temperature. If an empirical formula were found to fit the values of the Joule-Thomson effect over a *wide* range of temperature, then we might very well conclude—apart from any molecular hypothesis—that this formula was the proper one to employ. It is true that measurements of the Joule-Thomson effect are far from easy to carry out satisfactorily; still, the difficulties are not insuperable, and there is no reason why the success of Joule and Kelvin in this line should not be repeated. The measurements of these last-named experimenters appear to have been confined within narrow limits of temperature, not so much because observations were impossible at temperatures outside these limits, as because Lord Kelvin imagined he had already discovered the true formula for the Joule-Thomson effect.

The plan here advocated of repeating the Joule-Thomson experiments over a wider range of temperature is all the more feasible since we have shown that we require only the relative values of the Joule-Thomson effect. Thus any source of error which multiplies all the Joule-Thomson effects by the same factor would be eliminated The errors of experiment which give rise to ordinary "wobbling" would also be eliminated by the present method. Indeed, the only sources of error which are liable to affect the final numerical results to a sensible degree are those which tend persistently to increase or diminish the Joule-Thomson effect at higher temperatures as compared with that at lower. Provided that such sources of error were either abolished or properly allowed for, we could place considerable confidence in the final numerical results, and probably succeed in throwing great light on a fundamental problem of thermodynamics.

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## XXX. *On the Principle of Relativity and on the Electromagnetic Mass of the Electron. A Reply to* Mr. E. Cunningham. *By* A. H. BUCHERER, *D.Sc., Professor in the Bonn University* *.

IN the October number of this Magazine Mr. E. Cunningham raises some objections to the theory of relativity as defined by me in the April number. I wish to say a few words in reply in order to show that Mr. Cunningham's remarks are due to a misconception on his part of the real meaning and bearing of the principle as used by me.

As appears from my paper, my object has been to find a purely phenomenological method of calculating electromagnetic effects, which should harmonize with all the facts

* Communicated by the Author.

of observation, leaving it to future endeavours to find a physical interpretation of this method. No doubt this way of proceeding implies a certain resignation. But in view of the failure of the electromagnetic theories advanced as yet, it seems the safest road to follow.

My method rests on ths following principles and definitions :—

(1) The validity of the Maxwellian "differential" equations associated with ordinary kinematics.

(2) The distinction, for the mere sake of calculation, between active and passive systems, whenever forces are concerned which two electromagnetic systems in uniform relative motion exert on each other.

(3) The prescription: Whenever the force on one of the two systems due to the other is required, choose the former as the passive one and calculate the force on it exactly as in the original Maxwellian theory, as though it were "at rest in the æther" and the other "moving through the æther."

I have proved (*l. c.*) that this prescription is consistent, *i. e.* that the forces thus calculated are identical whichever of the two systems is chosen as passive. *This implies the principle of relativity of motion for the systems considered.*

Mr. Cunningham will admit that this method of calculating is perfectly definite, and by a careful comparison with the Lorentz-Einstein principle he will convince himself that the two principles are essentially different. The remark of Mr. Cunningham that my principle is identical with that of Einstein except that I omit the transformation of time and space coordinates, appears untenable also from the following consideration. Evidently, according to Mr. Cunningham, a transformation of the forces experienced by a moving electron in a condenser field and in the field of an electromagnet as calculated by me (*l. c.*) should yield the expressions given by Einstein and Lorentz. But an inspection of my equations proves the impossibility of such a transformation. In fact no other known theory of electromagnetism leads to these forces.

As I employ the ordinary kinematics, *only a spherical electron will fit in my theory.* Mr. Cunningham has overlooked this circumstance.

However, it does not follow that the same formula as Abraham's should be applied, as this formula is connected with the expression of the field energy, and the latter is introduced by me as a special hypothesis (*l. c.* p. 418).

Whereas it will be impossible to point to any discrepancy between my theory and experimental facts in the electrodynamics of the relative uniform motion of electric and magnetic masses, the Lorentz theory finds unsurmountable difficulties on theoretical grounds. As was first conclusively shown by Abraham, the Lorentz deformation excludes a purely electromagnetic basis of mechanics. The work of the external electric forces acting on the electron does not have its exact equivalent in the increase of the electromagnetic energy of the electron. Therefore a certain inner energy of non-electromagnetic character must be ascribed to the electron. The same conclusion must be drawn from Mr. Cunningham's calculations, *if they are properly interpreted.*

Mr. Cunningham says the Lorentz-Einstein theory is the only theory that can account for certain optical phenomena. In fact, he asserts that it is required "to explain how a light-wave travelling outwards in all directions with velocity C relative to an observer A may at the same time be travelling outwards in all directions with the same velocity relative to an observer B moving relative to A with velocity $v$." Mr. Cunningham then proceeds to show that this requirement is satisfied by the Lorentz-Einstein transformation.

I am not aware that such a "requirement" is necessary to explain *any known fact of observation.*

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## XXXI. *On the Factors serving to determine the Direction of Sound.* *By* T. J. BOWLKER *.

IN the summer of 1906, while on a steamship off the coast of Maine, U.S.A., I was roused about midnight by the blowing of foghorns, and presently followed the shock and grinding of a collision. It appeared to me that the accident could only be explained by a mistake in judging of the direction of the foghorns of the colliding vessels. This accident suggested a study of the factors determining the direction from which sound appears to come.

During the winter of 1906–1907 I made some experiments. In one of them I placed the ends of two rubber tubes of equal lengths at the ears and moved the end of one towards or away from the source of sound. With equal lengths of tube I thought that the friction and resonance effects would be the same. The sound, as heard through the tubes, did appear to move somewhat to one side or other of the head, but the movements did not appear to have any relation to the wave-length, and the movements were very irregular.

* Communicated by the Author.