LETTERS TO THE EDITOR.

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The University of London.

I AM anxious to make it clear that what Sir John Lubbock has sprung upon us is a radical change in the procedure of Convocation.

The object can only be, it appears to me, to obtain a reversal of its policy. As a political expedient it is, therefore, very similar to the action of those politicians who for analogous reasons would change the constitution of the House of Lords.

Sir John now defines what he calls his "suggestion" in the following words :—"That in voting on the new Charter, members of Convocation should do so 'as at a senatorial election,' *i.e.* by voting papers." I call this a radical change in the procedure of Convocation.

I put aside the not immaterial point that as a Statutory Commission is a delegation from Parliament, the result of its labours will not be embodied in a Charter, but will be virtually in effect an Act of Parliament when approved by that body.

Sir John has made the following statements about his "suggestion" :—

(1) "I am not asking that any privilege which they do not at present possess should be conferred upon my constituents, but only supporting what is now their *legal right* . . . This right I know they highly value" (NATURE, July 18, p. 269).

(2) "It is the *law* at present" (NATURE, August 8, p. 340).

The words which I have put in italics are definite and explicit, and are, of course, in flat opposition to my repeated statement that Sir John's suggestion amounts to a fundamental and, indeed, revolutionary change of procedure. This change consists in extending the mode of voting in a senatorial election to other matters. Now the mode of voting at a senatorial election is prescribed by the 21st clause of the Charter, which is printed in NATURE for July 25, p. 296. It embraces two very important points. First, the right of absent members to vote at all is not absolute but only *permissive*. The words are: "Power to the Convocation, *if it shall think fit*, to enable absent members of the Convocation to vote on such nominations . . . by voting papers." Secondly, this permissive right is strictly limited by the words "*but not so to vote on any other matter.*"

It is upon this vital discrepancy between Sir John's statements quoted above and the provisions of the Charter that I think it is imperative that he should give some explanation. This demand on my part he is pleased to call an "attack." Well, however that may be, he at least owes it to himself to meet it.

I trust, however, that I have now made it clear, and even to Sir John, that his "suggestion" is not the law, but that, further, it involves the abrogation of a portion of the Charter. I think as a member of Convocation that in making such a proposal without consulting that body he has exceeded his functions as our Parliamentary representative. At any rate it must, I think, be admitted that he is making short work of the "right" which his "constituents highly value." (NATURE, August 8, p. 340.)

I am unwilling to prolong a painful discussion. But as Sir John is pledged to bring forward his "suggestion" in Parliament, which of course can incorporate it in the Bill, if it thinks proper, it seems to me of extreme importance to dissipate his contention that it is already the "law." W. T. THISELTON-DYER.

Kew, August 23.

The Nomenclature of Colours.

THE interesting article of Mr. J. H. Pillsbury, published in your last number, recalls to me a passage in my autobiography, which, though it is already in print, will not be issued until after my death. As bearing on the question Mr. Pillsbury raises, this passage may, perhaps with advantage, be published in advance. The plan suggested aims at no such scientific nicety of discrimination or naming as that he proposes, but is one which is applicable with the means at present in use. It is, as will be perceived, based on the old theory respecting the primary colours; but whatever qualification has to be made in this, need not affect the method described. The passage is as follows :—

"I mention it here chiefly for the purpose of introducing an accompanying thought respecting the nomenclature of colours. The carrying on of such a scheme would be facilitated by some mode of specifying varieties of tints with definiteness; and my notion was that this might be done by naming them in a manner analogous to that in which the points of the compass are named. The subdivisions coming in regular order when 'boxing the compass,' as it is called, run thus :—North, north by east, north-north-east, north-east by north, north-east ; north-east by east, east-north-east, east by north, east. Applying this method to colours, there would result a series standing thus :—Red, red by blue, red-red-blue, red-blue by red, red-blue (purple) ; red-blue by blue, blue-red-blue, blue by red, blue. And in like manner would be distinguished the intermediate colours between blue and yellow and those between yellow and red. Twenty-four gradations of colour in the whole circle would thus have names ; as is shown by a diagram I have preserved. Where greater nicety was desirable, the sailor's method of specifying a half-point might be utilised—as red-red-blue, half-blue ; signifying the intermediate tint between red-red-blue and blue-red by red. Of course these names would be names of pure colours only—the primaries and their mixtures with one another ; but the method might be expanded by the use of numbers to each : 1, 2, 3, signifying proportions of added neutral tint subduing the colour, so as to produce gradations of impurity.

"Some such nomenclature would, I think, be of much service. At present, by shopmen and ladies, the names of colours are used in a chaotic manner—violet, for instance, being spoken of by them as purple, and other names being grossly misapplied. As matters stand there is really no mode of making known in words, with anything like exactness, a colour required ; and hence many impediments to transactions and many errors. In general life, too, people labour under an inability to convey true colour-conceptions of things they are describing. The system indicated would enable them to do this, were they, in the course of education, practised in the distinguishing and naming of colours. If, by drawing, there should be discipline of the eye in matters of form, so there should be an accompanying discipline of the eye in matters of colour."

Were some authoritative body to publish cards representing these various gradations of colour, arranged as are the points of the compass, each division bearing its assigned name, as above given, such cards might serve as standards ; and any one possessing them would be able to indicate, within narrow limits, to a shopkeeper or manufacturer, the tint he or she wanted. Of course to complete the method it would be needful that there should be a mode of indicating gradations of intensity, and if the numbers 1, 2, 3, were appended to indicate the degrees of impurity by mixture with neutral tint, *a*, *b*, *c*, might be used to signify the intensity or degree of dilution of the colour.

Very possibly, or even probably, this idea has occurred to others, for it is a very obvious one. HERBERT SPENCER.

The Mount, Westerham, July 23.

Clausius' Virial Theorem.

THE above-named theorem, which appeared in the *Phil. Mag.* for August 1870, much as it is now used in connection with the kinetic theory of gases, received little, if any, attention in England for some time after its introduction. Apparently the theorem was accepted without hesitation or discussion, and, as far as I can learn, neither on its first introduction or since has it received any adverse criticism, or, in fact, any criticism whatsoever. My object in writing this letter is, in the first place, to direct attention to the arguments used by Clausius to establish his theorem, which appear to me to be unsound, and secondly, by applying a simple test case, to show that the theorem itself is not true.

Clausius first proves the following equation.

$$\frac{m}{4t}\int_0^t \frac{d^2(x^2)}{dt^2}dt = \frac{m}{2t}\int_0^t x\frac{d^2x}{dt^2}dt + \frac{m}{dt}\int_0^t \left(\frac{dx}{dt}\right)^2 dt.$$

If for the moment, for the sake of simplicity, we divide both sides of the equation by $\frac{m}{2t}$, we get

$$\tfrac{1}{2}\int_0^t \frac{d^2(x^2)}{dt^2}dt = \int_0^t x\frac{d^2x}{dt^2}dt + \int_0^t \left(\frac{dx}{dt}\right)^2 dt,$$

and this may be written

$$ux = \int_0^t x\,du + \int_0^t u\,dx.$$

In this form it is easy to see that each term may be graphically represented by an area, and the equation simply expresses the fact that the rectangular area xu is equal to the algebraic sum of the areas $\int_0^t udx$ and $\int_0^t xdu$. It is obvious that for periodic motion the rectangle xu will vanish when a suitable value is given to t; but so also will the areas $\int_0^t udx$ and $\int_0^t xdu$. So that when $xu = 0$ we get, either

$$\int_0^t udx = 0 \text{ and } \int_0^t xdu = 0; \text{ or } \int_0^t udx = -\int_0^t xdu.$$

Again, in what Clausius calls "stationary motion" when xu does not vanish periodically, although we can make the expression $\frac{m}{2t}ux$ vanishingly small, by taking t very great, it is obvious that if the areas $\int_0^t udx$ and $-\int_0^t xdu$ are not equal before multiplying them by $\frac{m}{2t}$, the expressions so obtained are not so afterwards. Moreover, and finally, it should be observed that the expression $m\int_0^t udx$ *does not represent kinetic energy;* to represent which the expression should be $m\int_0^t udu$. The above considerations seem to me to entirely upset Clausius' demonstration.

In the tenth edition of Maxwell's "Heat" (p. 323), Lord Rayleigh has given an illustration of the manner in which he supposes the "virial" to act in opposition to kinetic energy, and we may take his illustration as a simple test of the theorem. He supposes two bodies, each of mass m, to revolve in a circular path with a constant velocity about their centre of gravity. Here, as there is no pressure, the so-called virial equation takes the form

$$\Sigma\tfrac{1}{2}mv^2 = \tfrac{1}{2}\Sigma Rr.$$

In the above equation v, the velocity, is constant, and $R = mf$. If we take ρ as the radius of the circle, then $r = 2\rho$, and the equation becomes

$$\tfrac{1}{2}v^2\Sigma m = \tfrac{1}{2} \times 2\rho f\Sigma m.$$

Hence

$$\tfrac{1}{2}v^2 = \rho f;$$

which equation *does not represent the ordinary law of centrifugal force.* Lord Rayleigh omitted to notice that

$$\Sigma R = \Sigma mf = f\Sigma m = 2mf.$$

When, however, we throw overboard all ideas of "virial," and look upon the term $\frac{1}{2}\Sigma Rr$ in the so-called "virial equation" as simply representing work and equal to $\frac{3}{2}pV$, also an expression for work, then the equation

$$\Sigma\tfrac{1}{2}mv^2 = \tfrac{3}{2}pV + \tfrac{1}{2}\Sigma Rr$$

is certainly true. But there seems no possible advantage to be obtained in splitting the right-hand member into two equal terms, instead of writing the equation

$$\Sigma\tfrac{1}{2}mv^2 = 3pV; \text{ or } \Sigma\tfrac{1}{2}mv^2 = \Sigma Rr;$$

in either of which forms—the first for preference—it is applicable to ideal gases. For natural permanent gases the equations become, either

$$\Sigma\tfrac{1}{2}\beta mv^2 = 3pV; \text{ or } \Sigma\tfrac{1}{2}\beta mv^2 = \Sigma Rr,$$

and not

$$\Sigma\tfrac{1}{2}\beta mv^2 = \Sigma\theta Rr,$$

as given in my letter (p. 221) on "Argon and the Kinetic Theory." C. E. BASEVI.

London, W., August 14.

Incubation among the Egyptians.

ARTIFICIAL incubation, like many another practice supposed to be peculiar to modern civilisation, is but a revival from very ancient times. Diodorus, an author who wrote about forty years before the commencement of the Christian era, tells how the Egyptians of his time, with their own hands, bring eggs to maturity, and how the young chickens thus produced are not inferior in any way to those hatched by the usual means.

The practice, probably with methods differing little from those of ancient times, survives to the present day among the fellahs of Egypt. In suitable places ovens are erected, and the proprietors go round the neighbouring villages collecting eggs. A sufficient number having been collected, they are placed on mats strewed with bran, in a room about 11 feet square, with a flat roof. Over this chamber, which is about 4 feet high, there is another built about 9 feet in height. The roof, which is vaulted, has a small aperture in the centre to admit light during the warm weather; below it another opening of larger dimensions communicates with the oven below. In the cold weather both are kept closed, and a lamp is kept burning within. Entrance is then obtained from the front of the lower chamber. In the upper room fires are made in troughs along the sides, and the eggs are placed on the mats below in two lines, corresponding to and immediately below the fires. The fires are lighted twice a day, the first time to die about midday, the second to last from about 3 p.m. to 8 p.m. The first batch of eggs are left for about half a day in the warmest situation, after which they are moved to make room for others, until the whole number in hand have had the benefit of the position. This is repeated for six days. Each egg is then examined by a strong light. All eggs that at this stage are clear are rejected, but those that are cloudy or opaque are restored to the oven for another four days. Then they are removed to another chamber, where there are no fires, but the air is excluded. Here they lie for five days, after which they are placed separately, about one or two inches apart, and continually turned. This last stage generally takes six or seven days. During this time a constant examination is made by placing each egg to the upper eyelid, when a warmth greater than that of the human skin is a favourable sign. The duration of the process generally extends over twenty-one days, but thin-shelled eggs often take only eighteen days. The average heat required is 86° F. Excessive heat is prejudicial. In Egypt the best time is from February 23 to April 24.

J. TYRRELL BAYLEE.

Mountain Sickness.

I HAVE just come back from a journey in the region of the Andes, and in looking over the numbers of NATURE, which had accumulated during my absence, I came across the extract, which you make in your notes of February 21, from the *Revue Scientifique*, on the subject of mountain sickness. I cannot agree with M. Kronecker's statement that beyond three thousand metres mountain sickness attacks all persons as soon as they indulge in the least muscular effort, as I made the acquaintance of many people, mostly railway men, living and working at altitudes of fourteen or fifteen thousand feet on the Oroya line and the Southern Railway of Peru, who had never experienced *soroche*, or mountain sickness. As far as my own experience goes, in three journeys across the Andes and several mountain ascents, including one to the top of the crater of the Misti, 19,300 feet above sea level, I had only one attack of *soroche*, and that was at the end of a ride on an oil engine from sea level to fourteen thousand feet in nine hours. But this was so complicated with suffocation by the oil fumes and scorching by the heat of the furnace while running through the fifty-seven tunnels on the line, that I cannot say how much was mountain sickness and how much was not. At any rate, I was perfectly well the next morning, and rode over a pass nearly seventeen thousand feet high without the slightest inconvenience. As regards the danger of a prolonged sojourn, my experience teaches me that it is almost entirely due to personal idiosyncrasy and unwise eating and drinking. A healthy person whose lungs and heart are all right, who does not over-eat and is very moderate in the use of stimulants, will not suffer from mountain sickness after the first few hours, and in many cases will not suffer at all if the ascent is sufficiently gradual. Of course very violent exertion produces distress by reason of the deficiency of oxygen. I do not think that there need be any difficulty about the officials of the proposed Jungfrau railway, if steady men, not of a full habit of body, are selected. I never heard of any trouble from mountain sickness among the Peruvian railway men unless they over-stimulated, and yet they are accustomed to go in a day from sea level to 15,764 feet on the Oroya line, and to 14,666 feet on the Southern line, and return to sea level on the following day. I may add that I have made both these journeys myself without the slightest inconvenience, and have been able to walk and ride without any trouble at the end of them.

London, August 20. GEORGE GRIFFITH.