concludes with a good index. Although the work has been designed for students of engineering and architecture—at least this is the modest claim of the author—he also hopes that it may prove a useful book of reference to those engaged in the profession generally. There is little doubt that these hopes will be fulfilled, for after careful perusal we have nothing but praise for the work.

On pp. 409 and 414, "Mr. B. Baker" is quoted. In a future edition it will be as well to give this eminent engineer his proper title. N. J. L.

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.*]

Prof. Van der Waals on the Continuity of the Liquid and Gaseous States.

I CANNOT but think that my friend Mr. Bottomley is a little hard on Prof. Van der Waals. I am not aware that there is any dispute as to the fact that the methods he employed are open to criticism, and that his formula is only approximately true. In spite of its defects the treatise was regarded by Maxwell at the time of its publication as of very great interest. If, however, Van der Waals is accused of not showing a "proper appreciation of the work of Andrews," the following facts should be considered before judgment is passed :—

(1) The celebrated Bakerian Lecture of Andrews is not directly referred to, but the full account of it which appeared in *Poggendorff's Annalen* (Ergänzungsband v. p. 64, 1871) is quoted (p. 406).

(2) This reference is followed by a long section headed "Experiments of Andrews" (p. 407).

(3) On p. 420 the following passage occurs :—"The signification of the temperature—the critical temperature of Andrews—is clear from what precedes. Below it the substance can exist in the so-called gaseous as well as in the so-called liquid state, &c. The honour of this remarkable discovery, which alters our views as to the so-called permanent gases, and the liquefaction of gases generally, belongs to Andrews. That it was not so easy to reach this conclusion from experiments appears, amongst other circumstances, from Regnault giving in good faith maximum pressures for carbonic acid above 40°."

(4) The phrase, "I have borrowed this remark from Maxwell," which follows the description of the continuous transformation from gas to liquid, is at all events a proof that Van der Waals did not claim priority in the conception of the possibility of such a transformation.

He can therefore have had no possible reason for desiring to credit Maxwell, rather than Andrews, with this idea, especially in view of the facts that Maxwell himself (p. 119, first edition, "Theory of Heat") laid no claim to it, and that it is most clearly expressed in the abstract of the work of Andrews (*Pogg. Ann.*, *loc. cit.*), to which Van der Waals himself refers his readers.

(5) The preface is not happily worded, but I think that the phrases employed do not necessarily bear the interpretation which Mr. Bottomley attaches to them.

The context shows that the "connection between the gaseous and liquid condition," which Van der Waals claims to have established, is not the possibility of a continuous transformation from one to the other through a series of stable states, but that "both portions of the isotherms belong to one curve, even in the case in which these portions are connected by a part which cannot be realized."

He is referring to the work of James Thomson, not to that of Andrews, and his claim, as I read it, is to have deduced "from theoretical considerations" a form of the isothermal which, as the passage on p. 416 shows, he fully admits that James Thomson was the first to suggest and to support by sound argument. Again, I do not understand that Van der Waals claims to be the originator of the "conception" of the continuity of the liquid and gaseous states. He only says that his conception of their *identity*, which, in the sense in which he uses the word, he admits to be doubtful, has proved a "fruitful" hypothesis. He defines *identity* to mean that the molecule is not more complex in the liquid than in the vaporous state. His calculations are based on this assumption, and he fully admits that they only apply in cases where it is justified.

While, then, I agree with Mr. Bottomley that an explicit tribute in the preface to Andrews and to James Thomson would have been graceful on the part of Van der Waals, I do not think that there is any evidence of an attempt to claim for himself credit which is due to others. A. W. RÜCKER.

SINCE my letter which was published in your last issue was written, I have found that the first edition of Maxwell's "Theory of Heat" contains a diagram, intended to represent the isothermals of carbonic acid substance, with all, or almost all, the faults of the diagram of Prof. Van der Waals; and from this, no doubt, Van der Waals's diagram was taken. Consequently I beg leave to withdraw absolutely the words used in my letter, viz. "The curves seem certainly not taken from Maxwell," and also a succeeding sentence which gave my reason for this opinion. I am sorry for my error; but I was not aware, or rather had quite forgotten, that Maxwell's first edition contained this faulty diagram.

My criticism of Van der Waals's essay is in no way altered, however, unless perhaps it is a little strengthened. Maxwell became alive to the faultiness of his diagram, at any rate prior to 1875, and corrected it. Unfortunately, Prof. Van der Waals and the translators had not reached a clear understanding of the physical meaning of these curves in 1890, even with the aid of Maxwell's second edition. J. T. BOTTOMLEY.

13 University Gardens, Glasgow, March 10.

Surface Tension.

I SHALL be obliged if you can find space for the accompanying translation of an interesting letter which I have received from a German lady, who with very homely appliances has arrived at valuable results respecting the behaviour of contaminated water surfaces. The earlier part of Miss Pockels' letter covers nearly the same ground as some of my own recent work, and in the main harmonizes with it. The later sections seem to me very suggestive, raising, if they do not fully answer, many important questions. I hope soon to find opportunity for repeating some of Miss Pockels' experiments. RAYLEIGH.

March 2.

Brunswick, January 10.

MY LORD,—Will you kindly excuse my venturing to trouble you with a German letter on a scientific subject? Having heard of the fruitful researches carried on by you last year on the hitherto little understood properties of water surfaces, I thought it might interest you to know of my own observations on the subject. For various reasons I am not in a position to publish them in scientific periodicals, and I therefore adopt this means of communicating to you the most important of them.

First, I will describe a simple method, which I have employed for several years, for increasing or diminishing the surface of a liquid in any proportion, by which its purity may be altered at pleasure.

A rectangular tin trough, 70 cm. long, 5 cm. wide, 2 cm. high, is filled with water to the brim, and a strip of tin about 1½ cm. wide laid across it perpendicular to its length, so that the under side of the strip is in contact with the surface of the water, and divides it into two halves. By shifting this partition to the right or the left, the surface on either side can be lengthened or shortened in any proportion, and the amount of the displacement may be read off on a scale held along the front of the trough.

No doubt this apparatus suffers, as I shall point out presently, from a certain imperfection, for the partition never completely shuts off the two separate surfaces from each other. If there is a great difference of tension between the two sides, a return current often breaks through between the partition and the edge of the trough (particularly at the time of shifting). The apparatus, however, answers for attaining any condition of tension which is at all possible, and in experiments with very clean surfaces there is little to be feared in the way of currents breaking through.

I always measured the surface tension in any part of the