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THE MAJORITY TEXT AND THE ORIGINAL TEXT: A RESPONSE
Wilbur N. Pickering

Editor's Note: *Notes on Translation* No. 82 (3-81) calls attention to Fee's very critical review of Pickering's book. As Interim Editor of *Notes on Translation* and *Selected Technical Articles Related to Translation*, I felt it would be appropriate to afford Pickering the opportunity to respond to some of Fee's objections. See also the incisive review of Pickering's book by Tony Pope in *Selected Technical Articles Related to Translation* No. 2 (8-80).

The January 1980 (Vol. 31, No. 1) issue of *The Bible Translator* contains an article by G. D. Fee (pp. 107-118) which is a critique of my book, *The Identity of the New Testament Text* (Nelson, 1977). In the interest of the truth and fair play, I offer the following response.

Fee starts the critique proper (his third paragraph) with an indictment by innuendo. Since no specifics or documentation are given, defense is impossible.

"An Overview of the Argument"

The section thus entitled contains several infelicities. Fee begins by affirming that "from P's point of view, the great fault of contemporary NT textual criticism is that it cannot offer us total certainty as to the original NT text" (p. 108). Not at all. I used the question of "certainty" merely as an entrance to get into the arena. "The great fault of contemporary NT textual criticism" is that it is wrong—wrong in theory, wrong in method, wrong in results.

Hort's genealogical method "suffers" (p. 109) mainly for its inability to deal with mixture, and more especially from the circumstance that Hort simply did not apply it to the MSS. Neither he nor anyone else has produced genealogical trees which vindicate the conclusions that he and others have drawn from this supposed method. Fee himself has recognized this: "Properly speaking, genealogy must deal with the descent of manuscripts and must reconstruct stemmata for that descent. This Hort never did [!]; rather he applied the method to text-types, and he did so *not* [emphasis Fee's] to find the original text, but to eliminate the Byzantine manuscripts from further consideration."¹ (His footnote 4 contains more innuendo.)

Fee represents me as holding that "all of the MSS offer independent witness to the original text" (p. 109). I nowhere make such a statement. The vast majority of MSS are independent in their own generation. The crucial question is how far back we must go to find their common point of origin—it is the determining question for the science of New Testament textual criticism.

Fee closes the section with a reference to "first-hand" knowledge of the data. His statement involves the gratuitous assumption that the extant evidence from the earliest centuries is representative. It remains true, however, that any theory of New Testament textual criticism must account for the available evidence—I believe mine does, but as far as I can see, Hort's (or Fee's) does not.

"The Question of Methodology"

The section thus entitled contains further infelicities. Fee begins by speaking of my "'method'" as "the return to counting noses" (p. 109). In his

former critique (which was condensed for *TBT*), Fee is even more thoroughgoing: "His 'new' method for identifying the NT text is the wholesale adoption of Burgon's seven 'notes of truth,' all of which are simply seven different ways of saying that the majority is always right."² It should be apparent to the reader of chapter seven of my book, just at a glance, that Fee's statement is irresponsible.

Fee next charges that my "understanding of eclecticism...is hopelessly confused" (p. 109). He feels that I have not adequately distinguished between "rigorous" (my "pure") and "reasoned" eclecticism and have thereby given a distorted view of the latter. Well, he himself says of the reasoned eclecticism which he espouses, "Such eclecticism recognizes that W-H's view of things was essentially correct..."³ My statement is, "But most scholars do not practice pure eclecticism—they still work essentially within the W-H framework" (p. 28). Are the two statements really that different?

The fairness of this assessment may be illustrated from the works of both Fee and Metzger (whom Fee considers to be a practitioner of reasoned eclecticism). In his "Rigorous or Reasoned Eclecticism—Which?", Fee says, "Rational eclecticism agrees in principle that no MS or group of MSS has a *prima facie* priority to the original text."⁴ But on the next page, he says of Hort: "if his evaluation of B as 'neutral' was too high a regard for that MS, it does not alter his judgment that compared to all other MSS B *is* a superior witness." Metzger says on the one hand, "the only proper methodology is to examine the evidence for each variant impartially, with no predilections for or against any one type of text,"⁵ but on the other hand, "readings which are supported by only Koine, or Byzantine witnesses (Hort's Syrian group) may be set aside as almost certainly secondary."⁶

But Fee has more to say. "It is simply untrue, to the point of being nonsensical, to assert that Elliott's method is under 'the psychological grip of W-H' (p. 29)" (p. 110). In his former "Critique" (p. 401), he explains that Elliott and W-H are on opposite ends of the internal evidence/external evidence spectrum because "it is well known that W-H gave an extraordinary amount of weight to external evidence, just as do Pickering and Hodges." And yet, on another occasion, Fee himself wrote:

...it must be remembered that Hort did *not* use genealogy in order to discover the original NT text. Whether justified or not, Hort used genealogy solely to dispense with the Syrian (Byzantine) text. Once he has [sic] eliminated the Byzantines from serious consideration, his preference for the Neutral (Egyptian) MSS was based *strictly* on intrinsic and transcriptional probability [emphasis Fee's].⁷

And again: "In fact the very internal considerations for which Kilpatrick and Elliott argue as a basis for the recovery of the original text, Hort used *first* [emphasis Fee's] for the evaluation of the existing witnesses."⁸

It seems to me that these latter statements by Fee are clearly correct. Since Hort's preference for B and the "Neutral" text-type was based "strictly" on internal considerations, his subsequent use of that text-type cannot reasonably be called an appeal to external evidence. In sum, I see no essential difference between "rigorous" and "reasoned" eclecticism since the preference given to certain MSS and types by the "reasoned" eclecticians is itself derived from internal evidence, the same considerations employed by the "rigorous eclecticians. If my reasoning is correct, then Fee's remarks about my "confusion" and "errors" become

rather empty. It follows that, when Fee concedes that "the prevailing eclectic method, which lies behind UBS³, for example, is indeed the true offspring of W'H" (p. 109), he vindicates the organization of my book.

In fact, in the one paragraph of the whole review (paragraph 4, p. 110) which I accept as containing an adequate characterization of my book, Fee recognizes my main concern.

On the other hand, his real reason for attacking W-H is not methodological at all, and has little to do with their use of internal evidence and its subsequent influence on textual criticism. Rather his problem is almost altogether with W-H's textual theory, which allowed them to judge the Byzantine text-type as a secondary textual development. It is *this* influence of W-H on subsequent textual criticism that is the real reason behind P's attack.

Just so! Since my discussion of the W-H textual theory occupies almost half of my book, it seems strange that Fee does not give more attention to it. At the close of the review, he asserts that my book fails "to open up the discussion anew as to the value of the Byzantine text" (p. 118), but he has not justified that assertion.

"The Nature and Causes of Textual Variation"

The section thus entitled contains still more infelicities. It begins with a clear illustration of the extent to which Fee avoids my critique of the W-H theory. He has already informed us that "fully two-thirds of his book" (a bit of hyperbole) is devoted to discrediting W-H (p. 109). Now he asserts that "*his whole case* rests on a single assumption: that the transmission of the NT text was 'normal' (pp. 104-110) [emphasis Fee's]" (p. 110)—an assertion neither fair nor true.

Fee seriously distorts my position by ignoring my discussion of the *abnormal* transmission. It would appear that the distortion was deliberate since he cites my pp. 104-110 for the "normal" transmission, whereas pp. 107-110 contain my treatment of the abnormal transmission. Fee tries to make me appear inconsistent in that I criticize W-H for treating the NT like any other book and yet myself claim a "normal transmission" for the Majority Text. The crucial point is that I also recognize an "abnormal transmission", whereas W-H did not.

Next, Fee claims that I confuse "deliberate" and "dogmatic" changes, and in consequence my critique of Hort's foundation fails. In his own words, "The vast majority of textual corruptions, though deliberate, are *not* malicious, nor are they theologically motivated. And *since* they are not, P's view of 'normal' transmission (which is the crucial matter in his theory) simply disintegrates" (p. 113). Fee fastens upon my use of the term "malicious", which I use only in discussing the *abnormal* transmission. I nowhere say that a majority of variants are malicious. The clear testimony of the early Fathers indicates that some must be, and I continue to insist that Hort's theory cannot handle such variants.

But the distinction between "deliberate" and "theological" changes may properly detain us. On one occasion Colwell wrote, "the majority of the variant readings of the New Testament were created for theological or dogmatic reasons."⁹ But just five pages later he says, "in the manuscripts of the New Testament most variations, I believe, were made deliberately," without referring to theology. What is Colwell's real meaning? We may no longer ask him personally, but I will hazard the following interpretation on my own.

The MSS contain several hundred thousand variant readings. The vast majority of these are misspellings or other obvious errors due to carelessness or ignorance on the part of the copyists. As a sheer guess, I would say there are between 10,000 and 15,000 that cannot be so easily dismissed—i.e. a maximim of 5% of the variants are "significant". It is to this 5% that Colwell (and Kilpatrick, Scrivener, Zuntz, etc.) refers when he speaks of the "creation" of variant readings. A fair number of these are probably the result of accident also, but Colwell affirms, and I agree, that most of them were created deliberately.

But why would anyone bother to make deliberate changes in the text? Colwell answers, "because they were the religious treasure of the church". Some changes would be "well-intentioned"—many harmonizations presumably came about because a zealous copyist felt that a supposed discrepancy was an embarrassment to his high view of Scripture. The same is probably true of many philological changes. For instance, the plain Koine style of the New Testament writings was ridiculed by the pagan Celsus, among others. Although Origen defended the simplicity of the New Testament style, the space that he gave to the question indicates that it was a matter of some concern (*Against Celsus*, Book VI, chapters 1 and 2), so much so that there were probably those who altered the text to "improve" the style. Again, their motive would be embarrassment, deriving from a high view of Scripture. Surely Colwell is justified in saying that the motivation for such variants was theological, even though no obvious doctrinal axe is being ground.

To judge by the emphatic statements of the early Fathers, there were many other changes that were not "well intentioned". It seems clear that numerous variants existed in the second century which have not survived in any extant MS. Metzger refers to Gwilliam's detailed study of chapters 1 to 14 of Matthew in the Syriac Peshitta as reported in "The Place of the Peshitta Version in the Apparatus Criticus of the Greek N.T.", *Studia Biblica et Ecclesiastica* V, 1903, 187-237. From the fact that in thirty-one instances the Peshitta stands alone (in those chapters), Gwilliam concluded that its unknown author "revised an ient work by Greek MSS which have no representative now extant (p. 237)."¹⁰ In a personal communication, Peter J. Johnston, a member of the IGNT [*International Greek New Testament*] editorial panel working specifically with the Syriac Versions and Fathers, says of the Harklean Version: "Readings confidently referred to in the Harklean margin as in 'well-approved MSS at Alexandria' have sometimes not come down to us at all, or if they have, they are found only in medieval minuscule MSS."¹¹

The second century variants which did not survive may include many (most?) of the malicious ones. (If that is so, we may reasonably conclude that the early Christians were concerned and able watchdogs of the true text.) However, the fact of widespread deliberate variation (whether or not it was malicious or theologically motivated) undermines any *a priori* preference that might be given to a manuscript just because of its age. (If Codex B were an eighth century MS, I doubt that Hort would have written his *Introduction*, and if P⁷⁵ were a fourth century, I suspect that the present climate in New Testament textual criticism would be quite different.) As Colwell has so well put it, "the crucial question for early as for late witnesses is still, 'WHERE DO THEY FIT INTO A PLAUSIBLE RECONSTRUCTION OF THE HISTORY OF THE MANUSCRIPT TRADITION?'"¹² Hort's history has been exploded. The "process" view is frankly impossible. Does Fee have a plausible substitute?

Fee seems to feel that I am ignorant of the causes of corruption (!) and that my "unhistorical" notions about deliberate variation render me incapable of appreciating the canons of internal criticism. Well, setting aside the question of

theological motivation, what are the implications of Fee's admission that the vast majority of textual corruptions are "deliberate"? Can the canons of internal evidence really handle such variants? Since "harmonizations exist on every page of the Gospels" (p. 114), what about them?

Fee himself recognizes the possibility that supposed harmonizations may reasonably have other explanations.¹³ On the next page he recognizes another problem.

It should candidly be admitted that our predilections toward a given solution of the Synoptic Problem will sometimes affect textual decisions. Integrity should cause us also to admit to a certain amount of inevitable circular reasoning at times. A classic example of this point is the well-known 'minor agreement' between Matt. 26:67-8 and Luke 22:64 (// Mark 14:65) of the 'addition' *τίς ἐστὶν ὁ παῖς σου*. B. H. Streeter, G. D. Kilpatrick, and W. R. Farmer each resolve the textual problem of Mark in a different way. In each case, a given solution of the Synoptic Problem has affected the textual decision.

At this point, one could offer copious illustrations. Fee's debate with Kilpatrick over atticism demonstrates that possible philological changes are capable of contradictory interpretations on the part of scholars who both use internal evidence.

In sum, I reiterate that the canons of internal evidence cannot give us dependable interpretations with reference to deliberate variants. Those who use such canons are awash in a sea of speculation.¹⁴

I must agree with Fee that my discussion of harmonization in the first edition of my book was weak. The second edition contains a completely revised and much enlarged discussion as well as a new appendix on the subject.

I do not agree with his discussion of the early Fathers' attitude toward the New Testament text. It seems to me that he confuses citing with transcribing. The evidence he cites applies to the citations in the Father's works. In my own preaching, if I have occasion to cite a passage from Scripture more than once, I habitually vary the phrasing each time, for stylistic reasons if nothing more. But depending on the context and my purpose, my first reference to a text is often not an exact quote. If I were transcribing the text, however, preparing a copy for someone's use, I would take care to reproduce it exactly. I believe in the verbal plenary inspiration of Scripture, including its inerrancy, yet have no guilty conscience about my manner of citing it. I, at least, cannot impugn the Fathers' orthodoxy or responsibility in *transcribing* the text on the basis of how they cite it.

"Other Problems"

The section thus entitled contains *more* infelicities. Fee takes up "the question of text-types" (p. 115) but in fact says nothing about them. What he does discuss is the date of the "Byzantine" text-type. Both Hort and Kenyon clearly stated that no "strictly Syrian" *readings* existed before the end of, say, the third century. We may commend Fee for his prudent withdrawal to the weaker statement that it is "all of these readings together" that had no early existence, but we cannot commend him for attempting to water down Hort's position. But to get to the

basic question, the very phrase "strictly Syrian" is part of a larger question begging procedure. All the early Fathers and MSS are arbitrarily declared to be either "Alexandrian" or "Western"; and the witness they bear to "Byzantine" readings is disallowed, thus maintaining the presupposed lateness of the "Byzantine" text-type. May I respectfully submit that the generally accepted norms of scholarship do not permit the continued begging of the question of the provenance of the "Byzantine" text-type.

Among the numerous dubious affirmations with which Fee favors us, none is more startling than his charge that "Burgon's and Miller's data are simply replete with useless supporting evidence" (p. 116). Anyone who studies their works with care (as I have) will come away convinced that they were unusually thorough, careful, and scrupulous in their treatment of Patristic evidence. Not so Fee. Of the reading "vinegar" in Matt. 27:34, he says, "I took the trouble to check over three-quarters of Burgon's seventeen supporting Fathers and *not one of them* [emphasis Fee's] can be shown to be citing Matthew!" (Ibid.). (The term ὄξος 'vinegar' also occurs in the near-parallel passages—Mark 15:36, Luke 23:36, and John 19:29.)

Before checking the Fathers individually, we may register surprise at Fee's vehemence in view of his own affirmation that it is "incontrovertible" that "the Gospel of Matthew was the most cited and used of the Synoptic Gospels" and that "these data simply cannot be ignored in making textual decisions."¹⁵ We are grateful to Fee for this information but cannot help but notice that he himself seems to be "ignoring" it. We might reasonably assume that at least nine of Burgon's seventeen citations are from Matthew. But we are not reduced to such a weak proceeding.

Even though a Father may not say, "I am here quoting Matthew", by paying close attention to the context, we may be virtually as certain as if he had. Thus, although all four Gospels use the word "vinegar", only Matthew uses the word "gall", χολή, in association with the vinegar (and Acts 8:23 is the only other place in the New Testament that "gall" appears). It follows that any Patristic reference to vinegar and gall together can only be a citation based on Matthew (or Ps. 69:21). When Barnabas says, ποτιζειν χολην μετα οξος (7:5), can there be any doubt as to his source? When the Gospel of Peter says, Ποτισατε αυτον χολην μετα οξους (5:16), must the source not be Matthew? When Gregory of Nyssa says, χολη τε και οξει διαβροχος (*Orat.* x:989:6), can there be any question at all? It may be noted in passing that Alford's Greek New Testament, in *loc.*, says plainly that Origen and Tertullian both support the "Byzantine" reading under discussion.¹⁶

Note also that Irenaeus wrote, "He should have vinegar and gall given Him to drink" (*Against Heresies*, XXXIII:12), in a series of Old Testament prophecies that he says Christ fulfilled. Presumably he had Ps. 69:21 in mind—"they gave me gall for food, and in my thirst they gave me vinegar to drink"—but he seems to have assimilated to Matt. 27:34 (the "Byzantine" reading). The Gospel of Nicodemus has, "and gave him also to drink gall with vinegar" (Part II, 4). The Revelation of Esdras has, "Vinegar and gall did they give me to drink." The Apostolic Constitutions has, "they gave him vinegar to drink, mingled with gall" (V:3:14). Tertullian has, "and gall is mixed with vinegar" (Appendix, reply to Marcion, V:232). In a list of Christ's sufferings where the readers are exhorted to follow His example, Gregory Nazianzus has, "Taste gall for the taste's sake; drink vinegar" (*Oratio* XXXVIII:18).

Whatever interpretation the reader may wish to give to Fee's statement, noted at the outset, it is clear that the reading "vinegar" in Matt. 27:34 has second

century attestation (or perhaps even first century in the case of Barnabas!). The reading in question passes the "antiquity" test with flying colors.

Fee also informs us that "all of Burgon's data...is suspect because of his use of uncritical editions" (p. 116). (Fee notes my use of the term "quibble"—I used it in the context of the Miller-Kenyon debate as a reflection of Kenyon's own statement, but I recognize the importance of the question.) But might not an edition prepared by an editor with an anti-Byzantine bias also be suspect? Certainly a critical edition of Irenaeus prepared by Fee could not be trusted.

In discussing the evidence for "in the prophets" versus "in Isaiah the prophet" in Mark 1:2 ("A Critique", pp. 410-11), Fee does not mention Irenaeus under the Majority Text reading, where he belongs, but says "except for one citation in Irenaeus" under the other reading. He then offers the following comment in a footnote: "Since this one citation stands alone in all of the early Greek and Latin evidence, and since Irenaeus himself knows clearly the other text, this 'citation' is especially suspect of later corruption." He goes on to conclude his discussion of this passage by affirming that the longer reading is "the only reading known to every church Father who cites the text." By the end of his discussion Fee has completely suppressed the unwelcome testimony from Irenaeus. (Is it mere happenstance that, whereas UBS³ faithfully reports Irenaeus' support of the "Byzantine" reading, Nestle²⁶ leaves it out?)

But is the testimony of Irenaeus here really suspect? In *Adv. Haer.* III:10:5, we read:

Mark...does thus commence his Gospel narrative: "The beginning of the Gospel of Jesus Christ, the Son of God, as it is written in the prophets, Behold,... [the quotations follow]." Plainly does the commencement of the Gospel quote the words of the holy prophets, and point out Him...whom they confessed as God and Lord.

Note that Irenaeus not only quotes Mark 1:2 but comments upon it, and in both quote and comment he supports the "Byzantine" reading. But the comment is a little ways removed from the quote, and it is entirely improbable that a scribe should have molested the comment even if he felt called upon to change the quote. Fair play requires that this instance be loyally recorded as second century support for the "Byzantine" reading.

Another, almost as unambiguous, instance occurs in *Adv. Haer.* III:16:3 where we read:

Wherefore Mark also says: "The beginning of the Gospel of Jesus Christ, the Son of God; as it is written in the prophets." Knowing one and the same Son of God, Jesus Christ, who was announced by the prophets....

Note that again Irenaeus not only quotes Mark 1:2 but comments upon it, and in both quote and comment he supports the "Byzantine" reading.

There is also a clear allusion to Mark 1:2 in *Adv. Haer.* III:11:4 where we read:

By what God, then, was John, the forerunner...sent? Truly it was by Him...who also had promised by the prophets that He would send His messenger before the face of His Son, who should prepare His way....

May we not reasonably claim this as a *third* citation in support of the "Byzantine" reading? In any case, it is clear that Fee's handling of the evidence from Irenaeus is disappointing at best, if not reprehensible. Nestle²⁶ is also disappointing at this point.¹⁷

Fee closes the section with another bit of innuendo. He speaks of my "misrepresentations of the papyrus evidence" and says with reference to it that I have "grossly misinterpreted the data" (p. 117). I invite the reader to check the evidence presented by H. A. Sturz and then to decide for himself whether or not there has been misrepresentation and misinterpretation.¹⁸

"Conclusion—A Test Case"

The section thus entitled is consistent in quality with its predecessors. The fourth sentence (and fourth infelicity) reads like this: "The evidence that P's method renders him incapable of doing textual criticism is found in the fact that he offers only one example in the entire book as to how his method works in actual practice" (p. 117). Will the candid reader not agree that Fee's statement is a quintessential *non sequitur*? The first edition of my book contains *no* examples of how to do textual criticism (the second does contain a few) for the simple and sufficient reason that I chose not to include them. The "one example" Fee mentions was designed to illustrate the effects of the argument from probability, nothing more. Since he wishes to use 1 Tim. 3:16 as a "test case", however, I am delighted to oblige.

The readings, with their supporting MSS, are as follows:

- δ - D
- ω - 061
- δς θεος - one cursive and one lectionary
- δς - κ, 33, 442, 2127, three lectionaries
- θεος - A, Cvid, F/Gvid, K, L. P, Ψ, over 300 cursives and lectionaries
(including four cursives that read δ θεος and one lectionary that reads θεου).

It will be observed that my statement differs from that of the UBS text, for example. I offer the following explanation.

Young, Huish, Pearson, Fell, and Mill in the 17th century; Creyk, Bentley, Wotton, Wetstein, Bengel, Berriman, and Woide in the 18th; and Scrivner as late as 1881 all affirmed, upon careful inspection, that Codex A reads "God". For a thorough discussion please see Burgon, who says concerning Woide, "the learned and conscientious editor of the Codex declares that so late as 1765 he had seen traces of the θ which twenty years later (viz. in 1785) were visible to him no longer."¹⁹ It was only after 1765 that scholars started to question the reading of A (through fading and wear, the middle line of the *theta* is no longer discernible).

Hoskier devotes Appendix J of *A Full Account* (the appendix being a reprint of part of an article which appeared in the *Clergyman's Magazine* for Feb. 1887) to a careful discussion of the reading of Codex C. He spent three hours examining the passage in question in this MS (the MS itself) and adduces evidence that shows clearly, I believe, that the original reading of C is "God". He examined the surrounding context and observes, "The *contracting-bar* has often vanished completely (I believe, from a cursory examination, more often than not), but at other times it is plain and imposed in the same way as at 1 Tim. iii.16."²⁰

Codices F/G read \overline{OC} wherein the contracting-bar is a slanting stroke. It has been argued that the stroke represents the aspirate of δ , but Burgon demonstrates that the stroke in question never represents breathing but is invariably the sign of contraction. He affirms that " δ is *nowhere* else written \overline{OC} in either codex."²¹ Presumably the cross-line in the common parent had become too faint to see. As for cursive 365, Burgon conducted an exhaustive search for it. He not only failed to find it but could find no evidence that it had ever existed.²²

The three significant variants involved are represented in the ancient uncial MSS as follows: O, OC, and \overline{OC} , meaning "which", "who", and "God" respectively. In writing "God", a scribe's omitting of the two lines (through haste or momentary distraction) would result in "who". Codices A, C, F, and G have numerous instances where either the cross-line or the contracting-bar is no longer discernible (either the original line has faded to the point of being invisible or the scribe may have failed to write it in the first place). For both lines to fade away, as in Codex A here, is presumably an infrequent event. For a scribe to inadvertently omit both lines would presumably also be an infrequent event, but it must have happened at least once, probably early in the second century and in circumstances which produced a wide ranging effect.

The collocation "the mystery...who" is even more pathologic in Greek than it is in English. It was thus inevitable, once such a reading came into existence and became known, that remedial action would be attempted. Accordingly, the first reading above, "the mystery...which", is generally regarded as an attempt to make the difficult reading intelligible. But it must have been an early development, for it completely dominates the Latin tradition, both versions and Fathers, as well as being the probable reading of the SyrP and Coptic versions. It is found in only one Greek MS, Codex D, and in no Greek Father before the fifth century.

Most modern scholars regard "God" as a separate therapeutic response to the difficult reading. Although it dominates the Greek MSS (97%), it is certainly attested by only two versions, the Georgian and Slavonic (both late). But it also dominates the Greek Fathers. Around A.D. 100, there are possible allusions in Barnabas, "Ἰησοῦς...ὁ υἱὸς τοῦ θεοῦ τυπῶ καὶ ἐν σαρκὶ φανερωθεὶς" (Cap. xii), and in Ignatius, "θεοῦ ἀνθρωπίνως φανερούμενου" (*Ad Ephes.* c. 19), and "ἐν σαρκὶ γενομένος θεός" (*Ibid.*, c. 7). In the third century, there seem to be clear references in Hippolytus, "θεὸς ἐν σωματι ἐφανερώθη" (*Contra Haeresim Noeti*, c. xvii), Dionysius, "θεὸς γὰρ ἐφανερώθη ἐν σαρκὶ" (*Concilia*, i. 853a), and Gregory Thaumaturgus, "καὶ ἐστὶν θεὸς ἀληθινὸς ὁ ἀσάρκῳ ἐν σαρκὶ φανερωθεὶς" (quoted by Photius). In the fourth century, there are clear quotes or references in Gregory of Nyssa (22 times), Gregory of Nazianzus, Didymus of Alexandria, Diodorus, the Apostolic Constitutions, and Chrysostom, followed by Cyril of Alexandria, Theodoret, and Euthalius in the fifth century, and so on.²³

As for the grammatically aberrant reading, "who", aside from the MSS already cited, the earliest version that clearly supports it is the Gothic (4th century). To get a clear Greek patristic witness to this reading pretty well requires the sequence *μυστήριον ὃς ἐφανερώθη* since, after any reference to Christ, Savior, Son of God, etc. in the prior context, the use of a relative clause is predictable. Burgon affirmed that he was aware of no such testimony (and his knowledge of the subject has probably never been equaled).²⁴

It thus appears that the "Western" and "Byzantine" readings have earlier attestation than does the "Alexandrian". Yet, if "which" was caused by "who", then the latter must be older. The reading "who" is admittedly the most difficult,

so much so that to apply the "harder reading" canon in the face of an easy transcriptional explanation for the difficult reading seems unreasonable. As Burgon so well put it:

I trust we are at least agreed that the maxim "*proclivi lectioni praestat ardua*", does not enunciate so foolish a proposition as that in choosing between two or more conflicting readings, we are to prefer *that* one which has the feeblest external attestation,—provided it be but in itself almost unintelligible?²⁵

As for Fee's discussion, I would say that it is characterized by hyperbole throughout. His statement of the "internal evidence" is gratuitous. What objective evidence is there to show that Phil. 2:6 or Col. 1:13 or 15 (or 1 Tim. 3:16, for that matter) were hymns? And how can he say, "in the latter case without an antecedent!" (p. 118)? The antecedent of the relative pronoun in Col. 1:15 is "the son" in v. 13, and the antecedent of the relative in v. 13 is "the father" in v. 12. Fee closes his treatment of the "test case" as follows: "The text 'he who' clearly refers to Christ, and *all* the Christological import is there in the original" (p. 118). I would say that the relative pronoun "who" does not "clearly refer" to anything—of course Fee is at liberty to suppose that it refers to Christ, but I seriously doubt that an uninitiated person would reach the same conclusion. Further, "*all*" the import is not there—the witness of this verse to the deity of Christ is seriously weakened if we read "who" instead of "God"; indeed, a naive reader could not reasonably be criticized if he missed the point entirely.

In conclusion, Fee informs us that "P's book fails on all counts" (p. 118). I would urge any concerned person to read my book with care and form his own judgment.

FOOTNOTES

¹"Modern Text Criticism and the Synoptic Problem", J. J. Griesbach: *Synoptic and Text-Critical Studies 1776-1976*, ed. B. Orchard and T. R. W. Longstaff, Cambridge: University Press, 1978, 155-6.

²"A Critique of W. N. Pickering's *The Identity of the New Testament Text: A Review Article*", *The Westminster Theological Journal* 41 (Spring, 1979) 423.

³*Ibid.*, p. 402.

⁴*Studies in New Testament Language and Text*, ed. J. K. Elliott, Leiden: Brill, 1976, 179.

⁵*Chapters in the History of New Testament Textual Criticism*, Grand Rapids: Wm. B. Eerdmans, 1963, 39.

⁶*The Text of the New Testament*, London: Oxford University Press, 1964, 212.

⁷"Rigorous", p. 177.

⁸*Ibid.*, p. 179.

⁹*What is the Best New Testament?*, Chicago: The University of Chicago Press, 1952, p. 53.

¹⁰*The Early Versions of the New Testament*, Oxford: Clarendon, 1977, 61.

- ¹¹This paragraph and the preceding two are taken from footnote 5 to chapter 4 of the second edition (revised and enlarged) of my book (Thomas Nelson, 1980).
- ¹²"Hort Redivivus: A Plea and a Program", *Studies in Methodology in Textual Criticism of the New Testament*, Leiden: Brill, 1969, 157.
- ¹³"Modern Text Criticism", p. 162.
- ¹⁴I would say that the "more than five hundred changes" (p. viii) introduced into UBS³ as compared with the second edition afford a clear vindication of my contention. Although UBS³ is dated 1975, Metzger's *Commentary* upon it appeared in 1971. The second edition is dated 1968. It thus appears that in the space of three years, with no significant accretion of new evidence, the same group of five scholars changed their mind in over 500 places. It is hard to quell the suspicion that they are guessing.
- ¹⁵"A Critique", p. 412.
- ¹⁶The research reflected in the discussion above was done by Maurice A. Robinson and kindly placed at my disposal.
- ¹⁷The three quotations from Irenaeus are taken from A. Roberts and J. Donaldson, eds., *The Ante-Nicene Fathers*, 1973, Vol. I, 425-6 and 441, and were checked for accuracy against the critical editions of the *Sources Chrétiennes* series (Vols. 34 and 211, edited respectively by F. Sagnard [1952] and A. Rousseau and L. Doutreleau [1974], and published by Editions du Cerf, Paris). I own this material on Irenaeus to Maurice Robinson—it is part of a much fuller response to Fee which he has submitted for publication.
- ¹⁸*The Byzantine Text-Type and New Testament Textual Criticism*, La Mirada, CA: Biola College Bookstore, 1972.
- ¹⁹*The Revision Revised*, London: John Murray, 1883, 434. Cf. 431-6.
- ²⁰*A Full Account and Collation of the Cursive Codex Evangelium 604*, London: David Nutt, 1890, Appendix J, 2. See also Burgon, 437-8.
- ²¹p. 442. Cf. 438-42.
- ²²p. 444-5.
- ²³Burgon, 456-76, 486-90.
- ²⁴p. 483.
- ²⁵p. 497. The above discussion is taken from the second edition of my book, footnote 32 to chapter 5.

SEMITIC ΚΑΙ AND GREEK ΔΕ
Randall Buth

A number of pages in this publication have recently been devoted to the significance and difference between καί and δε. The articles have been stimulating and

at the same time show how much needs to be done and how unresolved some of the issues are.

I would like to resurvey some of these issues so that they will get proper attention in future studies over a wider scope of material. (In fact, one of the issues is just what material to include.)

First of all, the parameters used by Levinsohn, Callow, and Hollenbach are admittedly different and need further work. In describing why $\delta\epsilon$ is used in some verse or other, Levinsohn might speak of *development*, where Callow would talk of a *change of participant*, while Hollenbach suggests a *change of paragraph* or a *function of tense* (i.e. $\delta\epsilon$ with the imperfect). Hollenbach went so far as to reject both the change-of-participant parameter and the hope of attributing a single summary feature for $\delta\epsilon$ like +DEVELOPMENT.

After looking at the different analyses, one might attribute a common feature of +DIFFERENT or +CHANGE to $\delta\epsilon$. The difference could include change of participant, change of paragraph or episode unit, change of theme line (from theme to background and vice versa) and a change (reversal of) expectation (in a limited binary frame this would be called contrast). This could be extended to include change of tense (e.g. aorist to imperfect) if such a parameter were needed. (I suspect that the examples listed by Hollenbach stem from a coincidence that some of the same factors that cause the imperfect to be used also cause the use of $\delta\epsilon$, e.g., background versus theme. Some of the examples from Acts will be discussed later in the paper.

Another issue, reflected in the title of the paper, is the foreign language influence on New Testament Greek. The distinction between $\kappa\alpha\iota$ and $\delta\epsilon$ is specifically Greek; neither Hebrew nor Aramaic have two words for "and", just ו . (This ignores the Hebrew distinction of -ו versus -וּ and the Syriac and Christian Aramaic assimilation of וִין and וִי to Greek $\delta\epsilon$.) The Semitic background, particularly in the Gospels and Acts, is a problem because of the tendency to use $\kappa\alpha\iota$ in Greek documents translated from Hebrew. $\delta\epsilon$ is very much a "marked" relator when Hebrew underlies a Greek document. It takes a clearer purpose or higher threshold to get $\delta\epsilon$ into translation Greek than into a natural, idiomatic document.

If one assumes that Jesus often taught in Hebrew (and/or Aramaic) and that much of the tradition was first written in Hebrew (and/or Aramaic), as contemporary parallels like the Dead Sea Scrolls and many linguistic aspects of the Gospels and Acts suggest, then there is a high probability that there are more $\kappa\alpha\iota$'s in our present documents than Greek discourse parameters would dictate. For example, if Luke used some Greek sources which went back to Hebrew originals, the $\delta\epsilon$'s in the text are the result of both Luke's and previous translators' sensitivity to Greek. Generally, the ו 's would become $\kappa\alpha\iota$'s unless the author or translator felt a need to intervene.

Now this does not leave us in as bad a position as one might fear. It means that rules must first be written for $\delta\epsilon$ and then $\kappa\alpha\iota$.¹ If there are places where the rules predict $\delta\epsilon$ but $\kappa\alpha\iota$ is found, one does not need to speak of a marked, emphatic $\kappa\alpha\iota$ but he can invoke "Semitic $\kappa\alpha\iota$ ". Of course, it is recognized that this can provide too easy a solution, a solution where any contradiction to $\delta\epsilon$ rules can be glibly written off. A partial answer to this problem is to recognize that the higher ratio of $\delta\epsilon$: $\kappa\alpha\iota$ in a document means a much smaller percentage of Semitic $\kappa\alpha\iota$'s allowable.

Some good examples of Semitic $\kappa\alpha\iota$ occur in Acts 2:1 and Marinus Wiering's composition of "Hickle and Pickle".² $\kappa\alpha\iota$ is *not* a normal Greek conjunction for beginning new episodes or paragraphs. Bruce Hollenbach was on the right track when he asked whether $\kappa\alpha\iota$ ever began paragraphs (START No. 1, p. 12). In the New Testament, one might quickly have countered, "All the time!" But generally in Greek literature $\kappa\alpha\iota$ at the beginning of a paragraph or episode is very much a marked form. (Of course, it does occur, but several times in a *book*, not several times on one *page*. For example, of twenty-two sections, only section VI in the *Martyrdom of Polycarp* (c. 160 AD) begins with $\kappa\alpha\iota$. This emphasizes the *continuity* of the circumstances at that point.)

Semitic $\kappa\alpha\iota$ is not limited to translation from Semitic sources. Acts 2:1 *may* go back to a written Semitic source. Marinus Wiering certainly does not.³ Two nontranslation subdivisions of Semitic $\kappa\alpha\iota$ can be made: Septuagintal and Jewish. If the rules turn out to be regular, over more than one author, one might talk of a Jewish-Greek $\kappa\alpha\iota$, while irregularities, if not attributable to sources, might be Septuagintal, i.e. in mimicry of Septuagintal style though not part of the author's natural style. A good example of a regular feature that could be termed Jewish-Greek is $\kappa\alpha\iota$ with $\iota\delta\omicron\upsilon$ 'behold'. It has become a frozen collocation in New Testament Greek via the Septuagint and Hebrew. In Hebrew, ו is so frequently appended to הנה 'behold' (particularly in narrative) that the two were thought of as a unit and $\kappa\alpha\iota$, which also functions at the Greek word level, became the collocate of $\iota\delta\omicron\upsilon$. This is almost a grammatical necessity when והנה 'and behold' introduces the complement of a verb like 'see' ('he saw and behold...'. Cf. Gen. 1:31, 18:2).⁴

Differences in ratio of $\delta\epsilon:\kappa\alpha\iota$ between authors may be quite significant when Semitic sources are included in the perspective. A number of statistical studies have already been made along this line. The figures given by R. A. Martin as quoted by N. Turner (Moulton, *A Grammar of New Testament Greek*, Vol. 4, *Style*, pp. 57-58) are suggestive:

<u>Source</u>	<u>Ratio of $\delta\epsilon$ to $\kappa\alpha\iota$</u>
Plutarch	1:0.24
Josephus	1:0.3
Philostratus	1:0.4
Acts ('We' sections)	1:0.5
Epictetus	1:0.6
II Acts (17-19)	1:0.6
Paul (1 Cor.)	1:0.6
Papyri selections	1:0.92
I Acts (1-15)	1:1
4 Maccabees	1:1
Luke (parallel to Mark 8:4-9:50)	1:1.2
Luke (unique pericopae)	1:1.4
Matthew	1:1.5
Lukan 'Q'	1:1.9
Testament of Abraham (rec. A)	1:2
Genesis	1:2.4
Mark	1:5
Luke (1:5-2:52)	1:5
LXX Minor Prophets	1:26
Ezekiel a	1:63
Revelation 4-21	1:73
Judges (A text)	1:93 (A translator who marched to a different tune!)

Turner would emphasize that all of Acts and most of Luke are paralleled in Greek sources. More significant are the changes of ratio in Luke's writings parallel to the intrinsic probability of Hebrew (or possibly Aramaic) sources.⁵ The 'we' sections and the second half of Acts have ratios expected of literary Koine. The first half of Acts and, even more so, the Gospel reflect more Semitic Greek (from sources, Septuagint influence, and/or a Jewish-Greek dialect; proof is not available at this stage).

These factors lead one to expect Semitic καὶ to be a factor in any discourse analysis of the Gospels and Acts, including the sections dealt with by Levinsohn, Callow, and Hollenbach. John Werner's suggestion (NOT (Complimentary Issue), 1-1979, p. 36)—that comparison studies be made with contemporary literature—becomes an imperative.⁶

Another fruitful area of investigation is the Septuagint itself. The 'markedness' of δε stands out in a book like Genesis with its 1:2.4 ratio. One can assume that the translator was conscious of a particular reason for inserting δε where he did.

A survey of Genesis 1-9 revealed a number of probable factors that could have been active in the third century BC:

New episodes: 3:1; 4:1; 6:9; 10:1

New paragraphs: 3:17; 4:18,23,25; 5:6; 6:5,11; 7:6 (with same subject); 7:19; 9:18,24,28; (in speech 6:16,17,18,21)

Paragraph climax: 3:17; (6:17; 9:17 within speech)

??Paragraph final: 2:14; 3:17; 4:16 (possibly paragraph initial); 6:8; 8:14.
(Verse 14 of chapter 2 ends a series of names. The others have better explanations.)

Parenthesis: 2:10; 6:4 (This is significant because there is no 'and' in the Hebrew text!)

Background: 1:2; 2:6,12,(17); 5:4; 8:5,14; 9:18

?Negative Response: 4:9 (Perhaps the lie is contraexpectation.)

?New participant: 6:2 (A better analysis is as contrast of sons of God with daughters of men, or as following an introductory setting.)

Binary contrast: (2:17); (3:3); 4:2 (chiasmus), 5 (chiasmus), (7), 22, (24); 6:2, (3), 8; 7:2 (twice, and parallel to two following καὶ's); 8:5b,14

Following an introduction: 1:2; 2:6; 4:26 (rather short); 5:3; 6:2?, 10; 7:7
(Verses 1:2 and 2:6 could be viewed as continuing background material with imperfect verbs; the others are a return to a main event line.)

Corresponding to Hebrew 'and'-subject-verb: 1:2; 2:6,10,12; 3:1; 4:1; 6:4 (no 'and'), 8; 7:6 (time predication), 19; 8:5 There are two spots where δε could have been used: 7:6b,10. In both of these cases, a time clause precedes the and-subject-verb construction.

Some observations on the Genesis material can be made:

1. A rule of δε with the imperfect is not needed as long as one includes a background/theme line contrast in the analysis. Genesis 7:18,19 is interesting because the imperfect verbs are not introduced with δε until it corresponds to and-subject-verb in Hebrew.

2. The relative infrequency of $\delta\acute{\epsilon}$ suggests that it sometimes marks paragraph changes rather than participant changes, since paragraph changes are more infrequent than participant changes. If a boundary introduction (i.e. episode or paragraph initial) is posited for $\delta\acute{\epsilon}$, there is no need for participant change to be counted as a factor here in Genesis. Now, if $\delta\acute{\epsilon}$ functions as a marker of participant change in first century Greek, then this may be a frequent area where Semitic $\kappa\alpha\iota$ protrudes in the New Testament.⁸
3. The apparent inconsistency in applying $\delta\acute{\epsilon}$ to episode and paragraph boundaries in Genesis provides a prospective area for finding similar phenomena in the New Testament. In other words, if Semitic $\kappa\alpha\iota$ is common in Genesis at episode and paragraph borders, it may commonly appear in the New Testament.

Having argued that Semitic $\kappa\alpha\iota$ should exist in the New Testament as a discourse factor, it remains to point out some possible examples in the material covered by the other articles (Acts 5:17-27 and 10:9-33) (references in parentheses deal with $\delta\acute{\epsilon}$):

Acts 5:17-27

(5:17) The fact that there are three finite verbs in one sentence (5:17,18) is not the concern of this paper, though it is a Semitic feature. The same holds true for the secondary subject $\kappa\alpha\iota\ \pi\acute{\alpha}\nu\tau\epsilon\varsigma\ \omicron\iota\ \sigma\acute{\upsilon}\nu\ \alpha\acute{\upsilon}\tau\omicron\iota\varsigma$ occurring with the singular verb. The linking $\kappa\alpha\iota$ between finite verbs would drop out if Greek used a participle, a style secular historians prefer. Consequently, some linking $\kappa\alpha\iota$'s are probably a lower-level Semitic $\kappa\alpha\iota$, but it is better to deal with them within the finite-verb/participle question.

(5:21b) The Genesis study would add support to Hollenbach's suggestion of paragraph break.

(5:22b) A binary contrast usage of $\delta\acute{\epsilon}$ is probably in focus here. However, the $\delta\acute{\epsilon}$ may mark a new sentence, a more literary usage.

5:27 This *might* be Semitic $\kappa\alpha\iota$. There is a change of participant and probably a change of sentence. However, I accept Callow's suggestion that the participants are being viewed as belonging to the same party in the struggle. It would help to know how frequent such a usage is outside the New Testament.

Acts 10:9-33

The $\delta\acute{\epsilon}:\kappa\alpha\iota$ ratio for this section is 1:1.5 (at the head of clauses). Assuming the clausal figures to approximate Turner's, we would be less skeptical of a few Semitic $\kappa\alpha\iota$'s in this passage.

(10:10a) The $\delta\acute{\epsilon}$ is probably signaling background. Notice the following imperfect $\eta\theta\epsilon\lambda\epsilon\nu$. (Hollenbach's 'aorist' tense is, in fact, an imperfect.)

(10:10b) This $\delta\acute{\epsilon}$ returns to the main line.

10:13 Most probably Semitic $\kappa\alpha\iota$. This is certainly a new participant. Notice 'Lord' in verse 14 and remember the Jewish proclivity to speak of the 'word' or 'voice' of the Lord 'happening', "there came a voice".

10:15 This is another Semitic $\kappa\alpha\iota$. $\kappa\omicron\iota\lambda\ (בַּת) \kappa\omicron\iota\lambda\ \text{עוד שבית אלין}$.⁹ (The lack of the Greek

article may suggest Bat Qol 'the Divine Echo'.)

(10:16) Hollenbach's translation should be revised. A better rendition would be "This happened three times", which would still fit with the imperfect aspect in Greek. Cf. Acts 19:10 for the idiom. In 10:16 the clause serves as a summary-background since two of the occurrences had already happened.

(10:27) Same participant—Callow is probably right. Hollenbach's tense patterns are unconvincing. Too many other examples have to be removed before his tense pattern emerges.

10:30 Semitic καὶ. This would remove the problem for Levinsohn, Callow, and Hollenbach.¹⁰

10:30b-31a (in speech) These two καὶ's are probably Semitic (as well as the parallelism in 31b). (The second καὶ is in lieu of a participle construction.)

If the five suggested Semitic καὶ's were changed to four δε's, the ratio for 10:9-33 would become 1:0.94—more in line with secular standards. Admittedly, proposing a Semitic καὶ wherever one meets a problem is a cop-out, but a cop-out which corresponds with the linguistic background of the Gospels and Acts.

Where does all this leave us?

1. Different parts of Acts and each Gospel will need separate study and separate rules.
2. The uses of δε and καὶ (and τε and τότε) *must* be studied in contemporary sources outside the New Testament. Septuagint studies will provide an additional perspective.
3. The Semitic influences on the discourse structures must be taken into account.
4. Somewhere down the road these studies will give us a much clearer view of many of the literary questions which have engaged scholars over the last two centuries.
5. The regular functions of δε in Acts include:
 - a. episode initial
 - b. paragraph initial
 - c. background
 - d. return to theme
 - e. contrast
 and probably:
 - f. change of participant (particularly after comparison with Mark)
 - g. climax
 and less probably:
 - h. sentence initial (in more literary style)
 - i. imperfect tense
6. δε has a feature of + DIFFERENT/CHANGE.

FOOTNOTES

¹Wiering was on the right track when he labeled καὶ as 'occurring elsewhere'. Mark is quite restrictive in his use of δε and fairly consistent. However, regular Greek usages crop up besides his three rules (participant change, parenthesis, participant introduction with 'to be'), e.g., paragraph initial at 15:16. And the rather frequent καὶ sometimes invades even Mark's limited sphere of δε, e.g.,

15:34 (new subject), 15:35 (new subject and background), 15:38 (background/off-stage).

²Levinsohn's comments on 2:1 in NOT (Complimentary Issue), 1-1979, p. 2, are not convincing. Hollenbach recognizes that something is amiss (START #1, p. 10).

³Wiering casually assumes something of what I am trying to describe: "Mark's first language was Aramaic and therefore his writings would reflect the discourse patterns of that language." (START #1, p. 2). Actually, I find more Hebrew influence in the discourse structure than Aramaic. The frequency of $\delta\acute{\epsilon}$ and $\kappa\alpha\iota$ reflects Hebrew, not Aramaic. Also, the narrative use of $\tau\acute{o}\tau\epsilon$ (an Aramaism) is completely missing. (See my paper, "Perspective in Gospel Discourse Studies" to be published in START #6 for a discussion of $\tau\acute{o}\tau\epsilon$.)

⁴The phonetics of $\iota\delta\omicron\upsilon\delta\acute{\epsilon}$ did not sound offensive to Greek ears (contra Hollenbach, START #1, p. 11); the sequence occurs often in a phrase like $\iota\delta\omicron\upsilon\delta\acute{\epsilon}\delta\omega\mu\alpha\iota$ 'behold I have given', and $\iota\delta\omicron\upsilon\delta\acute{\epsilon}$ itself also occurs in 3 Maccabees 2:13 $\iota\delta\omicron\upsilon\delta\epsilon\ \nu\upsilon\upsilon$.

⁵The infancy account is an anomaly and possibly constituted a separate source. See note 3 for a comment on Hebrew versus Aramaic.

⁶The *Martyrdom of Polycarp* can be classed in the same discourse genre as Acts (a long narrative [though *Martyrdom of Polycarp* is embedded in an epistle]). It would make a better comparison than Epictetus or the Didache, despite the time difference.

⁷This particular Hebrew construction regularly begins paragraphs or shows contrast and chiasmic grouping. It is not necessarily simultaneous in time or background information, though often that is the case. (For example, Genesis 4:1 and 4:17 have 'sleep with', which is neither background nor simultaneous, though only 4:1 uses and-subject-verb.)

Is the Greek $\delta\acute{\epsilon}$ coincidentally marking the same discourse functions, or is it marking a Hebrew surface structure? The contrast of 4:1 and 4:17 in both Hebrew and Greek suggests that this particular Hebrew structure is being echoed in the Greek with $\delta\acute{\epsilon}$. Consistency in the Abraham narratives confirms this.

⁸This does not say that Semitic sources to the New Testament must cause such a phenomenon, as though to assume that the same factors affecting the Genesis translator affected the Gospel transmission. In fact, a good translator could produce idiomatic $\delta\acute{\epsilon}$'s, if that was his aim. Look at Josephus' *Jewish War*. On the other hand, an individual author (e.g., Mark) may choose mainly the parameter of participant change for his $\delta\acute{\epsilon}$. However, Septuagintal factors need to be kept in mind.

⁹Comparing Genesis 15:4, 2 Samuel 14:29, and Haggai 2:20 in Hebrew, Aramaic, and Greek, leads one to conclude that Acts 10:15 is a Hebraism, not a Septuagintalism, and likely not an Aramaism.

Genesis 15:4
Septuagint
Onkelos
Samaritan Targum

והנה דבר יהוה אליו לאטר
 $\kappa\alpha\iota\ \epsilon\upsilon\theta\upsilon\varsigma\ \phi\omega\nu\eta\ \kappa\upsilon\rho\acute{\iota}\omicron\upsilon\varsigma\ \epsilon\gamma\acute{\epsilon}\nu\epsilon\tau\omicron\ \pi\rho\acute{o}\varsigma\ \alpha\upsilon\tau\omicron\varsigma$
והא פתגמא דיהוה עטיה לטיטר
והא טגלל (verb) יהוה לה

2 Samuel 14:29
Septuagint

וישלח עוד שנית
 $\kappa\alpha\iota\ \acute{\alpha}\pi\epsilon\sigma\tau\epsilon\iota\lambda\epsilon\nu\ \epsilon\kappa\ \delta\epsilon\upsilon\tau\acute{\epsilon}\rho\omicron\upsilon\ \pi\rho\omicron\varsigma\ \alpha\upsilon\tau\omicron\upsilon$

(The Septuagint does not ever use the double adverb $\acute{\alpha}\lambda\lambda\omicron\ \epsilon\kappa\ \delta\epsilon\upsilon\tau\acute{\epsilon}\rho\omicron\upsilon$.)

Haggai 2:20

Jonathan

ויהי דבר יהוה שנית אל חגי

והוה פתגם נבואה טן קדם ל"י תנינות עם חגי

(The preposition עם means 'with', though Greek πρὸς would cover over this difference between Aramaic and Hebrew.)

The missing ἐγένετο in Acts 10:15 is explainable from Hebrew. Cf. Genesis 15:4 where the Septuagint inserts it. (Cf. also Matt. 3:17 and Mark 1:11.)

¹⁰Semitic καὶ itself raises additional questions. If Luke had a source to Acts 10, why did he let certain καὶ's slip through, as it were? (The same question should be asked of any Septuagint translation.) There is a very good reason at 10:13 and 10:15: the subject is God, and the 'holy' Septuagintal style might be more acceptable here. The same could be said at 10:30b-31 where the angel is introduced. In 10:30a, there would be more of a problem. Perhaps this is an example of 'higher threshold' for δε. Δέ may not have been an important stylistic consideration for Luke at this point, so he let it through. Also, καὶ would not contradict Septuagint style.

If Luke were composing chapter 10 as a completely free composition, can there be such a thing as Semitic καὶ? The answer is a guarded 'yes'. There may have been a Jewish-Greek dialect or a Septuagintal style with a rule like: "When in doubt or if you pause, insert καὶ and keep writing/talking." Still, that would not explain something like nine successive καὶ's in the Western and Caesarean texts at Luke 4:1-13. At the present, I suspect that a Hebrew source does underlie Acts 10.

SOME NOTES ON SENTENCE LEVEL ΚΑΙ IN THE *MARTYRDOM OF POLYCARP* Randall Buth

These notes serve as a follow-up to some issues raised in "Semitic καὶ and Greek Δέ". I would like to compare some statistics of δε:καὶ in the *Martyrdom of Polycarp* with Acts. I would also like to point out some interesting uses of καὶ in the *Martyrdom*.

The use of statistics in discourse studies cannot give answers, but it can be a helpful guide within limits. In "Semitic καὶ and Greek Δέ", ratios for δε:καὶ were quoted from Turner over a large literature. Turner apparently used Acts 3-5 as a comparison base for his "I Acts" (Acts 1-15) figures. Some judgment needs to be exercised in excluding irrelevant καὶ's so that figures by different people may differ. I counted all clausal level καὶ's in Acts 3-5 except those joining two immediate participles or infinitives without any additional terms. My figures differed slightly from Turner's:

		<u>δε</u>	<u>καὶ</u>	<u>δε:καὶ ratio</u>
Acts	3	14	17	1:1.21
	4	12	22	1:1.83
	5	<u>32</u>	<u>32</u>	<u>1:1.0</u>
Total	3-5	58	71	1:1.22
Turner's	I Acts			1:1

Although my figures differ from Turner's, they serve as a control in a comparison with *Martyrdom of Polycarp*. Below are the figures for the *Martyrdom*

listed by narrative section:

	<u>δέ</u>	<u>καί</u>	
ii	3	4	
iii	0	1	
iv	2	0	
v	2	5	
vi	1	4	
vii	3	2	
viii	4	6	
ix	4	3	
x	5	0	
xi	5	0	
xii	2	2	
xiii	2	2	
xiv	3	0	
xv	2	2	
xvi	0	5	
xvii	2	1	
xviii	0	0	
xix	<u>0</u>	<u>2</u>	
Total ii-xix	40	39	δέ:καί Ratio 1:0.98

Martyrdom of Polycarp differs little from Acts 3-5 though in the direction expected. It should be noted that Acts 5 by itself produced an almost identical ratio. It is stating the obvious to say that small sections cannot be relied on statistically. That is where discourse studies become so important, because they can, or should, explain where and why the ratios differ.

Looking at καί in the *Martyrdom of Polycarp*, I found a few sentences that deserve comment since they clarify functions of καί not brought out in Turner's Acts studies.

Sections v and vi have a high frequency of καί. Five of the eight καί's simply continue the participant in a new clause or sentence. The other three are interesting:

v:1 δέ Polycarp...was intending to stay...
 δέ the majority were persuading him to skip out
 καί he skipped out...

Here, the καί probably begins a new sentence and picks up Polycarp as the *same* participant from the previous sentence. There was a choice of δέ versus καί. Καί was chosen to link this sentence with the beginning of the previous one, and probably also because Polycarp's action left the other group out of the picture.

vi:1 καί, while they continued searching..., he left...
 καί immediately those searching for him came
 καί when those searching did not find him they took into custody...

The first καί occurs with the genitive absolute and probably begins a new paragraph unit. This is a 'marked' usage of καί in non-Biblical Greek. It was suggested in "Semitic Καί and Greek Δέ" that here there is an emphasis on the

continuity of circumstances surrounding Polycarp. Also, there is no subject change between the paragraphs. The second καὶ technically occurs with a change of participant/subject. However, the same group was mentioned obliquely in the previous clause (probably sentence, since the subject is explicit here in Greek). This is an example (like v:1) of "switching subjects in mid-stream", probably because the other participant left the stage.

viii:1b-2 ...they set him on an ass....
καὶ Herod met him

This is a good example of participants being linked to the same party in a struggle by καὶ.

ix:1 δε while Polycarp entered...a voice came from heaven....
καὶ the one who spoke no one saw
δε some of us heard the voice.
καὶ so, being brought out (Polycarp),
there was an uproar, hearing that Polycarp was arrested.

Both of these καὶ's are what I would call same participant as FOCAL PATIENT + Nonfocal Subject. The first one puts the previously nominative 'voice' into the accusative 'one who spoke', without focusing on the Experiencer. The second καὶ picks up a focus on Polycarp in a Patient role. (The first δε begins a paragraph; while the second δε is a binary contrast within a sentence.)

ii:3 καὶ the fire was cold to them.

The dative Experiencer is the focal participant of the paragraph and the inanimate noun does not overrule with a δε. This is quite similar to ix:1 with continued focus on a nonsubject participant.

In Summary

1. Sentence level καὶ in the *Martyrdom of Polycarp* generally signals *same* participant.
 - a. In cases where two participants, A and B are mentioned in a sentence or clause, the following sentence or clause may use καὶ with either A or B, if the other does not remain "on stage". (Cf. v:1; vi:1.)
 - b. Καὶ probably only begins one paragraph in *Martyrdom of Polycarp* (vi:1). It is probably stressing the continuity of circumstances with the previous paragraph. This is a 'marked' usage.
 - c. Καὶ may begin sentences and mark same participant in a nonsubject slot where an inanimate noun or nonfocal and ill-defined participant is subject (cf. ii:3; ix: 1 [twice]).

INA CONTENT CLAUSES John R. Werner

The majority of ἵνα ('that, in order to, to') clauses are Purpose Clauses: i.e., they express an action or a state which someone purposes to effect, i.e. to bring about, by means of some other action. However, this "purpose" function was

shared with the infinitive phrase, even in Classical Greek; and in the New Testament the ἵνα clause also shares some of the other functions of the infinitive phrase, which it has entirely displaced in Modern Greek. An outstanding example is 1 John 1:9. Certainly God's faithfulness and justice are not means that He adopts "in order that He may" forgive us our sins; rather, He is faithful and just eternally in His character, with the result that He forgives us our sins if we confess them. So, while the Purpose Clause is the majority, unmarked, or "elsewhere" function of the ἵνα clause, students of the New Testament must be acquainted with the other functions, too. Translators, in particular, must be careful to translate each ἵνα clause according to its context, since their receptor languages may not have a parallel ambiguity such as allows English to simply substitute a 'to' for each ἵνα and leave the interpretation to the readers.

One important use of the ἵνα clause is to convey the Content of an expression of will (of any intensity, from "I am willing to..." to "I insist that..."), just as the Content of an expression of fact may be conveyed by a ὅτι clause, an infinitive phrase, or, after verbs of learning or perceiving, a participle clause. Examples of such ἵνα Content Clauses occur in private letters of the first century A.D.:

Ἔγραφα ἐπιστολὴν πρὸς Ἡράκλη(ον) τὸν π[ρ]οβατοκτη(νοτρόφον) ἵνα δοῦ σοι ὄνον, καὶ Ὀφελίῳ ἐνετειλάμην ἵνα καὶ αὐτὸς δοῦ ἑτέραν καὶ τοὺς ἄρτους μοι πέμψῃ. (A farm-owner to his tenant:) 'I wrote a letter to Heracles the shepherd [commanding/requesting] that he give you a donkey, and I commanded Ophelio that he, too, give you another (female) one and send me the bread.' P. Ryl. 229, A.D. 38, lines 3-7. E. J. Goodspeed and E. C. Colwell, *A Greek Papyrus Reader*, 1935, U. of Chicago Press, Papyrus #42.

οὔτε γὰρ εἶρηξε ἡμῖν ἀγόμενος ἵνα ἀπολυθῇ. (About Theophilus the Jew, who has come and said, "I have been drafted to be a cultivator, and I want to go to Sabinus:") 'He did not say anything to me when he was taken, asking to be released.' P. Fay. 123, about A.D. 100, lines 19-21. A. S. Hunt and C. C. Edgar, *Select Papyri*, 1932, Loeb Library; Vol. I, #110.

μη ἵνα ἀναστατώσῃς ἡμᾶς. 'Do not ruin us.' Serapion tells Heraclides to keep saying this (direct quote, introduced by λέγε αὐτῷ 'continually say to him') to Ptollariion, to whom Heraclides has undersold his goods. B.G.U. 1079, A.D. 41, lines 20-21. Ibid., #107.

That such ἵνα Content Clauses occur in the New Testament, too, is especially clear when the introducing verb is θέλω 'want', as in Matt. 7:12; Mark 6:25; 9:30; 10:35; Luke 6:31; John 17:24. For example, it would be ridiculous to translate Matt. 7:12 as "Therefore whatever you want in order that men may do it to you,..."

How, then, did the writers and hearers of Ancient Greek (and how may we) distinguish ἵνα Content Clauses from other ἵνα clauses? The following hypothesis is suggested.

A ἵνα clause is a Content Clause if and only if the following transformations result in a direct wish, request, exhortation, or command, without changing the meaning of the sentence:

1. Change the subject of the ἵνα clause according to whether it is identical with the subject of the (explicit or implicit) introducing verb of wanting, requesting, urging, commanding, etc.:

- a. If the subject of the ἵνα clause is the same as, or includes, the subject of the introducing verb, change it to "I" or "we" (unless it already is such).
 - b. If the subject of the ἵνα clause is the same as, or includes, the person(s) to whom the introducing verb is addressed, change it to "you" (unless it already is "you").
 - c. Otherwise, change the subject of the ἵνα clause to the appropriate noun or third-person pronoun (unless it already is such).
2. Change the subjunctive to an imperative of the same person and number, unless its subject is now "I" or "we".
 3. Make any necessary changes of the persons of the nouns and/or pronouns other than the subject, in the ἵνα clause.
 4. Replace the ἵνα with quotation marks and add quotation marks at the end of the clause.

Examples:

1. 1 Cor. 14:13 ὁ λαλῶν γλῶσση προσευχέσθω ἵνα διερμηνεύῃ. 'He who speaks in a tongue is to pray that he may interpret.' Since the person speaking in a tongue is to do both the praying and interpreting, transformation 1a applies and διερμηνεύῃ becomes διερμηνεύω. Since the subject of this is now "I", rule 2 makes no change. Result: *ὁ λαλῶν γλῶσση προσευχέσθω "διερμηνεύω." 'He who speaks in a tongue is to pray, "May I interpret!"' Since the direct expression "He is to pray, 'May I interpret!'" means the same as the indirect expression "He is to pray that he may interpret", the ἵνα clause is Content.
2. 2 Cor. 8:6 εἰς τὸ παρακαλέσαι ἡμᾶς Τίτον ἵνα...ἐπιτελέσῃ 'so that we urged Titus that...he complete...'. Since Titus is both the one urged and the one who is to complete, transformations 1b and 2 change ἐπιτελέσῃ to ἐπιτέλεσον. Result: *εἰς τὸ παρακαλέσαι ἡμᾶς Τίτον "...ἐπιτέλεσον." Since "so that we urged Titus, 'Complete...'" means the same as "so that we urged Titus that...he complete...", this ἵνα clause, too, is Content.
3. Rom. 15:30-31 συναγωνίσασθαί μοι ἐν ταῖς προσευχαῖς...ἵνα ῥυθῶ 'to wrestle together with me in prayers...that I may be rescued'. The one to be rescued, "I" (Paul), is neither the ones wrestling together with him in prayer, "you" (the Romans), nor the One prayed to, so 1c and 2 transform ῥυθῶ to the third-person imperative ῥυθῆτω. "To wrestle together with me in prayers, 'May he be rescued'" means the same as "to wrestle together with me in prayers that I may be rescued", so here is another ἵνα clause that conveys Content.
4. Rom. 16:1-2 Συνίστημι δὲ ὑμῖν Φοίβην...ἵνα αὐτὴν προσδέξησθε 'I commend to you Phoebe...that you (may) welcome her.' Although there is no explicit verb of wanting, requesting, urging, or commanding, it is evident that the transformation product, "I commend to you Phoebe...; welcome her", is what Paul has in mind rather than a mere statement of his purpose, "I am commending to you Phoebe...in order that you may welcome her." Therefore we judge that the Greek-speaking Christians at Rome would have understood this ἵνα clause as the Content of an implicit command.
5. The opposite phenomenon—a ἵνα clause that follows a verb of wanting, requesting, urging, or commanding and yet is not Content—is rare. But consider 1 Tim. 5:7, καὶ ταῦτα παράγγελλε, ἵνα ἀνεπύλημπτου ᾧσιν 'and continually command

this, that they (may) be irreproachable'. If the ταῦτα 'this' refers forward to the ἵνα clause, the Content transformation would give us "And continually command the following: 'Be irreproachable'", which seems too vague and general a command to fit the context. More likely, the ταῦτα refers back to the commands in verses 4 to 6 that the descendants of widows take care of them and that real widows hope in God and not live for pleasures, and the ἵνα clause is a Purpose Clause: "And continually command the above, in order that they may be irreproachable."

6. 1 Tim. 1:18 Ταύτην τὴν παραγγελίαν παρατίθεμαί σοι...ἵνα στρατεύῃ... 'I am committing this command to you: 'War...'' This direct form means the same as the indirect form, so the ἵνα clause is a Content Clause.

The situation is different when we have two imperative clauses side by side, and it is a question whether the relationship between them is somehow causal rather than merely additive. I will have to reserve judgment until I have been able to collect a good number of specimens—perhaps you have a list you can share with me—, but in a preliminary way, please note that Greek, being a hypotactic language, usually subordinates one command to another by making the subordinate command a participle, e.g. Matthew 28:19,

πορευθέντες οὖν	'going, therefore,	MEANS
μαθητεύσατε	'enroll'	COMMAND (PURPOSE)

which is quite legitimately translated into the paratactic English language with two imperatives, "Go, therefore, and enroll."

In 1 Tim. 5:22, a pair of imperatives seem to have a hypotactic relationship:

χεῖρας ταχέως μηδενὶ ἐπιτίθει	COMMAND (MEANS)
'lay hands on nobody swiftly	
μηδὲ κοινωνεῖς ἁμαρτίαις ἀλλοτρίαις	GROUND (PURPOSE)
'nor share in someone else's sins'	

"Do not ordain anybody swiftly, because if you do, you may be sharing in his sins" or "To avoid sharing in other people's sins, do not ordain anybody swiftly."

1 Tim. 6:12 is a metaphor from athletic competition:

ἀγωνίζου τὸν καλὸν ἀγῶνα τῆς πίστεως	COMMAND (MEANS)
'Compete hard in the good contest of the faith,	
ἐπὶ λαβοῦ τῆς αἰωνίου ζωῆς	GROUND (PURPOSE)
'take the prize of eternal life.'	

If our theology is bothered by that, it will also be bothered by the rather parallel passage in 2 Tim. 4:7, 8, "I have competed hard in the good contest, I have finished my race, I have guarded the faith; what is left is that the wreath of righteousness is reserved for me, and the Lord, the honest Judge, will award it to me on That Day." No doubt modern professional athletes are earning their rewards, providing entertainment in exchange for money; but the Greek athlete was an amateur, and like amateurs today he competed, not to earn anything, but to show that he was what he claimed to be—fully qualified, the best in his field. The wreath is not a reward, but an award; there is an important difference. Cf. 2 Thess. 1:3-7, in which the endurance of persecution, which shows that the Christians are the ones to

whom God will justly award entrance into His Kingdom, is an evidence of work that He has done in them.

Eph. 4:26-27 is especially interesting. First, 26 alone, quoting Psalm 4:4, is a pair of commands.

ὀργίζεσθε	'Be angry,	GROUND (CONCESSION)
καὶ μὴ ἁμαρτάνετε	but do not sin.'	CONCLUSION (CONTRAEXPECTATION)

(I justify translating the καὶ as 'but' on the grounds that it translates the Hebrew conjunction ו, which can be adversative.) Then, that quotation occurs with two further commands:

ὀργίζεσθε καὶ μὴ ἁμαρτάνετε	GROUND (GENERIC)
'Be angry, but do not sin;	
ὁ ἥλιος μὴ ἐπιδυέτω ἐπὶ παροργισμῷ ὑμῶν,	COMMAND (SPECIFIC, MEANS)
the sun is not to set on your rage,	
μηδὲ δίδετε τόπον τῷ διαβόλῳ.	GROUND (PURPOSE)
and do not give the devil any scope.'	

So, from this small sample, it seems so far that in a COMMAND-GROUND relationship the GROUND can be based, not only on MEANS-PURPOSE, but also on CONCESSION-CONTRAEXPECTATION and GENERIC-SPECIFIC and that the two commands can occur in either order, it being GROUND-COMMAND in Eph. 4:26, COMMAND-GROUND in 1 Tim. 5:22 and 6:12, and both in Eph. 4:26-27.

It is hoped that the above transformation hypothesis will help translators and other students to discern Ina Content Clauses, even though the proviso, "without changing the meaning of the sentence", may still leave us in doubt in some instances.

THE POSITION OF ΓΑΡ IN GREEK CLAUSES

Alan Healey

It is difficult to find a detailed description of when and why the Greek postpositive particles are delayed beyond the second position in a clause. (Very little is said in Robertson p. 424, Blass-DeBrunner-Funk §475(2), or Denniston p. lx.) To obtain a general picture of the matter, 340 instances of the postpositive conjunction γάρ were examined. These were sampled from the 1036 instances of γάρ in the New Testament as follows: the first 32 to 40 occurrences in each of Matthew, Mark, Luke, John, Acts, Romans, 1 Corinthians, and Hebrews, plus all instances in the books from James to Revelation, plus six other instances cited by grammarians.

The overall picture is that γάρ comes in second position 91% of the time, in third position 8% of the time, and in fourth position 1% of the time. But these statistics taken by themselves give a false picture; the position of γάρ is not a random pepper-and-salt phenomenon. The frequency with which it occurs in third and fourth positions is controlled by the grammatical context.

1. Two Postpositives Together

(a) When two postpositive particles occur in the same clause, they cannot both go

in second position; one must come third. There are three postpositive particles (μέν, τε, and ἄν) that are observed to cooccur with γάρ. They either relate only to the following clause, somewhat like an adverb, or they relate the following clause (or cluster) to a later clause (or cluster).

(b) Γάρ comes in third place when it cooccurs with μέν (4 cases) and τε (2 cases). When γάρ occurs with ἄν, in this sample, the first element is always a relative pronoun. Γάρ comes in second place followed by ἄν in 6 cases and in third place preceded by ἄν in one case (Luke 8:18).

(c) Γάρ does not cooccur with free δέ in this sample, but it does follow the combination οὐδέ in three instances (John 5:22; Acts 4:12; 4:34a) and is in second position. In all three of these cases, the -δέ coordinates the following clause(s) to the preceding clause(s). Γάρ does not cooccur with οὖν in this sample.

In the remaining 324 instances of γάρ discussed below, γάρ is the only postpositive involved.

2. Preposition Plus Nominal

When the first element (tagmeme) of a clause is a prepositional phrase consisting of a preposition followed by a nominal, γάρ comes in second position (thus splitting the phrase) in 8 cases (Luke 6:44; John 4:37; 5:46b; Acts 5:36; 8:23; Rom. 3:20; 1 Cor. 4:15b; 3 Jn. 7), in third position (usually at the end of the phrase) in 6 cases (Matt. 2:6; Mark 1:38; 9:34; Heb. 2:8; 1 Pet. 2:21; 4:6), and in fourth position (at the end of the phrase) in 2 cases (Luke 6:23b,26).

3. Preposition Plus Relative Clause

When the first element of a clause is a prepositional phrase consisting of a preposition followed by a relative clause, γάρ comes in third position (following the relative pronoun) in all 5 cases (Matt. 7:2; Rom. 2:1a; Heb. 2:18; 7:13; 2 Pet. 3:4).

4. Article Plus Nominal

When the first element of a clause is a multi-word nominal whose first word is an article, γάρ comes in second position (thus splitting the nominal) in 43 cases, in third position (only sometimes splitting the nominal) in 5 cases (Rom. 1:19; 1 Cor. 1:18; Heb. 7:11; 2 Jn. 11; Rev. 22:10), and in fourth position (still splitting the nominal) in 1 case (2 Cor. 1:19).

5. Other Multi-Word Nominals

When the first element of a clause is a multi-word nominal whose first word is not an article, γάρ comes in second position, thus splitting the nominal into two in all 28 cases.

6. Verb Phrase

When the first element of a clause is a verb phrase consisting of two verbs, γάρ comes in second position (thus splitting the phrase) in the only three cases observed (Mark 1:22; Luke 8:40; John 4:47).

7. Clause-Initial Conjunction

If there is a clause-initial conjunction, this conjunction is counted as part

of the clause, and γάρ comes in second position in all 25 instances. The initial conjunctions observed preceding γάρ are εάν, εἰ, ἐπειδή, ἥ, ὅτι, ὅταν, ὥστε, which all relate the following clause (or clause cluster) to another clause (or cluster) further ahead.

In five instances, initial καί could be viewed as a conjunction which helps to relate the following clause (cluster) to the preceding clause (cluster) rather than as an adverb 'in fact, indeed'. This view appears to be supported by the TEV, which leaves καί untranslated in these instances (Luke 1:66; John 4:23; 1 Cor. 5:7; Heb. 4:2; 5:12) as it does for many other instances of the conjunction καί by itself. However, neither Denniston nor Smyth discuss the possibility that καὶ γάρ may consist of two conjunctions.

8. Other Single-Word Initial Elements

8.1 When the first element of a clause is a single-word nominal, γάρ comes in second position in all 34 cases.

8.2 When the first element of a clause is a single verb, γάρ comes in second position in 73 cases, and in third position in one case (Heb. 11:32).

8.3 When the first element of a clause is a single attention-getter; adverb of manner, time, or place; relative; or interrogative word, γάρ comes in second position in all 41 cases.

8.4 When the first element of a clause is the single adverb οὐ, μή, or καί, γάρ comes in second position in 46 cases and in third position in 3 cases (John 4:45; Acts 4:20; 1 Cor. 16:7).

9. Conclusion

It appears that there are three kinds of grammatical context as far as γάρ is concerned.

- In most contexts (4, 5, 6, 7, 8), γάρ occurs in second position 97% of the time.
- When the clause commences with a preposition plus relative clause (3), γάρ appears to come in third position most or all of the time.
- When there are two postpositives together or when the clause commences with a preposition plus a nominal (1, 2), γάρ comes in the second position half of the time and in the third or fourth positions half of the time.

10. Further Research

10.1 A similar enquiry into the position of δέ, οὐν, and perhaps the other postpositives would show whether or not these constraints on positioning are a feature of this whole word class.

10.2 To enquire whether or not there are further grammatical factors (within the contexts already listed) which control the position of postpositives would demand a considerably increased body of classified data.

10.3 Where position appears to be a matter of the author's choice, it is important to investigate the basis of his choice—what meanings are demonstrably associated with the various choices. Again, such an investigation will need much more data.

10.4 A careful examination of all instances of two or more postpositives in the one clause should produce a statement of their mutual ordering, their relation to any subclass of postpositives, and their relation to their rank or level within the hierarchical structure of the text.

11. Postscript

After this paper was completed, the author discovered the far more detailed work by Blomqvist (based on 12,140 pages of text from the Hellenistic period (330 BC-30BC)). As Blomqvist's monograph is now out of print, a summary of his main findings (pp. 108-131) about the position of all postpositive particles is presented below.

11.1 The description is simplified by treating certain groups of two words as though they were a single postpositive filling a single position.

- a. Two postpositives. The first one is usually μέν, τε, or ἄν, and the second is usually inferential.
- b. A postpositive plus an emphatic ὅγ or γέ.
- c. An enclitic such as τος (when not governed by a preposition) plus a postpositive.

11.2 The necessary conditions for postponement are fairly straightforward.

- a. With few exceptions, a postpositive *may* be postponed to third or later position only when the clause commences with a prepositive word—ἢ, article, preposition, conjunction, relative (Dover, p. 14)—or with a negative.
- b. In this situation, if the postpositive is postponed, it will be placed immediately after the first word of the clause with mobile characteristics (i.e., not prepositive or postpositive) such as a noun, adjective, adverb, pronoun, or a relative or indirect interrogative governed by a preposition.

11.3 On the other hand, a complex of several factors seems to control whether or not postponement will actually occur in the contexts where it is permitted to occur.

- a. Postponement is especially frequent when the clause commences with a relative or indirect interrogative pronoun governed by a preposition (88%), an interrogative pronoun (93%), a "reflexive" pronoun (84%), or an adjective (82%). It is particularly infrequent when the clause commences with a personal name (17%), οὗτος (24%), or τος (20%).
- b. Postponement is especially frequent when the postpositive is οὖν (84%) or ἄρα (94%), and particularly infrequent when the postpositive is μέν οὖν (25%) or μέν οὖν (6%).
- c. When the first word is a preposition, postponement is particularly frequent with ἀνά (93%), διὰ (70%), εἰς (70%), ἐν (71%), and κατά (71%). Postponement is especially infrequent with ἀντί (10%), ἀπό (30%), μετά (15%), περί (22%), πρὸς (23%), σύν (18%), and improper prepositions (5%).
- d. When the first word is an elidable preposition, if the (potentially) next word commences with a vowel, there is 74% postponement, but if it commences with a consonant, there is only 35% postponement.
- e. Postponement is sometimes used for reasons of euphony (e.g., to avoid a sequence of three short syllables or to avoid hiatus), or to give special emphasis to the word preceding the postpositive.
- f. In the Hellenistic period, scientists, mathematicians, philosophers, and the authors of papyri used postponement more than the writers of literary prose.

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