moraine, rising 150 feet or more above the valley floor. But there were certain associated features which puzzled me at the time. Upstream from the supposed moraine the floor of the cirque appeared to be deeply buried by an accumulation of rock débris which was generally as high as and near the head of the cirque distinctly higher than the morainal ridge. This débris was in places, especially near the marginal walls, arranged in parallel ridges trending with the axis of the valley; and in the depressions between the ridges were patches of snow and some small ponds. Thus the moraine had a steep frontal slope, but at the back merged with the ridged rock débris which rose to still higher levels. There were some depressions in the rock débris, 25 to 40 feet deep, which I took to be ice-block holes. No bedrock was seen in the cirque floor.

During the recent meeting of the Geological Society of America, Professor H. B. Patton, of Boulder, Colorado, exhibited some photographs of the rock streams of Veta Mountain, Colorado. One of these photographs showed the high and steep front terminus of a rock stream, and resembled very closely the front slope of the supposed moraine in the San Francisco cirque. Others of his pictures showed the longitudinal parallel ridges which characterize some rock streams, with bands of snow lying in the hollows between the ridges, just as was the case in the San Francisco cirque at the time of my visit. If the concentric wave-like ridges pictured by Howe[6] were present in the San Francisco deposits, I did not notice them.

I am inclined to believe that the features which puzzled me at the time of my visit may have been due to landslides or rock streams. This does not mean that the depression in which the features occur is not a glacial cirque; nor that the moraines reported by Atwood are not true moraines. It simply means that I am not wholly satisfied with the evidence of glaciation as reported by myself. It would seem that the possibility of a landslide or rock stream origin for features apparently due to glaciation must be carefully considered, especially when glaciation in doubtful localities is involved.

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[6] "Landslides of the San Juan Mountains," U. S. G. S. Professional Paper, No. 67.

THE TEACHING OF ELEMENTARY DYNAMICS IN THE HIGH SCHOOL

To the Editor of Science: I have just finished reading "The Teaching of Elementary Dynamics in the High School," by Wm. Kent. I believe that Mr. Kent is right in most respects except his last paragraph, where he states: "It is high time they [teachers of physics in the high schools] change their methods and try the method that was successfully used fifty years ago." As one of the physics teachers in secondary schools, I wish to say that my own practise for many years has not been materially different from that of Mr. Kent and I wish to put in just a word for the most of the physics teachers of my acquaintance when I say that their practise and that of Mr. Kent do not differ in any essential particular.

Again and again the discussion of the *force* $=$ *mass* $\times$ *acceleration* formula has come up among groups of teachers and, in every case, the verdict of the teachers has been that it was not a formidable matter. Each knew a way to teach it so that the pupil got the gist of the matter even if he could not write a text-book on it afterwards. And this is true whether the instruction is given in English or metric units. One is as easy as the other.

Mr. Kent has evidently assumed from the large amount of discussion on this question of dynamics (kinetics) that there is something radically wrong with the teaching of secondary school physics and that the chief cause of any lack of efficiency is to be laid at the door of that one little formula—$f = ma$. We all may easily observe that those who are doing the teaching are not the ones who are doing the talking. It might be as readily discovered that the great majority of teachers are going ahead in a reasonably sensible way and are teaching physics (and other subjects as well) according to the dictates of common sense

without undue regard to "requirements" of any kind.

All *teachers* of physics, whether in the secondary school or the college are under great obligations to Mr. Kent for his clear, excellent and simple explanation of this debated subject.

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NOTE ON FREE PUBLIC MUSEUMS

WHILE reading Mr. Henry L. Ward's very excellent paper on "Modern Exhibitional Tendencies of Museums of Natural History and Ethnology designed for Public Use," recently published,[1] the following interesting statement was noted:

> In fact, to the best of my knowledge, the Public Museum of the City of Milwaukee was the first institution of this nature to throw open its doors for the free admission of the public on every day of the year, a regulation to that effect having been adopted and put into force in December, 1905.

It is exceedingly gratifying to note that this progressive institution has been among the first to recognize that public museums are for the people and that all should be admitted freely with as little hindrance as possible. In this commendable movement, however, the Chicago Academy of Sciences has about ten years' priority over the Milwaukee Museum, its doors having been continuously open to the public since October 1, 1894. The hours are 9 A.M. to 5 P.M. week days and 1 to 5 P.M. Sundays. It is interesting to note that the Willner bequest of $100,000 recently received by the academy was won because the children were allowed free access to the building, especially on Sunday afternoons, and were given more or less attention. Mr. Willner once said to a friend, as he observed the interest of the children in the museum exhibits, "I think this institution is deserving of support." The fact that the academy received one third of his fortune is ample evidence that he believed in the educative value of institutions of this character.

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[1] Trans. Wis. Acad. Sciences, Arts and Letters, XVI., pp. 325–342, 1908.

SCIENTIFIC BOOKS

The Theory of Electrons and Its Applications to the Phenomena of Light and Radiant Heat. By H. A. LORENTZ.

This book is based upon the course of lectures delivered by Professor Lorentz at Columbia University in March and April, 1906. But the author has introduced into the book considerable material not given in the lectures and has also given in the form of notes many mathematical proofs which were omitted in the lectures.

It was naturally expected that this book by an author, who is himself responsible for a large part in the remarkable development of the modern theory of electrons, would prove of absorbing interest to physicists and to those in general who have any knowledge of the importance and fascination of the subject. As was expected, this is the case.

The author states in his preface that he is perforce obliged to restrict himself greatly in discussing the applications of the theory as to the number of topics considered, and remarks that the work of Voigt on magneto-optical phenomena, of Planck on radiation and of Einstein on the principle of relativity, has not received the attention which its importance would justify. The scope of the book will to some extent be revealed in the present brief review.

In the first chapter the fundamental formulæ of the electron theory are derived from Maxwell's well-known theory, with the aid of auxiliary hypotheses which the nature of the subject demands. Referring to Maxwell's equations, the author calls attention to the fact that, while they are useful and adequate in the treatment of many problems, there are yet many problems for which they are not. He goes on to say:

> Moreover, even if they were so, this general theory, in which we express the peculiar properties of different ponderable bodies by simply ascribing to each of them particular values of the dielectric constant, the conductivity and the magnetic permeability, can no longer be considered satisfactory when we wish to obtain a deeper insight into the nature of the phenomena. If we wish to understand the way in which electric and magnetic properties depend on the temperature,