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

reezing temperature after entering the ground approaches by stages of quick or slow rates, in different parts, towards the supposed nearly even depth at last, if we might only presuppose that, because of the endless material obstruction to its motion in any horizontal direction, no channels for the earth's lateral expansion in freezing should subside; but that in all places and in all conditions where the freezing happens, the only line of escape of the earth's increase of volume should be vertically upwards towards a direction where no insuperable forces are, at least, opposed to it.

Were this assumption of upward reliefs only of all of the expansions a really true and valid one, every vertical fibre of the wet earth's mass would behave in freezing quite independently of every other one, and would take up its fully expanded length at last, no matter at what times and in what order congealing overtook its individual portions. A stone, in this supposition, just embedded in the ground, would have its lower half lifted at last in its socket, and the upper half of the socket lifted off the stone (whether its thermal conductivity is great or small), to the height, in either case, of a water-column's change of length by freezing, whose initial height is but half the vertically measured thickness of the round embedded stone—that is to say, about one-eleventh of an inch for a stone 2 inches in diameter, instead of nearly half an inch, which was about the depth of the settlement, in some of the large-sized flint stones, which was actually observed.

To return to the reality, however, from this artificial supposition, the actual course of the expansions, and the effects produced by the freezing dilatations must, no doubt, be very different. Supposing that the flint-stones, by their good thermal conductivities, soon become covered with a thickening coat of frozen earth, flow of the soft, unfrozen earth between them will really spring up and be maintained by direct outward expansions from the stones of the icy coats surrounding them. On account of the firm rigidity of the exposed earth-surface, to which the stones themselves must soon become fast fixed, the resultant flow of soft earth from between the stones, instead of finding an upward path the easiest, will rather choose a vertically downward one for its escape from its confinement, and lift the stones and icy covering together, rather than seek by an upward course to break through the latter. Yet this last effect may also perhaps occur to some extent, raising the frozen earth-caps in some measure off the stones' upper sides, and stretching them, it may be, a little upwards, so as to leave between them and the stones clear empty spaces. That this last effect must be only a secondary and inconspicuous one, however, seems to be pretty obvious from this passingly essayed, and as it now appears all too uselessly pursued and desultory *aperçu* of the frost's real mode and process of expansive action.

Regarding the peculiar structures, in fact, altogether from another point of view, and rejecting the imperfect explanation which any one of these presumed congelation processes might at first have been supposed to furnish, of the curiously sunken-looking assemblages of the wayside pebbles, an exactly opposite interpretation of their semi-interred condition seems, perhaps, indeed, to afford a more satisfactory and likely explanation of it, than the expansive effects of frost in the moist earth were ascertained and shown to have any capabilities and physical resources for. The warmth of the sun, or of wind and rain in some thawing daytime temperature of the generally frosty week, may in short be supposed (which the weather-table of the week, on the 26th and 27th ult. confirms) quite plainly and certainly enough, in consequence of the flints' good thermal conductivities, to have melted and shrunk again to its natural dimensions the hard frozen earth under them, without lowering the level equally of the badly conducting frozen earth surrounding them. Alternate days of thaw and nights of frost would, by progressive stages which can be easily traced out and understood, tend quite naturally to exaggerate this difference. Thus in another way, but complementary to and at returning times just fitly supplemented by that first supposed, the problem which the winter scene presented is, still more simply and clearly than before, seen to be solved quite truly and correctly by the relatively high thermal conductivity of the rounded flints as compared with that of the hard frozen earth in which they are enveloped.

This gradual subsidence, therefore, of flint stones during alternate frosts and thaws, into frozen earth, by consolidation and lateral expansion, followed by liquefaction and vertical contraction of the water in the earth beneath them, is, it would seem that we may reckon it accordingly, a phenomenon on land

just analogous and similar to the familiar thermal process which small stones scattered on a smooth frozen glacier-field display in summer-time, by intercepting the heat of the sun's rays, and by sinking to the bottom of the deep water-holes which they thus scoop and delve out for themselves, wherever they happen to have found a lodgment in the naked ice.

A. S. HERSCHEL.

Observatory House, Slough, December 9.

Foreign Substances attached to Crabs.

AT the last meeting of the Linnean Society I exhibited a number of crabs and certain shells of the genus *Phorus* having various foreign substances attached to them, about which it is desirable that more should be known. Some of the crabs manage to fasten bits of sea-weed to the hairs on the carapace and legs; Polyzoa, Balani, Serpulæ, &c., in their earlier stages fasten themselves on others; a crab of the Indian Seas—*Camposcia retusa*—is sometimes completely covered on every part with sand, small shells, and bits of sea-weed—*Corallina* chiefly. These could only be attached by some adhesive matter, but whence derived? *Dromia vulgaris* is occasionally found with a sponge extending over the carapace and almost completely hiding the animal. The species of this genus have the two hinder pairs of legs much reduced, flattened, and lying close to the back, and this is assumed to be an adaptation for the purpose of retaining the sponge. Out of a number of specimens dredged in the Bay of Naples, I recollect only getting one with a sponge on it, and that very soon shrivelled up, leaving a leathery-looking substance attached to the base of the carapace, not held by the legs apparently.¹ Two crabs—*Ethusa mascaroni* and *Dorippe lanata*—having similarly reduced hind-legs, but directed upwards, seem much better adapted for retaining a foreign substance, which, however, they are not known to do. In a Mauritanian crab—*Dynomene hispida*—the hind pair only are reduced, but to such an extent as to be merely rudimentary and incapable of any use. *Paramithrax barbatus*—a New Zealand crab—has, like some others, hooked hairs, but in the specimen exhibited they appear to be free of any foreign substances, although many small fragments of an uncertain nature appear between them.

In *Phorus* a strong cement only could hold on those large and heavy substances—shells, stones, &c.—completely covering the shell, as in *P. agglutinans*. I have not seen any account of their *modus operandi*, but, as the animals have a long proboscis, it is possible that that may be the organ employed, but it is difficult to believe that it would be able to lift any large substance, or that it could reach the top of the shell. Another difficulty is that they must cast off, from time to time as they grow, the smaller substances, to replace them by larger ones. There is one *Phorus*, however—*P. calyculatus*—in which small shells imbed themselves at short intervals along the whorls, leaving the greater part of the shell uncovered; these little cup-shaped depressions are marked inside, as far as the mouth of the shell will permit them to be seen, by corresponding protuberances. This would seem to indicate a certain softening of the shell at one time or other.

I do not see where protection comes in, in any of these cases.

December 14.

FRANCIS P. PASCOE.

A Marine Millipede.

IN the hopes of arousing the interest and the energies of British entomological collectors, "D. W. T.," in a short notice on p. 104 of the present volume of *NATURE*, draws attention to the recent discovery in Jersey, by Mr. Sinel, of that remarkable marine centipede *Geophilus (Schendyla) submarinus* (not *submaritimus* by the way), of Grube.

Those who observed this notice, and are interested in the fauna of Great Britain, may be glad to hear in addition that more than twenty years ago a number of specimens of this then undescribed species were taken by Mr. Laughlin at Polperro on the south coast of Cornwall. These specimens, which were presented to the British Museum in 1868, were found associated with *Linotenia maritima* (Leach)—also a marine centipede—

¹ Bell, in his "British Crustacea" (p. 371), states having received "numerous young specimens from Sicily, every one of which had the carapace entirely covered with a sponge, which had grown over it, concealing even the two hinder pairs of legs, which were closely placed against the back, and rendered immovable." No mention is made of a sponge on those that came from the Channel.