LETTERS TO THE EDITOR.

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Mr. H. O. Forbes's Discoveries in the Chatham Islands.

IN reply to Prof. Newton's letter, under the above title, in NATURE of last week (p. 101), in which he refers to the description by me of the Chatham Island Ralline bones under a distinct genus *Diaphorapteryx*, and observes "that one thing seems needed to make the discussion [on the probability of a land connection between the Chatham and Mascarene Islands] real, and that is the proof of the assertion that *Aphanapteryx* ever inhabited the Chatham Islands," I beg to say that in his letter there is a slight confusion of dates, which affects the question of the nomenclature. On July 29 last year I visited Cambridge for the purpose of comparing the bones from the Chatham Islands I had brought with me with the real *Aphanapteryx* remains in the Museum there. It turned out that Dr. Gadow, who was abroad, had laid them aside where Prof. Newton could not place his hand upon them, and I was, therefore, unable to see them. A week or two later. when in Edinburgh at the British Association Meeting, in a note intimating the return of Dr. Gadow, and kindly arranging for my examination of the bones, Prof. Newton adds, "I believe you will want a new generic name for what you have called *Aphanapteryx*," and suggests the name *Diaphorapteryx* instead. I was unavoidably long prevented from revisiting the Cambridge Museum, and so in describing as *Diaphorapteryx* the Chatham Island bones, at a meeting of the British Ornithologists' Club in December, 1892, I accepted the suggestion of Prof. Newton, who alone had till then seen the remains from both localities. On February 23, prior to reading my paper at the Royal Geographical Society, I again visited Cambridge, and in the most kind manner received every facility and assistance both from the Professor and from Dr. Gadow in comparing the specimens. On this occasion I was unable to recognise any sufficient characters, by which, in my estimation, to separate generically the bones from the Chatham Islands from those from Mauritius. This decision I stated at the meeting of the R.G.S. on March 13 last, and more recently in a communication to the Brit. Ornith. Club, which will appear in its forthcoming *Bulletin*. If I mistake not, however, Prof. Newton agreed with me that the Chatham Island form was nearer to *Aphanapteryx* than the latter was to *Erythromachus* of Rodriguez. Some of these remains from Mauritius have been figured by Prof. Milne-Edwards in his "Oiseaux Fossiles de France," and the remainder are fully discussed and illustrated by Dr. Gadow in a shortly-to-be-issued fasciculus of the Trans. Z.S. of London, while those from the Chatham Islands will appear shortly, I hope, in one or other of the scientific journals or Proceedings. After a careful study of all the material I have no hesitation, however, in stating meantime—as those who care will then have an opportunity of judging—that the bones from both regions are generically the same. I maintain also, that even if some osteologists should be disposed (from the somewhat larger size of the Chatham Island bones, though among them I found a number scarcely to be separated on even that ground) to make a generic distinction between them, the question would not only not fall, but I really cannot see that the argument based on their discovery in the New Zealand region would be in the least invalidated, as the forms are unquestionably so very nearly related. The importance of the distribution of the blue Waterhens, and the relationship between the Huias of New Zealand and the *Frigelupus* of Reunion—long ago pointed out by Mr. Wallace—and many other facts as far as birds are concerned recently urged by Dr. Sharpe at the Royal Institution, appears now to a fuller extent by the discovery of those unexpected forms in the Chatham Islands.

I must once more protest against the very erroneous statement that I have invoked this "tremendous hypothesis" to explain the distribution of the closely related forms of these two regions. I adduced, as I have said in my last letter, a great deal of other evidence in my paper at the Royal Geographical Society, which will appear very soon now. In addition to the facts there given I may point out the significance to this question of the results of the investigations of my lamented friend, Mr. W. A. Forbes—an anatomist of the highest acumen—on the genera *Xenicus* and *Acanthisitta* of New Zealand. He found that the affinities of the *Xenicidæ* are with the *Pipridæ* (including the *Cotingidæ*), *Tyrannidæ*, *Pittidæ*, and *Philepittidæ*—groups confined to the New Zealand, the Australian (ranging into the Oriental), the Mascarene, and the Neotropical regions, and that they have no relatives elsewhere. Nor are the following sentences from Mr. Wallace's "Geographical Distribution of Animals" without a bearing on this discussion:—"We have the pigeons and the parrots most wonderfully developed in the Australian region, which is pre-eminently insular, and both these groups have acquired conspicuous colours very unusual or altogether absent elsewhere. Similar colours [black and red] appear in the same two groups in the distant Mascarene islands. . . . Crests, too, are largely developed in both these groups in the Australian region only; and a crested parrot formerly lived in Mauritius—a coincidence too much like that of the colours as above noted, to be considered accidental." HENRY O. FORBES.

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The Fundamental Axioms of Dynamics.

AS Prof. Lodge refers in the letter published in this week's NATURE, p. 101, to my remarks on his paper on the Fundamental Axiom of Dynamics, I shall be obliged if you will allow me to state my views in your columns. Apart from all minor questions it appears to me that the main issue raised by Prof. Lodge is whether the law of the conservation of energy can be *proved* from the fundamental laws of dynamics and the assumption of contact action.

I have not the slightest objection (as he seems to suppose) to the mathematical investigation of physical facts being based on assumptions which are followed out to their logical conclusions, nor do I shrink from using such methods even when they fail in some points or lead to paradoxical conclusions. They may legitimately be accepted as convenient though imperfect mental pictures of the truth, sketches, but not finished drawings.

My objection to Prof. Lodge's "proof" is that in his attempt to avoid the unthinkable by discarding action at a distance, he adopts another equally inconceivable conception, viz. contact action.

He has already laid it down as an axiom that "material particles (atoms of matter) never come into contact." It is only by abstaining from the attempt to define the constitution of the ether that he avoids being driven to the conclusion that its various parts never come into contact either.

The assumption that he really makes is that when two bodies (including in that term both matter and the ether) act immediately upon each other, the distance between the mutually acting parts remains invariable during the action. This is not inconsistent with action at a distance. If then the phrase "contact action" be discarded the assumption of action at constant distance is a proper subject for investigation.

If the assumption be accepted the reasoning based on it is no doubt correct, but the value of the "proof" (regarded as independent or self-contained) depends entirely on the value we assign *à priori* to the fundamental assumption. I doubt whether an argument based upon it would by itself have convinced the world that the conservation of energy is a fact.

If, on the other hand, the assumption is regarded as a more or less arbitrary postulate to be justified, *à posteriori* by the fact that conclusions can be deduced from it which are otherwise known to be true, Prof. Lodge must not represent his course as the ascent of a firm ladder of argument to results which, though paradoxical, must be accepted under penalty of a *reductio ad absurdum*. On the contrary, it lies with him to justify his assumption by the use he makes of it. That the conservation of energy follows is no doubt an argument in its favour, and I for one shall look with interest for the other deductions which Prof. Lodge promises. ARTHUR W. RÜCKER.

June 2.

IF Mr. E. T. Dixon (NATURE, p. 103) will read what I have previously written on the subject of energy he will find most of his objections anticipated. I have pointed out, as he now does, that so long as potential energy is regarded solely as a "force function" the conservation of energy has no real physical meaning (pp. 532, 533, Phil. Mag., June 1881). I quite agree that potential energy belongs to a system rather than to a particle,