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

trough by the weight necessary to separate from it a small disk (6 mm. in diameter), for which I used a light balance, with unequal arms and a sliding weight.

I will now put together the most important results obtained with this apparatus, most of which, though perhaps not all, must be known to you.

I. *Behaviour of the surface tension of water.*—The surface tension of a strongly contaminated water surface is *variable*—that is, it varies with the size of the surface. The minimum of the separating weight attained by diminishing the surface is to the maximum, according to my balance, in the ratio of 52 : 100.

If the surface is further extended, after the maximum tension is attained, the separating weight remains *constant*, as with oil, spirits of wine, and other normal liquids. It begins, however, to diminish again, directly the partition is pushed back to the point of the scale at which the increase of tension ceased.

The water surface can thus exist in two sharply contrasted conditions ; the *normal* condition, in which the displacement of the partition makes no impression on the tension, and the *anomalous* condition, in which every increase or decrease alters the tension.

II. *Mobility.*—Upon the purity of the surface depends its mobility, and in consequence the persistence of a wave once set in motion. So long, however, as the water surface is in its anomalous condition, the damping of the waves is constant,[1] and just at the degree of purity at which *the tension ceases to alter the decrease of the damping begins.*

If the balance is loaded with just the maximum weight which the surface tension can hold, and the normal surface is contracted till the weight breaks away, a measure is obtained of the relative amount of contamination by the ratio of the length of the surface before and after contraction ; for, the purer the surface, the smaller must be the fraction to which it is reduced before it begins to enter the anomalous state. By counting, with different relative contaminations, how often a wave excited by a small rod at the end of the trough passed along the surface adjusted to a length of 30 cm. before it ceased to be visible, I obtained approximately the following values for the number of the passages :—

| Relative contamination... | 0 | 5 | 10 | 15 | 20 | 25 | 30 |
|---|---|---|---|---|---|---|---|
| Number of visible wave passages | 17 | 17 | 17 | 17 | 12 | 8 | 3 |

The numbers of the upper row indicate the length at which the surface becomes anomalous in 30ths of its whole length ; those of the second row are, as may be imagined, rather uncertain, particularly the greater ones, although they are the mean of many observations.

A *perfectly clean* surface, whose tension remains constant, even under the greatest contraction, can be approximately produced with the adjustable trough, by placing the partition quite at the end, and pushing it from thence to the middle. The surface on one side is thus formed entirely afresh, from the interior of the liquid.

III. *Effect on a water surface of contact with solid bodies.*—Every solid body, however clean, which is brought in contact with a newly formed surface, contaminates it more or less decidedly, according to the substance of which the body consists. With many substances, such as camphor or flour, this effect is so strong that the tension of the surface is lowered to a definite value ; with others (glass, metals) it is only shown by the increase of relative contamination. The contaminating current which goes out from the circumference of a body—for example, of a floating fragment of tinfoil—is easily made visible by dusting the water with Lycopodium or flowers of sulphur. I will call it, for the sake of brevity, "the solution current."

The solution current of a body which is introduced into a perfectly clean water surface lasts until the relative contamination produced by it has attained a definite value, which is *different for every substance.*

Thus the solution current for wax ceases at a relative contamination of 0·55 ; that of tinfoil at a still smaller one ; but that of camphor, not until the surface has become decidedly anomalous, and the separating weight gone down to within 0·80 of the maximum. If, on the other hand, the surface surrounding a small piece of tinfoil be restored to its previous purity, the cur-

rent begins again with renewed strength, and it appears that this process may be repeated as often as desired without the solution current ever quite disappearing.

From this effect of the contact of solid bodies, it follows that a perfectly pure surface cannot be maintained for long in any vessel, since every vessel will contaminate it. Whether the air and the matter contained in it have a share in the gradual increase of relative contamination which occurs on water left standing, I know not ; but the influence of gases and vapours does not appear to me important in general. The contamination by the sides of the vessel does not, however, always go so far as to diminish the tension, which remains normal, for example, in a glass of water, after four days' standing.

With a rising temperature the contamination from all substances seems to increase considerably ; but I have not yet investigated this in detail.

IV. *Currents between surfaces of equal tension.*—Between two normal surfaces, which are unequally contaminated by one and the same substance, a current sets in from the more to the less contaminated when the partition is removed ; much weaker, indeed, than that exhibited in the anomalous condition by differences of tension, but, all the same, distinctly perceptible. With *equal* relative contamination by the same substance, no current of course sets in. It is otherwise when the contamination is produced by *different* substances.

I contaminated the surface on one side of the partition by repeated immersion of a metal plate, on the other by immersion of a glass plate, which had both been previously carefully cleaned and repeatedly immersed in fresh water surfaces. I then made the relative contamination on the two sides equal (*i.e.* $= \frac{1}{2}$) by pushing in the outer partitions by which the surfaces were inclosed. After the water had been dusted with Lycopodium, the middle partition was removed. I repeated this experiment eight times, with different changes devised as checks.

On the removal of the partition a *decided current* set in each time, *from the surface contaminated by glass to that contaminated by metal ;* and when I replaced the partition after the current had ceased, and investigated the contamination on both sides, I always found it greater on the metal than on the glass side.

Thus equal relative contamination by different substances does *not* indicate equality of that (osmotic ?) pressure which is the cause of the current between surfaces of equal tension.

For further proof of this result I have made experiments with other substances ; for example, with a floating piece of tinfoil on one side, and of wax on the other, when, after they had been acting for a long time, and then the relative contaminations had been equalized, a current resulted from the wax to the tinfoil ; and again, with camphor on the one, and small pieces of wood and wax on the other side, which showed a current from the wax and wood to the camphor.

Since, therefore, the water surface assumes dissimilar qualities from contact with different substances, the conviction is forced upon me that it is these bodies themselves (glass, metal, wax, &c.) which are dissolved, though only feebly in the surface, and thereby render it capable under sufficient contraction of becoming anomalous.

V. *Further observations on solution currents.*—The following facts agree with this view. If a newly formed water surface be contaminated by small floating slices of wax until the latter cease to give solution currents, the relative contamination amounts to 0·55. If now another fresh surface is brought to the same relative contamination by tinfoil and a corresponding contraction, and then a slice of wax from the first surface be introduced, it will develop a considerable solution current. This therefore depends on the substance with which the surrounding surface was previously contaminated.

Substances which are properly soluble in water, such as sugar and soda, exhibit a similar behaviour when immersed in the surface, only they continue to act in the anomalous condition.

A crystal of sugar placed in a normal but not perfectly pure surface produces a great fall of tension. If the surface be then made normal again by immersing and withdrawing strips of paper, and if this process be repeated several times, a normal surface is at last attained, which is contaminated *by sugar only*, and on the tension of this the sugar produces no further effect. A piece of soda held in the surface containing sugar greatly lowers the tension ; and on the other hand on a surface rendered repeatedly anomalous by soda, soda acts but slightly, and sugar powerfully.

[In this experiment the sugar and soda crystals being instantly

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[1] This is not quite exact. I found the number of visible passages constant = 3 in the anomalous state ; but the velocity of transmission varying in some degree with the tension, the time required for the vanishing of the wave must really become a little longer when the tension is lowered.—*February 26.*

wetted, they do not really act by solution-currents, for the latter can only be produced by a dry body. The action here is an indirect one by intervention of the deeper layers.—*February* 26.]

VI. *Behaviour of the surfaces of solutions.*—The effect of soluble matter on the surface tension has absolutely nothing to do with the change which the cohesion of the water undergoes, through matter dissolved in the body of the liquid, for both sugar and soda solutions have a *higher* maximum tension than pure water, and yet these same substances introduced into the surface produce a fall in the separating weight.

In order to investigate the behaviour of the surfaces of solutions more closely, I introduced a saturated solution of common salt into the adjustable trough. The freshly formed surface of the solution of salt maintained its normal separating weight, (1·154 of that of water) even when most contracted, though it must necessarily have contained as much salt as the interior of the liquid. The entrance of the anomalous condition, then, does not depend on the absolute quantity of the contaminating substance contained in the surface ; but when I placed some salt in contact with the normal surface of the saturated solution, it gave a solution current and lowered the tension, as in the case of pure water. I obtained similar results with a solution of sugar. From these experiments I concluded (*a*) that the surface layer of water can take up *more* of soluble substances than the internal liquid ; (*b*) that the surface of a solution is capable of becoming anomalous under contraction, always and only, when it contains more of the dissolved substance than the interior of the liquid.

That the surface layer really possesses a higher dissolving power is further shown by the experiment, which is well known to you, in which a thin disk of camphor, so hung that it is half immersed in the cleanest possible water surface, is cut through in the course of a few hours. I will add by the way, that a newly formed surface of a saturated solution of camphor is *normal* according to my observations, *i.e.* that its tension remains nearly constant under contraction, and that small pieces of camphor floating on it still give solution streams and have slight motions. The solution stream seems in this case to cease just when the surface begins to be anomalous.

What I have further observed regarding solutions in the surface and the like, seems to me less remarkable, and part of it still very uncertain. I therefore confine myself to these short indications, but I believe that much might be discovered in this field, if it were thoroughly investigated. I thought I ought not to withhold from you these facts which I have observed, although I am not a professional physicist ; and again begging you to excuse my boldness, I remain, with sincere respect,

Yours faithfully,

(Signed)　AGNES POCKELS.

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## Modern Views of Electricity.

DR. LODGE'S doctrine of the slope of potential, explained in his note to my letter in NATURE of February 19 (p. 367), still presents great difficulties. A plate of zinc is covered by a film of air or oxygen in a different state from the surrounding atmosphere. We first consider a point outside of the film. Dr. Lodge says this point is influenced by the ordinary dielectric strain of a static charge imparted to the zinc in any adventitious manner. That is evident. Now, when the zinc was isolated, we had a negative charge upon it, or in the film, and therefore, at the point in question, a positive slope of potential upwards from the zinc. Call it R. When we make contact with copper we introduce a positive static charge on to the zinc. The effect of this at the point in question is a negative slope of potential—that is, downwards from the zinc. Call it – R'. Then, as the final result we have an upward slope of potential, R – R', which is less than before contact was made.

Dr. Lodge further says that the static charge imparted to the zinc does not alter the slope of potential *within the film*. By that, I understand the average slope of potential over a line drawn from end to end of the film at right angles to the zinc. Now if the new static charge—suppose $\sigma$ per unit of surface—be placed close upon the zinc, so as to have the film outside of it, it will diminish the upward slope of potential at all points within the film by exactly $2\pi\sigma$. If we are at liberty to place the new static charge at some distance from the zinc, we may modify this result in any way we please.

Mr. Chattock suggests that the essence of combination between

zinc and oxygen is that the zinc atom is + and the oxygen – By this, I understand him to mean that two atoms of zinc assume equal and opposite charges, and two atoms of oxygen assume equal and opposite charges ; and then positive zinc combines with negative oxygen, forming a neutral compound as regards electrification ; but the remaining zinc is negative, and the remaining oxygen positive ; hence the step of potential from zinc to oxygen. But how would he explain the permanence of these states of electrification ?

S. H. BURBURY.

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## The Flying to Pieces of a Whirling Ring.

HAVING had occasion lately to devise a high-speed whirling-machine, I examined the speed at which it might be safe to work, and some of the results surprise me. For instance, it is easy to show (by equating the normal component of the tension to the centrifugal force of any element) that the critical velocity at which a circular ring or rim of any uniform section will fly, unless radially sustained, is given by $T = v^2 \rho$, where T is the tenacity, and $\rho$ the density of its material. Thus a band of steel just able to bear a load of 30 tons to the square inch will fly to pieces at a peripheral speed of about 800 feet a second ; and this without reference to its angular velocity, or radius of curvature. It may be objected that no such accident could occur with purely rectilinear motion, but such motion at the critical speed would be very unstable—the slightest shiver of a vibration running along it would precipitate a catastrophe.

Hence a steel girdle round the earth's equator would burst, however thick it might be, were it not for its weight. Again, an Atlantic cable is only held together by its weight. In the early days of cable-laying, it was suggested to ease matters by attaching floating matter to the cable till it was of the same average density as sea-water ; but we now see that such a cable, if lying parallel to the equator, could not hold together, unless it were made of 30-ton steel and laid north of latitude 60°.

OLIVER J. LODGE.

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## Cutting a Millimetre Thread with an Inch Leading Screw.

IT is possible that many who possess a screw-cutting lathe with a leading screw of so many threads to the inch may wish to use it for cutting millimetre screws. While, of course, it is too much to expect that the absolute value of the millimetre, as given in terms of the inch, can be obtained by ordinary change wheels—and this is not of great importance, since, among other reasons, the two determinations of the value of the millimetre in inches differ by one part in a hundred thousand—yet it may not be well known that a most remarkable degree of accuracy may be obtained with wheels in ordinary use. After some trouble I lighted upon the following numbers, which, with a leading screw of eight threads to the inch, give as a result 25·3968, whereas the inch is 25·3995 millimetres. The wheels are 28 on mandril, 100 and 36 on stud, and 32 on screw. The error would therefore, with a perfect lathe, be less than one part in nine thousand, so that a screw cut in this way would for almost all purposes be correct ; in fact, it is doubtful if in the case of short screws many lathes could be trusted to cut inch threads more accurately. For leading screws of other pitches, such as 4, 5, 6, or 10 threads to the inch, the wheels can easily be altered so as to give the same result.

Of course it may be the case that this or an equally good arrangement is known to some ; but as I had to start working out the combinations of thirteen wheels taken four together, in which each combination contained six sub-combinations, in order to obtain the result, it is possible that it may be appreciated by those to whom it may be of use, but who would rather be saved so much trouble.　　　　　C. V. BOYS.

Royal College of Science, London.

P.S.—It may be worth while to add that the wheels taken in order—

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 28 | ... | *100* | ... | 36 | ... | *32* | with | *8* threads to the inch |
| are the same as | 28 | ... | *32* | ... | 36 | ... | *100* | ,, | *8* ,, ,, ,, |
| or as | 7 | ... | *8* | ... | 9 | ... | 25 | ,, | *8* ,, ,, ,, |
| or as | 7 | ... | *8* | ... | 9 | ... | 10 | ,, | 20 ,, ,, ,, |

where the followers or multipliers are printed in italics. The