DISCUSSION.

CONTINENTAL DRIFT, EIN MÄRCHEN

In the April number of the AMERICAN JOURNAL OF SCIENCE, my well-balanced colleague, Chester R. Longwell, who leans neither backward nor forward in scientific discussion, analyzes the evidence for continental drift, adopting what seems to be a most cautious attitude of skepticism. I would like to join him on the fence, but I cannot. I confess that my reason refuses to consider "continental drift" possible. This position is not assumed on impulse. It is one established by 20 years of study of the problem of former continental connections as presented by Wegener, Taylor, Schuchert, du Toit, and others, with a definite purpose of giving due consideration to every hypothesis which may explain the proven facts. But when conclusive negative evidence regarding any hypothesis is available, that hypothesis should, in my judgment, be placed in the discard, since further discussion of it merely incumbers the literature and befogs the minds of fellow students.

It is my firm conviction that the laws of mechanics and dynamics govern terrestrial structures in their developments and movements, whatever the magnitude of the masses and forces involved. Continents present no exception. The following is a test in point. The original postulate of the theory of Continental Drift is that South America drifted away from Africa to a distance of 2,000 miles at a rate of westward progress sufficient to push up the Andean mountain chain. Wegener says (page 3, 3rd ed.) "By the westward drift of the two Americas, their anterior margin was folded together to form the mighty range of the Andes as a result of the opposition of the well-cooled and therefore resistant floor of the Pacific." Now, it is a well-established principle of mechanics that any floating object moving through air, water, or a viscous medium creates behind it a suction of the same order as the pressure developed in front of it. This law applies equally to airplanes, ships, rafts, and drifting continents (if there are any). The pressure which could raise the Andes must, therefore, have been approximately equaled by the suction and tension in the rear. Sections of the continent must have been sucked off. They should now remain as islands in the Atlantic; but there are none such. Moreover, if such segments had been pulled off, the eastern outline of South America should not so closely resemble the other side of the supposed original fracture.

The close similarity of the two coasts presents us with the choice: either the laws of dynamics were suspended to preserve unaltered

the rear outline of the drifting continent or the continent did not drift.

Any engineer, confronted with the task of moving a continent, might well inquire by what force? Wegener suggested two: a tendency from the poles toward the equator and a "westward drift." The former is quite inadequate and should have been satisfied in early stages of adjustment, a thousand million years before the suppositious displacement, if ever. The second he merely infers, saying: "The westward drift of the continent becomes still clearer by a study of the map of the world. The large blocks move westwards in the sima." He does not say why (pages 191-192) Du Toit frankly faces the problem, but leaves it unanswered. Referring to the assumed break up of the hypothetical Gondwana continent he says: "The precise mechanism is not readily visualized." After assuming the extension of rift valleys into the continental mass, he remarks that "sliding began;" he does not, however, explain the conditions producing sliding, but concludes: "The dispersion of the continental fragments must have been a highly complicated business and not one lending itself to full mathematical analysis." This is begging the question. In general the attitude of advocates of the Theory is that since continents did drift there must have been some competent mechanism of some kind.

Fellow scientists who are not geologists cannot be expected to know that the geology upon which protagonists of the Theory rest assumptions is as antiquated as pre-Curie physics. Wegener and his successors are disciples of Edouard Suess, the Master of European geologists. I knew him well: a charming, genial German, who never traveled far, but assembled the observations of others and from them constructed speculations regarding the face of the Earth. His reading was prodigious, his memory marvelous, his imagination grand; but he gravely lacked critical faculty. And when some airy concept had grown in his mind, it became too firmly rooted ever to be dislodged.

Such a concept was Gondwana Land, the continent supposed to have extended from the East Indies westward to the Pacific, embracing India, Africa, and South America and occupying the sites of the Indian and South Atlantic oceans. It had no actual extstence. It was conceived to account for the transoceanic migrations of plants and terrestrial organisms, the identical line of reasoning of the argument for Continental Drift. In Suess's imagination, it was a reality; but there is no reality, no geologic fact to demonstrate the one time existence of such a mass as it would need to have been.

Charles Schuchert, our great paleontologist, was also a disciple of Suess and believed in Gondwana Land till convinced on the

evidence of modern geologic research that no such expanse of continental nature could have existed and have so completely disappeared. He eventually modified its outlines to those of a narrow land bridge or bridges, as stated in his paper, Gondwana Land Bridges, 1932.

Wegener followed Suess in the assumption that the Pacific is older than the Atlantic and upon that postulate based the concept that it had cooled further and become stiffer. Therefore it offered the resistance that stopped South America and folded up the Andes. This is mere wishful thinking. No one knows which of the two ocean basins is older; they may be more or less contemporaneous. But it is known that there was pronounced activity in and around the Pacific basin during the late Mesozoic and Tertiary eras, at the time when drifting South America should have been stopped by the congealed ocean bed. And it is also true that similar activity in the Atlantic realm had ceased more than 100 million years earlier. The facts are just the reverse of the postulates.

Furthermore, the assumption regarding a deep continental mass or block is erroneous. Wegener wrote (p. 4): "It is assumed that the continental blocks, with a thickness of about 100 km., swim in a magma out of which they only project about 5 km., and which is uncovered in the floor of the oceans." It is now known on seismological evidence that the granite of which continents chiefly consist has been erupted in a number of more or less contiguous bodies, at different times, some of them quite recently. The molten granite has come up from and rests on top of a general basaltic shell, and varies from 3 to 30 kilometers, more or less, in thickness. It may be described as a scum, like slag as it were. It completely lacks the unity, the solidity of a "block." It could not drift except as flotsam in a current of the basaltic sima, which according to hypothesis and in fact did not flow.

Thus the theory of continental drift is a fairy tale, ein Märchen. It is a fascinating fancy which has captured imaginations. It is one of a long line: Werner's hypothesis of the sedimentary origin of all rocks by precipitation from a universal ocean, which the charm of his personality and the force of his conviction maintained for forty years; Suess's pure assumption that there is no force stronger than gravity and any broad uplift of a part of the earth's crust an impossibility, which was disproved when W. M. Davis demonstrated the uplift of central Asia on physiographic evidence that is now beyond dispute; Lugeon's concept of the nappes de recouvrement by which the Alps were piled up to 40,000′, which was based on assumed simplicity of pressures and mistaken postulates of folding in lieu of shearing. They challenge by their stupendous appeal to the imagination and by the implication that there is no other explanation.

In the case of Wegener's theory, I think there is. The biological, botanical evidence of migrations by land where no land now is must be accepted as conclusive. But what is land? Let us define it as an area of the earth's surface which stands at present above sea level, or which in some former age was land when it so stood. For dynamic reasons areas of granite commonly rise to that position; but the lifting force must develop beneath them and may equally well develop beneath an area of the basaltic ocean bed.

Is the relation to sea level a permanent condition? There is abundant evidence in marine fossils raised to high altitudes and also in sediments piled to thicknesses of thousands of feet on subsided lands that it is not. We may recur to the fact of the eighteen thousand foot uplift of Tibet during the latest geologic epochs as proof of local increase of the earth's radius; an increase which on the evidence of gravity measurements is not due to added mass and must be attributed to augmented volume, that is to expansion; as of rising dough. William Bowie, the isostasist, cogently called the attention of geologists to the importance of changes of level attributable to changes of volume.

The causes of volume change beneath the crust are obscure, because we cannot measure the conditions or observe the effects in the laboratory of the earth. It is not merely that we cannot dig to such depths; the changes are so infinitely slow; a million years or ten millions being required for any marked change. But recent advances in geologic knowledge furnish some significant points regarding the processes at work beneath our feet.

It is established that the outer crust of the earth is a mosaic of relatively small erupted bodies, chiefly basaltic, partly granitic, which have been extruded in a molten condition at various times during the past two billion years and down to times so recent that the latest masses are probably still molten a few miles below the crystallized crust. Also it is known that these small molten bodies have been extruded from a solid shell 1800 miles thick, in which they presumably develop by melting, in consequence of the local generation of heat by disintegration of radioactive minerals. In the process of melting such a body expands and also generates gases, such as escape from volcanoes. The direction of least resistance is upward and the volume change must express itself by raising the overlying crust; as in simmering mush. The rate of heating is excessively slow; that of cooling by escape of gases and eventually by outflow of lavas becomes relatively more rapid, but is still geologically slow. The heating should result in uplift, the cooling in subsidence of local areas of the crust, a hundred to several hundred miles in diameter over the subterranean bodies.

Prof. Douglas Campbell of Stanford has recently called attention

to the remarkable likenesses that are found in the floras of Hawaii, the islands of Oceanica, Australia, and South America. He proves beyond question that there have been land connections between these now widely separated districts. At one time I would have sought to trace the connections as mountain ranges, as I did when coöperating with Charles Schuchert on the similar problem of identity of terrestrial organisms in Africa and South America in Permian time. The concept of isthmian links appeared sound, biologically, dynamically, and climatologically, as tested by the geographical requirements of the case. It does not, however, fit the Pacific conditions, except perhaps to link the Antarctic continent with Australia and South America. The connections in Oceanica are too broad and too complex to be explained by mountain ridges. The alternative assumption of uplift, followed by subsidence of the ocean bed, of the emergence of lands which have now subsided beneath the waters is more reasonable. That it contradicts certain preconceptions of mine regarding the permanent levels of continents and ocean beds does not affect the evidence. In the Philippines the Manila basin is a subsiding area. It is surrounded by volcanoes from which lavas are erupted and gases are constantly escaping. I attribute the subsidence to these conditions in a cooling mass a hundred miles in diameter. Closely adjacent lies the similar area of the Sibuyan sea, where raised coral reefs demonstrate active uplift. I consider the uplift to be due to increase of volume of a subjacent body in which the temperature is rising. Similar conditions on a larger or smaller scale are widespread throughout the southwestern Pacific. To them I would attribute the former expanse of land which once bore the now dispersed floras and also its disappearance beneath the waters. The average depth of the Pacific, 4,000 to 5,000 meters, is not excessive in comparison with known uplifts and subsidences in continental areas.

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