

deduction and Hamilton's Principle of Least Action. But the limitation is required by the postulate, viz. the Principle of Least Action, not by the deduction. Clausius expressly states (*Phil. Mag.* vol. xlv. (1872) p. 365) that his deduction holds good for systems to which the Principle of Least Action is not directly applicable, and in consequence he claims that his equations involve a new principle which is of more general application than Hamilton's Principle (*vide* Report, § 16). I think there can be little doubt that Clausius had in his mind the very objection which Mr. Burbury now raises, and that it was in order to meet it that he claimed this generalisation.

The assumption as to the conservative nature of the forces is not required except in § 17 of the Report, and at the end of that paragraph two methods of avoiding it are suggested. One is to assume that the force-function can be varied with the time, the other is a method adopted by Von Helmholtz.

If we allow the force-function to be varied with the time, then in Mr. Burbury's case (of a column of gas held down by a piston of constant mass) the potential of gravity can be altered and therefore the weight of the piston is disposable. This disposes of Mr. Burbury's objection, and it only remains to consider the investigations given at the beginning of § 17 of the Report.

Clearly the Second Law of Thermodynamics cannot be deduced from studying the behaviour of gas under constant pressure. To establish it we must make the working substance undergo a reversible cycle in which heat is absorbed and external work performed. To do this we attach the piston to a crank as in an ordinary steam engine, and make it turn a wheel which raises a weight by means of a windlass. Here we have a strictly conservative system, and one to which the arguments of § 17 are therefore strictly applicable. And for every single turn of the wheel we have

$$\int \frac{dQ}{T} = 0 \dots \dots \dots (I)$$

a relation identical in form with that which expresses the Second Law.

It seems to me that the real objections to Clausius' deductions are far more intricate and far less easily disposed of. The difficulty of assigning a physical meaning to the quasi-period \dot{i} is one of them, and there are other difficulties connected with the interpretation of \bar{T} as absolute temperature when intermolecular forces are taken into account. All these difficulties are alluded to in my Report, and they are not peculiar to the hypothesis of "quasi-periodic" motions; similar difficulties exist in some form or other in most so-called "proofs of the Second Law."

It may be interesting to mention that a proof of the Second Law based on the virial equation

$$pv = \frac{2}{3} (T + \sum \sum (\frac{1}{2}Rr))$$

was given by R. C. Nichols in the *Philosophical Magazine* for 1876 (v. Series I. p. 369). I hope later on to deal more fully with that portion of Mr. Burbury's letter which relates to the "virial proof." G. H. BRYAN.

December 21.

Flame.

I HAD hoped that after disavowing the unpleasant interpretation which had been put upon his first letter, Dr. Armstrong would have done me the honour, and himself the justice, of indicating precisely where he disagreed with my scientific arguments. Instead of doing so, he has imputed to me a sensitiveness to criticism so excessive that he feels it best to retire from the controversy with a mere statement that our standpoints are different. I must leave it to the readers of NATURE to judge whether Dr. Armstrong has any longer the right to claim a standpoint.

Mr. Newth will find a fuller discussion of my views about flame in the *Journal of the Chemical Society* for 1892, pp. 204-226. If after reading that he still has difficulty in understanding the fundamental points of my work, I shall be glad to help him if he will communicate with me privately. With a little care, Mr. Newth will find it quite easy to separate the two cones of a carbon monoxide-air flame in the ordinary apparatus without the use of a gauze cap. The air must be turned on very gradually. In the case of the hydrogen-air flame it is best to dilute the gases with nitrogen, as recommended in my first paper.

I am sorry that anyone should think I have slighted Dr. Frankland's work. I can, however, understand, and even admire, Mr. Newth's excessive zeal in the matter.

ARTHUR SMITHELLS.

"The Zoological Record."

WE are delighted to find such a consensus of opinion as to the desirability of retaining palæozoology in the *Zoological Record*. The recorders whom we have consulted, the editor, and now the secretary of the Zoological Society, all have expressed themselves in its favour. The question therefore is purely one of finance. Under these circumstances the publication of the correspondence with the Geological Society is of great interest, and the only addition that we could suggest would be the publication in your columns of that poverty-stricken society's balance-sheet.

We should like, however, to point out that the *Zoological Record* appeals to the Geological Society, not merely on the ground of its palæontological contents. Palæontologists go to the *Record* to learn what the neontologists are doing, quite as much as to read the titles of their own papers. Under any circumstances, then, the *Zoological Record* has some claim on the Geological Society, and we must all regret that financial distress prevents the Society from acknowledging that claim.

But the Record Committee of the Zoological Society need not despair; for the *Record* has no less claims on many other of our learned societies, and, by the converse argument, the inclusion of palæozoology merely strengthens those claims. Every biologist should be grateful to those who bring to his notice literature that he would otherwise never hear of. Apart from this, one-third of the volume is devoted to entomology. Why should the Entomological Society not be invited to contribute? Then there are the Royal and the Linnean Societies, and the British Association; the Microscopical, we would mention, did it not already do excellent work of a similar kind. At any rate, surely five of these bodies could be prevailed on to subscribe £20, or even £40 a-piece. The Zoological Society appeals through its *Record* to hundreds of workers who do not belong to it. It has long done an admirable work, of which it will never lose the credit. Everyone should sympathise with it in its present attempt to perfect this work, and should not permit it to suffer so large a pecuniary loss in that attempt.

December 17. R. I. POCKOCK.
F. A. BATHER.

On the Bugonia-Superstition of the Ancients.

LAST August, I published in the *Bulletin Soc. Entomol. Italiana*, 1893, p. 186-217, an article entitled "On the Bugonia of the Ancients, and its relation to *Eristalis tenax*, a two-winged insect." I desire to collect some more materials on that subject, in view of a second edition, and I would be very grateful to readers of NATURE who may be able to give me assistance in that matter.

The information I require may be expressed in two questions: (1) Whether travellers in out-of-the-way places in Europe or Asia have not come across vestiges of the superstition about oxen-born bees, still lingering among primitive people?

(2) Whether readers of Oriental literature have not come across passages evidently referring to this superstition, like the passage I reproduce here as an example. I found it in the "Golden Meadows" of the Arab traveller Massoudi (died in Cairo, 955), translated by Barbier de Meynard and Pavet de Courteille, Paris, 1861, vol. iii. p. 233. It relates a conversation which took place in Arabia, and of which this is a fragment. "Had the bees, which produced this honey, deposited it in the body of a large animal," asked Yiad? The surveyor answered: "Hearing that there was a hive near the sea-coast, I sent people to gather the honey. They told me that they found at that place a heap of bones, more or less rotten, in the cavity of which bees had deposited the honey that they brought with them."

I have sent separate copies of my paper to the Geographical, Linnean, and Entomological Societies in London, to the Natural History Museum, South Kensington, to the Athenæum Club, and to many friends in England. I should be happy to send a copy to anybody interested in the subject.

C. R. OSTEN SACKEN.

The Earliest Mention of the Kangaroo in Literature.

I TAKE advantage of the present opportunity to put another question to zoologists. In the same book of Massoudi, whom I quoted in the previous notice, I found the following passage (vol. i. p. 387):—"El Djahiz, in his 'Book on Animals,' relates that the female rhinoceros is pregnant for seven years,