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

elevate, rather than cater to, the present standard of physical instruction in the schools. W.

[The 'master mind' was distinctly recognized, and its presence cheerfully acknowledged, in the review to which the above refers. The reviewer heartily joins his critic in his desire for a 'closer examination' to determine the justice of the comments made. Such an examination will unquestionably show that every criticism made in the review is well founded. It will be generally admitted that an explanation which needs explaining is not extremely satisfactory. The points under discussion are such as are not usually considered in books with which the teacher is likely to be familiar; and erroneous and confusing statements will generally be accepted, although not understood. The result must be disheartening, if not disastrous. It seems wiser, therefore, to warn him to be on the lookout for errors which have not been eliminated from this first edition, but which are not likely to be found in a second. And this is especially true of a book which contains as many really good and original things as the 'New physics,' and which carries the weight which naturally and necessarily goes with any thing Professor Trowbridge writes. — REV.]

The earthquake of Jan. 2.

Supposing that reasonably exact determinations of the time and character of seismic phenomena are useful, I send the following note on the shock of Jan. 2 at Washington.

I recognized the character of the shock at the instant of its occurrence, and timed it. On the following day, comparing my watch with one set to the standard (not local) time adopted for this city, I found the shock occurred at 9 h. 16 m. P.M., civil time, to which the correction to the Washington meridian is to be applied. My residence is close to Ascension church, on the highest land away from the boundaries of the city: the grade is ninety-two feet above mean level of the river, and two feet higher than the base of the capitol. I was in the third-story back room, facing east into the back-yard, and south into an alley. The house is of brick, and above the middle of the second story is isolated. The shock was a distinct and very heavy and sudden jar, not accompanied by noise, unless by a slight rattling of the windows, and lasted less than a second. The sensation was as if a very heavy body had struck the earth, yet also as if the jar were partly upward rather than downward. There was no second shock within fifteen minutes, although I saw a paragraph in the daily press to the effect that one individual alleges that he felt a second shock about 11 P.M. at Alexandria, Va. W. H. DALL.

Itinerant science-teachers.

In *Nature* for Dec. 25, 1884, there is described an 'itinerant method of science-teaching,' which "has been carried out on a large scale and with the most gratifying success by the school boards of Birmingham and Liverpool." A science demonstrator is appointed for a number of schools; and he is provided with apparatus, which is conveyed from school to school in a handcart 'by a strong youth.'

"The system," it is said, "fairly meets the objections which have been urged against the introduction of science-teaching, on the grounds of want of qualified teachers, want of time [to prepare for the lessons], and cost of apparatus. It also secures systematic and continuous teaching throughout the school-year. The teaching is practical, and every fact or law is demonstrated experimentally."

Would it not be well to try a similar plan here?

J. R. W.

[It would answer in large centres, but would be limited in its application to places where it might be said to be least needed. — ED.]

The voice of serpents.

The text-books upon zoölogy represent that the vocal apparatus of serpents is very scantily developed, only enough to enable some of these creatures to hiss. A fact lately brought to my attention by Mr. George W. Leitch of Ryegate, Vt., is worthy of mention, and may lead herpetologists to search more carefully for the vocal apparatus of serpents. Mr. Leitch was stationed for several years at Manepy, Ceylon, as a missionary of the American board of commissioners for foreign missions. One day a serpent entered an apartment containing lumber, and it was deemed best to kill him. It became very angry, and made a loud noise, which Mr. Leitch says reminded him of the bellowing of a bull two years old. Perhaps others may know of instances in which these creatures make loud noises. This animal was of an uncommon variety, and was not preserved. It was of considerable size, say, seven or eight feet in length.

C. H. HITCHCOCK.

Hanover, N.H., Jan. 16.

The incandescent light on steamers.

In No. 102 of *Science*, in the article on 'Recent advances in electrical science,' Professor Trowbridge makes the statement that the Fall-River line took the initiative in adopting the incandescent system. This is certainly a mistake, as I myself saw it in full operation on the Virginia, of the Bay line (running between Baltimore and Norfolk), in the autumn of 1882, about a year before the Pilgrim was launched. Whether the Bay line was the first to adopt it or not, I do not know.

EVERETT HAYDEN.

U.S. geol. surv., Washington, D.C., Jan. 19.

Rainfall and crops.

Professor Snow's statement (*Science*, v. p. 13), that an annual rainfall of eighteen inches is entirely inadequate to maintain successful agriculture, is, I suppose, meant to apply only to Kansas, and, with that limitation, may be correct. In California, and especially in this portion of it, our experience is very different. Properly distributed, a rainfall of ten inches is ample to mature the cereals, and excellent crops are frequently raised with less. In the season of 1881–82 this place had 4.89 inches of rain, and there was an almost complete failure of crops, except on irrigated land. In 1882–83 there were 5.86 inches; and the distribution could hardly have been worse, almost all the rain falling after the 26th of March. Even under such circumstances there was some production on dry land, and the opinion was general that the crops would have been fair if the same amount of rain had come at the proper times. Last year the rainfall was almost unprecedented, reaching 18.32 inches. It was altogether too much. The crops were good, but they would have been far better if the last inch or two had been omitted. Of course, under different conditions of soil and climate,