

RASHI AS TEXTUAL CRITIC: A CLARIFICATION

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I recently published a critique of Haym Soloveitchik's theory of "the third yeshiva of Bavel," and Prof. Soloveitchik has responded at length to my criticisms.¹ There is no need to continue the arguments on most of the questions raised. I leave it to readers to form their own judgments with regard to the arguments offered on both sides. I have decided, however, that it is necessary to return to and expand upon one specific topic, the methodological implications of which exceed the significance of the question of the "third yeshiva".

One of my arguments against the theory that Ashkenazi Talmudic scholarship was founded by a group of Babylonian emigres is based upon the widespread consensus that Ashkenazi scholars, of whom the best example in this regard is Rashi, engaged in conjectural emendation of their received texts on a large scale,² which, I argued, would have been unthinkable to conservative Babylonian scholars of the Geonic era.³ Soloveitchik, however, downplays the role of conjecture in

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¹ Soloveitchik's original essay was included in H. Soloveitchik, *Collected Essays II*, Oxford and Portland, Oregon 2014, pp. 152-201. My critique was published as R. Brody, "On the Dissemination of the Babylonian Talmud and the Origins of Ashkenazi Jewry", *Jewish Quarterly Review* 109 (2019), pp. 265-288 (hereafter: Brody, "Dissemination"). Soloveitchik responded in the same issue, "On the Third Yeshivah of Bavel: A Response to Robert Brody", pp. 289-320 and continued his response by posting on his website (haymsoloveitchik.org) an additional 48-page response, "Reply to Brody II.pdf" which I refer to hereafter as "Response II".

² We should, I think, understand Rashi as emending a base text (as suggested by his terminology in promoting or rejecting readings). Even if he was aware of and had access to various manuscripts, he was not engaged in systematically comparing them and producing an eclectic text. In addition to the fact that this procedure would be implausibly cumbersome (see: P. Maas [tr. B. Flower], *Textual Criticism*, Oxford, 1958 p. 8), if Rashi had engaged in systematic comparison of two or more manuscripts, he would have had to decide between their readings far more often than is reflected in his emendations. I would imagine that if he indeed had more than one manuscript at his disposal, he would have consulted additional manuscripts only when he had occasion to suspect the reading of his base text.

³ With regard to the meaning of "conjecture," Soloveitchik writes (Response II, pp. 39): "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

Rashi's textual decisions and attempts to show that they are better understood as the product of Rashi's use of Eastern, and especially Yemenite, manuscripts of the Talmud. In addition to raising questions concerning the nature of the textual criticism practiced most prominently by Rashi, important for purposes of Talmudic textual criticism and exegesis, Soloveitchik's arguments raise methodological questions of import for textual criticism in general. To wit, how does one go about establishing a relationship between textual witnesses?

Before I proceed to deal with these questions, however, I must point out two factual errors in Soloveitchik's presentation which undermine his arguments. The first of these concerns a paper that Shai Secunda wrote in one of Soloveitchik's seminars, in which he collated Rashi's comments on textual issues in tractate Avodah Zarah with the direct textual witnesses to this tractate. Soloveitchik writes:⁴

Shai Secunda's research shows that in the tractate Avodah Zarah there is a 43 percent congruence of Rashi's emendations with manuscripts which, to use Friedman's typology, are either Mediterranean or of specifically Spanish provenance... Friedman has shown that one of the two manuscripts named Jewish Theological Seminary 15 is a composite. The first half (up to fo. 43) is of the "Mediterranean" type (in Friedman's orthographical typology), the second half (fos. 43-76) is Spanish. 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 degree of congruence of Rashi's emendations changes noticeably with the change of the textual tradition to which it is being compared. Apparently, Rashi was working from a manuscript that had more in common with the Mediterranean type than with the Sephardic one, and that differed considerably from the one that came to be called 'Ashkenazic'.

When Soloveitchik returns to this topic a number of years later, he provides different figures, with what appears to be a different explanation for the supposed discrepancy:⁵ "How," he asks,

editor/commentator (as unerring as that intuition may have been)." I doubt Soloveitchik really meant to say that "A fact can be wrong but remains a fact." Presumably he meant "datum" rather than "fact" – for example, a manuscript reading which is an undeniable datum may be wrong in that it departs from the original. Furthermore, a conjecture need not have corresponded "to nothing in the real world at that time," but only to no data known to the editor/commentator. It is a fairly common occurrence in the history of textual criticism that a reading offered as a conjectural emendation is later confirmed by the discovery of previously unknown textual witnesses. See for example: Maas, *Textual Criticism*, pp. 33-34, 40, and compare *ibid.*, pp. 37-38.

⁴ *Collected Essays, II*, pp. 124-125.

⁵ Response II, p. 40.

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? 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?

It would indeed be interesting if Rashi’s emendations agreed with the Spanish manuscript about 33% of the time in one part of the tractate and only 11-12% of the time in the other part of the same tractate, although one might interpret these hypothetical data in various ways.⁶ This diverges, however, from Secunda’s actual findings, which show a rate of agreement between 43.3% (13 of 30 cases) in the first part of the tractate and 35.3% (6 of 17 cases) in the latter part. It seems as if on the latter occasion, Soloveitchik divided the number of agreements in each section of the tractate by the *total number of cases in the tractate* instead of dividing by the *number of cases in the relevant section*. Such a calculation would give approximately, although not exactly, the results he cites ($13/47 = 27.7\%$, $6/47 = 12.8\%$).⁷

The second instance, to which I have called attention in my previous essay, Soloveitchik altogether fails to acknowledge. I wrote:⁸

For example, [Soloveitchik] 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

⁶ For example, in addition to the explanation(s) suggested by Soloveitchik, it could be that the “Spanish” part of his manuscript presented a better text – at least in Rashi’s estimation – than the “Mediterranean” part and therefore elicited less frequent emendation.

⁷ These numbers are based on Secunda’s paper which Soloveitchik uploaded on his website (haymsoloveitchik.org). Secunda notes numerous instances in which someone modified the base text of the manuscript in question by adding a variant reading or by emending the text, generally in order to bring it into line with Rashi’s reading. In all such cases I have taken into account only the original reading of the manuscript. In his original reference to Secunda’s paper (*Collected Essays*, pp. 124-125), Soloveitchik gave the figures of 47% agreement in the first section and 33% agreement in the second section, which are more nearly accurate; in note 74 to “Response II” he wrote: “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.” He does not, however, mention the discrepancy between the two figures he gave for the first part of the tractate.

⁸ Brody, “Dissemination”, p. 285.

tradition”. However, Sabato does not assert in even a single instance that Rashi’s emendation conforms to the Yemenite tradition alone.

In the accompanying note, I explained that Soloveitchik had misread Sabato’s summary table: the column with the number 13 represents the total number of agreements and 31 the percentage; Sabato never asserts that there are agreements unique to Rashi’s emendations and the Yemenite manuscript. Indeed, if Soloveitchik had read Sabato’s detailed discussions he would have seen that there are, in fact, no such cases, as I argue below.

Soloveitchik, however, responded to my correction as follows:⁹

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?

What is wrong with his statement, aside from the obvious and significant typo (13% instead of 31%), is that Sabato reported that Rashi’s emendations *never* agree with the Yemenite manuscript alone. Soloveitchik, by contrast, claims that according to Sabato, Rashi’s emendations agree with the Yemenite manuscript alone in 13% of cases. What follows from this, for example, is that if Rashi made some of his emendations on the basis of the manuscripts at his disposal, those manuscripts might as easily have been other (especially Ashkenazi) manuscripts, rather than Yemenite ones. Furthermore, even a cursory glance at Sabato’s summary table reveals that Rashi’s emendations agree with the Yemenite manuscript of Sanhedrin less frequently than they agree with any other manuscript of the tractate. I will return to the significance of these findings below.

Soloveitchik asks why I find it impossible that someone in early Ashkenaz would have obtained a Talmud manuscript (or manuscripts) from Yemen given the evidence for the existence of trade routes connecting Mainz and Yemen.¹⁰ I do not take issue with the evidence he has mustered with regard to the existence of such trade routes but find it implausible that Yemenite Talmud manuscripts made their way to early Ashkenaz for a number of reasons: (1) to begin with, it is not clear that Yemenite Talmud manuscripts even existed at this time;¹¹ (2) if they did exist, we

⁹ Response II, n. 80.

¹⁰ See: Response II, pp. 17-19.

¹¹ Two leading scholars of Yemenite extraction (Y. Tobi, *The Talmud in Yemen* [Hebrew], Tel Aviv 1973; Y. Ratzaby, *The Torah of the Yemenites: Authors and Compositions* [Hebrew], Kiryat Ono 1995) were unable to find any evidence of rabbinic learning in Yemen prior to the twelfth century. Maimonides in his letter to the scholars of Lunel, written at the end of the twelfth century (Y. Shilat [ed.], *Letters of Maimonides* [Hebrew], Jerusalem 1988, p. 559) laments that “in all the towns of Yemen and the Arabians few engage in Talmud but they do not understand... because they are at the ends (of civilization)”. The earliest extant Yemenite manuscript of any sort of rabbinic literature dates to the thirteenth, or possibly the twelfth, century. The Institute for Microfilmed Hebrew Manuscripts in Jerusalem has documented fewer than ten fragments of Yemenite Talmud

would have to assume that someone in early Ashkenaz knew of them, thought they would be particularly valuable, and then went to the trouble and expense of obtaining one or more such manuscripts;¹² (3) we would need to explain why Rashi (and other Ashkenazi sages) never mention relying on such manuscripts in determining the correct text.

True, Rashi generally does not offer justification for his textual decisions – and in many cases it is not even clear which prior reading(s) he is rejecting. But when he does, the justification is almost always based either on parallel sources or on his own reasoning and understanding of Talmudic style and argumentation. Reference to textual witnesses, whether direct or indirect, is very rare, and to the best of my knowledge when direct witnesses are cited they are always Ashkenazic.¹³ In stark contrast Spanish *rishonim* frequently argue for particular readings on the strength of “accurate manuscripts”, “manuscripts of the academies,” and the like. Rashi occasionally makes it clear, as do numerous comments by both Tosafists and Spanish scholars, that at least some of his emendations had no basis in any textual precedents.¹⁴ I cite one striking example in which Nahmanides writes:¹⁵

Rashi’s interpretation would have been preferable if his reading were written in the manuscripts, but it is impossible to emend the texts... [and] the books of the academies and the compositions of the earlier and later Geonim serve as proof, and if you can find among them one accurate manuscript which in its original form reads like Rashi we will accept it, but one cannot learn from erasures and interlinear additions in recent texts.

manuscripts, tentatively dated to the 14th-16th centuries, while the extant Yemenite manuscripts of complete Talmudic tractates date to the 15th-17th centuries. My thanks to Menahem Ben-Sasson for pointing me to Tobi’s book and to Benjamin Richler for most of the other information cited in this note.

¹² If they were interested and had the resources to obtain additional manuscripts, I imagine they would have turned either to Spain, which was much closer and maintained close ties with the Babylonian academies, or to Bavel itself, especially if the scholars themselves came from there and presumably retained some connections with Babylonian scholars. Yemenite manuscripts play a prominent role in contemporary investigations into the text of the Babylonian Talmud because they are among the very few non-Genizah, non-European manuscripts to survive. There is no reason, however, to think they would have stood out (assuming they existed) in the consciousness of early European scholars.

¹³ See for example Rashi’s commentary on Shabbat 63b, 74b, 119a, Pesahim 74a and especially Rosh Hashanah 28a and Sukkah 40a. Rashi mentions readings he found in Geonic responsa approvingly in his commentary on Eruvin 101a and Pesahim 4a (and compare his commentary to Shabbat 19b and the parallel at 156b). He treats readings found in other works of Geonic literature more skeptically, see for example his commentary on Berakhot 50b and Shabbat 135b.

¹⁴ See for example Rashi’s commentary on Besah 18b-19a and Pesahim 96a and Tosafot on Yoma 34b, 40a, 67b.

¹⁵ Novellae to Ketubbot 53b and *Milhamot Hashem* on Alfasi ad loc..

The evidence Soloveitchik presents for Rashi's dependence on Yemenite manuscripts is in fact illusory. He relies heavily on Vered Noam's work which stresses the many agreements between Rashi's emendations/determinations in tractate Sukkah and one or more Eastern manuscripts. She views this as evidence that "at least some" of Rashi's textual decisions were made with reference to texts "from Eastern traditions which he had", agreeing, however, that in other cases, Rashi's emendations are conjectural.¹⁶ In my critique of Soloveitchik's essay I wrote:¹⁷

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 case 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.

To this Soloveitchik responded:¹⁸

Textual studies like any field have numerous approaches. Stated abstractly, some seem to conflict with others. In practice, however... the researcher chooses the tools best suited to eliciting answers to his or her questions from the aggregating data. With all due respect to my distinguished colleague, Vered Noam chose the right tool, he the wrong one.

Sometimes, however, it appears that a researcher chooses the tools best suited to eliciting his or her desired answers. I doubt whether any reputable textual scholar would agree with Soloveitchik that Noam's method of simply counting correlations without examining their nature is preferable to the sort of painstaking analysis undertaken by Talmudic philologists such as Eliezer Segal and Sabato.

Soloveitchik provides the following justification for his advocacy of Noam's methods:¹⁹

¹⁶ V. Noam, "Early Textual Traditions in Rashi's Emendations of the Talmud" [Hebrew], *Sidra* 17 (2001-2002), pp. 109-150; the quotation is from p. 136.

¹⁷ Brody, "Dissemination", pp. 284-285.

¹⁸ Response II, pp. 39-40.

¹⁹ Response II, p. 38. On pp. 38-39 of his response, Soloveitchik recommends that I familiarize myself with Rabin Shustri's dissertation which was "submitted some eight years after Noam's essay" and "concludes that Rashi had eastern manuscripts". I am indeed familiar with this dissertation, which proceeds along the lines proposed by Noam and offers no methodological improvement. On p. 41 he writes that "I did not invoke Segal's thesis that Noam mentioned in her superb article... because the instances of *Sukkah* and *Sanhedrin* were sufficient." But in fact Soloveitchik relied on Noam and on her problematic interpretation of Sabato's work.

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?!

The question, however, as phrased is meaningless – two manuscripts cannot “have a common stemma” as the term “stemma” refers to an entire family tree, encompassing all the direct textual witnesses (and perhaps some indirect witnesses as well). Soloveitchik apparently intends to argue that agreement in, let us say, 50% of cases would show that two manuscripts belong to one family. But this too is incorrect – probably any two manuscripts of a single Talmudic tractate agree more often that they disagree. I would go far as to suggest that this is likely true of almost any two manuscripts of any single work in classical literature or any other, for even the accumulated depredations of the worst copyists and most violent emendators generally affect fewer passages than those they transmit accurately.

Let me offer a simplified example:²⁰ Suppose that Rashi succeeded in emending the text correctly half the time, either because he had access to additional manuscripts which did not contain certain errors found in his base text or because he was able to recognize such errors by virtue of his intimate familiarity with Talmudic style and with the foibles of scribes. Suppose further that in 90% of these instances the Yemenite manuscript preserved the correct reading; there is no reason *a priori* to expect that this manuscript should present a corrupt text in the same places in which Rashi’s base manuscript was corrupt. This would produce a rate of agreement of 45% overall between Rashi’s emendations and the Yemenite manuscript without presenting any evidence for a connection between Rashi and the Yemenite manuscript beyond their remote common source, the Talmud as first published or as first rendered in writing.

The critical question for textual critics is which agreements and disagreements are to be considered as significant and meaningful in determining textual affiliation. The approach favored by Noam and Soloveitchik misses two crucial points, the first of which is well formulated by Martin West.²¹ “It will be possible,” West writes, “to

²⁰ This example is simplified *inter alia* because of the stage of oral transmission that preceded that of written transmission, as a result of which we must on occasion treat more than one reading as “primary”. (I deal with these complexities at length in the introduction to my forthcoming commentary on tractate Ketubbot; see meanwhile: R. Brody, “Geonic Literature and the Talmudic Text” [Hebrew], *Mehqere Talmud I* [1990], pp. 237-303, especially pp. 237-242, 276-281.) For our present purposes, we may overlook this complication and concentrate on the identification of clearly secondary readings.

²¹ M.L. West, *Textual Criticism and Editorial Technique*, Stuttgart 1973, p. 32. I am not aware of theoretical discussions which deal with the comparison of a set of emendations to direct witnesses; in what follows, I treat Rashi’s emendations as if they represent a fragmentary witness. A full-scale

deduce” the relationship between texts from “from the pattern of agreements and disagreements among them.” He emphasizes, however, that it is important to realize that what is significant for this purpose is not agreement in true readings inherited from more ancient tradition, but agreement in readings of secondary origin, viz. corruptions and emendations, provided that they are not such as might have been produced by two scribes independently.

In other words, when two or more textual witnesses agree in a correct reading, we may deduce nothing about the processes of transmission which produced them other than that they ultimately derive from the same source. Only shared errors (and not even all of these) may be presumed to reflect a shared heritage at some later stage in the transmission of the text. The formulation of Paul Maas’ classic work on textual criticism reinforces this point and elicits the second as well:²²

It can be proved that two witnesses (B and C) belong together as against a third (A) by showing an error common to B and C of such a nature that it is highly improbable that B and C committed it independently of each other. Such errors may be called ‘conjunctive errors’ (*errores coniunctivi*).

As Maas’ formulation implies, the textual critic seeks to establish a relationship between two or more textual witnesses (B and C in his example) in contrast to one or more other witnesses (A in his example). This context cannot be ignored: if, for example, all textual witnesses share a given reading, one cannot draw any conclusions about which of them may be more closely and which only distantly related. The question therefore is whether we find errors common to Rashi’s emendations and to the Yemenite manuscripts and not to other textual witnesses. But before proceeding, a further caveat is necessary. Even if we were to find errors common to Rashi and to a Yemenite manuscript, it would be likely that these errors made their way *from Ashkenaz to Yemen* and not vice versa. All extant Yemenite Talmud manuscripts are very late (15th-17th centuries). Alongside the early readings these manuscripts may have preserved, they also incorporate numerous glosses, and at least in the case of the manuscript which includes tractate Sanhedrin, it is almost certain that some of these derive from Rashi’s commentary.²³ It would not be overly

theoretical discussion would require somewhat different formulations than those adopted in the comparison of direct textual witnesses but essentially the same arguments apply.

²² Maas, *Textual Criticism*, p. 43.

²³ M. Sabato, *A Yemenite Manuscript of Tractate Sanhedrin of the Babylonian Talmud and its Place in the Textual Tradition* [Hebrew], Jerusalem 1998, pp. 309-310 discusses glosses incorporated in the Yemenite manuscript of Sanhedrin which may derive from Rashi’s commentary but states that he found no unequivocal examples. In note 93 ad loc. he refers to E. Diamond, *A Specimen Critical Edition of and Commentary on Tractate Ta’anit of the Babylonian Talmud* [Hebrew], Ph.D. dissertation, Jewish Theological Seminary of America 1990, who found two clear examples of glosses based on Rashi’s commentary which found their way into the text of tractate Ta’anit in the

surprising if, in addition to inserting such glosses, Yemenite scribes occasionally introduced emendations from the same source.

What we find, however, at least with regard to tractate Sanhedrin, is a lack of common errors. The results of Sabato's analysis conform to what we would expect *a priori* to happen when a witness from a distant part of the stemma is brought to bear on the investigation of a series of emendations, namely that it provides confirmation of most of the good emendations and further evidence against most of the bad ones.²⁴ (I would add that a similar picture emerges when we compare Rashi's emendations with the Byzantine manuscript of tractate Ketubbot,²⁵ or more generally when we compare Rashi's emendations to the readings of early Genizah fragments.) Sabato describes twelve cases in which Rashi's emendations agree with the Yemenite manuscript.²⁶ In five of these cases, all the direct witnesses agree (sometimes with minor variations) with Rashi's emendation and it is not clear what reading(s) he was rejecting – presumably an idiosyncratic error in his base text.²⁷ In three additional cases, Rashi's reading is shared by all the direct witnesses except the Ashkenazic Karlsruhe manuscript. Rashi's base text presumably resembled this manuscript, and he rejected some of its peculiar errors.²⁸ In three cases the Yemenite manuscript is joined by several direct witnesses in agreeing with Rashi's emendation, and it is fairly clear that this emendation is correct. In the remaining case, the Yemenite manuscript is joined by one other direct witness and its agreement with Rashi's reading concerns what Sabato correctly describes as “an insignificant variant”.²⁹

same manuscript. (These examples, both on Ta'anit 2a, are discussed in Diamond's dissertation at pp. 146-147 and p. 152, n. 22.)

²⁴ I write “most” and not “all” because: (1) with regard to good emendations the manuscript in question may have a corruption at some of the relevant locations; (2) with regard to bad emendations this manuscript too may not provide the original reading, either because it has been accidentally corrupted or because it has been emended because of the same difficulty which triggered the emendation of the scholar whose work is under consideration (if the manuscript hit upon the same emendation as the scholar in such a case Maas, *Textual Criticism*, pp. 37-38 calls this “deceptive confirmation”).

²⁵ I demonstrate this in the introduction to my forthcoming commentary on this tractate of the Babylonian Talmud.

²⁶ I am not sure which example he counted as number 13 in his summary table, probably his example no. 9 in which Rashi gives two readings, one of which is similar but not identical to the reading of the Yemenite manuscript. I ignore this case in the summary below.

²⁷ See Sabato (n. 23 above), pp. 239 (example 14), 240 (example 16), 241 (example 19), 251-252 (example 38), 252 (example 39).

²⁸ *Ibid.*, pp. 231 (example 2), 239-240 (example 15), 249 (example 35).

²⁹ Examples 20 and 26 in Sabato's list are corrections of clearly secondary readings (see *ibid.*, pp. 241-242, 244); example 27 is more complex but it appears that Rashi once again rejected a secondary reading found in MS Karlsruhe, see Sabato, p. 244. Example 44 is the one which Sabato describes as insignificant. At issue is which of two cases which the Talmud contrasts is referred to as “here” and which is referred to as “there” (see Sabato, p. 255; as he points out, in a parallel discussion on folio 75b all the direct witnesses have the reading which Rashi adopts on 76a).

To summarize: (1) affiliations between different textual witnesses must be established on the basis of their common errors;³⁰ (2) Rashi frequently engaged in conjectural criticism of the Talmud text; and (3) the evidence overwhelmingly supports the picture I painted of “a fragile textual tradition, based on one or two manuscripts of any given tractate which had been imported to Ashkenaz,” or, in Professor Soloveitchik’s somewhat bathetic paraphrase, “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”.³¹ I believe early Ashkenazi sages were well aware of the fragile foundations of their textual tradition for the Babylonian Talmud and it was this awareness that led Rashi and others to strive to improve on their received texts with any means at their disposal, in spite of the risk of creating new texts that would be even more distant from the original Talmud.³²

³⁰ This formulation is slightly simplified; compare note 20 above.

³¹ Brody, “Dissemination”, p. 284; Response II, p. 35.

³² Rashi’s grandson Jacob b. Meir (R. Tam) famously testified in the introduction to his *Sefer Hayashar* that his grandfather had confined his emendations to his commentary and never touched the actual text of the Talmud and strongly criticized later scholars (including his own brother Samuel) for departing from this precedent.