

Time, Astronomy, and Calendars in the Jewish Tradition

Edited by

Sacha Stern and Charles Burnett



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CHAPTER FIVE

THE JEWISH CALENDAR CONTROVERSY OF 921–22: RECONSTRUCTING THE MANUSCRIPTS AND THEIR TRANSMISSION HISTORY¹

Marina Rustow and Sacha Stern

In the late summer of 921 CE, the son of the Palestinian ga'on Me'ir ascended the Mount of Olives during the pilgrimage festival of Sukkot and announced the calendar dates for 921/22 through 923/24. According to his pronouncement, Passover in 922 would fall on a Sunday. But the ge'onim in Iraq had calculated the calendar differently, and according to them, Passover would fall the following Tuesday.

The disagreement between them centered on the beginning of the month of Tishri in 923, but had a ripple effect beginning with Passover in 922. Both the Iraqi and Palestinian rabbinic calendars, over the course of the ninth century, had adopted the rule of *molad zaqen*, according to which, if the *molad* (lunar conjunction) of Tishri occurred after a certain hour, the beginning of the month (and thus the New Year) had to be postponed to the next day.² The Iraqis set the limit at midday precisely (at '7 hours' of the 12-hour daytime period), while the Palestinians set it at 641 parts of the hour later. This small difference usually did not affect the calendar, but because the *molad* of Tishri 923 occurred on a Saturday at

¹ The research presented in this article was supported by a Small Research Grant of the British Academy. We are grateful to Ronny Vollandt for agreeing to serve our research associate on the project for three months; Judith Olszowy-Schlanger for her paleographic expertise; Ben Outhwaite at the Cambridge University Library Genizah Unit for his unstinting help and enthusiasm for the project; Helen Weller, at Westminster College, Cambridge; David Kraemer, Sarah Diamant, Warren Klein, and Amy Gerbracht Armstrong at the Library of the Jewish Theological Seminary of America; Arthur Kiron and Bruce Nielsen at the Center for Advanced Judaic Studies Library at the University of Pennsylvania; Susan Vick, at the Goldsmith Museum, Baltimore; César Merchan-Hamann at the Bodleian Library and the Oxford Centre for Hebrew and Jewish Studies at the University of Oxford; and Roni Shweka, of the Friedberg Genizah Project. The images in this article marked Bodl. are courtesy of the Bodleian Libraries, University of Oxford. Images marked ENA are courtesy of the Library of the Jewish Theological Seminary of America, and from the Elkan Nathan Adler Collection. Images marked T-S are from the Cambridge University Library, Taylor-Schechter Collection, and are courtesy of the Syndics of Cambridge University Library.

² S. Stern, *Calendar and Community: A History of the Jewish Calendar, 2nd Century B.C.E. to 10th Century C.E.* (Oxford: Oxford University Press, 2001), pp. 192, 195–6.

237 parts after midday, for the Iraqis, the New Year had to be postponed, whereas for the Palestinians it was to remain on the same day.

The historical origin of the seeming arbitrary Palestinian limit of '7 hours 641 parts' is unknown, although it can be given a plausible explanation.³ By the 920s, however, the Palestinians themselves had entirely forgotten its rationale, and in the summer of 921, the ga'on, Me'ir, or his son, and the Palestinian faction defended their calendar decision on the basis not of the inherent truth of their calendar calculation, but of two related traditions: the early rabbinic injunction that the Jewish year be intercalated only from the land of Israel; and the Palestinian ga'on's allegedly ancient prerogative of declaring the calendar. The Iraqis, likewise, retorted not with an argument of computational superiority, but with the claim that their computation—an algorithm known as the 'four gates'—was an ancient tradition that could be traced back to the Babylonian Talmud. To this the Palestinians responded, at some point in the winter of 921/2, with an adaptation of the Iraqi 'four gates' that fit their own rules, a solution that might have satisfied both sides. But the argument instead quickly transcended technical questions of calendar calculation and became a dispute over who possessed the authority to determine a calendar that was to be observed uniformly by all Jews, everywhere.

For an entire year and possibly longer, Jewish communities in Egypt, Syria, and Iraq were riven in twain as some followed the Palestinian calendar and others the Iraqi.⁴ The Iraqi calendar and its 'four gates' are known from works on the Jewish calendar from the early twelfth century onwards⁵ to have become the dominant rabbinic calendar calculation in all parts of the Jewish world, and it corresponds to how the Jewish calendar is universally reckoned today. This, together with the fact that the Palestinian calendar, by the twelfth century, appears to have faded completely from memory, are some of the reasons nearly all twentieth century scholarship considered the Sa'adya–Ben-Me'ir controversy to have been the very last instance of calendrical differences among Jewish communities, the conflict that fixed the rabbinic calendar and settled the issue once and for all.

³ Stern, *ibid.* pp. 264–75.

⁴ One of the protagonists on the Iraqi side was Sa'adya ben Yosef al-Dilāṣī al-Fayyūmī, only later ga'on of the yeshivah of Sura in Baghdad (928–42, with interruptions). His centrality in the controversy has, until now, been assumed; but his total absence from the *Book of the Calendar Controversy*, one of the main sources for our understanding of these events, has convinced us that this presumed centrality must be rethought.

⁵ See, for example, the articles by Ilana Wartenberg and Israel Sandman in this volume.

New evidence has made it possible to refute this view decisively. In 2007, Sacha Stern and Piergabriele Mancuso discovered a divergent rabbinic calendar datable twenty-five years after the Calendar Controversy. This calendar was implied in the earliest extant manuscript of Shabbetai Donnolo's *Sefer Hakhmoni*, in an astronomical table that must have formed part of the original work, since Donnolo's text describes it. The table provides astronomical ephemerides for the month of 'Elul' in 946, whereas according to the Iraqi calendar, which had supposedly been imposed on the entire Jewish world twenty-five years earlier, this month should have been Tishri. The Donnolo manuscript demonstrates that a full generation after the Calendar Controversy, the Jews of southern Italy still maintained a different calendar from the Iraqi one.⁶

Was Donnolo's independence from the supposedly hegemonic Iraqi calendar due to his geographic distance from Iraq and the time it might have taken for news to spread as far as Italy? This is unlikely: communications across the expanse of the Islamic and Byzantine worlds, from Khurasān in the east to al-Andalus in the west, were frequent and relatively efficient by the tenth century. The Donnolo calendar thus raises the question of who really knew about the Calendar Controversy of 921–22, and who might have cared about it.

Other evidence raises this question as well. The Cairo Geniza preserved even later evidence of calendrical independence, this time from Palestine itself. In 1094, Evyatar ha-Kohen b. Eliyyahu, ga'on of the Palestinian yeshiva in Tyre (1083–1112, with an interruption in 1093–94), wrote a polemical treatise modeled on the biblical book of Esther in which he celebrated his triumph over an Iraqi rival for his office, a scion of the exilarchal line named David b. Daniel. Evyatar defended his claim to the gaonic chair on the reasoning that his Iraqi rival was disqualified from the Palestinian gaonate because only the Palestinian ge'onim possessed the esoteric secrets of the proper method of calendation, the *sod ha-ibbur*, which they had received in an unbroken chain of transmission stretching back to the fourth day of creation.⁷ Even if Evyatar's argument

⁶ Oxford, Bodleian Library (hereafter Bodl.), MS Heb. e 26.3 (recto); Sacha Stern and Piergabriele Mancuso, 'An Astronomical Table by Shabbetai Donnolo and the Jewish Calendar in Tenth-century Italy', *Aleph* 7 (2007), 13–41.

⁷ Evyatar ha-Kohen b. Eliyyahu, *Megillat Evyatar* (Cambridge University Library, Taylor-Schechter Collection [hereafter T-S] 10 K 7.1 and T-S 12.729). See Solomon Schechter, *Saadyana: Geniza fragments of writings of R. Saadya Gaon and others* (Cambridge, 1903), pp. 83–106; Moshe Gil, 'The Scroll of Evyatar as a Source for the History of the Struggles of the Yeshiva of Jerusalem during the Second Half of the Eleventh Century: A New Reading

seems to us far-fetched, in order for him to have made it at all, it had to have seemed reasonable for a Palestinian ga'on to exercise independent calendrical authority one hundred eighty years after the Calendar Controversy of 921/22.

The conflict in 1094 may have been purely political; there is no evidence that Evyatar's calendar differed from the Iraqis'. But if Evyatar could claim the *sod ha-ibbur* to have been a Palestinian secret, the Iraqis may not have succeeded in imposing their calendar calculations on Jews everywhere in 921/22, or at least not permanently.

Like the Donnolo table, Evyatar's polemic raises the question of when and to what extent the Calendar Controversy of 921/22 had become known to Jews outside the communities directly involved in it, that is, outside Iraq, Palestine, and Egypt. Armed with those questions, in 2011, we decided to reopen the case of the Calendar Controversy and find out how—and how far—news of it had spread during the Middle Ages.

The main sources for the controversy are copies of the polemical letters the two sides exchanged and copies of an Iraqi treatise about the controversy, all preserved in the Cairo Geniza. Though all these texts date from the period of the controversy itself, the copies the Geniza preserved were written later. In that chronological discrepancy, we saw not an impediment to knowledge but an opportunity: it would permit us to study the manuscripts as artifacts of their own time period, examining them from the point of view of paleography and codicology in order to determine how, when and where they were copied and their texts transmitted. We also wanted to discover the reason for scribes' interest in these texts, or whether there were significant variants among the manuscript witnesses that might hint at the frequency with which the texts had been copied before. What follows is an interim report on our project.

The texts—or most of them—had already been published between 1891 and 1915, in the first flush of Geniza research. Since then, scholarship on the controversy has referred almost exclusively to printed editions of the texts, especially Ḥayyim Yehi'el Bornstein's 'edition' of 1904.⁸ 'Edition' is a misnomer, because Bornstein worked not from the manuscripts them-

of the Scroll', in *Jerusalem in the Middle Ages: Selected Papers*, ed. B.Z. Kedar (Jerusalem: Yad Izhak Ben-Zvi, 1979), pp. 81–106; and Marina Rustow, *Heresy and the Politics of Community: The Jews of the Fatimid Caliphate* (Ithaca: Cornell University Press, 2008), pp. 337–39. The passage is on T-S 10K7.1, fol. 2, lines 26–28.

⁸ H.Y. Bornstein, *Mahloqet Rav Sa'adya Gaon u-Ven Meir* [*The Dispute between Saadya Gaon and Ben Meir*], in *N. Sokolow Jubilee Volume* (Warsaw: Shuldberg, 1904), pp. 19–189; also published as a separate pamphlet.

selves but from the previous editions of other scholars. There were some additional finds in the decade or so following Bornstein; but for the rest, for nearly a century, the manuscripts have been either studied piecemeal or ignored completely.⁹

Our first task was to find and study all the manuscripts. But the late-nineteenth- and early-twentieth-century publications rarely included shelf-marks, so it took us nearly one year to find the manuscripts, which are now scattered in all libraries around the world. We could not have done so without the help of Dr Ronny Vollandt, who served as our research associate for the project. Although we still cannot claim to have identified every fragment from the Geniza relevant to the controversy, and there are two published fragments for which we are still searching,¹⁰ we nonetheless now have more manuscript fragments than had hitherto been known to be connected to the controversy—already a net gain.

Our manuscript-hunting was made more complex by the fact that we initially spread our nets wide, looking not only at the main texts of the controversy, but also at texts to do more broadly with the rabbinic calendar, Jewish communal polemics, and polemics by Sa'adya b. Yosef al-Fayyūmī, who was involved in the controversy. At first, we thought this extra work might be in vain, since much of what we found could prove irrelevant to our project. But the method quickly yielded dividends. We came to realize that some texts that were only tangentially related had in fact formed part of the same quires with our texts, since medieval scribes tended to copy related works into the same manuscript. Codicological projects must think as medieval scribes thought and not remain focused narrowly on certain texts.

All in all, with Vollandt's considerable help, we identified eighty-one fragments. Our next step was to try to group them into distinct manuscripts. This was challenging, but again yielded gains we had not anticipated. Previous scholars had already made some joins, so we were not starting from scratch.

⁹ The first publication was by Alexander Harkavy, in 1891 (see next note); the last major publication was by Alfred Guillaume, 'Further documents on the Ben-Meir controversy', *Jewish Quarterly Review*, n.s. 5 (1914–15), 543–57, but a few additional documents were published subsequently, one much later by Ezra Fleischer, 'Literary Documents Concerning the History of the Gaonate in Erez-Israel' (Hebrew), *Zion* 49 (1984), 375–400.

¹⁰ The first was published in Alexander Harkavy, *Zikhron la-rishonim ve-gam la-aharonim*, vol. 5 [*Zikhron la-ga'on Rav Sa'adya*] (St. Petersburg, 1891), pp. 218–20. The second was published in Israel Lévi, 'Nouveaux fragments relatifs à Ben Méir', *Revue des Etudes Juives* 41 (1901), 229–232.

Our first advance was a story of complete serendipity. In 1924 B.M. Lewin published a copy of a letter that Sa'adya had sent to Fustat in the 920s; Lewin said the manuscript came 'from the Geniza collection of my friend the chief rabbi Israel Lévi in Paris,' but after his initial publication, it had never been seen again.¹¹ Lewin had furthermore connected this letter with another letter of Sa'adya, or possibly a continuation of the same one, contained in a folio from the Bodleian.¹² Then, in 1965, Sheraga Abramson realized that one of the missing sections between them was to be found in a third folio at Westminster College, Cambridge, written in the same hand as the Bodleian manuscript.¹³ Finally, in 1997, Moshe Gil published all three as fragments of a single manuscript; but since the Israel Lévi manuscript was still missing, he did not see the original of the intermediate folio and published it based on Lewin's edition.¹⁴

Then one day in 2003, Ben Outhwaite at the Cambridge Genizah Unit received an email from a synagogue in Baltimore, Maryland, saying that they had a Genizah fragment and wanted help identifying it.¹⁵ Outhwaite looked at the photo and realized that the lost Lévi manuscript was no longer lost. No one knows how the manuscript reached the synagogue in Baltimore; it is possible that Israel Lévi had given it to Lewin to publish, that Lewin had kept it, and that Lewin's descendants had then donated it to the synagogue, but this is sheer speculation, and the synagogue's archives have so far not yielded any information.¹⁶ So, through little effort of our own, our edition of this letter will be the first based on all the originals.

The second join we made based on one found in the existing literature began with a publication by Alfred Guillaume in 1915 connecting the torn upper half of a quire in Cambridge (first published by Solomon Schechter) with its torn lower half in Oxford.¹⁷ Guillaume's join was remarkable given the logistical difficulties involved in early twentieth-century travel, the advent of the Great War, and the expense of photography; like other scholars of the period, he could not easily compare manuscripts, so

¹¹ B.M. Lewin, 'Kitāb rosh ha-yeshivah al-fayyūmī, z(ikhrōn) l(ivrakha)', *Ginze Qedem* 2 (1924), 33–35.

¹² Bodl. MS Heb. c 13.22.

¹³ Cambridge, Westminster College, Biblica 6.52.

¹⁴ Most recent publication in Moshe Gil, *In the Kingdom of Ishmael* (Hebrew), 4 vols. (Tel Aviv: Tel Aviv University, 1997), vol. 2, doc. 8; see there for previous editions.

¹⁵ Chizuk Amuno Congregation, Baltimore, Maryland, Goldsmith Mus. ACC FIC 2005.7.

¹⁶ Haggai Ben-Shammai, 'Gilgulav shel qeṭa' genizah', *Ginzei Qedem* n.s. 7 (2011), 227–30.

¹⁷ T-S 13K2.1–6, first published in S. Schechter, 'Genizah Specimens. Saadyana I', *Jewish Quarterly Review*, o.s. 14 (1901), 37–63, Texts IV, V, Va (53–59); Bodl. MS Heb. d 74.27–31, in Guillaume, 'Further Documents' (above, n. 10).

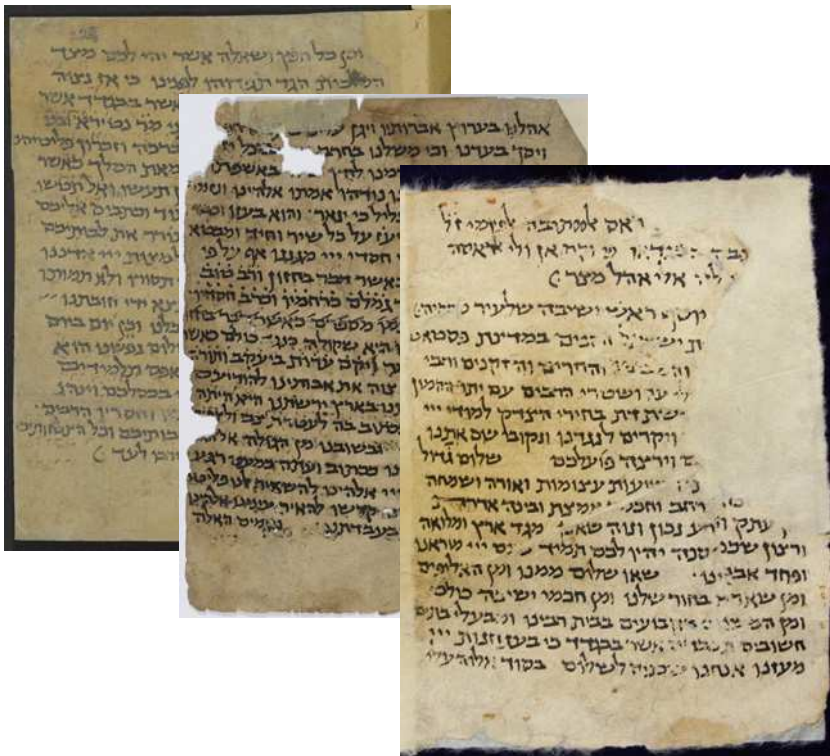


Figure 1: Copy from ca. 1028 of a letter of Sa'adya to Fustat, including (first folio) the formerly lost manuscript from the collection of Israel Lévi, now in Baltimore. (*Baltimore, Goldsmith Mus. ACC FIC 2005.7 + Cambridge, West. Coll. Biblica 6.52 + Bodl. MS Heb. c 13.22.*)

making joins depended on the texts being perfectly continuous (which was the case here), or one made drawings or had excellent visual recall. All in all, Guillaume pieced together two complete and two incomplete folios out of six total in the quire.

Unlike Guillaume, we had the benefit of the Friedberg Genizah Project website, digital photographs, electronic communications and Adobe Photoshop. These enabled us to be in four or five libraries simultaneously, at least virtually; to have speedy access to images of the manuscripts in our corpus; and to see what the quire had looked like before being torn apart. We were even able to improve on Guillaume's join. Vollandt had found the shelf-mark of an unpublished fragment catalogued in the Friedberg Project database as one of Sa'adya's calendrical works. We duly ordered it from Cambridge, and the instant the photograph arrived in our inboxes,

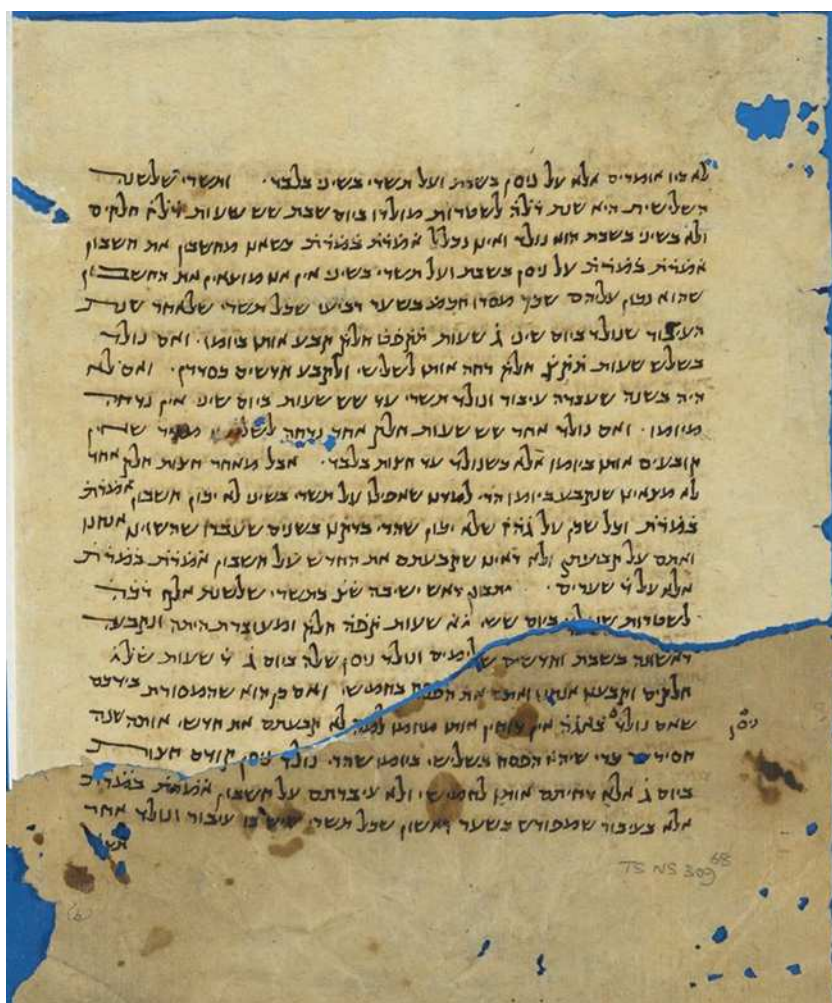


Figure 2: T-S 13K2.2v + T-S 309.68Bv

we knew it was the lower half of the second page in the quire.¹⁸ There is still half a page of the quire missing, but since it comes after the end of the text, that missing fragment is likely to be a mere blank scrap of paper.¹⁹

These two joins left us with sixty-nine more folios to piece together. Once we started fitting the puzzle together, the work proceeded remarkably quickly: during a period of about three months in the spring of 2012,

¹⁸ T-S 309.68b + T-S 13K2.2.

¹⁹ The bottom half of T-S 13K2.6.

we made several more joins. We did so because we used some methods that might seem counterintuitive at first: we deliberately avoided using the texts as our main evidence and, instead, began with a physical census of each folio entered into a database program together with its material (parchment or paper) and measurements. This allowed us to match up some of the manuscripts by size, taking into account variations of up to one centimeter due to decay, shrinkage, and imprecise measurement. Then we counted the numbers of lines per page. (Here, too, there was some imprecision, since scribes did not always have exactly the same numbers of lines on each page.) Last, we looked at handwriting, keeping in mind that one cannot always demonstrate beyond the shadow of a doubt whether two different texts are in the same hand, and that a single text can be written by the same scribe in two different scripts (this describes three bifolios of a single text in Philadelphia and Oxford). In other words, palaeography is not always a reliable guide to codicology. Last of all, we looked at the text, since many of the manuscripts we pieced together contained texts that were only loosely related. In all, and including previous scholars' joins, we were able to reconstruct seven manuscripts in total, and an eighth containing *Sefer ha-galui*.

Of these eight, let us offer two examples in addition to the two we have already offered.

One of the manuscripts we have reconstructed contains the copy of an early letter of Ben Me'ir. The manuscript comprises two parchment bifolios (one at the Bodleian, one now at the Jewish Theological Seminary of America), plus several single folios (also at the Bodleian); it probably originally consisted altogether of four bifolios from the same quire.²⁰ In 1891, Alexander Harkavy, working in St. Petersburg, first published the Bodleian part of the manuscript based on an inaccurate transcription sent to him by Adolf Neubauer, working in Oxford. Given the inaccuracies in Neubauer's transcription, one might assume that he may not have intended it for publication, but presumably logistical challenges prevented Harkavy from accessing the original. He thus published a manuscript he had never seen. He also knew that the folios he was publishing formed an incomplete quire with some text missing in the middle, but he could not have known at the time where the missing piece might be found.

It turned out that the missing bifolio was either in Elkan Nathan Adler's private collection of Genizah manuscripts or else still in the Genizah

²⁰ Bodl. MS Heb. f 26.1–6 + New York, Jewish Theological Seminary of America, Elkan Nathan Adler Collection (hereafter ENA) 2556.2.



Figure 3: *Oxford, Bodl. Heb. MS Heb. F.26.3b-4a, New York, JTS, ENA 2556.2 recto—after treatment*

chamber itself, waiting to be acquired by Adler (who made two separate trips to Cairo, in 1888 and 1895). In 1914, Adler himself published it; but he did not mention Harkavy's publication, either because he did not know of it or did not see the connection between them.²¹ Meanwhile, Ḥayyim Yehi'el Bornstein did see the connection, based only on the published editions, and in 1922 partially republished the texts, but without ever having seen either Adler's manuscript or the Bodleian quire. Although Bornstein knew the texts were connected, he had no codicological evidence that they had come from the same manuscript; in general, Bornstein did not involve himself in the question of manuscripts at all.²²

We went back to study all four bifolios, but quickly realized we faced a new problem: the parchment of the Adler bifolio (now in New York) was so puckered on one side as to be illegible, and it had been photographed that way for the Friedberg database. That was how it remained until we asked the librarian of the Jewish Theological Seminary, David Kraemer,

²¹ E.N. Adler, 'Nouveaux documents sur la dispute entre ben Mëir et Saadia', *Revue des Etudes Juives* 68 (1914), 50–52.

²² H.Y. Bornstein, *Divrei yemei ha-'ibbur ha-aḥaronim*, part 2, in *ha-Tequfa* 16 (1922), 228–92 (on 264–65).

whether his conservators could treat the manuscript and smooth it out. Not only did they agree to this; they did such a fine job that we now had a pristine bifolio in which every letter was clearly visible.

For each of the eight manuscripts that we reconstructed, there was some similarly complicated story to do with the manuscripts, followed by our realization that those who had published the texts either had never seen them or had little concern for the information they might yield beyond the words on the page. But the codicological and paleographic information turns out to be essential not just to understanding the transmission of the texts, but the texts themselves. The following, final example demonstrates this clearly.

One manuscript contains several letters of Sa'adya to his followers in Fustat, in Judeo-Arabic and Hebrew. It comprises one bifolio at the Bodleian, a second in Philadelphia, and a third and fourth in Cambridge.²³ These three shelf-marks had never been joined before. Gil had published each separately, indicating that they were written in the same hand, a conclusion we also reached, but he did not establish the connection between them.²⁴

Not only are they connected: they are continuous. Once we recognized that the bifolios belonged to the same manuscript, we were able to piece together a continuous text, with the Philadelphia bifolio belonging *inside* the Oxford one. The Oxford bifolio had been published as continuous text, but now we realized that it was not continuous at all, and also that it belonged in the opposite order from the one in which previous editors had published it (and in the opposite order from how it is bound at the Bodleian). As for the Philadelphia bifolio, now the center of our quire, we noted that the two sides of the page had not been written in the same script: the left side is in square script, and the right in semi-square script, though the scribe seems to have been the same. We took this as a clue that the text might not be continuous, and once we studied the text, it became clear that, in fact, there had once been another bifolio in the center of the quire, to form a quire of three bifolios total. Apparently the poor, tired scribe, after writing some part of the missing bifolio, took a break and then began again in clear, square script.

²³ Bodl. MS Heb. f 56.82–83; Philadelphia, Center for Advanced Judaic Studies, Halper 332; T-S 6 Ja.1.

²⁴ Gil, *In the Kingdom of Ishmael*, vol. 2, doc. 6.

We now had part of a quire with a missing centerfold—which meant that all previous editions of these fragments were now useless, because they had assumed that each fragment was a self-contained, continuous unit. Furthermore, previous editors had assumed that the Oxford bifolio contained the text of *one* letter to Sa'adya's followers in Fustat, a letter with a considerable number of repetitions. It now became evident that it contained—together with the Philadelphia and the missing inner bifolios—the text of *two* letters in Hebrew, which appear in fact to be two different versions of the same letter (hence a repetitive impression). The discovery of different versions of the same letter has important implications for our understanding of the textual transmission of the controversy's texts in the course of the 10th and early 11th centuries, which we have yet to evaluate in a detailed and comprehensive way.

What about the two bifolios of this manuscript that are now in Cambridge? Their first page begins at the beginning of a letter in Judeo-Arabic, which would be consistent with their being the beginning of a section. The last page contains the beginning of a letter in Hebrew, and is damaged and faded, which at first made it difficult for us to understand how it connected with the Oxford and Philadelphia bifolios. But close analysis of traces of ink from the last page of the Cambridge quire on the first page of the Oxford/Philadelphia quire demonstrated to us that they were once adjacent. Those ink traces also enabled us to reconstruct much of the faded text from the Oxford/Philadelphia quire and to obtain a continuous text for the first of the two Hebrew letters. That left us with a manuscript now consisting of two quires, the first with two bifolios (in Cambridge) and second with three (Oxford/Philadelphia, although the central bifolio is missing). We immediately checked to see whether there could be a missing bifolio in the Cambridge quire as well to make the quires symmetrical, but the text demonstrates that there is not; medieval scribes did sometimes make quires with unequal numbers of pages. That seems to have been the case here.

Our reconstruction of this manuscript has yielded a more accurate text than is currently available. This process was typical of the project: even though our initial aim had been quite modest—simply to study the text's witnesses codicologically and palaeographically and understand its transmission history—we discovered so many problems with the existing editions that we had to jettison them and decide to reedit the texts ourselves. We are currently at work on completing that task.

At the moment, we are planning for our edition to include diplomatic editions of each manuscript and a critical edition of the entire text. We

have chosen to pursue both options precisely because the various manuscripts had served previous editors only as evidence of textual overlap and variation, not as historical evidence in its own right. Those editors followed the classical Lachmannian method, in which manuscript witnesses serve the editor as evidence of an Urtext that in many cases is cleaner and more ‘authentic’ than any of the surviving manuscript witnesses to that text. We think that such a project is worthwhile, but also wish to examine the individual manuscripts as physical artifacts and records of transmission in their own right. We are proceeding, then, both synchronically and diachronically.

What can we say at this point about the text’s transmission over time? What follows are tentative hypotheses that are subject to revision.

First, the dating of our manuscripts. For all the manuscripts but one, we had to rely on palaeographic datings, for which we consulted Judith Olszowy-Schlanger of the Ecole Pratique des Hautes Etudes. Without exception, she dated each manuscript we showed her to the eleventh century, either early or late. One manuscript actually bore a date: the letter mentioned above from Sa’adya to the communities of Fustat, which is divided among collections in Baltimore, Cambridge, and Oxford. The Bodleian segment of this manuscript forms part of a *pinqas* (communal notebook or archive) from the Iraqi Jewish congregation in Fustat, into which were also copied a number of contracts and other business items, one of them dated (on the verso of our folio) **יום אלכמיס כז מן ניסן שלט**, ‘Thursday 27 Nisan 1339’ (of the era of contracts), 25 April 1028 CE.²⁵ It appears, therefore, that this copy of the letters was made in or around 1028 (dating it more precisely depends on a better understanding of the construction and composition of the *pinqas*).

Given the dates, and the absence of any direct statements as to why the letters were copied, we have looked for circumstantial evidence. The first question we asked was when the calendar conditions that had generated the debate in the 920s recurred. They recurred just a few years later, in 926/27–927/28, but there is no record of a controversy then—one of the reasons generations of previous scholars believed that the Iraqis had won the battle. They next recurred two hundred years later, in 1107/08–1108/09 and 1144/45–1145/46, but not at all in the eleventh century. The copy of Sa’adya’s letter mentioned above, made in Fustat in 1028, was therefore copied during a time when the calendrical issues

²⁵ Bodl. MS Heb. c 13.22.

were of no pragmatic concern whatsoever. What else was happening in Fustat that might explain interest in the Calendar Controversy? The 1020s and 1030s were a period of acute internecine battles between the Palestinian and Iraqi Jewish congregations of Egypt and Syria—so acute that on 25 Sivan/9 June in 1029 CE, the ga'on of Jerusalem, Shelomo ben Yehuda (1025–51), traveled to Fustat to excommunicate the entire Iraqi congregation in a special graveside ceremony. The pretext on which he excommunicated the Iraqis was that they had been butchering animals incorrectly; but the vitriol went much further than any single perceived sin: as he told his colleague Efrayim b. Shemarya in a letter, 'Your letter arrived on 24 Sivan, and on the 25th, on that very Monday, we gathered in the cemetery in a large group, and we brought out Torah scrolls and excommunicated every promulgator of sinful statutes and author of vanity and lies and those who foment quarrels between brothers in order to achieve their desires.'²⁶ This is just one example of the infighting between the Palestinian and Iraqi Rabbanite congregations during this period, but there are others. It therefore stands to reason that the Iraqi Rabbanites of Fustat might have seen fit to include in their communal *pinqas* a copy of the Sa'adya-Ben Me'ir correspondence, letters in which Iraqis attempt to show Palestinians who controlled the Jewish calendar not only in Fustat but even in Palestine itself.

This circumstantial hypothesis seems worth pursuing for two additional reasons. First, our codicological work led us to understand that some of the texts were transmitted together with a polemical text by Sa'adya, called *Sefer ha-galui* in Hebrew and *al-Kitāb al-tārid* in Arabic, which he wrote after having been deposed from the gaonate of Sura in 932 by the exilarch David b. Zakkai. In it, he briefly refers to the conflict with Ben Me'ir. Both are polemics that Sa'adya composed against other rabbinic Jews, be they Palestinian Jews or other Iraqis. The Sa'adya–Ben Me'ir texts were, on the other hand, not, as far as we know, copied with Sa'adya's anti-Qaraite writings, despite the differences between the Qaraite and Rabbanite calendars, despite Sa'adya having written a separate polemical treatise against the Qaraite calendar,²⁷ and despite evidence that Sa'adya's

²⁶ T-S 20.102, ed. Gil, *Palestine during the First Muslim Period, 638–1099*, Hebrew (Tel Aviv: Tel Aviv University, 1983), vol. 2, doc. 79. For the wider context of this infighting, see Rustow, *Heresy*, pp. 200–36.

²⁷ T-S 10K2; Schechter, 'Genizah specimens. Saadyana II', *Jewish Quarterly Review*, o.s. 14 (1902), 197–249 (Text IX, 197–203).

anti-Qaraite writings were widely transmitted and well known in this period.²⁸ The work seems to have fit, for those transmitting it, under the rubric of intra-Rabbanite polemic, so it is logical to look for some of the interest in copying it in intra-Rabbanite political tensions.

Second, our hypothesis that the transmission of the Sa'adya-Ben Me'ir texts played a role in polemics between Iraqis and Palestinians may find support in the provisional observation—which we still need to confirm through further research—that some manuscripts appear to have contained letters from Sa'adya and his party only, and others letters only from Ben Me'ir. Thus, one manuscript consists of at least three letters from Sa'adya to his disciples in Egypt; but others are copies of the 'first letter' of Ben Me'ir.

Finally, it seems that Evyatar Ga'on in 1094 must have known about the Sa'adya-Ben Me'ir letters, although he does not quote from them. Indeed, he makes arguments precisely congruent with Ben Me'ir's claims on behalf of the Palestinian ga'on's prerogative to declare the calendar and the obligation to intercalate the year only from the land of Israel. After all, Evyatar's main purpose in writing his *megillah* was to demonstrate his superiority over his Iraqi rival, much as Ben Me'ir had done one hundred eighty years earlier. It is even possible that at least one of the late-eleventh-century copies we have comes from Evyatar and his camp within the Palestinian yeshivah.

One last point before concluding. The textual evidence we have reconstructed so far points to the possibility that the texts were transmitted widely over the course of the subsequent two centuries, but not at all thereafter. And surely it is significant not just that these manuscripts were copied, but also that at a certain point, they *ceased* to be copied and all memory of the controversy was lost. There are only three external mentions of the controversy: by Sa'adya himself in the 930s, by Sahl b. Maṣliḥ of Palestine two decades later, and by Elijah of Nisibis in the early eleventh century.²⁹ By the twelfth century, all memory of the Great Calendar Controversy had been lost, to be rediscovered by scholars in the late nineteenth century. What does this silence mean? Had the issue of the calendar and of Iraqi authority been settled and so the texts lost their appeal? That is the argument one finds in the older scholarship. But it

²⁸ S. Poznanski, *The Karaite Literary Opponents of Sa'adiyah Gaon*, London: Luzac, 1908.

²⁹ Bornstein, *Maḥloket Rav Sa'adya Gaon u-Ven Meir*, 7. On the latter, see article of François de Blois in this volume.

is also possible that transmitting the controversy ceased to hold interest because the rabbinic politics that had sustained that interest ceased to exist with the Crusader conquest of Palestine and the realignment of rabbinic politics centering on Egypt. Egyptian rabbinic authorities could not have made the same kinds of calendrical arguments as Palestinians such as Evyatar. The complete amnesia regarding the controversy after the twelfth century is an intriguing historical problem for which we only have informed pieces of speculation.

There are still many unanswered questions that we hope to address. For example: what, exactly, is the text of the letters? Is it a number of collections of letters compiled by synagogue officials who had access to the originals, or a book that someone sat down and composed collecting all the stages of the controversy and drawing on epistolary material? Why do three of our manuscripts have letters from one side only—were they copied by interested parties who did not care about the counterarguments? Can we draw up a stemma of the text's transmission to determine how widespread interest in the letters was? And finally, once we have a text based on the direct evidence of all the manuscripts, will there be points of interpretation that we must radically revise? We are convinced that there will be scope for reevaluation, particularly on the question of how and even whether the controversy was eventually settled; but we are determined to keep an open mind until we have a properly edited text.

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