If this were so, by using broad bands instead of lines, one would expect to get a coloured line at each border, while the central portion remained uncoloured. Prof. Liveing's theory would lead one to expect a uniformly coloured band.

Accordingly, we prepared a disc, one half of which was blackened and the other half left white, as in Mr. Benham's top, and on the white half were described three black circular bands about one centimetre broad, with radii of about 4·7 and 10 centimetres respectively, and each with an arc of 45°. The outer band was described in an opposite sense to the two inner bands, that is to say, in such a manner that if the disc was rotating so that the order of succession for the two inner bands was black field, bands, white field, for the outer band it was white, bands, black. The disc was rotated by attaching it to the spindle of an electromotor, the speed of which could be graduated by friction against the spindle.

On rotating the disc at a slow speed, in such a direction that the order of succession for the two inner bands was black, bands, white, the following results were obtained:—The two inner bands were each bounded on both inner and outer edges by a bright red line, fading off towards the centre in a dark, somewhat reddish, ground, which became less coloured and darker as it approached the centre, The outer band presented a marked contrast to these two; the whole extent of the band itself was black, entirely free from colour, but on the white ground on both borders of the band, and apparently outside it, there appeared a brilliant coloured band, varying in colour from blue to green. On reversing the direction of rotation, the appearances were exactly reversed; the inner bands now became black bordered by green, and the outer band reddish black lined by bright red.

We do not contend that this experiment proves that the effect is one of irradiation, for it might easily be supposed that the heightened effect at the border is one of contrast with the surrounding whiter coloured field. Our next experiment, on the effect of change in the speed of rotation, also tends to negative the idea that the colours are due to irradiation, as there is no reason to think that if irradiation were the cause, there would be a change in colour with a change in the speed of rotation, as was found to be the case.

To test the effect of change in the speed of rotation, a disc of one of Mr. Benham's tops was detached, fixed to the spindle of the electromotor, and rotated in such a direction as to cause the three central bands to appear red with a slow speed of rotation. On gradually increasing the speed, a remarkable series of changes in appearance presented itself. The bright blood-red of the three inner lines gradually grew darker and duller, and then passing rapidly through a transition, the shades of which we were unable to observe, gave place to a most vivid green, which in turn, with still increasing speed, passed through another transition stage into blue, deepening into a full violet at the greatest speed we could obtain. On causing the motor to slow down, the same changes in an inverse order from violet to red were observed. These changes in colour *with the same direction of rotation* are very remarkable, and seem to us to be in direct opposition to Mr. Benham's explanation supplied with the top; for if the colours are due to a certain percentage of the etherial vibrations being cut off, this percentage will remain the same for all speeds, and there is no reason apparent why there should be a change in colour with difference in speed. Neither are they easily explainable on Prof. Liveing's theory that red is the first colour to appear, and blue the last to disappear; also, the green we got at the intermediate rate was certainly not a neutral grey or green, but a pure vivid green. Probably other observers have not used a greater speed than that of the first transition stage from red to green, which has a kind of neutral green tint.

An experiment was next made with a disc constructed similarly to that of Mr. Benham, but having white lines drawn on the black semicircle instead of black lines on the white part. On rotating this disc so that *white* lines on *black* ground succeeded the black surface, with slow rotation the colour obtained was red, but a different kind of red to the deep blood-red given by Mr. Benham's top in the case of *black* lines on a *white* ground also following a black surface, viz. a very bright red, evidently not saturated. With higher speeds there followed a light green and light blue, both evidently containing white. The difference in hue of the two series of colours seems obviously that in the case of the white-lined disc the colours are mixed with white, and in that of the black-lined are mixed with black.

Throughout the series of experiments we have tried to eliminate psychical errors as much as possible, by experimenting on persons unacquainted with the expected results.

Belfast, January 15. J. M. FINNEGAN.
B. MOORE.

The Kinetic Theory of Gases.

THE difficulty of reconciling line spectra with the kinetic theory of gases, has been referred to by Prof. Fitzgerald (NATURE, January 3, p. 221). The following considerations show that it is possible under certain suppositions to have a number of spectral rays with a very restricted number of degrees of freedom. Most of us, I believe, now accept a definite atomic charge of electricity, and if each charge is imagined to be capable of moving along the surface of an atom, it would represent two degrees of freedom. If a molecule is capable of sending out a homogeneous vibration, it means that there must be a definite position of equilibrium of the "electron." If there are several such positions, the vibrations may take place in several periods. Any one molecule may perform for a certain time a simple periodic oscillation about one position of equilibrium, and owing to some impact the electron may be knocked over into a new position. The vibrations under these circumstances would not be quite homogeneous, but if the electron oscillates about any one position sufficiently long to perform a few thousand oscillations, we should hardly notice the want of homogeneity. Each electron at a given time would only send out vibrations which in our instruments would appear as homogeneous. Each molecule could thus successively give rise to a number of spectral rays, and at any one time the electron in the different molecules would, by the laws of probability, be distributed over all possible positions of equilibrium, so that we should always see all the vibrations which any one molecule of the gas is capable of sending out. The probability of an electron oscillating about one of its positions of equilibrium need not be the same in all cases. Hence a line may be weak not because the vibration has a smaller amplitude, but because fewer molecules give rise to it. The fact that the vibrations of a gas are not quite homogeneous, is borne out by experiment. If impacts become more frequent by increased pressure, we should expect from the above views that the time during which an electron performs a certain oscillation is shortened; hence the line should widen, which is the case. I have spoken, for the sake of simplicity, as if an electron vibrating about one position of equilibrium could only do so in one period. If the forces called into play, by a displacement, depend on the direction of the displacement, there would be two possible frequencies. If the surface is nearly symmetrical, we should have double lines.

The only weight I attach to these speculations lies in the illustration it affords that a number of spectral lines does not necessarily mean an equal number of degrees of freedom. In the existence of the "electron" I firmly believe; and this necessarily implies a very restricted number of variables

ARTHUR SCHUSTER.

"Acquired Characters."

IT would appear that Prof. Lankester has not thought it worth while to read all the letters that have appeared in NATURE on the question raised by Sir Edward Fry, unless it is to be inferred from his remarks that he confines himself to the consideration of the arguments of those who have a place on the scientific Olympus of the Royal Society. In my letter, published December 6, I defended Lamarck's laws against the accusation that they were reciprocally destructive. Prof. Lankester reiterates his accusation without any further support. But this is not the whole question. In his last letter he suggests that acquired characters corresponding to Mr. Galton's definition should be taken, and an investigation made as to whether they are inherited or not in later generations. But in his former letter (November 29) he suggested very distinctly and deliberately that such an investigation was unnecessary, because the question was already settled. He has already condemned the heretic, and now consents to his trial. His words were—"Since the old character had not become fixed and congenital after many thousands of successive generations of individuals had developed it in response to environment, but gave place to a new character when new conditions operated on an individual, why should we suppose that the new character is likely to become fixed after a much shorter time of responsive existence?" To apply this