effect introduced by Laplace, and this Airy calls a "singular and unwarranted principle." Sir William Thomson says this unwarranted principle is in fact an "exquisitely subtle" method by which Laplace determined a certain constant, and Airy rejoins, "I look on Laplace's process as a mere sport with symbols and on Laplace's conclusion as a grievous error." Whether, however, Laplace is right or wrong, his conclusion applies to an ocean covering the whole surface of the earth, and would not help to determine the motion of the fluid as actually distributed in the existing seas.

The question of the earth's rigidity also would have to be settled before any theory could give a quantitative estimate of the true amplitude of the equilibrium tide.

Sir William Thomson (Thomson and Tait's "Natural Philosophy") states that unless the rigidity of the earth was at least as great as that of iron or glass, the tidal rise and fall would not be so great as it actually is. In view, however, of the want of deep sea observations and of the amplification which occurs near a coast-line, the necessity for such rigidity does not seem to be proved.

I think the only satisfactory way to ascertain the amplitude of the tides in the deep ocean is by direct measurement, and though this presents some practical difficulties, it ought not to be impossible.

A. MALLOCK.

9 Baring Crescent,
Exeter.

Evolution through Adaptation.

DR. BATHER'S lecture on "Evolution through Adaptation," printed in NATURE of Mar. 30, bristles with debateable points, but I will select a cardinal one which appears to present a fundamental difficulty in his theory. He speaks of the changes of depth and salinity in the waters which have taken place in geological time and draws the conclusion "that the surroundings of a race are continuously altering; the race has perpetually to catch up with the change." But even if the small changes that have taken place in the oceanic environment could account for the trend of evolution, for example, from an Asteroid to an Echinoid form in the Echinodermata, how could be explained the persistence of the original Asteroid type practically unchanged? The race has not changed, if certain members or groups of it have.

Dr. Bather points out that "there is some tendency for change of form and structure to proceed in a definite direction," but he goes much further in stating that "the direction will accord with the environment." Apart from lethal factors in inheritance and non-viable monsters, what evidence is there that new forms in animal evolution are necessarily more in harmony with their environment than were and are the forms from which they arose? For example, many Echinoid and Asteroid forms share the same environment in the sea, but the Echinoid type is believed to have evolved from primitive Asteroidea. How does the Echinoid trend of evolution accord better than does the Asteroid with the environment which they both share? Migration as a factor in isolation of species can be ruled out, of course, if the original and the 'evolving' line have always shared the same environment.

The mutations required by Dr. Bather's theories can of course be admitted, as they can be seen and investigated, but they only "provide that fundamental premise from which, *in combination with a varying environment* [italics mine], one can deduce irreversibility of evolution . . . and orthogenetic trends." This would be true only if it could be shown that the varying environment favoured the new forms at the expense of the old, but actually the older forms are often as well adapted to the varying environment as are the new ones. Another objection is that, while the slight changes that have taken place in the physical and chemical constitution of the ocean would affect such processes as fertilisation and early development in various ways, it is difficult to imagine how such changes can have directed the general "orthogenetic trends" in adult oceanic forms. Furthermore, the persistence of primitive or early forms in the same environment is evidence against such a view.

J. S. DUNKERLY.

IN speaking of "Dr. Bather's" theory and theories, Prof. Dunkerly pays me too much honour. That portion of my discourse which appeared in NATURE attempted a critical inquiry into other people's theories and a possible explanation of certain difficulties that they presented to my mind. To Prof. Dunkerly's mind the main theory presents yet another difficulty. He admits, apparently, the fact of evolution, and he admits some change of environment; but he urges (I understand), first, that the changes of environment are too slight to produce the great evolutionary changes seen along certain lines; secondly, that if they were a *vera causa* they would have affected all lines of descent in a more equal degree.

It is rather late in the day to be answering arguments of this kind, and space could not be afforded in NATURE for their adequate discussion. May I suggest, first, that Prof. Dunkerly underestimates the differences and the changes in the environment of sea animals? If he derives his conception from a single summarising sentence in my discourse, I would remind him that two-thirds of that discourse (not reported in NATURE) was devoted to an illustrated account of some among the numerous and varied habitats, conditions, and modes of life that a single class of marine invertebrates (and a statozoic class at that) has come to fill during its long history. It was emphasised that a single small patch of sea-floor, which we speak of roughly as sand or sea-weed or reef and so forth, really comprises many habitats. On the other hand, it was urged that, just as one cannot envisage a living creature apart from its environment, so one should not conceive of the environment without the reaction of the creature; further, that the whole creature constitutes the environment of any one of its parts.

Consider 'migrations,' on which Prof. Dunkerly seems to misapprehend me. Surveys of the sea-floor, notably by the Danes, have shown that the immigration or emigration of a single species from or to a faunal assemblage on a small patch must, and does, affect the life of all the other species, although purely physical conditions are unaltered. Or take mutation (which Prof. Dunkerly admits) and consider the Cladocera mutant found by Banta and Wood (see NATURE, Oct. 29, 1927, p. 632); here is a form that can live only at a temperature higher than the normal, and if it does find a warmer pool it will be preserved as a race adapted to a new environment. This does not mean that the original race will perish. Why starfishes should disappear because sea-urchins have (according to Prof. Dunkerly) been evolved from them, I quite fail to understand. They fill different places in the economy of Nature, and to say that any of them "share the same environment" is scarcely so true as would be a like statement about a groom and his horse. I wonder what my friend Dr. W. K.

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