bution prevailing in the earth. It can be demonstrated as a mathematical fact that in the absence of terrestrial magnetic observations within and without the earth's surface, an infinite number of different distributions is possible that will satisfy the effects observed on the surface alone.

In conclusion, it is my duty to make one more explanation. Mr. Wilde understood from my first letter that I am still in the employ of the U.S. Coast and Geodetic Survey. In view of the fact stated by him, that he sent, at considerable trouble and expense to himself, a duplicate of his magnetarium to the "Survey," and, hence, it might appear that it was somewhat discourteous in a member of the "Survey" to thus criticise him publicly, I may say that I severed my connection with the Survey two years ago, and that my criticisms have been made without any knowledge whatsoever of what has been accomplished with it by the "Survey."

Friedenau bei Berlin, October 31. L. A. BAUER.

Boltzmann's Minimum Theorem.

MR. CULVERWELL'S letter of October 25 ought to have received a much earlier answer. That it did not do so was owing to purely accidental circumstances which I very much regret.

In that letter Mr. Culverwell criticises my treatment of Boltzmann's familiar proposition concerning the properties of the H function on the following grounds:—

(1) The choice of the generalised coordinates. In investigating the circumstances of a collision or an encounter between two systems of molecules of m and n degrees of freedom respectively, he sees a difficulty in my choice of the coordinates as $Q_1, Q_2 \ldots Q_m, q_1, q_2 \ldots q_n$, where $(q_n = a)$ determines a collision, or encounter. But supposing the requisite number of degrees of freedom to be secured, the choice of the independent variables is surely quite optional. I had myself assumed this as self-evident, perhaps too hastily, but at any rate Mr. G. H. Bryan has placed this proposition beyond doubt, in the exhaustive report submitted by him to the British Association last August. Take for example sets of plane circular disks moving amongst each other in their own plane; here each pair of disks constitutes a material system, whose position is completely determined (assuming the orientation of each separate disk to be indifferent) by the following four variables, viz. the two coordinates of the centre of one disk of the pair, the distance ρ between their centres, and the inclination of that distance to a line fixed in the plane; this third variable ρ is the q_n of my proposition.

(2) Mr. Culverwell objects that the general Boltzmann proposition $\left(\frac{dH}{dt}\right.$, always negative unless $\left.Ff = F'f'\right)$, or H a minimum for one, and one distribution only, cannot be true, because if a system were started from any initial configuration (P, Q), and after the time t arrived at the configuration (p, q) and the definite integral H were evaluated in these two configurations, the proposition asserts that the second H must be less than the first H, or $H_t < H_o$, whence it would follow by the same proposition that if at the end of the time t each velocity component were reversed, the H_{2t} must be less than H_t, and this, doubtless, I do assert. But Mr. Culverwell maintains that such an assertion is obviously untrue because at the end of the second interval t the system has returned to its *original* condition, and therefore H_{2t} must be the same as H_o, and to this proposition I demur.

Doubtless when a material system in a field of any conservative forces is started from any initial position and velocities, arrives after a time t at another position and with other velocities, and here has each velocity reversed, it is true that at the end of the next interval t it will be in its initial position, with each velocity component reversed; but it remains to be proved, and cannot be asserted *à priori*, that the H_{2t} is equal to H_o, and the only proposition available for the investigation is this very proposition of Boltzmann's, which proves that H_{2t} will be less than H_t, and therefore less than H_o.

Finally, Mr. Culverwell inquires, somewhat despondently, if anyone will point out the use of the H function, and what is proved by it.

I have already said in my second edition, that the proposition is not of my invention, and therefore that I have no claim to answer this question with any authority, but to my own mind the proposition appears certainly to clear away one obvious difficulty. Without the aid of this proposition we are enabled to assert that if F (p, q) were a function of the coordinates and momenta of a molecule such that in the absence of encounters *with other molecules*, F remains constant for all time, then the form of F satisfying the condition $Ff = F'f'$ must render F $(p, q)\ dp\ dq$ a *permanent* law of distribution, and therefore if we can assert that F (p, q) must *of necessity* be of the form F (E) (E sum of potential and kinetic energy), then the e^{-hE} law of distribution is certainly a permanent law (neglecting, *i.e.*, all but binary encounters); but in the absence of this proposition, we cannot assert that the $Ff = F'f'$ is *necessary* as well as sufficient, because we cannot insist upon the necessity of an exact compensation in the passage from the $p\ q$ to the $p'\ q'$ state, and conversely, taking place at *each separate* encounter. The H proposition, therefore, removes this element of uncertainty, and reduces the question to that of the F (E) restriction, because it proves that unless $Ff = F'f'$ for *each pair* of encountering molecules, H and therefore F and f must be a function of the time. As I have already asked for a disproportionate share of your space, I will not enter upon the question of the F (E) restriction now.

H. W. WATSON.

I DID not exactly, as Mr. Burbury suggests, question Boltzmann's minimum theorem, but only the pages thereon in Dr. Watson's "Kinetic Theory of Gases." Indeed, I said that though I had not seen Boltzmann's proof, I supposed it to be all right.

It appears from Mr. Burbury's letter that in order to prove the theorem, even for the simple case of perfectly hard and elastic spheres, some amount of assumption as to an average state having been already attained must first be made, and of course the *à priori* reasoning in my letter is not applicable to such a theorem. Mr. Burbury's letter is exactly the kind of letter I hoped to elicit, and if he can say what assumption in a generalised system will replace the assumption of equal distribution of velocities in different direction in a system of hard spheres, he will clear up the whole difficulty. Unfortunately, the case with which he deals is one in which the error-law is known from other considerations to be the only permanent state.

I observe that Mr. Bryan, in his British Association Report, quotes the oversight I pointed out in Dr. Watson's proof, without making any criticism on it.

EDWD. P. CULVERWELL.

Trinity College, Dublin, November 24.

The Alleged Absoluteness of Motions of Rotation.

PROF. GREENHILL, in his review of Mach's "Science of Mechanics" (NATURE, November 15), writes as if he disapproved of that author's not accepting "Newton's distinction between the relativity of motion of translation and the absoluteness of motion of rotation." He appears to think that Mach would have obtained more insight into this distinction from a study of Maxwell's "Matter and Motion." It might more truly be said that Maxwell would have profited by a perusal of Mach's book The latter finally refutes the paradox contained in Newton's statement, and supported by Maxwell, and by so doing renders a great service to Mechanical Science. He has disposed once and for all of "absolute rotation." It is high time that writers on Mechanics should revise the preliminaries of their science so as to state their results in terms of relative motion, whether of translation or rotation. This has been partially done by Maxwell, and a further step has now been taken by Mach. It is unfortunate that the reviewer in drawing attention to this part of the book should have preferred to stand by the prejudice he owes to Newton and Maxwell when he might have done something to hasten its abandonment.

A. E. H. LOVE.

St. John's College, Cambridge, November 20.

MACH says truly (p. 237) "that precisely the apparently simplest mechanical principles are of a very complicated character; that these principles are founded on uncompleted experiences, which can never be fully completed," &c.

The modern student of theoretical mechanics is in a dilemma;