

### Is Darwinism Dead?

IN NATURE of Feb. 19, Sir Arthur Keith brought against me two specific accusations: (1) That I denied in my book "A Companion" (p. 12) the possibility of birds descending from reptiles; (2) that I had given a false reference to Vialleton's high authority. There was a clear issue: (1) Had I said this? (2) Had I misrepresented my authority?

On Mar. 8 I wrote you a brief letter showing that Sir Arthur Keith was ignorant of the great work of Vialleton to which I had alluded, and giving four detailed references. This letter you did not print. In its place you issued, on Mar. 19, a 'comment,' in which you substituted other issues, and repeated as your own the first of Sir Arthur's blunders.

I am therefore compelled to send you this further letter in order that readers of NATURE may be acquainted both with the real issue and its upshot.

(1) I made no affirmation upon the descent of birds. What I did say was that a very great authority (Vialleton) had given strong arguments against the reptilian origin of birds with the natural effect of such an authority so reasoning.

(2) So far from giving a false reference, I had worked upon Vialleton's latest and famous work, and in my letter I gave four page references (585, 588, 590, 592) to that work: of which apparently Sir Arthur had no knowledge, or he could not have blundered as he did.

Your comment leaves your readers under a directly wrong impression upon both points. You re-affirm the error of the first; you make no mention of my specific references in the letter, but only say vaguely that I have caught my critic "referring to the wrong book."

My accuracy, and Sir Arthur Keith's lack of that quality on this issue, can be verified as plain matters of fact by any one who will consult the texts in question.

H. BELLOC.

Reform Club,  
Pall Mall, S.W.1, Mar. 28.

MR. BELLOC is under a triple misapprehension. He believes he gave 'references' in his "Companion"; he did not. He left his readers to guess which Vialleton he had in mind; I guessed the right one, and quoted a pertinent passage from Prof. L. Vialleton's best-known work. If Mr. Belloc had been well advised he would have accepted that quotation without comment, for it is less discordant with modern knowledge than the passages of the later compilation to which Mr. Belloc has directed my attention.

In the second place, Mr. Belloc is under a misapprehension as regards Prof. L. Vialleton's range of original work; that writer has never claimed to be an 'authority' on the evolutionary history of birds, nor is he so regarded by zoologists or palæontologists of any country. I am sure Prof. Vialleton will smile when he learns of the claims which Mr. Belloc now makes for him.

Mr. Belloc's third misapprehension relates to the present state of our knowledge regarding the evolution of birds. The evidence drawn from embryology, geology, and anatomy leaves the expert student in no doubt as to their origin; they arose from a reptilian ancestry.

ARTHUR KEITH.

Royal College of Surgeons, W.C.2.

[No useful scientific purpose would be served by further correspondence upon the points at issue.—  
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### The Atomic Weight of Silver.

THE following few lines give a necessary elucidation to my critical note and to the answer given by Messrs. H. B. Baker and H. L. Riley (NATURE, Mar. 5, p. 348). My principal first theoretical argument is based on the *interdependence* of the atomic weights of the elements silver, nitrogen, and chlorine, resulting from the classical life-work of Richards and his school, in which I have the greatest confidence. If the atomic weight of silver = 107.876, then nitrogen =  $14.006 \pm 0.0011$ , and chlorine =  $35.456 \pm 0.002$ , most probably 35.458. If we accept Baker and Riley's value, silver = 107.864, then nitrogen would become 13.999, a value exceedingly improbable, especially having regard to the fact that Baxter found recently (*Proc. Amer. Acad.*, 12, 12, p. 699, Dec. 1926) by an extremely careful physico-chemical research the value  $N = 14.006(7)$ , which confirms the higher atomic weight of silver, namely, 107.876. This important argument was not referred to by Messrs. Baker and Riley.

My second, no less important, practical argument was based on the assumption that Messrs. Baker and Riley have lost exceedingly small quantities of silver vapour on fusing the metal in hydrogen, so that the atomic weight found by them is slightly lower than the true one. They did their best to convince themselves that no *visible* condensation of metallic silver could be observed in their tubes, and they say that they have begun a new series of experiments to investigate the volatility and condensability of silver. I beg to remark that some experiments on a large scale in this direction were published by J. S. Stas so long ago as 1865 ("Œuvres complètes," T. I, p. 457), who was my first "atomic weight teacher" in 1875 (but who would read such 'antiquated' papers to-day?). He describes the distillation of 50 gm. of his purest silver in the flame of the oxy-hydrogen blowpipe and says: "Je dois avouer toute fois que, dans les opérations que je viens de décrire, la moitié *au moins* de l'argent employé a été perdue. En effet, il a été entraîné à l'état de vapeur bleue pâle avec le courant de gaz tonnant, quoiqu'il fût cependant modéré, et sans excès trop grand d'oxygène; il a été répandu dans l'air ambiant dont il a troublé la transparence, et auquel il a communiqué une saveur métallique très sensible."

From this important observation it follows that when silver once passes into the state of vapour it is not easily condensed in a solid state, but forms only a colloidal dispersion as a fog. Large quantities of silver heated in tubes give a condensation of the metal, but when a small quantity was heated and fused, the silver vapour—the weight of which was, in the said experiments, of the order of 0.0001 gm. and which would occupy in the solid state 0.00001 cm.<sup>3</sup>—may have passed out of the apparatus.

Messrs. Baker and Riley say that they controlled the weight of the fused silver obtained by repeatedly melting and weighing it to constant weight. But the question arises: What was the weight of the silver obtained in a fine state of division immediately after decomposition of its oxide by heat and before fusion? Such silver has a very great surface, and during fusion a small loss by evaporation may have taken place. After fusion, its surface has become very small and, last but not least, it was "coated with a very thin film of dross consisting of silica." To these circumstances the fact is very probably due that no appreciable loss of weight of the silver was observed after repeated fusion.

BOHUSLAV BRAUNER.

Bohemian University, Prague, Mar. 10.