A Talmud in Exile

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Introduction

I

The Question

As a matter of both historical fact and rabbinic tradition, the Land of Israel was the birthplace of Rabbinic Judaism and the scene of most of its literary production between the second and seventh centuries CE. Rabbinic tradition points to Yavneh as R. Yohanan b. Zakkai’s chosen site for the reconstitution of Torah learning after the destruction of 70. The scholars of the Mishnah, Tosefta, and so-called tannaitic midrashim (the tannaim, who flourished between 70 and 220 CE) were overwhelmingly, although not exclusively, Palestinian,¹ and most of the classic rabbinic compilations—those already noted as well as the Talmud of the Land of Israel (Yerushalmi), Genesis Rabbah, Leviticus Rabbah, and Pesiqta de-Rav Kahana—were produced in Palestine.

But that is not the entire story. Further to the east, the rabbinic enterprise took root in Babylonia. Although the Babylonian contribution to the tannaitic enterprise was small, the productive amoraic period (the post-tannaitic period of scholars known as amoraim; sing. amora) in Babylonia began with the third-century arrival there of the Mishnah (ca. 220 CE). Rabbinic tradition credits Rav, said to be a student of the Patriarch R. Yehudah ha-Nasi, with the introduction of the Mishnah to Babylonia. Rav, together with his contemporary Shmuel, were the pivotal figures of the first of seven generations of Babylonian amoraim.

¹. Aside from the famous and myth-shrouded example of Hillel, who allegedly came to Palestine from Babylonia, tannaitic sources mention some others, notably Nathan “the Babylonian,” R. Yehudah b. Beteira (of Nisibis), Mattyah ben Heresh (of Rome) and Nahum “the Mede.”
The amoraic periods in Palestine and Babylonia were not of equal duration. The Palestinian amoraic period ended in approximately 360–370 CE, while that of Babylonia ended in approximately 500–501. The Talmud Yerushalmi came to a close shortly after the end of the Palestinian amoraic period, around 400, while the final editing of the Babylonian Talmud (Bavli) most likely occurred in the seventh century.

Both Talmuds represent that scholarly exchanges took place between the Palestinian and Babylonian rabbinic communities throughout their shared portion of the amoraic period. Babylonian scholars are quoted by Epstein and Sussman in their works on the date of the Yerushalmi.

2. On the basis of internal evidence in the Yerushalmi, Jacob N. Epstein asserted that the Talmud Yerushalmi was “sealed” during the years 410–420 CE, during the time of R. Ashi (the sixth generation of Babylonian amoraim) in Babylonia. Yaacov Sussman revisited the issue of the Yerushalmi’s dating in “Ve-shuv le-Yerushalmi Neziqin,” and concluded that the Yerushalmi was completed with the close of its amoraic period during the period 360–370. Sussman explicitly rejected Epstein’s calculation of the duration of the amoraic period in Palestine, arguing that the five Palestinian amoraic generations were actually much more compressed in time than Epstein had allowed. See J. N. Epstein, Introduction to Amoraitic Literature: Babylonian Talmud and Yerushalmi (Tel-Aviv: Dvir, 1962), 273–276 (Heb.); Yaacov Sussman, “Ve-shuv le-Yerushalmi Neziqin,” in Mehqerei Talmud: Talmudic Studies (ed. Yaacov Sussman and David Rosenthal; Jerusalem: Magnes, 1990), 1:132n187.


4. Both Epstein and Sussman implicitly assumed that the close of the Palestinian amoraic period was also the close of the Yerushalmi itself. But for this to be so, then the (or at least some) late Palestinian amoraim must have functioned as editors/redactors as well as amoraim. Neither Epstein nor Sussman provide any evidence of this. Second, as Baruch Bokser noted earlier in his study of y. Pesahim, the Yerushalmi’s sequences of topics and sugyot are clearly the products of a post-amoraic hand; no individual amora is aware of the larger context in which he is now found. This observation suggests that although the Palestinian amoraic period may well have ended in 360–370, there was a post-amoraic period of Talmud formation in Palestine. Thus, it remains reasonable to posit an early fifth-century date for the completed Yerushalmi. See Baruch Bokser, Yerushalmi Pesachim (ed. Lawrence Schiffman; vol. 13 of The Talmud of the Land of Israel: A Preliminary Translation and Explanation, ed. Jacob Neusner; Chicago: University of Chicago Press, 1994).

5. Although both Talmuds utilize the teachings of amoraim in both rabbinic centers, neither draws on the same set of amoraim; moreover, the Bavli includes the teachings of Palestinian post-amoraic scholars who lived and worked during the career of R. Ashi (d. 427 CE), after the completion of the Yerushalmi. Among the early Palestinian amoraim mentioned only in the Yerushalmi are R. Abba b. Tablai (e.g., y. AZ 1:1, 39b) and R. Shimon b. R. Yannai (y. Shab 13:7, 14b; y. MQ 2:2, 81a). Among the early Palestinian amoraim mentioned only in the Bavli are R. Yoshia...
name in the Yerushalmi, which also contains Babylonian sugyot (sing. sugya). Palestinian amoraim are ubiquitous in the Bavli, as are Palestinian sugyot and clusters of sugyot. Most of the aggadah (non-legal material) in the Bavli is of Palestinian provenance, despite the clear evidence of its having been reworked by later Babylonian editors. Indeed, the remarkable abundance of Palestinian materials in what is, after all, the Babylonian Talmud prompted one medieval commentator to declare that “most of the Babylonian Talmud is from them (i.e., Palestinian scholars such as R. Yohanan and Resh Lakish).” The ubiquity of Palestinian sources in the Bavli and the interactions between the rabbinic communities during the amoraic period are a given. This being so, is it possible that the redactors of a particular Bavli tractate relied on the earlier parallel Yerushalmi tractate, or not?

II

Factors Complicating Resolution of the Question

This question is deceptively simple and the obstacles to arriving at an answer rather complex. Both Talmuds are composed of sources from different time periods that underwent a lengthy process of transmission and reworking. While we can be fairly certain that many, or even most, sugyot are not identical to what they looked like when first formulated, we cannot always be sure we can precisely reconstruct their textual history—


8. R. Yonatan, quoted in Shittah Mequbetset to b. BM 65a.
contra the self-confidence of some earlier source criticism practitioners. While we know—because we have Talmudic tractates—that sugyot were linked to each other and that tractates of “Talmud” eventually emerged, our knowledge about the process by which sugyot, or even chains of sugyot, became a Talmud tractate is only partial. This undercuts the confidence of the redaction critics. Not only is our understanding of the formation of the Bavli partial, but there seems to be evidence that supports different, even conflicting, theories.

Concerns such as these have led some scholars either to abandon research into the formation of the Bavli entirely, or to focus research on the redacted Bavli alone, eschewing the older critical methods. Limiting research on the Bavli to the level of its final redaction is reminiscent of the recent literary turn to the study of the poetics and rhetoric of biblical narrative in biblical studies. But as Christine Hayes has pointed out, the Bible gives unmistakable evidence of wishing to be read synchronically, at the level of its final redaction. The Bible attempts to smooth over its prior sources by various narrative devices. The Bavli, on the other hand, gives unmistakable evidence of the opposite tendency: it wishes to be read diachronically, by constantly pointing the student to its diverse sources—diverse in both provenance and time period. The Bavli attributes source materials to various scholars in different amoraic generations, it calls attention to its citation of tannaitic or amoraic sources, and it provides multiple versions of individual traditions or sugyot. The Bavli’s call for attention to the diversity of its sources has caught the eye of recent critical Talmudic scholarship, which shows us that source and redaction criticism can still tell us a great deal about the Bavli and the scholars responsible for it. While we can never know for certain what happened between the

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12. This is not to say that the Bavli redactors never touch earlier source materials at all. Indeed, as we will see throughout this book they do rework them, sometimes extensively. But the point is that even in reworking prior sources, the Bavli redactors do not completely smooth over these sources’ diversity.

completion of the Yerushalmi and the redaction of the Bavli, critical scholarship gives us a justified confidence that judicious use of the appropriate critical methods, attention to detail as well as to the form and organization of whole tractates, and attentiveness to the non-literary remains of Late Antique Jewish culture will (as we will see in the course of this book) change our perception of the development of the Bavli.14

In preparing to answer the question of the relationship between the Talmuds, then, we must begin by distinguishing between different types of identifiable Palestinian materials in the Bavli: discrete traditions attributed to particular amoraim (called memrot), sugyot, clusters of two or more sugyot, aggadah, and any macro-level orderings of sugyot and topics attached to each mishnah (pl. mishnayot) that are common to the two Talmuds. It is this last category that is of the most significance as we probe the relationship of a Bavli tractate to its Yerushalmi parallel. Distinguishing between these categories of Palestinian materials is important because a satisfactory explanation of why one particular type of Palestinian source made its way into the Bavli is not necessarily an adequate explanation of another, let alone of all the others. As an example, the Bavli itself draws attention to the amoraim known as the nahote (נַהוֹטֵי, “those who descended”) who traveled between Palestine and Babylonia carrying rabbinic traditions.15 Earlier scholars who examined the nahote, especially Isaac Halevy and Ze’ev Wolf Jawitz, assumed that the nahote were responsible for the large Palestinian content of the Bavli. But the Talmuds’ portrayals of their activity do not permit us to infer that the nahote were responsible for all the Palestinian sugyot, clusters of sugyot, and Palestinian aggadah.

14. I hasten to point out that this book will focus on one Bavli tractate (Avodah Zarah) and its Yerushalmi parallel, and that the book’s conclusions are limited to that tractate alone. Further research is needed to determine whether or not this book’s conclusions are generalizable to the Bavli as a whole.

15. The principal nahote are R. Dimi, R. Yitshaq b. Yosef, R. Shmuel b. Yehudah, Rabin, and Ulla. The nahote are mentioned as a group at b. Suk 43b, b. Hul 101b and 124a, and b. Nid 10b and 39b, although in some of these places we find the variant reading “nahote yama” (“those who descend to the sea,” meaning “sailors”). Interestingly, at b. Ber 38b, Ulla is referred to as “raboteinu ha-yordim me-erets Yisra’el,” or “our master(s) who descended from the land of Israel.” There is as yet no comprehensive study of the nahote. Such a study is a desideratum that would help us understand the diachronic dimensions of the penetration of Palestinian learning into Babylonia.

16. Earlier scholars, working from the evidence of the nahote’s activity in the Bavli as well as from R. Sherira Gaon’s reference to them in his famous Iggeret, assumed that the nahote were the principal sources for the arrival of Palestinian...
we see in the Bavli. The Talmudic evidence certainly does not at all permit
the inference that the nahote were responsible for the macro-level ordering
of sugyot and topics attached to each mishnah by the Bavli. An examina-
tion of all the materials attributed to R. Dimi, the nahota who looms largest
in the Bavli, enables us to paint the portrait of a scholar whose primary
role was carrying individual legal traditions.17 Sometimes he conveyed
stories, and sometimes whole sugyot or discrete aggadic traditions,18 al-
though the transmission of these types of learning is clearly portrayed as
being of secondary importance in his work. Moving beyond R. Dimi, the
handful of references to the nahote as a group (in which the Bavli refers to
“Rabin and all the nahote”—or “nahote yama”) also portrays this anony-
umous collectivity as transmitting discrete (legal) traditions—not the col-
lected legal wisdom of the Palestinian rabbinic community. Further, the
hypothesis that the nahote were responsible for the massive presence of
Palestinian materials in the Bavli cannot explain an interesting pattern in
the Palestinian materials in the Bavli. The fourth Babylonian amoraic gen-
eration is one in which the influence of Palestinian halakhah, literary
forms, and/or terminology is more pronounced than in others.19 If nahote

17. This aspect of R. Dimi’s activity looms largest, so I will only provide a few
examples from each tractate in which he is mentioned. See b. Ber 6b, 44b; b. Shab
52a, 134b; b. Eruv 3b, 77a, 87a; b. Pes 110b; b. Yoma 55b, 88a; b. Suk 10a, 11b; b. Hag
15b; b. MQ 13b; b. Yev 78a, 84b; b. Ket 17a, 100a; b. Ned 40a; b. Sot 43b (three occurrences);
b. Git 59a; b. Qid 75a; b. BQ 76a; b. BM 105b; b. BB 73b, 80b; b. Sanh 7b, 57a,
69a; b. Shevu 20b; b. AZ 11b, 27a, 47a, 70b; b. Zev 20a, 115a; b. Men 26b, 55a; b. Hul
53a (two occurrences); b. Bekh 8a; b. Tem 12b–13a; b. Ker 25b; b. Nid 25a.
18. Stories: b. Ber 44a; b. Shab 13b (R. Dimi comments on a story, which implies
that he knows it); b. Shab 50a, 74a, 125b, 147a; b. Eruv 86b; b. Suk 16b; b. Yev 59b;
b. Qid 31a; and b. AZ 8b, 35b.
Sugyot: b. Shab 76a; b. MQ 10a; b. Ket 34b–35a, 57a, 104b, 107b; b. BB 27b, 129a,
152b; b. Sanh 70a; b. Zev 10a; b. Men 71b; b. Hul 103b, 134a; b. Meil 21b. A particularly
interesting subset of these sugyot are those in which R. Dimi is represented as
engaging with Abaye, and those in which Abaye and Rava use R. Dimi’s sugyot as
the basis for their own argumentation. Examples such as these require further re-
search; could it be that Palestinian learning such as that of R. Dimi—introduced
during the fourth Babylonian amoraic generation—helps account for the “spike”
in Palestinian influence that we see in that generation?
Aggadic traditions: b. Ber 31b; b. Hag 14a; b. Ket 105b, 111b; b. BB 74b–75a, 79a;
b. Sanh 100a, 108a; b. AZ 8b, 35a; and b. Zev 118b.
19. See Zwi Moshe Dor, The Teachings of Eretz Israel in Babylon (Tel-Aviv: Dvir,
1971) (Heb.); Richard Kalmin, Sages, Stories, Authors and Editors. Unfortunately,
had been moving back and forth between the rabbinic centers throughout the amoraic period, why is it that Palestinian learning is particularly prominent then?

When we extricate ourselves from the nahote hypothesis and look at the Talmuds as whole compilations, we see that despite the noticeable and unquestionable differences between them, the Talmuds do indeed seem sufficiently alike for us to raise the question: were the redactors responsible for the later Talmud aware of, and influenced by, the work of the redactors of the earlier Talmud? The two Talmuds are the only compilations of their genre (“talmud”) produced by the rabbis in Late Antiquity, and stand out as the only compilations produced during this period arranged in the form of sustained commentaries on the Mishnah. The Talmuds utilize similar terminology and share structures of argumentation. The

despite David Kraemer’s fine analyses of the fourth generation’s noticeably greater tendency to preserve argumentation than earlier or later amoraic generations, he did not investigate the possibility of a connection between this tendency and the Palestinian influence on that generation. See David Kraemer, “Stylistic Characteristics of Amoraic Literature” (Ph.D. diss., Jewish Theological Seminary, 1984); idem, The Mind of the Talmud (New York: Oxford University Press, 1991); and the review essay by Yaakov Elman, “Argument for the Sake of Heaven: The Mind of the Talmud.” JQR 84:2–3 (1993–1994): 261–282. Elman’s fine essay calls attention to key lacunae in Kraemer’s arguments, but does not note his failure to consider the Palestinian connection.

20. The differences between the Talmuds have been, and continue to be, extensively studied. For some of the more recent studies of the differences in their respective rhetorics and argumentation, see Jacob Neusner, Judaism: The Classical Statement (Chicago: University of Chicago Press, 1986); idem, The Bavli and its Sources: The Question of Tradition in the Case of Tractate Sukkah (Atlanta: Scholars Press, 1987); idem, The Bavli’s Unique Voice: A Systematic Comparison of the Talmud of Babylonia and the Talmud of the Land of Israel (7 vols.; Atlanta: Scholars Press, 1993); idem, Are the Talmuds Interchangeable? (Atlanta: Scholars Press, 1995); Jeffrey L. Rubenstein, Talmudic Stories: Narrative Art, Composition, and Culture. For a study of selected halakhic differences, see Christine Elizabeth Hayes, Between the Babylonian and Palestinian Talmuds. For a study of Babylonian/Palestinian cultural differences as reflected in their respective literatures (both the Yerushalmi and Palestinian sources preserved in the Bavli), see Richard Kalmin, Sage in Jewish Society.

21. For the notion of “talmud” as its own genre, see Jacob Neusner, The Bavli’s Unique Voice, 1:2; David Kraemer, Reading the Rabbis, 7–8.

22. On similar terminology, see Zechariah Fraenkel, Ma’avo ha-Yerushalmi (Breslau: n.p., 1870; repr., Jerusalem: n.p., 1967), 8–18, where Fraenkel explains a number of Yerushalmi terms, often giving their Bavli equivalents. An example Fraenkel did not discuss is the Yerushalmi’s niha (“it is well,” or colloquially, “it makes sense”) and the Bavli’s equivalent be-shelama. An example of shared structures is the common tserihah construction in the Bavli, whereby the Talmud explains the
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Talmuds share sugyot and have even been observed to order their parallel mishnah commentaries in structurally similar ways. Moreover the Talmuds—the only two of their genre—were not created by two rabbinic communities working alone, innocent of each others’ scholarly activities, but by communities with an historical, scholarly, and religious relationship that spanned over 150 years. The completed Talmuds themselves are separate by about two hundred years; certainly enough time for the Babylonian rabbinic community to have become aware of (at least part of) the Palestinian rabbis’ magnum opus. So, once again the question: can we find any evidence that the redacted Yerushalmi influenced the formation of the redacted Bavli?

This book answers, simply, “yes.” The argument of this book is that the redactors of tractate Avodah Zarah of the Babylonian Talmud were indeed aware of, and influenced by, elements of the structure and content of tractate Avodah Zarah of the Palestinian Talmud. They did not passively incorporate parts of the earlier Talmud, but selected, rejected, and reworked the portions they adopted in ways calculated to make them conform to Babylonian rabbinic linguistic, cultural, and religious norms.

III
Prior Research on the Relationship between the Talmuds

There is a large body of scholarship that bears in one way or another on the issue of the relationship between the Bavli and Yerushalmi. For heuristic purposes, we can categorize this scholarship under three headings: (1) scholars who argue that the redactors of the Bavli knew and relied upon the Yerushalmi and those who argue the diametrically opposing view; necessity of seemingly redundant clauses in a mishnah or baraita. This construction is also found in the Yerushalmi.


24. In chapter 6 we will examine some external evidence that buttresses this textually derived conclusion. Specifically, we will see that it is reasonable to assume that y. AZ was brought to Babylonia by a small coterie of Palestinian scholars who, in conformity with the precedent of the nahote and with the traveling habits of upper-class Romans, Christian clergy, and pagan philosophers, traveled with this learning to Babylonia in the sixth or seventh centuries.
(2) scholars who contend that what we see as the Palestinian contribution to the Bavli came to Babylonia incrementally throughout the amoraic period; and (3) scholars who study the edited Palestinian materials in the Bavli without pondering what, if any, implications their presence has for the redaction of the Bavli. All three bodies of scholarship are valuable sources of findings and methodological insights and I have drawn upon them all. But I will limit this survey to the scholars in groups (1) and (2), whose work has the most direct bearing on this book.25 And, since the scholars in group (2) are, in essence, proposing an alternative theory to the one offered in this book, I will subject their arguments to closer scrutiny than those of the scholars in group (1).

III.a. The Bavli Knew/Did Not Know the Yerushalmi

Although the current scholarly consensus is that the Bavli redactors did not know the Yerushalmi,26 a review of the research does reveal the existence of several contrary views and thus the potential for a reconsideration of the consensus. Although the validity of this book’s argument does not depend on the existence of such contrarians, they show that the issue is not as cut-and-dried as it has been made to appear.

A well-known statement by R. Isaac Alfasi (the “Rif”; Morocco and Spain, 1013–1103) at the end of his codification of tractate Eruvin has traditionally been used as the starting-point for discussion of whether or not the Bavli was aware of the Yerushalmi.27 There, the Rif writes:


26. For the most recent reiterations of this consensus, see Jeffrey L. Rubenstein, The Culture of the Babylonian Talmud (Baltimore: Johns Hopkins University Press, 2003), 159; Leib Moscovitz, “Designation is Significant: An Analysis of the Conceptual Sugya in bSan 47b–48b,” AJSR 27:2 (Nov 2003): 248n100. Moscovitz seems to leave the ultimate resolution of the question open.

27. See b. Eruv 35b (in the pages of the Rif). Louis Ginzberg and Leopold Greenwald both argued that the Rif was not actually claiming that the Bavli knew the Yerushalmi, and that his only concern was to argue that the Bavli was the halakhic batra of the Yerushalmi (and hence more halakhically reliable) in keeping with the geonic principle that hilkheta ke-batrai (the law follows the latest [scholars]).
And we have seen that a few rabbis hold like Ulla and rely on the gemara of the Westerners28 [the Yerushalmi] . . . but we do not hold thus, for since our discussion in our gemara [inclines toward] permissiveness, it does not matter to us what they prohibit in the gemara of the Westerners. For we rely on our gemara, for it is the later. And they [presumably the sages of “our” gemara] were more expert than we in the gemara of the Westerners. And were it not for the fact that they held that this statement [of prohibition] of the Westerners was not authoritative, they would not have permitted it to us.

At first glance, it appears as if the Rif is indeed saying that the Bavli knew the Yerushalmi.29 Yet the term “gemara” may not mean a finished Talmud.30 The Rif may thus actually be saying that the sages of “our” gemara (the Bavli) were more expert “than we” in the traditional (Chanoch Albeck) or concise and “apodictic” (David Halivni) learning of the Palestinian sages, a claim which is certainly credible and also certainly not


28. In the Bavli, the people residing in the Land of Israel are customarily referred to as the “Westerners,” since Palestine lies to the west of Iran and Iraq, the home of the Babylonian amoraim.

29. Interestingly, this statement of the Rif does not appear to have been cited by anyone prior to the nineteenth century, so we are at a loss to know how medieval scholars understood it. But see Rabbenu Yonatan, *Shittah Mequbetset* to b. BM 45: “for those latter scholars who arranged the Babylonian Talmud for us, brought us all of those rationales which are [of] legal [validity] in the Talmud Yerushalmi, and most of the Babylonian Talmud is from them [the Palestinian scholars of the Yerushalmi] such as the words of R. Yohanan and R. Shimon b. Laqish, and all those who are called by the name ‘Rabbai.’” An interesting variation of the Rif’s statement appears in a responsum attributed to R. Hai Gaon, found in Zvi Benjamin Auerbach’s edition of the medieval legal compendium *Sefer ha-Eshkol* (R. Abraham of Narbonne, 1110–1179). We shall discuss that responsum later in the Appendix to chapter 6. All we need note now is that the attribution of that responsum to R. Hai is doubtful, and it remains unclear who wrote the responsum and what the writer’s agenda was.

30. See Chanoch Albeck, *Introduction to the Talmud, Babli and Yerushalmi* (Tel-Aviv: Dvir, 1969), 4 (Heb.) (“the word ‘gemara’ . . . means . . . that which was passed down and received from previous generations”). See also David Halivni, *Sources and Traditions: Erubin-Pesahim* (New York: Jewish Theological Seminary, 1982), 92–94 (Heb.) (“therefore it appears that ‘gemara’ has the meaning as well of an apodictic formulation”).
equivalent to the claim that the Bavli was aware of the Yerushalmi. Without a thorough analysis of the various terms by which the Rif refers to the Yerushalmi in his “Halakhot” and his understanding of “gemara,” it cannot be said with certainty that the Rif believed the redactors of the Bavli to have been aware of, and influenced by, the Yerushalmi. And even if we do adopt that understanding of his statement, the Rif’s obvious anti-Yerushalmi polemic undercuts the value of his statement as an historical source—even though, as Martin Jaffee has pointed out, a polemical statement need not be presumed false.31

The first major investigations of the relationship between the Talmuds date to the nineteenth century. Shlomo Yehudah Rappaport (known as “Shir,” 1790–1867) and Zvi Hirsh Chajes (1805–1855) conducted investigations into the relationship between the Talmuds that yielded the result that the Bavli was aware of the Yerushalmi. Rappaport compared the similar structures of the Talmuds’ commentaries to the same mishnah, while Chajes focused on cases that seemed to show a Bavli sugya picking up on a concluding point in the Yerushalmi parallel or supplying information missing from the Yerushalmi parallel.32 Zechariah Fraenkel (1801–1875) was familiar with the work of Rappaport and Chajes, and vigorously attacked some of their examples. First, he rejected Chajes’ argument that the Bavli’s incomplete presentation of a Palestinian memra more completely presented in the Yerushalmi is an intentional intertextual reference. Many Palestinian memrot are found in the Bavli, Fraenkel argued, and so all Chajes has shown is that a given Palestinian memra found in the Yerushalmi is also found in another form in the Bavli. This phenomenon is unremarkable and not dispositive of the larger issue of the relationship between the Talmuds. Turning his attention to Rappaport’s work, Fraenkel points out that in many cases “the order [of sugyot and topics attached to the same mishnah in both the Bavli and Yerushalmi] cannot be any other way”—meaning, presumably, that if a given mishnah contains issues A, B, and C, one should not be surprised that both Talmuds present discussions of the mishnah in the order A, B, C.

Like Rappaport and Chajes, Isaac Halevy (1847–1914) argued in favor of the Bavli’s awareness of the Yerushalmi. Taking a different tack, he purported to demonstrate that the anonymous, redactional voice of the Bavli

was aware of its anonymous Yerushalmi counterpart, and that it incorporated conclusions from Yerushalmi sugyot into the Bavli parallels.33

Louis Ginzberg (1873–1953) briefly treated the issue of the relationship between the Talmuds in his *A Commentary on the Palestinian Talmud.*34 He rejected the notion that the Rif’s statement in Eruvin means that the Bavli knew the Yerushalmi, and also criticized Isaac Halevy’s argumentation.35 Yet Ginzberg did not present his own case for the view he obviously preferred—that the Bavli did not know the Yerushalmi.36 In the 1950’s, the independent scholar Yequtiel Yehudah (Leopold) Greenwald reached a similar conclusion through his analysis of the many differences between the Talmuds as to attributions of amoraic statements and versions thereof, as well as of materials present in the one Talmud but missing from the other.37

Jacob Nahum Epstein’s (1878–1952) views on the relationship between the Talmuds were published after his death in the book *Introduction to Amoraitic Literature.*38 Epstein’s comments display a sense of the complexity of the issue not seen in his predecessors. He opened his discussion of the relationship between the Talmuds with a telling emphasis: “it is an old dispute as to whether the Bavli knew the Yerushalmi—*our* Yerushalmi—or did not know it.”39 After presenting the major geonic and rishonic viewpoints on the subject, Epstein concluded “but anyone who compares the Bavli with the Yerushalmi in even a cursory way will see immediately that the Bavli did not know *our* Talmud Yerushalmi, nor did the Yerushalmi know *our* Talmud Bavli” (emphasis in original).40 What the Bavli *did* know, however (according to Epstein), was “not our Talmud, but in many, many places [it was] a Talmud in another edition—the edition of another yeshiva in another place in the Land of Israel, or a Talmud of a generation prior or subsequent to the editing of our Yerushalmi. Everything is according to the dating of the Bavli sugya and according to its lay-

33. Isaac Halevy, *Dorot ha-Rishonim,* 8:128–130. We will take up the issue of the anonymous Bavli and its (possible) role in the appropriation of Palestinian learning in chapter 5.
35. Ibid., 1:87.
36. Some hint of what that case may have been may be found on page 87, where Ginzberg discusses the existence of material found in the one Talmud but not in the other, a phenomenon that could be made to support an argument that the Bavli did not know the Yerushalmi.
39. Ibid., 290.
40. Ibid., 291.
ers” (emphasis in original). Epstein thus believed that the Bavli did not know “our” Yerushalmi, but did not entirely rule out that it did know “a” Yerushalmi. Ultimately, Epstein suggested that for those wishing to understand “the relationship between the Bavli and the Yerushalmi” (emphasis in the original), the best course is to compare statements cited in the Bavli with the term ma’arava (the “West,” meaning Palestine) with their parallels in the Yerushalmi, and to compare Babylonian statements introduced by taman (“there,” which for the Yerushalmi is Babylonia) in the Yerushalmi with their parallels in the Bavli.

In the 1960’s, M. A. Tennenblatt unequivocally concluded that “the Talmud of the Land of Israel’ or the ‘gemara of the Westerners’ or the ‘Yerushalmi’ as it was named afterwards as an edited work, was certainly known to those in Babylonia and even if [it was] not in the form [in which it is now] before us, then at least [the Babylonians had it as] scrolls [containing] sugyot or chapters, and certainly whole tractates.” Tennenblatt asserted even more forcefully a little later that “it makes sense that a fully edited ‘Yerushalmi’ arrived in Babylonia already at the end of R. Ashi’s life.” Tennenblatt provided the following rationale for this conclusion:

For it cannot be imagined that their [the Babylonians’] Talmud was based solely on what [traditions] the “travelers” brought to them or on the responses [to legal queries] that they received from the Land of Israel . . . it is more correct—and also easier—to build a Babylonian structure that completes what is missing [from the Palestinian] in time and in place . . .

Of course, such a statement—logical though it is—requires proof, which Tennenblatt rather incompletely provided. Of most interest in the context of this book are the proofs he draws from y. and b. Avodah Zarah. In the text, he presents a comparison of y. AZ 3:2, 41d and b. AZ 41b–42b. Tennenblatt notes that both Talmuds present the same R. Yohanan/Resh Laqish dispute in connection with the same mishnah, and also points out the Palestinian provenance of many of the materials the Bavli uses to turn the original, rather simple dispute into a complex sugya. In a footnote to this discussion, he points as well to a similarity in the selection and sequence of topics between y. AZ 2:3, 41a and b. AZ 30a.

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41. Ibid., 292.
42. Ibid.
43. M. A. Tennenblatt, Peraqim hadashim le-toldot Erets Yisra’el u-Bavel bi-tequfat ha-Talmud (Tel-Aviv: Dvir, 1966), 224.
44. Ibid., 240.
45. Ibid., 224.
46. Ibid., 263–270.
47. Ibid., 267n56.
Martin Jaffee’s pioneering comparison of y. and b. Horayot was a major turning-point in the scholarship on our question. He proposed to “argue that the appropriation of the Palestinian Talmud by the Bavli’s editors becomes clear, not at the level of individual textual parallels, but rather at the level of literary craft and organization, as large sequences of discourse are redacted in each gemara around the core of the same Mishnaic tractate.” Jaffee thus proposed—similar to S. Y. Rappaport in 1831 and M. A. Tennenblatt—to examine our question entirely from the macro perspective of comparing the Talmuds’ parallel commentaries on the same mishnah. His examination revealed three types of what he called “structural correspondences”: similar lengths of discussions of a given mishnah in both Talmuds, common placements of themes extraneous to the mishnah at similar points in the discussion, and, finally, use of the same mishnayot as opportunities for placement of aggadah. Jaffee ultimately concluded that “the post-Amoraic editors of the [Bavli] had something much like the extant version of the [Yerushalmi] before them and reflected upon the logic of its construction as they composed their own commentary.”

Jacob Neusner has devoted a great deal of attention to the relationship between the Talmuds, concluding that the Bavli does not at all know the Yerushalmi. Neusner’s work on the Talmuds is animated in part by a justified opposition to a traditional mode of study that views the entire vast corpus of rabbinic literature as representative of one uniform rabbinic point of view. He sets this view in opposition to his own “documentary hypothesis” according to which each rabbinic compilation (or “document”) exhibits particular and distinct characteristics that set it apart from the others. Neusner’s many studies of the Talmuds have led him to the view that the Bavli is completely independent of the Yerushalmi, notwithstanding some materials that the two can be observed to have in common. The Bavli has its own agenda, pursues its own rhetorical and topical program, uses Scripture as the basis of large units of discourse, and in general is not in any way dependent upon the Yerushalmi.
The key distinction between those scholars who do see a relationship between the Talmuds and those who do not is whether they are more engaged by the similarities between the Talmuds or by the differences between them. Rappaport, Chajes, Halevy, Tennenblatt, and Jaffee seemed to be drawn by a focus on similarity toward the acceptance of a possible relationship between the Talmuds, while Fraenkel, Ginzberg, Epstein, Greenwald, and Neusner focused on the differences between the Talmuds, and came to the opposite conclusion. This dichotomy informs the methodological approach of this book: we will pay close attention to the similarities between y. and b. Avodah Zarah and to the causes of the many differences between them. To the extent that these differences can be explained, the conclusion that b. Avodah Zarah knew and relied on y. Avodah Zarah is not undermined.

III.b. The Palestinian Contribution to the Bavli Was Made Incrementally Throughout the Amoraic Period: The Theorists of “Early Talmud”

Noah Aminoah, Yaacov Sussman, and Shamma Friedman point to the existence of a layer of “early talmud” (Aminoah and Friedman), “early arrangement” (Aminoah), or “early basic amoraic material” (Sussman) shared by both Talmuds.54 None of these scholars presents early talmud as a hypothesis which might explain some inter-Talmudic similarities; rather, each presents his analyses of sugyot as if early talmud is an established fact. In what follows, my goal is to evaluate early talmud as the hypothesis it is, assessing its utility as an explanation of inter-Talmudic similarities in discrete cases and its overall strengths and weaknesses. At appropriate points throughout the book, we will consider our findings in light of the early talmud hypothesis.

Let us begin with a relatively simple illustrative example.55 At b. Sanh 5b, the Bavli presents two sugyot in the same order in which they are at all unaware of the similarities between them. It is just that he does not subject the similarities to the same rigorous analysis to which he subjects the differences. Apropos of this point, Neusner is the only scholar of whom I am aware who raises the issue of whether the existing similarities between the Talmuds can be explained along the lines of a Q hypothesis analogous to that current in New Testament studies. We will return to this issue shortly.

54. Despite the differences in terminology between these scholars (and in approach), I will refer throughout this discussion to “early talmud.”

found at y. Shevi’1 6:1, 36a–b (¶ y. Git 1:2, 43c). Now, does this mean that the redactors of b. Sanhedrin knew y. Sheviit (and/or y. Gittin)? Friedman approvingly quotes Yisrael Levy’s 1870 review of Zechariah Fraenkel’s Mavo ha-Yerushalmi, in which he said, “There were already [in the amoraic period] complete sugyot taught in the Land of Israel which came from there to Babylonia—just as there were already sugyot taught in Babylonia prior to the compiling of the Bavli—[but] the edited Yerushalmi was not before the later Babylonian sages.”

Levy and Friedman correctly conclude that this example does not prove that the Yerushalmi was available to the Bavli redactors. But my agreement with their conclusion is based on a different point. This example should be studied together with all other Sheviit sugyot found in the Bavli. Only on the basis of such a macro-level study should any conclusions be drawn about the relationship between Sheviit and the Bavli—and even then, one should confine one’s conclusions to Sheviit and the Bavli and not claim to be opining about the relationship of the entire Yerushalmi to the Bavli. After such a study is done, it may indeed turn out to be the case that this example is part of a larger pattern of the Bavli’s incorporation of sequences of Sheviit sugyot, or it may not. But on the basis of this one example, it is unreasonable to conclude one way or the other about the relationship between the two Talmuds.

But there is more. In the Yerushalmi passage, R. Aha b. Yaaq ov in the name of R. Imi explicitly introduces the connection between the two sugyot by saying “From two cases [involving] Rabbi, we learn that Akko has characteristics of the Land of Israel and characteristics of the Diaspora.” When we examine the lengthy sequence of materials that follows this introduction, we do indeed find two stories about Rabbi and the status of Akko. There is therefore no question that these two juxtaposed stories likely circulated in the two rabbinic communities during the amoraic period. But these stories are separated by a good deal of material about the (im)propriety of a student’s issuing halakhic rulings in the vicinity of his master. Is this material also a part of what R. Aha b. Yaaqov is transmitting or not?

In order to answer this question, we must pay careful attention to the literary structure of the entire sequence. Immediately following R. Aha b. Yaaqov’s first story about Rabbi and Akko, R. Yaaqov b. Idi comments on the story “from that moment they decreed that no student should issue rulings.” R. Yaaqov b. Idi is thus aware of the first story, although it is unclear whether he knows it through R. Aha b. Yaaqov or not. (Alternatively, it is possible from a chronological standpoint for R. Aha b. Yaaqov to have quoted R. Yaaqov b. Idi, but it is unlikely that he did so, since the Yeru-
shalmi never portrays him as doing so.57) And if R. Aha b. Yaaqov did not quote R. Yaaqov b. Idi, we cannot assume that he nevertheless transmitted the other sources about students’ halakhic rulings that follow (and assume the quotation of) R. Yaaqov b. Idi and separate the first Rabbi/Akko story from the second. Also, R. Aha b. Yaaqov explicitly introduced his tradition as being two stories about Rabbi and Akko. Why would he say this and then transmit a good deal of other (irrelevant) material besides? It is far more likely that the Yerushalmi editors themselves introduced R. Yaaqov b. Idi and the other sources that follow about students’ halakhic rulings.

The real significance of this analysis becomes apparent when we look again at b. Sanh 5b. Although the Bavli editors have reworked R. Aha b. Yaaqov’s two stories (most notably by eliminating any reference to Akko), we can still see versions of the stories there. Not only that, but the Bavli has reworked the first story to be about students’ halakhic rulings, and the subject of students’ rulings separates the Bavli’s versions of the first and second stories. The point is that we have identified the hands of the Yerushalmi redactors at y. Shevi 6:1, 36a–b, and the Bavli’s version of this material includes the work of the earlier Talmuds’ redactors. Thus it is incorrect to attribute the readily identifiable similarities in the Talmuds’ presentations of these stories to early talmud. While some of the similarity is unquestionably early, as I have shown, the structure of the Bavli material as it now appears includes material that can only have been placed there by the Yerushalmi redactors—not by transmitters of early talmud.58 Although this isolated example does not prove that the Bavli knew y. Sheviit as a whole, let alone the entire Yerushalmi, it does show that the hypothesis of early talmud must be approached more critically and that the whole issue requires careful consideration on a case-by-case basis.59

Friedman discusses early talmud more generally in the introduction to the text volume of *Talmud Arukh*.60 He presents a sugya found at b. Yev 9a with a parallel at y. Yev 1:1, 2c and incisively analyzes how the Bavli recast the older Palestinian expressions in its own uniquely Babylonian idiom. Following this analysis, he comments:

> From the lengthy continuation [of the Talmudic discussion] in the two Talmuds we see that the two sugyot are parallels in their entirety, both in

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57. See y. Ber 5:5, 9d; y. Orl 3:1, 63a; y. Shab 1:1, 2d; y. Suk 1:1, 51d.
58. We will soon address the questions surrounding how this transmission may have occurred.
the language of the [amoraic] memrot and in the words of the Talmud (as we have seen in many places) . . . the give-and-take of the stam ha-Talmud here [in the Bavli] is parallel to the stam of the Yerushalmi there. And it is clear that it came along with the memrot from the early talmud that the sages of Babylonia received from the sages of the Land of Israel (for the two Talmuds before us draw from those same early traditions, to which the Yerushalmi is generally still closer in its expression).61

Friedman is thus saying that in the case of b. Yev 9a, the Babylonian sages received from Palestine the amoraic memrot together with anonymous material. Moreover, there existed in Palestine a pool of early traditions, from which both rabbinitic communities drew. This early material, as presented in the Yerushalmi, is closer to the form it had when it was part of these “early traditions.” So, to the extent that b. Yev 9a and y. Yev 1:1, 2c are similar, this similarity is due to the transmission from Palestine to Babylonia of this early talmud at some point prior to the redaction of the Bavli.

A bit later, Friedman expands this observation beyond the isolated case of Yevamot and points out that, in many cases, the structure of a given sugya is common to both the Yerushalmi and the Bavli.62 The existence of “frameworks of memrot and stam in the Bavli, just as they are found in parallel in the earlier traditions reflected in the Yerushalmi,” is a “foundational principle.”63 Now we must ask: what assumptions underlie this theory of early talmud? What burdens of proof must be met in order for the theory of early talmud to be ultimately persuasive? What questions can it help/not help answer?

The major assumption here is that there existed in Palestine early traditions from which both rabbinitic communities drew, and which account for the similarities in structure of parallel sugyot (the early talmud). The original form of these early traditions is unrecoverable, although the early talmud found in the Yerushalmi is closest to them. Friedman’s suggestion sounds intriguingly like a Q-style hypothesis.64

61. Ibid., 15.
62. Ibid., 16.
63. Ibid., 17. Interestingly, Friedman did hint in earlier work that the Bavli may use Yerushalmi sugyot. See “Yevamot X,” 340–343, where he analyzes the striking similarity between b. Yev 88b–89a and its Yerushalmi parallel using language such as “our sugya is exactly like the Yerushalmi sugya,” “. . . the expansions and additions of the Bavli to the Yerushalmi sugya,” and “from the fact that the redactor did not change the structure in the Yerushalmi . . .”
64. Interestingly, Friedman has argued against a Q hypothesis in other work. In his recent studies of Toseftan baraitot and their Bavli parallels, Friedman has argued that compelling evidence suggests that the differences between the Toseftan
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It is rare to encounter a Q hypothesis in studies of rabbinic literature. The only scholar who has explicitly pondered the existence of a rabbinic Q is Jacob Neusner. The Q hypothesis is well known in New Testament studies, where it still enjoys the status of the consensus view on the formation of the Synoptic Gospels (despite its recent detractors). Despite the differences between the New Testament Q hypothesis and early talmud and the resulting fact that the arguments for and against Q are not transferable in their entirety to this analysis of early talmud, some of the arguments recently advanced against Q are suggestive.

In the New Testament context, Q was a heuristic construct devised to explain a puzzle: the Gospels of Matthew and Luke share approximately two hundred verses that are not found in the Gospel of Mark, which is presumed to be chronologically prior to both. These shared verses came to be seen as a Quelle (“source,” hence Q) of Matthew and Luke, along with Mark and other materials particular to those two Gospels. As Mark Goodacre has recently pointed out, this classic formulation of Q assumes that Matthew and Luke used Mark independently and had no contact with each other. If that assumption is persuasively challenged, then Q

baraitot and their Bavli parallels may be accounted for by conscious Babylonian reworking of the Toseftan baraitot. He prefers this hypothesis to the traditional view that the different versions of the Toseftan baraitot are due to their origins in different collections. The rationale of the traditional view is that later scholars would not have consciously emended earlier material. Friedman rejects this rationale, insisting that evidence suggests that later scholars would have, and did, engage in such reworking. There is a similarity between the traditional view that he rejects in the context of Toseftan studies and the view that he suggests in the context of Bavli-Yerushalmi studies. Now, I do not deny that there may be compelling reasons for Friedman to hold opposing views in the two contexts. But nowhere, to my knowledge, does he articulate clearly why he sees the two cases as different; nor does he establish any other proofs for early talmud as an explanation for the similarities between the Talmuds. See Shamma Friedman, “Ha-baraitot she-be-Talmud ha-Bavli ve-yahasan le-Tosefta,” in Atara L’Tahím: Studies in the Talmud and Medieval Rabbinic Literature in Honor of Professor Haim Zalman Dimitrovsky (Jerusalem: Magnes, 2000), 103–201 (Heb.); idem, “Uncovering Literary Dependencies in the Talmudic Corpus,” in The Synoptic Problem in Rabbinic Literature (ed. Shaye J. D. Cohen; Providence, RI: Brown Judaic Studies, 2000), 35–57.


67. Goodacre, The Case Against Q. 47. Parenthetical references in the list below refer to this work.
may not be necessary. Goodacre aims right for this assumption, and advances a number of arguments to establish that Matthew knew Mark, and Luke knew Matthew as well as Mark. Goodacre’s arguments that have methodological implications for this study include:

1. Arguments for Q tend to stress differences between Matthew and Luke on the micro level and ignore their “striking” similarity at the macro level (47);

2. On the issue of dating, the greater the distance in time between Matthew and Luke, the less likely it is that Luke was unaware of Matthew (48);

3. Scholars have paid insufficient attention to the characteristically Matthean character of Matthew’s additions to Mark, and the characteristically Lukan character of his additions (51–61);

4. Instead of focusing on the obvious differences between Matthew and Luke, scholars might instead ask whether the points of contact between them are sufficiently strong as to suggest that Luke might have known Matthew (56);

5. Q’s workability as a theory and the fine scholarship behind it are not an argument against questioning it (76);

6. Occam’s Razor (the simpler interpretation is more likely to be true) militates against Q (77);

7. Without Q, scholars are in a better position to appreciate the literary creativity deployed by Luke in crafting his Gospel (117, 145);^68

8. If Q is right, then the common Matthew/Luke material will not resemble its surrounding Gospel context (since it is a separate source), but if Luke derived that material from Matthew, the common material may still bear the imprints of its Matthean context (182)—which it does.

^68 Goodacre also notes that

a narrow redaction-critical model in which it is regarded as inevitable for the evangelists to have taken over every congenial word, phrase, or theme from their sources . . . without consideration of broader narrative context . . . and the literary agenda of the evangelists is, in the end, a blunted instrument that can only detract from our appreciation of the Gospels and their writers. (145)

It is interesting to compare this observation to Friedman’s own observations about the relationships between Toseftan baraitot and their Bavli parallels.
Goodacre’s methodological points make a good deal of sense in the context of this study. To begin with, his emphasis on the importance of macro analysis is right on the mark; in our case, by studying y. and b. Avodah Zarah first as whole compilations (examples of the results of that study are presented in chapter 2) and following up that study with micro analyses of specific textual parallels (chapters 3–4), we will be better able to appreciate how the inter-Talmudic similarities we will observe are more likely the results of b. Avodah Zarah’s reworking of y. Avodah Zarah than of early talmud. Second, applying Goodacre I would say that it is not enough to focus on the differences between the tractates (although we will), but that we must pay careful attention to whether the points of contact between the tractates are sufficiently strong as to suggest that b. Avodah Zarah knew y. Avodah Zarah. As we will see in chapters 2–4, b. Avodah Zarah sugyot with multiple parallels in rabbinic literature tend to resemble y. Avodah Zarah sugyot more closely, b. Avodah Zarah has characteristic ways of reworking y. Avodah Zarah materials, and b. Avodah Zarah even exhibits the tendency to pick up on issues where y. Avodah Zarah left off. Third, many differences between the tractates can be explained on the basis of the differing intellectual, religious, and other agendas of the two Talmuds, which is sometimes glossed over by scholars’ focus on the mere fact of difference. Fourth, the first three points implicate Occam’s Razor—since we can explain the similarities and differences between y. and b. Avodah Zarah without early talmud, we do not need it as a global explanation. In a related vein Friedman himself has pointed out the tendency of older scholars such as Chanoch Albeck and J. N. Epstein to hypothesize the existence of now non-extant compilations in order to explain differences among our existing literatures because of the earlier (now outdated) assumption that the redactors of later compilations could not, would not, have intentionally changed the earlier. But, as Friedman has incisively demonstrated recently, this older assumption is flawed.\footnote{See the sources cited in n. 64, above.} There is thus little reason to hypothesize a pool of shared rabbinic traditions in order to explain similarities between y. and b. Avodah Zarah, when these similarities (and differences) can be well-explained by reference to the creativity of the Babylonian redactors, whose contributions in reworking their prior sources has been, and continues to be, amply demonstrated.\footnote{See, e.g., Rubenstein, Talmudic Stories; idem, Culture of the Babylonian Talmud.} Finally, Goodacre’s point about the chronological gaps between compilations is also on target. The Bavli (and hence b. Avodah Zarah) is at least two hundred years the Yerushalmi’s junior. That being so, it is not reasonable to assert without proof that b. Avodah Zarah...
Zarah’s redactors had no knowledge of y. Avodah Zarah, and that any similarities between the two are due to early talmud which pre-dated y. Avodah Zarah. The closer the Talmuds are to each other in time, the more sense early talmud makes as a global explanation of the similarities; the more separated in time, the more sense it makes to test the theory that a given Bavli tractate knew its Yerushalmi parallel.

To be sure, the Q hypothesis in New Testament studies and the early talmud hypothesis result from the intellectual creativity of superb scholars, and merit careful consideration. Friedman’s work in particular sets a high standard of erudition and methodological rigor. Moreover, early talmud does at times adequately explain certain similarities between the Talmuds. But none of this should preclude a re-examination of evidence. The evidence itself should decide whether the early talmud hypothesis or the hypothesis that b. Avodah Zarah knew y. Avodah Zarah is the more reasonable explanation for the similarities we see between the tractates.

Friedman is also unclear about the process by which early talmud was transmitted to Babylonia. The nahote, as we discussed earlier, do not suffice as an explanation for this transmission. Aminoah’s and Sussman’s work on early talmud is equally unclear on this point. Without an explanation of exactly how all these parallel materials went from Palestine to Babylonia, these scholars leave us to assume that the materials somehow “circulated” there. But what exactly does it mean that Palestinian material “circulated” until given final form in the Bavli? The issue is more complex than it seems. Palestinian materials traveled a geographical distance between the rabbinic centers, away from the living community of learners and interpreters in which they had originated and were a staple of study. Such conditions are not ideal for the preservation and transmission of literary productions, especially if their primary mode of transmission is oral. The reason for this is that when these literary productions are removed from the communities of those who studied them, an important check on variability in their content—the scholarly community itself—is also removed. Thus, these literary productions may come to change, perhaps significantly. A scholarly theory about the presence of Palestinian materials in the Bavli must take this into account and do more than merely assume that these materials “circulated” from one place to the other. It is far more reasonable to claim that—in the case of this pair of tractates—most of the Palestinian material came to Babylonia attached to mishnayot (as “Talmud Yerushalmi” to m. Avodah Zarah), whereupon it was then reworked and eventually incorporated into what became b. Avodah Zarah.

71. I will discuss this issue in greater depth in chapter 6 as part of my analysis of the impact of orality studies on the thesis of this book.
Lastly, if early talmud was being transmitted to Babylonia throughout the amoraic period, we should not see what, in fact, we do see: the preservation of argumentation noticeably increases with the fourth generation, along with Palestinian influence. The theory of early talmud would lead us to expect that Babylonian amoraic activity would be more uniform if early talmud was steadily arriving from Palestine. This being so, the hypothesis of early talmud is insufficiently grounded, despite its other merits.

Noah Aminoah studied the redactions of tractates Qiddushin, Betzah, Rosh Hashanah, Taanit, Sukkah, and Moed Qatan. He carefully studied the sugyot shared by the Talmuds on a case-by-case basis and broadly distinguished two types of shared Palestinian sugyot: sugyot edited early in Palestine that the Talmuds share, but which they differently interpret and expand, and completed Yerushalmi sugyot that form the core of sugyot that the Bavli subsequently edited in its own way. Within these broad categories, Aminoah isolated the following types:

1. Bavli sugyot that were Palestinian in their foundation;
2. Sugyot whose subject is a Palestinian source;
3. Palestinian sugyot in the Bavli that are essentially similar to Yerushalmi sugyot, except that the parallel Yerushalmi sugyot contain different attributions, or reversed positions;
4. Babylonian parallels to Yerushalmi materials that contain different attributions or reversed positions; and
5. Babylonian sources in the Yerushalmi.

Aminoah’s close attention to detail and heuristic classifications of the parallel materials are helpful. But although his individual analyses indicate his conviction that Palestinian sugyot came to Babylonia throughout the amoraic period, he, like Friedman and Sussman, fails to explain just how that might have happened.

73. See Aminoah, The Redaction of the Tractate Qiddushin, 328–365.
Aminoah is also commendably sensitive to the issue of which Babylonian amora'im seem to be aware of the Palestinian sugyot qua sugyot (which has implications for when those Palestinian materials might have become known in Babylonia). But while he recognizes that the presence of, say, Abaye in a Palestinian sugya could be an example of Babylonian reworking, he assumes—and never questions his own assumption—that Abaye's presence points to him as the Babylonian editor of the Palestinian material. But without a theory as to how the Palestinian material came to Babylonia (or even with such a theory), the assumption that Abaye was the editor of the material is not necessarily more logical than the assumption that later editors came into possession of the Palestinian material and deployed Abaye in the sugya themselves. Aminoah himself recognizes (following Chanoch Albeck) that the Talmudic editors did at times deploy amoraic statements themselves, in contexts of their own choosing, which makes it all the more curious that he does not seriously consider this possibility. Another significant lacuna is Aminoah's failure to employ the macro perspective of looking at the Yerushalmi and Bavli parallel tractates as whole tractates; had he done so, he may have noticed large-scale similarities in the selection and ordering of sugyot and topics in the two Talmuds. The micro perspective of sugya analysis is vital, but lacking the macro perspective, it is insufficient.

Aminoah illustrated his understanding of early talmud in an interesting paper entitled "Qit'ei talmud mi-siddur qadum be-massekhet Rosh Hashanah." In that paper, he studied a lengthy series of sugyot at b. RH 9b–15b which is similar in substance, form, style, and juxtaposition of sugyot to series of sugyot found at y. RH 1:2, 57a, y. Bik 2:5, 65a, y. Shevi 5:1, 35d, y. Shevi 2:7, 34a, and y. Shevi 4:1, 35d. Aminoah arrived at the conclusion that a common siddur qadum (early arrangement) of sugyot was shared by the Talmuds because "when we remove from the arrangement before us anonymous sugyot whose signs are obviously Babylonian and late and sugyot that are not [related] to excerpts from the mishnah of Rosh Hashanah . . . we obtain one continuous arrangement to the excerpts of mishnah Rosh Hashanah in both the Bavli and Yerushalmi." Aminoah acknowledges again and again that Babylonian "later editor(s)" rearranged materials and introduced uniquely Babylonian argumentation and language into the "early arrangement." If that is so, then we may well

74. See Aminoah, The Redaction of the Tractate Qiddushin, 332 and idem, "Qit'ei talmud," 188 (noting the Bavli’s addition of Abaye to a Palestinian sugya, which Aminoah interprets as evidence that Abaye edited the material).
75. Aminoah, “Qit’ei talmud,” 189.
76. Ibid., 187.
wonder why it is more reasonable to assume that the common arrangement of shared sugyot came to both rabbinic communities during the amoraic period rather than during the post-amoraic period of redaction. Aminoah does not adequately explain why the common sequence of sugyot in the Talmuds need not necessarily be viewed as a product of the amoraic period.

Aminoah does point to the presence of third-generation Babylonian amoraim in three of the parallel sugyot in the sequences in both Talmuds, and to the presence of Abaye and Ravina in two sequences in the Bavli alone. From this he concluded that Abaye’s school already had the earlier arrangement of sugyot before it, which it edited—as shown by the addition of Abaye. Aminoah asks rhetorically: “For if the sequence had been arranged . . . for the first time in the school of Abaye, how is it that neither follow-up to his words nor argumentation [about them] is found in the Yerushalmi? It may be inferred that their [Abaye’s and Ravina’s] words were added to the early arrangement.”78 Now, Aminoah’s question assumes an important point that requires proof, namely, that the “early arrangement” is in fact early. Only one who assumes that the shared sequence of sugyot is early would find the challenge of the rhetorical question compelling. But the sequence could very well have originated in the Yerushalmi and become known to the Bavli redactors, who edited it by adding Babylonian materials, terminology, and argumentation—including teachings of Abaye and Ravina. Aminoah’s argument from the names and generations of quoted amoraim does not suffice as a proof of “early arrangement.”

Moreover, we may make another observation about the shared sequence of sugyot studied by Aminoah that also casts doubt on his hypothesis of “early arrangement.” Aminoah himself points out that the sequences of sugyot from y. Rosh Hashanah, y. Bikkurim and y. Sheviit, when juxtaposed in that order, make up the sequence found at b. RH 9b–15b. In other words, whoever put together b. RH 9b–15b combined materials found in those three places in the Yerushalmi. Of even greater interest is the fact that b. RH 9b–15b utilizes Sheviit material in an order different from that found in the Yerushalmi (as Aminoah points out in his appendix bet). We thus have two levels of arrangement of the sugyot: the order in y. Rosh Hashanah, y. Bikkurim, and y. Sheviit, and then the order in b. Rosh Hashanah, which utilizes and alters them. Is it reasonable to assume (as Aminoah does) that the Bavli’s appropriation and adaptation of the Yerushalmi’s sequences of sugyot happened during the early to mid-amoraic period in Babylonia? This question must be answered with another question: can

78. Ibid., 188.
amoraim be demonstrated to have created and reworked this sequence of sugyot, attaching it to the respective mishnayot by which it is found? In order for amoraim to have been the ones responsible for creating and attaching the sequence of sugyot to mishnayot, they must be demonstrated to be aware of the sequence of sugyot qua sequence and to be aware of its attachment to the mishnayot by which it is found. If the amoraim cannot be demonstrated to have such an awareness, then it is more reasonable to assume that the sequence of sugyot and its linkage to certain mishnayot is the work of the redactors.

We find that no amora at b. RH 9b–15b is aware of the sequence of sugyot qua sequence of sugyot, or that it is now found as a sequence of sugyot attached to m. RH 1:1. Similarly, no amora at y. RH 1:2, 57a, y. Bik 2:5, 65a, or y. Shevi 5:1, 35d; 2:7, 34a; or 4:1, 35d is aware of those sequences of sugyot or that they are attached to the mishnayot to which they are attached. The work of forging the Yerushalmi sequences of sugyot was the work of the Yerushalmi redactors. The Bavli redactors took these sequences of sugyot, added to them, rearranged them, and edited them to conform to Babylonian norms of language and argumentation.

But what about the fact that the y. Bikkurim sugyot are Babylonian in origin and that early- to middle-generation amoraim are mentioned throughout the sequence of sugyot? This fact is undeniable, but must be appreciated for what it does and does not show. The amoraim in both rabbinic centers studied mishnayot and other sources, and created sugyot. It is an overstatement to claim—as some scholars do—that the Bavli is entirely, or largely, pseudepigraphic and that all sugyot, with all names of amoraim, are the creations of post-amoraic editors and redactors. But while amoraim created sugyot, it cannot be satisfactorily demonstrated that they linked sugyot into lengthy sequences, passed these sequences back and forth between the two rabbinic centers, and attached those sequences to particular mishnayot. I will say more about amoraic awareness and activity later in this chapter.

Yaacov Sussman also dealt with the issue of early talmud in his important article “Ve-shuv le-Yerushalmi Neziqin.” Sussman pointed out that despite the many striking differences between the Bavli and Yerushalmi Neziqin, the two share all the amoraic material relating directly to the mishnayot and topics of the tractate, which stems from the “first amoraic period” (the amoraic period from 220 CE until the end of the

79. According to rabbinic tradition, the three tractates Bava Qamma, Bava Metzia, and Bava Batra once constituted a large tractate called simply “Neziqin” (b. AZ 7a). These tractates were the focus of Saul Lieberman’s work on the so-called “Talmud of Caesarea,” which, in turn, was the subject of Sussman’s article.
amoraic period in Palestine, which Sussman dates to 360/370). Although the two Talmuds share this material, they differ radically in how they present it: y. Neziqin presents this early amoraic material as short traditions, without much of a dialectical context, while the Bavli—as is its way—embeds this material in complex dialectical constructions. Sussman refers to this shared material as “early basic amoraic material” (חוזי נבון סיני קדום), and stresses that owing to an extended process of development that the Bavli underwent for hundreds of years after the close of the Yerushalmi, it is the immense and different rabbinic compilation that it is, essentially the product of the “school of the last geonim.”

As with Friedman and Aminoah, Sussman’s interpretation of the role of “early basic amoraic material” in the making of the Bavli assumes without adequate argumentation that it somehow made its way from Palestine to Babylonia during the amoraic period. Once again, it bears noting that the temptation to attribute this transmission to the nahote must be resisted. Not all of the “early basic amoraic layer” is transmitted in the names of nahote, and it is methodologically unsound to assume that because many traditions were so transmitted, all the Palestinian amoraic material in the Bavli reached Babylonia through that channel.

But Sussman squarely confronts a more serious problem with the assumption of an amoraic-era transmission to Babylonia of an early amoraic layer. He acknowledges that it is often unclear in the Bavli where the “early basic amoraic material” ends and the dialectical elaboration of it begins. The early amoraic layer underwent editing and reworking by later amoraim, savoraim, and geonim over a long period of time, and the contributions of these other scholars “necessarily affected the formulation of the [early amoraic] teachings themselves. And just as it is not possible to distinguish between earlier and later, between ‘rationale’ and ‘addition,’ so also the boundaries between the form of the sugya and the body of the teaching, between the interpretation of the teaching and its formulation—are progressively blurred” (emphasis added). Although Sussman fails to consider the impact of this key admission on the assumption of an “early basic amoraic” layer, the impact is clear—it is difficult if not impossible in many cases to even know how the “early basic amoraic material” had originally been formulated, not to mention the impossibility of distinguishing it from the dialectical construct in which it is now embedded. And, once it is acknowledged that these distinctions cannot be made, then

81. Ibid., 99–106.
82. Ibid., 109–110.
83. Ibid., 110.
it might be the case that the “early” amoraic material reached Babylonia late in the amoraic period or even later as a fully-integrated component of already-formulated sugyot. In light of these problems, Sussman’s assertion of “early basic amoraic material” shared by both Talmuds cannot be accepted as formulated.

As with Aminoah, an example (although somewhat technical in this case) will illustrate the problems attendant upon Sussman’s admission that early amoraic material cannot always be distinguished from later amoraic or even redactional emendation. M. Avodah Zarah 3:4 records an alleged dialogue between “Proclus b. Philosophus” and Rabban Gamliel. Proclus asks Rabban Gamliel how the rabbi can bathe in Aphrodite’s bathhouse in light of Deut 13:18’s prohibition against deriving benefit from any forbidden idolatrous items. The rabbi declines to answer Proclus in the bathhouse but once outside, offers a number of alternative responses: he did not enter Aphrodite’s domain, she entered his; people build statues of Aphrodite for bathhouses, not bathhouses for Aphrodite (which shows that the statues are decorative rather than cultic); men behave disrespectfully in front of Aphrodite (standing around naked, urinating), which they would presumably not do if the statue were meant for worship. At y. AZ 3:4, 42d, R. Hama b. Yose is part of a chain of tradition reporting that “[R. Gamliel] responded to him [Proclus b. Philosophus] with an evasive answer. For if it [the answer] were not so [evasive], [Proclus] should have [further] responded to him from [the case of] Baal Peor, the worship of which is only by means of uncovering [one’s body].” The Yerushalmi’s anonymous voice then provides a way that R. Gamliel’s response can be responsive to Proclus’ question, and yet not provoke a further response: “What is the result? That [deity] as to which a person behaves [disrespectfully] because of divinity [like Peor], it is forbidden. And that [deity] as to which a person does not behave [disrespectfully] because of divinity [like Aphrodite], it is permitted.” Thus R. Gamliel’s response to Proclus was responsive, since he established that he was permitted to go into Aphrodite’s bathhouse, and Proclus could not respond to R. Gamliel from the case of Peor, which is in a different worship category than Aphrodite.

At b. AZ 44b, R. Hama b. Yosef—the very same amora—reports, “R. Gamliel responded to the general [Proclus] with a deceptive answer, but I say it was not deceptive.” The Bavli then presents four suggestions of the possible deceptiveness of the answer, and how that deceptiveness is more apparent than real. Each of these four suggestions ends with a suggested continuation of R. Hama b. Yosef’s memra, picking up from his words “but I say it was not deceptive . . . .” and then giving the reason the answer was not deceptive. The first suggestion presented includes mention of the name of Rava, while the other three are clearly presented in the names of Abaye, R. Shimi b. Hiyya, and Rabbah b. Ulla. The first solution (mention-
ing Rava although not in his name) most closely resembles that of the Yerushalmi, and the suggested ending to the *memra* is “and I say it was not deceptive. This one’s [Peor’s] worship is in this fashion, and this one’s [Aphrodite’s] worship is not in this fashion.” This suggested ending to the *memra* closely resembles the resolution attributed to the Yerushalmi’s anonymous voice.

Now, what is the “early basic amoraic material”? The likely response to this question is that it is R. Hama b. Yosef’s *memra*. If only the *memra* is the early amoraic material, then there is a problem. What did it look like in its form as “early basic amoraic material”? In the Yerushalmi, the *memra* includes mention of Peor, but in the Bavli it does not—which gives the Bavli the opening to suggest four possibilities for how to interpret it. (An alternative explanation of the difference in the Talmuds’ presentation of the *memra* will be discussed shortly). The Bavli’s first suggested interpretation, which is closest to the Yerushalmi, includes a mention of Peor by Rava. But as the Tosafot point out (s.v. *veha-amar*), Rava’s mention of Peor is introduced by “but did not Rava say . . .?,” which indicates to them that “Rava made his statement in another place.” Rava is thus unaware that he is being deployed to interpret R. Hama b. Yosef’s *memra*. It is not Rava who is proposing a solution similar to that of the Yerushalmi; it is the Bavli redactors, whose first interpretation of R. Hama b. Yosef demonstrates awareness not only of his *memra*, but of the Yerushalmi’s own interpretation of that *memra*. The similarity between the Yerushalmi’s and Bavli’s (initial) interpretation of R. Hama b. Yosef is thus more likely due to the Bavli’s appropriation of the Yerushalmi’s interpretation of the *memra* along with the *memra* than to a hypothesis of “early basic amoraic material.”

Alternatively, the difference between the Talmuds’ presentations of R. Hama b. Yosef’s *memra* could be due to conscious Bavli reworking of the *memra*. In other words, the Bavli could have received the *memra* from Palestine during the amoraic period as we find it in the Yerushalmi (which includes the mention of Peor) but deliberately shortened it so as to allow for the formation of the complex sugya we now find at b. AZ 44b. But if this is so, then the formulation of the *memra* as it appears in the Bavli is the product of editorial reworking that postdates R. Hama b. Yosef. We cannot then say with any confidence that the *memra* or any portion of it is convincingly identifiable as early talmud. The fact that both these possibilities for understanding the differences between the *memra*’s versions are equally plausible casts reasonable doubt on the facile assumption that the *memra* is early talmud.

Moreover, the Bavli’s deployment of the fourth-generation Rava in its first interpretation of the *memra* could only be by scholars who come later than Rava. “Later” can conceivably include fifth-, sixth-, or seventh-gen-
eration amoraim, and/or the post-amoraic redactors. It is more reasonable, however, to maintain that Rava was deployed by post-amoraic redactors. Looking at the Bavli sugya at b. AZ 44b as a whole, we see that it is a complex sugya organized in a tight, recurring literary pattern and including anonymous argumentation. If we argue that Rava was deployed by later amoraim, then we must demonstrate that fifth- to seventh-generation amoraim created sugyot of this sort. Now, scholarship has demonstrated both that middle-generation amoraim may have functioned as editors and that the later amoraic generations are characterized by greater and greater preservation of argumentation. But it does not seem to be the case that later Babylonian amoraim produced sugyot of this sort. This sort of work is more characteristic of the post-amoraic redactors.

The theory of early talmud is a cousin of the “historical kernel,” the search for which many scholars of rabbinics today eschew. Scholars once assiduously studied rabbinic stories for the historical kernels they contained about rabbinic sages and historical events, although the current scholarly consensus has moved in the direction of arguing that the stories themselves are hermeneutically and ideologically generated literary and cultural artifacts that likely do not contain historical kernels. Although the search for the historical kernel is out of scholarly fashion in the world of aggadah, it persists in the study of halakhic sugyot. Although the scholars whose work I have discussed in this section do not frame the issue in these terms, it is apparent that they see the halakhic sugya’s parallel to the aggadic “historical kernel” in the shared material found in parallel Bavli and Yerushalmi sugyot. Stripping parallel sugyot down to their shared skeletons (as Noah Aminoah did with his Rosh Hashanah example) is thought to reveal the “amoraic halakhic kernel.” Unlike the earlier investigators of the aggadic historical kernel, however, Yaacov Sussman is aware of the difficulty of separating the amoraic halakhic kernel from the argumentational matrix in which it is embedded. As I have demonstrated, his caution is well placed. Although the search for the “amoraic halakhic kernel” has much merit and may explain some inter-Talmudic similarities, its utility as a hypothesis must be explored carefully on a case-by-case basis. And, as this book will demonstrate, the theory of the amoraic halakhic kernel does not explain all of the similarities we see between b. and y. Avodah Zarah. The work of Friedman, Aminoah, and Sussman, as well as that of Jaffee, shows that bolder conclusions are justified.

84. See Kalmin, Sages, Stories, Authors, and Editors, 172n9.
85. See David Kraemer, “Stylistic Characteristics of Amoraic Literature”; idem, Mind of the Talmud.
To be clear, I am not denying that the Talmuds share material, or that some, or even much shared material may be the result of scholarly exchanges during the amoraic period. This cannot be denied in the face of the Talmuds’ portrayals of nahota activity. Scholars certainly moved back and forth between the rabbinic centers throughout the Talmudic period, diligently pursuing their studies. But I am questioning whether these scholarly exchanges alone can account for the structural similarities we observe. An argument that they do account for all the structural similarities must be based on the following assumptions (or better still, the argument must prove these points):

1. When we find similar sequences of “uncalled-for” topics and/or sugyot attached to a mishnah, this is the work of the amoraim;

2. When the Bavli juxtaposes a complex sugya (defined for this purpose as one with three or more steps in its argumentation) to a mishnah with the same (sizable) number of argumentational steps and/or a similar argumentational content as the parallel Yerushalmi sugya, this is the work of the amoraim;

3. Amoraim functioned as editors and original creators of all kinds of sugyot—simple as well as complex.

The scholar who assents to all three propositions is making large claims for the amoraim, essentially arguing that they functioned as sugya-editors and even redactors throughout the amoraic period. These claims are not reasonable in light of what we know at this point about amoraic activity. Amoraim certainly created and transmitted sugyot, and Richard Kalmin has even suggested that middle-generation amoraim may have functioned as editors. But when we examine sugyot actually transmitted by amoraim, these appear to be relatively simple affairs. As an example, let us look at b. AZ 24a. There, the Bavli presents R. Papa’s and R. Zevid’s versions of a sugya that consists of a teaching of the tanna R. Eliezer and the discussion of that teaching by the amoraim R. Yosef and Abaye. R. Papa and R. Zevid are represented as “reciting” or “teaching” (matnei)

86. I will explain the meaning and significance of this phrase later in this chapter. Briefly, this refers to sugyot and topics that are present in the Bavli’s or Yerushalmi’s commentaries on a particular mishnah, but which are not necessary for a complete explication of that mishnah.

87. See Richard Kalmin, Sages, Stories, Authors, and Editors, 169–173.

88. The amoraic sugyot I am discussing are those that can be clearly seen from context to be known to the amoraim as sugyot. This issue of what the amoraim were and were not aware of is critical in an assessment of amoraic activity.
that sugya. This sugya is striking in its brevity and non-analytic quality; it collects rather than analyzes teachings of the previous amoraic generation. A similar example is found at b. Git 3a. There, a small sugya begins with an anonymous question, to which R. Yohanan and R. Hanina are represented as supplying answers. Once again, this brief sugya is simply a short compilation rather than an analytic composition. An especially interesting example is found at b. Ket 57a. There, R. Dimi and Rabin both transmit different versions of the same sugya. The sugya consists of a statement by R. Shimon b. Pazi in the name of R. Yehoshua b. Levi in the name of Bar Qappara, a rather opaque comment on that statement by R. Yohanan, and then an interpretation by R. Abbahu of the initial statement in light of R. Yohanan. Once again, the amoraic sugya is rather simple. And even though R. Papa is portrayed as being aware of both R. Dimi’s and Rabin’s versions of the sugya (although they are not represented as being aware of each other’s!) and attempting to understand the difference between them, even R. Papa is not portrayed as being aware that these sugyot, and his comments upon them, are part of a larger context that ranges beyond these sugyot.

Although three sugyot are (admittedly) hardly a statistically significant sample, they suggest three significant points. The first is that although more research remains to be done to identify and study amoraic sugyot, it is not correct to assume—as proponents of early talmud implicitly do—that any and all types of sugyot traveled back and forth between the rabbinic centers during the amoraic period. Some types of sugyot did circulate, but the lengthier and more argumentational the sugya, the less likely it was created or transmitted by amoraim.89 Second, amoraic sugyot are not likely to display the multiple steps of argumentation and casuistic virtuosity characteristic of later sugyot. If the Talmuds are found to share sugyot of this variety, the sharing likely occurred later than the amoraic period. Third, these three sugyot suggest that the issue of amoraic awareness of context is an important factor. Amoraim were certainly aware of mishnayot, and it is certainly possible (although the matter requires demonstration, not mere assertion) that they may have attached baraitot, memrot, or simple amoraic sugyot to mishnayot. But we cannot proceed on the basis of unproven assumptions about amoraic activity. Many times amoraim are not portrayed as being aware of a dialectical context larger than their own quoted memra, or at best, larger than the short sugya in which they are found. Amoraim are not portrayed as being aware of a lengthy chain of topics attached to a mishnah, let alone of what sugyot are

89. But see b. Sanh 87a–88a, which appears to be a lengthy sugya taught by R. Papa at Rava’s request. This example requires further investigation.
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included in that chain and what topics they cover. As the example of R. Papa (b. Ket 57a) demonstrates, even an amora who is portrayed as being aware of two sugyot does not seem aware of the larger context of those sugyot. The point of this is that proponents of early talmud are not on firm ground when they simply assert (or imply) that chains of topics and sugyot could have become attached to mishnayot in the amoraic period. Although it is certainly not impossible that this could have happened in some cases, an assessment of amoraic awareness militates against it.

Early talmud works best as a theory explaining similarities between the Talmuds when a simple amoraic memra—or perhaps a memra with a brief interpretative comment or short sugya following—is what is present in the one Talmud, while the other uses that memra (and/or comment or sugya) as the basis of a longer sugya. The lengthier and more complex the parallel materials are in both Talmuds, the less well early talmud works as an explanation of the similarities.

Although the work of Aminoah, Sussman, and Friedman does not fully settle the issue of the similarities between the Talmuds, their sensitivity to the development of Talmudic literature and the complexity of sugya-formation raises another question: might it not be the case that Babylonian sugyot influenced Palestinian sugyot, as well as the reverse? We know that the Yerushalmi contains Babylonian sugyot; it has also been demonstrated that the rabbinic movement in Babylonia was extremely decentralized in comparison with Palestine.90 Taking these two data together, we may well ask whether a given sugya was originally formulated in Babylonia, transmitted to Palestine, and then re-introduced into Babylonia at another time and place. I will consider this possibility as it arises in specific cases in the course of the detailed textual work in chapters 2–4.

IV

The Method of This Book

In demonstrating that the b. Avodah Zarah redactors were influenced by y. Avodah Zarah, I employ two levels of analysis: “macro” and “micro.” Chapter 2 is a report of the findings of the macro analysis.

The point of the macro analysis of the Avodah Zarah tractates is to compare them as wholes—not to look simply at this or that parallel sugya, but to study the tractates as whole compilations. Macro analysis is the methodological result of taking seriously the notion that the redacted tractates as whole compilations are the only meaningful level at which re-

90. See, e.g., Richard Kalmin, Sage in Jewish Society.
search into whether or not b. Avodah Zarah relied on y. Avodah Zarah must proceed. In practice, this involves comparing each Talmud’s entire treatment of a given mishnah to the other’s. As Jacob Neusner has shown in numerous contexts (and as Shlomo Yehudah Rappaport and Martin Jaffee have shown in the context of Bavli-Yerushalmi studies), macro analysis is an important tool for uncovering major patterns in Talmudic phenomena.91 Analyses of isolated sugyot are important, but are not dispositive of large issues such as the relationship between the Talmuds. In elucidating such a large and complex issue, macro analysis is absolutely indispensable.

In examining y. and b. Avodah Zarah’s parallel mishnah treatments, I begin by noting what on a basic level looks similar in the two Talmuds’ treatments of the same mishnah. I then test these similarities to determine if they are “called for by the mishnah.” A topic is “called for by the mishnah” when the mishnah cannot be adequately explained without discussion of this topic. For example, in m. AZ 1:1, the mishnah rules that Israelites must abstain from various commercial activities with Gentiles during the three days prior to an idolatrous festival. This three-day period is an essential element of the mishnah, and it is therefore unremarkable that both Talmuds discuss it. That is, the mere fact that both Talmuds discuss the three-day period is “called for by the mishnah” since the mishnah cannot be adequately explicated without some consideration of that period. Nevertheless, closer examination of how the Talmuds go about discussing that three-day period is necessary in order to establish whether each Talmud independently dealt with it or whether the later Talmud’s discussion of it is in some way beholden to that of the earlier Talmud.92

If a particular topic is called for by the mishnah, what determines whether or not the Bavli’s treatment of that topic was or was not influ-


92. See my discussion of the Talmuds’ explications of this mishnah at pp. 89–101 and 189–193. In fact, on the basis of such a closer examination, it is indeed possible to establish that a structural similarity exists between the Talmuds’ discussions of the three-day period, although the topic itself is called for by the mishnah.
enced by the Yerushalmi? If both Talmuds take up a particular topic and deal with it in different ways, a good argument exists that at least as to that term of the mishnah, the Bavli was not influenced by the Yerushalmi. On the other hand, m. AZ 5:1 and its explication by both Talmuds is a good example of the Yerushalmi’s influence on the Bavli’s discussion of a topic that is called for by the mishnah. While both Talmuds take up the issue of the hired “worker” (kerja) mentioned in m. AZ 5:1 (which renders such a discussion “called for by the mishnah”), each Talmud’s treatment of the issue follows a nearly-identical five-step progression of argumentation. Given the largely similar nature of the discussion, an argument can be made that, in this instance, the later Talmud was influenced by a version of the earlier.93

Implicit in what I have been saying is a distinction between each Talmud’s decision to discuss a given topic, which may be “called for by the mishnah,” and the way it goes about developing that discussion, which is “not called for by the mishnah.” The mishnah neither contains instructions for how its constituent parts are to be explicated, nor for whether the Talmuds should explicate some, and not others. The decisions about what amoraic statements, sugyot, and aggadic materials to weave together into the discussion of a mishnah are the decisions of the Talmudic, not the mishnaic, editors. Dissimilarity in the selection and sequence of these amoraic statements, sugyot, and aggadic materials in each Talmud may indicate that the later Talmud was most likely not influenced by the earlier. The more similarity we see between the selection and sequence of these constituent materials in the two Talmuds, the more likely it is that the redactors of the Bavli tractate under study were influenced by the Yerushalmi.

A topic is “not called for by the mishnah” if the mishnah can be adequately explicated without discussion of that topic. For an example, let us return to m. AZ 1:1. That mishnah contains neither aggadic materials nor scriptural verses, and yet both Talmuds’ discussions of that mishnah include lengthy aggadic materials. Such aggadah is “not called for by the mishnah.” Another example from m. AZ 1:1 of a similarity between the Talmuds “not called for by the mishnah” is more subtle, and teaches us that we must be careful not to confuse what is required for an adequate explication of the mishnah with what we know (or sense) to be “typical” Talmudic give-and-take. The mishnah sets out normative behavior regarding Jewish commercial activity during the three days preceding a pagan festival which the mishnah’s editors presumably expected (or hoped) would be obeyed. Yet both the Yerushalmi at y. AZ 1:1, 39b and the Bavli at b. AZ

93. For detailed discussion of this example, see pp. 62–68.
6b utilize, inter alia, a tradition of R. Yohanan in order to explore the issue of the legal consequences of violating the mishnah’s prohibitions. Such a discussion is “not called for by the mishnah,” because the mishnah itself does not call for the Talmudic tendency to juxtapose baraitot or other sources to the mishnah which contradict its halakhic stance. The decision to do this is post-mishnaic, and cannot be assumed to be called for by the mishnah itself.

Finally, the Talmuds’ discussions of m. AZ 3:2 provide another good example of a discussion “not called for by the mishnah.” That mishnah rules that shards of (Gentile) vessels are permitted (per the Yerushalmi’s version of the mishnah), or that shards of (Gentile, non-idolatrous) images are permitted (per the Bavli’s version). Both Talmuds use that mishnah as the opportunity to discuss a R. Yohanan/Resh Laqish dispute about “an idol that was broken (y. Avodah Zarah) by itself” (b. Avodah Zarah). Even if one were to argue that a dispute about broken idols should be considered “called for” by a mishnah that talks about shards of vessels or images, the fact that the Bavli deals with the issue by taking over and expanding upon the same sugya as that found in the Yerushalmi is suggestive of its editorial reliance upon that Talmud.94

This initial step of the research allows me to exclude from further consideration materials that, being entirely “called for by the mishnah,” are not dispositive of the question of the relationship between the Talmuds. If both Talmuds discuss a topic that is called for by the mishnah, and their discussions of it differ sufficiently that they are appropriately judged independent, then there is no further point in looking for evidence of the Bavli’s reliance on the Yerushalmi in these cases. These materials may be excluded from further analysis.

Once I determine, on the contrary, that a given similarity between the Talmuds’ discussion of a mishnah is “not called for by the mishnah,” I examine this similarity against the other tannaitic materials (baraitot and other mishnayot) cited in the course of the Talmuds’ discussions of the

94. The alert reader may further contend that the presence in both Talmuds of a R. Yohanan/Resh Laqish dispute is attributable to early talmud. While that suggestion makes a certain intuitive sense, it must be dismissed in this case. The Bavli sugya expands the amoraic dispute by drawing on, among other sources, other materials found in the Yerushalmi that were explicitly marked by the Yerushalmi editors as being relevant to this R. Yohanan/Resh Laqish dispute. So, whether or not the amoraic dispute itself is early talmud found in b. AZ, the latter’s expansion of it draws on Yerushalmi editing. And, once we see the influence on the Bavli of the materials marked as relevant to the dispute by the Yerushalmi editors, we cannot be certain that what looks like early talmud did not in fact also come to the Bavli by way of the Yerushalmi.
mishnah. The reason for this second analytical step is identical to the first. If the observable similarity between the Talmuds is determined to be due to independent reliance on these tannaitic sources, the similarity is considered “called for by the other tannaitic sources” and is not dispositive of the question about whether the later Talmud was influenced by the earlier. And, since these materials are not dispositive of the question, they are excluded from consideration in this book. For example, t. AZ 2:695 is cited at both y. AZ 1:7, 40a and b. AZ 18b in relation to m. AZ 1:7. But each Talmud uses the Toseftan material in a radically different way. The Yerushalmi uses it to help explicate the mishnah, while the Bavli uses it as the starting-point of discussions having nothing at all to do with the mishnah. We may therefore conclude that each Talmud independently cited the Toseftan material for its own purposes. On the other hand, when tannaitic materials are found integrated within discussions shared by the Talmuds, the likely conclusion is that the common citation of these tannaitic materials is the result rather than the cause of the similarity of the overall discussion. In such a case, the similarity is “not called for by the other tannaitic sources.” For example, t. Shevi 6:2697 is cited at both y. AZ 5:1, 44c and b. AZ 62a in relation to m. AZ 5:1. Yet each Talmud’s anonymous voice integrates the Toseftan baraita into its own version of the same sugya; that is, it cites the baraita as part of a sugya explicating the mishnah rather than using the baraita as the starting-point for the formation of a sugya explicating it.

These two examples point to a key difference that is central to the question of whether a given similarity between the Talmuds is due to independent reliance on tannaitic materials or not. If one or both Talmuds use the baraita as the starting-point of new and dissimilar sugyot that explicate it rather than the mishnah, it is most likely that they cited it independently. On the other hand, if the Talmuds integrate the baraita in a sugya that explicates the mishnah, the common citation of the baraita is

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95. Tosephta: Based On the Erfurt and Vienna Codices, With Parallels and Variants (ed. M. S. Zuckermandel; Jerusalem: Wahrmann, 1975), 462. Hereinafter this work will be referred to as “Zuckermandel.”

96. Or, following Judith Hauptman, t. AZ 2:6 was part of a selection and sequence of baraitot that became attached to m. AZ 1:7 at an early stage, forming the earliest layer of “talmud” to that mishnah. See Judith Hauptman, The Development of the Talmudic Sugya: Relationship Between Tannaitic and Amoraic Sources (Lanham, MD: University Press of America, 1988).

more likely the result of the similarity between the Talmuds than its cause. Thus, if a Yerushalmi sugya appears in the Bavli, and the parallel Bavli sugya demonstrates evidence of consciously reworking the whole Yerushalmi sugya—amoraic and anonymous material as well as tannaitic—then it seems more likely that the Bavli received the sequence of baraitot as part of that sugya rather than prior to the formation of the sugya.

If a given similarity between y. and b. Avodah Zarah is neither called for by the mishnah nor by other tannaitic sources, and is not reasonably identifiable as early talmud, then it is what I call a “structural similarity” between the Talmuds. That is, the similarity is a similarity in the selection and sequence of topics and sugyot in relation to a given mishnah that is likely due to nothing other than b. Avodah Zarah’s reliance on y. Avodah Zarah. As a result of my macro analysis of the tractates, I identified nearly fifty structural similarities. Space considerations preclude the presentation of all these passages in this book, but in chapter 2 I present key examples of structural similarities that illustrate noteworthy patterns in b. Avodah Zarah’s appropriation of y. Avodah Zarah materials.98

As important as this macro analysis of structural similarities is, it is not enough to support the conclusion that the b. Avodah Zarah redactors were aware of the work of the y. Avodah Zarah redactors. These macro similarities must themselves be carefully studied in order to probe the key differences between the Talmuds. We must see whether and how b. Avodah Zarah made changes to the older y. Avodah Zarah sugyot and how it adapted them to their new Bavli context. This close analysis of particular textual parallels is what I call the “micro” analysis. Chapter 3 will present these results, offering the first systematic analysis of what may be termed the Bavli’s characteristic ways of reworking Yerushalmi materials. This work will continue in chapter 4, which will focus more pointedly on cases in which b. Avodah Zarah begins its treatment of an issue at the point at which y. Avodah Zarah ended its own, or takes up an issue that y. Avodah Zarah left unresolved.

Chapter 5 takes up the question of whether, and to what extent, b. Avodah Zarah’s anonymous (stam) voice offers clues about b. Avodah Zarah’s appropriation of y. Avodah Zarah materials. Finally, chapter 6 asks whether the hypothesis that b. Avodah Zarah was aware of y. Avodah Zarah is reasonable in light of literary, historical, and archaeological evidence from outside the Talmuds. The Talmuds did not emerge in an

98. The interested reader should turn to the Appendix (pp. 243–245) for a complete listing of all the parallel passages studied as part of the macro analysis. This listing is the source from which the textual examples were drawn for close analysis in this book.
intellectual vacuum, and any hypothesis about their relationship—no matter how plausible on the basis of textual analysis alone—that cannot be supported by a reasonable reading of the historical record beyond their pages is simply not well supported.

This book’s thesis departs from the reigning scholarly consensus about the relationship between the Talmuds. I am hopeful that skeptical readers will come away from this book persuaded that the interpretation offered here is the most reasonable reading of the evidence. But this book even has value for readers who remain skeptical because it demonstrates (at the least) that b. Avodah Zarah appropriated and reworked sizable portions of y. Avodah Zarah, and thus gives us insight into the activities of the Bavli redactors. Skeptical readers will have to articulate another hypothesis to explain this evidence that does justice to its richness and complexity.