

Returning to the term "second intellect" (or "Upper Eden"), we see that both Israeli and Ibn Mayor define it as a separate intellect. Now this is quite strange, for there seems to be no system in which the Active Intellect emanates from a second, separate, intellect. One possible explanation, to my mind, is that both Israeli and Ibn Mayor were in a sense amalgamating al-Kindī's and al-Fārābī's versions of the quadripartite division. Thus, they incorporated two different elements—the "second intellect" and the "acquired intellect"—into one expanded scheme. While retaining the term "second intellect" (*al-ʿaql al-thānī*) by placing it in the hierarchy of the intellect, they changed its original meaning: the "second intellect" became a separate intellect. Thus, both al-Kindī's and al-Fārābī's views of the division of the intellect found their place, though in a modified form, in fourteenth-century Jewish thought.

In conclusion, it should be noted that the intermediate links through which the quadripartite division entered Jewish thought are still not completely known. Hopefully, this gap will be filled with further research into the manuscripts and translations of the period.

RESPONSE TO HERBERT BASSER¹

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One can, heuristically speaking, differentiate between two types of academic reviews: those which critically engage the argument and evidence of a book in order to advance collective scholarly discourse on its subject and methodology, and those which use a book as a convenient platform from which to promote mainly the views of its reviewer. I would happily allow readers of this journal who have also read my book to decide for themselves into which category the review by Herbert Basser better fits, without the need for further comment from me. However, since we all often read reviews instead of reading books, I might be allowed to indicate some of the ways in which Basser has not only failed to represent or engage the central arguments of my book, but has twisted its evidence for purposes that I have difficulty determining.²

Perhaps I had better begin with the end of Basser's review, since that is where many of its readers will have begun, in which he flippantly assigns my book the highest grade for its "conception of the enterprise," but a mediocre grade at best for "its execution." From this it might be supposed that Basser endorses in general my approach to the critical study of *Sifre Deuteronomy* and rabbinic literature more generally, but takes issue with my application of that approach to the particulars of the passages that I treat, something which in the complex nature of such texts would be unavoidable, even in the most sympathetic of reviews. In fact, the presuppositions that underlie Basser's criticisms of my analyses of midrashic texts are very much at odds with my own. But since Basser never describes coherently my "conception of the enterprise" (to which I devote the whole first chapter of my book), it is difficult for someone who has not read my book to tell from his review how fundamentally we differ, both in our "conception of the enterprise" and in "its execution."³ These differences will become clear as I

¹ Herbert W. Basser, review of *From Tradition to Commentary: Torah and its Interpretation in the Midrash Sifre to Deuteronomy*, by Steven D. Fraade, *JQR* 84 (1993): 82-90.

² While I have tried to respond to most and the more significant of Basser's arguments and examples, I have not responded to them all, since several are irrelevant to the argument of my book and his critique, being more of the nature of academic "show and tell." For an example, see below, n. 25.

³ Basser provides a very brief and incoherent misrepresentation of the program of my book at the outset of his review. For example, the juxtaposed "tradition" and

review his criticisms of my treatment of four passages of *Sifre Deuteronomy*, which form the body of his review (and a very small sampling of the passages covered by me). Basser's principal charges that I display "a limited perspective of the rabbinic corpus" and "insufficient attention to literary development" are simply not supported by any evidence presented by him in his alternative readings and interpretations of these passages, which presumably represent my worst and his best cases. To anyone who has or will read my book, Basser's charges will seem all the more vaporous.⁴

Basser takes me to extreme task for choosing "corrupt manuscript readings which no amount of literary ingenuity can successfully negotiate." As his first example he considers *Sifre* 313, where he charges that I have "rejected" the "better reading" of רואין אמת ומשכילין וידועין for the "faulty reading" of מטהלילין בו וידועין, the latter "choice" leading me to "faulty exegesis." Basser omits to mention, however, that מטהלילין is found in the best early manuscripts, citations, and printings of the *Sifre*, whereas . . . רואין ומשכילין is found principally in the sixteenth-century manuscript called *Midrash Hakhamim*, which while citing the *Sifre* often takes considerable editorial liberties with it.⁵ Although Louis Finkelstein frequently, as here, adopted the "improved" readings of this manuscript, contrary to the evidence of better manuscripts, in his edition of the text, he was strongly criticized for this practice by J. N. Epstein and Saul Lieberman in their reviews of his work. Since then, stemmatic and linguistic work on the *Sifre*'s manuscripts have confirmed Epstein's and Finkelstein's judgments and provide the basis (as I carefully spell out in a prefatory note to my

"commentary" of my book's title do *not* denote respectively, as he avers, "the *Sifre*'s understanding of Deuteronomy" and "the affirmation of its rabbinic methods." Similarly, as I shall elaborate below, Basser judges the historical claims of my book by historicist criteria that I explicitly and at length argue against in my book's first chapter, but without his referring to those arguments or presenting his own as alternatives. He is left, therefore, in the curious position of claiming that "we have nothing at all to test Fraade's results" [sic] even as he proceeds at length to do so, but without any consistent critical criteria.

⁴ As I will demonstrate, these charges only make sense if we assume, with Basser, a seamless continuity between rabbinic textual culture of different periods as between early rabbinic culture and its second temple period antecedents. Few reputable critical scholars today maintain such a perspective.

⁵ For text-critical specifics, see pp. 222-223, n. 187, of my book. The version included in *Midrash Haggadol* is somewhat similar, but would not support Basser's argument: *Midrash Haggadol* is מטהלילין בו רואין אמת וידועין. MS Berlin, which often agrees with *Midrash Haggadol* and *Midrash Hakhamim* in their "improved" readings against the other witnesses, is unclear here in the manuscript copy that I examined. But even if it agreed with *Midrash Hakhamim*, it should not be followed against all the other manuscripts. See next note.

book) for my own text-critical choices.⁶ Rather than honestly acknowledging and taking direct issue with these underlying text-critical principles of my work and the strong text-critical ground upon which my reading in this particular case rests, Basser simply avers that I have "chosen" (as if by caprice) a "corrupt" text.

What then drives Basser's choice of ומשכילין . . . רואין over the far better attested מטהלילין, especially when the semantic differences between the two readings barely affect the overall meaning of the passage or its contribution to my book's argument? Basser prefers the text of *Midrash Hakhamim* to that of the *Sifre*'s earliest manuscripts because he considers the former to be exegetically richer. The presence of the verb ראה in *Midrash Hakhamim* allows Basser to connect its commentary on Deut 32:10 (with its combination of the verbs ראה, בין, and עכל) to *Mekhilta Yitro* 9 on Exod 20:10 (with its use of the verbs פרש and ראה) via Neh 8:8 (with its use of the verbs פרש, עכל, and בין). This exegetical discovery of the uncited Neh 8:8 as the missing link between the text of *Midrash Hakhamim* and that of the *Mekhilta* justifies Basser's choice of the former as the correct reading for the *Sifre* (against substantial manuscript evidence), and his criticism of my choice of a "corrupt" text and my having "missed" the role of Neh 8:8 that is absent from it.⁸ This is circular reasoning at its best, and a newly discovered text-critical principle to boot. As Basser correctly concludes: "Once [the verb רואין] removed [sic], the composition of the midrash's relationship to Neh 8:8 is destroyed," and with it Basser's innovative explication. Fortunately, the *Sifre* text in its best attested form, מטהלילין בו וידועין, makes clear

⁶ See p. xvii of my book, with reference to the early reviews by Epstein and Lieberman and the later stemmatic and linguistic analyses of Codex Vatican 32 (upon which I mainly rely, where extant) by Menahem Kahana and Moshe Bar-Asher. As Epstein states of *Midrash Hakhamim* with respect to its reuse of *Sifre Deuteronomy*: "It does not differentiate between the sources, adding from other sources, shortening, and altering, and therefore *Midrash Hakhamim* cannot be at all determinative when it is a single witness" (*Tarbiz* 8 [1936-37]: 376 [Hebrew]). For Kahana's negative assessment of the text-critical value of *Midrash Hakhamim* when it disagrees with the other major manuscripts, and also when it agrees with MS Berlin against the other textual witnesses, see his "Prolegomena to a New Edition of the *Sifre* on Numbers" [Hebrew] (Ph.D. diss., Hebrew University, 1982), pp. 92-97, 234.

⁷ As I indicate (pp. 222-223, n. 187), מטהלילין of the midrash, like the verb בין of its scriptural lemma, denotes both seeing and apprehension, but with added mystical associations. In this regard, Basser's notes 2 and 3, on p. 83 of his review, make no sense to me.

⁸ Basser asserts that *Midrash Hakhamim* preserves an "earlier tradition," but he provides no evidence from earlier sources to support this claim. While this is certainly possible, it cannot simply be asserted without textual support. Equally possible is that the editor of *Midrash Hakhamim*, or some source from which he drew, sought, like Basser, to improve upon the text of *Sifre* in the course of trying to understand it.

sense without Basser's textual and exegetical improvements, deriving, as we must assume in the absence of any better evidence, from later stages in its transmission. If by my "limited perspective of the rabbinic corpus" Basser means my refusal to view as interchangeable rabbinic texts of all periods, I plead guilty with pleasure to the charge.⁹

Basser's second salvo concerns *Sifre* 306, where he follows MS Oxford, whereas I follow MS London.¹⁰ The principal difference between these manuscripts in the present case is the inclusion of four (MS Oxford) rather than three (MS London) colors of grass. But this difference has no effect on my main point in commenting on this passage or Basser's demurral from it.¹¹ More significant is Basser's criticism of my use of the term "slippage" to describe the exegetical and metaphorical flow of the commentary. Since Basser misrepresents (or misunderstands) my use of this term, I shall need to restate my argument briefly.

In Deut 32:2, Moses compares his teaching (לקח) of the Israelites prior to his death to the rain as it descends upon the vegetation. The *Sifre*'s commentary unpacks this metaphor in several ways, but each time employing the same formulaic structure: "Just as (מה) the rain (שעל-מים/מטר) . . . , similarly (כך) . . ." In the first instance, the rain is compared (. . . מטר) to words of Torah (. . . דברי תורה), the latter being an obvious (rabbinic) replacement for לקח, but producing the somewhat asymmetrical result: just as the rain falls on the trees, producing different varieties of trees with different flavors, so too words of Torah are "all one," but are of different types, written (מקרא) and oral (מוענה), and several types of the latter. Thus, although the formal comparison is between the rain and the words of Torah (both being "one"), the words of Torah share the characteristic of variety with the vegetation.¹² In the second interpretation, the formal comparison

⁹ If, however, by his charge he means that I have not been attentive to the history of rabbinic tradition transmission and transformation beyond the "pages" of the *Sifre*, a quick perusal of my book's index to cited rabbinic sources (pp. 320-328), would reduce this charge to naught.

¹⁰ This was my consistent practice, in the absence of MS Vatican 32, which is not extant for this section. See p. xvii of my book.

¹¹ As to the number of types of grass, following MS London, I saw a consistent three-fold structure to the midrash, whereas Basser, following MS Oxford, sees possibly a "progression" from five, to four, to three (p. 84 of his review). But Basser finds "no reason to say the numbers are significant." Nor do they bear on my principal argument.

¹² The division of the words of Torah is first into "written" (מקרא) and "oral" (מוענה), and second into three types of the latter. Basser stresses the overall division into five, while I stress the double division: first between scriptural and rabbinic, and second between the different types of rabbinic discourse. On such divisions of the rabbinic "words of Torah," see p. 244, n. 111, of my book.

is still between the rain (. . . שעל-מים) and words of Torah (. . . דברי תורה), but now it is the variety of types of *people* infused with words of Torah that is emphasized.¹³ In the third of this set of interpretations, the formal comparison finally shifts to that between the rain (. . . מטר) and the disciple of the sages (. . . תלמיד חכמים): just as the former cannot always be seen in advance of its arrival, so too the disciple of the sages cannot be known as such until he teaches the variety of rabbinic Torah, bringing us full circle, but not exactly, to the varieties of words of Torah in the first interpretation.¹⁴ This overall structure remains the same regardless of which specific manuscript readings one adopts for the numbers of types of grass, or types of people, or types of Torah discourse.¹⁵

Basser may be correct in distilling this series of interpretations down to the "simple" idea "that the behavior of rain serves as a nice metaphor for the nourishing effects of Torah" (p. 86). However, the three interpretations are not simply expressing the same idea in different words with no rhetorical progression between them in their presently arranged order. Unmistakably, the rain metaphor, through a series of interpretations, has shifted from (primarily) signifying the words of Torah to (additionally) signifying the disciples of the sages in their teaching practice of those words of Torah. It is this semantic shifting that I denoted as "slippage" (as frequently elsewhere), as a rhetorical rather than pejorative term.¹⁶ Basser views the midrashic explication as being "beautiful in [its] simplicity." I see it as being even more profoundly beautiful in the rich complexity of the rhetorical strategy by which it shapes its students through their necessary participation in its hermeneutical web and flow.

¹³ Whether these are different types of sages, as I argue, or different types of people more generally, as Basser argues, has little effect on the overall pattern and the conclusions that I draw from it.

¹⁴ One other marker distinguishes the disciple of the sages, that "he be appointed administrator (פרנס) over the public." While I argue (pp. 98-99 with notes) that this unanticipated quality is of significance in understanding the rhetorical function of this passage in its historical context, Basser neglects to mention it in his review.

¹⁵ According to Basser (p. 86), "The wrong choice of manuscript reading precluded Fraade's chance to interpret the passage properly." However, our major differences of overall interpretation do not rest on such minor differences of textual reading, as suggestive as they might be.

¹⁶ Basser mistakenly attributes to me, in using this term, "that the rabbis messed up the passage" (p. 85, and again on p. 89). I never make or imply such a negative judgment. The term is simply intended to denote the dialectical agility with which the commentary engages Scripture as a revelatory text of multiple and therefore fluid significations. The very same pattern of multiple interpretations of a natural metaphor for "words of Torah," leading to a final interpretation that climactically shifts to "disciples of the sages," can be found in *Sifre* 343, my interpretation of which is similarly critiqued by Basser and defended by me below.

Basser's second major criticism of my work is that I pay "insufficient attention to literary development in the context of second temple evidence and within midrash itself" and "dismiss the possibility of antecedent literary development" (p. 86). Similarly, he charges that I reject the Qumran *pesherim* "as important antecedents to rabbinic midrash" and "exclude all but rabbinic texts from [my] discussion" (p. 87). These are astounding charges considering that I devote the bulk of chapter 1 to a careful comparison and contrast between the structures, methods, and functions of midrashic commentary and its extant second temple period antecedents. Furthermore, in chapter 4 I include detailed comparisons of midrashic texts with their antecedents in *1 Enoch* and *2 Baruch*, highlighting in each case both similarities and differences between the midrashic text and a non-midrashic "countertext."¹⁷ In my introductory chapter I carefully distinguish between exegetical tradition and method in general and the running commentary form per se. I never deny that Paul, or the Gospels, or forms of second temple period "rewritten Bible" exhibit exegetical traditions and methods that are similar to, yet antedate, those found in rabbinic literature. But to equate, as Basser does, the fact that "Paul shares exegetical forms in common with the rabbis" (with which I agree) with a claim for "Paul's extensive use of rabbinic-style commentary" (with which I disagree), ignores a central thesis of my book, never mentioned in Basser's review, namely the cultural-historical fact and meaning of the ancient Jewish (including pre-rabbinic) "turn to commentary."

Basser simply misrepresents me when he charges that I deny the possibility of "antedecedent literary activity." Rather, I am skeptical about our ability to reconstruct the actual lines of such development between extant second temple and rabbinic texts given the fragmentary nature of what has survived. Despite his seemingly self-assured assertions to the contrary, Basser gives us no reason to be more confident in this regard. He is unable to provide a single concrete case of an antecedent second temple source.

¹⁷ Briefer considerations of a wide range of second temple period sources (Apocrypha and Pseudepigrapha, Philo, Josephus, Dead Sea Scrolls, and Pseudo-Philo) are indexed on pp. 317-319 of my book. Basser misunderstands and hence misrepresents the purpose of these comparisons and contrasts. For example: "Where Fraade does find earlier tradition [sic] akin to those in midrash, he labels them 'countertexts' which have no bearing on rabbinic materials other than in showing that rabbinic materials are different" (p. 87). Or: "Fraade is inclined to see such nonmidrashic texts as these as if they were countertexts which midrash debates" (p. 88, n. 14). In this he errs. By using the literary term "countertext," I merely mean to set such texts opposite the midrashic texts for the sake of morphological and ideological comparison, thereby highlighting their similar and dissimilar aspects. Never do I discount their bearing on our understanding of rabbinic texts or suggest that rabbinic texts are "debating" them.

consideration of which would have affected my interpretation of passages of the *Sifre*.¹⁸ Needless to say, I would have welcomed his bringing to our attention such texts as I might have missed.

The fundamental difference between Basser and myself rests not on which manuscript reading or second temple source I favored or did not favor. Rather, we differ sharply on the need to understand a particular rabbinic text in relation to the particular cultural and historical context in which it assumed its present overall form so as to first function as a rhetorical work of continuous commentary. This is not to deny either that the construction we call *Sifre Deuteronomy* draws upon a long and multi-branched history of tradition formation, transmission, and transformation, or that it contributed to the continuation of such a history in subsequent periods of Jewish cultural history into the present. Rather, my method rests on the need to recognize that these histories are different from one another, and while mutually illuminating and intersecting, need to be kept distinct. This is what Basser identifies as "a major weakness of this work," namely, "that it assumes as its working base that the culture of the rabbis was different from other cultures of Judaism" (p. 87). To this charge, but not to Basser's valuation of it, I once again gladly plead guilty.

To claim, as I do, that rabbinic commentary is different from its antecedents and to explain those differences in terms of its cultural practice within the workings of rabbinic society and its desire to influence Jewish society more broadly is not to isolate it from those antecedents (or, for that matter, from the contemporary "turn to commentary" in early Christianity)

¹⁸ In note 14, Basser proposes *TNaph* 3:1-5 as a worthier candidate for consideration than my choice of *1 Enoch* 2:1-5:7 (treated by me on pp. 153-154) for comparison with the sixth interpretation of *Sifre* 306. Firstly, I do include *TNaph* 3:2-4 in a comprehensive list of second temple parallels (p. 277, n. 126). Secondly, most scholars would agree that the text of *1 Enoch* is a more certain antecedent (not necessarily genetically) to the *Sifre* than is the *Testament of Naphthali*, since being part of the *Book of the Watchers* and being extant in Aramaic fragments from Qumran, it can with certainty be dated to second temple Jewish Palestine. By contrast, the *Testaments of the Twelve Patriarchs*, in their present form, are regarded by most scholars as being of later Christian provenance. While the extant *Testaments* may integrate or have developed from Jewish antecedents of similar literary form and traditional content, in the absence of such extant Jewish texts (with the exception of the Aramaic Levi fragments), one can only guess how those Jewish antecedents might have looked. Basser's claim that *TNaph* 3:1-5 "intentionally cites Enoch [sic] in its fullest form," and, therefore, is a better candidate for comparison with the *Sifre*, is asserted in the absence of evidence. Similarly, there is no reason to assume with Basser that *TNaph* 3:1-5 contains exegesis of Deut 32 per se that is "pre-rabbinic" and that it incorporates the same midrashic tradition as does the *Sifre*.

but to view them in mutual relief. By imposing upon my work (and upon us all) reductionist "all or nothing" alternatives, Basser on the one hand exaggerates my claims for early rabbinic commentary's formal and rhetorical distinctiveness from its antecedents, while on the other hand proposes as its preferred alternative the absence of any significant difference at all. As he states: "Many literary forms identified as rabbinic are found earlier than the rabbis [while many are not!], and their theological culture is essentially that of the biblical prophetic books" (p. 87). To the cultural historian such essentialist conclusions conceal more than they reveal.

Since Basser employs a pedagogic metaphor in evaluating my book (p. 90), I may be permitted to do the same with regard to his argument here. An undergraduate student in my introductory history of ancient Judaism is expected (in part): (1) to recognize the differences between the writings of the biblical prophets, an allegorical commentary of Philo, a Qumran *pesher*, the letters of Paul, and early rabbinic midrashic commentary; (2) to understand each in terms of its particular historical and cultural context; (3) to appreciate the shared nature of their respective enterprises of scriptural interpretation and the common threads of tradition upon which they draw, albeit in different ways and for different purposes. Those students who are able to navigate this dialectic of cultural continuity and transformation receive higher grades than those who are not.¹⁹

The next of Basser's criticisms is similarly unsubstantiated. Nowhere do I state or suggest that "the midrashic enterprise [is] the center of Rabbinism" (Basser, p. 88). What I argue and demonstrate is that statements concerning the ideology and practice of rabbinic Torah study and teaching within the *Sifre* need to be applied to the *Sifre*'s own practices of scriptural study, just as they would need to be applied to the other branches of the rabbinic study curriculum (e.g., mishnaic halakhah and its talmudic commentary). Basser's single example of my purported error in this regard is my treatment of a passage from the *Sifre* that I never even discuss.²⁰

¹⁹ Compare Basser's note 4 (p. 83), in which he argues that because *Memar/Tibat Marqah* (third-fourth century CE Samaritan) demonstrates "many connections" (similar exegetical traditions) with the *Sifre*, the latter "is not as rooted in rabbinic culture as Fraade thinks." But *Tibat Marqah* is also very different, both in exegetical form and content, from the *Sifre*; they are unlikely to be confused or conflated by any serious student of both. The one example that Basser gives, but without citation, is a weak one at that. While *Tibat Marqah* 4:80 (ed. Ben-Hayim, p. 285) like the *Sifre* (in one of several interpretations) connects Deut 32:10 with the giving of the Torah at Mt. Sinai, the forms and contents of their respective exegeses are very different. My study is not of the "roots" of the *Sifre*'s commentary, which are largely unrecoverable, but of its "fruits," and their rhetorical functioning within the broader culture of mid-third-century rabbinism, for which we have much, but hardly enough, evidence.

²⁰ On p. 88 of his review, Basser refers to my introductory discussion (pp. 18–19) of metaphors employed by the *Sifre* for the study by sages and their disciples of

Finally, Basser accuses me of over-interpretation in that "I find the rabbinic sage on center stage" of the *Sifre*'s commentary. Again, this is an overstatement of my argument. I simply argue that since the text as we have it is the product of rabbinic Judaism and since the rabbinic sages and their disciples would have been its most immediate consumers, it needs to be considered first and foremost in terms of its rhetorical role in the social and ideological formation of that group, without disclaiming the text's broader, yet more difficult to recover, origins and impact. In the example Basser considers (*Sifre* 343 [ed. Finkelstein, 399.16–400.6]), the "disciples of the sages" are indeed the climax of the interpretation. Once again Basser misunderstands and denies what I call the exegetical slippage within the flow of the commentary.²¹ But the structure of the passage, ignored by Basser, attests unmistakably otherwise. That structure is the same as employed in the previously considered passage comparing words of Torah to rain, except that now the comparison is to fire. Deut 33:2, as rabbinically understood, refers to the revelatory words of God at Mt. Sinai as a "fiery law" (מִשְׁנֵה). The commentary unpacks this metaphoric linkage of fire to revelation through a series of structurally repetitive comparisons: "Just as (כִּמְט) fire . . . , similarly (כִּמְט) words of Torah. . . ." ²² It is only in the final comparison that the disciples of the sages are mentioned, how structurally taking the place of the words of Torah: "Just as (כִּמְט) fire leaves a mark upon the body of whoever uses it, and people who labor with it are recognizable among other people, similarly (כִּמְט) the disciples of the sages are recognizable by their speech, by their manner of walking, and by their dress in the market."²³ Clearly, as Basser states, it is the words of Torah, like the fire, which cause those who labor with them to be marked and thereby recognizable. While the words of

"words of Torah," which, as I make clear, refers to both scripture and rabbinic teaching (e.g., mishnah). I then state that "these descriptions of rabbinic study can be taken to describe the 'production' of the text of commentary before us," but not exclusively so. In fact, on p. 164 I state that the same program of inquiry needs to be conducted with regard to the Mishnah and the two Talmuds. Basser next cites *Sifre* 306 (ed. Finkelstein, p. 336, lines 13–14) as though this were the passage upon which the argument of my introductory discussion is based, and proceeds to criticize my interpretation of this passage as referring to midrash rather than mishnah. This is indeed bizarre, since I never quote or comment upon this passage in my whole book. I do make reference to it in two notes (p. 187, n. 65, and p. 244, n. 112), with regard to the nature of rabbinic study, but without suggesting that such study is limited to biblical exegesis. For the sage as money changer, whose coinage is legal tradition, see my discussion of *Sifre* 13 (ed. Finkelstein, p. 22, lines 1–5) on pp. 101–102 of my book.

²¹ For Basser's attribution of pejorative force to the expression "slippage," see above, n. 16.

²² Depending on which manuscript one follows, there are four or five such comparisons.

²³ For a discussion of the manuscript variants, see p. 208, n. 95, of my book.

Torah are still implicitly being compared to fire, it is the disciples of the sages that have taken their place as that which the verse signifies according to the commentary's concluding unpacking of the fiery metaphor.²⁴ Nowhere do I say that *only* rabbinic sages are being addressed here, since certainly the passage suggests that people in general (דָּרָן) gain benefit from the words of Torah as from fire. But with the repetitive series of comparisons concluding chimacically with disciples of the sages structurally displacing words of Torah, the rhetorical effect is subtly to equate the two and privilege the former among those who engage in Torah study as recipients of divine revelation.²⁵

It should be clear by now that these differences between Bassler and myself reflect not simply differences in detail with respect to the execution of shared methodological models of inquiry, but fundamental differences regarding the use of rabbinic sources for historical purposes. Bassler tips his hand to this effect in the beginning of his review when he states that a document such as *Sifre* cannot be used to "illustrate rabbinic society of the second and third centuries" unless "the texts explicitly refer to this very society." It is not clear what Bassler means by "explicit reference." Most critical scholars would agree that the *Sifre* makes explicit references to named rabbinic sages and institutions that existed during that period. They are less likely to agree whether or to what extent those references can be taken to be representational in a simply mimetic, or nonrhetorical, manner. The less simply representational and the more complexly rhetorical we regard these texts, the more our need to apply critical models of historiography and textual practice, however much we might disagree as to which such models best apply and the extent to which they need to be adapted.

Such representational complexity need not justify, as it does for Bassler, an ahistorical view of the *Sifre* as a hermeneutically innocent window onto an amorphous mass of tradition unbound by documentary limitations and extending back in time through the varieties of second temple Judaism to the Hebrew Bible itself and forward in time through the successive reformations of rabbinic textual practice (and ideology) to the present. How else are we to understand his protestations that there is little difference be-

²⁴ To have been structurally consistent, the comparison would have been: "Just as fire leaves a mark upon the body of those who use it, and people who labor with it are recognizable among other people, similarly words of Torah leave a mark on those who use it, and people who labor with it are recognizable among other people." Compare the variants cited by me on p. 208, n. 95, of my book.

²⁵ Instead of engaging this argument from the unquestionable structure of the midrashic passage, Bassler presents his own explanation of the relation of the biblical lemma to its rabbinic exegesis, based on a supposed *notarikon* that requires the forced doubling of some letters and the removal of others. In any case, Bassler's explanation is irrelevant to my characterization of the commentary's slippage and his rejection of it.

tween the form of the *Sifre* and the writings of Paul or Philo, or between what the text of the *Sifre* might have meant to a rabbinic student in third century Palestine and to one reappropriating it in sixteenth century Italy? To Bassler, the texts of the *Sifre*, when viewed against this broad canvas, are manifestly simple, and in no need of critical methodologies and models, which he therefore eschews altogether.²⁶

The alternative that I propose and pursue is decidedly historical, but in a cultural-historical way. Rather than trying to extract and piece together from within the texts of the *Sifre* historical events to which it refers, or failing this to throw up one's historical hands, I seek to understand the fact of the *Sifre* itself as a cultural event of great (but not exaggerated) significance. I attempt to understand its particular shaping of received tradition as commentary first and foremost in relation to its most immediate cultural context: the emerging society of third-century rabbinic sages and their disciples in their program to become Israel's teachers, judges, and leaders. If to Bassler such a task is "theological" rather than "historical," then I am afraid we begin with very different understandings of the critical craft of the historian as compared to the constructive craft of the theologian.²⁷

Had Bassler's review presented coherently the argument of my book in order to take issue with it and to present another model of his own, with accurate and relevant supporting evidence, I would have welcomed it as an alternative effort to advance through debate our understanding of ancient Jewish culture and history. Since Bassler's review accomplishes none of these, it must be judged by normal standards of academic peer review to be a failure. But perhaps these were not Bassler's goals, and therefore it would be unfair to judge his review in terms of aims that he does not set for himself. Since I cannot guess what alternative motivations drove him to review my book in such an unprofessional manner, I must leave judgment of his success to readers of this journal in the hope that they will first be drawn by this exchange to become readers of my book.

²⁶ For example (p. 88): "Literary theory can be used to justify anything, even where meanings are blatantly simple. It can be used to obfuscate the plain and the clear." I leave it to readers to determine whether Bassler's readings are any plainer or clearer than mine. But his readings, like any act of reading, are no less predicated on theories or models of literature (and history) than mine; they are simply unacknowledged.

²⁷ I find Bassler's concluding comment, "Rather than producing a book which delineates historical process, he has given us a book which illustrates an approach to rabbinic theology" (p. 90), to be inscrutable in terms of any commonly-held understanding of these terms.