REVIEWS • RESENSIES

SEMANTICS — A METHODOLOGICAL REPLY

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One generally assumes that a review of a publication in an academic journal intends to afford the reader with a knowledgeable account of the scope and content of the book in question along with a scholarly evaluation of the underlying theory and practice proposed by the author. Within such a framework many people tend to accept that the reviewer is someone who is not only well-read in the subject matter, but is also a specialist in the field. Therefore, even a reviewer who has never specialized in, or published anything in the same field of research, is often given the benefit of the doubt. As such, it may be profitable to discuss Professor Ronca's review of J.P. Louw *Semantics of New Testament Greek* (= *SNTG*) in *Acta Classica* 28 (1986) 95–106. This is especially important in view of the fact that though modern linguistic insights, particularly since the late fifties of our century, have gained general acceptance worldwide, it seems that there are still a number of classical scholars debating the question. R (= Ronca), for example, states that his review had 'its origin in a genuine concern for the methodological question of whether, and then to what extent, hermeneutical techniques derived from modern linguistic theory should be applied by classicists to a linguistic analysis of Greek and Latin prose' (p. 95). He also refers to a seminar on this issue at the University of Palermo/Italy in 1984.

It is remarkable that R hardly touches upon the essence of this question but is more concerned with detailed expositions of particular examples quoted as illustrations in the book under review for which he prefers to offer a different explanation. However, he does (in a single sentence) state that, regarding the theory, the author's method is 'a necessary corrective to the centuries-old misunderstandings and misuses of word semantics as "etymology" for exegetical purposes' (p. 96). He then proceeds to evaluate what he calls 'word semantics' in the light of etymology contending that 'the "negative" chapters, especially the fourth on etymology often throw the baby out with the bath-water by indulging in one-sided generalisations (sometimes disguised in impressive linguistic jargon)' (p. 97). This chapter (one of the shorter ones in the book — just over eight pages) seems to have captured the reviewer's total perspective of modern linguistic theory. Though the author of *SNTG* had deliberately tried to avoid jargon for the

sake of a wider audience, R's experience of what he calls 'impressive linguistic jargon' (p. 97) shows how unfamiliar he must be with linguistics if the terminology used in the book was considered by him to be 'impressive jargon'. This is even more perceptible if one considers the examples he selects to illustrate his point. In the chapter on etymology (*SNTG*, ch. 4) it is stated that 'one of the basic principles of semantics is that the relation between the form of the word and its meaning is an arbitrary one' (*op. cit.* p. 25). R objects that this is only partially true (p. 97). He then refers to instances of *onomatopoeia*, of *compounds* and of *derivatives* though he discusses only compounds, and comments that the book never even mentions the fact that compounds *can* represent various degrees of literalness (this seems to me the reference of his term 'total transparency') (p. 97). Yet, on page 28 of *SNTG* one will find the following as a new paragraph: 'On the other hand, one should not entertain the idea that compounds *cannot* have the meanings of their different parts. Sometimes this is true if the meaning coincides with the sum of the meanings of the parts of the word, but one can only recognize that it is so through the meaning which must be known beforehand.' This statement is then illustrated by *inter alia* ἐκπέμπω 'send out'; but in the case of the term διαχειρίζω in Ac 5.30 (occurring in the middle voice) one cannot explain the meaning as διά = 'through' and, therefore, to be understood as 'to cut asunder' which R seems to prefer (p. 97). In Ac 5.30, referring to the death of Jesus on the cross, the meaning seems to be merely 'to kill'. It should be noted that the standard Greek dictionary by Liddell, Scott, Jones, McKenzie gives for διαχειρίζω in Ac 5.30 the meaning 'lay hands on, slay', with additional examples from Plutarch, Polybius and Dio Cassius. In the case of Mt 24.51 διχοτομέω which in Greek *can* mean 'cut in two', it seems preferable to understand the meaning as a hyperbolic expression referring to severe punishment (p. 97). In the case of ὑπερβαίνω 'to sin' (R's third example) it is surprising that he insists on acknowledging a literal explanation involving 'movement over something', which would be the issue if ὑπέρ + βαίνω is explained on the basis of form.

The same can be said of derivatives. A term like ὑπέρβασις, when it has the metaphorical meaning of 'sin', should be explained in the same way as ὑπερβαίνω above. For *onomatopoeia* I would like to know how R explains the lexical meaning of 'bow-wow' from the form of the word. It is noticeable that even traditional dictionaries such as *The Oxford English Dictionary* or *The Random House Dictionary of the English Language* list these terms as 'imitative' without assigning lexical meaning(s) to the term.

R proposes that 'accurate historical documentation based on adequate parallels' (p. 97) will overcome the difficulty of knowing how a NT writer experienced the associative values (he calls it 'motivation') involved when a particular word is used. But, how will one know from historical parallels what the authors of all the parallels 'felt' when *they* used particular words? On the other hand when the use of ἀλώπηξ in Lk 13.32 is explained in *SNTG* as having a wider connotation than the English term 'cunningness' on the basis of the comment in a Patristic gloss from the *Catenae*, the reviewer himself refuses to accept this association by such

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an early source (p. 100). His own insistence on a historical philological exegesis as absolutely necessary for determining meaning may prove to be as much biased as the synchronic data he so adamantly opposes. He may find that what he holds against the *Catena ad Luc.*, namely, that 'the author may himself have extrapolated such a meaning from this very text of Luke' (p. 100) can equally be applied to his own arguments. What is more: when R concludes 'after all "cunningness" is still a connotation of both πανοῦργον and δύσπορον' (p. 100), i.e. terms used by the *Catena* to explain ἀλώπηξ, he clearly shows that he has missed the argument. Neither the writer of the gloss nor the author of *SNTG* contended that 'cunningness' was *not* a feature of ἀλώπηξ since 'rascal' as a translation of ἀλώπηξ in its metaphorical meaning would surely, as R suggests, include 'cunningness'. The argument was merely that for the ancients it seems that the metaphorical meaning of ἀλώπηξ had a wider application than the English term 'cunningness' as a metaphorical meaning of 'fox'. That is to say: the associations of one's own language and culture need not be transferred automatically, and to the same extent, to that of the ancients.

Still enlarging on etymology, R proceeds to discuss an 'even more questionable generalisation: that *single words* are nonentities in comparison with the all-important contextual meaning (p. 97).' I am sure that anyone with even a little linguistic background will seek in vain to find any such statement in *SNTG*. The reviewer's formulation is the result of his own failure to follow the argument, and is in itself a flagrant generalisation, if not a sweeping statement. He quotes a reference to E.A. Nida's book *Exploring Semantic Structures* (p. 14) on page 23 of the book under review, and concludes that not even Nida's statement can prove what R considers the erroneous assumption of *SNTG*, that single words have no meaning at all (p. 97). He misunderstood the issue. What Nida did say is that meaning is not 'an inherent property belonging to words . . . but . . . a set of relations for which the verbal symbol is a sign.' The matter at issue is the assumption of a 'Grundbedeutung' advocated since Plato's *Cratylus*, and has nothing to do with what the reviewer probably understood when he read the chapter, namely that 'single words have no meaning at all'. Nida's phrase 'a set of relations' refers to semantic relations, that is, features of meaning. What is even more discouraging is that R then proceeds to quote from a book by G. Stern *Meaning and Change of Meaning* published in 1931, stating that 'words *have* more or less permanent meanings . . .' (p. 98). Apart from the fact that Stern's book dates back to 1931 and that semantics, especially during the 1960's and 1970's, underwent a Copernican revolution,[1] R resorts to Stern whom he quotes from S. Ullmann *The Principles of Semantics*, 1963, p. 64. But Ullmann's book goes back to 1951 of which the 1963 edition is, except for additional bibliography, a photo-lithographed copy of the 1951 edition. R quotes these authors because they seemed to him to support his contention for a diachronic insistence in semantics, yet the section in Ullmann's book from which he quotes has nothing to do with the issues Nida's remarks refer to. In Ullmann the argument is related to philosophical propositions in reference to Bertrand Russell's distinction between

unambiguous words with negligible emotive potentialities.

It is extremely important to note that there is surely a historical dimension in semantics dealing with *the change of meaning* as associated with particular words, but R's plea for historical semantics in criticizing the arguments in the book he reviews, is unasked for in the sections he refers to. The argument in *SNTG* is not intended to discredit historical semantics, but to emphasize the wrong application of historical semantics when a synchronic issue is argued. In this respect R's review is often confusing. On p. 98 he laments what he calls 'the systematic exclusion of any *diachronic perspective*' in the book. This statement is repeated on p. 101 of his review: 'the Louw-Nida method is itself one-sided and fallacious *whenever it presumes* (as it frequently does) *to dispense with historico-philological* exegesis.' Yet on the very same p. 101 he says that the method proposed in the book 'usually (and tacitly) *presupposes* a sound historico-philological explanation of words and sentences.'

At the beginning of this reply it was stated that R's review is very much concerned with detailed expositions of particular examples quoted as illustrations in *SNTG* for which he prefers to offer a different explanation (pp. 98–99). These should now be taken into consideration.

The expression διακονεῖν τραπέζαις in Ac 6.2 is explained in *SNTG* (p. 36) as an idiom that can be glossed in English as 'to handle finances'. The context of Ac 6.2 refers to the complaint by the Greek-speaking Jews that their widows were being neglected in the care exercised to meet daily needs. Consequently the apostles proposed that seven men should be chosen to be put in charge of this matter since the apostles could not be burdened with handling financial matters; they had to devote their full time to prayer and preaching. The lexical expression 'financial matters' in this passage refers to the taking care of the needs of the poor widows (not to 'general financial administration of the community' as suggested by Jackson-Lake). R objects by saying 'why not give as much weight to other views from recent, accurate commentators?' (p. 99). He then adduces E. Haenchen's remark in his 1977 commentary on the Acts that διακονεῖν τραπέζαις pertains to 'die Armenfürsorge' (p. 106). R characterises the translation 'to handle finances' as 'banking jargon' (p. 98) because he fails to realize that the lexical meaning in Ac 6.2 *refers* to 'taking care of the poor widows'. He accepts Haenchen's 'Armenfürsorge', yet he states that the explanation of the expression as an idiom 'can hardly be' based on 'the (scanty) evidence from intra-textual parallels' (p. 98). But 'Armenfürsorge' is indeed as much idiomatic as 'to handle finances', of which R says: 'idiomaticity is totally unwarranted' (p. 98). What is more: Haenchen's rendering is likewise based on intra-textual considerations. It reflects the semantic *reference* (that is, the circumstances referred to in this particular instance) of διακονεῖν τραπέζαις in Ac 6.2, while 'to handle finances' reflects the meaning of the idiom *as such*. That is to say: what is referred to by the lexical expression in Ac 6.2 is the taking care of the material needs of the Greek-speaking widows. R's criticism reflects an insensitivity to distinguish between meaning and reference and consequently

between meaning and translation. These notions are basic features of semantics[2]. This is why he failed to see that Haenchen *supports* the view taken in the book.

The reviewer then takes up the term ἡμέρα in Cor 4.3 meaning a *law court* (p. 99). The most unmarked meaning of ἡμέρα in Greek (and also in NT Greek) is a *day*. But in 1 Cor 4.3 the meaning is *law court*. While it might be possible to trace a historic line of development from *day* to *law court* as insisted upon by R, such a finding would be quite interesting for studying the development of meanings within the framework of 'the change of meaning'. But for 1 Cor 4.3 one should be careful not to do what R suggests, namely that the usage in 1 Cor 4.3 must be linked to what he calls 'a parallel' expression such as ἡμέρα κρίσεως in Mt 10.15 in which ἡμέρα still means 'day' as a time unit, but in which κρίσεως seems to add the notion of judgment. This expression, however, is eschatological, referring to God's final judgment of this world. R observes that students should be alerted to this connection, but he should have added that if such a connection were behind the development of the meaning 'law court' — and let us grant the possibility for the sake of the argument — care should be taken not to read into 1 Cor 4.3 any connection with the final judgment day of the Lord. In 1 Cor 4.3 Paul says: 'I am not concerned about being judged by you or by any human court'. The meaning is quite clear without having to resort to parallels that can be misleading. This is especially true if one considers R's second example ἡμέρα Κυρίου (for which he quotes 1 Chr 4.41 though the expression does not occur in 1 Chr. 4.41. However, it does occur in some of the Prophets of the OT). The phrase ἡμέρα Κυρίου is likewise part of OT eschatology referring to the day (as a time designation) when God will interpose on behalf of his people to deliver them. This day will bring salvation to God's people and destruction to his enemies. The prophets of the 8th and 7th century B.C., like Amos for example, preached that though the people longed for the 'day of the Lord' they would find themselves on the wrong side along with God's enemies — and *in this respect* this day will involve judgment and punishment. In post-exilic times, however, the positive connotation focussing upon redemption and deliverance prevailed. Though there might be a remote connection between 'day' and 'judgment', the foci in the quoted 'parallels' are so different from the meaning 'law court' that this possible connection is of no importance in understanding the usage of ἡμέρα in 1 Cor 4.3. Such a link *may be useful* in a study on 'change of meaning' which is an important aspect of historical linguistics. The question is: what does ἡμέρα contribute to the understanding of Paul's censure in 1 Cor 4.3? As such, the meaning 'law court' sufficiently indicates that Paul is not concerned with anyone's judgment about him, whether it be a judgment by an institutionalized entity such as a judicial court or by the unofficial evaluation of the general public. Notions such as those intimated by the parallels R suggests should not be read into 1 Cor 4.3 and therefore these parallels are not relevant to 1 Cor 4.3. They might be relevant in answering the question how it came about that a word like ἡμέρα could have developed a meaning 'law court' which seems to be far apart in semantic space from 'day'. If the expressions ἡμέρα κρίσεως and ἡμέρα Κυρίου

are taken as parallels to 1 Cor 4.3, one can easily fall into 'illegitimate totality transfer' as Barr has indicated in his book *The Semantics of Biblical Language* — a statement to which R subscribes in his very next paragraph without realizing that he has just suggested the same type of transer in his previous paragraph.

More remarkable is R's next objection (p. 99) against the argument, at the beginning of chapter 6 of *SNTG*, that the tradition of consistently translating certain particular Greek words in the NT by the same equivalent in the target language, for example, σάρξ by 'flesh' reflects the idea that 'flesh' is *the* basic meaning of σάρξ. He insists that 'flesh' is 'imbued with . . . OT tradition' and that the 'opposition σάρξ-πνεῦμα is of Hellenistic-Gnostic origin'. These notions are important to the NT, he advocates. He seems to prefer 'flesh' as the only translation since he maintains that such key-words 'have entered the linguistic heritage of Western literature through centuries of *literal* translations of the Bible'. And 'as in the Greek original so in the Latin *Vulgate* and in modern European languages, these terms have kept their *polysemy*' (p. 99). He then continues: 'Neither a perfect semantic analysis nor the best of "dynamic" translation theories can in each case presume to eliminate that polysemy without destroying important connotations.' This statment of his is remarkable since polysemy involves different *distinct* meanings. What is wrong, then, to render John 1.14 ὁ λόγος σάρξ ἐγένετο as 'the Word became a human being' instead of 'the Word became flesh'? Such a rendering merely indicates which of the *different* meanings of σάρξ (or 'flesh' for that matter) is at stake in Jn 1.14. If the connotations attached to the different meanings of σάρξ were to be taken over in Jn 1.14 we would indeed have a case of Barr's 'illegitimate totality transfer'. In Jn 1.14 σάρξ points connotatively to the frail mundane state the Word assumed — and this is what the translation 'human being' in this context suggests. How, then, can R ask: 'ought we, then, to ban from usage . . . the term "incarnation" '? The fact of the matter is that translating σάρξ by 'human being' is exactly expressing the notion involved in 'incarnation'. It is just saying explicitly what 'flesh' leaves implicit. It may also be sincerely doubted whether modern readers of the NT do have all the distinct meanings of σάρξ ready at hand when they read the NT. Usually they have some general feature in mind which they regard as the basis of all the occurrences of the term. For some people 'flesh', for example, has become generally associated with sin, for others 'flesh' pertains mainly to one's inner nature. They seldom realize that these notions are meanings that are applicable only to specific contexts. One, therefore, simply cannot rely on saying, as R does, that 'as in the Greek original so . . . these terms have kept their *polysemy*'.

The enigmatic expression in Lk 9.60 ἄφες τοὺς νεκροὺς θάψαι τοὺς ἑαυτῶν νεκρούς has usually been explained by reading the expression literally, though such a reading has never given acceptable sense to the passage within its discourse context. R prefers the traditional explication, taking the saying as 'a pun on the double meaning of νεκρός, "dead" in a *physical* and in a *spiritual* sense' (p. 99). One could do so, but it hardly makes sense. Reading the expression as an idiom

still seems to be more satisfactory.

R (p. 101) is strongly outspoken about the fact that whenever a reader of a text presumes to uncover an ancient author's (he could have merely said 'an author's') intention, we have a rather subjective paraphrase of what the modern reader (or merely 'reader' for that matter) presumes to understand. This is perfectly true, but is of no use in order to write off a particular methodology, since all methodologies — even those he himself subscribes to — are subjected to the same objection. What we all do is to understand a text the way we read it. Often people say — and R (p. 101) endorses this contention — that 'intelligent reading' uncovers anyhow what is to be understood. This is not true, since what is understood is still the result of how the reader has read. R's reading of the book he has reviewed clearly shows how inadequate his own 'intelligent reading' is. For example, he often quotes without paying attention to the context. Therefore he misreads the many passages that have already been referred to above. These can by multiplied. A few more may suffice to illustrate the point. On p. 105 he quotes the words '. . . the experiencer of the event as in βάπτισμα Ἰωάννου'. He then comments 'John is not the experiencer but the agent'. He refers to p. 81 of SNTG, but on that same page it is argued that βάπτισμα Ἰωάννου, which in Mt 21.25 is designated by the context as the implied agent, can have other meanings in other contexts since it is in itself an ambiguous phrase. Therefore the final sentence on p. 81 of SNTG reads 'it must be determined from the context whether the object is the agent of the event, or is affected by the event or is the experiencer of the event as in βάπτισμα Ἰωάννου above'. R's 'intelligent reading' did not help him to understand the argument. Had he only read the page with due attention to the discourse structure, he would have had no problem in understanding the author's intention. The same applies to his objection against translating ἐλάβομεν χάριν in Rom 1.5 (p. 84 of SNTG) as 'I received grace'. He insists on 'We' (p. 105). But the discourse shows that Paul is talking of himself. The plural is a stylistic issue. On p. 112 of SNTG ἀεί is used in a sentence along with ἐποίησε. R remarks that 'this implies the possibility of the Aorist expressing an habitual or repeated action in the past, a prerogative of the Imperfect' (p. 105). He did not notice that the feature of habitual or repeated action is signified by ἀεί, not by the aorist. On p. 15 of SNTG he found the Latin phrase 'quae nos ferens extra' unsuitable, saying: 'delete quae or change ferens to fert' (p. 104). He did not read the passage carefully. The phrase referred to is a quotation and not a phrase coined by the author of SNTG. Similarly the idiom δεξιὰς δίδωμι (Ga 2.9), for which he proposes δεξιὰν δίδωμι (he writes δεξιάν) or δεξιὰς διδόναι (p. 105), is the citation form. Traditionally the verb is cited in all lexicographical contexts in the first person singular of the present indicative. On p. 64 of SNTG the phrase is cited as an idiom (literally, 'to give right hands'; meaning, 'to make a covenant'), and idioms are lexical units. Therefore, they are treated in lexicography as single items. The infinitive, granted, may be more suitable, but no dictionary ever uses infinitives as citation forms. However, this is surely a lexicographical problem pertaining to all idioms. On the other hand, the

tradition (as in the Greek dictionary by Liddell, Scott, Jones and McKenzie), would give for δίδωμι English equivalents such as 'give freely, to be ready to give, offer' though 'to be ready to give' is rather διδόναι, while δίδωμι is more properly 'I am giving'. Therefore, lexicographers generally refer to those forms as citation forms.

On p. 96 of his review R has a word of accord: 'the method seems to be most suitable to elucidating obscure nominal constructions with an ambiguous genitive or prepositional phrase. Such constructions are common especially in St. Paul's letters.' He then quotes two examples, the first from Mark's Gospel (Mk 1.4). This example, namely βάπτισμα μετανοίας, does have a genitive construction, but it is not ambiguous. The second example (Php 1.5) has three prepositional phrases (ἐπὶ τῇ κοινωνίᾳ ὑμῶν εἰς τὸ εὐαγγέλιον ἀπὸ τῆς πρώτης ἡμέρας), and likewise they can hardly be called ambiguous. The first example may indeed be called obscure, but the second seems to be fairly clear. This type of argumentation of the reviewer is significant in that he seems to be unaware of inconsistencies in his own statements. This is even more apparent in his next sentence referring to the two examples: 'they are excellently explained and cleared of ambiguity by means of Nida's semantic classes (in spite of the inevitable elements of subjectivity involved in such classifications)'. His argument on p. 102 of his review is another case in question which, in addition, would seem to illustrate his lack of attention to detail. He strongly objects to the two possible immediate constituent analyses of the phrase ὁ δὲ δίκαιος ἐκ πίστεως ζήσεται (Rom 1.17), followed by *many* Bible translations, and resorts to accepting R.S. Hays' understanding of the phrase in Gal 3.11. He rejects the reading (namely linking ἐκ πίστεως with δίκαιος rather than with ζήσεται) by saying that the argument for this option in *SNTG* (p. 105) is unconvincing. Hays maintains that the issue is not the ambiguity, but the agential referent: 'by whose faith shall the righteous one live: his own faith or God's faithfulness?' To R this is a more feasible explanation, but he fails to see that 'faith' and 'faithfulness' are semantically different and that, therefore, Hays' explanation also involves an ambiguity. However, R concludes his comment by saying 'I do not wish to suggest that Hays' exegesis is the only correct one, but merely to show the inherent shortcomings of Louw's 'discourse analysis' as an *alternative* to exegesis' (p. 103). One can hardly believe that any reputable scholar would argue that since there is another possible explanation for the said phrase in Rom 1.17, this is proof of 'the shortcomings of discourse analysis as an *alternative* to exegesis.' Once again indifferent reading is apparent. Nowhere is discourse analysis ever propagated as an *alternative* to exegesis. Discourse analysis is a tool — and for that matter one among many other tools — that can be used in exegesis.

The reviewer's way of reasoning, and of formulating his arguments, is hampered by the fact that he is not particularly well-read in the linguistic literature of the past decades, especially not on NT Greek for which he, as a Latin scholar, cannot be blamed. His arguments are based on views held many years ago and for Greek this is clearly apparent from statements such as 'this implies the possibility of the

Aorist expressing an habitual or repeated action in the past, a prerogative of the Imperfect' (p. 105). In the context where the said aorist occurs, the adverb ἀεί (as discussed above) expresses the durative notion. Formerly aspectual notions were only linked to the *tense* forms of *verbs*, but the views on verbal aspects in Greek have changed considerably in recent years.[3] On the same page he also contests the statement that chiasmus is a popular form of style in the NT by resorting to a remark made in 1896 by Blass-Debrunner, that chiasmus seldom occurs in the NT. This remark has been carried over in subsequent editions of Blass-Debrunner's Grammar, though much has been published to the contrary in recent years on chiastic constructions in the NT. Though there is still much controversy on recognizing particular chiastic patterns, the fact of chiasmus in the NT is certain as can be easily seen from Angelico Di Marco's study of the literature on chiasmus in *Linguistica Biblica* 1976. The reviewer is also mistaken in his prejudice against discourse analysis by saying that the chiasm discussed on p. 120 of *SNTG* was already mentioned as one of the few in Blass-Debrunner and therefore not uncovered by discourse analysis. The fact is that it is not argued on p. 120 that the discourse discloses the chiasm, but rather that the chiasm, once it is appreciated, facilitates the problem of structuring the discourse of the passage under discussion.

Finally it is apparent from R's many objections against the English used in the book that he has failed to realize that the book *SNTG* was published in America and has followed American usage. Interesting in this respect is his statement that the expression used as an illustration on p. 42 of *SNTG* 'the hall was taxed to capacity' is an 'invented expression' for which the author of the book is held responsible (p. 104). But then, without any ado, he declares the expression as being a neologism from substitution by analogy by saying that it is 'hardly English and can, at best, be adduced to illustrate the carelessness of contemporary English usage'. In fact, this expression was taken from a report in one of the most respected American newspapers, and the author of *SNTG* at the time checked its meaning ('filled' or 'occupied') with various native speakers of the language — and amazingly no one thought it should be traced back to paying taxes to the government, which shows that users of a language are not so keen on 'origins' as philologists tend to be.

One can be thankful that Ronca has found some misprints and some real errors of reference in *SNTG* and these *errata et corrigenda* are very helpful though they are not crucial to the methodological question the reviewer has set himself to contest.

NOTES

1. 'Copernican revolution' is used here in the sense of an almost complete reversal of approach. The historical-comparative method of linguistic analysis, predominant in the nineteenth century (see i.a. R.H. Robins, *A Short History of Linguistics*, 1970, pp. 164–197) which is also the underlying theory of classical philology (see O.L.J. Szmerenyi in *The Oxford Classical Dictionary* s.v.

Philology), was basically diachronic with little or no interest in synchronic semantic issues. Hermann Paul's *Prinzipien der Sprachgeschichte*, first published in 1880, with its emphasis on the contention that only the historical approach to language can be regarded as adequate and truly scientific, already contained hints of changes to come (see E.F.K. Koerner 'Hermann Paul and Synchronic Linguists', *Lingua* 29 (1972) 274–307). Though the beginning of the twentienth century marks (especially since Ferdinand de Saussure) a change for synchronic semantics, and though scholars like Stern and Ullmann (quoted by the reviewer) did take some cognizance of the first hints at a synchronic approach, it was not before the 1960's that diachronic semantics were confined to the (pre)history of linguistic forms in studying how meanings changed. The reversal involved (to quote J. Lyon's *Introduction to Theoretical Linguistics*, 1968, 46) 'that historical considerations are irrelevant to the investigation of particular temporal "states" of language'. For an introduction to the issues involved see J. Lyons, *New Horizons in Linguistics*, 1970; D. Crystal, *Linguistics*, 1971 and G. Leech, *Semantics*, 1974 (new ed. 1981). More technical is G. Wotjak, *Untersuchungen zur Struktur der Bedeutung*, 1971; V. Raskin, *A Concise History of Linguistic Semantics* 1978, and J.D. Fodor, *Semantics: Theories in Generative Grammar*, 1977. During the 1960's and 1970's synchronic approaches became the dominant method of determining meaning in the analysis of a written or spoken linguistic text. Diachronic analysis, however, was by no means discarded, but restricted to questions other than those pursued in *SNTG*.

2. See K. Stenning, 'On why making reference out of sense makes it so hard to make sense of reference', *Linguistics* 1980, pp. 619–633; E.A. Nida, 'Meaning vs. Reference — The Touchstone of Lexicography', *Language Sciences* 1981, pp. 51–57.

3. See, among many others, H. Weinrich, *Tempus, besprochene und erzählte Welt* 1964; W.F. Bakker, *The Greek Imperative*, 1966 and 'A Remark on the Use of the Imperfect and Aorist in Herodotus', *Mnemosyne* 21 (1968) 22–28; P. Kiparsky, 'Tense and Mood in Indo-European Syntax', *Foundations of Language* 4 (1968) 30–57; S. Levin, 'Remarks on the 'Historical' Present and Comparable Phenomena of Syntax', *Foundations of Language* 5 (1969) 386–390; C.M.J. Sicking, 'Aspect problemen, een discussie', *Lampas* 7 (1974) 260–282; J. Mateos, *El Aspecto Verbal en el Nuevo Testamento* 1977; A.L. Lloyd, *Anatomy of the Verb*, 1979; D. Armstrong, 'The Ancient Greek Aorist as the Aspect of Countable Action' in *Syntax and Semantics* Vol. 14: *Tense and Aspect*, 1981, 1–12; C.S. Smith, 'A Theory of Aspectual Choice', *Language* 59 (1983) 479–82.

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