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**Dawes on the Pronunciation of Greek Aspirates *The Pronunciation of the Greek Aspirates*, by Elizabeth A. S. Dawes, M.A., D.LIT. (Lond.). London: D. Nutt. 1895. 2 s. net.**

R. Seymour Conway

The Classical Review / Volume 10 / Issue 01 / February 1896, pp 59 - 60  
DOI: 10.1017/S0009840X00203156, Published online: 27 October 2009

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**How to cite this article:**

R. Seymour Conway (1896). The Classical Review, 10, pp 59-60 doi:10.1017/S0009840X00203156

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## DAWES ON THE PRONUNCIATION OF GREEK ASPIRATES.

*The Pronunciation of the Greek Aspirates*, by  
ELIZABETH A. S. DAWES, M.A., D.LIT.  
(Lond.). London: D. Nutt. 1895. 2s. net.

Blass's well-known treatise on Greek Pronunciation, which has served the present generation of students, exhibits here and there a want of precision that renders a re-examination of some of the more controverted points extremely desirable. Among these must still be reckoned the exact sound of  $\theta$ ,  $\phi$  and  $\chi$ , and Dr. Dawes' dissertation is an attempt to unsettle the orthodox theory of their purely aspiratic value. The evidence to be weighed is complicated and scattered in time and place, and if ever there was a subject in which strict accuracy of method, in chronology, in phonetics, and in the ordinary principles of logical inference is indispensable, this is one. It must be confessed that in these respects Dr. Dawes does not appear to be adequately equipped for her task, in spite of the zealous labour that she has devoted to it. The 'conclusion' runs as follows (p. 102):

'We consider the question one that does not admit of any definite solution because even the safest, viz. the internal evidence of the language itself, is both of an uncertain and a conflicting nature. This being so, we can, after carefully sifting the same, do nothing beyond forming a more or less certain hypothesis from estimating the value of the arguments on either side and trying duly to appreciate them. From such an estimate we obtain the following results.

'In support of the aspiratic theory we have the two analogical (*sic*) phonetic laws in Sanskrit and Greek, by which two consecutive syllables cannot begin with an aspirate. Add to this the *a priori* evidence found in the process of elision and we have the main arguments for the aspiratic theory.

'On the other hand, in support of the spirantic theory, we have the difference of phonetic law in Sanskrit and Greek by which in the latter language we find combinations of aspirates. As regards internal evidence, with the exception of that furnished by elision, it would seem to favour this theory. That it does so, we have attempted to show in our investigation of the evolution of the phonetic laws and the history of interchange (*sic*) which, in our opinion, seems to point to a continuity of pronunciation.

'As to the testimony of the grammarians, we think we have shown by our exposition, that, if considered impartially and in its entirety, it cannot be looked upon as reliable evidence for either theory.

'These are the broad conclusions at which we arrive, and we do not think they are such as to justify a final decision in favour of the two opposed theories (*sic*) which we have attempted to elucidate.'

I have quoted these paragraphs exactly as they stand (except for the italics), because they may be taken, I think, to represent

very fairly the whole essay. The words italicised in the first and last paragraph, if I understand them rightly, involve a practical contradiction, since no 'more or less certain hypothesis' is 'formed' or even suggested. Similar contradictions may be found elsewhere (e.g. between the last two paragraphs of p. 51, between the foot of p. 98 and the top of p. 99, etc.) along with a number of inferences on which the only possible comment is *non sequitur*, e.g. the first paragraph of p. 23, where we read that 'this Skt. and Lat. *h* [corresponding to Gr.  $\chi$ ] generally represents, it is true, an original *gh* whose "g" reappears in Gothic, etc., but the fact remains that in Skt. we have a guttural spirant *h*, and in Latin a spirant or breathing (*sic*) "h," and, as there is some possibility of  $\chi$  having been a spirant, these cases make such a supposition probable.' The inference on p. 21 as to the origin of the 'modern tenuis' in certain cases begs the question completely.

As the essay stands, it is difficult, or rather impossible, to discuss it as a whole, simply because while professing to deal with a strictly scientific subject, it shows no grasp whatever of any scientific method. There are pages and chapters about 'phonetic law,' but it is difficult to discover what is meant by the term, except that it has not the meaning of a definite uniform change of a given sound under definite conditions completed within definite limits of time and place, which—it is grievous to have to repeat—is the only sense in which it can be legitimately used. Any one who can still believe (as Dr. Dawes does) in Corssenian 'tendencies' is free to do so, but to call them 'laws' is to plunge into inextricable confusion. In some places (e.g. in the last line of p. 74) Dr. Dawes speaks of a 'law' in this sense; in others (e.g. p. 24) it seems to mean a rule describing any sounds in existence at a given period of a language; in the first three pages, and indeed the whole, of the chapter headed 'Phonetic Laws' I cannot discover any one definite meaning to attach to it. This unhappy union of antiquated principles with confused terminology vitiates every single argument that concerns a phonetic change, and explains, at least in part, why Dr. Dawes finds it impossible to arrive at a conclusion on the whole question, in spite of her vigorous interest in the subject, and much careful reading. I hardly think that

any one who has really grasped the argument<sup>1</sup> from the detachableness of the aspiration in vulgar Attic inscriptions of the fifth and fourth centuries B.C. (χιβών and κιβών for χιτών, εὐορχοῦντι for εὐ-ὄρκοῦντι κ.τ.λ.) can have any doubt that θ, φ and χ each contained an explosive and a genuine aspiration in Attic at that date. The evidence of transcription into and from other languages, to which Dr. Dawes hardly alludes, is equally decisive, and in the same direction; see for instance the well-known passages Cic. *Orator* § 160, and Quintilian 1, 4, 14, and there is a mass of evidence of the same kind in the transcriptions of Greek words into early Latin and the other Italic dialects.

We learn, however, from the preface that a complete collection of the evidence was not contemplated, and it is to be regretted that the essay was not restricted to what is clearly its chief purpose, an attack on the weak points of the case put forward by the orthodox school. The two chapters on the evidence of ancient Greek writers, so far as they are confined to pointing out the defects of Blass's account, are interesting, straightforward, and on the whole must be called successful, and here and there in the other chapters certain real and well-known difficulties in the present statement of the aspiratic theory (e.g. φιλόσοφον in Aristo-

phanes) are brought into notice. Had these points been assembled, and cogently stated in an article of a dozen pages, Dr. Dawes would have made a substantial though a negative contribution to our knowledge of the question.

Since the essay was published fresh evidence of a most conclusive character from the transcription of a very large number of Demotic words into Greek characters in the two gnostic papyri of London and Leyden respectively has been lucidly set forth by Hess, in the current number of *Indog. Forschungen* (vi. p. 123). The papyri are ascribed on palaeographical grounds to the second century A.D. φ is the invariable transcription of Demotic *p + h*, while Demotic *f* is represented by a special sign taken over from the Egyptian alphabet; χ is the invariable transcription of Demotic *k + h* and *g + h* (Demotic *g* is voiceless), never of the Demotic spirant *h*, which is represented by another borrowed Egyptian sign; while θ always transcribes *t + h*, except before *i* and *ε*, when it also represents *ts*, showing that in this position θ had become a spirant at this date. I may add that Hess shows by similar evidence that γ was then in all positions an explosive, and δ an explosive except before *i*, where it had become a spirant.

R. SEYMOUR CONWAY.

Cardiff, January 1896.

<sup>1</sup> This has been unfortunately misstated by Meisterhans, *Gramm. Att. Inschr.* ed. 2, p. 78, but is put quite clearly by Brugmann, *Gr. Gram.* ed. 2, p. 73.

## LORD ON THE PRONUNCIATION OF LATIN.

FRANCES E. LORD.—*The Roman Pronunciation of Latin; Why we use it and How to use it.* Ginn and Company. 1894.

It is not without considerable reluctance that I have acceded to the request to write a brief notice of this work. It is not a pleasing task to review a book to which so little praise can be given. But when one has commended the author's motive, the hope of giving help to teachers of Latin in secondary schools who desire to know the 'Why and the How' of the 'Roman Pronunciation,' the possibilities of favourable criticism are, I fear, exhausted. The author seems to have only a limited acquaintance with the modern literature of the subject. The introduction states, indeed, that free use has been made of the highest English authorities, of Oxford and Cam-

bridge. But the books of Ellis and Munro are not the most recent expressions of the best English opinion of to-day; and of German authorities, of such a work as Seelmann's *Ausprache des Latein*, we are forced to assume that the author has no knowledge. Else how, for example, could she so confidently pronounce the Latin accent one of pitch, as if no one had ever thought of its being anything else, whereas there is almost complete unanimity among scholars (we must except Havet, followed by Victor Henry) that the predominating characteristic of the Latin accent was stress? But more unfortunate still is the author's lack of critical judgment in weighing evidence, her ignorance of the history of the sounds in Latin and the other languages compared and, most of all, a fatal confusion of *sounds* and