

Dear Dr Zidon,

Your manuscript, "Main Manuscript for", has now been assessed.

We invite you to revise your paper, carefully addressing the comments from the reviewers and the editor. Please ensure the results are accurately reported, any overstated conclusions are rewritten and the limitations of the work fully explained. When your revision is ready, please submit the updated manuscript and a point-by-point response. This will help us move to a swift decision.

#### Editor Comments

"There are no specific comments from the handling editor."

-Shai Gordin

Handling Editor

Humanities and Social Sciences Communications

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We recommend submitting all revisions within the mentioned deadline.

If you need more time, please contact us and include your submission ID.

Kind regards,

Bhagyashree Gilada

Handling Editor

Humanities and Social Sciences Communications

[Support contact: hsscomms@springernature.com](mailto:hsscomms@springernature.com)

#### Reviewer 1

This article aims to discuss the relation between the Mishnah and the Tosefta by analysing the dispersion of the places mentioned in these corpora. However, the paper does not stand in conversation with the rich scholarly literature existing on this topic (both for the discipline of rabbinic studies and for the DH field). Since there is no conversation, it is difficult to estimate if there are any innovations in their conclusions. In addition, its methodology is not clear enough: What text was chosen for these corpora (manuscripts? print?)? How were the place names identified and tagged?

How was their geographical location identified? Since maps and even digital maps of Tanaitic places already exist, and anyway, they did not provide the reasoning, this is not the contribution of this article. The main conclusion that the places of the Tosefta are more widely spread and have more Galilee sites is already known, as the authors correctly stated. It is evident from simply looking at the map. It is not clear how the use of digital tools helped with that. It is also not clear what the relevance of the statistical analysis presented in Figure 1 is. It was not used in the discussion. Neither the digital and statistical tools nor the map can help explain the situation and choose between the chronological and geographical explanations proposed in the paper. The discussion here was shallow and again not in conversation with existing scholarship.

Moreover, there are contradictions within the data presented to the reader or the data is not well explained.

More detailed comments are added to the manuscript in case the authors would like to correct the article and submit it elsewhere.

Attachment(s):

- [Download Reviewer 1 attachment 1](#)
- [Download Reviewer 1 attachment 2](#)

Reviewer 2

I read this interesting manuscript with great appreciation, and I would like to begin by noting how much I enjoyed engaging with it. The article focuses on a comparative study of the places mentioned in the two central halakhic compositions of the Tannaitic period in the late Roman era, the Mishnah and the Tosefta, and presents a statistical and quantitative analysis of the notable differences between them. This comparison is particularly intriguing because, in many respects, the two works are closely related, although not identical: in their content, in their structure, and in the identity of the sages who appear in them, a point to which I will return below. The differences identified by the authors between the two corpora undoubtedly reflect an important aspect of their respective character. Each of these compositions has been extensively studied in its own right, and the relationship between them has likewise received considerable scholarly attention in recent years. Much of this research has been carried out within the traditional field of Rabbinic studies, alongside some latter work employing digital and computational methodologies. In this respect, the present article contributes to an important ongoing discussion among scholars of ancient Rabbinic literature. Let me also state what is almost self-evident: I strongly support the methodological approach reflected in the article, namely the application of statistical and quantitative analytical tools to the study of ancient texts. This methodological trend has been developing rapidly in recent years and holds considerable potential for advancing our understanding of literature and society in antiquity and for generating genuinely new insights. The challenge, of course, is not merely to collect large datasets and apply statistical and quantitative analysis that ultimately leads to conclusions already reached by earlier scholars. Although such confirmation can be valuable, since it provides additional support for established scholarly conclusions, and demonstrates that statistical and quantitative approaches are valid and reliable. Nevertheless, the broader aim of such methods should be to generate new and meaningful insights that earlier scholars, even highly distinguished ones, were not able to identify.

In this respect, I believe the present study has very significant potential, but in its current form that potential is not fully realized. I will elaborate.

Already the earliest discussion of the redaction of the Mishnah and the Tosefta, Rav Sherira Gaon's well-known Epistle, states that the Tosefta was compiled shortly after the Mishnah. Although both commentators and modern scholars have pointed to earlier sources embedded within the two compositions, and even to collections within them that may have been edited at relatively early stages, there is broad scholarly consensus that the principal redaction of the Tosefta postdates the principal redaction of the Mishnah. Scholars differ regarding how much later the Tosefta's redaction should be dated relative to that of the Mishnah, but the general chronological relationship between the two works has been recognized in scholarship for several generations.

Classical scholars have also carefully described the various generations of sages represented in these works, from early layers originating in the late Second Temple period, through the two generations of sages active in the "South" during the Yavnean period, and onward to the sages active primarily in the Galilee after the Bar Kokhba revolt, from roughly the middle third of the second century through the early third century. Scholars have further noted that the last major stratum represented in the Mishnah belongs to the generation of Rabbi Judah the Patriarch, around the turn of the second and third centuries. By contrast, the Tosefta contains a significant layer of traditions attributed to sages from the generation following Rabbi Judah the Patriarch, including even a statement attributed to Rabbi Abba, later known as the Amora Rav, who died in 247 CE.

Since the places mentioned in a composition such as the Mishnah or the Tosefta presumably reflect, at least to some extent, the locations in which the sages mentioned in these texts were active, or the contexts in which their teachings circulated, the expectation, based on the well-established reconstruction of the generations of sages in the two works, is that the Tosefta would contain a broader layer of references to Galilean locations. In the article, however, this observation is presented as a central insight, whereas in light of the classical scholarly literature on the Mishnah and the Tosefta it does not appear to constitute a new discovery.

I wish to emphasize that I do not reject the method employed in this study. On the contrary, I would strongly encourage the authors to continue developing it and to present the conclusions that can emerge from it. In my view, however, the dataset invites further analysis that could place greater emphasis on genuinely new insights while engaging more directly with the conclusions already reached by earlier scholars.

First and foremost, I believe it is essential for the authors to present more explicitly the existing scholarly discussion regarding the dating of the redaction of the two compositions, and the different strata of sages preserved within them. In other words, the present study should be more clearly situated within the framework of classical scholarship dealing with the formation and redaction of the Mishnah and the Tosefta. This point is particularly important in light of the authors' statement near the end of the Discussion that the Mishnah primarily preserves traditions from Judea. Such a claim does not align with the conclusions of scholars who have studied the formation of the Mishnah, especially in view of the prominent presence of sages associated with Usha in the Mishnah. This does not necessarily invalidate the findings of the present study, but it does require that those findings be discussed in relation to the classical scholarly literature addressing precisely this issue.

Similarly, the present study does not sufficiently engage with previous statistical and quantitative research that has already been conducted on the Mishnah, the Tosefta, and other corpora of Rabbinic literature, research that could help situate the present study's contributions within the existing scholarly landscape. I would note, for example, the work of Kazhdan and Kay, several studies by Zhitomirsky-Geffet and collaborators, as well as research by Satlow on the Babylonian Talmud.

In addition to this central observation, I would like to offer several more specific comments:

The study very interestingly identifies differences between various parts of the Mishnah, differences between different sections of the Tosefta, and certain affinities between particular sections of the

Mishnah and sections of the Tosefta. It might be worthwhile to explore these textual domains in greater depth and perhaps to examine whether these patterns correspond to conclusions reached by other scholars concerning the distinctive character of specific parts of the two corpora.

As mentioned above, the prevailing scholarly view holds that the Tosefta was redacted shortly after the Mishnah. However, some scholars, on literary grounds, have proposed dating the redaction of the Tosefta as late as the end of the fourth century. In this context, the present study might potentially contribute to the debate by examining whether the locations mentioned particularly in the Tosefta correspond to sites known to have been active during specific historical periods. Such an investigation could shed light not only on the dating of the Tosefta's redaction but also on the possibility that certain materials entered the composition at stages subsequent to its principal redaction. This line of inquiry could also be integrated with the authors' suggestion for future research, namely the possibility that the Tosefta refers to urban locations more frequently than the Mishnah and the implications this might have for dating the materials preserved in it.

There are also several tractates that appear only in the Mishnah and not in the Tosefta, and the reason for this remains uncertain. It would be interesting to consider whether the authors have anything to say about the character of these tractates and whether they exhibit any distinctive features.

Finally, I would like to reiterate my initial observation that the methodological approach of the study is highly promising and has the potential to make an important contribution to the scholarly discussion regarding the nature of these two major compositions. My comments are intended to help the authors further develop their research and deepen the connection between their findings and the scholarly literature. I very much hope that the authors will consider some of these suggestions and submit a revised version of their study. In my view, an improved version of this work could constitute an important contribution and would be worthy of publication in a leading journal such as *Humanities and Social Sciences Communications*.

Recommendation: Revise and Resubmit.