that the only assurance of freedom of action lay in the personal statements of 'one or two of our trustees.' His meaning will doubtless be clear to those familiar with the basis of agreement, but as a statement to scientific men in general, who are not fully cognizant of the true situation, it is somewhat misleading. It is due alike to the Carnegie Institution and to the scientific public to state that *the entire scientific management of the laboratory, under the proposed arrangement, is placed in the hands of a representative board of scientific men,* the constitution, powers and functions of which are fully defined in a set of by-laws roughly drafted by our own representatives in consultation with those of the Carnegie Institution, submitted in writing to every member of our board of trustees, discussed and modified in subsequent meetings of conference committees, and finally adopted by unanimous vote of the board at their last meeting before action by the corporation. Nominated to the Carnegie trustees by members of the laboratory, and subject only to the limits of the appropriations made by the Carnegie Institution and of income from other sources, this board of managers is given entire control of the scientific management of the laboratory and its dependencies, and is by the by-laws constituted an advisory council to the Carnegie Institution. The only conditions limiting the action of this board were that it should include one representative of the Carnegie trustees, and that, in accordance with the terms of Mr. Carnegie's endowment, the Carnegie funds were not to be devoted to purposes of elementary instruction. To many of the trustees and members of the corporation it has seemed that this organization not only gave the scientific management the utmost freedom consistent with sound financial management, but by the constitution of the board as an advisory council to the institution gave it full opportunity to exert its influence in molding the future policy and development of the laboratory.

Whether the working plan thus outlined is adequate to the present needs and future development of the laboratory is no doubt open to discussion; and it may be stated on good authority that it will not be consummated, either in its present form or with modifications, without giving abundant further opportunity for such consideration. To maintain, however, that such a plan involves the abandonment of the principles of scientific representation, cooperation and freedom, would I think be at variance with the facts. That the laboratory has hitherto stood for these principles, and owes its success largely to their successful application, is undeniable; and that such cooperation has been possible in so large a measure is a lasting honor to American biologists. But before adopting a pessimistic view of the prospects of retaining the real substance of these much-to-be-desired blessings under the proposed Carnegie reorganization, it may be well to ask ourselves, in all candor, whether the history of the laboratory under its existing organization has left us above criticism.

Edmund B. Wilson,
Chairman of the Executive Committee of the Marine Biological Laboratory during the period of the negotiations with the Carnegie Institution.

THE COOLING OF GASES BY EXPANSION AND THE KINETIC THEORY.

In Science for August 22 there appears an abstract of a communication presented by Mr. Peter Fireman at the last meeting of the American Association, in which the cooling and heating effects in the classical experiment of Joule are referred to a sort of fractioning process of the slow and swift molecules. How rigorous a treatment he has given the subject I am unable to judge from the abstract, in which it is merely stated that, if a molecule enters the vacuum receiver at a high velocity, it will retain this velocity, while if a slower moving one enters, it will soon meet with a swifter one and exchange velocities with it. Just how the fractioning process occurs is not very clearly stated.

This same explanation, only in a much more complete form, was given by Natanson more than thirteen years ago. His treatment will be found in *Wiedemann's Annalen,* Vol. XXXVII., page 341.

R. W. Wood.
San Francisco,
September 8, 1902.