

LETTERS TO THE EDITOR

The Eruvin in Brooklyn

RABBI ADAM MINTZ'S important historical review of the *eruv* in Brooklyn omits mention of the *eruv* established by the Sephardic Rabbinical Council. This *eruv* has since obtained the public endorsement of Rav Ovadia Yosef and other Sephardic *gedolei Torah*, copies of which are now to be found on the Sephardic Erub web site www.erub.org.

However, in 1983 it was not clear if Rav Menashe Klein's *beter* was appropriate only for Ashkenazim, or whether Sephardim too might rely on it. I therefore wrote to Rav Klein asking him for clarification. His response, dated the 4th night of Hanukka 5744 (1983), follows with my free translation. *Inter alia*, it reflects the acrimonious debate surrounding the *eruv* which Rabbi Mintz mentioned.

הנה ימחול נא לעיין בספרי משנה הלכות ח"ח סי' ק"ב דברוקלין מוקף מחיצות עשוי בידי אדם וכה"ג כ"ע מודים ועיין עוד שם סי' קמ"א ועוד בכמה מקומות האמנם לעדה הקדושה של אחינו בני ישראל הספרדים בכאן באמת שאין צריכין לסמוך על פסקי שלי כי לפני כמה שבועות בקרתי פה אצל הרב הגאון עובדיה יוסף שליט"א (מלפנים הרב הראשון לציון) ושהה

פה ונתפגשנו אחר ושוחרנו בד"ת והי' שם נוכח גם אחד מחשובי וגדולי הרבנים דעדה הספרדית בפלעטבוש ושאל אותו אודות העירוב בפלעטבוש מה דעתו ועכ"פ לנשים וקטנים והשיב לו שלא זאת¹ נשים וקטנים מותרים לטלטל על סמך עירוב אלא אפילו הוא בעצמו מותר לטלטל והרב הנ"ל בקש ממנו שיתן לו הדבר בכתב והשיב לו שמפחד מליכנס בדבר מפני החולקין (ואוי לנו שכך עלתה בימינו שגדולי הדור מפחדים מלהגיד דעתם אבל המציאות הוא מציאות) ובעזה"י ראינו ושמע הדברים ד' רבנים חשובים וגדולי התורה שהיו שם וכלנו חיים תהלה לא-ל ואין החי מכחיש את החי וא"כ יש להם לעדה הספרדית הקדושה בפלעטבוש פסק ברור מגדול הפוסקים שלהם וכדאי הוא לסמוך עליו אפילו שלא בשעת הדחק.

Please be good enough to see my *Mishane Halakhot*, vol. 8, *siman* 103. Brooklyn is surrounded by man-made *mehizot*, and in such a situation all agree to the possibility of establishing an *eruv*. But our Sephardic brethren do not have to rely on my *psak*. A few weeks ago I visited *HaRav HaGaon* Ovadia Yosef, *Shalit*^a, the former *Rishon leTzion*, and was able

¹ Unclear in original. Perhaps instead of זאת, read רק, ed.

to exchange *divrei Torah* with him. Present there too was one of the important rabbis of the Flatbush Sephardic community who asked his opinion regarding the Flatbush *eruv*: Could at least women and small children rely on it? He responded that not only could women and children rely on it, but he too could. When the rabbi asked him to put it in writing, he replied that he feared getting involved in the fighting regarding the *eruv*. (Woe to us that we have reached a point where *gedolei haDor* fear to say publicly their position, but such is the case.) With God's help, four important rabbis and *gedolei hora'a* were there and heard this and, thank God, we are all alive, and what was said cannot be contradicted. Therefore, the Holy Sephardic Community in Flatbush has a definitive *psak* from one of its major *poskim*, and it can be relied upon even in non-emergency situations.

Joel B. Wolowelsky
Brooklyn, NY

Dating the Exodus

JUDAH LANDA HAS provided us with an erudite discussion of the

various chronologies of the Exodus (*Hakirah* 14). However, he does not give sufficient weight to the possibility that Yosef's rise to power coincided with Hyksos rule, rather than preceded it. Here the internal evidence of the Torah is conclusive, in my opinion. No fewer than six passages of the Yosef story are best or solely explained by reference to Hyksos rule:

1) "Yosef was taken down to Egypt, and Potiphar, minister of executions, *an Egyptian*, purchased him" (*Bereishit* 39:1). One would hardly need to identify a high official in Egypt as "an Egyptian"—what else would he be?—were it not that, under Hyksos rule, a native-born minister was an anomaly. We choose to translate *sar ha-tabahim* as "minister of executions" rather than chief cook, because the prison system was within his purview (40:3-4, 41:10). Why, then, appoint an Egyptian as chief executioner? So that the hatred of the people be focused on him rather than on his Hyksos overlords. Much the same consideration prompted Polish landowners to appoint Jews as tax collectors.

2) "He gave him Asnat the daughter of Poti Phera, priest of On, as a wife" (41:45). The

Egyptians could not even eat together with the Hebrews “because it was an abomination to the Egyptians” (43:32), so how could they marry them? Rather, Asnat was not an Egyptian but a daughter of the Hyksos ruling class, which had no taboos against foreigners.

3) “Yosef recognized his brothers, but they did not recognize him” (42:8). Were Yosef a foreigner in an otherwise Egyptian court, the brothers would have made a special effort to note just who was this official with singularly Semitic features. As it was, as a minister in a quasi-Semitic Hyksos government his origins attracted no attention.

4) “The news reached Pharaoh’s house that Yosef’s brothers had come, and it was welcomed by Pharaoh and his servants” (45:16). The non-Egyptian rulers welcomed the arrival of more Semites, as reinforcements.

5) “So that you dwell in the land of Goshen, for all shepherds are an abomination to the Egyptians” (46:34). Goshen was “the best part of the country” (47:6), and why would the Egyptians give it to those they abominated? Rather, the Hyksos, themselves shepherds, ruled the country, and they took the best parts for

themselves and their allies.

6) “A new king arose in Egypt who knew not Yosef” (*Shemot* 1:8). A new, Egyptian dynasty arose that threw out the Hyksos. Following standard practice, it blotted out all memory of the previous rulers and administration.

Much of the above, particularly 1) and 6), has already been remarked upon by modern commentators. We will introduce, however, an additional hypothesis: Potifar, Yosef’s master, was an Egyptian, but his *wife was a Hyksos*.² Perhaps, as with Yosef and Asnat, the practice was to give new ministers a wife from the ruling circles—if only to keep watch over them.³

This explains the astonishing latitude Potifar’s wife gave herself in speaking about, and to, her husband. “She called the men of her house (*anshei beita*⁴) and

² This casts her infatuation with Yosef in a new light, both being non-Egyptian.

³ Another possibility is that he married her as a means of gaining access to the ruling circles. In either case, Potifar ignored his own people’s taboos.

⁴ Not to be confused with *anshei ha-bayit*, “men of the house” (servants) in v. 11, and see my *Hibah*

told them, ‘See, he brought us a Hebrew (*ish ivri*) to ridicule us’” (*Bereishit* 39:14). When Potifar returned, “She spoke to him in the same way: ‘The Hebrew slave you brought us came to ridicule [or: have relations with] me.’” It is remarkable for a high official’s wife to express such disdain for her husband, let alone to her servants, and inconceivable that she class herself together with the latter, “he brought *us...*” Rather, “men of her house” means men of her *family*. She called in her Hyksos relatives to complain to them about her Egyptian husband.

This is the sting in her accusation: “He brought us an *ish ivri* to ridicule us.” *Ivri* means one who came from over (*me-eiver*) the Euphrates River, and can refer to any Semite. Potifar, the Egyptian, had made a point of buying a Semitic slave in order to ridicule and denigrate the part-Semitic Hyksos in whose government he served!

Potifar was furious, but not at Yosef. Had he entertained the possibility that his wife was telling the truth, he would have executed Yosef, and certainly not have placed him in the highest-quality prison (39:20) and continued to look after his welfare

(40:4). But as a lone Egyptian in a Hyksos court, his hands were tied. He could not free Yosef without further incurring the wrath of his wife’s family, who were closer to the center of power than he was. Yosef knew this and so did not ask the chief cup-bearer to intercede with Potifar on his behalf, but only with Pharaoh (40:14).

The wider significance of the Hyksos connection is that it reveals the intrinsic fragility of Israel’s foothold in Egypt: the Hyksos were a foreign graft in Egypt destined to be rejected, and with their overthrow, the reaction against Israel was only a matter of time. The rise to power of Yosef under a Hyksos regime contained within it the seeds of Israel’s enslavement.

Rabbi Yehudah Henkin
Jerusalem

I WOULD LIKE to commend you for publishing Judah Landa’s article on the dating of the Exodus. I have also written an article on this topic (*The Date of the Exodus: A Guide to the Orthodox Perplexed*). I use largely the same sources, but come to a different conclusion. I defend the view that Ramesses II (1279–1213) and Merneptah (1213–1203) were the relevant Pharaohs. My article, written in April 2011, can be

found at seforim.blogspot.com.

Mr. Landa and myself are in agreement on two key issues:

1. We both agree that the date that the First Temple was built was approximately 966 BCE.

2. We both agree that the next issue is how Orthodox Jews, in attempting to date the Exodus, understand I Kings 6:1. This verse states clearly that 480 years elapsed from the Exodus to the building of the First Temple. In being willing to look at all the archaeological evidence and concluding that the Exodus occurred around 1600 BCE, Mr. Landa is willing to overlook (or perhaps adopt a difficult interpretation of) this verse. This verse points clearly to an Exodus date of approximately 1446 B.C.E.

Where Mr. Landa and I disagree is as follows. Mr. Landa focuses on the evidence for the destruction of Jericho around 1560 BCE, and suggests that this was the period that the Israelites entered the land of Israel. I argue that the late 13th century BCE was the period of the Exodus and the start of the invasion, since archaeology is now documenting that the late 13th - early 12th century BCE is the period that Israelite settlements begin to appear in the land. (Mr. Landa is aware of this difficulty and at-

tempts solutions to it. See pp. 228-230 of his article.) Moreover, the Philistines appear as a major enemy of Israel during the period of the Judges, appearing in chapters 3, 10 and 11 of the book. But they only arrived in the land of Canaan around the 8th year of Ramesses III (=1177 BCE). Thus, the period of the Judges seems to be the 12th century BCE, not centuries earlier. Finally, Egypt is never mentioned as one of the oppressors against whom Joshua or a leader in the book of Judges fought. This would be very strange for a conquest commencing around 1560 BCE. Egypt exerted strong control over the land of Canaan at this time and in the following centuries until c. 1200 BCE.

Most likely, the relevant Pharaohs are Ramesses II (1279-1213) and Merneptah (1213-1203). Exodus 1:11 tells us that the Israelites built a store city called רעמסס. Since this is an exact match to the name of a Pharaoh, this suggests that the Pharaoh who ordered this work (=the Pharaoh of the Oppression) bore this name. No Pharaoh bore this name until the 13th century BCE. The first to do so was Ramesses I. But he only reigned sixteen months (1295-94). Thereafter, after the reign of Seti I, Ramesses II reigned for over **six decades**. In all probab-

ity, he is the Ramesses that we should be focusing upon. Moreover, archaeology has shown that Ramesses II was responsible for building a vast city called Pi-Ramesse, which would have required vast amounts of laborers and brick.

Exodus 2:23 tells us that the Pharaoh of the Oppression died. If we take this verse literally (compare Exodus Rabbah 1:34), the Pharaoh of the Exodus would be Merneptah, who was the successor to Ramesses II. (But then the Merneptah Stele comes into play and raises issues of its own. I discuss all this in my article. See also the comments of Rabbi J. H. Hertz, *The Pentateuch and Haftorahs*, 2d. ed. 1975, p. 395, Exodus-Additional Notes.)

Just as the lack of evidence for Israelite settlement in Israel prior to the late 13th century BCE is difficult for Mr. Landa, the 1560 BCE destruction date of Jericho is difficult for me. (I rely on the solution mentioned by Mr. Landa on p. 205.) But it is preferable to rely on evidence from many regions in Israel (the evidence that Israelite settlement began in the late 13th and early 12th centuries BCE) than to build a theory based mainly on evidence from one specific location only. On the whole, a 13th century BCE Exodus date presents

fewer difficulties and requires less far-reaching reconstructions than does a 1600 BCE date.

(Aside from my own article at seforim.blogspot.com, I would recommend all readers interested in this topic to the following article available on line: James K. Hoffmeier, "What is the Biblical Date for the Exodus? A Response to Bryant Wood," *Journal of the Evangelical Theological Society* 50/2 June 2007, pp. 225–47.)

Mitchell First
Teaneck, N.J.

Judah Landa Responds:

I wish to thank both Rabbi Yehuda Henkin and Mr. Mitchell First for the time and effort they evidently devoted to the complicated and much-debated chronology of the Exodus. Both present what appear to be sound arguments in favor of alternative scenarios to the one I presented in *Hakirah* (vol. 14), and that differ from each other. R. Henkin's placement of the Yosef story in the Hyksos period (ca. 1650–1550 BCE) indirectly moves the exodus to around 1400 BCE, and First places the exodus in the low 1200s BCE. Both of these scenarios are contradicted by the three independent lines of scientific evidence that place the destruction of Jeri-

cho at about 1560 BCE, and by the other evidence I presented in support of ca. 1600 as the time frame for the exodus. These significant divergences, in turn, affect all of the history of ancient Israel.

Since only one of these dates can be correct, for the exodus in the Torah happens only once (contrary to some scholarly speculation), it is incumbent upon us to probe deeply into the veracity of the presented arguments.

Let us begin with R. Henkin's first point, based on the apparently superfluous phrase *ish mitzri* (an Egyptian man) in Gen 39:1. The question he raises, as to why the Torah finds it necessary to inform us that Yosef was sold to 'an Egyptian man,' when we already know that the event takes place in Egypt, is a good one. R. Henkin's solution is that we need to be informed of this detail because it was an anomaly. The Hyksos foreigners who ruled Egypt at the time appointed a native Egyptian man (to whom Yosef was sold) as minister of executions, so that the hatred of the people would be focused on him, rather than on his Hyksos overlords.

None of this, of course, is in the text and other, at least as plausible, explanations exist.

While the Hebrew *tabahim* does mean 'slaughterers,' many commentators translate the word here as 'butchers'—that is, of animals, not humans (see *Rashi, ad loc.*). Just as Pharaoh had a minister for baking bread and one for preparing drinks (40:2), so he had a minister for preparing meat. Nor is it at all clear that Yosef's master, Potiphar, was a prison warden, as R. Henkin asserts. Verses 39:21–23 refer three times to an anonymous prison warden, deliberately avoiding identifying him, when the Torah could have saved a few words by simply referring to him as 'Potiphar,' who has already been named. Later, Pharaoh puts the ministers of bread and drink, who sinned against him, in the custody (*mishmar*) of their colleague, the minister of butchers, Potiphar, who in turn placed them in the prison where Yosef was imprisoned (40:1–3), a prison that was not necessarily under his direct jurisdiction. Nor is there any basis in the text or historic justification for assuming that, at this time, executions were taking place in Egypt on a grand scale, to justify the title of 'slaughterer.' Ancient Egypt, we know, generally had a court system with an appeals process, with the vizier as the final arbiter of disputes and punishment.

The difficulties with R. Henkin's theory, however, run deeper than all this. The Hyksos did not take over Egypt in one quick step. Egypt's twelfth and thirteenth dynasties, in the decades preceding the Hyksos era, maintained a policy of tolerating, even encouraging, mass immigration from, and trade with, the east (Canaan). Egypt was teeming with foreigners, primarily Canaanites, before the Hyksos takeover. Many of these foreigners, we know, rose to positions of influence in (lower, northern) Egypt at this time. Eventually, as their numbers and influence grew, and the power of the pharaohs waned during the so-called Second Intermediate period, the foreigners took over the northern portion of the country **from within**. In this they probably had some help from their fellow Canaanites back home.

In this context (the 1800s BCE) the Torah needs to inform us that Yosef's master was not a Canaanite 'landsman' whose ethnicity he was familiar with, but a strange and alien native Egyptian, who would be expected to oppress him. And that despite this, "Yosef found favor in his eyes" (39:4) because "God was with Yosef" (39:2). Looking at it this way, the specification of *ish mitzri* is directly connected to

what the Torah says immediately afterward. This is not all that different from the Torah's informing us, also apparently unnecessarily, that God came to Laban 'the Aramean' (Gen 31:24) when we well know by then that Laban was an Aramean (31:20). The point is to emphasize that to protect Yaakov, God would communicate even with the likes of Laban, the Aramean (the deceptive, oppressive idol worshipper that we know he was).

In his next point, R. Henkin argues that in marrying Yosef, Asnat, the daughter of Poti-Phera, the priest of On, could not have been a native Egyptian, since we are told that Egyptians would not even eat with the Hebrews, as it was an abomination to them (Gen 43:32). Asnat must therefore have been, claims R. Henkin, a daughter of the Hyksos ruling class.

This is incorrect on multiple grounds. First, the word *ivri* in the Torah, in this context, cannot refer to 'Hebrews.' The Egyptians would not have adopted a custom not to associate with 'Hebrews' at a time when the Hebrews constituted one small family (Yaakov and his descendants) in a distant land. The Hebrews would not even have been on the 'radar screen,' so to speak, of the Egyptians.

Nor is it correct to say, as R. Henkin later asserts, that *ivri* here refers to the inhabitants of the other side of the Euphrates River, or to all Semites, as R. Henkin's third definition would have it. Rather, *ivri* here is cognate with the widely used term in the ancient Mideast, *hibaru*, a term applied by the urbane, settled and relatively well-to-do folk in reference to the nomadic 'riff-raff' out there struggling to eke out a living, such as the shepherd under-class that Yosef's brothers appeared to belong to. It was a condescending, derogatory appellation, not associated with a particular ethnic group but with an economic class of people.

Second, the priest of On is certainly to be identified with Heliopolis, known to the ancient Egyptians as *Iunu*. This ancient town housed the temple dedicated to the native Egyptian (as opposed to the Hyksos foreigners) sun-god (thus the 'Helio') known as *Ra*. This is reflected in the priest's name Poti-Phera, from the Egyptian *pa-di-pe-ra*, meaning 'gift of the house of (the sun-god) Ra.' While the Hyksos allowed the native Egyptians to maintain their priestly class and religious practices, they would be highly unlikely to honor their newly crowned vizier, Yosef, by giving him a wife associated with

a priesthood they did not revere.

It was the native Egyptian reigning Pharaoh, not a Hyksos ruler, who orchestrated the marriage of Asnat to Yosef, as the Torah informs us in 41:45. This renders mute all speculation as to his or her preferences in this regard. In marrying a woman associated with the elite and influential native Egyptian—not Hyksos—priesthood, Yosef was elevated from his former lowly status as a *hibaru* to a member of the upper class of Egypt. This was precisely what Pharaoh intended. When Yosef's brothers later arrived in Egypt, they appeared as the *hibaru* that they were and did not disguise (bearded shepherds, in contrast to the clean-shaven Egyptians), and the Egyptians of Yosef's household preferred not to associate with these *ivrim* (Gen 43:32).

R. Henkin's next point, that since the brothers did not recognize Yosef it must be concluded that he blended in with the Semitic Hyksos rulers, is not persuasive. For as stated above, Egypt during the twelfth dynasty, preceding the Hyksos era, was teeming with foreigners, many of them Semites, many of them achieving prominence. Yosef could easily have blended in with them. Also, as vizier over Egypt, Yosef's clean-shaven face (see Gen 41:14) was likely

masked, in whole or in part, as was the custom of the highest Egyptian officials in ancient times while performing their official duties. This would make it additionally difficult for the brothers to recognize the once bearded Yosef they saw twenty-two years earlier. (Unfortunately, this reverses the beard/no beard dynamic presented by *Rashi* on verse 42:8 from the Talmud and Midrash.)

R. Henkin's remaining arguments are similarly addressed by the above considerations. The speculation pertaining to the interaction between Potiphar's wife and Yosef, while interesting, is obviously debatable. Alternative interpretations abound.

Many of Mitchell First's points were addressed in my article, as he himself notes. I am, however, animated to make the following observations.

The appearance of the name Ramesses in Ex 1:11 pertaining to the store cities the Israelites built 'for Pharaoh' does not establish that the pharaoh's name at the time these store cities were built was Ramesses, just as the appearance of the name Ramesses in Gen 47:11 in the context of the Yosef story does not establish that the pharaoh in Yosef's time was named Ramesses (something no one supports). The only thing these

names establish is that the land (in the case of Gen 47:11) and the city (in the case of Ex 1:11) became known, at some point, by the name of Ramesses.

Consider the Torah's words in Ex 1:11. "And it (Israel) built store cities for Pharaoh, *et* Pithom *vi-et* Ramesses." This may mean that they built store cities **at** Pithom **and** at Ramesses. The Hebrew *et* is notoriously challenging to translate, as it is often not apparent what meaning it imparts to the text. It is clear, however, that it sometimes means 'at,' as it does, for example, in Gen 33:18. Now, the Hyksos capital at Avaris, known at the time as Hat-Waret, was located in the same place where the city Pi-Ramesse ('house of Ramesses') was later established during the reign of Pharaoh Ramesses. So the Israelites built Hat-Waret 'for (the Hyksos) Pharaoh' **at** (what later came to be known as) Ramesses (Pi-Ramesse).

The argument based upon the Philistines and the Book of Judges (*Shoftim*) is flawed on two grounds. One, the era of the Judges spans about six hundred years, from after Joshua to King Saul, and the Philistines appear only toward the end of that time span (despite all the attention paid to them in the book). Two, Jephthah's message with its

'three hundred years' comment (Judges 11:26) makes no sense if, as First contends, Joshua was active ca. 1200 and King Solomon built the temple at about 970 BCE, as discussed at length in section VI-d of my article.

Contrary to First's assertion, the archaeological data I presented in favor of ca. 1600 BCE as the date of the exodus were not based "mainly on evidence from one specific location (Jericho) only." Section VII of my article presents quite a range of other avenues of archaeological evidence, in addition to the overarching web of biblical and historical considerations. And the evidence pertaining to Jericho, approached from three independent scientific directions, is in my view mighty indeed. *Ha-ḥut ha-mishbulash lo bimhaira yinataik*. And Jericho's destruction must come after the exodus. That is a foundational aspect of the Torah's chronology; it is not a matter of interpreting a word here or a phrase there.

By contrast, the so-called 'solution' I present on page 205 of the article to conceivably negate the evidence from Jericho, upon which Mr. First says he relies, is quite anemic. It assumes a small, imaginary replacement city to the large but destroyed MBA Jericho, a city for which no evidence exists where we would

expect to find at least some supporting data. This city exists only in the inventive minds of those who need it to rescue their hypothesis. And the counter-evidence from the new Israelite settlements in the central highlands of ca. 1200 BCE, the centerpiece of Mr. First's position, is not persuasive. It demonstrates merely that the Israelites built new settlements at that time, in that area; it does not demonstrate that the Israelites were nowhere in the country in the decades prior to that period.

Mr. First recognizes that the Merneptah Stele of ca. 1210, in which the 'people Israel' appear in a list of that pharaoh's claimed conquests, poses serious difficulties for his position that Merneptah was the pharaoh of the exodus. Indeed it does. But the difficulty runs even deeper than Mr. First seems to realize. Up to very recently it was widely assumed that the Merneptah Stele represents the earliest extra-biblical reference to 'Israel,' thereby compelling the exodus to occur (more than four decades) before 1210 BCE (to allow for the Israelites' wandering in the wilderness of Sinai). In recent years, however, a previously ignored Egyptian stone inscription, resting unobtrusively in the Egyptian Museum in Berlin, has gained much attention in

the world of Egyptology. It almost certainly contains a reference to 'Israel,' and it is to be dated epigraphically to as early as ca. 1400 BCE (Van der Veen, Theis and Gorg, in *The Journal of Ancient Egyptian Interconnections*, vol. 2:4, 2010, p. 15–25). If this is correct, the earliest extra-biblical reference of 'Israel' as a people or state occurs some two centuries earlier than anyone previously thought. (This came to my attention after I wrote the article.) This, of course, moves the exodus to a date **much** earlier than the thirteenth century and Merneptah. Why not, in light of all the evidence, move it a bit further to ca. 1600 BCE?

Shemoneh Esreh ca. 250

I ENJOYED AND LEARNED much from Heshey Zelcer's article on the early *Amidah* ("Shemoneh Esreh in Eretz Yisrael ca. 220–250 CE"). There are, however, a number of points that merit further discussion.

The article's thesis is that (a) Yerushalmi Berkahot 2:4, 4d ("*Shemoneh Esreh Text One*" in the article) preserves the language of an early version of the *Amidah*'s petitionary (middle) blessings, (b) the first two words of *Shemoneh Esreh Text One*'s description of each of those blessings constituted the entirety

of the pre-*hatima* part of that blessing in this early version and (c) the Cairo Genizah texts cited by Mr. Zelcer (and, in particular, the text cited on pp. 94-95) is strong evidence for proposition (b).

To substantiate propositions (b) and (c), Mr. Zelcer must demonstrate that (i) there was an early, very brief, form of *Shemoneh Esreh* and (ii) the Cairo Genizah's brief version preserves that early form. If I understand Mr. Zelcer correctly, he relies heavily on the (ninth century or later) Cairo Genizah version of the *Shemoneh Esreh* he brings on pp. 94-95 for that proof. That is, he assumes that this Cairo Genizah text is a version of (an early third century) text reflected in *Shemoneh Esreh Text One*—and is thus proof that *Shemoneh Esreh Text One* embodies the entirety of the pre-*hatima* portions of the middle blessings. Apparently, the reason for this assumption is a supposition that the simpler a liturgical text, the older the version it preserves. If this supposition is true, goes the reasoning, the very brevity of the ninth century (or later) Genizah text is (i) proof that it is a preserved version of an ancient—perhaps 600+ year-old—tradition and thus (ii) evidence that the comparably brief (two word) opening parts of the intermediate blessings found in the

third-century *Shemoneh Esreh Text One* represent the entirety of the pre-*ḥatimah* portion of those blessings. Conversely, the more verbose versions of the middle blessings found in the Genizah preserve later traditions—and the longer the version, the later the tradition.

This assumption that the *Amidah* developed linearly from the simple to the more complex is, however, not in accord with most modern scholarship or with other evidence as to the early *Amidah*'s text. Having examined the evidence closely, most modern scholars conclude that while in some cases a given prayer or *Amidah* blessing followed the simple-to-more-complex (or shorter-to-longer) route, in other cases the opposite was true, while in yet other cases the text changed over time while the length did not change materially. See, for example, Menachem Kister's summary, "It is difficult to see the development of prayer as a simple linear one, from the simple to the complex, from the short to the long, from one *nusach* to that which developed from it." *Liturgical Formulae in the Light of Fragments from the Judaean Desert, Tarbiz* 77 (2009), p. 336.

An excellent illustration of these points can be found in, among other places, another re-

cent article in *Tarbiz* by Shulamit Elizur, *The Chains of Verses in the Qedushta and the Ancient Benediction*. That article focuses on the *Amidah* as it existed in the immediate post-Yavneh period and the centuries thereafter—in other words, approximately the same time period that Mr. Zelcer focuses on. In addition to examining many of the sources that Mr. Zelcer looks to, she examines perhaps the principal body of evidence that sheds light on the *Amidah*'s text in the approximately 700-year period between the end of R. Gamliel deYavneh's era and the Cairo Genizah—the earliest *piyutim*.

Professor Elizur concludes that "[s]everal ancient rabbinical sources indicate that a longer and more complex version of the *Amidah* was ... recited [in the post-Yavneh period], and it included biblical verses ... The *Amidah* prayer evidently underwent processes of change and abbreviation." She does hypothesize that there may have been an early, brief version that coexisted with the longer and more complex version, and that this version could be an 'ancestor' of the modern *Shemoneh Esreh*. However, she sees (i) that 'ancestral' version as the *mei'ein Shemoneh Esreh (Havineinu)* of Rav that incorporated the

hatima of each intermediate blessing, rather than, for example, Shemuel's version cited by Zelcer on p. 87 as a possible 'cousin' of *Shemoneh Esreh Text One* and (ii) the commonality of the two (Rav's *Havineinu* and the modern *Amidah*) as rooted in the structure each shares rather than in their specific language.

Other scholars (in particular Prof. Uri Ehrlich, several of whose articles are cited by Mr. Zelcer) reconstruct early (post-Yavneh/pre-Cairo Genizah) versions of several of the *Amidah*'s blessings—including of the intermediate blessings—that are longer and more complex than the laconic early versions hypothesized by Mr. Zelcer.

The conclusions and observations above are not flatly at odds with Mr. Zelcer's thesis. He acknowledges that "[w]e are implying not that this was the only version of *Shemoneh Esreh* that was recited at that time but rather that it was a version." It is thus *possible* that (a) there was an early, abbreviated version of the *Amidah*'s petitionary blessings, (b) *Shemoneh Esreh Text One* preserves much of that version and (c) the Cairo Genizah text from at least six centuries later is a 'fossilized' representation of that early, abbreviated *Amidah*. However, in the absence of any evidence that bridges that (at

least) six-hundred-year chasm, and given the existence of evidence to the contrary, the article's thesis is most properly characterized as intriguing speculation.

What Mr. Zelcer *does* demonstrate very nicely is that the first two words of *Shemoneh Esreh Text One*'s description of each middle blessing are two key words that were likely present in many early formulations of those blessings. In fact, Mr. Zelcer arguably 'undersells' the proof that *Shemoneh Esreh Text One* provides of the early presence of the two key words. He states that the Mahara Fulda's explanation of that text is that it is "a sequential list of asking something of G-d, and then after acknowledging that the request was granted asking Him to fulfill our next request."

An alternative description of the Mahara Fulda's explanation that both accords with the Mahara Fulda's words and (better) supports Mr. Zelcer's thesis would be as follows: the Yerushalmi is explaining that there is a logical and necessary relationship between each blessing, such that blessing #2 cannot be granted until we have been granted blessing #1, blessing #3 cannot be granted until we have been granted blessing #2, etc. Thus, the Yerushalmi should be

read as stating that “we utter ‘hōneinu dei‘ah’; once G-d has granted this prayer by giving us ‘dei‘ah,’ we have the knowledge to know that we should ask for forgiveness by uttering ‘retzeh be-teshuvateinu” etc. This seems to be the plain reading of the Yerushalmi and would produce precisely one of the conclusions that Mr. Zelcer reaches: that the first two words of each four-word description of a given blessing in *Shemoneh Esreh Text One* are (at least part of) the actual words of the blessing while the last two words are not part of the blessing, but merely a way of explaining why blessing #2 follows blessing #1, #3 follows #2, etc.

Finally, readers of *Hakirah* should know that, in addition to the articles and books cited by Mr. Zelcer, there is a plethora of recent scholarship on the early history of both Jewish prayer in general and the *Amidah* in particular. That scholarship should be read by anyone who wants to better understand the *tefilot* we recite every day. In particular, I refer readers to the sources noted in my article on the twelve words that open every *Shemoneh Esreh*—“*The Amida’s Biblical and Historical Roots: Some New Perspectives.*” The article can be found in the Fall 2012 issue of *Tradition*.

Again, Mr. Zelcer deserves our gratitude for a very interesting and thought-provoking article and for introducing readers to modern scholarship on Jewish liturgy.

Allen Friedman
Teaneck, NJ

Heshey Zelcer Responds:

I thank Allen Friedman for his careful reading of my article and for his detailed and worthy comments.

For the benefit of the reader I will limit my response to Mr. Friedman’s main critique of my thesis, which I believe is summarized by his statement that “This assumption that the *Amidah* developed linearly from the simple to the more complex is, however, not in accord with most modern scholarship...”

A careful analysis of the tables in my appendix (pp. 109 – 121) shows that the different versions of each blessing are not arranged linearly from shortest (simple) to longest (more complex) but rather are usually arranged by their two main branches: the so-called Palestinian versions first, and afterwards the so-called Babylonian versions. It is only within each of these branches that the different versions are arranged—for visual

ease—from shortest to longest.

My thesis is not that *Shemoneh Esreh* developed from the simple to the complex but that “*Shemoneh Esreh Text One*” contains “core phrases” that seem to be the “fathers” of the intermediate blessings (p. 96). Regardless of the length of the intermediate blessings recited in different geographic areas across different generations, the version of *Shemoneh Esreh* recited usually contained these core phrases.

Take, for example, the first intermediate blessing we colloquially refer to as אַתָּה חַוֵּנוּ. Two very different branches of this blessing are shown in our table (p. 109). In the first branch (Palestinian, lines 2 - 9) the blessing begins with a phrase similar to חַוֵּנוּ דַעָה and then expands to the left. The second branch (Babylonian, lines 14 - 36) also contain some form of חַוֵּנוּ דַעָה but its versions expand to the right. The two branches are very different but they both share the core phrase חַוֵּנוּ דַעָה.

This overlapping of core phrases in different branches is also obvious for סִלַּח לָנוּ (p. 111) where lines 5 - 10 show one branch and lines 14 - 26 show a very different branch; for גְּאֵלֵינוּ (p. 112); and perhaps others.

Mr. Friedman acknowledges that I argue only that “*Shemoneh Esreh Text One*” represents an

early version but not necessarily the only early version. In fact, in footnotes 22 and 42 I go much further and state that I find R. Heinemann convincing when he argues that many versions of *Shemoneh Esreh* were recited at that time—some very long, others very short. *Berakhot* 34a, *Mekhilta* and various other sources mentioned in the above footnotes seem to confirm this.

In conclusion, it is my hope that others who try to recreate the core phrases of *Shemoneh Esreh* will be motivated to give due consideration to “*Shemoneh Esreh Text One*,” a text that has been mostly overlooked by modern scholars studying the *Shemoneh Esreh*.

Sefer ha-Mitzvot

THANK YOU for the insightful article by Rabbi Buchman about the order of the *mitzvot* in *Sefer ha-Mitzvot le-ha-Rambam*.

It is interesting to note that in 1945 a book by the name *Seder ha-Mitzvot le-ha-Rambam*, by Rabbi Jacob Moinester, was printed in New York, addressing this issue. See <<http://www.hebrewbooks.org/pdfpager.aspx?req=15291&st=&pgnum=2>>.

Thank you.

Dovid Olidort
Brooklyn, NY

Learning Mathematics

THE ARTICLE “*Learning*” *Mathematics* concludes with suggestions on how and when mathematics is to be taught.

I am surprised that the authors do not recommend the GRA’s *sefer*, *Ayil Me’Shulash*. In my article “The Case for Secular Studies in Yeshivas” **The Jewish Press**, November 19, 2004 p. 1 I wrote:

In most “right-wing” yeshivas students take three years of mathematics consisting primarily of selections from topics in algebra, geometry, trigonometry, probability, logic, and statistics. In New York, passing the math Regents is the goal, while in other states, the state guidelines for public school curricula are adhered to. Often there is no mathematics taught in the twelfth grade. I do not understand why the yeshivas do not gear their mathematics courses to the goal of having their students study selections from the GRA’s *sefer*, *Ayil Me’Shulash*, in the twelfth grade.

The *sefer* *Ayil Me’Shulash HaMevuar HaGRA*, volume 1, by Rabbi Avinoam Solimani was published not long ago in Eretz Yisrael. It

contains the text of the first three sections of the GRA’s original *sefer* as well as modern-day diagrams and Hebrew explanations of these sections. If yeshiva students were to study this *sefer* they would not only learn some of the mathematics that the Vilna Gaon thought was important, but they would also have the benefit of studying these topics in Hebrew, something that would no doubt improve their mastery of the language.

These comments are, of course, geared to American high schools that teach secular subjects. However, I fail to understand why Israeli yeshivas, even the Chareidi ones, do not teach enough mathematics so that their *talmidim* can study the *sefer* *Ayil Me’Shulash* and incorporate its study into their curriculum. Clearly the GRA felt that the study of these topics in mathematics is important.

You may wonder why I focused only on the GRA’s *sefer* rather than on the “many available *sheilos u-teshuvos seforim*, *m’forschei ha shas* and journal articles that discuss a wide variety of Talmudic sources from a mathematical perspective” mentioned in the article. There are two reasons for this.

1. The GRA’s *sefer* is readily

available and hence implementation would be easy, assuming there are yeshivos that would be interested in doing this.

2. My suggestion to use this *sefer* rather than others was an attempt to make the study of mathematics important in the eyes of Chareidi high school boys. Today there is a far-too-prevalent attitude in many yeshivas that the study of secular subjects is a waste of time and *bitul Torah*. There was a time some years ago that I did a good deal of tutoring of high school mathematics and most of my students were from Chareidi yeshivos. Time and time again I heard a boy who came to me for help say, “I don’t want to study this, it is waste of time, but my mother (or father) wants me to.” Given the esteemed position of the GRA in Chareidi circles, I think that using the GRA’s *sefer* in yeshivos would go a long way to dispel this negative attitude at least towards mathematics.

Professor Yitzchok Levine
Dept. of Mathematical Sciences
Stevens Institute of Technology
Hoboken, NJ

The authors respond:

Thank you for your comments. Your suggestion to include the Gra’s *sefer* as part of a high

school level curriculum is certainly well taken. However to limit the curriculum to just this one *sefer* in order “to learn some of the mathematics that the Vilna Gaon in the 18th Century thought was important” we feel is too narrowly focused and insufficient. We prefer to include interesting “*lamdesha*” pieces from the Rashash, Chavas Yair, Maharam Shiff, etc. that tackle various types of geometric, algebraic and probabilistic problems. Expanding the range of topics and the accepted gedolim who demonstrate mathematical sophistication should dissuade anyone from saying that it is only someone like the Gra who would do mathematics.

Learning Mathematics (2)

IN THEIR RECENT article in *Hakirah*, “Learning’ Mathematics,” Vol. 14, Winter 2012, Epstein, Wilamowsky and Dickman describe some of the efforts in the Talmud to solve certain halakchic problems that rely on mathematical concepts and methods. This article represents one of many studies in this area, but is distinguished by its suggestions concerning mathematics education in Yeshivot. In the current paper, we consider a topic in the Talmud that was not analyzed from a mathematical

point of view, and demonstrate its analysis using a classical result in mathematical probability theory far removed from the halakhic context.

The Mishna in *Yevamot* 98b states: Five women gave birth to five sons who, just after birth, were “mixed up” so that the true mother-son relationships were unknown. Each of the mothers also has a son known to be hers, either born prior to these five or born subsequently to this incident. Each of the first five unattributed sons marries and dies without children, so that their five widows are subject to *yibum* by the surviving brothers. However, since the identity of the actual deceased brother of a surviving brother is unknown, the *yibum* ties between the widows and the surviving brothers are also unknown. Under these conditions, it appears at first sight that the only way to allow the widows to remarry—albeit not to any of the brothers—would be to have each surviving brother give *ḥalitza* to each of the widows.

The Mishnah proposes another solution that allows each widow a possibility of *yibum* (or marriage) with one of the surviving brothers—possibly her true *yavam*. One brother selects a particular widow for marriage, then each of the other four

brothers gives her *ḥalitza*, and then the first brother marries the selected widow. This is permissible because this brother is either the true *yavam* or, if not, then the true *yavam* has already given *ḥalitza*. A second brother then selects one of the remaining four widows and marries her after each of the other four brothers gives her *ḥalitza*. This is repeated for each of remaining three widows until they are all married to one of the brothers.

The Gemara notes that another solution would be for one brother to marry all five widows after the other four brothers have given *ḥalitza* to each of the five widows. However, the solution in the Mishna is considered preferable because, *by chance alone*, one or more of the widows might marry her true *yavam* (in addition to the fact that it is not necessarily practical for one man to have five wives). This raises the question: how many of the five widows are likely to marry their true brothers-in-law under the protocol stated in the Mishna? More exactly, the possible number of correct matches of widows and their true brothers-in-law is one of the numbers 0, 1, 2, 3, 5. (4 is not a possible number because four matches necessarily imply a fifth match.) A mathematical question is: what are the probabilities of 0, 1,

2, 3, 5, matches?

This is a special case of the following classical problem in probability theory known as the problem of “coincidences.” It was solved in 1708 by P. R. Montmort. Consider two decks of cards, each numbered 1 through n , where n is the number of cards in each deck. The cards in one deck are placed on a table with the numbers facing up. The other deck is shuffled at random and each card is placed face-down next to a card from the first deck. This produces a set of n pairs of cards, where each pair has a card facing up and a card facing down, so that the card number is visible on only one card of the pair. We say that there is a “coincidence” or a “match” if the numbers on each of the pair of cards are the same. Question: What is the probability distribution of the number of matches among the n pairs? That is, what are the probabilities of 0, 1, 2, ... matches? The solution to this problem depends on some basic but elementary concepts in probability theory, and is described in several elementary textbooks. See, for example, W. Feller, *An Introduction to Probability Theory and its Applications*, Vol. Third Edition, 1968; S. Ross, *A First Course in Probability*, Seventh Edition, 2006. The solution is summarized by a

formula for calculating the probability of getting a specified number of matches for a specified number of cards in each deck. In particular, for the case discussed in the Mishna, which is equivalent to five cards to be matched, the probabilities are as follows:

# of Matches:	0	1	2	3	5
Probability:	0.367	0.375	0.167	0.083	0.008

These probabilities are calculated under the assumption that the matching of the cards is purely random, that is, every card has the same likelihood of a match with every other card. This may be applied to the *yibum*-matching problem because it is likely that the mix-up described in the Mishna was purely random.

The mathematical theory of coincidences also provides the following striking result: The *expected number of matches* for a card deck (or widow set) of any size is always equal to 1. This implies that if the matching procedure is repeated many times for a card deck of any fixed size then the *average number of matches approaches 1 as the number of repetitions gets larger*. See, for example, K. L. Chung and F. AitSahlia, *Elementary Probability Theory*, Fourth Edition, 2003.

The probability calculations also illuminate the implications of the differences between the protocol of the Mishna and that proposed in the Gemara. Under the former, there is probability 0.367 that there will be no matches; however, the probability is $0.167 + 0.083 + 0.008 = 0.258$ of two or more matches. Under the latter protocol there will be exactly one correct match. The preference of the Mishna for the first procedure, stated in the Gemara as based on “chance,” can be analyzed in terms of these probabilities. Under the protocol of the Mishna there is at least a positive probability—though possibly small—that the mitzvah of *yibum* will be performed one or two or three or five times. By contrast, under the alternative protocol mentioned in the Gemara the mitzvah of *yibum* will be performed exactly once—no more and no less.

Simeon M. Berman
Emeritus Professor of Mathematics at the Courant Institute
of Mathematical Sciences
New York University

Pinchas Willig
Ph.D. in Mathematics
New York University

The authors respond:

We would like to thank Drs. Berman and Willig for their thoughtful insights into *Yevamos* 98b and use their analysis to further demonstrate the thrust of our paper. We are not offering “suggestions concerning mathematics education in Yeshivos” for the sake of mathematics. Rather, we are suggesting that a full appreciation of many Gemaras is not possible without a sophisticated appreciation of the relevant mathematics. Towards this end, we would suggest that their analysis of the Gemara in *Yevamos* may explain an issue touched on by the classical Mishnaic commentators (e.g., Tosfos Yom Tov, Tifferes Yisrael etc.): Why did the Mishna present the halacha in the case of 5 brothers? Would the Mishna’s preference of having each brother marry one woman also apply in cases involving less than 5 brothers? Yam Shel Shlomo suggests that in the case of only 2 brothers, the Gemara’s alternate approach of having one brother marry all of the women is better and Aruch LaNer extends it to the cases of 3 and 4 as well. Below is a chart of probabilities of all possible matches in cases involving 2 to 5 brothers. These probabilities may explain the Yam Shel Shlomo’s position in

the case of 2 brothers (i.e., 50% chance of having no *yebum* marriage using the Mishna's approach vs. 100% chance of getting one match using Gemara's approach).

Do these numbers, however, justify the Aruch LaNer's extension (i.e., the difference in probability for not getting a single match in cases 3 to 5 are almost identical)?



Probability Table

Matches	Number of Brothers			
	2	3	4	5
0	0.50000	0.33333	0.37500	0.36667
1	0	0.50000	0.33333	0.37500
2	0.50000	0	0.25000	0.16667
3		0.1667	0	0.08333
4			0.04167	0
5				0.00833