the acetylene explosion 114. Some experiments I have recently made in conjunction with Mr. J. C. Cain confirm these calculated pressures. When the explosion-wave was propagated through a mixture of equal volumes of cyanogen and oxygen it broke soda-lime tubing of 18 m.m. external diameter and 2·5 m.m. thickness. Pieces of this tubing broke at a mean hydraulic pressure of 70 atmospheres. Green glass tubing of 2·8 mm. thickness withstood the explosion; it broke at a pressure of 140 atmospheres. More exact results were obtained when the gases were diluted with an equal volume of nitrogen:—$C_2N_2 + O_2 + 2N_2 = 2CO + 3N_2$.

Pieces of the tube which were broken by the explosion were broken hydraulically at 63 atmospheres; pieces of the tube which withstood the explosion were broken hydraulically at 84 atmospheres.

Pressures in the Explosion Wave.

Gaseous mixture.	Calculated pressures.		Observed pressures.
	Riemann,	Dixon.	
$C_2N_2 + O_2$	140 At.	117 At.	70–140
$C_2N_2 + O_2 + 2N_2$	73·5 At.	57 At.	63–84

When oxygen is added to these mixtures the rate of explosion is diminished and the pressure falls. For instance, according to Riemann's equation, the pressures produced in the explosion of acetylene with increasing quantities of oxygen are as follow:—

Gaseous mixture.	Calculated pressure.
$C_2H_2 + O_2$	114 At.
$C_2H_2 + O_3$	98 At.
$C_2H_2 + O_5$	78 At.

In the same way the pressures produced in the explosion of ethylene with different quantities of oxygen may be calculated:—

Gaseous mixture.	Calculated pressure.
$C_2H_4 + O_3$	98 At.
$C_2H_4 + O_4$	91 At.
$C_2H_4 + O_6$	78 At.

The lowest of these pressures is probably sufficient to break the cylinders used by Prof. Lothar Meyer. As Prof. Thorpe says in Nature, the danger of acetylene lies in the rapidity with which the explosion-wave is initiated, even when the air alone is used as "tamping." Safety lies not in thickening the glass, but in shortening the tubes. H. B. Dixon.

Owens College, December 1.

The Kinetic Theory of Gases.

I have to thank Mr. Culverwell for his reply to my letter on the discussion at Oxford. To quote his own words (in answering Mr. Burbury, p. 105), Mr. Culverwell's letter was "exactly the kind of letter that I hoped to elicit," as I had not been able to recall the exact purport of Prof. Fitzgerald's "onslaught." Although Prof. Boltzmann made no attempt to answer Prof. Fitzgerald's objections in the short space of time available after the other speakers had concluded, he several times mentioned the question to me after the debate as one which had not been hitherto satisfactorily cleared up. In preparing my Report, the question of the spectra of gases came prominently before me, but I purposely refrained from expressing my own opinions on a subject about which so little had been written in a report which was intended to be chiefly a record of work actually done. My frequent allusions to the question of the *uniqueness* of the Boltzmann-Maxwell Law were intended, however, to pave the way, if possible, for an explanation of the discrepancies alluded to by Prof. Fitzgerald, and I should like now to attempt to answer some of his objections.

According to Mr. Culverwell, Prof. Fitzgerald asked why the ether, the solar system, and the whole universe were not subject to the Boltzmann-Maxwell Law? Let us take the solar system first.

The law is obviously inapplicable to a *single* system (as I pointed out in my Report, and hope to prove still more conclusively shortly). In order to apply it, Prof. Fitzgerald would have to take *an infinitely large number of solar systems*, each consisting of similarly constituted planets differing, however, in their motions. What the law states is that, if the coordinates and momenta of the different systems were at any instant distributed according to the Boltzmann-Maxwell distribution (*i.e.* with frequencies proportional to e^{-hE}), they would be so distributed at any subsequent instant. In the absence of mutual action between the various solar systems, this would *not* be the only permanent distribution, nor would there be any tendency to assume such a distribution. If, however, the different solar systems were to collide with or encounter one another *at random* in such a way that transference of energy was liable to take place between any of the coordinates of any one system and any of the coordinates of any other system, the Boltzmann-Maxwell distribution *would* probably be unique, and there would be a tendency to assume such a distribution as the ultimate result of a great number of encounters taking place. Will not Prof. Fitzgerald agree to this?

With regard to the ether, I notice that Mr. Culverwell emphasises Prof. Fitzgerald's contention that the investigations ought to take "ethereal" as well as "molecular" coordinates and momenta into account. But here I agree with Prof. Boltzmann that the *onus probandi* lies with physicists. If they will give us a clear and definite statement as to *what are the coordinates and momenta of the ether*, and *how transference of energy takes place between these and the molecules*, and if they will show that the Boltzmann-Maxwell Law is violated under conditions under which we have proved it to be unique, a "true bill will have been found."

At present all we assert is that if the "ethereal coordinates and momenta" satisfy a determinantal relation similar to that proved on p. 22 of Dr. Watson's new edition, the Boltzmann-Maxwell distribution, *if it ever once existed*, will be permanent *in the absence of disturbing influences*. But the test case in which molecules are regarded as smooth solids symmetrical about an axis (see my Report, § 45, case iii.) affords an instance in which partition of energy does not take place between *all* the coordinates of a system, the angular velocity of each molecule about its axis of symmetry being constant and unaffected by collisions, and therefore independent of the Boltzmann-Maxwell Law. And why should not a similar explanation be applicable to the ether? At any rate, this hypothesis is supported by the views advanced by Prof. Oliver Lodge at the Nottingham meeting of the British Association ("Nottingham Report," p. 688). G. H. Bryan.

Cambridge, November 30.

It appears to me that the difficulty raised by recent critics against Maxwell's law of partition of energy in the theory of gases, and Boltzmann's minimum theorem relating thereto, by consideration of the effect of a complete reversal of the motions, is capable of direct explanation; and that whatever weak points the theory may have, they are not in that direction. Indeed, if that were not so, the criticism would apply equally against the Second Law of Thermodynamics.

The theorem in question is that there exists a positive function belonging to a group of molecules, which as they settle themselves into a steady state maintains—on the average derived from a great number of configurations—a steady downward trend; that the Maxwell-Boltzmann steady state is that one for