



**Dalley, Stephanie**

***Esther's Revenge at Susa: From Sennacherib to Ahasuerus***

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The book of Esther has struck many readers as “strange.”<sup>1</sup> In modern times, with the historicity of the story effectively set aside,<sup>2</sup> the questions of its nature and origins have been thoroughly discussed, without much consensus. The search for the origins of the book of Esther has taken some interesting turns in recent decades. Some scholars have suggested that a pre-Masoretic version is preserved in the Greek so-called “Alpha Text,”<sup>3</sup> and initial publications suggested that 4Q550 from Qumran (named by the editor “Proto-Esther”) would assist in solving this crux, as well. These hopes were dashed in stages, however, and if 4Q550 is relevant, it is in a circuitous way.<sup>4</sup>

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1. Elias Bickerman, *Four Strange Books of the Bible: Jonah, Daniel, Koheleth, Esther* (New York: Schocken, 1967).

2. But see the discussion in Adele Berlin, “The Book of Esther and Ancient Storytelling,” *JBL* 120 (2001): 3–14, for discussion of the discussions of the book’s historicity in recent commentaries.

3. See especially David J. A. Clines, *The Esther Scroll: The Story of the Story* (Sheffield: JSOT Press, 1984); and Michael V. Fox, *The Redaction of the Books of Esther: On Reading Composite Texts* (SBLMS 40; Atlanta: Scholars Press, 1991).

4. For representative studies, see Sidnie White Crawford, “Has ‘Esther’ Been Found at Qumran? 4QProto-Esther and the ‘Esther’ Corpus,” *RevQ* 17 (1996): 307–25; John J. Collins and Deborah A. Green, “The Tales from the Persian Court (4Q550a–e),” in *Antikes Judentum und frühes Christentum: Festschrift für Hartmut*

More than a century ago, the search for a solution to the “problem” of the book pointed scholars toward Mesopotamia, since the names Mordecai and Esther suddenly seemed suspiciously familiar. But the Mesopotamian connection never compelled agreement, since the Mordecai and Esther in the book were undoubtedly mortal, and there was very little else about the book that seemed mythological. In recent treatments, there has been little attention paid to a Mesopotamian background, and Adele Berlin, for instance, utilizes Greek descriptions of Persia as her primary point of comparison.<sup>5</sup>

Assyriologist Stephanie Dalley has now made a case for reopening the possibility that it is specifically in Mesopotamia—and more particularly, in seventh-century Assyria—that the origins of Esther can be found. Reading this book is unlikely to be a relaxing experience for anyone. For this reader, it was alternately thrilling and maddening. The book (especially the first half) is full of creative and insightful historical reconstructions; it is also (especially in its second half) full of implausible suggestions and misreadings. To a degree, this checkered presentation is the result of a thesis that by necessity spans multiple cultures over many centuries, and it is exceedingly difficult for one person to master all the necessary sources to authoritatively review the data involved at all stages.

By necessity, this review will point out some of these flaws, but let me say up front that this is a doubly important book. For anyone interested in the prehistory, development, and meaning of the Esther story, the book is worth reading. Dalley has certainly not solved all the problems, but it has become clear in the past decades that there will be no simple answer to “what are the sources of Esther,” and a neglected piece of the puzzle has certainly been the Mesopotamian material. Dalley’s book, together with the recent article by Adam Silverstein,<sup>6</sup> reintroduce some of this material into the discussion; hopefully, with more material on the table, a synthesis will soon emerge.

The book falls neatly into two parts. The first (chs. 1–6, 11–162) offers primarily a synthetic presentation of Neo-Assyrian history from the reign of Sargon through that of Ashurbanipal. It is not a narration of that history, and it cannot be used to introduce the topic to students, but for those already familiar with the basic plotlines, it is a rich discussion. The ostensible goal of this section is to present the story of the Assyrians and Babylonians attacking the Elamites (in Susa) circa 646. But Dalley has not limited herself to a bare-bones presentation of just the points needed, and the reader will be indebted to her for that. Instead, she offers a story with multiple themes, including the power and

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*Stegemann zum 65. Geburtstag* (ed. Bernd Kollmann, Wolfgang Reinbold, and Annette Steudel; Berlin: de Gruyter, 1999), 39–50; and Michael G. Wechsler, “Two Para-biblical Novellae from Qumran Cave 4: A Reevaluation of 4Q550,” *DSD* 7 (2000): 130–72.

5. Adele Berlin, *Esther* (Philadelphia: Jewish Publication Society, 2001).

6. Adam Silverstein, “The Book of Esther and the ‘Enuma Elish,’” *BSOAS* 26 (2006): 209–23.

influence of Naqia in the courts of Sennacherib and Esarhaddon and, of particular interest to readers of this review, Judean connections with the Neo-Assyrian court.

The topic of these connections is one that Dalley raised in an article geared particularly at biblicists and published in *JSOT*.<sup>7</sup> Her argument hinges on the identification of two women found in royal burials in Nimrud, whom Dalley believes to have been Judeans on the basis of their names, Yaba (possibly יָבָהּ) and Atalya (16–17).<sup>8</sup> Since the former is identified as the chief consort of Tiglath-pileser III and the latter is the chief consort of Sargon, there is a virtual dynasty of Judean chief consorts in the Assyrian royal court. If true, this casts a number of episodes and details in a different light. Dalley explores the implications for understanding Hezekiah's revolt in 705 and Sennacherib's response to it, for example. She also claims (85–86) that when Ahaz refers to himself as “your son” in speaking to Tiglath-pileser, this is justified by the presumed familial relationship. All other exegetes have always taken this as a metaphor, but Dalley does not pursue the point. Elsewhere she suggests that Hezekiah's attack on Gaza (2 Kgs 18:8) was not in preparation for 701 but *on behalf of Assyria*, since Hezekiah may have been acting as the Assyrian tax collector in the region (88–90); this is also said to explain Hezekiah's massive wealth.<sup>9</sup>

In this first half of the book the novel interpretations and reconstructions derive from intimate knowledge of the textual sources available to the historian, as Dalley is primarily an Assyriologist. A few examples will suffice. On 38–39 she questions Parpola's interpretation of ABL 1091,<sup>10</sup> which has gained widespread acceptance. Parpola took this letter as evidence of a plot spearheaded by Arda-Mulišši, Sennacherib's son, to kill Sennacherib and take the throne while the appointed crown prince, Esarhaddon, was away and unable to act. As Parpola notes, this matches fairly well the fragmentary biblical notice of these events found in 2 Kgs 19:36–37 = Isa 37:37–38. Dalley argues that the letter is not reliably restored and anyway is undated and that biblical text is not reliable. She rejects Parpola's reconstruction on the basis of the oddity of the assassin himself

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7. Stephanie Dalley, “Recent Evidence from Assyrian Sources for Judaeen History from Uzziah to Manasseh,” *JSOT* 28 (2004): 387–401; for the women, see 394–97.

8. This was discussed in more detail in the *JSOT* article on pp. 394–97 and earlier in Dalley, “Yabâ, Atalya and the Foreign Policy of Late Assyrian Kings,” *SAAB* 12 (1998): 83–98. See also the doubts expressed by K. Lawson Younger, “YHWH at Ashkelon and Calah? YHWistic names in Neo-Assyrian,” *VT* 52 (2002): 216–18.

9. A very different explanation for the wealth is proposed by John S. Holladay Jr., “Hezekiah's Tribute, Long-Distance Trade, and the Wealth of Nations ca. 1000–600 BC: A New Perspective,” in *Confronting the Past: Archaeological and Historical Essays on Ancient Israel in Honor of William G. Dever* (ed. Seymour Gitin, J. Edward Wright, and J. P. Dessel; Winona Lake, Ind.: Eisenbrauns, 2006), 309–31.

10. Simo Parpola, “The Murderer of Sennacherib,” in *Death in Mesopotamia: Papers Read at the XXVIe Rencontre assyriologique internationale* (ed. Bendt Alster; Mesopotamia 8; Copenhagen: Akademisk, 1980), 171–82.

fleeing rather than benefiting from the murder (although Parpola had, of course, suggested a way of explaining this). On Dalley's reading of the events (told on 37–46), the murder may actually have been orchestrated by Naqia, mother of Sennacherib, in order to get her son on the throne; the biblical record would be an unquestioning repetition of the “official” version of events promulgated by Esarhaddon, which blamed Arda-Mulišši for the assassination.<sup>11</sup> Troublingly, there is no evidence for this reconstruction, not *even* a fragmentary letter; it rests entirely on the assumption (reasonable as far as it goes) that all texts are polemical and have to be read against the grain in order to make them yield reliable information. The question for the historian is whether this is good enough evidence.

Three final examples relate to the well-trod events of 701 in Judea; even here Dalley is able to contribute new perspectives. First, a minor detail: the Rabshakeh's Hebrew eloquence led to Hayim Tadmor's theory that the Rabshakeh may have been an Israelite exile, risen through the Assyrian ranks over the preceding two decades. Dalley, having developed her theory of the Judean origin of the Assyrian royal women Yaba-Banitu and Atalya, suggests simply that Hebrew was spoken at the court because of their presence (92).<sup>12</sup>

Second, regarding a stage in the events leading up to Sennacherib's arrival in Judah: Dalley suggests (90) that Padi of Ekron had *fled* to Jerusalem for refuge—as opposed to the conventional wisdom, which holds that he was forcibly removed by Hezekiah and held in Jerusalem, perhaps for ransom. On Dalley's view, Padi was an anti-Assyrian activist, but this seems very unlikely in light of the fact that Sennacherib proudly reports that he freed Padi from his imprisonment in Jerusalem and restored him to the throne in Ekron. Padi was most likely a “pro-Assyrian” holdout who suffered for his political views at the hands of the anti-Assyrian Hezekiah.

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11. It should be noted that even here there is enough to trouble the historian with regard to methodology. Dalley's awareness of the potentially misleading nature of texts is admirable. Additionally, few would argue that the biblical record should uncritically be trusted as a historical source. But to discount the biblical narrative, which almost certainly dates to within a century of the events recounted (and according to Dalley—who seems to date the account to Manasseh's reign—within half a century at the most), and then to rely in part on a tale told by Diodorus Siculus from 600 years later, without any comment on this eclecticism, seems methodologically questionable, and the reader wishes the author at least revealed a self-awareness regarding her reliance and nonreliance on various sources.

12. It would be more comforting if Dalley showed awareness of Tadmor's suggestion (which comes along with important observations about the usual role of the Rabshakeh), but there is no mention of it. This is a running problem throughout the book: one is never sure if Dalley is conversant with earlier literature and simply setting it aside or is working in an intellectual vacuum without awareness of competing theories.

Finally, we arrive at the climax of the story. Regarding the events at Jerusalem, Dalley argues (93) that (1) Sennacherib never laid siege to Jerusalem and (2) never intended to. Her evidence for the first claim is that in his inscriptions Sennacherib never compensates for the failure to capture the city by describing how he cut down its trees, which she says is the standard literary retribution for besieged cities that stubbornly refused to fall. It should be added that, despite the conventional wisdom, there is actually very little reason to think there was a siege of Jerusalem in 701. From the biblical account alone one would assume that Sennacherib never got to Jerusalem: he is last mentioned as fighting against Libnah in the Shephelah and then is struck by the famous plague and leaves for home.<sup>13</sup> In his own inscriptions, too, Sennacherib never claims to have even attacked Jerusalem. The only claim that may have given rise to such a belief is the claim that he shut Hezekiah up in his royal city like a bird in a cage—but since his royal city was precisely where Hezekiah wanted to be, this would have taken no more than a few soldiers posted at a lookout point on a hill near Jerusalem. In sum, Sennacherib probably never did besiege Jerusalem, and Dalley is probably right on this point. She further suggests that the mistaken claim of the existence of an Assyrian camp at Jerusalem, which goes back to Josephus, is due to the confusion of the Assyrians with Nebuchadnezzar's Babylonians (93; see also her *JSOT* article, 392–93).

Claim 2, that Sennacherib never *intended* to lay siege to Jerusalem, is more difficult, however: it is difficult to imagine what other end to the story he had in mind when he set out on his journey.

The real problems in the book relate more to the second, shorter part, however (chs. 7–9, 165–226). This is the core thesis of the book: the real-life military events of 646, when Assyria (protected by Ishtar), accompanied by Babylon (protected by Marduk), attacked and destroyed Susa (protected ineffectively by Humban), were mythologized and told as a story of Ishtar and Marduk defeating Humdan. This later became demythologized and Judaized and told as a story of Esther (< Ishtar) and Mordecai (< Marduk) defeating Haman (< Humban) in Susa.

Dalley makes some important observations about the story of Esther; probably most important and challenging is the specifically *Assyrian*, rather than Babylonian, nature of story (esp. 167), as seen, for example, in the use of the Assyrian word *pûr* rather than

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13. See also Arie van der Kooij, "The Story of Hezekiah and Sennacherib (2 Kings 18–19): A Sample of Ancient Historiography," in *Past, Present, Future: the Deuteronomistic History and the Prophets* (ed. Johannes C. de Moor and Harry F. van Rooy; Leiden: Brill, 2000), 107–19, and earlier in "Des assyrische Heer vor den Mauern Jerusalem im Jahr 701 v. Chr.," *ZDPV* 102 (1986): 93–109.

Babylonian *isqu*.<sup>14</sup> The lexical evidence collected there is impressive enough that an argument for an Assyrian origin seems reasonable. The tricky part is to connect the dots between an Assyrian story and the biblical book of Esther; Dalley's attempt is unconvincing. She admits that she knows of no other case where a myth morphed into a story of human actors. This is an important point, because this is the basic thesis of the book, but it is unsustainable in its current form.

The details are messy as well. She suggests (back on 102) that the Septuagint version of Esther, which differs in important respects from the MT, derives from versions of the Esther story told by soldiers in Elephantine in the sixth–fifth centuries, rather than being a revision of the MT, as is commonly believed.<sup>15</sup> There is no plausible story to tell that can explain the transmission of the story from fifth-century Elephantine to second-century Alexandria, and in any event the LXX version is set (like the MT) in the Persian court of Ahasuerus. At one point she writes: “A variant for Nadin gives the name Haman who is better known as the villain of the Esther story.” There is no reference given and no indication of where this variant may be found or even what language it is in. If it is in Akkadian, the alleged variant could only be Ḫaman, and irrelevant.<sup>16</sup>

Also very troubling is the use made of what are conventionally called postbiblical sources. Dalley apparently believes that all later tellings of the Esther story are not retellings or interpretations of the MT but reflect lost older versions. Targum Sheni compares Mordecai to Venus, “presumably translating a Hebrew text no longer extant” (144), for

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14. In this part, too, however, there are enough problems to drive the philologist mad. שרביט is indeed non-Persian (171; did anyone ever claim otherwise?), but it is most obviously West Semitic rather than Akkadian. Whatever the pre-history of the Aramaic word כלילא (and I would not rule out the possibility of it being a loanword from Akkadian), it was well-entrenched in Aramaic by the time of the Targum, and no pun on an epithet of Ishtar is necessary (or plausible; see 172). It is true that the *-ūt* ending for abstract nouns becomes more popular in LBH (175), but this has long been noted without appealing to direct loans from Akkadian (Avi Hurvitz, *ללשון לשון בין*, [Jerusalem: Bialik, 1972], 79–82). Contra Dalley, it is not at all strange that the word זקיפא is *not* used in the Hebrew texts (180) and that instead the phrase העץ על תלה is used. On this she hangs the suggestion that the phrase was chosen because עץ = giš and תלה (*tālāh*, contra Dalley's transliteration *tall*) are to be read as Akkadian, *gištallu* “a crossbar or cross-beam above a doorway.”

15. In this she is partially following Karen H. Jobes, *The Alpha-Text of Esther: Its Character and Relationship to the Masoretic Text* (SBLDS 153; Atlanta: Scholars Press, 1996), 228–31 (whom she cites).

16. Typographical annoyance (lack of diacritics) becomes a substantive issue on 169, when Dalley suggests that “a word almost identical to *hadassah* is Akkadian *hadaššatu*.” This is almost identical in English transcription, but since the Hebrew /s/ is not the same as Akkadian /š/ and (as she later notes) Hebrew /h/ is not the same as Akkadian /ḫ/, these words actually share only the /d/ among their consonants. For a typological comparison between Nadin and Haman, see Shemaryahu Talmon, “‘Wisdom’ in the Book of Esther,” *VT* 13 (1963): 419–55.

example.<sup>17</sup> The fact that Mordecai is named as uncle of Esther (as opposed to cousin, as in the MT) in Josephus and Targum Rishon shows flexibility in the *original* storytelling (114). The Targumim “incorporate pagan material which was later filtered out of the Hebrew version that achieved orthodoxy” (211).<sup>18</sup>

The use of rabbinic sources is also problematic. She takes a reference to reading the megillah in “Eilamean” (for which reference [b. Meg. 18a] she thanks Adam Silverstein) as evidence of an early pre-MT version of the story in Elamite still known in the third century (183). The reference (which is mentioned in the hypothetical case of reading in עילמית לעילמים “Elamite to Elamites,” alongside reading in Coptic for Copts, Greek for Greeks, and so on) is actually to a *translation of the MT Esther into Elamite* (or any other language), and there is no way the rabbis would have allowed anyone to fulfill the obligation to read the story by reading an proto-Masoretic version had they known of one.

Most surprisingly, she develops a theory (149–54) about a practice of eating the “ears of Ishtar,” which she claims was transferred to “ears of Haman,” derived from Mesopotamian practices of eating deities to benefit from them. There are numerous problems here; clearly the practice of eating hamantaschen is not meant to benefit from Haman, and the practice is usually dated to early *medieval* Ashkenazic culture.<sup>19</sup> Later she specifies that hamantaschen resemble pubic triangles with moist contents, so their consumption may have originally represented the consumption of a female—later transferred to the consumption of the vanquished (male) Haman.

The discussion of biblical passages, in Esther and elsewhere, is likewise frustrating. To take one example, Dalley writes (191), “In the Hebrew story, Esther was deported from Jerusalem in the time of Nebuchadnezzar II, around 586 BC.” That Esther was deported then is certainly never said, and even that Mordecai is said to have been deported by Nebuchadnezzar is not clear: it depends on the syntactic ambiguity of Esth 2:8, namely, who the head of the relative clause אשר הגלה מירושלם is, Mordecai or Kish. The latter

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17. See also 192, where Dalley writes that “an Aramaic version of the book names Vashti as the daughter of Amil-Marduk.” The reference is to the Targum. She queries: “What kind of story is it that shifts from one dynasty to another, from one king to a much later one?” For an answer, see Geoffrey Herman, “Ahasuerus, the Former Stable-Master of Belshazzar, and the Wicked Alexander of Macedon: Two Parallels between the Babylonian Talmud and Persian Sources,” *AJSRev* 29 (2005): 283–97.

18. But as she notes later, a planetary interpretation is found in the thirteenth-century Bahya ben Asher. Does he also preserve pre-MT materials?

19. The derivation of the word hamantaschen as a folk etymology of Mohntaschen “mon pockets” supports an origin (at least of the name and the current form) in Ashkenaz. The reference to the “ears” of Haman apparently derives from modern Hebrew terminology; hamantaschen are called אזני המן in modern Hebrew. I do not know when this began.

possibility is grammatically defensible (compare, e.g., 2 Sam 4:8, הנה־ראש איש־בשת (בן־שאול איבך אשר בקש את־נפשך) and avoids an obvious historical blunder, so should be preferred.<sup>20</sup>

One could quibble also with the use she makes of the Purim panel in the Dura Europos synagogue (203, 224–25)—for example, the figure she identifies as “Mordecai seated on a throne like a god” is almost certainly Ahasuerus; Mordecai is to the left, on a horse—but there is little to be gained by this at this point.

To reiterate the point made at the beginning: despite its many flaws, this is a doubly important book. It should be required reading for anyone interested in Assyrian and Israelite history during the Neo-Assyrian period. It is also recommended for its provocative theories about the prehistory of Esther, which, although I cannot accept them in their current form, will no doubt move us a step forward in the search for the complex origins of Esther. The book is sure to make the reader’s brain race and blood boil, but this reader, at least, feels enriched for having gone through that.

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20. Another example is her discussion of Isa 14, which according to Dalley preserves a deeply emotional lament over the death of Sargon, who, after all, was married to a Judean: “Particularly closely affected by the disaster was the kingdom of Judah where the prophet Isaiah wrote lamenting the death of Sargon in deeply emotional words” (20). But the chapter is clearly and harshly critical of Sargon; she cites Marvin A. Sweeney, *Isaiah 1–39, with an Introduction to Prophetic Literature* (Grand Rapids: Eerdmans, 1996), but he entitles the section a “taunt song” (224–25, 228, 229, 237, 238, etc.), as do the other sources cited by Dalley, e.g., H. L. Ginsberg, “Reflexes of Sargon in Isaiah After 715 B.C.E.,” *JAOS* 88 (1968): 50.