

best. "I have written," says the author, "not for wealthy amateurs, nor for people who do not care to think, but for men and women who have to give up something else to spend a sovereign on their own education. Nearly all the apparatus described in this book can be made by anyone with a few tools and a little finger-skill." In conformity with this laudable desire, technical terms are rarely introduced without being explained, and by simple words and apt illustration the way to electrical knowledge is made as easy and pleasant as it possibly could be. Indeed, popularity of style appears to be the book's sole *raison d'être*, for, with one or two exceptions, the facts described are to be found in a number of elementary text-books. However, it can be said that there are very few, if any, books of the modest dimensions of the one before us in which so much information is imparted in a more popular manner. The descriptions of experiments and principles are easy reading without being diffuse; the hydrostatic and other analogues are numerous, yet they are never used where likely to lead to a misconception. The illustrations, however, are not worthy of the text. They should have been far more numerous and less sketchy in order to appeal to the public for whom the book has been specially designed.

LETTERS TO THE EDITOR.

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The Supposed Glaciation of Brazil.

IN the second volume of NATURE, p. 510, I reviewed the late Prof. Hartt's "Geology and Physical Geography of Brazil," and called attention to the author's views, as well as those of the late Prof. Agassiz, relating to the supposed glaciation of that country. From their very positive statements I concluded that the evidence as described by them did actually exist, and that until it was disproved it should not be ignored. In my "Darwinism," p. 370, I stated, on the authority of my friend, Mr. J. C. Branner, now Professor of Geology in the Stanford University, California, who succeeded Prof. Hartt in Brazil, and had a much more extensive knowledge of the country, that the supposed glacial drift and erratic blocks were all results of subaërial denudation. Recently, however, Sir Henry Howorth has quoted some passages from my review in illustration of the wild and incredible theories of some geologists, as samples, in fact, of the "Glacial Nightmare"; and, as no authoritative disproof has yet been given of the exceedingly strong and positive statements of Agassiz and Hartt, I beg leave to lay before the readers of NATURE some extracts from a paper on "The Supposed Glaciation of Brazil," by Prof. Branner, which will shortly be published, and of which he has kindly sent me a type-written copy in advance. As a partial justification of what has now proved my too hasty acceptance of the statements of these gentlemen, I will give one passage in which Prof. Agassiz refers to the supposed glacial phenomena near Ceara:—"I may say that in the whole valley of Hasli there are no accumulations of morainic materials more characteristic than those I have found here, not even about the Kirchel; neither are there any remains of the kind more striking about the valleys of Mount Desert in Maine, where the glacial phenomena are so remarkable; nor in the valleys of Loch Fine, Loch Awe, and Loch Long, in Scotland, where the traces of ancient glaciers are so distinct." Both Agassiz and Hartt were equally strong as to similar phenomena near Rio.

It is to be first noted that Hartt had only spent eighteen months in Brazil when he wrote his book, and his views on the glacial phenomena were thus based on a very hasty survey of that enormous territory. Prof. Branner went with him when he again visited Brazil in 1874, helped him in his geological work till his death in 1877, and himself remained five years longer making a geological survey of the country; and he states that, before his death, Hartt's views underwent a radical change. Prof. Branner says:—

"Under his direction I did more or less work in the mountains about Rio de Janeiro for the purpose of sifting the evidence of glaciation in that region, and I am glad to say, in justice to the memory and scientific spirit of my former chief and friend, that long before his death he had entirely abandoned the theory of the glaciation of Brazil, and that the subject had ceased to receive further attention, even as a working hypothesis."

A few extracts must now be given showing to what causes the phenomena which deceived these observers are really due. And first as to what were supposed to be erratic boulders often embedded in boulder clay.

"The boulders believed to be erratics are not erratics in the sense implied, though they are not always in place. The first and most common are boulders of decomposition, either rounded or subangular, left by the decay of granite or gneiss. Sometimes they are embedded in residuary, and therefore unstratified, clays, formed by the decomposition in place of the surrounding rock. And everyone has heard of the great depth to which rocks are decomposed in Brazil. The true origin of these boulders and their accompanying clays is often obscured by the 'creep' of the materials, or in hilly districts by land slides, great or small, that throw the whole mass into a confusion closely resembling that so common in the true glacier boulder clays. In this connection too much stress cannot be placed upon the matter of land slides; they are very common in the hilly portions of Brazil, and aside from profound striations and facetting produce phenomena that, on a small scale, resemble glacial till in a very striking degree."

"The second method by which these boulders have been formed is quite similar to the first, but instead of being cores of granite or gneiss, they have been derived by the same process of exfoliation and decomposition from the angular blocks into which the dikes of diorite, diabase, or other dark-coloured rocks break up. Their colour marks them as quite different from the surrounding granites, and the dikes themselves are almost invariably concealed. The residuary clays derived from the decomposition of these dikes are somewhat different in colour from those yielded by the granites, so that when 'creep' or land-slides add their confusion to the original relations of the rocks the resemblance to true glacial boulder clays is pretty strong. The chance of discovering the source of such boulders is further decreased by the depth to which the mass of the rock has decayed, and by the impenetrable jungles that cover the whole country, and so effectually limit the range of one's observations. Dikes, such as these last mentioned, are not uncommon in the mountains about Rio de Janeiro. Indeed, what have generally been regarded as the very best evidences of Brazilian glaciation, some of the boulders near the English hotel at Tijca, fall under this head, though some are of gneiss. The fact is that the great mountain masses about Rio are of granite or gneiss, while some of the boulders come from the dikes of diabase or other dark-coloured rock high on their sides—dikes which were not visited by Agassiz or Hartt."

Prof. Branner then describes a third class of supposed erratic, derived from certain sandstone beds of the tertiary deposits, which, by exposure, change to the hardest kind of quartzite, and when the surrounding strata are removed by denudation, and a few blocks of this quartzite are left, they are so unlike the rocks by which they are surrounded that unless the observer has given a special study to the tertiary sediments, he is liable to be misled by them.

The wide-spread coating of drift-like materials that covers considerable areas of the country, consisting of boulders, cobbles, and gravels, sometimes assorted and sometimes having clay and sand mixed with them, are then described, and are shown to be due to the denudation of the tertiary beds during the last emergence of the land, aided by subsequent subaërial denudation and surface wash. Prof. Branner thus concludes:—

"I may sum up my own views with the statement that I did not see, during eight years of travel and geological observations that extended from the Amazon valley and the coast through the highlands of Brazil and to the head waters of the Paraguay and the Tapagos, a single phenomenon in the way of boulders, gravels, clays, soils, surfaces, or topography, that required to be referred to glaciation."

The very clear statement above given of the real nature of the phenomena which deceived Prof. Agassiz and Mr. Hartt, is very instructive, and it shows us that a superficial resemblance to drift, boulder-clay, and erratic blocks, in a comparatively unknown country, must not be held to be proofs of glaciation.

We require either striated rock surfaces or boulders, or undoubted *roches moutonnées*, or erratics, which can be proved not to exist sufficiently near to have been brought by "creep" or landslides. In view of these liabilities to error, we may be almost sure that the supposed evidences of glaciation described by the late Mr. Belt in his "Naturalist in Nicaragua" (p. 260), are explicable in the same manner as the Brazilian evidences, since he nowhere found glacial striæ or any boulders that could be proved to be true erratics; and this is the more certain because he himself states (p. 265), "I have myself seen, near Pernambuco, and in the province of Maranhão, in Brazil, a great drift deposit that I believe to be of glacial origin."

All students of the past and present history of the earth are indebted to Prof. Branner for having relieved them of a great difficulty—a true glacial nightmare—that of having to explain the recent occurrence of glaciation on a large scale far within the tropics and on surfaces not much elevated above the sea-level.

ALFRED R. WALLACE.

Telegony.

DR. ROMANES' letter inviting a discussion concerning the remarkable phenomenon of telegony will be welcomed by many who have felt that too little notice has been paid by men of science up till now to one of the most obscure problems of heredity. At the conclusion of his remarks, Dr. Romanes rejects Prof. Weismann's hypothesis that sperm elements are capable of penetrating into the ovary, and fertilising immature ova *in situ*, on the ground of their obvious incapability of doing so. It seems, however, possible to doubt whether the spermatozoa are so incapable of penetrating such tissues as the stroma surrounding an ovarian follicle. Although, as far as I am aware, the actual penetrating of spermatozoa through ovarian tissues has in no case ever been shown to take place, yet we are bound to take it for granted that in some cases this actually occurs, from facts observed in many Invertebrata.

Prof. Whitman, in an exhaustive paper published in the *Journal of Morphology*, January 1891, has collected a considerable mass of evidence to show that in many widely-differing animal groups the spermatozoa make their way through the external body wall at many different points, usually a large number being bound together to form spermatophores. Perhaps the best examples of animals where this occurs are found among the Turbellarians and Leeches. In these forms the spermatozoa pass directly through the epidermis, basal membrane, and the layers of muscular and connective tissue till they reach the body cavity. Here the spermatophores break up, and in some instances the individual spermatozoa undoubtedly must penetrate the wall of the ovary in order to fertilise the ova *in situ*.

As in many mammals, the immature ova lie very near the surface of the ovary, it does not seem to be beyond possibility that even in the higher vertebrates some similar process may occur.

On the other hand, as Prof. Weismann points out, if such be the case we should expect to find animals pregnant several times in succession after once being crossed, of which no instance has ever been recorded.

Dr. Romanes' suggestion that the followers of Weismann may explain the facts of telegony by supposing the spermatozoa to disintegrate and that their "ids" and "determinants" somehow enter the unripe ova, must for the same reason be dismissed as impracticable, unless it be assumed that enough "ids" never reach one egg to supply the place of those "ids" which have been got rid of by the reducing division of the egg nucleus, and would be replaced in the ordinary course of things by the spermatozoon. Such an assumption would be obviously unscientific and unwarrantable.

It seems, therefore, unsafe, until more definite experimental work has been done with regard to this obscure and interesting problem, to attempt to give any very definite explanation of the facts as they now stand, if we adopt Prof. Weismann's views as to the continuity of the germ plasm. The facts, as Dr. Romanes very rightly insists upon, show that telegony is on the whole, of very rare occurrence, and on this account it is premature to go so far with Mr. Spencer as to maintain that the few isolated instances of telegony are sufficient to knock down at a blow the far-reaching theories of heredity which Prof. Weismann has put forth.

M. D. H.

September 29.

The Use of Scientific Terms.

PROF. LODGE has made a valuable statement regarding scientific terms in last week's NATURE as follows:—"The words used in the current language of biology are extremely classical and as unlike the language of daily life as can be contrived. This is done to keep free from the misunderstandings arising out of the attempt to give to popular words a scientific, *i.e.* an accurate meaning." Botanists have not always been as careful as Prof. Lodge would have us believe, and as an instance to the contrary I would cite the following: I was recently lecturing on forest utilisation, and used the word bark in its ordinary meaning of the outer envelope of a tree. One of the students in the class interrupted me to point out that I was speaking loosely, as *bark* is now a scientific term, meaning the transformed outer envelope of a plant, the German *Borke*, after the growth in it of corky or stony tissues.

I appealed to the sense of the class as to whether botanists have any right to adopt a common English word for something beyond its ordinary meaning, and the class took my view of the subject unanimously, carrying eventually even the objector with them.

The substance now scientifically termed *bark* might be styled *rhynchidome*, as is done in France (*vide* "Flore Forestière," by A. Mathieu, edition 1887, p. 595); or can any reader of NATURE propose a better term?

W. R. FISHER.

Coopers Hill, October 16.

Rustless Steel.

SOME months ago I noticed, in *The Field*, the statement that steel containing twenty per cent. of nickel was free from rust and, on that account, very suitable for the manufacture of small arms of high quality. From its use in the manufacture of ordnance and armour-plate I presume, moreover, that the nickel alloy does not compare unfavourably with ordinary steel in point of tenacity and hardness.

If this proves to be the case—and it is the object of this letter to elicit the information from some of the numerous readers of NATURE—nickel-steel would form an invaluable material for the construction of certain parts of astronomical and geodetic instruments, notably the pivots and axes, which, as made at present, slowly deteriorate from rust when of steel, or from wear when of bronze. With geodetic instruments, continually set up as they are in exposed situations, sometimes near the sea, it is seldom there is not, after a few years' use, evidence of rust enough on the pivots to have destroyed much of the extreme perfection of figure attained by makers like Repsold or Ertel. The wear of bronze pivots is even worse. I am informed that the earlier meridian observations at a leading observatory are not comparable in accuracy with those taken after the original bronze pivots had been replaced by steel ones.

Cape Town, September 27.

H. G. FOURCADE.

RESEARCH LABORATORIES FOR WOMEN.¹

THE session which we inaugurate to-day will in the future be regarded as of prime importance in the history both of Bedford College and of the higher education of London at large.

It will be remembered in the history of London, for in the course of it the Gresham Commissioners will issue their long-expected report. Whatever the nature of that report may be it will be important; most important if the Commissioners succeed in solving the difficult problem which has been proposed to them, and enlist in favour of their recommendations so strong a sentiment of public approval that a teaching university is at length established on the lines which they lay down. Important, though no doubt less important, if they add to the long list of failures to find the true solution, and thus only prove that another route to the desired end is barred.

As regards Bedford College itself, we meet this session under the shadow of a loss. Miss Martin, for many years the Lady Resident, who has done so much in helping to

¹ Inaugural address delivered at the Bedford College for Women, by Prof. A. W. Tucker, F.R.S.]