chorda dorsalis* runs right on to the front, that in others it fails to enter the skull at all, while in the majority it passes for a certain distance into the base. Johannes Müller, on such grounds, concluded thirty years ago that the presence of *chorda dorsalis* was not necessary to constitute a cranial vertebra; and there seems no reason to doubt that he was right. Looking at the early embryo, the cerebro-spinal axis is seen to be one continuous structure; and the walls of the canal containing it are likewise manifestly continuous, not at first distinguishable into a spinal and a cranial portion. Looking at the adult condition, in the higher classes the vertebræ of the tail are seen dwindling into mere bodies developed round the *chorda*, and giving off rudimentary processes without separate centres of ossification, while towards the head the bodies diminish and the arches enlarge; and in the skull the *chorda*, round which the bodies in the rest of the column are developed, comes to an end, and the neural arches are enormously enlarged and have additional centres of ossification, precisely as in the mammalian thorax costal centres of ossification are found which do not exist in the costal elements of cervical vertebræ. It would therefore be quite as justifiable to object to the term vertebra as applied to a joint of the tail because it has no *laminæ*, or none with separate centres of ossification, as to object to its applicability to segments of the skull because the *chorda* is absent, or the osseous elements different in number from those found usually in the segments of the trunk.

However, it is gratifying to observe that among the most recent additions to morphological anatomy there is a highly suggestive paper by Prof. Huxley, appearing in the Royal Society's "Proceedings" for December last, and entitled "Preliminary Notes upon the Brain and Skull of *Amphioxus lanceolatus*," in which the learned Professor, who has for many years been the most determined opponent to the mention of cranial vertebræ, declares, so far as I can apprehend his meaning, that the region of the head represents no less than fourteen segments, all of which he terms *protovertebræ* in *Amphioxus*. This determination of correspondences is made the more remarkable by being followed up with a suggestion that the numerous protovertebræ lying in front of the fourteenth in *Amphioxus* are represented only by muscles and nerves in the higher vertebrates.

I hail this paper as being practically at last an ample acknowledgment that there is no escape from admitting the correspondence of the region of the head with the segments of the trunk: but the details of the new theory scarcely seem convincing; and I might have preferred to leave its discussion to others, were it not that the notions which it opens up are far too important to allow it to be passed over in any account of the present state of opinion on the subject of vertebrate morphology. The argument in this new theory runs thus: that the palate-curtain of *Amphioxus* is homologous with that of the lamprey, and that the palate-curtain of the lamprey is attached below the ear; that therefore all the seven segments seen in front of the palate-curtain of *Amphioxus* are represented by parts in front of the ear in the lamprey and the other Vertebrata. Again, the branchial arches of the higher Vertebrata are assumed to be of the nature of ribs, and in none of the Vertebrata next above *Amphioxus* "are there more than seven pairs of branchial arches, so that not more than eight myotomes (and consequently protovertebræ) of *Amphioxus*, in addition to those already mentioned, can be reckoned as the equivalents of the parachordal region of the skull in the higher vertebrates." Everything, observe, depends on the segment to which the palate-curtain of *Amphioxus* belongs. Now I have already pointed out to you that the segmentation of the vertebrate body is not perfect; and there is no method by which the alimentary canal, of which the mouth and palate are the first part, can be divided into segments corresponding with the cerebro-spinal nerves. Most certainly we cannot judge that a portion of a viscus belongs to a particular segment from its lying underneath some other structure in definite relation, like the ear, to the cerebro-spinal system; for then should we be obliged to grant that one-half or more of the heart belongs to segments in front of the ear, since it is undoubtedly so situated in a chick of the thirty-sixth hour. But the branchial arches are in front of the heart, and, according to the theory which we are considering, are behind the ear; thus the principle assumed in the starting-point of the theory is taken away.

Again, it is important to observe that the branchial skeletal arches cannot be ribs, for they lie internal to the primary circles of the vascular system formed by the branchial arteries and veins, while the ribs are superficial to both heart and aorta. If the ribs are represented at all in the branchial apparatus (and I doubt it very much), it is by the cartilages superficial to the gills in sharks, rays, and dog-fishes; and it would seem impossible for anyone who has dissected them to doubt that those cartilages are homologous with the branchial skeleton of the lamprey, which they somewhat resemble. In fact, if the external and internal branchial openings of the lamprey be enlarged, its gills are reduced to a form similar to those of the shark.

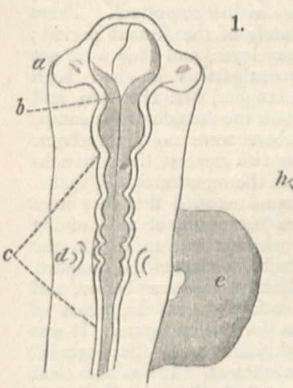
There is nothing in this, however, which interferes seriously with the proposed theory of the skull. It is merely a point in the argument which I have thought right to clear. More important it is to remark that, on the supposition that numerous protovertebræ are represented in the region of the head, there are most serious difficulties interfering with the idea that they are, as Prof. Huxley states, "represented only by muscles and nerves in the higher Vertebrata," and that there is any correspondence between "the oculo-motor, pathetic, trigeminal, and abducens nerves with the muscles of the eye and jaws" and the regular nerves and muscle-segments of the fore part of *Amphioxus*. Even in the lamprey the eye-balls are supplied with muscles similar to those to which, in other vertebrates, the oculo-motor, pathetic, and abducens are distributed; and I find in the large species that, notwithstanding this, the series of regular muscle-segments is continued over the head, not indeed in the same way as in *Mixine*, but in a highly instructive and curious manner. After further dwelling upon this point, Prof. Cleland said:—

It may be noticed as a wholesome symptom in anatomical speculation, that the new theory which has led to these remarks is founded on arguments drawn altogether from comparison of different species, and not from embryology, a very remarkable circumstance as coming from one who so lately as last autumn reiterated in this Section his slowness to believe in reasonings founded on adult forms, and even on "later development." The wisest know so little, that humanity must be content to gather information from every possible source, and leave no set of ascertained facts out of view in attempting to arrive at generalisations. If we had before us all the adult anatomy of every species that ever lived on the earth, we should only then have the record completed from which to frame a full system of morphology; and as matters stand we must translate embryological phenomena with the aid of the series of adult forms, as well as translate the teachings of the adult series with the aid of embryology.

Falling back on my proposition, that the segments of the vertebrate body are nowhere complete, and that segmentation at one depth may exist to a greater extent than at another, I may mention certain embryological phenomena in the brain, which have received too little attention, and which to some extent warrant belief in a larger number of segments in the head than is usually admitted; although I do not see that they are necessarily at variance with that theory of seven segments in every ossified skull which I indicated in 1862. In the chick, in the middle of the second day of hatching, already is the third cerebral vesicle divided into a series of five parts, separated by slight constrictions, the first part larger than those which succeed, and the last part narrowing to the spinal cord. The auditory vesicle lies opposite the constriction between the fourth and fifth parts. At the end of the second day and during the third, these divisions assume dimensions which give them a general appearance exceedingly similar in profile to the protovertebræ of the neck. In the following day they exhibit a more complex appearance, and after that the first compartment alone remains distinct as cerebellum, while the divisions between the others disappear in the thickening of the cerebral walls. In their first two stages, Mr. Huxley, whom I have already referred to so often, has figured these crenations, but he has not, so far as I know, described them.

I may also direct attention to another embryological point, to which I referred last year at Belfast as a probability. I speak now from observation. That which is termed the first cerebral vesicle in the early part of the second day of hatching of the chick, is an undifferentiated region of the brain from which a number of parts emerge successively from behind forwards. As early as the thirty-sixth hour the optic nerves can be traced, separated from the rest of the vesicle by distinct elevations of the floor of the brain, reaching inwards to the constriction between the first and second vesicles: and as early as this date the first trace of bifidity of the brain in front may be discerned—

that bifidity which, to my thinking, is only one of several instances of longitudinal fission in the fore part of the head, the trabeculæ presenting another instance of the same thing, and the cleft between the maxillary lobe and the part of the head above it a third; while in the muscular system such longitudinal cleavage or fission is common even in the trunk. In a chick of the third or fourth day, when rendered very transparent, the optic nerves can be seen extending from beneath the front of the



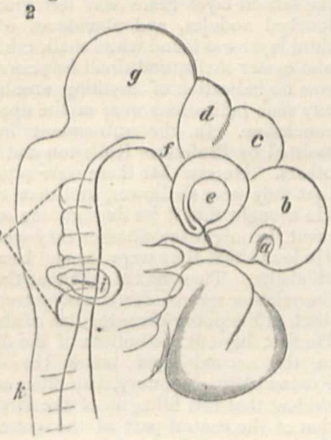


FIG. 1.—Embryo chick of 36 hours. *a*, primary optic vesicle; *b*, optic commissure; *c*, third cerebral vesicle; *d*, ear; *e*, heart.
FIG. 2.—Chick three days old. *a*, nostril; *b*, hemisphere; *c*, *d*, divisions of first cerebral vesicle; *e*, eye; *f*, optic nerve; *g*, optic lobe; *h*, crenations of third cerebral vesicle; *i*, ear; *k*, first primordial vertebra.

optic lobes; while in front of the optic lobes there are placed in series from behind forwards a posterior division of the first vesicle, an anterior division, the cerebral hemispheres, and the olfactory lobes. Thus there is a large supply of material presented in the brain for the study of segmentation; the difficulty to be overcome by future inquiry and careful collation of all available facts is to determine the value of the parts placed one in front of another.

Perhaps I have occupied time too long with matters involving a large amount of technical detail; but I trust that I may have, in some measure, illustrated that both in aim and in accomplished work anatomy is no mere collection of disconnected facts, no mere handmaid of the physician and surgeon, nor even of phy-

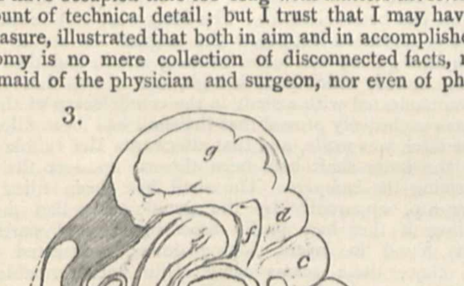


FIG. 3.—Chick of fourth day. Letters the same as previous figure.

siology. I do not doubt that it is yet destined, as dealing with the most complex sequences of phenomena, to take the highest place among the sciences as a guide to philosophy. One cannot help noticing the increased importance now given to Natural History studies as a part of education; and it is worth while to note that it is most of all in anatomy and physiology that the close connections of matter with mind are brought under review,—physiology exhibiting the relations of our own mental being to our bodies, and anatomy revealing a body of organised nature, whose organisation points to a source of beauty and order beyond.

The people of Bristol do well to rally round their Medical School. They do well to furnish it with buildings suitable for the prosecution of all the Natural History studies which adhere to medical education; and they do well to join with that school a complete college of literature and science. Let us hope that they will make it worthy of so wealthy and historic a city. But if they will have their medical school the success which in so flourishing a locality public enthusiasm may well make it, and if they will have it aid as well as be aided by a school of general education, let them follow the system latterly adopted in Oxford and Cambridge, long carried out in the Universities of Scotland, and recognised, though not in all instances sufficiently provided for, in Ireland. Let anatomy, human and comparative, receive its place as an important and fundamental science. Let thorough and adequate provision be made for its being taught as a science; and see that it do not, as in too many medical schopls which shall be nameless, degenerate to the etymological and original meaning of the word, a mere cutting up of carcases.

Mr. H. B. Brady exhibited a series of micro-photographs chiefly from physiological and pathological preparations, taken by a new and simple process, devised by Mr. Hugh Bowman, of Newcastle. The apparatus was also shown and described. It consisted of a simple mirror of speculum metal, placed at an angle of 45 degrees in front of the eye-piece of the microscope, and directed downwards. The image was received upon a collodion plate set in the frame of a common photographic camera, and the photograph taken in the usual way. About 11 seconds was stated to be a sufficient exposure for the purpose.

A paper was read by Dr. Martyn entitled *Some New Researches on the Anatomy of the Skin.* Dr. Martyn had discovered that the cells which appeared "spinous" or "echinate," when isolated from their connection, if they could be at any time seen in single layers, were simply united together by delicate bands. These are so constantly seen broken across that they assume the form of tubercles or "prickles." As repeated observations confirmed this, the name "conjoined epithelium" had been proposed for this form or stage in the cell life.

A paper *On the Physiological Action of the Chinoline and Pyridine Bases*, by Dr. J. G. M'Kendrick and Prof. Dewar, was read by the former gentleman. The following are the general conclusions arrived at:—1. There is a marked gradation in extent of physiological action of the members of the pyridine series of bases, but it remains of the same kind. The lethal dose becomes reduced as we rise from the lower to the higher. 2. The higher members of the pyridine series resemble in physiological action the lower members of the chinoline series, except (1) that the former are more liable to cause death by asphyxia, and (2) that the lethal dose of the pyridines is less than one half that of the chinolines. 3. In proceeding from the lower to the higher members of the chinoline series, the physiological action changes in character, inasmuch as the lower members appear to act chiefly on the sensory centres of the encephalon and the reflex centres of the cord, destroying the power of voluntary or reflex movement; while the higher act less on these centres, and chiefly on the motor centres, first, as irritants, causing violent convulsions, and at length producing complete paralysis. At the same time, while the reflex activity of the centres in the spinal cord appear to be inactive, they may be readily roused to action by strychnine. 4. On comparing the action of such compounds as C_9H_7N (chinoline) with $C_9H_{13}N$ (parvoline, &c.), or $C_8H_{11}N$ (collidine) with $C_8H_{15}N$ (conia, from hemlock), or $C_{10}H_{10}N_2$ (dipyridine) with $C_{10}H_{14}N_2$ (nicotine, from tobacco), it is to be observed that the physiological activity of the substance is, apart from chemical structure, greatest in those bases containing the larger amount of hydrogen. 5. Those artificial bases which approximate the percentage composition of natural bases are much weaker physiologically, so far as can be estimated by amount of dose, than the natural bases; but the kind of action is the same in both cases. 6. When the bases of the pyridine series are doubled by condensation, producing dipyridine, parapicoline, &c., they not only become more active physiologically, but the action differs in kind from that of the simple bases, and resembles the action of natural bases or alkaloids having a similar chemical constitution. 7. All the substances examined in this research are remarkable for not possessing any specific paralytic action on the heart likely to cause syncope; but they destroy life either by exhaustive convulsions, or by gradual paralysis of the centres of respiration, thus causing asphyxia. 8. There is no appreciable immediate action on the sympathetic system of nerves. There is probably a secondary action, because after large doses the vasomotor centre, in common with other centres, becomes involved. 9. There is no difference, so far as could be discovered, between the physiological action of bases obtained from cinchonine and those derived from tar.

This paper, besides its purely scientific value, is of some interest to general readers on account of the fact discovered by Vohl and Eulenburg, that chinoline and pyridine are produced during the combustion of tobacco, and that the effects of tobacco smoking are to a great extent due to the action of these and similar bases.

Department of Anthropology.

Mr. John Evans, in moving a vote of thanks to Prof. Rolleston for his address, said it supplied the strongest evidence of the necessity for the application of the natural history method to anthropology; and the value of the study was shown by the way in which it had been brought to bear on questions of the present day.—Dr. Carpenter, in supporting the resolution, desired to refer to Dr. Prichard as a Bristol man, and because he had been mainly instrumental in directing his course at the outset of his public life; by his advice he had read his first paper before the British Association at its former visit to Bristol. His thoughts were those of a physiologist among physiologists, and a scholar among scholars, but he was resolved to keep the threads together if possible. He was perhaps the first to bring a large idea of species to bear upon the origin of man, and to trace out intermediate links and gradational characters, and to investigate the analogous features in the history of domesticated animals. With regard to the antiquity of man, he believed that Prichard was the first to propound the doctrine, now so generally accepted, of the much greater antiquity of man than could be supposed if the genealogies of Genesis were accurate. He made a careful and scientific investigation of those genealogies, and found it absolutely necessary to conclude that they could not be relied upon for chronological evidence; and when he further came to consider the amount of time necessary to produce such strongly marked races as the Jewish and the Egyptian, on the hypothesis of the unity of the race and the time which would be required to produce such divarications of language as existed in the early historical period, he was additionally supported in his view as to the antiquity of the human race.

Col. Lane Fox gave a most interesting account of recent excavations in Cissbury Camp, near Worthing, of which full details will be published at the earliest possible time. He said that the entrenchment was one of the largest in the south of England, and had all the peculiarities of a British earthwork. Camden spoke of this camp as the work of Cissa, the Saxon king of the district, from whom, in his opinion and in great probability, it derived its name of Cissbury. He believed the first notice of the place as a flint factory was by himself in 1868, when, finding a large quantity of flint flakes on the surface and a number of large pits which filled the interior of the camp on the west side, he dug into some of them to a depth of four or five feet, and found in them a still further number of flakes, together with finished and unfinished flint tools. It was evident that here was a flint factory, and that it was established because of the much greater ease of working the flints when first removed from the chalk. He had no idea then of the great extent of the mining operations of these chalk people, nor did he think it necessary to dig deeper, some of the pits as left open, and further opened by himself, being twenty feet deep. They appeared to be quite deep enough for a sufficient quantity of flints to be got. The true nature of these flint works was illuminated by accident, viz., by the cutting of a railway from Franières to Chimay, when fifty-five deep shafts of this kind were cut through, with galleries proceeding from them. In 1870 Canon Greenwell had excavated pits at Brandon, and found similar shafts and galleries. Since then Mr. Tindall, of Brighton, had opened one of the pits at Cissbury, and found a shaft thirty-five feet deep, with *Bos primigenius* and other remains of wild animals. Mr. E. Willett had excavated another twenty-five feet deep, and found galleries leading from them; and it was established that the flints did not exist so near the surface as he (Col. Lane Fox) had supposed. The question now became of great importance as to the relative age of the flint factory and the entrenchment in which it was situated. Since June last, and up to the week before the meeting of the Association, he had superintended work at these pits, aided by subscriptions from members of the Anthropological Institute. In April last he had opened a section in the ditch round the entrenchment in layers of eighteen inches to two feet, and found, in the upper layer, two oval flint implements, fragments of red earthenware, oyster shells, snail shells, bones of domestic animals, and fragments of Romano-British pottery. In the second layer there was ferruginous chalk rubble, with untouched nodules, and abundance of *Helix nemoralis*. In the third layer was found white chalk rubble, with bones of pig and ox, and oyster shells, and small fragments of British pottery. There was no indication of anything absolutely of the Roman period; any such indications were in the upper layer, and they were not conclusive. In the subsequent investigations he had been assisted by Professors Rolleston and Hughes, Mr. Harrison, and others. He saw that there were pits on the outside of the camp, that they were shallower, and that there were no flints about. He thought that if he dug out one or two nearest the entrenchment, he might see whether they were the mouths of old shafts. He found that they were so, and became satisfied that they were all shafts. Then he thought that the best means of ascertaining the relative ages of shafts and entrenchment was to dig in the ditch at the point where the line of shafts intersected the rampart. He first dug out the bottom of the ditch in layers as before, and in the second layer below the surface found fragments of Romano-British pottery, but none in the bottom layers. It was evident that this filling in of the ditch was due to the degradation of the central part of the entrenchment. It was also clear that pottery of the kind found was *not* used when the ditch was sunk. The side of the ditch sloped inwards to the west, but towards the east the inner side was perfectly upright, and the rubble near the upright part was quite white instead of yellow, showing that the excavators, when they came to the shafts, had cut through the rubble which had been used previously to fill up the shaft; the excavation was then continued into the shaft to 6 feet 6 inches below the old bottom of the ditch, and deer-horn tines and the scapula of an ox were found. In the bottom of the shaft galleries were found opening out of it; one ran north for twenty feet, and was two feet high. It rose at an angle of 5°, which was the angle of stratification of the chalk. In the sides of the galleries, at a height of a foot and a half, here and there, flints were found *in situ*, so that it was plain that the seam of flints had been followed. The flints were not reached till they got to seventeen feet below the original surface of the ground. Another gallery was found running south, and then a chamber was entered, which became high, and it was discovered that it communicated with a shaft in the counterscarp of the ditch. It was conclusively proved that the shaft had been filled in before the ditch was made, and that afterwards the rubble which filled in the inner shaft had been thrown up over the outer shaft forming the rampart. The shaft had been filled in up to the top, apparently by the people who had made it. In filling it they had partly used rubble and partly clay. It was found in layers sloping down, intersected with seams of clay; these seams were quite unconformable with the shaft or with the surrounding strata, but they were evidently derived from clay which was to be met with near the surface of the upper part of the entrenchment. They then followed out another shaft, and when nearly at the bottom he was astounded by a human jaw-bone falling down at his feet from the wall of the shaft; and looking up, he saw the skull resting with the base downwards between two of the blocks of chalk rubble. He procured at once other eyes to see it in its actual position, and then it was taken away, for it was in a very precarious situation. It and the accompanying human bones would be commented on by Prof. Rolleston. This and another shaft gave the same evidences, and had galleries running out of them, and it was clear that the outer rampart had been formed by the rubble thrown out from these filled-up shafts. A seam of flints was found in one of the galleries, and around and on the surfaces of some of them were found a number of marks which corresponded exactly with the deer-horn tines found, so that evidently the flints had been picked out by the aid of deer-horns. As to the implement found, in his opinion there were all transitions between Palæolithic and Neolithic implements. But the resemblance to palæolithic might be, he thought, more apparent than real, and partly might be due to their being unfinished. It was very difficult to command the breaking of flints, as he had found by actual experiment; and thus many unfinished implements were left in the pits. But there was one celt which was finished at the thin end, and was evidently of the Palæolithic type; and others were plainly roughened at the broad end so as to be held in the hand. He thought at any rate that these flints were of a very early Neolithic period, and showed considerable traces of the Palæolithic, though there might yet remain a gap between the periods. The shafts had been kept open, and would still be open for another fortnight, when, by agreement with the owner, they were to be closed up. Many of the leading authorities in this department had visited the place, but subsequently the only actual plan of the workings would be the wooden model which he exhibited, showing all the strata, shafts, and galleries.

Prof. Rolleston then proceeded to speak of the animal remains. He said the snail and other shells found were of great use, and supplied a cogent argument, without ambiguity, as cogent as Euclid. Here was the sharp line of the shaft, and at a depth of fourteen feet from the original surface were found an immense number of *Cyclostoma elegans*, *Helix nemoralis*, and other hybernating snail shells. They were not brought down to be eaten by the excavators, as was supposed, but the opercula were still found exactly *in situ*, giving evidence that they had gone down for warmth and shelter while still alive. They had also found plenty of food in the moist conditions of the shaft. They had undoubtedly gone down the shaft at a time when it was still open; and further evidence was that no snail shells at all were to be found in the rubble which had gradually dribbled down. He thought that the abundance of the shells was to be explained by the fact that these prehistoric Britons, like our own countrymen of the present day, were a little negligent in putting an end to nuisances, whether they were open shafts or otherwise, and thus the shaft had been open a good while, and a large series of snails had lived here. And among other results of this negligence was, he believed, the fall of a young British lady into the shaft. At any rate her bones had been found in a position quite compatible with this idea. With the skeleton were found a large number of pig-bones; there were at least four individuals, one old and three younger. We have not the entire skeleton of any one of them, and it was quite compatible with the evidence that these bones might have been thrown in piecemeal. Other bones were found, all of domestic animals, especially a small animal which might be a sheep or a goat; the critical pieces of the skeleton were absent. As to the *Bos primigenius* there was not the smallest doubt, and it was found in a position which showed it to have occurred before the advent of the small domestic animals; and wild animals of the same kinds must have then been much larger, or they would speedily have been exterminated by the wolves. As to the female skeleton, nearly all the bones were preserved, scarcely three of the vertebræ being missing. She was evidently between eighteen and twenty-five years of age, by various indications of the bones. She had a large head, yet it was an early type of skull, older than that of the people who built the rampart. The lower jaw contained a large number of teeth. The wisdom teeth were just through, and were scarcely worn at all; yet the two molar teeth in front of them on each side, above and below, were ground down nearly to the stumps. From this he inferred that the food had been of such a character as to produce wearing of the teeth. The evidence of the bones was conclusive as to her youth. The only parallel he could find to this was in the Indians of Vancouver's Island, who fed on fish dried in the sand-blowing winds, and their teeth were thus worn down to the stumps. Similarly these people might have fed on food dried in the wind, in which a large amount of fine sand got embedded. The cubical capacity of the cranium was very large; as measured by rape-seed it was 105·75 cubic inches; and the largest cubical capacity he knew of was one of a great Roman officer out of their burial grounds, whose capacity was 108 cubic inches. The people who made the shafts were undoubtedly older than the Britons who made the great rampart, and *they* were still in a stone-using period.

Mr. John Evans, in the discussion which followed, said that the main difference between Palæolithic and Neolithic was not that in one the implements were merely chipped and in the other polished, but in the manner of occurrence in the strata and the animals associated with them. In the Neolithic he estimated that ninety-five per cent. of all that was found was unpolished—all the smaller tools, &c. He acknowledged that he was not justified in saying that the pointed end of some of the implements from Cissbury was not intended to be used; and there were some cases in which it was impossible to tell which end was to be used. Even granting this exceptional resemblance, there is a great Neolithic facies in the things found at Cissbury. Still, he was quite willing now to accept the particular implement found at Cissbury as a new type of implement to be held in the hand; it might have been used in digging up roots.

SECTION E

Geography.

Address by Lieut.-General R. Strachey, R.E., C.S.I., F.R.S., President.

In accordance with the practice followed for some years past by the Presidents of the Sections of the British Association, I propose, before proceeding with our ordinary business, to offer for your consideration some observations relative to the branch of knowledge with which this Section is more specially concerned.

My predecessors in this chair have, in their opening addresses, viewed geography in many various lights. Some have drawn attention to recent geographical discoveries of interest, or to the gradual progress of geographical knowledge over the earth generally, or in particular regions. Others have spoken of the value of geographical knowledge in the ordinary affairs of men, or in some of the special branches of those affairs, and of the means of extending such knowledge. Other addresses again have dwelt on the practical influence produced by the geographical features and conditions of the various parts of the earth on the past history and present state of the several sections of the human race, the formation of kingdoms, the growth of industry and commerce, and the spread of civilisation.

The judicious character of that part of our organisation which leads to yearly changes among those who preside over our meetings, and does not attempt authoritatively to prescribe the direction of our discussions, will no doubt be generally recognised. It has the obvious advantage, amongst others, of ensuring that none of the multifarious claims to attention of the several branches of science shall be made unduly prominent, and of giving opportunity for viewing the subjects which from time to time come before the Association in fresh aspects by various minds.

Following, then, a somewhat different path from those who have gone before me in treating of Geography, I propose to speak of the physical causes which have impressed on our planet the present outlines and forms of its surface, have brought about its present conditions of climate, and have led to the development and distribution of the living beings found upon it.

In selecting this subject for my opening remarks, I have been not a little influenced by a consideration of the present state of geographical knowledge, and of the probable future of geographical investigation. It is plain that the field for mere topographical exploration is already greatly limited, and that it is continually becoming more restricted. Although no doubt much remains to be done in obtaining detailed maps of large tracts of the earth's surface, yet there is but comparatively a very small area with the essential features of which we are not now fairly well acquainted. Day by day our maps become more complete, and with our greatly improved means of communication the knowledge of distant countries is constantly enlarged and more widely diffused. Somewhat in the same proportion the demands for more exact information become more pressing. The necessary consequence is an increased tendency to give to geographical investigations a more strictly scientific direction. In proof of this I may instance the fact that the two British naval expeditions now being carried on, that of the *Challenger* and that of the Arctic seas, have been organised almost entirely for general scientific research, and comparatively little for topographical discovery. Narratives of travels, which not many years ago might have been accepted as valuable contributions to our then less perfect knowledge, would now perhaps be regarded as superficial and insufficient. In short, the standard of knowledge of travellers and writers on geography must be raised to meet the increased requirements of the time.

Other influences are at work tending to the same result. The great advance made in all branches of natural science limits more and more closely the facilities for original research, and draws the observer of nature into more and more special studies, while it renders the acquisition by any individual of the highest standard of knowledge in more than one or two special subjects comparatively difficult and rare. At the same time the mutual inter-dependence of all natural phenomena daily becomes more apparent; and it is of ever-increasing importance that there shall be some among the cultivators of natural knowledge who specially direct their attention to the general relations existing among all the forces and phenomena of nature. In some important branches of such subjects, it is only through study of the local physical conditions of various parts of the earth's surface and the complicated phenomena to which they give rise, that sound conclusions can be established; and this study constitutes physical or scientific geography. It is very necessary to bear in mind that a large portion of the phenomena dealt with by the sciences of observation relates to the earth as a whole in contradistinction to the substances of which it is formed, and can only be correctly appreciated in connection with the terrestrial or geographical conditions of the place where they occur. On the one hand, therefore, while the proper prosecution of the study of physical geography requires a sound knowledge of the researches and conclusions of students in the special branches of science, on the other success is not attainable in the special branches without suitable apprehension of geographical facts. For these reasons it appears to me that the general progress of science will involve the study of geography in a more scientific spirit, and with a clearer conception of its true function, which is that of obtaining accurate notions of the manner in which the forces of nature have brought about the varied conditions characterising the surface of the planet which we inhabit.

In its broadest sense science is organised knowledge, and its methods consist of the observation and classification of the phenomena of which we become conscious through our senses, and the investigation of the causes of which these are the effects. The first step in geography, as in all other sciences, is the observation and description of the phenomena with which it is concerned; the next is to classify and compare this empirical collection of facts, and to investigate their antecedent causes. It is in the first branch of the study that most progress has been made, and to it indeed the notion of geography is still popularly limited. The other branch is commonly spoken of as physical geography, but it is more correctly the science of geography.

The progress of geography has thus advanced from first rough ideas of relative distance between neighbouring places, to correct views of the earth's form, precise determinations of position, and accurate delineations of the surface. The first impressions of the differences observed between distant countries were at length corrected by the perception of similarities no less real. The characteristics of the great regions of polar cold and equatorial heat, of the sea and land, of the mountains and plains, were appreciated; and the local variations of season and climate, of wind and rain, were more or less fully ascertained. Later, the distribution of plants and animals, their occurrence in groups of peculiar structure in various regions, and the circumstances under which such groups vary from place to place, gave rise to fresh conceptions. Along with these facts were observed the peculiarities of the races of men—their physical form, languages, customs, and history—exhibiting on the one hand striking differences in different countries, but on the other often connected by a strong stamp of similarity over large areas.

By the gradual accumulation and classification of such knowledge the scientific conception of geographical unity and continuity was at length formed, and the conclusion established that while each different part of the earth's surface has its special characteristics, all animate and inanimate nature constitutes one general system, and that the particular features of each region are due to the operation of universal laws acting under varying local conditions. It is upon such a conception that is now brought to bear the doctrine, very generally accepted by the naturalists of our own country, that each successive phase of the earth's history, for an indefinite period of time, has been derived from that which preceded it, under the operation of the forces of nature as we now find them; and that, so far as observation justifies the adoption of any conclusions on such subjects, no change has ever taken place in those forces, or in the properties of matter. This doctrine is commonly spoken of as the doctrine of evolution, and it is to its application to geography that I wish to direct your attention.

I desire here to remark that in what I am about to say, I altogether leave on one side all questions relating to the origin of matter, and of the so-called forces of nature which give rise to the properties of matter. In the present state of knowledge such subjects are, I conceive, beyond the legitimate field of physical science, which is limited to discussions directly arising on facts within the reach of observation, or on reasonings based on such facts. It is a necessary condition of the progress of knowledge that the line between what properly is or is not within the reach of human intelligence is ill defined, and that opinions will vary as to where it should be drawn; for it is the avowed and successful aim of science to keep this line constantly shifting by pushing it forward; many of the efforts made to do this are no doubt founded in error, but all are deserving of respect that are undertaken honestly.

The conception of evolution is essentially that of a passage to the state of things which observation shows us to exist now, from some preceding state of things. Applied to geography, that is to say to the present condition of the earth as a whole, it leads up to the conclusion that the existing outlines of sea and land have been caused by modifications of pre-existing oceans and continents, brought about by the operation of forces which are still in action, and which have acted from the most remote past of which we can conceive; that all the successive forms of the surface—the depressions occupied by the waters, and the elevations constituting mountain-chains—are due to these same forces; that these have been set up, first, by the secular loss of heat which accompanied the original cooling of the globe; and second, by the annual or daily gain and loss of heat received from the sun acting on the matter of which the earth and its atmosphere are composed; that all variations of climate are dependent on differences in the condition of the surface; that the distribution of life on the earth, and the vast varieties of its forms, are consequences of contemporaneous or antecedent changes of the forms of the surface and climate; and thus that our planet as we now find it is the result of modifications gradually brought about in its successive stages, by the necessary action of the matter out of which it has been formed, under the influence of the matter which is external to it.

I shall state briefly the grounds on which these conclusions are based.

So far as concerns the inorganic fabric of the earth, that view of its past history which is based on the principle of the persistence of all the forces of nature may be said to be now universally adopted. This teaches that the almost infinite variety of natural phenomena arises from new combinations of old forms of matter, under the action of new combinations of old forms of force. Its recognition has, however, been comparatively recent, and is in a great measure due to the teachings of that eminent geologist, the late Sir Charles Lyell, whom we have lost during the past year.

When we look back by the help of geological science to the more remote past, through the epochs immediately preceding our own, we find evidence of marine animals—which lived, were reproduced, and died,—possessed of organs proving that they were under the influence of the heat and light of the sun; of seas whose waves rose before the winds, breaking down cliffs, and forming beaches of boulders and pebbles; of tides and currents spreading out banks of sand and mud, on which are left the impress of the ripple of the water, of drops of rain, and of the track of animals; and all these appearances are precisely similar to those we observe at the present day as the result of forces which we see actually in operation. Every successive stage, as we recede in the past history of the earth, teaches the same lesson. The forces which are now at work, whether in degrading the surface by the action of seas, rivers, or frosts, and in transporting its fragments into the sea, or in reconstituting the land by raising beds laid out in the depth of the ocean, are traced by similar effects as having continued in action from the earliest times.

Thus pushing back our inquiries we at last reach the point where the apparent cessation of terrestrial conditions such as now exist requires us to consider the relation in which our planet stands to other bodies in celestial space; and vast though the gulf be that separates us from these, science has been able to bridge it. By means of spectroscopic analysis it has been established that the constituent elements of the sun and other heavenly bodies are substantially the same as those of the earth. The examination of the meteorites which have fallen on the earth from the interplanetary spaces, shows that they also contain nothing foreign to the constituents of the earth. The inference seems legitimate, corroborated as it is by the manifest physical connection between the sun and the planetary bodies circulating around it, that the whole solar system is formed of the same descriptions of matter, and subject to the same general physical laws. These conclusions further support the supposition that the earth and other planets have been formed by the aggregation of matter once diffused in space around the sun; that the first consequence of this aggregation was to develop intense heat in the consolidating masses; that the heat thus generated in the terrestrial sphere was subsequently lost by radiation; and that the surface cooled and became a solid crust, leaving a central nucleus of much higher temperature within. The earth's surface appears now to have reached a temperature which is virtually fixed, and on which the gain of heat from the sun is, on the whole, just compensated by the loss by radiation into surrounding space.

Such a conception of the earliest stage of the earth's existence is commonly accepted, as in accordance with observed facts. It leads to the conclusion that the hollows on the surface of the globe occupied by the ocean, and the great areas of dry land, were original irregularities of form caused by unequal contraction; and that the mountains were corrugations, often accompanied by ruptures, caused by the strains developed in the external crust by the force of central attraction exerted during cooling, and were not due to forces directly acting upwards generated in the interior by gases or otherwise. It has recently been very ably argued by Mr. Mallet that the phenomena of volcanic heat are likewise consequences of extreme pressures in the external crust, set up in a similar manner, and are not derived from the central heated nucleus.

There may be some difficulty in conceiving how forces can have been thus developed sufficient to have produced the gigantic changes which have occurred in the distribution of land and water over immense areas, and in the elevation of the bottoms of former seas so that they now form the summits of the highest mountains, and to have effected such changes within the very latest geological epoch. These difficulties in great measure arise from not employing correct standards of space and time in relation to the phenomena. Vast though the greatest heights of our mountains and depths of our seas may be, and enormous though the masses which have been put into motion, when viewed according to a human standard, they are insignificant in relation to the globe as a whole. Such heights and depths (about six miles) on a sphere of ten feet in diameter would be represented on a true scale by elevations and depressions of less than the tenth part of an inch, and the average elevation of the whole of the dry land (about 1,000 feet) above the mean level of the surface would hardly amount to the thickness of an ordinary sheet of paper. The forces developed by the changes of the temperature of the earth as a whole must be proportionate to its dimensions; and the results of their action on the surface in causing elevations, contortions, or disruptions of the strata, cannot be commensurable with those produced by forces having the intensities, or by strains in bodies of the dimensions, with which our ordinary experience is conversant.

The difficulty in respect to the vast extent of past time is perhaps less great, the conception being one with which most persons are now more or less familiar. But I would remind you, that great though the changes in human affairs have been since the most remote epochs of which we have records in monuments or history, there is nothing to indicate that within this period has occurred any appreciable modification of the main outlines of land and sea, or of the condition of climate, or of the general characters of living creatures; and that the distance that separates us from those days is as nothing when compared with the remoteness of past geological ages. No useful approach has yet been made to a numerical estimate of the duration even of that portion of geological time which is nearest to us; and we can say little more than that the earth's past history extends over hundreds of thousands or millions of years.

The solid nucleus of the earth with its atmosphere, as we now find them, may thus be regarded as exhibiting the residual phenomena which have resulted on its attaining a condition of practical equilibrium, the more active process of aggregation having ceased, and the combination of its elements into the various solid, liquid, or gaseous matters found on or near the surface having been completed. During its passage to its present state many wonderful changes must have taken place, including the condensation of the ocean, which must have long continued in a state of ebullition, or bordering on it, surrounded by an atmosphere densely charged with watery vapour. Apart from the movements in its solid crust caused by the general cooling and contraction of the earth, the higher temperature due to its earlier condition hardly enters directly into any of the considerations that arise in connection with its present climate, or with the changes during past time which are of most interest to us; for the conditions of climate and temperature at present, as well as in the period during which the existence of life is indicated by the presence of fossil remains, and which have affected the production and distribution of organised beings, are dependent on other causes, to a consideration of which I now proceed.

The natural phenomena relating to the atmosphere are often extremely complicated and difficult of explanation; and meteorology is the least advanced of the branches of physical science. But sufficient is known to indicate, without possible doubt, that the primary causes of the great series of phenomena, included under the general term climate, are the action and reaction of the mechanical and chemical forces set in operation by the sun's heat, varied from time to time and from place to place, by the influence of the position of the earth in its orbit, of its revolution on its axis, of geographical position, elevation above the sea-level, and condition of the surface, and by the great mobility of the atmosphere and the ocean.

The intimate connection between climate and local geographical conditions is everywhere apparent; nothing is more striking than the great differences between neighbouring places where the effective local conditions are not alike, which often far surpass the contrasts attending the widest separation possible on the globe. Three or four miles of vertical height produce effects almost equal to those of transfer from the equator to the poles. The distribution of the great seas and continents give rise to periodical winds—the trades and monsoons—which maintain their general characteristics over wide areas, but present almost infinite local modifications, whether of season, direction, or force. The direction of the coasts and their greater or less continuity greatly influence the flow of the currents of the ocean; and these, with the periodical winds, tend on the one hand to equalise the temperature of the whole surface of the earth, and on the other to cause surprising variations within a limited area. Ranges of mountains, and their position in relation to the periodical or rain-bearing winds, are of primary importance in controlling the movements of the lower strata of the atmosphere, in which, owing to the laws of elastic gases, the great mass of the air and watery vapour are concentrated. By their presence they may either constitute a barrier across which no rain can pass, or determine the fall of torrents of rain around them. Their absence or their unfavourable position, by removing the causes of condensation, may lead to the neighbouring tracts becoming rainless deserts.

The difficulties that arise in accounting for the phenomena of climate on the earth as it now is, are naturally increased when the attempt is made to explain what is shown by geological evidence to have happened in past ages. The disposition has not been wanting to get over these last difficulties by invoking supposed changes in the sources of terrestrial heat, or in the conditions under which heat has been received by the earth, for which there is no justification in fact, in a manner similar to that in which violent departures from the observed course of nature have been assumed to account for some of the analogous mechanical difficulties.

Among the most perplexing of such climatal problems are those involved in the former extension of glacial action of various sorts over areas which could hardly have been subject to it under existing terrestrial and solar conditions; and in the discovery, conversely, of indications of far higher temperatures at certain places than seems compatible with their high latitudes; and in the alternations of such extreme conditions. The true solution of these questions has apparently been found in the recognition of the disturbing effects of the varying eccentricity of the earth's orbit, which, though inappreciable in the comparatively few years to which the affairs of men are limited, become of great importance in the vastly increased period brought into consideration when dealing with the history of the earth. The changes of eccentricity of the orbit are not of a nature to cause appreciable differences in the mean temperature either of the earth generally or of the two hemispheres; but they may, when combined with changes of the direction of the earth's axis caused by the precession of the equinoxes and nutation, lead to exaggeration of the extremes of heat and cold, or to their diminution; and this would appear to supply the means of explaining the observed facts, though doubtless the detailed application of the conception will long continue to give rise to discussions. Mr. Croll, in his book entitled "Climate and Time," has recently brought together with much research all that can now be said on this subject; and the general correctness of that part of his conclusions which refers to the periodical occurrence of epochs of greatly increased winter cold and summer heat in one hemisphere, combined with a more equable climate in the other, appears to me to be fully established.

These are the considerations which are held to prove that the inorganic structure of the globe through all its successive stages —the earth beneath our feet, with its varied surface of land and sea, mountain and plain, and with its atmosphere which distributes heat and moisture over that surface—has been evolved as the necessary result of the original aggregation of matter at some extremely remote period, and of the subsequent modification of that matter in condition and form under the exclusive operation of invariable physical forces.

From these investigations we carry on the inquiry to the living creatures found upon the earth; what are their relations one to another, and what to the inorganic world with which they are associated?

This inquiry first directed to the present time, and thence carried backwards as far as possible into the past, proves that there is one general system of life, vegetable and animal, which is coextensive with the earth as it now is, and as it has been in all the successive stages of which we obtain a knowledge by geological research. The phenomena of life, as thus ascertained, are included in the organisation of living creatures, and their distribution in time and place. The common bond that subsists between all vegetables and animals is testified by the identity of the ultimate elements of which they are composed. These elements are carbon, oxygen, hydrogen, and nitrogen, with a few others in comparatively small quantities; the whole of the materials of all living things being found among those that compose the inorganic portion of the earth.

The close relation existing between the least specialised animals and plants, and between these and organic matter not having life, and even with inorganic matter, is indicated by the difficulty that arises in determining the nature of the distinctions between them. Among the more highly developed members of the two great branches of living creatures, the well-known similarities of structure observed in the various groups indicate a connection between proximate forms which was long seen to be akin to that derived through descent from a common ancestor by ordinary generation.

The facts of distribution show that certain forms are associated in certain areas, and that as we pass from one such area to another the forms of life change also. The general assemblages of living creatures in neighbouring countries easily accessible to one another, and having similar climates, resemble one another; and much in the same way, as the distance between areas increases, or their mutual accessibility diminishes, or the conditions of climate differ, the likeness of the forms within them becomes continually less apparent. The plants and animals existing at any time in any locality tend constantly to diffuse themselves around that local centre, this tendency being controlled by the conditions of climate, &c., of the surrounding area, so that under certain unfavourable conditions diffusion ceases.

The possibilities of life are further seen to be everywhere directly influenced by all external conditions, such as those of climate, including temperature, humidity, and wind; of the length of the seasons and days and nights; of the character of the surface, whether it be land or water, and whether it be covered by vegetation or otherwise; of the nature of the soil; of the presence of other living creatures, and many more. The abundance of forms of life in different areas (as distinguished from number of individuals) is also found to vary greatly, and to be related to the accessibility of such areas to immigration from without; to the existence, within or near the areas, of localities offering considerable variations of the conditions that chiefly affect life; and to the local climate and conditions being compatible with such immigration.

For the explanation of these and other phenomena of organisation and distribution, the only direct evidence that observation can supply is that derived from the mode of propagation of creatures now living; and no other mode is known than that which takes place by ordinary generation, through descent from parent to offspring.

It was left for the genius of Darwin to point out how the course of nature, as it now acts in the reproduction of living creatures, is sufficient for the interpretation of what had previously been incomprehensible in these matters. He showed how propagation by descent operates subject to the occurrence of certain small variations in the offspring, and that the preservation of some of these varieties to the exclusion of others follows as a necessary consequence when the external conditions are more suitable to the preserved forms than to those lost. The operation of these causes he called Natural Selection. Prolonged over a great extent of time, it supplies the long-sought key to the complex system of forms either now living on the earth, or the remains of which are found in the fossil state, and explains the relations among them, and the manner in which their distribution has taken place in time and space.

Thus we are brought to the conclusion that the directing forces which have been efficient in developing the existing forms of life from those which went before them, are those same successive external conditions including both the forms of land and sea, and the character of the climate, which have already been shown to arise from the gradual modification of the material fabric of the globe as it slowly attained to its present state. In each succeeding epoch, and in each separate locality, the forms preserved and handed on to the future were determined by the general conditions of surface at the time and place; and the aggregate of successive sets of conditions over the whole earth's surface has determined the entire series of forms which have existed in the past, and have survived till now.

As we recede from the present into the past, it necessarily follows, as a consequence of the ultimate failure of all evidence as to the conditions of the past, that positive testimony of the conformity of the facts with the principle of evolution gradually diminishes, and at length ceases. In the same way positive evidence of the continuity of action of all the physical forces of nature eventually fails. But inasmuch as the evidence, so far as it can be procured, supports the belief in this continuity of action, and as we have no experience of the contrary being possible, the only justifiable conclusion is, that the production of life must have been going on as we now know it, without any intermission, from the time of its first appearance on the earth.

These considerations manifestly afford no sort of clue to the origin of life. They only serve to take us back to a very remote epoch, when the living creatures differed greatly in detail from those of the present time, but had such resemblances to them as to justify the conclusion that the essence of life then was the same as now; and through that epoch into an unknown anterior period, during which the possibility of life, as we understand it, began, and from which has emerged in a way that we cannot comprehend, matter with its properties, bound together by what we call the elementary physical forces. There seems to be no foundation in any observed fact for suggesting that the wonderful property which we call life appertains to the combinations of elementary substances in association with which it is exclusively found, otherwise than as all other properties appertain to the particular forms or combinations of matter with which they are associated. It is no more possible to say how originated or operates the tendency of some sorts of matter to take the form of vapours, or fluids, or solid bodies, in all their various shapes, or for the various sorts of matter to attract one another or combine, than it is to explain the origin in certain forms of matter of the property we call life, or the mode of its action. For the present, at least, we must be content to accept such facts as the foundation of positive knowledge, and from them to rise to the apprehension of the means by which nature has reached its present state, and is advancing into an unknown future.

These conceptions of the relations of animal and vegetable forms to the earth in its successive stages lead to views of the significance of type (*i.e.* the general system of structure running through various groups of organised beings) very different from those under which it was held to be an indication of some occult power directing the successive appearance of living creatures on the earth. In the light of evolution, type is nothing more than the direction given to the actual development of life by the surface conditions of the earth, which have supplied the forces that controlled the course of the successive generations leading from the past to the present. There is no indication of any adherent or pre-arranged disposition towards the development of life in any particular direction. It would rather appear that the actual face of nature is the result of a succession of apparently trivial incidents, which by some very slight alteration of local circumstances might often, it would seem, have been turned in a different direction. Some otherwise unimportant difference in the constitution or sequence of the substrata at any locality might have determined the elevation of mountains where a hollow filled by the sea was actually formed, and thereby the whole of the climatal and other conditions of a large area would have been changed, and an entirely different impulse given to the development of life locally, which might have impressed a new character on the whole face of nature.

But further, all that we see or know to have existed upon the earth has been controlled to its most minute details by the original constitution of the matter which was drawn together to form our planet. The actual character of all inorganic substances, as of all living creatures, is only consistent with the actual constitution and proportions of the various substances of which the earth is composed. Other proportions than the actual ones in the constituents of the atmosphere would have required an entirely different organisation in all air-breathing animals, and probably in all plants. With any considerable difference in the quantity of water either in the sea or distributed as vapour, vast changes in the constitution of living creatures must have been involved. Without oxygen, hydrogen, nitrogen, or carbon, what we term life would have been impossible. But such speculations need not be extended.

The substances of which the earth is now composed are identical with those of which it has always been made up; so far as is known it has lost nothing and has gained nothing, except what has been added in extremely minute quantities by the fall of meteorites. All that is or ever has been upon the earth is part of the earth, has sprung from the earth, is sustained by the earth, and returns to the earth; taking back thither what it withdrew, making good the materials on which life depends, without which it would cease, and which are destined again to enter into new forms, and contribute to the ever onward flow of the great current of existence.

The progress of knowledge has removed all doubt as to the relation in which the human race stands to this great stream of life. It is now established that man existed on the earth at a period vastly anterior to any of which we have records in history or otherwise. He was the contemporary of many extinct mammalia at a time when the outlines of land and sea, and the conditions of climate over large parts of the earth, were wholly different from what they now are, and our race has been advancing towards its present condition during a series of ages for the extent of which ordinary conceptions of time afford no suitable measure. These facts have, in recent years, given a different direction to opinion as to the manner in which the great groups of mankind have become distributed over the areas where they are now found; and difficulties once considered insuperable become soluble when regarded in connection with those great alterations of the outlines of land and sea which are shown to have been going on up to the very latest geological periods. The ancient monuments of Egypt, which take us back perhaps 7,000 years from the present time, indicate that when they were erected the neighbouring countries were in a condition of civilisation not very greatly different from that which existed when they fell under the dominion of the Romans or Mahometans hardly 1,500 years ago; and the progress of the population towards that condition can hardly be accounted for otherwise than by prolonged gradual transformations going back to times so far distant as to require a geological rather than an historical standard of reckoning.

Man, in short, takes his place with the rest of the animate world, in the advancing front of which he occupies so conspicuous a position. Yet for this position he is indebted not to any exclusive powers of his own, but to the wonderful compelling forces of nature which have lifted him entirely without his knowledge, and almost without his participation, so far above the animals of whom he is still one, though the only one able to see or consider what he is.

For the social habits essential to his progress, which he possessed even in his most primitive state, man is without question dependent on his ancestors, as he is for his form and other physical peculiarities. In his advance to civilisation he was insensibly forced, by the pressure of external circumstances, through the more savage condition, in which his life was that of the hunter, first to pastoral and then to agricultural occupations. The requirements of a population gradually increasing in numbers could only be met by a supply of food more regular and more abundant than could be provided by the chase. But the possibility of the change from the hunter to the shepherd or herdsman rested on the antecedent existence of animals suited to supply man with food, having gregarious habits, and fitted for domestication, such as sheep, goats, and horned cattle; for their support the social grasses were a necessary preliminary, and for the growth of these in sufficient abundance land naturally suitable for pasture was required. A further evasion of man's growing difficulty in obtaining sufficient food was secured by aid of the cereal grasses, which supplied the means by which agriculture, the outcome of pastoral life, became the chief occupation of more civilised generations. Lastly, when these increased facilities for providing food were in turn overtaken by the growth of the population, new power to cope with the recurring difficulty was gained through the cultivation of mechanical arts and of thought, for which the needful leisure was for the first time obtained when the earliest steps of civilisation had removed the necessity for unremitting search after the means of supporting existence. Then was broken down the chief barrier in the way of progress, and man was carried forward to the condition in which he now is.

It is impossible not to recognise that the growth of civilisation, by aid of its instruments, pastoral and agricultural industry, was the result of the unconscious adoption of defences supplied by what was exterior to man, rather than of any truly intelligent steps taken with forethought to attain it; and in these respects man, in his struggle for existence, has not differed from the humbler animals or from plants. Neither can the marvellous ultimate growth of his knowledge, and his acquisition of the power of applying to his use all that lies without him, be viewed as differing in anything but form or degree from the earlier steps in his advance. The needful protection against the foes of his constantly increasing race—the legions of hunger and disease, infinite in number, ever changing their mode of attack or springing up in new shapes—could only be attained by some fresh adaptation of his organisation to his wants, and this has taken the form of that development of intellect which has placed all other creatures at his feet and all the powers of nature in his hand.

The picture that I have thus attempted to draw presents to us our earth carrying with it, or receiving from the sun or other external bodies, as it travels through celestial space, all the materials and all the forces by help of which are fashioned whatever we see upon it. We may liken it to a great complex living organism, having an inert substratum of inorganic matter on which are formed many separate organised centres of life, but all bound up together by a common law of existence, each individual part depending on those around it, and on the past condition of the whole. Science is the study of the relations of the several parts of this organism one to another, and of the parts to the whole. It is the task of the geographer to bring together from all places on the earth's surface the materials from which shall be deduced the scientific conception of nature. Geography supplies the rough blocks wherewith to build up that grand structure towards the completion of which science is striving. The traveller, who is the journeyman of science, collects from all quarters of the earth observations of fact, to be submitted to the research of the student, and to provide the necessary means of verifying the inductions obtained by study or the hypotheses suggested by it. If therefore travellers are to fulfil the duties put upon them by the division of scientific labour, they must maintain their knowledge of the several branches of science at such a standard as will enable them thoroughly to apprehend what are the present requirements of science, and the classes of fact on which fresh observation must be brought to bear to secure its advance. Nor does this involve any impracticable course of study. Such knowledge as will fit a traveller for usefully participating in the progress of science is now placed within the reach of everyone. The lustre of that energy and self-devotion which characterise the better class of explorers will not be dimmed by joining to it an amount of scientific training which will enable them to bring away from distant regions enlarged conceptions of other matters besides mere distance and direction. How great is the value to science of the observations of travellers endowed with a share of scientific instruction is testified by the labours of many living naturalists. In our days this is especially true; and I appeal to all who desire to promote the progress of geographical science as explorers, to prepare themselves for doing so efficiently, while they yet possess the vigour and physical powers that so much conduce to success in such pursuits.

FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THERE seems to have been few papers of striking importance read at the Nantes meeting of this Association, though the large number and the solid character of most of the papers show that the scientific activity of France continues to be well sustained. The following are some of the principal papers read in the various sections:—

Zoology.—M. Bureau presented some very interesting observations on the *Aquila pennata*, Brehm and Briss, which he has had the opportunity of closely observing. He is convinced that all the varieties belong to two types, which he has named the white and the black types. Pairs belong sometimes to one type, very often to two different types; generally the young are completely black or completely white.

M. Giard gave an account of his researches on some controverted points in the embryogeny of the Ascidians, more especially *Molgula socialis*, which he has studied in the zoological laboratory of Wimereux. He has been able to supplement and correct in several respects the conclusions of previous observers. M. Giard also, after long research into the embryogeny of animals belonging to the various classes into which Cuvier divided the *Articulata* and *Mollusca*, proposed another limitation of these two groups. Another paper by the same was concerned with the embryogeny of the pectinibranchiate *Gasteropoda*.

Prof. Sirodot described in detail the results of his researches on Elephants. M. Sirodot remarked that, having had at his disposal a very large number of teeth, he had been able not only to correct the errors committed by Falconnet and De Blainville, but, moreover, to feel confident that the different species of *Elephas* hitherto described as closely allied to the Mammoth have no value whatever. There are a multitude of intermediate forms connecting the *Elephas primigenius* with *Elephas indicus*.

M. Lortet, while in Syria, made some investigations into the organisation and reproduction of fibrous sponges. He has been able to prove the presence and to follow the formation of the male and the female egg. Apart from these genital products, he did not meet, in the sponges which he examined, any other cellular element. M. Lortet did not observe, moreover, any canals running into the great canal of the ovule, canals referred to by a large number of zoologists. M. Lortet also described his observations on the very peculiar fauna of the Lake of Tiberias. This fauna appears to indicate a former communication between the waters of the lake and those of the sea.

Physics.—M. Cornu indicated a very simple process for determining with accuracy the focal distance and the principal points of lenses.

M. Merget explained the very interesting results of his researches on the thermo-diffusion of porous and humid pulverulent bodies. A thermo-diffuser is generally a porous vessel, filled with an inert powder, in the middle of which is a glass tube or a metallic tube riddled with holes. On heating such an apparatus, after having moistened it, steam is disengaged in abundance through the porous substance, while dry air traverses the apparatus in an inverse direction, and escapes by the tube. If this escape be prevented, there is produced a pressure which reached three atmospheres at a dull red heat. If the pulverulent mass or the porous body ceases to be moist, no gas escapes. The author did not explain the fact, but he showed that the explanation of it given by M. De la Rive cannot be accepted. M. Merget is convinced that there is here a thermo-dynamic phenomenon. Thermo-diffusion must play an important part in the gaseous exchanges of vegetable life; the author showed this by taking a leaf of *Nelumbium* as a thermo-diffuser.

M. Gripon communicated to the Section and repeated various experiments which he had performed with films of collodion. In receiving upon a Savart polariscope light polarised by a lamina of collodion, we have there systems of fringes, one normal, the other due to phenomena of secondary interference. By illuminating a film of collodion with the light reflected by a second film, we easily obtain fringes of interference, as in the experiment of Brewster. Collodion films are very diathermanous for luminous heat; they are less so for dark heat.

M. Mascart showed some very curious experiments on the condensation resulting from the expansion of moist air. If a little water is placed in the bottom of a perfectly clean flask, closed by a glass tube terminated by an indiarubber syphon bag, we have a closed space, which soon becomes saturated with moisture. By pressing on the bag the temperature rises, and there can be no condensation. But by allowing the bag to resume, by its elasticity, its original form, the air expands, is consequently cooled, and, contrary to what is usually observed, no condensation takes place. To produce the condensation ordinarily observed, it is sufficient to introduce into the flask some unfiltered air, while filtered air produces no effect. In the same way very beautiful clouds are obtained by introducing a little tobacco smoke, or gases resulting from any kind of combustion. These experiments may be of some use in explaining the formation of clouds.

M. Deprez presented an ingenious electric chronograph, intended to estimate by the graphic method intervals of time extremely small, as the duration of a shock.

M. Cornu explained his experiments on the rate of light, by the method of M. Fizeau. (See NATURE, vol. xi. p. 274).

Dr. Moreau explained some points in his investigations on the swim-bladder of fishes, and showed particularly that in proportion as a fish sinks the effort which it must make diminishes.

M. Dufet read a paper on his researches into the electric conductibility of pyrites.

In the Section of *Geology* and *Mineralogy*, most of the papers referred to local topics. Of those of general interest we mention the following:—M. Henry Dufet described his experiments on the thermic conductibility of certain schistose rocks, from which he drew some interesting conclusions regarding the deformations of the fossils contained in such rocks. M. Charles Vélain read a paper on his exploration of the islands of St. Paul and Amsterdam, while on the expedition for observing the Transit of Venus. M. Lory presented some considerations on the dislocation of rocks in mountainous countries.

Botany.—In this section M. Sirodot gave an account of his researches on the classification and development of *Batrachospermum*, and M. de Lanessan spoke on the floral organogeny of *Zostera*.

M. J. Chatin described the results of his histological and histogenic researches on the interior leaf glands and some analogous productions. After having studied the mode of formation of the structure of these various organs in many families, he draws the following conclusions:—1. The interior leaf glands originate always in the mesophyll. 2. These glands are formed by differentiation from a cellule in which multiplication by division is rapidly produced, so that except in some Lauraceæ the gland is always formed, in its perfect state, from a cellular mass, more or less considerable. 3. The products of secretion are constantly forming in the cellules proper of the gland. 4. The elements of the latter are re-absorbed from the centre to the circumference, and thus form a reservoir where the product of secretion is amassed. 5. In certain plants, and by an analogous phenomenon, there may be formed in the leaf true secreting canals. 6. The leaf-glands are almost constantly situated in the vicinity of the fibro-muscular bundles. 7. In many plants there exist at different points of the stalk, of the branches, and of the petioles, certain productions on the whole comparable to the interior leaf-glands.

M. Merget gave the result of his researches on the interchange of gases between plants and the atmosphere. He concluded with the following statements:—1. The means by which the interchange of gases is effected in plants are the stomata and accidental openings; it is by diffusion in the stomata, and not by dialysis through the cuticle, that exterior gases penetrate into the interior of a plant, and that internal gases escape. 2. The entry of atmospheric gases is due to the action of the physical force produced by the phenomena of gaseous thermo-diffusion. M. Merget concluded by some interesting details on the function of chlorophyll.

M. Baillon read a very interesting communication on the Amentaceæ.

In the Section of *Anthropology*, we note the following papers:—Dr. Lagneau read a careful and elaborate memoir on the ethnogeny of the populations of the N. W. of France, in which he reviewed the various peoples which have contributed to the formation of the former and present population of the region comprised between the sea, the Saône, and the Loire.—M. Chauvet read a report relative to the excavations undertaken by the Archæological Society of Charente, in the tumuli on a woody plateau near a Roman road, and entered into details of a nature to clear up certain controverted points of prehistoric archæology. From the objects found in these explorations, M. Chauvet develops a doctrine according to which there was no gap between the various civilisations from an industrial point of view.

As usual, a very large number of papers belong to the Section of Medical Sciences; some of these are of more than merely technical interest, but our space prevents us from referring to them in detail. A full report of the proceedings will be found in the *Revue Scientifique* for August 28 and following weeks.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.—DETROIT MEETING

THE American Association for the Advancement of Science held its twenty-fourth annual meeting at Detroit, Mass., from Aug. 11 to 17 inclusive. Some of its previous meetings have surpassed this one in respect to the number of members present, but none can be regarded as superior to it in the general excellence of the communications presented. The causes of the slight falling off in attendance may be briefly mentioned. The cities of the Atlantic sea-board where local scientific societies have been longest in existence, and where a large proportion of the membership of the Association is resident, are 750 to 1,000 miles, chiefly eastward, from Detroit. That city, on the boundary line between the United States and Canada, is also considerably to the northward of the larger centres of population in the Western States. Thus, then, the assembling at Detroit required, in the great majority of instances, a long, tedious, and rather expensive journey. It need not be concealed that, owing to the widespread effects of the depression in all branches of business in the United States—extending even to the learned professions—the pecuniary means of members were in many cases more restricted than usual; and this fact in many cases decided adversely the question of attendance at the meeting.

A protracted series of discussions in that and previous years resulted at the meeting of 1874 in the Association's adopting a new constitution, which first displayed its general effects at Detroit. The two prominent features of change were modelled upon the system of the British Association. A division was made between Fellows and the rest of the members, prominence or usefulness in science being required for election to the honours of Fellowship. This elective process did not, however, apply to the Fellows who became such between the meetings at Hartford and Detroit, and consequently many have been admitted to the dignity who have no claim to it by scientific labours. To the Fellows rather than to the general membership, the guidance and management of the Association is confided. The effect of this change was very apparent at Detroit in the exclusion of a large number of communications which would easily have passed the ordeal of committees and been read at the meetings of previous years. The chosen remainder reached a higher average of excellence than has been hitherto attained, and in the section of Physics, Mathematics, and Chemistry, the weeding process so reduced the number of communications that the supply gave out before the close of the meeting; but this may also be accounted for by the fact that the sub-section of Chemistry, for the first time organised and separately at work, much facilitated the dispatch of business in Section A. A variety of concurrent causes presented a like result from being reached in Section B, devoted to Geology and Biology. The geologists are always largely in force when the Association meets west of the Alleghanies, the development of the mining resources of the newer States and Territories rendering their labours of immediate economic interest and value. There was an extraordinary accession of ethnological papers, prompted chiefly by numerous discoveries recently made in new and very thorough explorations of Indian mounds. The great injuries which the food crops of the United States have suffered from insects within a year or two, called forth several papers of merit from the leading entomologists, as well as much debate and some action on the part of the Association. Besides all the foregoing subjects, there was an unusual number of papers on specific investigations in natural history. These were largely the fruit of the seed sown at the Anderson School on Penikese Island, by the lamented Agassiz. The pupils there instructed, mostly for the first time, in observing the habits of animals, dissecting their forms and studying their differences, were from all parts of the Union. Nearly all of them are teachers in high schools and the smaller colleges. Having been thus started on the path of original investigation, they already find something new to relate, and their papers had a charm of freshness, very different from those of older members who have found their own easier grooves of thought and lapsed into routine.

Another important feature introduced at this year's meeting by the new constitution, resulted from the election of two vice-presidents, who were the presiding officers respectively of Sections A and B. Following in this respect the system of the British Association, each of these officers opened his Section with an address, in which a department of science was made the subject of a broad survey. Hitherto the address of the retiring President has been the only one at each meeting of this character; the change gives two such addresses in addition, and may in future years give a greater number. At the Detroit meeting the address of Prof. John L. Le Conte, of Philadelphia, the retiring President, brought forward in a general way the aid to a knowledge of past conditions on the globe, which might be derived from a study of existing forms. Prof. Le Conte's own lines of investigation have been more especially confined to the study of insects, and from the facts thus derived he drew most of his illustrations. He regards organic life as furnishing everywhere evidences of design, and a principal portion of the address was devoted to deprecating the conflict between science and religion, and to urging patience rather than controversy. Prof. H. A. Newton, the astronomer, of Yale College, delivered the opening address of Section A. He urged the study of pure mathematics as a basis for work in all the sciences; adducing, through a wide range of illustration, the evidences of its value in advancing knowledge. The want of a thorough knowledge of the higher mathematics he regarded as a frequent defect among American men of science, while their dependence upon mathematical methods in all branches of investigation was every day becoming more absolute.

The address of Prof. J. W. Dawson, Principal of McGill College, Montreal, before Section B, was one of the most important given at the meeting. He is well known as the most able and prominent anti-Darwinian in America. His address took the form of a discussion of the question, "What do we know of the origin and history of life on our planet?" Space will not permit an analysis of this address, which reviewed the evidence furnished by the Silurian fossils at great length, regarding it as inconclusive when applied to the support of evolution theories. Prof. Dawson vigorously opposed the hypothesis that organic life is a product of mere physical forces.

Thus the weight of utterance in two of the addresses is adverse to Darwinian theories, but this is no index to the general sentiment of the leading students of biology in the Association. The officers chosen for next year include names noted in connection with the advocacy of the most advanced evolutionary doctrines. The venerable President-elect, Prof. Wm. B. Rogers, of Boston, was, in years gone by, the most successful antagonist, in discussions of the new theories, that Prof. Agassiz encountered in America. Prof. Edward S. Morse, Vice-president-elect, of Section B, has attained prominence in the expression of strong Darwinian views before large popular audiences in almost every city of the United States. Prof. Charles A. Young, of Dartmouth College, well known by his spectroscopic researches on the sun's chromosphere, was elected Vice-president to preside over Section A. It is a somewhat remarkable circumstance that six out of eight of the officers for next year are residents of the New England States, the three highest positions falling to their share. The citizens of Detroit did everything in their power to make the visit of the Association pleasant. Several social entertainments and excursions by boat and rail were provided, and the Detroit Scientific Association aided materially in these hospitalities. The next meeting will be held August 23, 1876, at Buffalo.* W. C. W.

* Next week we shall refer to some of the principal papers in detail.

NOTES

WE have received from the Central Meteorological Institute of Sweden the Daily Weather Charts published by the Office for the months of January, February, March, and April last. These charts, constructed from data supplied from nine stations in Sweden, nine in the British Isles, four in Norway, two in Denmark, and four in Russia, including Arkangel, are valuable additions to the daily weather literature of Europe, and supply important data, showing more particularly the influence of the Scandinavian mountains and of the Baltic at different seasons on European storms, and the influence of the systems of high and low pressures over the Baltic and neighbouring regions on the weather of Great Britain at the time.

IN the *Bulletin Hebdomadaire* of the Scientific Association of France for September 5, Prof. V. Raulin, after referring in strong, but not too strong, terms to the practical neglect with which the investigation of inundations has been treated in the south-west of France, energetically urges the organising of Hydrometric Commissions similar to that of Lyons, to collect together observations of the rainfall and heights of the rivers, and compare and discuss them with the view of deducing therefrom the laws which rule the commencement, development, and progress down the several river basins, of ordinary floods, but more particularly of those great inundations which prove so disastrous to life and property. He recommends the formation of Hydrometric Commissions at Bordeaux for the basin of the Gironde; at Libourne, for the basin of the Adour; and at Carcassonne or Narbonne, for the basin of the Aude. When the enormous saving to life and property which would have been effected through such organisations, had they existed, is considered, during the late deplorable inundation, we cannot for a moment doubt that Hydrometric Commissions similar to that of Lyons will at once be organised in the basins of the Garonne and its affluents.

THE annual Provincial Congress of the Iron and Steel Institute was opened in the Owens College, Manchester, on Tuesday, Mr. William Menelaus, the President of the Institute, in the chair. The Mayors of Manchester and Salford and the Bishop of Manchester were present by invitation, at the opening proceedings, and the more distinguished members of the Institute present included Mr. Henry Bessemer, Sir Joseph Whitworth, Mr. J. Lowthian Bell, and Mr. Crawshay. The Bishop of Manchester gave a very happy address. Referring to the fact that the Duke of Devonshire is an ordinary member of the Institute, one indication among others that the Duke is a man of high scientific attainments in the department of science with which the Institute is connected, the Bishop said that what struck him was how the old order had changed, "giving place to the new," and he was rather inclined to think the new order perhaps somewhat better than the old. The local authorities and the leading industrial firms in Manchester and the surrounding districts have done their part towards rendering the meeting a success. On Tuesday evening the Reception Committee received the members of the Institute at a *conversazione* in the Town Hall, and last evening the members dined at Hulme Town Hall. A large part of the time of the meeting will be spent in visits to places of industrial interest in Manchester and neighbourhood.

THE proposed University College for Bristol received some impulse from the members of the British Association at a meeting held last week. Sir John Hawkshaw said foreign industrial competition with England was a very real thing, and would soon be much greater unless scientific education was fostered. Sir W. Thomson begged the promoters not to starve the literary department, and Prof. Balfour Stewart said that would not be any departure from science, for there was now a science of culture and literature. Prof. Jowett said that the appointment of the first professors would be the most critical event in the history of the College, for on their force of character depended the creation of the College out of nothing. Although not more than about 20,000*l*. has been already promised, it is intended to commence operations soon, in the belief that practical successful working will eventually bring in all the funds that are required.

LAST Saturday evening the Brothers Henry, the great French asteroid finders, visited the equatorial buildings of the Paris Observatory, under the guidance of M. Leverrier. Along with them was Mr. Watson, the celebrated American astronomer, who has himself discovered no fewer than nineteen small planets. Mr. Watson was the head of the American Transit Expedition to Pekin.

THE death of M. de Rémusat renders almost certain the election of M. Dumas to fill the place vacated by the demise of M. Guizot in the French Academy. It is not only that M. Rémusat voted for M. Jules Simon and that the votes were equal, when the election was postponed for six months, but M. Jules Simon has desisted from his candidature, and intends come forward for the seat of his friend Rémusat.

AT the Radcliffe Observatory, Oxford, on Sept. 3, 9h. 55m. Greenwich mean time, a meteor was observed about three times the apparent magnitude of Jupiter, proceeding from Saturn downwards about twelve degrees, in the direction of δ Piscis Australis. Colour, blue to green; time visible, five seconds. At disappearance it threw off a piece about the apparent size of Saturn.

THE Geological Society of France held a congress at Geneva last week, and visited some of the places most interesting to geologists in that part of Switzerland.

BARON FERDINAND VON MUELLER, of Melbourne, has just published a second supplement to his previous lists of "Select Plants readily eligible for Victorian Industrial Culture." These lists of Baron Mueller's are useful to a certain extent, many economic plants being thus brought together, arranged alphabetically under their scientific names, and short descriptions given of their uses. Whether many of them are worth the trouble of cultivation as industrial or economic plants, is a question which the cultivator can only know by experience, but which the botanist will be able to decide upon by a mere glance at the list. Thus we find included *Aloe dichotoma*, the Tree Aloe of Damara and Namaqualand, referred to in NATURE, vol. xi. p. 89; scarcely an industrial plant, we should say. A peculiar and interesting addition to this second supplement is a geographic index, the plants being alphabetically arranged under distinct heads, such as "Northern and Middle Europe," "Countries at or near the Mediterranean Sea," "Middle and Temperate Eastern Asia," &c.

THE coffee plant has been grown in Queensland for some years, but it is only of late that its cultivation has been attempted with a view to its exportation as a commercial article, and we now learn that the plants have become attacked by blight, or fungus, which has given rise to some anxiety and inquiry as to whether the disease is identical with the *Hemileia vastatrix*, which has proved so destructive to coffee plants in Ceylon. We shall probably soon hear more about this, as the subject of the extension of coffee culture in Queensland is about to be taken up by Mr. L. A. Bernays, F.L.S., Clerk of the Legislative Assembly of Queensland, and a vice-president of the Queensland Acclimatisation Society, and who moreover is known as the author of a little work on the cultivation and propagation of the olive in Australia.

THE Literary and Natural History Society of Keswick has commenced the formation, in a small room in the Town Hall, of a collection to illustrate the natural history of the district. They have already got together a considerable number of birds, birds' eggs, fishes, and insects, as well as the commencement of a herbarium; also a collection of the rocks and ores and of the scanty fossil fauna of the neighbourhood. A few very interesting celts and other prehistoric remains have been found in the district, some of them close to the celebrated "Druids' Circle" in the immediate vicinity of Keswick. There is evidently here a rich field for the zeal and energy of the local naturalists and archæologists.

THE *Quarterly Journal of the Meteorological Society*, No. 15, has just been published, containing among other matters papers on a Universal System of Meteorography, by Prof. F. Van Rysselberghe; Results of Meteorological Observations at Patras, Greece, during 1873, by the Rev. H. A. Boys; and Notes on Sea Temperature Observations on the British coasts, by R. H. Scott, F.R.S.

A ZOOLOGICAL collection of remarkable interest, the *Times* states, more particularly to Londoners, has been added during the present year to the British Museum. It consists of the Thames Valley series of remains of British elephants, rhinoceri, deer, ox, &c., which have been discovered in the Ilford Marshes, near Stratford, during the last thirty years, and has hitherto formed the unique private collection of Sir Antonio Brady, of Stratford-le-Point. The nature and value of this collection, as now exhibited at the British Museum, will appear from the following facts:—It contains remains of no less than 100 elephants, all of which have been obtained from Ilford. These are referable to two species, viz., *Elephas primigenius*, the mammoth, and *E. antiquus*, a more southern form. The skeletons of each species are represented by many fine examples, and the collection of teeth and jaws represents elephants of every age and size, from the sucking calf, with milk molars, to the patriarch of the herd, whose last molars are so worn that they must have become useless for grinding his food. One characteristic of the Ilford elephants is the number of the plates in the last molar tooth, which has never been found to exceed nineteen or twenty, as against the twenty-four and sometimes twenty-eight in other species. The largest tooth is ten inches in length. The rhinoceri of the Thames Valley are represented by eighty-six remains, of three species, distinguished by the character or the absence of the bony nasal septum—viz., *Rhinoceros megarhinus*, *R. leptorhinus*, and *R. tichorhinus*. The British lion, which recent geology shows to have been no myth, is represented by a lower jaw and a phalanx of the left forefoot. The Brady collection also includes the Thames Valley hippopotamus, which is found at Grays, as well as at Ilford. The ruminants, such as the stag, bison, and ox, constitute fully one-half the collection, numbering more than 500 specimens. They include seven specimens of the great Irish Elk (*Megaceros hibernicus*) and fifty of the Red Deer.

WE learn from the *Lancet* that the sanitary authorities of Leicester have determined to institute an inquiry into the causes and conditions of the high mortality in that town from diarrhœa, and Dr. Beck and Dr. Frankland have been appointed to carry out the inquiry. It was recently shown in NATURE (vol. xii. p. 281) that the average mortality in Leicester from diarrhœa, and among infants, has far exceeded that of any other large town in England, and that whereas the average highest mortality from diarrhœa in any other large town during any week of the year has not exceeded 10·5 on an annual mortality per 1,000 of the population, in Leicester the average reaches 15·8. This large mortality from diarrhœa has been a characteristic of Leicester each year since the Registrar-General began to publish the returns for Leicester in his weekly reports, the distribution of the deaths during the warm weeks and the number being plainly and directly dependent on the temperature. During the six weeks ending 14th August last the deaths from diarrhœa in Leicester have been 121; during the same six weeks of 1874 when the temperature was higher, the deaths were 156. The peculiarity of the mortality of Leicester lies in this: whilst the rate of its infant and diarrhœa mortality is enormously high, its annual death-rate for the whole population is moderately low, being only 26 per 1,000 of the population; whereas in Liverpool and Manchester it is fully thirty, or one-fifth more. Hence, in commencing a scientific inquiry into the causes and conditions of this great destroyer of the infant life of our large towns, no better beginning could have been made than with Leicester. For reasons stated by Mr. Buchan and Dr. Mitchell in their recently published paper "On the Influence of Weather on Mortality" (Jour. Scot. Met. Soc., vol. iv. p. 232), a separation of the infants that die, or are attacked, into three classes—viz. (1) those nursed at the breast, (2) those fed on cows' milk, and (3) those fed on slops—is most desirable in such inquiries, particularly since facts seem at present to point to the intimate bearing, on this vitally important question, of high summer temperatures on milk exposed to them, especially on the small portion of milk which may be carelessly left in the apparatus used in the case of those infants that are fed on cows' milk.

WE have before us three contributions to American Botany:—1. Conspectus of the North American Hydrophyllaceæ, by Prof. Asa Gray. The genus *Eutoca*, well known under that name to flourish in this country, is here merged in *Phacelia*, which numbers about fifty species. 2. Revision of the genus *Ceanothus*, and descriptions of new plants, by Sereno Watson. 3. Botanical observations in Southern Utah in 1874, by Dr. C. C. Parry; a series of papers reprinted from the *American Naturalist*. The south-western portion of the vast territory of the United States has been for some years one of the most fertile portions of the surface of the earth in yielding new species of plants; very little having been done, before Dr. Parry's visit, since the working up by Torrey and Gray of the results of Col. Fremont's expedition in 1844. A very interesting sketch of the botany of the district is contained in these papers, together with notes of many new species described by Prof. Gray and others.

PROF. PALMIERI has discovered a new instrument which he calls a "diagometer," and which is constructed for the rapid examination of oils and textures by means of electricity. What the apparatus will do, Prof. Palmieri details thus:—1. It will show the quality of olive oil. 2. It will distinguish olive oil from seed oil. 3. It will indicate whether olive oil, although of the best appearance, has been mixed with seed oil. 4. It will show the quality of seed oils. 5. Finally, it will indicate the presence of cotton in silken or woollen textures. The professor has been complimented for this invention by the Chamber of Arts and Commerce at Naples, who have published a full description of the apparatus, with instructions for use.

THE additions to the Zoological Society's Gardens during the past week include an Indian Leopard (*Felis pardus*) from India, presented by Mr. G. Jasper Nicholls; an Arctic Fox (*Canis lagopus*) from the Arctic Regions, presented by Mr. C. R. Wood; a Montagu's Harrier (*Circus cineraceus*), European, presented by Capt. Hadfield; a Lesser Sulphur-crested Cockatoo (*Cacatua sulphurea*) from Moluccas, presented by Mrs. H. M. Smith; a Wrinkled Terrapin (*Clemmys rugosa*) and five American Box Tortoises (*Terrapene carinata*) from Nicaragua, presented by Mr. Edmond Isaacson; a West African Tantalus (*Tantalus ibis*) from West Africa; two Brazilian Tortoises (*Testudo tabulata*) from South America, an Abyssinian Pentonyx (*Pelomedusa gehafi*) from Abyssinia, deposited; an Indian Fruit Bat (*Pteropus medius*) from India, purchased; a Wapiti Deer (*Cervus canadensis*) born in the Gardens.

SCIENTIFIC SERIALS

THE first fascicule of this year's *Bulletin de la Société d'Anthropologie de Paris* gives the new president, M. Dallas' inaugural address, in which he draws attention, amongst other points, to the importance in reference to anthropology of the study of "demography," or that branch of sociology which treats of the influence of prosperity on populations in determining the maxima and minima of births and deaths. After speaking with just pride of the merit due to the Paris Society of having inaugurated the systematic study of anthropology, and of having served as the model for similar institutions in all the great cities of the old and new Continent, the President announced that in consequence of the appointment of two new secretaries, MM. Astézat and Gerard de Rialle, and of a general-assistant secretary, M. Magitot, as well as through the adoption of different rules for the transmission of papers, the publication of the *Bulletins* would no longer be subject to the delay which had of late years marked their appearance. In the discussion which followed M. de Mortillet's paper on the circles drawn on a fragment of a human skull found in the dolmens of Lozère, M. de Leguay took occasion to express his conviction that the men who constructed these megalithic monuments must have been possessed of tools of metal, and provided with textile fabrics such as strong ropes, capable of being used to lift and pull heavy weights. He does not venture to give an opinion as to the probable antiquity of these remains, but he believes that no one acquainted with practical mechanics can attach faith to the commonly accepted theory that these stones have been conveyed from distances and elevated to their present positions by slides or rollers. The speaker, moreover, pointed to the fact that a bronze bracelet of indisputable Gallic fabrication was found below one of the Lozère dolmens; and he is of opinion that the men who erected the latter used iron as well as bronze.—In discussing the human remains belonging to upwards of 200 individuals found by M. de Baye in the Baye caverns on the Marne, among which were skulls having circular lines and perforations similar to those of the Lozère fragment, M. Broca drew attention to the two distinct cranial types which they presented, the one being dolicephalic, while the other was sub-brachycephalic.—Those interested in abnormal types of humanity will find much suggestive matter in several papers referring to the so-called Aztecs introduced into Europe twenty-five years ago, in whom microcephalism—whatever its cause may be—is more strikingly exhibited than in any other known case.—M. Hamy's learned paper on artificially produced microcephalism among the sacerdotal classes of Central America, gave rise to an animated discussion in which Dr. Broca and Madame Royer took part.—Dr. Mondières has laid an interesting report before the society, in which he supplies much hitherto unknown information in regard to the prevalent diseases of the natives of Cochin China, the remedies applied, and the practices resorted to by the bonzes for working pretended miracles. The author describes the physical characteristics of the two distinct races, the Minghuongs and true Cambodians.—M. Broca exhibited the skeleton of a Peruvian mummy-fœtus which had been taken from an ancient cemetery near Callao, laid bare by an earthquake. It was found in the portion of the ground appropriated to infants, and where each little body was tied tightly into a cloth and had enclosed with it a number of minute toy-like vessels, utensils, and arms. The fœtal mummy was examined with special reference to the existence of the supernumary cranial bone, which some Spanish writers affirm to be a characteristic of the Inca race. No such bone could, however, be detected in the Peruvian mummy, whose skull was precisely similar to those of Europeans at the same period of fœtal existence.

THE *Journal de Physique théorique et appliquée* for July contains the following original papers:—On the acoustic theory of beats, by Terguem and Boussinesq.—On the use of collodion films in Physics, by E. Gripon.—On the interior double reflexion of uniaxal crystals, by M. Abria.—A note by M. Henri Becquerel, on the action of magnetism upon the induction spark.—On a new method to produce sonorous vibrations and interferences on mercury, by C. Decharme.—On the channelled space spectra of MM. Fizeau and Foucault, by M. Nodot.

Gazzetta Chimica Italiana (fasc. vi. 1875.)—This number contains the following papers:—Defence of the old theory of electrostatic induction, by G. Pisati.—Chemical dissociation as applied to the interpretation of some volcanic phenomena; analysis and synthesis of a new mineral from Mount Etna, which is of common origin in volcanoes, by Prof. O. Silvestri.—Experimental researches by Dr. L. Pesci, on peroxide of iron as generator of nitric acid, and on the origin of nitre in some experiments of Cloëz.—Chemical and toxicological researches by Dr. C. Bettelli, on oleandrine and so-called pseudocurarine.—On albumen assisting the solution of the tricalcic phosphate of the blood, by M. Mercadante.—On the presence of leucine in vetches, by A. Cossa.

THE *Notizblatt des Vereins für Erdkunde zu Darmstadt*, series iii. heft xiii. contains but one paper of scientific interest, all the rest of the contents being devoted to statistical reports from the central station for statistics of the Grand Duchy of Hessen, and to tables relating to these reports. The paper referred to records the meteorological observations of the Kataster Office at Darmstadt during the whole of the year 1873, and is accompanied by a very elaborate table.

THE *Journal de Physique théorique et appliquée* for August contains the following original treatises:—On double spectra, by M. G. Salet.—Exposition of some experiments relating to the theory of induction, by M. Felici.—On a new method to determine quickly the refractive index of liquids, by MM. Terquem and Trannin.—On a new form of electro-magnet, by M. A. Camacho.—On elliptic polarisation, by L. Mouton.—The remainder of the journal contains extracts and translations from *Poggendorff's Annalen* and from the *American Journal of Science and Arts*.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 30.—M. Frémy in the chair.—The following papers were read:—A note by M. Leverrier on Jupiter's mass and on some new researches on Saturn.—On the formation of hail, by M. Faye.—Tenth note on the electric conductibility of bodies known to be bad conductors, by M. Th. du Moncel.—Report by a commission appointed to examine a memoir by M. Haton de la Goupillière, entitled, Direct and Inverse "developpoids" of successive Orders.—A note by M. J. Künckel, on Lepidoptera with perforating proboscis as destroyers of oranges (Ophidera).—Remarks on the granitic diluvium of plateaus; lithological composition of the caolinic sand of Montainville (Seine et Oise), by M. Stan. Meunier.—On the germination of Chevalier barley, by M. A. Leclerc.—Researches on the ferments contained in plants, by M. C. Kossmann.—A number of communications of minor interest.—On the formation of aniline black, obtained by the electrolysis of its salts, by J. J. Coquillion.—On the development of unfertilised ova of frogs, by M. G. Moquin Tandon.

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