THE MAJORITY TEXT AND THE ORIGINAL TEXT: A RESPONSE

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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 from 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 NT 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 NT 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?

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[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.

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."[1] 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,"[2] 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."[3]

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]."[4] 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."[5]

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" eclecticists is itself derived from internal evidence, the same considerations employed by the "rigorous" eclecticists. 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 UBS^3, 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-third of his book" (a bit of hyperbole) is devoted to discrediting W-H (p. 109). Now he asserts that "**his whole case**

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[1] *Studies in New Testament Language and Text*, ed. J.K. Elliott, Leiden: Brill, 1976, 179.
[2] Chapters in the History of New Testament Textual Criticism, Grand Rapids: Wm. B. Eerdmans, 1963, 39.
[3] *The Text of the New Testament*, London: Oxford University Press, 1964, 212.
[4] "Rigorous," p. 177.
[5] *Ibid.*, p. 179.

rests on a single assumption: that the transmission of the NT text was 'normal' [emphasis Fee's]" (p. 110)—an assertion neither fair nor true.

Fee seriously distorts my position by ignoring my discussion of the **ab**normal 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" [emphasis his] (p. 113). Fee fastens upon my use of the term "malicious," which I use only in discussing the **ab**normal 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."[1] 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 maximum 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 NT writings was ridiculed by the pagan Celsus, among others. Although Origen defended the simplicity of the NT 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 1-14 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 31 instances the Peshitta stands alone (in those chapters), Gwilliam concluded that its unknown author "revised an ancient work by Greek MSS which have no representative now extant (p. 237)."[2] In a personal communication, Peter J. Johnston, a member of the IGNT editorial panel working specifically with the Syriac Versions and Fathers, says of the Harklean Version: "Readings confidently referred to in the

[1] *What is the Best New Testament?*, Chicago: The University of Chicago Press, 1952, 53.
[2] *The Early Versions of the New Testament*, Oxford: Clarendon, 1977, 61.

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."[1]

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 P^{75} were fourth century I suspect that the present climate in NT 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?'"[2] 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.[3] 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" *tis estin 'o paisas se*. 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.
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> 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.[4]

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 NT text. It seems to me that he confuses citing with transcribing. The evidence he cites applies to the citations in the Fathers' 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

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[1] This 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).

[2] "Hort Redivivus: A Plea and a Program," *Studies in Methodology in Textual Criticism of the New Testament*, Leiden: Brill, 1969, 157.

[3] "Modern Text Criticism," p. 162.

[4] I would say that the "more than five hundred changes" (p. viii) introduced into UBS^3 as compared with the second edition afford a clear vindication of my contention. Although UBS^3 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.

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, well, **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 Matthew 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 *oksos*, "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."[1] 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 17 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," *xole*, in association with the vinegar (and Acts 8:23 is the only other place in the NT 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, *potizein xolen meta oksos* (7:5), can there be any doubt as to his source? When the Gospel of Peter says *Potisate auton xolen meta oksous* (5:16), must the source not be Matthew? When Gregory of Nyssa says, *xole te kai oksei diabroxos* (Orat. x:989:6), can there be any question at all? It may be noted in passing that Alford's Greek N.T., in *loc.*, says plainly that Origen and Tertullian both support the "Byzantine" reading under discussion.[2]

Note also that Irenaeus wrote, "He should have vinegar and gall given Him to drink" (*Against Heresies*, XXXIII:12), in a series of OT 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 Mt. 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).

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[1] "A Critique," p. 412.
[2] The research reflected in the discussion above was done by Maurice A. Robinson and kindly placed at my disposal.

Whatever interpretation the reader may wish to give to Fee's statement, noted at the outset, it is clear that the reading "vinegar" in Matthew 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 UBS^3 faithfully reports Irenaeus' support of the "Byzantine" reading $Nestle^{26}$ 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. $Nestle^{26}$ is also disappointing at this point.[1]

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.[2]

[1] The 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 Chretiennes* 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 owe this material on Irenaeus to Maurice Robinson.

[2] The Byzantine Text-Tpye and New Testament Textual Criticism, La Mirada, CA: Biola College Bookstore, 1972.

"Conclusion--A Text 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 I Tim. 3:16 as a "test case", however, I am delighted to oblige.

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

o - D
w - 061
os theos - one cursive and one lectionary
os - 01, 33, 365, 1175, 2127, three lectionaries
theos - A, C^{vid}, F/G^{vid}, K, L, P, 044, over 550 cursives plus most lectionaries (including four cursives that read *o theos* and one lectionary that reads *theou*).

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 Scrivener 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."[1] 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."[2]

Codices F/G read OC wherein the contracting-bar is a slanting stroke. It has been argued that the stroke represents the aspirate of ὁς, but Burgon demonstrates that the stroke in question never represents breathing but is invariably the sign of contraction. He affirms that " ὁς is **nowhere** else written OC in either codex."[3] Presumably the cross-line in the common parent had become too faint to see.

The three significant variants involved are represented in the ancient uncial MSS as follows: O, OC and ΘC, 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.

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[1] *The Revision Revised*, London: John Murray, 1883, 434. Cf. 431-6.
[2] *A Full Account and Collation of the Cursive Codex Evangelium 604*, London: David Nutt, 1890, Appendix J, 2. See also Burgon, 437-8.
[3] P. 442. Cf. 438-42.

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 Syr^p 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 (98.5%), 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, "*Iesous . . . 'o uios tou theou tupo kai en sarki fanerotheis*" (Cap. xii), and in Ignatius, "*theou anthropinos faneroumenou*" (*Ad Ephes*. c. 19) and "*en sarki genomenos theos*" (*Ibid*, c. 7). In the third century there seem to be clear references in Hippolytus, "*theos en somati efanerothe*" (*Contra Haeresim Noeti*, c. xvii), Dionysius, "*theos gar efanerothe en sarki*" (Concilia, i. 853a) and Gregory Thaumaturgus, "*kai estin theos alethinos 'o asarkos en sarki fanerotheis*" (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.[1]

As for the grammatically aberrant reading, "who", aside from the MSS already cited, the earliest version that clearly supports it is the Gothic (4th cent.). To get a clear Greek patristic witness to this reading pretty well requires the sequence *musterion 'os efanerothe* 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 equalled).[2]

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.”[3]

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 vs. 13, and the antecedent of the relative in vs. 13 is "the father" in vs. 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.

[1] Burgon, 456-76, 486-90.
[2] P. 483.
[3] P. 497. The above discussion is taken from the second edition of my book, footnote 32 to chapter 5.