

A SURREJOINDER TO DOUGLAS J. MOO

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The order of this surrejoinder will follow, for the most part, the order of Moo's "Rejoinder." He starts by asking what makes my view preferable to others. My answer: its adequacy and economy in explaining the textual data. But the answer does not satisfy him, because I have not usually compared my view with other views so as to show in detail how it surpasses them in adequacy and economy. He chooses as his parade example the way I handle the interpretation of the parable of the sower (Matt 13:18-23): "He [Gundry] assumes throughout that Matthew had Mark's text before him as he wrote. . . . Yet not a single argument for the priority of Mark in this passage is advanced. And this despite the fact that David Wenham has made a strong case for the view that all three synoptics are using pre-canonical tradition in this pericope and that, if anything, Mark uses Matthew."

Moo is criticizing me for failing to do what in my *Commentary* I invite readers to do ("make critical comparisons of their own," p. 1; cf. my ETS paper, pp. 1-3, 12). Yet a commentary the size of mine has too little room not only to present the author's own views fully but also to survey and criticize the views of others, which, as Moo knows, are legion. Occasionally I do take up another view.¹ Frankly, however, I did not think Wenham's case strong enough to deserve special treatment. Moo's and my judgments obviously differ here. But if limited space keeps him from detailing why Wenham thinks all three synoptics are using pre-canonical tradition and Mark may be using Matthew, why he thinks Wenham's case is "strong," and why he still holds to Mark's priority (at least in general) despite Wenham's "strong case," the rug slips out from under his criticism that I should have detailed why I think Wenham's view is weak in comparison with mine. Apparently Moo expects his readers to look up Wenham's article, compare his arguments with mine, and make their own judgments. Well, then, I can expect readers of my *Commentary* to follow the same practice.

More specifically, Moo is asking for the impossible: the kind of treatment for each pericope in Matthew that one can find only in full-scale articles and monographs. Not even the bulky two- and three-volume commentaries (with 500-1000 pages in each volume) being published nowadays meet that demand. And despite their apparent comprehensiveness they contain numerous gaps. Moo even wants me to supply "reasons in particular texts" for my dependence on the Mark-Q hypothesis—i.e., he wants me to argue, pericope by pericope, with Farmer, Lindsey, Rist, and Léon-Dufour (whom he lists, and he might have added many others) over "source assumptions." We do not find such discussions in most of the standard commentaries (Allen, McNeile, Grundmann, Gaechter, Bonnard and Beare

¹See, e.g., my comments on Matt 15:24 (*Commentary* 313).

on Matthew; Taylor and Lane on Mark; Plummer, Creed and even Fitzmyer² on Luke; Morris and Barrett on John; Bruce and Haenchen on Acts—to name only a few of the larger ones). Perhaps Moo wants me to write a commentary like Marshall's on Luke, a students' handbook that surveys and assesses the interpretative field. It has great value, but it has also received criticism for obscuring Marshall's own views and not allowing enough space for adequate support of them. And Marshall himself writes regarding the Mark-Q hypothesis: "Although several alternative theories are being canvassed today, it would have been out of place to discuss them in detail throughout the commentary."³

Apparently Moo does not appreciate the comment of J. M. Robinson: "The success of *Redaktionsgeschichte* in clarifying the theologies of Matthew and Luke on the assumption of dependence on Mark is perhaps the most important new argument for Marcan priority, just as perhaps the main ingredient lacking in William R. Farmer's argument for Marcan dependence on the other written Gospels is a convincing *Redaktionsgeschichte* of Mark based on that assumption."⁴ My enlarging Q and seeing a secondary use of Matthew by Luke show that I do not accept the Mark-Q hypothesis simplistically or uncritically. On the synoptic question, then, I am unrepentant and confident that on the whole the world of scholarship will see my *Commentary* as contributing to greater certainty about Mark's priority and Matthew's and Luke's use of Q. It is too early to predict the effect of my enlarging Q and seeing a secondary use of Matthew by Luke (the latter not a new suggestion).

Yet more specifically, Moo says that I am "particularly struck by the fact that Matthew twice 'adds' to the Markan original the theme of understanding (vv 19, 23 [of Matthew 13])." He goes on to argue that since "the word *syniēmi* ('understand') stems from the Markan tradition and eventually, of course, from Isa 6:9," Matthew, in adding a quotation of Isa 6:9-10, "wanted to make more clear the OT background" rather than to shift the focus from the original four distinctions between kinds of soil to the distinction between understanding and not understanding (so my *Commentary*). I have no objection to Matthew's wanting to clarify the OT background. But Moo should not dismiss the possibility that Isa 6:9-10 attracted Matthew because of its emphasis on understanding. Furthermore, Matthew did not have to import *syniēmi* twice from his quotation into the interpretation of the parable, where the synoptic parallels lack it. He also includes unique material on understanding later in his parabolic chapter (vv 51-52), omits at 14:33 the statement in Mark 6:52 that the disciples did not understand concern-

²Moo quotes against me J. A. Fitzmyer, who although an advocate of the Mark-Q hypothesis despairs of "a definitive and certain solution to it, since the data for its solution are scarcely adequate or available to us"—but (I add from the same page cited by Moo) only in the sense that "we are forced to live with a hypothesis or a theory," which is exactly what Moo criticizes me for doing. In his massive Anchor Bible commentary on Luke 1-9 Fitzmyer assumes Mark's priority, builds his comments on that assumption, and does not evaluate any other source-critical view of the parable of the sower and its interpretation (to take a typical example, and one that relates to Moo's criticism of me).

³I. H. Marshall, *The Gospel of Luke* (Grand Rapids: Eerdmans, 1978) 30.

⁴J. M. Robinson, "On the *Gattung* of Mark (and John)," *Jesus and Man's Hope* (Pittsburgh: Pittsburgh Theological Seminary, 1970), 1. 101-102; similarly J. A. Fitzmyer, *The Gospel According to Luke I-IX* (AB 28; Garden City: Doubleday, 1981) 65. Cf. my *Commentary* xi, 3.

ing the loaves, changes to understanding in 16:12 what Mark 8:21 says was the disciples' failure to understand, and inserts into paralleled material their understanding (17:13). Elsewhere he expands the theme with the use of other words and at the expense of the disciples' ignorance,⁵ as almost all specialists in Matthew agree. Moo does not bring in these data, nor does he note the contrast between the larger attention to understanding in Matthew's interpretation of the parable of the sower and "his failure to make theologically substantive changes in the interpretations of those sown on rocky and thorny ground" as showing "that his primary interest lies in the distinction between understanding and not understanding" (*Commentary* 260). Of course, if Matthew does not do what I say he does, Mark or someone before him redacts as much in the opposite direction.

Moo thinks my view requires me to demonstrate the probability that Luke was quite conservative with Q. "Yet . . . Gundry assumes virtually throughout his comments on Matt 5:3-12, 21-48 that Luke's version [6:20-49] provides us with a word-for-word summary of what Matthew found in his tradition. Contrast such an assumption with I. H. Marshall's comment (on Luke 6:27-38): 'Both Gospels show signs of systematizing the traditions before them, and this makes it almost impossible to reconstruct a hypothetical original.' " But throughout his commentary Marshall often tells us what he thinks is traditional and what redactional, just as I do for Matthew, and in each pericope assumes without argument the Mark-Q hypothesis. (What gives Moo a handle is Marshall's complicating belief that Matthew and Luke used different recensions of Q.) Concerning Luke 6:20-49, the passage where Moo says I underestimate Luke's redaction in favor of Matthew's, Marshall says, "A greater degree of freedom has been shown by Matthew."⁶

"One reads through Gundry's comments on Matthew 5 in vain to find any place where it is admitted that Matthew may preserve the more original form of the material." Yet a reader who opens to my comments on Matthew 5 will run into several contradictions of Moo's charge. On p. 74 I devote the whole middle paragraph to arguing for Matthew's greater originality. The paragraph peaks in the statement that Luke "assimilates an original exultation [so Matthew] to a leaping for joy in accord with Luke 1:41, 44." The next paragraph starts on the same note: "*hoti* looks more original than the Lukanism *idou gar*." Similarly at the bottom of the next page. Again on p. 87 the whole paragraph after the fine print takes the position that in several respects Matthew is original, Luke redactional.

At times I make no choice between Matthew and Luke as to originality versus redaction (see e.g. p. 77). At other times I do not merely "assume" Luke's originality and Matthew's redaction but argue for it—as on pp. 67-68, where I argue from Matthew's changing the baptismal voice from second person to third for a similar change in the beatitudes; also from his concern (evident throughout his gospel) with antinomianism and from his interest in OT language and the predominance of third-person beatitudes in the OT. Further arguments follow in the fine print on p. 68, and a refutation of a counter-argument. And so on. I also appeal constantly to what Matthew does outside chap. 5. On pp. 98-99 I argue

⁵See the pages listed under "Understanding" in the Topical Index of my *Commentary*.

⁶Marshall, *Luke* 243.

rather than assume that Luke's *charis* is traditional rather than redactional. Cf. also the fine print on p. 100. I am disappointed in the failure to take more seriously the responsibility of a reviewer to read carefully the material he is reviewing.

Though Moo correctly notes that some specialists in Luke attribute to him a great deal of redaction, it is a pity he does not deal with the citations in my "Response" concerning a growing consensus in favor of Luke's conservatism, especially with regard to Jesus' words (primarily Q). I no more assume "without argument" that "Matthew rewrites his beatitudes by means of the woes" than the scholars cited by Moo assume that Luke adds or "heightens the economic dimension." In view of Matthew's free treatment of Mark and the growing consensus that Luke is conservative with Jesus' words, it seems more probable that the beatitudes and woes in Luke are largely original and that Matthew follows his usual habits of redacting more freely than Luke. The strong economic dimension turns out to be characteristic of Jesus' own teaching—hardly an unpopular view in current scholarship. To argue that in this passage "the more Jewish orientation of Matthew could be taken as evidence that his is closest to the actual words of Jesus" (so Moo) runs afoul of Matthew's regular conformity to the OT in clearly editorial parts of his gospel, beginning with its very first phrase and continuing with the formula-quotations and much else. It seems likely he conforms to the OT in noneditorial sections too.

Why do my appeals to borrowings of OT phraseology, word statistics, tight parallelism, and the like not count in Moo's mind for "close argumentation"? Why should a probably dwindling school of Lukan scholarship hold Matthean scholarship captive? I had hoped to give synoptic scholars something to weigh by showing that the increasingly popular conservative estimate of what Luke does with Jesus' words results in a to-be-expected and confirmatory redaction of Q by Matthew after the manner of his redacting Mark—i.e., something to weigh from Matthew's side just as Lukan scholars have offered something to weigh from Luke's side.

Moo asks how we can know that "Matthew had before him a text virtually identical to Luke 6:20-38." In writing my *Commentary* I tried to find out by checking Luke-Acts, examining word statistics concerning Luke's diction, and studying commentaries and other literature on Luke-Acts and the synoptics. Where serious disagreement with a significant opinion in Lukan scholarship turned up, I tried to make a brief comment and argument, as, for example, in the very paragraph from which Moo takes his quotation in n. 11. In that paragraph my comment and argument oppose those who argue "against the authenticity of the woes—or at least against the originality of their association with the beatitudes" (*Commentary* 69). In asserting that "most Lukan scholars would take strong exception" to my statement that "none of Matthew's dependencies has to do with likely Lukanisms," does Moo mean that most Lukan scholars would strongly argue that Matthew does reflect likely Lukanisms? He would have a hard time proving that one. Yet I can think of no other way to construe his assertion.

Moo exclaims, "If considerable question about the 'tightness' of the fit between Gundry's source hypothesis and the textual phenomena exists in his use of Mark and Q, how much more so is this the case with respect to Matthean peri-

cope with no clear parallel!" But his first example, 8:28-34, has clear parallels in both Mark 5:1-20 and Luke 8:26-39. There is no "must" to my interpretation of the pericope. My interpretation of Matthew's two demoniacs is fairly common, and I give a number of reasons for holding it. With regard to my view that Matthew's parable of the two sons is a counterpart to Luke's parable of the prodigal son, Moo omits my qualifier "apparently" and my other two points that the parable reflects Matt 20:1-16 and 21:23-27. The exegetical comments put emphasis on these other two points almost to the exclusion of the first point.

Why does Moo not offer a rejoinder to my "Response" concerning Matthew's changing the Lukan offering of a pair of turtledoves into Herod's slaughtering of the babes in Bethlehem instead of repeating himself? On the parable of the unforgiving servant I give dictional and other reasons for Matthew's borrowing from the parable of the two debtors and for redactional creativity. Similarly for the parable of the laborers in the vineyard in relation to the parable of the wicked vinedressers. In view of my detailed treatment of verbal and material resemblances, Moo needs to prove rather than assert that they are "often very slight." His comment on my source-critical view, "We seem to be left with Gundry's word for it," is liable to turn back on him.

Presumably Moo means that even from the standpoint of the redactions I see later in Matthew, those I see in the nativity story outstrip the others so far that they are incomparable. I have already indicated why we should expect Matthew to redact the nativity story more than the rest of the dominical tradition and have detailed several similarities between his redactions of the nativity story and of other dominical traditions (see my "Response"). Moo has not rejoined specifically to any of those considerations. Nevertheless I spell out the nature of the redactions to show that they are not incomparable. Matthew shifts actions from one person to another: the speaking in 21:41 from Jesus (so Mark 12:9) to the chief priests and elders (cf. 21:23) and the editorial comment in Mark 14:1 from Mark to Jesus (Matt 26:2) as well as the naming of Jesus from Mary (Luke 1:31) to Joseph (Matt 1:21). Matthew transforms the character of actors: Herod Antipas turns from being John the Baptist's protector (Mark 6:14-31) into being his enemy (Matt 14:1-12), and Pontius Pilate turns from being a weak and vacillating pagan judge (Mark 15:1-15) into a disciple who defends Jesus (Matt 27:1-2, 11-26), just as the Jewish shepherds (Luke 2:8-18) turn into the Gentile Magi (Matt 2:1-12). Matthew imports dreams: Pilate's wife has a dream (Matt 27:19), just as Joseph and the Magi have dreams (Matt 1:20-21; 2:12-13, 19, 22). Matthew creates antitypical incidents: Peter's walking on the water as an antitype of Jesus' walking on the water (14:22-33) as well as the journey to Egypt and back as an antitype of Israel's history (2:13-23). And in general Matthew's redaction in 3:14-15; 14:1-12; 16:17-19; 27:3-10; 27:15-26; 28:1-10 and large parts of the five great discourses approaches in transformativeness and creativity his redaction of the nativity story.

"Any theory must account for all the data"? Yes (though perhaps overstated). But Moo needs to show what data the hypothesis of Matthew's redacting a nativity tradition like Luke's does not account for. Until he does I cannot and need not say more on the subject. In my "Response" I indicated why he should concentrate his efforts outside the nativity story. That he reverts to it for his argument-clinching examples betrays a weakness in his overall case.

With regard to word statistics, "the issue is not, as Gundry suggests, whether Matthew adds sentences to the tradition." Here Moo is trying to shift ground, for in his article he devoted two whole paragraphs to arguing that my "decision to base this crucial statistic [concerning Matthew's insertions] on the paragraph rather than on the sentence" underlies what he considers to be my exaggeration of Mattheanisms, and that "identifying insertions by comparing clearly parallel sentences is the more normal procedure and surely yields the more accurate figure." For stronger contrast his manuscript had "paragraph" and "sentence" underlined at two points. Now he says "simply" that "verbal insertions can validly be spoken of only where the specific material is parallel." But "specific material" is only a synonym for "sentence," as his later use of "sentence" in the same paragraph shows; so he is saying nothing new. He has not succeeded in shifting ground.

"Yet Gundry chooses a different method [from the one that uses 'the sentence for the basis of comparison'] that just so happens to provide a better statistical basis for his theories of Matthean creativity." The phrase "just so happens" suggests that I knew what I was after and chose my method to insure I got it. But I chose my method only in the process of seeing the state of affairs in the text. That state turned me from the usual dependence on the sentence. Surely we all need some openness to change. Does my method "cater to possible bias" when time after time Matthew's unique sentences, often in succession, consist entirely (or nearly so) of words and phrases he alone has in other sentences paralleled in Mark and Luke? The method caters to the evidence. It is not quite true to say that "Gundry . . . characterizes any other suggestions as 'assumptions'." Actually I said that each suggestion rests on an assumption, and argued that the assumption beneath my suggestion (that we use the paragraph) fits the textual phenomena better.

"To characterize as insertions all the words in ten verses of Matthew that are not found in one verse of Luke that is loosely parallel to Matthew is obviously to stretch the meaning of 'insertion' to the breaking point." Perhaps so—unless the whole of Matthew's passage consists of words and phrases that he inserts in other passages where even Moo admits a true synoptic parallel. But he does not identify a single passage where Matthew's having ten verses and Luke's having only a single, loosely parallel verse has skewed my statistics. Indeed I can find none. Among pericopes for which Aland's *Synopsis* prints a Lukan parallel, six uniquely Matthean verses are the most I have found in a single pericope. They are beatitudes widely acknowledged as Matthew's expansions. Ordinarily the unique Matthean verses come in ones and twos per pericope, and often not at all. (I have counted so as to err in favor of Moo's side where judgment is called for.) Once we find three, and twice we find five; but the fives come in long pericopes of fourteen and seventeen verses, where we might expect more insertions. The total number of uniquely Matthean verses in pericopes paralleled in Luke is about forty, and the Lukan parallel usually has considerable more than a single verse—in contrast with Moo's darkly hinted example.

The same situation obtains for uniquely Matthean verses in pericopes for which Aland's *Synopsis* prints a Markan parallel (with a partial exception in Matt 23:1-36, which is, however, an extremely long pericope). Moreover, a number of the uniquely Matthean verses are obviously editorial, so that everybody

would count their words as insertions; and others consist of OT quotations introduced with Matthew's formula of fulfillment. With these exceptions, the total number of uniquely Matthean verses in pericopes paralleled in Mark is about seventy-four. Since Matthew consists of about 1,070 verses (depending on some text-critical decisions), we can see how little my statistics are skewed even from Moo's standpoint.

Perhaps Moo thinks I counted insertions according to my expanded Q rather than according to the traditional Q reflected in Aland's *Synopsis*. If so, he is wrong and should have taken me at my word when in the *Commentary* (3-4) I said that Aland's *Synopsis* was my standard—precisely to avoid stacking the deck in my favor. For the same purpose I counted Matthew 1-2 as unparallelled despite the parallels printed in the synopsis. Moo should have caught these points in my ETS paper, where I emphasize them, if he found the *Commentary* unclear. His wanting "a complete statistical picture" seems to overlook my Greek Index, which gives not only insertions and occurrences in unparallelled passages but also totals for all three synoptics, and which in italics on p. 3 of the *Commentary* I urge readers to consult.

Yes, "a case for Matthean creativity built on the basis of statistics demonstrating Mattheanisms" would "look as strong if similar statistics relating to non-Mattheanisms were cited." Any reader can find out for himself by using a concordance and a synopsis for every significant word in Matthew, as I did with innumerable flippings of pages. To include the information that such-and-such a word is never inserted by Matthew or used in one of his unparallelled passages, but occurs only when paralleled, seems pointless, however. How would that information help anyone judge the significance of insertions and occurrences in unparallelled passages? Furthermore, my appeal to the absence of non-Mattheanisms answers Moo's question, "How many are found on the other side?" The answer is "None" (excepting for borrowings from the context and the OT, as explained in my "Response"). The analogy of a public-opinion poll is inapt, because it has to do with the opinion of five million individuals over against the different opinions of many more millions of individuals, whereas in Matthew we are ferreting out the diction of one and the same individual, the author. Moo wants me to say clearly if the word statistics vary in significance. I have said so—at every stage from the *Commentary* onward. But if he means that I should interpret the significance of every single statistic, he is again asking for the impossible even for a fairly long commentary. It also seems plain enough that the statistics on *basileus* are more impressive than those on *bastazō* (to take but two random examples—see the Greek Index) without my having to say so. The intelligence of readers deserves some respect.

Moo knows there are no "accepted methods" for interpreting statistics such as mine. So he is safe in demanding a withholding of judgment (though he himself does not withhold the judgment that I have given "a biased view"). But accepted methods will come into being only through scholars' proposing and using methods and through receiving and answering criticisms and making revisions where necessary. I am happy to receive Moo's criticisms. Others will have to judge my answers. Thus far I have not seen a necessity for revision. Notably Moo fails to take account of using typically Matthean theological emphases, tight parallelism, and conformity to OT phraseology as checks and confirmations of specially Matthean

diction—i.e., the statistics do not stand alone, to say nothing in detail about contextual and comparative remarks I often devote to Matthew's insertions.

Moo argues that I neglected the probability of many casual changes in Matthew's treatment of the traditions. But I simply skipped most of the casual changes as not worth commenting on. Moo sees more of them than I do, but harmony with Matthew's unmistakable theological emphases looks like a good reason to see a change as deliberate rather than casual, especially when Matthew makes it time and again.

Moo asks whether the term "brother" must always possess theological significance and answers, "Of course not," with particular reference to Matt 1:2. But the intrusiveness of the term in 1:2 (most of the names in the genealogy stand alone), its absence in 1 Chron 1:34b (the OT genealogical source), and the omission of the term at other points in Jesus' genealogy where it would be appropriate (e.g., we could easily imagine the addition of "and his brother Esau" to Jacob's name—but that would not serve the purpose of comparing the brotherhood of the Israelite nation with the Christian brotherhood) all point to its theological significance here as elsewhere in Matthew.

Again Moo asks, "But what is there about 14:15 that makes Gundry conclude that Matthew goes out of his way to insert it [the phrase 'when evening came'] there, while he does not think that he has to go out of his way to insert it in 14:23 (where the Markan parallel also lacks it)?" But the parallel to Matt 14:23—viz., Mark 6:46-47—does have the phrase. And the reason for saying Matthew goes out of his way to insert it in 14:15 is that it substitutes for another phrase ("And when the hour had already become late"; Mark 6:35) that contains several words for which he elsewhere shows some fondness (*ēdē*—3,0; *hōra*—6,5; *polys*—23,1). Why should Moo think I put the burden of proof on him when I show every willingness to cite evidence for my theological interpretations rather than simply demanding contrary evidence? I can accept the reasoning he attributes to me in the words, "Matthew is a creative theologian because he transforms the tradition in theological ways." But the rest of his sentence ought to have the additions I enclose in brackets: "Matthew should be seen as transforming the tradition in theological ways [where Moo has doubts] because he is a creative theologian [where Moo is 'willing to acknowledge Matthean theology' of the same sort]."

Yet again Moo asks, "If Matthew were recording from his own reminiscences an incident in the life of Jesus, would we not expect to find his own vocabulary?" Not entirely—not by any means. Suppose, e.g., that Matthew were writing the parable of the sower from his own memory of Jesus' speaking it in Aramaic. How many of the following non-Mattheanisms, which I listed in my "Response," could he replace with other Greek words that are both synonymous translations for the Aramaic and favorites of his elsewhere: *embainō*, *speirō*, *peteinon*, *katesthiō*, *petrōdēs*, *exanatellō*, *bathos*, *kaumatizō*, *rhiza*, *xērainō*, *akantha*, *apopnigō*, *hekaton* and *hexēkonta*? Not many. He would even be hard pressed to find synonymous translations that are not favorites of his elsewhere. So Moo has only avoided my argument that the absence of non-Mattheanisms coupled with a profusion of Mattheanisms, tight parallelism, OT phraseology, and unmistakably Matthean theological emphases points to Matthew's creativity because a more straightforward use of tradition would leave a number of non-Mattheanisms visible.

Moo's tongue-in-check suggestion that the parallels between Matthew's story of the guards at the tomb and Luke's story of Jesus' walk to Emmaus "are as close as others Gundry interprets in similar fashion" is unconvincing, and will remain so until he details the parallels, compares them with the others he has in mind, and does better than the single example of *kleptō* as a verbal play on *Kleopas*. "Why could Matthew not have spun the story of the guards off one of these lost traditions [to which Matthew no doubt had access]?" If "spun" implies heavy redaction, I believe that he did—i.e., that he heavily redacted an otherwise lost tradition (we have no canonical parallels), the core of which is represented by the non-Mattheanisms listed in my "Response." Why do I think these non-Mattheanisms represent history, since the story could hardly be told without these words even if it were legendary? For the same larger reasons Moo has for believing in the general historicity of the gospel tradition. Also for the reasons of 28:15b (see my "Response") and of Matthew's limiting himself to his favorite diction, OT phraseology, and contextual borrowings where he seems to be creative in other parts of his gospel.

In complaining that "the inadequacy of Gundry's method here illustrates the quagmire of historical uncertainty into which his approach leads us," Moo commits "the fallacy of argument *ad consequentiam* . . . an attempt to prove or disprove a reasoned argument by reference to the consequences which flow from its acceptance or rejection."⁷ Nobody treats all parts of the Bible as historical; so to avoid "the quagmire of historical uncertainty" Moo needs to tell us what clean-cut, obvious way there is to distinguish unhistorical material.⁸ If he really does want a storm-free text on matters of historicity, harmonizing as much of the gospels as possible and explaining the remaining problems as a result of loose language will not deliver the goods. For the harmonizations often disagree with one another, and loose language lets in midrash under an alias. The domino theory works in both directions: If Moo can push the row from one end with the result that the last domino falls into the quagmire of historical uncertainty, I can push the row from the other end with the result that the last domino falls into the quagmire of obscurantism. Let us not think that such obscurantism does not threaten us. When in the interest of historicizing harmonization a respected scholar doubles the Zechariah who dies as a martyr in the court of the temple and multiplies the fires in Caiaphas' courtyard,⁹ the danger shows itself to be real and present.

On discrepancies between Matthew and the other synoptics, I can agree that it was normal to wear sandals. Mark's Jesus allows them. I take this allowance to be historical. The prohibition of sandals is due to Matthew's unhistorical redaction (cf. Diogenes Laertius 6.34, cited in the *Commentary* 187); so Moo is confused when he asks, "Can Gundry or anyone else plausibly argue that Jesus would have sent the disciples on a journey through the rough Palestinian countryside with bare feet?"

⁷D. H. Fischer, *Historians' Fallacies* (New York: Harper, 1970) 300-302.

⁸Cf. my *Commentary* 633, 637-638.

⁹G. Archer, *Encyclopedia of Bible Difficulties* (Grand Rapids: Zondervan, 1982) 337-341. Other examples are available from other evangelical scholars.

Again I can agree that "it is quite reasonable to suppose that Jesus forbids packing an extra tunic." That is what the Biblical texts say. But as the *Commentary* notes, "When he [Jesus] wants to prohibit taking more than necessary . . . he says, 'Do not acquire . . . two tunics,' but not 'Do not acquire . . . two pairs of sandals or two staffs' " (187-188). Moo has not refuted this argument. But he asks whether I do not have to "resort to some sort of harmonizing explanation" for Luke 10:4, which also disagrees with Mark in prohibiting sandals. No, because I believe that Matthew influenced Luke here (*Commentary* 186-187), that Luke's standard of historiography allowed his taking over a tradition containing such an unhistorical element (just as our standard of historiography may allow us to take over items in a way that will look uncritical in spots to future historians—cf. the *Commentary* 637-638), and that that allowance did not run so deeply or widely that we should classify him, against his prologue, with Matthew as another midrashist (cf. the *Commentary* 628 and my ETS paper, pp. 11-12).

It is strange that Moo should bring up the word *parachrēma* in Matthew's version of the withering of the fig tree. For it stresses immediacy even more forcefully and strictly than the word he ordinarily omits from Mark.¹⁰ Moo's observation thus backfires on his argument by sharpening Matthew's contradiction of Mark's chronology.

Moo cites three OT passages to argue that Mark's phrase "from the roots" refers to completeness rather than to gradualness of withering. But to this phrase Job 18:16 adds "the branch above" to get the notion of completeness. In Ezek 17:9 uprooting marks the start of a process that ends in withering. Similarly in Hos 9:16 the drying of the root will somewhat later result in no fruitbearing. Thus though I do not deny the thought of completeness, the cross-references support my interpretation that "withered from the roots" most naturally refers to a withering that started in the roots and spread to the rest of the tree by the next day, in contrast with Matthew's version, where strict immediacy makes "from the roots" inappropriate.

Moo's further comments on this pericope I can only regard as desperate: the hint that Matthew has another source; the defining of *parachrēma* so as to allow an interval of twenty-four hours despite Matthew's usual omission of the notion of immediacy and despite his double insertion of it here with an even stronger word than Mark's favorite, and despite his omission of "from the roots" and of Peter's lone observation the next day according to Mark; and the suggestion that "we refrain from seeking chronology in a narrative that does not intend to give it," again despite Matthew's double insertion, usual omission of immediacy, and so on.

In denying that Matthew's emphasis on the disciples' understanding distorts the facts as presented by Mark, Moo is swimming against the current of Matthean scholarship. Is he right in doing so? Let us take his question concerning the feeding of the 5000: "But why do Matthew's omissions of some of the clearer expressions of the disciples' doubts in Mark result in a distortion?" Such omissions in an isolated pericope probably would not. But when this sort of difference from Mark appears time after time, often more strongly than in the pericope about

¹⁰See esp. W. R. Telford, *The Barren Temple and the Withered Tree* (JSNT Supplement Series 1; Sheffield: JSOT, 1980) 74-75, 87. LSJ gives the meaning, "on the spot, forthwith, . . . at the moment." *Ek tou parachrēma* means "on the spur of the moment."

the feeding of the 5000 (in my "Response" I offer several other examples that Moo does not answer), we can hardly fail to speak of historical distortion. I have never denied, and I know of no one else who has denied, that Matthew's presenting the disciples as having little faith is "clearly derogatory." But that presentation does not negate Matthew's historical distortion in ascribing to the disciples understanding in place of ignorance. Little faith and ignorance are not identical. Mere "selection of different themes" does not satisfy the wording of some of the texts, as those who reread my examples will see for themselves.

The attempt to minimize the distinction between pre- and post-enlightenment historiographies does not pass muster. Certainly Christians were concerned "for detailed historical accuracy" for seventeen centuries. But then an exponential jump occurred in such concern, and methods of criticism developed that increasingly refined the definition of detailed historical accuracy. W. Nelson (*Fact or Fiction* [Cambridge, MA: Harvard, 1973]) gives an historical survey that should make those who insist on purely factual reporting in Biblical narratives face up to the narrowness of their literary vision and should lead them to ask whether some of that narrowness owes more to modern intellectual history than to the Bible (or than to early and medieval Church tradition).

I used the label "Church traditional bias" (qualified with the introduction "I often wonder whether . . .") for "*objections* to my view" (italics added), not for the opposite view (so Moo). The distinction is not picayune, because Moo's redaction of my phraseology makes it appear that I—to use his own words—"ignore the fact that all scholars, in or out of the Church, have understood Matthew that [historicizing] way," whereas my original wording merely notes the possibility of natural resistance to a new idea. Besides, I have repeatedly emphasized that large midrashic elements in Matthew do not eliminate the historical elements alongside them. Moreover, it is untrue that "all scholars" have understood Matthew as writing with a purely historical intention. And many a scholar has recognized that historical intention in the modern sense implies far more refined historiography than characterized historians before the enlightenment.

If one wants to talk of "temporal arrogance," he might consider the possibility of imposing a modern understanding of historical intention on Biblical writers and the danger of having this modern understanding outdated in time to come (cf. my ETS paper, p. 35). And if we believe the Spirit is still at work in the Church (as Moo believes the Spirit "has been at work" in it), a better understanding of Matthew's intention becomes theologically possible. It is not as though there has been no progress of understanding throughout the centuries. Conservative as they are, Moo's own critical views of the gospels (historical questions included) show huge advances beyond anything we read in the fathers, scholastics and reformers (cf. my ETS paper, pp. 34-36).

Moo's "second (and ultimately most important) reason for seeking historicizing explanations of Matthew's peculiarities is that the gospel provides the reader with ample clues that the author intends his narratives to be understood as factual, albeit interpreted, accounts." It takes a while before Moo comes around to identifying these clues (if he identifies them at all—I give him the benefit of the doubt), but when he does they turn out to be Matthew's (1) following Mark's order of events "very closely"; (2) taking over "the exact wording of Mark and Q frequently"; and (3) when departing from them writing "nothing that is obviously

incompatible with their portrayals of Jesus' person and ministry." But Matthew does change Mark's order of events, drastically and often, particularly in the first half of his gospel. Just look at a synopsis. He changes the wording of Mark and Q very often. I cannot fathom how Moo can talk of Matthew's taking over "the exact wording" of Q "frequently" after he, Moo, has urged against me that "it is almost impossible" to reconstruct the original wording of Q. The phrase "nothing obviously incompatible" presumes what is to be proved in a debate where the recent history of interpretation puts complete historical compatibility under increasing doubt in evangelical as well as nonevangelical circles. Despite Moo's phrase, it is quite obvious to many (perhaps most) scholars that the gospels are not always historically compatible.

Moo says I do not address myself to the psychology of midrash. But I did address myself to this question, and in some detail. See the long middle paragraph on p. 636 in the *Commentary*. Moo's colleague D. A. Carson made the same charge in his review, and I called attention to its falsity in my ETS paper (pp. 31-32). Surprisingly Moo, who read his paper in the same session where I read mine, has overlooked both passages. This is the third time he has overlooked a whole paragraph or more and based a criticism on the oversight. I need only refer readers to my *Commentary* and ETS paper.

On the question of the acceptability of mixing history and nonhistory, readers may look at the lengthy discussions in my *Commentary* (630-636) and ETS paper (29-36) and review the last four paragraphs of my "Response." I see only a few details in Moo's "Rejoinder" that need additional comment. He can cite Lucian, Polybius and Josephus for a purely factual ideal of historiography. But those citations do not prove that Matthew set out to write according to that ideal, or that his readers would have insisted on his doing so, or that this ideal was the only acceptable one in the ancient world, or even that those three historians always had the same notion that we have of what a "fact" is (a question that arises especially because they did not always live up to their ideal—cf. Moo's admission—to the degree we would expect if they did share our notion in all respects). Furthermore, Moo moves a bit too fast from Lucian and Polybius, who were Greeks writing for Greeks and Romans, to Matthew, who was a Jew writing for Jews used to midrash. The qualification "a bit" pays due to the hellenistic element in Matthew and his audience but does not negate the point. Josephus, though a Jew, not only wrote in Greek, as Matthew did, but also adopted a Greco-Roman model and wrote for a Greco-Roman audience. (Even so, midrash crept into his writing.) The synoptic data and comparable Jewish literature of the NT era speak far more eloquently and directly to the issue before us. I have already treated Matthew's and his readers' concern with "the fact of God's intervention in history in the recent past" (Moo's phrase, which says nothing disagreeable to me but which does not necessarily imply, as he thinks it does, lack of homiletic freedom such as we see in midrash).¹¹

Moo argues that "Gundry . . . also admits that if Matthew did not furnish sufficient clues to his readers of his intention, then he was either a deceiver or inept (*Matthew*, p. 632)." But the burden of p. 632 (and of pp. 634-635) is to argue that we overestimate the need ancients felt to distinguish always and consciously be-

¹¹See my *Commentary* 637-638; ETS paper, pp. 42, 45; "Response," final paragraph.

tween historical facts and embellishments. Hence we also overestimate the need for clues in order to save Matthew from the charge of deception or ineptness (see also my ETS paper, pp. 31-32, and the third-from-last paragraph in my "Response"). When Moo says, "Thus the nub of Gundry's case is that the first-century reader would have recognized that Matthew was taking inaccuracies with his sources and that this would have signaled Matthew's intention to write not history but edifying fable," he uses pejorative terms ("inaccuracies" and "fable") that we do not use for similar, accepted preaching practices right up to the present. Rather we speak of "homiletic liberty," "sermonic license," and the like (cf. my discussions of R. C. Sproul's sermon on inerrancy [*Commentary* 631-632] and of Carson's attempted refutation [ETS paper, pp. 19-20, 29-31, 45-46]).

Yes, in my "Response" I did back off from the term "literary genre" with respect to Matthew's midrashic practice, and I can understand why Moo says "backs off" in view of my statements on p. 632 of the *Commentary*. But those were general statements not directly related to Matthew. It is less excusable for Moo to omit that in my section "The Literary Form of Matthew" I started by saying, "The first gospel defies easy classification," and ended with the conclusion, "Matthew remains a literary anomaly" (599). At the head of my later discussion concerning Matthew and midrash lies the statement, "There are differences between the Gospel of Matthew and midrash and haggadah in ancient Jewish literature" (628). I say guardedly that Matthew has "midrashic flourishes" (632); that "in such places Matthew . . . is writing as a midrashist and haggadist" (633); that there are "midrashic and haggadic characteristics in the first gospel" (634); that Matthew exhibits a "midrashic and haggadic style" (635-636 *ter*); that "Matthew's gospel contains midrash and haggadah" (637); that "elements of Matthew" can be classified as midrash and haggadah (637); and that "Matthew edited . . . in accord with midrashic and haggadic practices" (639; italics added throughout). Therefore it would misrepresent me to imply that I identified Matthew as a whole with the literary genre midrash. The bulk of Matthew still passes on a huge amount of historical data.

"If . . . free embellishment was the sole characteristic that Matthew shares with midrashic-type works, while in form and content his gospel differs from these," Moo asks, "how could the reader have made this identification?" He means the identification of Matthew with "the category of writings that did not have historical intention." Add "entirely" before "historical intention" to avoid a straw man I have consistently denied. More to the point, the "midrashic-type works" differ among themselves in form and content. Yet we easily recognize in them the spirit of free embellishment; so Matthew's difference in form and content poses no problem. The fourth-from-last paragraph in my "Response" has already answered the question of how a first-century reader could have recognized free embellishment in Matthew (cf. my ETS paper, pp. 36-37). Moo persists in failing to recognize that he has as much of a problem—in view of the kinds of differences among the synoptics, probably a greater problem—in telling how a first-century reader could have recognized that Matthew is thoroughly historical.

"Why not compare Matthew rather with 1 Maccabees or Josephus' *Wars*?" Because comparison with the other synoptics shows kinds of differences that look like the changes Jewish midrashists made in "preaching" the OT. "Why not associate Matthew with a Greco-Roman literary genre—the 'laudatory biography,'

for example?" I have given my reasons in a review of P. L. Shuler's book (which Moo cites) to appear in *Reformed Journal* 33 (June 1983). Moo's noting that the Greek language of Matthew sets it apart from most Jewish literature of the NT era has little probative force now that we appreciate more than ever how mixed were the Jewish and Greco-Roman cultures in the hellenistic worlds, including Palestine.¹² Cf. Josephus' universally acknowledged incorporation of midrashic traditions in his *Antiquities*, written in Greek.

"What may or may not have been acceptable among first-century Jews may or may not have been acceptable among first-century Christians"—true. How then do we decide between the possibilities? By comparing Matthew first with the other synoptics to see whether the results of that comparison tally with a further comparison between Jewish midrashic works and the OT. Moo claims that "the importance of recent history for religion decisively differentiated Christians from 'mainstream' Judaism." But, as I indicated in the "Response," we see midrash on recent history already in the OT. And on what grounds does Moo exclude—with his qualifier "mainstream"—a comparison between Christians and the covenanters at Qumran, for whom recent history was decisive? And in view of 1-2 Maccabees how can he be so sure that mainstream Judaism differed so radically as he thinks? In particular, 2 Maccabees bestows some lavish amounts of midrashic treatment on recent history.

In another connection I have criticized Moo's saying that Matthew "follows Mark's order of events very closely, takes over the exact wording of Mark and Q frequently and, when departing from them, writes nothing that is obviously incompatible with their portrayals of Jesus' person and ministry." Moo uses these observations to argue that "Matthew is significantly more faithful to his sources than those works that can appropriately be labelled 'midrashic'." But Jewish midrashists could and did follow the order of the OT text, take over its exact wording and, when departing from the text, write things compatible with it (Jubilees and the *Genesis Apocryphon* provide good examples). And like Matthew they could and did also step over the traces in these respects.

Verbal similarities among the synoptics are often striking. But the dissimilarities are often just as striking. Moo's quoting F. G. Downing to the contrary does not erase this well-recognized fact, and Downing himself goes on to say in his same paragraph that "there *are* considerable divergencies" (*italics* his) among the synoptics, divergencies that, as the rest of his article makes clear, Moo would find unacceptable.

Nor does Moo's quoting Downing against an analogy between Jewish midrashists' taking liberties with the OT and Matthew's taking liberties with Mark and Q adequately substitute for a refutation of numerous similarities in detail that I set forth in my "Response." Moo does not take up a single example from among the many examples I cited. He can skirt the similarities only by overestimating Matthew's faithfulness to his sources—i.e., by glossing over his changes of Mark and Q. Besides, all the details of Downing's argument arise out of a comparison between treatments of sources by Josephus and Luke, not Matthew. And

¹²On this point M. Hengel's two volumes *Judaism and Hellenism* (Philadelphia: Fortress, 1974) provide the standard.

an attentive reader of Part I of Downing's article¹³ will note parallels between Josephus' and Matthew's redactions (e.g., Josephus' addition of "gifts," "a dream," "joy," to the story of Ehud—cf. Matt 2:1-12; Josephus' feeling "quite free to create a fresh order of events"—cf. Matthew on the cleansing of the temple and withering of the fig tree; Josephus' habit of conflation—cf. "Conflation" in my Topical Index; Josephus' shifting words from one speaker to another—cf. Matthean examples mentioned earlier in this "Surrejoinder"). Though Downing does not think Josephus created " 'out of his head' any major incident," he does think Josephus "inherited, it would seem, an already extensive tradition of rabbinic *midrash*"¹⁴ (cf. my paralleling Matthew's *midrash* and that of Josephus "or his source"—"Response" 17). Worst of all, the lengthy passage Moo quotes from Downing takes aim against those who, like J. Drury and M. D. Goulder (whom Downing cites), replace a Q full of dominical tradition with wholesale intervention by Matthew and Luke, a view quite unlike mine, where Q is not only accepted but also expanded. My "Response" contains a discussion of degrees of midrashic liberty.

To defend the thoroughgoing historicity of Matthew, Moo appeals to P. L. Shuler, who argues "that the gospels are closer to the hellenistic *bios* than to any other form." The appeal self-destructs, for Shuler emphasizes that ancient authors themselves distinguished the *bios* from history-writing and injected into the *bios* all sorts of historically false and distortive material to laud their heroes.¹⁵ Perhaps not less self-destructive—in view of Moo's earlier stress on "detailed historical accuracy"—is his using the term "historian" for Matthew "in a loose sense to mean someone who is writing about historical events."

I grant that in writing about recent history (in much of which he himself played a role) Matthew is more like Josephus in his *Jewish War* than like Josephus in his *Antiquities*. But we should note H. St. John Thackeray's argued conclusion that "notwithstanding the emphasis laid on the author's [Josephus'] personal contributions as eyewitness and reporter, it is probable that, apart from the early Galilean campaign and a few other scenes in which he plays a prominent part, his own notes and recollections are a comparatively minor factor in the narrative [of the *Jewish War*]. The bulk of it appears to be derived from a documentary source of Roman origin."¹⁶ And the sacredness of the traditions Matthew took from Mark and Q make him look more like the Josephus of the *Antiquities* even in Moo's view, according to which Matthew drew the bulk of his material from Mark and Q. Comparison with Mark and Luke does not encourage the view that Matthew is more like the Josephus who in his *Jewish War* has "a far greater concern for 'objective,' factual history." Thackeray sharply qualifies that concern, moreover; and even H. W. Attridge, whom Moo tries to turn against me, ad-

¹³In *JSNT* 8 (1980) 46-65.

¹⁴*Ibid.*, p. 47; see also pp. 55-56, 60.

¹⁵Cf. Moo's reference to H. Koester's discussion of history-writing in the ancient world. Since the gospels look more like biographies than like general works of history, he might have gone on to cite Koester's immediately following description of ancient biographies as "uncritical": "anecdotes, legends, and romantic glorifications predominate."

¹⁶H. St. J. Thackeray, *Josephus: The Man and the Historian* (New York: Ktav, 1967 reprint [1929]) 37.

mits that "the claim of Josephus to detached, non-partisan objectivity is hardly supported by the details of the narrative of the *Bellum [Jewish War]*."¹⁷

Dismiss these qualifications and Moo has still not gained his point, for Josephus states his intention outright. Matthew does not. His failure to renounce embellishment, as Josephus does, leaves no such basis on which to rest the charge of plain and simple errors if he disagrees historically with Mark and Luke. And what will Moo say of modern preachers, particularly evangelical ones, who emphasize not adding to or subtracting from the Bible, but who embellish it, including its historical sections, a great deal in their sermons and do so with acceptance, understanding and appreciation on the part of audiences who share their strict views of the Bible? "Errors, plain and simple," does not allow enough for cultural differences in the definition of an error.¹⁸ I will not argue for Josephus' inerrancy; but, if I were Moo, before charging Josephus I would ask myself whether what looks like unembellished truth in the twentieth century will look entirely the same another two thousand years from now.

To Moo, "it seems plain that Matthew is to be compared with Mark and Luke rather than with other Jewish works, and the comparison must extend to a concern for historical accuracy." But the number and character of Matthew's differences from Mark and Luke contradict both the exclusion of other Jewish works from the comparison and the exclusiveness of a concern for historical accuracy. Matthew is to be compared both with Mark and Luke and with other Jewish literature. And we need not presuppose that taking homiletic liberties at some points negates a concern for historical accuracy at other points, or negates concern for the historicity of the traditions with which homiletical liberties are taken. "Why should Papias be the only one in Church history, Gundry besides, who correctly understood Matthew's intention?" Today the woods are full of scholars who, though they may not agree with me in every detail or use the description "mid-rashic," do not believe that Matthew intended to write unembellished history.

On the outside the hammers are dinging, as Moo says, "from both ends of the theological spectrum." But let me assure him that inside I have never felt more comfortable. I do not mean comfortable in the smug assurance of thinking I know the answer to every theological question or the solution to every problem in Scripture, but comfortable in the joyful discovery that through grammatical-historical exegesis I can reap the best fruits of both historic Christianity and modern Biblical scholarship. What Moo calls a "halfway house" is for me a "house of feasting" from which I see no need to move.

¹⁷*Ibid.*, pp. 47-49; Attridge, *Interpretation* 50.

¹⁸Cf. Downing, "Redaction Criticism" 48.