

The exception mentioned above is an extract from Clerk Maxwell, which is certainly erroneous, and from which Mr. O'Toole gets a good deal of fun. We will not suggest that the addition of a single word would make the passage correct, for we should be told that text-books ought to be perfect. But it is only just to mention that the error occurs in an explanation of the name; in the definition of the thing the error does not occur; nay, it is expressly contradicted.

After this it is not unkind to condemn those doctors who drop the name "potential E." and replace it with such phrases as "E. of repose," &c., implying that the energy in question is not due to motion? By-the-by where is the bull in "passive energy"? and what is the "action" that may be confounded with kinetic energy?

B.—*Potential E. as meaning "Energy related to Potential Functions."*

The word Potential may be used in a second sense. This of itself is a trouble to Mr. O'Toole; but—remembering that your readers may not sympathise with his undisguised antipathy to verbal skylarking—he hastens to add that the two meanings are not only heterogeneous but incompatible. "Surely there is no occasion to stop to prove this." Please do, Mr. O'Toole; we should like to hear you prove something.

It may be noted that in this opinion and in paragraph 9 he appears to differ from Thomson and Tait. (See their definition of Potential, *Nat. Phil.*, vol. i., § 485).

C.—*Potential E. as meaning "Energy of Potency"*

It appears from a foot-note that "potency" may mean a force. If so, it is strange that the O'Toole—who, throwing off his thin disguise, at the end of his letter undertakes the "duty" of a doctor, and tells us that potential E. should be the "energy of a force"—it is strange that Dr. O'Toole should object to the name on this ground.

But the remarks under this head are chiefly interesting, as indicating the *modus operandi* of our pseudo-Publius. He does not trouble to examine the definitions of "potential energy." He only looks for explanations of the word "potential." Finding scant material in the doctor's utterances, he resorts to his dictionary, hunts up the different meanings of "potential," adds to these their antitheses, and sends his phantoms to pieces. It is scarcely a parody upon his letter to say—we won't trouble about what a civil engineer is, but let us examine the meaning of *civil*. Now *civil* has,——meanings: (A.) polite, (B.), &c. Therefore "*civil E.*" means "*polite E.*," and "*civil E.*" used as a *distinguishing* title cannot mean anything else than this, that the other E. is unpolite E.

*As to the whereabouts of Potential Energy.*

"We shall now pass from the perplexities connected with this unlucky name, 'potential E.,' to consider the behaviour of our teachers towards the thing itself." At last Mr. O'Toole will deign to discuss the definitions given by the doctors. Nay, he wanders away into an examination of such rash—but perhaps not inexcusable—phrases as "the potential E. of a raised weight," &c. The proper remedy for the troubles arising on this point is "to use words discreetly and consistently." But this is not sufficiently heroic. A local habitation must be found for this "potential E.," although it would seem as vain to inquire into the whereabouts of potential E. as into the whereabouts of Mr. O'Toole's scientific erudition. It is proposed to lodge this E. in the forces, and perhaps it won't do much harm, as we don't know where the forces are. It is proposed, moreover, to substitute "energy of tension" for "potential E." This done, the doctor's millennium will have come. Never mind about altering your conception of this kind of energy; call it by another name; give it a *weisnichtwo* lodging. There will be no more "confusion about fundamental principles;" there will be no more slips of the pen or tongue; there will be no more puzzled Publii; and last, but not least, there will be no more O'Tooles to bother the doctors. Well may "verbal skylarking" be despised. What is it beside such gigantic fun as this?

And yet I am sceptical. We started by hearing that it was "principally—though not entirely—the doctors who were to blame for this confusion about fundamental principles." Is this proved? Is not another cause indicated in the letter of of "E. G." (vol. xvii. p. 9)? And shall the doctors expect to be rightly understood when Dr. O'Toole's amanuensis admits (vol. xvi. p. 520) that Dr. O'Toole himself has been misapprehended upon almost every point by one reader at least?

Cirencester, November 13

H. W. LLOYD TANNER

Smell and Hearing in Moths

IN NATURE (vol. xvii. p. 72) your correspondent "E. H. K." observes: "'J. C.' seems to draw inferences that moths have not the power of smell, but have that of hearing. I feel quite certain they possess the former, but am in doubt about the latter."

"With reference to the sound of the glass, is it not the quick motion of the hand which disturbs the moth?"

May I draw the attention of both your correspondents to some experiments of mine on this subject which were published in NATURE about a year ago? These experiments, I remember, were quite sufficient to prove to me that moths have the power of hearing shrill notes; and, until I read the query of "E. H. K." above quoted, I thought that my account of these experiments must have been equally conclusive to any one who read them. On now referring to that account, however, I find that I there omitted to state one of the experiments which was resorted to for the purpose of avoiding the possible objection which "E. H. K." now advances. This experiment was a very simple one, consisting merely in making a sudden shrill whistle with my mouth by drawing the breath inwards, so as not to disturb the air in the neighbourhood of the insect. The latter, however, always responded to this as to other sounds in the way described, although throughout the experiment I took care not to move any part of my body.

GEORGE J. ROMANES

It was because of my knowledge of facts like those named by "E. H. K." that I was surprised at the apparent inability of moths to smell ammonia. Being no physiologist, I ventured to draw no inferences; but it occurred to me to wonder whether the sense of smell differs in kind with different organisations; whether, for instance, some substances strongly odorous to us may be quite inodorous to insects, and *vice versa*.

As to the experiment on hearing, I do not think it was the movement of the hand which startled the moths. It may conceivably have been the vibration of their wings set up by the sound; but the experiment can easily be repeated with variations by any one interested in the subject.

Loughton

J. C.

Meteorological Phenomenon

THIS morning at about a quarter before ten the sky here presented a most unusual appearance. The air was calm and the sun shining, but not brightly, through a slight veil of cirro-stratus. The sky was mostly covered with fibrous clouds of cirrus or cirro-stratus (I am not quite sure which I ought to call it), the fibres being quite parallel to each other, but in two different strata; those of one stratum were approximately from north-east to south-west, those of the other from north-west to south-east—so that they seemed to cross each other like the threads of a woven fabric. I think the fibres from north-east to south-west were the highest, but am not quite sure, though it seemed the same to another who was looking on with me.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, November 25

OUR ASTRONOMICAL COLUMN

STELLAR SYSTEMS.—M. Flammarion, in various notes communicated recently to the Paris Academy of Sciences, has been drawing attention to stars which appear to be affected with a common proper motion, or a motion similar in amount and in its direction. Several of his cases, however, are by no means to be styled "Nouveaux systèmes Stellaires." Thus the large and uniform proper motions of the southern stars  $\zeta^1$  and  $\zeta^2$  Reticuli, to which he refers in the *Comptes Rendus* of November 5, were the subject of remark in NATURE, vol. xi. p. 328. That there was a probability of a common proper motion in these stars would be evident to any one who inspected the columns in the British Association Catalogue, published in 1845, but as Taylor had not observed them, and the comparison was consequently dependent upon Lacaille and Brisbane only, there was a possibility of mistake. The first confirmation of the large proper motion of the B.A.C. in  $\zeta^1$  was afforded in Jacob's "mean places of 1440 stars"—from the Madras observations 1849-53; and