enough that variations must be governed by some law. But as we are absolutely ignorant what that law is, he thought it allowable to make provisional use of the word accidental. But the "neo-Darwinians" (as Prof. Ray Lankester calls them) are not content with this dethronement of their idol, Fortuity. The supreme and everlasting rule of pure accident is their creed and worship. Hence comes Prof. Ray Lankester's simile of the kaleidoscope, by which he illustrates the genesis of "new characters" in organic life. There is, he indicates, no more connection between those "new characters" and their origin in the parent, than there is between the new patterns which tumble in a kaleidoscope and the tap upon the tube which shakes them out.

There is no argument so false as a false analogy. And this is a case in point. Every illustration or analogy must be false which confounds mere mechanical arrangement with organic structure. They are not only different, but they are different in kind. Neither mechanical aggregation, nor mechanical segregation, can possibly account for the building up of organic tissues. To attempt to account for such structures by causes similar to those which determine the arrangement of tumbling bits of glass, is even more irrational than it would be to account for the structure of a great cathedral by explaining to us how its bricks or its stones were made. There is one grand peculiarity in all organic structures which all such illustrations are framed to conceal. That grand peculiarity is this—that they are all made for work, for the discharge of some function. They are where they are not merely because somehow they have been put there. But they are what they are, and where they are, because they have some given work to do. But more than this: they all pass through stages of development in which their work cannot as yet be done. In all these stages, that work lies before them in respect to time, and behind them in respect to adaptation. They are all of the nature of an "apparatus." This is the word which the profound but unconscious metaphysic of human speech has invented for them. It is the word chosen by natural selection, and, as such, it ought to secure the homage even of Prof. Ray Lankester himself. The idea, however, comes before the word—shapes it, and inspires it—just as the needs of function, and the organic necessities imposed by inorganic laws, have shaped and inspired the growth and development of every organic apparatus.

I am very glad to see that under the stress of controversy the Professor admits—and even hotly denies that it has ever been doubted—that natural selection cannot account for the pre-existence of the structures which are presented for its choice. And not only must selected organs exist before they can be chosen by natural selection, but they must have been already sufficiently developed to possess some functional activity. This was my contention thirty years ago, and to this day I have always found it either denied or evaded by the whole ultra-Darwinian school. I rejoice to see it now admitted as unquestionable. "Natural selection can account for the origin of nothing"—so says Mr. Cope. The Professor indignantly replies: "How can Mr. Cope presume to tell us this? Who has ignored it? when? and where?" So ends a long and a hard fight. The enemy not only lays down his arms, but denies he has ever carried them.

ARGYLL.

Who Discovered the Teeth in Ornithorhynchus?

It is almost superfluous to add anything to Prof. Flower's reply (p. 151) to Dr. Hart Merriam. In justice, however, to Mr. Poulton, it ought, I think, to be stated that he fully refers to Home's paper in the Philosophical Transactions. In the *Quart. Journ. Micr. Sci.*, vol. xxix. p. 27 (a paper to which Dr. Hart Merriam alludes as though he had read it) Mr. Poulton, describing the horny plates of Ornithorhynchus, writes as follows: "Home (Phil. Trans., 1802, p. 71) correctly describes these horny plates as differing 'from common teeth very materially, having neither enamel nor bone, but being composed of a horny substance only embedded in the gum,'" &c. I observe too, with great interest, that in the same paper Home makes use of the expression (p. 70) "the teeth, if they can be so called." On p. 28 Mr. Poulton quotes in full the passage from Owen given by Prof. Flower. Perhaps Dr. Hart Merriam does not accept Owen's correction of Home's hypothesis. It is hardly necessary to point out that the teeth which Mr. Poulton describes (p. 15 *et seq.*) under the headings (1) tooth papilla; (2) dentine; (3) enamel; (4) inner epithelium of enamel organ; (5) stratum intermedium of Hannover; (6) middle membrane of enamel organ;

and (7) outer membrane of enamel organ, *must* be very different from those which Home calls "cuticular," and further qualifies as in the sentence which I have quoted.

Comparison of Home's figures with Mr. Oldfield Thomas's (Proc. Roy. Soc., vol. xlvii. pl. 2) renders it highly probable that the true teeth of Home's younger specimen had only recently dropped out from the horny plates; the dimensions given by the two authors being almost identical. But Home's description is perfectly definite, and no hint whatever is made to true teeth situated upon the horny plates such as those described and figured by Mr. Oldfield Thomas. The length of the skull of Home's specimen, as given in his figure, is 71 millimetres, while that of Thomas's female specimen is 65 millimetres; the male is slightly larger. Probably, therefore, Home's specimen was considerably older than Thomas's, and had lost the true teeth for some little time.

The only conclusion at which I can arrive is that Dr. Hart Merriam did not read any of the three papers bearing on this subject with sufficient care and attention to enable him to fully understand the facts ascertained by their respective authors, if indeed he proceeded further than the introductory remarks prefacing Mr. Oldfield Thomas's communication to the Royal Society.

OSWALD H. LATTER.

Anatomical Department, Museum, Oxford, December 20.

Galls.

IN answer to Mr. Ainslie Hollis, I should like to observe that, in my opinion, the theory of natural selection is *not* "seriously assailed by investigations into the formation of galls by insects." On the contrary, in reply to what appeared to be a challenge from Mr. Mivart, I pointed out the manner in which natural selection might here be fairly supposed to have operated. But, while doing this, it appeared desirable to add that the case is a highly peculiar one. If galls were merely amorphous tumours or even if they presented but as small an amount of specialization for the benefit of the larvæ as is presented by animal tissues for the benefit of their parasites, the case would not be so peculiar. But the degree of morphological specialization which the "pathological process" presents in the case of some galls—and this, of course, for the exclusive benefit of the contained parasites—is very remarkable. And although I doubt not that it is but a higher exhibition of the same principles as obtain in the case of animal tissues and their parasites, it is a case of much greater interest from the Darwinian point of view. For, if the explanation given in my last letter be accepted, the facts show how enormous must be the power of natural selection in building up adaptive structures, seeing that it can do this in so high a degree even when working, as it were, at the end of a long lever of the wrong kind—*i.e.* acting *indirectly* on the vegetable tissues through the benefits thereby conferred on their animal parasites. I am not aware that there is any other instance of "symbiosis" where so high a degree of adaptive specialization is presented by one of the "partners" for the exclusive benefit of the other.

GEORGE J. ROMANES.

London, December 13.

MR. W. AINSLIE HOLLIS has involuntarily misrepresented me as saying that the theory of natural selection can be "seriously assailed" by investigations respecting galls. I said, indeed (NATURE, November 14, p. 41), that it would be "very interesting to learn how" natural selection could have caused them; but I was careful to add that doubtless an explanatory hypothesis was ready to hand. I do not myself believe they were so caused; but if they were not, they would none the less, like almost all biological phenomena, be explicable by an unlimited use of gratuitous hypotheses concerning physiological correlations and imaginary ancestors.

I confess I do not see that calling them "pathological" (an epithet I certainly would not deny them), and comparing them with inflammatory renal foci due to Bacilli, will explain them, unless it be affirmed that pathological conditions favourable to parasites are always due to the action of "natural selection" on the parasites themselves—an affirmation which appears to ask too much.

Herr Wetterhan's argument from *symbiosis* sins against natural selection itself. For that theory requires that, in the arduous and incessant struggle for life it supposes, any prejudicial growth should, in time, be eliminated unless carrying with it some preponderating advantage. The insect and the plant are

not "partners," for the latter does not participate in the gain of the former. How, then, on symbiotic principles, can "natural selection" have been the means of producing a growth which, though important, if not necessary, to the animal symbiont, is more or less prejudicial to the symbiont vegetable organism?

There can, of course, be no doubt, as Mr. McLachlan says, that the various peculiarities of gall-structure "could be" explained "on purely physiological grounds if carefully studied;" but that "natural selection" will suffice to explain them, seems to me by no means equally free from uncertainty.

ST. GEORGE MIVART.

Hurstcote, Chilworth, December 13.

The Permanence of Continents and Oceans.

I CAN find no flaw in the reasoning on the dynamical question of the permanence of continents and oceans, in Mr. Starkie Gardner's letter in NATURE of December 5 (p. 103), by which he endeavours to show the universal "tendency for deep oceans to become deeper, and for mountain chains to grow into higher peaks." But when he says it is opposed to no known facts, I wish to ask how it is to be reconciled with the fact of the general distribution of marine deposits over the face of the earth, so that every part of what is now land appears to have once been ocean?

I fully concede that the change of ocean spaces into land spaces is an extremely slow process, taking, probably, millions of years, but it seems to me that it must have occurred, though I cannot suggest through what agency.

Belfast, December 14.

JOSEPH JOHN MURPHY.

Does the Bulk of Ocean Water Increase?

MR. JUKES-BROWNE (NATURE, December 12, p. 130) admits that "if the area of the land were larger, and the depth of the oceans less," in early geological times, a further inference must be drawn—"that the bulk of the ocean water was less than it is now."

So far we are in agreement; indeed, we could scarcely be otherwise, as the proposition admits of complete demonstration. When, however, Mr. Jukes-Browne proceeds to give his reasons for holding that the bulk of ocean water *was* less in early times than now, he enters upon a more controversial subject.

I am familiar with the arguments he urges partly on the authority of Mr. Fisher, and have to some extent discussed them in chapter xii. of the "Origin of Mountain Ranges." I desire, however, to point out a further objection that when stated will, I think, appear extremely obvious.

According to Dr. George Darwin and many other astronomers who follow him, our satellite, the moon, was once an integral portion of the earth, having been thrown off when the earth was in a molten condition. If this theory be correct, it is a fair assumption that the magma out of which the moon has consolidated was composed of matter similar to that of our earth. Even if their relations were never so intimate as this, I think most physicists and astronomers will admit a similarity of material constitution of the two spheres.

If then volcanic action on the earth is, as Mr. Jukes-Browne contends, accompanied by a separation of water initially contained in the magma, and its condensation on the surface in such quantities as to materially increase the bulk of ocean water, why has not the same effect followed volcanic action on the moon? Why, in fact, do we not see oceans on the surface of the moon instead of a dry and desert waste of volcanic rings, mountain protuberances, and arid plains? In face of this great fact it appears to me that ingenious arguments as to the amount of water contained in the fluidal cavities of granite, which most geologists think is explicable by percolation, have not much weight.

At all events, it seems a reasonable question to ask why oceans should be supplied with water from the perspiring pores of mother earth, while her offspring, the moon, is so dry as to have absorbed into herself all evidence of any aqueous envelope that may have formerly existed.

T. MELLARD READE.

Park Corner, Blundellsands, December 14.

A Natural Evidence of High Thermal Conductivity in Flints.

A RATHER curious effect of the recent frost attracted my attention in the gravel foot-paths leading over Addington Hill,

near Croydon, on the beautifully bright day of the 1st inst. The clear nights and frosty air of the closing week of last month had been productive of continued low temperatures in that locality, and the result observed was that the flint pebbles, which in neighbouring gravel-beds and here and there on the paths, are of the size of hens' eggs, and remarkably well rounded, had, in places, sunk in the frozen clunch or clay-earth of the foot-paths, and in the peaty ground or turf beside the paths, as it appeared, like filberts shrunk and resting at the bottoms of their shells; or else as if the pebbles' earthy moulds had, by expanding upwards, left such a large vacuity above each stone, that the tops of some of the large ones, instead of being level (as at first they must have been, by the appearance of the moulds) with the surface of the ground, were now, in a somewhat turfy place, about as much as half an inch below it. The physical enigma which hereupon offered itself for elucidation was, how the pebbles could remain at the much lower level, while such a considerable expansion upwards had been brought about by freezing in the moist earth immediately surrounding them; and this problem had certainly, in looking at the thickly-clustered cavities in the frozen ground, at first a very paradoxical appearance.

But if the question how the inclosing cavities of moist earth round flint pebbles which are nearly embedded in it, are distended upwards so curiously by a strong frost's predominance, has presented, it may be, to some of your readers who may have noticed in similar conditions a similar appearance, as it at first did to me, a subject for rather puzzled contemplation and conjectures, it will be worth pointing out, perhaps, that there is a well-ascertained thermal property of siliceous rocks and flint, of which it seems not improbable that this not unfrequently occurring action of a strong frost, in such conditions, may really be an interesting illustration.

Among a series of about a hundred different descriptions and varieties of commonly occurring rocks whose thermal conductivities were experimentally determined by a Committee of the British Association in the years 1874-78, it was found that such entirely siliceous ones as quartz, flint, and pure siliceous sandstone, &c., so much surpass all other ordinary rocks in their rates of transmitting both heat and temperature, that in flint pebbles these conducting powers are, for example, about four or five times as great as in damp sandy mould, or in wet clayey earth.

Instead of the layers of cold temperature, therefore, produced in wet pebbly ground by continued frosty winds and radiation, proceeding in plane levels downwards from one depth below the surface to another, large flints exposed in it must grow cold very quickly through their whole substance, and must freeze the wet earth under them almost as soon as the soil's surface-layer round them is beginning to be frozen. The effect of this freezing process's expansion, it seems evident, will hardly be so much to raise the pebbles and the earth's exposed surface upwards very differently from each other, by the frost's nearly equal action on them both, as, during the frost's continuance, to force up towards the surface a large superfluity of soft earth from between the bedded stones, carrying the cast or mould of the stone's upper sides, itself to some height above them. We would require, perhaps, as an aid to this interpretation of the process, to regard the congelation round the stones, as rooting them down, perhaps to lower-lying ones, so that the upward thrust of the extruded earth may not be able to dislodge them, but can be effective to raise up their frozen caps; but some such supposition as this does not appear to be a very impossible conjecture. By this recourse to the pre-eminent thermal conductivity of flints above that of moist turf and clay, in which they are embedded, it seems at least not impracticable to give a somewhat intelligible explanation of the frozen ground's abnormal elevation round them, lifting the moulded caps of earth-covering off their upper sides until their roadside clusters present the curious appearance of shrunken petrifications of some nest of fossil yolks in half-empty egg-shells.

It is, indeed, true that when by long continuance of a frost the sodden earth may have become entirely penetrated and frozen by it to some considerable and tolerably even depth (we may suppose) below a layer of embedded flints, it should be noticed, to simplify the process's consideration, that the form which the frozen ground will then have acquired between and round the flints could be nowise affected in the end by any various shapes, plane or contorted by irregularly formed and differently conducting solid bodies in its course, wherewith the tract of