

I pause momentarily to apologize to the reader for the gross corpulence of my reply. However, any response is a function of the critique to which it replies. Brody cites me and then often misstates what I said. I must quote Brody's citations and his misstatements and restate what I said. To prove that this is, indeed, what I said, further passages from what I had written must be adduced. Considering the uncommon length of Brody's review, the ungainly stoutness of my response is, I, hope, understandable.

In order to give full force to Brody's argument, I have cited all of his footnotes in the passages that I reproduce. Some of the footnotes pose objections to my argument. I have responded to all these objection either in the text or after the footnote itself, in the form <HS replies:>, as, for example, n. 8: <HS Replies: I shall address these....> .Since Brody's notes and mine are intertwined and this may confuse the reader, I have placed in brackets and yellowed <[Brody's note]> and further printed his notes in Times New Roman font and mine in that of Segoe UI Symbol. The reader is thus clearly positioned to observe our points of difference and judge the argument made by the disputants

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Let me first backtrack to the first section of the essay for there are numerous objections of Brody that I left unaddressed.

Brody writes: (p. 270)

Perhaps even more important for Soloveitchik than their independence in halakhic decision-making is a second point: the difference between the Ashkenazi sages' talmudic curriculum and style of commentary and those which we associate with the Geonic academies. Here things are somewhat less clear. In terms of the curriculum, Soloveitchik believes that the Ashkenazi scholars set out from the beginning to study and comment on every tractate of the Babylonian Talmud with the sole and surprising exception of tractate *'Avodah Zarah*, which they intentionally avoided in order to be able to contravene its prohibitions on various sorts of commercial intercourse with Gentiles in blissful (if willful) ignorance.<sup>1</sup> According to a unique Genizah fragment, however, various talmudic tractates – not including *Avodah Zarah* – were absent from the (Geonic) curriculum.<sup>2</sup>

<sup>1</sup> Ibid., pp. 163-165, 189-192. Brody's note.

<sup>2</sup> MS Adler 2639.46-47, published by A. Marmorstein, "Mitteilungen zur Geschichte und Literatur aus der Geniza" (second installment), *Monatsschrift für Geschichte und Wissenschaft des Judenthums* 67 (1923), pp. 132-137. Soloveitchik (*Collected Essays*, p. 187) points out that

To Soloveitchik this suggests that the "founding fathers" must have received their talmudic training in non-Geonic, although still Babylonian, academies. However, we must bear in mind that Geonic responsa and other sources of the period utilize these tractates, and they were undoubtedly studied by some members of the Geonic academies, so there is no reason to think that any "founding father" who studied these tractates must have received his talmudic training in a non-Geonic academy.<sup>3</sup>

Of course, there were people in the Geonic yeshivot who studied *Seder Kodashin* and *Nedarim* and *Nazir*, just as there have always been many talmudic scholars in every generation and in all Jewish societies who were at home in these tractates. I was not speaking of personal knowledge, but of institutional curriculum, the way a society sets up a body of knowledge which must be mastered if one is to be considered a scholar. No other diaspora set up a curriculum of all the tractates of the Talmud, only Ashkenaz did. Babylonia itself didn't. Five tractates of *Kodashim* and both *Nedarim* and *Nazir* were not in their curriculum. To be sure, if the Geonim received a question in these tractates (other than perhaps *Nedarim*), they would answer it. However, they never made its knowledge culturally obligatory. I asked why did Ashkenaz set up a curriculum different from that of the *yeshivot* from which they originated. That's not what immigrants do. They usually seek to reproduce, *mutatis mutandis*, the institutions of their homeland. Why did Ashkenaz alone do this? From this, I inferred that the founders of Ashkenaz did not come from Sura and Pumbedita. However, historical inference, habitual conduct of immigrants, institutional curriculums, not to speak of the retrospective method, are not categories in Brody's thinking. So, this entire section passed him by.

Brody then writes: (p. 271)

A third argument concerns the style of commentary. Soloveitchik lays great stress on the Ashkenazi sages' determination to produce a line-by-line commentary on the Talmud, which he contrasts with the nature of talmudic commentaries or digests produced elsewhere to the detriment of

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one could argue that this source describes the curriculum of one academy only but the curriculum of the other was broader, but I am inclined to agree that it reflects the curricula of both Geonic academies, see Brody, *Geonim*, pp. 155-156. [Brody's note].

<sup>3</sup> Consider for example volume 11 of B.M. Lewin's *Otzar ha-Geonim* (Jerusalem 1942), most of which is devoted to the tractates *Nedarim* and *Nazir* (although Lewin admittedly included much material which is not truly Geonic) and N. Danzig, *Introduction to Halakhot Pesuqot with a Supplement to Halakhot Pesuqot* (Hebrew), Jerusalem and New York 1993, pp. 193-197. Danzig is almost certainly wrong in claiming that the original version of *Halakhot Gedolot* did not include the chapter on *Nedarim* and at least some version of the chapter "*Nazir*"; see A. Shweka, *Studies in Halakhot Gedolot: Text and Recension* (Hebrew), Ph.D. dissertation, Hebrew University 2008, pp. 117-123. Geonic responsa also regularly refer where appropriate to passages in other "unusual" tractates such as *Arachin*. [Brody's note]

the latter, and which he believes shows that they mastered the Babylonian Talmud to a greater extent than the Geonim.<sup>4</sup>

I never claimed that Early Ashkenaz mastered more of the Talmud than did the Geonim, but that they incorporated more of the Talmud into their institutional curriculum, and that this shaped talmudic studies in Ashkenaz eventuating in the commentaries of Rashi and of the Tosafists on the printed page of the Talmud to this very day.

Brody continues: (p. 271)

But even if we leave aside the relative merits of various styles of commentary, we should bear in mind that all the Geonic commentaries with which we are familiar were written after the middle of the tenth century; the Ashkenazi sages were embarking on a new enterprise, and the parameters they set for themselves cannot be compared with anything produced by their Babylonian contemporaries.<sup>5</sup>

The question is not only parameters of the curriculum, but the conception what understanding Talmud meant. It entailed the detailed analysis of the entire give-and-take (*masa u-matan*) of the sugya and that included explicating every passing idea (*salka da'atakh*) of all and any *amora*. This, indeed, cannot be compared with anything produced by the heads of the two famed Babylonian academies. All of which points to a conception of Talmud study other than the one that obtained in Sura and Pumbeditha.

Brody's next paragraph begins: (p. 271-272)

Another point on which Soloveitchik lays great stress, and which in his opinion serves as further evidence of the Babylonian origins of the earliest Ashkenazic sages, is their supposed "sovereign command of Babylonian Aramaic, their ability to explicate with ease and in detail the aggadic portions of the Talmud with their vast, variegated vocabulary".<sup>6</sup> He seems to take the Ashkenazi sages' willingness to attempt to interpret the language of the entire Talmud as proof of their ability to do so successfully, and to consider any attempt to evaluate their success in this

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<sup>4</sup> See *Collected Essays*, pp. 159, 168-169. [Brody's note]

<sup>5</sup> See Brody, *Geonim*, pp. 270-272. Stampfer has found traces in one of Samuel ben Hofni's works (late 10<sup>th</sup> – early 11<sup>th</sup> centuries) of reliance on an anonymous commentary which he refers to as "the *perush*", but we can say almost nothing about the nature of this commentary or its precise date. See Y.Z. Stampfer, *Laws of Divorce (Kitāb al-Talāq) by Samuel ben Hofni Gaon* (Hebrew), Jerusalem 2008, pp. 18-19. [Brody's note]

<sup>6</sup> The quotation is from *Collected Essays*, p. 187; see the extended discussions on pp. 161-163, 210-213. [Brody's note]

endeavor superfluous. Furthermore, he excuses any errors they may have made in sweeping fashion:<sup>7</sup>

This is not to claim that all their descriptions of talmudic realia are correct or that their traditions in these matters were uniform and univocal. The settlers of Ashkenaz were thoroughly conversant with Babylonian Aramaic of the 10<sup>th</sup> century; the words that they commented upon had been spoken more than half a millennium earlier. The meaning of many words... changes over so long a period of time... Even accurate linguistic knowledge is no guarantee of the accuracy of a commentary. To recognize, for example, words as names of flora and fauna is one thing; to correctly identify these plants is another...

In other words: we may assume that any word incorrectly interpreted by Ashkenazi sages was no longer in use in 10<sup>th</sup>-century Babylonian Aramaic; furthermore, knowing that a word refers to a plant, for example, counts as accurate linguistic knowledge, even if one cannot identify the plant in question. Both of these axioms are quite problematic; I will begin with the second. On the one hand, one would expect native speakers of a language to be familiar with the names of most plants and animals in their language, and not simply to put them in a category such as "flora" or "fauna"; on the other hand, such a general sort of definition is often obvious from the context, which leads me to a broader point. It would be very desirable for a properly qualified linguist to conduct a systematic assessment of the degree to which early Ashkenazic sages can be shown to have known (or not known) Babylonian Aramaic, rather than basing themselves on familiarity with other dialects of Aramaic (e.g., that of Targum Onqelos<sup>8</sup>) and guessing the meanings of many words from context, sometimes successfully and sometimes incorrectly.

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<sup>7</sup> *Collected Essays*, pp. 161-162 n. 25. [Brody's note]

<sup>8</sup> Soloveitchik (*Collected Essays*, p. 160) makes much of the fact that Rashi apparently expected his readers to understand Targum Onqelos rather well, and thinks this proves that their ancestors approximately 140 years earlier had been Aramaic speakers. I fully agree with Berger's rejection of this argument and contention that (in Soloveitchik's paraphrase, *ibid.*, p. 212) "even without a spoken tradition, a literate person could understand Targum on the basis of his Jewish studies". I would only add that we have no way of knowing how wide a readership Rashi expected for his commentary nor whether his estimate of his readers' linguistic abilities was accurate. [Brody's note] {HS Replies: I shall address this last point in the text, see, below, p. 8.

Following Brody's sequence of arguments, I will address 'the second of these 'axioms'. I am pleased that Dr. Brody believes that most talmudic scholars understand in full its language. Most of those that I know (including professors of Talmud) don't. Few of them can read a page-long narrative of *aggadata* in the Bavli without recourse to Rashi, the 'Arukh or Jastrow. I recall the perplexity of some truly fine scholars when asked at a wedding what exactly does *arag* in the phrase *arag nikhsin* found in the text of the *ketubbah* mean. They knew, of course that this effectively meant 'idit' or 'shufra' (the finest property), but that its literal meaning was 'desirable' eluded them, even though it is in Onkelos's translation of *ki neḥmad ha-ets le-haskil* (Gen: 3:6). I already made that point in my reply to Dr. Berger and gave several examples of far more common Aramaic words in the Targum, as *akhsanta*, 'alela, tsaba' which few contemporary talmudic scholars would know off the bat. (They are respectively the translation three simple biblical words—יְרוּשָׁה, תְּבוּאָה, and כֶּבֶס.)<sup>9</sup> I am sure that Dr Brody is far better connected to the world of talmudic scholarship than I am, so I suggest that he try the same words out and see whether his learned acquaintances do any better. If Dr Brody believes that the first scholarly settlers in Ashkenaz were *not* native Aramaic speakers or brought up in homes that Aramaic was spoken, at least occasionally, why does he think that their command was superior to that our contemporaries?

Brody continues: (p. 273)

As for the first axiom, one should perhaps be troubled a priori by what seems to be an unfalsifiable claim of this nature – any expression correctly interpreted counts as evidence, while any word which is misinterpreted is simply deemed irrelevant. But in fact this proposition is falsifiable at least to some extent, as the following example illustrates. Twice in the Babylonian Talmud we find the idiomatic expression "מחו לה מאה עוכלי בעוכלא" 'they have struck it a hundred blows with an 'ukhla', meaning that a particular dictum has been thoroughly discredited.<sup>10</sup> Hayye [i.e. Hai, HS] Gaon, writing in 1015 or 1016, explains that 'ukhla is a hammer or mallet: "Know that 'ukhla in Aramaic is a hammer... and sailors in Babylonia nowadays, the names of their tools are in Aramaic, and they call the mallet with which they strike the pin 'kl' and 'kl'".<sup>11</sup> Early Ashkenazi commentators, unfamiliar with this Aramaic word, identified it with the Hebrew homograph עוכלא, a small dry measure, and translated along the lines of "they have struck it a hundred blows with a measuring cup!"<sup>12</sup>

<sup>9</sup> P. 212.

<sup>10</sup> *Ketubbot* 53a and *Baba Batra* 85b. [Brody's note]

<sup>11</sup> S. Assaf (ed.), *Gaonic Responsa from Geniza MSS.* (Hebrew), Jerusalem 1928, pp. 106-107; for the attribution and date see *ibid.*, p. 104. The definition is also supported by Akkadian, see M. Sokoloff, *A Dictionary of Jewish Babylonian Aramaic of the Talmudic and Geonic Periods*, Baltimore and Ramat-Gan 2002, p. 131. [Brody's note]

<sup>12</sup> See Rashi's commentary to *Ketubbot* 53a and the commentaries of "Rabbenu Gershom" and Rashbam to *Bava Batra* 85b. The meaning of "mallet" is also attested in *Bekhorot* 43b, where

To recap: Soloveitchik essentially rests his case on the ambitious scope of early Ashkenazi commentary, the supposed sovereign command of Babylonian Aramaic displayed by early Ashkenazi sages, and their lack of deference to the Babylonian Geonim. Even if we were to grant that the "founding fathers" of Ashkenaz were native speakers of Babylonian Aramaic, this would, in my opinion, be a totally inadequate basis for positing the existence of institutions which are never mentioned in our quite numerous sources for the period. For example, one might hypothesize that the earliest talmudists to arrive in Ashkenaz were disaffected members of one or both of the Geonic academies, whether they differed with the heads of these academies for personal or ideological reasons or both;<sup>13</sup> surely no one familiar with academic institutions would claim that ex-students of an institution cannot deviate from its approaches in matters such as those discussed here. I will suggest another hypothesis in the last section of this essay.

Let us take Brody's restatement of my position (above, p. 4) sentence by sentence:

In other words: we may assume that any word incorrectly interpreted by Ashkenazi sages was no longer in use in 10<sup>th</sup>-century Babylonian Aramaic; furthermore, knowing that a word refers to a plant, for example, counts as accurate linguistic knowledge, even if one cannot identify the plant in question.

I never stated that any word unknown to scholars of Mainz and their immigrant teachers was not part of Eastern Aramaic Jewish vocabulary in the tenth century. No person or group of people (not to speak of a very small and homogeneous group as that which migrated to the unknown corners of the German Empire) control the entire vocabulary of a spoken language. Such a claim would be patently absurd. I simply argued that to control the large vocabulary of

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"Rabbenu Gershom" and Rashi interpret the word in a totally conjectural fashion. For *`ukhla* as a small dry measure see Tosefta *Sotah* 3: 1, *Bava Batra* 5:4 and parallels. It would be easy to multiply examples in which Geonim interpreted words correctly and early Ashkenazi authors interpreted them incorrectly but in which the Geonim did not state specifically that these words were still in use at their time. The fact that the author of the medieval dictionary *Sefer he-'Arukh* used the commentaries produced in Mainz as one of his main sources cannot (pace Soloveitchik in *Collected Essays*, pp. 160-161) serve as evidence of their lexicographical accuracy; R. Nathan may have rated these commentaries more highly than they deserve, or simply collated the few lexicographical sources at his disposal, in the absence of tools which would have allowed him to assess their accuracy. {**HS replies:** I shall address these points in the text, see below, pp. 7-8.}

<sup>13</sup> We know about tensions and dissension in the Geonic academies around this time inter alia from the Epistle of Sherira Gaon (B. M. Lewin [ed.], *Iggeret Rav Sherira Gaon*, Haifa 1921, pp. 116-121); cf. *Collected Essays*, p. 184. {**Brody's note**}



the Babylonian Talmud would reasonably be an attainment of native Aramaic speakers, far less so for native Judeo-Arabic speakers. And that was one of several reasons why the scholars of early Ashkenaz, and only those scholars, could undertake the systematic interpretation of all the aggadic portions of the Talmud.

I never assigned grades to the quality of their specific interpretations. If knowing that X is a name of a plant suffices for the understanding of the talmudic narrative, then that is an accurate explanation; if it doesn't advance the comprehension of the passage, it is deficient. (We will detail this below as Brody recaps this point)

Brody footnotes (n. 8) that remark with a statement that he concurs with Berger's statement

I would only add that we have no way of knowing how wide a readership Rashi expected for his commentary nor whether his estimate of his readers' linguistic abilities was accurate.

I assume that Rashi addressed those members of his community and community-to-be, who were reading the Bible, or listening to it being read in the synagogue (the *sidra*), perhaps even reviewing the weekly portion (*ma'avir sidrah*). As for the accuracy of Rashi's estimate of his readers, I would rely upon the talents of *the parshandata*, of *the Commentator par excellence* of the Bible and the Talmud of the past millennium to estimate his audience correctly. He succeeded in assessing correctly all their other skills, why should he have erred in their linguistic ones?

Brody continues: (p. 273)

As for the first axiom, one should perhaps be troubled a priori by what seems to be an unfalsifiable claim of this nature – any expression correctly interpreted counts as evidence, while any word which is misinterpreted is simply deemed irrelevant. But in fact this proposition is falsifiable at least to some extent, as the following example illustrates. Twice in the Babylonian Talmud we find the idiomatic expression *מחו לה מאה עוכלי בעוכלא* "they have struck it a hundred blows with an 'ukhla, meaning that a particular dictum has been thoroughly discredited.<sup>14</sup> Hayye Gaon, writing in 1015 or 1016, explains that 'ukhla is a hammer or mallet: "Know that 'ukhla in Aramaic is a hammer... and sailors in Babylonia nowadays, the names of their tools are in Aramaic, and they call the mallet with which they strike the pin 'kl' and 'kl'".<sup>15</sup> Early Ashkenazi commentators, unfamiliar with this Aramaic word, identified it with the Hebrew homograph *עוכלא*, a small dry

<sup>14</sup> *Ketubbot* 53a and *Bava Batra* 85b. **Brody's note.**

<sup>15</sup> S. Assaf (ed.), *Gaonic Responsa from Geniza MSS.* (Hebrew), Jerusalem 1928, pp. 106-107; for the attribution and date see *ibid.*, p. 104. The definition is also supported by Akkadian, see M. Sokoloff, *A Dictionary of Jewish Babylonian Aramaic of the Talmudic and Geonic Periods*, Baltimore and Ramat-Gan 2002, p. 131. **Brody's note.**

measure, and translated along the lines of "they have struck it a hundred blows with a measuring cup"!<sup>16</sup>

I never deemed any word misinterpreted as irrelevant. Only those which failed to correctly explicate the talmudic text. If a mistaken interpretation explicated the text, even though it may be technically erroneous, that interpretation is valid. The example that Brody brings well illustrates that principle. The point of the passage is that the cited opinion has been decisively refuted, whether it had been pounded with a mallet (an excellent metaphor) or a measuring cup (a bad one) makes no difference. The function of that statement is the same. Indeed, the early Mainz scholars did no worse here than did Rashi (and the Rashbam). Is Dr. Brody contending that Rashi too did not know Aramaic? Pray tell then, how did he interpret the halakhah and *aggadah* of the entire Talmud?

In his three footnotes to this passage, Dr. Brody documents the correct meaning of *‘ukhla*, and adds in the final note (n. 23) the following remark:

The fact that the author of the medieval dictionary *Sefer he-‘Aruch* used the commentaries produced in Mainz as one of his main sources cannot (pace Soloveitchik in *Collected Essays*, pp. 160-161) serve as evidence of their lexicographical accuracy; R. Nathan may have rated these commentaries more highly than they deserve, or simply collated the few lexicographical sources at his disposal, in the absence of tools which would have allowed him to assess their accuracy.

I never contended that the attention given by the R. Natan of Rome evidences the *accuracy* of the Mainz commentary. I argued that the great attention, evidenced by his citation of no less than eleven different groups of Mainz scholars (*rav mi-Magentsa*, *talmid ḥakham mi-Magentsa*, *ḥasid mi-Magentsa*, etc.) shows the *importance* that he attached to the school of Magentsa (which raised the question why was such weight given to the understanding of Aramaic words by Rhineland scholars). Rabbi Natan may have been mistaken; he may not have been a connoisseur of Aramaic, as Dr. Brody contends. I don't think many talmudic scholars (both traditional and academic alike) share Dr. Brody's low opinion of the lexicographical judgment of the Ba'al he-‘Arukh.

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<sup>16</sup> See Rashi's commentary to *Ketubbot* 53a and the commentaries of "Rabbenu Gershom" and Rashbam to *Bava Batra* 85b. The meaning of "mallet" is also attested in *Bekhorot* 43b, where "Rabbenu Gershom" and Rashi interpret the word in a totally conjectural fashion. For *‘ukhla* as a small dry measure see Tosefta *Sotah* 3: 1, *Bava Batra* 5:4 and parallels. It would be easy to multiply examples in which Geonim interpreted words correctly and early Ashkenzi authors interpreted them incorrectly but in which the Geonim did not state specifically that these words were still in use at their time. The fact that the author of the medieval dictionary *Sefer he-‘Aruch* used the commentaries produced in Mainz as one of his main sources cannot (pace Soloveitchik in *Collected Essays*, pp. 160-161) serve as evidence of their lexicographical accuracy. R. Nathan may have rated these commentaries more highly than they deserve, or simply collated the few lexicographical sources at his disposal, in the absence of tools which would have allowed him to assess their accuracy. **Brody's note.**



Let us return to Brody's recapitulation: (pp. 273-274)

To recap: (1) (numbers mine, HS) Soloveitchik essentially rests his case on the ambitious scope of early Ashkenazi commentary, the supposed sovereign command of Babylonian Aramaic displayed by early Ashkenazi sages, and their lack of deference to the Babylonian Geonim. (2) Even if we were to grant that the "founding fathers" of Ashkenaz were native speakers of Babylonian Aramaic, this would, in my opinion, be a totally inadequate basis for positing the existence of institutions which are never mentioned in our quite numerous sources for the period. (3) For example, one might hypothesize that the earliest Talmudists to arrive in Ashkenaz were disaffected members of one or both of the Geonic academies, whether they differed with the heads of these academies for personal or ideological reasons or both;<sup>17</sup> surely no one familiar with academic institutions would claim that ex-students of an institution cannot deviate from its approaches in matters such as those discussed here.

Let us take the first two sentences of this four-sentence paragraph.

Indeed, the existence of yeshivot other than Sura and Pumbeditha are not to be found in the sources, but their existence can be reasonably reconstructed by the retrospective method of Maitland and Bloch, described in detail above. Unfortunately, I did not explicate the retrospective method, as it is one of the common tools of any medieval historian. It has yielded a rich picture of both Anglo-Saxon England before the Norman Conquest and the rural geography of medieval France, and there is no reason to assume that it cannot be equally productive for Bavel in the closing centuries of the first millennium. Not knowing the method employed or of its widespread application, Dr. Brody, as I noted before,<sup>18</sup> didn't understand much of my essay.

As for the concluding two sentences of Brody:

I have no quarrel with the Mainz academy being founded by "disaffected members by one or both of the Geonic academies" "who differed with the heads of these academies... for ideological reasons", which included a continuation of the Savoraic culture of political non-involvement, anonymity and an all-encompassing involvement in the entire talmudic heritage, halakhic and *aggadic*. Indeed, as I wrote before:

Let me also be clear what I mean by "a third yeshiva." I mean an institution that had its own, ancient and independent traditions of learning and, as we shall soon see, its own *Weltanschauung*. If, to use a modern metaphor, someone were to claim that the Kolel Hazon Ish had a separate wing or room in the Ponivezh or

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<sup>17</sup> We know about tensions and dissension in the Geonic academies around this time *inter alia* from the Epistle of Sherira Gaon (B. M. Lewin [ed.], *Iggeret Rav Sherira Gaon*, Haifa 1921, pp. 116-121); cf. *Collected Essays*, p. 184. **Brody's note.**

<sup>18</sup> In the first part of my Reply printed in the *JQR*, 109, 2(2019). p. 319.

Hevron yeshivah, I would have no objection. My essay concerns a distinctive culture, not to where and how that culture is housed.

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Brody opens the next section of his critique thus: (p. 274)

The most obvious problem with Soloveitchik's theory is that it depends on supposing that important talmudic academies – in his view, more than one<sup>19</sup> – existed in Geonic Babylonia but failed to leave any direct trace in the sources at our disposal. (For Soloveitchik's claim that they left indirect traces see the next section of this essay. Soloveitchik himself admits that he has no idea why Ashkenazi Jews would have remained silent about their Babylonian origins, and in fact such silence would be well-nigh incredible in view of their veneration of their ancestors and predecessors.) Babylonia in the Geonic period was not an obscure corner of the Jewish world for which we have so few sources that almost no conjecture can be rejected out of hand; at least until the very end of this period it was one of the two centers, alongside Palestine, around which Jewish life revolved, and we have numerous and varied sources which enable us to paint a relatively clear and full picture of its Jewish life. True, this picture is in many respects not as detailed as we would like, but it boggles the imagination to suppose that it included major institutions of higher Jewish learning which remained completely "under the radar" until now.

What are our sources for Bavel in this period? The *Iggeret R. Sherira Gaon*, the report of R. Natan ha-Bavli, and of course the responsa and codes of the Geonim. Rav Sherira was asked (in post-talmudic matters) about the succession of the Rabbanan Savora'i and that of the Geonim of the Sura and Pumbedita, and he replied accordingly. Why would he mention the existence of another institution? He was not asked by the Kairouan community for a survey of institutions of higher Torah leaning in Bavel. He would also not inform his respondents of the existence of other Torah establishments as one of his purposes was to strengthen both the financial and intellectual ties of Kairouan with Pumbedita? R. Natan's account is simply 'Pomp and Circumstances' and *Peoples* magazine of the financial squabbles of the high and mighty. He wouldn't deign to mention some politically nondescript institution, venerable as their traditions may have been. Nothing could have interested him less.

Regarding responsa: Is Dr. Brody sure that all the anonymous responsa in the Genizah are those of the Geonim of Sura and Pumbedita? I gave some criteria for identifying the non-Sura and Pumbedita ones. One was to see whether the anonymous response of the Geonim employ to a greater degree than the known authored ones of the four registers of Aramaic

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<sup>19</sup> See *Collected Essays*, pp. 172-173. [Brody's note]

described by Matthew Morgenstern.<sup>20</sup> Has Dr Brody attempted to sift through the agglomerated mass to see whether we indeed have responsa of the third and other *yeshivot*? Or does he simply know this without even checking? In the essay, I gave the Morgenstern test; I would now add that of Kurdistan. Do some of the anonymous responsa share to a greater degree than others the syntax and phonology of Mandaic? This would indicate a locus of scholars in the large Jewish population in the Kurdistan, near the Silk Road, substantially north of Sura and Pumbeditha and well off the beaten track of sight seekers and that of R. Natan ha-Bavli.

As to codes: Just where does R. Shim'on Kayyara fit into Dr. Brody's picture? He was unquestionably a towering scholar. His work, the *Halakhot Gedolot*, was of great influence. Arguably the most influential work produced in the geonic period until the halakhic monographs of the tenth and eleventh century. The Geonim themselves thought it probative of the correct interpretation and upshot of the talmudic discussion (*sugya*), though they took care to emphasize that it was not dispositive. Only their rulings were. Had R. Shim'on Kayyara opened a yeshiva in Bavel, I certainly would have attended, as would, I suspect, many others. Did anyone stop him from opening another yeshivah in Bavel? *Could* anyone have stopped him from doing so? Why are the existence of other *yeshivot* so difficult to imagine?

Brody's question why the Ashkenazic sources are silent about their Babylonian ancestry is a non-starter; there is no reason to be prouder of one's Babylonian origins than of one's Palestinian one. The source of pride is their intellectual descent from the Third Yeshivah with its traditions and curriculum. This is a question that I myself raised, and to which I have no answer.<sup>21</sup> Indeed, that lacuna was one of the reasons I called my essay in the title 'A Proposal'.

I don't know on what basis Brody writes: 'such silence would be well-nigh incredible in view of their veneration of their ancestors and predecessors.' If there are passages in the literature of Early Ashkenaz in print or manuscript that express 'veneration of their ancestors and predecessors', they have eluded my notice.

Truth to tell, it would appear that they did retain a tradition of who the original settlers were, at least of some of them, though it came to expression quite *en passant* in a private family correspondence precisely because no one except, perhaps, the *benei ha-Makiri*<sup>22</sup> in Ashkenaz,

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<sup>20</sup> *Studies in Jewish Babylonian Aramaic Based upon Early Manuscript Sources* (Harvard Semitic Studies, 2011)

<sup>21</sup> *Collected Essays*, II, 201

<sup>22</sup> On the *benei ha-Makiri*, see Grossman, *Hakhmei Ashkenaz ha-Rishonim* (Jerusalem, 1991), 361-385. I agree with Grossman's assessment that the entire context militates against this being any reference to a settlement in Prague. It clearly refers to the founders of Mainz.

had any veneration of their ancestors.<sup>23</sup> In a family exchange of letters one of the Makiri brothers wrote:<sup>24</sup>

This responsum was a reply of R. Natan ha-Makiri to R. Yehoshua,<sup>25</sup> and he had asked about our custom that we write the *ketubbah* on Friday [and the marriage takes place on Friday] and they sign the *ketubbah* [only] on Saturday night and at times they [the groom and the witnesses] make a *kinyan* [*sudar*] on Saturday night [authorizing the witnesses to sign] and the *ketubbah* was written before that [i.e. on Friday] and [yet] they [the witnesses] sign it [on Saturday night].

And they [not clear who joined R. Natan in the reply] strongly reprimanded him: What need is there to investigate [the correctness] of the custom of the holy congregation since the days of the sainted [founders], the elders who passed (or: passed through) (!) [*yeshishim asher 'avru*]<sup>26</sup> and the settlers [*meyushavim=mityashvim*]<sup>27</sup> of Mainz, R. Yehudah b. from Arledi [Arles], R. Shelomoh b. Matsliaḥ, and R. David b. Yakar did not challenge the practice nor protested it... and praise He who watches over Israel... and who knows for what reason they established this practice...

The respondents were understandably stumped. At issue here is a post-dated document, something which could generate serious questions as to the lien it created on landed property should difficulties arise between the couple. And a *ketubbah* is written for the very purpose of precisely of protecting a woman when the marriage goes awry. Because of the severity of the

<sup>23</sup> זו התשובה שהשיב ר' נתן לר' יהושע ששאלו—שנוהג[ים] במקומינו בשביל(!) כתובה שכותבין בששי ועומדין וחותמין במוצאי שבת ופעמים קוין גם קניין והכתובה נכתבה בששי בשבת. והוכיחו בתוחכות--מה צריך לבדוק לו על מנהג קהילות קדושות מימי קדושים אשר עברו בישישים במגנצא וגם המיושבים ר' יהודה בר' משה ור' שלמה בר מצליח ור' דוד בר יקר ולא ערערו ולא מיחו... שבח לשומר ישראל[ל]...ומי יודע על מה נהגו כך.

There is no parallel to this filio-pietistic passage in the literature of Early Ashkenaz. For an extended discussion of this subject in pre-Crusade Ashkenaz, see my essay 'Minhag Ashkenaz ha-Kadmon: An Assessment' in *Collected Studies*, ii. 29-69

<sup>24</sup> *Ma'asei Geonim*, ed., A. Epstein, (Berlin, 1910); *Shibbolei ha-Leket, Part II*, ed. M. Z. Ḥasida, *Ha-Segulah*, 1934-1937, seriatim, #59, p. 134; photo stated and distributed as separate work in 1969 with identical pagination (no place of publication given).

<sup>25</sup> An otherwise unknown figure.

<sup>26</sup> A reference to the talmudic passage (*Mo'ed Katan* 25b) *geza' yeshishim 'alah mi-Bavel*. (Perhaps also an allusion to the origins of the *yeshishim*, though I wouldn't bet on it.)

<sup>27</sup> The explanation is that of A. Grossman, *op. cit.* (above, n. 22), 370, n. 44.

problem they confronted and their understandable inability to solve it, they fell back upon their faith in the wisdom of their ancestors, a trait common to the Ashkenazic culture, provided that its extent is not exaggerated.<sup>28</sup>

Brodie continues: (pp. 274-275)

Soloveitchik attempts to explain the supposed shortcomings of the sources in the following way:<sup>29</sup>

Suppose the tsar of Russia had had a generally benevolent, or at least a neutral, attitude to the Jews, and in 1880 had conferred the status – de jure or de facto – of ultimate arbiters of things Jewish upon the *rashei yeshivah* of Volozhin and Mir. Does one imagine that Vilna, Cracow, Lublin, or Brisk (Brest-Litovsk) would have deferred to these two institutions? Or would they have indicated, in deed if not in word, that they had been handling their affairs quite well for hundreds of years and were quite capable of continuing to do so?... Mir and Volozhin would have been the fonts of rabbinic knowledge and the supreme decisors for the emerging Russian Jewish diaspora in England, America, and South Africa, but scarcely for the Jews in the Russian Empire itself.

Presumably Soloveitchik is aware that there is no basis for the theory put forward by Graetz and Ginzberg that the Geonic academies enjoyed any sort of official governmental recognition, which is why he adds "de jure or de facto", but in fact there is no evidence of any de facto recognition of the Geonic academies either.<sup>30</sup> Nor is it easy to imagine why a Muslim caliph would have had any interest in conferring such recognition on particular Jewish academies in preference to others if such existed.<sup>31</sup> But even if we were to imagine the Geonic academies were "looked upon with favor" by the caliph, why would this have made any difference to Jews living under a different government, such as the Fatimid empire? Or, to take Soloveitchik's example, why should we imagine that "Mir and Volozhin would have been the fonts of rabbinic knowledge and the

<sup>28</sup> See "'Religious Law and Change" Revisited', *Collected Essays*, I. 259-277.

<sup>29</sup> Ibid., p. 166. [Brody's note]

<sup>30</sup> See Brody, *Geonim*, pp. 337-340. [Brody's note]

<sup>31</sup> Soloveitchik (*Collected Essays*, p. 179) writes: "To the extent that the Jewish communities in the far-flung Islamic Empire were subordinate to establishments in the Caliph's backyard, they were all the more subject to his direct control." In the context it appears that he means to say that the Geonic academies moved to the newly established capital of Baghdad around the year 750 and were thus "in the Caliph's backyard", but this move took place much later; see Brody, *Geonim*, p. 36. [Brody's note.] {HS replies: I was using 'backyard' metaphorically. Sura and Pumbeditha were far closer to the Abbasid capital after it relocated to Baghdad than they were before when the Caliphate resided in Damascus.}

supreme decisors for the emerging Russian Jewish diaspora in England, America, and South Africa" because the tsar had smiled upon them? Would rabbis in those countries, including alumni of competing *yeshivot*, have renounced their ties with Vilna or Brisk, stopped sending them halakhic queries and lost interest in obtaining the writings of their *rashei yeshivah*?<sup>32</sup>

Let us take this four-sentence paragraph sentence by sentence.

1] I was well aware of Brody's point that there is no document which states that the Geonim were recognized by the Caliphate, that is why I wrote "de jure or de facto." Brody seems unaware of what led Graetz and Ginzberg to assume that had at least de facto recognition. Brody asks: "Why the Caliphs would have had any interest in conferring such recognition on particular Jewish academies in preference to others if such existed." When the Caliphate moved from Damascus to Baghdad in 750, the two major functioning *yeshivot* were Sura and Pumbedita. (The size of the Third Yeshivah is unknown throughout Geonic period.) The Caliphate would have had a deep interest in obtaining the services of the Jewish merchant community for intelligence. It travelled widely, had interests throughout the Abbasid Empire from south of the Pyrenees to the Ganges. It could provide valuable information about the countries in which they travelled and regularly did business. It could apprise of political struggles and intrigues, financial resources and tax avoidance, and report the local politics of innumerable locations in this far-flung empire. They also were dhimmis and could never mount a challenge to the Caliphs. Because of their vulnerability, Jews have often been used in administrative and intelligence gathering positions by rulers first establishing themselves. The Jews, on their part, knew the first rule of politics: If you turn to an address, you create a return address. Their loyalty and utility would stand them in good stead when they needed to appeal to the Caliph is their struggle with their religious enemies or business competitors.

2] "But even if we were to imagine the Geonic academies were "looked upon with favor" by the caliph, why would this have made any difference to Jews living under a different government, such as the Fatimid empire?"

The answer flows from the above. The Fatimids first begin their rise to power in 909. This means that for over 150 years the two *yeshivot* had been favored by the Caliphate, and their

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<sup>32</sup> Even if we were willing to explain away the relatively numerous instances in which Geonim refer to their sister academies and to no other Babylonian academies by some sort of conspiracy theory, we would hard pressed to account for the failure of a putative Babylonian outsider such as R. Nathan the Babylonian to make any mention of these institutions (cf. Brody, *Geonim*, pp. 26-30). Soloveitchik (*Collected Essays*, pp. 167-168) cites approvingly a suggestion by the late Zvi Groner that some of the anonymous responsa thought to be Geonic were actually issued by other Babylonian academies of the Geonic period, but there is absolutely nothing but conjecture to support this suggestion. [Brody's note] {HS replies: Groner's remark was not a conjecture, but a question, and one that merits investigation, which Brody, as of yet, has declined to do.}



reputation as the central yeshivot of the Islamic empire had been long established. Moreover, Sura and Pumbedita in Bavel itself—the mother-country of the other settlements on the southern and western rim of the Mediterranean—were *mekuravei malkhut*, institutions long favored by the dominant political establishment for a century and a half, and a long-established friends and fixers in a foreign government is far better than no friend at all-- at home or abroad. The Jewish communities in the diaspora had every reason to give great weight to their words.

3] Or, to take Soloveitchik's example, why should we imagine that "Mir and Volozhin would have been the fonts of rabbinic knowledge and the supreme decisors for the emerging Russian Jewish diaspora in England, America, and South Africa" because the tsar had smiled upon them?

For the same reasons, *a fortiori*, that the communities in the Fatimid Empire looked upon them as fonts of rabbinic knowledge. They had long established reputations as the most prestigious Torah centers and with the most political clout.

4] Would rabbis in those countries, including alumni of competing *yeshivot*, have renounced their ties with Vilna or Brisk, stopped sending them halakhic queries and lost interest in obtaining the writings of their *rashei yeshivah*?

This is exactly the point of my essay. If other yeshivot existed in Bavel (and to that contention the bulk of my essay was devoted), there is no reason whatsoever to assume that they did not have the allegiance of their alumni and that many of the anonymous responsa in the Genizah were authored by these *rashei yeshivah*; especially, if a very important yeshiva continued the policy of anonymity of the Sabora'im/Setama'im.<sup>33</sup>

Brody continues: (pp. 275-276)

Soloveitchik also argues for the existence of numerous Babylonian academies in the Geonic period in the following manner:<sup>34</sup>

While hard numbers are notoriously difficult to come by, the clear impression in the Talmud is of a large Jewish population. There is no reason to assume that the numbers dropped in the geonic period. Is it reasonable to assume that only two yeshivot served such a large community? Scholars attended the gathering of *kallah* in Sura and Pumbedita, where tractates, or chapters thereof, were studied intensively... These scholars came intellectually equipped. Where did they receive their education? Clearly, other yeshivot or battei midrash of all sorts provided this instruction. Why should we assume that they were all of a 'secondary school' level?

<sup>33</sup> See our remarks in 'The Third Yeshivah of Bavel' at pp. 175-6.

<sup>34</sup> *Collected Essays*, pp. 194-195. [Brody's note]

One might as well ask: Is it reasonable to assume that only the universities of Cambridge and Oxford served the much larger population of England in the late Middle Ages?<sup>35</sup> Higher education in general, and talmudic learning in particular, was the preserve of a very small portion of the population in pre-modern times, and the fact that the Jewish community of Babylonia was able to maintain two large and well-staffed academies more or less continuously throughout the centuries of the Geonic period is remarkable enough. Furthermore, we know that these academies were supported by taxes on Jewish communities throughout a wide geographical area extending beyond the borders of present-day Iraq;<sup>36</sup> how would other academies have been financed? As for the question where the scholars who attended advanced sessions at Sura and Pumbedita received their earlier talmudic training, we do not know for sure, but it is possible that this also took place within the framework of the Geonic academies.

I wouldn't use medieval England as a basis of comparison with Abbasid Bavel. However, let us deal with the issue as Brody presents it. Oxford and Cambridge, indeed, serviced a larger population, but the curriculum was such that their admission, in effect, was restricted to those who sought to enter the administration of Church or government or follow the path of theologians, Romanists and canonists. By home instruction, by a host of local schools (later known as 'petty schools') where, under a single teacher, the rudiments of reading and perhaps writing and arithmetic were imparted, grammar schools, cathedral schools, and some monastic schools which serviced the local needs--reading the breviary and understanding some of the basic vocabulary of the Latin service, especially providing basic, barebone education for those who aspired to enter the monastic order.<sup>37</sup> There is no reason to think that Jewish Babylonia, or post-Usha Roman Palestine, had not, *mutatis mutandis*, an equally wide instructional 'network'. The large Jewish communities of Eastern Europe had one for well over half a millennium.

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<sup>35</sup> According to D. Herlihy's entry "Demography" in *Dictionary of the Middle Ages*, ed. J.R. Strayer, New York 1984, IV, pp. 136-148, at p. 141, an earlier estimate of 3.7 million for the population of England in the early fourteenth century now "appears much too conservative. Estimates of England's population at its medieval height now range as high as 7 million." Estimates of the medieval Jewish population are largely based on guesswork, but are mostly in the range of one million worldwide. [Brody's note] {HS remarks: I would genuinely like to know what is the basis of this last estimate. }

<sup>36</sup> See Brody, *Geonim*, pp. 58-59, 125. [Brody's note]

<sup>37</sup> See N. Orme, *Medieval Schools* (Yale, 2006), 13-218; *idem*, *Medieval Children* (Yale, 2002), 237-72. 'Petty schools', D. Turner, *The Old Boy Network: The Decline and Rise of the Public School* (Yale, 2015), 2-3.

As for financing, the other *yeshivot* would have been supported by contributions of alumni or by the local communities in which they found themselves, as did *yeshivot* or *kollelim* for some half a millennium in Eastern Europe and now in America. As we have no idea of the size of the other *yeshivot* in Bavel, we have no way of knowing what this expenditure entailed, but there is no reason to assume that it needed a centralized *rashut*, as did the major institutions of the Geonate, Sura and Pumbedita.

I have no objection to the two famed *yeshivot* of Bavel reaching out to provide elementary instruction to their co-religionists. I only claimed that there is no reason to assume that other institutions of higher learning might not have done the same.

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We now come to Brody's critiques of my use of talmudic manuscripts and the implications that they have for the existence of *yeshivot* other than those of Sura and Pumbedita. Let us sort out Brody's critique of my notion of editing and copy editing of the Talmud from any factual presentation of mine. The writings of mine that are criticized by Dr Brody are found in a preceding article in the same volume 'Communications and the Palestinian origins of the Ashkenazic Community'.<sup>38</sup> The thrust of the article is that the alleged Palestinian origins of the Ashkenazic community are based on a misguided perception as to the location of the Ashkenazic community in the first half of the Middle Ages. Ashkenaz is perceived as the tip of an isolated community whose only connection to Palestine was via an umbilical cord that goes from Mainz to Lucca to Byzantine Otranto to Byzantine Palestine. In reality, Mainz was the hub of international trade to the Near and Far East. A Muslim traveler around 930 wrote wonderingly, how astonishing it was to find in the market of Mainz 'which sits on a river called Rin' coins from Samarkand and spices from India. Not surprising, as Mainz was opposite the Imperial court at Ingelheim, the capital of the first Carolingians and of Ottonian Empire. It was the great emporium of Western Europe at that time, and there alone could one find buyers rich enough to purchase the products of one- and two-year trips to the lands of the distant East. Both Jews and Christians were in the far West, while their religious origins and their sacred narratives were in the East. It is not surprising then that a rich market developed equally for spiritual goods from the East—*reliquia*, which had been studied by Michael McCormick, and I reproduced some of his maps.<sup>39</sup> Jews were not interested in the staff of Mosheh Rabbenu or the mantle of Eliyahu ha-Navi'. They were interested in anything that told them of the nature of their God who so differed from that of their Gentile neighbors--as the *Shi'ur Komah*, the nature of his celestial abode--as

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<sup>38</sup> Ibid., 122-44.

<sup>39</sup> *Origins of the European Economy: Communication and Commerce (700-900)*, (Cambridge, 2001). I have reproduced some of these maps for the readers convenience in the photostats appended to this online Reply, pp. 1-3.

the *Sifrei Heikhalot*, and, of course, any scrap of information of the law by which they lived which so set them apart from their Christian acquaintances. A copy of the *Halakhot Pesukot* or *Halakhot Gedolot*; a tractate of their Talmud which had begun circulating in the ninth century would fetch a great price from Jews at the Mainz fair. I then cited Vered Noam's article which summarized the work of a generation or two that had been done on early manuscripts of the Talmud that were in circulation at the time, writing:<sup>40</sup>

[E. S.] Rosenthal has noted that there are two manuscript traditions [of the Talmud]: an eastern one, [best] reflected in the writings of R. Ḥanan'el [of Kairuoan], and another widespread version, which he called the "vulgate", which is reflected not only in the writings of Rashi and the Franco-German Tosafists, but also in Spanish manuscripts and even in very old eastern manuscripts and Genizah fragments. This would indicate that the split in the traditions had already occurred in the East, and that the Ashkenazic tradition is an eastern one. [S.] Friedman has found that the Ashkenazic manuscripts of tractate *Bava Metsi'a* reflect the same text as that found in the writings of the Babylonian Geonim. [M.] Segal's researches have revealed remarkable similarities between the Ashkenazic version of tractate *Megillah* and fragments from the Genizah. A striking likeness has been found to exist between the superb Sephardic manuscript of tractate *Megillah* (located in Göttingen) and the Franco-German textual traditions. [M.] Sabato has discovered two clear textual traditions in tractate *Sanhedrin*: an eastern one reflected in the Yemenite manuscripts and in the works of Rabbi Yitshak of Fez (Alfasi); the other reflected in the Ashkenazic tradition, which is mirrored, surprisingly, in the version used by Rabbenu Ḥanan'el [of Kairouan] and that of R. Me'ir Abulafia [Ramah] of Toledo. He further surmised that the split had taken place quite early and, in the East, and that this eastern version somehow got to Ashkenaz. This tradition has readings as good as [the Yemenite one] and at times even superior [to it]. The general picture that emerges from all these 'partial' studies [of individual tractates] is confirmed by a broad examination of the orthography of [the majority of] extant talmudic manuscripts. Friedman's morphological study has shown that many of the so-called 'Palestinian' spellings are, in fact, Babylonian, and that to a large extent this orthography is found in late Ashkenazic manuscripts. These manuscripts preserve many of the distinctive Babylonian spellings, as do the [highly regarded] Yemenite manuscripts.

Does Brody challenge the work of all these scholars? Is he claiming that manuscripts of the Talmud were not circulating throughout the Diaspora at this time? If so, he has a lot of disproving to do.

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<sup>40</sup> P. 123-124.

What use did I put this data to? I wrote:<sup>41</sup>

The map of the slave trade shows many roads leading to Baghdad and on the map of communication and commerce numerous lines converge on Aachen, the capital of the Carolingian Empire, as well as on the Rhineland, a major pillar of the Ottonian Empire. Is it at all surprising that a superb Spanish manuscript of tractate *Megillah* has many readings typical of Ashkenazic manuscripts or that there are striking similarities between readings in Ashkenazic manuscripts and those found in the Cairo Genizah? Is it any wonder that the Ashkenazic manuscripts of tractate *Sanhedrin* reproduce scribal traditions of the Maghreb (R. Ḥanan'el) and of Spain (R. Me'ir Abul'afia), or that Rashi's textual emendations to that tractate reflect a text in part similar to that found in Yemen? With spices came books and even, perhaps, as Noam has surmised, commentarial traditions. Yemen and Ashkenaz, seemingly the antipodes of the Jewish world, were linked in this period by ongoing commercial contacts.

Is Brody claiming that there were no ongoing contacts between Mainz and Iraq and India? Is he denying that goods from these distant countries streamed continuously into the great emporium of Mainz? If not, why does the notion of numerous manuscripts from the various communities of the Diaspora circulating in Mainz strike him as problematic? Why does he have any difficulty with the conclusion that a Yemenite manuscript (or one copied from or strongly derivative of a Yemenite manuscript) was available in Mainz in the tenth and eleventh century, and that Rashi, who studied in Mainz, took care to take a copy of it back to Troyes? He may believe that Rashi emended on his own, but the alternative suggestion that if 70% (or even a smaller but still large percentage) of Rashi's emendations correspond to what is found in a Yemenite text, he had a copy of that text (or one very much like it,) is scarcely far-fetched.

Let us now address Brody's critique of my 'copy-editing' and 'inscription' theory of talmudic inscription (p. 279). He writes:

Soloveitchik seeks to deflect the anticipated criticism of his theory based on the absence of any sources which mention non-Geonic talmudic academies in Babylonia during the Geonic period by attempting to demonstrate indirectly the existence of such academies, which for some reason the Genizah (and, I would add, other sources), completely ignored. He does this by means of what he calls the "copy-editing" and "inscription" of the Babylonian Talmud. In his formulation:<sup>42</sup>

For centuries everyone assumed that the written versions of the Talmud that we possess originated in Sura and Pumbedita. There was no

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<sup>41</sup> P. 140.

<sup>42</sup> The citation is from *Collected Essays*, p. 172, and is part of an extended discussion of Talmudic transmission on pp. 170-175, cf. pp. 197-198, 204-206. [Brody's note]

reason, therefore, to think that any other institutions were involved in the creation of the written talmudic corpus, or even to contemplate their existence. Once this assumption has been discredited – and discredited thoroughly in 2005 by Sussmann's great article – the conclusion appears inevitable. The initial inscription of this great corpus in the pre-geonic period (before 700-750) and its two final copy-editings – the manuscripts divide into two versions – in the geonic era took place *outside* Sura and Pumbedita...The massive undertaking of copy-editing almost every line of the talmudic corpus was carried out in Bavel in this era in two different locations, in two different institutions – yet there was no reference to any of this in the Genizah. (p. 172)

In my opinion, "there was no reference to any of this in the Genizah" for the simple reason that none of it took place. The article by Sussmann which Soloveitchik highlights is in fact important but it does not assert, let alone prove, that the academies of Sura and Pumbedita could not have been "involved in the creation of the written talmudic corpus".

Of course, Sussman does not say that the two famed yeshivot of Bavel did not inscribe their text of the Talmud. Yet consider the situation. The rise of the Abbasid Empire both creates the Geonate and at the same time renders it an embattled institution. It must wage a war with the Exilarchy, with the Geonate of Palestine and equally establish its authority over the new and rapidly growing Jewish communities scattered across the vast expanse of Islam. The Geonate's only claim to supremacy is that they possess the authentic text of the Talmud, the *Vox Talmudica*, the source of meaning and order to Jews the world over. It alone is guaranteed by time-tested controls of the memories of the numerous *garsanim* who recited the text in the great assemblies of *kallah* and for the various study groups that studied within the walls of Sura and Pumbeditha. The Age of Orality is over and that of Inscription has arrived, and the Geonim though having opposed it longer than most, recognize the inevitable. They refer with equanimity to these written texts, insisting only that if any contradiction be discovered between these inscriptions and their oral traditions, their oral traditions are in the right. If they themselves were to inscribe a text of the Talmud, give their stamp of approval--their imprimatur--to a written transcript, their supremacy would rapidly erode. All could make equal claim to word of the Talmud and test their traditions and authenticity by that word. Rabbinic leaders in such communities such as Kairouan, long restive under the Geonic hegemony could rise to shake off the Geonic yoke, in fact if not in word. Such surrender by Sura and Pumbeditha would have been institutional suicide. And suicidal the Geonim were not.

Brody concludes the paragraph thus:

– nor could it [Sussman's article assert, let alone prove Soloveitchik's claim that the academies of Sura and Pumbedita could not have been "involved in the creation of the written talmudic corpus"], as we have evidence that explicitly contradicts this claim.



He later documents this claim, and we will address it there.

Brody continues: (pp. 280-281)

Furthermore, I believe that the entire model of an "initial inscription" of the Talmud before 750 followed by "massive copy-editing" which must have been carried out "in two different institutions" in Babylonia is completely unsupported by the evidence and in fact is largely anachronistic. Soloveitchik writes:<sup>43</sup>

Some have entitled 'transmission' what I have here called 'copy-editing'. The difference in terminology may be important to some views of the history of the talmudic text. From the point of view of this essay it is irrelevant. Call it what you will, various centers were empowered to give the final shape to a 'fixed but fluid text, fixed in content and basic formulation but open to rephrasing'.

The difference between 'copy-editing' and '(oral) transmission' is, however, crucial. The relevant aspect of Sussman's article – and this is not where it innovates – is that for the Babylonian academies the preferred mode of transmission of the Talmud was oral recitation. I have further emphasized that – as might be expected in such a context – the text was not fixed at the lexical level. As Friedman, quoted by Soloveitchik, says, the text was "fixed in content and basic formulation but open to rephrasing". Not only was there in practice no fixed text, but there was no aspiration to establish such a text. Numerous individuals recited various portions of the Talmud, and any of them could introduce changes in wording. There was, so far as we know, no system for supervising the reciters and, although much of the oral recitation of the Talmud undoubtedly took place within the milieu of the Geonic academies, it was carried on outside the academies as well. In general the Geonim downplayed the significance of such variants; only rarely did they stigmatize a "reading" as erroneous, on one occasion attributing the error to "the reciters in the villages".<sup>44</sup>

Furthermore, anyone who knew the Talmud or a portion of it by heart could write that text down (or dictate it to someone else who would do so), whether for his own convenience or for the use of others. The "inscription" of the Talmud was not a one-time event and we have no way of knowing how many times a given tractate was reduced to writing, even if it may be that in many cases all surviving copies were generated by one or two initial inscriptions.

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<sup>43</sup> *Collected Essays*, pp. 173-4; the citation at the end is from a personal communication with Shamma Friedman. [Brody's note]

<sup>44</sup> See R. Brody, "Sifrut ha-Geonim v'ha-Tekst ha-Talmudi", in *Mehqerei Talmud*, I, Jerusalem 1990, 237-303, at 238-240. For the reference to the "reciters in the villages" see B.M.Lewin (ed.), *Otzar ha-Geonim*, 5, Jerusalem 1933, *Megillah*, no. 11. [Brody's note]

However, Brody ignores the basic point that I made. For all the various rephrasing and textual fluidity, there are strikingly few legal differences between the texts. And I argued:<sup>45</sup>

The authority to give final form to the central and authoritative of a religion is no minor matter. As any lawyer can tell you, draftsmanship can be determinative, and no junior scholar would be authorized to undertake such a task.

If, as Brody describes the situation, scholars and self-proclaimed scholars, inside and outside the two famed yeshivot, inside and outside of Bavel were busily inscribing their well- or poorly-remembered texts of the Talmud or tractates thereof, we are astonished to find that there is so little difference between the surviving manuscripts. It's simply inconceivable that in such a babble of conflicting voices, the 'content and basic formulations' have remained 'fixed' and that the 'rephrasing' has altered next to nothing. There is no way that a legal text can remain unaffected by such inscriptional chaos. I suggest that he take a million and a half word treaty or piece of legislation, such as the Affordable Care Act of Obama, have it 'copy-edited' or 'transcribed' (as he chooses) by all the sundry inscribers and transcribers, good and bad, that he has depicted above and see if the 'content and basic formulations' of the original 'remains fixed'. It's simply impossible. The simplest difference in a legal text, not to speak of a difficult and involved legal text can have the greatest implications. With all respect to my distinguished colleague, he does not subject his theories to common-sense judgment, something a historian must do in all waking moments. Dr. Brody is a textual scholar, and if all is well with a theory of his, how such a construct would work in practice-is irrelevant.

Moreover, the central point of my argument seems to have eluded Dr. Brody. Call it what you want-- 'editing', 'inscription', 'copy-editing', or 'snarking', if you will, —the result was a text which is indeed extraordinarily 'fixed in content'. This could only happen if all those connected with this 'snarking' were scholars of the highest caliber. Anything less would have reduced this 1.5 million-word work to chaos. Seeing that it is implausible that Sura and Pumbeditha themselves were engaged in this operation, and that at least two, possibly three, versions (i.e. 'editings') of the work have been detected, this means that there were four to five major institutions, academies, schools, assemblages or whatever, involved in the 'snarking' of the Talmud. These I entitled the third, fourth and fifth *yeshivot* of Bavel, and which evidence the polycentric and multi-vocal nature of the rabbinic culture of Bavel in the Geonic time.

Brody continues:

Rather than a model of inscription followed by copy-editing we should think in terms of a stage of oral transmission which gave rise to numerous variations in wording, followed by a stage of "inscription" and a still later stage of written transmission.<sup>46</sup>

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<sup>45</sup> P. 174.

<sup>46</sup> I say "stage" and not "period" because there was a considerable chronological overlap; for much of the Geonic period the Talmud was transmitted primarily in writing outside the Geonic milieu and orally within it. See Brody, *Geonim*, pp. 240-244, 278-281. [Brody's note]

I would fully subscribe to this portrayal, and I don't see how it substantively differs from the description that I gave in 'The Third Yeshivah of Bavel'.

Dr. Brody then writes:

More concretely, the sources at our disposal contradict Soloveitchik's assumptions that the dissemination of written versions of the Talmud must have been the work of institutions and that these institutions could not have been the academies of Sura and Pumbedita. The earliest occasion reported by our sources on which the Talmud was written down was a Babylonian émigré writing the Talmud down from memory after arriving in Spain about the middle of the eighth century, and the second such occasion was when Paltoy Gaon, head of the academy of Pumbedita in the mid-ninth century, is said to have sent a written text of the Talmud to questioners who had requested it.<sup>47</sup>

Granting the Gospel-like truth of these accounts, there are some developments in the textual history of the Talmud still unaccounted for. How did Yemen obtain its copies of the Talmud? Clearly, from Spain (that is where the émigré wrote it down and that is where the request to R. Paltoy came from) —yet the Yemenite texts of the Talmud display little kinship with the Hamburg manuscript of *Bava Kamma*, *Bava Metsia*, *Bava Batra*, which Kutscher and his school considered the best Spanish manuscript (and hence the closest to what that émigré wrote and what R. Paltoy sent);<sup>48</sup> nor do those Spanish manuscripts which Shamma Friedman has shown to be the most authentic bear much resemblance to those of Yemen.<sup>49</sup> And closer to (my) home, how does one account for the scroll (!) manuscript -- the length of which is no less than ten folio-size pages of the printed Talmud of *Hullin* (fos. 101a-105a) -- published by Shamma Friedman of Eastern (i.e. of Byzantium and what is now Transjordan and Iraq) origin that shares the same rough syntax and loose popular (*vulgus*) syntax of the Ashkenazic manuscripts of the Talmud

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<sup>47</sup> Brody, "Sifrut ha-ge'onim," 242–43. I know of no sources to support Soloveitchik's assertion (quoted above) that the "initial inscription" of the Talmud took place "in the pre-geonic period (before 700–750)," despite the convoluted attempt by David Rosenthal, "Mishnah 'Aboda Zara'" (Hebrew; Ph.D. diss., Hebrew University, 1981), 1:101–6, to show that Talmud texts were being written by 688. [Brody's note] **HS Replies** As to Rosenthal, see our remarks in the first part of our Reply in JQR 109 (2019), 298. I relied on Rosenthal for my "pre-geonic" date of inscription.

<sup>48</sup> Cod. Ebr. 19 (Cat. #7106), E. Y. Kutscher, *Meḥkarim be-ʿIvrit u-ve ʿAramit* (Jerusalem, 1967), 252–55.

<sup>49</sup> 'Kitvei ha-Yad shel ha-Talmud ha-Bavli--Tipologiyah shel Ktiv', *Meḥkarim ba-Lashon ha-ʿIvrit u-vi-Leshonot ha-Yehudim Muggashim le-Shelomoh Morag*, M. Bar-Asher, ed. (Jerusalem, 1996), 163–190.

(including, in this case, the standard printed text of *Ḥullin*)?<sup>50</sup> Ashkenaz and Yemen seem to be getting their texts from Bavel, and it was in that spirit that I portrayed them in my essay. The same caravans or ships that carried manuscripts could carry other things as people with outlooks on life and, as Vered Noam would have it, with traditions of interpretation.

Brody concludes this section by writing of the first account in the just cited paragraph, that

‘the earliest occasion reported by our sources on which the Talmud was written down was a Babylonian émigré writing the Talmud down from memory after arriving in Spain about the middle of the eighth century’

thus:

The first of these reports thus illustrates how inscription of the Talmud could take place independently of institutions.

All one-and-a-half million words of it--accurately.<sup>51</sup>

In the section that Brody has critiqued above, I had sought to point out that historians and Talmudists had not been communicating with one another, that the great progress that has been made in understanding and mapping the evolution of rabbinic texts had been operating in isolation of the cultural history of the Jewish settlement in Iraq, and that each had much to learn from the other. This, however, seems to have passed my distinguished critic by, as did the polycentric nature of the Torah studies in Bavel and the multiple institutions of inscription of rabbinic literature, the ‘portable homeland’ of the Jews, central themes of my essay.

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Brody opens the next part of his critique thus: (p. 282)

There is a further argument that, to my mind, makes it extremely unlikely that one can trace the origins of Ashkenazi talmudic culture to "founding fathers" who were Babylonian emigrés and had studied in

<sup>50</sup> ‘An Ancient Scroll Fragment (*bḤullin* 101a-105a) and the Rediscovery of the Babylonian Branch of Talmudic Hebrew’, *Jewish Quarterly Review*, 86 (1995), 50-90.

<sup>51</sup> 1,452,440 to be more precise, and that is without the *mishnayot*. With the *mishnayot*, he wrote down from memory 2,059,000 words. See Y. Elman, ‘Orality and the Redaction of the Babylonian Talmud’, *Oral Tradition*, 14 (1999), 68-69.

academies which were more conservative than those of the Geonim. The Geonim, although willing to accept various (oral) versions of the Bavli as legitimate, and sometimes preferring one over another or rejecting one as corrupt, virtually never rejected the textual tradition they had received.<sup>52</sup> I think it was essentially inconceivable to them that the living tradition of which they were the latest embodiment, and which stretched back to the promulgation of the Talmud, could have been corrupted and require emendation. The attitude of early Ashkenazi scholars to textual emendation was so fundamentally different that I find it very hard to imagine it originating in any Babylonian institution which saw itself as a more faithful representative of talmudic Bavel than the Geonic academies. Their proclivity for emendation reflects a degree of suspicion of their received text which would be much easier to explain if they received it as a written text and were aware that any manuscript is likely to contain interpolations and other sorts of scribal errors.

I agree fully with Brody's facts, indeed cited them in his name in my essay, but drew a different conclusion.<sup>53</sup>

In conclusion I would only add that Robert Brody has observed to me that, while the Jews of Spain or North Africa did not easily entertain the notion of multiple texts of the Talmud, variant readings are part and parcel of the Ashkenazic commentarial tradition. He is unquestionably correct. Textual variants in Ashkenaz were taken almost as a given and deciding between them was perceived as an inevitable component of the exegetical enterprise. Authority seeks to speak in a single voice, and the impression given in the responsa that issued forth from the two great yeshivot is that there is one authoritative text of the Talmud, and while written texts do circulate, the living Vox Talmudica is to be found within the four walls of Sura and Pumbedita. The Third and other yeshivot of Bavel had no need to speak in authoritative tones and were actively involved in 'editing' (and expounding) the written texts of the Talmud. They knew only too well the measure of fluidity of the text and even the

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<sup>52</sup> The first hesitant steps in such a direction can be found in a few responsa of Hayye Gaon at the very end of the period, decades after the time when Soloveitchik believes the "founding fathers" left Babylonia for Ashkenaz. Furthermore, as Hayye reports, such attempts aroused great resistance among the "old guard". See: Brody, *Geonim*, pp. 157-159; U. Fuchs, "Rav Hai Gaon's Emendations to the Talmud: A Study of his Commentary on Berakhot 59b-60a and Other Sources" (Hebrew), *Ta Shma: Studies in Judaica in Memory of Israel M. Ta-Shma*, ed. A. Reiner et al., Alon Shevut 2011, pp. 601-626. (Brody's note)

<sup>53</sup> Pp. 196-197.

occasional differences between the versions, and they imparted this awareness to the Ashkenazic community.

Indeed, one may wonder whether the men of the Third Yeshiva and other *battei midrash* in Bavel shared this aversion to writing, which Sussmann has so magisterially chronicled. Sussmann himself wondered why this orality persisted long after both Christian and Muslim cultures had turned to inscribing their canon, and he hesitantly proffered some suggestions. One may also propose that the orality of a vast text furthers a monopoly of authority. Texts can be transported to distant lands and commentaries then written which open them to the understanding of the broader public. A recited text is inhospitable to commentarial exposition, and how many people exist who have phonographic memories and can accurately recite verbatim huge amounts of 'text', especially if it lacks the rhythms and alliterations of poetry? Diffusion of the 'text' of the Talmud is thus sharply limited and its explication greatly complicated. Furthermore, who is to certify these 'reciters' in a distant country, and what guarantee is there that errors have not slipped into their repertoire over the course of the years? *Tanna'im* (reciters, Hebrew) and *garsanim* (reciters, Aramaic) function best in temples of authority, ancient centers of learning, which had in the past, when orality was obligatory, developed the necessary controls to ensure the integrity of the transmission, true and tried techniques that are still in place. If Sura and Pumbedita were to establish the authority of the Bavli over the far-flung Diaspora, they had to project their uniqueness and authority by all possible means. Working quietly in the hinterlands and making no claims to power, what need had the other yeshivot and *battei midrash* of Bavel of the mystique of orality?

Brody proceeds: (p.282)

Ashkenazi scholars have often been described in modern scholarship as textual anarchists who treated all received texts as if they were their own compositions and rewrote them at will.<sup>54</sup>

Let us stop at this sentence and dispel a widespread misconception. Ya'akov Sussman's great studies, to which we have frequently referred, have generated more misunderstanding in this generation than anything else in talmudic studies. The common view is that Ashkenaz edited with a bold hand, indeed rewrote entire texts to adjust it to their traditions and preconceptions. The prize exhibit is Sussman's set of articles on the Yerushalmi of *Shekalim*, where he demonstrated that text that circulated in Ashkenaz was

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<sup>54</sup> See Y. Sh. Spiegel, *Amudim be-Toledot ha-Sefer ha-Yvri: Haggahot u-Maggihim*, Ramat Gan, 1996, pp. 100-101, and the sources cited by Spiegel [in the next chapter, HS] in notes 43-46. [Brody's note]



an edited text of the original which was been altered to align it with *sugyot* of the Bavli on *Shekalim*.

First, let it be noted that this is a phenomenon with one tractate only. There was no such editing process in any other tractate of *Mo'ed* of the Yerushalmi, or in *Seder Nashim* or that of *Nezikim*. Put differently, *Shekalim* is an anomaly. Second, this anomaly occurred only with a non-normative text. There is no evidence of this happening with any tractate of the *Bavli*. Finally, and most importantly, most didn't read Sussman's essays carefully. (I grant you reading Sussman's voluminous articles with all their magisterial footnotes and appendices is not one of the easier tasks in Jewish Studies.) I refer them to '*Masoret-Limmud u-Masoret-Nusakh shel ha-Talmud ha-Yerushalmi-le-Verur Nushkheoteha shel Yerushalmi Shekalim*',<sup>55</sup> p. 37, n.120a, added while the essay was in galleys.

After the essay was already in production, Sussman pieced together from fragments of the Genizah originating in the East (i.e. in Byzantium and Islamic East) an entire page in which the original text of *Shekalim* is transcribed and then on the margins, a different Eastern hand, aligned the same text to accord with Babylonian dicta on *Shekalim*. In other words, as with so many other so-called 'ashkenazic' texts, the heavily edited version of *Shekalim* originated in the East! Ashkenaz (and Provence) receive that one and faithfully transmitted it, as they did the other talmudic texts they possessed, without making any changes or alterations. Other Jewish communities in the Diaspora received the other text. Ashkenaz is a commentarial culture and not an 'editorial' one. Any notion that Ashkenaz edited any tractate of either the Bavli or the Yerushalmi is without foundation. When Ashkenaz saw a contradiction between sources, they used dialectic or offered alternative explanations of one of the *sugyot*. These two modes of resolution are found on every page of the vast tosafist literature.

Now let us turn to the continuation of Brody's remarks: (p. 282-284)

Recent studies by Noam and others have provided a necessary corrective by demonstrating that in a number of cases textual traditions which are most amply attested in Ashkenaz actually have older, often Oriental, roots.<sup>56</sup> This of course does not mean that these textual traditions, once they reached Ashkenaz, were preserved in pristine form. There remains considerable evidence that Ashkenazi scholars were particularly ready to emend

<sup>55</sup> *Meḥkarim ba-Sifrut ha-Talmudit: Yom 'Iyyun le-Regel Mel'ot Shemonim Shanah le-Sha'ul Lieberman, 8-9 be-Sivan, 1978* (Jerusalem, 1983), 12-76.

<sup>56</sup> See: V. Noam, "Early Version Traditions in Rashi's Emendations of the Talmud" (Hebrew), *Sidra* 17 (2002), pp. 109-150"; Shweka (n.16 above), p. 361; R. Brody, *The Textual History of the She'iltot* (Hebrew), New York and Jerusalem 1991, pp. 134-136. [Brody's note]

and rework received texts, including Rabbenu Gershom's famous ban on emendation and R. Tam's no less famous attack, two centuries later, on those who emend the text of the Talmud.<sup>57</sup>

This is Noam's position as well. Immediately after summarizing the research of several decades which serves to correct the earlier picture, she writes as follows:<sup>58</sup>

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<sup>57</sup> For both of these see S. Schlesinger (ed.), *Sepher Hayashar by Rabbenu Tam* (Hebrew), Jerusalem 1959, p. 9.[Brody's note]

<sup>58</sup> Noam ("Early versions Traditions in Rashi", p. 112. The lengthy passage from *ibid.*, p. 111, which Soloveitchik quotes in *Collected Essays*, pp. 123-124, shows that at least in many cases what appear to be Ashkenazi branches of the textual tradition have their roots in the East and were not as it were created *ex nihilo* in Ashkenaz, but it does not prove that "either the Babylonian material that reached Yemen, the Maghreb (Kairouan), and Spain equally arrived in Ashkenaz, or Ashkenaz received its traditions from these locales" (*ibid.*, p. 124). [Brody's note] {HS replies: From where then did they receive them? If Maghrebi texts come from Maghreb; Yemenite texts come from Yemen, Spanish text from Spain, etc. and none come from Bavel, how does Brody explain the Eastern scroll(!) of the length of ten folio-size pages of the Talmud, that closely conforms to the standard Ashkenazic printed one that S. Friedman published and analyzed in his article in the *Jewish Quarterly Review*, cited above, n. 50? Friedman's conclusions are supported by everything that we know of trade and communications in the closing centuries of the first millennium, to which I devoted an entire chapter in my book under review (pp. 122-144)? Does Brody deny that spices, brocades and luxury goods from the world over circulated in the great emporium of Europe, 'Mainz which is on the river Rin', all of which is discussed in that chapter? Why then does he have difficulty in conceiving that *reliquia* (for the Christians) and 'sacred' books (for the Jews) also made their way to the markets of Mainz? The monasteries and bishops of the Rhineland desired them and had the wherewithal to pay for them as did no less their Jewish factotums (*ma'arufyas*).} Note also that at the beginning of this citation Soloveitchik has incorrectly supplied in brackets, in a description of E.S. Rosenthal's research, "of the Talmud", while Rosenthal's work actually deals only with half of one tractate! {HS Replies: I plead guilty to the error. My mistake, however, was understandable. Anyone who was fortunate to have studied under Rosenthal and who, over the years, was gifted by him, as I was, with Friday evening conversations often going on into the late hours of the night, knows just how wide and deep his range was, and how his discussion of even the smallest topic was informed by a broad conception

But no one denies that the early textual branch which reached Ashkenaz was subjected to intensive emendation, and students and scribes treated it as if it were their own (composition). We must now ask: which characteristics of the Ashkenazi textual tradition are worthy of being considered seriously as an early textual tradition, and which of them are nothing but the product of emendators...

Soloveitchik minimizes the role of conjecture in the textual emendations of early Ashkenazi scholars and presents their emendations as if they were mostly (always?) a matter of deciding between extant variants. This topic is barely touched on in the essay on the "third yeshiva", but it is treated at length in an earlier essay reprinted in the same volume.<sup>59</sup> For example, he explains Rabbenu Gershom's ban as follows:<sup>60</sup>

R. Gershom of Mainz... (d. 1028) issued a ban on anyone who emended the text of the Talmud. Let us remember that he wrote at the dawn of Ashkenazic culture, in a period before any commentary on the Talmud had been composed.... Who was so confident of his understanding of the abrupt and gnomic text of the Talmud that he would regularly presume to emend it? Who was so confident of his control of eastern Aramaic that he *could* emend the talmudic text?... No doubt there were some bold souls who rushed in where angels fear to tread, but was the phenomenon so widespread that it demanded a communal ban? Is it not more plausible that if emendation was rampant, or in danger of becoming rampant, these corrections were being made on the basis of extant manuscripts?

Soloveitchik thus pictures a robust textual tradition in which numerous copies of a given tractate, representing different strands of textual tradition, circulated in early Ashkenaz, and imagines that R. Gershom was anxious to prevent the readings of one tradition from being imported into another. It seems to me much more likely, however, that he was concerned with protecting a fragile textual tradition, based on one or two manuscripts of any given tractate which had been imported to

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of Talmudics. He was, as Jacob Katz once remarked to me, one of the few in any field who have developed their own independent views on most of the major problems of their discipline.}

<sup>59</sup> *Collected Essays*, pp. 123-140 (especially 123-127), 196. [Brody's note]

<sup>60</sup> *Ibid.*, pp. 125-6. [Brody's note]

Ashkenaz,<sup>61</sup> which was exposed to the depredations of the "bold souls who rushed in where angels fear to tread". Contrary to Soloveitchik's assumptions, I would think unfounded conjectural emendation was particularly likely to occur in a setting in which understanding of the text being studied was somewhat limited. After all, the easiest way of dealing with an interpretive crux is to cut the Gordian knot by rewriting the text, while a more profound understanding will often vindicate the received version.

There are a number of points that need making. First, I was discussing the situation in Rabbenu Gershom's time, ca. 1000, where I believe the number of scholars engaged in talmudic exegesis was very small, and I do not believe that they or any of their contemporaries would easily have emended a talmudic text, for reasons I shall soon spell out. Rabbenu Tam is writing 150 years later after both the *Perushei Magentsa* and the *Perush Rashi* have been in the public domain for well over a half-century. The sense of knowledge, indeed control of the talmudic *sugya* is incomparably greater than before, and the danger of numerous emendations, in themselves small but cumulatively large, was far greater than before. However, the simple fact is that there is no whisper in the Tosafist literature of any frequent emendation by would-be scholars, as we shall see. True, Rabbenu Tam did decry 'emenders of the text', but let us remember that Rabbenu Tam had an agenda, and when Rabbenu Tam had an agenda, he could occasionally speak somewhat sweepingly.

For example, in his famous exchange with Rabbenu Meshullam of Melun, who sought to abolish some customs of the Jewish community of Champagne, including the much venerated one reciting a blessing on the Shabbat candles on Friday evening, Rabbenu Tam pointed out that one does not simply lay out the religious rites of a community on the Procrustean bed of the Bavli and chop off any and all protuberances. Not every practice of Bavel is recorded in the Bavli, some find their first expression in works of the Geonic period; indeed, many are reflected in the *Midrashim* and minor tractates, that is to say, are of Palestinian origin. Strictly speaking, not having been mentioned in the Bavli, they are not normative; however, by no means are they to be discarded. Quite the contrary, these venerable practices should be reverently preserved, and he admonishes R. Meshullam:

And anyone who is not thoroughly familiar with the *Seder Rav 'Amram* and the *Halakhot Gedolot* and the tractate of *Soferim* and *Pirkei R. Eli'ezer* and the [*Midrash*] *Rabbah* and *Tanhuma* and the other works of the Aggadah should not destroy the works [i.e. traditional practices] of [our] ancient predecessors, for one should rely upon them in

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<sup>61</sup> Of course other copies would in time have been made locally from these imported manuscripts, but they would have been able to improve on the text of the imported exemplars only by means of conjectural emendation. [Brody's note]

matters in which they do not contradict our Talmud [i.e. the Bavli] but add to them, and many of our customary practices are based on them.<sup>62</sup>

which seems a very accurate characterization of the role of custom in Ashkenazic law and practice. However, in another letter penned to Rabbenu Meshullam in that same controversy, he writes that our customs are Babylonian according to the Palestinian sages who are ‘the main figures [i.e. decisors] of adjudication’ (מנהגינו כבבל ע"פ חכמי ארץ ישראל שהם עיקר ההוראה). Does Rabbenu Tam really believe that *‘ikar ha-hora’ah* is in accord with the Palestinian sages? I doubt whether anyone would contend that that Rabbenu Tam is advocating following the Yerushalmi in places where they conflict with the Bavli. Perhaps, he has in mind the Palestinian *amora'im* in the Bavli? If this be the case, one would expect that the geographic origin of the various *amora'im* would play a role in adjudication of controversies. Does the Rabbenu Tam ever invoke such origins when he rules in a talmudic controversy? Is there any reference to the geographic origins of an *amora* in the thousands of pages of Tosafist discussions that we possess? I have difficulty privileging the second statement of the great Rabbenu Tam over the first. To me, at least, it seems an overstatement made in the heat of controversy.

Nor is this the only overheated claim made by Rabbenu Tam in this polemical exchange. Ashkenaz treated vinegar as wine in the matter of *yein neseikh*, even though the Mishnah explicitly states that the Gentile vinegar is permissible. Rabbenu Tam claims that the ban on vinegar is not unique to Ashkenaz but is widespread in Europe:

And should one argue that in his kingdom [i.e. area of residence—that is to say Provence, the place of origin of Rabbenu Meshullam] they were permissive in this matter, this is a falsehood – for everywhere...they forbid it.<sup>63</sup>

Rabbenu Meshullam denies this and, in fact, we find no mention of such a ban in any Provencal or Spanish source. Indeed, the treating vinegar as wine makes a great deal of sense in places in the temperate zone, as Champagne and the Rhineland, located close to the northern boundary of viticulture; the sky is often clouded, and grapes don't fully ripen. In their thirst for alcoholic drink, people often made from such grapes wine that was scarcely indistinguishable from vinegar.<sup>64</sup> It makes no sense in the semi-tropical zone, in countries around the Mediterranean littoral, where there is an abundance of sunshine, vinegary wine is neither made nor drunk and the difference between wine and vinegar is stark.

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<sup>62</sup> וכל שאינו בקי בסדר רב עמרם ובהלכות גדולות ובמס[כת] סופרים ובפרקי דר' אליעזר וב(ד)רבה (ובתלמוד) [ובתנחומא] ובשאר ספרי אגדה אין לו להרוס דברי הקדמונים ומנהגם, כי יש עליהם לסמוך בדברים שאין הם מכחישים את תלמוד שלנו אלא מוסיפין, והרבה מנהגים בידנו על פיהם. *Sefer ha-Yashar, Helek Teshuvot*, ed. F. S. Rosenthal, (Berlin, 1898), #45:3, p. 81.

<sup>63</sup> Ibid., ואם יטען הטוען [ש]במלכותו מקילין עליו, שקר מילין -- שהרי בכל מקומות... נוהגין בו חומר[א]. #45:1, p. 80.

<sup>64</sup> See *Collected Essays*, ii. (the volume under review here) 115-116.

Let us step out of the charged atmosphere of that bitter dispute and simply turn to a ruling of Rabbenu Tam about counting a minor for a religious quorum of ten men needed for the recitation of certain prayers (*minyan*). Many small Jewish communities were hard pressed to find ten adult men for their religious services. Rabbenu Tam ruled that one could count a minor for a quorum. When some pointed out that there was a passage in the Talmud that seemed to rule to the contrary and that Rabbenu Ḥanan'el (Raḥ) had endorsed that view, Rabbenu Tam responded that Rabbenu Ḥanan'el was a known *maḥmir* (one inclined to stringency).<sup>65</sup> No one has yet detected such supererogatory tendencies in the writings of Raḥ. If the reason for Raḥ's stringency is because one should not to take the Lord's name in vain שלא להוציא שם שמים לבטלה – i.e. recite a superfluous blessing), as Rabbenu Tam here states, how much more so must Rabbenu Ḥanan'el have been in matters of *Shabbat* and *Kashruth* (*ḥayyevei mittot u- khritut*). Yet no such austere inclinations have been detected in those areas either

In general, history of any discipline isn't written by studies of declarations or introductions to books, but by the investigation of the intellectual activities of the practitioners. The louder the proclamation, the more avowed the announcement in the introduction, the more skeptically should they be read. Such public notifications tell you what the author thinks he is doing, perhaps what he wishes the reader to think that he is doing; never what he actually is doing. Only an in-depth study of his writings will reveal what the writer is in fact doing. And, importantly, one must know the players--when their words reflect accurately their considered opinion and when they are somewhat intemperate remarks sparked by circumstances.

If one seeks a more balanced view of the liability of the talmudic text in the High Middle Ages in Ashkenaz, it would best, to my thinking, not to make an assessment on the basis of a sharp statement of the polemically inclined Rabbenu Tam, but rather derive it from the entire halakhic literature of that culture (including that of Rabbenu Tam), the vast corpus of writings of the French and German Tosafists. They themselves rarely emend, not do they decry or describe others as doing so. There is no mention of, not to speak of outcry against popular emendation in the voluminous rabbinic literature of the thirteenth century, and there is equally no reference to such breaches in the twelfth. Nor do we find such complaints in the 2000-plus responsa that have come down to us from this period. The only emenders mentioned are the two great commentators of Ashkenaz, Rashi and Rashbam. The text of the *Perushei Magentsa* (printed in the standard Romm Talmud as *Perush Rabbenu Gershom*) is strikingly free of any emendations of the text.<sup>66</sup> I do not recall coming across more than an instance or two of this in the six-seven tractates on which they comment on. Or, for that matter, in the rich response literature that has come down to

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<sup>65</sup> *Tosafot R. Yehudah Sirleon 'al Massekhet Berakhot*, ii (Jerusalem, 1972) *ad* 47b, *s.v. de-'amar R. Yehoshua' b. Levi* (p. 620). (Rabbenu Tam subsequently retracted his view, as we learn from the soon-to-be published *Teshuvot ha-Ri*, A. Reiner and P. Roth, eds. [Jerusalem, 2019], #2, pp. 3-5.)

<sup>66</sup> Already noted Y. M. Ta-Shma, *Ha-Sifrut ha-Parshanit le-Talmud be-'Eropah u-vi-Tsfon Afrikah* (Jerusalem, 2004), 38.

us from Early Ashkenaz, the two-hundred-year period from R. Gershom (d. 1028) to the First Crusade (1096).

This being so, I have a more sanguine view of the stability of the text in Ashkenaz in this period than does my distinguished colleague. In a lifetime spent in medieval ashkenazic writings, I have never felt the talmudic text to be labile. The text is anything but putty. Ashkenazic scholars have a received text, they have some other *seforim* (books, i.e. manuscripts) which here and there have a different reading, and ever since the early twelfth century they have the *Commentaries of Rabbenu Hanan'el* which, on occasion, have a variant reading. With this material, they go about their task of exegesis and dialectic, using distinction and alternative interpretation-- not emendation--as their primary tools. Such is my impression and, I think, this would be the impression of the textual resources and procedures of Rashi and the Tosafists of anyone who studies *Gemara* regularly. I see little basis for Brody's dictum (in the above-cited passage<sup>67</sup>): 'There remains considerable evidence that Ashkenazi scholars were particularly ready to emend and rework received texts'. If such evidence exists, I have not come across it.

To be sure, the great commentators do occasionally emend the text, but this is an inevitable, if infrequent, part of the commentarial enterprise. The emendations of Rashi and Rashbam, and those of Rabbenu Tam himself, pale in degree and number to those made in the Shakespearean text by its great interpreters. For example, let us take Rashi. The number of his emendations has been assessed at some 1,500.<sup>68</sup> Let's assume that the number of words in a *hakhi garsinan* ranges from 1-20 and take ten words as an average (which is a bit on the high side). This would amount to 15,000 words of correction. The number of the words in the Talmud have been assessed at 1,500,000, that yields a repair rate of 1%. Take the Folio or Quarto edition of Shakespeare and compare it with any standard contemporary text of his plays. The number of emendations is 10 to 15 times greater.

In his concluding footnote, Brody writes:

Soloveitchik (*Collected Essays*, p. 124 and n. 4) refers to a paper which Shai Secunda wrote in one of his seminars on Rashi's emendations in tractate 'Avodah Zarah and which was at one time posted on the Internet. However, the paper is no longer available at the site he gives as a reference, and Dr. Secunda informs me that he himself can no longer retrieve it.

Seeing that we have been colleagues for over forty years, it would have been much easier to simply contact me rather than wrestling with blogspots, and I would have sent him Secunda's

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<sup>67</sup> P. 283.

<sup>68</sup> A. Reiner, 'Mumar Okhel Neveilot le-Te'avon—Pasul? Mashehu {'}al Nusah u-Ferusho bi-Yedei Rashi', *Lo Yasur Shevet mi-Yehudah. Hanhagah, Rabbanut u-Kehilah be-Toledot Yisra'el: Mehkarim Muggashim le-Professor Shime'on Schwarzfuchs*, J. Hacker and Y. Harel, eds. (Jerusalem, 2011), 222.



paper with its Excel spread sheets and all. The offer still stands. {I have placed it on my website, where this reply is found.}

More significantly, Brody does not mention how Secunda's research came into the picture. It was invoked in support of both Noam's and Sabato's thesis.<sup>69</sup> After adducing the passage from Noam that Brody cited in my name, I wrote:

The upshot of all this is that the Babylonian material that reached Yemen, Kairouan (near Tunis, in the Maghreb), and Spain, equally arrived in Ashkenaz, or that Ashkenaz received their traditions from these locales. A third possibility is that Ashkenaz received some of its manuscripts independently from the East, others via the mediation of Yemen, Kairouan and Spain. One might argue that Ashkenazic manuscripts are late -- the earliest is from 1177<sup>70</sup> and most others are far later. What relevance can these manuscript findings have for pre-Crusade Ashkenaz? Let us turn to the emendations of Rashi. In the same article, Noam has shown that in the tractate *Sukkah*, seventy-one percent of his emendations are confirmed by eastern or Spanish manuscript traditions. This is an extraordinarily high figure. Shai Secunda's research shows that in the tractate *'Avodah Zarah* there is a forty-three percent congruence of Rashi's emendations with manuscripts which, to use Friedman's typology, are either Mediterranean or of Spanish provenance.<sup>71</sup> (Unfortunately we have no Yemenite manuscripts on *'Avodah Zarah*.) One might argue that a congruence of forty-three percent could equally be random; chance would have it that at least close to fifty percent of all good emendations would be corroborated by some manuscript or another.<sup>72</sup> Reply can be made that first, there are only three and not a dozen

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<sup>69</sup> V. Noam, "Mesorot Nusah Kedumot be-Haggahot Rashi ba-Talmud", *Sidra* 17 (2001-2002), 110-11; M. Sabato, *Ketav Yad Teimani le-Masekhet Sanhedrin (Bavli) u-Mekomo be-Masoret ha-Nusah* (Jerusalem, 1998), 231-78, especially the table on p. 258.

<sup>70</sup> *Talmud Bavli: Ketav-Yad Firentseh*, 'im Mavo me'et D. Rosenthal (Jerusalem, 1972) Mavo, p. 1.

<sup>71</sup> S. Friedman, 'Masekhet *'Avodah Zarah*, Ketav Yad New York, Ketav Yad she-Hu'atak bi-Shenei Shelavim', *Leshonenu*, 56 (1992), 371-74.

<sup>72</sup> In view of Brody's objection this sentence can be misleading. If I have eight or nine manuscripts from multifarious sources whose variants go in every which direction, there is a good chance that any emendation will have one or two other manuscripts (call them A and G) corroborating it. The next emendation will a different set of manuscripts (B and H) corroborating it, the third will have A and Q as correlatives. There is, however, little chance that the percentages of such correlations will rise to so much as the level of 20%. What is central to the issue of the manuscripts on *Sanhedrin* and *Sukkah*, that will be discussed below, is that 31% of Rashi's emendations of the Ashkenazi manuscripts in *Sanhedrin* are found in Yemenite manuscripts, and in *Sukkah* 68% (at a minimum 54%)

manuscripts of this tractate. Second, Friedman has shown that one of the two manuscripts, Jewish Theological Seminary 15, is a composite. The first half (up to f. 43) is of the "Mediterranean" type (in Friedman's orthographical typology), the second half (fols 43-76) of the Spanish one.<sup>73</sup> In the first half of this manuscript, the congruence of its readings with Rashi's emendations is 32.5--35 percent, in the latter half only 11-12 percent. The degree of congruence of Rashi's emendations changes by some 66 percent with the change of the textual tradition to which it is being compared.<sup>74</sup> Apparently, Rashi was working off a manuscript that had more in common with the Mediterranean type than with the Sephardic one, and that differed very considerably from the one that came to be called 'Ashkenazic'.<sup>75</sup>

This changes somewhat the picture portrayed by my distinguished colleague of the *bet ha-midrash* of Troyes where Rashi sat as barren and bereft of manuscripts from the Diaspora.

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I turn now to Brody's critiques of both my use of talmudic manuscripts and that of Vered Noam:

Brody writes: (p. 284-286)

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of his corrections are found in two manuscripts that are of Mediterranean or Spanish origin.

<sup>73</sup> S. Friedman, 'Kitvei ha-Yad shel ha-Talmud Ha-Bavli –Tipologiyah shel Ktiv', *Mehkarim ba-Lashon ha-Ivrit u-vi-Leshonot ha-Yehudim Muggashim le-Shelomoh Morag*, M. Bar-Asher, ed. (Jerusalem, 1996), 163-190. [This reference, though necessary to the documentation, is missing in the original text. HS]

<sup>74</sup> I must correct what I wrote in that essay. 'In the first half of this manuscript, the congruence of its readings with Rashi's emendations is 47 percent, in the latter half only 33 percent.' The latter number is in error. The actual difference is far greater. It is not a drop of 25 percent (as would be ratio of 47-36), but approximately 66 percent.

<sup>75</sup> Shai Secunda's paper, written for a seminar of mine, contains both an analysis of the variants and emendation together with a transcription of all the manuscripts readings (including those of the Genizah fragments) of the talmudic passages emended by Rashi, both as found in the printed version of his Commentary and in Ms. Parma de Rossi 1292 (Richler 727). Deciding which version an author had in front of him often hinges on fine nuances. One may disagree with one point or another of Secunda's analysis, but the overall picture that he draws is, to my thinking at least, beyond question. As both the paper and the transcriptions variants have been placed online at <http://www.azyn.blogspot.com>, the reader may draw his own conclusions. (My n. 5 at p. 124 in 'Communication and the Palestinian Origins of Ashkenaz' in the book under review here by Brody. It is to this the Brody refers, above, p. 33.)

Soloveitchik is profoundly influenced by Noam's article, and especially by her assertion that 71% of Rashi's emendations in tractate *Sukkah* are paralleled by at least one Spanish or Eastern manuscript.<sup>76</sup> But it is a cardinal rule of textual criticism that variants must be weighed and not counted, and Noam has not undertaken to do so. Without further analysis, cases in which Rashi's emendation is supported by several textual traditions including a Yemenite or Spanish manuscript one prove absolutely nothing; in many of these cases he may simply have been rejecting an obvious mistake (or suggesting an obvious emendation) based on his judgment and intimate familiarity with the talmudic idiom.<sup>77</sup> Even a glance at the appendix to Noam's article shows that while Rashi bases one emendation in this tractate on a manuscript of R. Gershom, numerous others are based on parallels elsewhere in rabbinic literature and at least one is based on conjecture.<sup>78</sup>

While admitting that Rashi's emendations were partly due to "intuition", Soloveitchik argued that his emendations were based to a large extent on "a broad spectrum of manuscripts of different provenances and traditions, all of which came from places far removed from the city of Troyes... and from the Rhineland academies..."<sup>79</sup> However, while it is obvious that Ashkenaz received the Babylonian Talmud, directly or indirectly, from the east, most of Soloveitchik's evidence for the presence of Oriental manuscripts in early Ashkenaz is based on a misreading of studies by several scholars who measured the correlation between Rashi's emendations in certain tractates and the surviving manuscripts of these tractates.

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<sup>76</sup> See *Collected Essays*, pp. 123-125, especially p. 124: "...Noam has shown that in tractate *Sukkah* 71 percent of Rashi's emendations are confirmed by eastern or Spanish manuscript traditions. This is an extraordinarily high figure." The reference is to Noam (n. 49 above), pp. 135, with the underlying evidence presented on pp. 138-146. [Brody's note]

<sup>77</sup> See for example items 4, 7, 18, 29 and 43 in Noam's list. In the first two cases in her list virtually all witnesses agree with the version championed by Rashi; Noam believes that he intended to reject a corrupt reading found only in a single Genizah fragment, and concludes (p. 138 n. 237) "that unique Eastern readings were seemingly before him", but it is not even certain that this is the corruption with which Rashi was familiar, let alone that this particular corruption reached Ashkenaz from the East rather than occurring independently in two locales. [Brody's note]

<sup>78</sup> The case in which Rashi refers to R. Gershom's manuscript is number 41 in Noam's list; emendations based on parallels are found in numbers 23, 25, 31, 55, 60 and a clearly conjectural emendation in number 22. [Brody's note] {HS replies: With all due respect to my colleagues, I find some of these determinations questionable. See below nn. 80, 82-85.}}

<sup>79</sup> *Collected Essays*, p. 125; see also the continuation of the passage cited above from p. 126. [Brody's note]

For example, he claims that a study by Sabato shows that "31 percent of Rashi's emendations in tractate *Sanhedrin* correspond to the Yemenite tradition; 13 percent are found only in the Yemenite textual tradition". However, Sabato does not assert in even a single instance that Rashi's emendation conforms to the Yemenite tradition alone. Moreover, there is nothing idiosyncratically Yemenite or Eastern or even Spanish about any of the readings which agree with Rashi's emendations, and in case after case Sabato's analysis leads him to conclude that Rashi was engaged in conjectural emendation, which in several instances (including some pointed out by later medieval authors) was unjustified.<sup>80</sup> In several cases the point of Rashi's emendation is obscure; in no case does he mention manuscripts of the tractate in question; in several cases he argues on the basis of parallels in other works of rabbinic literature, and in several other cases his language clearly indicates that his emendation is conjectural.<sup>81</sup> A similar picture emerges from the most detailed study to date of Rashi's emendations, in Eliezer Segal's Ph.D. dissertation, which Soloveitchik does not discuss.<sup>82</sup>

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<sup>80</sup> *Collected Essays*, *ibid.*; see M. Sabato, *A Yemenite Manuscript of Tractate Sanhedrin and its Place in the Text Tradition* (Hebrew), Ph.D. dissertation, Hebrew University 1996, pp. 231-258, especially (in addition to those listed in the next note) the examples numbered 3, 5, 7, 8 (discussed by Sabato on p. 204), 32, 36, 37, 41. Soloveitchik apparently misread the summary table on p. 258: 13 represents the number of cases in which Rashi's emendation agrees with the Yemenite MS (31% is 13 out of 42). {HS replies: Indeed, 13 out of 42 is 13%, and that is the figure which Sabato gives and which I reported. What's wrong with my statement, pray tell?} Soloveitchik was probably misled to some extent by Noam's (mis)interpretation of Sabato's findings, see Noam (n. 69 above), p. 135. [Brody's note] {HS replies: And what is wrong with Noam's statement? She says that 34% of Rashi's emendations in *Sukkah* correspond to the Yemenite manuscript which is strikingly close (*karov le-haftia*) to the percentage which Sabato found in *Sanhedrin*.} {I have placed photostats of p. 258 in Sabato's study and p. 135 in the photostats appended to this online Reply, pp. 4-5.}

<sup>81</sup> For emendations based (at least in part) on parallel sources see numbers 6, 7, 10, 33 in Sabato's list; for conjectural emendations see numbers 12 (?), 21, 25, 28 (?). [Brody's note]

<sup>82</sup> E. Segal, *The Textual Traditions of Tractate Megillah in the Babylonian Talmud* (Hebrew), Ph.D. dissertation, Hebrew University 1981, pp. 127-212. As to my 'non-use' of this doctorate, which Noam employed, see below p. 41.} On the other hand Soloveitchik (*Collected Essays*, p. 124 and n. 4) refers to a paper which Shai Secunda wrote in one of his seminars on Rashi's emendations in tractate *Avodah Zarah* and which was at one time posted on the Internet. However, the paper is no longer available at the site he gives as a reference, and Dr. Secunda informs me that he himself can no longer retrieve it. {HS Replies: I have addressed these points, above, pp. 33-34.} I doubt that any of the evidence cited by Soloveitchik, including that from orthography, actually shows that a wide variety of Talmudic manuscripts was available in Ashkenaz even by Rashi's time; certainly none of its speaks to the situation in the

Let us begin with his critique of the inferences that Noam and I drew from the statistics of congruence in the tractate of *Sukkah* and cut to the quick: Does my distinguished colleague really think that if two manuscripts have a congruence in variants--both significant and insignificant--of some 68%, or, for argument sake, only 50%, that these two manuscripts are not of one family, do not have a common stemma?! If he does indeed so think, I would strongly advise him not to play poker.<sup>83</sup> Let us grant for the moment that all Brody's criticisms of Noam's proofs are correct (including two that he himself has marked with a question mark<sup>84</sup>), that would make for a congruence of thirty-three out of sixty-two for an average of 54%, which are odds that any card player would gladly take.

. Noam's results should be further correlated with Miller's recent study (2017) of the first chapter of *Gittin* which yields figures strikingly close to that of Sabato on *Sanhedrin*. One third of Rashi's emendations in *Ha-Mevi Get* are corroborated by manuscripts that originate in the East, that are independent witnesses, that is to say, are free of any emendations of Rashi.<sup>85</sup> I

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mid-tenth century, and Soloveitchik's attempt (ibid., pp. 125-141) to "push yet further back in time" is purely conjectural. {**HS Replies:** See our remarks on p. 48.}

<sup>83</sup> See the beginning of the preceding note. I have reduced the 71% ratio to 68% because one emendation is clearly conjectural. As to the emendation based on Rabbenu Gershom's text, to which Brody points, this only pushes the same question back by some two generations, as pointed out in the text (p. 40).

<sup>84</sup> See, above, n. 81.

<sup>85</sup> Y. A. Z. Miller, ' 'Iyyunim be-Haggahot Rashi le-Nusah ha-Talmud be-Ferek Rishon shel *Gittin*', *Kovets Hitsei Gibborim*, 16 [2017], 644-63, and see summary on p. 643. I have my doubts about a number of Brody's objections (in nn. 77-78, 80-81) to Noam's count, as noted in the above notes. To give two examples of questionable criticism in the first two critical notes: 1] #31 in n. 78. Rashi here is not basing himself on a parallel source, as Brody contends. Rashi (32a) is here following the text of Rabbenu Gershom, as noted by the R. Tsidkiyahu min ha-'Anavim of Rome in his *Shibbolei ha-Leket*, M. Buber, ed. (Vilna, 1887), #354, who notes that his text of the Talmud lacks this passage (cited by the *Dikdukei Soferim ad loc. [he'arah pe]*) as does Ms Munich, Bayerische Staatsbibliothek 140, all noted by Noam, as do the Genizah fragments that she listed. The important point that Rashi is making is that the erroneous can be authentic. Even though the initial line is grossly mistaken and immediately rejected by the *sugya*, this error did occur in the *bet midrash* of the Amoraim. Such is the textual tradition of the Third Yeshivah. The record of mistake is genuine and should *not* be stricken from the text, as other traditions have done. 2] #29 in n. 77. Rashi (31b, s.v. *shenayim*) emends his text to *shenayim beyado a{h}at*. The contrary reading is found in Rabbenu Hanan'el *ad loc.* in the standard Romm Talmud, who writes: *she-yohaz shenehen ha-lulav ve-ha-etrog beyado ha-'ahat*. That Rashi would have a text before him that read as did that of Rabbenu

would suggest that Brody familiarize himself with Rabin Shustri's doctorate, '*Masoret ha-Nusah shel Massekhet Sukkah*',<sup>86</sup> submitted some eight years after Noam's essay and who concludes that Rashi had eastern manuscripts--which from the point of view of my historical argument is the essential point.

Brody further criticizes Noam on methodological grounds, writing: "But it is a cardinal rule of textual criticism that variants must be weighed and not counted". True; equally true is that with variants, distinctions must be made between conjecture and fact. A fact can be wrong but remains a fact; a conjecture may be right but remains a conjecture. A 'fact' in textual criticism means that it corresponded to something 'out there', something which existed in the real world at the time of the editor/commentator, such as a manuscript. A conjecture corresponded to nothing in the real world at that time. It was purely the intuition of the editor/commentator (as unerring as that intuition may have been). When an emendation that we confront corresponds to a manuscript that we possess, numbers and origins begin to count. When the percentage of correspondences between emendations and extant manuscripts begin to rise, and the existing manuscripts begin to align themselves in noticeable groups, any significant correlation between the emendations and a specific group's origins suggests that manuscripts from this geographic area (or stemma) were on the desk of the commentator/editor. When the percentages climb to 50 or 60 percent, this conclusion appears highly probable.

Textual studies like any field have numerous approaches. Stated abstractly, some seem to conflict with others. In practice, however, as the data gathered begin to form themselves into groups, by places of origin, periods of time, linguistic traits and the like, the researcher chooses the tools best suited to eliciting answers to his or her questions from the aggregating data. With

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Ḥana'el is not surprising. As Noam wrote (cited above p. 18, and in her article, p. 111): "[M.] Sabato has discovered two clear textual traditions in tractate *Sanhedrin*: an eastern one reflected in the Yemenite manuscripts and in the works of Rabbi Yitsḥak of Fez (Alfasi); the other reflected in the Ashkenazic tradition, which is mirrored, surprisingly, in the version used by Rabbenu Ḥanan'el [of Kairouan] and that of R. Me'ir Abulafia' [Ramah] of Toledo. He further surmised that the split had taken place quite early and, in the East, and that this eastern version somehow got to Ashkenaz".} {I have placed photostats of the *Dikdukei Soferim* and of the *Perush ha-Raḥ*, both in the edition of the Romm Talmud and that of Metzger in the photostats appended to this online Reply, pp. 6-8.}

(To prevent misunderstanding: Brody (n. 77) refers to Rashi, *Sukkah* 40a (s.v. *hakhi garsinan*), who cites the talmudic text of Rabbenu Gershom [and that of R. Yitshak b. Menaḥem of Orleans]. I am addressing Rashi's emendation at 32a and pointing out that is not a conjecture on his part as Brody contends, but is based on the reading of Rabbenu Gershom even though he does not mention this fact. [On R Yitshak b. Menaḥem, see A. Grossman, *Hakhmei Tsarfah ha-Rishonim* [Jerusalem, 2001], 107-120.]

<sup>86</sup> PhD diss. University of Bar-Ilan, 2010), 287-359 and see the concluding sentence on p. 359.

all due respect to my distinguished colleague, Vered Noam chose the right tool, he the wrong one.

Take Rashi's emendation to '*Avodah Zarah* that Secunda has studied, for example. If 43 percent of his emendations have a parallel in Spanish and Mediterranean manuscripts, is it not reasonable that he had manuscripts with these readings before him. Someone might contend that the 43 percent only proves Rashi's genius. He intuited close to half the variants of the manuscripts of a tradition other than his. Let us grant this contention, how does Brody account for the figures derived from the split manuscript of '*Avodah Zarah* of the Jewish Theological Seminary, that had been copied from two separate sources, the first half from one of Mediterranean provenance, the second half from one of Spanish origins—as we mentioned above?<sup>87</sup> If Rashi is intuiting readings other than that of his own text, why does his rate of intuition drop by almost two-thirds (from 33% to 11-12%) when treating the second half of the tractate? Is Brody challenging the simple explanation that in working on the latter half of '*Avodah Zarah*, the text in front of Rashi was closer to the Spanish one and, therefore, there was much less to emend in that part of the tractate?

Finally, Brody criticizes Noam for counting an emendation that is based on Rashi's explicit statement that he found this reading in the talmudic manuscript of Rabbenu Gershom.<sup>88</sup> I venture to suggest that this only pushes Noam's evidence back by some two generations and right into my wicket: How did Rabbenu Gershom get a text which matches that of Yemen?!

My distinguished colleague doesn't believe in statistics; he also doesn't attach much importance to either probability or implausibility. He sees no problem in having Sura and Pumbedita inscribe the text of the Talmud, thus losing their monopoly of the Vox Talmudica, which was tantamount to institutional suicide.<sup>89</sup> He deems it likely that any number of individuals, inside or outside the *yeshivot*, skilled or unskilled, with good or bad memories, could inscribe the vast million and a half word corpus of the Talmud, and the end product would not be judicial chaos, but a text that is 'stable in content', with remarkably few differences in legal meaning and implication.<sup>90</sup>

What underlies this strange mode of thinking of my colleague? The answer I believe is simple. If I have before me two manuscripts which by all rules of probabilities can have no relationship between them, yet nevertheless show a surprising degree of textual correspondences, the natural approach is to separate out the significant convergences from the insignificant ones. Should the total be, let us say, 12%, and the analysis show that 6% are trivial correspondences, the remaining non-trivial 6% can be written off as being statistically random. Yemen and the Rhineland are nowadays antipodes of the known world, and a manuscript found in Mainz could

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<sup>87</sup> P. 34-35.

<sup>88</sup> Above, p. 36 and n. 78.

<sup>89</sup> Above, pp. 20.

<sup>90</sup> Above, pp. 23-24



not conceivably be derived from one in Yemen, nor could the two reasonably be thought to share a common ancestor. What, however, was the situation in the ninth and tenth century? To answer this question, one must turn to the history of trade and communications in this period. Brody refuses to do this and contends that even a 68% (at the very least, a 54%)<sup>91</sup> correlation can't be a family resemblance, can't be seen as demonstrating a common ancestry. I would go even further and argue that a 6 to 10 percent can yet be written off as random; what can't be disregarded is a 39 percent one, not to speak of one of the magnitude of 50 or 60 percent. Even had I no evidence of trade in the ninth and tenth century, such percentages would set me off looking for them with the retrospective method.

The same thought process takes place with the way Brody reads Sabato's evidence from *Sanhedrin*. Sabato, of course, does not say that the text before Rashi is identical with that of Yemen ('conforms with the Yemenite tradition', as Brody puts it), and Sabato may well believe (and eminent scholars may agree) that some of Rashi's emendations (*hakhi garsinan*, 'read thus') are in error. However, a 31% correspondence of convergence of passages--controlling and trivial--may well be too large to be random, *seeing that* in another tractate (*Sukkah*) the textual convergences amount to some 68% (54%, at the minimum) *and one also knows* that goods from Yemen circulated regularly and in abundance in the great emporium of Mainz. *Add to this* that the Ashkenazic manuscripts in *Hullin* (fos. 101a-105a) are penned in the same popular (*vulgus*) Aramaic dialect as the 'extremely early' manuscript scroll(!) from the East that Shamma Friedman published close to a quarter of a century ago<sup>92</sup>, coupled with Shustri's findings<sup>93</sup> and the cumulative probability of your eastern contact with Ashkenaz becomes very high. The probability has only risen when it was recently shown that 33% of Rashi's emendations in *Ha-Mevi Get* are corroborated by manuscripts that originate in the East.<sup>94</sup> How can one be indifferent to such cumulative probabilities?

I did not invoke Segal's thesis that Noam mentioned in her superb article anymore than I did that of Shustri<sup>95</sup> because the instances of *Sukkah* and *Sanhedrin* were sufficient to illustrate that the Jews of the Rhineland in the closing centuries of the first millennium were not situated at the end of the known universe at the time, connected by an umbilical cord to Lucca and the Palestine as hitherto portrayed by Jewish scholarship. They were rather located at the economic center of Europe in those centuries--the Rhineland--adjacent to the imperial palace of Ingelheim and in the heart of Franconia, what was later to be called 'the backbone of the Empire'. Not surprisingly, both commercial and spiritual goods (*reliquia* and manuscripts) from the East and the Mediterranean made their way to the economic capital of Europe in the ninth and tenth century, as both German bishops and monasteries together with their Jewish factotums

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<sup>91</sup> See above, p. 38 and n. 83 for the figures of 68 and 54 percent

<sup>50</sup> Above, n. 50.

<sup>93</sup> Above, p. 39.

<sup>94</sup> See above, n. 85.

<sup>95</sup> Above, p. 39.

(*ma'arufyas*) were both able and willing to pay for their acquisition.<sup>96</sup> It is not at all surprising then that Rashi had manuscripts of the Talmud before him when he was penning his great commentary which originated or shared a stemma with those originating from Spain, from the southern rim of the Mediterranean, from Yemen and, last but not least, from Bavel.<sup>97</sup>

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Disbelieving of statistics, indifferent to implausibility, skeptical of inference regardless how high its cumulative probability, my distinguished colleague apparently believes that the only thing that existed in the past is what is explicitly stated on the documents of the past. He picked up my essay on the Third Yeshivah of Bavel and said, 'I know all the texts of the Geonic period, and there is no mention of any Third Yeshivah of Bavel'. He finished reading it and said, 'There is still no mention of a Third Yeshivah of Bavel'.

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Let us remember, however, that while Dr Brody wrote the review, he also drew upon the expertise of no less than four other internationally recognized authorities in Jewish Studies. If this is all the damage that the concentrated critical fire of five outstanding Judaic scholars can do to the 'Third Yeshivah of Bavel', that institution rests on a firmer foundation than I ever dreamt when I first cautiously proposed it.

## Appendix

The last two and a half pages of Brody's article is less a criticism of what I have written as an attempt to present an alternative to the origins of Ashkenazic culture that I advanced in my article. I have decided to treat it in an appendix to set it off from the criticism proper and to present what I consider some of its weaknesses He mentioned alternatives en passant in the body of the criticism, and I thought it best to defer treating them until they had been grouped together by Brody and formed a coherent whole.

Brody writes: (p. 286)

I believe I have demonstrated that Soloveitchik's innovative theory rests on a very weak foundation and that the notion of a multiplicity of high-level

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<sup>96</sup> *Collected Essays*, ii. 134-135.

<sup>97</sup> Above, pp. 18-19, 28, n. 57.

talmudic academies in Geonic Babylonia should be rejected. I have already discussed two alternatives to this theory. One possibility is that the origins of Jewish culture in Ashkenaz go back significantly farther than the sources at our disposal document, and Ashkenazi Jews could have been engaged in the study of the Babylonian Talmud for a century or more before the *floruit* of Rabbi Leontin, the teacher of Rabbenu Gershom, in the middle of the tenth century. This would have given them ample time to develop in relative isolation a home-grown approach to the study of the Talmud which might have differed in any number of respects from the approach or approaches current in contemporary Babylonian academies.

I have no problem with texts of the Bavli circulating in Ashkenaz in the early to mid-ninth century, though I'm more than a bit surprised that Brody is so readily agreeable to such an early date for the inscription of the Talmud, or tractates thereof.<sup>98</sup> Nor would I be opposed to the notion of some 'reciters' of the talmudic text (*garsanim*), hailing from Babylonia, who sought their fortunes in the (Jewish) wastelands of Ashkenaz. Tulsa, Oklahoma and Omaha, Nebraska had distinguished rabbinical figures who tried their luck in the "Wild West" of America at the turn of the twentieth century and practiced their trade there with distinction. Such individuals, while real, were rarities however, and the notion that they could develop an indigenous tradition of talmudic exegesis strikes me as historically improbable. A migration of an ideologically motivated elite, located at the very frontier of talmudic studies, confronting a collapsing Jewish culture (that of the Sabora'im/Setamaim) and situated in a disintegrating country and failing economy seems, to my mind, better suited to the historical role of the founding fathers of the talmudic culture of Ashkenaz.

Brody continues:

The second possible explanation accepts Soloveitchik's assumption that Ashkenazi culture in fact began to develop only in the mid-tenth century, and accounts for the rapid ascendance of the Babylonian Talmud and the advanced state of its study by supposing that the "founding fathers" of Ashkenazi talmudic culture were disaffected ex-students of one or both of the Geonic academies of Babylonia.

I have already noted, when Brody first breached this idea above,<sup>99</sup> that if this 'disaffected group' had a distinctive Weltanschauung of total involvement in the talmudic heritage, both halakhic and aggadic and an opposition to involvement with the alien wisdom of the Gentiles, I have no objection to the proposal. As I wrote: 'Let me also be clear what I mean by 'a third yeshiva'. I mean an institution that had its own ancient and independent traditions of learning and, as we shall soon see, its own Weltanschauung. If, to use a modern metaphor, someone were to claim that the Kolel Ish had a separate wing or room in the Ponivezh or Hevron yeshivah, I

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<sup>98</sup> See below, p. 48.

<sup>99</sup> Pp. 9-10.

would have no objection. My essay concerns a distinctive culture, not to where and how that culture is housed.'

Brody concludes: (pp. 286-287)

In the final section of this essay I will permit myself to indulge in speculation and propose a further possible explanation of the course of events: perhaps we should maintain some version of the regnant theory of the Palestinian roots of Ashkenazi Jewry despite the declared allegiance of the "founding fathers" to the Babylonian Talmud.

We know that the Babylonian Talmud and Babylonian halakhic culture made substantial inroads in Palestine, including Palestinian Geonic circles, over the course of the Geonic period, and it appears that by the end of this period Palestinian scholars, who had pursued a diverse curriculum, acknowledged the superiority in the talmudic-halakhic field of the Babylonians, who had concentrated almost exclusively on this area.<sup>100</sup> If we suppose that the Babylonian Talmud had already achieved a central and perhaps even predominant position even in Palestinian Geonic circles by about the middle of the tenth century, we can suggest a scenario like the following: one or more talented young Italian Jews – perhaps R. Leontin, the teacher of Rabbenu Gershom, whose name points to an Italian origin – traveled to Palestine, with which the Jewish community of Italy had long-standing ties, to pursue advanced talmudic training at its Geonic academy. Convinced of the superiority of the Babylonian over the Palestinian Talmud (whether or not this had not been acknowledged in the Palestinian academy) they returned to Europe, bringing with them written texts of the Babylonian Talmud (and perhaps of the Palestinian Talmud as well<sup>101</sup>), and

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<sup>100</sup> See Brody, *Geonim*, pp. 111 (and n. 35), 116-120, and cf. pp. 105-109. Simcha Emanuel has recently presented (in a lecture delivered in Jerusalem in September 2015) a preliminary report on a remarkable find from the "European Genizah" which, when published, will provide further support for these assertions. (Brody's note)

<sup>101</sup> Soloveitchik (*Collected Essays*, pp. 158-159) states, with reference to the scholars of early Ashkenaz: "In their large library, however, there was no copy of the Yerushalmi (Palestinian Talmud)... A sentence or dictum from that corpus is occasionally evoked, but never the analysis of a *sugya*, of an actual Talmudic discussion, in the Yerushalmi." In the earlier essays to which he refers in n. 18 here (*Collected Essays*, p. 61 and n. 94, p. 149 and n. 94) he made this suggestion rather more circumspectly, stating that whether early Ashkenazi scholars had the Palestinian Talmud was "more than doubtful" or that there was no evidence to prove they did, and suggesting that citations from the Palestinian Talmud could have been taken from a florilegium. I am quite skeptical about the argument from (relative) silence, i.e., from the absence of detailed analysis of Yerushalmi passages; even authors such as R. Hananel and R. Isaac Alfasi, who regularly cited the Palestinian Talmud and undoubtedly had direct access to it, rarely if ever analyzed the passages they cited. [Brody's note] {HS replies: No one is drawing

settled in the Rhineland where they founded the Ashkenazi school of talmudic learning. Having acquired their knowledge of the Babylonian Talmud in a Palestinian academy, it would not be surprising if their approach to the study of this Talmud were different than that of the Babylonian academies or if they failed to defer to the Babylonian Geonim.<sup>102</sup>

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conclusions from the 'absence of a detailed discussion'. Dr Brody's invocation of Rah and Alfasi is a comparison of non-comparables. R. Alfasi and Rabbeinu Hanan'el quote on occasion an entire *sugya* or the main passages of a *sugya* of the Yerushalmi. The eleventh-century Ashkenazic literature quotes a single line or two. I don't recall a single citation of a *sugya*, an actual *masa u-matan*, not to speak of a simple discussion of a *sugya* in the Yerushalmi, in the halakhic literature of this period. See, for example use of the Yerushalmi in so problematic an issue as sitting in the *sukkah* on *Shemini 'Atseret* (*Sukkah* 4:5; Academy of Hebrew Language [Jerusalem, 2001], col. 651) discussed in *Ma'aseh Geonim*, A. Epstein, ed. (Berlin, 1910), 42. It cries out for some discussion, even be it un-detailed, but none is forthcoming. (See *Sefer Ravyah*, A. Aptowitzer, ed. [reprint: Jerusalem 1964] ii. #695-6 (pp. 401-3). For a survey of the subject, see Yitshak (Erik) Zimmer, *'Olam ke-Minhago Noheg: Perakim be-Toldot ha-Minhagim, Toldotehem ve-Gilgulehem* [Jerusalem, 1996], 163-73.)

I suggest that Dr Brody first study the citations of the Yerushalmi found in the literature of Early Ashkenaz and then form an opinion of the employment of that work by the scholars of pre-Crusade Ashkenaz. A fairly comprehensive list of the Yerushalmi citations are found in the Introduction of S. Buber to the *Sefer ha-Oreh* (Lvov, 1905), 21, 59, 83, 114-17; and the parallel introduction of S. Hurwitz, to the *Machzor Vitry* (Nürnberg, 1923), 69-72. M. Higgers essay, 'The Yerushalmi Quotations of Rashi' in the *Rashi Anniversary Volume* (New York, 1941), 191-227, is still useful. My life-long impressions were confirmed by seminar papers done under the direction of Yaakov Sussman which arrived at the conclusion that there is no evidence of a text of the Yerushalmi (or even tractates thereof) in Ashkenaz before the twelfth century.}

<sup>102</sup> A variation on this theory, which might appeal especially if we take Soloveitchik's "cultural DNA" seriously, would be that the "founding fathers" of Ashkenaz were ex-students of the Palestinian academy who were reacting against its "pomp and circumstance" and against assimilation to Arabic culture. Certainly the Palestinian Gaon was much more of a politician than his Babylonian counterparts (see Brody, *Geonim*, p. 105 and n. 18), and it is perhaps easier to imagine resistance to Arabic culture among Rabbanite Jews in Palestine than in Babylonia, given the leading role played by Karaites in the intellectual and cultural life of Palestine by the beginning of the tenth century (ibid., pp. 88-89, 109) and the leading role played by the Karaites in adopting important elements of Arabic culture (see R. Drory, *Reshit ha-Maga'im shel ha-Sifrut ha-Yehudit im ha-Sifrut ha-'Aravit ba-Me'ah ha-'Asirit*, Tel Aviv 1988). [Brody's note]

Again I have in principle little opposition to this proposal. As I wrote in my reply to David Berger:<sup>103</sup>

It makes no difference to my central argument as to the origin of Ashkenazic culture whether the men of the east came west (as I presented it) or a man of the west went east and returned home (as Professor Berger would have it), as long as what he brought back with him to the west was the amoraic heritage in its entirety.

The difficulty that I have with this scenario is that one would have to assume that these Babylonianized Palestinians also had a curriculum that encompassed the entirety of the Talmud, including all of *Seder Kodashim*, that they had a novel conception of what *talmud Torah* consisted of – namely, total comprehension of the entire give-and-take of every *sugyah* of every tractate of the Talmud, and that Aggadah, no less than halakhah, was a central component of the talmudic study and explication. To me this seems a bit much for converted Palestinians; but nothing less will do for role of the ‘founding fathers of Ashkenaz’.

Brody continues: (p. 288)

I will conclude by pointing out a further piece of evidence for the long-standing ties between Ashkenaz and the Palestinian academy: among the very few surviving responsa issued by this academy we find two which were sent to Ashkenaz, one reportedly in 960 and the second probably in the 1060's.<sup>104</sup> Rather than seeing the first of these responsa as the putative trigger which led wealthy Ashkenazi merchants to break with the Palestinian center and recruit learned immigrants from Babylonia, as Soloveitchik suggests,<sup>105</sup> I would argue that these responsa demonstrate

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<sup>103</sup> 'The Third Yeshivah', 208. [Brody's note]

<sup>104</sup> I agree with Soloveitchik (*Collected Essays*, pp. 183-184) that there is no good reason to question the authenticity of the first responsum, reported by a twelfth-century scholar, although he backtracks a bit (for what seems to me no good reason) in *ibid.*, p. 193 n. 91. {HS replies: I didn't backtrack as regards its authenticity, but as to the date of the migration--it would place it post-960. I would now backtrack from that *terminus post quem* and return to the 930-970 span that I advocated throughout the essay. The simple reason being: Such trips cost a hefty sum, and if a wealthy donor wants to know when the Messiah is coming, you add that question to the ones being asked. And why not? Who doesn't want to know when the Messiah is coming?} For the second responsum, addressed to Moshe b. Meshullam of Mainz, see M. Gil, *Palestine during the First Muslim Period (634-1099)*, 1 (Hebrew; Publications of the Diaspora Research Institute, 41), Tel Aviv 1983, pp. 450-451 and n. 777. [Brody's note]

<sup>105</sup> *Collected Essays*, pp. 183-184. Note too that, as Soloveitchik remarks, this hypothesis would force us to take 960 as a *terminus post quem* for the migration to Ashkenaz, considerably

that the Ashkenazi Jewish community continued to regard the Palestinian Geonic academy as the highest authority to which it could appeal.<sup>106</sup>

Two responsa, cited by Brody in his footnote, constitute the basis of his claim. The second addressed to R. Mosheh b. Meshullam, of the second half of the eleventh century, is dated by Gil as written in 1070<sup>107</sup>. At this time, neither Sura nor Pumbeditha were functioning, as Brody himself writes. It is hardly surprising, then, that in the absence of an effective Geonate of Bavel, the Jews of Ashkenaz turned to that of Palestine. As to the second responsa, that of 960, I really don't know what the pecking order was in matters messianic. If any of the two institutions had traditions, whose were viewed as more authentic? We are left then with the first of the two questions, that of *the kashrut* of an animal with a lesioned heart (*sirkha de-liba*). One can't base anything on a lone instance. It may be that they rated the Palestinian Geonate higher in things millennial, and since they were sending that question to Palestine, they included the second. (Whether the reply was courteous or not, whether it triggered off a reaction which shaped Ashkenaz for a millennium has nothing to do with the initial impulse to inquire.)

One should emphasize that no one imagined that a person who lived by the laws of '*Trefot of Erets Yisra'el*' was eating non-kosher food. In the heterogeneous Jewish population that congregated in the tenth century in the Rhineland, some or many certainly followed the Palestinian rite. Thus, an answer from Palestine would do. It could equally be that a caravan for the spice road, which would usually entail a Babylonian land or sea segment, had already left, and that a caravan of pilgrims for Jerusalem was available. Any number of scenarios can be envisioned for this inquiry, but the most that any or all of them will yield is a single instance – which can serve as the basis of no inference.

I would like to conclude by adding that the last thing that the men of the Third Yeshivah were interested in was opening a quarrel with the Geonate of Palestine. Their purpose was to build 'a city on the hill', a new 'holy community', unpolluted by any involvement with the alien wisdom of Gentiles, a settlement dedicated to the study of the entire amoraic inheritance and

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narrowing the chronological window of 930-970 with which he began (*ibid.*, p. 157). [Brody's note] {HS replies: See above, n. 104.}

<sup>106</sup> Of course if Gil's dating in the 1060's is correct it is unclear what sort of functioning academies there were at that time in Babylonia; see Brody, *Geonim*, pp. 11-13. It is not impossible that scholars who saw themselves as "Babylonians" would nevertheless have applied to the Palestinian Geonic academy for guidance if there were no functioning Babylonian academy to which they could turn, but this possibility seems to me rather far-fetched. [Brody's note]

<sup>107</sup> Brody writes 1060; this, however, is a slip of the pen as Gil dates the inquiry as being made in 1070. See M. Gil, '*Megillat Evyatar—Mekorot le-Toledot Ma'avakeha shel Yeshivat Yerushalayim be-Mahatsit ha-Sheniyah shel ha-Me'ah ha-Ehat-Esreh*', B. Z. Kedar, ed., *Perakim be-Tolddot Yerushalayim bi-Yemei ha-Beinayim* (Jerusalem, 1979), 42.



with a view of the nature of *talmud Torah* far broader and capacious than that of Bavel, not to speak of other diasporas. As long as they were given a free hand to do this, a turning to a Palestinian Gaon, acceptance of a ruling by him, was utterly trivial. They accepted without a qualm Palestinian *piyyutim* (liturgical poetry) and even aspects of the Palestinian rite of prayer.<sup>108</sup> Why disturb the peace over some inquiries sent to the 'lions of Jerusalem', Eliyahu and Eyyatar ha-Kohen?<sup>109</sup>

#### אולי יאלפוני משהו חכמה

As I put down the pen, I would like to share with the readers my perplexity. For twenty-one pages Brody argues forcefully, even sharply, that my essay has no basis in reality. In the concluding three pages, he offers variations of my theme. Why write variations on a proposal that has no basis in reality? Again, he writes at the conclusion of the first part: (p. 286, n. 55) 'I doubt that any of the evidence cited by Soloveitchik, including that from orthography, actually shows that a wide variety of Talmudic manuscripts was available in Ashkenaz even by Rashi's time; certainly none of its speaks to the situation in the mid-tenth century, and Soloveitchik's attempt (ibid., pp. 125-141) to "push yet further back in time" is purely conjectural.' Yet in the next paragraph, the opening paragraph of the last section, he pushes the existence of talmudic texts in Ashkenaz to the mid-ninth century, stating: (p. 286) that 'One possibility is that the origins of Jewish culture in Ashkenaz go back significantly farther than the sources at our disposal document, and Ashkenazi Jews could have been engaged in the study of the Babylonian Talmud for a century or more before the floruit of Rabbi Leontin, the teacher of Rabbenu Gershom, in the middle of the tenth century.' The two parts of his essay seem to have been written by two different people.

At the end of a long night's day, I still don't know what Robert Brody really thinks.

<sup>108</sup> *Collected Essays*, ii. 141-143.

<sup>109</sup> A. Epstein, 'Die ahronideschen Geonim Palastinas und Meschullam b. Mose aus Mainz', *Monatsschrift für Geschichte und Wissenschaft des Judentums*, 47 (1903), 340-5. *Sefer ha-Pardes*, H. L. Ehrenreich, ed. (Budapest, 1924), 216.