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# Buist Fanning Misrepresents *The Development of Greek and the New Testament*

Lately there has been a discussion about the *raison d'être* of the genre of review. As a matter of fact a number of scholars have come around to questioning the legitimacy of the review, because they have seen much misuse of this instrument. The genre of review has traditionally had a given place in scientific journals, helping the busy scholar, who is increasingly in lack of time to keep abreast of all that is published even within his own field of research, to get an idea of what is afoot: new areas of research, new questions, new attempts at answering old and new questions, and occasionally an interesting product of research.

The authors' profile has been enhanced by "positive" reviews, opening up greater opportunities for scholarly work, advancement in position, and influence. And not least, the publishers have profited by the increased sales. We may then agree that the genre of review—when 'positive'—has done much for author and publisher.

Now leaving aside author and publisher and their respective gains in fame or in lucre, the question is, how much has the review done for the advancement of Truth, of scientific Truth? Ideally, a scientific book (or an article) ought to be written not for the author to get a better position—as is usually the case today—nor for the publisher to pocket more, but in order to serve the interests of science and progress in the subject. But how many authors today write for science and how many publishers publish impelled by love for Truth. Indeed, the exigencies that are there and the hard competition make almost all decisions in the art of publishing have as the central point of reference mammon's tangible results. Naturally, the publishers must make things go round in order to continue to 'serve science' with the sweat of their authorial *andrapoda*. However, nothing of all this answers the question posed above: how is Truth served by reviews? What should an ideal review look like? If it is positive, it helps author and publisher; if negative, it ruins them both. But which of them serves science? The first, the second, both, or none?

This question is more complex than that. The demand for scientific Truth is not an onus that is to be laid only on the Author; it is equally an onus to be placed on the Reviewer. For the Reviewer at least does not have the same press on himself to achieve recognition and advancement by his review as the Author has by his monograph. The Reviewer is the Judge. And as a Judge, the Reviewer must be unimpeachable. It is his duty to administer justice. The readers demand it. This, of course, implies that the Judge is well versed in the rules of the game as well as in the specialties of the case, and his overriding interest is scientific, objective Truth and nothing else. He must judge the Author with fairness and integrity. Whether the result will be a positive review or a negative review is beside the point. A good review is a review that corresponds with the merits or demerits of the Author! The ordinary judge, too, sometimes will acquit and sometimes will condemn. If the accused is guilty, it is not the judge's fault that he is condemned. But if the accused is guiltless, and is condemned, then we have to do with a corrupt, unjust judge. A fair, critical, and just review, therefore, is neither a positive nor a negative review. It is a review that corresponds with what the Author and his book deserve, whether it is praise or blame. I wonder how many reviewers fulfill this criterion! I have read adulating reviews, where the reviewer was in a dependent relation to the reviewed author. And I have read negative reviews, where the reviewer felt sufficiently independent of the Author as not to fear any reprisals. Neither the one nor the other have anything to do with scientific Truth. They are degenerate forms of this genre.

The Reviewer should be acquainted with the subject treated in the reviewed book. But ideally he should not have a stake in the matter. Otherwise, he is likely to fall victim to his instinct of self defence and self-preservation. A Reviewer who succumbs to that has lost the right to write a review. A Reviewer should be self-critical and in his decisions and comments constantly ask himself whether he is being honest with himself and the Author on whom he passes judgement. It goes without saying that the Reviewer's presentation of the thesis / theses and argumentation of the Author must be such that the Author will recognize his book, his thought and his expression, and say "Yes, that is my book"! Only then can the Reviewer proceed to advance his criticisms, and during the process, he must take care that his criticisms correspond with the Author's claims factually and are no twisted misrepresentations of the Author's meaning.

Naturally, a Reviewer is not a robot; he has feelings and preferences and views of his own, standpoints and commitments that sometimes clash with the views of the reviewed Author. The trick is how to keep these personal, subjective preferences in check, when writing about another Author. Failure to self-contral here may result in a review that tells more about the Reviewer than about the Reviewed Author. In the *Westminster Theological Journal,* for example, there appeared a 'review' of my book by Dr Moses Silva as well as a Response by me. This gentleman made it his habit to seek for the most unnatural interpretation of my words, concoct his own construction and then present it as my thesis and criticize it! As I showed in my Response, he hardly ever interpreted my words in a natural way, let alone in the way intended by me. The whole review was a gross misrepresentation of my meaning. No doubt Silva had his reasons for doing that, as becomes obvious in his Postscript to which I made a detailed Reply (see my web site under "Debate").

Sometimes, however, the Reviewer may have a professional interest to protect. This appears to be the case with Dr Buist Fanning. As is well-known, he has engaged along with others in the investigation of Tense and Aspect. My book took up this subject and explained how Greeks of all time have dealt with it. It found, moreover, a number of serious inadequacies in the claims that have been advanced, claims that fly in the face of both the natural users of the language and the ancient texts themselves, thereby introducing confusion. It is understandable that Fanning should be unhappy with this and feel that a central pillar of his work was knocked down. What should he do? Should he desist from writing a review of a book that undermined much of his thesis? Should he acknowledge that he had erred? Or should he fight, using any means available, not even shrinking from misrepresentation? Unfortunately, he chose the last avenue. He decided to misrepresent my book's thesis and arguments in order to devalue it, though I would very much have preferred to look upon it as a failure to understand me. But there are difficulties in explaining it away as a mere misunderstanding.

That this Response appears on my web site is owing to the following:
Dr Fanning's review of my book was published in the ***Bulletin for Biblical Research***, edited by Dr **Rick Hess** of Denver. I sent to Dr Hess an earlier draft of my present Response to Dr Fanning's review, requesting of him to publish the Response (the final version) in the same journal. However, Dr Hess refused to publish my Response on the grounds that (a) it is not the custom of ***BBR*** to publish responses, and (b) they did not want to set a precedent by publishing my Response. I wrote back, arguing that while I appreciated the general principle of not publishing responses, if an Author had been grossly misrepresented, it was the moral duty of the Editor, whose journal had been the vehicle for the misrepresentation, to give the wronged Author the chance to respond and to set the record straight. Dr Hess's refusal was final. Evidently, Dr **Hess** and his **BBR** are not bothered by moral questions. They apparently think that it is quite all right to misrepresent authors and feed their readers with lies.

The above circumstances explain why my Response is placed on my web site. This is visited by people from many countries in the world, especially from the USA, and not only by readers of the BBR.

I would ask the reader to read my Response carefully and thoughtfully and try to understand what I have written and compare it with what I am represented as having written. Naturally, the best result would be achieved if one had access to the book itself in order to look up the arguments and the pages to which the Reviewer refers. I am confident that anyone who does that, will see that Dr B. Fannings's 'review' is not a fair representation of the concerns of my book.

# *The Development of Greek and the New Testament*

## A Response to Dr B. Fanning's Review

Dr Fanning begins his presentation of the contents of the book with a reductionistic statement: "This lengthy and detailed book argues for two main points: (1) the unity and continuity of the Greek language ... and (2) the importance of later Greek for NT interpretation". The informed reader is likely to raise an eyebrow at this information on the contents and scope of this book. This minimizing tone permeates the entire review; the 'positive' statements are hidden behind generalities mainly in the presentation, occupying barely 12% of the review and a few *en passant* comments strewn here and there, while almost 90% is devoted to a search for points to criticize. He makes it also his habit to refer to pages, a circumstance that easily gives the impression that the criticism is substantiated ... until one checks the references! Using my words out of context seems to be a frequent line of procedure.

I will attempt to keep this "Response" within reasonable limits, nevertheless, I must take up briefly each one of the points raised and indicate what my book actually says.

The first charge, aimed at undermining a central thesis of the book, namely that later Greek can inform NT Greek, is the loose remark "I would be more comfortable ... if he [Caragounis] showed himself more consistently aware of the opposite possibility (that later Greek may mislead us about NT meaning or usage)." The Reviewer is obviously unaware of the intensive scholarly work by highly esteemed Hellenic scholars (many of whom are cited in the book), or else he thinks that Hellenic scholars just make unsubstantiated claims, hoping that they will not be caught out. At any rate, any objective reader who examines my book in detail, will know that any evidence that has been used, whether from pre-classical, classical, post-classical, Byzantine, Mediaeval or Neohellenic times is presented in the most critical and acribic manner and that no conclusions have ever been drawn without the case having been argued properly and at length.

The charge that "Caragounis hardly ever refers to the possibility of discontinuity (exceptions: pp. 248, 254, 283)" is patently untrue and implies that the reviewer has missed the whole point. Discontinuity has been taken for granted ever since Erasmus committed his error (sc. introduced the pronunciation that bears his name, which divided the Hellenic language). My book was written to show for non-Greek scholars something of which they are unaware, sc. the continuity of the Hellenic language. This is the important thing, not the discontinuities, which exist and of which I have also spoken at appropriate places. Moreover, the very title itself (*The Development of Greek* ...) implies change. Furthermore, the pages Fanning refers to are not the only pages in which I speak of discontinuity or changes. For example in my discussion of "Time and Aspect"—precisely the section that mentions Fanning's work—I write: "Sometimes Neohellenic casts light on developments, on changes that took place between the classical times and our own day ... This has been demonstrated repeatedly in Chapters Three, Four, and Five. But sometimes the significance of Neohellenic lies in its continuity with the ancient phase" (p. 336). Here then, it is expressly stated that not only continuity but also changes have been discussed in chs. Three, Four, and Five and not only on pp. 248, 254, 283!

With regards to the future tense, Fanning claims that I "misunderstand the real sense of the future in NT Greek (that it is purely temporal and not aspectual)" and he quotes Blaß-Debrunner-Funk as support. It is true that Blaß-Debrunner-Rehkopf (§ 348) claim that: "Das Futurum ist das einzige Tempus, das nur die Zeitstufe ausdrückt." However, as Robertson, *A Grammar of NT Greek*, p. 888, points out, "The future is mainly aoristic (punctilear) ... but sometimes durative." To the same effect Moulton, *Prolegomena*, 149 f. The careful reader of my book will note that I discuss the future *diachronically* (i.e. in its historical development). I write (p. 157): "Like the present, the futute, too, expresses both durative and instantaneous (effective) action. This is so already in A [= Attic] times, though from EH [Early Hellenistic] times on there is an increase of the durative future". In three footontes to the above text I give long lists of examples in classical authors, in the LXX and even in the NT. My statements are also borne out by the historical grammarian of the Greek language, A. N. Jannaris. I am sorry if this evidence is inconvenient to Fanning's theory.

He calls the evidence I have presented for the causal-ἵνα "an illegitimate linguistic option" without explaining why. He concedes that "Apollonios Dyskolos mentions a causal use of ἵνα and it can be found in later Greek" but refuses to see it in the NT. Apollonios Dyskolos' dates are uncertain. However, he was the father of Herodianos, who flourished in the reign of Marcus Aurelius (161-180) and, according to *Souda*, was younger than Philon of Byblos (A.D. 70-160 acc. to *Oxf. Class. Dict.)*. If Herodianos was of the same age as Philon, then Apollonios, who flourished in early second century, must have been born after mid-first century A.D. This means that Apollonios' evidence is not late, as Fanning seems to think, but quite relevant for the NT. In fact, the causal-ἵνα occurs at Jn 8:56, about which Fanning is silent. As I have shown, this causal use occurs also in Epiktetos (I-II A.D.) and the LXX! It appears, then, that my suggestion is an "illegitimate option" not because it lacks evidence—I have presented plenty—but evidently because it does not fit espoused theological viewpoints. Accordingly, his objection to the parallel of Mk 4:12 and Mt 13:13 is based on theories of synoptic dependence, Marcan priority, and the different theological twist Matthew is supposed to give to his words. Here theology is made to decide over grammar. Though there is a remote possibility for that, statistically the far greater likelihood, however, is that authors often express the same idea with different vocabulary and syntax (cf. e.g. Jn 8:51-2, where θεωρήσῃ and γεύσηται refer to the same thing). More gravely, however, the Reviewer speaks with an air of authority, which, does not appear to be warranted.

The Reviewer refers to pp. 234, 263, 301-3 and says that Caragounis has "a tendency to overstate his conclusions and minimize or avoid evidence counter to his thesis ... to account for opposing arguments, but Caragounis seems to regard explicit acknowledgement of counter-evidence ... as crippling weakness". I have looked at the pages referred to, but I am mystified as to what occasions these charges.

My statement that Neohellenic "preserves all the basic grammatical categories in tact" (p. 59) is felt as a problem. Evidently, Fanning takes it woodenly, failing to pay attention to the long list of "basic categories" that I cite immediately after to substantiate the nature of my statement. Thus, when at other contexts (e.g. pp. 145, 152-53, 174, 185), dealing with various details, I point out discontinuities (which, by the way, he did not credit to me, above), he draws the conclusion that I contradict my ealier statement. I am afraid the 'contradiction' lies in his own perception. On p. 59 I speak summarily of the basic continuity. In the other contexts I take up various individual issues for further comment and elucidation. There is absolutely no contradiction in my statements, if they are understood properly and in context.

The Reviewer cavils at my statement that Neohellenic "has at its disposal the entire linguistic treasure of the Greek language from the very beginning to the present" (p. 60). In fact, I have shown that even Mycenaean words are still in use! This statement was followed by certain statistics on vocabulary. At the end of those statistics, in conclusion, I summarized: "Neohellenic can use any term from any period of the language, *so long as it is understood*" (63). The Reviewer clutches at the Italicized words and tries to create a contradiction between this and my above statement. But no one who reads these statements in their contexts with proper linguistic sense and feeling can find a contradiction here. I have nowhere claimed that Neohellenic is exactly the same in vocabulary and syntax as Attic Greek. In that case we would be speaking of identity, not continuity.

In my discussion of the problem of 1 Cor 7:36-38, Fanning says: "Caragounis makes the point that Greek can use the neuter adjective for the abstract substantive ... omitting to mention that in 1 Cor 7:36-38 the relevant usage is feminine ... not neuter ... (he conveniently omits the telltale feminine article)." This is quite unfair. There is no "telltale" thing that I "omit". Perhaps the Reviewer does not rule out the possibility that I might sink so low? But does he honestly believe that even if I had wanted to hide from the reader the fact of gender, that I would have succeeded? Does he think so low of the intelligence of NT scholars, so as to be duped by such cheap tricks? Since I took the trouble to quote the Greek words with their article (e.g. τὸ παρθένον being used instead of παρθενία!), it is obvious that there was neither any intention on my part to deceive anyone, nor could anyone be deceived. NT scholars are supposed to know the difference between neuter and feminine. But more importantly, Fanning tries to give the impression that I build my argument and my interpretation of the passage on just the fact that Greek can use the neuter adjective for the abstract substantive. This is emphatically not the case. The neuter used as substantive is only a part in a series of arguments I use, but not the only or the main one. The reader only needs to turn to my text to discover that Fanning's criticism is groundless. As for what it means for a young man to "give his virginity in marriage", I have—I think—explained the matter adequately.

Fanning uses some loaded expressions: "Caragounis includes a lengthy and vituperative attack on what he perceives to be the views expressed in three somewhat recent books on verbal aspect in Greek". The issue here is the false teaching that the Greek verb expresses Aspect but not Time. Since Fanning has figured in that debate, it is understandable that my critique is felt as "vituperative". Here Fanning accuses me of "resistence to considering other points of view besides his own". But I ask: "What was there for me to consider and pay deference to in this false teaching, that turns upside down NT language and exegesis?" For me Scholarship means search after the Truth. In Truth there is no place for deference to or compliments for false teachings. And the truth is that the Greek verb expresses both Time and Aspect. Fanning complains that I have misrepresented him. I do not think so. I have said expressly that in my discussion I concentrated mainly on the most radical position. I drew the others in (including Fanning) only "insofar as [i.e. to the extent to which] they assume a similar stance and arrive at similar conclusions" (317). I cannot see that I have acted unfairly.

Emotional language pops up again: "[Caragounis] is petulantly dissmissive of ... my attempts to define the meaning aspect carries in the Greek verb (318)". This is in reaction to the remark I made that he needed "eighty pages to define aspect". I can understand his disappointment, but my remark was hardly undeserved. However, when he says: "Our [he and his Verbal aspect associates] contention is that Greek aspect itself ... must be clearly seen as a *viewpoint* feature, a more subjective way of portraying an action or state ..." he is not really saying anything new. Greeks have always taught that the speaker himself chooses how to describe an action, so long as no other constraints prohibit him from doing so. Again Fanning states that “Caragounis’s simplistic reference to the ‘durative’ and ‘instantaneous’ or ‘punctilear’ meanings carried by the present and aorist ... shows that he has missed this point entirely”. Now quite apart from the fact that these terms have been used extensively in grammatical discussions and readers of Greek are at home with them, what sense does it make to say—as Fanning does—that Caragounis, who daily in his speech and writing uses the present and the aorist and the imperfect and the perfect, has completely misunderstood the meanings of these tenses, whereas some people in Texas have got them right?!

Fanning goes on to make a statement that under any view can only be seen as unreasonable. He not only thinks that the Greek users’ *Gefühl* of their language has no significant bearing on whether the Greeks expresses time through their verbs or not (as though Greeks do not know how to express time), but he actually goes so far as to set aside the opinion of Modern Hellenic scholars of Greek on the ground that “grammarians have been wrong before”! So, expert linguists and grammarians of the calibre of G. Hatzidakis and A. N. Jannaris, who also had the entire history of the language at their fingertips are assumed to be wrong, whereas certain followers of general linguistic theory, whose teaching jars with the genius of the language, are right. I have explained the matter before: no language can have a meaning unit that has not consciously been expressed by someone in the language group. Since the days of Ferdinand de Saussure general linguistic theory has tended to assume such abstract theoretical stances that not infrequently it has clashed with the actual use of language and failed to explain the phenomena. It must be laid down that no linguistic theory can invalidate the empirical use of language. I have illustrated in my book the untenable conclusions often reached by such linguists. In its work, the modern science of linguistics must let itself be constantly informed by the empirical use of language to keep it from running amok in its theories. Where linguistic theory conflicts with the empirical use of language, its theories are wrong and must be given up. The stance that Fanning and his associates in the matter have taken implies that even if St Paul arose from the dead to protest against the violence done to his language, Fanning would tell him: "You do not understand, Paul. You do not mean what you think you mean. For according to our linguistic club's rules, you mean something else, because this is how we think language functions"! It is time for such 'linguists'[1] to come down from their high horses, if they want to do any service in the cause of NT exegesis. The text of this book is far too important with a message of life and death for men and women (2 Cor 2:15-16), to play around in this way.

The next issue Fanning takes up is my work on the pronunciation of Greek. Once again his introductory statements are reductionistic: "Caragounis again is convincing in regard to the basic point: the sounds of Koine Greek were much more like modern Greek and not like the Erasmian system of pronunciation ..." In astonishment we may ask: "Is this all Caragounis has proved in this chapter? " For then it may be asked: "What is special about Caragounis' demonstration, if all he has shown is that Koine Greek was closer in pronunciation to modern Greek?" Anyone who takes a look at Robertson's *Grammar*, will see that he has said the same thing. If this is what Caragounis' work amounts to, then he must be reiterating positions that were established almost a century ago. One wonders here why Fanning does not want to tell the reader *the truth* about what Caragounis has demonstrated.

Without reviewing the massive evidence that I have supplied (mainly from the inscriptions, which are older than the papyri and hence more important about the beginnings of the *Historical Greek Pronunciation* [HGP]) and drawing the necessary conclusions from it, he tries to find defects in my treatment, cf. e.g. the loose charge: "he [Caragounis] seems uninterested in specifying pronunciation differences that may have existed within this long period of time and especially between Koine and Neohellenic". A statement such as this betrays carelessness in the reading of my text. I have indicated clearly the exact date when each 'change' in pronunciation is witnessed epigraphically and when the particular pronunciation becomes rife. I have also indicated that the nature of the evidence is such that it does not allow us to say at what point exactly a given pronunciation had totally replaced the previous one

[1] NB. I am not attacking proper linguistics, i.e. the study of language, but only the *Abarten* (= degenerate species) of linguistics.

in a particular locality. But that is totally irrelevant for the problem at hand. On p. 377 I have actually anticipated his demurral: "*The important thing is not when this process ended, but when it started.*" In the past, the claim has been made that the pronunciation of Attic Greek was Erasmian. Hence, my work concentrated mainly on the Attic epigraphical evidence. This dialect was the most important dialect in the Greek world and lies at the basis of the so-called Koine Greek, a branch of which is NT Greek. My study proves that the above Erasmian claim is a fraud. My work shows quite conclusively that from early classical times the pronunciation is moving in one direction, that within classical times all the letters had received their HGP sound, that in post-classical times this was the established norm, that the Christian era was using the HGP, and that this process brings us unfaulteringly to Neohellenic times. And last, but not least, the great fact—with inestimable consequences for the exegesis of Greek literature and of the NT in particular—of the dichotomy of the Greek language which «the error of Erasmus» brought about. All of these important facts are supressed in Fanning's 'review'.

Still unwilling to pay attention to my statements, Fanning writes: " 'HGP' comes to be used in most places as a substitute for 'modern Greek pronunciation' without further qualification (391-392, 396)". This information comes to grief by such statements of mine as: "The current practice among Erasmians to speak of the pronunciation used in Hellas as the "Modern Greek pronunciation" cannot stand critical historical scrutiny" (383). On pp. 391-2 I go on to say: "... we have sufficient evidence to know that the present Greek pronunciation was in all essentials establishing itself already in Vth and IVth c. B.C. This process was in some cases completed rather soon, while in other cases it was protracted. This means that the so-called 'Modern pronunciation of Greek' *is not modern at all*. Hence it is incorrect to speak of 'the Modern Greek' and of 'the scientific (i.e. Erasmian) pronunciation of Greek'. The correct procedure rather is to speak of *the Greek* or (still better) *the Historical Greek Pronunciation of Greek* and of *the un-Greek*, or *artificial*, or *Erasmian*, or *Etacistic pronunciation of Greek*". On p. 395, in criticism of Allen's *Vox Graeca*, I write: "If it is so clear then [that is, from what Allen himself concedes] that the pronunciation (in the strict sense, not only of the value of the various letters, but also the sound quality) of Homeros and of classical antiquity is, in the absence of magnetic tape recordings, forever lost to us and beyond the possibility of recovery or reconstruction, *is it not, in that case, historically and scientifically more honest and correct to pronounce the language according to its own natural and historical development, rather than to impose upon it foreign sounds imported from other sister or rather 'nice' languages within the Indo-European family?* If only one pronunciation is to be used in pronouncing all these type of writing—coming as they do from a time span of 1200 years and more, during which period the pronunciation in fact evolved—then surely the Historical Greek Pronunciation (whose roots go back to the Vth and IVth c. B.C.) is the only legitimate candidate, not the artificial construct of Erasmus". I think that here the reader has plenty of explanation and qualification. I am surprised that the Reviewer can call this "without further qualification".

Finally, with regard to ch. seven, "The Acoustic Dimension in Communication", in an attempt to vindicate Erasmianism, Fanning again misconstrues my statements. He writes: "... but as he [Caragounis] admits, there is benefit even in using the Erasmian system to sound out the Greek (pp. 423, 442, 450-51)". First, "to admit" in a context such as this carries the connotation of conceding or acknowledging (by implication, unwillingly). That I should have 'admitted', whether willingly or unwillingly, to any "benefit of using the Erasmian system" must be a joke. What I have written is: "Because the Erasmian pronunciation gives to most letters the same value as the Historical Greek Pronunciation, it is not always possible to show clearly the difference in a given rhetorical figure. But when one considers the softer, more elegant, and more pleasing quality of the historical pronunciation, then it becomes obvious that even those examples (pronounced in the Erasmian way), which Weiss regards beautiful and well-sounding, exhibit these qualities to an even higher degree when pronounced with the natural Greek pronunciation". I am afraid this is quite different to what the Reviewer represents me as having said.

I regret to say that Fanning has not acted his Reviewer's part responsibly. His 'review' represents neither the views nor the arguments of my book correctly.