

LETTERS TO THE EDITOR

Mezizah be-Peh

ḤAKIRAH is to be commended for publishing Shlomo Sprecher's outstanding article on *metẓitzah ba-peh*. In today's environment I am sure it took a good deal of courage to do so, and your readers are grateful. Sprecher's article has now become the place to turn for those who want information on this topic. I would therefore like to call attention to some other relevant sources, in order to round out the picture. Since Sprecher has focused on the Lithuanian Torah world and its attitude towards the practice, let me note the following.

R. Avraham Yitzhak Kook, whose stringency in ritual matters is well known, had no difficulty recommending that a tube be used for *metẓitzah*. He also testifies (in 1914) that this was done in Jaffa (*Da'at Kohan*, no. 141, and see also *ibid.*, no. 142).

R. Yitzhak Herzog, first chief rabbi of the State of Israel, in a responsum to Bernard Homa (author of the pamphlet *Metẓitzah* [London 1960]), states the following: ברור לענ"ד כשמש בצהרים שאין המציצה חלק מהמצוה לא לכתחילה ואצ"ל לא דייעבד. He further notes that even if only some experts declare that *metẓitzah be-peh* creates a danger, העומד על כך שהמציצה תיעשה דווקא בפה הרי הוא לדעתי טועה ומטעה בדבר שיש בו חשש של סכנה . . . ועליהם לחזור מדעתם זו ואדרבה לדאוג לכך לצאת ידי הרופאים הגדולים. As for the Hatam Sofer's controversial responsum on the topic, R. Herzog states that the

former did not intend to keep it hidden, for if so, he would have written that it was intended only as a *hora'at sha'ah*. According to R. Herzog, since in those days so few problems arose with *metẓitzah ba-peh*, the Hatam Sofer had no reason to abolish the practice *in toto*. Without actual evidence of specific danger, the principle of *shomer petaim Hashem* applied (*Pesakim u-Khetavim, Yoreh Deah*, no. 84).

One would have expected R. Jehiel Jacob Weinberg to share these sentiments, but that was not the case. Surprisingly (to me at least), he gives credence to the view that *metẓitzah* is not only an essential component of the *mitzvah*, but that it might even be *halakhab le-Moshe mi-Sinai*. He concludes his responsum, which was sent to Rabbi Oscar Fasman, president of the Hebrew Theological College in Chicago, by urging him not to get involved in any controversy regarding *metẓitzah*. He is very concerned that if Fasman expresses an opinion against *metẓitzah*, it will be picked up by the Conservatives, and at the same time lead to attacks on HTC by the *haredim*. He adds: ולצערנו התגברה מאוד הקנאות ואי הסבלנות בחוגי החרדים וכל מי שיש לו דעה אחרת משלהם פוסלים אותו ויורדים לחייו (*Kitvei ha-Gaon Rabbi Yehiel Yaakov Weinberg*, vol. 1, no. 9).

As part of his discussion of the Lithuanian practice, Sprecher notes that R. Hayyim Soloveitchik permitted *metẓitzah* without oral contact. R. Moshe Sternbuch recalls being told the same thing by R.

Isaac Ze'ev Soloveitchik, and that R. Hayyim's ruling took place during a tuberculosis epidemic (*Teshuvot ve-Hanhagot*, vol. 1, no. 588). Yet the story as recorded by R. Sternbuch is hardly proof that R. Hayyim did not regard *metzitzah ba-peh* as important, since in such a circumstance all *poskim* would agree that it must be waived. What is significant is that R. Sternbuch quotes other Lithuanian *poskim*, including R. Isaac Elhanan Spektor, who, barring extreme circumstances, were unyielding when it came to *metzitzah ba-peh*. This shows that even in Lithuania there was never anything approaching an absolute consensus that *metzitzah ba-peh* can be easily waived.

As for the Hazon Ish, Sprecher mentions that he consented to serve as a *sandek* even when *metzitzah ba-peh* was not performed. He further rejects R. Shmuel Wosner's attempt to turn the Hazon Ish into an opponent of using a glass tube (although the reference given is mistaken, since nothing about the Hazon Ish appears there). Sprecher does not note that in *Shevet ha-Levi*, *Yoreh Deah*, vol. 6, no. 148, R. Wosner reports that he heard from the Hazon Ish that for those places that still practice *metzitzah ba-peh*, one should fight to keep it that way. Only with regard to those communities that had abandoned *metzitzah ba-peh* did the Hazon Ish believe that it was not a battle worth fighting. It therefore makes perfect sense why the Hazon Ish would have no problem serving as a *sandek* when there was no *metzitzah ba-peh*, but that doesn't mean

that he would support abolishing it, since at the very least he regarded it as an important *minhag*.

I would also like to point out an error on p. 51. Here Sprecher refers to "repulsive practices that certainly are not part of any mitzvah, but were thought to be therapeutic." He cites the Hida, *Mahazikot ha-Berakhot*, *Yoreh Deah* 79:2, as permitting the practice of providing the freshly removed foreskin to barren women as a cure for their condition. In fact, the Hida does not permit the practice but states that it appears to be forbidden (although he acknowledges that according to the Tosafists it would be permissible).

The unfortunate fact is that we live in a world where many *poskim* are unfamiliar with basic science. Whether it be the sad spectacle of *poskim* who insist on the reality of spontaneous generation or deny the efficacy of DNA, one can only hope that Sprecher's article, and others like it, will finally shine some light on this problem, and perhaps help rectify matters.

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I AM WRITING in response to the article that appeared in your journal, "Mezizah Bepeh — Therapeutic Touch or Hippocratic Vestige?" by Shlomo Sprecher. First, I congratulate Dr. Sprecher on his diligent research and outstanding presentation. I also stand corrected, as Dr. Sprecher has demonstrated that there is reasonable evidence that there were infections

such as syphilis and tuberculosis transmitted through *metzitzah bepeh* in the 19th and early 20th century. Dr. Sprecher quoted a statement that I had written in a letter to the Forward that there has not been a documented case of death attributed to *metzitzah bepeh*. Dr. Sprecher agrees that “absolute laboratory corroboration” had not been done, as the technology was not yet available. However, it is certainly reasonable to accept that there were fatal cases of syphilis and tuberculosis transmitted through *metzitzah bepeh*. (One has to wonder what kind of *mobel* was suffering from syphilis, a sexually transmitted disease.)

I would like to comment on several points made by Dr. Sprecher. First, the possible transmission of syphilis and tuberculosis was caused by *mobelim* who had active disease. No *posek* would ever allow *metzitzah bepeh* to be performed by a *mobel* with active infectious lesions. The last of these reports appeared in 1946, and there have been no reported cases since then.

Dr. Sprecher refers to “a state of fatalities among the newly circumcised infants” in Vienna in 1837, observed by Dr. S. Wertheim. This was the outbreak that led to the famous letter of the Hatam Sofer that Dr. Sprecher discusses in detail in his article. Dr. Wertheim, “although he could not identify any lesions in the *mobel*’s mouth ... attributed the outbreak to *metzitzah bepeh*, since the afflicted all suffered initially with incurable rashes on the *brit-milah* wound.”

From the description given, it is not clear why *metzitzah bepeh* was perceived to be the source of the presumed infection, rather than the circumcision itself. If Dr. Sprecher believes that this episode led to the Hatam Sofer’s recommending that *metzitzah bepeh* should not be practiced, perhaps the Hatam Sofer should have reevaluated the whole practice of circumcision.

For the past 60 years, other than the sporadic cases of herpes infection temporally related to *metzitzah bepeh*, no other infections have been reported associated with *metzitzah bepeh*. This includes viral infections such as hepatitis and HIV, as well as bacterial infections. This is a remarkable safety record. With regard to herpes, it is certainly possible that there are rare cases of transmission. Until there is DNA evidence of transmission of a single case, the possibility still remains that the infections in question were contracted in a manner other than *metzitzah bepeh*. Herpes virus is easily transmitted. It has been reported that 100% of children in lower socioeconomic groups, probably because of close living quarters, are infected with herpes by puberty.

Other religious practices are associated with slight risk. There appears to be a point that risk is so small that modification of religious practice would never be considered. Two men have died as a result of accidents related to crossing the street in front of my synagogue following services in the past five years. I do not think that the *poskim* would consider ending communal

prayer in the synagogue. Children have perished as a result of accidents related to lit candles in the home. I cannot imagine the *poskim* ending the practice of lighting Shabbos and Chanukah candles. It is for the *poskim* to decide what degree of risk warrants modifying religious custom.

There should not be separate camps of pro *metzitzah bepeh* and anti *metzitzah bepeh*. *Poskim* should decide for families on how to proceed. However, it should be clear to all that the government's reaction has been severe and inconsistent with its response in other health hazards affecting children. The Centers for Disease Control (CDC), entrusted to guarding public safety on a national level, in response to outbreaks of life-threatening bacterial infection linked epidemiologically and genetically to three petting zoos across the United States; involving 108 persons, including many children, recommended only stronger infection control measures in these zoos. The CDC's report said nothing about closing *these* petting zoos, *all* petting zoos, or strongly recommending that children no longer go to petting zoos. The CDC also reports that each year an average of 384 children die from bicycle-riding accidents. The CDC's response is to increase bicycle helmet usage. There is no consideration to stopping children from riding bicycles.

The heavy-handed approach of the government in this matter out of proportion to its response in other situations should be of great

concern to the Jewish community. There is a realistic fear that the government may come after other Jewish religious practices. Among ourselves, we can discuss whether to continue a custom that may carry slight risk. I believe it is our obligation to determine through DNA testing if there is any risk. The *poskim* can then make recommendations based upon full knowledge of the subject. In the meanwhile, there is an urgent need to resist the forces on the outside who are acting in what appears to be an unusually harsh way against the Jewish community.

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I READ Dr. Sprecher's article on *mezitzah ba-peh* with great interest, especially because I spent a lot of time studying the issue from a halakhic standpoint, and read much of the halakhic material on the subject. Dr. Sprecher's article added greatly to my understanding of the medical issues, the medical background, etc. For example, I was familiar with Dr. Halperin's claims, but would not have been able to determine how valid they were. I was familiar with some of the early cases of infant mortality due to MBP, but some of the cases that Dr. Sprecher cited moved me to tears, because I am so pained that, as if we don't have enough enemies who harm us, there are those among us who will not learn from mistakes of the past, and Dr. Spre-

cher pointed out more such cases than I was aware of. In Israel, where I live, the practice is still very common, and not just among *haredim*, but among many people who are simply unaware, or among people who think that it is always better to do things the way their ancestors did (and remember that there are many people whose roots are in places that did not have an enlightenment), and among many newly religious who seem to be attracted to any practice that someone tells them is “*mehadrin*.”

Following my study of the issue, which was initially done and written as a paper as part of my requirements towards a master’s degree in Jewish studies, I wrote a paper entitled “*Metsitsa Ba-peh*—the Legacy of the Orthodox Controversy with Reform Judaism.” I think that my paper has some perspectives that have not appeared in the other, vast literature on the subject, including discussion of how contemporary *poskim* like Rav Elyashiv are influenced by the continuing polemics and political issues. With your permission, I’d like to copy here some of the opening paragraphs:

Now, over thirty years following that publication (the article by Dr Shields which was cited by Dr Sprecher), we find that the practice is still common, and that it is mainly the fear of AIDS, and recently, the fear of herpes, that seems to have had any significant impact on the attitudes in halakhic literature towards the practice. Nevertheless, rabbinic leaders shy away from forbidding direct

oral suction, in spite of its health hazards. The Rabbinical Council of America’s (RCA) latest statement, which included summaries of four opinions, among which is the opinion that direct oral suction is a requirement of halakhic circumcision, came so far as declaring that: “Those who wish to follow their customs in accordance with the above-noted authorities are certainly entitled to do so, but the RCA is firmly of the opinion that in light of current realities and medical knowledge it is proper, and preferable, to use a tube.”

Why the hesitation to state that in light of the current realities and medical knowledge, it is forbidden to use direct oral suction and that the opinion of the authorities who required it is not applicable? Why are people still “entitled” to follow an opinion that puts their child at risk? Isn’t the commandment of **אל תעמד על דם רעך** applicable? Further, in reaction to the recent events surrounding the death of an infant from herpes, some *poskim* hardened their views and returned to forbidding that which they permitted previously in light of the advent of AIDS.

The reaction of nineteenth century and early twentieth century *poskim* to proposals for change has been well documented. Because the concerns about *metzitzta* were raised by people whose loyalty to halakha was questioned (even though at times they were observant Jews) and/or their motives were confused with those of the opponents of circumcision in the nineteenth century, most of the

poskim at the time either opposed any change, even if proving their case required using creative halakhic methods, or reluctantly allowed use of an implement, while making it clear that this was a compromise and that oral suction was the preferable method.

Much evidence suggests that as a result of the nineteenth century controversy, most *poskim*, even today, view *metzitzta ba-peh* as the ideal practice, in spite of obvious hygienic problems, and in spite of halakhic justification for *metzitzta* by other means. Many of the *poskim* today who allow non-oral *metzitzta* at all, are willing to allow it as a compromise, only because of the threat of AIDS, and very recently, because of the dangers of herpes. And some *poskim* recently reacted to the leniencies that were expressed after the herpes incident in the same manner that the strict nineteenth century rabbis reacted to proposals to use non-oral methods in their time. We therefore find some *poskim* who allowed non-oral suction because of the fear of AIDS and who have hardened their view during the last year.

Further, though the fear and danger of gonorrhea, syphilis, and tuberculosis were probably at least as great as the fear and danger of AIDS today, rarely, if ever, is it granted that the *poskim* of the nineteenth century who opposed non-oral *metzitzta* were in a similar position to *poskim* today who make the allowance because of AIDS or herpes, and thus should have allowed non-oral *metzitzta* then. Though ruling differently from

these earlier *poskim* in light of new medical concerns and awareness is a step towards better protection of Jewish infants, recognizing the errors of the past would help adjudicators today base their decisions on the halakhic issues without feeling obligated to incorporate or defend the misguided rulings of their predecessors.

Rather than recognizing the influence of nineteenth century polemics on their predecessors, present-day adjudicators have continued to treat what is a technical addendum to the rite of circumcision as if its change would have ramifications for the overall halakhic validity of the performance of the ritual. It is my hope that there are, today, some religious leaders who will have the courage to clearly state that following the opinion of those who said that direct oral suction is a requirement, no matter how great those rabbis were, is, in fact, forbidden. Michael Broyde recently wrote that one of the characterizations of a modern Orthodox Jew is that s/he can recognize that “even the best of Torah scholars or rabbis can make mistakes.”

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Shlomo Sprecher responds:

Prof. Marc Shapiro’s insightful comments and corrections are much appreciated. However, I have to disagree with his apparent acceptance of R. Isaac Ze’ev Soloveitchik’s characterization of his

father's lenient ruling as a *Hora'at Sha'ah* promulgated during a TB "epidemic." Tuberculosis was en-
demic to 19th century European cities, which means there were no episodic flare-ups of TB that would lead a *posek* to declare a temporary ruling. Rather, the situation was one of a static rate of infection (much like HSV, though, of course, resulting in a far greater number of fatalities). I also thank Dr. Debby Koren for her encouragement, and I commend her scholarship.

As for Dr. Dan Berman's letter, I believe his great zeal for the cause of MBP leads him to unfortunate error. For example, his comparison between the morbidity/mortality of infants harmed by MBP and accident victims struck down en route to attending *Minyan* is imprecise, to say the least. Of course nearly every human activity entails some aspect of risk, but these risks are tolerated, because of the desired purpose of the action in question. Contrast that with MBP, whose sole purpose was intended to be a therapeutic one, but now is clearly understood to present only risk, with no therapeutic benefit whatsoever. Clearly, a purposeless action that provides only risk, with no attendant benefit, cannot be justified.

The other *Hareidi* physician advocate of MPB, Dr. M. Halperin, unfortunately did not respond to, or comment on, my article. However, he did recently publish (in the current issue of Jewish Action, [release date 11/06], Winter 5767/2006, Volume 67, No. 2, pp.

25, 33–40) an article entitled, "The *Metzitzah B'Peh* Controversy: The View from Israel." His article confirms several of my contentions. For example, on page 34 he writes:

From this *gemara* [Talmud Bavli, Shabbat 133b] it seems fairly clear that medical considerations are the only reason for *metzitzah*, for the *gemara* states that the very fact that *metzitzah* is permitted on Shabbat indicates that failing to perform it poses a risk to life. From this we can derive that *metzitzah* cannot be defined as part of the ritual of *brit milah*, which overrides Shabbat in and of itself, independent of the laws of *pi-
kuach nefesh*.

This *gemara* seems to be the source of the interpretation (i.e., that *metzitzah* is performed because of medical reasons) adopted by many *posekim*, both Rishonim and Acharonim, including the Rambam, the Shulchan Aruch, the Chochmat Adam and, as mentioned above, the Chatam Sofer, the Ketzot Hachoshen, the Netziv of Volozhin, the Avnei Nezer, Rabbi Auerbach and the Tzitz Eliezer.

The article was also courageous for its acknowledgement that as early as May 2002, he and his staff, reacting to the data gathered by Dr. Gesundheit (which would not be publicly disseminated until its August 2004 publication in Pediatrics), attempted to modify the risk of HSV transmission from *mobel* to

infant. Dr. Halperin also documents his lengthy (mostly failed) negotiations with Rabbi Wosner, and he concludes (p. 38),

In the course of these discussions, it became evident that at the time in Israel an ideological war [!] was being waged against the performance of traditional *brit milah*. During an ideological war of this nature, great dedication and self-sacrifice [!] as well as absolute insistence on observing tradition are demanded of us.

(I cannot help but wonder who exactly is called on to endure the “self-sacrifice.”)

These positives aside, I am disappointed by the article, because it continues to promote Dr. Halperin’s theory that, infectious possibilities notwithstanding, MBP is the most effective method of preventing a rare but dreaded complication of *Milah*—penile necrosis. His persistence calls for some additional refutation, beyond the discussion in my original article. I will begin with Dr. Halperin’s textual basis for this insight, which he somehow finds lodged in the very simple and clear words of Rabbi Yaakov HaGozer (see p. 34). Perhaps Dr. Halperin assumes that Rabbi Yaakov, an otherwise unknown 13th-century *Mohel*, was not medically sophisticated enough to articulate the actual medical function of MBP. But then, in a stunning extension, Dr. Halperin writes (on p. 35),

Likewise, the words of Rambam may now be interpreted unequivocally. “Until blood in the further reaches is extracted” constitutes only an indication that *metzitzah* has been performed with the requisite exertion of force. Extraction of blood from the further reaches tells us that the *metzitzah* has achieved its purpose, and any existing blockage of the dorsal arteries has been cleared.”

Now the Rambam, without question the greatest pre-modern Jewish medical authority, certainly utilized, in his own practice, Galen’s sphygmology, i.e. the technique of examining the patient’s pulses in assessing the patient’s overall health. The Rambam’s *Pirke Moshe BeRefuah*, Chapter 1, Sections 3–21 represents his own summary of the seven (!) works Galen composed detailing the pulse’s quality in different disease states. (I am not making the absurd claim that either Galen or the Rambam understood the physiology of the circulatory system, but only that they had the terminology to describe the derangement posited by Dr. Halperin.) Who then, if not the Rambam, should have been able to articulate clearly the true rationale of this practice, which, in Dr. Halperin’s estimation, was apparently well known. Note especially Dr. Halperin’s application of *lomdus* in parsing the Rambam—“Until blood in the further reaches is extracted” represents only a *siman* (indication) and not the *sibab* (in-

tent) of the process. While I am as avid a proponent of *lomdus* as anyone, its goal should be to get at the truth, and not to score points in favor of one's pet theory.

As for Dr. Halperin's physiological basis for the practice—it too appears to be without a shred of rational evidence. Again, I ask, how does a momentary application of suction to the distal capillaries reverse proximal arterial spasm? Where has Dr. Halperin ever encountered this technique in any medical practice? How does Dr. Halperin know that MBP can cause an “increase in pressure gradient (by a factor of four to six)?” He provides no evidence that he, or anyone else, has ever measured this phenomenon. Furthermore, if this is the desired intention, then why not advocate applying an elastic tubing to seal the freshly cut glans and then attach the tubing to a suction pump, which can achieve a reproducible, quantifiable degree of negative pressure?

Dr. Halperin's other pronouncements also appear to be misleading. For example, on page 35 he writes:

“History demonstrates that *Chazal* scrutinized medical findings with a critical eye and did not see themselves as bound by Aristotelian dogma. For example, they stated that heredity is not only maternal, but paternal as well, contradicting the Greek sages.”

Dr. Halperin cites *Sbu"t HaRibash* (Responsum #447) as his source, and while he is correct that Rabbi Yitzchak Bar-Sheshet does

make that claim, we should have expected Dr. Halperin to verify that contention before promoting it. In fact, Dr. Halperin could have easily done so by reading Dr. Edward Reichman's outstanding article in *Tradition* (Volume 31, No. 1, Fall 1996) entitled “The Rabbinic Conception of Conception; An Exercise in Fertility.” On page 37, Dr. Reichman writes: “It seems clear that the rabbis, similar to Galen and in contrast to Aristotle, clearly acknowledged both a male and female seed, the female seed appearing to be identified with the menstrual blood. It is interesting to note that the [Talmudic] list of organs that are derived from the respective seeds roughly resembles that of Galen.” Although Dr. Reichman concludes that since neither Galen nor Hippocrates is ever explicitly mentioned anywhere in the Talmud, “cross-cultural borrowing remains speculative,” I believe that “highly probable” should replace “speculative.” Proof for this can be found in M. Bar-Ilan's “*ba-Refuah be-Eretz Yisrael be-Me'ot ha-Rishonot le-Sefirah*,” *Cathedra* 91 (1999) pp. 31–78. See also M. Stern's *Greek and Latin Authors On Jews & Judaism* (Jerusalem, 1980) pp. 306–328 for evidence of Galen's personal familiarity with Jewish physicians and Jewish customs; Galen even knew that a conventional Jewish year is comprised of alternating months of 29 days and 30 days for a total of 354 days, and that some years have an additional intercalated month. A century later we have evidence of the *nasi* R. Gamaliel devising special

remedies for splenic ailments. (See P.W. Van Der Horst's essay "The Last Jewish Patriarch(s) and Greco-Roman Medicine" in *Jews and Gentiles in the Holy Land*, M. Mor, editor (Jerusalem: 2003)).

What I find especially ironic is how a simple reading of this entire Responsum of Rabbi Yitzchak Bar-Sheshet should easily refute Dr. Halperin's very argument. The issue placed before the Rivash concerned the tragic case of a young widow whose husband died only one month into their marriage. Just one week shy of nine months after her final cohabitation with her husband, she delivered a seemingly healthy and developmentally mature baby girl. On the twenty-ninth day of her otherwise uneventful life, the newborn baby contracted an illness and died within a few hours. Declaring her a viable child would, of course, spare her mother the travail of having to wait several years for the siblings of her deceased husband to reach *bar-mitzvah*, the minimum age when they could participate in a *chalitzah* ceremony, and so enable her to remarry. As an additional complication, it seems a *Kohen* was a strong contender to be her new spouse. To the horror of the young woman and her family, the Rivash ruled that because of the Talmud's assessment that only seventh-month and ninth-month fetuses are viable, the deceased infant girl cannot be considered a *valad shel kayyama*, and therefore her mother may not marry until after *chalitzah*, which would then make marriage to a *Kohen* impossible.

This Talmudic ruling declaring a child born two days into its twenty-fifth week of gestation as fully viable (and therefore mandating *chillul Shabbat* to preserve its life), whereas a newborn of thirty-five weeks gestational age is to be treated "as a stone" (and therefore considered to be an **object** bearing the *issur* of *muktzeh*, and certainly not a viable human entity for whose behalf one may violate the Sabbath), is itself **fully** reflective of contemporaneous Hellenistic medical science. (For documentation of this claim, please see the Hebrew translation of R.E. Reiss and A.D. Ash, "The Eight-Month Fetus: Classical Sources for a Modern Superstition," *Obstetrics & Gynecology* 71:2, 270–273 (1988), which appeared in *Assia*, No. 45-46, Teveth 5949 (January 1989) (Vol. 12, No. 1-2), pp. 112–117. Dr. Halperin was certainly familiar with this article, since he was then, and still remains, the editor of *Assia* and he referenced the aforementioned article on p. 93 of that issue in his own article on pre-term infants. For insight into how *poskim* of the 20th Century grappled with this disparity between Chazal's pronouncement and current reality, see Rabbi N. M. Gutel's definitive article on pp. 97–111 in that same issue of *Assia*, and his *Sefer Hishtanut ha-Tevo'im*, pp. 77–80.)

Dr. Halperin concludes his paean to *Hazal's* medical pronouncements by stating that they "recognized pathological anatomy 1500 years earlier" (p. 36). I wonder, for example, how he would explain the passage in Talmud

Bavli *Bekhorot* 44b, which posits two pathways in the male genital organ, one for urine and the other for semen. (See pp. 50–52 in Dr. Reichman’s article cited above in regard to how *poskim* of the 20th Century grappled with this disparity between Chazal’s pronouncement and reality.) Dr. Halperin’s approach is typical of this prevalent *Hareidi* methodology that somehow “overlooks” scores of problematic texts while selecting passages that can be shoehorned into proving how scientifically advanced *Hazal* were. While this approach might work for the credulous and the unsophisticated, it certainly does not represent an honest and forthright manner of dealing with these issues.

Finally, I’d like to conclude by citing Dr. Berman’s conclusion—“there is an urgent need to resist the forces on the outside who are acting in what appears to be an unusually harsh way against the Jewish community.” Again, let me remind Dr. Berman that the officials in the NYC Dept. of Health had difficulty in comprehending why the *Hareidi* community’s own self-policing, which had forced a prominent *Hasidic mobel* to abandon MBP after he was linked to several non-fatal HSV incidents in 1998, should be abandoned, some years later, in the case of another *mobel*’s linkage to a **fatal** incident in October of 2003; and why, in November of 2004, the parents of an infant who contracted HSV Meningo-Encephalitis refused to divulge the identity of their son’s *mobel* so that he could be tested. It

appears that it is not “the forces on the outside” that have changed as much as the *Hareidi* community itself.

Tekhelet

MENACHEM EPSTEIN is to be congratulated for his excellent summary of the evidence identifying the murex trunculus snail as the *tekhelet*-producing *hilazon*, and for his refutation of the misleading assertions disseminated by the *Halachah Berurah* (HB) newsletter (Volume 9, Issue 2).

As Epstein demonstrates, the evidence identifying murex trunculus as the *hilazon* is extremely persuasive. Against this overwhelming evidence, the HB article raises a number of issues that are addressed by Epstein, as well as one scientific argument, which HB describes as the most profound objection to the acceptance of murex *tekhelet*:

At the time of the Gemara it was universally recognized that authentic *tekhelet* and *kela ilan* (indigo) were visually indistinguishable and, indeed, a *baraita* in *Menahot* (42b) states that *tekhelet* should be purchased only from a knowledgeable dealer since there is no way of independently verifying its authenticity. However, R’ Yitzhak b. R’ Yehuda and R’ Avira were said to have had chemical tests for distinguishing *tekhelet* from indigo.

HB argues that since it has been determined that the murex and indigo dyes are not only visually indistinguishable, but also have the

same chemical structure, the murex cannot be genuine, for no chemical test could possibly distinguish between them. Any test the murex dye passes would presumably be passed by indigo as well.

Epstein replies to this question by citing the late Otto Elsner, professor of ancient dye chemistry at the Shenkar College of Fibers, who explained that the fastness of a dye reflects the molecular structure of not only the dye itself but also the substances that were used in the reduction process. Thus the tests may have been able to distinguish between *tekhelet* and indigo due to differences in the way these dyes were produced in the time of the Gemara.

Another response to this question is offered by Rav Avrohom Mordchai Katz, who points out¹ that the *Sifrei* at the end of *Parshas Shlach* indicates that fraudulent *tekhelet* was made out of a **combination** of *kela ilan* **and** other pigments, so it may have been these other pigments that caused the *kela ilan* threads to fail the Gemara's test.

HB argues that it is unreasonable to believe that *Chazal* would make a test that was based on "impurities," as the test would vary from batch to batch, with some batches failing the test, and the test itself could be prone to manipulation by forgers.

These assertions are misguided on several counts. There is little

evidence to suggest that *Chazal* actually designed the *tekhelet* tests themselves. It would seem far more likely that *Chazal* became aware that a certain chemical compound could distinguish between *tekhelet* and *kela ilan* of the kind that was commonly produced at the time. There is no compelling reason to assume that *Chazal* (or potential forgers) necessarily understood how the process worked and which ingredients or elements of the dyeing process made the dye more likely to react.

We can also not be sure that the Gemara's tests always worked. To the contrary, R' Mani is lauded in the Gemara for buying *tekhelet* only from a knowledgeable dealer, and he is told that he is destined to achieve great wealth in the merit of this virtuous deed. If there were a completely reliable means of testing the authenticity of the *tekhelet*, why would it be so important or so praiseworthy to purchase *tekhelet* only from a knowledgeable dealer?²

² Since the Gemara had previously determined that *tekhelet* must be purchased from a knowledgeable dealer to ensure that it was dyed for the sake of the mitzvah and not merely to test the dye, purchasing from such dealers would not be singled out for special praise. The Nimukei Yosef therefore explains that R' Mani purchased the *tekhelet* dye from others and then dyed the threads himself. Thus the praiseworthy action lies in his refusal to rely on the tests to ascertain the dye's authenticity, which implies that the tests were not completely reliable.

¹ Ohr Yisroel, 1997, issue # 10, p. 12, accessible at tekhelet.com/pub.htm.

It should also be noted that the Gemara indicates that *tekhelet* sometimes fails the primary test (R' Yitzhak b. R' Yehuda's test) and, in that case, a secondary test (R' Avira's test) was to be applied. This would appear to refute the basic assumption underlying HB's objection. If *tekhelet itself* sometimes passes the primary test and sometimes fails, it is certainly not unreasonable to believe that murex can pass the test while indigo fails, despite their molecular similarities. Thus the basic premise underlying the HB hypothesis—that two dyes with identical chemical structures must always yield the same result—is not necessarily true.

Finally, it is important to understand that the nature of the Gemara's tests and the mechanism of their action are not well understood.³ While a number of theories have been put forth, it appears that no one has been able to recreate the Gemara's test such that indigo fails. Accordingly, pronouncements about the results that *would or would not* obtain if the Gemara's tests were applied to murex must be regarded as speculative.⁴

³ HB concedes this point early in the article (page 3, column 2), stating: "At present time [sic], no one is familiar with the exact procedures Chazal used in conducting their tests." However, this point is ignored in the subsequent discussion about murex.

⁴ Interestingly, when an attempt was recently made to recreate the Gemara's primary test in accordance with the positions of Rashi and the Rambam,

In marked contrast to these speculative arguments, we are faced with the primary argument supporting murex: the incontrovertible fact that a blue dye, that is indistinguishable from indigo, has been extracted from an indigenous sea creature; and this finding can be reconciled with the Gemara **only** if this dye is, in fact, genuine *tekhelet*, since the Gemara's discussion rests on the presumption that if a blue dye is determined not to be indigo, it must be *tekhelet*.

Indeed Rav Gershon Henoch Leiner, the Radzyner Rebbe, explicitly states that: "... if after searching we discover blood of any species of *hilazon* that can be dyed the color of *tekhelet*, which maintains its beauty and does not fade, then we can certainly fulfill the mitzvah of *tekhelet* without any doubt..."⁵ There is absolutely no doubt that murex trunculus satisfies this criterion.

It is worth adding that today, *poskim* as diverse as Rav Hershel Schachter, Rav Mordechai Avrohom Katz, Rav Zalman Nechemia Goldberg and Rav Amram Oppman (of the Eidah HaChareides in Jerusalem), among many others, advocate the use of murex *tekhelet* for the proper fulfillment of

the murex dye was found to *pass* both tests. See Dr. Baruch Serman, "The Source of Tekhelet: Response to Dr. Singer," *Journal of Halacha and Contemporary Society*, No. 43, Spring 2002, p. 116, accessible on the web at tekhelet.com/pub.htm.

⁵ *Maamar Sefunei Temunei Chol*, p. 18.

the mitzvah of *tzitzis*.

Moreover, Rav Katz relates that when he discussed this issue with “Gedolei Yisroel,” many asked him to procure murex *tebbelet* for their personal use (even though some were not prepared to take a public stand on the issue).⁶ This position is hardly surprising, as the Radzyner Rebbe emphatically states that when *tebbelet* is available but there is uncertainty as to whether it is genuine, we are **required** to use the *tebbelet* in our *tzitzis* in order to avoid the possibility of transgressing one positive biblical commandment and two negative ones.⁷ Moreover, the Gemara relates in *Menachot* 41(a) that when R’ Katina failed to wear *tebbelet* because he wore linen garments (which are exempt from *tebbelet*) in the summer and rounded (rather than squared) woolen garments in the winter, an angel warned him of dire punishment that could befall him for evading this mitzvah.

The *Halachah Berurah* newsletter seriously misleads the Torah community by presenting an ostensibly comprehensive analysis of the *tebbelet* issue that makes no mention of the primary argument identifying the murex-dye as *tebbelet* and the supporting citation from the Radziner Rebbe’s *Maamar Sefunei Timunei Chol*,⁸ presented above.

Similarly, HB disregards all of the *poskim* who believe that murex *tebbelet* should be worn and implies that the only authoritative proponent of murex *tebbelet* is an unnamed Israeli organization that “**markets** [sic] blue woolen strings to be [used] as *tebbeiles*.” However, a visit to this organization’s on-line library (at www.tekhelet.com), which contains dozens of *tsibvos* and scholarly articles on this topic, readily demonstrates the breadth of interest and support that this issue has actually engendered.

The possibility of properly fulfilling a *mitzvat aseh d’oraisa k’tikinah* after a 1,300-year hiatus is, without a doubt, an issue of enormous significance to *b’nai torah* and *yirai shomayim* of all stripes.

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IN THE PREVIOUS issue of *Hakira*, Dr. Menachem Epstein writes in response to an article that appeared in *Halacha Berurah* regarding the identity of the *chilazon*. Dr. Epstein’s defense of the murex trunculus position is primarily a rehash of old arguments made by the Ptil Tekhelet Foundation (PTF). Essentially all of these claims can be found in articles in the PTF’s online library, and many, if not most, of these claims are

⁶ *Ohr Yisroel*, 1997, issue # 10, p. 14, accessible at tekhelet.com/pub.htm.

⁷ *Ayn Hatebbeiles*, p. 215

⁸ This is particularly surprising since HB cites the Radziner Rebbe’s views

throughout the article and even paraphrases the above citation at the beginning of the article (page 3, column 1), but then simply ignores it in the section that deals with murex.

factually incorrect or misleading. The PTF online library is quite extensive, and it includes my article in the *Journal of Halacha and Contemporary Society*⁹ along with the PTF's response.¹⁰ It does not, however, include my follow-up in the same journal that documented many of their errors. Proper debate is greatly inhibited by the vast amounts of misinformation promulgated by the PTF. Some years ago I went to the trouble of checking the original sources they cite. I read Pliny the Elder, and I read the articles in the archeology and chemistry journals. I contacted the leading secular scholars in these subjects to see if the PTF's claims were true. They aren't. While a complete documentation of their errors is beyond the scope of this letter, and some of this is available to the readership through online sources,¹¹ with the editor's indulgence I will touch upon a few of the most important areas of concern in the article by Dr. Epstein.

Dr. Epstein states that the main reason for believing the murex trunculus is the *chilazon* is the color of the dye produced by it. He says that the *Gemara* states that there is an exact look-alike for *techeilet*, and it comes from *k'la ilan*, which is

identified by many *Rishonim* as the indigo plant. Therefore, *techeilet* is exactly the color of indigo, and since the murex trunculus makes indigo, it is a powerful proof that the murex trunculus is the *chilazon*. This logic is faulty. One problem is that the *Gemara* (*Menachot* 42b-43a) does not actually state that the two colors are identical, nor is it clear that *k'la ilan* means unadulterated indigo. The *Gemara* says that *K'la ilan* can imitate *techeilet*. This could mean that the range of colors produced by natural dye processes, and variations in the dipping techniques, is such that indigo can look like *techeilet*. It could also mean that *techeilet* can be imitated by a slightly altered indigo, such as the addition of a small amount of red kermes dye to produce a purple tinge. In any case, one cannot conclude that *techeilet* is the color of indigo. Indeed, *Tosafot* say this explicitly. *Tosafot*¹² state that indigo resembles the sky, but only slightly resembles *techeilet*, and that one can distinguish indigo from *techeilet* before sunrise. Thus, according to *Tosafot*, *techeilet* is *not* the color usually associated with indigo. This removes the main argument for *murex*.

Dr. Epstein writes that the "second argument for the authenticity of the *murex* is from the *Gemara*'s statement (*Shabbat* 75a) that the *chilazon* must be kept alive while the blood is extracted in order for the dye to turn out right." He then notes that murex dye degrades once the murex has been

⁹ Mendel E. Singer, "Understanding the Criteria for the *Chilazon*," *Journal of Halacha and Contemporary Society*, vol. 42, Sukkot 2001.

¹⁰ Baruch Sterman, "Reply to Dr. Singer," *Journal of Halacha and Contemporary Society*, Vol. 43, Pesach 2002.

¹¹ See, for example, www.chilazon.com.

¹² *Chulin* 47b.

dead for 2 hours. Since the process of extracting the dye from a murex snail is a matter of seconds to minutes, there would be concern to keep it alive only if the dye would be greatly compromised within minutes, not hours. Dr. Epstein seems to admit this in saying, “this may be true, but both Pliny’s and Aristotle’s descriptions of the murex state that the dye must be obtained from live snails.”¹³ Yes, but Pliny and Aristotle also state the reason why the *murex* must be kept alive—it is because when it dies it releases the dye substance and much of it will be lost. It is a practical matter of losing much of the precious little dye substance in each *murex* snail. The P’til Tekhelet Foundation often cite this Pliny and Aristotle as if it supports their position, but they cite only the first half of the sentence without supplying the rest of it¹⁴—which opposes the murex position.

Dr. Epstein, like the P’til Tekhelet Foundation before him, claims that the ancients knew that you could dye blue from the murex snails.¹⁵ This is false. There is no historical evidence whatsoever to support this claim. It is based on a quote from Vitruvius that is taken out of context. Vitruvius doesn’t say that the murex was used to dye leaden blue, as claimed, but that depending on the region it pro-

duced various shades of purple: black, leaden blue, violet and red.¹⁶ Quite obviously he was not suggesting that murex was used to dye black. He is referring to shades of purple, not different colors. This is how it is cited in modern works,¹⁷ and also by Rabbi Herzog, who further demonstrates that Vitruvius is not even speaking of murex trunculus but of other *murex* snails.¹⁸

Dr. Epstein’s third argument for the murex is another argument used by the P’til Tekhelet Foundation, and is also erroneous. The *Raavya* on *Berachot* (9b, *siman* 25) quotes a *Yerushalmi*: “between *techeilet* and *karti*, between *porpurin* and *parufinen*,” and *Raavya* adds, “which is a coat that is called in latin *purpura*.” Dr. Epstein, like the PTF before him, suggests that the simple explanation is that the items in the 2nd half of the statement correspond to the colors in the 1st half, thus equating *techeilet* with *porpurin* and *karti* with *parufinen*. Dr. Epstein wonders why the *Halacha Berurah* article rejected this “simplest” explanation. Perhaps because it doesn’t work. The *Raavya*’s statement about the coat with pur-

¹³ Aristotle, *Historia Animalium*, Book V, ch. 15; Pliny the Elder, *Naturalis Historia*, Book 9, ch. 60.

¹⁴ See for example Serman, *ibid*.

¹⁵ A claim also made in Serman, *ibid*.

¹⁶ Vitruvius, *De Architectura*, Book 7 Chapters 7–14.

¹⁷ Daniel V. Thompson, *The Materials of Medieval Painting* (New Haven, 1936), pp. 156–158.

¹⁸ Rabbi Isaac Herzog, “Hebrew Porphyrology,” in Ehud Spanier, ed., *The Royal Purple and the Biblical Blue: Argaman and Tekhelet* (Jerusalem, 1987), pp. 26 and 34.

ple dye is referring to the word immediately preceding it—*parufinen*, not the word before that, *porpurin*. This is abundantly clear from a look at an ancient Greek dictionary where we find that *parufaino* is defined as a coat with a purple stripe.¹⁹ Furthermore, *porpurin* may not be referring to purple at all. The root is undoubtedly from *purpura*, but that root is often used for reds, as well as for purples. For example, even today, porpurin and porphyrin refer to red pigments. I would suggest that a more logical explanation of the *Yerushalmi* is that the time at which one can distinguish between *techeilet* and *karti* is the same time as when you can tell the difference between red and purple.

Dr. Epstein states that the strongest argument against *murex trunculus* involves the chemical tests from the *Gemara* (*Menachot* 42b). Since the dye produced by the Ptil Tekhelet Foundation from the *murex trunculus* snail is itself indigo, shouldn't snail *techeilet* fail any test that plant indigo fails? Dr. Epstein quotes a response from Dr. Baruch Sterman of the Ptil Tekhelet Foundation. Dr. Sterman makes a speculation, and then quotes a Nobel Prize-winning chemist as saying that his proposition is theoretically possible. Aside from the issue that being "theoretically possible" does not imply there is any basis for believing it, and putting aside the issue that this chemist

told me that he has no expertise in this area and was merely speaking of a hypothetical possibility, there are fundamental problems with Dr. Sterman's response. Dr. Sterman says that "though we are not one hundred percent certain, it would appear that snail *techeilet* and indigo were reduced in different ways," and he refers to the method of chemical reduction described by Pliny, which differs from what was used to reduce plant indigo. In fact, Pliny is speaking of reducing *murex* dye that is still purple, not reducing snail-derived indigo. No historical sources, including Pliny, claim that *murex* was used to dye blue, so claims made about how *murex* indigo was reduced in ancient times is purely fiction. Snail *techeilet* is made from the mucus of the snail, and bits of snail meat may be mixed in. Could the *Gemara's* test be based on having sufficient snail meat to produce a different reaction, namely to inhibit some process created by the *Gemara's* tests? Perhaps this is a theoretical possibility, but it is absurdly unlikely for several reasons. One, the amount of snail meat would be small and vary from batch to batch, so a test would likely be unreliable. Second, it ignores the intent of the tests. In one test, we have a concoction based on fermented urine. Not coincidentally, indigo was chemically reduced via a fermentation vat often made up primarily of fermented urine. When Rabbi Herzog consulted with renowned chemist Dr. A. C. Green, the chemist recognized the process as a chemical reduction.

¹⁹ Liddell-Scott-Jones *Lexicon of Classical Greek*, <http://www.perseus.tufts.edu>.

This is also the conclusion of Dr. Irving Ziderman, who did much of the pioneering work on the murex trunculus theory. When indigo is reduced, its color changes to yellow, which would indicate failure of the *Gemara's* test. The test is merely a replication of a fermentation vat, and will reduce indigo. In other words, the test is designed to recognize indigo, which is understood to be *ke'la ilan* (or at least its primary ingredient). Dr. Sterman has cited research that shows snail meat has the property of aiding chemical reduction. This would make snail *techeilet* reduce easier, lose its color and fail this test. Furthermore, one can fail this test without complete reduction. The test requires only that the blue color be adversely affected. Thus, for any other substance that gets mixed in with the snail mucus, it would have to be able to completely inhibit the reduction process for snail indigo to pass the test.

Dr. Epstein also addresses the common argument that the murex trunculus does not meet the Talmudic description of *gufo domeh l'yam*. His defense, also offered by the P'til Tekhelet Foundation in its writings, is that green, or blue-green algae cover the shell (along with everything else in the area), so an observer would say that the color of its shell is the color of the algae that he sees. This suggests that *Chazal* were sloppy in their choice of words, and leaves one wondering why *Chazal* would have made such a statement. If everything else in the area is also covered with algae, why is this feature

noteworthy or even helpful? If it is said to describe an area covered with algae, then say that. I would further argue that someone seeing a snail covered with blue-green algae like everything else in the area would say not that the shell is blue-green, but that the color of the shell cannot be determined because the algae is covering it. If a person sees a car that is completely covered in a blue tarp, would he describe the car as being blue? Now imagine a parking lot filled with various cars, vans, buses and motorcycles—all covered by blue tarps. Would anyone describe a particular white car as being blue because people will see it with a blue tarp over it, like everything else?

The subject of identification of the *chilazon* is an important one. Unfortunately the debate is marred by the proliferation of misinformation originating with the P'til Tekhelet Foundation. I hope that from these few illustrative examples, the readers will recognize the lack of credibility of the P'til Tekhelet Foundation and view with proper skepticism any claims they make.

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Menachem Epstein responds:

1. KELA ILAN

The Gemara is absolutely clear that *kela ilan* was successfully used to fool people (Bava Metzia 61b), and that it was impossible to differentiate between *techeiles* and *kela ilan* without a chemical test (*Menachos* 42b-43a). Thus, Dr. Singer's claim that "the Gemara does not actually state that the two colors are identical" is highly unlikely. Dr. Singer adds, "nor is it clear that *k'la ilan* means unadulterated indigo." However, the Gemara refers to the wearers of fake *techeiles* simply as "one who wears *kela ilan*." The *Rishonim* then identify *kela ilan* simply as indigo. (Five sources are mentioned in the article.) To hypothesize about adulterations is pure conjecture with no basis in the sources. Moreover, when we couple this with the fact that the Murex produces the exact same molecule as *kela ilan*, an occurrence whose probability is practically *nil* if in fact neither of them is *techeiles*, Dr. Singer's conjecture becomes absurd.

The Tosafos in *Chulin* identifies *karsi* as the color of indigo. Since *karsi* and *techeiles* have different colors, Dr. Singer concludes that "according to Tosafos, *techeiles* is not the color usually associated with indigo." A close look at Tosafos will show that they do not bring any source that identifies the exact color of *karsi* as indigo. As a matter of fact, the sole basis of Tosafos for the connection between *karsi* and indigo is the very

similarity of *karsi* to *techeiles*! Tosafos' point is only to identify both *techeiles* and *karsi* as indigo-like colors, as opposed to the colors yellow and green, and he does not necessarily mean that it is fully indigo. (Even if Tosafos do mean that *karsi* is indigo, they are a minority opinion, relative to that of the many other *Rishonim* we have quoted who believe that *techeiles* is an exact match to the color of indigo. Moreover, Tosafos did not have *techeiles* or the evidence of the matching molecule that we spoke of.)

2. LIVE SNAILS

Chazal relate that the dying material for *techeiles* should be removed from a live *chilazon*. Pliny says the same about extracting dye from the murex snails. This alone serves as evidence that we are talking about the same dying process, no matter what reason the Greeks give for the process. There could be more than one reason for using live snails, and *Chazal* and the Greeks could record different reasons, and perhaps the Greek writer was not even aware of the most important reason for a practice that had existed many years. Dr. Singer's citation of Pliny—"because when it dies it releases the dye substance and much of it will be lost"—tells us only that they saw the process fail when they brought a dead murex to the dyer. *Chazal* say as well that sustaining the life of the *chilazon* while gathering its blood-dye ensures that the dye will be of better quality (דליציל צבעיה) or the dye

will be successful (דלצלה צבעיה). In fact, both ancient sources mean the same thing. With modern science we understand that nothing is in fact physically lost and express the same idea by saying that the dye degrades starting at the moment of death. Were they concerned only with the animal excreting the dye in the wild as Dr. Singer understands, they would be more likely to capture and kill it in a bowl to save any of the liquid it excreted at death, rather than extract the dye while the animal was alive and squirming and risk losing it at that time.

3. BLUE FROM THE MUREX

Dr. Singer claims that there is no proof from the quote from Vitivrius that it was known in the ancient world that a blue dye could be extracted from the murex, and the quote means only that bluish-purple could be produced. That is how we understand the source as well. Indigo is considered a shade of purple, bluish-purple, and immediately borders violet on the rainbow. Sir Isaac Newton placed it between blue and violet in 1766. In any case, seeing how easily murex trunculus turns out a blue color just by exposure to sun rays at the time of reduction, it is hard to believe that the great dying experts of the ancient world were unaware of this phenomenon.

4. RAVYA

Dr. Singer quotes the *Yerushalmi* brought by the Ravva “between

techeiles and *karti*, between *porpurin* and *parufinen*,” and the Ravva’s additional clarification “which is a coat that is called in latin *purpura*.” Dr. Singer rejects what we consider the simple reading of the *Yerushalmi* that the items in the 2nd half of the statement correspond to the colors in the 1st half, primarily because “the Ravva’s statement about the coat with purple dye is referring to the word immediately preceding it—*parufinen*, not the word before that, *porpurin*.” This point is in any case totally irrelevant to the issue at hand. Our argument in favor of murex *techeiles* was not from the additional statement of the Ravva about the *purpura* coat but rather from the original statement of the *Yerushalmi* that brings *porpurin* and *parufinen* as examples of *techeiles* and *karsi*, respectively.

(Moreover, Dr. Singer’s claim that Ravva’s comment "מעיל שקורין בלע"ז פורפירא" is explaining the word פריפנין rather than the word פורפירין—its almost exact match—would call for yet another major coincidence. What we can gather from the Greek (not Latin לע"ז) definition that Dr. Singer has found, and what is consistent with the words of Ravva, is that the *Yerushalmi* is speaking of two coats that have the color of וכרתי. Since in the times of the Gemara, most people would not necessarily have known what the colors mentioned in the Mishnah were with exact precision, the *Yerushalmi* refers to two coats that had stripes with the exact shades of these two colors. It would be a major coincidence for the word פורפירין to show

up here, if it was not related to *techeiles*, but Dr. Singer's entire approach is to assume coincidence after coincidence, rather than to recognize that a lost *mitzvah* can now be restored.)

5. CHAZAL'S TEST

Dr. Singer does not dispute the point that the murex and indigo dyes were likely reduced in different ways. Thus we have a plausible explanation for the difference between these two in regard to *Chazal's* test. It is true that all this is only a possibility, but since we have a working explanation of how snail and plant indigo might react differently to *Chazal's* test, this argument cannot be considered a refutation of murex *techeiles*. Dr. Singer's claim, that the known ancient reduction processes refer only to purple dyes, is completely irrelevant to this issue. Today the purple and blue dyes of the murex are reduced in the same manner, so why should we assume that it would be different back then?

Also, Dr. Singer concentrates on the fact that urine was used in the testing and feels he understands why it was used and thus claims if snail meat was used it would not help to pass this test. Even if this is so, we must remember that other tests were also used and apparently steps were taken by the frauds that enabled them to pass the urine test alone. The snail meat may have prevented failure from some other test that *kela ilan* could not pass.

6. גופו דומה לים

Dr. Singer claims it would be sloppy language for *Chazal* to call the *chilazon* by the color of the algae. I disagree. If it were discovered that the color of orange and apple peels were due to tiny microorganisms that can be removed only in a laboratory, we would definitely not start referring to these fruit as white! Furthermore, Dr. Singer asks, "If a person sees a car that is completely covered in a blue tarp, would he describe the car as being blue?" This *mashal* must be modified on two points. First of all, please allow me to compare algae (that can't be easily washed or scraped off) to paint, as opposed to tarp. Secondly, *Chazal* **do not** describe the snail's body as being **blue-green, but** rather describe it as looking like the seabed!²⁰ Using this *mashal*, upon seeing a car parked in a parking lot in which everything was painted blue, I would definitely describe the car as looking like the parking lot. This is no sloppiness of language at all.

Dr. Singer asks, "If everything else in the area is also covered with algae, why is this feature noteworthy or even helpful?" Dr. Singer's argument assumes that this statement of *Chazal* was intended to identify the specific *chilazon* of the *techeiles*. This is obviously not the case. *Chazal* knew what the *chilazon* was, and in the *aggadic* comments made in *Menachos* 43b and then

²⁰ The word *yam* can mean seabed as in *mayim layam michasim*.

Menachos 44a, the Gemara first explains how the color of *techeiles* is like that of the sea *שהתכלת דומה לים*—and later that the *chilazon* from which it is taken also outwardly resembles the sea. As with all *aggadic* statements, we must look deep for why *Chazal's* statement here is “noteworthy.” I understand that *chazal* are contrasting the outward appearance of the snail, which appears to be a lifeless part of the seabed, with its inner nature as a living, breeding creature of the sea—*גופו דומה לים וברייתו דומה לדג*. Perhaps the message is that when looking at our *tzitzit*s, and remembering our Creator, our focus begins with the vastness, the unknown and the unfathomable associated with the sea.

Editor's Note:

We are grateful to Dr. Singer and Dr. Epstein for presenting both sides of the *murex/techelet* argument so clearly.

It is our hope that readers can now better judge for themselves whether *techelet* wearers have been misled by an overzealous Ptil Tekhelet Foundation blinded by the thrill of its find, or whether it is others who are being misled by those who are willing to forego the fulfillment of one of the 613 *mitzvot* since the impetus to fulfill this one came from the scientific and modern orthodox camp rather than their own.

Chronology Problem

I ENJOYED the article in Vol. 3, *A Y2K Solution to the Chronology Problem*. However, having written a book on the subject (*Jewish History in Conflict*), I feel compelled to respond.

The authors claim that R. Yose knew the true chronology of the Persian period but purposely shortened it. He did this so that the publication of the Mishnah would coincide with the end of the 2000-year period of Torah referred to in *Tanna De-vei Eliahu*.

This approach ignores the fact that:

-R. Yose places Achashverosh in the wrong period (before the Darius in whose reign the Temple was built). In fact, Achashverosh is Xerxes, the king who reigned immediately after the Darius in whose reign the Temple was built. See the discussion in my book, pp. 166-67. (Xerxes' queen was called Amestris by the Greeks. Most likely, this is Ester. The “is” at the end is just a Greek addition.)

-R. Yose assigns a figure that is 29 years too long for the length of the First Temple period. According to conventional chronology, the year when the First Temple began to be built (the 4th year of Solomon) was 967 BCE. (See the Sept./Oct. 2001 issue of *Biblical Archaeology Review*, pp. 32–37). The First Temple was destroyed in 586 BCE. 967 minus 586 equals 381. Yet R. Yose assigns 410 years to the First Temple period.

All of this suggests that what we have in *Seder Olam* is not a purposeful rearrangement of conventional chronology, but an erroneous chronology by R. Yose based on insufficient sources: the limited data provided in the Bible and insufficient other sources.

It is critical to point out that the book of Ezra mentions only 4 Persian kings: Koresh, Daryavesh, Achashverosh, and Artahshasta. Daniel 11:2 also supports a chronology of only a few Persian kings.

The correct approach to R. Yose's Persian period/Second Temple period chronology is the one outlined in my book (pp. 128–137), first suggested by Azariah de Rossi in the 16th century, and followed by many others thereafter. Briefly, R. Yose was trying to make sure that his work included a complete chronology that ran all the way to the destruction of the Second Temple, and he was trying to base this chronology mainly on the Bible. The only Biblical verse that could help him out in this regard was Dan. 9:24. Once he decided to rely on Dan. 9:24 and the 490-year period referred to there (which he interpreted as running from the First Destruction to the Second Destruction), he was left with only 40 years for the period from the rebuilding of the Temple until the beginning of the Seleucid era (490 less 70, and less the 380 years that ran from the Seleucid era to the Second Destruction). Of necessity, the Persian period had to be a short one. R. Yose was even forced to equate Daryavesh with Artahshasta, even though it is clear

from Ezra 6:14 that these are 2 separate kings. Did R. Yose know that Daryavesh was not identical with Artahshasta and that the Persian period was actually longer than he himself was assigning to it? Probably. Did he know that it was much longer than he was assigning to it and included numerous Persian kings after Daryavesh? Unlikely. The burden is on those asserting that he did to prove their assertion.

The approach based on Dan. 9:24 requires a little more elaboration. In particular, we must explain why R. Yose assigned to the 490 years of Dan. 9:24 the endpoints that he did. This elaboration is contained in my book.

Farfetched theories can be considered only if there is a problem with the simple solution. But there is no real problem with the simple solution here, in its basic outline. R. Yose cites Dan. 9:24 in his work as his source for assigning 490 years to the period from the First Destruction to the Second Destruction. There is insufficient reason not to believe him.

Mitchell First
Teaneck, NJ

The authors respond:

We thank Mitchell First for his comments on our article "A Y2K Solution to the Chronology Problem." The crux of his argument is expressed in his assertion that:

"Farfetched theories can be considered only if there is a problem with the simple solu-

tion. But there is no real problem with the simple solution here, in its basic outline.”

Of course there are problems, and we outlined them extensively in our paper, e.g.:

- “Without resorting to arguments about the infallibility of the Tannaim and Amoraim, it is simply not credible to think that less than one century after the destruction of the 2nd Temple, the חכמי התלמוד had inadvertently lost track of about one third of the time span that the 2nd Temple existed.”

- How could R. Yose get the dating of the Temple wrong without affecting the timing of Shmita and Yovel?

In his letter to the editor, Mr. First says:

“Did R. Yose know that Daryavesh was not identical with Artahshasta and that the Persian period was actually longer than he himself was assigning to it? Probably.”

If we agree that R. Yose knew that his numbers were wrong—even by a small amount—how do we resolve his being willing to assert these numbers and ignore their effect on Shmita and Yovel? Moreover, why would R. Yose want to disseminate something he knew was wrong?

- How do we explain the Amoraim in the Gemara who clearly were aware that R. Yose’s chronology was missing a considerable amount of time?

In the absence of answers to these questions we feel that de

Rossi’s solution is untenable. Moreover, we do not find de Rossi’s answer simple and ours farfetched. We spent much time in the paper showing that the Gemara adjacent to the one with R. Yose’s world chronology viewed world history as consisting of three distinct 2000-year intervals. We simply extended this idea to R. Yose’s chronology.

While we feel comfortable assuming that R. Yose knew the time span of the Second Temple, we have no reason to believe that he or his contemporaries were experts in Persian history and the Persian monarchy. Clearly, when you remove over 150 years of history, JC will have major conflicts with CC as First pointed out.

Finally, with respect to *Seder Olam’s* chronology for the 1st Temple differing from the one presented in the *Biblical Archeology Review*, it is reasonable to believe that R. Yose used the Tanakh as his source and any discrepancies are inadvertent and due to “limited data provided in the Bible and insufficient other sources.” An error of 30 years in the First Temple period that had no, or limited, halachic ramifications for R. Yose, is not comparable with an error of over 150 years in the Second Temple period that had halachic ramifications in his era. The latter, we therefore concluded, is more likely deliberate.

Chronology Problem

CONGRATULATIONS for putting together another superb volume of

Hakira. I thought that the authors of “A Y2K Solution...” tackled a very difficult problem and came up with a very creative solution. However, I have a few questions regarding their solution:

1. The authors propose that R. Yose and Rebbe changed the chronology to let them publicly proclaim that they were at the threshold of a new era. Who was aware of the “true” chronology, and whom was the revised chronology most intended to influence: The Tannaim? The masses?

2. Presumably, R. Yose and Rebbe were relying on the principle of *bora-at sha-ab* to change the way the Oral Laws were to be taught and transmitted from generation to generation. That principle allows the leading Rabbis of a generation to nullify a commandment if they are convinced that its continued fulfillment would be a detriment to Judaism. If R. Yose and Rebbe were convinced that writing and disseminating the Mishnah was the correct thing to do, shouldn't they have just done their best to introduce their change in a more straightforward way, relying on their own abilities as well as the help of *Hashem*? Didn't they have enough faith in *Hashem* to believe that He would help them succeed without the necessity to resort to changing the chronology?

3. I've always been a strong proponent of expounding on the pasuk “*Tzedek tzedek tirdof*” as “*Betzedek tirdof abarei tzedek*,” i.e., do right-

eousness in a righteous way, the ends do not justify the means. I am uncomfortable with the idea that great leaders with the stature of Rabbenu Haqadosh and R. Yose would purposely change history in order to garner support for the acceptance of Mishnah.

The above notwithstanding, it is quite evident that a great deal of work went into finding and explaining the many passages that support the Y2K theory, and the authors did a fine job presenting their arguments.

Raymond Arking
Brooklyn, NY

The authors respond:

We appreciate the very thoughtful letter and in responding would like to take the opportunity to clarify some issues that might not have been fully explained, and dispel inadvertent misconceptions.

Although our paper stresses how the change in chronology was used to support the acceptance of the Mishna, we suggested that the manipulation of the dates began considerably before the completion of the Mishna and was undertaken for a different, but very important reason. Note that according to the CC, the 2nd Temple was destroyed in 3994 (Figure 3). Thus, as the year 4000 approached, despair about the future of the nation amongst everyone must have been great. As explained in the paper, after 1500 years of living with a Mishkan-Temple/Torah combina-

tion (with only a 70-year break after the destruction of the 1st Temple), the nation now had only: relentless persecution, no independence, no Temple, and no apparent future. Faced with a despondent nation, leaders of the post-Temple destruction era had to do something. In the years immediately following the *Churban*, leaders like R. Yochanan ben Zakai directed their efforts toward saving the remaining Jewish scholars and revolutionizing the observance of Mitzvos in a non-Temple environment. It was left to the leaders of subsequent generations to deal with the problem of sagging public morale.

We do not believe that common people of that era kept track of time as closely as we do today. In *Avodah Zara 9a* immediately following R. Yose's history chronology, the Gemara offers a formula for transforming from/to a *Minyan Shtarot* date (Greek) to/from a *Churban Bayit* date. The Gemara says that this formula is meant to help scribes and rabbis convert from one system to the other, and Rashi asserts that they would each be knowledgeable on the 100s and 1000s digit but less so on 10s and units. We assume that the common man was even less knowledgeable and probably would not have known offhand when the Temple was built. The point, however, was for the Rabbis to preemptively deal with the people's anxieties vis-à-vis the destruction of the Temple and put things in perspective in a way they could appreciate and understand without forsaking Judaism.

Our paper demonstrates from a number of Gemaras that in that era, history was viewed as working in cycles, and guidance was sought in trying times to determine where their era fit in this cosmic system. In this vein we showed how predictions about *Masbiach* are products of such analysis (be the critical period 400 years, 850 years, 1000 years or 2000 years) and not necessarily of Eliyahu confiding secrets to any particular individual. If the idea of repeating cycles appealed to the people, then this was the vehicle that the *Chachamim* would use to get through to them.

We have no doubt that in the same way that many in the modern world viewed the recent year 2000 as special because it began a new millennium, so the people in the ancient world viewed the year 4000 as a year of dramatic consequence. Certainly, this is the import of the statement of *Tanna D'Bei Eliyahu*. By initially dropping 166 years, the *Chachamim* perhaps figured to uplift the spirits of the people and buy some time to the next millennium, in order to see whether a turnaround in the fortune of the Jewish people would take place, as it did after *Galut Bavel*. (See page 111 of our article and the discussion of *Rosh Hashanah 19b*.) As the year 4000 JC approached, the fortunes of the people in general had not changed, but the *Chachamim* saw the upcoming millennium as a way of touting the completion of the Mishna and equating this work with the giving of the Torah itself. This represented a momentous national event for the Jewish peo-

ple and portended a bright future for the upcoming millennium. Thus we view the change in chronology as a noble attempt by the leadership to bolster the sagging morale of an oppressed people. When a second opportunity then arose to support a new system of learning, one that would further uplift the morale of the people, they went along with it. Yet they kept the truth alive in the leaders of the subsequent Amoraic generations who demonstrated that they were aware of CC.

We feel that our analysis in the article offers ample support for our conjectures. The proper observance of *shmitah* fits best with a second Temple that stood 166 years longer, and the words of at least three *Amoraim* are understandable only if they too included the 166 years. With these markers, the question becomes not *if they knew about the years* but *why did they act as if they did not?* We are suggesting that the most reasonable explanation is that changes were introduced to allay the fears of the people about the future of Judaism after the destruction of circa 70 CE, and to give them further encouragement and renewed direction with the introduction of the Mishna. We do not believe that anyone can deny that Rabbinic Judaism as we know it with its text (Mishna and Gemara), detail and flexibility was the salvation of the Jewish people for all these many years in exile. Without a Temple and without a State, Judaism would not have survived the last 2000 years were it not for the Talmud.

With respect for the need to “promote” the Mishna rather than just relying on its acceptance as *bora-at sha-ab*, we once again stress Rambam’s introduction to *Mishneh Torah*, which describes how the Mishna project went against the basic way Torah was transmitted for the first 1500 years after Sinai. Even in the face of the “need” factor that Rambam stresses, we cannot see how this new style of “learning” would not have been a hard sell. In *Hilchos Mamrim*, Rambam says that any *bet din*, even one of lesser stature, could disagree with a prior *bet din* of greater stature when dealing with a decision based on logical deduction. This ended with the completion of the Mishna. From then on, the era of free-roaming debates was over, and future generations were to be limited in their decision making by what the Tannaim said. It was only someone of Rebbe’s unrivaled stature (see for example *Horiyos* 11b where Rebbe compares himself to a King, and page 83 of our article where we discuss *Sanhedrin* 36a) who could have succeeded in creating this new style of Torah study and he utilized any support he could muster to ensure the success of the project.

Epitaph Acronym ע"ה

THANK YOU for publishing Rabbi Brojde’s thoroughly convincing article concerning the abbreviation ע"ה.

In the course of the article, the author speculates about the origins

of the phrase עליו השלום and finds only an “echo” in Isaiah 57. My son, Rabbi Mordechai Sonnenschein, pointed me to the Gemara in *Shabbos* 152b:

... אף הקב"ה על גופן של צדיקים אומר "יבא שלום ינוחו על משכבותם" ועל נשמתן הוא אומר "והיתה נפש אדוני צרורה בצרור החיים." על גופן של רשעים הוא אומר "אין שלום אמר ה' לרשעים" ועל נשמתן הוא אומר "וזאת נפש אויביך יקלענה בתוך כף הקלע."

Not only does this seem to be the conceptual origin of the phrase, it also explains its structure. We conventionally greet each other with שלום עליכם, yet here we don't use שלום אֵלַי or אָתוּ. Furthermore, instead of saying עליו השלום, we reverse it into an apparently awkward “upon him is—or, shall be—[the] peace.”

According to the Gemara, this is precisely in accordance with what *Hashem* does. Unlike שלום על גופן, עליו השלום, עליכם, nor prayer. It is recognition. על גופן, upon the צדיקים, *Hashem* pronounces שלום. (See Maharsha to *Kesubos* 104a, where he discusses this concept of שלום and שלמות following מיתה.)

Aaron Sonnenschein
Brooklyn, NY

Parsha Management

THE AUTHORS of “Parsha Management—Doubling, Halving, Accuracy” make a good argument for every community choosing the arrangement that “they felt made the most sense.” Good point. The Talmud even goes a step further, as Rabbi Yochanan in *Shabbat* records that two sages in the same community (Sidon) lit Chanukah candles differently, one according to Bet Hillel, the other according to Bet Shammai—and there is no criticism, explicit or veiled, of either in the *sugya*.

The relative insularity of pre-modern Jewish communities was a factor in the development of varying community *minhagim*. With increased interaction between communities—including Israel/Diaspora travel on an unprecedented scale—the staggering of *parshiyot* does create problems that are not beyond solution.

One of these—not paramount, but very annoying—is that the one-week delay prevents many Israelis from receiving (and giving over) some of the excellent *Parashat Hashavuah sheurim* that emanate from *Chutz Laaretz*.

Kadish Goldberg
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