

On seeing it in print, however, a natural answer occurs to me, which it may be worth while to give. The whole point of the reasoning depends on assumed properties of vacuum.

The assumptions are as follow :—

(1) That a perfect vacuum is an absolute non-conductor of electricity.

(2) That no contact E.M.F. exists between a metal and a vacuum.

(3) That vacuum has a specific inductive capacity.

Grant all these, and the argument is sound. Decline to admit any of them, and it proves nothing. Break down the first two of them, and it proves too much: it proves the non-existence of any thermal contact-force whatever between conductors. For if there were any E.M.F. at the metallic contact, and none at the other or vacuum contacts, a continuous current would flow, propelled by energy derived from a cold place.

This argument is indeed the ordinary one to prove that the algebraic sum of the E.M.F.'s at all the junctions of a closed conducting circuit in which no energy but heat is supplied must be zero when the temperature is uniform.

The proof scarcely holds when insulators are interposed, though the *fact* may be true nevertheless. When chemically active substances with their extraneous supply of energy are interposed, the fact itself is no longer true. But how do we know what is true when vacuum is interposed? The hypothesis on which the argument is founded is a baseless conjecture.

But it may be said, Are not the hypotheses probable? Do you not yourself believe them? I believe in (1) and (3) provisionally, but certainly not in (2). The contact E.M.F. between two substances is probably some surface action or skin phenomenon, and I see no reason why it should not occur as well in the boundary between metal and void as in the boundary between one metal and another. Indeed, it is not improbable that the sum of the E.M.F.'s in every circuit of chemically inert substances, whether conducting or not, and inclusive of vacuum, is zero under uniform temperature conditions.

All that is wanted to establish this is the knowledge that in a circuit of any one substance at non-uniform temperature the total E.M.F. shall be zero,¹ or that the Thomson effects in a single substance always balance each other; *i.e.* that the total E.M.F. in a circuit shall depend on a potential function of temperature, or $dE = f'(t)dt$.

Now it is quite true that this $f'(t)$ is the Peltier coefficient divided by absolute temperature, and that $f(t)$ in its most general form contains an arbitrary constant, but what of that? Nothing is known of $f(t)$ except that it is a potential function: it is not known to represent any physical effect. I never said that the Peltier effect enabled us to find the most general form of the function $f(t)$; I said it gave us the E.M.F. at a junction.

And there is much ground for the assertion; for it is easy to show that in a simple AB circuit, with junctions at t_1 and t_2 , the total E.M.F. is

$$E = \Pi_1 - \Pi_2 + \int_{t_2}^{t_1} (\Theta_A - \Theta_B) dt ;$$

just as if the resultant E.M.F. were the algebraic sum of two Peltier E.M.F.'s and of two Thomson E.M.F.'s.

My only contention is that this equation, which is undeniably true when the Π are interpreted as heat-coefficients, is also true and immediately interpretable when they stand for contact E.M.F.'s. The burden of proof as to the physical existence of an unnecessary and in every sense arbitrary constant rests with those who doubt this simple explanation.

It is difficult to see how a doubt can arise, or how the Peltier and Thomson productions or destructions of heat can be accounted for without local E.M.F.'s. Nohow, so Dr. Hopkinson has proved, and I also have insisted (*Phil. Mag.*, October 1885, and March 1886), except by some wildly gratuitous assumption of an actual physical specific heat for electricity, dependent on the temperature and on the metal in which it happens to be.

Liverpool, December 14, 1889.

OLIVER J. LODGE.

Mirages.

THE article in NATURE of November 21, 1889 (p. 69), recalls to me mirages I saw in March 1888, while travelling in the East on the steam yacht *Ceylon*.

On the 29th we were crossing the Black Sea from Sebastopol.

¹ Hopkinson virtually pointed this out, *Phil. Mag.*, October 1885.

It was a fine cool day and quite calm. In the afternoon a false or mirage horizon about 3° above the true one was visible for a few hours. No objects were within range of vision. The mirage disappeared as the sun declined.

The next day was very much warmer, and we saw a more marked and interesting mirage in the afternoon as we were steaming across the Sea of Marmora away from Constantinople. In this case it appeared only in the west, and objects were seen reflected in an inverted position. A small conical-shaped island was seen with its inverted image at times distinct from and at times blending with the original. The image was distinctly seen of some land, which was actually below the horizon. The mirage of the reflection of the sun in the sea was, when seen through a glass, especially beautiful. It resembled a glorious cataract of golden water. This mirage lasted till quite the dusk of the evening, and then gradually thinned down and died away.

I do not know whether mirages at sea are uncommon; but as the officers on board did not remember seeing one before, I thought these instances might be worth recording.

ARTHUR E. BROWN.

Thought Cot, Brentwood, December 31, 1889.

Self-luminous Clouds.

I AM very sorry that I took no notes, some six or seven years ago, on the first and only occasion of my seeing self-luminous clouds, but though I can give neither date nor positions, the following facts are still fresh in my memory.

Passing through Bushey Park after dark, I noticed an aurora borealis, and, as I had only recently seen the rather rare phenomena of the rays of the setting sun converging towards a point in the east, I followed the direction of one of the principal beams of light towards the south, when, at a point somewhat south of my zenith, I noticed an equatorial belt of luminous clouds. I found that each cloud belonged to a ray, and faded and brightened with it, but was separated by about 60° of clear sky. This belt of clouds extended down to the western horizon, the eastern one was obstructed by trees, while shortly afterwards small dark clouds appeared on that side, and the sky soon became overcast.

The luminous clouds were quite transparent, so that even faint stars could be seen through them when at their brightest. I have heard from Scandinavian captains that these luminous belts are sometimes seen in northern latitudes, and are sure signs of bad weather. I have written these few remarks in the hope that those of your readers who may have the chance of seeing an aurora borealis will also look out for these clouds, and if possible determine their position.

C. E. STROMEYER.

Strawberry Hill, January 4.

The Revised Terminology in Cryptogamic Botany.

THE anglicized forms of most of the terms in common use, employed in the "Hand-book of Cryptogamic Botany" recently issued by Mr. G. Murray and myself, have not up to the present time found much support from our fellow-botanists. I propose, therefore, to give, in some detail, the reasons which have induced us to adopt them, and to urge their general use on writers on cryptogamic botany. For this purpose we will take as our text extracts from three reviews of the "Hand-book," marked, as all the critiques have been, with only one or two exceptions, by a generous appreciation of the difficulties of our task, and a too great leniency to the many shortcomings of the work:—"The most conspicuous, though not the most important, of these [changes] is the adoption of anglicized terminations for Latin and Greek technical words. This is a matter in which it is hard to draw the line aright. . . . As a matter of taste we think the authors have gone much too far in this direction. They complain of the 'awkwardness and uncouth form of these words'; we should have thought the reproach applied much more strongly to 'cenobe,' 'sclerote,' 'nemathece,' and 'columel'" (NATURE). "An Englishman may guess what 'archegone' is short for, for example; but why puzzle a foreigner with a new form of a word with which he is familiar in every treatise hitherto written on the special subject in any European language?" (*Academy*). "Too sanguine expectations on this head might well be toned down by remembering the complete failure of the somewhat similar experiment made by Lindley. . . . Primworts, spurgeworts, bean-capers, and hip-