



On Comparisons with Ancient Greek Traditions: Lessons from the Mid-Century

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Résumé de l'article

In the twentieth century, at two crucial instances, comparisons with ancient Greek traditions played an important role in forwarding the argument in favor of the early origins of the pentateuchal narrative: Martin Noth's "amphictyonic hypothesis" and Frank Moore Cross's account of the Israelite "epic." This article argues that success of these arguments owes to a hidden "parallelomania" at work in the mid-century, which successfully made the evidence of ancient Greek phenomena seem like something that it was not in service to desirable arguments. Finally, it explores how contemporary approaches can avoid repeating past mistakes.

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ANDREW TOBOLOWSKY

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ON COMPARISONS WITH ANCIENT GREEK TRADITIONS: LESSONS FROM THE MID-CENTURY

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This article has two purposes. First, I will identify a heretofore unrecognized pattern in the mid-twentieth century use of comparative evidence from ancient Greece—in the two famous mid-twentieth century instances in which it *was* used, most Martin Noth’s “amphictyonic hypothesis” and Frank Moore Cross’s account of an early, oral epic behind the Pentateuch. Second, I will explore the ramifications of this pattern for the contemporary use of comparisons drawn from ancient Greek traditions generally. I consider both important topics—we ought to understand what came before as well as possible—and the first will take up by far the most space because of the technical nature of the issues involved. At the same time, the second is why this inquiry remains necessary, given two arguments that are no longer current. These are cautionary tales, but we cannot heed their cautions until we properly understand what they are.

What I am interested in, in particular, is what in other contexts has already been called “parallelomania.” This is a term that was originally popularized by Samuel Sandmel in a presidential address to the Society of Biblical Literature. Here, he referred to a certain over-eagerness to see meaningful parallels in “rabbinic literature and the gospels, Philo and Paul, and the Dead Sea Scrolls and the NT,” in service to arguments about how one influenced another.¹ Since then, it has indeed been used, from time to time, to refer generally to instances in which a scholar sees parallels that are not really there in comparative materials in order to make a particular kind of argument.² Personally, I have

¹ “We might for our purposes define parallelomania as that extravagance among scholars which first overdoes the supposed similarity in passages and then proceeds to describe source and derivation as if implying literary connection flowing in an inevitable or predetermined direction” Samuel Sandmel, “Parallelomania,” *JBL* 81 (1962): 1.

² E.g. Lutz Doering, “Parallels without ‘Parallelomania’: Methodological Reflections on Comparative Analysis of Halakhah in the Dead Sea Scrolls,” in *Rabbinic Perspectives: Rabbinic Literature and the Dead Sea Scrolls*, ed. Steven D. Fraade, Aharon Shemesh, and Ruth A. Clements (Leiden: Brill, 2006), 13–42; Yisra’el Ya’akov Yuval, “Christianity in Talmud and Midrash: Parallelomania or Parallelophobia?” *Transforming Relations: Essays on Jews and Christians throughout History in Honor of Michael*

most often heard it used to describe the early-to-mid twentieth century tendency to see dramatic confirmation of pentateuchal episodes in Near Eastern materials especially associated with the Albright school in America. In any case, parallelomanias, I will attempt to show, represent the working out of a kind of mathematical formula: desire + obscurity = parallelomania. The element of desire comes from what the scholar already wishes, even quite unconsciously, to demonstrate, or to have demonstrated. As for the obscurity, it need not be a very great obscurity. But an obscurity *relative* to more cognate materials, from the perspective of typically trained Hebrew Bible scholars, is often enough to make room for desire to transform what is to what it would be useful for it to be in order to make one argument or another viable.

In Noth and Cross's cases, I will argue that the operative desire was for hard proofs that the pentateuchal narrative emerged in its essentials rather early on and had at least some relationship to historical realities—even if the nature of this relationship was perceived differently on opposite sides of the Atlantic. A great many scholars had been making this case since, more or less, the moment the skeptical view—associated with Julius Wellhausen—had emerged, and more and more frequently since the turn of the twentieth century. These studies offered the hope of establishing these positions with new strength through new evidence, Cross's directly and Noth's through his *Grundlage*, or “common basis” of the pentateuchal source documents, whose formation he associated with the amphictyony, and through his belief that the twelve tribes tradition must be based on early historical realities.³ I will show that the presence of these desires and their operation in these reconstructions is specifically what explains otherwise unexplained, even uninvestigated, aspects of these arguments—and most especially their odd combination of fundamental flaws and extraordinary success.

Finally, I will suggest that what we can learn from the operation of this mid-century parallelomania begins with the fact that obscurity remains and other desires exist. In other words, ancient Greek comparanda are likely to remain obscure, relative to the study of Near Eastern and Levantine comparisons, for a long time to come and that produces a perilous situation for the operation of desire. And what I will argue is that we can avoid many of the pitfalls of inapt comparison here by focusing less on the potential similarities between ancient *texts* in different contexts and more on shared scholarly *problems* and questions. In

A. Signer, ed. by John Van Hengen and Franklin T. Harkins (Notre Dame: University of Notre Dame Press, 2010), 50–74; Reimund Bieringer, “Parallelomania Revisited: Critical Reflections on the Use and Abuse of Parallels in Biblical Studies,” paper presented at the *European Association of Biblical Studies Annual Meeting, 30.07-02.08.2013, Leipzig*, 2013; Robert Brody, “Irano-Talmudica: The New Parallelomania?,” *JQR* 106 (2016): 209–32.

³ For an account of the *Grundlage*, see generally Martin Noth, *A History of the Pentateuchal Traditions*, trans. Bernhard W. Anderson (Chico, CA: Scholars Press, 1981), 38–45.

other words, a safer, but in my opinion, very productive approach to ancient Greek comparanda would not begin with attempting to discover identical genres or institutions, but identical issues faced by scholars in studying them, allowing us to draw useful suggestions from each other's approaches.

MARTIN NOTH

The situation that I will describe in the next two sections, which deal with the unrecognized aspects of Noth's and Cross's arguments that concern us, is much like the one Collin Cornell has recently identified in the study of the Elephantine papyri – a tendency to use them as a “pseudo-Bible or Bible-look-alike” which gives scholars “greater ‘room to maneuver’” for “editorializing” of particular sorts.⁴ As we will see, however, the ancient Greek materials did not function as a pseudo-Bible but instead as what we might call a “more malleable other.” That is, the world of ancient Greece often presents similar traditions, from equally long ago and not very far away. And so, to Noth, Cross, and their enthusiastic audiences, it seemed reasonable to imagine that what was true *in* or *about* this other corpus might also be true in or about the corpus of ancient Israel and Judah. And this imagination could take on a life of its own, operating on materials that were less familiar and, frankly, less closely scrutinized even by those who were using them.

In Noth's case, as is well known, this meant imagining that an institution that was to be found in ancient Greece was also to be found in ancient Israel—the amphictyony. In fact, Noth had argued, beginning with 1930's *Das System der zwölf Stämme Israels*, that Israel had *originated* as an amphictyonic league.⁵ This conclusion was partially the result of the influence of Albrecht Alt's “peaceful infiltration” theory—just as, as we will see, Cross's was an evolution of an earlier argument made by his teacher and mentor, William Foxwell Albright.⁶ In addition, both Noth and Alt were influenced, as Megan Bishop Moore notes, by Max Weber's vision of early Israel as “a covenant community” of “semimads or transhumants.”⁷ And, Noth certainly drew inspiration from the simple fact that the biblical narrative itself so consistently describes the twelve tribes of Israel as the fundamental

⁴ Collin Cornell, “Elephantine Trespasses: Theological Arguments in Recent Religion-History,” *Religion & Theology* 28 (2021): 4.

⁵ The bulk of his discussion of the comparative evidence appears in Martin Noth, *Das System der zwölf Stämme Israels* (Stuttgart: Kohlhammer, 1930), 39–60. As he would later put it, “Israel was constituted as a historical entity in the form of an amphictyonic twelve-tribe association; this fact was of basic significance for the whole subsequent course of its history” (Martin Noth, *The History of Israel*, 2nd ed. [New York and Evanston: Harper & Row, 1960], 96).

⁶ Noth had added the crucial point that the elements which “infiltrated” Israel at this time were not, in fact, Israel yet. They would become so only on “the soil of the arable land of Palestine” and in amphictyonic form Noth, *Pentateuchal Traditions*, 43.

⁷ Megan Bishop Moore, *Philosophy and Practice in Writing a History of Ancient Israel* (New York, NY: T & T Clark, 2006), 59–60.

expression of what Israel *was*.⁸ On one level, he was simply trying to square this narrative fact with his well-known skepticism about the representative accuracy of much of the Pentateuch and Joshua.

On the practical level, however, nothing about the diverse roots of Noth's case changes the three central facts about how he constructed it: 1) that there is virtually no biblical evidence that Israel ever acted in an amphictyonic fashion, 2) that Noth's view of what amphictyonies are and how they operate was therefore built almost exclusively from his understanding of the ancient Greek evidence; and 3) that he dramatically mischaracterized this evidence in a way that could easily have been obvious from the beginning. Some of this is visible even from the basic premise—"amphictyony" is a Greek word, and one that only appears in Greek sources. It was already an innovation—one for which Noth offers little evidentiary support—to argue that "amphictyony" can instead be understood as a "technical term" for any association of tribes dedicated to the "upkeep of the common shrine and its worship" which, in Israel's case, was supposed to be at Shechem.⁹ But few, if any, scholars seem to have noticed just *how* little Noth had, on the biblical side, for quite some time.

It is, therefore, still worth saying that almost the entirety of Noth's textual argument rests on a singular reference to a tribal assembly at Shechem, in Joshua 24, without any clearly amphictyonic performances, and a few other rather unprepossessing references connecting the tribes only to the *vicinity* of Shechem.¹⁰ Beyond that—as he readily admits!—biblical texts "say very little

⁸ "It is impossible to explain either the choice of these particular twelve names or the fact that the system has come down to us [in different forms] . . . if the whole system is attributed to a more or less arbitrary compilation. Obviously definitely, quite concrete historical pre-suppositions are involved." Noth, *History of Israel*, 87.

⁹ Noth, *History of Israel*, 88, 91–92. He says little more than that the number twelve is "customary in tribal societies which were still settled political institutions" and that even though we learn little about these in the Hebrew Bible, "[t]he fact that similar associations of twelve tribes existed in ancient Greece and Italy is more helpful" Noth, *History of Israel*, 87–88.

¹⁰ Noth, *History of Israel*, 92. These include "statements" in Deut 11:29, 27:1–26, Josh 8:30–35 "according to which after their occupation of the land, the tribes of Israel set stones and an altar near Shechem" and references in Gen 33:20, Jos 8:30, and Josh 24 itself (2, 23) in which "Yahweh appears to have been worshipped as the 'God of Israel' at this very shrine near Shechem" (Noth, *History of Israel*, 92). In all but one of these cases—Gen 33:20—Shechem itself is not mentioned, but instead the altar on Mt. Ebal. The Joshua 24 passages likewise refer only to instances in which Joshua exhorts the people to worship YHWH, with no additional description of place or league. Gen 33:19–20 describes Jacob's purchase of Shechem and construction of an altar there. But, in none of these cases is there any evidence of an institution actually headquartered at or near Shechem, nor are any of these stories terribly distinct from other accounts of altar construction scattered throughout the text (Noth, *History of Israel*, 88).

about the purpose of these organizations,” and “[t]he Old Testament tradition provides us with almost no direct information about the life and function of the Israelite twelve-tribe association.”¹¹ He knew, too, “that there is no evidence” that Shechem was an amphictyonic headquarters.¹² He argued that neither of these things was “surprising” because “it is not usual to record and transmit much about institutions which function in an orderly way and are therefore not particularly noteworthy” and because “in the period of which we have direct historical records, the central shrine... had already been shifted from Shechem.”¹³ But even if so, that leaves him with virtually no biblical proof—and he does not explain why we would then have even the *Greek* accounts of amphictyonies and their operation to work with to flesh out this picture. They, presumably, were functioning in an orderly way, too.

What we see here is just how completely Noth’s vision of Israel’s amphictyony actually depends on simply importing his understanding of Greek amphictyonies into a context where there was no good (internal) reason to imagine the institution existed. And this is where we reach the shores of the parallelomania I am describing—because the Greek amphictyonies he so relies on *do not operate in the way he imagines either*. Here, Noth staked his claim largely on the fact that there were twelve tribes of Israel and that twelve, or six, appear to be typical numbers for amphictyonies as well, including of the best known, the Delphic.¹⁴ Noth imagined that this number must be not just typical but *essential*, and *functional* for amphictyonies – which, incidentally, led him to suppose that there were quite a few other amphictyonies in the Levant besides Israel.¹⁵ Specifically, he believed that these numbers were the key to allowing amphictyonic organizations to serve their sanctuaries in an orderly “monthly or bimonthly *rota*.”¹⁶ Thus, even without internal evidence, it might indeed be reasonable to think that the number twelve was by itself suggestive of a similar organization and attempt to reconstruct it as far as possible.

¹¹ Noth, *History of Israel*, 88, 97. This, in his view, was precisely why “[t]he fact that similar associations of twelve tribes existed in ancient Greece and Italy is more helpful” (Noth, *History of Israel*, 88).

¹² Noth, *History of Israel*, 97.

¹³ Noth, *History of Israel*, 97, 93.

¹⁴ It has been suggested to me (by an anonymous reviewer) that Noth’s typically classics-infused gymnasium education brought him into contact with primary sources in a way that provided an enduring influence.

¹⁵ There were “quite practical reasons for the fixed and constantly maintained number twelve (or six)” (Noth, *The History of Israel*, 88). Thus, “[t]he number twelve proves to be a historic factor shedding light on the origin and significance of the tribal system as a whole” – and not just for Israel but “for lists of twelve tribes – occasionally of six tribes also” briefly mentioned here and there in Hebrew Bible, including “twelve Aramean tribes . . . twelve Ishmaelite . . . twelve Edomite . . . six Horite” (Noth, *History of Israel*, 87).

¹⁶ Noth, *History of Israel*, 88.

The problem is that this is simply not true—and the fact that it is not true is visible in easily accessible sources such as Strabo.¹⁷ Amphictyonies can have a wide variety of numbers of members, with no clear preference for six or twelve. The Kaurian amphictyony Strabo mentions has seven, the Boeotian had different numbers at different times, the Achaean “began with two members,” while the Lykian league had twenty-three.¹⁸ The Dorian Pentapolis had, of course, five, and the Aeolian league eleven.¹⁹ In addition, as subsequent studies would show, there was simply no evidence of Noth’s “orderly rota” of holy site service. Instead, the number of amphictyons turns out to be “entirely incidental . . . to the practical functioning of the Amphictyons,” and “known amphictyonic functions could have been performed equally conveniently by an assembly of any arbitrary number.”²⁰ And so, there is no biblical evidence for Noth’s claims – but there is no classical support either.

Or we might put it another way. Noth’s reconstruction of Israel’s origins relies almost completely on what he *imagined* the Greek evidence to say—not what it did say, nor what the biblical evidence said. That he was able to build an argument this way is part of what should interest us. After all, there is nothing that appears complicated about the available counter-arguments. There is, however, a still greater mystery, the unplumbed mystery that requires the solution I have suggested here: why his argument was so popular, beyond him, given how little it seems like it would take to refute it. And it was very, *very* popular. C.H.J. de Geus makes the claim, accurate in my opinion, that it “almost completely dominated all concepts of earliest Israel” for “more than three decades”—from the publication of *Das System* into the 1960s and ‘70s.²¹ During that time, it was additionally almost unique for the currency it enjoyed on both sides of the Atlantic. William Foxwell Albright himself described it as “standard.”²² David Noel Freedman treated it much the same way.²³ In addition, for that entire period, it was scarcely criticized at all, and

¹⁷ Strabo 8.374. As for accounts in classical scholarship of odd-numbered amphictyonies that would have been available to Noth, see G. W. Botsford, “The Constitution and Politics of the Boeotian League,” *Political Science Quarterly* 25.(1910): 274; J. Penrose Harland, “The Kaurian Amphictyony,” *AJA* 29 (1925): 161.

¹⁸ Bruce Donald Rahtjen, “Philistine and Hebrew Amphictyonies,” *JNES* 24. (1965): 103–4.

¹⁹ N.P. Lemche, “The Greek ‘Amphictyony’ - Could It Be a Prototype for the Israelite Society in the Period of the Judges?,” *JSOT* 2 (1977): 57.

²⁰ G. R. H. Wright, “Shechem and League Shrines,” *VT* 21(1971): 585.

²¹ C.H.J. De Geus, *The Tribes of Israel*, *Studia Semitica Neerlandica* 18 (Assen: Van Gorcum, 1976), 40. U

²² William F. Albright, *Archaeology and the Religion of Israel* (Louisville, KY; London: Westminster John Knox Press, 2006), 102.

²³ David Noel Freedman, “Pentateuch,” in *The Interpreter’s Dictionary of the Bible*, by E. Dinkler, ed. George Arthur Buttrick, vol. 3 of K-Q (Nashville, TN: Abingdon, 1962), 714.

even more rarely on the grounds of its use of the comparative evidence it so relied on.²⁴

Here, it is perhaps useful to offer a reminder. My purpose is not to re-make the point that Noth's argument *is* flawed. Neither, though it may seem otherwise, is it to insult the contributions of two scholars who made a great many that have proven enduring. It is, instead, simply to point out that the way he made his argument, the combination of its success, and its extraordinary lack of evidentiary support, constitute a phenomenon that requires an explanation it has never received—even though we no longer need to ask whether the argument itself is valid. Indeed, it is not in opposition to the identification of a parallelomania at work here, but in support of it, that the argument fell apart just as soon as scholars seriously revisited the evidentiary basis for Noth's reconstruction. This did, however, take de Geus's 30 years.²⁵ It actually seems as if the importance of the argument, and the influence it exerted for such a long period of time, should quite naturally have resulted in a level of scrutiny at least sufficient to ask the question of whether the Greek evidence supports the claim that amphictyonies more or less always have six or twelve members, and more or less always for functional reasons. In other words, we might expect scrutiny to be commensurate with the important role the argument played. For quite some time, the opposite was true, and we can—and should—still ask why.

What I suggest, of course, in answer to the question is parallelomania. Noth saw a parallel between the Israelite tribes and amphictyonies, like the Delphic, that had twelve members because that, on some level, was what he wanted to see—and it

²⁴ De Geus, too, notes that “it remains strange that a hypothesis so radically affecting the reconstruction of Israel's earliest history was so little criticized” (De Geus, *Tribes of Israel*, 54). There were a few early critics – Otto Eissfeldt, for example – but Eissfeldt's objections were about ancient Israelite history, including the fact that the book of Judges – which described the presumed era of the amphictyony – offers perhaps the least evidence for clear tribal organization. Otto Eissfeldt, “Israel und seine Geschichte,” *TLZ* 76 (1951): 335–40. G. Fohrer made a similar point (see Fohrer, “Altes Testament: ‘Amphiktyonie’ und ‘Bund?’” *TLZ* 91 [1966]: 804). Elias Auerbach was closer to the mark, though ignoring the issues raised here, when he pointed out that even in ancient Greece, amphictyonies did not serve as the setting for ethnogenesis. Elias Auerbach, *Wüste und Gelobtes Land I* (Berlin, Germany: Schocken, 1936), 72. See the discussion in De Geus, *Tribes of Israel*, 55–61.

²⁵ Lemche's statement, in 1977, that “no Old Testament scholar has undertaken a more precise survey of the material from the Greco-Roman world, drawn into the light by Martin Noth” is less apt than Rathjen's in 1965 that “no serious investigation of the validity of the designation [amphictyony] has been forthcoming.” But, Lemche is not *far* wrong and Rathjen is quite right (Lemche, “The Greek ‘Amphictyony,’” 52; Rathjen, “Philistine and Hebrew Amphictyonies,” 100). See also Frank Crüsemann, *Der Widerstand gegen das Königtum: d. antikönigl. Texte d. Alten Testaments u. d. Kampf um d. frühen israelit. Staat* (Neukirchen-Vluyn: Neukirchener Verlag, 1978).

became proof that the twelve tribes tradition was based on an early historical reality. His larger scholarly audience felt just the same way. And well they might, because Noth's reconstruction had more to offer than this vision of a historical memory of sorts behind the biblical account of Israel's tribes.

That is, Noth linked the origins of Israel explicitly with the origins of the Pentateuchal narrative itself. Indeed, he argued that what he called the *Grundlage*, or "common basis" of the Documentary sources had formed at more or less the same time as the amphictyony, and in more or less the same way.²⁶ He had a Romantic view of the process—that this narrative "had no particular 'author' or even 'authors,' but rather emerged, developed, and was transmitted through the mouths of 'narrators' within the anonymous totality of the tribes."²⁷ Here, as a kind of aside, we might note that something of the nature of how he did, and more importantly, did *not* engage with ancient Greek texts can be seen in the fact that the so-prominent classicist, Martin Nilsson, already knew that there was "something of mysticism" about the "romantic idea of a popular collective poetry which cannot be ascribed to the interference of any individual but which grows up unconsciously as the product of the collective mind of the people" more than a decade before Noth wrote these words, around the same time as *Das System* itself.²⁸ But the more important point for us is that the appeal of Noth's argument, which again extended across the Atlantic, likely came from this—that his was one that offered proof of the early origins of the narrative, and its basis, to some extent, in early realities.

Indeed, I would go further. Nilsson, in this section of his work, is not talking about Noth, but Karl Otfried Müller, the early 19th century classicist who composed such well-known contributions as *The Dorians* (*Die Dorier*). Müller, too, had a vision—as Nilsson puts it—of a process through which "myths were transferred to other regions with the wanderings of the tribes and that, as the tribes met and mixed, their myths met and were fused," which is more or less what Noth imagined.²⁹ Nilsson correctly notes—again, before Noth wrote either of his histories—that the idea of a narrative as nothing more than a distorted memory of *Völkervanderung*, so popular once upon a time, is belied by the fact that *Beowulf*, for example, refers to Danes, Swedes, and Geats, but no Angles (or "English").³⁰ Meaning that an attempt to reconstruct the history of England from this text would suggest that these had invaded Britain before the Angles rather than the other way around. Noth, whatever the nature of his intellectual debt to Müller, was clearly trading in similar ideas, and it seems the market for them was as

²⁶ For an account of the *Grundlage*, see generally Noth, *Pentateuchal Traditions*, 38–45.

²⁷ Noth, *Pentateuchal Traditions*, 44.

²⁸ Martin P Nilsson, *Homer and Mycenae* (New York, NY: Cooper Square Publishers, 1968), 12.

²⁹ Martin P Nilsson, *The Mycenaean Origin of Greek Mythology* (Berkeley, Los Angeles: University of California Press, 1972), 8.

³⁰ Nilsson, *Mycenaean Origin of Greek Mythology*, 8.

hot as ever.³¹ Even if someone like Nilsson could already be aware that they were ill-formed.

We might, then, first place Noth's argument, and Cross's with it, in the context of the long counter-reaction to the skeptical arguments of Julius Wellhausen in the late 19th century. Wellhausen, of course, had argued that the Pentateuch was not only a physical product of the post-Exilic period but mainly reflected the historical imagination of that period, rather than historical realities. And while the Albright school would more aggressively reject his legacy, it was actually the German, Hermann Gunkel who first and most influentially offered a path to escape it with his famous "form criticism."³² Here, as was broadly typical well into the 1970s, he acknowledged the validity of the basic outlines of the Wellhausen chronology of Pentateuchal composition, but purported to be able to recover even something of the oral traditions upon which the original texts were based. And this would inaugurate the pattern of much of the rest of the century—accepting Wellhausen's view of the operation and era of the Priestly authors, denying its relevance to assessments of the age of the traditions involved, and to differing extents, on their historical reality.

Thus, Noth, by suggesting that so many texts about the tribes are primarily memories of a sort, and even about pre-Israelite experiences, was adding more fuel to this particular fire. I suggest that the fact that the fuel was so welcome solves the mystery of his argument's success—and that the overwhelming character of the latter came from the fact that the evidence he offered for it was unusually hard to scrutinize, and therefore, plausibly accepted with serious investigation. In other words, here as in the next section, it was this combination of the desire for a particular scholarly outcome with the relative obscurity of the ancient Greek corpus from the perspective of the typically trained twentieth century Hebrew Bible scholar—that produced, as it often produces, a parallelomania. Desire made the ancient Greek traditions appear to be something they were not; obscurity defended them from a scrutiny that would reveal as much. The end

³¹ Rogerson points out that Müller was a colleague of Heinrich Ewald's at Göttingen and suggests that this influenced Ewald's turn away from the emerging critical scholarship of "De Wette and others" on the subject of the presence of historical memories in biblical texts about early periods John Rogerson, *Old Testament Criticism in the Nineteenth Century: England and Germany* (London: Fortress Press, 1985), 92–93.

³² Actually, Gunkel himself preferred the term "literary history" to *Formgeschichte*, but so it would come to be called (Peter Benjamin Boeckel, "Exploring Narrative Forms and Trajectories: Form Criticism and the Noahic Covenant," in *Partners with God*, ed. Shelley L. Birdsong and Serge Frolov, vol. 2 of *Theological and Critical Readings of the Bible in Honor of Marvin A. Sweeney* (Claremont, CA: Claremont Press, 2017), 27. See especially Hermann Gunkel, "Die Israelitische Literatur," in *Die Orientalischen Literaturen*, ed. P. Hinneberg (Leipzig: B.G. Teubner, 1906), 51–102.

result was a so-popular argument that was based almost entirely on a vision of Greek amphictyonies that never were.

FRANK MOORE CROSS

One more time, it is worth taking a moment to be explicit. My purpose here is not, and has not been, to point out the flaws in that widely acknowledged to be fatally flawed. Such an exercise would not be quite as pointless in this case as with Noth's amphictyony—Cross's epic does survive a little better, if much more as an influence than in anything like its original form. Specifically, it tends to survive as an influence in places where his argument is treated as a much more general version of itself—more like the general possibility that the Pentateuchal narrative was based on epic traditions of some sort, and to some extent, than the quite specific case he actually made—that it was based on *an* oral, poetic epic of long ago, in the mode of Homeric epic.³³ Still, it is not an active hypothesis in its original form, and it was never as influential to begin with.

Indeed, in addition to certain early critiques raised by John Van Seters, which I will discuss below, Mark S. Smith's more recent and comprehensive rebuttal is worth acknowledging. Here, he drew attention to the flaws in Cross's dichotomization of "myth" and "epic;" the subsequent downdating of many of the texts Cross regarded as early; the lack of evidence for the oral dictation of epics in Ugarit; and the general absence of the same for the view that poems of the *length* of the epics that so interested Cross were ever written in ancient Israel and Judah.³⁴ I quite agree with Smith, as well, that it is better to regard the Pentateuchal narrative as actually a kind of "anti-epic," a "reaction to Israel's older origins story or stories," which is to say that it is an effort to do something new *with* whatever of Israel's early tra-

³³ See, for example, Susan Niditch's relatively recent statement that Cross's work remains "among the most seminal and influential recent treatments" on the topic of epic (Susan Niditch, "The Challenge of Israelite Epic," in *A Companion to Ancient Epic*, ed. John Miles Foley (Malden, MA; Oxford: Blackwell Publishing, 2005), 287. Niditch is responding in part to Conroy's argument that figures like Abraham and Isaac are not warlike enough to appear in the traditions usually called epic – "Conroy's exclusion of an unheroic Abraham from Israelite epic seems not so necessary, and Cross's inclusion entirely possible" (Niditch, "Challenge of Israelite Epic," 280). However, her own view—that "Israelite literature does preserve epic-like traditions in tales of the judges and early kings"—is much more like that of Umberto Cassuto and others than Cross (Susan Niditch, "Epic and History in the Hebrew Bible: Definitions, 'Ethnic Genres,' and the Challenges of Cultural Identity in the Biblical Book of Judges," in *Epic and History*, ed. David Konstan and Kurt A. Raaflaub, *The Ancient World: Comparative Histories* [Malden, MA; Oxford: Wiley Blackwell, 2010], 100).

³⁴ Mark S. Smith, *Poetic Heroes* (Grand Rapids, MI: Eerdmans, 2014), 38–39. I do not deal with the way Cross defined epic in opposition to myth here, but I discuss it in Andrew Tobolowsky, "The Hebrew Bible as Mythic 'Vocabulary': Towards a New Comparative Mythology," *Religions* 11 (2020): 459.

ditions survive here, even if it is based on something much earlier—which I am not convinced it is.³⁵

Still, my focus remains on the dimensions of these arguments that operate under the conditions we might call parallelomania. And throughout, I have explored the basic formula: desire + obscurity = parallelomania. Obviously, just as there are many fields upon which parallelomanias may alight that are obscure by virtue of not being typical primary focuses of inquiry in a discipline, there are many different desires that might shape how they operate. Indeed, if this essay focuses on high-profile instances in which ancient Greek evidence was improperly used to make a case for the early origins of the Pentateuchal narrative and its relationship to historical experiences, today we might be experiencing something of the opposite problem. In other words, today there are those who see in the same comparative corpus the potential for demonstrating its late, *Hellenistic* origin.³⁶ I have no objection to this suggestion and think it should certainly be investigated with an open mind. However, I also think that at least some of these studies treat superficial similarities between these two corpora as decisive parallels in order to make their case in just the same way that their predecessors did, towards quite different conclusions.³⁷ And the endurance of specifically classics-based parallelomanias in the study of the Hebrew Bible is of course why an essay of this sort remains necessary.

At the same time, it *is* the fact that mischaracterizations of ancient Greek evidence play the same role in both Noth's and Cross's arguments, towards the same conclusions, and for the same reasons, that makes what I am describing visible as a parallelomania. And the additional contribution Cross's account of

³⁵ Smith, *Poetic Heroes*, 39–40.

³⁶ Studies such as Philippe Guillaume, "Hesiod's Heroic Age and the Biblical Period of the Judges," in *The Bible and Hellenism: Greek Influence on Jewish and Early Christian Literature*, ed. Thomas L. Thompson and Philippe Wajdenbaum (Durham: Acumen, 2014), 146–64; Russell E. Gmirkin, *Plato and the Creation of the Hebrew Bible* (Routledge, 2016); Yakov S. Kupitz, "Stranger and City Girl: An Isomorphism between Genesis 24 and Homer's *Odyssey* 6-13," in *The Bible and Hellenism: Greek Influence on Jewish and Early Christian Literature*, ed. Thomas L. Thompson and Philippe Wajdenbaum (Durham: Acumen, 2014), 117–45.

³⁷ For example, Kupitz's treatment amounts to a catalogue of supposed similarities between the scene in the *Odyssey* where Odysseus meets Nausicaa and the one in Genesis 24 where Isaac meets Rebekah that includes supposedly striking convergences such as "mention of the girl's father's house," "importance of the girl's mother," "mention of the spring (well) where the city people would go to draw water" (Kupitz, "Stranger and City Girl," 126–27). From evidence such as this, Kupitz arrives at the extraordinary conclusion that "nearly ever verse in Genesis 24 [is] . . . modeled on some passage or idea of *Odyssey* 6-13" (Kupitz, "Stranger and City Girl," 118). It is hard for me to believe that any of these are more than a reflection, simply, of societies that are set up in fairly similar ways in terms of gender roles, the importance of wells, and so on.

an early, oral Pentateuchal epic can make here begins with the fact that by the era in which he offered it, more so than in the era of the amphictyony, it might well have seemed rather anachronistic already—because of the Romantic dimensions of *any* version of the view that a surviving, relatively late tradition is really just a type of an early narrative that itself represents the distillation of the collective memory of an ethnic nation regarding its own formative experiences. That it did not seem so showcases a still powerful willingness to find proof of this proposition when it should hardly have even been considered discoverable, any longer.³⁸

Something of the foundational relationship between how these arguments were made and why is certainly presaged in an issue noted by both Dennis Pardee and Mark S. Smith. Pardee and Smith state that Cross's vision of an oral poetic epic of the Israelite "league" essentially *is* Noth's idea of the *Grundlage* of the amphictyony; but Cross does not offer any explanation of why this model should have survived the downfall of the older reconstruction.³⁹ A still greater insight in this direction, however, is offered by Robert D. Miller II who observed that Cross and some of his students "re-envisioned tradition history with Oral

³⁸ One reviewer of this piece posed the important question of how well contemporary inquiries into "collective memory" might actually bear out the sense described in these case studies of the likelihood that historical realities inspired these texts to some extent. There is not much room to go into this difficult issue here, but my sense of things is that too often the concept of collective memory is used *as if* it were only a contemporaneous means of arriving, once again, at conclusions like Noth's and Cross's. It seems quite clear that in any given context, societal influences help give shape to how many people understand the past, which is collective memory, but much less clear that the older, Romantic model of a society handing down *one* set of memories from generation to generation—let alone memories that were created by actual experiences—has any basis whatsoever. In other words, my sense is that what appears to be collective memory is as dramatically reshaped over time as any other aspect of tradition. For apt discussions of misconceptions of how collective memory works in the study of the ancient world, see Ian Douglas Wilson, *Kingship and Memory in Ancient Judah* (New York: Oxford University Press, 2016), 1–33; Ian D. Wilson, "History and the Hebrew Bible: Culture, Narrative, and Memory," *Brill Research Perspectives in Biblical Interpretation* 3 (2018): 1–69; Robert D. Miller, *Oral Tradition in Ancient Israel* (Eugene, OR: Cascade Books, 2011), 7. See also Astrid Erll, "Cultural Memory Studies: An Introduction," in *Cultural Memory Studies: An International and Interdisciplinary Handbook*, ed. Astrid Erll and Ansgar Nünning (Berlin: De Gruyter, 2010).

³⁹ "... the word 'league' ... is never defined nor is the assertion itself ever defended at length ... The term is so reminiscent of the 'amphictyony' of a former generation, however, that one would have wished, in place of apodictic pronouncements, a reasoned exposition of the progression from the first concept, now generally disavowed, to the second." Dennis Pardee, review of *From Epic to Canon: History and Literature in Ancient Israel*, by Frank Moore Cross, *JANES* 62 (2003): 134. Smith, too, notes that Cross's epic is "essentially, Martin Noth's *Grundlage*" (Smith, *Poetic Heroes*, 37).

Formulaicism substituted for form-criticism.”⁴⁰ What I like about this observation is that it, too, stresses the continuity of desire that I am trying to bring to life.

In other words, as mentioned above, a great many twentieth century scholars were in the business of asserting the viability of the recovery of Israel’s early traditions, and the study of early Israel’s historical experiences through it. And it is good to see both Noth’s and Cross’s arguments as efforts in the same vein, part of the same pattern. Noth was certainly explicit on this point: “the decisive steps on the way to the formation of the Pentateuch were taken during the preliterate stage, and the literary fixations only gave final form to material which in its essentials was already given.”⁴¹ Cross would be, too: “[the epic’s] essential shaping came not from the Yahwist, but from the singers of the early Israelite league . . . There is no reason to doubt the Israelite epic traditions preserve accurate reflections of the social institutions, and especially the religious lore of the old time of which it sings.”⁴²

Still, oral-formulaicism would play only a limited role in Cross’s argument as he would come to develop it.⁴³ In fact, its roots, actually—and this is part of the problem—lay in an unrelated earlier argument made by Albright himself. It had been Albright who first suggested that the (prose) Pentateuch might be based on an (oral and poetic) epic, and even by referring to classical analogies—in this case, a supposedly common “Graeco-Roman” practice of “secondary adaptation.”⁴⁴ And, it would certainly appear to be the Albright model that shapes the few re-

⁴⁰ Robert D. Miller, *Oral Tradition in Ancient Israel* (Eugene, OR: Cascade Books, 2011), 19. Van Seters would make a similar point - that the appeal of “an original poetic epic behind the source of the Pentateuch” is that it allowed scholars to reassert “the great antiquity of its ‘history’ through a long stage of oral tradition.” John Van Seters, *In Search of History: Historiography in the Ancient World and the Origins of Biblical History*, 2nd ed. (Winona Lake, IN: Eisenbrauns, 1997), 30.

⁴¹ Martin Noth, *A History of Pentateuchal Traditions*, trans. B.W. Anderson (Englewood Cliffs, NJ: Prentice Hall, 1981), 1–2.

⁴² Frank Moore Cross, “The Epic Traditions of Early Israel,” in *The Poet and the Historian*, ed. Richard E. Friedman (Chico, CA: Scholars Press, 1983), 38.

⁴³ Not no role. Cross, who was a colleague of Lord’s—as well as Cedric Whitman and Gregory Nagy—at Harvard, did make claims in this direction and a system of formulae *like* the one found in Homeric poems but identified a supposedly indigenous system going back to ancient Ugarit. As he put it, “Canaanite verse also reveals characteristic oral formulae and themes. It does not use, of course, epic hexameter verse. Its prosody is characterized by parallelism in bicola and tricola . . . Israel’s early poetry stands in this Canaanite tradition using substantially the same repertoire of formulae and themes” (Cross, “Epic Traditions,” 15).

⁴⁴ “This secondary prose stage is found in many Graeco-Roman logographers and historians who narrate Homeric or other saga” (William F. Albright, *From the Stone Age to Christianity: Monotheism and the Historical Process* [Baltimore, MD: Johns Hopkins University Press, 1946], 35).

marks on the subject of what an epic is, and the Pentateuch's relationship to it, that Cross offers in 1973's landmark *From Canaanite Myth to Hebrew Epic*.⁴⁵ Here, he asserts that "the epic cycle" of the Israelite league "was taken up into the prose Epic (JE) sources in the course of the early monarchy" and that the Pentateuch "may be described as a baroque elaboration of these Epic sources."⁴⁶ Not surprisingly, too, other Albrightians of Cross's generation would make similar claims, including David Noel Freedman and—a scholar whose parallelomanias could certainly be discussed here as well—Cyrus Gordon.⁴⁷ In time, however Albright—and Freedman—would move away from the idea of an early epic, even as Cross doubled down on it.⁴⁸ And now his argument would be based as much on the power of analogy as Noth's had been.

Briefly, then—Cross's most comprehensive statement on the subject appears in a 1983 essay called "The Epic Traditions of Israel," which would later appear in lightly modified form as the second chapter of 1998's *From Epic to Canon*. To some extent, we might read this essay as a response to certain criticisms, especially those by Charles Conroy, on the point that epics, as traditionally understood, tend to revolve around heroic glory in a way that the Pentateuch certainly does not.⁴⁹ Here, Cross quickly concedes the point: "that early Israel was not a 'heroic society' in the Homeric pattern."⁵⁰ Now, however, he proposes a new def-

⁴⁵ Conroy notes that one has to gather Cross's views [on epic] from scattered remarks" here (Conroy, "Hebrew Epic," 13).

⁴⁶ Frank Moore Cross, *Canaanite Myth and Hebrew Epic: Essays in the History of the Religion of Israel*, 9th ed. (Cambridge, MA: Harvard University Press, 1997), viii–ix.

⁴⁷ See Freedman, "Pentateuch," 18–19; Conroy, "Hebrew Epic," 12–13; Van Seters, *In Search of History*, 18–20; Cyrus Gordon, *Before the Bible: The Common Background of Greek and Hebrew Civilization* (London: W.W. Norton, 1962), 293. Van Seters, *In Search of History*, 20. Cross himself explored the idea of an Israelite epic as early as 1962, in an essay that would later appear as the first chapter of *Canaanite Myth* (Frank Moore Cross, "Yahweh and the God of the Patriarchs," *HTR* 55 [1962]: 225). As for Freedman, his early studies offer a telling blend of what would be Cross's argument and Noth's earlier one, referring to a to a *Grundlage* which "was a poetic composition, orally transmitted, relating the official story of Israel and its forebears" with a "cultic locus in the amphictyonic festivals" (Freedman, "Pentateuch," 714).

⁴⁸ "The theory of a poetic substratum or an underlying epic poem remains attractive, but so far it is not only unproved but unprovable. I doubt that this epic ever existed" David Noel Freedman, "Pottery, Poetry, and Prophecy: An Essay on Biblical Poetry," *JBL* 96 (1977): 17. For the evolution of Albright's ideas on the subject, see the discussion in Conroy, "Hebrew Epic," 18–20.

⁴⁹ "This is most evident in the Patriarchal stories where the figures of Abraham, Isaac and Jacob (not to mention Joseph) are in no way representative of the heroic attitude to life" (Conroy, "Hebrew Epic," 20–21).

In any case,

⁵⁰ Cross, "Epic Traditions," 17–18.

inition, based not on what epics *say*, but on what they supposedly do in society.

Actually, in this essay, he offers a two-part definition. First, that it was “permissible to define epic as the traditional narrative cycle of an age conceived as ‘normative,’ the events of which give meaning, self-understanding to a people or nation.”⁵¹ Second, and crucially, he proposes to define the term “rather narrowly, drawing upon studies of Homeric epic for delineating the traits of epic and for analogies to aid understanding the nature of Hebrew epic lore.”⁵² Later in the essay, he reiterates both points. Epics, as a genre, are supposedly the “normative” expression of an entire nation’s *beliefs*, as well as “national” compositions, offering the definitive account of how a nation understood its own formative experiences.⁵³ And, as “the Homeric epics shaped the Greek self-consciousness and gave normative expression to Hellenic mythology. The Hebrew epic recounted crucial events of developing nationhood and gave classical expression to Yahwistic religion.”⁵⁴

There are a multitude of problems here and they, too, start before Cross even picked up the pen. First and foremost, *there is no such thing* as a classical practice of “secondary adaptation”—and this, of course, should have been obvious to anyone who bothered to look. In fact, John Van Seters would make this point already in a study that was originally published the same year as Cross’s essay, 1983:

The earliest Greek prose writers used the Homeric epic, as well as the epic cycle and the myths and legends preserved in Hesiod and his followers, as a rich source of information for their many books . . . they made *no* effort to reproduce epic works in prose. There are no prose versions of the *Iliad* or the *Odyssey* or the works of Hesiod.⁵⁵

⁵¹ Cross, “Epic Traditions,” 17–18.

⁵² Cross, “Epic Traditions,” 14.

⁵³ Cross argued that “the Homeric epics shaped the Greek self-consciousness and gave normative expression to Hellenic mythology. The Hebrew epic recounted crucial events of developing nationhood and gave classical expression to Yahwistic religion” (Cross, “Epic Traditions,” 18). They were “‘national’ compositions especially on recited at pilgrimage festivals” and “a composition describing traditional events of an age conceived as normative or glorious” (Cross, “Epic Traditions,” 19). Later in the same essay he argues that epic “has a concrete social function; its oral performances were public, and more specifically in cultic or national festivals . . . [a]lways an aspect of these celebrations was the reconstitution of the nation, the rearticulation of the identity of the community, the reinforcement of the unity of the people” (Cross, “Epic Traditions,” 27).

⁵⁴ Cross, “Epic Traditions,” 18.

⁵⁵ Van Seters, *In Search of History*, 22. Cross would later say of Van Seters’ counter-arguments, “I had not seen the first of these volumes when writing in 1983, nor did van Seters – evidently – know of my essay” Frank Moore Cross, *From Epic to Canon: History and Literature in Ancient Israel* (Baltimore, MD: Johns Hopkins University Press, 1998), 29 fn. 21.

Indeed, as Van Seters also noted, this is true—as far as we can tell—in ancient Ugarit also. There, too, we have surviving “epics” —if that is really the appropriate term—but no prose versions. Thus, in Cross’s argument “we are asked to believe that in ancient Israel the opposite was the case—that only the abbreviated prose versions survived and not the original epics.”⁵⁶ Or, as we might put it here—the phenomenon Cross imagined was not only unsupported by classical evidence but had no basis in *either* of the contexts he referred to as supports.

Thus, it is already the case that someone less taken with the conclusions Albright had reached would have presumably shown more care in advancing this argument. The central problem, however, still lies not with this, but with the analogy Cross makes, and the extent of his dependence on it, just as it did with Noth. And here, his reconstruction of the Israelite epic was based, more or less, entirely on how he understood what Homer was to the Greeks—Cross no more had an actual epic, prose or otherwise to work with, than Noth did a biblical account of the Israelite amphictyony. And here, too, it should have been obvious, not only in retrospect, that this was very much *not* what Homer was to the Greeks.

Specifically, the Homeric epics should simply never have seemed to be “national” or “normative” compositions in the way Cross meant. Not even Harvard’s Homerists—Cross’s colleagues—would have claimed as much. To be sure, they would likely have agreed, especially Lord, that the presence of formulaic expressions in almost any literature was enough to assert the presence of an original oral composition of surpassing importance. But they *could not* have gone farther than that — they could not have claimed that the Homeric poems were *the* national or normative account of the early Greeks, with respect to their era of legendary origins.⁵⁷ The counter-arguments are too obvious.

After all, Cross describes the Israelite epic as the collective “creation of the Israelite league,” a distillation of *Israel’s* memories of *Israel’s* formative experiences.⁵⁸ But within the ancient Greek corpus, the Homeric poems are not even the only poems in the “Epic Cycle,” concerning the Trojan War.⁵⁹ The “Epic

⁵⁶ Van Seters, *In Search of History*, 30. Whether or not the Ugaritic poems should be called “epics” is not an issue I have space to deal with here.

⁵⁷ Even Milman Parry himself, who generally credited Homer with the *least* capacity for personal invention, was insistent on the presence of a large *repertoire* of important traditions of which Homer’s would have represented only part — and was, generally, studying how South Slavic poets preserve and interact with that repertoire: “The young poet learns from some older singer not simply the general style of poetry, but the whole formulaic diction. This he does by hearing and remembering many poems” (Milman Parry, “Studies in the Epic Technique of Oral Verse-Making. II. The Homeric Language as the Language of an Oral Poetry,” *Harvard Studies in Classical Philology* 43 (1932): 8.

⁵⁸ Cross, “Epic Traditions,” 31.

⁵⁹ The epic cycle would eventually be defined as an eight-poem se-

Cycle” is not, despite the name, the only early epic cycle, and epic cycles do not contain the only important early traditions or important traditions *about* early Greece, many of which conflicted.⁶⁰ As for Cross’s claim that these “gave normative expression to Hellenic mythology,” the same problem emerges.⁶¹ Again, these poems were tremendously influential, often the most influential – but even in Homer’s own supposed day there was Hesiod, whose accounts of the gods could be preferred to Homer from time to time. It is Hesiod’s story, not Homer’s, for example, that makes Aphrodite the daughter of Uranus, rising from the sea. Homer makes her simply the daughter of Zeus and Dione.

One point I would make here—and return to later—is that there is a certain irony in this, in the sense that a straightforward consideration of the phenomenology of Homeric epic should actually have made the *opposite* point to the one Cross wanted to make. In other words, the diversity of early Greek traditions shows how unlikely it is that there was ever one, single, ruling epic in ancient Israel and Judah. And the creativity of those who were Homer’s heirs show how unlikely it is that the same charter tradition remained not just dominant but stable throughout Israel’s history. This is a point Van Seters makes as well – that there were, of course, those who drew on Homeric epic in ancient Greece, and Hesiodic as well. But they did not “feel any need to stick very close to the ‘facts’ as presented by Homer and Hesiod. They were often busy discovering new legendary events or inventing new connections between the heroes through genealogical constructions.”⁶² This is another reason why it is terribly unlikely that the Pentateuch is simply a version of a very early story, which was *the* story of early Israel.

In fact, an interesting point in a similar vein had long since been made by Wellhausen himself. Commenting on his belief that the Pentateuchal narrative was not just a largely post-exilic composition, but one that had no significant prior roots, Wellhausen notes that “it would puzzle the very best intentions to beat up so many as two or three unambiguous allusions to the Law” in the books of Kings “and these cannot be held to prove

quence, of which the *Iliad* and the *Odyssey* were second and seventh. In earlier periods there may have been fewer poems, but certainly more than these two.

⁶⁰ See especially John M. Foley and Margalit Finkelberg, “Meta-Cyclic Epic and Homeric Poetry,” in *The Greek Epic Cycle and Its Ancient Reception*, ed. Marco Fantuzzi and Christos Tsagalis (Cambridge, UK: Cambridge University Press, 2015), 126–38; M.L. West, *The Epic Cycle: A Commentary on the Lost Troy Epics* (Oxford: Oxford University Press, 2013); Marco Fantuzzi and Christos Tsagalis, eds., *The Greek Epic Cycle and Its Ancient Reception* (Cambridge, UK: Cambridge University Press, 2015). West reckons there were at least four largely independent early “cycles,” including the traditions surrounding Jason and the Argonauts, and the Theban cycle. See M.L. West, *The Hesiodic Catalogue of Women: Its Nature, Structure and Origin* (Oxford: Oxford University Press, 1985), 137–38.

⁶¹ Cross, “Epic Traditions,” 18.

⁶² Van Seters, *In Search of History*, 22.

anything when one considers, by way of contrast, what Homer was to the Greeks.”⁶³ In other words, the fact that it is so easy to prove that Homer *was* tremendously important to the Greeks, a centerpiece of Cross’s argument, brings into profile how remarkable it is that there is *no* proof of an early, important pentateuchal narrative, suggesting its non-existence.

Ultimately, then, the fact that Cross, or Albright, or whomever did not arrive at any of these recognitions of course sharpens the sense that their perception of the ancient Greek evidence was shaped, first and foremost, by what they desired to find there. Indeed, I would go so far as to say that from a technical perspective, what Cross did is—quite inadvertently—*reverse* the direction of his comparison at the crucial moment. That is, rather than extrapolating what an epic was from the Homeric poems and applying it to the Israelite context, as he claimed to be doing, he seems instead to have imposed the apparent singularity of the pentateuchal narrative as early Israelite charter tradition onto the world of early Greek traditions. This singularity is, however, *only* apparent in the biblical case—an accident of survival—and it manifestly does not fit the busy and varied world of early Greek visions of legendary events.

In other words, it is the Pentateuch, not Homer, that *appears* to be the national and normative account of Israelite origins simply by virtue of the fact that it is the only one complete narrative of this sort that *survives*. Homer, by contrast, looks like what Homer is. His are, and were, the most popular of many different traditions about the same events, alongside many other traditions about important early realities and experiences, some of them quite different from the rest. And this is why Cross should indeed have drawn the opposite lessons from his foray into comparison, but it is also why it is so clear that he saw what he wanted to see, rather than what was there.

Indeed, with Cross, the pattern of selective use, conflation, and mischaracterization appears even in his use of scholarly arguments. I could point to a number of examples. For one thing, it is at least possible that Cross may have intentionally failed to refer to Martin Nilsson by name at one point in order to strengthen his rebuttal of a skeptical view *with* another of Nilsson’s arguments.⁶⁴ And I have already noted that Cross shared

⁶³ Julius Wellhausen, *Prolegomena to the History of Israel. With a Reprint of the Article Israel from the “Encyclopedia Britannica,”* trans. J. Sutherland Black and Alan Menzies (Edinburgh: A. & C. Black, 1885), 5.

⁶⁴ At any rate, he discounts the “extreme skepticism about the ability of ancient memory to preserve ancient elements of tradition” citing an unnamed “recent scholar” who “asserts that ‘folk memory’ at most spans two generations.” He suggests this is “now shown to be without warrant” with reference to such phenomena as “the atlas of Mycenaean Greece preserved in the so-called Catalogue of Ships” Cross, “Epic Traditions,” 25. However, it is Nilsson whose most famous contribution concerns the importance of the correlation between the major cities in the Homeric poems and the major cities of Mycenae itself (see, for example Nilsson, *Mycenaean Origin of Greek Mythology*, 27–28. And, Nilsson also argued that if the Homeric poems “were preserved by the

the blithe dismissal of the important challenges to the Parry Lord formulation of oral-formulaic theory of Lord himself, among others, despite their validity. The most notable example of Cross's selectivity and mischaracterization of the scholarly evidence concerns the work of Moses Finley – who, in his *The World of Odysseus* (1954), is best known for being the first scholar to publish an influential refutation of the Nilssonian conviction that archaeological and epigraphic evidence demonstrated the rootedness of the Homeric poems in Mycenaean realities.

To be sure, Cross does credit Finley with a “contrasting perspective on the extent of Mycenaean survivals.”⁶⁵ But he also cites Finley—and only Finley—as the support for his central claim that “[t]he Homeric epics shaped the Greek self-consciousness and gave normative expression to Hellenic mythology” just as “[t]he Hebrew epic recounted crucial events of developing nationhood.”⁶⁶ And here he is presumably referring to the fact that Finley did extol the popularity of Homer in ancient Greece—“their pre-eminent symbol of nationhood, the unimpeachable authority on their earliest history, and a decisive figure in the creation of their pantheon, as well as their most beloved and widely quoted poet.”⁶⁷

Finley, however, was referring explicitly to the importance the Homeric poems assumed *as* literary artifacts circulating in Archaic and especially Classical Greece, and not what Cross's comparison required, which was the importance of the oral original on which the poems were based. This is very different, and another instance in which the comparison might easily lead to the opposite conclusion—that an oral poem can *become* important as a textual composition without having been a centrally important oral tradition earlier on. Neither does Cross cite or acknowledge one of Finley's conclusions from the same study, running directly counter to his own case: that the world of ancient Greece “*could not possibly* have produced a unified, consistent national mythology.”⁶⁸ This was because of the wide and varied character of the Greek world and a case could be made that smaller and more contained Israel was likelier to have a more unified national *mythos*—but not by someone whose main evidence for the early national character of the Israelite narrative was *only* the phenomenon of the Homeric poems.

Such, generally, is the transformative power of desire in a scholarly context where inquiry lacks the external controls that often come along with greater familiarity. Not just Noth's and Cross's arguments, but their impact—their influence—were the result. The remaining question is what these efforts suggest for

memory of the minstrels and handed down by oral tradition, it is difficult to imagine that they survived more than two or at most three generations” Nilsson, *Mycenaean Origin of Greek Mythology*, 11–12.

⁶⁵ Cross, “Epic Traditions,” 16 fn. 10; Moses Finley, *The World of Odysseus* (New York, NY: The Viking Press, 1978).

⁶⁶ Cross, “Epic Traditions,” 18. Here, he cites Finley, *World of Odysseus*, 15.

⁶⁷ Finley, *World of Odysseus*, 15.

⁶⁸ Moses Finley, *World of Odysseus*, 24.

contemporary efforts, in an era where the world of ancient Greece is once more coming into view as a source for useful comparisons for the study of the Hebrew Bible's traditions. And the answer begins not only with thinking seriously about how to avoid some of the issues described above, but acknowledging which ones *cannot* be avoided in a systematic way.

WHAT NEXT?

Let us contemplate once more the formula I have advanced: desire + obscurity = parallelomania. Again, this need not be a very considerable obscurity—I can hardly claim that no biblical scholar had read the works of Homer, or Strabo. Noth, as an early reviewer of this essay pointed out to me, would even have gotten quite a good classical education at Gymnasium. But neither are any of the other parallelomanias mentioned above premised on *real* obscurity. Anyone can read the Elephantine papyri, or rabbinic, or Near Eastern materials these days. There is simply a difference between a text a scholar knows through intensive investigations, and within one's own field, and one that may be familiar as a primary source, but not on so intimate a level. In the latter case, the scholar may well see in the text what they want to see, through quite unintentional motivated reasoning.

Of course, desires change—certainly the intensity of the one discussed here has waned. They were, I think, still more intense than is often acknowledged, even now. While researching this essay I was actually most struck, not by either of the examples above but by a claim made by Albright—not a terribly consequential one, really. But in the course of his *Archaeology and the Religion of Israel* he asserts that

Once we can read the tablets of Cnossus and Pylus . . . it will be possible to use the Homeric epics with a great deal more confidence as direct sources for our knowledge of Aegean religion toward the end of the second millennium. Until then we cannot effectively disprove the views of radical critics who insist that the Iliad was not composed until about the eighth century B.C.⁶⁹

Here, he is referring to the fact that Linear B had not yet been deciphered by the time this book was first published, which was quite true.

Even so, it is easy to miss how odd an argument this is to make. For one thing, what is it doing in a book about the “archaeology and religion of ancient Israel” in the first place? The obvious answer is what puts it in conversation with what we have seen so far—that this argument is meant to work by analogy. If one set of Iron Age texts can show what Bronze Age religion was really like, why not another? But Albright never really explains why this should be the case any more than Noth or Cross explained why Greek amphictyonies and epics should be found

⁶⁹ William F. Albright, *Archaeology and the Religion of Israel* (Baltimore, MD: The Johns Hopkins University Press, 1942), 58.

in the Levant.⁷⁰ Then—of course—we can ask why critics should be considered “radical” *before* the evidence that proves them to be is even available—and most of all, why should anyone presume to know what tablets that *have not been translated* will or will not prove in the first place?

It is not surprising, then, that the pattern of Albright’s argument conforms to what we see above, too, in other ways—most especially in the fact that it very much seems as if he allows the ancient Greek evidence to play by different rules than the biblical or Near Eastern. This is visible in more than the above gloss of unreadable evidence—because, in fact, there were editions of this book published after the decipherment of Linear B and Albright does update the text somewhat to reflect as much. He shows himself, however, far less interested in the reality than he was, initially, in the power of the analogy. Not only does he do little more than change the phrase “once we can read” to “once we can understand,” he refers to the decipherer, Michael Francis Ventris, by the wrong name—“A. Ventris.”⁷¹ If it is hard to imagine Albright, who was typically so meticulous in philological matters, making this kind of error with Champollion or Rawlinson, it is not hard to see why.

Similarly, while the amphictyony—and Noth’s method of extrapolating tribal history out of the differences between tribal lists—have a central role to play in both his *Geschichte Israels* and *Überlieferungsgeschichte des Pentateuch* of the late ‘40s and early ‘50s, there are hardly even any *references* to classical texts or classical scholarship. And this is despite the fact that so many of the most influential classical inquiries occurred between the publication of the former and the latter.⁷² Meanwhile, Cross, in the version of his 1983 essay that appears in *From Epic to Canon* responds to Van Seters’s important critiques only in a lengthy footnote—which is also one of the only major changes I was able to discover between the two. Here, however, he largely ignores Van Seters’s criticisms to focus on the latter’s view that the Pentateuchal narrative was more like something out of Herodotus than Homer. He accuses Van Seters of “theorizing on the basis of questionable analogies” rather than on the supposedly more objective ev-

⁷⁰ Noth at one point acknowledges the “question” of “how far it is permissible to use this comparative material to complete the picture of the Israelite twelve-tribe system” and even that it “derives from a relatively remote area, from a comparable, but different historical setting.” Even here, however, he offers no real answer other than that “if statements on the Old Testament tradition correspond to it . . . we should not disregard it” (Noth, *History of Israel*, 90–91).

⁷¹ He called him – Michael Francis Ventris – “A. Ventris” (Albright, *Archaeology and the Religion of Israel*, 58). Additionally, the footnote that in 1942 referred “provisionally” to a 1939 article by Carl Blegen, relating the discovery of the tablets, remains unchanged even by the addition of a citation to any of Ventris’ work, or, for that matter, by the subtraction of the word “provisionally.”

⁷² Including nearly the entire scholarly output of Milman Parry, who died in 1935 at 33 and Nilsson’s famous *Mycenaean Origins of Greek Mythology* (1932) and *Homer and Mycenae* (1933)

idence of the “typological sciences,” meaning the typologies of “narrative genres” like epic.⁷³

We can note, of course, that it was Cross himself who supplied the fairly idiosyncratic definition of the genre of epic described above, which is to say, the typology of his typological science was self-supplied in this case. Just as importantly, however, Van Seters was, of course, comparing two *existing* prose accounts of the heroic past. Cross’s comparison between two surviving Greek poems and an *entirely hypothetical* oral composition is of course far more “questionable.”⁷⁴ In these refusals and lack of attention to detail, as much as in these original arguments—and almost as much as in Albright’s willingness to see evidence written on what was, from his perspective, *tabulae rasae*—we see how powerful desire can be, and why we ought to regard it with a healthy respect. Especially when it has the obscured shape of something beheld at a distance to operate on.

Today, this particular desire has indeed changed, if not quite so much as we might like to believe. But others exist, and always will. And that is where the central problem of this last discussion comes in—that the relative obscurity of ancient Greek traditions, from the perspective of the typically trained Hebrew Bible scholar, has not changed, and presumably will not change. This is not to say that individual scholars cannot gain enough expertise to make these comparisons useful—I am certainly among those who have tried!⁷⁵ And if being as expert in a comparative corpus as in a primary one was a prerequisite to making the effort, no one would ever do it, and we could hardly ever think it yielded useful results.

At the same time, there is no real prospect of a *critical mass* of Hebrew Bible scholars gaining enough proficiency with ancient Greek materials not only to make, but to assess these kinds of comparisons unless the field changes dramatically. In other words, as long as facility with Near Eastern and Levantine comparisons remains the *sine qua non* of Hebrew Bible scholarship—with perfectly good reason!—there hardly seems to be any possibility of simply adding another corpus to the mix. And this state of affairs holds two dangers rather than one. Certainly, it remains likely that scholars will continue to see what is not there. But, in a somewhat more responsible era of scholarship, it may also be the case that some actually sterling contributions will be ignored because they rely on data sets that are too much out of the way for the scholarly community to know what to make of them.

There are not hypothetical concerns. In one direction, I mentioned above the existence of a contemporary tendency to

⁷³ Cross, *From Epic to Canon*, 30 fn. 21.

⁷⁴ And really, three hypothetical compositions since he was comparing the oral epic to the oral precursors of Homer.

⁷⁵ Andrew Tobolowsky, “Reading Genesis Through Chronicles: The Creation of the Sons of Jacob,” *Journal of Ancient Judaism* 7 (2016): 138–68; Andrew Tobolowsky, *The Sons of Jacob and the Sons of Herakles: The History of the Tribal System and the Organization of Biblical Identity*, FAT 2.96 (Tübingen: Mohr Siebeck, 2017); Andrew Tobolowsky, “The Problem of Reubenite Primacy,” *JBL* 139 (2020): 27–45.

see proof in biblical texts that biblical authors had Moses, or Homer, or Plato open in front of them while composing, rather than having perhaps received certain influences from a more general set of circulating ideas, if that. And I noted this is most egregious in cases where the author wants to make arguments, through comparison, for the Hellenistic origins of surviving texts. But in the other direction, there are also now quite a number of studies, again including my own, which have made the point, say, that similarities between ancient Greek genealogical traditions and biblical ones suggest quite a different way of interpreting their evidence than is still common in studies of Israel's history.⁷⁶ As far as I can tell, these have had very little impact on how the latter have pursued. In addition, I think scholarship on the construction of ancient Greek ethnicity tends to be more advanced than scholarship on ancient Israelite ethnicity, which is very often scholarship on ancient Israelite *origins* only—perhaps because the clearly late debut of Panhellenism itself has required these scholars to acknowledge the fluidity of ethnicity over time in a way that biblical scholars can avoid, if they wish.

The question then is indeed how *do* we go about pursuing potentially very useful, neglected, but treacherous comparisons with ancient Greek traditions? And I would offer two answers to this question, both of which come from a modification of the phrase I used above—the “more malleable other.” Malleable is what we must avoid. But other, the corpus of ancient Greek traditions is. It is a body of often similar narratives, composed at just the same time, and not very far away from the world of ancient Israel. This is especially true when we consider that “Greece” in the ancient world was not mainland Greece but a string of colonies that extended deep into the Eastern Mediterranean, Levant, and Anatolia. The two regions also occasionally experienced some of the same watershed events.⁷⁷

One way forward, therefore, is simply to consider the world, experiences, and traditions of these other ancient people *as* a counterpart—as a fellow traveler in the ancient world, not unlike the Near Eastern peoples who have more often been treated in this way. And this work, thankfully, is already beginning to be done. That is, a number of studies now simply include

⁷⁶ Gary N. Knoppers, “Greek Historiography and the Chronicler’s History: A Reexamination,” *JBL* 122 (2003): 627–50; Tobolowsky, *Sons of Jacob*; Tobolowsky, “Problem of Reubenite Primacy”; Andrew Tobolowsky, *The Myth of the Twelve Tribes of Israel: New Identities Across Time and Space* (Cambridge: Cambridge University Press, 2022); Mark McEntire and Wongi Park, “Ethnic Fission and Fusion in Biblical Genealogies,” *JBL* 140 (2021): 31–47.

⁷⁷ Arnaldo Momigliano, in considering the precise issue of points of contact between the disciplines notes that “Jewish historiography developed at least from the 5th century B.C. amidst conditions shared by Greek historiography” (Arnaldo Momigliano, “Biblical Studies and Classical Studies: Simple Reflections about Historical Method,” *The Biblical Archaeologist* 45 [1982]: 227). He notes, as an example, interactions with Persia, which conquered the Levantine region in the early 530s and had met the Greeks at Marathon by 490.

data sets from the Greek world alongside Near Eastern and Levantine in their inquiries, including those by Laura Quick, Anselm Hagedorn, Martti Nissinen, Raleigh Height, T.E. Kelle and others.⁷⁸ And these are motivated by something that is quite true – as Nissinen puts it, that “[t]he threefold breakdown” of the study of various topics, in his case prophecy, “into biblical, Near Eastern, and Greek reflects the current division of academic disciplines and the present state of communication between them.”⁷⁹ It does not represent the realities of the ancient world in a much more significant way. And I would add that that the centrality of the Near East in the study of the Hebrew Bible is certainly reasonable. However, it has also had its edges long-since honed by an older sense of the basic accuracy of the biblical account of the descent of the Israelites from Mesopotamian nomads and a Romantic sense of the close affiliation of a great Semitic nation, neither of which can be supported today. And so, Greece is closer, and the Near East is farther away than we often realize, and our studies can reflect that.

My primary suggestion, however, focuses on the specific danger of parallelomania, which is, in many cases, the problem of inaccurately assessing when *similarity* tips over into *identity*. We can say that there is a certain similarity between various kinds of Greek tribal organizations, including amphictyonies, and the twelve tribes of Israel, but it is far too much to say that any of them are actually the same, or that it is reasonable to suppose they could be. We can acknowledge that both the Pentateuch and Homer concern legendary figures associated with foundations of all kinds without overstating the little they have in common. Sometimes, as with segmented genealogical traditions in particular, it may actually be reasonable to suppose that texts in one context or another share a genre, or close enough that the operation of one holds lessons for the study of the other. More often, however, the appearance of identity will be a deception that can be dispelled by a closer inquiry, and we are better off evolving our notions of genre from *within* contexts, rather than assuming their universality between.⁸⁰ Especially when we have no reason to imagine significant points of contact between tradition production in each context.

⁷⁸ Laura Quick, *Dress, Adornment, and the Body in the Hebrew Bible* (Oxford: Oxford University Press, 2021); Anselm C. Hagedorn, *Between Moses and Plato: Individual and Society in Deuteronomy and Ancient Greek Law* (Göttingen: Vandenhoeck & Ruprecht, 2004); Martti Nissinen, *Ancient Prophecy: Near Eastern, Biblical, and Greek Perspectives*, First edition. (Oxford: University Press, 2017); Raleigh Heth and T.E. Kelley, “Isaac and Iphigenia: Portrayals of Child Sacrifice in Israelite and Greek Literature,” *Biblica* 102 (2021): 481–502.

⁷⁹ Nissinen, *Ancient Prophecy*, 6. Later, “[c]onstructing the big picture of ancient Eastern Mediterranean prophecy has long been obstructed by the separate lives of the disciplines of biblical and classical studies” (Nissinen, *Ancient Prophecy*, 9).

⁸⁰ See the discussion in Jacqueline Vayntrub, *Beyond Orality: Biblical Poetry on Its Own Terms*, The Ancient Word (London: Routledge, 2019), 18–35.

Yet if the various genres that exist between ancient Israel and Judah and ancient Greece are most often *similar*, rather than identical, the *problems* they present to scholars, and the questions they raise, are very often precisely identical. A story about the founding of a city or holy site will always raise identical questions about how reliable historical memory can be, even if the stories are more different from each other than we sometimes acknowledge, and even if the answer is different from case to case. A story about an ancient ancestor and their family will raise identical questions about the role of genealogical discourses in society, a description of an ancient religious ritual of its accuracy for that era or the era in which the text was written, and so on.

Then, there are the identical problems scholars face with respect to the general phenomenology of ancient tradition. We would like to know, for example, how to responsibly correlate archaeological evidence to historical narratives, how much to rely on texts for the reconstruction of religious practices, or how to integrate contemporary recognitions about the fluidity of ethnic identity or historical memory over time into our work. We share these problems, and we share a need for creative approaches to answering them—again, even if the answers will not necessarily be the same from one case to the next. We are lucky in that building a community of scholars working on these questions and developing approaches to them does *not* require completely mastering each other's disciplines. Instead, we can simply get a good idea, or see an interesting approach tried, or the work of an insightful theorist employed, and see for ourselves whether it might make sense in our own context. We should take advantage of these facts.

Here, however, is why it matters so much that, as I noted above, the phenomenology of ancient Greek traditions broadly points not only to a refutation of conclusions like Cross's in particular, but their opposite. In other words, the number and variety of ancient Greek traditions and their fluidity over time underscore how unlikely it is that the Pentateuch, or other traditions in the Hebrew Bible, present *the* dominant ancient Israelite charter tradition throughout the entire history of that place. And they make the same point about how unlikely it is that this single, surviving collection offers a representative sample of the diversity of ancient Israelite and Judahite visions of Israelite origins. Then, the historical development of Greek visions of the past over time shows how unlikely it is that we should regard the Pentateuchal narrative as, in essence, little more than an early version of Israelite history repeatedly adapted. The ancient Greeks indeed had many traditions of origin, and not one, many different accounts of early heroes and events, not one basic vision of who they were, passed hand to hand for centuries. Ancient Greek authors who adapted early traditions did not tell the same basic stories with new flourishes, but dramatically changed, combined, subtracted, and invented. In addition, fundamental ideas about who the Greeks were—Panhellenism especially—were not original but developed over time.

Thus, it is either the case that the inheritance of traditions worked very differently in ancient Israel or Judah than ancient Greece, or that many familiar assumptions about the authority and importance of surviving traditions prior to their inclusion in the familiar Pentateuch are off base.⁸¹ It may well be that we, like Cross, have not sufficiently considered how the *artificial* similarity between the biblical corpus and the Romantic paradigm of a single people with a single set of traditions – because, after all, it is only one book, however many compositions it contains – has shaped our thinking. The corpus of ancient Greek traditions can provide useful examples for ongoing discussions about just how varied the world of ancient Israelite and Judahite traditions might have been, how much early visions might have changed over time, and how representative the surviving text is of what “the” Israelites or “the” Judahites believed and practiced. And how scholars of ancient Greece talk about these topics can help re-evaluate approaches in our field. These would be very valuable things.

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⁸¹ Though not, it must be said, *universal* assumptions any longer – see, for example, John H. Choi, *Traditions at Odds: The Reception of the Pentateuch and Second Temple Period Literature* (New York: T & T Clark, 2010); Eva Mroczek, *The Literary Imagination in Jewish Antiquity* (Oxford: Oxford University Press, 2016).

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