Jacob Mann's contribution to the history of the Jewish calendar

Jacob Mann was an expert in many fields, but the Jewish calendar was not one of them. Nevertheless, he made several discoveries that shifted the modern scholarly understanding of the origins and history of the Jewish calendar, in ways that he did not always fully appreciate himself. These discoveries suggest a certain flair for identifying what was historically important, although they were surely not serendipitous: they were only made possible through the hard, relentless work of sifting through large quantities of untapped Genizah materials. As Mann himself wrote, not without a tinge of humour:

> Search among these tattered and torn leaves is occasionally rewarded by the finding of an important and interesting fragment as a recompense for the labor of several days that appears to have been wasted.[1]

In this paper, I shall discuss two important findings of Mann in the field of calendar research: a Geonic responsum from the last decade of the tenth century, and the letter of an exilarch dated 835/6 CE. The discoveries were historically very significant, but as I wish to argue, Mann's treatment of these texts was limited and evinces excessive haste, a certain slackness and lack of attention to detail. My presentation of these two discoveries will also lead to a number of general reflections on Mann's methodology as a philologist.

<u>The history of the Jewish calendar, as known in Jacob Mann's time</u>

By the 1920s, when Jacob Mann's research activity began to peak, significant advances were being made in the modern, scholarly understanding of the origins and history of the Jewish calendar. I shall begin by outlining the issues at stake, and then what was known in Mann's period.[2]

Early rabbinic sources assume that the Jewish calendar was set on a monthly basis by sightings of the new moon crescent. By the tenth century, however, the empirical calendar had been abandoned by Rabbanites in favour of a fixed calculation, which become dominant in world Jewry and has remained unchanged until today. The origins of this calculated calendar are yet to be fully understood, although some of its elements can be traced back to Talmudic sources. it is unclear, to this date, when and why this new calendar was instituted.

The traditional narrative is that this calendar was instituted by a rabbinic figure named Hillel, descendant of the Patriarchal family, in the middle of the fourth century CE. This narrative – not to mention the figure of Hillel himself – is not attested or even hinted at in Talmudic and Midrashic literature; but this did not prevent it from achieving dominance in late medieval Jewish historical memory. Although this tradition was subsequently disproved by early twentieth-century scholars, largely on the basis of recent Cairo Genizah discoveries (as we shall presently see), it has remained tenacious enough to be still quoted, uncritically, in encyclopaedias, university course syllabi, and general handbooks of history.

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[1] J. Mann, 'Gaonic Studies', *HUCA Jubilee Volume*, 1925, pp. 223-62, on p. 237.

[2] On what follows, see further S. Stern, *Calendar and Community: A History of the Jewish Calendar 2nd century BCE – 10th century CE*, Oxford: Oxford University Press, 2001, 175-9.

The earlier attestation of the Hillel tradition is in a responsum attributed to Hayye Gaon, dated 991/2 CE, that was preserved by Abraham bar Ḥayya in his treatise on the calendar, *Sefer ha-Ibbur* (3:7), completed in 1123 CE. Abraham b. Ḥayya indicates that the responsum was originally written in Aramaic (which he calls אשורית), although he quotes it in Hebrew, and he emphasizes that he is quoting 'from beginning to end' and that he 'did not need to add to his words'. In response to a question about the origins of the calendar and its calculation, Hayye Gaon writes in the responsum:

> הוי יודע כי החשבון הזה אשר בידינו לא מן אדם הראשון ירשנו אותו ואין אנו יודעין בימי אדם הראשון האיך נעשה. וכך[3] אנו אומרין כי משה רבינו ע'ה' למד את ישראל עקרו של הסוד הזה ... וא'ע'פ' שנתן להם הסוד הזה הזהיר אותם שכל זמן שסנהדרי קיימת יש להם לשנות מן המסורת ולהקדים ולאחר ... עד ימי ר' הלל בר' יהודה בשנת ת'ר'ע' שמאותה שנה לא הקדימו ולא איחרו אלא אחזו הסדר הזה אשר היה בידם
>
> Know that this calculation that we possess – we did not inherit it from Adam, and we do not know how in the days of Adam it was done. And so we say, that Moses taught the foundation of this secret to the Israelites … And even though he gave them this secret, he exhorted them that as long as the Sanhedrin was in existence, they would be able to deviate from the tradition, to advance and to postpone … until the days of Rabbi Hillel son of Rabbi Judah, in the year 670 (SE), from which year they neither advanced nor postponed, but rather maintained this order which was in their possession.[4]

Context indicates that Hayye is referring specifically to the practice of intercalation (adding a second month of Adar), which originally, in the days of the Sanhedrin, was flexible and could be advanced or postponed, until the days of Hillel when intercalated years were fixed in the form of a 19-year cycle within which seven intercalations are made. But in the mid twelfth century, the Hillel tradition expanded to include not only the cycle of intercalations, but the entirely of the fixed rabbinic calendar. Referring to the postponement rules which lie at the heart of the rabbinic calendar (whereby the New Year is always postponed from Sunday, Wednesday, and Friday, together with other postponements), Zeraḥiah ha-Levi wrote in his Talmudic commentary, *ha-Maor ha-Qatan* (*Rosh ha-Shanah*):

> והוא מסור בידינו על פי החשבון בסוד העבור מימי הלל בן יהודה בן רבינו הקדוש שהנהיג לקדש על פי החשבון
>
> This has been transmitted to us according to the calendar calculation from the days of Hillel son of our Holy Rabbi,[5] who instituted to sanctify (the months) on the basis of the calculation.

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[3] From this point onwards Mann argued that the text was not original to Hayye Gaon, but interpolated by Abraham b. Ḥayya, as we shall see below. The printed edition (H. Filipowski, *Sefer ha-Ibbur le(...)Avraham bar Ḥayya,* London: Longman, Brown, Green and Longmans, 1851, 97-8) reads: וכך אני אומר, but this is an error, as will be explained below.

[4] Text and translation are based on a draft prepared by Israel Sandman, as part of the AHRC Major Research Grant project at UCL, 'Medieval Monographs on the Jewish Calendar' (2008-2013). Among the best manuscripts is, for example, Ms Vatican Urb. Ebr. 48, where the passage appears on fol. 49r.

[5] A common title, in early rabbinic literature, for Judah the Patriarch. This identification possibly differs from Hayye Gaon, who calls Hillel's father 'Rabbi Judah', a title never used for Judah the Patriarch.

The theory that the entirety of the calendar was instituted by Hillel was confirmed in the thirteenth century by Nahmanides and his disciples, who conceived of Hillel as sanctifying all the months in advance, through calculation, until the end of times; and this theory remained unchallenged until the nineteenth century.

In 1852, the Polish polymath Ḥayyim Selig Slonimski was perhaps the first to cast doubt on the Hillel tradition. He questioned how a rabbinic institution of such importance could not have been mentioned, let alone described and discussed, in the Palestinian or Babylonian Talmud; and he concluded, for this reason alone, that the fixed calendar must have been post-Talmudic.[6]

The next scholar to cast serious doubts on the tradition, now on the basis of evidence, was Hayyim Yehiel Bornstein in Warsaw, in 1904. He pointed out that the date of death of Rav Aḥai b. R. Huna on Sunday 4 Adar 817 SE (506 CE), as given in the *Epistle of Sherira Gaon*, was incompatible to the fixed rabbinic calendar; this proved to him that the fixed calendar had not yet been instituted in the sixth century.[7] He also noted that the *Baraita de-Shemuel*, first published in 1861 and referring to an astronomical event in 776 CE, employed a different, more simple and considerably less accurate calculation of the *molad* (new moon).[8] Moreover, new Cairo Genizah discoveries were rapidly shedding light on a great calendar controversy that broke out between Palestinian and Babylonian Rabbanites in 921/2, due to exceptional conditions that applied to the calendar calculation in that year. Besides the mere fact that a controversy over the calculation proved that it was not yet completely finalized, Bornstein found that the same conditions should have arisen in 783 CE, yet there was no record of such a controversy in that year – suggesting, again, that the calendar calculation was not yet in existence in this period.[9] Finally, he intimated that if, as it seemed, the *molad* calculation in the rabbinic calendar was based on values drawn from Ptolemy's *Almagest*, this borrowing could only have been made after the *Almagest* became accessible to the Jews in Arabic translation; translations of this work only began to appear from the early ninth century.[10] All this pointed to the institution of the fixed rabbinic calendar at some point in the ninth century.

This was the state of the art, with regard to the history of the Jewish calendar, when Jacob Mann began researching the Cairo Genizah.

<u>Hayye Gaon's responsum</u>

In 1925, Jacob Mann discovered and published a Cairo Genizah manuscript from the Bibliothèque Nationale et Universitaire de Strasbourg, containing the responsum of Hayye

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[6] H. S. Slonimsky, *Yesodey ha-Ibbur,* Warsaw, 1852. See also the second edition (1865: pp. 34-7 and n.2), where he responded to his traditionalist critiques.
[7] H. Y. Bornstein, 'The dispute of Rav Saadya Gaon and Ben Meir' [Hebrew], in *Jubilee Volume for Naḥum Sokoloff*. Warsaw: Shuldberg, 1904, 19-189 (paginated 5-180 in a separate edition, dated 1904, which I use here), on p. 18; also id., '*Divrei yemei ha-ibbur ha-aḥaronim*', in *Ha-Tequfah*, 14–15 (1922), 321-372, on p. 370.
[8] Bornstein (1904) 115-16, with further evidence from *Pirqei de-Rabbi Eliezer* (ch. 7) and other sources.
[9] Bornstein (1904) 30, n. 1; (1922) 349-50.
[10] Bornstein (1904) 19-20; (1922) ibid.

Gaon which until then was only known through its secondary quotation by Abraham b. Ḥayya.[11]

The finding was particularly significant in that it preserved a different version of the responsum, with some significant differences. There were extensive passages unique to only one or the other version: for example, a reference to the calendar controversy of 921/2, only in Mann's manuscript.[12] Another difference was the date: in this manuscript, the year used as paradigm (and thus normally taken as the year of composition) was 994/5 CE, whereas in Abraham b. Ḥayya's version, the paradigm year was 991/2 CE. Perhaps most importantly, the narrative of Hillel instituting the calendar (or at least, the cycle of intercalations) was absent in the Genizah manuscript.

Jacob Mann argued that the Genizah manuscript preserved the authentic version, whereas the text of Abraham b. Ḥayya was heavily tampered, by none other than him. The evidence in support of this contention was close to non-existent. It contradicted Abraham b. Ḥayya's own insistence, as we have seen above, that he was quoting Hayye's responsum 'from beginning to end' and 'without need to add to his words'. It also raised the question of why, among other interventions, Abraham b. Ḥayya would have changed the date of the paradigm from 994/5 to 991/2.

Aware of this difficulty, Mann suggested alternatively, and in my view not unreasonably, that both versions could have been authentic, and represented letters that were written and sent by Hayye Gaon at different times to different addressees, although much of the text of the first letter was borrowed and re-used in the second. This explanation has much to be commended, since the Genizah version (which is the later version, of 994/5CE) refers explicitly to a similar letter that had been sent previously by the same author 'to the land of Egypt'.[13] Yet inexplicably, Mann did not carry through this line of argument, but without signalling this or stating any reason, he reverted immediately, in the rest of his short discussion, to the assumption that Abraham b. Ḥayya's version was inauthentic and interpolated. Thus, his partiality for the Genizah fragment very quickly prevailed.

Mann proceeded to argue, on this basis, that the Hillel narrative that only appears in the version of Abraham b. Ḥayya was not an authentic part of Hayye Gaon's responsum, but rather an interpolation of Abraham b. Ḥayya's own making. Other than the absence of this narrative in the Genizah version, there was little evidence to support this view. Mann observed that the supposedly interpolated passage was introduced with the phrase 'so I say' (וכך אני אומר) in the singular, whereas Hayye Gaon (and indeed, Geonic responsa in general) conventionally referred to themselves in the plural. This might have served as a valid philological argument, were it not for the fact that the printed edition of Abraham b. Ḥayya's *Sefer ha-Ibbur*, which Mann relied on, was erroneous at this point: the reading of all the

[11] Ms Strasbourg 4038, fols.2r-6v; Mann (1925) 237-48. Mann argued that the responsum was authored jointly by Sherira Gaon and Hayye, but I am not fully convinced.

[12] The relevant passage is re-edited and studied in detail in S. Stern, *The Jewish Calendar Controversy of 921/2 CE*, Leiden: Brill, 2019, 440-51.

[13] Fol. 2r, a passage that Mann acknowledges and refers to explicitly. This is the line of argument that I have favoured in Stern (2019: 441), although I misrepresented Mann by failing to indicate that Mann did at least raise this argument as a possibility.

manuscripts is in fact in the plural, 'so we say'.[14] Given that his argument depended entirely on these few words, Mann (then in Cincinatti) could have made the effort of checking at least some of the manuscripts. Less than a quarter a century later, Menahem Kasher swept away Mann's argument on the basis of three manuscripts of *Sefer ha-Ibbur* in the New York JTSA library.[15]

Mann's argument was flawed not only on specific textual grounds, which could be put down to sloppiness on his part (although this is perhaps unfair), but also, I would argue, on more general conceptual or methodological grounds. Having established, as he believed, that the Hillel narrative was an interpolation, Mann went on to claim that his discovery cast additional doubt on the attribution of the fixed calendar to Hillel, which scholars in his generation – led by H. Y. Bornstein, whom Mann refers to – has already been questioning for some time. His general assumption appears to have been that if any text was interpolated, it was *ipso facto* inauthentic. Mann did not stop to ask from where the interpolated text about Hillel might have been drawn – as it seems completely unlikely that Abraham b. Ḥayya made it up entirely himself. Even if an interpolation within Hayye's responsum, this text could well have been drawn from a respectable ancient source. But for Mann, the mere fact that this text was interpolated was sufficient to identify it as non-Geonic and therefore to delegitimize it. His approach evinces a certain confusion of textual integrity with historical reliability, or to put it more generally, a blurring of philology and history, which was perhaps not untypical of scholarship in his generation.[16]

Equally problematic, in my view, was Mann's apparent partiality for the Cairo Genizah version. As noted above, the case against Abraham b. Ḥayya's version was not strongly made, and it is far from clear it that the Hillel narrative within it was interpolated. Mann may have been keen to promote his new discovery, but this alone was surely not the reason why he hierarchized the Genizah version over that of Abraham b. Ḥayya and assumed it, seemingly *a priori*, to be textually superior.

Was such a hierarchization justified? Ms Strasbourg 4038 can be dated, palaeographically, to the end of the eleventh or early twelfth centuries[17] (Mann may not have been able to establish this – but he could have considered this dating as a possibility). This makes it almost contemporary to Abraham b. Ḥayya's *Sefer ha-Ibbur* (1123 CE), leaving it the only

[14] See above, and notes 3-4; the printed edition is erroneous.
[15] M. M. Kasher, *Torah Shelemah,* vol.13, New York: American Biblical Encyclopedia Society, 1949, p. 24. Israel Sandman has confirmed that this is the reading of all the manuscripts extant, including the two manuscripts that were used by Filipowski for his edition.
[16] As a comparison, one can point to the early twentieth-century controversy surrounding Benjamin Zevi Auerbach's edition of *Sefer ha-Eshkol* (1868-9). Shalom Albeck convincingly proved, in 1909, that text was not the authentic *Sefer ha-Eshkol* of Abraham ben Isaac of Narbonne, which was subsequently edited by Albeck on the basis of other manuscripts. In the protracted controversy that followed and that has never been satisfactorily resolved, Auerbach was accused by Albeck of forgery. Insufficient attention was given, arguably, to the possibility that Auerbach was not a forger, but had used (with or without some tampering) a genuine medieval manuscript containing a different, unidentifiable medieval work. Without joining in the debate, I would only draw attention to how, in the course of this controversy, scholars often shifted seamlessly from doubts over identity and authorship of a certain text to doubts over its historical authenticity and legitimacy. See most recently, with bibliography, A. Breuer, 'Fragments of a New Manuscript of *Sefer Ha-Eshkol*', 2021 (updated 2022), https://bwb.hypotheses.org/394#a5 (accessed 15/02/2024).
[17] Stern (2019) 447.

advantage of having been scribed in Egypt, closer to Hayye's Babylonia, whereas Abraham b. Ḥayya was writing in distant France. Both were Hebrew translations of the (likely) Aramaic originals, and in this respect – I would argue – both were equally susceptible to an element of intervention and redaction.

Mann seems to have been led, however, by a philological assumption – misplaced, in my view – that manuscripts or 'primary sources' are inherently superior, in textual terms, to secondary quotations. Yet it is well known that Talmudic quotations, for example, in Geonic sources and in the early Rishonim are often more pristine and reliable than the text of the extant Talmudic manuscripts, mostly European and late medieval, that often show signs of textual revision under the influence of Rashi and the Tosafists.[18] After a century of progress in philology and the study of manuscripts, it should be abundantly clear that Mann's Genizah manuscript was not to be privileged over Abraham b. Ḥayya's version. There is no way of knowing, *a priori*, whether the Hillel narrative was interpolated or belonged to the Geonic response, and whether either or both of the versions were authentic or tampered.

The discovery of this fragmentary manuscript of Hayye Gaon's responsum was important for a number of reasons (we shall return to this later); but what Mann sought to infer from it with regard to the Hillel tradition was not particularly impactful. Far more significant was a discovery that Mann made just a few years earlier, that without his realizing it, was critical to research on the origins of fixed rabbinic calendar.

The letter of the exilarch

Proof that the fixed rabbinic calendar was not instituted before the ninth century did not come from the Genizah version of Hayye Gaon's responsum, but from a different document that Mann published three years earlier, in the texts volume of *The Jews in Egypt and in Palestine Under the Fatimid Caliphs*.

This single folio in Cambridge University Library, T-S 8G7.1, contains part of the copy of a letter dated precisely to 835/6 CE. Its author identifies himself as Babylonian and refers to the heads of the *yeshivot* as his colleagues, which enabled Mann to identify him, in all likelihood, as the exilarch. In this letter, which may be addressed beyond Baghdad to all the communities under his jurisdiction, the exilarch calls for obeying the calendar decisions of the Palestinian leaders, more specifically of the head and the members of the *havurah*, who have traditionally always been in charge of these decisions. The letter also explains the basis for their calendar decision.

This letter reveals several important points. The first is that in this period, the Babylonian leadership still deferred to Palestinian authority with regard to the calendar. The second is that calendar dates were calculated in Palestine in advance, apparently at the beginning of each year. How the calculation is carried out is unclear, but there is a rule that the New Year cannot fall on Sunday, Wednesday, and Friday. The time of the first appearance of the new moon crescent, as predicted in advance on the basis of the *molad* ('birth' of moon, i.e. the

[18] This observation is by no means new. The relative merits of manuscripts and secondary quotations are already evaluated, for example, by Mann's contemporary, Avigdor Aptowitzer (*Mavo le-Sefer Rabiah,* Jerusalem: Maqitzei Nirdamim, 1938, pp. 116-17).

astronomical conjunction), is an important part in the decision making. Unlike the later, fixed rabbinic calendar, the system in place was thus a combination of calendar calculation and new moon crescent visibility, which must have entailed some flexibility in the process of decision making. This explains why decisions had to be taken on an annual basis. These decisions were taken in Palestine and disseminated to Babylonia in the form of an annual calendar called עיבורא – perhaps the first attestation of this word in this general sense.

All this should be enough to demonstrate that the present-day calendar calculation was not yet instituted or in force in this period. But even more convincing is the fact that the date it announces for Passover 836 CE, following the Palestinian decisions, differs from what would have been the date in the fixed rabbinic calendar: Passover was to be held on a Tuesday, whereas according to the rabbinic calendar calculation, it would have been two days later on Thursday. This shows decisively not only that the calendar system differed, at the time, from what was later to emerge as the fixed calendar calculation, but also that its dates, in practice, were different.

Although the text of the letter was not published until 1922 in the second volume of *The Jews in Egypt and in Palestine Under the Fatimid Caliphs*, its discovery was announced already in the first volume of 1920, in which Mann summarized and explained the contents of the letter.[19] Mann demonstrates a clear understanding of its calendrical aspects, with only a few exceptions which he readily acknowledges.[20] He commits one blunder, which may be related to a misreading; I shall return to this later. Otherwise, his description of the letter is sound. In spite of not claiming expertise in the field, his well-rounded education in what is now called 'Jewish Studies', and particularly in all aspects rabbinic *halakhah*, shines out remarkably well in this passage.

Mann understood, but understated, the significance of his discovery. He writes at some length, not inappropriately, about the implications of the letter as to the evolving political relations between Babylonia and Palestine in the ninth century. But the most important outcome of this discovery – that the dates of Passover, in 836 CE, differed from those of the later, standard rabbinic calendar – is only mentioned by Mann in a footnote, and not very clearly at all. All he says is that the date calculated by Mahler in his *Handbuch der jüdischen Chronologie* differs from that of the exilarch's letter. An uninformed reader might understand this to mean that Mahler made a mistake, not that the dates of 836 CE differed from the rabbinic calendar.[21]

To a calendar researcher like Hayyim Yehiel Bornstein, in contrast, this discovery was close to sensational. Before Mann had time to publish his edition of the folio (in his second volume, which came out in 1922), Bornstein obtained a copy of it from him through the intermediary

[19] J. Mann, *The Jews in Egypt and in Palestine Under the Fatimid Caliphs*, 2 vols., London: Oxford University Press, 1920-2, in vol. 1 pp.52-3 and vol. 2 pp. 41-2 (no. 13).
[20] He expresses trouble understanding the date, which he knows should be 835/6 CE, yet is given as 4595 from the Creation, apparently one unit short (p. 52, n. 2). He does not know, or probably has forgotten, that in Babylonia the custom was to reckon the Creation years in this manner, as is evident already in the Babylonian Talmud, *Avodah Zarah* 9b.
[21] Ibid. n. 1, referring to E. Mahler, *Handbuch der jüdischen Chronologie* (Leipzig: Fock, 1916), at that time a relatively recent publication.

of his colleague in Warsaw, Samuel Poznanski, before the latter died in December 2021. Bornstein published Mann's edition, with all due acknowledgments, as part of an article in the Warsaw journal *Ha-Tequfah* in the same year as Mann's second volume, 1922.[22] In this article, Bornstein describes this document as 'without a match' (אשר אין ערוך לו), a 'rare pearl'. Indeed, it provided for the first time incontrovertible evidence that the fixed rabbinic calendar, as now known, was not instituted by Hillel, but actually considerably later. Jacob Mann, out of ignorance or modesty (or both), never claimed any fame for this discovery.

To this date, the exilarch's letter remains one of the most significant pieces of evidence in the history of the rabbinic calendar. Attempts to refute it have never succeeded. Menahem Kasher devoted a whole volume of his series *Torah Shelemah* to refuting Bornstein's theories and defending the tradition that Hillel instituted the fixed calendar. In his discussion of the exilarch's letter, towards the end of the book, Kasher quibbles with a detail of Bornstein's analysis (the exilarch's time of the *molad*), but he evades completely the simple fact that the date of Passover 836 does not agree with the fixed rabbinic calendar.[23]

In 1987, Tzvi Langermann published a Hebrew translation of Muḥammad ibn Mūsā al-Khwārizmī's treatise on the Jewish calendar, dated 823/4 CE, which implied to him that the fixed rabbinic calendar was already in place.[24] He sensed this contradicted the evidence of the exilarch's letter, and wrote (my translation):

> We admit that this is a severe difficulty. In a first draft of this article, we suggested a completely different explanation of the exilarch's question, according to which this problem would be eliminated. However … we have decided to leave a full treatment of this document until we are able to study properly the original, because the image (microfilm) that is in Jerusalem is not sufficiently clear. (p. 162)

Back in the 1980s, long before the launch of the Friedberg Genizah Project website, it was still possible to cast doubt on an earlier text edition – in this case, Jacob Mann's – on the basis of one's having limited access to the originals. To dispel the doubt that had been cast, I published a few years later a new edition of the exilarch's letter together with high-resolution photographs of the original folio, which largely confirmed Mann's reading and interpretation, and the document's considerable historical implications.[25]

Jacob Mann as a text editor

My main motivation for re-editing the exilarch's letter in 2001 was that I found Mann's edition to be marred with numerous small errors. In what follows I propose to go over a selection of these errors (from the recto only), not for the purpose of criticizing Mann's work, but rather for gaining an understanding of how he worked and why these mistakes

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[22] Bornstein (1922) pp. 346-9. It is unclear which of the two, Bornstein's article or Mann's second volume, was published first. *Ha-Tequfah* 14-15 is dated to the first half of 1922; Mann's preface is dated February 1922; neither are helpful indications as to when they were actually published. At the time of writing, Bornstein was unsure whether or not Mann's book had already come out (p. 346, n. 1). It should be noted that Bornstein's tone is constructive and well-meaning; there is no sense of any rivalry or competition between them.

[23] Kasher (1949) 170.

[24] T. Langermann, *'Ematay nosad ha-luaḥ ha-ivri?'*, *Asufot* 1 (1987) 159-168. This article, I hasten to add, was in its own right an equally important contribution to the history of the rabbinic calendar.

[25] Stern (2001) 277-83. On the apparent contradiction that Langermann considered problematic, see ibid. 180-1 and 184-6; and I plan to add further to this discussion in the near future.

were made. The general impression is that when it came editing the texts he discovered (at least, those that I am familiar with), his work became somewhat sloppy, rushed, and slack with regard to detail.

My first remark is that Mann provides the wrong shelf mark: T-S 8J7.1 instead of T-S 8G7.1. This small error was carried through in nearly all citations through the rest of the century.[26] The fact that it remained uncorrected reveals the extent to which Mann's edition was relied on: the original was seldom called up in Cambridge University Library, and as consequence, the error was never picked up.

Some of the errors in his edition of T-S 8G7.1 could be blamed on the typesetters: for example, on (recto) line 7, אף עלגב instead of אף על גב (as clearly in the manuscript) is highly unlikely to have been Mann's error. Some casual errors, however, appear to be his: thus the omission of the final *nun* of מדברין in line 2, or the transcription פורים כ]יום[on line 14 instead of פור כ]יום[(or possibly: כיפור ד]יומא[). None of these errors affect the meaning, but they evidence the sloppy result of someone working too fast.

Mann also has a tendency to focus on the text and ignore the scribal features of the manuscript. Thus the letters לש (the latter, broken) at the end of line 4, that introduce לשטרות at the beginning of the next line, are left out of the edition. Also omitted, in the middle of line 15, are the letters וא with an erasure mark above them, ostensibly in the same hand; although this is clearly an error that the scribe himself corrected, a record of it in Mann's edition would not have been superfluous. These omissions could be put down, again, to carelessness or excessive haste, but they also betray a fundamental lack of interest in paratextual features of manuscripts, and more generally in manuscripts as material objects. The primacy of abstract, disembodied texts over the material evidence of manuscripts is not untypical of early twentieth-century philology.[27] For the same reason, Mann does not reflect on when this manuscript might have been scribed, or why this ephemeral letter about an obsolete calendar year was preserved and copied much later on in a quire or codex. To the modern scholar these are fundamental historical and philological questions, but they were only raised in later generations after Mann.

In line 10, Mann is guilty not of error or omission, but of excessive editorial caution. The word שעות (hours) at the end of the line is given by Mann as ות[שע]; the first two letters are indeed partially missing, but they are nevertheless quite legible (certainly the *ayin*). Mann's caution gave subsequent scholars the licence to conjecture alternative readings, on the misunderstanding that the first two letters were missing altogether in the manuscripts: Zvi Hirsch Jaffe thus suggested ידות (fractions), דנקות (sixths), or שתות (sixth).[28] This word was particularly significant for calendar researchers, as it is part of the time that is given for the *molad* (new moon) of Nisan 836 CE (quoted below). Excessive caution, in this case, was not helpful but actually misleading.

[26] E.g. Langermann (1987) p. 162 n.16.

[27] I have made similar reflections about Bornstein's approach to Genizah texts: Stern (2019) pp. 35-6, 105, 254-5.

[28] Z. H. Jaffe, *Qorot Heshbon ha-Ibbur*, Jerusalem: Darom, 1931.

The biggest howler in this text edition occurs in the same passage at the end of line 9, where Mann reads מיתלי instead of מיתליד. How he overlooked the last *dalet* is puzzling, as this letter is especially elongated by the scribe to make it reach the end of the line (which happens to terminate here by a large hole in the parchment). Sometimes it is the biggest, most obvious things that do not get seen. Again, one cannot help thinking that a minimal amount of checking or cross-checking would have rectified this error; this was probably the result of working too fast.

In this case, the error was significant, as it effected the meaning of a critical passage of the letter, in which the *molad* of Nisan (836 CE) is provided. Lines 9-10 read as follows:

משום סיהרא דניסן דקא מיתליד
ביממא דתלתה בש[ב]ה בארבע שעות

since the moon of Nisan is to be born
in the daytime of the third (day) of the week (Tuesday) at four hours

The exilarch is explaining that the reason why the first of Nisan (and hence Passover, two weeks later) needs to be set on Tuesday is that the new moon crescent will be visible in the evening; since Passover can never fall on a Wednesday, and Thursday would be too late after the appearance of the new moon crescent, the date must be brought back to Tuesday.

Knowledge that the new moon will first be visible on Tuesday evening comes from the calculation of its *molad,* i.e. the time when the moon will be 'born', a reference to the astronomical conjunction (when sun and moon are at the same longitude, and the moon disk is completely invisible). The *molad* is calculated in this passage as occurring on Tuesday at four hours of the day, i.e. about 10 a.m. This agrees approximately with the *molad* as calculated in the later, fixed rabbinic calendar. But in 835/6 CE, it seems to have been used only to predict when the crescent would first become visible. It is probably following the Talmudic tradition that if the moon is born before noon, it will first be visible in the evening (Babylonian Talmud, *Rosh ha-Shanah* 20b).[29]

The term used in this passage is מיתליד, literally 'is born', from the same verbal root as *molad*Mann, however, misread it as [30]ת ״יוביוד במצפראטקרט

He writes that in that year, 'the new moon of Nisan would be
visible on Tuesday, 10 a.m.'. As is basic knowledge to anyone in the field, the new moon is only visible in the early evening; visibility at 10 a.m. is astronomically impossible.

[29] As it happens, astronomical criteria firmly establish that the new moon crescent of Nisan 836 CE would not have been visible until Wednesday evening; but Tuesday evening is the prediction that the exilarch assumes was made in Palestine, perhaps solely on the basis of this Talmudic tradition.

[30] This form is attested in Babylonian Talmud, *Gittin* 73a as מיתיליד in some versions, and in T-S F6.6 (an Italian manuscript) in the shorter spelling