

Giving a Dog a Bad Name

David Aune's Review of my Book

Professor C.H. Dodd once said: “If you don’t like a dog, give it a bad name and then kill it”. This advice of Dodd’s has been followed time and again, when scholars find it difficult to silence their opponents by means of sound, evidence-based arguments. The resort to attaching a “bad name” to the scholar they wish to get rid of appears to be the best way out of the embarrassment which the lack of evidence-based arguments causes them.

Evidently, this advice of Dodd’s has been followed also by Dr David Aune. Aune wrote a review of my book *New Testament Language and Exegesis: A Diachronic Approach* (WUNT 323, Tübingen: Mohr 2014) in the *Theologische Literatur Zeitung*, 140 (2015), cols. 929–931.

Aune commences his review by presenting me as saying: “a diachronic approach to Greek is *the only valid* linguistic approach to New Testament Greek” (Col. 929) [my emphasis]. This is patently incorrect, otherwise I would have adopted the position that until now no one has ever given a valid interpretation of the New Testament. What I say, and have always said, is that *for a more exact understanding* of the New Testament we need to apply the diachronic approach. And I have proved this in my various publications, by showing how a myopic treatment of the language of the New Testament has resulted time and again in distortions of its meaning.

A little further down, Aune writes “C.[aragounis] unwisely rejects the relevance of the ‘illiterate *Greek + papyri’ [*sic*] from Egypt (112; cf. 68, n. 118), which he never cites” (col. 929).

First, Aune’s criticism fails to take into account the rich and nuanced discussion on p. 112 as well as note 118 of p. 68 of my *New Testament Language and Exegesis*. At the latter place, I write, *inter alia*, “Howard’s contention (*Accidence*, p. 313) that the word ἐπιούσιος in the papyri has only the meaning of “property or estate” only shows the erratic approach of these authors to the Hellenic language by means of the exaggerated importance which they attached to the papyri, as though they were determinative of the meaning of the New Testament vocabulary, in other words, their failure to perceive the importance of the diachronic approach to the Hellenic language as the only correct approach to solve linguistic problems that relate to words with no pre-NT history as well as such words as underwent a development of meaning during their history”, etc. NB! here I speak of “the exaggerated importance” attached to the Egyptian papyri – not of their total irrelevance, as Aune represents me as advocating – and I speak of the diachronic approach as the only way to determine “the meaning of words with *no pre-NT history* as well as such words *as underwent a development of meaning* during their history”. Can anyone find fault with these claims? And have they not been vindicated in my publications?

Second, with regard to the use of the papyri, Aune says that “he [sc. Caragounis] never cites” (col. 929). This is not strictly correct, for I do cite a few papyri in this book. In the context, this can only mean that without investigating the papyri, I just reject them dogmatically, because they do not fit into my thesis. Now Aune has given indications that he is aware of my volume *The Development of Greek and the New Testament* etc., inasmuch as this volume, too, comes under fire later in his review (see below). I wonder, has he forgotten that in that volume I cite a great many papyri? In fact, I have indicated that in my various searches for linguistic evidence, I have read

thousands of papyri. I would, thus, think that it would be no exaggeration to state that few NT scholars have searched the papyri more than I.

Aune's de Saussure-inspired idea that "a language as spoken is the only reality known to a community of speakers, while diachronic linguistics focusses not on an existing language, but on its modifications over time" (col. 929–930) is not only simplistic but would also raise a whole series of objections, for instance, reading the literature of previous generations. That de Saussure's dictum does not apply to the New Testament ought to be obvious to every intelligent reader, since, for example, Aune himself does not belong to "the community of speakers" among whom the New Testament came into being! He, too, has to study it from a later point of time rather than "as an existing language". And in this respect he researches the NT in the same way as I do, except that he limits himself to the meagre evidence of a century or two around the NT times (the so-called synchronic or we should rather say, myopic method), whereas I take into account the entire history of the language (the diachronic or holistic approach), pinpointing the NT construction to the stage of development of the language. And this procedure, as Aune himself admits (cf. his comments about Part Two, below), has been crowned with success.

Further, he thinks, for example, that because Hellenic has a long history, we should speak of it as "a series of historically connected languages" (col. 930). In other words, since language changes in every generation, we should think of Hellenic as a series of some eighty-four Hellenic languages from Homeros to the present day! Needless to say, no one thinks of Hellenic in this aberrant way. Besides, this point of view plays havoc with the whole situation in Hellas, where the Hellenic

of earlier periods has been present to the Hellenes of later periods.

It is worth quoting, at this point, what a British scholar, Robert Browning, (London University) has to say on this matter in his book *Medieval and Modern Greek* (Cambridge: CUP 1969): “Since the time of Homer Greek has enjoyed a continuous tradition down to the present day ... Change there has certainly been. But there has been no break like that between Latin and the Romance languages. Ancient Greek is not a foreign language to the Greek of today ... Earlier stages of the language are thus accessible to speakers of later stages ... a long and continuous literary tradition which makes all elements of Greek from antiquity to the present day in a sense accessible and ‘present’ to any literate Greek” (vii, 3, 13).

The quotations from Browning show that non-Hellenic scholars who have taken the trouble to make themselves acquainted with ancient-, medieval-, and Neo-Hellenic, have a very different view of the true situation of the Hellenic language than Aune. And no one could reasonably accuse Browning of a “nationalistic ideology”! (see below). We might even say that scholars who lack knowledge of Hellenic in all its periods cannot really advise against the opinion of those who have that knowledge. Responsible scientific criticism does not proceed from mere whim but from sound and thorough knowledge of the object of criticism.

And now comes the “bad name” of the dog that we dislike. “The unity of the Greek language”, writes Aune, “was part of nationalistic ideology in modern Greece” (col. 930). By this statement Aune tries to throw my book to the ravens, because it is supposed to be inspired by “nationalistic ideology”. Such a remark is quite unworthy and out of place in a scholarly review of a book, whose every statement and claim has been backed by concrete evidence from all periods of the Hellenic

language. Any unbiased reader, on reading my book, will see that my various statements are based on cited evidence, and have nothing to do with nationalistic concerns. Or is it, perhaps, the case, that we do not like the evidence, and hence we have to give the dog a “bad name” in order to kill it?

The second part of my book, containing three chapters, comprises c. 25% of the whole book, but Aune devotes to it a mere seven lines! The reason for this ought to be obvious. I quote his whole text about these three chapters:

In three chapters of Part Two, C. applies the diachronic method to great effect in relation to New Testament exegesis: the nominative used as vocative (focussing on *theos*, *thee*), interrogative, confirmatory and asseverative particles and a brilliant discussion of the text-critical problem of *nēpios/nēpioi* in 1 Thess 2:7. These discussions are uniformly excellent and fortunately unburdened with the subjective value judgments that permeate Part One (col. 930).

The opinion expressed in the above quote stands in sharp contrast to the opinions and evaluations expressed in the rest of the review. Now, how can these three chapters be “uniformly excellent”, when they apply the diachronic method, which according to Aune, works “not [with] an existing language ...” or the “language as spoken” which “is the only reality known to a community of speakers” ... (i.e. the synchronic linguistics method), “but its modifications over time” (929) (i.e. the diachronic method)?

Moreover, Aune speaks of “a brilliant discussion of the text-critical problem of *nēpios/nēpioi* in 1 Thess 2:7”. It must certainly have escaped him that this chapter is not *merely* about the text-critical problem of 1 Thess 2:7, but *primarily* about the diachronic evidence relating to the meaning and use of the various terms of this text throughout the Hellenic lan-

guage, which alone can decide the issue! It is this diachronic approach that shows up the weaknesses of the arguments of previous treatments of the passage, which were based on the synchronic approach, and it is this diachronic approach that points to the meaning that Paul intended – which result Aune applauds.

His final words in this quotation “fortunately unburdened with the subjective value judgments that permeate Part One” bring up once again the theme of the dog that is being disliked.

Commenting on my last chapter on “Sublimity and the New Testament”, Aune, who has nothing whatever to say of this longest of all chapters, picks up a minute point, namely that I never mention “prose rhythm” (col. 930), even though he concedes that I do identify a number of “powerful passages” (in other words, verselike, poetic or rythmical passages). However, his charge that I never mention rhythm is patently incorrect. I do take up ‘rhythm’ a number of times (devoting many pages to such compositional effects), as for example on p. 287, where with reference to the beautiful composition of 1 Cor 15:42–43, I write: “There is no doubt here that Paul has paid attention to composition, achieving an almost perfect isokolon, with beautiful symmetry and rhythm” (*New Testament Language and Exegesis*, 287) and p. 292 I write: “This suggests that though we do not consciously look for metrical compositions in the New Testament, not infrequently, we will find that some things are said more metrically than others”, etc. etc.

But when Aune tries to instruct us that according to Dionysios Halikarnasseus, “elevated prose should be rhythmical” (col. 931), he is actually carrying coals to Newcastle! As a matter of fact, on p. 291 of the book he is reviewing, I write: “Yet according to Dionysios Hal., all words consisting of

more than one syllable – and this goes for prose works as well – have some sort of rhythm”. Furthermore, Aune seems to have forgotten that I have written a whole chapter (in *The Development of Greek* pp. 397–474), taking up Dionysios Halikarnasseus’ treatise on beautiful, rhythmical, etc. composition, of which prose rhythm forms a part!

It is, however, curious that whenever I have treated a point in my first book (*The Development of Greek and the New Testament*), which Aune wishes to present as absent from my second book (the book under review), he omits all reference to first book (although I have, actually, emphasized that the second book presupposes the first book), but when he wants to criticize a point in the first book, which he is not actually reviewing, he throws himself on it without hesitation.

Thus, he takes up the question of pronunciation, a matter that is not treated in the book under review (since it was treated at length in *The Development of Greek*) and devotes to it sixteen lines, i.e. about two and a half times as much as he devoted to the three chapters together of Part Two of the present book!

But alas! He has no arguments at all against the Historical Greek Pronunciation. All he can appeal to is the “scholarly consensus”. Now, may I ask: “Which scholarly consensus is he thinking of?” “How many of the scholars of this ‘consensus’ have actually worked with the question of Pronunciation?” I know of not one single NT scholar in the guild (during my lifetime) who has worked with the pronunciation of the Hellenic language. They were all told in their student days – including Aune – that this is how they should pronounce the Hellenic language. And they followed the advice of their teacher. What kind of consensus is this? We can speak of consensus only when we have a large number of scholars making

their research independently of one another and arriving at the same conclusion.

Furthermore, Aune says nothing of the immense evidence I brought together from the first beginnings that there was such an evidence (VIII B.C.) all the way down to A.D. times: inscriptions, papyri, and other documentary evidence. It is this evidence that tells us that since classical times we have to do with the Historical Greek Pronunciation, and not with the miserable, counterfeit, un-Hellenic pronunciation that a Dutchman propounded by mistake.

Finally, Aune thinks that one can never speak of the correct and the wrong use of language and chides me and Hatzidakis for distinguishing properly expressed things from those things which are not expressed properly, as if there are no such things as grammar and syntax. He thinks that everything goes. He thus implies that the language of the New Testament or of the Egyptian papyri, if you like, is, from the literary point of view, just as good as classical Hellenic. It is only different. Even in English, people are aware of good English and of bad English!

And here, finally, comes the killing of the dog we dislike. He concludes his review with:

The potential value of this monograph is somewhat compromised by an ideologically-driven approach to the Greek language, apparently anchored in a nostalgia rooted in past struggles for Greek linguistic and nationalistic independence.

This remark is so despicable, that it would soil my pen to answer it. My challenge is: read this book and you will find that its concerns and evidence are above reproach. It will become evident to you that this review, with its misrepresentations and incorrect allegations about my statements, is not an

impartial, scientific examination of it but was written with the sole purpose of killing the dog we dislike: the Historical Greek Pronunciation along with the diachronic or holistic approach to the Hellenic language.

One pertinent question pops up, however: Is this review perhaps Aisopos' myth about the fox and the grapes all over again?

PS. I am perplexed about one thing. I have no objections to Aune's disagreeing with my positions or disliking the Historical Greek Pronunciation and my diachronic–holistic approach. It is his right and his personal choice. But I am at a loss to make sense of the prompting I received sometime ago to add his name to the list of scholars who use the Historical Greek Pronunciation, in as much as he would appreciate it, since he was actually using it in his classes!