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THE ORIGIN OF MOUNTAIN RANGES

The Origin of Mountain Ranges considered Experimentally, Structurally, Dynamically, and in Relation to their Geological History. By T. M. Reade, C.E., F.G.S. (London: Taylor and Francis, 1886.)

THAT the skin of our mother Earth's face is wrinkled and shrivelled, is one of the common facts of geology. True it is that with fine feminine instinct she strives to hide the ravages of time beneath a fair covering of grass, moss, and herbage, and gracefully does her best to make her old age comely; and we love her for her skill in covering up the signs of her years. But we are fain, directly we look below the surface, to admit that the wrinkles are there. And the parallel is not a fanciful one, for it has long been a favourite theory among geologists that the wrinkled skin of old age, and the foldings and bendings which are everywhere to be discerned in the layers of the skin of the earth, are due to similar causes. In both cases, something underneath the skin, which in youth kept it stretched and tense, has shrunk away, and the skin has shrivelled up. In the case of the earth it is the gradual contraction of the interior as it cools, which has caused it to draw away from the outer shell; and the crust, as it follows down the shrinking nucleus, has to pack itself into a smaller space, and consequently becomes crumpled up. This explanation is known as the "Contraction Hypothesis." Numbering as it does many supporters, it has had at the same time some vigorous opponents. In his "Physics of the Earth's Crust," the Rev. Osmond Fisher was led to the conclusion that the contraction hypothesis would not furnish anything like the amount of elevation that has actually occurred in the case of the earth. We admire the ingenuity and elegance of Mr. Fisher's mathematical work, but we cannot help recollecting Prof. Huxley's warning, that mathematics is like a mill, and that what you get out of it depends entirely on what you put in. Mr. Fisher puts in a supposition made by Sir W. Thomson, as to the way in which the earth cooled. There have been people bold enough to think that in making this supposition a great master of physics for once lent his name to an hypothesis which is in itself physically not very probable; and these same people are inclined to hold that probably Mr. Fisher's calculations tend to show that this is the case, rather than that the contraction hypothesis is inadequate.

Capt. Dutton, of the United States Geological Survey, is another doughty opponent of the contraction hypothesis. His notion as to what that hypothesis appears in the following passage, which is quoted in the volume before us (p. 126, note):—

"The line of argument which is relied upon to sustain a cooling globe proves, when pushed to its consequences, that the great interior of the earth has not as yet undergone any sensible amount of cooling. The only cooling which that argument admits of has been located in a thin external shell. . . . In short, the cooling would be only skin deep, while the nucleus is about as hot as ever."

This may be called "pushing an argument to its consequences"; but if it be, that phrase certainly means, in

plain English, putting, of course unintentionally, into your opponents' mouths, statements which they never made. If it is asserted that the crust cools faster than the nucleus, which is all the supporters of the contraction hypothesis ask for, is this the same as saying that the nucleus does not cool at all?

Now Mr. Mellard Reade joins the attack, and in an elaborately illustrated volume of some 300 pages gives us his reasons for dissenting from the contraction hypothesis, and for preferring a modified form of the explanation put forward originally by Scrope and Babbage. These geologists pointed out that whenever a great thickness of sedimentary deposits was laid down the subterranean surfaces of equal temperature would necessarily rise, the increase in temperature would cause expansion, and as a result of this a rise in the surface would follow. Mr. Reade maintains that vertical elevation would not be the only result, but that pressures would be set up in the mass competent to produce folding, contortion, inversion, crushing, and all the violent disturbances which are found in mountain-chains and other disturbed portions of the earth's crust.

The book has two merits: it takes nothing for granted, and it does not err on the side of assuming too much knowledge on the part of its readers. But it is a question whether virtue may not run to excess, and there is reason to fear that this has been the case here, for a very large part of the volume is taken up with the establishment and illustration of physical facts of the most elementary character, and of geological truths which are to be found in every text-book. For instance, Chapters III. and IV. are devoted to the establishment of the facts that metals and stone expand when their temperature is raised; that, when they are prevented by constraint from relieving themselves by lateral expansion, they buckle up; and that if their elasticity, or, as the author prefers to call it, their tensile strength, is small, they do not return to their original shape on cooling. The cases quoted, and the experiments which are illustrated by six full-page plates, are apt and to the purpose; they would be admirably suited for illustrating class-teaching in an elementary school. But it is hard to believe that any one could have seriously thought they would be required by the class of readers to whom the book is presumably addressed.

Chapter V. opens with the statement that "It has been a subject of remark and wonder to more than one eminent geologist that all the greatest mountain ranges are, geologically speaking, so comparatively modern." A broad generalisation like this deserves to have stress laid upon it, but it has been so long one of the common truths of geology, that it has ceased to be, if it ever was, a source of wonder to any one who has an elementary knowledge of the science. Indeed, there is an air of *naïve* surprise running through the book which now and again moves a kindly smile as we read, and we feel it refreshing to discover that truths, which have grown somewhat hackneyed to the majority of geologists, still retain a certain charm of novelty for the author. Thus Mr. Reade seems in more than one passage to take a little credit to himself for having discovered that what are generally called "anticlinals" are really elongated ellipsoidal domes—a fact which is rapidly brought home to any one who happens to work for a few weeks in a

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country where the rocks are moderately folded, and which was insisted on and fully illustrated in a manual of geology published fourteen years ago, and then could hardly have been said to be new. The distinctive characters of mountain chains are treated of in the same spirit. Little, if anything, is here added to the masterly summary of these characters which Dana gave us years ago, but much space is taken up with illustrations of truths which every one admits, in the form of long quotations from sundry sources. All this speaks to much reading and patient industry, and we cannot but admire the conscientious care with which Mr. Reade has striven to inform himself of all that has been said and done on the subject of which he is treating; but we cannot help asking ourselves whether it was necessary to print at length the contents of his note-books. This accumulation of evidence, where it is not needed, has brought with it an attendant evil: it has swelled the book to an undesirable size. Now in these busy days, and in the interest of readers, if there is one thing against which, more than anything else, a resolute stand ought to be made, it is unnecessary printing. The day is yet far distant when every page of printed matter shall contain something that is new, and nothing that is not new; but this is the impossible ideal, the asymptotic consummation, which all writers should ever keep prominently in view.

But we would not press this point, because, even if the author has given us rather an excess of matter that is not new, what he has given is good of its kind; and it is of more importance to weigh his arguments against the contraction hypothesis, and in favour of the Scrope-Babbage explanation with the additions he suggests to it. Holding as he does that expansion by heat is the main factor in producing the disturbances of the earth's crust, he made experiments to determine the coefficients of expansion of sundry rocks. He arrives at a mean which agrees very nearly with that found by Mr. Adie. Mr. Reade's experiments ranged from temperatures of 60° to 220° F., and he assumes that the coefficient of expansion will be the same for the enormously higher temperatures with which he has to deal when considering the case of the earth—a risky proceeding, to say the least. But he has fallen into a far more serious mistake: he has assumed that rocks weighted with a thickness of twenty miles of overlying strata will expand to the same extent for a given increase of temperature as rocks under atmospheric pressure. The oversight involved in this assumption so thoroughly vitiates all his numerical results that no conclusion can be drawn from them.

In Chapter XI. we have the objections to the contraction hypothesis succinctly stated. The numerical results we put aside for reasons just given, but in his general argument the author does not seem to us to realise the full meaning of the hypothesis. He seems to hold that according to the contractionists crumpling is produced by unequal contraction *in the solid shell itself*, which certainly is not their view. And he entirely omits all reference to the one fact which is the life and soul of the hypothesis, that the earth's crust is not strong enough to stand by itself without support, a fact which admits of rigid mathematical demonstration. It is a decided case of a seriously mutilated representation of the play of "Hamlet."

We cannot therefore admit that Mr. Reade's arguments are very damaging to the hypothesis against which they are directed; and we cannot see how expansion due to rise of temperature could alone produce the results which he attributes to it. The strains produced in this way would tend to be relieved by yielding in the direction of least resistance—that is, vertically; and if there were no impediment to the perfect transmission of strain, the yielding would be wholly in this direction. In the actual case a certain amount of deformation would doubtless be produced within the heated mass itself, but hardly enough, it would seem, to cant over a huge anticlinal, and lay it nearly flat on its side. The machinery invoked by the contraction hypothesis may or may not have been the means by which such overthrusts were brought about, but it is the only machinery yet suggested which seems competent to produce them. "Seems," we say throughout, for the question between rival hypotheses is as yet only one of probability.

And we think no one will contend that the Scrope-Babbage hypothesis ought to be entirely put on one side when we speculate on the cause of earth-movements; great broad folds, such as those of the plateau-region of Utah, described by Capt. Dutton, and figured on Plate 39 of the book, may have been caused by the bulging up of heated masses below, though they can be explained equally well by the contraction hypothesis.

It remains to notice that the book is rich in figured illustrations. A number of the plates are devoted to somewhat diagrammatic landscapes of contorted rocks, and these bring out well the points they are intended to illustrate; but they do not add to the stock of our knowledge. They would be serviceable to a geologist, if such a one there be, who had never stirred out of the fen-country, but he of course would do still better if he took an excursion ticket to some of the localities from which the views are taken.

A. H. GREEN

ORGANIC EVOLUTION

The Factors of Organic Evolution. By Herbert Spencer. (London and Edinburgh: Williams and Norgate, 1887.)

MR. HERBERT SPENCER has done well to reprint in a permanent form his two articles on the "Factors of Organic Evolution," which were published last year in the *Nineteenth Century*; for, although they present substantially the same doctrines as are to be met with upon this subject in his "Principles of Biology," they do so in the light of fuller knowledge and more matured judgment.

The object of the essay is that of taking stock, so to speak, of natural selection as compared with other "Factors." Mr. Spencer's treatment of this subject is admirable, and ought to be read by all working naturalists who have any interest in the problems of evolution. The literature of Darwinism has now become so extensive that even first-rate naturalists who are engaged on other lines of work are apt to get left behind, or, with respect to Darwinism, themselves to become examples of what are now called "vestigies": their ideas are the superseded survivals of some previous phase of evolutionary science. And most of all is this true with regard to the funda-