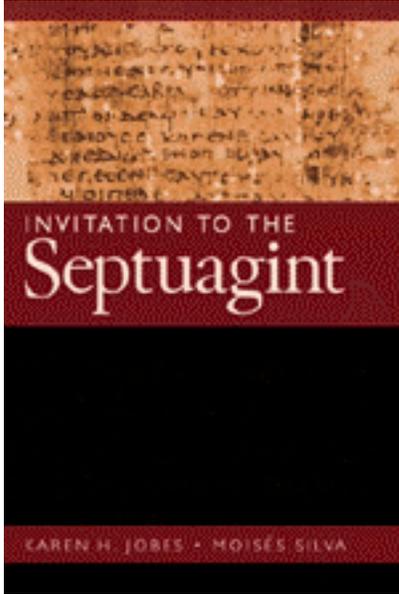


RBL review: 10/2002
response: 03/2003



Jobes, Karen H., and Moisés Silva

Invitation to the Septuagint

Grand Rapids: Baker, 2000. Pp. 351. Hardcover.
\$29.99. ISBN 0801022355.

James Barr
Claremont, CA 91711

[Response by Karen H. Jobes and Moisés Silva](#)

1. General

The authors (hereafter referred to as JS) plan their work as “a relatively brief and inviting introduction for the student who has no prior knowledge of the Septuagint” (9). Indeed, the first of several parts is intended (27) to be useful to readers who have no knowledge of either Greek or Hebrew. Undoubtedly such an approach promises well to expand awareness of the LXX, especially among theological students. It is especially likely to expand it among evangelical students, who will trust the writers and be guided by them.

2. Plan

There are three main parts. After an introduction “Why Study the Septuagint?” (19–28), part 1 is on “The History of the Septuagint,” part 2 on “The Septuagint in Biblical

Studies,” and part 3 on “The Current State of Septuagint Studies.” To expand this somewhat, part 1 begins with chapter 1, which provides definitions of terms and then goes on to the original translation and the later versions. Chapter 2 is on the transmission: it first explains about the Hexapla and other recensions and then goes on to the MSS (including some quite clear photographs), the daughter versions, and patristic citations. Chapter 3, “The Septuagint in Modern Times,” introduces the modern editions and explains the different canons and numbering systems. Chapter 4, “The Septuagint As a Translation,” has two parts: the first expounds the character of the translation process in the ancient world, and the second discusses interpretational factors, theological and other.

In part 2 the first element, chapter 5, is on the language, including Hebrew/Aramaic influence on the Greek; chapter 6 is on “Establishing the Text of the Septuagint” and ends with one very useful feature: facsimiles of pages from major editions, with facing-page “keys” explaining their complexities. Chapter 7 is on “Using the Septuagint for the Textual Criticism of the Hebrew Bible”; chapter 8 discusses the relation of the Qumran scrolls to the subject; chapter 9 goes on to relations with the New Testament, and chapter 10, “Interpreting the Septuagint,” takes three key examples for full discussion: Gen 4:1–8; Isa 52:13–53:12; and Esth 5:1–2 with (interestingly) the addition D and the Alpha Text. Chapters 7 and 10, we shall see, are probably the most important in the book.

In part 3 we have chapter 11, a pleasant biographical excursus telling the life-story of some past scholars in the field, then chapter 12 outlining some current linguistic studies in the LXX, and chapter 13 on the search for the original Greek and its various recensions. The final chapter (14) is on theological development, with attention to messianism, eschatology, Hellenistic philosophy, and the three later translations. Appendixes cover descriptions of organizations and projects, basic bibliography with some annotation, a useful glossary of technical terms, and a list of places where verse numbering differs as between the LXX and the English Bible.

One or two general remarks. We have noted the facsimiles of specimen pages from the main editions (138–45) with an explanatory “key” on the facing page. It would have been good if a similar facing-page keys had been provided for the facsimiles of manuscripts, because few readers will be able to read these scripts without a transliteration into modern printed Greek. A related feature is that, in the major commentary on particular texts, especially in chapter 10, the texts are very well displayed, in a vertical alignment of MT and LXX with English translations and a clear numbering of the words or phrases which facilitates comment.

3. The Central Issue

Now, to take the bull by the horns: the problem with this book is that it appears to take the side of the MT much too heavily and thus to minimize any positive importance of the LXX for the text of the Hebrew Bible. It was truly amazing to find, after working through the many examples and problems discussed, that there was hardly a single verse in the whole book for which in the end any confident assurance was offered that the Greek text should be preferred to the MT. Even when good arguments for the LXX are presented, in almost every case the authors end up suggesting a way out, offering considerations that will permit a final decision in favor of the MT. The book contains, indeed, many other themes and aspects of LXX scholarship and much originality, but the question of the LXX/MT relationship is so central that it seems necessary to concentrate on it.

4. First example: “And it was so”

Take the very first example in the book, one likely to be especially influential because it is in the section available to those without Hebrew or Greek. Approaching the example, the authors tell us that “The Greek version also has great value for the study of the Hebrew text.” There are complex issues, but “the fact remains that the Septuagint was translated from some Hebrew text that was not identical to the Hebrew text that we use today” (21). In theory, therefore, the LXX “should allow scholars to reconstruct that earlier Hebrew text, though in practice this activity is fraught with difficulties.” This is in itself quite correct and looks cautiously positive. It is a good beginning.

The passage discussed is Gen 1:6–7, the creation of the firmament. The texts are well set out in parallel (21–22), but the very first comment is ruinously damaging: “the Greek has transposed the words ‘and it was so’ from verse 7 to verse 6.” This is very unfortunate, for it assumes exactly what we do not yet know. It implies that the MT is *the text*: if in the Greek the location is different, then it is the Greek that has moved the phrase. It would be more correct to say: the Greek and the MT have this clause in different locations, and JS know this perfectly well. Elsewhere they sometimes use the more neutral, proper, and accepted terms like “plus” and “minus,” but they often fall back, as here, into a usage that implies from the beginning that it is the translator who has transposed or altered the text. This would not matter if it were an occasional case, but in fact it symbolizes what will recur at many points in the book.

What follows at the same point is an argument that the translators were also *interpreters*. They came to the text with the theological and political prejudices of their time. This “is

bad news to the textual critic” (a very premature comment). Reconstruction of a different Hebrew text on the basis of LXX is fraught with difficulties. This in itself is quite true, but the discussion that follows entirely emphasizes these difficulties and shows little enthusiasm for preferring, or even considering, a Hebrew text from which the Greek would have been translated. It is difficult, we are told, to be sure that the Greek exactly represents its Hebrew *Vorlage*, because “translation between any two languages always involves a degree of interpretation.” It is “possible” (not a very strong expression) that the Greek translator moved the phrase, “perhaps because it sounded out of place” (a rather lame explanation). The Greek translators came to the text “with the theological and political prejudices of their time and had to deal with hermeneutical issues similar to those we face today.” But why would this not be true also of the Hebrew editors and scribes of the MT? How did *they* not have prejudices and hermeneutical issues? Why was it not *they* who moved the phrase from one location to another, because it sounded out of place?

A whole page follows that expands on these ideological influences. For the reader (perhaps against the authors’ intention) a simple impression is left: the MT is right and basic, and differences in the Greek are to be explained as interpretation. But what Hellenistic ideology, in this passage, caused the translator to move the words “and it was so”? Moreover, it is very likely, from comparison with other similar elements in Genesis, that the moving of the phrase from one place to another, whether upward or down, was done within the transmission in *Hebrew*. If so, the two arguments, first about how one language cannot express exactly the same as another, and second about the influence of Hellenistic ideology, are entirely irrelevant.

5. Some Central Examples

The case of Gen 1:6–7 is a first and very simple introductory example. The examples that are discussed in the central zone of the book, however, are not all of the same kind, and the questions raised are different as between one and another. A selection of these different types will be examined here, in no logical order but starting with some that may seem simpler than others.

Page 214, Gen 4:8. This case is very similar to that of Gen 1:6–7 above. MT was commonly taken to say that Cain “spoke” or “talked” to his brother Abel, but it does not say what Cain said (so KJV “and Cain talked with his brother Abel”). In the LXX, on the other hand, Cain *said* “let us go into the field”; so also the Samaritan, the Syriac, and the Vulgate. General scholarly opinion is that the Hebrew verb means “to say (something)”

but hardly ever, if at all, “to speak” or “to talk” without indication of what is said. A recent dictionary says that this verb “never means to say without indication what is stated.” JS, on the other hand, repeat an old argument that **רָמַס** can mean “he spoke” and not “he said”—there are said to be three cases elsewhere in the Bible, not many out of about two thousand even if they are valid exceptions. They write: “We should therefore leave open the possibility that, as the more difficult reading, the MT, which omits the phrase, is to be considered original.” By contrast, the distinctly conservative evangelical NIV follows LXX and other versions, translating in pleasant colloquial style “Let’s go out to the field” and noting that MT does not have these words. Whatever the force of the argument from the use of the Hebrew verb, the LXX reading is massively more probable. We remark: that JS’s decision appears to depend on a point in Hebrew grammar and not on the LXX itself. The argument from the “more difficult reading” will be discussed below. But in any case, if their argument about the three exceptional uses of the Hebrew verb is right, it was, by their own argument, not a very difficult reading anyway. In my opinion, the LXX reading here is by far more likely to be original.

Page 223, Isa 53:7: “For no apparent reason, the Greek ignores the first word” (**נָגַג**). But this again assumes MT as basis. We do not know that the word was there when the translation was made. Since there was “no apparent reason” why the Greek would omit the word, the obvious conclusion is that it was not there. The evidence seems to give no reason for decision as between the readings of LXX and MT.

Pages 194–95. At Amos 9:12: LXX has **ἐκζητήζωσις** where MT has **רָשַׁן**, and this is followed by the quotation in Acts 15:16–17. Hence some are said to have proposed **רָשַׁן**, a very improbable suggestion to my mind, since it cannot make any sort of sense within the context in Hebrew. It is not cited in *BHS*. It is to my mind clear that the word was written as in MT but was read as with a D and not a Y. This verb is very commonly translated by **ἐκζητεῖν**. This entirely supports the MT, as JS probably maintain. They go on to argue, however, that even if Hebrew characters were “misread” the translator was “primarily motivated by hermeneutic concerns.” I see it as exactly the opposite. The verb (as seen) was rendered literally, and this restructured the syntax of the rest of the verse. If there is “little reason to posit a different Hebrew *Vorlage* here,” there is little reason to “posit” a “possible” line of hermeneutic argument either.

Pages 179–80. This is a case where only a small piece of LXX text is directly present, but it is a good example of the problems. Just before 1 Sam 11:1 we have from Qumran, in Hebrew, a passage of several lines about Nahash the Ammonite that is lacking in the MT and is not found in the LXX either (cf. NRSV for an English version). It is a story about how Nahash gouged out the eye of each of the Israelites he controlled. This leads up to his attack on Jabesh-Gilead, which leads on to one of the stories of the rise of Saul. The

story is of the same genre as that of 2 Sam 10 about Hanun, again king of the Ammonites. So one could say: the similarity of the two stories supports the presence of them both. Alternatively, the newly found story could be a secondary imitation drawing motifs from 2 Sam 10. JS point out that there is no trace of this passage in the LXX. “It raises more questions than it answers about the textual history of Samuel”. This seems to mean: we should stick with the MT. But that is not the question: whether it solves questions about the textual history, it is a tremendously powerful and satisfying complement to the text we already had. This newly found passage is not something conjectured or translated from the Greek or floating in the void: it is fitted into the actual story of 1 Sam 11. The combination of the כַּחֲרֵי־יָ of 10:27 MT with the שָׁחַדָּה retroverted from the Greek ὡς μετὰ μῆνα “about a month later” of 11:1, correctly noted by JS in their note 33 but not given a place in their argument, is exactly what happens in this kind of overlapping text and seems to me irresistible. It seems reckless to ignore the judgment of the cautious and conservative Emanuel Tov (*Text-Criticism*, 342), who writes that the “original, longer” text is probably preserved in the Qumran fragment, while the MT along with Targum, Syriac, and Vulgate are “based on a scribal error.” According to Tov, the Qumran text is “the original text, which was subsequently corrupted.”

Pages 215–17, Isa 52:15: MT פָּרַס: “sprinkle”(?) but LXX θαυμάσονται, hence various emendations in *BHS*, which, I must say, do not look convincing to me. “It seems unlikely that the MT reading (*lectio difficilior*) would have arisen if the original Hebrew text had been as easy as suggested by the Greek translators.” But we do not know that they found it easy. The opposite is more probable. The verb may be a rare one: BDB thought it to be a different root, homonymic with the one meaning “sprinkle.” The translator was puzzled and made a guess. The case is likely to belong to the category I designated as “favorite words” (J. Barr, *Comparative Philology and the Text of the Old Testament*, 251–53). Isaiah LXX uses this Greek verb six times, every one of them for a different word in MT, and hardly any of them clearly meaning “admire” or “wonder at,” which is the meaning of the Greek. On the explanation as *lectio difficilior* see again below. It is unlikely that the Greek here will lead us to a true solution, but this does not in the least prove the correctness of MT. The ultimate cause of the problem does not lie in the verb of the Greek but in uncertainty about the identity and meaning of the Hebrew verb.

Pages 162–65, 1 Kgs 2:5. In the MT, verse 5 appears to end with the two phrases “blood of war in peace” and “blood of war on his girdle and shoes.” The texts are well set out and quite well discussed. There is no one LXX reading; B does not have the phrase “in peace and put blood of war” but other MSS include it, and Rahlfs has it in his text in the form “in peace and put innocent blood” (illustration 14 on p. 140). This last phrase, αἱμα ἄθωπον, might well stand for an original חַנּוּן חַגְרֵי (as in v. 31) or נֶקֶדַי. Its inclusion indicates Rahlfs’s judgment that the phrase was original in the LXX. Various

complications of the Greek MSS are discussed by JS. In the end, they say, “we reach an impasse.” It is more reasonable to assume that the MT has “the original reading.”

JS see this as a very good example and urge the reader to go over it again and again until it is clearly understood. I am not so sure. They write as if the LXX readings were the basic or only reason why a Hebrew text differing from MT was suggested here. In fact there are literary reasons within the Hebrew text itself that might well have provoked a similar proposal. If we have two sentences:

He put blood of war in peace
He put blood of war on his belt and shoes

immediately together, we produce a rather comic zeugma, the same figure of speech as:

She arrived in a flood of tears and a bath-chair.

Parallels for this in the Hebrew Bible might not be easy to find. The double use of “put blood of war” looks very like what some textual fault would produce. This is not so obvious in English translations that follow MT: they smooth over the incongruity with their wording, but it is rather obvious in the Hebrew. Moreover, if we follow the MT we have no specific mention of the *innocence* of the persons slain, though that is repeatedly stressed in the story (see 1 Kgs 2:28–35). Personally, I think that the textual proposal is quite a good one, and, even if we could not be sure of the reconstruction on the basis of the Greek, I am sure that it is certainly as good and strong as the MT.

Pages 195–97. This is a case of different type but notable for an original suggestion made by JS, with significant effect on the New Testament. In Ps 40:7–9 (LXX 39) the strange Greek σῶμα where MT has “ears” has been well understood as an inner-Greek misreading: ΩΤΙΑ, with a Σ from the previous word, came to be read as ΣΩΜΑ and is so quoted with emphasis in Heb 10:5. “Few scholars are persuaded by this solution, however.” I do not know how JS know this. Perhaps they refer to New Testament scholars only, but James Moffatt in his commentary on Hebrews in the ICC series (1924) has a quite sensitive discussion of this “pivot of the argument,” though the specific solution proposed was probably unknown to him. Whether the details are exact or not, to me it is a highly intelligent and persuasive suggestion and likely to be right. And JS do accept that ὠτία was the original LXX reading. But, between the LXX and the New Testament, rather than have a scribal accident, they suggest that the reading σῶμα was an imaginative construction of the writer to the Hebrews, who deliberately made this change, a kind of metonymy. This New Testament reading was then read back into the

text of LXX Psalms. This original, but fragile, solution would avoid the implication that Hebrews built its argument upon a scribal accident.

There is some misunderstanding here, however. It is not clear whether Rahlfs knew, or implied, the specific confusion of Greek letters involved in this proposal. If he had known it, one would imagine he would have cited it in his Psalms edition. The argument he actually used in his apparatus did not depend on it. To him the reading ὥτία was already “secure” (to use JS’s terminology) from the manuscript evidence he cited, both the Latin and the Syro-Hexaplar, as well as the fact, properly recognized by JS, that to his expert knowledge of the Psalms translator it was impossible that he would have made so very remote a rendering as σῶμα here.

This is relevant for our review because the book seems not to give the reader an adequate picture of the importance of the Latin versions, especially the Old Latin. The OL is mentioned several times (e.g., 67), but its importance is not properly conveyed. The reader of page 196 is likely to gain the impression that the *aures* of one OL manuscript plus the Gallican Psalter (printed as “Gallic”) amounts to very little. The essential point is that the OL was translated from the LXX without reference back to the Hebrew. An OL reading is thus, for many purposes, effectively the same as an LXX reading and goes back to a very early stage in transmission. This is significant for a number of cases where OL is cited together with LXX in the apparatus, often at points relevant for the New Testament. The imaginative suggestion that σῶμα was an innovation made by the letter to the Hebrews, whether probable or not, remains logically possible and is a question for New Testament scholars. But that the Greek Psalms tradition would accept back from the New Testament a change so great and so radical in so short a time would seem to require the support of some analogies. It looks more probable that σῶμα was already there before the author of Hebrews utilized it. In this case, it is good to note, the question of preference between LXX and MT does not arise.

Pages 223–24, Isa 53:8. The last word is לָמוֹ, but Greek εἰς θάνατον has suggested לָמוֹת. The comment is: “Remarkably, not a few scholars accept לָמוֹת as the translator’s *Vorlage* and as the original Hebrew text. Considering the Greek translator’s performance as a whole, one is tempted to view this curiosity as an example of the blind leading the blind.” (224). Here JS lose their customary calm and descend to something more like an imputation of incompetence. The Greek may be wrong, but there is nothing “remarkable” in the acceptance of it, nor is there any basis for blaming it on the incompetence of the translator. I do not see any purely textual basis for a decision between the two readings: it would depend on the literary possibilities attaching to the use of the term “death” within the passage as a whole.

Pages 225–27, Isa 53:11: MT “he will see,” but LXX and “three” Qumran scrolls add the object “light,” LXX also rendering as “show,” which would imply hiphil. The comment of JS includes: “Of all emendations suggested by the LXX renderings in Isaiah 53, this is the most persuasive one, since three distinct scrolls of Isaiah from Qumran have precisely this reading. To be sure, even here the evidence is not conclusive. The MT reading is more difficult.” Three or four further utterances of negativity are added, for instance: “We have some evidence that the translator had a theological preoccupation with the notion of light as knowledge.... A case can be made that an ideological motivation lies behind the Qumran reading.” In the end, however: “On balance, however, it seems likely that the LXX reading reflects a parent text different from MT and that this reading is original.” Curiously, JS fail to notice yet another way of saving MT found in the *BHS* apparatus, namely, to read the verb as a form from **רָוַה** “be satiated,” which goes along with the next verb.

This very hesitant support for one reading, where LXX has strong support from Qumran, is in fact the strongest I have found in this part of the book. The general impression given is that to stay with the MT could be the safest course. Paradoxically, at this one point at which JS painfully come to follow LXX rather than MT, I remain more inclined to favor MT, on grounds some of which they express on their page 227. I would give MT 50 percent or a little more.

6. More Positive Statements

This constant negativity toward the LXX might give the impression that the book is a straightforward conservative argument for the absolute inerrancy of MT. But there are other areas in the book that appear to be somewhat more accepting toward corrections of the MT on the basis of the LXX. And the authors have already stated that it was a “fact” that the LXX was translated from a text different from MT. Here are some examples that seem to look more favorably on understandings that follow the LXX:

Page 148. At Isa 5:17 MT has **גְּרִיִּם** but LXX has **ἀρνες** “lambs” and the proposal **גְּרִיִּם** “kids” (printed here with wrong punctuation) has naturally followed (NIV “lambs shall feed”). It is “at least worth considering.” Three decisions, “reasonable but also debatable,” are involved in favoring the LXX wording (the likely parallelism with “lambs” or “bulls” might well have been added as a fourth [148–49]). But this moderately favorable view of a case in Isaiah, JS write, should not be taken too far. There is a “large number of differences” in this book but we have first of all “to remove those that appear to be the result of the translator’s own method of work.” As it turns out, the vast majority

“do *not* seem to have resulted from the use of a parent text at variance with MT.” Isaiah 5:17, then, is an exception, one of the few in Isaiah that are “at least worth considering.”

And here, though differing in the account of the “decisions” involved, I share the final assessment of JS: the proposal is worth considering and *could* be right, but not more than that. Perhaps it would gain a figure like 30 or 40 percent. But if we reject the proposal, we have not thereby proved the MT to be right, for there are other problems in other words of the verse, as JS perceive.

Pages 154–56. A group of other cases worth considering may be found in these pages. At Isa 29:3 the reading “like David” (against MT “like a circle”), though not taken as decisive, is sympathetically treated: confirmation by two medieval Hebrew MSS “certainly strengthens the argument.” I agree with JS. Perhaps we might give it 50 percent this time.

Page 155. At Deut 31:1, MT “Moses went and spoke,” against LXX “Moses ceased speaking,” is a difference only in the order of consonants (וִיכַל or וִיִלֶךְ), and “intrinsic probability supports the LXX reading” (155); other factors are rather against it, but it has at least something in its favor. Further, a Qumran fragment agrees with it. So “One would have to have a very good reason to deny that the *Vorlage* of LXX had this reading” (156). And exactly the same idiom with the same rendering occurs in close proximity, at Deut 31:24, 32:45. The JPS version gives it honorable version in its margin. I would say that the LXX reading has more like 80 percent probability. JS are quite right, except in one thing: before an LXX reading can become reasonable or probable, every precaution has to be observed, every test applied, favorable evidence must be massively amassed, while in order to favor a word of MT, little of this effort is necessary. Also, this example may show how decisions have to be made not on grounds traditionally known as “textual” (MS evidence, etc.) but on grounds more literary in character, such as, How often, and where, in Deuteronomy should Moses be expected to “finish speaking”? This question moves us toward the idea of various editions and away therefore from the quest for what is “original.”

In general, then, these few pages are much more positive toward the value of LXX than the fuller discussions that precede and follow.

On the other hand, JS seem to have another reserve argument in store, which they deploy here. They argue that, even if the *Vorlage* of an LXX reading agrees with a Hebrew text from Qumran and has its support, this still does not show that it is a “better” reading than MT. Thus (156) at 2 Sam 14:30 the LXX has a whole clause of about eight words telling something more about the setting of a field on fire. This can be easily retroverted into

Hebrew. In the past people might have been skeptical about such a retroversion. Now however a Qumran text has turned up with exactly this clause, right there in Hebrew. One might think this fairly decisive. But no! It “merely” offers a second Hebrew reading! That does not prove that it was “original.” They deploy against it the highly dubious argument of S. Pisano (157 and n. 25; cf. Aejmelaeus *ZAW* 99 [1987]: 68–69) but do not mention the powerful contrary verdicts of Tov and Kyle McCarter.

7. Variants in Hebrew

This leads on to the general effect of both the Samaritan text, long known to scholarship, and the Dead Sea Scrolls, the epoch-making discovery of last century.

The book has one serious misstatement: it is on page 177 and states that “the great Isaiah Scroll (1QIsa^a) and the Hebrew Minor Prophets Scroll (MurXII) contain essentially the same Hebrew text as found in Codex Leningradensis dating from about a thousand years later.” In fact, when 1QIsa^a was discovered, one of the first aspects noted was its striking confirmation of emendations suggested by scholars of earlier times, some of them on the basis of the LXX. As most readers will recognize, the authors must surely mean the B scroll of Isaiah, not the A scroll. The latter does not support the assertion that the MT “was already stable before the time of Jesus.” But the mistake makes an unfortunate impression, coming as it does within a paragraph that in the main emphasizes the antiquity of MT and tends either to diminish the importance of the non-MT scrolls or to emphasize their difference from LXX. Actually, the A scroll has plenty of readings that differ from MT, a substantial proportion being similar to LXX, as a glance at the *BHS* apparatus makes clear. The reading with the plus of “light” at Isa 53:11, which as we saw is favored by JS themselves, is supported not only in the A scroll but also in the B scroll, which is to be classed as proto-Masoretic.

But here once again the authors make some more positive statements. A very important one is this: “While the Masoretic tradition is astonishingly uniform and trustworthy, the earlier transmission of the text was not as careful” (123). Correct, and exactly my own opinion, and one seldom heeded. But, this being so, we should be ready to admit, and indeed to expect, gaps, uncertainties, and mistakes in Hebrew texts, including the MT, going back to that earlier transmission, which may often have been interrupted by breaks in oral tradition, scribal and dialectal changes, wars and migrations. Because of this less-careful early transmission, the uniform transmission from later times is not relevant as a guarantee for the original text. Not, perhaps, until the second century C.E. did the care of the text begin to develop toward the precision attained in the Masoretic tradition. From,

say, Amos down to that time, most of a thousand years, some difficulties might reasonably be expected. And the Qumran discoveries suggest a sea of varying texts, among which the proto-Masoretic was one but, at least in places, perhaps only one among others. It becomes clear that an LXX reading cannot be regarded as a mere product of an isolated translation process but has, or may have, a location as one within a variety of Hebrew text-forms.

JS accept this, but then they seem to warn against letting it go too far. Agreement of LXX with a Qumran text shifts the balance and strengthens the weight of the LXX, but it does not mean that such a text is the “original” or necessarily better than the MT, and this is quite true. But in the context of the present discussion, the importance of the embedding of the LXX within a body of Hebrew texts is something else: it seriously undermines the argument, much emphasized in this book and quoted above, that the LXX text was the product of interpretation and hermeneutical concerns and that the difference of languages made the version different from the original. For much or most of the “interpretation,” at least in some books, may or must have been done *within Hebrew*, before any translation was done, and thus these arguments amount to very little.

Even before the Qumran discoveries this should have been obvious from the Samaritan. In the many cases of agreement between LXX and the Samaritan Hebrew, it was always vastly more likely that the Greek translated from a Hebrew that already incorporated some of the differences that we know as Samaritan, than that the Greek, with the MT in front of it, introduced into it interpretations that they happened to have heard from Samaritans or some other such circle. The importance of this for the readership to which this book is addressed is increased by the several cases of important Old Testament readings quoted or used in the New that are in agreement with both Samaritan and LXX: e.g., the 430 years of Gal 3:17 (not mentioned here). Similarly the question of the sixth or seventh day at Gen 2:2, where LXX has the support of the Samaritan and other versions against MT, is mentioned on page 98. The comment confidently draws the wrong conclusion: “This is an example where the Greek translator apparently chose not to follow the Hebrew text, but to make his translation consistent with the traditional exegesis of the law.”

Moreover, even the astonishing uniformity of MT is not as complete as many people imagine. It depends at what level of detail one works. Comparison with the New Testament makes the MT seem extremely uniform. but serious textual work within Hebrew gives a different impression. Kennicott and Rossi did not fill their volumes of variants without some basis. To pooh-pooh them as minor details is to fail to learn from them. The insistence of some scholars that these medieval variants had no connection with the older scribal tradition is dubious. As we noted above, JS themselves at Isa 29:3

noted the existence of two MSS of the Masoretic Text that agreed with the LXX reading (against the “MT”!) and “certainly strengthened” the argument in its favor (154). And not all variations within the MT are so very minor. For two major Masoretic codices to drop out two entire verses, which other manuscripts of similar authority include, would seem to me to be a rather serious thing to happen. Readers of *BHS* have often failed to notice this, though it is strikingly marked at Josh 21:36–37.

8. A Different Approach

Fundamentally, for textual criticism we have to depart from the older tradition under which the MT was “*the* Text” to which others had at most an ancillary or heuristic function. The MT is in principle *only one* within a large body of available text. “The Text” is this whole body of available text. Religiously, of course, it is quite proper to hold that the MT is authoritative, as is the case in Judaism, just as it is proper for Greek Orthodoxy to take the LXX as authoritative (JS give a good statement of this [84]). But for evaluating the character and history of the text there is a variety of possibilities within which the MT and its predecessor, the proto-Masoretic texts from Qumran, are only one. The MT has a special and central status, but this is because it is the *only complete text in the original language*. And for Isaiah, this has already ceased to be the case; and for the Pentateuch it has never been the case, for the Samaritan has been a second Hebrew text, available since centuries back. Whether one set of Hebrew readings is better than another remains to be worked out; it is not a principle given as definitive from the beginning. The LXX and other translated texts have a necessarily secondary status, in that they are translated, and a reading in Greek or Syriac, however well supported and retroverted into Hebrew, never has the same status as the same word in Hebrew. That is a working difficulty, as JS well point out. But adequate knowledge of the various ancient versions and their ways of working will provide a substantial alleviation of this difficulty, and this is what JS’s work, and other works of introduction to LXX, do much to provide.

It is odd, when one looks back at *BHK* and *BHS* and the volume of criticism that has been leveled at them both, to realize how misplaced much of it was. People complain of the editors and the apparatus criticus that the edition endlessly “changed” or “emended” the text. But it did nothing of the kind. The text as printed was very emphatically intended to be the text as written and checked by the best of Masoretes, though this was modified in certain matters such as the alignment of lines and the S and P section divisions. In this respect and with these exceptions (and that of the Masora) JS (72 n. 7) correctly recognize that *BHS* is a “diplomatic” edition. It was not “emended” at all. The apparatus *reported* readings of other texts like LXX, sometimes philological information, and

various suggestions and proposals. But it did not “change the text” at all; the most it did was to *recommend* this or that different reading. In this sense these were not at all critical editions, such as is normal in the obvious comparison, namely, the Greek and Latin classics, where the editor commonly places within the text the wording that he or she considers right.

It may be a long time before this is done in the Hebrew Bible, but already an example has been given us in Hendel’s edition of Gen 1–11. It is surprising that JS mention this edition favorably on 214 n. 17, for it opposes their view of Gen 4:8 and undercuts much of their general position. It may prove for a long time practically difficult to carry out Hendel’s plan over the entire Bible, but the principle is an important one. At least scholarship ought to cease talking of an “emendation of the text” when a reading from the LXX is considered right. Apart from the risks of any retroversion, the LXX reading is part of “the text” as much as the corresponding MT reading is.

9. Conflicts in Scholarship

JS write well and in an irenic, nonpolemic style most of the time. They avoid dogmatism and do not pretend that they have a ready answer to every question. They are appreciative of the scholarship that they report and discuss and do well in setting out good examples of contrary opinion. But, having done this, they seem often to avoid arguing out the case. Rather, they leave it standing as two opinions—though from what follows it is commonly clear which one they favor.

Their wisdom and caution is well displayed when they say (86) that “It is commonly said that every translation is an interpretation.” Certainly it is “commonly said,” but they do not here commit themselves to it. Perhaps they do not know of the arguments to the effect that in ancient biblical translation this is only partly true (J. Barr, *Typology of Literalism*, 292–293; B. Albrektson, “Är Översättning också tolkning?” *SEÅ* 51–52 [[1986]: 13–22). Once again, in spite of their wise caution they go on to write as if what is “commonly said” must be taken as correct.

Again, on pages 149–50 they set out excellently a contrast between Wevers and Aejmelaeus. But the argument is not pressed to a decision; rather, it goes on to a quite proper, but rather general, discussion and ends up with an assertion of “the unique value of the LXX as a source of Hebrew textual variants.” From the contrast between the two scholars quoted, nothing sharp or clear emerges. But what if Aejmelaeus is right? What is the point of the contrast if the question is not answered?

Immediately following (151–54) is a statement of the contrast between the competing principles of “retain the MT if at all possible” and “adopt any easier variant without hesitation.” Mediating positions “have the appearance of objectivity” but are too vague to be of much value. What follows seems to be intended as a mediating position improved by the use of certain detailed criteria (153). But the criteria, or most of them, are very largely a reprint of Wevers’s position, in parts even almost word for word. The first criterion is that no mediating position can be adopted except in so far as “We need to assure ourselves that the LXX reading is not the result of interpretative thought or of carelessness in the process of translation,” and the third demands that we have good reason to believe that the presumed Hebrew reading “truly existed in a manuscript and not only in the mind of the translator.” This last one is nonsense: there is no way of *knowing* that a reading existed in a manuscript unless we have the actual manuscript. But the importance of this section is that JS do not consider themselves to belong to either of the two “competing principles” with which this section began. This is something of an advantage.

There are other places where JS describe alternative views but seem to sit on the fence rather than resolve the opposition. Thus on the question of anti-anthropomorphisms they describe some contrasting approaches (95, 117) and do not come down clearly on one side or the other, except that their remarks about calling the deity a “rock” (95) would suggest that they do think that avoidance of anthropomorphisms is a true feature of the LXX.

They thus do well in outlining some contrary positions in a stimulating way, without coming to a definite decision. And this is praiseworthy in some ways, but there is one trouble about it: in fact, in many questions they tend to follow the guidance of John Wevers. This is to be seen again and again. Unfortunately, this has an unbalancing effect, and we have to consider it more deeply.

10. Influence of J. W. Wevers

We continue, therefore, with this contrast, as set out on page 149. Wevers thinks one should not “automatically presuppose” (a caricature: no serious scholar ever *automatically presupposed* in such a matter) that there should be a different Hebrew parent text. Rather, one should first seek for and pursue other explanations. “Only” through this, he thinks, will one learn about the attitudes of the translators, their theological prejudices, and cultural environment. Aejmelaeus, on the contrary, insists that “weighty arguments” have to be produced before a scholar can claim to detect deliberate

changes, harmonizations, completion of details, and new accents and to assert that differences can not arise from the use of a different Hebrew text by the translator. “All that is known of the translation techniques employed in the Septuagint points firmly enough in the opposite direction,” she writes. An example from Exod 2:22 is offered on page 150.

But though JS obviously recognize the scholarly eminence of Aejmelaeus and describe or quote parts of her work with approval (157–58, 268–70), they do nothing to analyze the essential argument that they themselves quote on page 149 and do nothing to work out her basic difference from Wevers. They simply ignore the principle that she puts forward and proceed as if the position of Wevers was indubitably correct.

In this JS rather misrepresent the present state within LXX studies. It is Aejmelaeus rather than Wevers who represents the cutting edge of LXX studies today. Yet, though they quote her thoughts and return later to further mentions of her, they never pursue her arguments nor try to argue out the conflict of her position with that of Wevers. JS rightly tell their readers (166) that “recent scholars are less sceptical than Wevers and Goshen-Gottstein regarding the value of the LXX for the textual criticism of the Hebrew Bible.” But do they agree with the “recent scholars”? In fact they align themselves much more with the approach of Wevers, and he remains the pilot whom they follow much of the way. They are, perhaps, impressed by his voluminous work of text edition and commentary.

Now there should be no doubt about the great debt that LXX scholarship owes to John Wevers, especially for his diligence, determination, and sheer hard work, evidenced in the enormous achievement of the volumes of the Göttingen Pentateuch, plus accompanying textual studies. His criticisms of the misuse of LXX evidence by *BHS* are also often noted (JS mention them on 166). On the other hand, his work shows more of stubborn determination than of imagination. One of his principles is to be “distrustful of texts which are extant only in the minds of scholars.” Carried out seriously, this would mean that no LXX text could ever compete with an MT text, since the retroversion exists only in the mind of the scholar.

JS do disagree with Wevers at some points (212 n. 15, perhaps 285 n. 37). They are aware of criticisms of his work, for example by both Ulrich and Tov (170). Following Ulrich, they record (170) that in his Leviticus edition Wevers “did not choose even one of the Qumran readings as original.” They go on to report Ulrich’s judgment that Wevers assumed that the MT was the *Vorlage* of the original Greek and then “chose the readings of the uncials because they render the MT more literally than do the Qumran texts.” This is a very serious criticism, one that if valid threatens the reliability of the Göttingen

volumes on which JS depend so heavily; but JS seem to do nothing to penetrate into it or valorize it within their own work. More recently other criticisms have been published: see for example J. A. L. Lee's remarks in his review of Wevers' *Notes on the Greek Text of Deuteronomy* in *JSS* 45 (2000): 177–79. Lee comments also on defects in the Greek of that study (Lee's review was, of course, not available to JS at the time of their writing). JS should have been more cautious. They quote Wevers' *Notes* on books of the Pentateuch quite a lot and advise their readers (235–36) to take them as their next basic reading. But Wevers's stubborn negativity toward any idea that the LXX might point toward a different Hebrew text is ingrained into these volumes: thus the preface to the Exodus *Notes* enshrines the policy “that one must begin with a prejudice towards the text that we actually have”—as if we do not “have” the LXX! And JS have left themselves open to the suspicion that their attachment to Wevers is based on the fact of his favoritism toward the MT.

Now of course it is perfectly proper and correct to propose explanations of LXX texts which explain them not on the basis of a Hebrew text other than MT but on the basis of ideology, harmonizations, and hermeneutical interests. What Aejmelaeus demands is that, if these are to be proposed, it must be done on the basis of *evidence*. In this respect JS have not served their own purposes well. For, although hermeneutical interests have been put forward as explanations of variant readings at many places in this book, the absence of evidential backing is glaring. Every case of alleged hermeneutical motivation for variants as cited here is lacking in serious evidence and in many cases is no more than guesswork and pathetically unconvincing. As we saw with Gen 1:6, the idea that the Greek moved a clause “perhaps because it sounded out of place” is very weak. In Exod 2:22, which is quoted (150) in the comparison between Wevers and Aejmelaeus, the plus “and she conceived and” before “bore a son” is explained by Wevers as an “embellishment” originating with the translator's “fine biological logic.” This is probably no more than a contemptuous throw-away line. Without evidence of the biological interests of the Exodus translator, it has no value. Other cases where hermeneutical reasons have been alleged, but with no evidence to support them, have been seen above at Gen 4:8, Amos 9:12, and others. In this respect the book under review actually counts as a serious dissuasive against the hermeneutical option.

11. Preference for the More Difficult Reading

The principle of the more difficult reading is mentioned only briefly in the section on the “canons” of criticism (128), but it is ubiquitous in the work as a whole. Some of the examples above show how it was relied on in the discussion of various texts. JS seem to

feel that, if the MT is the more difficult reading, that is something in its favor (Gen 4:8; Isa 52:15). At 1 Kgs 2:5, where an explanation through “error of sight” is cautiously judged to be plausible, they say that if this is so “we would not need to insist on the principle that the more difficult reading is preferable” (164). This suggests that the argument from the more difficult reading is thought to be something like a final argument, without which other arguments are left weaker.

The principle that the more difficult reading should be preferred is quite mistaken when applied as a criterion of decision in the context of MT/LXX and analogous comparisons. Though dinned into the ears of generations of students and at first sight paradoxical and interesting, it is quite wrong and should be abandoned. At the most it has a certain validity as a sort of description of what has happened in certain types of scribal transmission. What it says is: in those cases where a reading has altered, consciously or unconsciously, from a more difficult reading to an easier reading, then it is true that the more difficult reading was the original one. It is in other words tautological. For cases where the reading did not change because it was more difficult to an easier one, it is of no importance. The obvious reason is, firstly, that there are other causes for alterations than difficulty, and, secondly, that, in a large proportion of cases, a scribal mistake or corruption produces a garbled text, often rubbish. These and other reasons have already been pointed out by B. Albrektson, *OtSt* 21 (1981): 5–18; and E. Tov, *Textual Criticism of the Hebrew Bible* (1992), 302–305. One reason why people allow themselves to think of the principle *lectio difficilior potior* is that they have already sifted out in their minds the numerous garbled readings that lie in the manuscripts and in the apparatus of critical editions. In the LXX—especially if one confines oneself to critical editions that have sorted out the garbled readings—there are hundreds of readings that, if they were taken seriously, would be difficult or impossible and would therefore, by the principle of difficulty, be superior to Vaticanus and to MT itself. The limitations, or better the fallacy, of the principle that the more difficult reading is preferable should now be made clear to all students.

To this general perception another, and a more particular, should be added. There might well be some place for the rule of *difficilior lectio potior* if it was used within the context of *monolingual* textual comparisons: thus, instance, if we are dealing with two Hebrew texts, such as one in Kings and one in Chronicles, or perhaps the New Testament, or a Latin text in which we may compare a word as it was in an early manuscript with copies of the same as made a thousand years later. But the case of a translation in which an ancient document is rendered into another language by persons who have only a limited access to forms, grammar, and meanings in that older language is different. JS themselves betray this in their occasional passing statements that imply that the LXX will be “easier.” Quite so. In many ways it may be. But this carries with it the obvious, but often

unnoticed, implication, namely, that the priority of the more difficult reading will naturally play into the hands of the MT. And that means, seeing it from the other direction, a preference for the MT will naturally favor a preference for the principle of the more difficult reading.

In any case it is extremely doubtful when a reading can properly be assessed as “more difficult.” It is unfortunate that this assessment is frequently used in the book under review.

12. Primacy of Certainty about the Greek Text

At several points our authors seem to insist that the first step in the critical process lies in gaining certainty about the correct Greek text. Chapter 6, “Establishing the Text of the Septuagint,” precedes chapter 7, “Using the Septuagint for the Textual Criticism of the Hebrew Bible.” Thus we are told: “Before we can use the LXX to help us determine the Hebrew text, we need to establish the Greek text itself” (120). And there follows a perfectly good discussion of problems of manuscripts, assessment of evidence, probabilities, and the like. And when we come to chapter 7, on page 154 we have a “checklist” type of procedure in which the first item is: 1. *Ascertain the Greek Text itself*. How do we do that? As illustrated from the example of Deut 31:1, we should “consult the apparatus in Wevers’s edition of Deuteronomy” (154–55).

Now there is indeed some reference in chapter 6 to aspects of the Hebrew, but these are not emphatic enough to constitute a correction to the principle that has just been set out. For my point is this: in the LXX one cannot ascertain the Greek text without considering the Hebrew from which it may have been translated. The *apparatus criticus* to an edition of the Greek may give us five or ten or more Greek readings. But our selection between them cannot be rightly made on the basis of Greek-text categories alone. Just as the Greek text may be helpful in the establishment of the Hebrew, for the correct establishment of the Greek text it is necessary to perceive a path that leads from a Hebrew text to a Greek text. I here use the indefinite article (*a* Hebrew text) because the perception of the path may sometimes imply a Hebrew text differing from MT, perhaps only in having different vowels or letters in a different order, and similarly the path may bring us to a different one among the variety of Greek readings, a different one from what might be “established” as best on grounds of Greek text alone. In other words, there is no linear sequence under which the Greek text is first “established” and its connection with the Hebrew is then investigated. JS touch on this aspect on pages 135–36 but scarcely advance to a proper recognition of it. Indeed, their expressions seem to emphasize a

linear procedure that gives priority to the establishment of the Greek text. Again, later, at page 277, they do indeed write:

Because the LXX is a translation document, another level of complexity must be considered, namely, the need to take into consideration what Hebrew form is being translated. This question, in turn, is itself an intricate problem because we cannot assume that the Hebrew text has remained basically unchanged since the Greek translation was made.

But this blindingly obvious remark undermines the approach of chapter 6. What I say was certainly known to the great text-editors like Rahlfs and was taken into consideration by them in the choice of readings. One can often see how Rahlfs or Ziegler based their choice of one Greek reading rather than another on the basis of its relation to the Hebrew. But this has not always been adequately formulated. If I may cite an example on which I worked myself (see J. Barr, “ἐρίζω and ἐρείδω in the Septuagint: A Note principally on Gen. xlix.6,” *JSS* 19 [1974]: 198–215): in Gen 49:6 the major readings offered by the collation of Greek MSS are forms from the verbs ἐρίζω and ἐρείδω, meaning respectively “strive, conflict” and “support.” I was able to show that a path existed back from the former of these to the Hebrew, involving a d/r variation and a reading with different vowels, both of which are common. The reading ἐρείσαι, from ἐρείδω, has no path from the Hebrew that I have perceived and none that I know to have been suggested. Rahlfs’s edition placed it as his main text, but he probably had not thought of a path that fit. Having none, he probably printed ἐρείσαι because it was the reading of B. Critical editions of the LXX, or of any text, commonly do not have enough space to indicate the argumentation involved in cases of this kind. I required seventeen pages to set out the argument for this one word. This does not mean that I claimed to explain the MT or to discover the true Hebrew reading; that would be another matter. But the choice between Greek readings depends entirely on reference to the Hebrew.

This is only one small example, but even if my example is wrong in some way it illustrates the kind of investigation that needs to be done. In principle, Hebrew/Greek interrelations must be involved at every stage. There is no linear priority for the establishment of the Greek text.

13. Reliance on Authoritative Editions

Following this, we note that JS tend to give some emphasis to the authority of certain major sources.

A peculiar expression that is repeatedly used is “secure” or “textually secure” (155, 190–92). At 155 it means that the Göttingen edition shows no variants (a footnote adds one rather slight variant). At 190 it is in a New Testament context: “secure” is explained as “i.e. the NT manuscripts have no competing variants.” On 192 we read that at Rom 9:33 “the LXX text, which does not have $\pi\hat{\alpha}\varsigma$, is secure: only manuscript 407, a ninth-century minuscule, includes the word.” This would appear to mean that, if a word or phrase is supported by all the manuscripts, or by all but one marginal one, it is beyond all doubt or question. But if that is the meaning, how does it fit with page 136, where we are told, “One must allow for the possibility that the original reading has not been preserved in any of our witnesses”? The need for conjectural emendation, this passage goes on, is acute in ancient writings, but in the LXX, with numerous witnesses, “it is ... likely that the original reading in any one problem passage has indeed survived somewhere.”

This is a peculiar approach, and one that seems rather out of harmony with the mood of the rest of the book. On the one hand it displays a remarkable confidence in the standard editions. These editions are of course excellent but not infallible. Where different Greek readings exist, especially in difficult cases, there is no objective or authoritative means that will decide which reading is to be printed. It depends on the wisdom and experience of the editor and the perspective in which he or she perceives textual problems in general. Thus at Gen 49:6 both Rahlfs and Wevers print $\epsilon\rho\epsilon\acute{\iota}\sigma\sigma\alpha\iota$ as their main text, as mentioned above, but I think this was mistaken. On the other hand, by stressing that the right word must be there somewhere in the LXX tradition, the view of JS may tend to obscure for the reader the significant place that editorial conjecture has had in the tradition of the LXX.

14. Conjecture within the LXX

Something needs to be said, therefore, about this, because it is often not mentioned in introductions for beginners and ought to have been explained here. The Rahlfs edition, which readers will mainly use (or the larger Göttingen edition, for that matter), actually contains quite a considerable number of emendations, which are marked very unobtrusively with a tiny note in the apparatus. Moreover, unlike the recommendations of a text such as *BHS*, as explained above, these are real emendations that are inserted in the main text, with an indication added in the apparatus. A first example, at Gen 1:30, is the indication $\pi\alpha\nu\tau\alpha$ Gra. This means that the great German/English scholar Johann Grabe (1666–1711) proposed the insertion of this word, a word that is absent from all the Greek manuscripts but, when added, provides exact agreement with MT. A large proportion of the emendations in LXX editions come from Grabe or, in more recent times, from Rahlfs himself. Those by Rahlfs are similarly noted: e.g., $\pi\epsilon\rho\iota\mu\acute{\epsilon}\nu\omega$ Ra. at Gen 49:18, a reading

that contradicts the great MSS B and A but yields a better correspondence with MT. Conjectures are often thought of as damaging to the authority of the basic text, but quite a number of the readings in our LXX editions that produce agreement with the MT are in fact the product of conjecture. Grabe appears again at 3:17. Rahlfs himself produced many such, the first (a small one) appearing at Gen 3:11. Another, and of major importance, is the reading ὠτία “ears” at Ps 39(40):6, already mentioned above. All Greek MSS have the reading σῶμα, used also in the New Testament, but the reading in the Latin of the Gallican Psalter (marked as Ga in Rahlfs’s apparatus) restores a Greek reading that correctly renders the MT. The emendation is not pure guesswork but is well grounded, as JS correctly see (196).

15. General Balance

The book is likely to suffer some criticism on the ground that it failed to display some of the major passages at which the LXX is most likely to have been superior to MT. Some of the texts that might have been most decisive are not mentioned or discussed at all.

Consider for example the outstanding case of 1 Sam 14:41. Here the LXX, retroverted into Hebrew, produces a much richer and more meaningful text than the MT:

MT

And Saul said to YHWH the God of Israel,

“Give *tamim*.”

LXX

Therefore Saul said, “O Lord God of Israel, Why have you not answered your servant today? If the guilt is in me or in Jonathan my son, O Lord, God of Israel, give Urim; but if you say, [it is] in your people Israel,

give Thummim.”

Note that a version based on the LXX brings the incident into the context of the ancient use of the sacred lot and provides an access to the correct vocabulary of it. The loss of this section from MT caused the word translated as *Thummim* to be misunderstood and consequently revocalized as if it were the common adjective meaning *perfect*. The LXX, it should be observed, does not provide an actual transliteration of the Hebrew terms Urim and Thummim. It provides etymologically related renderings, δῆλοι and ὀσιότης, and similar or related terms are used for Urim and Thummim elsewhere.

With this case we touch upon a criterion that JS tend to pass by: the criterion of sense or meaninglessness of the MT. Naturally this is a ticklish point, and they are not entirely to be blamed for not facing it. But it is a reality in the whole history of textual criticism. Naturally, it can be said that it is “subjective” to decide whether a word or a text makes sense or not. But there are cases where it cannot be avoided.

An obvious case—yet, curiously, one of which many students are ignorant—is at 1 Sam 13:1. Here the Hebrew can mean only one thing:

Saul was one year old when he became king, and he ruled two years in Israel.

The KJV could see that this was straightforwardly impossible, for the rest of the Saul story makes it clear that he did not become king at one year old, nor would it have been easy for him to carry out the many things related about him within two years. Thus, far from remaining “faithful to the text,” KJV simply falsified it, writing:

Saul reigned one year; and when he had reigned two years over Israel...

Now the LXX certainly does not provide the perfect answer, for the best MSS seem just to omit the verse, plausibly because they, or the manuscript tradition that they followed, could see that it was nonsensical and perhaps left it blank, hoping that the correct words could be inserted later. Later LXX traditions, however, do point in a direction that might be acceptable, furnishing some figures that might have fallen out from the Hebrew or might come from assimilation to the figures of David and Solomon. Thus Acts 13:21 has “forty years.” Many modern versions follow this or leave a gap for figures that have fallen out.

Such examples from 1–2 Samuel form only one of the areas in which important examples might have been mentioned. Another, and a quite different, area is that of the chronological figures in Gen 5 and 11, plus the key Exod 12:40, mentioned already above. To these we may add the *seventy-five* of Acts 7:14, agreeing with LXX at Gen 46:27, Exod 1:5, and with not only these verses but also the entire text of Gen 46:8–27. Other numerical lists produce similar problems, for example, the list of Canaanite city-kings in Josh 12:9–24.

These same examples indicate another aspect, namely, that not only the possibility and impossibility of reports are relevant but also the meanings of words, their place in Hebrew grammar, and their vocalization.

15. *Vorlage*

The book makes frequent use of the term *Vorlage*, which is of course the common technical term. But for an introductory book the German term is unfortunate, for it fails to make a distinction that is very essential. What “lay before” the LXX translators was a string of consonant symbols (some of them functioning as signs for vowels). This was all that they could “see” in the visual sense. Even if the MT in a sense “existed” then, it means only that the consonants of the MT were there. The other component of the text existed not in writing but only acoustically. There was no actual “MT” because the MT by its nature is a pointed text that gives written registration of vowels and thus provides the full phonological information of the text. Any text visually seen by the LXX translators gave only partial information of this kind. There were two ways in which they could “read” this. They could scan the consonantal string for semantic possibilities, and the semantic possibilities would offer both a definition of the meaning and a phonetic realization. This is how one reads a Hebrew newspaper today. Or, the translator would know or would hear or would remember the traditional vocal realization of the entire text, which would include both consonants and vowels but would not exist in writing. The importance of this for study of the LXX is made evident in a conversation between the writer and Emanuel Tov, which seems to be unknown to JS (J. Barr, “Vocalization and the Analysis of Hebrew among the Ancient Translators,” in *Hebraische Wortforschung: Festschrift zum 80. Geburtstag von Walter Baumgartner* [VTSup 16; Leiden: Brill, 1967], 1–11; E. Tov, *Text-Critical Use*, 160–65; J. Barr, “‘Guessing’ in the Septuagint,” in *Studien zur Septuaginta—Robert Hanhart zu Ehren* [1990], 19–34). A further note that is not often considered: the unpointed Hebrew script provided in written form only one component of the total “text,” and the other component was transmitted acoustically. Thus in common contexts ךָּן did not distinguish between “he gave” and “giving.” A translation into Greek, however, had to make that decision. Thus, for instance, in Deuteronomy the frequent phrases “that YHWH your God gave to you” and “that YHWH your God gives to you” are clearly distinguished in the LXX. Of forty-nine relevant cases, in only three does LXX differ from MT (participles are not spelled with *waw* in this area). From the point of view of the information conveyed, and in the limited cases where it works in this way, the LXX can be said, paradoxically, to be anterior to the MT.

16. Inspiration

The authors are interested in maintaining a position of divine inspiration of scripture, and they probably anticipate that many readers will have the same view. Some would wish to

rule this out as irrelevant for an academic discussion, but to the present writer it is to be respected and perceived as an important ingredient in the complex of ideas involved. My interest here is not to weaken or to challenge their position but to ask for information. How does it fit with the work on the LXX here presented? For they write (152 n. 12), “Even in the view of the most theologically conservative scholars, whether Jewish or Christian, divine inspiration applies to the original writing of the Scriptures and does not extend to scribal activity.” This is the traditional Warfieldian Protestant position. It has, or seemed to have, the advantage of asserting the divine inspiration of the Bible while leaving room for textual criticism, in that the various inconsistencies within the Bible could properly be subjected to critical analysis, but the original inspiration would remain untouched.

Within the body of their study, however, it seems to run into several questionable areas. One is their not absolute but extremely strong preference for the MT. How can an inspiration that attaches only to the original delivery of Scripture support the centrality and authority of the MT for Christianity? This latter would seem to require exactly what their statement excludes, that is, an inspiration of the Jewish scribal activity. Or, in other words, a double doctrine: firstly, divine inspiration attaches to the original delivery of scripture, and secondly, in addition, divine inspiration continues to preserve scribes and guardians of the MT throughout the centuries. JS may well have a ready answer to this, but it is not immediately obvious.

The second question comes with the New Testament. In many passages the New Testament follows the LXX or comes close to it, and in a number it departs substantially from the MT. Since the New Testament is inspired and infallible, does this not mean that the LXX is the true form of the ancient scripture? JS do not evade this sort of question, and they put it before their readers. Did Jesus himself quote the LXX form of Isa 61 (Luke 4:18 [194])? If, according to an inspired and infallible source, Jesus quoted the LXX text, whether in Greek or in Hebrew, surely he did quote that text. But, if so, why is it so essential to maintain the superiority of the MT?

Again, modern LXX study makes it highly likely that Jeremiah (and perhaps some other books) came out in two editions, one (being shorter and probably older) represented by the LXX another by the MT. The differences of quantity and order are very large. Which then had the divine inspiration? JS raise the question correctly (123).

17. Older Scholarship

An interesting section is chapter 11, “Our Predecessors: Septuagint Scholars of a Previous Generation.” It furnishes a simple biographical picture for Lagarde, Rahlfs, Thackeray, and some others. This is interesting and readable and suitable in form for students. But it could be improved by some modifications, some in fact and some in value judgements. Misstatements in these areas can be damaging to the reputation of a scholar.

The most important case is that of Lagarde. What is written about him by JS is entirely right except for one thing. Readers may well ask: Why is he so important? What did he really do that put LXX studies on a track that most are still following? Obviously, he knew numerous languages and published ancient texts of all kinds, including, they tell us, “the Arabic theory of colors.” He was praised as phenomenal by Wilamowitz-Moellendorff, of whom however most readers will never have heard.

But everything he did seemed to go wrong. His Genesis edition was “roundly criticized as an uneven work” and was “his biggest failure.” Why, then, readers may ask, should we revere him as the author of some fundamental principles for work on the LXX? Did he not work in part by abstract reasoning? And did his identification of recensions not depend in considerable measure on Jerome’s statements about the distribution of recensions over three regions of the ancient world? And, especially, how did he become convinced that the LXX tradition went back to one single original, which editions should seek to discover and to print? In not specifying an answer, JS follow some other handbooks on the subject.

Surely the answer must be obvious. First, Lagarde had enormous experience in the editing of patristic and oriental texts, of which the LXX was only one. Second, he had the advantage of the collations published by Holmes and Parsons (the British publishers sent him a complimentary copy). He could easily see two things: firstly, that from the entire mass of readings one could separate out those that were the product of certain revisions or recensions, of which he sought to edit and publish the Lucianic; secondly, that the mass of other readings, however they differed from one another, could readily be explained as the product of standard scribal variation starting from one original text, even if it was sometimes difficult to see what the original reading had been.

One figure who should have been included in this chapter is Paul Kahle. JS do mention him but not in this particular chapter, and they treat him rather negatively, mainly on pages 35–36 and 274–75. They say, correctly, that he attacked the Lagardian position and argued that there never was a single original translation and that various separate versions had been created; there was therefore no meaning in a quest for a single original. They

also rather emphasize the opposition that Kahle's views encountered. His theories "created heated controversy during his lifetime" (36). "Most scholars rejected" his hypothesis (275). Wevers (36 n. 11) and Orlinsky (275 n. 10) are quoted as opposing him (in 1964, 1988 and 1941).

I am not so sure of this. If things were so bad, why did anyone pay any attention to Kahle at all? As I remember it, Kahle exerted an enormous influence upon many scholars in the first decades after 1945. I myself was one of these. Though I came in due course to differ from essential aspects of his thinking, Kahle opened to me a huge area of knowledge that had been virtually unknown for most students in biblical studies. In matters of the Hebrew Bible text, Kahle was a towering store of information, much of which had not penetrated into the average channels of textual criticism before him. In particular, his analysis and publication of the earlier stages of the Masorah gave a new historical dimension to study of the biblical text, in any language. As against the negative verdicts expressed by Orlinsky and Wevers, see the balanced survey by S. Talmon in *The Cambridge History of the Bible I* (1970), 177–80. More recently E. Tov, *Text-Critical Use*, 41: "the usual polarized description of these theories is not justified because they are, in fact, not mutually exclusive." JS themselves also, following Fernandez Marcos, express a more moderate view on page 276.

18. Conclusion

I believe that this book could be greatly improved by some fairly minor alterations. The most obvious is to reduce its apparent negativity toward the LXX. As it stands, the book runs the risk of discouraging its own readership, many of whom may feel that LXX study, however worthy and informative, provides in the end practically no valid readings that can compete with those of the MT. This is not a good way to commend interest in the LXX. We do not want students to feel that study of the LXX is a vast and complicated body of knowledge designed to prove that no new insights into the Hebrew Bible can be gained from it. This is not the actual opinion of JS, and all that would be required is to emphasize somewhat more their more "positive" remarks that are already there and if possible to add more textual examples of the kind already categorized as "worth considering." It would be good also to avoid the impression that a total decision for or against should be aimed at and that something more like a proportion of probabilities would be better.

Another basic question is this: Can the relation between MT and LXX really be adequately pictured as one between two competing texts? In other words, even in the bad old days it

was rather unusual for scholars to start with the LXX and declare that it, purely as a text, might be better than MT. Most proposals actually took their start from problems visible in the MT: aberrant syntax, meanings that contradicted, breaches of literary style, conflicts with parallel passages, historical improbabilities or impossibilities. These are not given sufficient visibility in JS's book as it stands. Too often it emphasizes the mountainous "difficulties" to be overcome before an LXX reading can be made acceptable, but when the MT is considered no such mountain of difficulty is encountered. Small adjustments might alleviate this imbalance.

A good example lies in the question of retroversions, i.e., working out from a Greek text the Hebrew words from which it may have been translated. This is one of the difficulties that JS stress, and they insist (153) on "rigorous criteria" for doing it, enough to frighten off most students from ever looking at a retroversion. But they do nothing to instruct students in what is the most elementary step toward doing it, i.e., to know how to work with Hatch and Redpath's concordance, which is barely mentioned (155, 241, 253, 301). An explanation of its peculiar method of classification and of how to use it both from Greek to Hebrew and from Hebrew to Greek would have been an enormous advantage.

There remains a question of how the LXX should be introduced to students. The design of this book favors the idea of a survey of the field, and with this aim it covers the ground very well with its chapters on the different aspects. Personally, I would go about it in a quite different way. For students who may have only limited Greek or Hebrew, much that is included here is too advanced. They do not need to bother with such terms as the *κρίσις* Recension or scholarly fusses about whether it is correct to call it the Septuagint or the Old Greek or such matters. What they need is to gain experience in the language and in the interaction of the two languages, Hebrew and Greek, and above all the varieties of translation technique. This is certainly done by JS but not to the extent I have in mind. This should be done to a considerable extent in a way that leaves aside the question of right and wrong readings, justification or nonjustification of the MT, and so on.

**Response to J. Barr's review of *Invitation to the Septuagint*
by Karen H. Jobes and Moisés Silva**

We are pleased that Professor Barr has given so much—and such careful—attention to our book. We are moreover very appreciative of the generous remarks, as well as constructive suggestions, found at various points in the review. It is also the case, however, that his assessment gives an inaccurate picture of the book, and readers may find a brief response helpful.

Barr's critique focuses almost exclusively on the use of the LXX for the textual criticism of the Hebrew Bible. While it is quite appropriate for a reviewer to select for special attention an important issue, such as this one certainly is, it is also fair to point out that the book was never intended as a manual for the textual criticism of the Hebrew Bible—a topic to which we devote one chapter out of fourteen (and even if we include scattered statements elsewhere, it is doubtful that more than fifty pages out of over three hundred address this issue). Our primary concern is rather to help readers understand the Greek versions in their own right. Insofar as the review does not clearly set our treatment of the MT within the context of that larger (and logically prior) aim, it runs the risk of giving quite a false impression not only about the book as a whole but also about what it really says regarding the value of the LXX for the Hebrew text.

In particular, Barr's overarching evaluation that the book shows an “apparent negativity toward the LXX” (concluding section of the review) is very surprising to the authors and is not borne out by the facts. It is certainly a false evaluation if one thinks of the book in general, but even when one keeps in mind that Barr has in view the text-critical use of the LXX (and only that, we assume), it must still be said that the evidence does not support him. For example, out of twelve specific passages discussed by him, he actually agrees with our judgment in a majority of them. Indeed, only with regard to four of the variants (Gen 4:8; 1 Kgs 2:5; Isa 53:7, 8) does he find that we have rejected the LXX reading without sufficiently valid reason; moreover, it is only in the case of the two variants in Isaiah (in a passage where the Greek translator has indisputably made several mistakes) that we express any confidence about the inferiority of the LXX. Barr himself elsewhere (sect. 6) acknowledges that at several points we make positive statements about the text-critical value of the LXX, and in fact there are more of these in the book than the three he mentions. For example, “The primary source [for variants] consists of the ancient versions, and inasmuch as the LXX was the only translation of the whole Bible produced prior to the standardization of the pre-Masoretic text, it takes on *unique importance*” (*Invitation*, 148, emphasis added). Elsewhere (152) we explicitly distance ourselves from “scholars who abandon the readings of the MT in favor of a LXX variant only as a matter

of last recourse”; we add that “such an approach cannot be easily defended, and it is likely to lead us astray.” Several other statements could be adduced.

In the light of these and various other facts, one wonders what would have led Barr to assess the book as he did and to say that we have an “extremely strong preference for the MT” (sect. 16). A possible answer arises from reading his comments on “Inspiration” (a topic to which he devotes a whole separate section, even though we mention it only in passing in a footnote). Knowing that the authors regard themselves as evangelicals (though the book nowhere says anything about that) and apparently assuming that there is a standard evangelical position on “the centrality and authority of the MT for Christianity,” Barr proceeds to attribute that position to us. His comments, however, misrepresent our view of the role of the MT in textual criticism. In truth, if any such considerations had been at play in the book, they would have run in precisely the opposite direction. While we recognize that the MT, being the only extant text of the entire Hebrew Bible, enjoys a privileged standing in some respects, it is our firm conviction that, *in the attempt to make text-critical decisions*, no preference should be given to any surviving textual tradition on theological or religious grounds. We do believe (along with many other scholars with differing traditions) that, for most books of the Bible, the textual form preserved in the MT is *generally* more reliable than that found in competing witnesses, but that is a conclusion we reach strictly on the basis of widely accepted text-critical criteria.

Among a number of Barr’s specific criticisms, at least a few should be addressed.

(1) According to him (sect. 7), the book “has one serious misstatement,” namely, our comment that the Qumran scroll A of Isaiah contains “essentially the same Hebrew text as found in Codex Leningradensis” (177). He believes that “the authors must surely mean the B scroll of Isaiah.” Actually, we do mean the A scroll, and the fact that he thinks we have made a mistake may be a clue to where are our real differences lie. While it is of course true that the A scroll contains many important variants (a few of which are reflected in the LXX), the point at issue is whether this scroll—once we set aside obvious errors by the scribe of that document, as well as orthographic/morphological variations that do not affect the sense—gives evidence of an underlying consonantal text that is essentially different from the MT. Most assuredly not. The language we use in the comment quoted above is commonplace in the literature. For example, E. Würthwein (*The Text of the Old Testament* [2d ed.; Grand Rapids: Eerdmans, 1995], 33) says that the scroll “essentially supports” the MT; S. Talmon (in *ASTI* 1 [1962]: 62), though arguing that this manuscript should be given greater weight than some scholars do, speaks of the “basic identity between MT” and the scroll; J. Hoegenhaven (in *JSOT* 28 [1984]: 19) uses the phrase “fundamental agreement.”

(2) Particularly misguided, in our view, is Barr’s strong—indeed, drastic—rejection of the principle *lectio difficilior potior* (sect. 11 of the review). This section is frankly one of the most disappointing in the review, primarily because it shows great insensitivity to the way some of our best textual critics have both articulated and applied the principle. For example, Barr comments: “In the LXX ... there are hundreds of readings that, if they were taken seriously, would be difficult or impossible and would therefore, by the principle of difficulty, be superior to Vaticanus and to MT itself.” But who has ever argued that “impossible” readings are preferable? Moreover, Barr here ignores the extremely important qualification that the canon in question has in view readings that are superficially, not intrinsically, difficult. Barr adds that while there might be some place for this principle in monolingual textual comparisons the situation is different in the case of a translation. But this distinction is patently invalid. A variant based on LXX evidence is worth considering only if we can offer a credible “retroversion” into Hebrew, but once we have done that, the comparison becomes monolingual: we need to decide between the Hebrew reading of the MT and the Hebrew reading of the text underlying the LXX. It should be added that Barr’s comments in this section are quite inconsistent with his own perceptive discussion in *Comparative Philology and the Text of the Old Testament* (Oxford: Oxford University Press, 1968), 266–68 (e.g., “Where it is a matter ... of obscure words in *normal* contexts [i.e., in contrast to technical architectural passages, etc.] and of strange meanings for common words, there was a strong tendency towards the levelling of the vocabulary and the interpretation of that which was rare as if it was that which was more normal” [p. 268]; this fine statement undercuts Barr’s argument against the MT at Gen 4:8 in sect. 5 of his review).

(3) In section 13, where Barr faults us for placing “remarkable confidence in the standard editions,” he characterizes our position as “stressing that the right word must be there somewhere in the LXX tradition.” Both here and in the following section he seems to imply that we say nothing about the need for conjectural emendations. Our true position (which Barr himself quotes) is that, given the numerous surviving LXX witnesses (in contrast to most Greek and Latin works), it is “likely that the original reading in any one problem passage has indeed survived somewhere” (*Invitation*, 136). How our word “likely” metamorphosed into “must” is unclear, particularly when we immediately add, “But ‘likely’ is not the same as ‘certain.’” Moreover, we go on in that paragraph to commend Ziegler’s conjecture at 53:2, and on page 225 we adopt an old conjecture at Isa 53:11 not mentioned by Ziegler.

(4) In the fourth paragraph of section 9 of his review, Barr references our statement: “We should have good reason to believe that the presumed Hebrew/Aramaic reading truly existed in a manuscript and not only in the mind of the translator (whether by a mistake or by a conscious emendation)” (*Invitation*, 153; Barr leaves out the parenthetical clause,

which gives some indication of where one might look for the evidence). Barr goes so far as to call this principle “nonsense” because “there is no way of *knowing* that a reading existed in a manuscript unless we have the actual manuscript.” But we mean no more and no less than the inevitable kind of judgment that every scholar must make—as indeed Barr himself makes in the fourth paragraph of section 5, where he expresses great assurance (“It is to my mind clear”) that the verb at Amos 9:12 “was written as in MT but was read [by the Greek translator] as with a D and not a Y.”

With regard to Barr’s other (and milder) criticisms, some of them are certainly worth discussing—and we are genuinely grateful for the stimulus they provide—but we do not find them persuasive. In any case, we encourage readers to consult our book directly before forming their own opinion regarding our views.