III.

I DO not wish to continue the discussion on this subject, but I think that Dr. Wallace has not, in two instances at least, in his letter published in the November number of NATURAL SCIENCE, quite understood my arguments.

Dr. Wallace states that he cannot understand why the connection between Africa and India by means of existing continental areas would not suffice for the explanation of certain relations between the faunas and floras of India and the Mascarene Islands, and why I should prefer to bridge an ocean between 2,000 and 3,000 fathoms deep, in order to reduce the distance slightly. I may say that I do not regard the question of distance as material; and the majority

of the assumed zoological and botanical migrations, though not all, might have taken place equally well by either route, but the geological evidence appears to me only consistent with the former existence of a belt of land across what is now in places deep sea.

This geological evidence is, briefly: (1) That South Africa, India, and Australia, in late Palæozoic and early Mesozoic times, formed parts of one continental area, and that this area was separated by a wide sea, probably of considerable depth, from Northern Asia, Europe, and North America, which were also connected by land; (2) that in Cretaceous times part of Western India, a little north of Bombay, and also the South Coast of Arabia, were part of a sea that extended over a wide area in South-western Asia and in Europe, and that the Khasi Hills in North-eastern India, Trichinopoly, south of Madras, and Natal, in South Africa, were on the shores of another sea, divided from the first by a land barrier; (3) that, as shown by Neumayr, there are indications of this same land barrier in Jurassic and Neocomian times. I do not see how these facts can be explained by land connections within the present 1,000-fathom limit, and as it has been shown that a depression of part of the old barrier, the Mozambique Channel, to a depth of upwards of 1,000 fathoms, has taken place within comparatively recent times, almost certainly since the Miocene, and probably since the Pliocene period, it is not improbable that another part of the old barrier may have undergone depression to a greater depth, even to 2,000 or 3,000 fathoms in places, since the Cretaceous or Eocene. It must not be forgotten that the connecting barrier, which I believe to have once existed, is represented by a tract of ocean still dotted over with islands and shoals; that the soundings are far from sufficient to show the true contour of the ocean basin; and that there is, so far as I am aware, no geological or biological indication of probable land connection in Palæozoic or Mesozoic times in the direction preferred by Dr. Wallace.

The other point is of no great importance. Dr. Wallace says that my suggestion of a possible Mesozoic girdle of land, chiefly in low latitudes, from Peru to New Zealand and the Fiji Islands is inconsistent with the permanence of the great ocean basin of the Pacific. He must, I think, have overlooked the circumstance that the supposed girdle in question was explained as passing through Australia, India, Madagascar and Africa, and not across the Pacific Ocean (*Proc. Geol. Soc.* 1890, p. 106).

The real difference between Dr. Wallace's views and mine is, I think, this, that Dr. Wallace regards ocean permanence as an established law, only to be disregarded on the clearest evidence, while I look upon it as a theory supported by some important and valuable data, but by no means proved; and I contend that there is ample proof that even if the law of permanence prevails it is not universal. As a result of this, I think that every questionable case must be judged upon its merits, and if the evidence tends to show that land formerly

existed where deep sea is now, or *vice versâ*, that evidence must not be summarily dismissed because it is opposed to a theory that, as it appears to me, is very far from being satisfactorily established.

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