I submitted this response to *JBL*, but after almost 8 months the editorial board made a decision that my “response seems too focused on the Evans article to be of broad interest to our readership” and that my “differences might best be discussed personally … rather than published in an academic journal.” I maintain that I should have had an opportunity for a public response in the journal, since, in my opinion, Evans made gross misrepresentations of my work and that of others, even after I had had private interactions with him to correct his misrepresentations of my own work. In fact, in my opinion, the *JBL*’s peer review process failed both me and Evans, because his article never should have been published. Thus, I have decided to post my response here for anyone who might be interested.

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**TEXTS PERFORMED—NOT READ—BY ILLITERATE**

**SCRIBES? A RESPONSE TO EVANS**

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Abstract: In “Creating a New ‘Great Divide,’” Paul Evans critiques “recent applications of orality studies” to the Hebrew Bible, focusing primarily on the work of Susan Niditch, David Carr, and Raymond Person. Person demonstrates how Evans’ critique is full of misrepresentations because it is (1) based on only a few, earlier publications, thereby ignoring the majority of their recent publications on the topic, (2) is a misrepresentative reading of those few works to which he refers, and (3) is a selective reading of secondary sources from which they drew.

In “Creating a New ‘Great Divide’” Paul Evans critiques “recent applications of orality studies” to the Hebrew Bible, focusing primarily on my work and the work of Susan Niditch and David Carr. His critique, however, is full of misrepresentations based on a highly selective reading due to an insufficient engagement with the scholarly literature. His critique suffers from this in three different ways: (1) it is based on only a few, earlier publications, thereby ignoring the majority of our relevant publications on the topic; (2) it is a misrepresentative reading of

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those few works to which he refers, and (3) it is a selective reading of secondary sources from which we drew, especially in the comparative study of oral traditions. The title of my response “Texts Performed—Not Read—by Illiterate Scribes?” is a revision of one of Evans’ subtitles—“Texts to be Performed—Not Read”—with the addition of a term that he invented to describe my work (“illiterate scribes”). As such, I think it fairly represents his mischaracterization of our work or at least that of Carr and me, since he tends to agree more often with Niditch. Below I will show how Evans’ understanding of each term in this title—that is, “text,” “performed,” “not read,” and “illiterate scribe”—is selective and misleading, so that their combination is even more problematic. In fact, I know of no scholar of ancient literature—whether in biblical studies, ancient Near Eastern literature, or classics—whose work could be fairly represented by Evans’ characterization.3

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Text

Too often Evans uses “text” in a generic sense, which results in a misrepresentation of the argument he is summarizing. For example, he summarized Carr’s thesis as follows: “unlike modern texts, ancient texts were *not* designed actually to be read.”⁴ Carr’s conclusions did not concern ancient texts in general, but a specific genre of ancient texts (“written copies of long-duration texts like the Bible, Gilgamesh, or Homer’s works”); however, nowhere does Evans acknowledge this, thereby possibly giving the wrong impression that Carr is referring to all ancient texts.⁵ John Miles Foley has insisted that the comparative approach must include explicit discussions of genre, because some features of oral traditional literature follow the principle of genre-dependence.⁶ Although Carr does not refer to Foley’s principle of genre-dependence, he certainly follows it when his conclusions are directed not to all ancient texts, but to a particular genre or class of texts. Evans’ critique misses this important nuance.

Performed

Although there are some places where Evans’ description of our understanding of “performance” is accurate, his summary nevertheless includes the following generalization that misrepresents our work: “By *performance* scholars seem to mean something like ancient oral delivery (rather than the reading of these texts) with texts being recited from memory and presuming interaction with their audiences.”⁷ Such a conclusion may apply to some scholars, but not to Niditch, Carr, or me. However, this quote is followed soon thereafter with a quote from my 1998 article as representative of this problem. Although I do not reject that some literary texts may have been recited on some occasions, my understanding of performance is much broader. That is, scribal performance does not apply to only one scribal function, but applies to how scribes perform the various tasks that they undertake, including copying from physical manuscripts to produce a new copy, reciting texts or reading them aloud in various scribal

⁴ Ibid, 753. Emphasis his.
⁵ Carr, *Writing on the Tablet of the Heart*, 5.
contexts (whether within the guild itself or in public), and instruction based on texts within the guild or in public (including, when necessary, translation). This position is found throughout my work, both in references to secondary literature and by phrases like “the literate activity of copying texts,” “recited aloud,” “read aloud and used as the basis of the oral instruction of the people,” “the process of copying,” “reciting a written text,” and “reading historiographical texts as oral performance.”

A broader understanding of scribal performance is consistent with studies of other ancient and medieval literatures, including Old English and Homer, as well as with the process of editing oral traditional texts by some of those who recorded oral texts in their fieldwork.

Not Read

Here I return to Evans’ summary of Carr’s thesis: “not designed actually to be read.” It is unclear what Evans means by “read” here, but his characterization of Carr’s work is certainly based on a misreading: “ancient readers must have already memorized these texts in order to read them.” This is clearly an exaggeration of Carr’s argument and Evans should have been aware of this, based strictly on what he himself quotes from Carr in the following footnote: “Carr writes, ‘The visual presentation of such texts presupposed that the reader already knew the given text and had probably memorized it to some extent.’ Carr allows that ‘some masters of the tradition could sight-read such texts,’ but most could not.” Note that Carr’s “such texts” is not to all ancient texts, so that Evans’ misreading of Carr on “texts” (see above) may have contributed to his misunderstanding here. To set Evans’ footnote in a broader context I provide

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10 Evans, “Creating a New ‘Great Divide,’” 753.
11 Ibid., 753.
12 Ibid., 753 n. 20. Quoting Carr, Writing on the Tablet of the Heart, 5, 4.
the following quote from Carr: “[S]uch written copies were a subsidiary part of a much broader literate matrix, where the focus was as much or more on the transmission of texts from mind to mind as on transmission of texts in written form. Both writing and oral performance fed into the process of indoctrination/education/enculturation.”

Evans should have paid more attention both to the phrase “to some extent” in his first quote of Carr and to what he seems to understand as an exception to whatever rule he imagines Carr is creating about “not read.” That is, the master scribes knew (to a greater extent) the tradition recorded in the written texts from their past interactions with recitations and reading the texts so well that they participated in the education of other scribes who were learning to internalize the tradition as they interacted with texts that they may have been (to some extent) mostly unfamiliar with. Neither Carr nor anyone else to my knowledge concludes that these texts were “not read.” Of course, they were read, but the important questions are “How were they read?” and “If that form of reading differs from our own, how does it differ?” Evans sets up a false dichotomy between “performed” (in his understanding meaning “recited” by memory) and “read” (presumably sight-reading?) that misconstrues Carr’s argument that refers to “both writing and oral performance” and, by implication, reading and hearing.

**Illiterate Scribes**

Evans coined the phrase “illiterate scribe” as his characterization of my discussion of how the mind-set of scribes in a primarily oral society, including in ancient Israel, differs from the mind-set of modern writers in ways that are analogous to performers of oral traditions (ancient or modern). He focuses in on my use of the phrase “oral mind-set” to the exclusion of other phrases. Before discussing in more detail how I think Evans misses the nuance of my description, I first want to admit my difficulty with the limitations of our language to describe something that is counter-intuitive to our modern ways of thinking. One of the most difficult

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14 Evans, “Creating a New ‘Great Divide,’” 753, 757, 759. “Illiterate” appears nowhere in Person, “Ancient Israelite Scribe as Performer” or Person, *Deuteronomic School* and is found in Person, *Deuteronomic History and the Book of Chronicles* only once in the phrase “illiterate bards” as performers of oral traditions (40).
things I have faced in working on issues of ancient media is using language that necessarily implies a great divide between oral and written discourse and between oral and literate cultures. Therefore, I will also admit that my use of “oral mind-set,” “literate mind-set,” and “oral mentality” is somewhat problematic in that these phrases fall much too easily into the kind of dichotomous thinking we all need to avoid. However, having said all of that, I nevertheless think that Evans’ emphasis on my use of “oral mind-set” is somewhat misleading. First, he does not acknowledge that these phrases are lacking in my later work, including those to which he had access, because I later understood them as problematic. Second, he overlooks other phrases in my work that he explicitly states in his article are better than “oral mind-set.” He wrote the following in his conclusion falsely implying a significant contrast between my work, on the one hand, and that of Niditch, on the other: “The work of Niditch has shown that oral characteristics vary with different types of biblical texts that are located at different places or stages on the orality-literacy continuum. In my judgment, her approach is by and large compatible with the present study…” Niditch divided her monograph into chapters related to her idea of an “oral-literate continuum” and “the interplay between orality and literacy,” so that she could conclude the following: “Thus, even at the literate end of the continuum, the oral mentality is present and active, informing the way writing is used. Exploring the interplay between orality and literacy is

15 The phrase “oral mind-set” occurs in the following: Person, “Ancient Israelite Scribe as Performer,” 602, 603, 605; Person, Deuteronomic School, 88, 89, 93; and Person, Deuteronomic History and the Book of Chronicles, 47, 65. The phrase “oral mentality” occurs in the following: Person, “Ancient Israelite Scribes as Performer,” 604, 607 (3x), 608 (3x), 609 (3x) and Person, Deuteronomic School, 91, 94, 94-95, 95, 96 (2x), 97 (3x). The phrase “literate mind-set” occurs in the following: Person, “Ancient Israelite Scribes as Performer,” 602; Person, Deuteronomic School, 88; Person, Deuteronomic History and the Book of Chronicles, 47. As Evans notes (“Creating a New ‘Great Divide,’” 757 n. 39), “Ancient Israelite Scribes as Performer” was revised and included as sections of Deuteronomic School (83-97) and Deuteronomic History and the Book of Chronicles (43-51). Note the following: (1) in my later monographs, these phrases occur only in the sections that are revisions of the 1998 article with only one exception (Deuteronomic History and the Book of Chronicles, 65) and (2) “oral mentality” was excised completely from Deuteronomic History and the Book of Chronicles.

16 When I e-mailed him pre-publication proofs of Person, “Text Criticism as a Lens” and Person, “Biblical Historiography as Traditional History” on November 30, 2014, I did so specifically to demonstrate to him that I was no longer using “oral mind-set” in my work. This was directly in response to my hearing the following: Paul S. Evans, “The Illiterate Scribes: Assessing the Thought Processes of the Chronicler in Light of Modern Fieldwork with Illiterate and Literate Subjects,” Society of Biblical Literature annual meeting, San Diego, 2014. In a private conversation following his paper, I also stated that I thought “illiterate scribe” was misleading.

17 Evans, “Creating a New ‘Great Divide,’” 764.
essential to understanding the social contexts of reading and writing in a traditional culture.”

Evans does not acknowledge that I use phrases drawn from Niditch’s work in my own work. In “Ancient Israelite Scribes as Performers” I used the following: “evidence of an oral mentality and a literate mentality in the copying of the text,” “an oral-literate continuum,” and “interaction of orality and literacy.” Similar phrases occur in The Deuteronomy History and the Book of Chronicles: “oral-literate continuum,” “interplay between the oral and the written,” “an interplay between the oral tradition and the written texts that represent the oral tradition,” “an interplay of texts and communal memory,” and “a constant interplay between literary texts and the broader oral tradition preserved in the community’s memory.” My most recent publication, the introduction in The Dictionary of the Bible and Ancient Media co-authored with New Testament scholar Chris Keith, cautions readers against possible pitfalls in the volume, including, for example, separate entries on “orality” and “literacy,” emphasizing rather “the intricate interplay of orality and literacy as two sometimes competing and sometimes complementary modes of expression both of which served as tools to facilitate personal and collective memory.” Thus, the gulf he strives to create between my work and that of Niditch is clearly a misrepresentation based on a highly selective reading.

What may be seen as Evans’ identification of what he considers to be the most critical flaw in my work is my dependence upon Foley’s citation of an interview of a Serbo-Croatian guslar from Parry and Lord’s fieldwork, which he described as “extremely limited example and such outdated research.” Evans understands somewhat correctly that the example shows that, in his words, “one musician/poet [guslar-ist] has considered a ‘word’ to be not literally a word and

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18 Niditch, Oral World and Written Word, 98.
20 Person, Deuteronomy History and the Book of Chronicles, 46, 47; 21, 57, 58, 61, 63, 65, 81, 161, 170; 152; 160; 170.
22 In fact, had Evans read some of Niditch’s more recent work, he would have realized that she often refers to my work in a favorable way. For example, see Niditch, “Hebrew Bible and Oral Literature,” 8; Niditch, Judges, 34.
that a change in a sentence really was ‘no change.’”24 However, he must have missed that I provided this example to illustrate a “general observation” based on comparative “[s]tudies in oral traditions of various cultures.”25 Had he been more familiar with the secondary literature in the comparative study of oral traditions, especially the work of Foley, he would have known that this is not in fact an “extremely limited example” based on “outdated research.” In fact, this very example continues to be used in recent scholarship to illustrate how the units of meaning (“words”) in oral traditions and literature with roots in oral tradition differ from simply lexemes. Let me provide two especially recent examples that will demonstrate his error and support my contention that this is an illustration of a general observation. First, in his 2018 monograph, *The Homeric Simile in Comparative Perspectives: Oral Traditions from Saudi Arabia to Indonesia*, Jonathan Ready refers to similar interviews with the *guslars* citing Foley and then provides a list of twenty-one other folklorists who have gathered similar statements from their interviews with informants of oral traditions throughout the world.26 Second, I am contributing a chapter to a forthcoming volume entitled *Formula: Units of Speech, ‘Words’ of Verbal Art* co-edited by the folklorist Frog and the linguist William Lamb. In the preface, the editors’ description of the volume’s title includes the following:27

‘Words’ of verbal art refers to John Miles Foley’s use of ‘word’ in quotation marks as the vernacular term for ‘word’ as used by oral poets to refer to a unit of utterance that conveys a traditional unit of meaning. Accordingly, a formula is a ‘word’ of the poetic register describable ‘as an integer of traditional meaning,’ corresponding to a morpheme-equivalent unit.

24 Ibid., 758 n. 45. Emphasis his.
Thus, what I referred to as a “general observation” made by Foley defines the topic of this edited volume and many of the contributors refer to Foley’s insights in their discussion of various oral traditions and literature with roots in oral traditions.

After characterizing my example in such negative terms, Evans attempts to demonstrate how “outdated” my work is. Apparently unaware of the irony, he begins by quoting from Lord’s 1960 monograph The Singer of Tales, in which he rejected the idea of “transitional literature.” If Evans had been more familiar with Lord’s work, he would have known that Lord recanted this position in his later work: “In The Singer of Tales I had argued against the existence of ‘transitional texts,’ a concept that constantly haunted us. That ghost has, for the moment at least, been laid to rest. There seem to be texts that can be called either transitional or belonging to the first stage of written literature.” That is, the comparative study of oral traditions and literature with roots in oral tradition became more sophisticated based on the expansion of comparative oral literature beyond the evidence that Parry and Lord first uncovered in their fieldwork with the guslari of the former Yugoslavia; therefore, Lord acknowledged that some of the early conclusions that they had reached needed to be changed away from positions that were too much influenced by the “Great Divide” of orality versus literacy. That is, Lord understood more and more about the importance of what Niditch, a student of Lord who draws significantly from the Parry-Lord school, characterized as an “oral-literate continuum” and “the interplay of between orality and literacy.” Thus, as a student of Foley who draws significantly from the Parry-Lord school, including Niditch’s application to the Hebrew Bible, I stand by my understanding of scribes as performers as neither “an oxymoron” nor “an impossibility” “according to the Parry-Lord school,” as Evans erroneously asserted.

Conclusion

30 Evans, “Creating a New ‘Great Divide,’” 759.
Although I have not attempted to correct all of his misrepresentations, I hope that what I have discussed above has illustrated that Evans’ characterization of my work and that of my colleagues, Susan Niditch and David Carr, is highly selective in ways that is a gross misrepresentation of our work. I recognize that there are subtle differences between the three of us, so that sometimes we disagree with each other, but Evans missed that subtlety. I acknowledge that my use of language to describe counter-intuitive concepts has evolved over time, so that some of my earlier ways of describing the interplay between oral and written may have contributed to some misunderstandings. I am fully aware that my work in media studies (and that of others, including Niditch and Carr) directly challenges the standard historical-critical methodology that has undergirded biblical scholarship for a long time. I, therefore, am not surprised that some colleagues may be dismissive of my work and that some others may disagree vehemently with it. I welcome criticism and disagreement, because that is often what helps move the field forward. However, misrepresentations as made in Evans’ article do not bring clarity to the much-needed discussions concerning our presuppositions and methodologies, but rather have the potential to muddy the waters unnecessarily. Hopefully, my response will help return some clarity to the ongoing discussion, at least in its correction of Evans’ misrepresentations.

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