Moreover, the author on p. 183 says that London coal gas has an illuminating value of 16 to 17 candles, and a calorific value of about 668 B.T.U.'s, which is much more nearly true for the gas supplied by the Gas Light and Coke Co. The error is of importance, as an engineer working at the problem of the gas-engine and consulting records of efficiency made with London gas might be seriously misled.

In dealing with water gas, no mention is made of the more modern processes such as the "Dellwik," now so largely used for the production of blue gas for welding, as well as for diluting coal gas.

It is admitted in the preface that the article on practical photometry has not been brought up to date, and this is a pity, as more than seven pages are devoted to the Bunsen photometer and the manipulation of candles, now practically extinct in all but name as a standard of light, whilst a couple of pages on pentane standards would have been of real value.

In spite of a few blemishes, the whole work is so good that no engineering chemist can afford to be without it.

*Die Photographie im Hochgebirg.* By Emil Terschak. Second edition. Pp. xxiii+62. (Berlin: Gustav Schmidt, 1905.) Price 2.50 marks.

EVERYONE who is of a roving disposition, and takes his camera to Switzerland or the Tyrol, or any other region where mountain climbing is pursued, should, if he wishes to gain by the experience of others, read this book. It is written by a photographer to photographers, and is not only very interesting to read, but contains a great amount of very useful photographic information of a particular kind.

The successful photography of mountain scenery, of ice, snow, and clouds at high altitudes requires not only forethought, but much experience. As it is necessary to carry all the apparatus that is required, the equipment must be well attended to, and since also one does not necessarily wish to climb high altitudes to take again a particular view that has not turned out photographically successful, one must be sure of securing a good negative at every exposure.

The first edition of this book appeared in 1900, but the author has since gained much more useful knowledge, which he has embodied in the present edition. The book is clearly printed in Roman characters on good paper, and the illustrations are numerous and well reproduced.

*The Royal Medical and Chirurgical Society of London. Centenary* 1805–1905. Written at the request of the President and Council by Dr. Norman Moore and Stephen Paget. Pp. 337. (The Aberdeen University Press, Ltd., 1905.)

THOUGH not the oldest of the medical societies of London, the Royal Medical and Chirurgical Society holds a position second to none, and the present volume of chronicles will not only be welcomed by its Fellows as giving a history of their society, but forms a useful record of the art and science of medicine during the nineteenth century, with comments by the compilers. A noteworthy feature of the volume is the list which is given for each year of the principal papers read before the society, both published and unpublished, extracts being given from the more important ones. Thus, for the year 1833, we find Hilton's unpublished account of *Trichina spiralis* in human muscle, which ante-dated Paget's discovery of this parasite. Short bibliographies of all the presidents and a full index complete this interesting volume, which contains several illustrations of the various premises occupied by the society and a photogravure frontispiece of William Saunders, the first president.

R. T. HEWLETT.

## LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

### Tidal Researches.

IN NATURE for January 11 (p. 248) appear some criticisms upon my paper entitled "Cotidal Lines for the World."

The critic says:—"The construction of these charts is, unfortunately, but vaguely indicated." In reply to this it may be said that the charts embody all data known to me at the time of their construction, and to such data references as copious as space seemed to permit are given. What is meant by cotidal lines is given in § 17. Notions relating to the local modifications or peculiarities of cotidal lines have been given in considerable detail by means of lemmas and examples. In the construction of these lines, large detailed charts showing soundings wherever known were employed, and these depths were carefully considered in each step of the process. The ranges of tide written along the shore-lines simply represent data, and in no way depend upon any theory or hypothesis. The same is essentially true of the cotidal lines where observations or data are sufficient. Wherever harmonic constants are available, the length of the series analysed is of secondary importance in the construction of cotidal lines, the results from two months being about as satisfactory as those from twenty years.

If we are not permitted to extend cotidal lines outward from the shore, we might about as well draw them upon the land as upon the water, for in either case they would only serve to point out the shore values. The reviewer thinks well of Berghaus's chart, and so do I. However, it is difficult to believe that a philosophical critic could long rest content with cotidal lines extending but a short distance off shore, and forming no connected or consistent system. Of course, the attempt, on my part, at covering all seas does not imply that all charts are equally good. In some instances the data were very meagre, and attention was directed to this fact more than once in the paper.

It seems strange that any serious misunderstanding could exist in reference to the method employed in inferring the times when the water particles are at elongation in particular directions. Does anybody doubt the conclusions reached in § 56, part iv. A? If these conclusions are wrong, let us hear the correct ones. If §§ 60–65, part iv. A, are not clear as they stand, it seems as if § 24, part iv. B (to say nothing of a reply to former criticism, NATURE, April 23, 1903), ought to remove all obscurity.

Perhaps the following remarks may be of some service in this connection:—

Unless the free period of a body of water, or of some portion of this body, approximately agrees with the period of the tidal forces, the tide in the body proper must be small, and generally smaller than the theoretical equilibrium tide for the body in question. But in many parts of the oceans the tide is several times greater than that which could be raised by the forces, even if we could suppose sufficient depths and sufficiently complete boundaries for enabling equilibrium tides to occur. Hence regions the dimensions of which approach critical values must exist in the oceans and account for the principal tides. If the aërial vibrations accompanying a musical tone act upon a series of resonators suited to various pitches, the one or more constructed for the given tone will respond to it, while all others will be practically silent; that is, the dominant impressed motions belong to resonators having critical dimensions, and not to the resonators in general.

That stationary oscillations of unexpectedly large amplitude exist in the oceans there is abundant evidence. In fact, a glance at the charts under criticism will show regions of large ranges over each of which the time of tide varies but little. As a nodal line is approached the range diminishes, and the time of tide changes rapidly in a comparatively short distance. Moreover, the dimensions of the oceans are such that areas having nearly critical