The Development of Greek and the New Testament A Response to Dr. M. Silva

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Moises Silva has attempted to write a review of my book *The Development of Greek and the New Testament: Morphology, Syntax, Phonology, and Textual Transmission*.

The above book is the first attempt, since the fatal error of Erasmus (who unintentionally through his propagation of an un-Greek pronunciation brought about a division in the Greek language) to rehabilitate the Greek language for NT scholarship and in particular to place NT Greek in perspective, that is, as part of an ongoing process of development from the beginning (of written documents in Mycenaean times) to the present, where not only pre-NT but also and especially post-NT linguistic developments are germane for a more correct understanding of the NT text. This book is thus a critique of the self-complaisant way in which NT Greek has thus far been examined, that is, without regard for the unity of the language as a whole or the light which post-NT developments throw on the NT idiom. This is asserted in spite of much laudable work performed by NT and classical scholars of non-Greek descent. It is thus understandable that the strong challenge of this book will cause discomfort and perhaps even irritation to many, unless they be willing to listen open-mindedly and pay heed to the evidence presented.

Now in a scholarly review (especially of the length of Silva's review), it is expected that it presents adequately the thesis and contents of the book against the background of scholarly discussion, its methodology, its argumentation, its evidence, what it has achieved, and to what extent the attempt has been

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successful. Having dealt with the main concerns of the book, the review may also contain—to the extent this is feasible—a critical treatment of details which are not central to the thesis.

Silva's review does not conform to the above standard. His presentation is brief and rhapsodic in the extreme: the reader is simply never told what this book attempts to do or how it accomplishes it. There is no discussion of its methodology, no exemplification of its argumentation, no presentation of its evidence and its results, nor any indication as to the areas in which it advances the discussion.

Silva's review is basically polemical. His stated basic criticism is (a) that Caragounis romanticizes Greek, and especially (b) that he does not use modern linguistic theory and jargon. In what follows I shall scrutinize each one of the points Silva raises to show that his criticism is unfair. But first a few remarks on why I do not use the jargon and categories of modern general linguistics.

I. *The Grammatical Terminology of My Book*

My book is not in the least concerned with how the theories of the discipline of *modern general linguistics* are developing, at what stage they are at present, or what the competing positions and arguments are. Therefore, any criticism directed at my book from that perspective is totally irrelevant. My book is a *diachronic discussion* of the development of the Greek language from ancient times to the present, in which I seek by means of an immense number of texts from all periods of the language to place the NT within its proper historical development showing both continuities and changes and how these continuities and changes affect NT interpretation. This means that valid criticism would have to proceed from the stated aims and remain within the stated parameters. Silva does not show in what respects the terminology of modern linguistic theory is more appropriate than the terminology I use, or what would have been the gains which I have now forfeited.

My choice of approach was not only dictated by the material, that is, the Greek sources, but also by the following considerations:

1. General linguistic theory, which admittedly has given some interesting and useful insights for which we are thankful, has not yet superceded classical grammar (though its supporters wish it had). Thus, to claim that unless one applies modern liguistic theory and categories, one is out of touch (review,p. 6) is not only reminiscent of Stanley Porter's grotesque statements with regard to aspect and time, but the claim is thoroughly ludicrous. Does Silva really mean to say that, during the twenty-three centuries that elapsed from the Alexandrian Grammarians to our own time, students of Greek and Greeks in particular have not understood Greek until the advent of modern linguistic theory? And what about the study of the European languages, which has been executed on the basis of the grammatical categories of the Alexandrian Grammarians? This is, indeed, Porter's claim with regard to aspect. But I hope that that viewpoint has come to grief. It must then be emphasized that modern general linguistic theory is not a *sine qua non* for the study undertaken here, as Silva appears to claim. Indeed, in the opinion of many classical scholars, the jargon and categories of general linguistics tend to confuse rather than elucidate the issues.

2. Since my book is concerned with historical developments and there is an immense diachronic discussion taking place with texts, grammarians, and literati of all periods of the language, it is obvious that I could not introduce into my discussion categories that had been foreign to the discussion, that is, the jargon of a few scholars who try to outcry one another (the disagreements between them are well-known). For such a work the more settled and known terminology of

traditional grammar, which is followed by all these authors, was necessary.

3. Since my study encompasses a deep-going discussion with past grammarians, it is again obvious that the grammatical terminology used by them and generally understood was the right terminology to use.

4. My book is directed to every NT scholar who has learned his or her Greek with the traditional grammatical categories. The question is, how many NT scholars would understand the arcane categories of modern linguistics? My concern was to communicate sense, not to parade the latest concepts discussed by a few general linguists in debate among themselves.

5. Finally, the proof of the pudding is the eating thereof. That general linguistic theory cannot adequately deal with the issues I set as my goal in this book becomes obvious not only from the fact that the consistent application of the modern science of linguistics played havoc with regard to aspect and time in Greek, but also from the fact that, for all their linguistic expertise, neither Silva nor his companions were able to discern and to refute the falsity of those claims. My approach did. This is because what is needed is not new terminology and categories, but a better understanding of the Greek language—which is a rare commodity in these days.

With these preliminary remarks, I proceed to take up each one of Silva's criticisms. Because, according to my request, my response is to appear in the same issue as Silva's review, I refer to it according to his twelve-page computer-script.

II. *Criticism of Details*

1. In setting the stage for my discussion (*Introduction,* p. 2), I begin by mentioning a number of respects in which Greek is unique or unparalleled among the Indo-European languages.

Greek has “the longest documented history,” that is, a continuous use of 3,500 years. More importantly, “from the point of historical linguistics, Greek affords unique research material for the study of the development of dialectology.” Finally, unlike Sanskrit and Latin, which have given birth to many languages, Greek never produced any daughter language, but is the same language (albeit with changes sustained). These are indisputable facts. Silva misunderstands these historical remarks as implying that I hereby argue for a “superiority in the Greek language” (review, p. 3) over the other languages or that “Greek is qualitatively distinct” (p. 4)—an expression that I have never used. That Silva has misunderstood and misrepresented me here becomes obvious from the fact that my statement that Greek has not produced any daughter languages could just as well be interpreted as indicative of sterility on its part! I am speaking of pure facts; I am not making evaluative judgments. Unfortunately, Silva construes an imaginary scenario, and then criticizes his own concoction as though it were my own words, namely that I had claimed “some inherent fine quality” for Greek that is lacking in the other languages.

2. On p. 66 I refer to Hatzidakis’s statement that the changes from ancient Greek to Neohellenic have not been accidental, but have followed certain linguistic laws. Silva, failing to understand that in the context there is no explicit or implicit comparison with any other language, gratuitously interprets these words as though *I* implied by them that “the changes in other languages have been accidental or arbitrary” (his own words) and calls my position, or more correctly, his own misconstrual of my position, “preposterous.”

3. With reference to my book (pp. 33-35), which contains a long quote from E. M. Blaiklock, Silva cavils at my statement that during the Attic period “the Greek language reaches its highest degree of perfection,” following which I mention as examples a few inflectional points as well as other syntactical

aspects that made it what it was. Blaiklock, in fact, has succeeded well in setting forth some of the excellences of Attic. Silva brushes him aside as a romantic of the nineteenth century (Blaiklock was active in the second part of the twentieth century). It would have been more appropriate if Silva had shown by argument that Blaiklock was mistaken. Silva also demurs at quotes from two other scholars, who speak eulogistically of Attic Greek, which shows how far some scholars are prepared to go. These scholars are no nineteenth-century romantics, as Silva implies, but were active in the second part of the twentieth century; they have linguistic feeling! However, the very fact that I use a language that is more restrained than other scholars ought to have indicated to Silva the sober character of my comments. Not only does he miss the point here, but he even thinks that I hereby claim—again wrongly—Attic superiority over other languages, when no such question of superiority or comparison with other languages ever arises in the context. The Attic form is simply understood as superior to the other forms within the Greek language for certain literary genera. If Silva has a quarrel with this, then he is at variance with the ancient Greeks who thought so, with the Greeks of Hellenistic times who kept to the Attic standards, and with the Greeks down to our day who allowed Attic Greek to influence and shape Neohellenic! Further, he takes out of the context of discussion, for example, my reference to the fifteen meaning-units of the noun, and says that, if development is based on many forms, Swahili must be superior to Attic since it counts “nearly twenty different forms” (review, p. 5)! This nonsensical comparison makes clear that Silva has misunderstood what I am saying. The development of a language, which goes hand in hand with the intellectual development of a people and is a mirror of that people’s spiritual, intellectual, and aesthetic accomplishments, is best seen in the literature, art, and ideas that the language has inspired and produced. (Would Silva, please, produce a Swahili counterpart to Platon or Demosthenes so that his comparison would stand?) It is in this respect that Greek is said to have been "fully developed." There is never any mention of superiority in my book, simply because I never broach that issue.

4. On pp. 60-63 I discuss the present state of Neohellenic and its relation to other languages. In order for the reader to appreciate this so misunderstood form of Greek, I present a comparison of vocabulary between Neohellenic and English to show that Neohellenic (having at its disposal all previous periods) is actually quite rich and far from "the sickly offshoot of ancient Greek," which has often been the scholarly opinion. To do this I use English as a reference point, not—as Silva thinks—in order to compare them. Again I hold myself to facts and refer to the painstaking investigations of others. Silva takes offense at these facts and thinks that I thereby claim that "Greek is 50% more sophisticated than English" (review, p. 5). The reader will look in vain for such a claim in my book; it is Silva's own unwarranted inference. What I discuss is vocabulary. I show, for instance, by the citation of concrete evidence that, if the Greek vocabulary were taken out of English, "American English would not be able to meet all the communicative needs of its users" (p. 61). My own admiration of English comes through when I speak of "such a highly developed and rich instrument of human speech as English," a eulogium that is tacitly bypassed by Silva. That English is dependent on Greek as are other European languages, including Silva's own mother tongue, has been underlined not least by the great Spanish scholar F. R. Adrados. There is no reason for Silva to take offense at this.

5. Chapter Two deals with the relevance of Neohellenic for the NT, presenting a variety of evidence to show that relevance. Silva says practically nothing about this whole chapter and its

most significant facts for the thesis. But he picks at one of my examples. To show the continuity in conceptualization that comes through in the feeling of present-day Greeks for the language of the NT—in this case relating to the notorious question of aspect—I relate that an uneducated old Greek woman, without knowing the terminology, was able to perceive the difference between an aorist subjunctive (*hamartêsête* corresponding to 1 John 2:1: *hamartête*) and a present indicative (*prattei* corresponding to 1 John 3:9: *poiei*). The Neohellenic Katharevousa paraphrase quoted often uses the original NT verbal form, but sometimes a more modern equivalent. In this particular case the translator could have used *poiei*, but chose to use its equivalent *prattei,* which is also classical. With regard to the aorist subjunctive, the form *hamartêsête* was more usable than *hamartê*, and he therefore used that. Silva takes offense at this and unwarrantly accuses me of implying that "an uneducated speaker of Modern Greek is more adept at exegetical interpretation than scholars" (review, p. 5)! As if this were not enough, he goes on to say that my quoting the Neohellenic paraphrase to the woman (i.e., *hamartêsête* instead of *hamartête* and *prattei* instead of *poiei*) "already prejudges the question by using the verb *prattei* in the latter verse" (review, p. 5). Would Silva, please, oblige us by telling us in what way the Neohellenic equivalents of the original wording "already prejudge" the issue? For, surely, the Neohellenic equivalents function in precisely the same way as the original forms they paraphrase, and they express exactly the same aspectual distinctions! The original and the translation say exactly the same thing. The old, uneducated Greek woman had understood the texts correctly.

6. Silva exemplifies "Caragounis's penchant for overstatement and misleading claims" (review, p. 5) by mentioning that I use "the outdated *centum/satem* distinction to characterize the Indo-European family of languages." I would

like to point out that these terms are still used by linguistic scholars, and in any case how can this be called a "penchant for overstatement" and a "misleading claim"? He also demurs at my characterizing Koine as a "sub-standard language," referring to pp. 40 and 121-22. As a matter of fact Silva is less than careful in his allegations. On p. 40 I speak of the "motley character of many of its new users from Spain to India and from the Krimaia to Aswan, who had neither the feeling nor the ability to speak and write Greek correctly. With them Koine Greek was reduced to a sub-standard language"! And on pp. 121f., I speak of "countless barbarians . . . as is witnessed by many Egyptian papyri." Where in all this have I called Greek Koine *itself* sub-standard? Unless Silva thinks that the barbarous documents I refer to are examples of fine Greek literature, I cannot see that I have used any overstatements. Nor does he take any notice of the fact that the Greeks themselves considered Attic the given linguistic medium for their compositions during Hellenistic times and even later.

7. Chapter Six deals with the epigraphic and papyrological evidence for the Historical Greek Pronunciation (HGP) from the time inscriptions become available down to the first century. This chapter offers a very full and detailed discussion of the pronunciation in classical times, the reasons for the changes, and the proofs for those changes. The evidence is overwhelming. As usual, Silva fails to get to grips with the contents of this chapter, though he chides me for not using modern linguistic terminology. One is tempted here to ask, why didn't he, with his modern linguistic terminology, solve the problem of the correct pronunciation of ancient Greek? And if he is so certain that I am wrong about it, then why does he use the HGP in his private devotions (review, p. 3)? Scholarly integrity would seem to demand a consistent course of action: if the HGP is correct, he should scrap the Erasmian counterfeit in the classroom, too, as I have done—not only in his private

closet; but if the Erasmian pronunciation is the correct one, then he should keep to it also in his private reading.

Further, he cavils at my not using phonetic helps to set forth the sound of Greek. Evidently he has not paid close attention to p. 351, where I write: "*The Historical Greek Pronunciation* (= *HGP*) is indicated only approximately; as in all other languages, *the sound quality can be learned only from native speakers.*" For this reason, too, I have produced a CD interactive program for learning the HGP. Further down on p. 6 Silva takes up the worn-out subject of the musical pitch accent. He disagrees strongly with me, not on the basis of evidence (he has none), but because "virtually all recognized authorities believe that . . . Greek was, to some extent at least, a tonal language." At the end, however, realizing how problematic his assertion is, he concedes that "there is no conclusive evidence or 'smoking-gun' argument in support of the conventional approach, and in principle one cannot fault Caragounis or anyone else for raising questions." But if that is so, then why criticize me at all?

8. Silva dislikes my claim that the HGP goes back to classical times, but in doing so he goes against the immense inscriptional evidence of this chapter. Nor does he really sufficiently appreciate the issue that the important thing with linguistic changes is the beginning of a process, *not* its end. Moreover, what do we really care about when those who learned Greek in Spain or in Krimaia began pronouncing it in the HGP? What is important is what the pronunciation at Athens and mainland Greece was, because it affected Asia Minor and the other areas with which the NT is concerned. If Silva had paid careful attention to my statements, he would have known that my concern is with the Athenian pronunciation. It was the Athenian pronunciation that Friedrich Blass, with whom I take issue, wanted to prove Erasmian. And all studies on

pronunciation (e.g., Sturtevant, Allen) have concerned themselves with Athenian (Attic) pronunciation.

His characterization of "5th and 4th century instances of interchange" as falling within "the (late) classical period" is simply a historical blunder. He also complains that I "seldom tell [the reader] where the inscriptions come from." This remark shows inattentive reading. On p. 335 n. 32, I write: "The following statement is based on the evidence of the *Inscriptiones Graecae*, particularly on the volumes of the *Corpus Inscriptionum Atticarum* (*CIA*, the most relevant material for Athenian pronunciation), the *Inscriptiones Graecae Antiqvissimae* (*IGA*), the *Supplementum Epigraphicum Graecum* (*SEG*) and the *Corpus Inscriptionum Graecarum* (*CIG*). Of these I have read most B.C. inscriptions in *CIA*, all of the inscriptions in *IGA*, all the Attic inscriptions in the 41 volumes of *SEG*, and consulted the rest as well as other publications." Then, I go on to mention papyrological publications. I believe this information is quite clear, if only one reads it.

9. In my discussion of criteria for establishing the pronunciation of Greek, I referred to four types of criteria traditionally applied, though more recent Erasmians disregard the most important of them, namely, the inscriptions and the papyri as well as the internal history of the Greek language. Instead they concentrate chiefly on phonetic speculation. In this connection I have judged Latin to be of *meagre value* because Latin sounds do not necessarily correspond to Greek sounds, any more than Spanish *c* (e.g., in Cervantes) corresponds to English *c* or English *z* to German *z*, and so forth (pp. 362 ff.). On pp. 375-82 in treating diphthongs and consonants, after citing the Greek evidence at considerable length, I referred briefly to the fact that also Latin has transliterated the diphthong *eu* not as *eu* but as *ev* (e.g., *evangelium*). Similarly, Latin *U* or *V* (e.g., Vergilius) was transliterated with a Greek *B* and *OY*, which indicates that Greek

B was understood as equivalent to Latin *U / V*, not Latin *B*. Here Silva claims that all of a sudden I put "great value" on Latin transliterations (an unwarranted overstatement), which earlier I had judged as of "meagre value" (review, p. 8). For me "meagre value" is not the same as "no value at all." Nor do I actually put any "great value" on Latin, as Silva claims, by citing one or two examples as confirming what already has been proved on the basis of the Greek evidence.

10. In the same paragraph he charges me with giving to the Semitic letter *waw* the sound of *v*, countering that "all specialists take it as a given that *waw* in the ancient Semitic languages as a whole stood for the semiconsonant *w*, not the full consonant *v*" and draws his sweeping and triumphantly condemnatory conclusion, "Thus an important element in his [Caragounis's] reconstruction rests on mistaken evidence"! This simply leaves me dumbfounded, wondering how careful Silva is in his reading. I have mentioned *digamma* at several places, but nowhere have I committed the error (if it can be called an error, since many languages use *v*) that he attributes to me. On p. 375 in dealing with the diphthongs, I say that *Y* (*hypsilon*) in, for example, the diphthongs *AY* and *EY* had the sound of *f* or of *v* depending on the following letter, and I continue: "This is proved beyond possible doubt by the mistake of the stone-cutters in substituting *F* (*digamma*), which corresponded to the Phoenician letter *waw*, and had the sound of *v*." The sentence makes it crystal clear that what had the sound of *v* was not the Phoenician *waw*, as Silva construes me as saying, but the Greek *digamma*. Similarly, on p. 380 (to which he also attributes the same "mistake") I write: "Moreover, the [Greek] *B* replaces almost always the *F* (*digamma*), which was sounded as *v*." Thus, once again he criticizes his own misunderstanding as though it were my own words.

11. Silva returns to the question of aspect (pp. 8-10). He expresses disappointment that my discussion does not turn out

to be what he had expected. And he faults me for not discussing what he had on his own agenda. In this particular section, I set myself the task of showing that Porter's claims that the Greek verb does not express (or grammaticalize, if you like; I use both expressions) time were false. And I did just that. I proved that Porter, applying modern linguistic theory and speculation, had done violence to the Greek verb and its syntax. In the process I also showed that this matter exemplified very clearly the importance of the diachronic understanding of Greek. Neohellenic confirms the ancient evidence and thoroughly refutes this standpoint, from which Silva (now) distances himself. The critical question here is, why did Silva and his fellow-linguists not succeed in refuting this erroneous point of view? Silva admits that my argumentation has succeeded in "refuting the view that the Greek verb does not express time" (p. 9). But if that is the case, I must have delivered what I promised.

Silva goes on to claim that I paint "other specialists, such as Kenneth McKay and Buist Fanning, with the same brush" (p. 9). The reader may decide himself if this is fair. I write: "What is said of Porter's work applies also to the work of the other two scholars, *insofar as they assume a similar stance and arrive at similar conclusions*" (p. 317). I use "insofar as" (= *in such measure as, to such extent as*), not the causal "inasmuch as" (*because*). This shows clearly that I do differentiate between Porter and the other two scholars and that Silva's charge is unfair.

12. On p. 10 he implies that I might hold to the doctrine that the aorist expresses "once-for-allness" because I have used the term "instantaneous," and proceeds to lecture us that such a view would imply "objective reality rather than subjective perspective," as though Greeks never use the imperfect or the aorist of concrete (objective) events! This perspective is no doubt a favorite theme of linguists such as Silva and Porter but

Greek experience is something else.

13. On p. 10 he misunderstands the value of Neohellenic when he argues that since there is sometimes evidence for a certain construction already in pre-NT times, it proves that Neohellenic is unnecessary. Here it is important to ask, why was this evidence, which was there all the time, not discovered prior to my book? The answer is simple. Sometimes someone may accidentally stumble on such evidence. At other times non-Greek scholars are not even conscious that a certain NT construction could be understood differently and therefore they have no reason to investigate. Indeed, the very problems Silva cites as not needing Neohellenic (i.e., John 15:1ff. and Matt 12:28) have gone totally unnoticed by NT scholars, and I, as the first one to point to other possibilities of interpretation, was allerted to the problems by my familiarity with Neohellenic! Thus, a Greek, who has an overview of the entire spectrum of the language, becomes easily conscious of meaning problems, shifts of meaning, and so forth, of which non-Greek scholars are usually unaware. If Silva had understood the importance of this issue, he would never have made such a remark.

14. Chapter Seven (pp. 397-474) is totally bypassed. Silva takes up haphazardly a few points in Chapter Eight, which deals with textual transmission. In this chapter I have discussed some of the latest and most important contributions to textual criticism. At one point I write that the criterion that the best reading is the one that best explains the rise of the others "is winning the approval of textual critics across methodological boundaries" (p. 481). Silva thinks that since this "has always been considered both valid and of great importance," I must be judged to be "not fully at home with the discipline" (review, p. 10). He forgets, however, that I make this statement in the context of the recent discussion when all criteria have been put on the table of discussion afresh, and that I am referring to a

freshly emerging consensus (cf. textual critics quoted on p. 481). Moreover, he accuses me of slandering Westcott and Hort because I write that their aim was to get rid of the Byzantine MSS, something that is common understanding among textual critics (cf. pp. 476f.); hence their genealogical method has been abandoned. In a footnote in the same pages I refer to G. Fee, who also shares my viewpoint, and note that Silva calls himself "an unrepentant and unshaken Hortian" (Silva's own words). I realize now that I stepped on his toe!

15. On p. 10 he misquotes the pages that present the mistakes of P^{66} as pp. 502-8 (they are actually 502-14) and then bemoans that "Caragounis gives the impression that textual critics are unaware of how frequent such errors are." This is again unfair, since my book is directed not to textual critics, although I discuss with them, but to NT scholars in general, and they, as a rule, do not collate NT manuscripts.

16. On p. 11 he accuses me of "confus[ing] the discussion by chastising E. J. Epp for regarding orthographic differences as insignificant (p. 490)." Throughout this chapter I have quoted and referred to Epp with respect and mostly approval. The particular problem here relates to the way Epp regards *itacism*, etc., which, as I show, has unfortunate consequences for textual criticism. Silva's words are simply misleading. My discussion is very nuanced and cannot be reproduced here apart from a brief quote: "[Epp] claims that mere orthographic differences in the form of 'itacism', movable n, and abbreviations, 'are insignificant . . . ; they cannot be utilized in any decisive way for establishing manuscript relationships . . .' While this is true of many cases of orthographic differences, it may not be generalized, and in spite of Epp, as will amply be documented in this chapter, the orthographic issue, in not a few cases, does point the way towards the original text" (p. 490). Surely, my objections cannot be characterized as "confusing the discussion" nor as "chastising E. J. Epp."

17. On p. 11 Silva makes one of a couple of brief references in his review to Chapter Four, which covers 92 pages and deals with such an important topic for NT exegesis as syntactical developments. It is astonishing that Silva has practically nothing to say about the contents of this chapter except to call into question my suggestion that *hina* in Rom 5:20 should be understood causally, in accordance with developments exemplified by one of the finest of ancient Grammarians, Apollonios Dyskolos. He does this not by linguistic argument but by how it has been "commonly understood" and translated by the NIV and NRSV—I must say, an original way to discuss grammatical and linguistic problems! My discussion of Rom 5:20 is part of a larger section dealing with *hina* and comprising nine closely argued pages full of evidence for the various uses of this conjunction. For obvious reasons, it is impossible to reproduce this complex argument here. I therefore regretfully refer the reader to my book, from which I am sure the reader will perceive that Silva's objections are unjustified.

18. The following example shows Silva's lack of feeling for the Greek language. He has failed to understand my explanation of how the verb *apolambano* has come to take on the meaning of "to enjoy," exemplified in Rom 1:27 and Luke 16:25, and writes ironically: "Caragounis, however, tells us in all seriousness that just as the rich man 'enjoyed' his good fortune, so also Lazaros 'enjoyed' (i.e., *suffered in*) his misfortune (p. 289)." No one should blame Silva for his lack of feeling for Greek. As K. Krumbacher, a specialist in Byzantine literature, long ago pointed out with regard to himself: "What I lack is the feeling for the [Greek] language, which everyone usually has only for his own mother tongue" (see pp. 53f.). But Silva perhaps could be more careful with ironizing those who do have a feeling for Greek.

19. Finally, Silva refers to my study of 1 Cor 7:36-38, and

without presenting my argumentation or any objections to my arguments, he characterizes the results of my research as "astonishing." If he questions my argumentation, he ought to have pointed out its weaknesses and controverted my findings. Insinuations are not acceptable in scholarly discussion.

III. *Concluding Reflections*

It is a truism that almost all reviewers are liable to committing some error against the reviewed author. What, however, has astonished me in this review is Silva's consistent misunderstanding or misrepresentation of my position. I have examined my text in the light of his criticisms, but in no case have I discovered that his criticism was valid or justified. I have been partly misrepresented by reviewers before, but I have never been in the position of having to say—as in this case—that the entire review is an aggregate of misunderstandings, misrepresentations, and criticisms of the reviewer's own construals of my meaning. This is a unique review in this respect, and I cannot explain it.

I understand that there is a problem here. My book deals with the entire history of the Greek language. A reviewer who does not share this wide perspective is greatly hampered and is liable to occasionally misunderstand and to misrepresent. But so consistently? Secondly, it is a truism that every author carries a certain package of implicit background information that goes along with what he or she writes down. Now unless the reader shares that information or is at least open to it, he or she is liable to misunderstand what is written, to take offense, or to consider the argument unconvincing. In this particular case, if a reader has been exposed to the diachronics of the Greek language, that reader will be better equipped to appreciate the argument and accept the evidence I present than one who lacks this background knowledge. This can go a long way to explain Silva's failure to understand me correctly and to deal fairly with my book.

In Silva's case, however, there is another problem. He seems to confuse general linguistic theory and speculation with pure knowledge of the Greek language and how it works. Because of this confusion, all his criticisms stem from the particular parameters he has set without regard for the author's own parameters. He is unable to see that the general linguistic science is still in ferment, and, therefore, cannot be made the arbiter in solving historical linguistic problems. Thus, he cannot appreciate the factors at work in the development of Greek and is unable to treat Greek as the entity which it is and to respect the continuities and changes within it.

It appears, therefore, that my book will engender some uninformed criticism. But such criticism will not stand the test of time. International scholarship has already through private communications to me as well as through reviews in important theological journals given to my book an enthusiastic welcome.