

ONCE AGAIN, "MATTHEW AND MIDRASH":
A REJOINDER TO ROBERT H. GUNDRY

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The "genuine dialogue" that Robert Gundry perceives to exist between him and me will, I hope, be maintained in this rejoinder. "Dialogue" implies a willingness to learn and a charitable attitude toward those with whom we may disagree. Surely these qualities should characterize any discussion held within the confines of a Christian academic theological society. To charity must, of course, be added a passion for the truth as the individual sees it. I sense that Gundry and I both share that as well. Who of us possesses the clearer vision must be left for others to decide. In this rejoinder I would like to expand on some of my arguments in light of Gundry's criticisms. In doing so I will not always repeat the previous discussion. The reader is therefore encouraged to read the two foregoing papers carefully before this one. Since Gundry's "Response" follows the order of arguments in my original critique, I will use that same order to structure this rejoinder.

Gundry's response to my criticisms of his source theory are instructive. Essentially, he replies, the commentary is to speak for itself. Markan priority is justified by the "adequacy and economy" with which it accounts for the data; the use by Matthew and Luke of an "expanded" Q can be assumed once Matthew's creativity elsewhere is seen; Matthew used few other traditions because passages peculiar to his gospel exhibit a large number of Mattheanisms. Further, we are told that no one can accuse Gundry of "assuming" Markan priority because he started with the opposite assumption.

Now as much as it may be helpful to know something of Gundry's thought processes, such information does not take us very far in assessing the logical validity of his case for this particular source reconstruction. The reader must look at what Gundry has written—and when he does, he will look in vain for an even-handed attempt to justify this source theory. In pericope after pericope Gundry explains Matthew's text according to his changes of Mark and Q. Alternative explanations are rarely considered. We are left, then, with Gundry's claim that this particular approach makes the most sense of the phenomena. But surely we are entitled to ask "Why?" What is it about this theory that makes it preferable to others? How can we know that this approach best explains the data if no others are mentioned? We seem to be left with Gundry's word for it.

We touch here on one of the major distinctives—and drawbacks—of the commentary: the lack of interaction with other views. This kind of approach no doubt has its positive side: All of us grow weary of sifting through masses of bibliography, assorted views, arguments and counter-arguments—wondering at the end of it just what the author said, anyway. Yet for all its drawbacks such a method at least assures us that the author has sought to deal fairly with views contrary to his own and furnishes us with reasons for accepting his view rather than another's. Gundry's commentary reminds me of the warning issued by James D. G. Dunn: "He who defines too closely what he is looking for at the start of a NT

study in most cases will find it soon enough, but usually in his wake will be left elements which were ignored because they were not quite what he was looking for and material and meaning will often have been squeezed out of shape in order to fit the categories prescribed at the outset."¹ I am sure that Gundry would reply that he came to his work with an open mind and that the phenomena convinced him that his theory was the only tenable one. Yet by failing to indicate how in specific passages his approach explains the text better than any other, he leaves himself open to this criticism.

As an illustration of Gundry's method at this point, let us look at the way he handles the interpretation of the parable of the sower (Matt 13:18-23). He assumes throughout that Matthew had Mark's text before him as he wrote: "Matthew drops . . .," "Matthew omits . . .," "Matthew inserts . . .," "Matthew revises . . .," "Matthew conforms . . .," etc., etc. 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.² Now one might think that I am making a mountain out of a molehill by saying so much about Gundry's source-critical assumptions. But, in fact, for Gundry the supposition about sources is a mountain. Thus in the passage before us Matthew's changes of the Markan text bear the weight of the interpretation. Gundry is particularly struck by the fact that Matthew twice "adds" to the Markan original the theme of understanding (vv 19, 23). He thus concludes that (with respect to v 22) "his [Matthew's] interest lies in the distinction between understanding and not understanding (see vv 19, 23), not in the four distinctions original to the interpretation" (p. 260). Yet the word *syniēmi* ("understand") stems from the Markan tradition and eventually, of course, from Isa 6:9 (Mark 4:12=Matt 13:13=Luke 8:10b, all alluding to Isaiah). To be sure, only Matthew quotes Isa 6:9-10, with its two references to *syniēmi* (which Gundry then counts as Matthean "insertions" in parallel material, even though there is no parallel in Mark and Luke and the language is not his but Isaiah's), but did Matthew quote verses because of the presence of *syniēmi* or (more likely to my mind) because he wanted to make more clear the OT background? But to return to the original point: If in fact Mark was not Matthew's source, if Matthew is transmitting rather accurately the pre-canonical tradition before him (as Wenham thinks), then we have no reason for thinking that "understanding" is a peculiarly Matthean theme here,³ and the suggestion that this is a Matthean theological redac-

¹J. D. G. Dunn, *Christology in the Making: A New Testament Inquiry into the Origins of the Doctrine of the Incarnation* (Philadelphia: Westminster, 1980) 9.

²D. Wenham, "The Interpretation of the Parable of the Sower," *NTS* 20 (1974) 299-319; cf. also "The Synoptic Problem Revisited: Some New Suggestions about the Composition of Mark 4:1-34," *TB* 23 (1972) 3-38.

³Wenham, "Interpretation" 309. Interestingly, Wenham himself points out the implications of his theory for the redactional study of Matthew and shows how those who find a motif of understanding in Matthew rest their case to some extent on these verses. If Wenham is right, it is questionable whether the references to the disciples' understanding in these verses really represent his own peculiar view (pp. 308-309 n. 5). I am inclined to think that the understanding of the disciples is something of a redactional theme in Matthew. But Wenham's argument demonstrates the need for caution in accepting so quickly the sorts of arguments one finds in Gundry's work.

tion, shifting the intent of "the original," has absolutely no basis. I cite Gundry's treatment of this passage because it is typical of his method throughout. He argues that the data have convinced him of Markan priority. But the data have convinced Farmer of Matthean priority,⁴ Lindsey of Lukan priority,⁵ Rist of independence,⁶ Léon-Dufour of dependence on Aramaic Matthew,⁷ and so on. Until Gundry supplies reasons in particular texts for his source assumptions, he is hardly likely to convince anyone else of his theory. Gundry's great confidence in the complete validity of his view of synoptic relations stand in stark contrast to the considerable caution now found among synoptic scholars on this question⁸ and is all the more surprising in light of the fact that he has tested it against only one gospel.

Gundry challenges his critics to "show in what ways the fit between hypothesis and textual phenomena is not so good as it appears to be." I think it can be demonstrated that for at least some pericopes, Mark's priority vis-à-vis Matthew does not provide a satisfactory solution. I would refer here to the studies of David Wenham mentioned earlier and the work of J. A. T. Robinson on the parable of the wicked vinedressers.⁹ With respect to Matthew's use of Q, the question is to be asked: How much of the textual phenomena is to be explained? Gundry seems to imply that he is referring to Matthew only. But if one is to argue that Matthew generally uses Q in the form in which it is found in Luke's gospel, one is called upon to demonstrate the probability that Luke is being so conservative with this tradition. In many cases this is most improbable. Take for instance the sermon on the plain (Luke 6:20-49). Lukan scholars generally acknowledge considerable Lukan redaction in this section, observable especially in the "social-economic" stress of the beatitudes and woes and in the downplaying of the OT element in vv 27-38.¹⁰ This would make any assumption that Matthew has used a tradition

⁴W. Farmer, *The Synoptic Problem: A Critical Analysis* (Dillsboro: Western North Carolina Press, 1976).

⁵R. L. Lindsey, "A Modified Two-Document Theory of the Synoptic Dependence and Interdependence," *NovT* 6 (1962-63) 239-263.

⁶J. M. Rist, *On the Independence of Matthew and Mark* (SNTSMS 32; Cambridge: University Press, 1978).

⁷X. Léon-Dufour, "The Synoptic Problem," *Introduction to the New Testament* (ed. A. Robert and A. Feuillet; New York: Desclée, 1965) 283-286.

⁸Joseph A. Fitzmyer, who builds a persuasive case for the "traditional" two-document source theory, nevertheless cautions that "we cannot hope for a definitive and certain solution to it, since the data for its solution are scarcely adequate or available to us" ("The Priority of Mark and the 'Q' Source in Luke," *Jesus and Man's Hope* [Pittsburgh: Pittsburgh Theological Seminary, 1970], 1, 132).

⁹J. A. T. Robinson, "The Parable of the Wicked Husbandmen: A Test of Synoptic Relationships," *NTS* 21 (1975) 443-461.

¹⁰With respect to the beatitudes and woes, some scholars think that Luke has taken them from his source (and Matthew omits them; H. Frankenmölle, "Die Makarismen [Mt 5, 1-12; Lk 6, 20-23]: Motive und Umfang der redaktionellen Komposition," *BZ* 15 [1971] 52-75), others that Luke himself adds them (J. Dupont, *Les Béatitudes* [Louvain: Nauwelaerts, 1958], 1, 299-342, followed by J. A. Fitzmyer, *The Gospel According to Luke, I-IX* [AB; Garden City: Doubleday, 1981] 627). But in either case it is clear that Luke heightens the economic dimension of the beatitudes. This redactional motif in his gospel is as clear as (and perhaps clearer than) any redactional theme Gundry discerns in Matthew. Yet Gundry assumes

close to that preserved in Luke 6 very problematic indeed. Yet while acknowledging that “here and there we may detect the hand of Luke in his version of the beatitudes and woes,” Gundry assumes virtually throughout his comments on Matt 5:3-12; 21-48 that Luke’s version 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.”¹¹ The “textual phenomena” of both Matthew and Luke strongly suggest to my mind that each evangelist has redacted original dominical material, that it is very difficult to say which form of the sermon corresponds most closely to the original, and that in fact the more Jewish orientation of Matthew could be taken as evidence that his is closest to the actual words of Jesus. These observations could be duplicated in kind for most of the pericopes in which Gundry attributes to Matthew a utilization of Q in virtually its Lukan form.

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 pericopes with no clear parallel! How close is the fit when it must be argued that Matthew mentions two demoniacs in 8:28-34 to “compensate” for the omission of a Markan exorcism (pp. 158-159), that Matthew’s parable of the two sons is a “counterpart” to Luke’s parable of the prodigal son (p. 422), that Matthew “changes” the Lukan offering of a pair of turtledoves “into” Herod’s slaughtering of the babes in Bethlehem (pp. 34-35), that the parable of the unforgiving servant (Matt 18:25-35) is adapted from the parable of the two debtors in Luke 7:41-43 (p. 371), that Matthew, again, “adapts” the parable of the laborers in the vineyard to the parable of the wicked vinedressers (p. 395), and so on, and so on? Why should we think that Matthew has worked this way when verbal and material resemblance are often very slight? Gundry argues that Matthew’s known creativity with respect to Mark and Luke makes such “borrowing” understandable. But not only is there considerable question about the degree to which Matthew redacts Mark and Q

without argument that Matthew rewrites his beatitudes by means of the woes (*Matthew*, p. 68).

Scholars are not agreed on the situation with respect to the antitheses of Matt 5:21-48. Some think that all the antithetical formulations are traditional (J. P. Meier, *Law and History in Matthew’s Gospel* [AnBib 71; Rome: Biblical Institute, 1976] 127; E. Percy, *Die Botschaft Jesu* [Lund: Gleerup, 1953] 148-150 [on the crucial fifth and sixth antitheses]), others that some of them are (probably the majority view, following Bultmann’s seminal work, *History of the Synoptic Tradition* [New York: Harper, 1963] 134-136), still others that none of them is (cf. M. J. Suggs, “The Antitheses as Redactional Products,” *Essays on the Love Command* [Philadelphia: Fortress, 1978] 95-101). Again the point is that it cannot be assumed, without close argumentation, that Matthew is redactional while Luke is traditional. Yet this is exactly what Gundry does.

¹¹I. H. Marshall, *The Gospel of Luke* (Grand Rapids: Eerdmans, 1978) 257. Marshall, following H.-T. Wrege (*Die Überlieferungsgeschichte der Bergpredigt* [WUNT 9; Tübingen: Mohr, 1968] 75-94) thinks that the differences between Matthew and Luke are great enough here that one must suppose they are following different forms of Q. Yet 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. Gundry throughout presumes that Matthew had before him a text virtually identical to Luke 6:20-38. (While, as we saw above, Gundry does allow Luke some editorial hand in the material, he explains that “none of Matthew’s dependencies has to do with likely Lukanisms” [p. 69]. But how can we know this? Most Lukan scholars would take strong exception to it.)

(as we suggested above). The jump from discovering that an author redacts traditions to the assumption that virtually everything an author says must be a redacted tradition is surely a long one. Gundry rehearses his reasoning for us at this point: "If Matthew can change Mark and tradition Q so freely . . . could it be that he had a nativity tradition like Luke's and changed it with similar freedom?" Yes, I would reply, it could be. But is it likely? Is this a better hypothesis than others? Does it make more sense to think that Matthew turned the Lukan visit of the shepherds into the adoration of the Magi, the going up to Jerusalem of Jesus' family in Luke 2 into the flight into Egypt, the "praiseful return of the shepherds" (Luke 2:20) into "the Magi's flight from persecution" or that Matthew is reporting reliable tradition or just making the whole thing up? Either of these last two options seems to be a better explanation of the data than the incredibly strained effort to link Matthew's text with Luke's. Gundry claims that his approach must be evaluated on the basis of the changes Matthew can be demonstrated to be making with his known sources, Mark and Q. But two things have to be said about the argumentation at this point. First, I can think of no place where Matthew's use of Mark or "traditional" Q comes anywhere near to looking like what Gundry finds in the infancy narrative and at other points in the gospel where a parallel tradition does not exist. Second, any theory must account for all the data. A theory that requires the implausible correspondences uncovered by Gundry has something very wrong with it.

Ultimately, Gundry's source theory can "fit" the data of the text only because the text on which it is being fitted has already been tailored by Gundry's approach to Matthew. Once the latter is accepted, the source theory does not appear implausible. But too much of Gundry's case for Matthew's method depends on the source theory. In other words, the argument appears to be circular to an inadmissible degree.

Despite Gundry's response, I still find his statistical method equally biased. Of concern here is the question of what should count as "insertions in parallel material." The issue is not, as Gundry suggests, whether Matthew adds sentences to the tradition. The question is rather how much material in Mark and Luke one should allow as "parallel" in identifying Matthean insertions. Gundry continues to argue for the legitimacy of using the paragraph and characterizes any other suggestions as "assumptions." But the point I would make is simply this: Verbal insertions can validly be spoken of only where the specific material is parallel. 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. What is clearly the case here is that Matthew uses the parallel (if true parallel it be) as a jumping-off place for further material of his own. To be sure, one may well want to characterize this material as an insertion, but it is hardly appropriate to speak of all the individual words as insertions. Nor is this procedure an "assumption" no better than the one Gundry makes. It is rather the only way to justify retention of the term "insertion." Again, Gundry's technique is instructive. The use of the sentence for the basis of comparison in such studies is usual.¹² Yet Gundry chooses a

¹²See e.g. L. Gaston, *Horae Synopticae Electronicae* (SBL SBS 3; Missoula: Society of Biblical Literature, 1973).

different method that just so happens to provide a better statistical basis for his theories of Matthean creativity. I grant that statistical methods are inevitably somewhat subjective. But the degree of subjectivity can be reduced by allowing for possible bias (instead of catering to it, as Gundry does), by providing a complete statistical picture and by interpreting rather than simply citing statistical figures. Thus, for instance, would a case for Matthean creativity built on the basis of statistics demonstrating Mattheanisms look as strong if similar statistics relating to non-Mattheanisms were cited? How can one set be significant unless we know how many are found on the other side? One may as well poll the American public and conclude that Americans are against nuclear war at any cost because five million people responded in that way. Until we know how many were polled, or are given a percentage figure, the sheer number is nothing more nor less than useless. With respect to words peculiar to Matthew's diction, one must always ask about the size of the data base. Are, for instance, ten words a significant number of unique occurrences, granted that there are approximately 300 unique verses and 6000 unique words in Matthew? They may or may not be, depending on the word. But the point is that, whatever Gundry may say in his "Response," he lists uninterpreted statistics throughout the commentary and draws significance from them. If the statistics vary in significance, then he should say so.

It is this last point that I was trying to make (with admittedly poor phraseology) in my initial response: Gundry's statistics are consistently skewed (for the reasons mentioned earlier), but I would not disagree with his labeling most of the words he does as "Matthean." The point is, though, how "Matthean"? Do the data enable us to say that a word is certainly, or probably, or possibly Matthean? Until such qualifications, based on accepted methods of interpreting statistics, are included, I would maintain that we are being given a biased view of the extent to which Matthean diction can be found in any particular pericope.

We come now to what is the heart of the issue raised by Gundry's commentary: the nature of Matthew's redactional activity. Gundry, it will be recalled, argues that Matthew creatively embellishes the dominical tradition in the interests of providing a homiletical "reflection" on the significance of Jesus. He finds evidence for this creativity in the way Matthew transforms the tradition in accordance with observable Matthean theological interests. Several matters call for comment.

First, Gundry denies that he has "overtheologized" Matthew's redaction. My suggestion, that many of the changes he attributes to theology should be attributed to nontheological factors, he labels a "guess." But, surely, while not capable of mathematical proof it is more than that. It stems from the consideration that many changes made in the copying of written materials, when exact verbal accuracy is not required, are "casual" changes, due to the copyist's own habits of speech, a desire to abbreviate or expand, or pure inadvertence. None of these factors is allowed to play very much of a role in Gundry's analysis of Matthew's redaction. Whether Gundry has overemphasized the theological motivations in Matthew's redaction must be left for the reader to judge for himself. But two examples that Gundry brings up will suffice for me to make my point. I had criticized Gundry for thinking that the addition of "and his brothers" in 1:2 had theological implications (cf. also Matt 17:1, where Gundry argues that the addition of "his brother" to the name of John, along with the use of a single definite article

with the three names, emphasizes Christian brotherhood). Gundry replies that passages such as 4:18-22 demonstrate Matthew's interest in bringing out the "brotherhood" theme of discipleship as a means of application to the Church of his day. But while the term "brother" is undoubtedly theologically significant in many texts in Matthew (cf. 12:46-50; 18:15-35), must it always possess theological significance? Of course not, and I am sure Gundry would agree. What, then, are the demonstrable textual data that justify us in reading theological significance into the term in 1:2; 17:1? To take another example: Gundry suggests that the phrase "when evening came" in 14:15 is, despite Matthew's fondness for it, significant theologically because Matthew does not go out of his way to insert it elsewhere (except perhaps in 16:2). But what is there about 14:15 that makes Gundry conclude that Matthew goes out of his way to insert it 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)? Gundry suggests that the other allusions to the last supper narrative in the feeding-of-the-5000 episode encourage us to think that this phrase is a conscious parallel also. But if one is not persuaded by the other parallels (as I am not), what value does this have? Apparently Gundry is taking the tack that the burden of proof rests on those who deny that a particular Matthean editorial change is theological. To the extent that Matthew's emphases vis-à-vis Mark and Luke can be discerned, I am willing to acknowledge Matthean theology. But such changes are not nearly so abundant as Gundry implies. Once again we are left with an argument that comes close to being circular. Matthew is a creative theologian because he transforms the tradition in theological ways; Matthew should be seen as transforming the tradition in theological ways because he is a creative theologian.

Second, Gundry infers Matthean creativity from the fact that "we rarely discover non-Mattheanisms in peculiar passages—we find, rather, a profusion of Mattheanisms." I will not again detail here my reasons for thinking that this evidence from Matthew's diction is not very conclusive. What is more interesting is that Matthew is apparently not allowed to write in his own words about things he himself saw and heard. If Matthew were recording from his own reminiscences an incident in the life of Jesus, would we not expect to find his own vocabulary? Gundry maintains that Matthew would have been as faithful to the "brute data of Jesus' words and deeds" as he is to the wording of Mark and Q. But the difference is, of course, obvious: When using Mark and Q, Matthew was following tradition already recorded in Greek; when reciting the words and deeds of Jesus from his own memory, no such Greek tradition existed (I am assuming that Jesus spoke, at least most of the time, in Aramaic¹³). Matthean diction in passages peculiar to Matthew cannot be used as evidence that the incidents or words recorded are not historically reliable.

This is the place to bring up a further point about Matthew's use of traditions other than those recorded in Mark and "expanded" Q. As we have seen, Gundry is anxious to confine Matthew's use of tradition, as much as possible, to these two sources. As I pointed out in my initial paper, however, Gundry regards the story of the guard at the tomb as historical despite numerous Mattheanisms. He justifies this by citing a number of non-Matthean words in the narrative and by point-

¹³Note Gundry's own remarks here ("The Language Milieu of First Century Palestine: Its Bearing on the Authenticity of the Gospel Tradition," *JBL* 83 [1964] 404-408).

ing out that we have no access to a tradition from which Matthew could have derived the account. Now with respect to the first point it must be noted that five of the seven “non-Matthean” terms are rather obviously due to the subject matter of the narrative. The remaining two are hardly significant. But the second point is even more instructive. Gundry is so convinced that Matthew’s embellishments stem from previous tradition that, lacking previous tradition here, he infers historicity. But why could not Matthew’s story of the guard at the tomb be his attempt to replace Jesus’ walk to Emmaus with the two disciples (Luke 24:13-35)? The parallels are as close as others Gundry interprets in similar fashion (and the use of *kleptō* in Matt 27:64 may even be a verbal play on the name *Kleopas* in Luke 24:18). I make this suggestion facetiously, of course, but it makes a serious point: How do we choose which incidents recorded only in Matthew are historical? Gundry suggests that the existence of tradition from which Matthew could have derived the narrative is crucial. But does it make sense to base such crucial judgments on the vicissitudes of history? There can be no reasonable doubt that Matthew had access to traditions that are now lost. Why could Matthew not have spun the story of the guards off one of these lost traditions? The inadequacy of Gundry’s method here illustrates the quagmire of historical uncertainty into which his approach leads us.

A third issue relating to Gundry’s understanding of Matthew’s redactional method is what to make of the apparent contradictions between Matthew and Mark. Gundry claims that these discrepancies demonstrate beyond any doubt that Matthew and Mark cannot both be “inerrant” if both are writing with historical intent. And since Matthew’s “changes” of Mark fall into clear tendential patterns, the discrepancies cannot be accidental but must be deliberate. All this gives Gundry what is perhaps the key to his novel approach to Matthew. I cannot here detail explanations for all the alleged contradictions Gundry cites in his “Response.” For some of them, I am aware of rather convincing harmonizations that do not do violence to the contexts of either gospel. For others, satisfactory explanations are admittedly more difficult, but reasonable alternatives can nevertheless be suggested. Much will depend on how we view the gospels to begin with (we will say more about this in a moment). But lest I be accused of completely ducking the issue, let me comment on some of the contradictions Gundry brings up.

It is helpful to divide these into two classes: those instances in which an outright contradiction occurs, and those in which Matthew’s emphasis on a particularly theological theme leads to what Gundry calls “an historically distorted picture.” In the first category, Gundry mentions the fact that Matthew prohibits the disciples from “acquiring” a staff or sandals on their ministry, whereas Mark allows both staff and sandals. If Gundry’s interpretation of *ptaomai* as meaning “acquire” in Matt 10:10 were acceptable, the difference may be between what the disciples are to take for the ministry (Mark) and what they are to acquire during it (Matthew). But I find this suggestion implausible. Much more reasonable is the suggestion that, since it would be normal to travel with sandals on one’s feet (and 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?) and a staff, Matthew prohibits the taking along of extra baggage. Gundry does not like this suggestion because of his understanding of *ptaomai* and the reference

to "two tunics" in Matthew. But it is quite reasonable to suppose that Jesus forbids packing an extra tunic. Before leaving this pericope, it might be worthwhile raising a question for Gundry. Since Luke's account appears to contradict Mark in exactly the same way Matthew does Mark, and since you claim that both Luke and Mark are historical in intent, do you not have to resort to some sort of harmonizing explanation here—of exactly the same type you reject for Matthew?

Another difficulty of this sort is the chronology of the withering of the fig tree. Mark has Jesus curse it one day, and only on the next do the disciples perceive that it has withered. Matthew, on the other hand, stresses that the withering took place "immediately" (*parachrēma*). In his "Response" Gundry notes that Matthew usually omits Mark's stress on immediacy. True, but he does not tell us that the word Matthew uses here is never used by Mark. He also suggests that Mark's phrase "from the roots" (*ek rizon*, 11:20) implies "gradualness of withering over the course of an intervening day." How the phrase can imply all that I do not know. Most commentators understand it to be stressing the completeness of the withering (see Job 18:16; Ezek 17:9; Hos 9:16).¹⁴ Furthermore, the idea that Matthew creates indications of time that contradict Mark rests on the assumption that Matthew is using Mark (and only Mark) here. (These comments point out Gundry's tendency to magnify any difficulties he finds in the accounts.) I would contend that Matthew's account does not require that the disciples perceived the withering of the fig tree on the same day as Jesus cursed it. Matthew does not say that it withered "so quickly that everyone was still looking on." *Parachrēma* ("immediately") certainly implies extreme suddenness, but the complete withering of a tree within the space of twenty-four hours certainly justifies that description. What must be kept in mind is that the evangelists are seldom interested in chronological precision (or, many times, in chronology at all). Thus one evangelist or the other is often more specific with respect to temporal indicators. In this case, Mark has separated the two parts of the incident (which, by the way, is a characteristic of his redaction) while Matthew has joined the two parts together. Once we refrain from seeking chronology in a narrative that does not intend to give it, the difficulty disappears.

As representative of the second category we may take the Matthean emphasis on the disciples' understanding, a theme that Gundry thinks Matthew pursues to such an extent that several contradictions of Mark result. Here I can agree with Gundry that Matthew does, indeed, focus on the understanding of the disciples, whereas Mark generally tends to portray them as ignorant and "hard in heart." The crucial question is this: Does the emphasis in Matthew distort the facts as presented by Mark? I do not think so. In the feeding-of-the-5000 pericope, Gundry continues to argue that Matthew downplays the disciples' misunderstanding to the extent that a distorted picture emerges. But why do Matthew's omissions of some of the clearer expressions of the disciples' doubts in Mark result in a distortion? Matthew does not present the disciples as understanding, and the fact that they have "little faith" is clearly derogatory in Matthew (in 6:30 it is associated with the "anxiety" from which the pursuit of the kingdom should free us; in 14:31 it is associated with doubt). Does a fair, objective reading of the

¹⁴Cf. e.g. C. E. B. Cranfield, *The Gospel According to St. Mark* (Cambridge: University Press, 1966) 360; W. L. Lane, *The Gospel According to Mark* (NICNT; Grand Rapids: Eerdmans, 1974) 409.

accounts of Matthew and Mark together, without a previous bias in favor of a particular theory, justify the accusation of distortion here? I think not. The same thing is true in the other pericopes where Gundry finds contradictions on this point: Differences of emphasis are present, but such differences should not be viewed as contradictions. Any historian will choose to highlight certain aspects of the events he narrates and interprets. Matthew obviously stresses certain theological themes in his portrayal of the ministry of Jesus. But I would contend that these emphases do not contradict but complement other themes highlighted by Mark and Luke. As long as none of the evangelists claims to be giving us an exhaustive interpretation of these events, the selection of different themes does not constitute "distortion" of the events.

Ultimately the decision to seek harmonization or to admit contradictions depends on the view one takes of the nature and purpose of the gospels. Gundry, beginning with the data, finds the difficulties so insuperable that he cannot believe Matthew was intending to write with historical accuracy. But perhaps Gundry has focused too much on certain kinds of data. By writing a commentary that seeks to lay bare Matthew's theology, he has concentrated almost exclusively on what is peculiar to Matthew. Add to that his tendency to distort the degree of Matthean creativity by relying on a dubious source theory, misleading statistics and numerous forced exegeses, and one gets as a result a significant magnification of the problem. To be sure, others, starting from a belief in Matthew's historical reliability, can err on the opposite side by sweeping the difficulties under the rug. But if it can be shown that Matthew does intend to portray what really happened and one approaches the texts with a recognition of their character and concerns, the great majority of problem texts can be satisfactorily resolved. Gundry, however, challenges his critics to advance reasons for thinking that Matthew intends his narrative to be an historical account of Jesus' ministry. Let me enumerate some of the more important ones.

Gundry displays an attitude to Christian tradition that one finds far too often among NT scholars: The first seventeen centuries of Christian scholarship are written off because "the methods of historical criticism, with its concern for detailed historical accuracy" had not yet appeared on the scene. To begin with, such an understanding is of course a gross distortion of the situation. Scholars, both pagan and Christian, have always been concerned with "historical accuracy." Witness the manifold attempts down through Church history to harmonize the gospels, so that "detailed historical accuracy" could be preserved. Moreover, to label as "Church traditional bias" the view that Matthew wrote with historical intention is to ignore the fact that all scholars, in or out of the Church, have understood Matthew that way. More seriously, Gundry's attitude typifies the temporal arrogance of the modern Biblical scholar who thinks that only "now" has true "scientific" investigation of the scholar's field been attained (in the scholar's own work, of course). I am not suggesting that a view held by a minority, or even a small minority, of the Church could not be correct. But I would suggest that a view that no one in the history of the Church has held before is suspect—particularly as long as we believe that the Spirit has been at work in the Church. Therefore I would put forth as one reason to accept Matthew's historical intention the witness of twenty centuries of Christian laymen and scholars.

But the second (and ultimately most important) reason for seeking historiciz-

ing 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. Let us approach the situation from the standpoint of the first-century reader. For Gundry's thesis to be accepted, he has to demonstrate two points: (1) that a mixture of history and nonhistory was a readily recognizable form of communication in the first century, and (2) that Matthew furnishes the reader with sufficient clues that he is writing this kind of mixture.

As to the first point, it can be quickly granted that ancient writings mixed history and nonhistory—perhaps even all ancient authors (Biblical writers excepted for the moment)—to the extent that even the most careful historian is bound to err at some point. A question to which Gundry does not address himself, however, is the psychology of this process. To focus on his analogy: Did the midrashist believe he was mixing history and nonhistory? Or did he think that even his "additions" to the OT had historical status because of his role as a transmitter of "oral torah"? The answer may be obvious, but Gundry does not address it.

A second point relating to the mixture of history and nonhistory is the question about its acceptability. Gundry argues that the first-century Jew did not bat an eyelid over unhistorical additions. But the idea that the ancients did not care to distinguish between history and nonhistory is very questionable. Lucian, a second-century Greek, wrote that "history cannot admit a lie, even a tiny one, any more than the windpipe, as sons of doctors say, can tolerate anything entering it in swallowing" (*Works* 4.10). Polybius, in the second century B.C., encouraged the good historian to "simply record what really happened and what really was said, however commonplace" (*Hist.* 2.56.10). And, closer to home, Josephus makes known his intention to record the truth of what had happened to the Jews "with great accuracy" and severely criticizes other historians who incorporate inaccuracies in their accounts (*Ant.* 20.8.3). These statements come, it is true, from some of the better historians, and they (still less some of their contemporaries) do not always live up to their theory. But the theory did exist; the ancients were concerned with historical accuracy—and it cannot be assumed that Matthew's readers were not.¹⁵ In fact, one would expect precisely Matthew's readers to be deeply concerned with historical reliability. Their new system of convictions, their daily living, their hope for eternal life rested squarely on the fact of God's intervention in history in the recent past.

Would Matthew's first readers have expected historical accuracy in his gospel, or would they have readily identified Matthew's intention and not bothered themselves about it? This is an important point for Gundry for, although he argues that it is Matthew's intention (whether recognized or not) that is ultimately crucial, he 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). Interestingly, Gundry backs off from speaking of a literary genre in his "Response" (in *Matthew*, as the first full paragraph on p. 632 reveals, he does argue in terms of genre). The "sole point of comparison" justifying aligning Matthew

¹⁵A. W. Mosley concludes his article on "Historical Reporting in the Ancient World" (NTS 12 [1965-66] 10-26) with the observation that the first Christians would very likely have been alert to the differences between history and fabrication. On this whole question see also H. Koester, *Introduction to the New Testament, Vol. 1: History, Culture and Religion of the Hellenistic Age* (Philadelphia: Fortress, 1982) 128-131.

with Jewish midrashim is the “spirit of free adaptation and embellishment.” 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. But does this mean that any ancient writing can be excused of “error” simply by regarding any mistakes as interpretive storytelling, not intended to represent reality? Of course not. Therefore Matthew’s readers would have to recognize both Matthew’s historical inaccuracies and be able, on some basis, to place Matthew in the category of writings that did not have historical intention. If, however, free embellishment was the sole characteristic that Matthew shares with midrashic-type works, while in form and content his gospel differs from these, how could the reader have made this identification? Gundry claims that the “spirit of free embellishment” characterizes “a large body” of Jewish literature dated from the late OT period to several centuries after the NT and that Matthew’s place in the middle of that period favors our finding such a spirit in his work also. But that presumes that Matthew is to be compared with this “large body.” Why not compare Matthew rather with 1 Maccabees or Josephus’ *Wars*? In many ways Matthew’s form and content is more similar to them than to “midrashic” works. Or, remembering that Matthew was written in Greek, setting it apart from the outset from most of the books in the “large body” of Jewish literature that Gundry cites, why not associate Matthew with a Greco-Roman literary genre—the “laudatory biography,” for example?¹⁶ The point we are making here is that the presence of historical embellishments, as such, offers no help in placing Matthew into a category according to which such embellishment was anything other than simple error. Yet this embellishment is all that Gundry appeals to.

In addition to the failure to find in Matthew any clues such as would enable us to associate his gospel with “midrash” rather than a variety of other kinds of literature, we also find numerous reasons not to make such an association. First, there is the matter of cultural-religious milieu, to which I referred earlier. What may or may not have been acceptable among first-century Jews may or may not have been acceptable among first-century Christians. And the importance of recent history for religion decisively differentiated Christians from “mainstream” Judaism. Second, also mentioned above, is the difference in language. Third, Matthew is far more conservative with regard to his sources than any of the midrashic-type works Gundry mentions. He challenges me to find “any piece of Jewish literature for the NT era that shows less, or significantly less, freedom in treating the OT than Matthew does in treating Mark and Q.” Indeed, I cannot. But that is just the point: Matthew is significantly more faithful to his sources than those works that can appropriately be labelled “midrashic.” He 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. F. Gerald Downing correctly notes that “it is not the divergencies among the synoptists (or even between them and John), in parallel contexts, that are remarkable: it is the extraordinary extent of verbal similarities.” He goes on in the same article to men-

¹⁶See the argument of P. L. Shuler, *A Genre for the Gospels: The Biographical Character of Matthew* (Philadelphia: Fortress, 1982).

tion the theory that some of the distinctive material in the synoptics "was produced by a form of 'free association' of ideas, following the free (but not arbitrary) methods of rabbinic 'midrash'" and comments on the theory as follows:

But none of these [rabbinic midrashim, Qumran, Philo, Josephus and the apocalypses] offers any at all precise precedent or analogy for gospels so conceived. For the gospels we would have to suppose a willingness often to quote almost word-for-word, along with a willingness to invent out of scriptural hints, along with a quite incredibly painstaking willingness to sort out for rejection occasional examples of precise quotation; and all this in the portrayal of a supposed recent historical figure. We have no evidence for anyone else adopting so mixed and self-contradictory a method; and I would suggest that it is so bizarre as to be beyond credence.¹⁷

A fourth reason for disassociating Matthew from the midrashim, as Downing suggests in his remarks, is the concentration on a recent historical figure in Matthew. Shuler has recently argued that this factor suggests that the gospels are closer to the hellenistic *bios* than to any other form.¹⁸ And the difference between an eyewitness writing about recent historical events and a scholar, remote from the events, forced to contemporize a static, centuries-old religious tradition is no small one. True, Matthew has used sources, as did the midrashists. But then so does any historian. In Matthew's case, however, we have an historian (using the term in a loose sense to mean someone who is writing about historical events) who was there. The example of other ancient historians shows how eyewitness testimony was highly valued and used to correct reports from others. Gundry suggests that, because I think Matthew used Mark and Q, I have Matthew "more interested in traditions about Jesus than in the person of Jesus himself." But what I maintain is that Matthew's own experiences enabled him to use Mark and Q selectively in the way he did. He respected their accuracy to the extent that he was willing to take over massive portions of their material, but he added, omitted and revised their work according to his impressions of Jesus and the needs of his Church. Gundry assumes that such redaction must have been done without regard for "what really happened." I continue to maintain that this is most unlikely in the case of one, like Matthew, who had walked and talked with the most impressive personality ever to live on the face of the earth.

Gundry cites the example of Josephus as one who embellished his sources. Interestingly, however, Gundry quotes Attridge's comment on Josephus' procedure in the *Antiquities*. Yet Attridge himself notes that "the theoretical perspective of the *Antiquities* is quite different from that of the *Bellum*."¹⁹ In the former, Attridge argues, Josephus writes "antiquarian," "educational" history that concerns itself with the "remote past." In the *Bellum*, however, Josephus writes on relatively contemporary affairs—affairs in which he himself had played a role. Along with this circumstance, notes Attridge, is far greater concern for "objective," factual history.²⁰ Gundry should have quoted Attridge on the *Bellum*, not

¹⁷F. G. Downing, "Redaction Criticism: Josephus' *Antiquities* and the Synoptic Gospels (I)," *JSNT* 8 (1980) 33, 46-47.

¹⁸Shuler, *Genre* 36.

¹⁹H. W. Attridge, *The Interpretation of Biblical History in the Antiquities Judaicae of Flavius Josephus* (HDR 7; Missoula: Scholar's, 1976) 56.

²⁰*Ibid.*, pp. 44-47.

on the *Antiquities*, if he sought to make a meaningful comparison with Matthew. Furthermore the parallel of Josephus is not a happy one for Gundry's case. For Josephus clearly claims to be writing factual history in the *Antiquities*, adding nothing "for the sake of embellishment" (4.197). In light of this claim, Josephus' discrepancies with the Biblical account must be labeled as errors, plain and simple.

In the last analysis, it seems plain that Matthew is to be compared with Mark and Luke rather than with other Jewish works and that the comparison must extend to a concern for historical accuracy. Why? Simply because such a concern is so much of the essence of Mark and Luke that meaningful comparisons with them could not fail to include this element. And—let me say again—when we look at Luke we find a gospel that, according to Gundry, is historically accurate, yet does to Mark what Matthew also does. I do not have to appeal to the conclusions of other scholars at this point: Luke introduces as many apparently "tendential" changes in Mark as does Matthew. It is obvious that, if Mark and Luke are both historically accurate, the same kind of approach to harmonization Gundry rejects for Matthew must be applied to Luke. It will not do to appeal to Luke's prologue and Papias as evidence for a difference in intent between Luke and Matthew. For on the one hand, Luke's prologue is obviously written with an eye to his audience; Mark does not have one, yet is historically reliable. And on the other hand, Gundry's appeal to the Papias quotation as evidence of Matthew's "midrashic style" is an argument built on several layers of sand. The quotation is found in Eusebius, who is passing on Papias' sayings. The crucial word is difficult to interpret, and Gundry argues for a minority view. Even those who have interpreted the word as Gundry does do not find any allusion to "midrashic" style. The whole case is precarious to a degree. And quite apart from that, why should Papias be the only one in Church history, Gundry besides, who correctly understood Matthew's intention?

In conclusion, then, the halfway house that Gundry has chosen to occupy—maintaining inerrancy only by drastically revising accepted understandings of Matthew's intent—is a most uncomfortable location. He is doubtless being hammered from both ends of the theological spectrum. I applaud his desire to maintain a commitment to inerrancy. But I would encourage him to examine Matthew again in light of the criticisms I and others have made to see if, indeed, inerrancy can only be salvaged by resort to what seems to me a most unconvincing hypothesis.