

Letters to the Editor.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, nor to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

On Editing Newton.

THE recent presidential address of Dr. J. L. E. Dreyer at the Royal Astronomical Society, remarking on the lack of a standard edition of the writings of Sir Isaac Newton, has given rise to some misconception in the daily Press. It would, of course, not be correct to infer any want of interest in that great heritage, or in the mode in which it has come down to us. Dr. Dreyer has naturally a special claim to attention in matters of scientific bibliography. His work on Tycho Brahe and on Sir W. Herschel, including the valuable introductions, will be a permanent possession. The writings of Newton involve, however, difficulties that were absent in these cases, as also in the immense outputs of Huygens and Euler and Laplace, to which he refers. There all is straightforward: the material had been published in standard form, and the main task was to collect and arrange it. With Newton scarcely anything was systematically published except the "Principia," that stupendous result of eighteen months' labour, and the "Opticks." The rest circulated largely in fragments, printed often long afterwards, and in part the subject of intricate international controversy, now perhaps mainly of antiquarian interest, if not largely obsolete. Moreover, the effect of Newton's writings reverberated all through the eighteenth century, and their adequate presentation should involve discussion of their indirect influence on the mode of progress of physical science in Britain and elsewhere.

An edition merely bibliographical is scarcely needed. There is no part of them that is not readily accessible, sometimes in numerous editions published in various countries, except two sections. One of these is Newton's correspondence, which has been accumulating in public collections but has not hitherto been systematically dealt with; the other is his work at the Mint, especially in relation to the restoration of the coinage, which, as one learns on high authority, includes documents fundamental for the history of economic science.

I have been anticipated in offering reasons for caution by Prof. Sampson. In the last Monthly Notices of the R.A.S. he has given a very attractive account of a project developed at Cambridge twenty years ago, in which colleagues in the University, some of them now deceased, were closely concerned along with me. We succeeded in gaining Prof. Sampson's active and stimulating support. The valuable memorandum, planning an edition of Newton's works, which he prepared for us as the basis of a working scheme, has now been printed by him. The intention was to begin with a collection of the letters, in which his special knowledge would have made him the main agent: the rest was not urgent, for the reasons indicated above. But as time passed it became clear, to me at any rate, that a committee of editors could not be very effective, especially when it consisted of men primarily intent each on his own scientific pursuits. Thus no effort has since been made to push the project.

It is perhaps not superfluous to record this entire confirmation, by independent opinion, of Prof.

Sampson's own matured judgment on the difficulty and even tedium of the task. But one may be permitted to express the view that a systematic collection of the letters remains most desirable, and the hope that some day that part of the scheme may be realised. It might possibly best be done by weaving them into a biography; though the "Life" by Brewster, now nearly seventy years old, is wonderfully adequate when the limitations of the writer on the mathematical side are remembered. The notes to the edition of the correspondence with Cotes, prepared by Edleston in 1850 for Trinity College, are an additional mine of concise information drawn from the original documents; and there are shorter historical accounts by Mr. Rouse Ball and others, including a re-issue of Prof. De Morgan's critical essays by Mr. Jourdain.

The Report of the Cambridge University Syndicate which dealt exhaustively with the Portsmouth Manuscripts, now deposited at Cambridge, has scarcely received the recognition that it merits, though it is gratifying to observe that Dr. Dreyer has studied it closely. The rather hurried final publication (in 1888) perhaps reduced it too much to a catalogue. It, however, provided an adequate though very condensed summary (pp. xxx, 58) of the results of a scrutiny spread over many years by Adams, Stokes, Liveing, with Luard for University history and theology, dealing with a mass of manuscripts which, as one learns on direct authority, came to them after many vicissitudes in most disordered and often seemingly hopeless condition.

JOSEPH LARMOR.

Cambridge, May 7.

The Band Spectrum of Boron Nitride.

DR. R. S. MULLIKEN has described in three letters his recent work on the application of the quantum theory of band spectra to the study of isotopy. In the first of these letters (*Science*, vol. 58, p. 164, August 31, 1923) he establishes the isotope effect in the case of boron by the analysis of my data for a band spectrum which I had shown to be due to a nitride of boron (*Roy. Soc. Proc., A*, vol. 91, p. 120, 1915). At the time he accepted this attribution. Since then, however, Dr. Mulliken has undertaken a new experimental investigation and obtained data, a theoretical consideration of which has induced him to ascribe the spectrum to BO rather than to BN, although, as he states in the second letter (*NATURE*, vol. 113, p. 423, March 22, 1924), "the possibility of BN is not yet altogether excluded." Apart from this passage, the letter and its title (in which the band spectrum is called that of boron monoxide) leave the impression that in Dr. Mulliken's view the possibility of BN is definitely excluded, an impression which finds later justification in the third letter (*NATURE*, vol. 113, p. 489, April 5, 1924), where it is stated that "In the case of the BO bands, it was in fact the magnitude of the observed isotope effect which first definitely indicated that they were not due to BN as formerly supposed."

Dr. Mulliken in his second letter (footnote 7) supports his attribution of the bands to the oxide by his observation that, as developed by the action of active nitrogen on BCl_3 , they are far more intense when a small amount of oxygen is present than when the nitrogen is freed as completely as possible from oxygen by means of phosphorus; whereas the reverse is the case with the spectrum produced when SiCl_4 is used instead of BCl_3 —a spectrum which I attributed to a nitride of silicon