

## References

- Franklin, N. 2004. Samaria: From the Bedrock to the Omride Palace. *Levant* 36:189–202.
- Reisner, G. A., C. S. Fisher, and D. G. Lyon. 1924. *Harvard Excavations at Samaria, 1908–1910*. 2 vols. Cambridge, MA: Harvard University. [*HES I* and *HES II*].

## Response to Norma Franklin's Review

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I appreciate the opportunity to respond to Franklin's review of my recent book, though I am disappointed that she did not see more value in it. We disagree on a number of decisions about the volume's contents, several interpretive questions, and readings of previous researchers' work. I will explain the logic behind my decisions below. Franklin has already offered her thoughts in the review above.

Franklin begins by criticizing my inclusion of a brief account of the biblical story; a tribute to Jakob Schiff, without whom there would have been no Harvard Project (Appendix D); unpublished field sketches of the ostraca from Reisner's diaries (Appendix E); and excerpts from the field records concerning the discovery of the ostraca (107–10). Unlike Franklin, I believe that this material greatly enriches the volume and enhances its usefulness.

Franklin then reveals what she takes as "two principal errors" that "severely detract" from my study. The first matter involves Ahab's "Main Wall" ("Wall A"). She correctly notes that the architecture in my figure 25 lies east of Wall A, not west of it (although the pavement "p" actually begins ca. 7–7.5 m east of the wall, not 13 m as she claims). The context in question relates to Room 13 on *HES II*: Plan 5 (see my figures 22, 25; also *HES II*: Plan 8c). Reisner's narrative (*HES I*: 58) places these features just east of the complex labeled 776. So Room 13 lay in the northern portion of Reisner's Grid G.12, while 776 lay immediately west in Grid F.11 (and encroached on F.12).

Though my figure 25 lay slightly east of Wall A, it shows a wall-floor relationship apropos to my larger discussion. In my narrative, I make clear that figure 25 neither connects to nor directly represents Wall A. Emending those

allusions as follows would tighten my three references to this graphic: (1) page 26b: "(in Ch. 2, fig. 25, compare another, nearby wall—east of Wall A—that Reisner also attributed to Ahab)"; (2) page 38: "Only a single drawing . . . mudbricks (cf. the area east of Wall A, in fig. 25, for a wall-floor relationship similar to the published description of the western face of Wall A itself)"; and (3) page 96b: "Fig. 25" should be corrected to "Fig. 26." In any event, Figs. 25–26 reflect similar circumstances concerning walls and floors attributed to Ahab, and the fact that my figure 25 lay in Grid G.12 while figure 26 shows F.12 does not "nullify" the relative value of the two drawings in the larger context of my discussion.

Franklin identifies my supposed second "error" as involving the foundations of the Ostraca House (OH) and the question of whether an earlier structure existed in the area. Although she says that I understand the widest walls resting on the bedrock beneath the OH as possible evidence of an earlier building phase, she complains that "on [my] figure 26 the bottom courses of these wide foundations are clearly represented by plain (unhatched) additions to Rooms 406, 407, . . ." (emphasis added). Then she writes that I "did not realize that this was Fisher's way of showing that the foundations were wider than the superstructure." In his field diaries, Reisner actually sketched the stepped foundations discovered beneath the massive GFW (see my figures 27–28). Even a novice archaeologist who compares these graphics with Fisher's rendering of the wide walls beneath the OH will see that the two architectural styles hardly match. Rather, the red and green walls on my figure 41 represent two completely different phases, not multiple risers or a series of pyramid-like steps on foundation pilings. Moreover, Reisner's purported level for Ahab's courtyard floor sometimes runs above both phases, sometimes through the red phase, and sometimes even through the lower, green phase.

In the following paragraph, Franklin opts (as I do) for interpreting the unhatched walls as foundations for the OH, but only because she somehow *knows* that Harvard/Reisner/Fisher committed a "serious mistake" (which, in turn, "led [Tappy] astray") by mislabeling the unhatched architectural phase in figure 26 as "Later Additions" when they really meant to write "Earlier Construction." Franklin nowhere explains how she knows this claim to

be true. Ultimately, it is not at all established that I was “led astray”—I *reinterpreted* Fisher’s drawing as labeled and published, while Franklin *changed* the labeling in a way that would, conveniently, support her interpretation.

Franklin continues her discussion by acknowledging that I did “raise the important question” of whether an earlier archaeological phase existed below the OH. She notes: Tappy “agrees” that such a phase did exist there; “continues to confirm the presence of such a building”; “acknowledges that others before him” (including, of course, herself) had expressed similar views; and “correctly proposes” that his so-called “Two-Room Structure” represents remains from the earlier phase. I explicitly cite her as the originator of the phrase “lost Monumental Building” in reference to this ephemeral structure. But while in my book I recognize that Franklin and I came to roughly the same interpretation on this matter, I also quite willingly acknowledge that the true genesis of the idea lies not with Franklin *or* with me but mainly with the insightful, intuitive Ivan Kaufman (whom she does not cite in her related articles).

Ultimately, Franklin concedes that I properly affirm the disassociation of the ostraca from the so-called OH. But she also seems to imply that the ostraca came from the “dirty yellow” debris labelled “c” on my figure 41 and says that I “correctly not[e]” this fact. Some ostraca did derive from this matrix. But against the position she erroneously attributes to me, these deposits represent heavy fills that buried the defunct OH, not, as she writes, the “constructional fill underlying” it. Only the clean yellow deposits (labeled “o,” immediately on bedrock) preceded and were cut by the later walls. Here, Franklin’s mistake leads her to state that the ostraca “originated in an earlier stratum” than the OH. In fact, the bulk of the inscriptions came from contexts that postdated the building.

In addition to the “two principle errors” Franklin believes she identified in my book, she also challenges my readings of Fisher’s sections. The problem is that Fisher’s section drawings often deviate significantly from their courses as plotted on his phase plans. One cannot, therefore, look at his plans and know that the related section drawings will remain true to the lines shown there. Franklin acknowledges this reality as “annoying” and “very different from today’s . . . conventions” but adds that it is acceptable to *plot* a section line along one

route but *draw* it along a different course to take in “the largest number of architectural features.” But Fisher’s unexpected deviations are not, as Franklin claims, “immediately noticeable to anyone.” Awareness of them comes only after a thorough, comparative study of Fisher’s various drawings (sections vis-à-vis plans), which sometimes freely follow divergent paths *without his alerting the user* to the diversions. I identify what Fisher was doing and precisely why he was doing it in greater detail than anyone before me (75–80 and figure 36 in my book).

Franklin does note in passing that “apparently[ ] Ussishkin was the first to mention” Fisher’s technique. She cites my reference to Ussishkin on page 95, where I state that prior to my work only he had identified this problem. I subsequently built on Ussishkin’s observations by elaborating the challenges (and risks of misinterpretation) arising from Fisher’s conventions and lack of explanation to those who encounter them, while also acknowledging that Fisher had a right to draw as he wished. It is worth noting that Ussishkin criticized the conclusions in Franklin’s own work based on her *nonrecognition* of Fisher’s technique [Ussishkin 2007: 62–68]. So presumably my detailed explanations of that technique, rather than being superfluous, should be helpful to Franklin, as well as to others.

My placement and connection between two blue lines representing rock scarps in one of my graphics (Fig. 21b = 44) evokes another criticism by Franklin. Based on published and unpublished records of the excavators themselves, I argue that these artificially created scarps defined the inner summit—the elevated platform on which the palace, courtyard, and other official buildings were situated. In fact, I should have placed my vertical blue line farther east, but by only one to two meters, toward the westernmost palace wall. A better understanding of the western scarp’s location is gained by looking at my figure 22, since it presents the actual Harvard plan, not an overlay of Harvard’s scarp lines on the later Crowfoot-Kenyon drawing (Fig. 21b = 44). My narrative specifies what my drawing perhaps did not: I state that this western scarp ran basically parallel to and roughly 13 meters east of Wall A (38); I also recognize that Rooms 13–14 lay at the *foot* of the scarp (54, n. 9). Reisner recorded that the “Omri scarp” lay “just east of the apparent modern summit” (*HES I*: 65) and that the western

edge of the palace was set back only 10–15 cm from the scarp's upper face (*HES I*: 61). (In fact, Reisner also indicated that this scarp divided Summit Strip 2 in the middle, i.e., at the approximate center of Grids G.10–15 [*HES I*: 71] and precisely where I have placed the line in Fig. 21b = 44—though admittedly Reisner's statement does not accord well with his two previous observations.) So Franklin's point is well taken; for accuracy's sake, my longitudinal blue line should move just slightly eastward. Even so, such movement would not negatively affect my argument.

Franklin also complains that my two blue lines should not join, since the scarps they represent “are quite separate” and “do not meet.” Steering by what the excavators themselves recorded nearly 110 years ago, however, one finds that, according to Reisner, the “Omri scarp” ran “all around the summit” (*HES I*: 61), and in Summit Strip 6 (G.3/4–6/7), “About five metres north of the [Roman] altar [in G.7], there was a vertical rock scarp about three metres high, a continuation of the rock scarp of the Omri palace” (*HES I*: 77). The last notation seems to refer to the northern scarp (my laterally oriented blue line), as clarified in a crucial corollary passage from Fisher (*HES I*: 93–94, though he places the scarp's height at 4 meters): “West of the altar this scarp *turned towards the south* in line with the scarp in G 11–14. This was the northwest corner of the Omri building” (emphasis added). (I interpret “building” to mean Omri's general construction activities on his artificially raised summit platform, not a specific edifice, since the altar itself was situated 30–40 meters north of Omri's palace, in an area where the plans indicate no Israelite buildings, and just inside the point at which *HES II*: Plan 5 actually depicts the northwestern corner.) Fisher continued by saying that, once it turned southward, the northwestern scarp ran 15 meters to the south, “turned out at a right angle for 5.2 m.,” then turned in again and continued 23.8 meters “to the southwestern corner.” From this corner, a lateral southern scarp proceeded toward the east. These descriptions suggest that both the excavator and architect saw that the northern and western scarps did meet to form a northwestern corner—a corner that may not now be visibly detectable due to the “partial backfilling and a century of soil erosion” Franklin herself notes while also stating that only “large sections of the scarp are still visible today.” Thus, while I

perhaps should tweak the position of my blue lines left or right, up or down, I am not ready to erase the northwestern juncture the excavators apparently revealed. And, as I have noted elsewhere, Fisher concluded “that this elaborate rock scarping marks the area, and to a large extent determines the plan, of the original Omri structure.”

I appreciate the mutual interest in Samaria that Franklin and I share, and I can even appreciate her pointing out a couple of needed adjustments to two of my figures. But my interpretive decisions on the issues Franklin raised are demonstrably justified, as I have detailed here.

### References

- Reisner, G. A., C. S. Fisher, and D. G. Lyon. 1924. *Harvard Excavations at Samaria, 1908–1910*. 2 vols. Cambridge, MA: Harvard University. [*HES I* and *HES II*]
- Ussishkin, D. 2007. Megiddo and Samaria: A Rejoinder to Norma Franklin. *Bulletin of the American Schools of Oriental Research* 348:49–70.

### *Greek Baths and Bathing Culture: New Discoveries and Approaches*. Edited by Sandra K. Lucre and Monika Trümper.

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This book comprises the more fully developed papers presented at a conference of the same title, which was the first to exclusively devote itself to Greek baths and was held at the American Academy in Rome in 2010. Four additional contributions on baths, which were in the process of being identified or excavated at the time of the conference, round out the volume.

This collection is fairly specialized and aims at scholars and the advanced student of baths, as it presupposes knowledge on the development of bath houses as well as the pertinent research questions. As this is the first collection of papers exclusively on Greek baths, it will soon become a seminal work in this (up to now) fairly