SCIENCE.

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COMMENT AND CRITICISM.

THE LACK of truly demonstrative evidence, in the solution of certain geological problems that have been regarded as settled for years before they are overthrown, finds new illustration in the remarkable results lately announced by the geological survey of Great Britain, which form the subject of a paper by one of the contributors to our paper this week. The conclusion, that now seems to be erroneous, rested on what may be called the argument from continuity of deposit. The same argument, involving the same error, was used by Werner nearly a century ago to prove the aqueous origin of his 'floetz-trap.' These old lava-flows apparently formed part of a continuous series with the underlying sedimentary strata, and hence were thought to be, like the latter, of sedimentary origin; and this conclusion held until an abrupt contact-line, that had previously escaped notice, was found between the dissimilar formations. Precisely the same reasoning has been employed in recent years to support the aqueous origin of the old lavas in the Palisades of the Hudson; but the method of disproving the error in such a case is now too well known, and in this example is too easily applied, to allow any general acceptance of so visible a mistake. In the same way, the essential element in the observations which Murchison and Geikie considered conclusive as establishing the Silurian age of certain Highland schists and gneisses was the continuity of the series, without break by unconformity or dislocation, from the underlying fossiliferous beds to the overlying crystalline members; and, on the strength of their report to this effect, the Silurian age of the now crystalline masses has been for years accepted by many geologists.

Now, it appears that these early observations were too hasty. Examination by more sceptical observers, and recent deliberate official studies mapped on the ideal scale of six inches to a mile, discover a most peculiar discontinuity in the form of a nearly horizontal surface of dislocation, across which the overlying mass has been driven actually for miles from its normal inferior position. Whatever possibilities may be discovered elsewhere, the paleozoic date for the metamorphism of the Sutherland crystalline series must now be regarded as incorrect, and the origin of their crystalline texture must be set back into earlier ages. The character of the dislocations thus revealed is as important as the disproof they afford of a serious error; and the inverted attitude that has been observed elsewhere between fossiliferous and crystalline beds will be examined over again in the light of these fruitful discoveries. These overriding Scotch gneisses may thus prove to be the connecting-link between the well-established alpine inversions that lay the fundamental gneiss on mesozoic limestone, as on the northern cliffs of the Jungfrau, and the still unsolved mystery in Norway, where crystalline schists seem to overlie the fossiliferous paleozoic sediments across wide areas, and thus give an abnormal character to the structure of the mountains, as shown in Törnebohm's section of the peninsula.

Then there is the extraordinary measure of ten miles for the horizontal displacement that is accountable for the whole difficulty in the Highlands; and along with this goes the occurrence of a number of (so-called) 'reversed faults,' in which the uplifted member has been thrust up an inclined plane. All of this is strong in evidence of the modern view that disordered mountain structures are characterized less by the gain of height than by the loss of breadth that they have suffered. The almost

incredible transgression of an older mass upon a newer one, now reported, has few parallels, unless one may be found in the famous overturning of the Windgällen Alps, studied out by Escher von der Linth, and confirmed by Heim. In the face of such an example, so utterly beyond explanation without the aid of irresistible lateral compression, we feel that the contractional hypothesis gains new support; and against the English school of physical geologists, who claim to show its insufficiency, the conclusion of Heim may be now quoted with new force: more may be learned of the earth's structure from critical observations on its surface than from calculations founded on physical assumptions concerning its interior. Besides these extraordinary facts of motion, the production of chemical changes during the mechanical stresses and distresses of the Highlands is hardly less remarkable. Sandstone passes into gneiss, and gneiss acquires schistosity, in a new direction in obedience to distorting forces. All this is beautifully confirmatory of Lehmann's conclusions from his researches in Germany during the past few years. Mechanical metamorphism thus gains the support of a series of facts that chemical metamorphism can ill afford to lose.

The article on this subject, contributed to the present number of *Science* by a well-known writer on these matters, contains certain statements to which exception may be taken. The questions raised with regard to the New-England rocks here referred to certainly cannot be considered 'settled' in the manner implied by our contributor, as was very evident at the Montreal meeting of the British association last summer; and the metamorphic origin of certain alpine rocks is not yet generally abandoned. As to 'regional metamorphism,' the revelations of recent detailed and minute studies in the field are not always such as to disprove it, but rather to attribute the metamorphic changes, where they occur, to mechanical instead of simply to chemical action; and, when disturbed and distorted rocks are found in altered mineralogical conditions over considerable areas, 'regional metamorphism' does not seem to be a misleading or confusing term to apply to them. Finally, the implication that Mr. Geikie makes insufficient reference to the results of his predecessors is certainly unwarranted. He states sufficiently that other observers have preceded him in the views he has now come to hold, and promises that they shall be duly mentioned in the detailed report which is to follow the present brief and preliminary publication. His outspoken frankness in admitting his previous error leaves nothing to be desired, and sets an example worthy of imitation.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Trowbridge's Physics.

In a recent number of *Science* will be found a criticism of Professor Trowbridge's 'New physics.' Those who have carefully read the work alluded to have doubtless found errors here and there, which would not appear in a second edition; but no one can fail to recognize a master mind in the organization of this new method of teaching natural science. The allegation that Professor Trowbridge has misstated some of the fundamental laws of mechanics is not sustained by a closer examination.

It is hardly necessary to point out that the formulae for the ballistic pendulum become perfectly intelligible if we understand by the first h, not the maximum height attained, but the observed distance through which the pendulum is acted upon by the force F, whose average value is thus determined 'without involving the element of time;' that the laws for the lever, which caused the critic even more surprise, are perfectly correct, when, as in the case in point, angular acceleration is considered, since the work spent upon equal masses, like their moment of inertia, is in this case proportional, as stated, to the square of the distance from the fulcrum; that it is indifferent, in the experiment, whether we find the length, or the radius of gyration, of the equivalent simple pendulum, since the two are identical; and that force is constant over the concentric spherical equipotential surfaces in question. The last two statements, therefore, as made by the author, need only to be restricted.

Such oversights as the critic is easily able to detect are not insidious, like some of those which have crept into many modern text-books. The underlying principles are brought out by the course of experiments in their clearest light; and therefore the work must be regarded by teachers as a safe and trustworthy guide.

It may be added that the experiments considered so difficult by the critic have already been employed with success in an elementary course, and are undoubtedly in place in any work whose object is to