can never be transformed into a mammal." There is something more therefore than blind chance at work here.

But within the limits, it is a matter of experience that every possible variation may occur. If anyone will take the trouble to examine the leaves of the ribbon-grass so commonly cultivated in gardens, he will find it impossible to obtain any pair in which the green and white striping is exactly alike. If it were possible to raise to maturity all the progeny of some prolific organism, the same diversity (in different degree, of course) would manifest itself; but the whole group of variations in respect of any one organ would obey Quetelet's law. When we attempt to give some physical explanation of this fact, we know from the objective facts which have been made out about fertilization that, although the protoplasmic content of the fertilized ovum is, in a general sense, uniform, its actual structure and physiological components must be combined in as endless variety as the green and white stripes of the leaves of the ribbon-grass. If, with Prof. Lankester, we say that the combinations are kaleidoscopic, I do not see that we go beyond the facts. And it appears to me quite permissible to correlate the ascertained variable constitution of the ovum arising from this cause with the equally ascertained varying structure of the organism developed from it.

Of the varied progeny, we know that some survive and others do not. And what Darwin has taught us is, that the reason of survival is the possession of favourable variations. The surviving race necessarily differs somewhat from its progenitors, and Darwin has further stated that it is probable that by the continued repetition of the process all the diversity of organic nature has been brought about.

The area of fortuity is narrowed down therefore, on this point of view, to the variable constitution of the individual ovum. And it is upon the recognition of this fact, for which there seems to be good scientific evidence, that the Duke of Argyll founds his charge that the neo-Darwinians make fortuity their idol. The reason appears to be that it comes into collision with teleological views. But such collisions are no new event in the history of the biological sciences. And teleology, like a wise damsel, has generally, though temporarily ruffled, managed to gather up her skirts with dignity and make the best of it. For some element of fortuity is inseparable from life as we see it. It is at the bottom one of the most pathetic things about it. Nowhere is this more vividly portrayed perhaps than by Addison in the "Vision of Mirzah." Yet I do not remember that anyone was ever so unwise as to taunt Addison with making fortuity his idol.

But, philosophically considered, what is gained by this tenacity about out-works? I reply, exactly as much as was gained by the tenacity of the Church in respect to the geocentric theory of the planetary system. Scientific men cannot be stopped in the application of their best ability to the investigation of Nature. If their conclusions are false, they will detect the falsity; if true, they will not be deterred from accepting them by some *a priori* conception of the order of the universe. It is not justifiable to say that this is due to any devotion to such an empty abstraction as fortuity. No scientific man is, I hope, so foolish as to suppose that, however completely mechanical may be his conception of Nature, he is in any way competent to account for its existence. The real problem of all is only pushed further back. And the Duke of Argyll's difficulty resolves itself into the old question, whether it is more orthodox to conceive of the universe as an automatically self-regulating machine, or as one which requires tinkering at every moment of its action.

It may be replied that this is all very well, but that it is not the way the neo-Darwinians state their case. I may be, therefore, excused for quoting some passages to the contrary from Weismann's "Studies in the Theory of Descent":—

"This conception represents very precisely the well-known decision of Kant: 'Since we cannot in any case know *a priori* to what extent the mechanism of Nature serves as a means to every final purpose in the latter, or how far the mechanical explanation possible to us reaches, natural science must everywhere press the attempt at mechanical explanation as far as possible' (p. 638).

Further, he quotes from Karl Ernst von Baer:—

"The naturalist must always commence with details, and may then afterwards ask whether the totality of details leads him to a general and final basis of intentional design" (p. 639).

Again, he says:—

"We now believe that organic nature must be conceived as mechanical. But does it thereby follow that we must totally deny a final universal cause? Certainly not; it would be a

great delusion if anyone were to believe that he had arrived at a comprehension of the universe by tracing the phenomena of Nature to mechanical principles" (p. 710).

In truth, this revolt of teleology against Darwinism is a little ungrateful. For, if Darwinism has done anything, it has carried on and indefinitely extended its work. In the last century, teleology was, it seems to me, a valuable motive-power in biological research. Such a book as Derham's "Physico-Theology" (1711) may be read with interest even now. I well remember that my first ideas of adaptive structures were obtained from the pages of Paley. Thirty years ago I do not know, except from them and the notes to Darwin's "Botanic Garden," where such information was to be obtained. The basis of research was, however, too narrow to continue; it did not look beyond the welfare of the individual. The more subtle and recondite springs of adaptation opened up by the researches of Darwin, which look to the welfare of the race, were not within its purview. Consequently it dried up, and virtually expired with the Bridgewater Treatises.

To return, however, to the Duke of Argyll. "Neither mechanical aggregation, nor mechanical segregation, can possibly account for the building up of organic tissues." Who has said they did? The Duke has entirely misunderstood the matter. Prof. Lankester never suggested that it was possible to put so much protoplasm into a vessel, and shake out a cockatoo or a guinea-pig at choice. His image of the kaleidoscope had nothing to do with the building up of organisms, only with the varied combination of the elements known to take part in the formation of the fertilized ova from which organisms originate.

I am not sure that I perfectly comprehend what follows. Perhaps some further emendation than that already published is needed in one of the sentences. But it seems evident that the Duke is re-stating his old doctrine of "prophetic germs." He has already defined what he means by these (NATURE, vol. xxxviii, p. 564). "All organs," he says, "do actually pass through rudimentary stages in which actual use is impossible." Here, again, as in the case of the transmission of acquired characters, what one wants is not a reiteration of the assertion, but some definite observed evidence. For the production of this, if only in a single instance, Prof. Lankester pressed the Duke more than a year ago (NATURE, *l.c.* p. 588). None, however, has as yet been forthcoming; and it appears to me that it is not permissible to persist in statements for which he does not attempt to offer a shadow of proof.

The Duke exults in a very amazing fashion over what he strangely calls Prof. Lankester's admission that "natural selection cannot account for the pre-existence of the structures which are prescribed for its choice." I am afraid I have already trespassed on your space too much with quotations; but I have done so in order to show, in some measure at any rate, what is the consensus of opinions amongst students of Darwinism; and I must answer the Duke with one more from Prof. Huxley's admirable biography. It is true that the Royal Society publishes these things in the least attractive way possible; but this particular paper could hardly have escaped attention, as it won the notice and admiration of even a journal so little occupied with scientific discussion as *Truth*.

"There is another sense, however, in which it is equally true that selection originates nothing. 'Unless profitable variations . . . occur, natural selection can do nothing' ('Origin,' ed. 1, p. 82). 'Nothing can be effected unless favourable variations occur' (*l.c.*, p. 108). 'What applies to one animal will apply throughout time to all animals—that is, if they vary—for otherwise natural selection can do nothing. So it will be with plants' (*l.c.* p. 113). Strictly speaking, therefore, the origin of species in general lies in variation; while the origin of any particular species lies, firstly, in the occurrence, and, secondly, in the selection and preservation of a particular variation. Cleanliness on this head will relieve one from the necessity of attending to the fallacious assertion that natural selection is a *deus ex machina*, or occult agency."

And the Duke says he has been waiting for this for thirty years. One can only wonder what Darwinian literature has been the subject of his studies during that time.

W. T. THISELTON DYER.

Royal Gardens, Kew, January 6.

The Microseismic Vibration of the Earth's Crust.

IN Mr. White's article on British earthquakes (NATURE, Jan. 2, p. 202) he refers to me as having *discovered* the microseismic

vibration of the earth's crust. My brother Horace and I were, we believe, the first to verify in England the observations of Bertelli, Rossi, d'Abbadie, and the other (principally Italian) pioneers in this interesting subject.

In our Reports to the British Association for 1881 and 1882 on "The Lunar Disturbance of Gravity," some account will be found of the earlier literature on the subject.

January 9.

G. H. DARWIN.

Meteor.

ON Sunday, 12th inst., about 8.10 p.m., a bright meteor was seen here, coming into view near δ Aurigæ. It was of a reddish colour, moved slowly, leaving a short tail, and burst above ϵ Leonis, then with diminished light continued its course to the horizon.

T. W. MORTON.

Beaumont College, Old Windsor, January 13.

MAGNETISM.¹

I.

AS old as any part of electrical science is the knowledge that a needle or bar of steel which has been touched with a loadstone will point to the north. Long before the first experiments of Galvani and Volta the general properties of steel magnets had been observed—how like poles repelled each other, and unlike attracted each other; how the parts of a broken magnet were each complete magnets with a pair of poles. The general character of the earth's magnetism has long been known—that the earth behaves with regard to magnets as though it had two magnetic poles respectively near the rotative poles, and that these poles have a slow secular motion. For many years the earth's magnetism has been the subject of careful study by the most powerful minds. Gauss organized a staff of voluntary observers, and applied his unsurpassed powers of mathematical analysis to obtaining from their results all that could be learned.

The magnetism of iron ships is of so much importance in navigation that a good deal of the time of men of great power has been devoted to its study. It was the scientific study of Archibald Smith; and Airy and Thomson have added not a little to our practical knowledge of the disturbance of the compass by the iron of the ship. Sir W. Thomson, in addition to much valuable practical work on the compass, and experimental work on magnetism, has given the most complete and elegant mathematical theory of the subject. Of late years the development of the dynamo machine has directed attention to the magnetization of iron from a different point of view, and a very great deal has been done by many workers to ascertain the facts regarding the magnetic properties of iron. The upshot of these many years of study by practical men interested in the mariner's compass or in dynamo machines by theoretical men interested in looking into the nature of things, is, that although we know a great many facts about magnetism, and a great deal about the relation of these facts to each other, we are as ignorant as ever we were as to any reason why the earth is a magnet, as to why its magnetic poles are in slow motion in relation to its substance, or as to why iron, nickel, and cobalt are magnetic, and nothing else, so far as we know, is to any practical extent. In most branches of science the more facts we know the more fully we recognize a continuity in virtue of which we see the same property running through all the various forms of matter. It is not so in magnetism; here the more we know the more remarkably exceptional does the property appear, the less chance does there seem to be of resolving it into anything else. It seems to me that I cannot better occupy the present occasion than by recalling your attention to, and inviting discussion of, some

of those salient properties of magnetism as exhibited by iron, nickel, and cobalt—properties most of them very familiar, but properties which any theory of magnetism must reckon with and explain. We shall not touch on the great subject of the earth as a magnet—though much has been recently done, particularly by Rücker and Thorpe—but deal simply with magnetism as a property of these three bodies, and consider its natural history, and how it varies with the varying condition of the material.

To fix our ideas, let us consider, then, a ring of uniform section of any convenient area and diameter. Let us suppose this ring to be wound with copper wire, the convolutions being insulated. Over the copper wire let us suppose that a second wire is wound, also insulated, the coils of each wire being arranged as are the coils of any ordinary modern transformer. Let us suppose that the ends of the inner coil, which we will call the secondary coil, are connected to a ballistic galvanometer; and that the ends of the outer coil, called the primary, are connected, through a key for reversing the current, with a battery. If the current in the primary coil is reversed, the galvanometer needle is observed to receive a sudden or impulsive deflection, indicating that for a short time an electromotive force has been acting on the secondary coil. If the resistance of the secondary circuit is varied, the sudden deflection of the galvanometer needle varies inversely as the resistance. With constant resistance of the secondary circuit the deflection varies as the number of convolutions in the secondary circuit. If the ring upon which the coils of copper wire are wound is made of wood or glass—or, indeed, of 99 out of every 100 substances which could be proposed—we should find that for a given current in the primary coil the deflection of the galvanometer in the secondary circuit is substantially the same. The ring may be of copper, of gold, of wood, or glass—it may be solid or it may be hollow—it makes no difference in the deflection of the galvanometer. We find, further, that with the vast majority of substances the deflection of the galvanometer in the secondary circuit is proportional to the current in the primary circuit. If, however, the ring be of soft iron, we find that the conditions are enormously different. In the first place, the deflections of the galvanometer are very many times as great as if the ring were made of glass, or copper, or wood. In the second place, the deflections on the galvanometer in the secondary circuit are not proportional to the current in the primary circuit; but as the current in the primary circuit is step by step increased we find that the galvanometer deflections increase somewhat, as is illustrated in the accompanying curve (Fig. 1), in which the abscissæ are proportional to the primary current, and the ordinates are proportional to the galvanometer deflections. You observe that as the primary current is increased the galvanometer deflection increases at first at a certain rate; as the primary current attains a certain value the rate at which the deflection increases therewith is rapidly increased, as shown in the upward turn of the curve. This rate of increase is maintained for a time, but only for a time. When the primary current attains a certain value the curve bends downward, indicating that the deflections of the galvanometer are now increasing less rapidly as the primary current is increased; if the primary current be still continually increased, the galvanometer deflections increase less and less rapidly.

Now what I want to particularly impress upon you is the enormous difference which exists between soft iron on the one hand, and ordinary substances on the other. On this diagram I have taken the galvanometer deflections to the same scale for iron, and for such substances as glass or wood. You see that the deflections in the case of glass or wood, to the same scale, are so small as to be absolutely unappreciable, whilst the deflection for iron at one point of the curve is something like 2000 times as

¹ Inaugural Address delivered before the Institution of Electrical Engineers, on Thursday, January 9, by J. Hopkinson, M.A., D.Sc., F.R.S., President.