OBSERVATIONS ON THE CAUSES OF ELEVATION AND SUBSIDENCE OF THE EARTH'S CRUST.

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Any enquiry into the causes which have produced, and are still producing, mountains and mountain chains cannot be satisfactorily carried out apart from the study of the causes which originally produced the grand irregularities of surface, which primarily determined the original areas of continents and oceans.

In offering any opinion of my own upon the merits or defects of the rival hypotheses assumed by so many eminent physicists to account for elevation and subsidence, I hope it will be conceded that I do so with much diffidence; for where so many eminent men fail to be satisfied with each other's views on a subject which necessarily rest so largely on arbitrary values for unknown data, it is obvious that any view which I may tend to favour must in my own mind be attended with a large measure of doubt, and on some obscure points my judgment may be suspended entirely. As a novel theory, however, has recently been referred to by Mr. F. Danvers-Power, relating to the elevation of mountain chains, it may be of some interest at this time to enter more fully into the consideration of those hypotheses of causation which have gained the most favour among physicists and geologists. Of course the correct observation of effects and the true interpretation of immediate or primary causes are very different things. The geologist, as such, is entitled to speak with authority as regards the former, but undoubtedly the correct interpretation of causation is more purely within that realm of science where the physicist, as such, has the greater claim to be heard. The geologist may best know what are the magnitude and characteristics of certain obvious changes in a given region, but his interpretation of the cause or causes at work—not so open to observation—may be liable to errors from which the knowledge of the physicist might have saved him. The apparent cause to the geologist may, to the mind of the skilled physicist, be altogether objectionable when tested by his more exact methods for determining its nature and efficiency. With some the mere parallelism of certain phenomenal features—great ocean boundaries and certain mountain chains—often appear to stand as cause and effect. This may be so; but is it true? The answer to this query will be given hereafter. As it is desirable before

discussing any one view to summarise briefly the principal hypotheses which have gained the greatest acceptance among scientific observers generally. The evidences for the vast extent of movements of elevation and subsidence, past and present, are so well established that I need only confine my attention to the leading theories concerned in their causation.

Formation of Continental Areas and Ocean Basins.

Assuming that at first the earth was a heated sphere around which the existing waters of ocean seas and rivers were gathered in the form of a gaseous envelope, we are led to conclude that the radiation of heat, immediately prior to the cooling and consequent condensation of water vapours, would gradually tend to form a solid crust. The question of importance at this stage is: Was the surface matter homogeneous and smooth immediately before and after it hardened into a crust, or was it heterogeneous, containing irregularities of surface? If the former, we could not imagine how the condition for determining continental areas and oceanic basins could be initiated. We are therefore led to accept the alternative hypotheses as more in accord with known facts. But the root matter here contemplated involves the conclusion that prior to the cooling and condensation of the gaseous vapours, which eventually occupied the primæval ocean basins, the forces of themselves then at work were equal to the task of producing irregularities on the earth's surface, whether by contraction expansion, or transfer, sufficiently grand in scale to form more or less elevated continental areas in the midst of basins grand enough to receive the oceanic waters. There may not have been at this stage either deep abyssal regions on the one hand nor high mountain peaks on the other, but it is almost certain that forces then at work sufficed to produce such relative elevation and depression, as are now supposed by many to be only rendered possible by changes in the loaded surface caused by the transfer of superficial solid matter mainly through aqueous agencies. A diversified distribution of the surface magma is assumed with good reason by Mallet, J. D. Dana, Prof. Hennessy, Archdeacon Pratt, Geikie, and many other eminent physicists and geologists, as a primary condition; and this primary condition, owing to the unequal rates of cooling, and differences of density of different masses of magma, is assumed to be the initial factor in producing elevated and depressed surfaces. Dana in his last work ("Manual of Geology," 3rd edition, 1879) states:—"The fact that the continental and oceanic areas were determined in the first cooling of the globe signifies that

in the cooling or the radiation of heat into space there were areas of greatest and least contraction. This difference in cooling and the resulting level of the surface must have been owing to some difference of quality or condition in the material." One quality has been brought to light by pendulum experiments in India, proving that gravity is greater at the coast stations than at the continental stations, and greater at the island stations than at the coast stations. In harmony with these observations are the opinions advanced by Archdeacon Pratt (The Figure of the Earth, 1860; on the Constitution of the Solid Crust of the Earth, *Nature*, 1871) who first attributed the origin of oceanic depressions and continents, and also of mountain chains, to unequal contraction in a cooling globe, but in the last refers the formation of mountains to lateral pressure, and there concludes "That the crust beneath the ocean is of greater density than the average portions of the surface—that is, where the contraction was greatest the density of the rich material below is greatest and proportionately so." Mr. George H. Darwin, in investigating the bodily tides of viscous spheroids (Phil. Trans., 1879), among other important matter also points out that under the combined influence of rotation and the moon's attraction the polar regions tend to outstrip the equator, and to acquire a consequent and slow screwing motion from east to west; and Dr. Geikie, who has certain objections to offer, admits in respect of Darwin's theory, that it is conceivable in earlier conditions of the planet, that this screwing motion may have had some influence in determining the surface features of the planet. In a body not perfectly homogeneous it might originate wrinkles at the surface running perpendicular to the direction of greatest pressure. "According to this theory the highest elevations should be equatorial, and should have a general north and south bend, while in the northern hemisphere the main direction of the masses of land should bend round towards the north-east, and in the opposite hemisphere towards the south-east." While there are many difficulties standing in the way of the full acceptance of Mr. Darwin's theory, Dr. Geikie still thinks "It is well worth consideration whether the cause suggested by Mr. Darwin may not have given their *initial trend* to the masses of land, *so that any subsequent wrinkling of the terrestrial surface due to any other cause would be apt to take place along the original lines.*" I might also add that to such influence in some measure might be still ascribed those remarkable lines of weakness in both hemispheres, which mark the course of volcanic action, and which, in a large measure, *may determine the curves of certain coastal mountain chains.* Apart, however, from the universality or antiquity of alleged causes advanced as the principal factors in mountain making (I here use the

word mountain-making in preference to mountain-building, for the latter term is more appropriately restricted to peaks and cones built up by volcanic agency), it must be confessed that whatever mountain chains were formed in the earliest geological periods they have been largely obliterated in later times. This is becoming more evident as geological observation extends; for it is now well established that in Asia, South America, North America, and in Europe, the great era of mountain making was during the Tertiary period, and even in Australasia it is probable that its principal Alpine ranges were inconspicuous ridges during the early part of the Mesozoic period. Still the intensity and universality of the elevations then produced demand a corresponding universality of cause, or causes. A considerable number of hypotheses at different times have been proposed by eminent investigators, all of which commend themselves in some particulars, but all of which, taken separately, involve difficulties which at present appear to be insurmountable. Perhaps the truth lies in several, and, *at least*, does not demand the rejection of one cause because another seems to answer all the conditions when confined to a particular case. It appears to me, however, that there is one which may be well considered as the governing or mother cause to two or three of the rival hypothetical causes; the latter being consequents standing in the relation of child to parent rather than absolutely independent causes. The parent cause, in my opinion, appears to be the Contraction Theory so intimately associated latterly with the name of Robert Mallet.

Contraction Theory.

If our planet has been steadily losing heat by radiation into space a corresponding diminution in its volume must also be looked for; for cooling is implied in contraction. A succinct account of the effects which Mr. Robert Mallet demonstrated might be expected from the secular cooling of our globe is given as follows by Dr. Geikie:—"According to Mallet the diameter of the earth is less by 189 miles since the time when the planet was a mass of liquid. But the contraction has not manifested itself uniformly over the whole surface of the planet. The crust varies much in structure, in thermal resistance, and in the position of its isogeothermal lines. As the hotter nucleus contracts more rapidly by cooling than the cooled and hardened crust the latter must sink down by its own weight, and in so doing requires to accommodate itself to a continually diminishing diameter. The descent of the crust gives rise to enormous tangential pressures. The rocks are crushed, crumpled, and broken in many places. Subsidence must have been the general rule, but every subsidence would, doubtless, be accompanied with upheavals of a more limited

kind. The direction of these upheaved tracts, whether determined, as Mr. Darwin suggests, by the effects of internal distortion or by some original features in the structure of the crust, would be apt to be linear. The lines, once taken as lines of weakness or relief from the immense strain, would probably be made use of again and again at successive paroxysms or more tranquil periods of contraction. Mr. Mallet has ingeniously connected these movements with the linear direction of mountain chains, volcanic vents, and earthquake shocks. If the initial trend to the land masses were given as hypothetically stated by Mr. Darwin we may conceive that after the outer parts of the globe had attained a considerable rigidity, and could then be only slightly influnced by internal distortion, the effects of continued secular action would be seen in the intermittent subsidence of oceanic basins already existing, and in the successive crumpling and elevation of the intervening stiffened ridges." To overcome the difficulties which arise out of one or other of these hypotheses various modifications of the contraction theory have been generally adopted, the greater number still adhering to the view that the *main cause* of terrestrial movements must be sought in secular contraction. The theory which seems to be most favoured next in importance to the Contraction Theory is:—

> The alleged expansion and contraction of the underlayers resulting from a rise or fall of temperature caused by the loading or unloading of the areas affected.

Great uncertainty still exists as regards the present condition of the earth's interior; some conceiving it to have a fluid or viscous central core; some again incline to the view that a viscous intermediate layer separates the solid crust from the solid nucleus; while others have given reasons for the view that on the whole it has now attained a rigidity equal to that of glass or steel.

Expansion and Contraction of the Underlayers Resulting from a Rise or Fall of Temperature.

Babbage seems to have been the first to suggest this theory. In his memoir (1834) on the Temple of Serapis, "Besides recognising the relations of isothermal planes, and the effect upon them by the surface changes, whether removals of rock material, or accumulation, the memoir accounts for changes of level caused through the expansion or contraction caused by changes in the subterranean heat or in the position of these isogeothermal planes." Mr. Mellard Reade has lately, "Origin of Mountain Ranges," strongly insisted on this factor as an important if not the main factor in producing subsidence and elevation. Similar views have also been strongly advocated

by some of the American geologists who have explored the western territories of America. These have pointed "in proof of its truth to evidence of continuous subsidence in tracts where there was prolonged deposition and of the uprise and curvature of originally horizontal strata over mountain ranges like the Uintah Mountains in Wyoming and Utah, which have for a long time been out of water." Dr. Geikie, in commenting upon this theory, admits that in so far as the internal structure of rocks may be modified by such progressive increase of temperature as would arise from superficial deposit, the cause of change must have a place in geological dynamics, but he cannot allow that the removal and deposit of a few thousand feet of rock should exert such an influence as to affect the equilibrium of the crust; for to admit this, "would evince such mobility in the earth as could not fail to manifest itself in a far more powerful way under lunar and solar attraction." He, however, goes on to say "that there has always been the closest relation between upheaval and denudation on the one hand and subsidence and deposition on the other, is undoubtedly true." But he adds the significant words that "denudation *has been one of the consequences* of upheaval, and deposition has only been kept up by continual subsidence." Certain questions bearing upon the permanence of continental areas and great oceanic basins are involved to a great extent in the views under discussion, and may have led to the adoption of restricted views as to fundamental laws of causation.

Prof. C. Loyd Morgan in a recent article on elevation and subsidence (Geol. Mag., July 1888) in criticising the views of Mr. Mellard Reade and others as regards the effects alleged to arise out of the transfer of sediments suggests other ways in which the loading and unloading of the earth's crust may indirectly bring about subsidence and elevation, and at the same time ingeniously advances reasons in favour of the existence of an underlying liquid or viscous substratum. Accordingly, without committing himself to the acceptance of the theory held by those who attribute subsidence to mere weight he suggests to the upholders of that theory that the added weight of the sediment above would entail on this hypothesis an added weight below---that is if we suppose that the solidified rock adheres to the lower surface of the crust region. In a region undergoing denudation, on the other hand, the lightening of the load would entail the melting of some of the solidified or crystallised magma, assuming with Mr. Mellard Reade and Mr. Davison, that owing to the cooling and contraction of the earth's crust there is at some depth beneath the surface a level of no stress, where there is neither lateral compression nor extension, though the rocks are subject to the vertical pressure of the overload. He then suggests that throughout the zone of maximum tension

due to circumferential contraction the rocks may be rendered fluid by relief of pressure. Such melting, he adds, would be accompanied by expansion manifesting itself at the surface by an uplift. By the expansion of the melting underlayers tensile stress in the overlying strata would be called into play, and this would throw those strata into a state of tensile strain, thus giving origin to normal faults to the gradual gaping of mineral veins and dykes, into which the molten matter would be injected by the expansive force. Without denying, therefore, the influence of secular refrigeration, he suggests that we have on this hypothesis an efficient primary cause of volcanic eruptions. Commenting upon the differences of opinion as to the level of the stress he points out that according to Mr. Davison it lies five miles deep; according to Mr. Mellard Reade it is taken at one mile; while Mr. O. Fisher would reduce it to less than a mile; and accordingly Prof. Morgan urges caution as to the use of precise imposing mathematical calculations based upon arbitrarily selected data where it concerns problems "concerning which the most noteworthy feature is our profound ignorance."

Mr. Danvers-Power's Reference to the Supposed Influence of the Pressure of Ocean Waters upon the Formation of Parallel Mountain Chains.

In regard to this hypothesis I must confess that to me it seems to be a most extraordinary one. Mr. Danvers-Power does not give us the slightest indication to help us to conceive how, of all agencies that may be concerned in the dynamics of mountain making, "the presence of the ocean is the greatest." It is hardly necessary to remind us how great is the influence of water in the work of denudation, and in the redistribution of wasted rock sediments over lower levels of sea and land; but how the gravity or even surface movements of ocean waters extending over wide ocean areas can concentrate their force of gravity or pressure laterally by thrust upon the margins of continents is a nice puzzle to any physicist. Surely it must have occurred to Mr. Power that the vertical radii of a column of rock is from $2\frac{1}{2}$ to 3.4 times the specific gravity of an equal column of water; that the radii of a continental area are much greater than the radii over the more depressed ocean areas, capped as they are with the light element water; that the pressure of a fluid upon any of its limiting surfaces is altogether independent of its quantity (known as the *hydrostatic paradox)*; that is—the total pressure of water, (still) against and perpendicular to any surface is equal to the weight of a uniform column of water, the area of whose cross-section parallel to its base is everywhere equal to the area of

the surface pressed; and whose height is equal to the *vertical* depth of the *centre* of gravity of the surface pressed below the surface of the water. It is a natural mistake among young students of hydrostatics to fall into the error that the magnitude of the horizontal expanse of any body of water intensifies the pressure upon its limiting surfaces; but the determining factors of pressure given hereinbefore shows that the horizontal pressure upon the vertical face of any wall or embankment, say 100 feet deep and 100 feet wide, would just be the same if the width were extended 1,000 miles or to any conceivable extent *so long as the area pressed* and the vertical depth of its centre of gravity below the level surface remain unchanged. That Mr. Danvers-Power has fallen into this elementary error as regards the lateral pressure of oceanic water is beyond doubt, for there is no escape on the plea of misconception or ambiguity in the following statement intended by him to show that greatness of *expanse* or area of ocean intensifies lateral pressure upon coast lines. Thus in accounting for the parallelism of high chains of mountains with great ocean basins, he states—"We almost invariably find that the highest mountain ranges have been developed more or less parallel to the past or present sea-coasts that are washed by the largest bodies of water. Thus, in South America, we find the Andes on the west higher than the mountains of the east coast; also that the western shores of America are washed by the Pacific Ocean, which is greater in area and depth than the Atlantic. Coming nearer home for an example we find the highest Australian Mountains on the East Coast, the Pacific again being larger than the Indian Ocean. In Tasmania, however, matters are somewhat different, for there is an unbroken stretch of water from its West Coast to South America; while on its East Coast some twenty degrees distant *New Zealand acts as a breakwater*, and braces up the ocean so to speak, relieving the East Coast of Tasmania of much pressure from the main body of water;" and further on he again states:—"We not only have the horizontal pressure *of the ocean*, but also the vertical pressure." In his reference to Tasmania he is extremely unfortunate, for there is nothing more probable than that Tasmania, as *a whole*, forms but a southern prolongation of the Eastern Alps of Australia, and the causes which led to the original determination of the main chain operated in the formation of its southern extremity. And again he does not recognise the fact that the highest Alps of the Australasian region run along the *Western Coast* of New Zealand in a line north easterly and parallel to the south eastern coast of the mainland of Australia, and are not found on the eastern border, which is washed by the full sweep of the South Pacific Ocean, to whose influence he ascribes the smaller Alps of the Eastern border of the mainland of

Australia. But apart from such consideration it is clear from his observations that he regards the largeness of the *ocean area* as a factor in intensifying the horizontal pressure of its waters, and thus runs counter to one of the elementary laws determining the horizontal pressure of fluids. When we consider this limitation we can perceive how insignificant must be the influence of lateral pressure of ocean waters upon coast lines when compared with the enormous force required to bulge the solid earth into mountain chains. Again, as regards the energy of water set in motion by winds, and the currents induced by lunar and solar action, it is clear that the amount and direction of such forces do not harmonise so closely with the direction of the lateral thrusts, which have produced those remarkable chains of mountains like the Andes, that their influence *should be added* to the supposed influence of the pressure of oceanic waters already referred to. As regards the reference to Dr. Carpenter I cannot see that the quotations given have any bearing upon the pressure exerted by oceanic *waters* on their land border. Indeed, Mr. Danvers-Power does not seem to be aware that the last quotation referred by him to Dr. Carpenter is nearly exactly word for word identical with, and, no doubt, adopted from Dana's theory of the elevation of mountain chains parallel to the great oceans. (Dana in p. 828, Manual of Geology, 3rd edition, 1879). Dana here affirms (prior to Dr. Carpenter), "The fact that the largest and loftiest mountain chains, greatest volcanoes, and other results of uplifting and disrupting force characterise the borders of the largest oceans, shows that the shoving *action from the direction* of the oceans was approximately proportional to the ocean *basins*; but Dana clearly explains that this shoving motion is not due to the superficial waters contained in these basins, but that " the landward action of the force seems to be a necessary consequence of the fact that the *crust* over the oceanic areas was, and is abruptly *depressed below* the level of the continental, so that the lateral pressure from its direction would have *the advantage of leverage beneath the continental crust*, or rather would have acted *obliquely* upward against it." There is a fundamental difference here between the lateral pressure of ocean water, and the oblique upward lateral thrust of the ocean bed, and Dr. Carpenter's quotation is in entire harmony with Dana's theory, and gives no support to Mr. Danvers-Power's *ocean water theory*. The idea originated by Archdeacon Pratt that the rocky material under the ocean is more dense than that under continents has also influenced Dana in ascribing greater stiffness or rigidity to oceanic areas, and so determining in some measure the peculiarities connected with mountain chains running parallel to the great ocean. At any rate their arguments in no way support the novel theory of Mr. Danvers-Power. As regards the parallelism observed between

oceans and mountain chains, I have only to observe that such parallelism need not be interpreted as cause and effect. Although, if we assume original flexures on Darwin's theory, trending in the present direction of these chains it is certain that if the continental areas so sculptured were slowly submerged in the direction of the great ocean areas, that the basal contour lines of height would certainly form the barriers to the ocean basins, and these of necessity would produce the parallelism referred to—*i.e.*, the downward sinking of the land would better explain the parallelism observed than the reference to pressure from the direction of the ocean, whether from its bed or its superficial waters. I think, therefore, that good reasons have been given for rejecting the theory put forward by Mr Danvers-Power.