LETTERS TO THE EDITOR.

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On a New Law Connecting the Periods of Molecular Vibrations.

IN the current number of NATURE you were good enough to print a short article by me, announcing a discovery which I believed was new. My attention has now been drawn to the fact that it was published a few months ago by Rydberg (*Wied. Ann.*, vol. lviii. p. 674), to whom the honour of the discovery therefore belongs. My excuse for being unacquainted with Rydberg's paper must be found in a prolonged absence from home last summer, and the large amount of unread scientific literature which I consequently found on my return home. There is, moreover, nothing in the title of Rydberg's paper which would indicate the important nature of its contents. If by writing to you on the subject I have drawn the attention of physicists to what I consider the most important fact yet brought to light concerning molecular vibrations, my article will have served some good purpose. ARTHUR SCHUSTER.

The Athenæum, Pall Mall, S.W., January 2.

The Pound as a Force.

A VERY few words are necessary from me in answer to Prof. Perry's letter on page 177. First and foremost (though referring to the latter part of his letter, not to the cow and bridge portion), if any sentence in my previous communication can have led any one to imagine that I consider Prof. Perry anything but a most admirable teacher of his own subject, that sentence must have been villainously expressed. Secondly, when I said that engineers had mostly to deal in their calculations with bodies either at rest or in uniform motion, I thought I was speaking in the sense of Prof. Perry's original article (he said the same thing himself near the top of column 1, page 50), and that I should have his concurrence: I would not for a moment argue such a point with him. If I had thought it necessary to be cautious I would have used the word "suggest" instead of the word "tell" in my sentence about acceleration: to the idea in which however I still respectfully adhere. And in general I adhere to all the *matter* of my last communication, though with full deference to his criticism on the *manner* of it. Thirdly, I cannot remember that I have ever specially "advocated" the poundal. I have never much liked it, but it is useful as a stepping-stone to higher things, in a way that the familiar pound-weight is not. Fourthly, I agree with Fitzgerald that Newton's second law furnishes by no means the only measure for quantity of matter (chemical equivalence also furnishes a measure), but inertia is the fundamental property and measure for dynamical purposes. Fifthly, we do not "assume" that inertia is proportional to weight; we verify it within certain limits of error by dropping bodies (like Galileo), or (like Newton) within narrower limits by swinging pendulums: essentially the same process. Sixthly, I do not, alas, find it at all easy to give full marks to a student for his answer to such a question as "What is Ohm's law?"; and, although I cannot plead guilty to the accusation of having spoke disrespectfully either of Gravity or of Engineers, I do find that occasionally the treatment of the former by the latter leaves something to be desired in point of clearness; the occasional educational remarks of the periodical called *The Engineer*, for instance, seem fairly representative of a large and influential class. And lastly, although a remark immediately following his citation of a familiar electrical equation leads me to think that Prof. Perry still misses the chief point of my letter, yet there are quantities of things in the present correspondence on which we agree; and chief among them is the profound conviction we share that there is a crying need for reform in our whole system of secondary education. OLIVER J. LODGE.

Liverpool, December 27, 1896.

The Theory of Dissociation into Ions.

THE numerical agreement obtained when certain properties of solutions are interpreted on the theories of osmotic pressure and ionic dissociation is undoubtedly very striking, and it is, consequently, not very surprising that these theories have obtained such a ready acceptance. Whatever may be our opinions as to the validity of the theories, and even of the harm which has been done by pressing them too far, we cannot but recognise that they have been the origin of much good work on a condition of matter which is, at the same time, one of the most obscure and one of the most important, both from the physicist's and chemist's point of view. But, however convenient such theories may be as working hypotheses, their advocates should not have forgotten that they depend solely on the numerical relations alluded to, and that something more than this is required before such hypotheses can be raised to the level of acceptable theories, and far more before they should be held up as an indispensable article of faith, which unless a chemist believe he cannot be saved.

For a theory to be acceptable it should, at the very least, be reasonably probable, and should not violate any fundamental and well-established facts; it should stand the test of any apparently crucial experiments brought forward to settle between it and its rivals, and, I think we may add, it should give some explanation, not simply of the behaviour of matter in the condition in question, but also of why matter ever assumes such a condition.

The theories of osmotic pressure and ionic dissociation, I believe, have not done this. Even if we can accept as probable the view that atoms united so firmly together, as we have every reason to believe are those of, say, chlorine and hydrogen, will fly affrighted from each other at the mere approach of a few water molecules, which are represented as being more or less inert and destitute of any strong attraction for the dissociated atoms; even if we can imagine that these atoms, so strongly charged with electricity of opposite signs, can meander about in the liquid, with a supreme disdain for their former associates and the attractive charges which they carry; even if we can reconcile this indifference with the behaviour of these very atoms to a similar electric charge on other similar companionable atoms, when these latter happen to be agglomerated into the form of an electrode; even if we find no difficulties in all this, still we must admit that the theories in question afford no explanation whatever why a substance should dissolve at all, and they can, therefore, hardly be accepted as a sufficient explanation of solutions. We cannot treat Nernst's statement that a substance goes into solution because it has a "solution pressure" seriously, and, in cases where the dissolved substance is known to form hydrates, the view that an excess of water will decompose these hydrates, and free the substance entirely from its union with water, without the formation of any other compounds, is quite opposed to our knowledge of the action of mass in chemical changes.

Nor can we ignore the thermal difficulties in which the theory of dissociation lands us; for if, to satisfy the facts of the case, we admit that dissociation is accompanied by a large evolution of heat, we must suppose, either, that the evolution which accompanies the reverse action when the water is absent (*e.g.* $H_2 + Cl_2 = 2HCl$, gases), is due to heat being evolved by the dissociation of molecules of elements into their atoms, or, as has been asserted, that the atoms of the dissolving electrolyte evolve heat by combining with their electric charges, a novel method of evolving heat, which should long ago have made the fortunes of the discoverers, especially as the charges with which the atoms combine come into existence of their own accord, and without the expenditure of any external energy.

Turning to the "crucial" experiments suggested, we do not find the results to be any more satisfactory from the point of view of the theory. We have on the one side two experiments heralded in by Prof. Ostwald with great flourish of trumpets; the "imaginary" experiment already quoted by Dr. Herroun, in which an ultra-microscopical trace of liquid is electrolysed by an electrostatic discharge, and the "arm-chair" experiment of "chemical action at a distance," the results of both of which might have been predicted, as I have shown elsewhere, by any one possessing an elementary knowledge of electricity, long before the dissociation theory was dreamt of.

On the other side we have two experiments, which would seem to be conclusive, but which the dissociationists have hitherto thought fit to ignore.

Osmotic pressure, they hold, is due to the quasi-gaseous pressure of the solvent and dissolved substance acting on a diaphragm, which, being permeable to the solvent only, renders the pressure of the dissolved substance inoperative, and hence