in the moving matter. This means a moving force $-(U_1/c_1)\nabla c_1$. But if there is compression, $c_1$ probably always varies intrinsically as well.

It will be found that the omission of the auxiliary **h** has the result of complicating instead of simplifying the force formulæ. Similarly the omission of **e** complicates them. Now the use of **e** is founded upon the idea that the electric polarisation is produced by a separation of ions under the action of **E**, for $\mathbf{E}_1$ is the moving force on a moving unit electric charge. Analogously $\mathbf{h}_1$ is the moving force on a moving unit magnetic charge or magneton. If there are really no such things, the interpretation must be made equivalent in other terms. But the categorical imperative is not easily to be overcome.

The application to plane waves I described in a recent letter (NATURE, March 9) will be found to harmonise with the above in the special case.

But a correction is needed. In the estimation of the moving force on "glass" receiving radiation, the assumption was made that the electric and magnetic energies in the transmitted wave were equal. So the result is strictly limited by that condition. The conditions $E=wB$ and $U=T$ are not coextensive in general, though satisfied together in Lorentz's case. When U not $=T$, we have instead of (8), p. 439,

$$p_1v-p_2v-p_3w=w(T_3-U_3),$$

and the rate of loss of electromagnetic energy is

$$2\mu H_1H_2u+(w-u)(T_3-U_3).$$

Now this is zero when $e=0$, or the polarisation is proportional to the electric force. The question is raised how to discriminate, according to the data stated above, between cases of loss of energy and no loss. To answer this question, let **e** and **h** in the above be unstated in form; else the same. Then, instead of (4), the activity equation will be

$$-\nabla\mathbf{W}=\dot{U}+\dot{T}+\{\tfrac{1}{2}E^2(\partial c_0/\partial t)+\ldots\}+(\mathbf{f}_0\mathbf{q}+\mathbf{f}_1\mathbf{u})-(\mathbf{eJ}_1+\mathbf{hG}_1), \quad (5)$$

where **W** is as in (4), whilst $\mathbf{f}_0$ and $\mathbf{f}_1$ are the forces derived from the stresses specified (not the same as $\mathbf{F}_0$ and $\mathbf{F}_1$), and $\mathbf{J}_1$, $\mathbf{G}_1$ are the electric and magnetic polarisation currents, thus, $\mathbf{J}_1=\dot{\mathbf{D}}_1+V\nabla\mathbf{h}_1$, &c. It follows that it is upon **e** and **h** that the loss of energy depends in plane waves, when **u** and **q** are constant. For the stresses reduce to longitudinal pressures, so that by line integration along a tube of energy flux we get

$$\Sigma(eJ_1+hG_1)=\Sigma(U+T). \quad (6)$$

Thus, when a pulse enters moving glass from stationary ether, the rate of loss of energy is $\Sigma(-eJ_1)$. If $e$ is zero, so is the loss, as in the special case above. There is also agreement with the calculated loss in the other case. That the moving force on the glass should be controlled by **e** is remarkable, for it is merely the small difference between the electric force on a fixed and a moving unit charge. The theory is not final, of course. If the electromagnetics of the ether and matter could be made very simple, it would be a fine thing; but it does not seem probable. OLIVER HEAVISIDE.

April 5.

## The Dynamical Theory of Gases.

IN a letter to NATURE (April 13) Lord Rayleigh makes a criticism on my suggested explanation of the well known difficulty connected with the specific heats of a gas. He considers a gas bounded by a perfectly reflecting enclosure, and says "the only effect of the appeal to the æther is to bring in an infinitude of new modes of vibration, each of which, according to the law (of equipartition), should have its full share of the total energy."

The apparent difficulty was before my mind when writing my book. Indeed, as Lord Rayleigh remarks, something of the kind had already been indicated by Maxwell. (I think the passage to which Lord Rayleigh refers will be found in the "Coll. Works," ii., p. 433:—"Boltzmann has suggested that we are to look for the explanation in the mutual action between the molecules and the æthereal medium which surrounds them. I am afraid, however, that if we call in the help of this medium, we shall only increase the calculated specific heat, which is already too great.") It seemed to me, however, that the difficulty was fully met by the numerical results arrived at in chapter ix. of my book.

Suppose, to make the point at issue as definite as possible, we take a sample of air from the atmosphere, say at 15° C. Almost all the energy of this gas will be assignable to five degrees of freedom—so far as we know, three of translation and two of rotation. Let us surround this gas by an imaginary perfectly reflecting boundary. The total energy of matter and æther inside this enclosure will remain unaltered through all time, but this total energy may be divided conveniently into two parts:—

(1) The energy of the five degrees of freedom, say A.

(2) The energy of the remaining degrees of freedom of the matter plus the energy of the æther, say B.

As Lord Rayleigh insists, the system is now a conservative system, so that according to the law of equipartition, the total energy A+B is, *in the final state of the gas,* divided in the ratio

$$A:B=5:\infty \quad . \quad . \quad . \quad . \quad . \quad (1)$$

whereas observation seems to suggest that the ratio ought to retain its initial value

$$A:B=5:0 \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This I fully admit, but a further point, which I tried to bring out in the chapter already mentioned, is that the transition from the ratio (2) to the ratio (1) is very slow—if my calculations are accurate, millions of years would hardly suffice for any perceptible change—so that, although (1) may be the true final ratio, it is quite impossible to obtain experimental evidence of it.

If the sample of gas were initially at a much higher temperature than we have supposed, the transition would undoubtedly be much more rapid; but even here we could not hope for experimental verification. For the assumed boundary, impervious to all forms of energy and itself possessing none, cannot be realised in practice, and as soon as the energy of the enclosed æther becomes appreciable, the imperfections of our apparatus would become of paramount importance in determining the sequence of events. J. H. JEANS.

## Growth of a Wave-group when the Group-velocity is Negative.

THE following may be of interest in connection with the recent discussion on the flow of energy in such cases.

Let the energy of an element of a linearly arranged mechanical system be

$$\{(d^2y/dxdt)^2+y^2\}dx/2.$$

Such a system can be approximately realised by taking a bicycle chain, loading it so that the radius of gyration of each link has the same large value, and suspending it by equal threads attached to each link so that the chain is horizontal and the axes of the links vertical. By the principle of least action we immediately find the equation of motion to be $d^4y/dx^2dt^2=y$. A simple harmonic wave is given by $y=\sin(pt-x/p)$. The group velocity is $-p^2$, and is negative. Let such a system, extending from $x=0$ to $x=\infty$, be at rest in its position of equilibrium at time $t=0$, and then let the point $x=0$ be moved so that its position at any subsequent time is given by $y=1-\cos t$.

By application of the usual method *viâ* Fourier's integral, the motion of the system is found to be given by either of the equivalent formulæ

$$y=\Sigma(-1)^n(t/x)^{n+1}J_{2n+2}2\sqrt{(tx)},$$

or

$$y=1-\cos(t+x)-1+\Sigma(-1)^n(x/t)^nJ_{2n}2\sqrt{(tx)},$$

where the J's are Bessel's functions and the summations extend from $n=0$ to $n=\infty$. There are some doubtful