well be doubted whether such a definition can be framed in the present state of knowledge. What Mr. Galton's definition or phrase does accomplish, is to point out *some* characters which may certainly be classed as "acquired" and not "inherited," and from the study of which we may accordingly start in the inquiry as to whether or not the acquired characters of one generation may become inherited in a subsequent generation. "Characters," writes Mr. Galton, "are said to be acquired when, &c." This by no means asserts that there are not other characters which should be regarded as acquired, if we knew fully about their history; for instance, at the very moment when our observation is being made on a group of individuals, some might conceivably be exhibiting a character inherited from the last generation, and other specimens might be exhibiting exactly the same character acquired *de novo*. Such cases (supposing that they ever occur) would not help us at all in the attempt to determine whether acquired characters are transmissible; and the fact that they are not included in Mr. Galton's definition (though their existence is not expressly denied) renders that definition a more practical one, and more useful to the experimental naturalist than a more comprehensive definition which could not be brought to a practical issue.

Lastly, it seems to me that Mr. Galton's definition is precisely what Lamarck pointed to in his "Première loi" and the first sentence of his "Deuxième loi." The reciprocally destructive nature of the propositions contained in those two laws I pointed out, in a former letter, and have not yet had the pleasure of seeing, in reply, any defence of Lamarck's position from one of his adherents. E. RAY LANKESTER.

Oxford, January 4.

Boltzmann's Minimum Theorem.

THE remarkable differences of opinion as to what the H-theorem *is*, and how it can be proved, show how necessary is the discussion elicited by my letter on the oversight in Dr. Watson's proof. Each of the four authorities who have replied takes a different view.

Dr. Larmor enforces the view I put forward at the close of my letter, and says that the theorem *is* what I said appeared an *à priori* possibility; and I may here point out that his letter is a complete answer to the argument I used in the *Phil. Mag.* 1890, p. 95, urging that, as there were as many configurations which receded from the permanent state as approached it, there was an *à priori* improbability that a permanent state would ever be reached. This argument was criticised at some length, not really answered, in Messrs. Larmor and Bryan's Report on Thermodynamics (British Association Report, 1891), but the suggestive remarks there given helped me, I think, to arrive (independently) at the complete answer given in Dr. Larmor's recent letter. But my present use of the argument is not that which Dr. Larmor criticises; I now use it as a test of a particular proof of the H-theorem. I say that if that proof does not somewhere or other introduce some assumption about averages, probability, or irreversibility, it cannot be valid.

Mr. Burbury appears to consider that the theorem can only be proved if we assume that some element of the distribution does tend to an average (quite a different position from Dr. Larmor's), and he is as yet unable to state the appropriate assumption except for the case of hard elastic spherical particles colliding or "encountering" (for since a is constant in his last letter, it seems as if the $q_1 \ldots q_{n-3}$ coordinates are really dummies). Yet Mr. Burbury has already given what purports to be a *general* proof of the theorem for any number of degrees of freedom.

Mr. Bryan thinks that a condition which excludes the reversed motion is implied in Dr. Watson's proof, for he says that in taking unaccented letters Ff as proportional to the number of molecules passing from one configuration to another in the reversed motion, I make a less "natural" supposition than Dr. Watson, who takes accented letters $F'f'$. I cannot see what virtue there is in putting accents on or leaving them off, and after a very careful study of Mr. Bryan's letter, I can only think that he has fallen into some confusion owing to the way in which he uses at one time *accented* and at another time *unaccented differentials*, although (as he himself remarks) there is no difference whatever between their accented and unaccented *products*. But even if Mr. Bryan be right, would he put us any "forrarder"? What we want is a *proof* that the collisions will make H decrease, and we can hardly be satisfied with a proof which depends on the previous assumption that the particles do "naturally" tend to move in the desired way.

Dr. Watson meets my reversibility argument by saying that H decreases even in the reversed motion, when the system is confessedly *receding* from its permanent state. No other correspondent agrees with him in this view, which would indeed *take away all physical meaning from the H-theorem*, for the decrease of H would then be quite unconnected with the approach to a permanent state. As to the other point, Dr. Watson does not amend his proof himself, but says it is "easy" to do, and so does Mr. Bryan. Yet one has an instinctive distrust of things which are said to be "easily seen," and at all events Dr. Watson's reference to the case in which the theorem is *applied* does not help one in the *proof*, where it is necessary to express *separately* the products of the differentials expressed by the small and capital letters respectively in his "Kinetic Theory."

Mr. Burbury asks why I say the error law has been proved for the case of hard spheres without the use of Boltzmann's Minimum Theorem. I thought Tait had done so (*Trans.* R.S.E. 1886), and at all events I thought the ordinary investigation showed that there was but *one* solution, that of the error law, in that case; but perhaps I am mistaken.

Mr. Bryan says Lorenz gives the clearest account of the assumptions in Boltzmann's theorem. He would earn our gratitude if he would state them in his next letter.

EDW. P. CULVERWELL.

Trinity College, Dublin, December 29, 1894.

Aurora of November 23, 1894.

OBSERVATIONS of this aurora, by Mr. James T. Pope, at Dingwall, in the north of Scotland, have been sent to me by Mr. H. Corder, of Bridgwater, a few particulars having also been recorded here of the appearance, which, although the distance of this place from Dingwall falls but very little short of 400 miles, yet showed some very excellent agreements with Mr. Pope's description.

Beginnings of the aurora were seen by Mr. Pope between 6 and 7 p.m., as a glow which brightened gradually along the eastern, and sent up a few faint streamers from the western parts of the horizon towards the north, until 6.30, gradually fading out, after that, till nearly 7 p.m. The glow then gradually reappeared as a bright band, brighter in the east than in its western half, stretched across the sky from east to west, somewhat southward from the zenith. This band of light continued very bright for some time, but faded out gradually towards 7.30, the streamers in the north-west at the same time increasing continually in brightness.

Near Slough the display was first noticed about 7.15 p.m. as a low ill-defined white bow, stretching, at about half the altitude of those stars above the horizon, from under ζ to under ν Ursæ Majoris (altitudes 19° and 24°, azimuths 13° W. and 16° E.) from north). A little later, towards 7.30 p.m., this arc had become a bright narrow band, a degree or two in width, and about 25° long, extending from η Ursæ Majoris in the west (altitude 15°, 19° W. of north) to a few degrees under γ and β Ursæ Majoris (altitudes 16° and 19°, 2° W. and 6° E. of north) on a slightly downward slope to some degrees eastward from the latter star. It faded out partially about 7.30 p.m., leaving two bright remnants across η, and under β Ursæ Majoris, each about 8° long, while a third just similar wisp of light appeared on the same line's far leftward prolongation; this western offshoot of the band continued with the other two short segments till all had faded out at 7.45 or 7.50, marking the arc's considerable but not otherwise traceable extension westwards, across ϵ, ζ Herculis (altitudes 17° and 15°, 60° and 55° W. from north).

Dingwall is about 390 miles distant from Slough, in the direction 18° or 19° west from north; so that it appears that the strong part of the glow-band seen most brightly in the east from Dingwall, was alone observable here (if we except the light-wisp in Hercules towards the west, at last), in the vapoury sky near the horizon.

Beginning with an average altitude of between 9½° and 12°, or of about 11°, at 7.15, the band in growing stronger reached an altitude, at Slough, of 14° or 15° towards 7.30, during about the space of time when it was most distinct, and seen most strongly in the east at Dingwall extending from east to west somewhat southward from the zenith. If its altitude there was at that time about 60°, and at Slough about 13°