THE ARCHAEOLOGY OF THE OSTRACA HOUSE AT ISRAELITE SAMARIA

EPIGRAPHIC DISCOVERIES IN COMPLICATED CONTEXTS

BY RON E. TAPPY

ANNUAL OF THE AMERICAN SCHOOLS OF ORIENTAL RESEARCH, VOLUME 70

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In honor of my mother,

Hazel Marie Nauman Tappy

who intuitively embodies and who endowed me
with the Latin schola, *"leisure for learning,"*
and whose love of learning I continue to find contagious

and
In memory of

Anson F. Rainey

who informed my learning as teacher, colleague, and friend

καὶ ὡμοιώθη λέοντι ἐν τοῖς ἔργοις αὐτοῦ . . . . καὶ ἐπίκρανεν βασιλεῖς πολλοὺς
καὶ εὔφρανεν τὸν Ιακωβ ἐν τοῖς ἔργοις αὐτοῦ
καὶ ἕως τοῦ αἰῶνος τὸ μνημόσυνον αὐτοῦ εἰς εὐλογίαν

He was like a lion in his deeds . . . . He embittered many kings,
but he made Jacob glad by his work,
and his memory is blessed forever.

1 Maccabees 3:4a, 7

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Preface

On October 5, 1909, during his first season as field director of the Harvard Excavations at Samaria, George Andrew Reisner exuded an enthusiasm for his work that seemed irrepressible. He projected that, with the publication of the 1909 excavation results, "the world [would] gain a decidedly new view of the power of the Israelite kingdom." Unbridled exuberance even led him to the questionable claim that "Herod's city wall is paltry compared to the… Omri-Ahab city wall." With passion in full flight, he proclaimed privately, "This is surely the greatest piece of work I ever hope to have a part in." He seemed genuinely and abundantly gratified. But only ten months later, on August 7, 1910, he was crestfallen. "I have given up hope of making any sensational finds…the hill is so vast that it is a mere matter of chance whether we strike anything even in a ten year's campaign…. Nothing in the way of scientific research can be hoped for from indiscriminate grubbing in a dozen different places on the odd chance of finding anything." Thus, by summer's end in 1910, Reisner experienced a glum discouragement as he stood among the ruins of the ancient Israelite capital at Samaria.

A number of factors coalesced to make it so. More than a year earlier, and at the request of his home institution (Harvard University), Reisner had left his beloved Egypt to salvage this major project in Palestine—one launched in 1908 with prodigious outside patronage. His early diaries display little appreciation for the work completed during the inaugural season under his predecessor, Gottlieb Schumacher. Reisner had struggled to iron out countless details of land use contracts and labor agreements with local officials, as well as licensing issues and the division of finds with bureaucrats in Istanbul. An uneasiness attending the waning years of Ottoman rule and the coming of the Young Turk Revolution in 1908, coupled with an appetite for artifacts by Istanbul's fledgling national museum, had made everything on the administrative side of the project both delicate and thorny. In Sebastiyeh, he constantly contended with local officials for whom he harbored complete disdain. Moreover, the quality and ethic of work shown by the laborers drawn from around Samaria proved, in Reisner's judgment, far below that of the Egyptians with whom he was used to dealing. It provoked within him a rancorous incredulity. Inclement weather (hard, early autumn storms), equipment failures (the camera used to photograph the ostraca proving "about as useful as a feather fan in a rain storm"), and other problems often addled his efforts. The excavation itself had occasionally yielded interesting results, but ultimately nothing seems to have intrigued him or captured his full imagination; nothing had proffered a notable sense of pride or honor. Though his assignment thus far spanned only a brief period, the work had consumed considerable toil and capital, and no "sensational finds" lay in sight.

But in early August 1910, a series of discoveries began to surface that, for Reisner, would make all the labor worthwhile. Fighting what he had come to view as constant, insurmountable aggravation with local leaders, and battling his personal disappointment with the pace and quality of discoveries at the site, Reisner had decided that the 1910 Season should steer the entire expedition

to a logical if premature conclusion. Thus, after photographing the remains of a recently exposed building near the southwestern corner of the summit compound and requesting that his architect, Clarence Stanley Fisher, produce a plan of the structure, on August 10 he instructed the workers to begin removing the floors of the building's various rooms. Early the next morning, the first potsherd bearing an Israelite inscription emerged from a layer of dirt packed along the eastern wall of Room 401. Over the next few weeks, dozens of laconic shipping dockets came to light. Their concise, business-like content detailed small quantities of commodities (mostly wine and oil) sent to the Israelite capital from a tightly drawn constellation of lineage-based villas and towns around the city. All these ancient villages were situated fewer than 20 km from the Israelite king's very throne room in Samaria's palace. But the place names and personal names contained in the writings promised to elucidate the socio-political and economic conditions within some unnamed ruler's wider kingdom—probably, thought Reisner, the legendary kingdom of Ahab.

The centripetal force exerted by king and capital came into sharper focus as the history of scholarship relating to this series of discoveries took root and began to grow. Virtually all the attention given to the ostraca over the ensuing decades focused on either the palaeography or the historical occasion and function of the collection as a whole. (Many of the inscriptions appear in an exquisite hand in unpublished field diaries kept by Reisner.) But very few studies have attempted to discern, describe, and evaluate, from an archaeologist's point of view, precisely where Reisner found each ostracon. For the most part, assumptions about their archaeological context (and here I use the singular intentionally) built on several rather misleading statements made by Reisner in the official excavation report of 1924. And the report itself provides not even the bare minimum of provenance data necessary to reaching viable conclusions, whether positive or negative, concerning the stratigraphic integrity of the ostraca-bearing loci. The quest to assess the specific findspots, therefore, must take one on a trail behind and beyond the official report.

In various earlier studies, I have addressed the stratigraphy, pottery, and magnificent cache of ivory carvings found at Samaria during the British-led Joint Expedition, which recommenced work at the site in the early 1930s. In the present investigation, I turn my attention to the corpus of ostraca recovered by Reisner in 1910, during his work on behalf of Harvard. Although I touched on this subject in a previous book, I now focus exclusively and much more fully on this particular body of writings. Reisner registered and published 63 ostraca made up of 75 ceramic fragments. Contrary to common understanding, he found these writings not only within the parameters of the aforementioned building, which he immediately called "the Ostraca House," but also scattered over a wide swath of excavated area stretching nearly 40 meters in length and incorporating multiple levels from at least five successive historical periods spanning more than a millennium. The depositional history of Samaria's densely built summit proved extraordinarily complex and at times vexing.

Inevitably, the collection of inked inscriptions provoked numerous questions about the sociology and specific roles of the people named in the writings. For example, scholars have wondered whether the key personal names attested on these sherds (the so-called *l-men*) belong to absentee landlords temporarily residing in the royal compound of Samaria and receiving the shipments from their own local estates. An alternative view has understood these figures as clan or household heads who maintained their residence in the network of villas surrounding the capital city while sending the listed goods to the king (a view that I accept), perhaps as tax payments (a view that Reisner himself originated).

In addition to the published corpus of ostraca, Reisner recovered 44 other inscriptions that he registered but did not present in the official report. Later, Ivan T. Kaufman located all but four of these fragments in the Istanbul Museum and provided photographs of them in his 1966 Harvard dissertation. During the 1930s, the Joint Expedition recovered a smaller group of 11 Hebrew inscriptions that differ from Reisner's 1910 ostraca in certain aspects of form and content. (W. F. Albright, among others,

voiced regret that the British team had not found more.) Generally, analysts have presumed that these inscriptions belong later in the eighth century BCE, sometime around 735, and that the Harvard ostraca predate them by half a century or more. Whatever epigraphists decide concerning the overall nature of the ostraca script, one must also certainly consider the archaeology of the Ostraca House itself in any full-bodied discussion of the writings' original contexts and function. The present investigation addresses exactly this side of the equation.

Seeking out the history of scholarship for any particular archaeological find from point of discovery to present day affords a rewarding endeavor in itself. It seems a peculiar aspect of such research that, in the process, scholars who aspire to the highest objectivity also often come to feel an almost personal relationship with those who lie at the heart of their pursuits, even though those learned forbearers lived and died long ago. Never to have encountered these early explorers in the flesh means knowing them now only through the writings they left behind. Still, from the written word springs personality. This phenomenon is especially true the more one steps away from the beaten academic track trod and worn by the published record and into the hidden world of unpublished, private journals, field diaries, or random notes left on stained cards in some file box long forgotten in a museum basement. Such writings perhaps lie closer to an author's own heart and, revealing more than the mere accounting of measurements and strata, more effectively withstand the ravages of time by bridging the personal distance between our academic ancestors and us.

Thus, while in this study I myself have focused on properly interpreting the data as I understand them, I have at the same time come to know and appreciate the various individuals who played pivotal roles in the early exploration of Samaria. These two lines of learning started intersecting nearly three decades ago as I developed an initial interest in Samaria and eventually wrote about the work undertaken by the British in the 1930s. Now turning to the earlier Harvard project, I see once again that I cannot examine the broader time and circumstance of the recovery of the ostraca without venerating the inveterate determination of George Andrew Reisner, the steady and artful hand of Clarence Stanley Fisher, and so on. But the veneration implies only a professional esteem, not worship. I do not refrain, therefore, from discussing their methodological shortcomings even while knowing that I cannot and should not scrutinize them by today's standards. The practice of field archaeology has improved with age.

In their own time, then, Reisner, Fisher, and others emerged as standard bearers. Consequently, I wish to close this opening statement by noting one participant whose overall contribution to the Harvard Expedition impressed me perhaps more than any other. Alongside the extraordinary generosity of Jakob Heinrich ("Jacob Henry") Schiff, the vision, indefatigable commitment, dependability, and wide-ranging skills of one administrative and academic figure came to the fore as my look at the ostraca evolved—those of David Gordon Lyon. While Reisner undoubtedly desired to elevate the method and theory of fieldwork above the conventions of his day—an aspiration for which he deserves and receives due credit—the supportive planning and assiduous work of Lyon, often completed behind the scenes, all too often goes unheralded. Not only did he bring the immense resources (financial and otherwise) of the Schiff family into the orbit of the Semitic Museum, but he also oversaw, year after year, every aspect of its day-to-day operation. He clearly became the liaison between Schiff and the Museum and between the Museum and work it underwrote in faraway lands. He accepted the grinding responsibility of publishing regular, preliminary reports on progress made in the field, and he guided the Museum to a respectable position within the larger world of Harvard University, all the while keeping two successive presidents of this institution thoroughly informed of progress both at home and abroad. No one, in my judgment, was more involved in and committed to the success of the Samaria project than Curator David Lyon. He truly embodied the wattle and daub of the entire venture.

In June 1993, wilderness guide and educator Fred Blackburn hiked with editor and author David Roberts (both of whom held deep commitments to the study of the bygone Anasazi culture of the American southwest) to Sandal House, an Anasazi ruin along the Mancos River in Ute Mountain Tribal Park in southwestern Colorado. Sandal House represents the first Anasazi site discovered in 1895 by the now-famous Wetherill brothers. While at the site, Blackburn found initials or signatures from four of the five Wetherills scrawled on the cave's walls. After a while, he and Roberts chanced upon a faint penciled inscription on a stone in a kiva (a chamber probably used for ceremonial or religious purposes). The notation was placed there long ago by Richard Wetherill, perhaps the most energetic, systematic, and famous of the archaeological Wetherill brothers. It read:

R. Wetherill
Sept. 28.88
3½ P.M
1–3 gal Jar
return for it
about Oct. 2

Blackburn and Roberts concluded that Wetherill had cached a large Anasazi pot in the ruins of Sandal House and that he intended to return for it four days later. After some silence, Blackburn said softly, "This is as close as we're ever going to get to talking to him" (Roberts 1996: 50). Through published and, even more so, private, unpublished records, I have come close to talking with George Andrew Reisner, David Gordon Lyon, Gottlieb Schumacher, and Clarence Stanley Fisher—and through them to the kings of Samaria.

R. E. Tappy
July 27, 2015
Jerusalem, Israel

Acknowledgments

The following work represents the continuation of my interest in the history and archaeology of Israelite Samaria—interest that has received earlier expression over the course of a dozen books and articles. As with all my previous investigations into this site, I could not have completed the present study without the kind assistance of many people. At the outset, the idea and motivation to write this book grew out of conversations with Christopher Rollston, who plans to follow up on my archaeological discussion with his own full-scale treatment of the paleography and historical content of the ostraca. Together, we hope that the ensuing volumes will settle at least some remaining questions surrounding this important corpus of writings, perhaps raise other leading questions, and serve as a basis for future analyses by other scholars. I am also deeply grateful to Andrew Vaughn, Executive Director of the American Schools of Oriental Research, both for his interest in my project and his encouragement to publish it as an Annual of ASOR. Kevin McGeough, editor of the Annual series, provided invaluable guidance and assistance throughout the publication process. Thanks also to Susanne Wilhelm, design and production manager at ISD, whose expert skills and dedicated work lie behind the ultimate presentation of not only my volume on Samaria but many additional issues within the *Annual* series.

The ability to incorporate so many graphics in the text of my study comes through the generosity of a number of people and institutions. I am especially indebted to Joseph Greene, Deputy Director and Curator of the Semitic Museum at Harvard University, Felicity Cobbing, Executive Secretary of the Palestine Exploration Fund in London, England, and the editors of the *Bulletin of the American Schools of Oriental Research*. I also acknowledge the kind assistance of Rita Sapozhnikov at The Israel Academy of Sciences and Humanities, Todd Bolen of BiblePlaces.com, and Richard Cleave of ROHR Productions Ltd.

My ability to color code the plans and section drawings, a treatment that surely renders them more user friendly, was possible only through the generous financial support of Alan and Francine Lefkowitz, Anita Newell, Peter and Susie Smerd, Rev. Ronald Wakeman, John N. Sayer, Miguel Feitosa, and the staff of Rodef Shalom Synagogue in Pittsburgh. I also acknowledge the steady and kind encouragement of Dr. Walter Jacob, Rabbi Emeritus at Rodef Shalom. Within this group of friends and benefactors, Fran Lefkowitz, who for 36 years served as Founding President of the Biblical Archaeology Society of Pittsburgh, led the way in marshaling support for this publication. I am deeply grateful for her faithful encouragement and friendship. At Pittsburgh Theological Seminary, both David Middleton, Director of Information Technology, and Mark Russell, Technical Services Librarian at The Clifford E. Barbour Library, invested much time and effort in computer services, particularly during the initial scanning of the line drawings presented in my text. Without the assistance of these two stalwart colleagues, the more than century-old plans, sections, and diary leaves presented in this study would certainly look their age.

The Semitic Museum at Harvard University has produced a tremendous aid for students of

Samaria by making available electronic copies of not only the two official excavation reports from the Harvard Expedition to Samaria in 1908–1910 but also the private, otherwise unpublished field diaries of George Andrew Reisner, David Gordon Lyon, Gottlieb Schumacher, and Clarence S. Fisher. These resources are accessible through the *Harvard University Library Open Collections Program: Expeditions and Discoveries, Sponsored Exploration and Scientific Discovery in the Modern Age*. Their online availability surely sets a standard for other holders of valuable research materials to follow.

I am indebted to my fellow members of the Biblical Colloquium, colleagues who listened patiently to a presentation based on an early manuscript of this book and who made many wise and stimulating observations and suggestions. I am fortunate to enjoy the counsel of such an esteemed group of scholars, whose detailed review improved my analysis at many turns. Similarly, I am grateful to Christopher J. Davey, Director of the Australian Institute of Archaeology, who invited me to deliver my work on the Ostraca House as the annual Petrie Oration in the spring of 2014. I found the academic discussions in that wonderful land down-under both rich and helpful, and my visits with colleagues in Melbourne, Sydney, and Armidale gave me great personal pleasure.

The renowned naturalist John Muir once groused that writing "is like the life of a glacier; one eternal grind." In contrast, my writing of this book was enjoyable, hardly a grind. By necessity, however, the work is steeped in detail. My wife, Connie Gundry Tappy, Senior Writer at Pittsburgh Theological Seminary and one of the best editors I know, graciously read various permutations of the book and made many helpful suggestions that brought greater clarity to a highly technical discussion. Her keen eye made this a better study.

Finally, since the days of my youth no other person has invested in me, waited through times when I strayed into this or that dead end, helped shape my academic desires, or assisted in the achieving of my goals more than my mother, Hazel Marie Nauman Tappy. Her warm heart holds much wisdom, and from her steady hand flows immeasurable kindness. It is with deep affection that I dedicate this book to her.

R.E.T.

Abbreviations

1. Samaria: Excavation Reports

HES I Reisner, G. A.; Fisher, C. S.; and Lyon, D. G. *Harvard Excavations at Samaria I, 1908–1910*, Vol. I: *Text.* Cambridge, MA: Harvard University, 1924.

HES II Reisner, G. A.; Fisher, C. S.; and Lyon, D. G. *Harvard Excavations at Samaria II, 1908–1910.* Vol. II: *Plans and Plates.* Cambridge, MA: Harvard University, 1924.

SS I Crowfoot, J. W.; Kenyon, K. M.; and Sukenik, E. L. *The Buildings at Samaria*. London: Palestine Exploraton Fund, 1942.

SS III Crowfoot, J. W.; Crowfoot, G. M.; and Kenyon, K. M. *Samaria-Sebaste III: The Objects*. London: Palestine Exploration Fund, 1957.

2. Publications Relating to Samaria

AIS I Tappy, R. E. *The Archaeology of Israelite Samaria:* Vol. I, *Early Iron Age through the Ninth Century BCE*. Harvard Semitic Studies 44. Atlanta, GA: Scholars, 1992.

AIS II Tappy, R. E. *The Archaeology of Israelite Samaria:* Vol. II, *The Eighth Century BCE*. Harvard Semitic Studies 50. Winona Lake, IN: Eisenbrauns, 2002.

3. Other Publications

Hazor I Yadin, Y.; Aharoni, Y.; Amiran, R.; Dothan, T.; Dunayevsky, I.; and Perrot, J. *Hazor I: An Account of the First Season of Excavations, 1955*. Jerusalem: Magness; The Hebrew University of Jerusalem, 1958.

Hazor II Yadin, Y.; Aharoni, Y.; Amiran, R.; Dothan, T.; Dunayevsky, I.; and Perrot, J. *Hazor II: An Account of the Second Season of Excavations, 1956*. Jerusalem: Magness; The Hebrew University of Jerusalem, 1960.

Hazor III–IV Yadin, Y.; Aharoni, Y.; Amiran, R.; Ben-Tor, A.; Dothan, M.; Dothan, T.; Dunayevsky, I.; Geva, S.; and Stern, E. *Hazor III–IV: An Account of the Third and Fourth Seasons of Excavations, 1957–1958*. Jerusalem: Israel Exploration Society; The Hebrew University of Jerusalem, 1989.

Hazor VI	Ben-Tor, A.; Ben-Ami, D.; and Sandhaus, D. *Hazor VI: The 1990–2009 Excavations, The Iron Age.* Vol. 1, *Text;* Vol. 2, *Plates*. Jerusalem: Israel Exploration Society; Institute of Archaeology, The Hebrew University of Jerusalem, 2012.
Keisan	Briend, J., and Humbert, J.-B. *Tell Keisan (1971–1976): une cité phénicienne en Galilée*. Paris: Gabalda, 1980.
Megiddo I	Lamon, R. S., and Shipton, G. M. *Megiddo I: Seasons of 1925–34, Strata I–V*. Chicago: University of Chicago, 1939.
Megiddo II	Loud, G. *Megiddo II: Seasons of 1935–39,* Vol. 1, *Text* and Vol. 2, *Plates*. Chicago: University of Chicago, 1948.
Megiddo III	Finkelstein, I.; Ussishkin, D.; and Halpern, B., eds. *Megiddo III: The 1992–1996 Seasons*. Tel Aviv: Institute of Archaeology, Tel Aviv University, 2000.
Taʿanach	Rast, W. E. *Taʿanach I, Studies in the Iron Age Pottery*. Cambridge, MA: American Schools of Oriental Research, 1978.
Yoqneʿam I	Ben-Tor, A.; Avissar, M.; and Portugali, Y. *Yoqneʿam I: The Late Periods*. Jerusalem: Institute of Archaeology, The Hebrew University of Jerusalem, 1996.

Chapter 1

Introduction and Background

When it is remembered that every excavation destroys historical material which has been accumulating for ages, it is clear that no pains in recording the work are excessive.

~ *G. A. Reisner,* HES I, *43*

George Andrew Reisner counted the Israelite ostraca among the most important finds ever recovered by the Harvard Expedition to Samaria (*HES I,* 62).[1] But the precise provenance of these historic inscriptions has remained murky at best. To date, the most incisive and intuitive statement on their archaeological context comes from a brief treatment in I. T. Kaufman's unpublished dissertation (Kaufman 1966: 101–21). The present study builds on that work by considering in much greater detail the depositional history of the Ostraca House and its immediate surroundings. This study, therefore, is archaeological in nature. Its findings will undoubtedly prove of interest to historians and epigraphists alike, but archaeology lies at the core of the inquiry. More specifically, this study examines Reisner's presentation of the ostraca from 1910 in his official excavation report.[2] Concerning chronology, the investigation attempts to clarify the date of the archaeological contexts from which excavators recovered the inscriptions. The time of their actual writing, while certainly informed by this date, may also vary from it to a greater or lesser degree. Given the limited quantity and sometimes uncertain quality of data available from the Reisner era, one may anticipate results that provide only incremental improvements, not grand theories. Still, any increase in our knowledge of the ostraca's stratigraphic position will prove useful to the dating and perhaps overall interpretation of this valuable cache of artifacts. Throughout this study, I base my comments and observations not only on the official, published records from the Harvard expedition to Samaria but also on unpublished materials contained in the following documents: Reisner's field diaries (cited as *Reisner Diary I–VII*); David Gordon Lyon's personal journals (identified as *Lyon Diary I–III*); and,

1 For my earlier, abridged comments on the Hebrew ostraca, see Tappy 2002: 496–503. For a full-scale discussion relating to the stratigraphy and pottery of Israelite Samaria, see Tappy 1992 and 2002 (hereafter *AIS I* and *AIS II,* respectively).

2 For various data and tabulations relating to Reisner's corpus of legible ostraca, see Appendixes A, B, and E at the conclusion of this study. Reisner recovered 44 additional inscriptions, whose registration numbers he scattered between 3868 and 4628 (see *HES I,* 246). For some of these barely visible writings, he attempted preliminary drawings in his field journal by rendering as many letters as he could discern (e.g., see *Reisner Diary V,* 524, for Reg. No. 3877; 525 for Reg. No. 3880; and 528 for Reg. Nos. 3892–3893). More often, he simply noted the presence of unreadable script or listed only the registration numbers (e.g., *Reisner*

Fig. 1 *Samaria: View from the Southwest.*

to a lesser extent, both Fisher's and Schumacher's private journals (hereafter *Fisher Diary I–II* and *Schumacher Diary I–II,* respectively). (These records are available online through the *Harvard University Library Open Collections Program*; see Acknowledgments.) Information drawn from these hand-written accounts not only provides supplementary data crucial to a study of the ostraca, it also enlivens the story behind the discovery of the inscriptions and reveals the archaeological and administrative trials persistently faced by the excavators. As my analysis of the archaeological remains advances through the following narrative, then, an important subplot also unfolds there and in the extensive footnotes.

Diary V, 519, 525). After considering illegible the writings on these mostly small fragments, he excluded them from his publishable list and provided no provenance data for them. Later, Kaufman located all but four of these sherds in the Istanbul Museum and presented photographs of them in his dissertation (1966: 145–47; pls. XLIII–LIII). Although his efforts confirmed that one can read very little on these poorly preserved fragments, two ostraca from Year 10 (Reg. Nos. 3892–3893) proved quite legible (compare Lemaire 1977: 37–38). Reisner's field notes actually contain excellent drawings of these two inscriptions, and he noted that the reading on the first one was "absolutely certain" (*Reisner Diary V,* 528; for photographs, see Kaufman 1966: pl. LIV). New, multispectral imaging techniques may offer some hope for more precise readings of these and other faded, ink-written inscriptions in the future (see Sober et al. 2014: 185–97). During the 1930s, the Joint Expedition recovered a smaller group of 11 Hebrew inscriptions, including the famous "Barley Ostracon." These writings differ in form and content from Reisner's 1910 ostraca group, and about half of them constituted inked or incised fragments while the remainder appeared on intact vessels. Generally, analysts have presumed that these inscriptions belong later in the eighth century BCE, with nearly half of them clustering around 735 BCE (see the conclusions of S. A. Birnbaum in *SS III,* 11–25; pls. I–II; for Birnbaum's linking of the 1910 Harvard ostraca to Jeroboam II, see Ch. 5, n. 2).

Fig. 2a *Three Highland capitals at the head of the Wadi Far'ah: Map (by R. E. Tappy).*

Fig. 2b–d *The Wadi Far'ah: b. aerial from the northwest (courtesy ws040315148: Bill Schlegel/ BiblePlaces.com); c. satellite view toward the Jordan River Valley (courtesy of R. Cleave, ROHR Productions Ltd.); d. satellite view of upper bend toward Tirzah and Shechem (courtesy of R. Cleave, ROHR Productions Ltd.).*

A. The Site: Physical Setting and Early History (fig. 1)

The hill country of Ephraim—not of Judah—became the cradle of Israelite civilization. At the head of the Wadi Far'ah and near the heart of this area lay a triangle of three cities (Shechem, Tirzah, and Samaria; fig. 2a.b), which served as religious and/or political centers. According to biblical accounts, however, the claim on royal power proved short-lived in both Shechem and Tirzah. These texts recall how, in the early ninth century BCE, Zimri's failed coup against King Elah, who was drinking himself drunk in Tirzah (1 Kgs

Fig. 3 *A new capital west of the watershed (courtesy of R. Cleave, ROHR Productions Ltd.).*

16:8–20), beset the kingdom with such political turbulence that it threatened the stability of the entire realm. But army commander Omri, who apparently enjoyed popular acclaim among his troops, responded decisively. Leading his forces from their camp in Gibbethon, he besieged and controlled strife-ridden Tirzah, overcame threats from another contender for the throne (Tibni; 1 Kgs 16:21–22), and became King of Israel around 884 BCE.

Omri quickly purchased a modest-sized, family-owned estate that lay on the seaward slopes of the Ephraimite hills (opposite the eastern-oriented Shechem/Tirzah; fig. 3), transferred his political capital there, and called the new city Samaria (biblical Shomron; שֹׁמְרוֹן). To the end of the Israelite kingdom, no city usurped Samaria's place as political hub. Even the surrounding region took on its name (1 Kgs 21:1; 2 Kgs 1:3), and over 160 years later leading nations continued to refer to the city as the "House of Omri" (fig. 4; compare Summary Inscription 4 from Tiglath-pileser III plus the *lamassu* and cylinder inscriptions of Sargon II from Dur Sharrukin/Khorsabad). The symbolism and status of Samaria grew so impressive that later biblical writers spoke, even if disparagingly, of the city as the undisputed "head of Ephraim" (Isa 7:9), or as Jerusalem's "elder sister" who ruled and influenced numerous "daughters" (outlying villages) of her own (Ezek 16:46, 53, 55, 61; 23:4–5). The choice of Samaria both freed Omri from the baggage of earlier political turbulence and reoriented his kingdom's economic outlook westward toward the lucrative trade moving along coastal routes and throughout the Mediterranean world. Under his watchful eye, the family-villa-turned-royal-center assumed its place as the undisputed seat of northern power.

By excluding ordinary residential and domestic quarters from within the city walls that defined the

KUR(*māt*) É(*bīt*)-*Ḫu-um-ri-a*

"the land of (belonging to) the House(hold) / Dynasty / Kingdom of Omri"

Undisputed 'Head of Ephraim' (Isa 7:9)
Jerusalem's 'elder sister' (Ezek 16; 23)

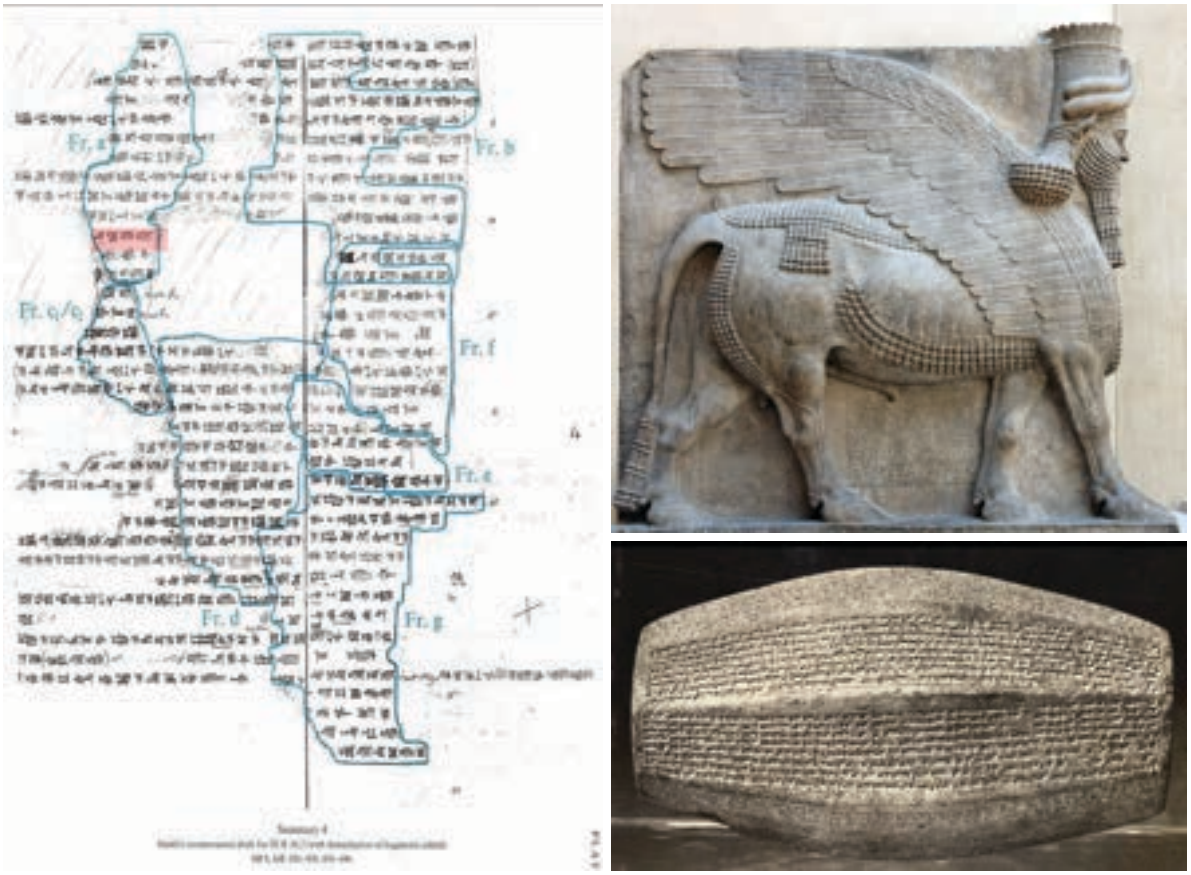


Fig. 4 *The House(hold)/Dynasty of Omri (in Assyrian texts); a. Summary Inscription 4 from Tiglath-pileser III; G. Smith's reconstructed draft (see Tadmor 1994: 136–43, ll. 6', 15', pl. LI; also Tadmor and Yamada 2011: 104–07, ll. 6', 15', et passim; © The Israel Academy of Sciences and Humanities; reproduced by permission); b. Lamassu bull sculpture; c. Cylinder Inscription from Sargon II.*

summit,[3] Omri distinguished his capital as a place of royal administration and power focused exclusively on the king and his family. Omri's son and successor, Ahab, augmented this ideal by increasing the area of the royal acropolis and constructing an impressive casemate fortification system that now enclosed lavishly appointed royal buildings, one of which was remembered in the Chronicles of the Kings of Israel as the בֵּית הַשֵּׁן, the House of Ivory (1 Kgs 22:39; Am 3:15; fig. 5). From an apparently opulent Samaria, Ahab propelled Israel's new status into international politics by marrying the Sidonian princess Jezebel (1 Kgs 16:31), fighting protracted wars against the Aramaeans of Damascus (1 Kgs 20), struggling for hegemony over Transjordan (1 Kgs 22; Mesha' Stele), and participating in the

3 In certain periods, a residential zone may have existed around the slopes of the site, at least on its northern, western, and southern sides (cf. Finkelstein 2000: 114–38; 2011: 194–207; 2013: 87–94).

Fig. 5 *Selected ivory carvings from Samaria (for the archaeological context of the ivories, see AIS II, 443–95).*

anti-Assyrian league at Qarqar (Tell Qarqur), on the eastern bank of the Orontes River (the Kurkh Stele II.90b–97, espec. 91–92).

Throughout its life, Samaria remained cosmopolitan in nature but small in size—more a royal compound than a multifaceted city (notwithstanding n. 3). In the center of the acropolis, workers artificially extended various scarps (figs. 21b–22, below) in the bedrock to create a raised, rectangular platform (ca. 6,732 m²) that stood approximately 3.5 m above the surrounding rock. This elevated area accommodated the royal palace,[4] a large courtyard, and smaller royal buildings. The lower, flanking rock supported storage and food processing facilities, possibly an ivory-carving workshop, and other service-related areas. This overall design effectively constituted the classic "upstairs-downstairs" arrangement, thus allowing pack animals to enter and circumnavigate the central compound without rising to the level of the royal quarterdeck. One auxiliary structure, the so-called "Ostraca House," lay west of the rock crest, on the lower level below the main palace. Excavation of this building in 1910 yielded 63 legible administrative dockets that appear, on

4 At the conclusion of the 1908 Season, Schumacher observed locations where the soft bedrock on the summit had undergone artificial shaping. "On the rock below the north Temple wall there are distinct marks of depressions where Israelite stones (masonry) have had [*sic*] been placed … we found last day 2 stones probably in situ, on the rock, placed into a cut of the soft māri rock" (*Schumacher Diary I,* 146; also *Fisher Diary I,* 60; one of the stones bore a mason's mark similar to ones known from Mutesellim/Megiddo; see also *Schumacher Diary II,* 153–54, for such activity in

palaeographic grounds, to date to the early eighth century BCE (close in time to the epigraphically similar Kuntillet ʿAjrud inscriptions, which may reflect Samaria's contemporaneous, state-sponsored connection to a distant caravanserai in northern Sinai). These inscriptions record small shipments of wine and oil to the capital from lineage-based communities in the surrounding countryside, including Shechem but not rebuilt Tirzah. Personal names (the so-called *l-men*) attested on the dockets belong either to absentee landlords temporarily residing in the royal compound at Samaria and receiving the commodities from their own local estates, or to clan heads who were sending tax payments to the king (see *AIS II,* 498, n. 163; Schloen 2001: 155–65, n. 41). Either way, these records corroborate Samaria's continuing wealth and centripetal force during the peaceful and prosperous reign of Jeroboam II.

As the number of villages increased on the seaward slopes of the Ephraimite hill country, sparsely populated Samaria preserved its concentration on the elite. With the rise of the Neo-Assyrian Empire in the decades following Jeroboam II's reign, however, Israelite rule from this royal enclave grew increasingly unstable. Competing leaders—such as Shallum, Menahem, Pekahiah, and Pekah (ca. 750 to 733 or 731 BCE; 2 Kgs 15:8–31)—descended into outright anarchy as they contested the right of rule. (All but one man, Pekahiah, the son of Menahem, seized the throne through regicide.) Eventually, the Assyrians took political control over the entire Ephraimite hill country and effectually transformed the highland region into the province of *Sa-me-ri-na-a-a* (Samerina).

Following Reisner's discovery of the Hebrew ostraca at Samaria in 1910, scholars tended to assign the corpus to one of the three main historical periods outlined above: the rising royal center under King Ahab; the long, affluent reign of Jeroboam II; or the ever darkening days under King Manasseh. Typically, however, they grounded their various arguments in historical and palaeographical considerations (or assumptions), not in detailed archaeological data drawn from the soil of Samaria.

B. The Backstory: Early Challenges for the Harvard Expedition[5]

By the outset of the twentieth century, American interest in the archaeological exploration of Palestine had led to the establishment of the American Schools of Oriental Research in 1900. Under the capable leadership of ASOR's first director, Charles C. Torrey, the new society acquired permanent quarters in Jerusalem, collected and maintained a credible library, and established positive working relations with other similar international institutions, such as the Palestine Exploration Fund and the École Biblique (Hallote 2011: 158–59). Torrey soon turned his attention to fund raising with an eye to launching a field project somewhere in the region. The Rev. James Buchanan Nies and Mrs. Jane Dows Nies showed immediate interest in helping to achieve this goal. Having visited many archaeological sites in Palestine, Rev. Nies eventually contributed articles to several of the mainstream, scientific journals of the day (e.g., Nies 1914; 1916; 1917; 1918; 1919). But already by 1901

Trench F). See also *Reisner Diary V,* 467, for a field sketch showing the juxtaposition of the western palace wall and the rock scarp. Another journal entry (Saturday, July 9, 1910) in *Reisner Diary V,* 469, indicates that the artificial shaping of the bedrock also extended along the southern side of the summit platform that supported the palace. At L.T.E. b (figs. 18–19a.b, below), a tunnel entered the scarp face and led to a chamber that ran to the east, apparently somewhere near Grid L.14–15 on *HES I,* Plan 5 (fig. 22). The function of the cave remains unclear. Reisner suggested it served as a wine cellar, hiding place, or tomb. The cave yielded mainly Roman-style lamps and a lead sling-shot but also "Seleucid and even Israelite potsherds and objects" (*Reisner Diary V,* 475–76, 478, 480). Similarly, Reisner recorded his first observation of the rock scarp on the northern side of the summit (*HES I,* Plan 5, Grid G-H.7) in *Reisner Diary V,* 479–80. (For a graphic presentation of the northern scarp, in the area just north of the Augusteum's great staircase, see published Section AB, Squares 3–8 and *Reisner Diary V,* 492.)

5 For information on the planning and funding of the Samaria expedition, see the annual reports by David G. Lyon, Curator of the Semitic Museum, to the President of Harvard College (Lyon 1906m: 308–309; 1908m: 304–306; 1909m: 307–309; 1910m: 277–81; 1911m: 227–29; 1912m: 224–25; and others, as cited below).

he had written about the benefits for any American institution that would sponsor and conduct archeological field work in Palestine (Nies 1901: 31–37). The Nies couple had substantial financial means,[6] and at the founding of the American Schools they pledged a sum of $50,000 for the excavation of Samaria (Hallote 2011: 161), which Rev. Nies counted among "the sites of cities of the highest importance to science" (Nies 1901: 33). Nies garnered funds from seventeen new donors, but despite his own substantial enthusiasm and resources he remained unable to obtain an excavation license from the Ottoman authorities in Istanbul (Cobb 1901: vi; Moore 1903: 34; the license was known as a *firman*; see Chs. 4, n. 10, and 6, n. 16).[7]

During this same period, Jakob Heinrich Schiff, a German-American banker, philanthropist, and Jewish leader, became the financial founder and most generous benefactor of the Semitic Museum of Harvard University (fig. 6).[8] Established in 1889 (very near the opening of the national archaeological museum in Istanbul, which would eventually house the Samaria Ostraca; see Ch. 4, n. 14), the Semitic Museum represented an emergent resource under the vision and care of Curator David Gordon Lyon (figs. 6, 7a.c) and reflected the growing interest in both Europe and America in the archaeology of eastern Mediterranean areas.[9] Construction of the Museum's permanent home on Divinity Avenue concluded in 1902 at an approximate cost of $80,000 (around 2.2 million dollars today). After recording the construction date and cost in his notes on the Harvard Class of 1889, Secretary James Hardy Ropes added that

> …the collections, illustrating the manners, customs, and history of the Semitic peoples, housed therein were begun ten years or more before. The building also contains a department library with a valuable collec-

6 Jane was the daughter of Alexander Ector Orr, a prominent New York businessman, and Juliet Buckingham Dows, heiress to her father's huge grain fortune, then the largest in the United States.

7 James and Jane Nies remained stalwart supporters of ASOR and the exploration of ancient sites in Palestine. By 1919, Jane Nies donated $50,000 to ASOR for the construction of permanent headquarters for the institute on a parcel of land lying north of the Damascus Gate, between the École Biblique (the Dominican Convent of St. Stephen) and Saint George's Cathedral, land purchased on behalf of the School in 1909 (*BASOR* 1919/1: 4; 1920/2: 3–4). Complications ensued, however, and not until April 1922 did Rev. Nies and Director W. F. Albright secure the services of architect Federick Ehmann. But Rev. Nies died in Jerusalem on June 18 of that year (Montgomery 1922: 3; Albright 1922: 12–13; see *The New York Times,* "Rev. Dr. James B. Nies, Orientalist, Dead," June 20, 1922), and the Nies estate eventually transferred the funds to ASOR in July 1924 (Montgomery 1922: 5–6; *BASOR* 1924/15: 1–2). Although it remains somewhat unclear when and where Jane Dows Nies died (an announcement appeared in *BASOR* 1919/1: 4), her generous gift allowed the construction and occupancy of much of the building over the course of the following year. Contractors completed "the central building, the left-hand wing, and the basement (covered over temporarily) of the right-hand wing" between October 1924 and October 1925 (*BASOR* 1925/19: 2; a map and three plans of the building follow p. 12). In memory of their benefactor, the Trustees of ASOR named their new home *The Jane Dows Nies Memorial Building.* In his will, Rev. Nies also bequeathed to ASOR the sum of $10,000 to establish the *Jane Dows Nies Publication Endowment* (see *BASOR* 1922/7: 3–4; Montgomery 1922: 7), which generated decades of support for the publication of the Society's *Annual.*

8 For Schiff's vital participation in and long-lasting commitment to the Semitic Museum at Harvard University, see Hallote 2009: 225–47. Schiff's heirs, particularly his daughter Frieda, continued the family support of work at Samaria, even into the 1930s (see Ch. 6, n. 5). For Jacob Schiff's contemporaneous and generous support of program development at other institutions of higher learning, including Barnard College, Columbia University, Cornell University, The Johns Hopkins University, and the University of Frankfort, see Adler 1921–1922: 43. Interestingly, both Schiff and Alexander Orr, the father of Jane Dows Nies (see above), became members of the so-called "Committee of Fifteen," a citizen's group that attempted in 1900–1901 to work with the New York Tenement House Commission to improve living conditions (especially for the children) in the tenement houses of New York City by eradicating certain social vices (particularly prostitution) (see *The New York Times,* "The Social Evil in Tenement Houses," March 25, 1901).

9 Note, for example, the establishment of The Palestine Exploration Fund (London, 1865), The Society of Biblical Archaeology (London, 1870), The Palestine Exploration Society (New York, 1870), Deutscher Palästina–Verein (Germany, 1877), École Biblique (France, 1890), American Schools of Oriental Research (United States, 1900), and the British School of Archaeology (England, 1919). For a review of these and related societies, see R. de Vaux 1970: 64–80; P. R. S. Moorey 1991: 1–53; et passim.

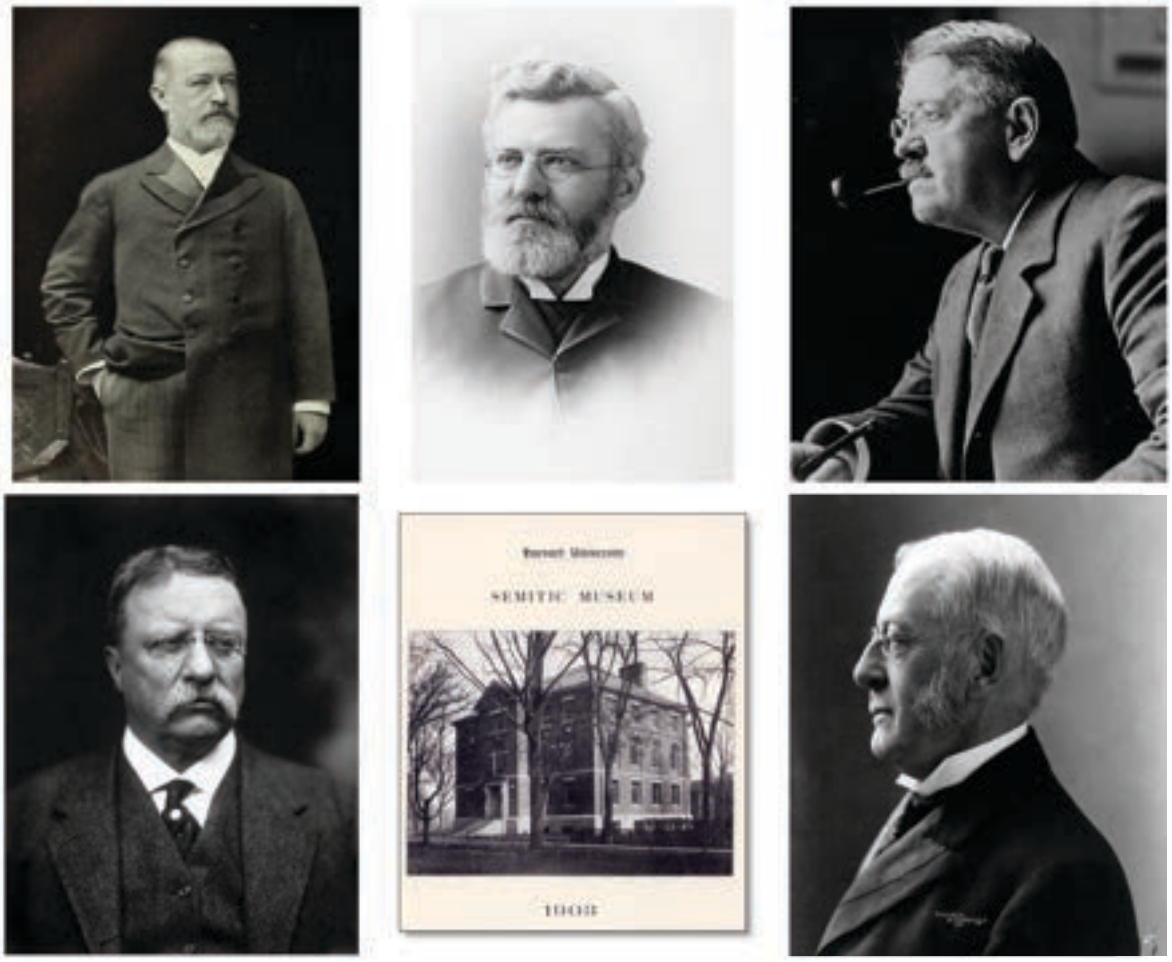


Fig. 6 *Key figures in the Harvard Expedition to Samaria (top, left-to-right: Jakob Henirich Schiff; David Gordon Lyon; George Andrew Reisner; bottom right: Charles W. Eliot; bottom left: Theodore Roosevelt).*

> tion of Arabic and Syriac manuscripts, and lecture rooms for the courses in Semitic subjects. Mr. Jacob H. Schiff of New York has been the steady patron of Semitic studies at Harvard, has contributed generously to the purchase of collections, gave the building, and gave the money necessary for important excavations at Samaria, which were carried on largely under the direction of our classmate, Reisner.
>
> (Ropes and Fisher 1914: 195)

In January 1905, Schiff offered the Museum $50,000 in support of a five-year excavation in Palestine. Although he initially pledged $10,000 per year, Schiff modified his terms in 1908 and deposited the full amount; he also allowed for the expenditure of more than $10,000 in a single year. He soon supplemented his 1905 gift with an additional $5,000 toward anticipated expenses for the initial application to Ottoman authorities. (By most inflation calculators, the total donation would equate to between 1.4–1.5 million dollars today; see Ch. 6, nn. 4, 7.)

Harvard quickly formed a steering panel, the Committee on Exploration in the Orient, which in turn appointed George Andrew Reisner as project director. Armed with this substantial financial backing and, this time, with letters of support from Charles W. Eliot, President of Harvard, and, indeed, from Theodore Roosevelt, President of the United States, Reisner arrived

Fig. 7 *Principal field staff of the inaugural 1908 campaign: a. David Gordon Lyon, Administrative Director (at Samieh, March 27, 1907; courtesy Harvard Semitic Museum); b. Gottlieb Schumacher, Inaugural Field Director (excavating dolmens on the Mount of Olives, February 25, 1907; photograph by D. G. Lyon; courtesy Harvard Semitic Museum); c. Gottlieb Schumacher (white suit), David Gordon Lyon, and Clarence Stanley Fisher (from right to left) standing in a trench at Samaria (courtesy Harvard Semitic Museum).*

in Constantinople in November 1905 and presented to the Ottoman Sultan a proposal to excavate at Samaria. Generally, such requests were granted only with approval from the Director of the Imperial Ottoman Museum in Constantinople. But despite the impressive patronage and further support from the American Minister to Turkey, the permit was not granted until the autumn of 1907.[10] Because the unfortunate delay had already exceeded time limitations imposed by Mr. Schiff,[11] in 1906 Reisner accepted an invitation by the Egyptian government to undertake a three-year period of work in that country (see Reisner 1910). Thus began his life-long association with the Harvard University–Boston Museum of Fine Arts Egyptian Expedition.

When the American proposal for Samaria finally gained approval, Mr. Schiff somewhat hesitantly renewed his offer—contingent on Reisner's presence at Samaria to oversee the initial planning of the project. Harvard then engaged Haifa resident Gottlieb Schumacher (fig. 7b.c) as field director, and former member of the Philadelphia Expedition to Nippur Clarence S. Fisher as architect (fig. 8). Working on behalf of the German Society for the Study of Palestine, Schumacher had excavated at strategically located Megiddo from 1903–1905, where he applied relatively rudimentary field methods to open a wide (20–25 m), north–south trench across the impressive mound. Reisner and Schumacher

10 Some confusion remains regarding the patronage behind the eventual work at Samaria, and the view that the 1908–1910 expedition, the first of its kind prior to World War I, was financed "by funds raised by Episcopal Minister James D. Nies" (Meyers 2001: 9) seems incorrect.

11 Schiff's offer came with the stipulation that Harvard would secure an excavation license from the Turkish authorities within six months (i.e., by the summer of 1905). When the initial trip to Istanbul ran beyond this time limit, he graciously extended the deadline to October 1906 (*HES I,* 3). As outlined above, the organizers failed to meet even that deadline.

Fig. 8 *Clarence Stanley Fisher, architect (photograph from Glueck 1941, courtesy of American Schools of Oriental Research; for photographs of Fisher later in life, at Kh. Et-Tannur 4 years before his death in 1941, see McKenzie et al. 2013, figs. 16–17, 19).*

met at Samaria on Friday, April 24, 1908, and outlined the scope and methods of the project. That this meeting transpired implies, then, that Reisner understood and approved Schumacher's field tactics prior to the start of work, and Schumacher's own journals confirm their agreement (*Schumacher Diary I,* 5). Beyond the excavation strategy, Reisner signed a power of attorney on May 9, 1908, granting Schumacher the right "to act on his behalf at the excavations" (*Lyon Diary I,* 18).

Excavation began on the very day of Reisner's meeting with Schumacher (April 24) but only five and a half days later was interrupted by rain as well as administrative and financial discord with local authorities. Work finally resumed in Trial Trench A from May 22–June 3, 1908, with sporadic interruptions, and continued again from July 11–August 21, now with 130 men and women (*Lyon Diary I*, 15, 20, 24, 59; *II,* 5; *III,* 25; *Schumacher Diary I,* 142; *Fisher Diary I,* 49). Thus began a three-year period of exploration of this famous capital city.

During the course of the field work, the Harvard team promptly disseminated the results of its findings to the scholarly community. A preliminary statement on the 1908 season appeared in October of that year (Lyon [?; unsigned] 1908), while a fuller report came in January (Lyon 1909). Similar updates and reports followed the 1909 fieldwork (Lyon 1910; Reisner 1910), and the ostraca received their own discussion in separate articles soon after the close of the project (Lyon 1911a; 1911b) and then in a detailed but undated report with drawings published by E. O. Cockayne in Boston (Reisner undated). Ultimately, further analysis emerged in the final, official excavation report (*HES I,* 227–46; also with drawings), which garnered immediate and favorable reviews (e.g., Smith 1924). Despite the laudable, two-volume report issued by Harvard in 1924, serious problems exist in both its presentation and omission of critical data relating to this epigraphic corpus, as outlined below (see Ch. 3.A).

C. The Harvard Expedition: Excavation Strategy, Methods, and Ensuing Challenges

1908 Fieldwork

During the nine weeks of actual digging in 1908, the excavation focused on a row of standing columns (fig. 9a.b.c) leading to the site and began cutting a series of connecting trenches, labelled E-F-G (fig. 10), across the western summit and slope (an approach that echoed, in some ways, Schumacher's previous strategy at Megiddo).[12] These tandem cuts ran diagonally (southwest to northeast) from excavation grid B.11 to K.7, and the director intended by means of their joint exposure to clarify the basic depositional history, i.e., the chronology, of remains at Samaria. As the wide, diagonal swath stretched farther across the summit of the site, spectacular discoveries ap-

12 Lyon's private records indicated that Schumacher had opened seven trial trenches by the end of April 1908 (*Lyon Diary I,* 21–23). Two unpublished field journals kept by Clarence Fisher provide much more technical detail and contain many more drawings than does Lyon's laconic account of Schumacher's 1908 work that appears in *HES I,* 3–27. In 1909, when Reisner became the on-site field director and, later, when he compiled and published the full excavation report, he made little effort to correlate Lyon's statement from 1908 with the main body of work dealing with the 1909–1910 Seasons.

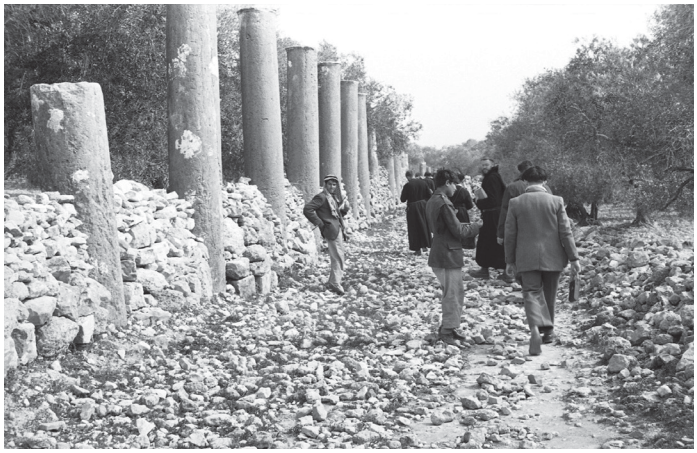


Fig. 9 *Samaria's columned access road, with Bazaar area: a. between 1890–1900 CE (Library of Congress, Prints & Photographs Division; reproduction number LC-DIG-ppmsca-02736); b. in 1954 (photograph by Leo Boer; courtesy of B. Wagemakers; photograph from Gibson 2014: 65, fig. 4.5); c. in modern day (see green shading in fig. 21a, below).*

peared. With a contingent of unskilled local workers that varied between 150 and 400 men, women, and children, Schumacher managed to expose a dedicatory altar, monumental stairway, and forecourt of a large, Herodian temple complex on the summit (fig. 11). The great staircase emerged in Trench G (figs. 10, 12a–e),[13] and a "large, deep, vaulted chamber," measuring 6.7 by 12.5 m, appeared below its western side (fig

13 See *HES II,* Plan 8, Grids F-G-H.7–10; for a sectional view of the steps, see *Fisher Diary I,* 67, and *II,* 29; for another section and a plan of the stairs, see *Schumacher Diary I,* 47; for a plan, see *Fisher Diary I,* 87; for notes on the altar at the foot of the stairs, see *Fisher Diary I,* 71; II, 29; also *Lyon Diary II,* 79–80, plus 89 and 92 for dimensions; for the vault, see *I,* 76, 82–83; *II,* 26–28; on the exposure of the stairway, see *Lyon Diary II,* 13–14). When Schumacher returned to Samaria in early July 1908 from a trip to Istanbul, he discovered significant vandalism on and around the great staircase, "unlawful things" thrown into excavation trenches, and surveyor's benchmarks that had been uprooted and thrown away. As a result, he requested that local officials send a soldier to the site (*Schumacher Diary I,* 65, 68, 71, 73).

On Monday, July 27, 1908, Lyon recorded the discovery of a marble statue of an emperor—presumably Augustus—in Trench G (see fig. 10; *Schumacher Diary I,* 96, 99). It lay north of the lowest step, with its back parallel to the stairway. Lyon concluded that the figure had fallen from an original position either on the 19th step of the stairway (*Lyon Diary II,* 43–44, 46) or on the altar located "exactly north of the middle of the stairway" (*II,* 79–80; cf. *III,* 12; for photographs of the statue and base, see *HES II,* pl. 79.e-f; for dimensions of the statue, see *Lyon Diary II,* 48–49; for its pedestal base, see *Schumacher Diary I,* 118). Later, on August 10, 1908, workers recovered the palm and three fingers of a marble hand, "doubtless from [the] Augustus statue" (*Lyon Diary II,* 90), from the area between the altar and stairway, and on August 20 workers found its marble head near the southern edge of the summit (*Lyon Diary III,* 22–23). Amazingly, at the conclusion of the 1908 Season, Lyon recorded that "the great statue is to be boxed and left where it lies for the present, its removal being a work of much

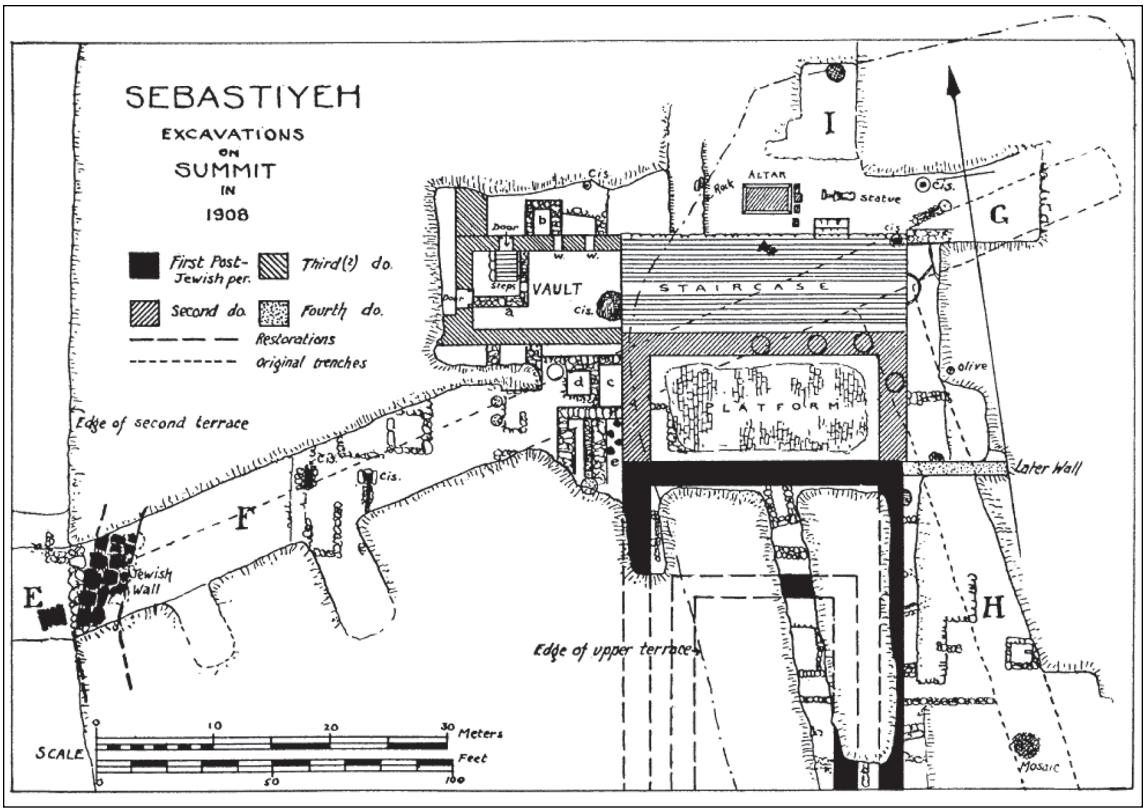


Fig. 10 *Schumacher's oblique summit cut (Trenches E, F, and G).*

13a.b). Ancient workers had cut the eastern portion of this vault into a scarp of native rock, while masons constructed the remaining three sides with massive blocks of dressed stone coated in heavy plaster. Doorways gave access to the barrel vault from both the north and the west,[14] and two cisterns were cut beneath the chamber's floor.

In Trench E, approximately 54 m southwest of the stairway, Schumacher penetrated (ca. 9.5 m!; fig. 14) to bedrock in Grid B.11 by July 23, 1908, and there found several hewn stones bearing letters or mason marks that he understood as Hebrew *záyin* and *tāw* (fig. 15a.b; see *Lyon Diary II,* 32–33; cf. *Schumacher Diary I,* 86, 91–92, 136, 138, 141,

difficulty and expense" (*Lyon Diary III,* 32).

During the 1908 Season, Schumacher had worked with only 50 or so workers daily (and even fewer during harvest season), many of whom were "of very inferior quality" (see, for example, *Lyon Diary I,* 26, 28, 37, 42, 47, 56, 59, et passim). Following an unwanted hiatus in the excavation owing to misunderstandings with the local leadership, the number of workers steadily rose when Lyon (and Schumacher) restarted the fieldwork on July 11, 1908 (e.g., 119 laborers on July 11, 166 on July 13, 170 on July 14, etc. [*Lyon Diary II,* 5–9]). By Tuesday, July 28, 1908, the daily force had expanded to 393 diggers and peaked at 441 by August 5—a size deemed by Lyon to lie well beyond any hope of "adequate supervision" and recording (*Lyon Diary II,* 34, 37, 42, 63, 66, 86–87; *III,* 11; cf. *Schumacher Diary I,* 114). Having concluded by this point that Reisner would replace Schumacher in the role of field director, and having informed Schumacher of that decision, Lyon henceforth began assuming greater control of field activities. Lyon also apparently passed his determination by President Charles Eliot at Harvard, for by September 8, 1908, Reisner telegrammed Lyon stating that he had received a cable from Eliot "to the effect that Reisner is to have charge at Samaria next year" (*Lyon Diary III,* 44). The President of the University, then, seems at this point to anticipate and approve a second season of fieldwork. On May 24, 1909, when Reisner requested a transfer of funds adequate to engage 300 workers, Lyon complied without objection or reservation (*Lyon Diary III,* 68), perhaps signaling a greater confidence in the new field director.

14 A flight of steps gave access to the chamber from the north. (Although, in his report, Reisner placed the steps on the south [*HES I,* 173], photographs in *HES II,* pls. 22a and 24b, appear to locate them on the north, as does *Lyon Diary II,* 73; compare also *Fisher Diary II,* 26).

Fig. 11 *Aerial: the altar, grand stairway, and general area of the Augusteum (courtesy of Sonia Halliday Photo Library, Gregory House, Oxford).*

and *HES I,* 8; *HES II,* pls. 87e, 90e).[15] On the early end, Schumacher exposed a wine- or oil-press and several "funnel-shaped holes" cut into the rock surface east of the stairway (fig. 16). In a prescient moment, he (or perhaps Lyon) concluded that "the press was of early origin, and probably antedates Omri's buildings on the site" (*HES I,* 10; compare now Stager 1990 and Franklin 2004).

Following these discoveries, the excavators projected that the earliest, major Israelite stratum lay directly on bedrock and dated to the ninth century BCE, the time of King Ahab (*HES I,* 11–12). Schumacher recorded a heavy accumulation of black debris that overlay the Israelite level and that apparently dated to sometime beyond the Iron Age. The pottery, objects, and coins recovered from this deposit indicated that "the site was continuously occupied by a considerable population from before 400 B.C. to about 100 B.C. and was probably inhabited before 500 B.C." (*HES I,* 55). Except for a substantial city wall and a thick-walled house in Summit Strip 1, however, no coherent architectural elements survived from this lengthy period—and most notably from its earliest phase, spanning the seventh to the fifth-fourth centuries BCE, when Schumacher believed the Greek Fort Wall was built (but see Ch. 3, n. 10; also *HES II,* Plan 7, which shows a number of additional wall fragments).

15 For a plot of Trench E, see *Schumacher Diary I,* 86, and *I,* 136, for a sketch of the "Israelite stones"; for a plan of the area, see Lyon 1909, fig. 3; cf. *HES II,* Plan 14.

Fig. 12a–b *Trench G and the Augusteum's grand staircase: a. at the outset of digging (*HES II, *pl. 85g); b. diagonal swath across staircase (*HES II, *pl. 86a).*

Fig. 12c–d *Trench G and the Augusteum's grand staircase: c. the grand stairway cleared (*HES II, *pl. 86b); d. altar, stele and bases, floor, and staircase (*HES II, *pl. 18c).*

Fig. 12e *Trench G and the Augusteum's grand staircase: e.1. stairway to Roman temple (Library of Congress, Prints & Photographs Division; reproduction number, LC-DIG-matpc-06981); e.2. Schumacher's plan and section of the staircase (*Schumacher Diary I, *47).*

1909 Fieldwork

When Reisner returned and assumed the duties of on-site field director in 1909,[16] he brought with him a more experienced staff of Egyptian supervisors, photographers, and assistants to oversee the work of the locals, whom he continued to draw mainly from Sebaste and surrounding villages (fig. 17a–e; see *HES I,* 31; also Ch. 6, n. 16). Work began on Monday, May 31, 1909 (compare *Lyon Diary III,* 69 and 72), and the quality of both excavation and record-keeping improved considerably. (For example, Schumacher took 483 photographs

16 Though Lyon's private diaries are replete with signs of a strained relationship with Schumacher over the course of the 1908 Season, Schumacher rarely alludes to such problems. On Thursday, July 9, 1908, however, he recorded a conversation in which Lyon informed him "confidentially" that Reisner "intended to resign his position in Egypt and would likely take charge of the Sebastiye works next year" (*Schumacher Diary I,* 71). The news seems to have troubled Schumacher, for after retiring that evening he "had a restless night" (*I,* 72). Afterward, the

Fig. 13 *Vault beneath western side of Augusteum's grand staircase: a. temple vault, looking ESE. August 25, 1908 (*HES II, *pl. 22a); b. vault cleared to hard floor, looking E. May 25, 1909 (*HES II, *pl. 24b).*

schism began to widen. On Tuesday, August 4, 1908, Schumacher recorded privately that he

> Asked Prof. Lyon to correct a few sentences in his report to Harvard[,] especially regarding my nomination as Chief of the Excavations. He had said that I "offered" my services, while I have been able to state according to his letter of Dec. 11th and July 4th and 9th and 16th that the position was offered to me by the Committee and I further stated that it was not supposed to be a provisional one, for Prof. Lyon writes to me in his letter of July 4th after detailing the money situation "As you will see we cannot say how long your services will be needed but we hope for a term of years" (*Schumacher Diary I,* 107–8).

in 1908; Reisner shot 1,345 in 1909 and 1,093 in 1910 [see *HES I,* 45 and *Reisner Diary VII,* 701].)[17] Reisner divided his work force into nine "gangs" (*Reisner Diary IV,* 411), with each consisting of three to five pickmen under one to two Egyptian supervisors, five to eight hoemen under one to two Egyptians, and 12 to 25 basket carriers directed by four Egyptians. Gangs worked in specifically assigned excavation areas of 50–100 square meters (*HES I,* 33). Thus anywhere from 26 to 46 people labored together in relatively confined quarters (two to four five-meter squares today).[18] This situation surely made for cramped and chaotic working conditions, even though it may have represented an actual improvement over the labor force and strategies employed in the previous season by Schumacher.

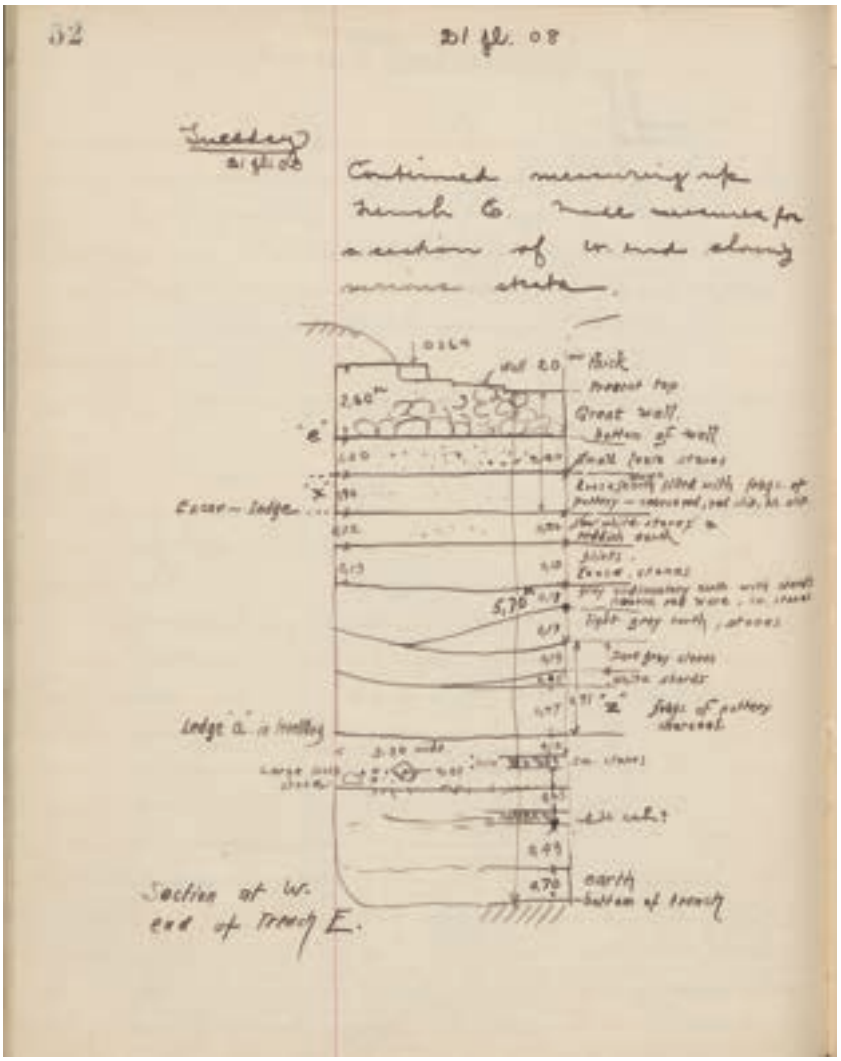


Fig. 14 *Clarence Stanley Fisher's section drawing of remains in Trench E (*Fisher Diary I, *52).*

17 One journal entry at the conclusion of the 1909 Season reveals both Reisner's concern for detail and his frustration with the photographic equipment: "Photography went steadily all day. Big camera of last year very weak and shaky—has cost me six big plates which I can ill afford to lose, getting a knowledge of its bad prints. The American dealers invariably recommend these complicated weak unstable cameras to explorers; and they ought to be warned of the injury they are thereby doing to the pursuit of knowledge. The big camera is so weak that when photographing vertically downwards the back sags out of focus with the mere weight of the double-back and the plates. As for out-of-doors photography in wind and weather, it is about as useful as a feather fan in a rain storm. Anthony in New York played me exactly the same trick with the first camera I bought for work in Egypt. The lens was good; but I threw the camera part away as soon as I could get a new one (after the first season)" (*Reisner Diary IV,* 348–49; Reisner's underscoring). Later, another journal entry written near the close of the field portion of the project (Sunday, October 16, 1910) indicates that Reisner experienced "great difficulty" in photographing the Israelite ostraca (*Reisner Diary VII,* 645). By 1924, Reisner had written his own manual on archaeological field methods, and Der Manuelian (1992: 4–34) has now published the portion of this handbook ("6. Photographic Record") that reflects Reisner's painstaking attention to on-site photography.

18 The availability of local laborers becomes apparent in a journal entry on Monday, June 13, near the start of the 1910 Season: "Over 1000 people appeared this morning seeking work. Over 100 letters received asking for work" (*Reisner Diary IV,* 403).

To maximize time and output, it appears that this large work force spent very long days in the field, as one journal entry indicates that quitting time generally did not arrive until 5:45 p.m. Reisner adjusted this schedule to 5:15 p.m. during Ramadan, though with a shortened pause at noon in the expectation of a full day's work, "fasting or not" (*Reisner Diary VI,* 571). Late in the 1909 Season, in October, Reisner even attempted to quicken the pace and interest in productive output by paying "a mettalik for each object of interest" recovered by a local worker (*Reisner Diary III,* 289; Reisner's underscoring). But within a few days (October 8, 1909), the downside of this strategy manifested itself when workers began producing coins they pretended to find (*Reisner Diary III,* 308). When he eventually had to allow a number of his Egyptian supervisors to return home, he recorded in his journal, "The Egyptians left are the very pick of the lot and they are making it a point of honor that there shall be an *increase* in the work done instead of a diminution" (*Reisner Diary III,* 302; italics added). On Monday, October 18, 1909, Reisner wrote: "Wakened by the thud, thud, thud of the baskets of dirt dumping in the new rubbish heap. They are going out faster than one a second (rate of over 200 tons a day)" (*Reisner Diary III,* 325). Thus the size and capability of the overall team and the rapidity of work might well have compromised the precision of both discovery and recording.

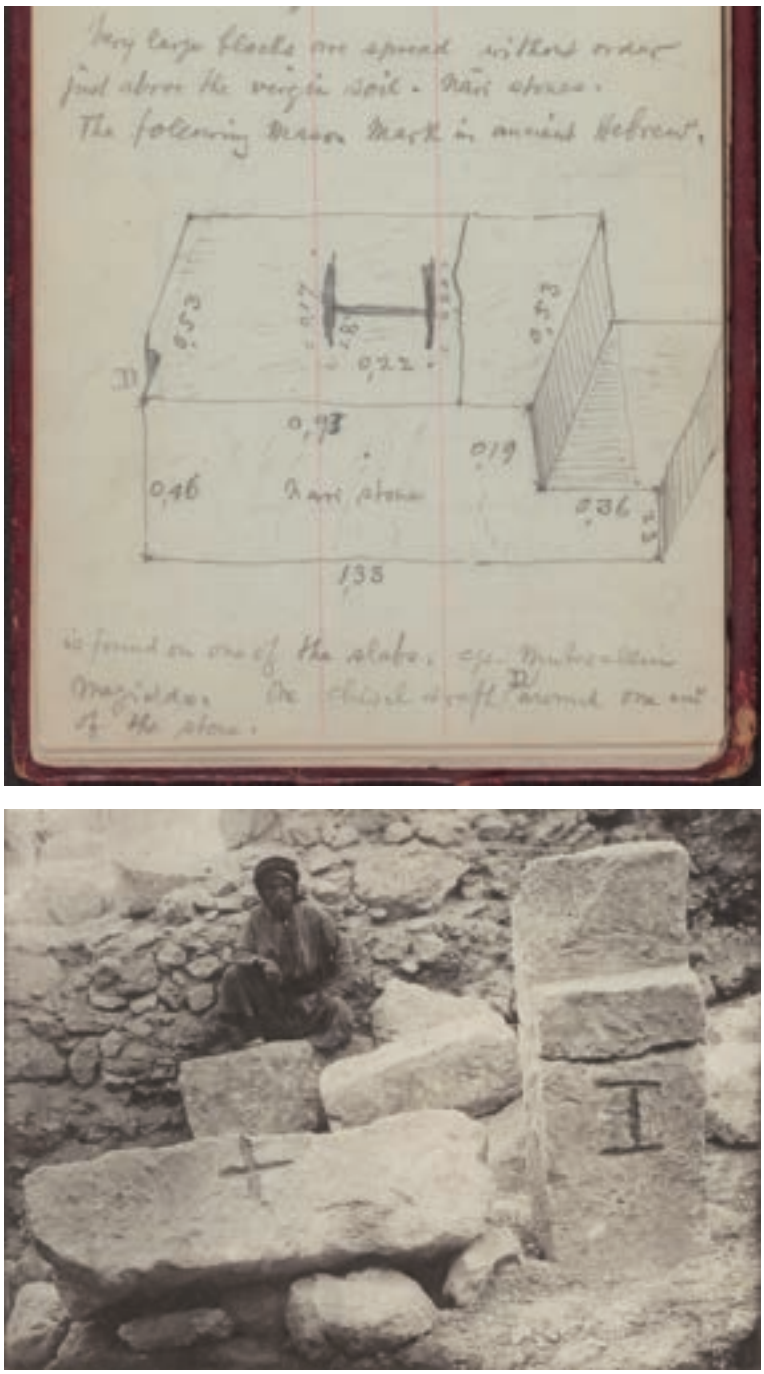


Fig. 15 *Early Signs of Israelite Levels: a. Schumacher's drawing of a building block with Hebrew letter/mason's mark (Schumacher Diary I, 92); b. local worker with incised block from Trench E (*HES II, *pl. 90e).*

Despite striving to upgrade his supervisory staff's level of experience, Reisner faced other severe challenges. Because he had to pay for the land he excavated (see Lyon 1909, fig. 1; cf. *Reisner Diary VII,* 641) and also to refill each excavation area and restore it to suitable agricultural use, he devised the so-called "strip system"

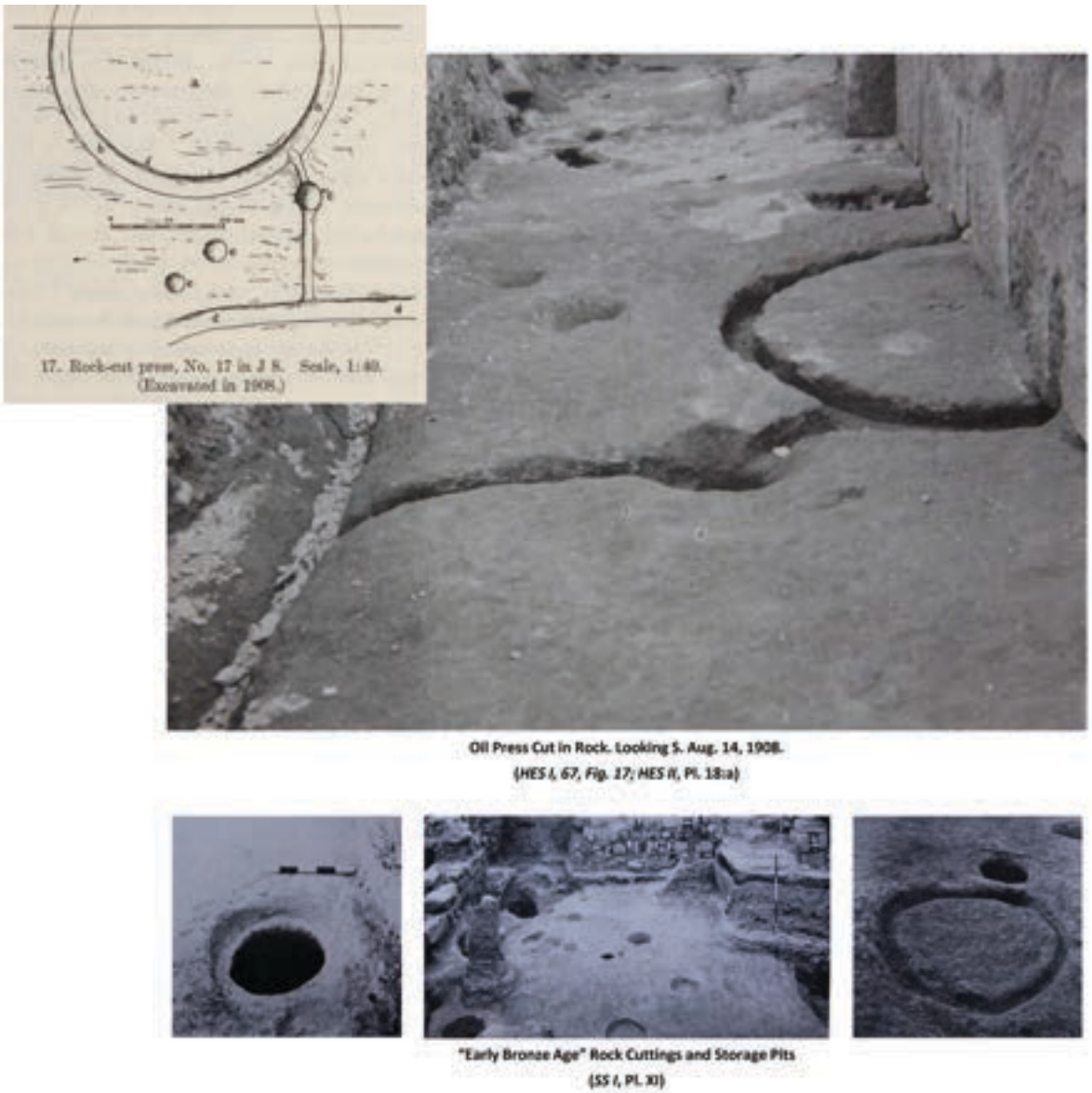


Fig. 16 *Pre-Omride oil presses and other rock cuttings: a.* HES I, *67;* HES II, *pl. 18a; b. rock cuttings and storage pits originally identified as "Early Bronze Age" (*SS I, *pl. XI).*

of digging (fig. 18).[19] That is, he divided the summit into a series of numbered strips and augmented this plan with a broad trench-cut down the southwestern slope of the site and across the Herodian Road of Columns (figs. 18–19a.b). After spreading the earth removed from the trench across a nearby olive grove (to a depth of ca. 1 meter), workers sent the soil from Summit Strips 1 and 3 down into this so-called Lower Terrace trench. (One of Schumacher's 1908 dumps, which overlay Summit Strip 3, went into the Roman vault west of the great staircase [*Reisner Diary II,* 170]; see figs. 10, 18, 21b, with the two dumps depicted on 16b having been laid out by Schumacher on Wednesday,

19 Reisner's unpublished record of the final financial settlements relating to nine separate deeds and other contracts appears in *Reisner Diary VII,* 655–83. (A later reference mentions eleven deeds; *VII,* 694.) Apparently, Lyon and Schumacher had settled the original terms and schedule of payment for land and trees in an official document (called a *mazbata*) as early as October 1906 (*Lyon Diary I,* 8). Blackmail attempts by the local *mutesarrif* to alter the details

July 8, 1908 [*Lyon Diary II,* 1].) This scheme left Strips 1 and 3 open and available to receive the soil lifted from Strips 2 and 4/5, respectively. But the unpublished field journals tell a more detailed story with regard to Strip 4, the area of the Ostraca House. Because of his desire also to leave Strip 2 open at the conclusion of the 1909 Season, Reisner adjusted his dumping strategy for Strip 4 (*Reisner Diary III,* 328). Workers poured the excavated soil from Strip 4's longitudinal sectors (f–g) "over the enclosure wall to L.T.," i.e., over what came to be recognized as the Casemate System and into the Lower Terrace trench running down the southern slope just west of the rectilinear tower (*HES II,* Plan 5; figs. 18, 19a.b, 22).[20] The debris from latitudinal sectors 4.a–e, in turn, went to the west, past what would become Summit Strip 5, and down the slope into Schumacher's old Trench F (fig. 10) from the 1908 Season (see *HES II,* pl. 87.f).

Thereafter, as the excavation progressed northward across the summit, the soil from each newly exposed area refilled the previously excavated strip (9 into 8; 10 into 9; 11 into 10; and so on). Ultimately, this strategy resulted in eleven distinct excavation areas (or "working sections") on the summit of the site, plus the Lower Trench on the southwestern slopes and a subterranean corridor that ran beneath Summit Strips 8, 9, and 11 in the northwestern corner of the compound. Given the overall layout and numbering of the summit strips, the earth removed from Strip 7—which included the area inside and north of the so-called Osorkon House—was dumped into and over the previously exposed remains of the Ostraca House in adjacent Strip 4 (and possibly also into 5). Workers finished clearing Strip 4 and began backfilling it on Thursday, August 18, 1910 (*Reisner Diary V,* 535–36). In short, the adopted procedure reburied the Ostraca House beneath the displaced, jumbled, and contaminated deposits taken from the area to its north. While this method allowed Reisner to leave open Summit Strip 2 (which included the bulk of the Israelite palace high on the rock crest above the steep scarp, Pavement 13, and Rooms 81 and 83—all west of the palace; see fig. 22), it ultimately produced a kind of modern but staggered and inverted (and contaminated!) depositional history across the western portion of the summit and thereby rendered any future exploration of this area problematic in the extreme, perhaps even hopeless.[21]

of the agreement began early in the first season of fieldwork, on April 30 (*Lyon Diary I,* 15–16), and throughout his diary Lyon records that he sent letters concerning the dispute over paying owners for trees and land even to Charles Eliot, President of Harvard University (*Lyon Diary I,* 35, 39, 49). Meanwhile, Schumacher drew up a precise plan for each tract of land acquired by the expedition. (Survey plots appear in *Schumacher Diary I,* 3, 6, 36, 70, 88–89, 104, and on two displaced pages following page 146.) As time passed for the project, and despite the fact that the *mutesarrif* had concluded that "no commission could legally be appointed to re-value our land occupied, and that our *mazbata* would hold good for all times" (*Schumacher Diary I,* 108), the negotiations over prices and signatures clearly grew tortuous (see *Lyon Diary I,* 44–49). By Thursday, November 3, 1910, after resolving the eighth round of arbitrations, Reisner confessed privately that "it is impossible to give an idea of the wearisome talk that accompanies all these negotiations" (*Reisner Diary VII,* 676). Importantly, these extensive proceedings strongly suggest that he already knew that another field season would not occur under his directorship. In any event, he subsequently noted that "if the work is continued, an entirely new form of contract must be used" (*VII,* 684) and that their return to Samaria was "too problematical" (*VII,* 694) to justify keeping the house the expedition had used since the inception of the project in 1908. (The house was apparently situated near the western limit of the village of Sebastiyeh and belonged to the Baptist Mission Society in London, for whom Dr. Gascoin Wright from the English Hospital served as local agent; see *Lyon Diary I,* 65; *II,* 91. See *Fisher Diary I,* 2–3, for a description and floor plan of this structure; see also *Lyon Diary I,* 11–13.) Near the end of his private journals, on Thursday, December 22, 1910, while in Constantinople, Reisner recorded that "Mr. Schimarmian [sp.?] will make an application for a new permission to dig at Sebaste and forward it to me at Cairo. I hope before I leave for America to get directions from the committee [i.e., The Committee on Exploration in the Orient, at Harvard University] as to whether I am to make the application or not. I can then forward it with the map from Cairo" (*Reisner Diary VII,* 712; Reisner's underscoring).

20 The hollow, casemate system had begun to appear in Trench E along the western perimeter of the summit already during the 1908 Season (*Schumacher Diary I,* 138–41).

21 On occasion, exposed tracts of excavation also served as the dumping ground for stores of pottery from previous seasons (e.g., *Reisner Diary VI,* 545, where pottery collected during 1908–1909 was poured into Strip 6 in 1910).

Fig. 17a–b *A Cast of Hundreds: a. Strip 7, clearing a Herodian floor, looking E. August 10, 1910 (*HES II, *pl. 53e); b. Northern views, unearthing Ahab's palace, circa. 1900–1920 (Library of Congress, Prints & Photographs Division; reproduction number LC-DIG-matpc-22580).*

Fig. 17c–d *A Cast of Hundreds: c. Strip 6, northern part, looking NE. August 17, 1910 (*HES II, *pl. 20c); d. a supervisor and his basket carriers (north face of vaulted chamber, showing window. Looking S. of W. July 28, 1908).*

Fig. 17e *A Cast of Hundreds: e. Northern views, clearing away debris (Library of Congress, Prints & Photographs Division; reproduction number LC-DIG-matpc-22588).*

Rather than adopting Schumacher's trenching technique, which the latter had executed at Megiddo and in the 1908 Season at Samaria, Reisner chose to expose wide tracts of horizontal space. (Later on, Kenyon more or less reverted to Schumacher's trenching approach.) But because Reisner immediately recognized the complexity of the site—that it "presented a mass of broken horizontal and vertical strata" (fig. 20a.b; *HES I,* 42)—he soon decided (perhaps under persuasion from draftsman Fisher) to excavate by architecture rather than by depositional layers. Workers excavated down along the face of walls until they reached a perceived floor level, then cleared the deposit horizontally away from the wall at that elevation (*HES I,* 42). This tactic would prove a pivotal one in Reisner's subsequent analysis of the ostensible Ostraca House floors.

1910 Fieldwork

The third season of fieldwork at Samaria began on June 15, 1910 (*Lyon Diary III,* 88). By then, Reisner had concluded that King Ahab extended the area of Omri's original palace northward and westward. After viewing the long, north–south wall in Grids F.9–14 (unlabeled in Fisher's Plan 5 but identified in the legend as "Ahab Main Wall" [hereafter "Wall A"; fig. 22]) as the western limit of the palace annex, he perceived that all the floor levels inside (east of) that wall and north of the original main building had suffered destruction and removal during the construction of the nearby Herodian temple. Whatever the original date and function of this long wall, the feature proves expedient to a study of the Ostraca House, since it provides an anchor point on various lateral sections that were cut and drawn through the Ostraca House area (e.g., see the lower right-hand corner of *HES I,* 63, fig. 14). Reisner understood the area west of this wall as part of Ahab's palace courtyard. And, as noted, of all the amazing discoveries spanning the Iron Age through the Late Roman (Severan) period, he felt that this area—precisely that of the Ostraca House—"yielded the most interesting material found on the whole site" (*HES I,* 62).

The upper courses along the western face of Wall A were dressed down to 20–100 cm above bedrock (see Ch. 2, fig. 25). Despite the apparently erratic

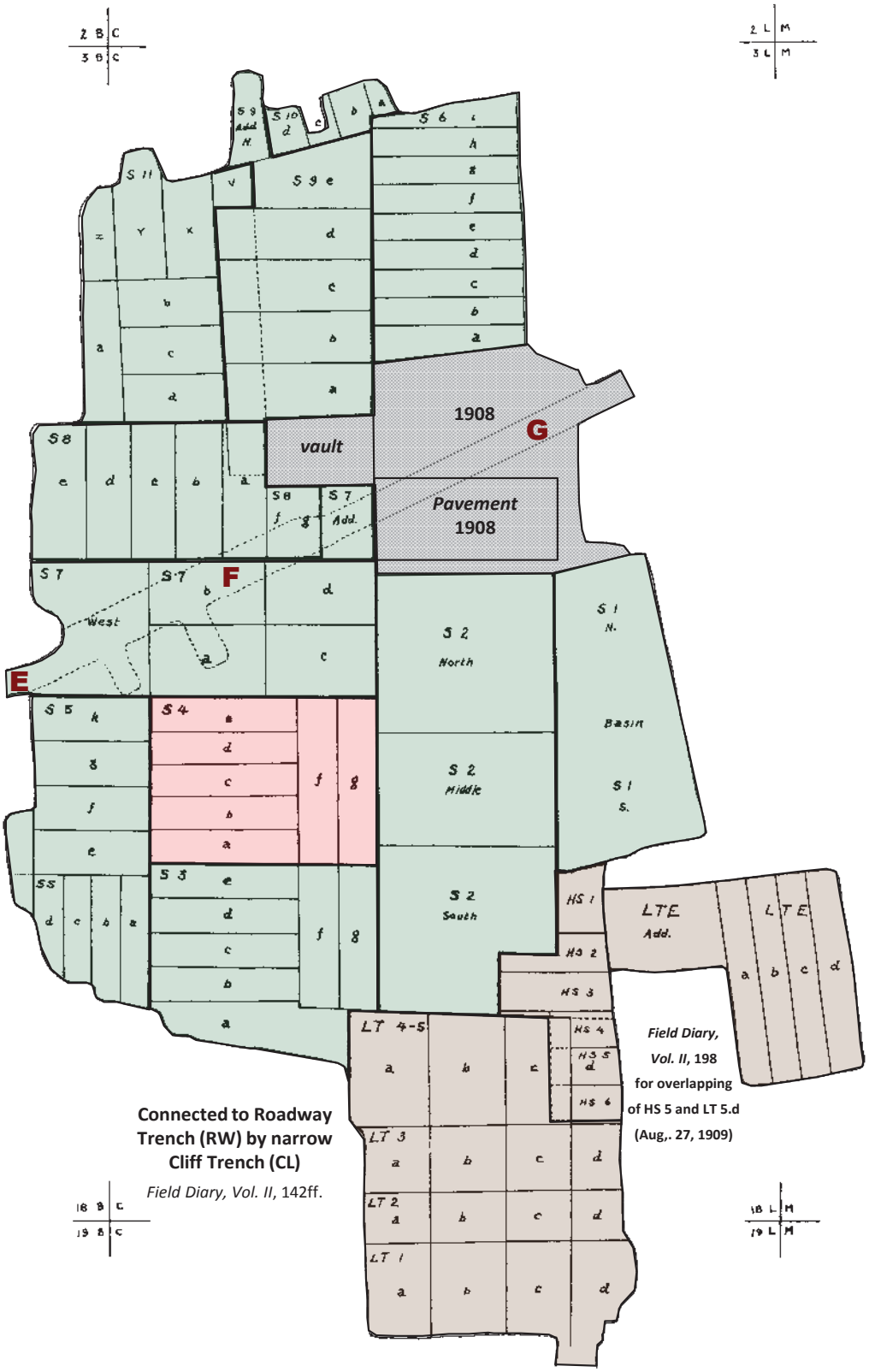


Fig. 18 *Reisner's "strip system" across the western summit and southern slope (adapted from* HES II, *Plan 2).*

Fig. 19 *The Lower Terrace Trenches: a. Sectors 2–5, looking W. September 15, 1909 (*HES II, *pl. 16a); b. Sector 5, looking W. September 27, 1909 (*HES II, *pl. 16b).*

Fig. 20 *Complex stratigraphy and phasing on the summit of Samaria: a. Strip 8–840, remains of Herodian stair, looking S. September 23, 1910 (*HES II, *pl. 40c); b. Strip 2, cleared to rock, looking N from S wall of apse. July 2, 1910 (*HES II, *pl. 11b).*

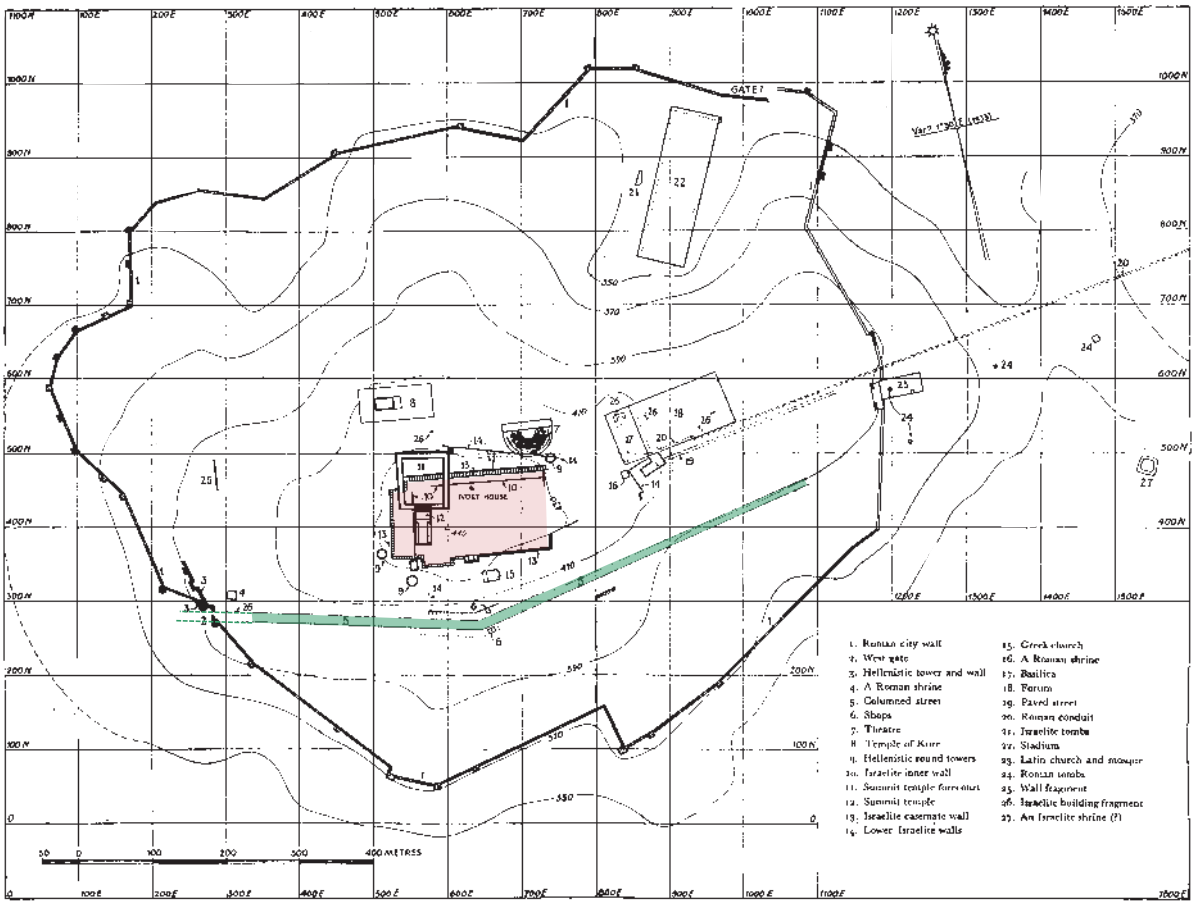


Fig. 21a *The Joint Expedition to Samaria: site plan showing columned road on the south (*SS I, *pl. I).*

nature of this line, Reisner recorded that a hard-trodden floor, 10–40 cm thick, extended westward from this mark and through excavation squares D-E-F.12–13–14, i.e., across the entire breadth of the Ostraca House (cf. *HES II,* Plan 5; see shading in fig. 22). He described the floor as "an accumulation of fine black debris" and saw in its thickness the passing of "a considerable period of time." Notably, this deposit purportedly yielded the Israelite ostraca, the Osorkon vase, Scarab No. 3715, an ivory handle with *uraeus,* a lion-headed ivory dagger handle No. 3862, and "an abundance of Israelite potsherds" (*HES I,* 62–63).[22] The veracity of this statement will become more specious as our study progresses. In any event, immediately beneath this ostensible floor level lay a 20–100 cm-thick, heterogeneous matrix of "dirty yellow" fill—a mix of brownish surface earth and yellowish mason's debris. And below this mottled filling, a deposit (10–40 cm) of "clean yellow mason's debris" rested directly on bedrock. The top striations of this construction debris formed, according to Reisner, a trodden surface that had apparently resulted from the considerable foot traffic of the ancient workers. Importantly,

> The courtyard contained a series of roughly built rooms, perhaps magazines or storerooms, the walls of which *pierced through the mason's debris* [i.e., the clean yellow deposit], but were *in position when the filling of the courtyard* [i.e., the dirty yellow deposit] *was made.*
>
> (*HES I,* 63; italics added)

22 Based on these ivory objects, Reisner privately anticipated the future discovery of additional, similar items "somewhere on the hill" (*Reisner Diary V,* 521)—an expectation that came to pass during the work of the Joint Expedition in the 1930s (see fig. 5).

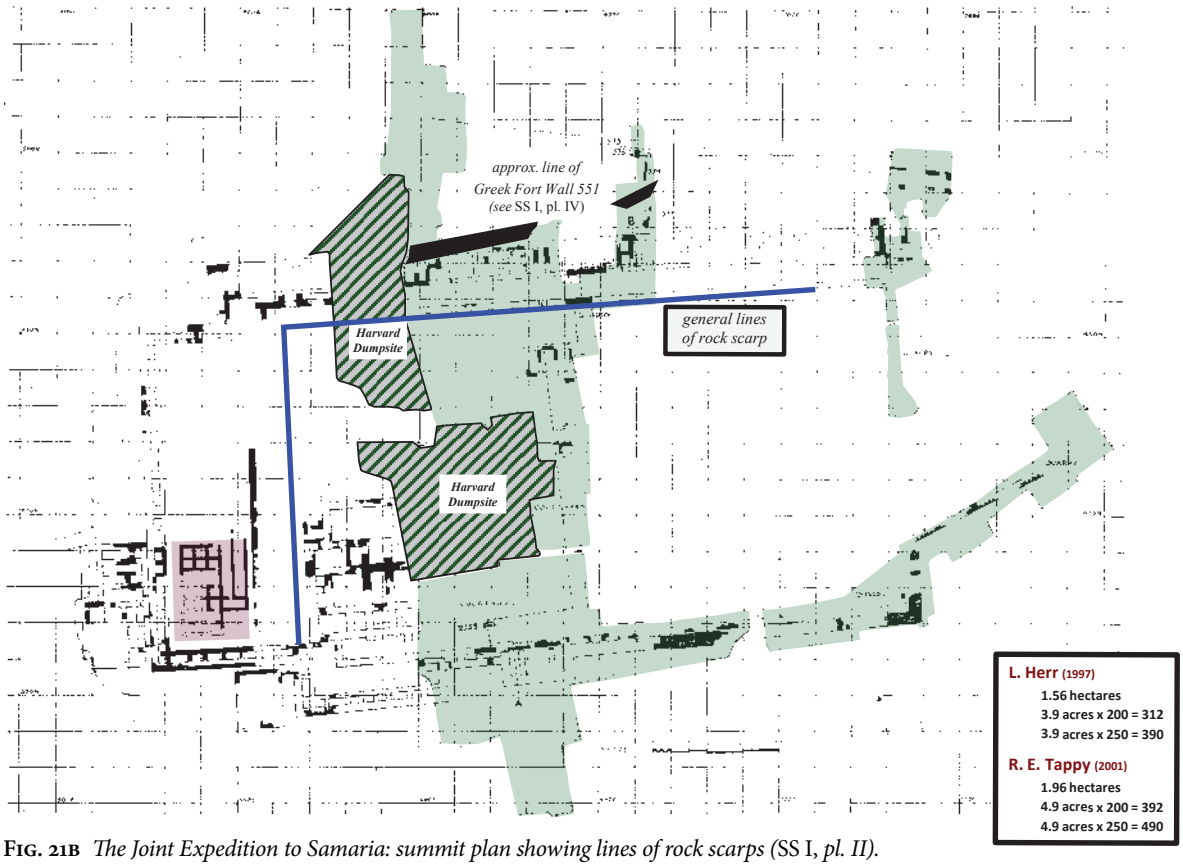


Fig. 21B *The Joint Expedition to Samaria: summit plan showing lines of rock scarps* (SS I, *pl. II).*

In his journal entries of August 1, 1910, Reisner understood that this loosely packed, dirty yellow matrix also covered architecture belonging to the later Greek/Hellenistic period (see *Reisner Diary V,* 504). After cutting through the floors of the Roman Atrium House (Rooms 345–356), there appeared two successive floorings. The walls of the earlier phase (which lay either at 120 cm [*Reisner Diary V,* 504] or 150 cm [*V,* 498; see Ch. 3, n. 5] below the Roman level) seemed to have been "filled and covered with [dirty yellow] dirt excavated from deposits of Israelite debris (for example possibly from the foundation trenches of the Herodian walls). In this yellow debris, [there lay] a great deal of coarse red pottery pre-Seleucide, also molds and copper slag" (*Reisner Diary V,* 504–505; Reisner's underscoring). Field sketches of some of this pottery reveal Iron-Age-style jugs, jars, and lamps as well as straight-sided bowls in brown ware and red wash, similar to the vessel types from which the ostraca fragments themselves derived. Then, by August 3, it became clear that, in fact, no fewer than four floor levels were traceable in the northern portion of Summit Strip 4, and Reisner now identified these disparate strata as (1) Roman, (2) Seleucid I, at 30 cm below the later surface, (3) Seleucid II, 60 cm below the Roman level, and (4) the "main series containing yellow dirt," at a depth of 120 cm below the Roman Atrium floors (*Reisner Diary V,* 506). His notes go on to say that the yellow dirt yielded a large number of pottery vessels as well as a flat, green-glazed scarab which "is not Egyptian" (see *HES II,* pl. 56.e.2).

These valuable descriptions will inform our reading of the sectional data that Reisner's team gathered from several cuts made through the Ostraca House rooms in Summit Strip 4.East and that Fisher subsequently published in the final report (see Principal Sections CD, GH, and Subsidiary Section AB in *HES I,* 63, fig. 14 and 115, fig. 43 = Ch. 3, figs. 31–32; *HES II,* Plans 4–5). But the section drawings immediately reveal problems

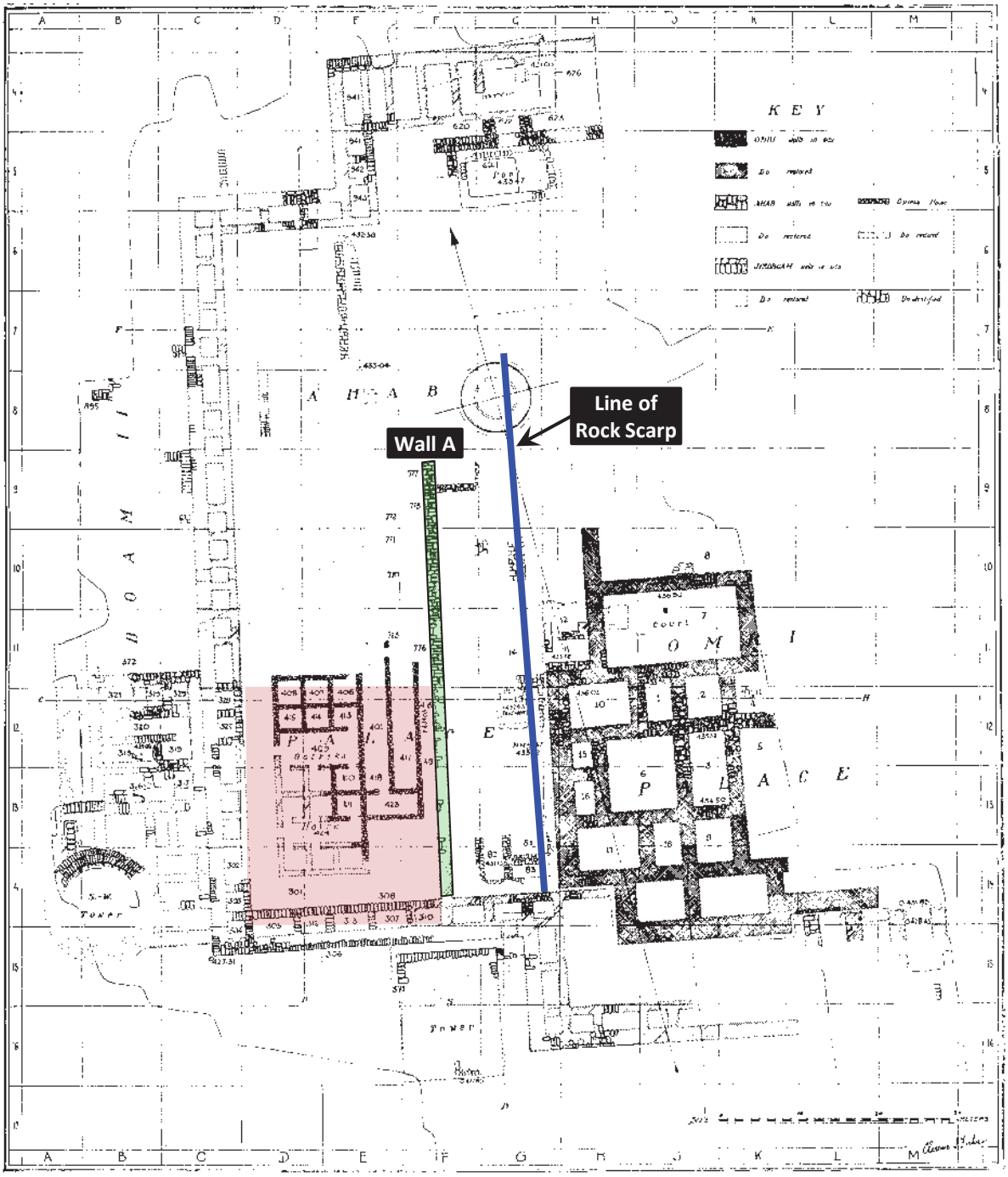


Fig. 22 *Harvard plan of Israelite period, showing location of the Ostraca House and Israelite Palace (shading shows area of Reisner's purported Ahab courtyard floor; adapted from* HES II, *Plan 5).*

in reconciling the vertical positions of the various deposits mentioned above. For example, if the lower dressing on the face of Wall A and the associated floor level ran, as Reisner wrote, between 20–100 cm above bedrock, and if the floor itself was 10–40 cm thick, then the bottom level of the floor would have to lie, at many points, very near or virtually directly on the bedrock. How, then,

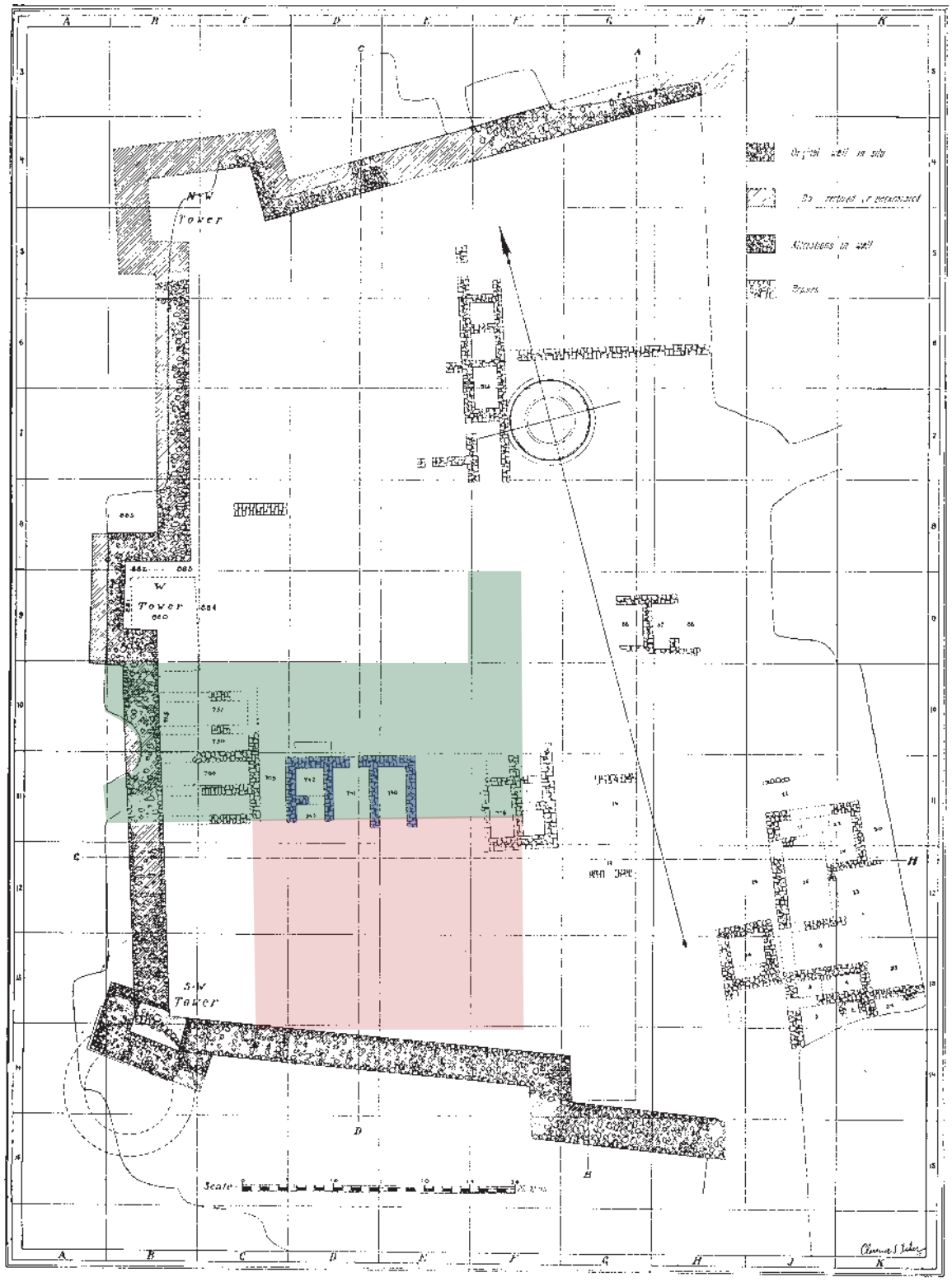


Fig. 23 *Harvard plan of the Babylonian period, showing location of Summit Strips 4 and 7, the Osorkon House, and Room 776 (green shading shows area of Summit Strip 7; red shading shows Summit Strip 4, which included the Ostraca House; adapted from* HES II, *Plan 6).*

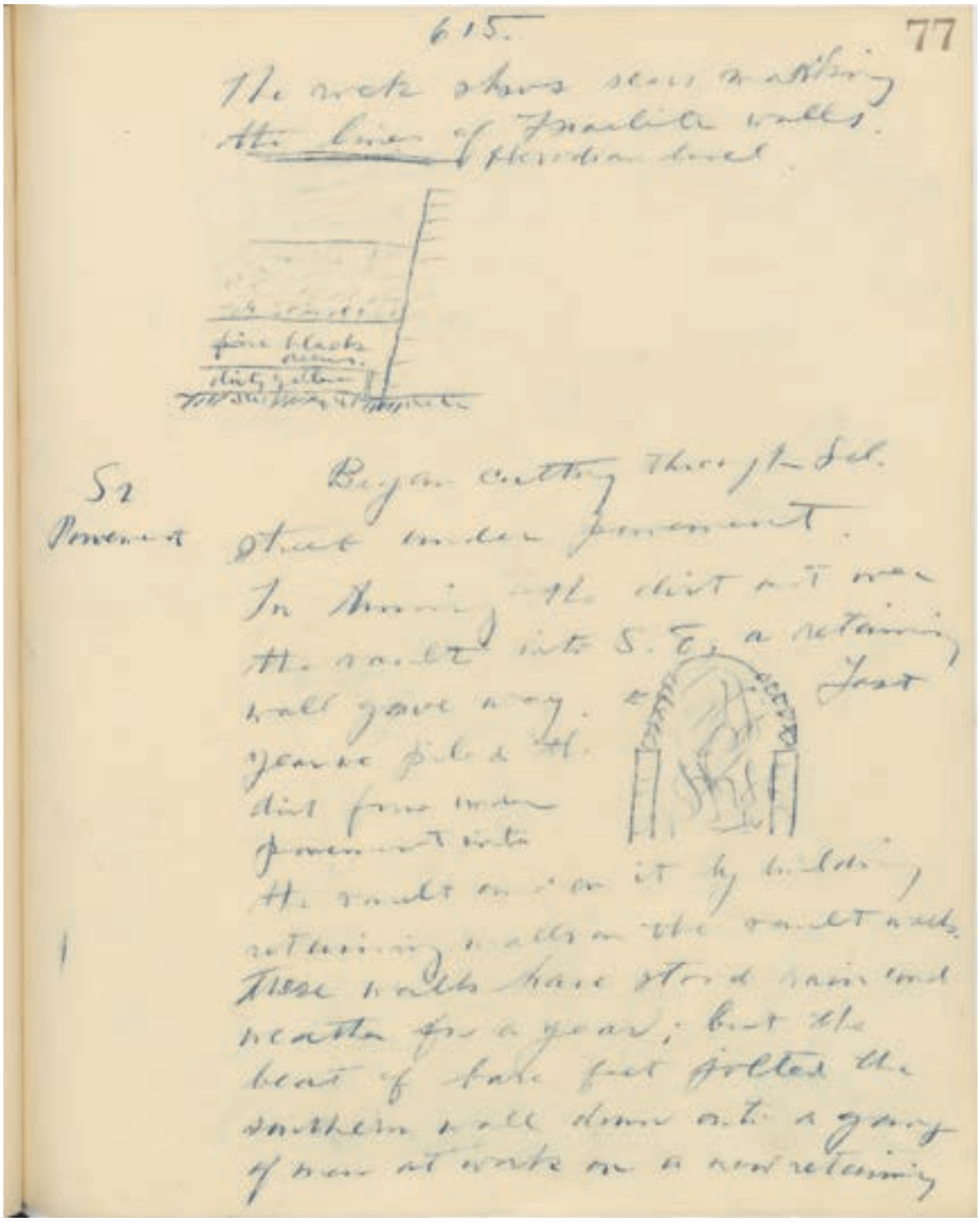


Fig. 24 *Field sketch of stratigraphy abutting north face of the "Babylonian Wall" in Summit Strip 10, Grid E.4 (*Reisner Diary VI, *615; Thursday, September 29, 1910).*

could a 20–100 cm-thick deposit of dirty yellow fill plus another 10–40 cm-thick layer of clean mason's debris exist beneath this floor and above the rock? Neither the narrative descriptions nor the drawings allow enough space to accommodate all the various deposits. And the fact that the reported depths of these layers and features match each other so closely (10–40 and 20–100 cm) may suggest the presence of mixed assignments or dittography in the field recordings.

For now, it bears noting that on September 29, 1910, near the end of the field work at Samaria, when work gangs were clearing the northern face of the so-called "Bab. wall" (ultimately, the Greek Fort Wall) in Summit Strip 10 (*HES II,* Plan 6, Grid E.4; fig. 23), Reisner once again encountered fine black debris overriding a layer of dirty yellow matrix. In a quick field sketch of the stratigraphy (fig. 24), he clearly indicated that this late second-century wall cut through the deeper yellow deposit but not the black soil (see *Reisner Diary VI,* 614–15), which filled the wall's foundation trench and ran against its northern face. If these black-yellow deposits relate to those in the area of the Ostraca House farther south, as the descriptions seem to imply, then it would appear that any pottery or objects recovered from the black debris must stem from a late, post-Iron-Age archaeological context.

D. Summary

The Samaria Ostraca comprise a sizeable corpus of texts that help to clarify the social and political history of the Northern Kingdom of Israel during the Iron Age IIB period. The brief, descriptive content of these laconic shipping dockets belongs to one of the oldest genres witnessed in the history of writing.[23] Any epigraphic find recovered in Israel from such a symbolic and powerful center as Samaria—regardless of the writing's length or subject matter—proves of interest to scholarship. In the case of the Samaria Ostraca, the size of the overall corpus, the plainly inked script, the uniform content, and the chronological formulae they include make this cache of writings significant for historical and palaeographic research. From the beginning, excavator Reisner clearly understood these facts. But how comprehensively did he present the archaeological data relating to the retrieval of the ostraca repertoire?

The published and private records recounting the discovery of these writings come to us from one of the earliest major excavations in Palestine and from a time when nearly all such expeditions were sponsored by western institutions. Both the context that gave rise to the Samaria project (the emergence of the Semitic Museum at Harvard University) and that in which the fieldwork played out (amidst the struggle to establish a national museum in Istanbul, the Young Turk Revolution, the waning of the Ottoman Empire, and the unfolding of events that would lead to World War I; see later chapters) combined with myriad local issues relating to land use rights, political leadership around Samaria itself, and more, to provide a complex setting in which western excavators had to manage an unusual number of peculiar facets in their work. Thus the excavation records from the Harvard Expedition to Samaria not only provide details pertaining to the archaeology of the site, they also serve as a window into the early method of excavating and level of reporting, while also containing valuable political and ethnographic notes on an extraordinarily significant period of world history (see Tappy 2016).

In this chapter, I have shown that the precise archaeological context of the ostraca remains among the most vexing questions for modern interpreters. My preceding summary of the work conducted at Samaria alone justifies a renewed look at the archaeology of the site and especially of the ostraca themselves. At this point, any attempt to grapple with the lingering questions must consider data drawn from both the published and unpublished records produced by that work. But since no site to date has been perfectly excavated,

23 For example, thousands of stamp seals and the clay tags on which they were impressed to indicate the contents, quantity, origin or destination, and/or owners of shipped goods have appeared from the third millennium BCE at Indus Valley sites such as Harappa (see Rao 2010).

recorded, and reported, one can go only as far as the extant data allow in the quest for clarity and deeper understanding. Along the way of this sometimes tantalizing, sometimes frustrating route, however, the unpublished diaries in particular help to illumine on many fronts the relationships and attitudes manifested between the western explorers and the locals with whom they interacted.

Chapter 2

Horizontal Axis: The Ostraca Building's Original Footprint and Surrounding Area

A. The Ostraca House

The surviving portion of the Ostraca House lay west of and below the main Israelite palace, in Summit Strip 4, Grids D-E-F.11–14. Fisher's own illustrations of the ostraca building and his Plan 5 in *HES II* (see Chs. 1, fig. 22, and 3, fig. 32) hint at the complex phasing of architectural remains in this area. Even a casual review of these drawings raises fundamental questions: Did the Ostraca House constitute the only major building occupying this area? If so, did this one structure undergo two-to-three distinct phases of construction? Or did more than one building occupy this space during the Iron Age IIB period?

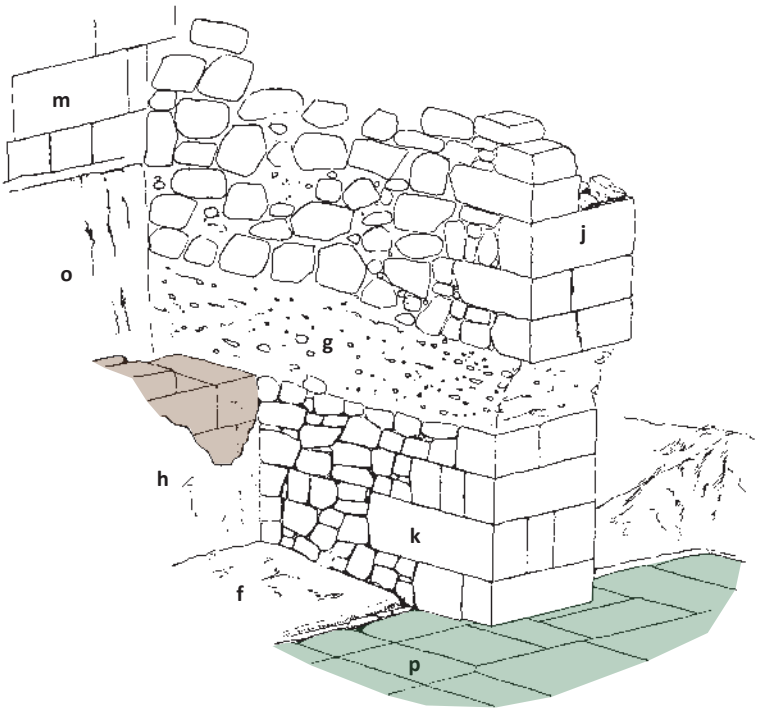
Reisner and Fisher presented the Ostraca House on several different plans, each with varying degrees of useful detail and each with serious limitations. For example, the scale of published Plan 5 proves too small to accent the detailed phasing witnessed on other drawings. In addition, Plan 5 does not include the presumed northern extension of the building. A second drawing, presented as the frontispiece in an undated report printed by E. O. Cockayne in Boston (hereafter referred to as the Cockayne Plan), shows the various phases in greater detail but also fails to outline completely both the northern and southern ends of the building as restored by Reisner. The best available plan for viewing the full parameters of the structure appears at two locations in *HES I* (64, fig. 15, and 114, fig. 42; see my fig. 26). This drawing provides a number of particulars important to determining the original footprint of the building, at least as understood by Reisner. For example, its legend records four stages of construction for the principal section of the Ostraca House, none of which aligns itself with the stratigraphically later Osorkon House (to the immediate north) or with an intervening "Two-Room Structure" (positioned over the northernmost elements of the Ostraca House). The four building phases presented in fig. 26 include: (1) Main Wall, assigned by Reisner to King Ahab; (2) the Ostraca House proper; (3) reconstructions of various Ostraca-House features; and (4) later additions to the Ostraca House. In the following pages, I shall address each of these phases before attempting to relate them to the subsequent Osorkon House.

1. Wall A: Ahab's "Main Wall"

While the massive Israelite Casemate System defined the western limits of the area available for the construction of the Ostraca House, another

longitudinal wall established the easternmost parameter (figs. 21b–22, Grids F.9–14)—not only for the area associated with the ostraca building but also for the spatial distribution of the ostraca themselves (see *Reisner Diary VI*, 610). Reisner dated this feature to the mid-ninth century and called it Ahab's "Main Wall" (hereafter Wall A). Although I introduced this wall earlier in my brief survey of the 1910 fieldwork, I shall now add a few more details to the picture.

Wall A ran parallel to and approximately 13 meters west of the artificially cut rock scarp that rose to support the palace area, and also parallel to but east of the Ostraca House Long-Room 417. Reisner labeled the 1.8-meter-wide space between 417 and Wall A "416/419." While no portion of the Ostraca House extended into this strip (the bedrock here having remained exposed and uneven; figs. 25–26), I have noted Reisner's claim that a 10–40 cm-thick floor ran away from the western face of Wall A. I have also mentioned that this purported floor followed a line that undulated between 20 and 100 cm above bedrock, and that it supposedly continued "westward through sections DEF 12, 13, 14," i.e., across virtually the entire area of the Ostraca House. Only a single drawing (fig. 25; cf. *HES I*, 58, fig. 13), however, shows a floor level (labeled *p*), one apparently constructed of flat-lying stones or mudbricks. But this feature does not connect to Wall A (*h*); additionally, a post-Israelite wall (*k*) rests directly on it. None of the three independent sections drawn through the Ostraca House indicates such a floor level, described as "an accumulation of fine, black debris such as is laid down by the occupation of earth-floored courtyards, representing, perhaps, a considerable period of time" (*HES I*, 62). Moreover, the material culture assigned to this "living floor" (including the Osorkon jar and the ostraca) surely came from different stratigraphic contexts and can hardly provide "complete proof" of contemporaneity between the floor and the main palace on the crest of the rock. Neither can it show "beyond question" that all these features date from the ninth century BCE, as surmised by Reisner (*HES I*, 60).



13. S2 IV 13.—o, Omri scarp; m, Omri wall; h, Ahab wall; p, Ahab floor; k, Post-Israelite wall; g, debris; j, Greek wall; f, rock surface.

Fig. 25 *Wall and floor purportedly dating to King Ahab (adapted from* HES I, *58, fig. 13).*

2. The Ostraca House: Entrance Halls, Storerooms, and Eastern Corridors (Long-Rooms)

In figure 26, the principal phase of the Ostraca House, whose surviving walls are represented by diagonal hash marks, includes both the preserved segments of the building and the excavator's proposed restoration of the lost portion of the structure. When combined, the projected original design included a number of square and rectangular rooms in the main building, which was flanked on the east by two long, corridor-like chambers. The layout of rooms in the main building shows a

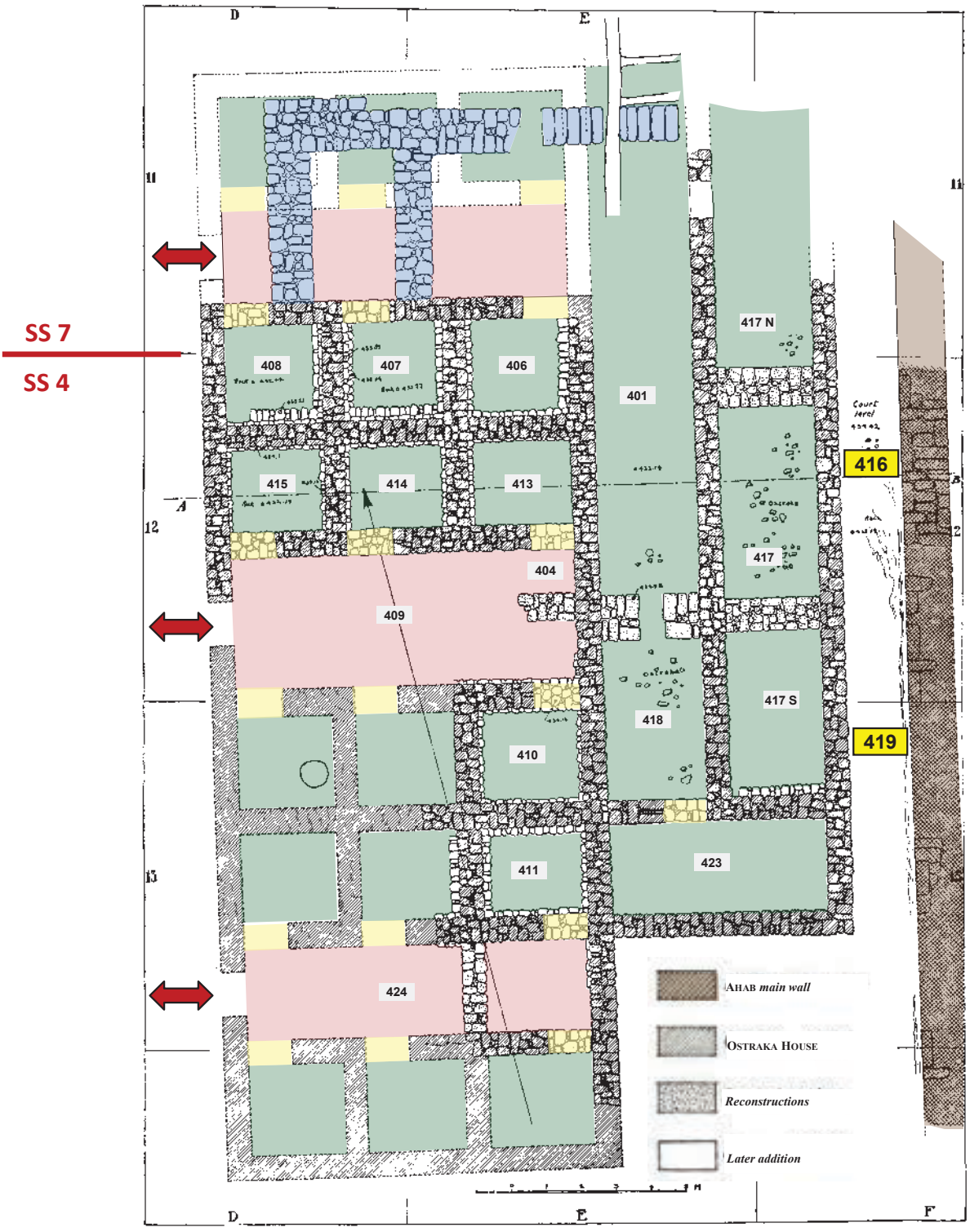


Fig. 26 *Ostraca House design and phasing according to Reisner and Fisher (adapted from* HES I, *64, fig. 15, and 114, fig. 42).*

symmetrical arrangement, with groups of small-sized rooms apparently interspersed between rectangular-shaped entrance halls, as follows (from south-to-north):

(2.a) a series of three square rooms (unlabeled) across the southern side of the structure, with doorways in their northwestern corners or, for the easternmost room, in the northeastern corner (this proposed pattern follows that seen in the surviving rooms to the north); owing to disturbances caused by the later construction of the so-called Greek Fort Wall (see below), Reisner had to deduce the erstwhile existence of these chambers based on traceable remains of their foundation trenches;

(2.b) a southern entrance hall (424), ca. 2.6 m in width, situated immediately to the north of the three rooms just mentioned; as with the other entrance halls, this area originally spanned the entire breadth of the square chambers, which likely served as storerooms, perhaps in the basement of the structure (see below); a doorway in the building's main western wall gave access to this hall; only the eastern half of this area survived (the longitudinal wall that partitions off the eastern portion of this area on the plan represents a later addition; see below);

(2.c) a double set of three square rooms (6 total) in the south-central building, with only the two easternmost ones (410–411) having foundations or superstructure that survived (the restored rooms remain unlabeled); two of the three southernmost rooms opened into Entrance Hall 424 at their southwestern corners, while the easternmost room had a doorway in its southeastern corner (i.e., these doorways mirrored those located directly across from them, so that they aligned with and faced each other on opposite sides of the hallway); the three northern rooms originally opened into Central Entrance Hall 409 in the same pattern;

(2.d) a wide, central entrance hall (409), located in the middle of the overall complex; this hall was slightly wider than its counterparts to the south and north (ca. 3.6 m versus 2.6 m); once again, the partially preserved entrance to 409 existed in the building's western perimeter wall, and architecture in the southwestern portion of this space did not survive;

(2.e) another set of six square rooms (3 × 2 m) in the north-central building (from east-to-west, 413–414–415 [southern row] and 406–407–408 [northern row]); all these chambers were well preserved, owing to their distance from the Greek Fort Wall construction area; the location of their doorways (western Rooms 414–415 and 407–408 opened at their western corners, while the more easterly Rooms 413 and 406 opened at their eastern corners) established the pattern that Reisner applied in his projected restoration of rooms farther south.

As noted, Reisner believed the original building continued to the north and maintained its symmetry with the layout of rooms and entrance halls to the south. Following the foundation trenches, he projected

(2.f) a northern entrance hall (unlabeled) that equaled 424 in width (2.6 m) and that lay immediately north of the last-mentioned set of six rooms; all doorway locations adhered to the now familiar pattern;

(2.g) a final series of three square rooms across the northern side (unlabeled) of the structure; these chambers mimicked their restored counterparts on the southern side of the building in size, orientation, doorway locations, and overall design; together with the northern entrance hall, they extended 6.6–6.7 m north of the surviving parts of the Ostraca House, i.e., they ran into Reisner's Summit Strip 7, where (as in Strips 8 and 11) "the place of the Israelite walls was occupied by black-filled trenches crossing the yellow debris" (*HES I*, 40); recall that, as part of Summit Strip 7, workers would have dumped the soil removed from those rooms back into the area occupied by the rooms described above, in

Table 1 Horizontal distribution of ostraca (* for the count of 75, see *AIS II,* 496).

	No. of Ostraca	Percentage of 75*
Summit Strip 4		
Room 401	1	1.33
Room 416	1	1.33
Room 417	22	29.33
Room 417-N	12	16.00
Room 418	10	13.33
Summit Strip 7		
Room 723	1	1.33
Room 772	13	17.33
Room 772-N	1	1.33
Room 772-W	2	2.70
Room 773	8	10.66
Room 776	4	5.33

Summit Strip 4; this observation suggests that the black deposit post-dates the robbing of the Israelite level and does not represent Israelite occupational debris.

Lying immediately east of these chambers but apparently associated with them, additional rooms showed a markedly different configuration, as follows:

(2.h) two longitudinal "Long-Rooms" or Corridors (401/418 and 417), each ca. 2.75 m in width; Reisner himself initially understood these strips as streets or corridors (see *Reisner Diary VI*, 563), probably because he had already excavated streets from later periods that overrode this very area (Street C from the Roman period and Street Alpha from the Hellenistic Period); at some point in the life of these corridors, newly added walls subdivided both of these chambers (see below); for the understanding of these long-rooms as receiving areas, see *AIS II*, 498;

(2.i) a generally vacant space in 416/419 separated these corridors (and, indeed, the entire building to which they were attached) from Wall A, the high rock scarp, and the palace area on the summit plateau to the east.

The overall design of the Ostraca House, then, consisted of 18 nearly square rooms, neatly organized around northern and southern entrance halls of equal width, and a wider, centrally located hall. Note that these cubicles opened only into the three hallways, not to outside areas, and the entrance halls themselves opened toward the west, not to the east. Reisner himself understood that the Ostraca House "faced towards the west" (*HES I*, 114). The published plans, therefore, offer no indication of a direct connection between any of these spaces and the so-called Long-Rooms or Corridors ("Streets") that flanked the House on its eastern side. So whatever activities transpired in these presumed storerooms occurred *below* and *away from* the elevated palace area and without any direct connection to the contiguous long corridors to their immediate east. Yet the construction history and function of these corridors prove vitally important to an investigation of the ostraca, since Reisner recovered the bulk of the inscriptions from these very areas (i.e., the eastern Long-Rooms and their presumed extensions northward). Only a single ostracon came from the gap *between* the Ostraca House and Wall A ("Room" 416). In fact, Table 1 shows that 60 percent of the ostraca derived from Corridors 401–417–418, while nearly 39 percent of the inscriptions came from areas lying farther north, in the 700-series of rooms belonging to Summit Strip 7. Importantly, excavators found none of the fragments in the rectangular entrance halls or squarish storerooms situated in the main part of the building. What factors might account for such a spatial distribution of the ostraca, one in which the majority of the building failed to yield even a single inscription?

Understanding the actual function of the various rooms presented in figure 26 becomes important to any interpretation of the surviving architecture and to explaining the somewhat peculiar absence of writings from the so-called entrance halls and presumed storerooms. Two options immediately present themselves. First, the spaces appearing in

figure 26 and in the published section drawings (see Ch. 3) may, in fact, represent functional (storage?) rooms, perhaps basement rooms, in a much taller building whose superstructure did not survive. Eventually, with the destruction or dismantling of that structure, or in preparation for a successive building phase, the areas between the truncated walls of these erstwhile chambers were buried in a deep deposit of debris. One might assume from Reisner's description of the dressing on Wall A to the east—dressing which at points ran down to within 20 cm of the bedrock—that the western face of this feature remained exposed down to this level. If so, and if Wall A was contemporary with the Ostraca House, then the lower, ground-floor chambers of the latter structure might also have stood open and ready for use as entrance halls flanked by multiple storage facilities. Unfortunately, I remain unable to locate any description of the facings on the Ostraca House walls. But, judging from the published sectional data reviewed below, whatever floor levels might have existed inside these rooms, not one such level appears to have survived. Even so, if the square rooms and entrance halls served in any real capacity related to the ostraca, it seems likely that at least one epigraphic find would have emerged somewhere between their surviving wall stubs. But such was not the case.

In the second scenario, the walls of figure 26 might represent not open, functional rooms but simply a subterranean foundation-support system, i.e., pilings, for a sizeable building whose entire superstructure appears to be missing.[1] In this case, the deep dirty yellow fill depicted in all the published sections constitutes not a later burial of an actual, erstwhile living space but part of the building's original construction strategy. If so, the designers of these "rooms" would never have intended for them to remain open and useable. In fact, the support they provided for the occupied superstructure would have gained much of its strength from their having been filled in. So no floor levels appear in any of the excavation drawings because none ever existed inside the smothered features they depict. Importantly, the published section drawings (particularly GH and Subsidiary AB) show that the massive fills lay not only in the square rooms and associated hallways but also in the long corridor rooms to the immediate east.

Consideration of both published and unpublished records from the Harvard excavations provides disparate details that might support one or the other options outlined above. For example, several factors seem to militate against the latter picture (a subterranean buttress system) and to offer some support for the former one (open, serviceable rooms). First, judging from Reisner's publications, one may assume that he himself clearly seems to have understood these spaces as accessible and functional during their lifetimes. Second, that each chamber originally included a doorway (unlike the "cellar rooms" mentioned in n. 1) leading into one of three hallways (424, 409, unlabeled)—doorways that were eventually blocked in a later construction phase—also recommends seeing them as functional spaces, not simply buried foundation supports. Moreover, as noted, the so-called entrance hallways themselves gave access and egress from the west. Finally, if all these features represent only subterranean pilings, why would the design have changed so radically in the long, eastern corridors 401–417–418? And why, if the chambers were built and then intentionally filled in, would the drawings reflect multiple phases in the construction history?

On the other hand, elevation readings provided for the best-preserved rooms (406, 407, 408, 413, 414, and 415) may concur with certain descriptions in the unpublished field records. For example, in one fieldbook Reisner both sketched (figs. 27–28) and described a later, major wall unit in nearby Summit Strip 3d (located just south of the Ostraca House area in Strip 4) as follows:

1 Interestingly, when Reisner cleared away late deposits and debris from the southern section of Summit Strip 2 (roughly in the area of Grids G-H.14–15, south of the palace and southern rock scarp), he described what was apparently the emerging Casemates in much the same way: "Have now cleared away enough of the Roman walls in the south of S2 to reveal clearly the plan of the Israelite rooms (Ahab). These rooms appear to be cellar rooms with no connecting doors. Unless they were filled rooms, they may have been entered by a trap door" (*Reisner Diary V,* 493).

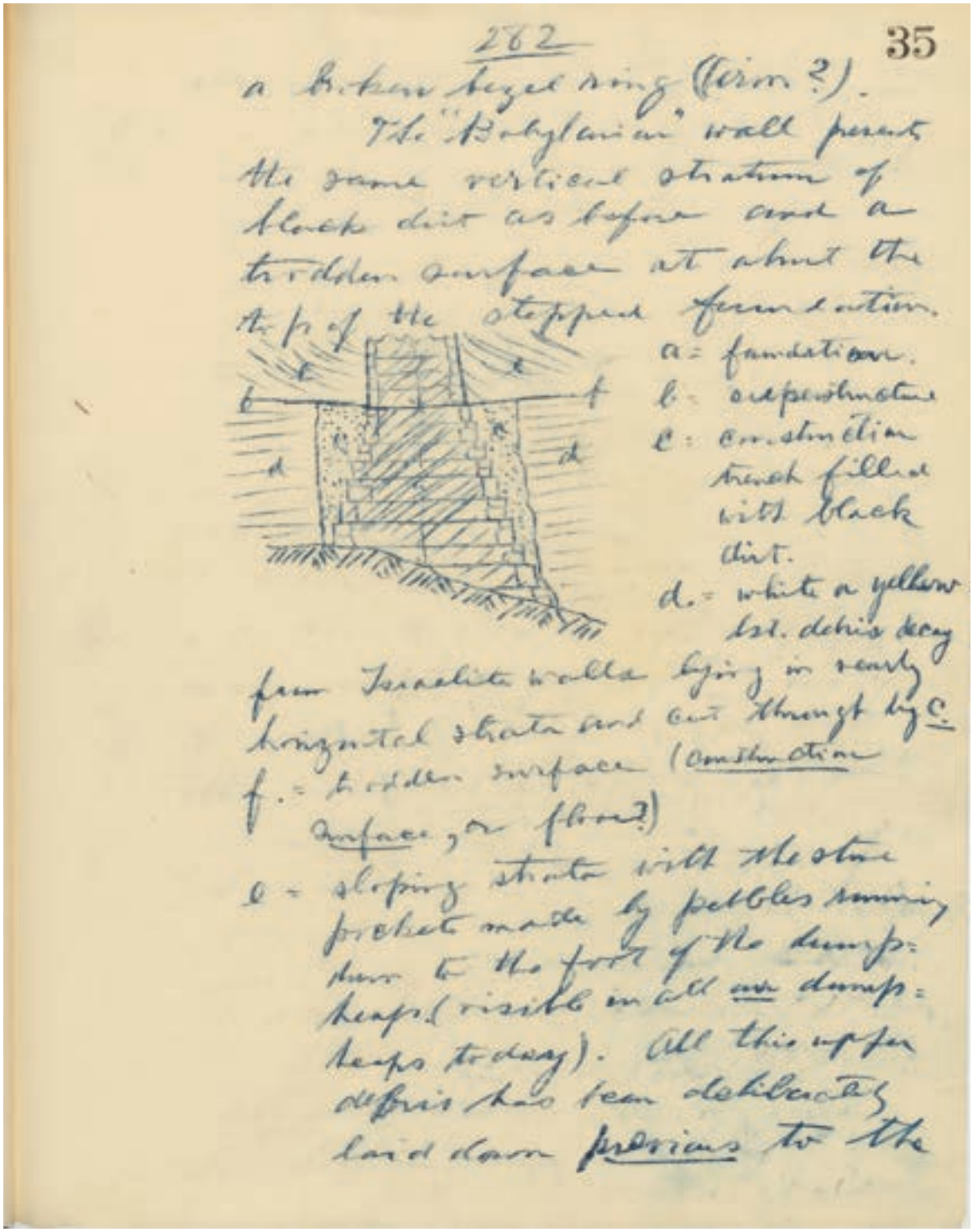


Fig. 27 *Field sketch no. 1: wall construction technique (*Reisner Diary III, *282; cf.* V, *532).*

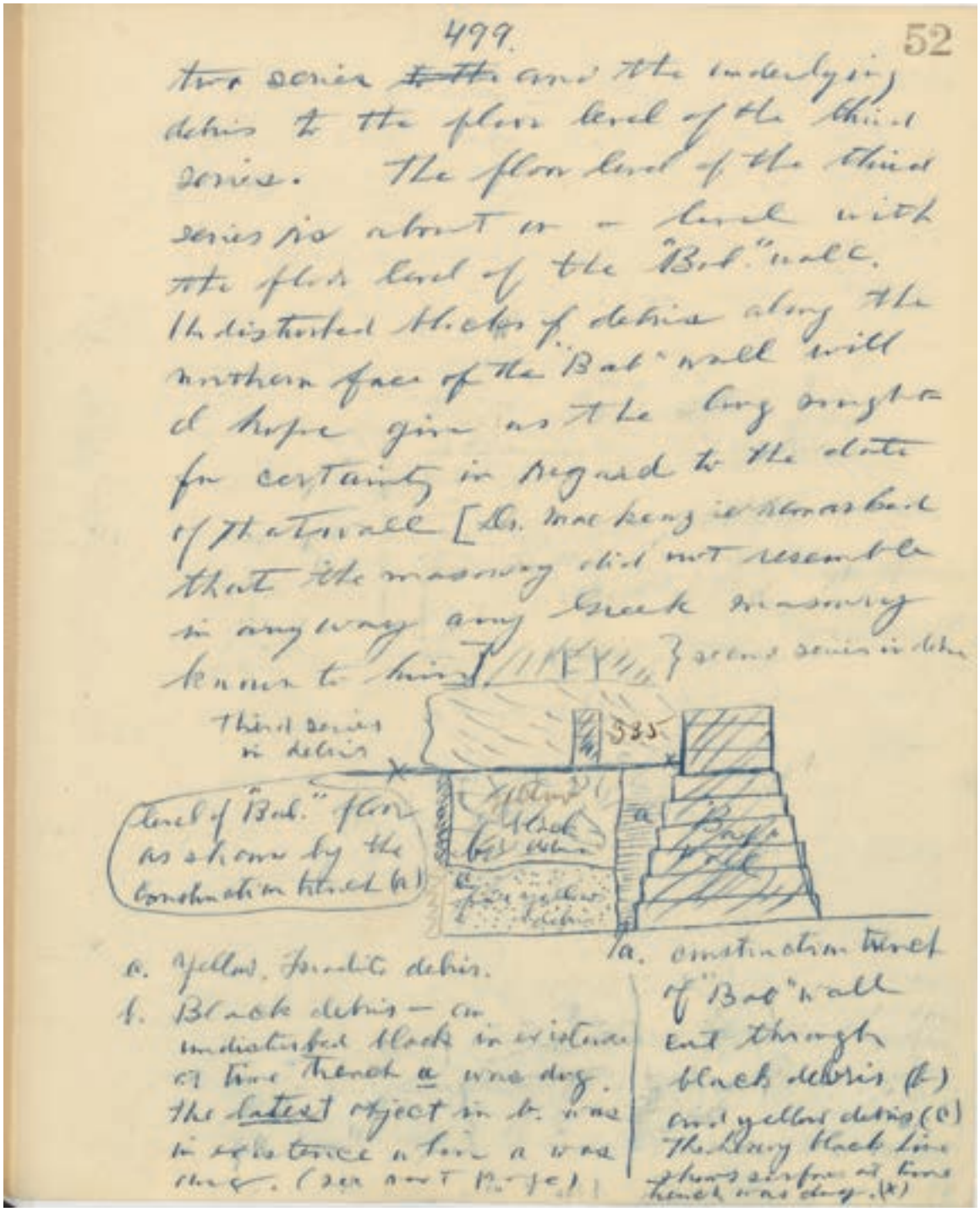


Fig. 28 *Field sketch no. 2: wall construction technique (*Reisner Diary V, *499; cf.* V, *532).*

Table 2 Relative heights of stone courses in walls of selected Ostraca House rooms (cf. fig. 21, above).

Ostraca House Room	Elevation of Bedrock	Elevation of Courses in Wall	Height of Courses above Rock	Variation in Elevations of Bedrock Relative to Room 415	Adjusted Relative Heights of Stone Courses
Room 407: lower step	432.77	433.14	0.37	+ 0.58	0.95
Room 407: upper step	432.77	433.85	1.08	+ 0.58	1.66
Room 408	432.42	433.21	0.79	+ 0.23	1.02
Room 415	432.19	434.10; 434.12	1.91 1.93	–	1.91 1.93

> The "Babylonian" Wall presents the same vertical stratum of black dirt as before and a trodden surface at about the top of the stepped foundation. a = foundation. b = superstructure. c = construction trench filled with black dirt. d = white or yellow cst. [construction] debris decay from Israelite wall lying in nearby horizontal strata and cut through by c. f = trodden surface (construction surface, or floor?). e = sloping strata with the stone pockets made by pebbles running down to the foot of the dump = heap (visible in all our dump = heaps today). All this upper debris has been deliberately laid down previous to the Herodian period or very early in it.
>
> (*Reisner Diary III*, 282–83; Reisner's underscoring)

This feature likely represents the massive enclosure wall assigned to the Babylonian Period on Reisner's published Plan 6 but labeled "Greek Fort Wall" on Plan 7 (*HES II*; see Grid D.14; note also the gradual in-stepping of the wall as presented in Section CD, Squares 4 and 14). In any event, the stepped foundation courses of this structure clearly cut through the horizontally accumulated, light-yellow construction debris (which, I believe, Reisner ultimately called the "clean yellow" matrix) and rested directly on bedrock (see fig. 28). Each ascending course narrowed to form a kind of stepped tower, which then supported another wall, presumably the exposed superstructure (unstepped and with dressed facings). The basal courses of the superstructure rested on some sort of flattened surface created either through repeated tamping by the feet of the workers or as an actual living floor for the occupants of the completed building. The trench cut for the foundation itself was backfilled with black dirt, and the relative narrowness of the trench itself created what Reisner aptly termed "vertical stratigraphy." Eventually, probably in the pre- or Early Roman period, workers poured additional heavy fills diagonally across the entire area.

Interestingly, information available from certain rooms in the Ostraca House plan may suggest a similar pattern of construction. In figure 26, above, Fisher included a course of stones apparently set beneath the main walls of Room 415. And in both 407 and 408 he depicted two courses of stones beneath the walls there. Table 2 summarizes the elevations recorded for the rock base and each of the rising courses of stones in these chambers. While some expected variation exists in the relative heights of the individual stone courses, a general pattern of gradation emerges that seems reminiscent of the later, subterranean, stepped foundation wall recorded in Summit Strip 3d to the south.

Ultimately, Reisner's own hand-written field notes may seem to suggest that he, in fact, understood the Ostraca House to have utilized wide foundation walls on which to rest the building's superstructure. But his description of these architectural features lies buried in *Reisner Diary V*, where he presents a field-drawn sketch of two walls

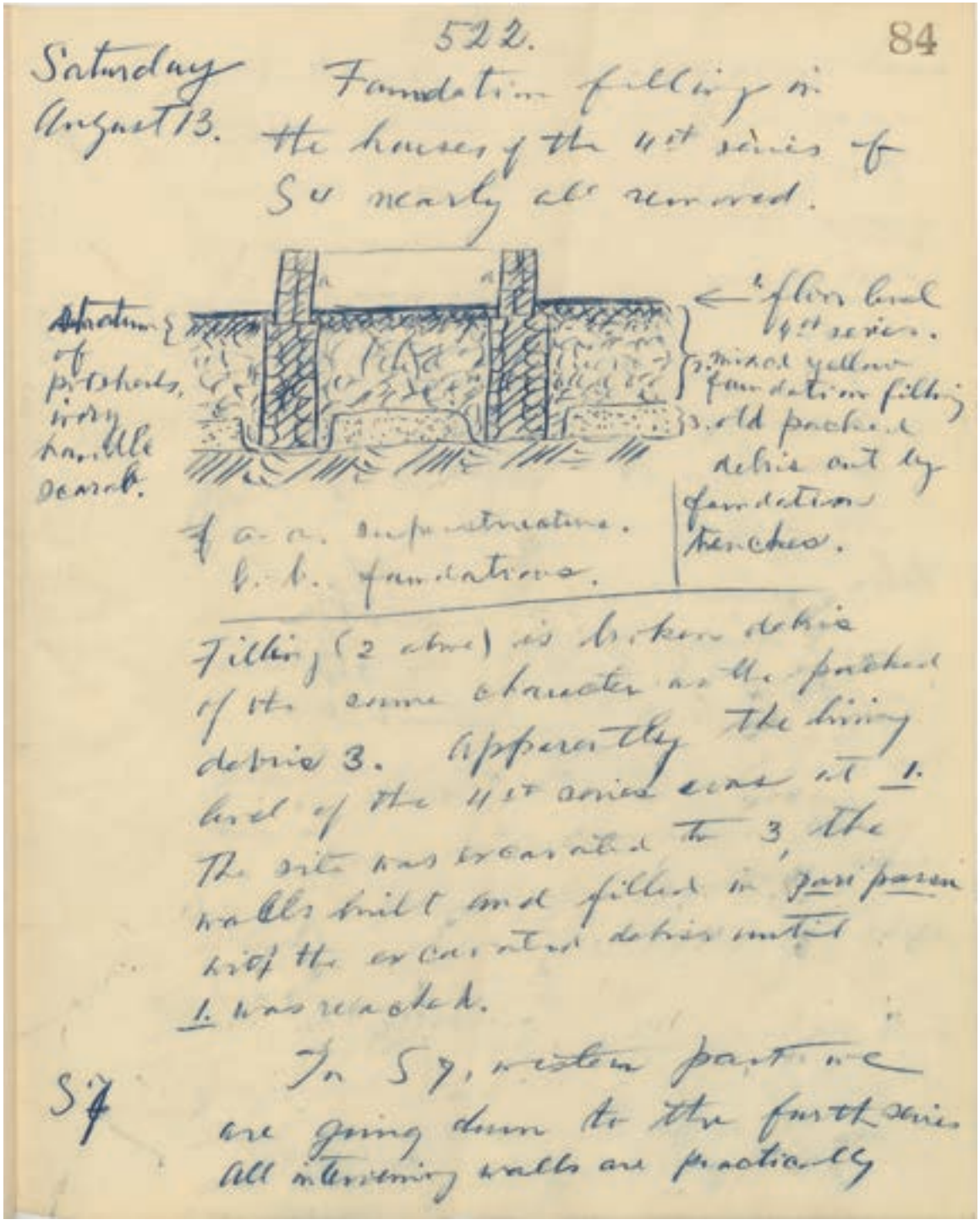


Fig. 29 *Field sketch no. 3: proposed Ostraca House construction and depositional history (*Reisner Diary V, *522).*

representing the larger Ostraca House (fig. 29) and a laconic summary of the original construction sequence. These records may reveal some affinity with the second explanatory option above. Based on the presupposition that only one building phase existed here, Reisner interpreted the deeper, wider walls (some of which are depicted on figs. 26 and 29 and published Sections CD, GH, and Subsidiary AB) not as demarcating open, useable cellar rooms (as with the first option above), and not as evidence of a previous building phase, but as the true foundations for the Ostraca House superstructure. The pivotal field record outlining this understanding of the original construction sequence provides the aforementioned sketch (fig. 29) and narrative description:

> Filling (2 above) is broken debris of the *same character* as the packed debris 3. *Apparently* the living level of the 4th series runs at 1. The site was excavated to 3, the walls built and filled in pari passu ["with equal pace; side-by-side"] with the excavated debris until 1. was reached.
>
> (*Reisner Diary V*, 522; Reisner's underscoring; italics added)

To conclude this portion of my discussion, several observations merit attention. First, the features taken by Reisner as foundation walls are clearly not stepped upward in the published drawings. In fact, they appear to exhibit flat, nicely dressed facings, albeit on different phases of construction. Second, construction of the foundation walls cut through even the hard-packed, clean yellow deposit that lay directly on bedrock and the walls themselves were then buried in a deep foundation fill of mixed yellow matrix (the so-called "dirty yellow" matrix in *HES I*, 63). These two deposits, then, reflect different, unrelated activities at the site. Reisner's claim in his field notes that they are "of the same character" must refer to their overall nature as artificially laid deposits (yellow construction debris topped by mixed fill material), not to their true color or their composition, texture, contents, or contemporaneity. Third, on Monday, August 15, 1910, he recorded,

> The dirty yellow debris (not the clean Isr. yellow) continues to yield inscribed fragments of pots. A very complete series of nearly duplicate texts occur[s] on some hard sherds of large two-handled jars.
>
> (*Reisner Diary V*, 526; Reisner's underscoring)

Reisner locates these inscribed "potsherds" and other significant finds in a relatively narrow band of debris located *beneath* the proposed floor level, where the basal courses of the superstructure rested atop wider foundation walls. Thus, even if these particular inscriptions were ostraca-related, they comprised part of the floor's make-up and so constituted derived elements in a band of construction material, not occupational debris lying above the floor. As such, they could only help to provide a *terminus post quem* for the floor and building, not a date for the structure's functional life. Neither could they rule out the possibility that the lower, wider walls belonged originally to an earlier building.

It is, therefore, important to recognize that, while Reisner's unpublished field sketch gives the impression that these thick (10–40 cm), well-preserved and unbroken floors extended across the entire building and covered and sealed all the material trapped beneath them, his accompanying notes qualify this notion by stating that such was "apparently" the case. At this point, his observations seem to reflect a projected rather than observed scenario, one based on an initial, limited exposure to the stratigraphy and material. Unfortunately, his failure to identify in the field drawing the specific walls in question leaves one unable to anchor this sketch directly to a precise position on the published plans and sections or to a particular room or location within the Ostraca House generally. Moreover, this field record came very early in the recovery of the ostraca, when workers had found no more than a half dozen ink-inscribed fragments. The full state of preservation for the overall context had not yet become evident. Ultimately, no such floors—certainly none that ranged up to 40 cm in thickness—appear in the section drawings produced, signed, and published by Fisher.

Finally, only some of the upper walls depicted in those illustrations have broader, foundation supports beneath them. (Compare, for example, the tall walls on both sides of Room 417 in Subsidiary Section AB [see fig. 41], the wall between Rooms 415 and 409 and possibly the southern edge of 424 in Section CD [see figs. 33, 35], etc.) The published plan for the Ostraca House (fig. 26) also shows an inconsistent number (0, 1, 2) of possible subterranean walls, whether serving as foundations or otherwise. The sporadic presence of broad walls beneath many but not all the apparent Ostraca House walls might well suggest the presence of an earlier building whose truncated remains served to buttress certain portions of the stratigraphically later Ostraca House.

While many of the ostraca, then, appear to have come from deep fills that lay below whatever floors once existed in the Ostraca House, other ostraca seem to have emerged from later but similar contexts of fill that served as makeup for surfaces dating as late as the Herodian period. For example, Reisner recorded that

> Under the floor of S7–357-N which is the continuation northwards of the same structure and the same series of debris as in S4–417, we found six or seven fragments with Israelite ink inscriptions. Two of these fitted onto Reg. no. 3897 [published Ostracon No. 48]; and one fitted Reg. no. 3895 [Ostracon No. 31a].
>
> (*Reisner Diaries V*, 538 and *VI*, 539)

Judging from Fisher's final Plan 8 in *HES II*, feature 357 refers to longitudinal Street C, which ran along the eastern side of the Roman-period Atrium House (along the gridline between E-F.10–12). Although the published ostraca registry does not relate any inscriptions to 357, at least a dozen specimens[2] apparently came from an *archaeological* context associated with the construction of that later street. This passageway lay directly along the line occupied earlier by the Hellenistic Street Alpha and, more importantly, the eastern Corridor/Long-Room 417 in the Ostraca House. As seen in Appendixes A and B, some of the fragments discovered here (417) actually connected with published ostraca Nos. 31a and 48. Thus the fact that these joining pieces came from findspots that were separated both horizontally and vertically (stratigraphically) immediately suggests a provenance in disparate, secondary deposits.

In sum, one must hold various possible interpretations in balance between the evidence presented in Reisner's unpublished field notes and that witnessed in Fisher's published section drawings (see Ch. 3). But however one interprets the functionality of the walls that both Reisner and Fisher assigned to the Ostraca House, a less-than-optimal situation results for the provenance of the inscriptions themselves. At some point, whether at the construction and back-filling of the building's foundations or at the destruction and smothering of its one-time superstructure (or basement), massive fills packed and blocked all the rooms traditionally associated with the Ostraca House, including the long corridor rooms to the east, from where excavators retrieved a significant percentage of the inscriptions. It seems that any inscription found in Room 401, 417, or 418 would have arrived in its secondary archaeological context either (1) as imported fill for the final construction activity of burying the foundational pilings before adding the true "ostraca house" (now missing) above this level; (2) as fallen debris when the upper rooms were dismantled or destroyed and collapsed into the once functional basement level; or (3) as imported fill brought here much later to achieve a suitable building level for one or more post-Iron Age structures. All in all, the graphic presentations and narrative descriptions (in both the private field notes and the published

2 To the half dozen inscriptions mentioned here, other journal entries increase the number found in similar contexts. For example, on Monday, August 22, 1910, Reisner recorded that "In the continuation of S4-417 along east side of S7, we found today four or five more potsherds with Israelite ink inscriptions" (*Reisner Diary VI,* 542; this area lies roughly in Grids F.10–11). In some journal entries, he actually related Room 417N to Summit Strip 7 as opposed to Strip 4 (e.g., *VI,* 544, says that "In S7-417-north, a few more Israelite potsherds, curiously enough one with the name of a man called Elisha").

reports) concerning the earth that Reisner found filling all the "rooms" of the Ostraca House, to the very tops of their surviving walls, strongly recommends that one understand this matrix as imported fill material, not classic destruction debris and, certainly, not as reliable occupational debris. Under no circumstance is it possible to see a primary archaeological context for the inscriptions associated with this building.

3. Reconstructions

Notwithstanding the variety of possible interpretations for the discrete architectural layers on figure 26, the different levels may not reflect a stacked foundation system at all. According to Reisner and Fisher, in fact, they represent diverse reconstructions and additions to one and the same building, namely, the Ostraca House.

Reisner concluded that, at some point, the Samarians added new walls at certain locations inside the Ostraca House; figure 26 identifies them with stippled shading. Reisner did not clarify specific dates for these repairs or renovations. The features involved clearly show a different, more substantial construction technique from that witnessed in the preceding phase of the Ostraca House. One must, therefore, ask whether "reconstruction" is the most apt term for these components, since this rubric seems to imply pre-existing walls that fell into disrepair and required mending. But such a scenario is not at all clear from either figure 26 or the narrative in the report itself. Rather, these walls give the appearance of later, more impressive, *new* constructions, not repairs to previously existing, dilapidated ones.

As part of these alterations to the original building, ancient workers added two new features in Corridor 417. The new walls divided the once Long-Room into three separate spaces by partitioning off its northern and southern sectors. In his composite plans, Fisher labeled these newly enclosed spaces 417N and 417S. Around the same time (judging from the drawing), a similar sub-division occurred in Room 401, at least in its southern area. Reisner labeled the resulting chamber Room 418. Based on the materials used plus the scale and technique of construction, some or all of these "reconstructions" (particularly the large walls subdividing 401 and 418) may well belong with the mysterious Two-Room Structure that overlay the northern end of the Ostraca House.[3]

Another new wall, which survived only as a truncated stub, extended into the eastern portion of Entrance Hall 409. But, once again, the scale of neither the materials nor the finished feature seems compatible with the other walls labeled "reconstruction." The eastern third of the southernmost Entrance Hall 424 was now also partitioned off into a separate room. The additions in these hallways may reflect a phase of activity entirely distinct from that of the heavier walls added to Corridors 401 and 417, but a lack of sufficient elevation data precludes certainty.

The new walls between 401–418 and 417N–417 seem compatible in materials and design and could very well have been constructed around the same time. Similarly, the two new walls in Corridors 409 and 424 appear compatible in most respects; but, as noted, they do not seem congruent with the additions in 401 and 417N. On the other hand, the addition between 417–417S seems somewhat different from all the others but most similar to the new work in the two corridors (409, 424), not to that of the two Long-Rooms. So it seems possible that, in fact, Fisher's plan portrays two to three distinct phases of work here, though one cannot expect every phase to exhibit total consistency in its technique or materials.

Apparently, the narrow space between Corridor 417 and Wall A (which I am calling "Alleyway 416/419") remained without any architectural features, old or new, during these renovations. At least one ostracon, however, came from this area,

3 The more massive construction might explain the mysterious presence of presumed wall stones that appear in quite a regular pattern throughout the fill of Ostraca House Rooms 408, 407, 406 (and even into 401) in published Section GH and, to a certain extent, in Rooms 410, 411, and 424 in Section CD. (In Chapter 3, I shall explain how Fisher frequently incorporated into his section drawings portions of architectural features that did not lie directly on the section line, thus giving the drawings depth as well as height and breadth.)

while the majority of the others emerged from the adjacent Long-Rooms. As demonstrated in Table 1 above, Reisner's provenance nomenclature (e.g., his use of 417-N and 418, which together contained 22 of the 75 inscriptions, or 29.3 percent) may imply that the subdivision of the original Long-Rooms had already occurred by the time of the deposition of the ostraca. But this suggestion remains uncertain, and some of the ostraca might well relate to the original, undivided rooms.

4. Later Additions (?)

In figure 26, Fisher identified all the later walls, whether surviving or merely projected, by leaving them without shading of any kind.[4] Note that these so-called "later additions" actually give the impression of lying *beneath* the phase of hatched stones representing the original Ostraca House (e.g., see the northern wall of Room 410). If they did, the Ostraca House was constructed directly over a pre-existing building (or its remains). In this scenario, the phrase "later additions" seems more apt as a reference to the eventual blockage of the original doorways seen inside the preserved portions of the building (Rooms 407, 408, 413, 415, 410, 411, 423, and the unlabeled southeastern chamber). Beyond the doorway areas, the building itself may once again reflect two or more distinct phases of activity. (See, for example, Rooms 407–408; some of these chambers may have had benches around some or all of their perimeters.)

A consideration of the width and construction technique of the other "later additions" suggests a similarity between these features and those of the somewhat mysterious features that I have called the "Two-Room Structure" (see figs. 26 and 30, Grids D-E.11). These remains overlay but did not align with the northernmost portion of the original Ostraca House. Since the wide walls of the Two-Room Structure appear stratigraphically later than those of the Ostraca House, the ancient builders may well have robbed a number of stones from the Ostraca House or entirely plundered its northern extension to obtain materials for their new, somewhat offset building. While this suggestion must remain speculative in nature, Reisner himself seemed to have accepted its conclusion, for he stated that "the northern end of the building [i.e., the Ostraca House], if it existed, as assumed, must have been torn down before the construction of the Osorkon House" (*HES I*, 117). Understanding a sequence of robbing and secondary building here would help clarify the depositional history of the area as follows: (1) a possible early building that preceded the Ostraca House; (2) the Ostraca House, itself perhaps evolving over several phases; (3) the Two-Room Structure;[5] and finally, (4) the Osorkon House. Thus Kaufman's (1966: 105–6) suspicion that another major building lay in the area of the Ostraca House seems quite plausible.

B. The Osorkon House

HES II, Plan 6, shows a new building occupying Grids D-E.11 and extending very slightly westward into C.11. This structure constitutes the so-called Osorkon House, situated in Summit Strip 7a.c, and including Rooms 740–741–742–743. Osorkon II ruled Egypt in Dynasty XXII, sometime during the early ninth century BCE (883–855 BCE according to Baer 1973: 11; 874–853 BCE according to Reisner [*Reisner Diary VI*, 555]; 874–850 BCE according to Kitchen 1996: 313). Plan 6 seems to present a fairly simple, straightforward building phase with few contemporary structures around it.

4 Although the northernmost corridor and set of three rooms also appear without hatching in this drawing, the report's narrative clarifies that neither Reisner nor Fisher intended to present these features as later additions; rather, they belong with the original Ostraca House, as outlined above.

5 The Two-Room Structure itself may have had at least two phases (judging from a close observation of the underlying two courses of stones in Rooms 407–408), or else some phase of the Ostraca House included bench-like features around some of the interior walls. Based on figure 26 alone, it remains difficult to settle this matter or other questions that arise from the published plans. Moreover, renewed excavation would pose its own challenges, given Reisner's method of backfilling areas excavated on the summit, as outlined above. Only a detailed analysis of the available sections (CD, GH, Subsidiary Section AB), assuming they were thoroughly and accurately drawn, can illuminate the place of these "later additions" in the stratigraphic sequence of the area (see Chapter 3).

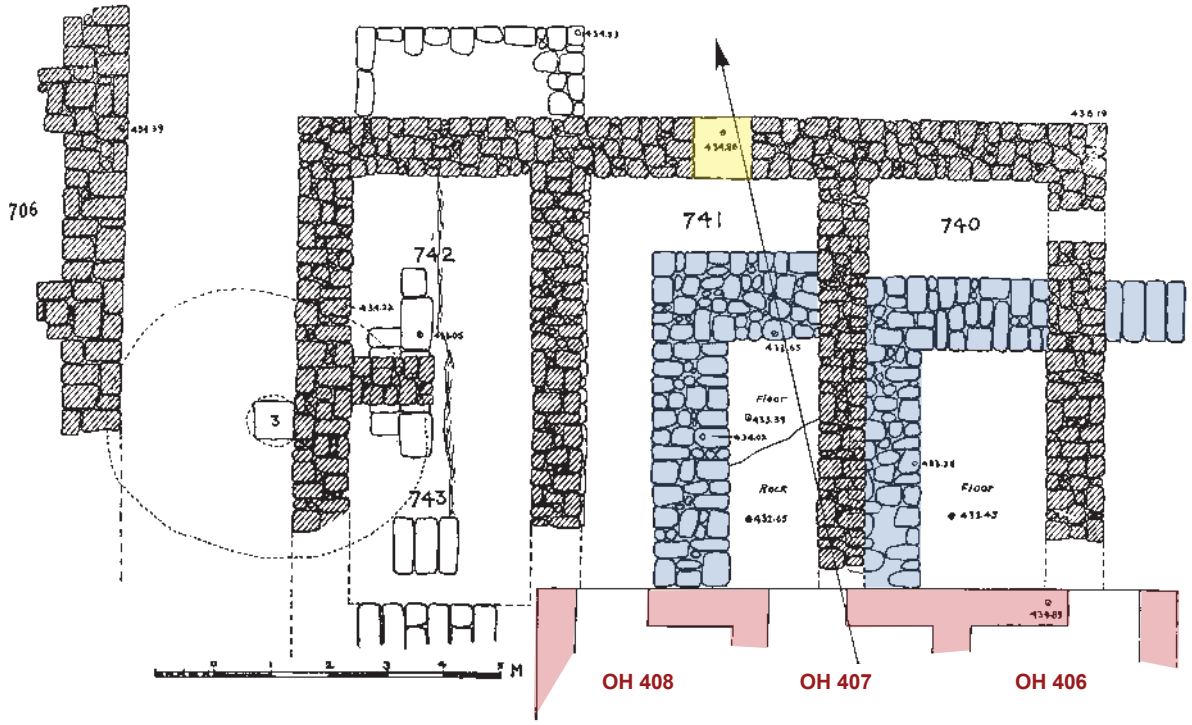


Fig. 30 *Relative positions of the Osorkon House (gray), Two-Room Structure (blue), and Ostraca House (red) (adapted from* HES I, *131, fig. 58).*

But upon close inspection of related plans and sections (e.g., fig. 30 = *HES I*, 131, fig. 58), the picture becomes more complicated. Even the attempt to determine the stratigraphic relationship between the Osorkon House and the Ostraca House brings to light the complexity of the depositional history on the western side of the summit compound.

Judging from *HES II*, Plans 5–6, the western wall of the Osorkon House ran along the line between Grids C-D.11 and, consequently, would have sat on top of or directly against the inside face of the earlier Casemate System.[6] The building's preserved dimensions measured roughly 8.2 m (north–south) by 14.1 m (east–west). A doorway in the north-central wall led to a central reception hall, labeled Room 741, at an elevation of 434.88 m (*HES I*, 59). Although Reisner wrote of this entryway, he did not comment on another apparent door in the eastern wall of Room 740 (see fig. 30). The floor of the Osorkon House lay roughly 50 cm higher in elevation than the Ostraca House level (*Reisner Diary VI*, 573).

The famous alabaster jar bearing the cartouche of Osorkon II may have come from the northeastern corner of Room 741, somewhere near or just east of the entryway area, although the precise findspot remains quite unclear.[7] In any event, photographic evidence of the jar *in situ* (*HES II*, pls. 37.b, 54.b;

6 Plan 6 also shows that a 3-meter-wide passageway (705) ran between the Osorkon House and a series of long, horizontally oriented rooms to the west (Rooms 706, 750, 751, 755)—rooms built against the inside face of the 4-m-thick, so-called Greek Fort Wall (see also *SS I*, Plan IV), dated to the second century BCE (*SS I*, 118–21). Ultimately, this Hellenistic wall was likely destroyed or heavily damaged by John Hyrcanus and repaired or rebuilt entirely by Gabinius. The eastern wall of 706–750–751 actually ran over the line of the erstwhile Israelite Casemate System. These observations show that, whatever the original purpose of this building, it post-dated the functional life of the Casemate System and that I may, therefore, omit it from my discussion.

7 Drawings of the jar and inscription appear in *Reisner Diary VI*, 554, and were also published with the official ostraca drawings. See *HES I*, 132; 243.a,b,c, for a drawing of the jar and its inscription; and 247, C.1, for a description of the findspot and the inscription. For alabaster Jar Type 6, see *HES I*, 334, 6a, fig. 205. For photographs, see *HES II*, pls. 54.b (for the *in situ* context) and 56g (for a close-up of

no sectional data available) suggests that, in fact, the fragments came from *beneath* the northeastern wall of 741, just east of the doorway. In this photo, the wall actually looks somewhat undermined by the excavators' technique. (Recall the nature of the excavation crews, as outlined above; by today's standards, excavation areas were greatly overpopulated, and the work undoubtedly progressed at an undesirably fast pace; see p. 21 above.) A journal entry from Thursday, September 1, 1910—a full seven days after the initial discovery of the jar—helps confirm that the related fragments were not found in one spot and that at least some of them were situated beneath walls.

> Carrying out the walls of the third series revealing the walls of the fourth series. Found *under wall* of third series *several more fragments* of the Osorkon vase.
> (*Reisner Diary VI*, 561–62; Reisner's underscoring; italics added; the "third series" seems to designate the second of two Hellenistic strata; but for Reisner's confusing enumeration of the four superimposed "series" of rooms, see Ch. 3, n. 5)

Thus the sherds of this jar did not emerge from a single, primary locus, and some of them actually lay trapped beneath the foundation courses of much later walls. These observations render somewhat spurious any chronological inference drawn from Reisner's assertion that his team retrieved the cartouche from a primary locus inside the room itself—"In the debris in room 741, near the north wall, were the fragments of a large alabaster jar bearing the cartouche of Osorkon II (Pl. 54 b)" (*HES I*, 132; see also n. 7 above). And how much more specious becomes the apparent inference that the Osorkon jar and Hebrew ostraca were removed from the same layer of black floor debris (a position that undoubtedly led to Reisner's associating the ostraca with Osorkon's reign and, locally, with the palace of Ahab in the ninth century BCE; see *HES I*, 227, and *Reisner Diary V*, 534). In fact, the wall and, by extension, the building to which it belonged clearly *post-date* the manufacture and functional life of the jar (and the reign of Osorkon II, with its terminal date in the mid ninth century BCE). In other words, one cannot with archaeological certainty relate the "Osorkon House" directly to the time of Osorkon II.

My more detailed analysis below of published Section CD reveals that this building likely belongs altogether in the post-Israelite era. In fact, workers retrieved this jar inscription from the same general context that yielded a fragment of ornamental ivory in the shape of a winged *uraeus* wearing the double crown of Egypt (*Reisner Diary VI*, 547). That Reisner, himself a trained Egyptologist, dated this artifact to the Ptolemaic period, "possibly somewhat earlier," recommends an understanding of the jar as a long-lost heirloom. His immediate attempt to relate this discovery directly to King Ahab and, by extension, to the emerging ostraca collection suffers from an unusual but serious lapse in his method of dating. He seized upon the jar's inscription, which was itself largely restored (only the –n of Osorkon having survived), as a chronological rudder by which he could steer when assigning a historical date to the Israelite palace and ostraca.

> The importance of this dated jar is inestimable for confirming our dating of the various walls, our Israelite potsherds (ostraca), and

the inscription itself). Note that in the last two citations in *HES I*, Reisner assigns the jar to *S7–742 sub*, i.e., to the subfloor makeup in Room 742, and this same provenance notation appears in his published diary of the "Progress of the Excavations" for Summit Strip 7, August 19–25 (*HES I*, 403). Yet in *HES I*, 132, he locates the findspot in the debris of *Room 741*, to the east of 742. In his unpublished diary (*Reisner Diary VI*, 747, 554), on the other hand, he clearly places the jar in *S7–712 sub*, i.e., below the floor of a room (712) that he dated to the Hellenistic period, and that was constructed directly above and that straddled Osorkon House Rooms 741–742 (*HES II*, Plan 7, Grid D.11). Elsewhere (*HES II*, pl. 54b), however, he assigns it to 7–747, i.e., to a room which is not identified on any published plan. (Perhaps the last 7 represents a dittography of the first 7, or a miswriting of an intended 1.) A close comparison of *HES II*, pls. 37.b and 54.b, suggests that the jar fragments came from the northeastern corner of 742 or 741, not from 747. But the two photos remain somewhat inconclusive. In any event, the findspot seems to have rested not only beneath the floor level but also under a wall.

> our identification of the palace. Moreover I do not see how we can reasonably escape the conclusion that this jar represents a present from Osrokon II to Ahab. Osorkon II made a campaign into Syria imitating the successful raid of his grandfather(?) Sheshonk (Shishaq). As I remember it, Samaria is not in Osorkon's list of hostile towns and probably Ahab and Osorkon were on a friendly footing. Possibly this jar was part of a present to Ahab in acknowledgement of some valuable present or some service during the campaign.
>
> (*Reisner Diary VI*, 555–56; Reisner's underscoring)

The obvious problem with such reckoning lies in the fact that Reisner dates everything by the sheer presence of the jar and its very fragmentary inscription, not by the overall stratigraphic context that yielded the jar. Certainly, if this vessel emerged from the fills beneath Hellenistic Room 712 (as his field notes maintain), and if the *uraeus* carving was—as Reisner believed—Ptolemaic in origin, then, historically speaking, the provenance of this inscription has no direct bearing on the life and activities of Pharaoh Osorkon. It represents a relic from the distant past that got caught up in the leveling fills for a house constructed half a millennium after the rule of that pharaoh. As indicated by Plan 6, this late dating may signal that the so-called "Osorkon House" was contemporary with Rooms 704–750–751–755 to its west. In any event, the jar's uncertain context (Room 712, 741, 742?) compromises any use of this artifact as a reliable chronological marker. Still, one can at least say that the Osorkon structure clearly post-dated the Ostraca House, since the southern walls of the former were built *over* the northernmost surviving wall of the latter.

Thus, at this point, two important stratigraphic observations emerge concerning the putative Osorkon House. First, the cartouche of Osorkon II provides only a *terminus post quem* for the building from whose fills it was recovered. Second, other remains from the Osorkon building itself must help to provide a *terminus ante quem* for the Ostraca House, since the former structure lay at a higher elevation and even partially overran the ostraca building. But if the Osorkon building post-dates the Ostraca House by a significant period, as one might suspect, then even that potential contribution is limited.

Although the plans presented for the Osorkon structure (*HES I*, 131, fig. 158; *HES II*, Plan 6) did not show its southern wall, and although this building extended roughly 5 m farther west than did the Ostraca House, Reisner reported that "the southern wall of the Osorkon House was built in part *over the foundations of the north wall of rooms 406, 407, and 408*" (*HES I*, 131; italics added). In fact, the published plans all seem to agree that the southernmost wall of the Osorkon House did not survive; therefore, any wall found along this line may have represented the northernmost surviving wall (versus the northernmost restored wall) of the Ostraca House. (Compare figs. 26 and 30 with *HES II*, Plans 5–6, D-E.11; see also the Cockayne Plan, which includes both buildings.) But, as outlined above, the northernmost end of the Otraca House also suffered destruction at some point. In fact, Reisner's own record seems to imply a rather widespread demolition of the Ostraca House architecture, especially the northern end of the building (*HES I*, 117, 131).

One result of this situation is that the eastern wall of the Osorkon House (which, judging from the published plans, did survive) was built against the previously blocked doorway located at the northeastern corner of Ostraca House Room 406. (See the combined plans of the two "Houses" in the Cockayne Plan.) Moreover, the much wider and better-built walls of the so-called "Two-Room Structure" also lay in the area of the Osorkon House (beneath Rooms 740–741).[8] Fisher clearly seems to have presented these walls as stratigraphi-

8 In figure 30, the two rooms appear somewhat offset, whereas in the Cockayne Plan they do not. In both depictions, the northern wall of these rooms seems to have continued eastward in a series of large ashlar blocks (drain covers?). At any rate, the construction style is quite different from that seen in the rest of the Two-Room Structure.

cally earlier than those of the Osorkon House. But they certainly do not belong to the Ostraca House. Thus, an intervening phase *between* the final use of the Ostraca House and the construction of the Osorkon House must have existed in this relatively circumscribed portion of the southwestern summit. Unfortunately, only one section, Principal Section CD, runs through this area (between Ostraca Rooms 407–408). It should, therefore, pick up the northern wall of this intervening phase; its doing so would help clarify the stratigraphic relationship between three successive buildings: the Ostraca House, the Two-Room Structure, and the Osorkon House.

But, as unfortunately demonstrated in detail below (Ch. 3), Section CD is not easily understood. First, as plotted on Plan 5, CD runs directly along/over the line of the wall between Ostraca House Rooms 407–408; this wall should, therefore, appear as a single, long, lateral feature in Section CD. Instead, the plan presents slices through Ostraca House Rooms 408–415–409, thus suggesting that the line of CD lay farther to the west than is plotted on published Plan 5. Second, it shows the northern wall of Ostraca House Room 408 as having survived to an approximate height of 2+ m. Third, there seems to be a different phase of wall built directly on top of the truncated northern wall of 408. Fourth, north of 408 the Ostraca House appears to have been cut away; later fill and a new floor level run north to an unidentified wall that rests on bedrock. This wall, which Reisner did not address, may represent the northern wall of the so-called Two-Room Structure or, more likely, yet another large building that preceded the Ostraca House level (see Ch. 3, B.1.a below). If the former possibility proves true, then its associated structure was built sometime *after* the Ostraca House but *before* the Osorkon House. In any case, the resultant logical conclusion suggests that, rather than seeing only two buildings in this area, one must recognize at least three major structures here (and perhaps even a fourth, if yet an earlier building phase preceded the Ostraca House).

C. Problems North and East of the Ostraca and Osorkon Houses

Another note regarding the horizontal plane around the Ostraca House merits attention before closing this portion of my study. Reisner's published Plan 6, representing the Babylonian period, shows the remains of a well-preserved structure (incorporating Room 776; Ch. 1, fig. 23) situated to the east of the Osorkon House in Grid F.11, near what would have constituted the northeastern corner of the Ostraca House.[9] This point lies in the presumed northward extension of the longitudinal Corridors 416–417, which ran along the eastern side of the preserved Ostraca House. In fact, however, the number for Room 776 (Plan 6) and another adjacent room (723) also appear in the plan of the Israelite Ostraca House (Plan 5; Ch. 1, fig. 22). Yet the lateral position, orientation, general construction technique, and configuration of rooms adjoining 776 on Babylonian Plan 6 seem to distinguish at least this structure from Corridor 417 in the Ostraca House proper on Plan 5. Still, the numbers for both Room 776 and nearby 723 appear on the plan of the Israelite ostraca building. In like manner, a related series of rooms (770, 771, and 772) also ran northward from 723 and westward from 776–773–777 (Plan 5, Grids E.9–11). Rooms 773 and 777 (*HES II*, Plan 5) lay directly

9 Interestingly, additional wall fragments also appear on Plan 6 to the east of these rooms and appear to align with Room 14 and Room/Pavement 13, situated near the foot of the rock scarp. These walls may represent the western extension of Rooms 12 and 11, i.e., the chambers that featured prominently in Franklin's (2003: 1–11) proposed identification of the royal tombs of Samaria. But see Ussishkin's (2007: 49–70) thorough rebuttal of her arguments. In any event, the architecture associated with this area, regardless of its original function, was likely more extensive than Franklin recognized. Reisner's suggested identification of Franklin's "royal tombs" as a "treasure chamber" or "prison" still seems more likely to be correct. Even if the tomb had been robbed, the walls certainly would have proven more ornate than those of Entryway 12 and Chamber 7. Note that on Principal Section GH, Fisher stippled in Chamber 7 below the main palace complex, even though this chamber lay roughly eight meters to the north of the actual section line. (Compare the astute observations in Ussishkin 2007: 63–64.) In Section GH, Reisner (or Fisher?) did, in fact, label it a rock "tomb."

north of 776, which suggests that at least some portions of those rooms were traceable to this point by the excavators. In his private field notes related to Ostracon No. 54 (assigned to S7-723 in the publications), Reisner places the findspot "in the northern end of the long street or corridor of the 4th series numbered 417 in S4" (*Reisner Diary VI*, 563; for explanation of the various "series," see Ch. 3, n. 5).

Judging from the banks of numerals that Reisner used to identify the area's architecture in each historical period he published,[10] all these rooms appear, in his view, to have belonged to the post-Israelite, Babylonian period. Indeed, that is clearly where Fisher presented Room 776 on Plan 6 Grid F.11, and the rest of these chambers likely belong there as well. These observations are important, since 29 of the 75 ostraca fragments (38.7%, well more than one-third) were assigned to these rooms (723, 772, 772N, 772W, 773, 776; see Table 1, above). Those inscriptions assigned to 772 were found "under Roman walls" (*Reisner Diary VI*, 589) and apparently in the extreme northern end of the area (*Reisner Diary VI*, 600), while those from 773 and 776 lay in debris poured against the face of Wall A (*Reisner Diary VI*, 597–98)—i.e., a locus that lay east of even the long corridor rooms of the Ostraca House, in what would represent the northward extension of the architecturally bare Alleyway 416. Moreover, additional diary entries reveal that ostraca continued to emerge from the floor(?) debris of Rooms 770–777 (*HES I*, 403, Sept. 16–26), in the northern extremity of S7 and considerably farther away from the Ostraca House proper than 723–776. Unfortunately, no more information is available. Lateral Section GH and Subsidiary Section AB ran just south of these rooms; longitudinal Section AB passed to their east; and longitudinal Section CD passed west of them.

The ambiguity that attends this area (Grids E-F.9–10–11), both in the written records and on Israelite Period Plan 5, may stem from the quick and somewhat destructive nature of work completed here already in 1908. An unpublished journal entry from Monday, September 19, 1910, underscoring Reisner's pursuit of detailed stratigraphic observation, both recognizes the northward continuation of an intact Wall A and laments the state of deposits left by Schumacher just west of that line.

> Along the west wall of the Herodian Temple in a piece of ground belonging to S7 but cut to pieces by Sch.'s trench, we have been cleaning away some Herodian walls. Underneath the newly found Israelite wall [= Wall A; *HES II*, Plan 5, F.9–14] runs straight north up to the vault (1908). Schumacher saw both sides of this wall as shown by his plan but of course was deceived by its being immediately under the Roman. One of the great faults of Pal. excavat. has been the failure to study deposits, intrusive excavations and the destruction of strata. There are places on the summit where we can prove that <u>at least</u> 4 buildings have been swept away leaving the Roman walls resting directly on the Israelite.
>
> The deposit of dirty yellow debris and the walls of the street behind the "ostraca house" continue northward along the face of this long Israelite wall. And here also, the surface of the yellow debris yields Israelite ostraca (see also p. 589).
>
> (*Field Book VI*, 592–94; Reisner's underscoring)

In short, early on in the project the 700-series of rooms may have suffered at the hands of the excavators themselves. Nevertheless, this diary entry, its cross-reference to "p. 589," and the ostraca sketches that accompany the entry help to situate the following published ostraca, discovered on September 16 and 19, 1910 (see Appendix B): Nos. 9, 11, 12, and 40 (Strip 7, Room 772); 27, 28, 29 (two of three fragments), 37, and 58 (Strip 7, Room 773); and 41 (Strip 7, Room 776). Alongside these inscriptions there

10 In rough order of excavation: Severus period = mid 300s; Herodian period (peristylium) = mid-to-upper 300s; Preherodian period = lower 700s + 800s; Babylonian period = mid 700s; Israelite (Ostraca House) = 400s. Note that the last series is the sequence that seems out of place, given the logical order of the excavation process.

appeared "a large potsherd (bowl) with only a few letters on the rim" (*Reisner Diary VI*, 595; Reg. No. 4557; Reisner chose not to publish it). A few days later, on September 22, another group of inked fragments emerged from the area of Room 772.

> In S7-772 north end, several more ostraca. This is now in the old Trench F. Luckily the workmen in 1908 did not go down deep; for they would certainly have missed these ostraca.
>
> (*Reisner Diary VI*, 600–601; see Ch. 1, fig. 10, for Trench F)

The last cache mentioned in the preceding quotation included ostraca published as Nos. 3 (S7–772 sub), 39 (S7–772 [sub?]), 47 (S7–772), and 56 (S7–772) plus a short, unpublished writing. (For further notes on this group, see Appendix B.) Subsequently, on Friday, September 23, "a few more Israelite potsherds came with the early morning from S7–772 (Trench F)" (*Reisner Diary VI*, 602; Reisner's underscoring). These additions incorporated into the growing and now near-complete collection Nos. 50 (S7–772 N), 52 (S7–772), 60 (S7–772), and yet another unpublished inscription.[11] Finally, on Monday, September 26, Reisner recorded two further epigraphic finds: "In clearing away Roman wall along W. side of S7–772 found two Israelite ostraca at the usual level" (*Reisner Diary VI*, 608). These inscriptions represent published ostraca Nos. 51 and 55, both correctly assigned to the horizontal space in S7–772 W. As for their vertical (stratigraphic) position, a more precise description than "at the usual level" remains a desideratum. But judging from even this laconic account, it certainly appears that they emerged from foundation trench materials deposited during the Roman period.

On rare occasions, Reisner's field notes appear to introduce their own confusion into the recording of findspots. On Tuesday, September 20, 1910, workers were

> clearing corner between vault (1908) and temple (Sch.'s trench). In space 772, an Israelite ostraca [*sic*] and some ~~potsherds~~ fragment[s] which join up Reg. nos. 4555 and 4556.
>
> (*Reisner Diary VI*, 596; Reisner's strikethrough)

The drawing of the first inscription found that morning (Reg. No. 4578) clearly matches that of published ostracon No. 6 (Reg. No. 3997), which Reisner had already assigned not to S7–772 but to S4-417 N on August 19.[12] But the fragment's own registration number, 4578, agrees with published ostracon No. 7, which did come from S7–772, as expected from the narrative. The other fragment from Room 772 received Reg. No. 4579 and, in fact, did join sherds 4555 and 4556—from Room 773—to provide a fuller reading for Ostracon No. 29 (*HES I*, 241; *Reisner Diary VI*, 597).[13] In any

11 After recovering this last group of inked fragments on September 23, 1910, Reisner noted that he "sent off [a] report on [the] ostraca to Prof. Lyon" (*Reisner Diary VI,* 603), thus fulfilling a desire he had harbored since September 18: "Have been hoping each week to get the report on these ostraca off to Prof. Lyon; but each time I have found some corrections which delayed the dispatch" (*VI,* 592).

12 Reisner's initial drawings of the ostraca supplemented the text in his field diaries (see Appendix E). The sketches generally display an exceptional overall quality and capture the attributes (palaeography, stance, scale, spacing, etc.) of the ancient script. These depictions, which also provide the sequence of discovery (see Appendix B), are particularly impressive through the presentation of Ostracon No. 54 (Reg. No. 4171) on Thursday, September 1, 1910 (*Reisner Diary VI,* 563). Starting with the next batch of drawings, entered on Saturday, September 17 (*VI,* 598–90; Nos. 9, 11, 12, 40; Reg. Nos. 4524, 4526, 4525, and 4527, respectively), the unpublished sketches appear to have been completed more hurriedly and with slightly less care. They remain, nevertheless, an invaluable resource for anyone interested in this corpus of writings. On Wednesday, October 26, 1910, Reisner wrote: "Photographing every Isr. ostracon. The inscriptions do not show on more than a ⅓ of them. I am making as good a facsimile as I can in ink on the prints" (*Reisner Diary VII,* 652; Reisner's underscoring). He concluded the collation and photographing of the ostraca on Wednesday, November 2, 1910 (*VII,* 671).

13 Reg. No. 4555 supplied the end of line 3 (מסק), while Reg. No. 4556 provided the beginning of lines 1–2 (ultimately read as אחמלך // בשת ∧ד מש); apparently, Reg. No. 4579 gave the remainder of the reading published in *HES I,* 235.

event, these fragments came from the small tab of excavated space that lay south of the Roman vault and that was incorporated into the northeastern corner of Summit Strip 7 in figures 18 and 23 above (see Grids E-F.9 on *HES II*, Plans 5 and 8).[14]

D. Summary

This discussion of the horizontal plane in and around the Ostraca House has demonstrated that the original footprint of this structure was much larger, certainly along its north–south axis, than is apparent on most of Fisher's published plans. The building's potential east–west axis proved more circumscribed for the ancient workers. A tall rock scarp and adjacent architectural elements (including Wall A) restricted its eastern extent, while the old Israelite Casemate System defined its available western limits. The local depositional history and the findspots of the ostraca themselves entail greater complexity than previously understood. (Only Kaufmann began to suspect this situation, and he raised very good questions in this vein.)

This chapter has demonstrated that a close examination of Fisher's Ostraca House drawing (fig. 26) reveals a well-planned, symmetrical design for the building. The impulse may exist to interpret the walls presented there simply as foundation features, with the original doorways having been located at a higher elevation, thus allowing for a connection between the square chambers to the west and the long, rectangular rooms to the east. The drawing, however, clearly shows existing passageways between all the square rooms and even between some of the Long-Rooms, but no pathway between the two areas. Thus, even if the walls belonged to basement rooms, the depiction of 20 internal doorways and three exits to the outside certainly suggests that the chambers did not serve merely as a subterranean support system, but as serviceable rooms. I have shown that, while Reisner himself seems at certain points in his diary to have considered the possibility that these walls were ordinary foundation pilings, elsewhere in his private notes and throughout the published report he presents them as exposed features of functioning rooms—a conclusion consonant with the presence of so many doorways in the various walls.

I have also shown that the hard-packed, clean yellow debris, through which the walls penetrated, must represent earlier activity in this area of the western summit. Moreover, all epigraphic finds deriving from the subsequent, massive imported fill of dirty yellow matrix constitute artifacts from a secondary context, certainly not primary occupational debris lying directly on a thick, well-preserved floor level. This soil could have come from anywhere on or around the summit of the site, or perhaps even beyond. That fact compromises any attempt to tie inscriptions found there directly to the functional life this particular building. Similarly, in the Long-Rooms along the eastern side of the structure, other ostraca appear to have derived from comparable fills associated with the construction of the overlying Hellenistic Street Alpha.

In addition, I have demonstrated that it seems quite unlikely that the Osorkon jar fragments, whose provenance was entirely distinct from that of the ostraca, can assist in assigning a firm, tightly defined date either to the Ostraca House or even to the Osorkon House itself (despite Reisner's exuberant statements to the contrary). This unlikelihood is especially true if, as it seems, those fragments emerged from beneath the northern wall of the Osorkon House, i.e., in an apparent foundation trench deposit of simple backfill. The most they can do is contribute to establishing a *terminus post quem* (a construction date) for the building, a datum that proves only nominally helpful to an analysis of the ostraca. As welcome as it always is to find a pharaoh's cartouche, here this evidence

14 On Saturday, October 22, 1910, yet another fragment appeared in Summit Strip 11, in the far northwestern corner of the excavation, roughly 50 m north of the Ostraca House. Reisner wrote: "On Saturday, in the yellow debris in the mouth of the cave in S11, we found a small piece of the rim of an Israelite bowl (burnished pottery) inscribed (scratched)" with the letters ליה (*Reisner Diary VII,* 651). In Reisner's field sketch the script of this incised writing matches that of the ostraca quite well, though this particular sequence of letters does not appear anywhere in the ostraca corpus.

proves much less useful for dating the functional life of the Osorkon House or determining the time of its demise (i.e., its *terminus ante quem*). The jar fragments can hardly provide "complete proof" (*HES I*, 60) for an *absolute* date of this building, as claimed by Reisner in 1910. If the Ostraca House and its associated inscriptions date to the early eighth century BCE, as per conventional wisdom, then the Osorkon House postdates that period, but we cannot know by how much time based only on this jar. In any event, its archaeological context cannot belong to the rule of Osorkon II. If it did, and if the Osorkon jar represented a prize gift to Ahab from the Egyptian pharaoh, the Israelite king would have had to smash the vessel almost immediately and to use the fragments as part of a foundation deposit.

In short, at this point in my study one can hardly see a clean, well-preserved, primary context for the ostraca discussed thus far. Evidence strongly suggests that either more than one building existed in this area, or a single building passed through several distinct phases of construction and use. In any event, a significant robbing of its stones occurred during the erection of the so-called Osorkon House to the north (which itself remains difficult to date with precision due to the poorly-documented locus of its principle artifact, the Osorkon Jar) and, later still, in the construction of the Greek Fort Wall to the south.

Chapter 3

Vertical Axis: The Ostraca Building's Complex Depositional History

A. Stratigraphic Analysis: Summary of Sections Published by the Harvard Expedition

The official report from the Harvard Expedition contains a generous number of section drawings dispersed throughout the course of the narrative in *HES I* and among the master drawings published in *HES II* (e.g., see fig. 31). But serious challenges present themselves when using these resources. For example, the plans and especially the sections are very poorly, even awkwardly labeled. Walls and other features appear so scantily clad with locus or identification numbers that associating them with descriptions in the report's narrative proves difficult. Section lines sometimes angle east or west as many as 5–8 m from their paths as plotted on the plans, with no indication of these course adjustments on the section drawings themselves—a situation that renders the false appearance that the sections followed a straight, uninterrupted route. Occasionally, portions of a drawing were simply left blank, without any of the local stratigraphy shown for that space and without accounting for its absence. At other times, features that lay well off a section line were nevertheless incorporated into the published drawing, thereby creating a false impression that certain direct spatial and perhaps even temporal relationships existed when they did not. That is to say, in repeated but unnoted instances, section drawings reflect not only the expected plane of the vertical cut, but they also have depth; rather than remaining a two-dimensional slice with height and width, they become, without notice, three dimensional in nature. As a result of these and other challenges posed by the final report, one must invest considerable work in coordinating the architecture on the plans with the features in the section drawings. So despite Reisner's apparent dedication to detailed recording (see the introductory quotation in Ch. 1), a host of problems manifest themselves in studying the Ostraca House.

At least fourteen named section drawings appear in the official Harvard report, along with a smattering of unnamed drawings.[1] The principal drawings include the following (cf. figs. 31–32):

Section AB: a longitudinal cut extending from Grid G.1 southward to Grid H.20; to obtain the full plot of this section, one must piece to-

1 E.g., see *HES I* , 97, figs. 20, 22; 103, figs. 25–26; 105, fig. 27; 121, fig. 48; and others from the Israelite period alone.

Fig. 31 *Overview of published Sections CD, GH, JK, and M* (HES II, *Plan 4*).

gether a composite picture from *HES II,* Plans 5 (Israelite), 7 (Preherodian), and 8 (Herodian); this section includes both significant adjustments in its course that are nowhere indicated on the actual drawing and various architectural elements that lay well off the section line (see the discussion of subsidiary sections below);

Section CD: a longitudinal section running parallel to and ca. 30 m west of AB, in Grids D.2–16/17; although this section plot *appears* to maintain a straight course, without breaks or unnoted changes, it makes a significant course adjustment near the very center of the Ostraca House; *HES II,* Plan 5 does not present CD in its full length; again, one must patch together a composite picture from *HES II,* Plans 7 (Preherodian; D.3–17) and 8 (Herodian); this drawing passes through the western half of the Ostraca House (*HES II,* Plan 5, D.11–14) and therefore relates directly to the present study (see nn. 2–3, below);

Section EF: a lateral section following a straight course through the center of Grids B-K.7; while *HES II,* Plans 5 (Israelite), 8 (Herodian), and 9 (Severus Period) each contain a full plot of this section, the published drawing itself (Plan 11) presents only a small portion of the cut; the report nowhere provides a rendering of the complete section (see n. 3, below);

Section GH: a lateral section following a straight course through Grids A-L.12, ca. 1.5 m inside (south of) their northern edge; the published drawing in *HES II,* Plan 4 presents Grids B-K.12; but again, to gain a full horizontal plotting of this plane one must cobble together a composite picture from *HES II,* Plans 5 (Israelite), 7 (Preherodian), and 8 (Herodian); this section also becomes quite relevant to this study, since it passed directly through the three preserved northern chambers (406, 407, 408) and two eastern corridors (401, 416) of the Ostraca House (see *HES II,* Plan 5, Grids D-E-F.12);

Section JK: an apparent longitudinal section partially presented in *HES II,* Plan 4 (Grids 10–13); no horizontal plan in the report contains a plot of this section;

Section M: an obliquely oriented section traversing the northern half of Grids J-K.11 (see *HES II,* Plan 7, Preherodian); the published portion of this section appears in *HES II,* Plan 4 and relates to Grids J-K.11; again, this section does not appear on any horizontal plan in the report.

The final report also contains a generous number of subsidiary sections; unfortunately, many of them carry the same identification tags as the already-named principal sections. For example, Reisner (or Fisher, who signed all the drawings) used the rubric "AB" to identify at least four auxiliary sections.[2] The first one seems to have cut laterally through the pool of Samaria (Grids F-G-H.5) and was labeled AB even though the Israelite Plan 5 shows that the Principal Section AB itself ran longitudinally through the eastern pool area (in G.5).[3] Another Subsidiary AB ran

2 Additional subsidiary sections appear here and there throughout the report but are not germane to my study. For example, see Section YY, from the Preherodian period, which cut through Cistern 14 (assigned to Room 45 in *HES I,* 146, fig. 67, but to Room 45–46 in *HES II,* Plan 7); also an unlabeled and unplotted section in *HES I,* 154, fig. 69, that ran through Street Lambda and Rooms 314 + 318 in the plan for the Preherodian town (*HES II,* Plan 7, Grids D.15–16); compare *HES I,* 157, fig. 72, for another unlabeled section through Preherodian Room 53 (*HES II,* Plan 7, Grid G.16); a course that must lie near to the line of Principal Section AB as it cut through Room 53; and a transverse Section R in Summit Strip 11 (*HES I,* 168, fig. 81).

3 Moreover, just west of the Principal Section AB, there seems to have been yet another subsidiary, longitudinal cut labeled CD (a tag already assigned to Principal Section CD) that ran through the eastern end of the smaller rebuild. Fisher plotted the two subsidiary sections (AB and CD) on the plan in *HES I,* 112, fig. 40, and he drew them in section in *HES I,* 113, fig. 41. For another duplicate section label, note Section EF, near the West Gate (*HES I,* 120–21; *HES II,* Plans 1, 11); this drawing appears with virtually no identifying labels for the features it contains, and it is not plotted on any published plan. The pool itself, located in Summit Strip 6, received first mention in Reisner's unpublished journals in *Reisner Diary V,* 535, written on Thursday, August 18, 1910.

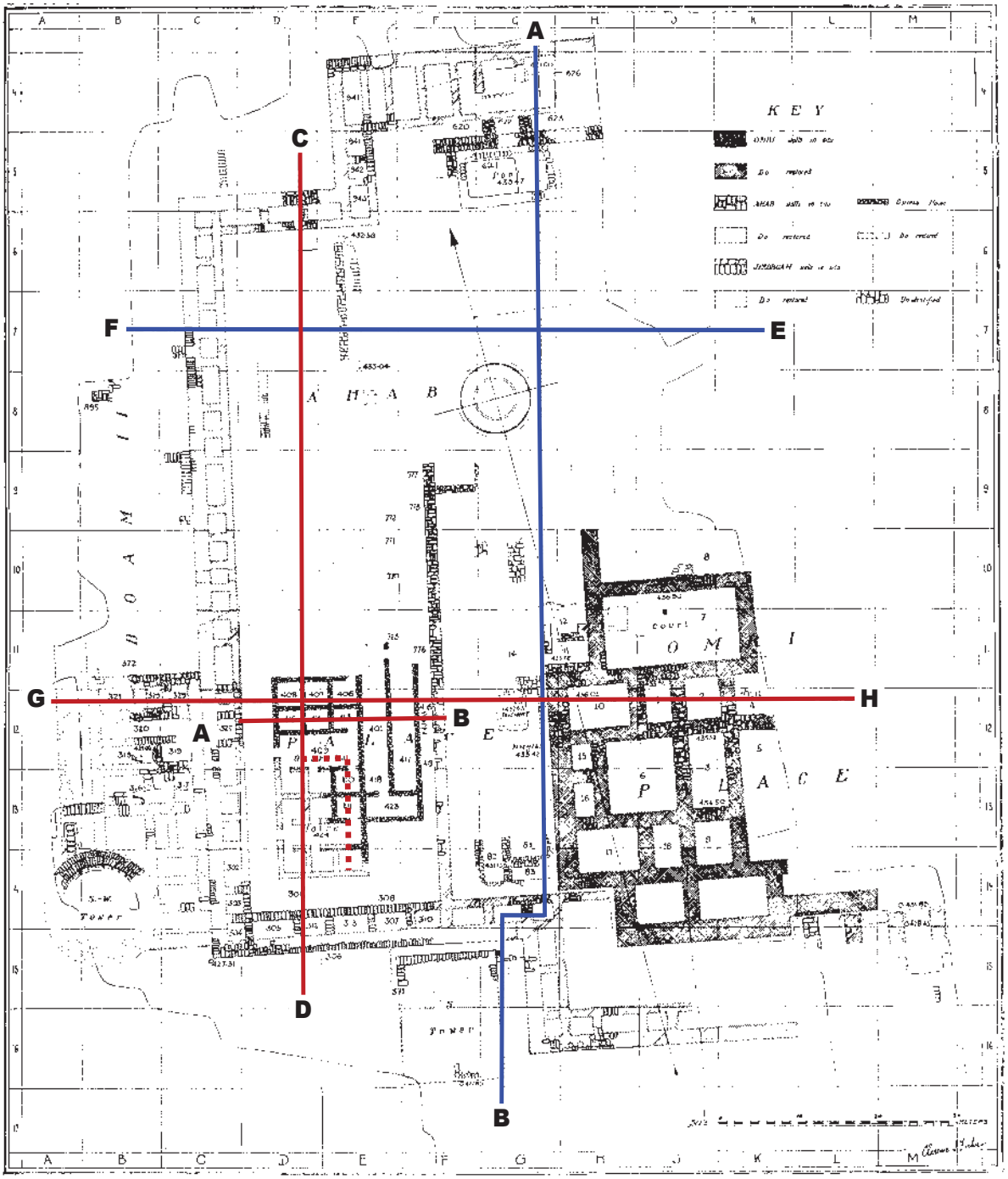


Fig. 32 *Plots of selected section cuts on western summit (red = sections germane to a study of the Ostraca House and its environs; adapted from* HES II, *Plan 5).*

longitudinally through the vault associated with the Roman-period temple (*HES I,* 172, fig. 86; *HES II,* pl. 8, E-F.8), while yet another lateral subsidiary section shown on this plan remains unidentified. A third Subsidiary AB proceeded through the so-called Atrium House in Grids C-E.9–12 (*HES I,* 181, fig. 97, for a plan and 183, fig. 99, for a section). This Herodian-period House overlapped

the two northernmost rows of rooms in the earlier Ostraca House. Thus, stratigraphically speaking, the so-called Osorkon House should lie *between* the Israelite Ostraca House and the Atrium House. (See the full discussion below.)

Finally, Fisher labeled a fourth subsidiary section "AB" and presented it on plans in *HES I,* 64, fig. 15; 114, fig. 42. This cut took a latitudinal course directly through the Ostraca House while running parallel to and only slightly (ca. 2.6/2.7 m) south of Principal Section GH. On the east, both sections sliced through the long eastern corridors 401, 417, and the alleyway in 416/419. But Subsidiary Section AB then proceeded through Rooms 415, 414, and 413 (with the last chamber apparently mislabeled as Room 405 in the drawing), while GH crossed Rooms 408, 407, and 406 to the immediate north (fig. 32). Taken together, these two drawings provide valuable comparative data for investigating the Ostraca House.

I need not multiply examples of duplicate section tags. Clearly, Reisner (or Fisher) unwisely chose to identify such auxiliary sections by repeating the labels already appropriated for the principal sections. Duplication of this sort within a record-keeping system—whether with sections, loci, basket numbers, or features—breeds potential confusion during all phases of the field work and publication process.

B. Stratigraphic Analysis: Discussion of Sections Relevant to the Ostraca House

Of all the drawings mentioned above, Principal Sections CD and GH plus Subsidiary Section AB (in red on fig. 32) prove germane to an investigation of the Ostraca House and its environs. While CD passed through the Ostraca House from north to south, GH and Subsidiary AB followed closely spaced routes along an east–west axis. The paths of these sections (CD + GH/Subsidiary AB) intersected in the storerooms situated north of the central Entrance Hall 409, in the best-preserved portion of the overall structure (see fig. 41, vertical arrow, upper-left). Together, these sections will guide my discussion of the vertical axis, or depositional history, of the Ostraca House. On the macro level, my examination of these drawings seeks to clarify the stratigraphic relationship between four distinct buildings that appear to have occupied roughly the same area near the southwestern corner of the summit: a possible pre-Ostraca House building, the Ostraca House itself, what I have called the Two-Room Structure overlying the northern side of the ostraca building, and the so-called Osorkon House. Looking at the drawings from a micro or more local perspective, I shall identify and describe in full the depositional history inside and around the Ostraca House, specifically in the areas from which excavators retrieved the inscriptions.

1. Principal Longitudinal Section CD

Since Primary Section CD supposedly transected the outlines of all four structures mentioned above, these buildings should appear in a temporal sequence on the vertical axis of this drawing. As plotted on *HES II,* Plan 5, the line of CD proceeded from north to south and passed through the area of the Osorkon House and Two-Room Structure before entering the westernmost chambers of the Ostraca House (Rooms 408, 415, Hallway 409, two unlabeled chambers in the middle of the overall structure, Hallway 424, and another, unlabeled room in the southwestern corner of the building). South of the Ostraca House, the section plot continued through 301 (a space between the ostraca building and the southern Casemate Wall) before entering and passing through Casemate Chamber 312 and continuing farther south.

In the published section drawing (*HES II,* Plan 4), the first three spaces (408, 415, 409) received clear labels in Grids D.11–12 (fig. 33). But following that point, the drawing shifts (without any notation) at least 5.5 m to the east, where it suddenly depicts Rooms 410, 411, and Hallway 424, all in Grid E.13 (figs. 32, 35). Any examination of this drawing must, therefore, distinguish between Grids D.11–12 and E.13–15. Failure to do so will result in misguided conclusions.[4]

4 Note also that Fisher drew the E-squares at a slightly different vertical scale from that of the D-squares.

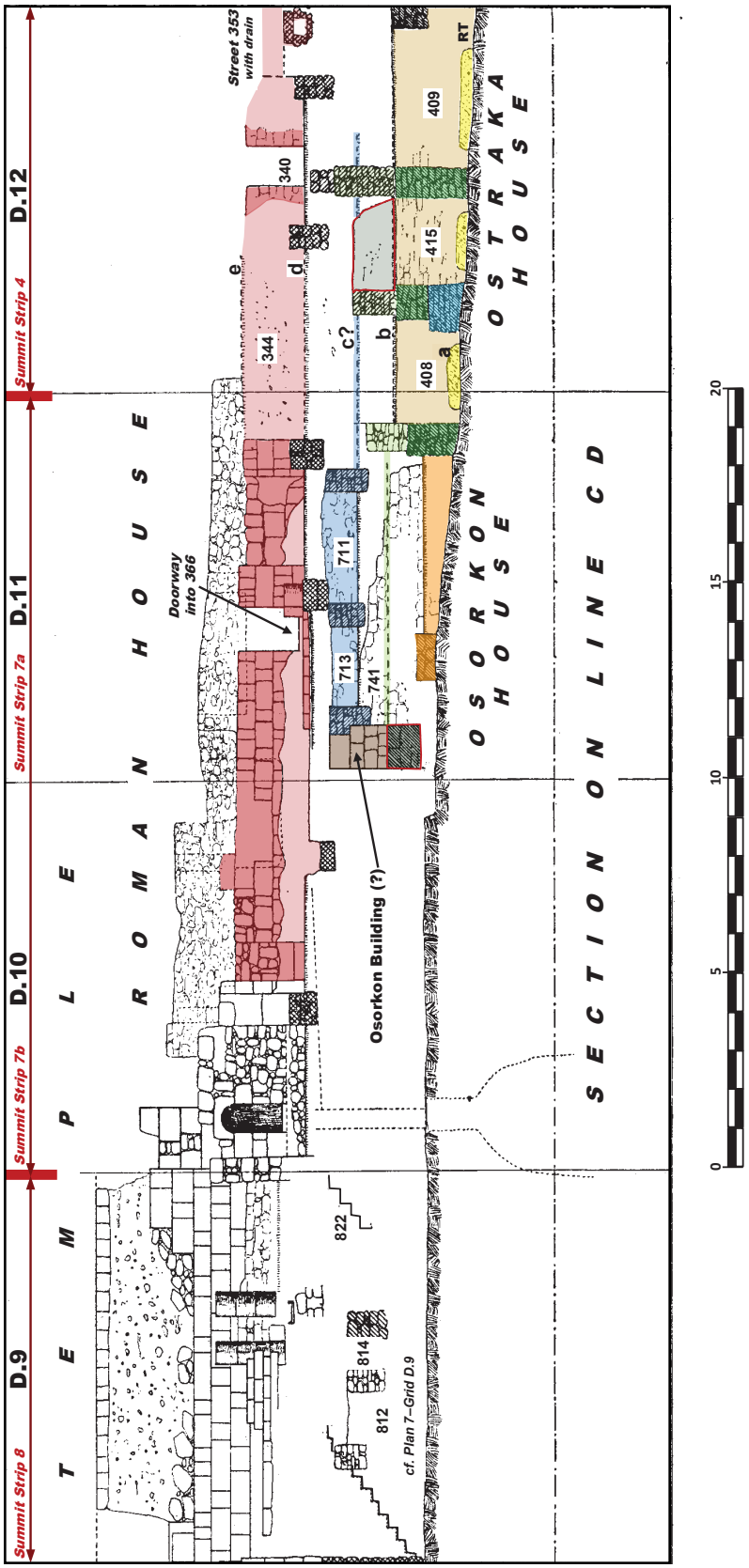


Fig. 33 *Section CD: Grid D (adapted from* HES II, *Plan 4).*

This situation, unforeseen by most readers, undoubtedly owes to the fact that in the southern set of six rooms the walls of the eastern chambers proved much better preserved than those of the more westerly ones, which Reisner reconstructed on the basis of foundation trenches (see fig. 26 above; also *HES II,* Plan 5, comparing D.13–14 with E.13–14). Rather than continuing his drawing of CD through a totally restored or even hypothetical portion of the building, then, Fisher simply shifted its course eastward, where architecture survived intact. Any excavator knows the dramatic difference that can occur in the local stratigraphy over a 5–6 m run. Minimally, it was incumbent upon Fisher (who completed and signed the section drawings in 1912) to notify the reader of this significant change in the line of CD—an obligation he did not satisfy.

1.a. Section CD: Excavation Grids D.10–11–12 (fig. 33)

A full review of this drawing requires the mention of all features situated above and around the Ostraca House remains. The Roman-period (Herodian) Atrium House (red level) with peristylium (a columned/colonnaded porch or courtyard, here with heart-shaped columns on the corners, as seen in Plan 8) stretches across Grids D.10–12.[5] The clearly labeled Room 340 appears in D.12. The longer Room 344, whose number I have added, must lie directly to its north (left on the drawing), while the somewhat elevated level of Street 353 and its underlying conduit (*HES I,* 169) run parallel and just south of 340. Immediately north of 344, an open walkway passes south of the peristylium, and then comes the area of the peristylium itself. Note that this section cuts through this area slightly east of center; therefore, what appear to be fills and a solid wall running across the floor of the peristylium and the walkway to the south actually constitute the western wall of Rooms 366–367, a feature situated ca. 5.5 m east of CD. In this wall, just to the right of center, a doorway opened into Room 366 (compare *HES I,* 181, fig. 97, and *HES II,* Plan 8).[6] The same situation applies to the blocks

5 For a plan of the Atrium House, see *HES I,* 181, fig. 97; for longitudinal Section CD through the Atrium House, see *HES II,* Plan 8 = Reisner's Level "d" in Section CD; a lateral Subsidiary Section AB (see above) also cut eastward from Street W west of the Atrium House to Room 366 and Street C, with its sub-paving conduit, on the eastern side of the Atrium House; *HES I,* 183, fig. 99.

In his field diaries, Reisner records four series of rooms in this area. But his numbering system for these levels proves quite confusing. Initially, he clearly seems to have labeled the strata from earliest to latest as follows: 4 = Roman/Herodian period; 3 = second Greek level; 2 = first Greek level; 1 = Israelite/Ostraca House level. This understanding receives support from his first encounter and description of the Atrium House rooms (cf. *Reisner Diary V,* 498–99). Here he noted Room Nos. 330–345 and even sketched 340, 341, 342, and 345 (which ultimately became No. 356 on published Plan 8), all of which received the general identification of "top level." He noted the juxtaposition of the bath house to the Herodian Temple wall and then drew the stratigraphic observation that 40 cm "beneath the floor of the top series" lay "the floor of a house cut to pieces in the construction of the top series." Farther down, at "about 150 cm below the top level we have the floor of a *third series* of structures…. The floor level of the third series is about on a level with the floor level of the 'Bab.' wall" (i.e., with what became the Greek Fort Wall; pp. 498–99, italics added). These features, then, likely date to sometime in or near the second century BCE (see Section 1.b. and n. 10, below; see also the "Hellen. I" phase on Subsidiary Section AB = fig. 41 below).

By *Reisner Diary V,* 504, however, it gradually appears that this "third series" now represents the level that lies over the Ostraca House (which is, in turn, the "fourth series"). In fact, "the debris in [the] rooms of the third series is a curious dirty yellow, loosely packed [matrix], as if the rooms had been filled and covered with dirt excavated from deposits of Israelite debris (for example possibly from the foundation trenches of the Herodian walls)" (Reisner's underscoring). And, finally, on page 506 Reisner explicitly lists and labels the levels in question as follows: 1 = Roman; 2 = Seleucid I; 3 = Seleucid II; 4 = "main series containing yellow dirt" (the Israelite stratum). Now his numbering system runs from top to bottom. Note also that, in the later official publication, he once again inverted the ordering of the two Seleucid levels, as seen on the left-hand side of fig. 41.

Following page 506 in *Reisner Diary V,* the notes seem consistent in their references to the Israelite stratum as the fourth series (cf. pp. 515–16, 534, et passim). Workers recovered a fragment of ornamental ivory in the shape of a winged *uraeus* wearing the double crown of Egypt from "just under the floor of a house of the third series…" (*Reisner Diary VI,* 547). Reisner dated this object and an accompanying alabaster jar to the "early Ptolemaic period, possibly somewhat earlier." These items, then, appeared in the fills situated somewhere *between* the Ostraca House and the earliest Hellenistic levels.

6 The Subsidiary Section AB (fig. 41), discussed below, complements this view; it ran from the area beneath

Fig. 34 *Preherodian town plan, showing relative location of Ostraca House (red) (adapted from* HES II, *Plan 7).*

and doorway depicted in Room 340. The doorway seen here actually led to Room 341, situated ca. 3 m to the east of the longitudinal line of CD. Thus this drawing provides further examples of the misleading inclusion of features set well off a section line. Fisher's proclivity in this regard will reemerge in the analysis of earlier deposits.[7]

Reisner assigned the features below the Herodian fills—features that include a wall, floor level, and thick accumulations inside Rooms 711 and 713—to the Preherodian Town (*HES II,* Plan 7 = blue level "c" in Section CD, fig. 33; compare the auxiliary section in *HES I,* 183, fig. 99). Subsidiary Section AB, discussed below, identifies this level as the First Greek Period, or Hellen[istic] I, Insula VI (*HES I,* 134–35, 152–53; also n. 5 above). Reisner dated this period between 300–60 BCE (*HES I,* 134). During this time, the site continued as a large city fortified by a new enclosure wall that followed the same lines as the preceding Babylonian and Israelite city walls.

The Hellenistic city showed a well organized street plan, and Reisner called the main street, which ran north–south, Street Alpha (*HES II,* Plan 7, E-D.5–14; see my fig. 34). Alpha measured 2.5 m wide down to where it intersected with the perpendicular Street Beta in E-F.9; from there south, it apparently narrowed to 2.1 m in width. This street and its cross streets divided the houses into groups, which Reisner called *insulae*. Excavations revealed eight such groups, labeled I–VIII. Insulae I, III, and VI lay west of Street Alpha; II, IV, and V lay to its east (with Insula IV containing the best-preserved houses); VII and VIII both lay to the south of these two neighborhoods, along the northern and southern sides of Street Lambda. (Insula VII, especially, suffered heavy destruction; *HES I,* 153.)

The area occupied earlier by the Ostraca House fell within Insula VI (fig. 34, with the footprint of the Ostraca House highlighted), over and immediately west of the 2.1-m-wide Lower Street Alpha. Its surviving portions relate primarily to the central and southern rooms described below, but Reisner's proposed expanded footprint, drawn to accommodate the projected northern rooms, extends beneath the area of Hellenistic Rooms 711 and 713 (as outlined in fig. 34).

On Section CD, which followed a longitudinal course, the more northerly Rooms 713 and (to the immediate south) 711 appear clearly labeled in excavation Grid D.11 (fig. 33, blue level). These chambers lay in Insula VI, just north of the *surviving* remains of the Ostraca House (i.e., the northern wall of Room 408; see *HES II,* Plan 7). But, as noted, they overlay the area of the northernmost, unpreserved extension of the Ostraca House (as reconstructed in Ch. 2, fig. 26). The eastern end of Room 711 was "carelessly rebuilt" at some point during the occupation of the house, "… and the new walls at the northeast corner did not line up with the older walls" (*HES I,* 152). Reisner believed that these two rooms opened into a large courtyard area to the immediate west, labeled 712. Judging from Fisher's section drawing, the floor level inside these rooms also seems to have survived relatively intact. But this conclusion remains questionable, since Reisner stated that "the floor of these rooms, *as established by several doors still in situ,* was from 75 cm. to 90 cm. below that of the court of the Roman [Atrium] house" (*HES I,* 152; italics added).

Note that in Section CD the northern wall of 713 actually abutted the top and inside (southern) face of a wall labeled in Reisner's key as "unidentified" (fig. 33, brown feature, above the first "o" of

Roman Street C/347 on the eastern side of the Atrium House westward through Room 356 (originally labeled 345 in Reisner's field notes; see n. 5), and to the wall between 356 and Room 344; Room 343 would then lie just to the west (left on the drawing) of 344. (To serve as guides, I have added these identifying labels to figs. 37 and 40.)

7 These facts also confirm Ussishkin's (2007) critical observations relative to Franklin's arguments (see Ch. 2, n. 9). The lateral Subsidiary Section AB, cut farther to the south and through a portion of the Ostraca House, shows the same drawing methodology. This auxiliary section ran through Room 356 (but south of Principal Section GH). Once again, the northern wall of that room, which ran parallel to the section and ca. 4.5 m north of it, is depicted (without notice) as a kind of background to Room 356. The two doorways are visible—one near the northwestern corner of the room; the other near the center of room and leading into adjacent Room 365. (The elevation inside this doorway lay at 436.40 m.)

"Osorkon"). This feature likely represents the surviving northern wall of Room 741 of the Osorkon House (cf. below). While the floor level (or portions of it) may have survived in Rooms 711 and 713, south of Room 711 all floors seem to have suffered destruction (at least partially, as indicated by the dotted line in Fisher's drawing) during preparations for the later Atrium House. Once again, Fisher drafted in faint traces of the *eastern* walls of these two rooms, walls that had to lie 1–3.5 m east of the section line.

South of 711, as witnessed on both Section CD and Plan 7, a disturbed area with few architectural remains stretched ca. 4 m farther south before surviving walls reappeared in the central area of Insula VI—now clearly over the preserved area of the Ostraca House. No more offset background walls or other features appear in CD for the Hellenistic remains in this area. Fisher's hatching patterns tie together two or possibly three walls belonging to these more southerly rooms (in Grid D.12) and assigned to the Preherodian phase (fig. 33, light green). These rooms lay directly over the northernmost surviving chambers of the Ostraca House (406, 407, 408 + 413, 414, 415 + Entrance Hall 409 + Long-Rooms 401 and 417 to the east). Street Alpha, to the immediate east of these rooms, directly overran the strip of space related to Ostraca House 417. (It is noteworthy how closely later wall lines followed courses established five to six centuries earlier.)

These truncated walls vary in preserved height and rise to just below or just above the theorized, extended floor level of the early Hellenistic structures. The northernmost feature rests directly on the remains of at least two phases of earlier walls that supposedly separated Ostraca House Rooms 408 and 415. (Reisner/Fisher assigned both of these earlier wall phases to Jeroboam II in the Section CD drawing, but apparently to Ahab in the Subsidiary Section AB; see below.) The overriding Hellenistic(?) wall had a nearly 2-m-thick deposit of fill poured against its southern face (outlined in red in fig. 33). Although the nature and date of this deposit remained unspecified, Fisher's drawing clearly shows that the next set of walls to the south cut through it.

The more southerly wall (in the center of Grid D.12), which penetrated the aforementioned fill deposit, also sat directly on the remains of an ostensible Ostraca House wall, this time between Room 415 and Entrance Hall 409. Moreover, this Preherodian wall clearly seems to have yet another, later, smaller wall built on top of it. This observation suggests the presence of at least two significant phases of construction that likely represent two entirely different building periods. No apparent floor level is associated with the narrower, upper wall, which stops short of the Herodian Period levels (Room 340). Fisher tried to extend the Preherodian (Hellenistic) floor line through the center of the lower, wider wall; but this attempt remains totally unconvincing. In short, these two wall phases plus the wall to their north (with the fill deposit poured against it) are drawn beneath the floor level of Herodian Rooms 340–344 and, in the absence of a fuller depiction of local stratigraphy, appear simply to be floating in Herodian fills. Without a more complete ceramic picture, it remains difficult to refine their dating (or even to assign any date to them) with confidence. At this point, the most credible conclusion suggests that at least the taller, earlier phase of the more southerly wall somehow survived its original functional life and was incorporated into the Preherodian, Hellenistic structures. The upper portion, a smaller wall stub of only three courses, must remain simply "unidentified" or associated with Reisner's "Hellenistic II" phase. But it appears to represent a phase that does not go with anything else in the section drawing. While Reisner originally thought of these two stacked features as an "Unidentified Phase," Fisher hatched them both as Preherodian in his section drawing.

One notices immediately, however, that all the walls of these rooms (light green) were set much deeper (ca. 1.5 m) than those of Rooms 711 and 713 to the north. Moreover, the significant fill that workers poured against the southern face of the more northerly wall appears to have risen up to or even just above the elevation of the proposed early Hellenistic floor levels in the other rooms.

Reisner nowhere commented on or identified this 2-m+-thick deposit (although he would likely have included it in his "Greek construction debris" = deposit "f" on lateral Subsidiary Section AB). Finally, the construction of the taller, more southerly wall clearly seems later in time, since it cut through this fill deposit. So the drawing of these enigmatic walls may depict two or more actual phases of building. There is yet another sizeable wall fragment floating in fill at the very southern end of Ostraca House Room 409.

These observations raise several questions concerning the overall phasing of these features. Do the three southerly walls depicted in Grid D.12 even belong together? Should one posit a stratigraphic relationship between these walls and simply leave them somewhere in the long span between the Iron Age and the Hellenistic Period? Could they represent even earlier (pre-Greek) walls that survived and were reused, in the Hellenistic period? If so, is it possible that these walls constitute a southerly extension of the pre-Hellenistic Osorkon House (see below). If they are pre-Hellenistic but *not* the Osorkon House, then what building and time period do they represent? In any event, Section CD becomes quite convoluted at this level. Nevertheless, one must recognize that decisions reached here will affect the interpretation of earlier deposits related to the Ostraca House.

Reisner recorded at least five important stratigraphic observations relating to the full roster of aforementioned features (*HES I,* 152). First, Greek workers built their rooms "largely on the foundations of the Ostraca House" and copied the basic size and shape of the Ostraca House rooms. Second, "very little filling had been done, as the surface of decay of the Ostraca House was left undisturbed." Third, "where the new walls did not coincide with those below, the foundations rarely cut through this surface of decay, but generally rested immediately upon it." Fourth, excavators found traces of walls and earthen floors from a *second phase* of Hellenistic rooms 30–40 cm above the first phase (represented by the dark-color walls positioned immediately below the Atrium House red level in fig. 33). Fifth, this second Greek phase suffered almost total removal during the subsequent construction of the Atrium House, whose floor levels rested only a few centimeters above the level of these rooms. Subsidiary Section AB corroborates this situation, wherein the Hellenistic Phase II appears relatively shallow.

Logically, the next level down in Grid D.11 (brown feature) should represent the Osorkon House (*HES I,* 131–32, fig. 58; *II,* Plan 6; pls. 35.b; 37.b; 54.b). As noted, stratigraphic relationships in Section CD now become quite tricky, owing in part to Reisner's own statement in his daily diary, "Progress of the Excavations"—Summit Strip 7 (*HES I,* 403).

> Aug. 19–25. Removing debris under the Atrium House and alongside it on the north. In d, or the north, there were no walls between the Atrium walls and rock. On the rock there was disturbed yellow debris. Under the Atrium House there was black debris, containing fragments of walls of a Seleucidan house, and an older house whose walls broke through the Ahab courtyard strata leaving blocks of these strata undisturbed. The floor of this house (Osorkon House) was 179 cm. below the floor of the Preherodian house, and on about the same level as the courtyard floor. In room 742, northern part, in floor debris of a block of courtyard strata, ivory handle in the form of a uraeus and fragments of an alabaster jar with the names of Osorkon II of Egypt.

Like the drawing of Section CD, this passage spawns a number of questions. If no walls survived between the Roman-period Atrium House and the bedrock, why should one see any architectural remains from the Preherodian, Babylonian, or Israelite periods on this drawing? (Note that Fisher did leave major gaps in his depiction of deposits farther north in fig. 33, Grids D.9–10.) Reisner's statement seems to relate only to areas lying north of the Ostraca House. For this area, he follows his opening observation by noting that walls of "Seleucidan" and pre-Seleucid houses appeared *beneath* those of the Atrium House. These two phases likely relate to his Hellenistic

I/II levels mentioned above and not to the unexplained features shown in light green. But he clearly desired to see in the earlier phase the pre-Hellenistic Osorkon House. He attributes to the Osorkon House the only floor level found among these deposits. Thus the floors and walls of this structure should appear on Section CD. Finally, here Reisner assigns to Room 742 the Osorkon Jar fragments, when they likely derived from Room 741 (see above). To help sort out this somewhat murky situation, one should begin by noting that

> the Osorkon House was built at the northern end of the Ostraca House, over the site of the assumed northern rooms of that house [see figs. 26, 30, above]. The superstructure of the Ostraca House had been destroyed except for a few fragments; and the southern wall of the Osorkon House was built in part over the *foundations* of the north wall of Rooms 406, 407, and 408. (*HES I,* 131; italics added)

Osorkon central Room 741 had an entrance from the north (Ch. 1, fig. 23; Ch. 2, fig. 30). It showed no interior crosswall, but extended undivided across the full 7 m to its southern wall. This wall never appears in the plans but reportedly rested on top of the northernmost surviving wall of the Ostraca House (= northern wall of Rooms 406, 407, 408). Reisner understood 741 as an "entrance hall." At least in one narrative, he assigned the Osorkon Jar to this hall, near its doorway. He understood the solid mass of masonry, positioned just west of this doorway and mostly set against the northern face of Room 742, to represent "the foundation of a pedestal of some sort" (*HES I,* 132). Although he also assumed a subdivision of Room 740 on the east, similar to the situation in 742–743 on the west, no evidence remained to confirm this interpretation.

Since Section CD passed through Room 741, which had no interior crosswall, the smaller wall above Ostraca House Room 408N must either (1) represent the southern, less substantial wall of the Osorkon building or (2) remain unidentified in fig. 33 (as do, in my judgment, the other light green walls to the south in Grid D.12). The width of the House's northernmost wall (ca. 2.1 m), as presented on the published plan, matches the width in Section CD of a lower, wide, unidentified wall (sitting directly above the first "o" in "Osorkon" and floating 1+ m above bedrock). Also, this wall rests just inside the northern edge of excavation Grid D.11, a position that agrees with its placement in Plan 6. At the point where Section CD ran through the Osorkon and Ostraca houses, the northern face of this Osorkon wall lay between 7–8 m beyond the northern edge of Ostraca Room 408 (which, unlike the proposed restoration of some Ostraca House rooms, survived in the archaeological record). Thus, both the plotting and the scale of these features on Plans 5–6 and Section CD prove congruent. The evidence recommends that at least one of the two apparent phases of wide walls in Section CD, Grid D.11, must represent the northern wall of the Osorkon House.

Two principal problems, however, arise with this understanding. The first difficulty lies in Reisner's statement that the Osorkon walls "*carried down to rock*. The masonry was of large stones not fitted by dressing, but well built, with smaller stones in the crevices. The thickness of the walls varied from 85 cm. to 1 m." (*HES I,* 132; italics added). According to Fisher's drawing, the basal courses of this wall obviously did not rest on bedrock but, as noted, remained at least 1 m above the rock. Secondly, Fisher's shading of the lower three courses of this wall (framed in red in my figure) indicates his own understanding that at least two building phases existed here. And judging from the hatching pattern (left oblique) on these courses, Reisner and Fisher assigned this initial phase to Jeroboam II. One must, therefore, assume either that Phase 1—the bottom half of the northerly, wide wall in D.11—should extend down to bedrock, since it is the only wall that is of proper distance from Ostraca Room 408 to represent the Osorkon House, or that another sizeable but poorly preserved structure existed in this area, probably belonging to a time between the decommissioning of the Ostraca House and the construction of the Osorkon House.

Another observable, intervening, and unlabeled wide wall (depicted above the second "o" of "Osorkon" [fig. 33, orange level])—which Fisher

did draw on the rock surface—lies too close to the Ostraca House to represent the northern perimeter of the Osorkon House. It must belong to yet another building phase, whose existence relative to the construction of the Ostraca House remains difficult to determine. Kaufman (1966: 105–6) perceptively recognized the probable existence of an earlier structure beneath the ostraca building. Although Franklin (2004: 196) did not cite Kaufman, she also briefly commented on this likelihood by calling the erstwhile structure "a lost monumental building." Franklin proposed to date this mostly lost building to the time of King Omri and to relate it stratigraphically to the long north–south drain seen in Grids E.6–11 on *HES II,* Plan 5. She then extrapolated "that there were other Building Period I monumental buildings west of the palace but at a slightly lower elevation."

Whether or not the building and drain functioned together remains uncertain and open to question. But it is very likely that an unaccounted-for building once occupied the space eventually (or previously) taken by what is now known as the Ostraca House. At least, the building *partially* occupied this space, for, in my judgment, the wide wall set on bedrock likely relates stratigraphically (i.e., temporally) to the wide feature (in blue) that also rests on the rock surface *between* Ostraca House Rooms 408 and 415 in Section CD, Grid D.12. If I am correct, this early complex preceded that of the Ostraca House, notwithstanding the inexplicable fact that in the drawing the associated floor and fills (orange level) immediately north of Ostraca House Room 408 were poured against 408's pre-existing wall. But if the orange wall in D.11 and the wide blue wall in D.12 (plus the obviously wide Robber Trench at the southern end of 409) do, in fact, belong together, then this structure clearly postdated the thick, level deposit of clean yellow masons' debris likely created by Omri's extensive quarrying activities in the area and his shaping of the rock crest just west of the main palace area. But the structure would have pre-dated the series of walls (dark green) above and around it.

Kaufman (1966: 105) slightly narrowed the date for this phase by suggesting that it existed "between the time of the completion of the casemate wall [traditionally assigned to Ahab] and the time of the building of the Ostraca House complex . . . ." If his suggestion is correct, a possible date for this structure would fall somewhere in the mid-to-late ninth or very early eighth century BCE, probably sometime during the rule of Jehu (841–814/13 BCE) or Jehoahaz (814/13–798), his son, or perhaps even during the early years of Jehoash (798–782/81) prior to the start of his coregency with Jeroboam II in 793/92 BCE. Kaufman also concluded that, subsequent to this stratum, "the builders of the Ostraca House cleared the entire area down to the masons' debris in order to lay their foundations on solid ground." If, in fact, Fisher intended the darker green walls in figure 33 to represent Ostraca House architecture, then they reflect a second phase of building activity, even though the northern walls of Ostraca Rooms 408 and 409 also clearly penetrate the masons' level and rest directly on bedrock. The wall dividing Room 408 from Room 415, however, used the wider, solid base of a partially surviving wall from a previous structure, which had already cut through the clean yellow masons' deposit.

Based on this reading of Section CD, then, the depositional history presented in Grids D.11–12 potentially reflects at least five (or more) independent phases of building on the lower rock west of the main palace area. The sequence might appear as follows:

1) the level of hard-packed, clean yellow masons' debris, which itself might easily have become a kind of early (perhaps Omride) piazza or courtyard;

2) the mysterious monumental structure represented by the wide (blue) wall between 408–415 plus its robbed counterpart in the southern end of 409 (which I have labeled "RT"), and possibly also the wide orange wall to the north. This structure could have appeared anytime between the rule of Omri and the construction of the Ostraca House;

3) the Ostraca House, smothered in a subsequent phase by deep fills;

4) another large building occupying at least the area to the north, represented by the lower three courses of blocks floating above bedrock just inside Grid D.11;

5) the wall resting on top of this floating base—the northern wall of Entrance Hall 741 in the Osorkon House. At least a partially-preserved floor level extends southward to a slightly narrower wall built directly on top of the northernmost surviving wall of Ostraca House 408, as Reisner indicated in his published narrative.

This stratigraphic sequence raises a tantalizing question: since most of the Hebrew ostraca derive from disparate deposits of subfloor fill, and not from occupational debris lying on living surfaces as Reisner suggested, could they have belonged originally to an early building situated beneath the purported Ostraca House? With the dismantling of that structure, perhaps to gather stones for the Ostraca House's construction, the inscriptions—which by now would no longer serve a meaningful purpose—became part of the churned-up soil used as the makeup beneath the floors of the new building, a building that has erroneously become known as the "Ostraca House."

Whether or not this explanation could also apply to the large percentage of ostraca found much farther north in the 700-series of rooms remains impossible to say. But this scenario would add grist to Kaufman's (1966: 104) suspicion that the building that has long carried the name "Ostraca House" represents a complete misnomer, an accident of the archaeological record. If, on palaeographic grounds, the ostraca collection ties the earliest building to the early-to-mid eighth century BCE, then the architectural elements commonly accepted as the "Ostraca House" must themselves date to a later period, one probably near the fall of Samaria to the Assyrians or later still. Since, from August 1–5 in the 1910 Season, Reisner (*HES I,* 400–401) concluded that the debris above that level dated to the "early Post-Israelite period," the Osorkon House must also post-date "Israelite Samaria" (compare Kaufman 1966: 110).

Section CD incorporates the flat face of an apparent, longitudinal wall extending south from both phases of the wide wall in D.11-north, a wall that I have suggested relates in some way to the Osorkon House. The inclusion of these stippled stones further exemplifies the strange drawing method seen before in Fisher's hand. These blocks (or stones) possibly represent the western face of the wall that separated 741 from 740 and lay just over a meter east of the actual section line. The upper half of these stones may relate to the unidentified top phase of the wide wall and, if so, would belong to an unnamed longitudinal wall constructed above and on the same line as the Osorkon House wall between 741 and 742. Judging from the dotted versus solid lines that represent it, the floor itself appears to have suffered destruction throughout most of 741—where it runs at an elevation of ca. 434.10 m (based on Reisner's datum line at 430 m). This elevation seems to concur with the readings taken on the tops of preserved walls of the Osorkon House (see *HES I,* fig. 58: 434.22 m on the western wall of Room 742, and 434.88 m on the northern wall of Room 741). The floor which the publications associate with the famous Osorkon Vase would have run somewhere near this elevation. (As noted above, however, the vase appears to have come from foundation fill beneath the floor and wall, not from on the purported floor itself.)

8 See also *HES II,* pls. 34.b–c, 36.a; for the southern part of the Ostraca House, in the area of Corridor 424, see pl. 53.a. Other relevant photographs appear in pls. 33.e (Room 417 floor), 33.f and 34.d (space labeled 419), 33.d (409E/424 = northeastern corner of Corridor 409 and Room 405 [location uncertain]). Regarding photograph pl. 33.d, note *HES I,* 115–16, for the claim that workers found a pottery cache lying on the original floor of the Ostraca House. But, judging from this evidence, the floor itself seems in a poor state of preservation. Moreover, the pottery itself appears to lie a bit lower than the apparent floor level and at least to rest in the floor makeup, or perhaps even partially under the eastern wall of Corridor 409 (both of which contexts would relate to the *terminus post quem,* or construction date, of this room, not directly to its range of occupation—i.e., contexts that resemble that of the Osorkon Vase). Still, these bowls prove instructive since they represent the vessel type that contained the ostraca.

We come, finally, to the elements on Section CD that depict the Ostraca House (Grid D.11.south–Grid 12; see figs. 33 [dark green level] and 41–42; also *HES II,* Plan 5).[8] In this portion of the published drawing, the section line ran just barely inside the eastern edge of Rooms 408 and 415. Once again, Fisher faintly sketched in some of the stones from the eastern wall of Room 415, which lay off but very close to the plotted course of the section. But he extended a few of these background stones into Entrance Hall 409, even though the eastern wall of that area lay up to 6.75 m east of the section line. In any event, here the section clearly seems to depict more phases of activity than Reisner reported, as explained above. In Subsidiary Section AB (see below; fig. 41), similarly enigmatic stones are not lightly sketched but are clearly drawn, this time apparently along the northern wall of Room 415.

One observes immediately that starting in Grid 10 the bedrock appears artificially worked (albeit less extensively so than on the northern side of the compound, in Grids 4–6). From that point southward, it begins a gradual descent in elevation. This declining rock underlies both the Osorkon House (whose remains begin in D.11-north) and the Ostraca House (whose *surviving* remains start in D.11-south; as explained earlier, Reisner's reconstruction of the original building extended it farther to the north in Grid 11). North of this worked rock and the wide wall situated just inside D.11, the Preherodian fills and architectural fragments apparently cut down to bedrock, thereby destroying all earlier remains, since Grids D.9–10 show a relatively blank slate north of the Osorkon and Ostraca houses. In this sense, the section below the northern half of the Atrium House's peristylium agrees with Reisner's statement (above) that he found no walls between the Atrium and bedrock. For not until Grid D.9, when one reaches several large but floating features belonging to Preherodian chambers 812 and 822 separated by street 814, does the drawing include a few architectural fragments (but no local stratigraphy). Still, this lacuna in the section obviously does not represent a vacuum; it had to incorporate multiple layers, whether naturally or artificially deposited, and Fisher should have represented them in his drawing. But, as always for this expedition, the methodological and hermeneutical focus lay primarily on architecture.

Fortunately, the deep cutting by the Romans did not extend to the southern half of the peristylium and points south, where remains from multiple phases appeared beneath the Herodian stratum. In Excavation Grids 11–12, the area of the surviving portion of the Ostraca House, a nearly half-meter-thick deposit covered the artificially worked rock. Reisner described this matrix as clean yellow construction debris. (One also sees it represented in the lateral Subsidiary Section AB, described below.) The earliest series of walls in D.11–12 (assigned to the Ostraca House but perhaps stemming from an earlier building) cut through this deposit of clean yellow mason's debris and used the bedrock itself as a firm foundation (see 408, 415, 409). These walls, then, are stratigraphically later than that debris, which itself must reflect some earlier activity.

Note that Entrance Hall 409 appears, as expected, somewhat wider than the two storerooms (418, 415) to its immediate north (recall my discussion of fig. 26 in Ch. 2). Another wall fragment floats in the upper-right (southern) portion of the debris that filled 409; this feature likely represents a remnant of some unidentified phase or part of the Osorkon House (as noted above). Its base rests precisely at the same elevation as the smaller, "unidentified" wall that sits directly on the northern wall of Ostraca House Room 408, the southernmost point at which Reisner reported finding clear remains of the Osorkon House (*HES I,* 131). The restored elements in figure 26, however, clearly indicate his belief that the Ostraca House originally extended roughly 6.5 m to the north of 408. If he was correct, this portion of the Ostraca House would have run northward through much of Grid D.11, almost to the lower-right corner of the wide Osorkon Wall seen in Section CD (above the first "o" of "Osorkon"), and beneath both Hellenistic rooms, 711–713. But the yellow construction debris and all traces of the Ostraca House itself seem to have disappeared from this portion of D.11, possibly during the preparations for and construction of yet another building that appears, stratigraphically, to have existed *between* the final use of the Ostraca House and the construction of the Osorkon House (orange

level). At face value, the drawing shows that ancient workers poured the leveling fill for this structure against the northern face of Room 408, which it must, therefore, postdate. Fisher's drawing even suggests that a clear floor level ran across the top of this fill deposit. In my discussion above, however, I held open the possibility that this presentation of floor and fill is somehow inaccurate and that the wide orange wall relates to the wide blue wall and wide robber trench farther south in Grid D.12. In this case, the architectural remains would point to a building phase *prior to* that of the Ostraca House. But if Fisher's rendering of the orange floor and sub-floor make-up appear precisely as they should, then there exists here a previously unidentified *post-Ostraca House phase of construction* that must now join the overall sequence of activities in this area. Above the apparent floor of the orange level lay the fill, poorly-preserved floor, and northern wall of Osorkon House Room 741 (see *HES II,* Plan 6).

The puzzling question, then, centers on how to relate the larger architectural elements resting directly on bedrock—i.e., the wide, orange wall and fill north of Ostraca Room 408, the wide blue wall below the southern edge of 408, and the wide robber trench at the southern end of Entrance Hall 409. As seen in figure 26 (Ch. 2), during the later construction of the so-called Greek Fort Wall whatever feature had once occupied the last area ultimately suffered destruction, along with the entire southwestern portion of the building. But, in my judgment, it seems reasonable at this point to pose a stratigraphic correlation between the two wide walls and robber trench and to leave the orange fill depicted in Grid D.11 as an enigma.

In any event, the architecture that divides Ostraca House Room 408 from Room 415 clearly shows two to three phases of construction, as previously noted. Using Fisher's own scales as a guide, the earliest phase (the wider bottom portion) measures more than twice as wide as the 408–415 dividing wall shown in *HES II,* Plan 5. Applying the scale of the other Ostraca House walls as a guide (north of 408 and between 415–409; also between 410–411–424 in E.13), the middle phase (darker green) appears most consistent with the metrics and construction style of other Ostraca House walls. This observation helps confirm the presence here of various, unnoticed phases of building both prior to and succeeding the establishment of the Ostraca House. (Recall that the wide base wall itself cut through the lowest soil deposit in this area, the "clean yellow mason's debris," thus endorsing this deposit as an earlier phase of activity.)

In the northern portion of Grid D.12, then, one sees as many as five potential phases of depositional history, excluding the Osorkon building: (1) the clean yellow deposit lying directly on the rock; (2) the broad wall between what eventually became Ostraca House Rooms 408–415; (3) the actual Ostraca House wall between these two chambers; (4) a still smaller wall constructed directly on the truncated top of the Ostraca House wall, which likely served during the First Greek period (Reisner's "Preherodian" town); and (5) the more inexplicable remains of a pre- or post-Ostraca House structure to the north (orange level).

A review of the few elevations recorded on the Ostraca House plan (Ch. 2, fig. 26) informs the proposed earlier construction phases. In Section CD, Fisher drew the floor level for the Osorkon House at ca. 434.88 m (cf. *HES I,* 59), while the floor for the Ostraca House appears at ca. 434 m. Table 3 provides a comparison of the elevations taken at various locations across these features. Generally speaking, the "floor levels" recorded inside the Ostraca House lie at a lower elevation than the suggestive line that Fisher drew to indicate the floor on Section CD. The line on CD (at 434.88; cf. *HES I,* 59, for the recorded level of the Osorkon House floors) agrees only with the reading taken inside the doorway of 741 in the Osorkon House. But, as Table 3 demonstrates, many of the Osorkon walls, which were considered stratigraphically later than those of the Ostraca House, did not survive to that height. Moreover, the so-called "later additions" (of broader walls) generally sit lower than the Osorkon House but higher than the Ostraca House, which situation suggests the presence of three, not two, occupational phases. In the Ostraca House itself, Reisner acknowledged the broad wall between 401 and 418 (fig. 26) as a later addition. But most of the rest of the so-called "later additions" lay *below* the putative floor level of the Ostraca House.

Table 3 Relative elevations of three periods of building.

General Location	Room	Elevation
"Later Additions" (not hatched)	Room 401 (floor level?)	433.14
	Room 407	433.85 and 433.14
	Rock inside 407 (apparently on a surface)	432.77
	Room 408	433.21
	Rock inside 408	432.44
	Room 410	434.16
	Room 415	434.12 and 434.1
	Rock inside 415	432.19
	Broad wall between 401 and 418	434.58 (stippled "reconstruction" on plan)
	Space 416 (court level btw. Ostraca House and Ahab's Wall)	434.42 (not part of the Ostraca House)
	Rock inside 416 (apparently on a surface)	432.19
Osorkon House (*HES I,* 131, fig. 58)	East Wall of 706 (W of Osorkon House)	434.39
	Room 740 (NE cornerstone)	436.19
	Room 741 (center of doorway)	434.88 (cf. HES I, 59)
	Room 742 (W wall)	434.22
	NE cornerstone on pedestal built against N face of Room 742	434.53
	Point above wall of Ostraca House 406	434.89
Broad walls overlying area occupied by Osorkon House (called "earlier walls")	Corner walls in 741 (ca. 1.40 m in width)	434.02 and 433.65
	Floor level (inside corner formed by above walls)	433.39 and 432.65
	Two corner walls in 740 (offset from those in 741 and slightly narrower, 1.30 m)	433.36
	Floor level (inside corner formed by above walls)	433.45

These observations increase the likelihood that an unidentified building phase preceded that of the Ostraca House, while yet another unrecognized phase occurred between the Ostraca House and the Osorkon House. In any event, the section drawings fail to show a coherent floor level on top of the clean yellow layer. And even if such a floor actually existed at this level, the earliest large building preserved here already cut through these deposits. No recorded layer could possibly represent a consolidated floor level for the structure traditionally known as the "Ostraca House."[9] Unfortunately, none of the published photographic records sheds significant light on these multiple phases or on the quality of preservation of floors inside the main storerooms of the Ostraca House.

1.b. Section CD: Excavation Grids E.13–14 (fig. 35)

South of Entrance Hall 409, across the entire southern half of the Ostraca House, the natural rock dropped more dramatically and, at points, was artificially worked (Section CD, Grids E.13–16). Contrary to the plot of Section CD on *HES II,* Plan 5, the section drawing now shifts 5–6 m eastward, into Rooms 410–411 and the southern Entrance

9 One must also bear in mind that all these deposits lay within the presumed "storerooms" of the Ostraca House's main sector (which apparently failed to yield a single inscription), not in the long corridors to the east (which produced more than 61 percent of the epigraphic corpus).

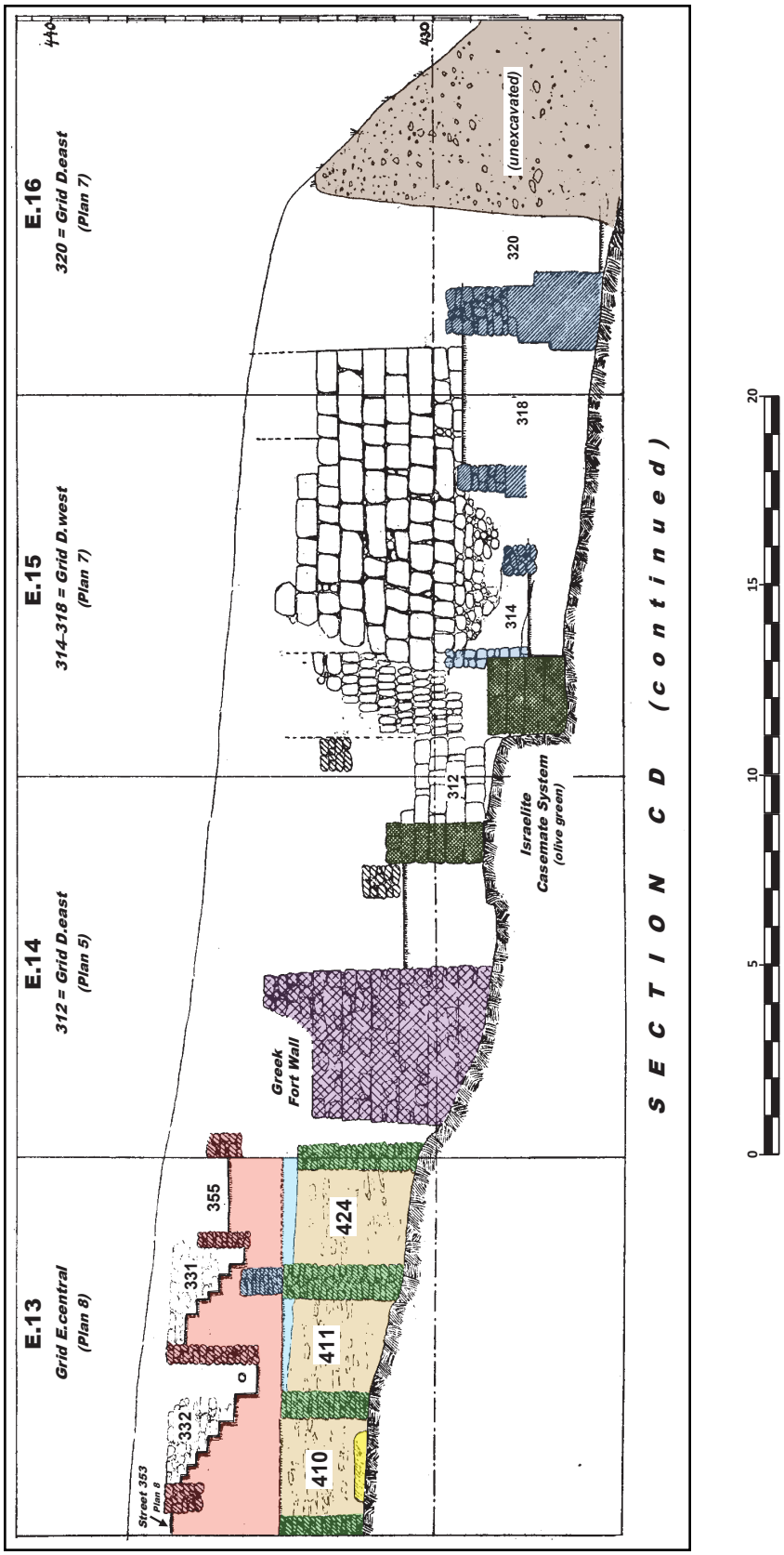


Fig. 35 *Section CD: Grid E (adapted from* HES II, *Plan 4).*

Hall 424 (fig. 36). This move likely resulted from the fact that the western rooms of the Ostraca House, whose area actually lay on the original line of the section, survived only as traceable foundation trenches, not in the form of articulated architecture. And the three southernmost rooms in the original footprint of the Ostraca House (i.e., the unlabeled cubicles south of Entrance Hall 424, in Grids D-E.14) succumbed to the later construction of the imposing Greek Fort Wall. By comparing the location of these rooms on *HES II,* Plan 5, with the southern course of the Greek Fort Wall on Plan 7 (fig. 34), one can easily verify the close proximity of this massive wall that ran through the area immediately south of Hall 424, in Grid E.14. It is not surprising, given the somewhat diagonal path of the Greek wall, that excavators found the southwestern portion of the Ostraca House so poorly preserved while its southeastern portion proved somewhat better protected. (The opposite situation attends the "Preherodian"/early Hellenistic structures constructed later over this very area.)

With the realization that, in Grids E.13–14, Fisher's drawing shifts from the western half of the Ostraca House to its eastern half, orientation to the features displayed on this portion of CD proves quite straightforward. On the drawing, at least three major strata either overlie or surround the Ostraca House. From earliest to latest, we see first the foundations of the old Israelite Casemate system straddling Grids E.14–15 (olive green features). The narrower, inner wall and broader, outer wall enclose Casemate 312 (compare Plan 5). Builders set the wide, outer wall in an artificial angle of worked rock (cf. *AIS I–II*), the shaping of which likely produced some of the clean yellow mason's debris often cited by Reisner. The Ostraca House (see below) came next in this stratigraphic sequence.

Following this period, one sees the massive Greek Fort Wall[10] (shown in purple) and other remains from Reisner's Hellenistic II phase (fig. 35, blue level), which probably relate to the Late Hellenistic period (ca. 167–37 BCE). The Fort Wall lies in the northern side of Grid E.14 very close to the southernmost *surviving* wall of Ostraca House Hall 424. This feature (*HES II,* Plan 6; compare *SS I,* Plan IV), which dates from the second century BCE to the Herodian period (*SS I,* 118–21), suffered destruction or heavy damage by John Hyrcanus (134–104 BCE) and underwent subsequent repairs

10 This impressive new city wall appears with the Osorkon House on Reisner's published plan for the Babylonian period (*HES II,* Plan 6). It likely dates, however, to a significantly later time—the Hellenistic Period (see *SS I,* 119, pl. VII, Section CD). It seems that Reisner consistently referred to this impressive feature as the "Babylonian Wall" and, at one point in his daily journals, concluded that it dated "earlier than Alexander the Great" (*Reisner Diary III,* 305). But he consistently placed the notation "Bab." in quotation marks and continually acknowledged that this dating remained quite uncertain (see *Reisner Diary V,* 497). When excavating the foundation trench of the "Bab." wall along the area of Room 335 (assigned to the Herodian Period on Plan 8), Reisner found "not a scrap" of Greek, Seleucid, or Roman pottery in the trench. Instead, the trench yielded fragments quite similar to the inscribed ostraca pottery (*V,* 532–33). These observations formed the basis of his dating the "Bab." wall later than the Ostraca House and earlier than the Seleucid period. He wrote: "That is, we have found nothing to alter the conclusion that the 'Bab.' wall is between 700 and 300 B.C." (*V,* 534). A short time later, when workers removed both the top stratum of dirty yellow debris and a nondescript, nearly sterile lower level of soil from Room 335 (August 17–18, 1910), they found the same style of Israelite ceramic fragments (*V,* 534–35). Still, Reisner apparently assigned Room 335 to the Herodian period (see Plan 8). By September 16, 1910, after excavating a portion of the western "Bab. wall" (roughly in Grids B.8–9) and finding Greek pottery and a stamped jar handle underneath it, Reisner concluded: "It is becoming more and more probable that my suspicion of the date of this wall is correct and that it is nearer 300 B.C. than 500" (*Reisner Diary VI,* 587).

While Reisner also believed that the construction of the "Bab." wall had incorporated stones from the dismantled Israelite Casemate Wall (*V,* 496), sorting out the stratigraphic relationship between the "Bab." wall and what became the beautifully built round Hellenistic towers at the southwestern corner of the summit proved extremely difficult for Reisner (e.g., see his ongoing journal entries in *Reisner Diary V,* 466, 468, 473–74, 477, 480–82, 484, 486–88, 491, 495–97, et passim). Finally, on Saturday, July 30, 1910, Reisner arrived at his conclusion: "I think we have at last every scrap of information attainable from the rock marks in S5. The plan is clear. There is no doubt that we have a third great addition to the palace of the Israelite kings . . . . This third addition includes the round towers already noted. Temporarily, as a working hypothesis, I have named this third addition that of Jeroboam II" (*Reisner Diary V,* 500–501; Reisner maintained this preliminary interpretation as the official one in *HES I,* 117–19).

Fig. 36 *Relative locations of two portions of Section CD, as plotted (light red) and as drawn (red) (adapted from* HES I, *64, fig. 15, and 114, fig. 42).*

by Gabinius (around the mid-first century BCE; see Ch. 2, n. 6, and Ch. 6, n. 17). On Section CD-Grid E.15, also from the Hellenistic Phase II, Room 314 and the area of Street Lambda (318) lie outside (south of) the route taken by the old Israelite Casemates (compare *HES I,* 154, fig. 69). Although Fisher had already shifted his drawing *east* of its plotted course (Plan 7) to pick up the better-preserved Ostraca House rooms, here the drawing moves on average 4 m *west* of its recorded plot to reach the center of 314–318. Once again, without giving notice, he significantly offset his drawing, this time in the opposite direction, in order to incorporate better-preserved architecture (figs. 32, 36). South of 318, this rather serpentine drawing returns eastward to the actual section line and to the well-preserved Room 320.

Directly above the Ostraca House remains from the Herodian period (red level) lie Room 355, Staircases 331 and 332, and (at the very northern edge of E.13) the level of lateral Street 353 (which relates to the street level and drain drawn at the very southern edge of D.12). Once again, the drawing shifts from its plotted line—this time between 5.5 and 6.25 m eastward in order to catch these features. Thus the two portions of Street 353 depicted in D.12 and E.13 are related but not physically connected as in *HES II,* Plan 4 (compare figs. 31 and 33 + 35 above). So, with no guidance from Fisher or Reisner, those who study Section CD must decipher a westward shift to reach the Hellenistic structures and an eastward shift to include the Roman ones.

The portrayal of the Ostraca House itself takes in Rooms 410–411 and southern Entrance Hall 424 (Plan 5, Grid E.13; green level). As explained earlier, these features also lay east of the plotted section line in the same horizontal plane as the Herodian rooms and stairways just mentioned (compare Grid E.13 in *HES II,* Plans 5 and 8). And, once again, Fisher etched in background walls that ran parallel to the section, not across it (e.g., the eastern walls of Staircases 320–321). As I shall show, he also applied this drawing strategy to the Ostraca House rooms, whose walls generally appear more uniform in Grid E.13 than in D.11–12. Thus the remains in E.13 do not exhibit as many noticeable phases as in D.11–12 (except, perhaps, for those on the northern side of Entrance Hall 424). No clearly distinguishable earlier building phase comparable to the wide wall between 408 and 415 in D.12 appears here. Thus, whatever building might have existed in this area prior to the construction of the Ostraca House does not seem to have extended into the E-Grids of Section CD. Moreover, the so-called clean yellow construction debris did not run the entire way across this area; rather, it stopped in Room 410. But clearly these features and layers relate not to the establishment of the Ostraca House but to earlier phases that the Ostraca House walls either cut through or used secondarily as bases of support. And, as suggested earlier, the massive dumps of dirty yellow fill likely postdate the erection and possibly the use of walls in 410–411–424.

No possible floor level prior to the Herodian peristylium/Atrium House existed in this portion of Section CD. When the drawing makes its unannounced move from Grid D to Grid E, it incorporates some background elements (remnants of deposits and features that lay off the actual section line) but certainly fails to corroborate the presence of the purported courtyard of Ahab or a 10–40 cm-thick floor inside the Ostraca House. As noted above, the thick deposit of clean yellow construction debris appears only in Room 410. In contrast, Room 411 and Hall 424 show only deep deposits of dirty fill between their walls. Fisher stippled in the suggested level of the erstwhile Ostraca House floor, but whatever that level represents is clearly missing from 411 and 424. The architect's record from Grid E.13 on Plans 4 and 8 shows that the heavy fills beneath a series of Herodian staircases destroyed earlier floors and that the underlying Hellenistic fill (light blue level) itself impinged on the dirty yellow debris inside 411 and 424. Thus no floor level prior to the Herodian period has survived near the southern half of the Ostraca House (certainly at least from 410 south, and probably not in 410 itself).

Beyond the southern wall of Entrance Hall 424 (and lying directly on the transition from Grid E.13 to E.14 in Plans 4–5), the rock dips even more dramatically, and all traces of the Ostraca House were lost in the construction of the later Greek Fort Wall. This descending stretch of rock underlay additional Ostraca House rooms projected by Reisner

to have extended southward from Corridor 424. (These unlabeled rooms appear on fig. 26 and *HES II,* Plan 5, but not on the Cockayne plan.) Beyond these restored chambers, a stretch of open space (301) ran southward from the Ostraca House until it reached the southern Casemate System. (See the transition from Grids E.14–15 on CD.) Note that at this point the published section has returned to its plotted course in the western part of the Ostraca House, for the casemate that it passes through is 312 (and not 313, which actually lay directly south of Ostraca House Rooms 410–411).[11] Owing to the declining rock across this entire area, the walls (and subsequent fills) of the Ostraca House now necessarily rose to greater heights. (For example, the partition wall between 411 and 424 survived to a height of 4 m, nearly twice as high as the tallest wall in D.11–12.)

Unlike areas farther north, in which Reisner claimed to have encountered no architectural subterfuge between the Roman Atrium level and bedrock, the situation in E.13 presents at least four or five possible phases: (1) clean yellow mason's debris, at least in 410; (2) the purported Ostraca House walls; (3) copious quantities of dirty yellow fill; (4) the subfloor fill, floor level, and wall for a post-Ostraca House phase (Hellenistic[?] light blue level); and (5) further substantial fills supporting the walls and stairways of the Herodian period Atrium House, where Fisher once again, as in the Ostraca House level, etched in background walls that lay farther off this already errant section line. While the temptation may exist to associate Phase 4 above with the Osorkon stratum, caution must reign since Reisner clearly and repeatedly stated that the southern wall of that structure rested on the northern wall of the ostraca building. That is, the Osorkon remains did not approach Ostraca House Rooms 410–411, much less Entrance Hall 424 to their south. Item 4, then, must relate either to the Hellenistic period or to intervening phases that Reisner left unidentified and undiscussed but which I have addressed in my earlier analysis of Section CD. (Perhaps this phase belongs with the "floating" wall fragment seen at the southern end of 409 in Grid D.12 [fig. 33] or, more probably, with the light green walls shown there.)

In essence, then, the stratigraphic record south and west of Room 410 proves somewhat disappointing with regard to the Iron Age deposits. While there may have existed a swatch of flooring that overrode Room 410 (Fisher's line appearing at about the correct elevation, 434 m), the area yielded only wall fragments buried in heavy deposits of imported fill. Even if a portion of floor did manage to survive here, no record assigns any of the ostraca to this particular room. The wall stub built atop the Ostraca House wall separating 411 from 424 in Grid E.13 began at this very level (434 m) and used the pre-existing wall beneath it as a base. This smaller wall seems to cohere with the problematic light green walls of figure 33 or possibly the "Preherodian" features that elsewhere stood atop Ostraca House walls (e.g., between Rooms 408–415 and 415–409 in Grid D.12, although Fisher did not draw any wall from E.13 on his Plan 7). In any event, this wall was also eventually smothered by the deep Herodian fills meant to shore up Staircase 331 and related features. Preparation for these heavy Hellenistic–Roman fills apparently cleared away whatever floors might have existed across this general area. Beyond these observations, one cannot draw further reliable conclusions from published Section CD, E.13–14.

2. Principal Lateral Section GH (figs. 37–40)

In addition to longitudinal Section CD, two parallel and closely-spaced latitudinal sections bear directly on a study of the Ostraca House remains. The first, Principal Section GH, extended through Grids A-L.12 (*HES II,* Plan 5), although the published

11 At bedrock, 312 should represent the open chamber of a casemate. Once again, the large, horizontally laid blocks that carry the label "312" probably represent the crosswall separating Casemate Chambers 312 and 313—i.e., these blocks lie well (more than 2.5 m) to the east of the designated section line and, technically speaking, should not be conflated with deposits on that line. The smaller, more vertically laid blocks shown immediately east of 312 and directly above the thick, outer wall of the Casemate System must (judging from the plans) belong to later buildings (of the Herodian or Severan periods).

drawing presented only B-K.12 (Plan 4). The purview of this section extended eastward from the Greek Fort Wall in the west (Summit Strip 5, B.12) through the area occupied by the Ostraca House (Summit Strip 4, D-E-F.12; fig. 37). The cut intersected Section CD in the southeastern quadrant of Ostraca House Room 408. The eastern half of the GH drawing (fig. 38) included the rock scarp (G.12) and raised, central summit area that supported the Israelite palace (H-J.12), as well as various architectural phases ranging from the late Hellenistic through the late Roman periods (F-K.12). As I shall note below, Fisher once again incorporated into his drawing various features that lay well off the plotted section line. A second, auxiliary cut, labeled AB, ran parallel to and ca. 2.5 m south of this primary section. Rather than including this subsidiary section in the report's presentation of major sections (which already included a drawing labeled "AB"), Reisner published it at two separate points in the course of his narrative (*HES I,* 63, fig. 14; 115, fig. 43; see my figs. 41–42, below). For this reason, I shall refer to this drawing as Subsidiary Section AB and shall describe it in detail following my survey of Principal Section GH.

Principal Section GH once again dramatically illustrates the natural and artificial shaping of the bedrock across the summit of Samaria. Moving from west to east in fig. 37, an artificially stepped rock surface appears in B-C.12, beneath the Greek Fort Wall and the remains of both the outer and inner walls of the Israelite Casemate system (C.12), in casemate chamber 328 (cf. Plan 5).[12] The bedrock then rises noticeably as it approaches the Ostraca House and swells even more beneath the area occupied by that structure (see *AIS II,* 169). As a result, Room 408 lay near the edge of good, relatively level rock. Farther east (fig. 38), Grid G.12 shows a slight depression in the rock and

12 On the section drawing, Fisher labeled the space immediately west of this chamber as 321; Plan 5, however, identifies it as 329. In any event, this area represents later activities and is not germane to the present discussion.

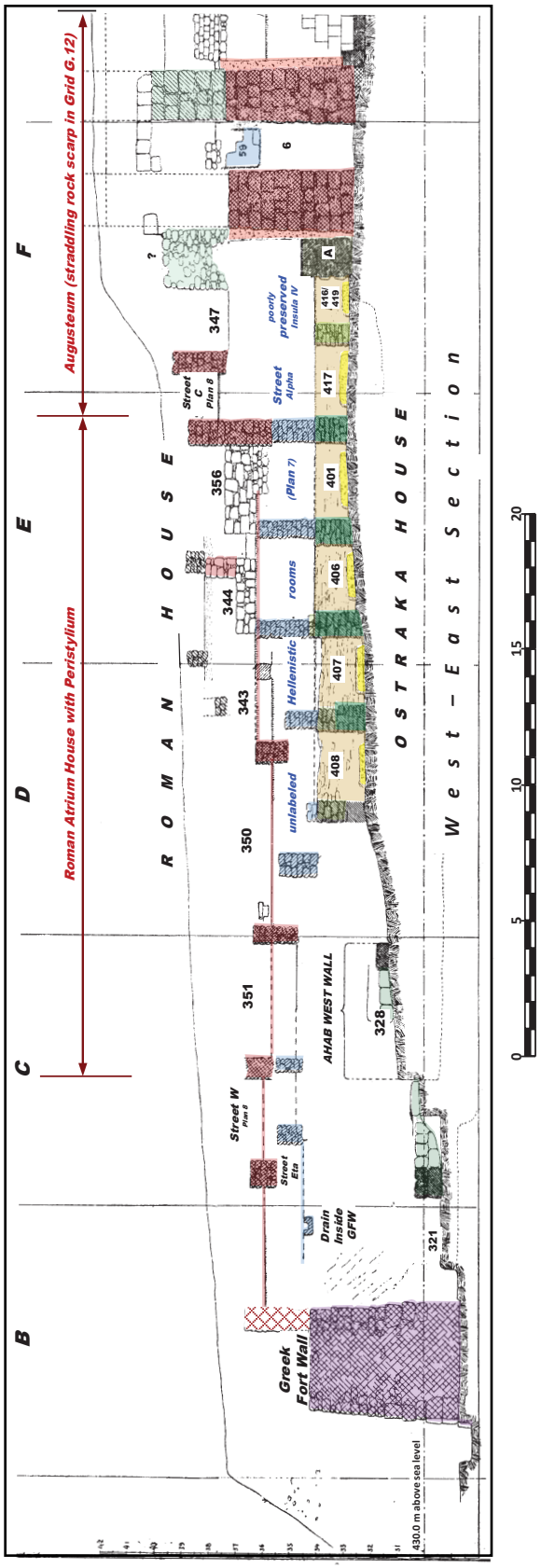


Fig. 37 *Section GH: West—along Horizontal Grid 12 (adapted from* HES II, *Plan 4).*

an artificially created step up existing apparently beneath "Pavement 13" on Plan 5 (cf. Franklin 2003: 5, fig. 1; 2004: 193, fig. 4). Then, in the eastern half of G.12, a substantial rock scarp rises several meters to the crest of the summit platform, thereby creating a kind of quarterdeck for the royal precinct. From this height, the rock continues eastward at a fairly even elevation beneath the Israelite royal palace. East of the palace area, below a large open-air courtyard, the rock included occasional depressions and cuttings not portrayed on Fisher's drawing (see *SS I*, pl. II; also the north–south section in *AIS I*, 74, fig. 14). In general, however, these pre-Omride rock installations and features decrease in number as one moves across the elevated summit east of the palace area (see Franklin 2004: 191, fig. 1; 193, fig. 4).

The eastern half of Section GH (Grids F-K.12; figs. 38, 39b) not only shows a number of important features but also demonstrates the continuation of Fisher's puzzling drawing scheme. The basal courses of walls in Omri's Palace Room 1 (ca. 3+ m in width) appear in H-K.12 (shown in olive green). The even wider, westernmost wall of the Palace, which rested just at the top of the rock crest and formed the western boundary of Palace Room 10, also seems represented on this drawing. (Because of the tentative identification of these blocks, I have shaded them in light green.) Beneath these rooms, especially Palace Room 1, Section GH presents a rock-hewn, rectilinear chamber labeled "Rock 'Tomb' 7." Franklin understood this subterranean hollow as a second royal tomb, and she labeled the chamber "Tomb B"

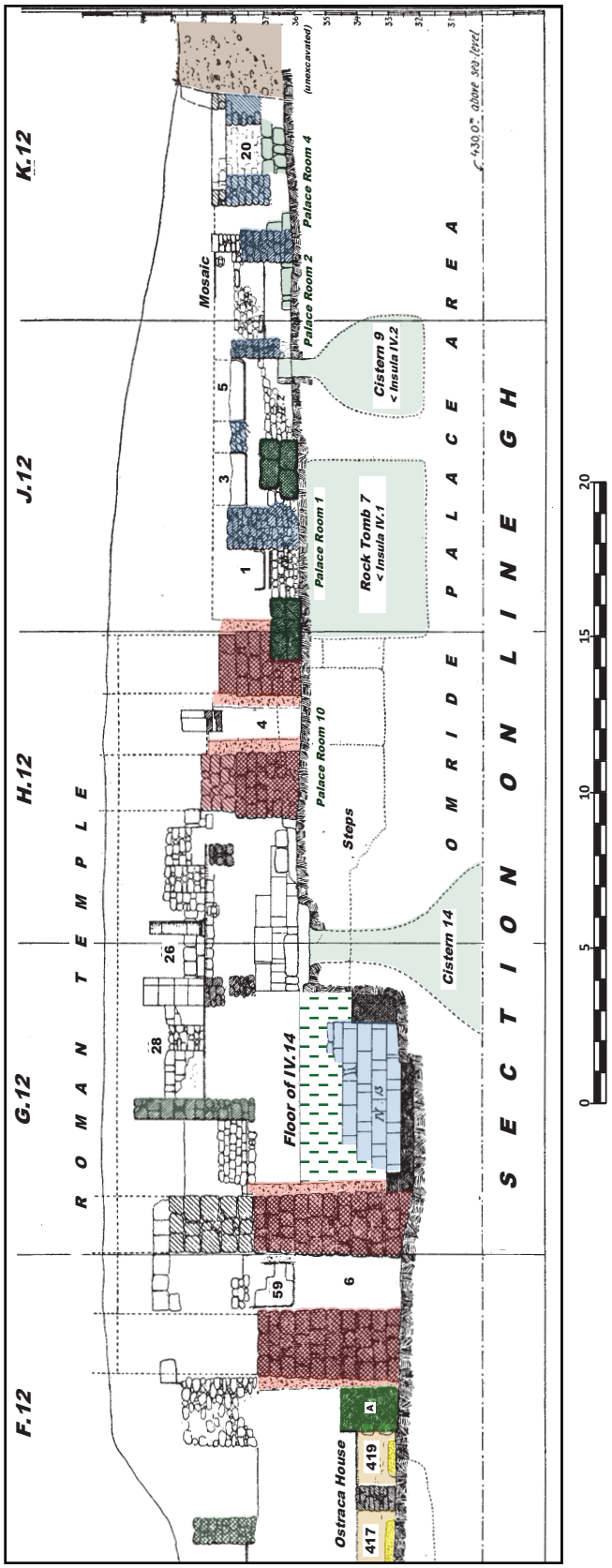


Fig. 38 *Section GH: East—along Horizontal Grid 12 (adapted from* HES II, *Plan 4).*

(Franklin 2003: 5–7; fig. 1). She related this feature to a nearly identical one situated to the north in the rock base beneath Court 7 (see *HES II,* Plan 5). That underground compartment consisted of a main chamber (Grids J.10–11), a small tunnel or hallway connecting it to an antechamber to the west, and a continuation of the tunnel westward into Room 12 (Grids H.10–11). But a second complex of similar design does not appear on Plan 5 beneath Palace Rooms 1 and 10. And Reisner nowhere describes a second tomb complex in his narrative report. It seems that Franklin's proposal of a second tomb resulted from her acceptance of Fisher's Section GH at face value. Consequently, she posited the existence of two royal tomb sites, the real one beneath Palace Court 7 and a phantom one beneath Palace Rooms 1 and 10. Whether or not the space below Court 7 served as one of the royal burial chambers, the feature presented beneath Room 1 on Section GH appears in stippled outline because its southern edge actually lay 8+ m north of the line taken by that drawing. Once again, Fisher allowed a feature that sat well off the course of the section cut to have an echo on the final drawing (see Ussishkin's strong critique of Franklin's proposal in 2007: 62–65; cf. also Ch. 2, n. 9 and n. 7, above). In light of Fisher's rather unorthodox drawing habits, as attested elsewhere in this study, it seems more reasonable to assume that he pulled Rock "Tomb" 7 into Section GH than to believe that he knowingly omitted it from *HES II,* Plan 5.

Yet another graphic conflation of Hellenistic walls and rooms representing features in Insula IV, Chambers 1–2, lay above and impinged on this Palace Room 1. But, judging from *HES II,* Plan 7, these rooms belong in Grids J-K.13, roughly 15 m south of Israelite Room 1. Above these Hellenistic features, the drawing presents Rooms 1, 3, 5, and Circular Stone-lined Installation 20 from the Severan Colony (Plan 9, J-K.12). Once again, a collation of Fisher's published Plan 9 (J.12) shows that the three rooms should, in fact, appear along the axis of Section GH. The center of Circular Installation 20, however, lay at least 3 m north of the plotted section line. The installation's appearance on GH, therefore, remains a curious feature of that drawing. A large, bottle-shaped Cistern 14 appears at the G-H.12 transition, just before the rock drops dramatically to the lower level. Immediately below the scarp, in G.12 on the summit's lower deck, lay the remains of Pavement 13 (west of and below the palace area; see Plan 5), beneath walls and floor levels from Hellenistic Insula IV.13–14 (see upper-right quadrant of G.12 on Plan 7).

Immediately west of Omri's Palace Room 1 (Grid H)—and, in fact, partially overlying it—are two massive walls with wide foundation trenches cut down to bedrock. These features represent the eastern enclosure of the cella situated next to Aisle 4 in Herod's Augusteum Temple (figs. 38, 39a–b, with arrow, red level). Aisle 4 comprises one of two longitudinal side chambers that flanked the temple's main room/cella; its deeply-set, western counterpart, Aisle 6, appears in Grid F.12. Herod's builders once again penetrated all the way to bedrock to obtain a foundation for these large walls, even though the rock surface in this area lay well below the level of rock reached in Aisle 4 on the eastern side of the complex. (Foundation trenches appear in light red.) Thus the eastern edge of the temple came down inside Israelite Palace Room 10, and the greater temple complex actually straddled the now-smothered rock scarp seen in G.12. West of the old scarp, the temple fills overrode a preserved floor in Hellenistic Insula IV, Room 14. But the western walls of the later Augusteum (Aisle 6) destroyed any continuation of Insula IV in that direction (see *HES II,* Plan 7). West of Aisle 6, the upper portion of Section GH tracks through Herodian Room 347; longitudinal Street C; Rooms 356, 344, 343, 350, 351; and Street W (red level; for proper orientation, I have added these designations to figs. 37 and 40; compare *HES II,* Plan 8.) The drawing presents only a few wall fragments from the Severan Colony above this level.

Various "Preherodian" and "Unidentified" wall fragments appear between the massive outer walls of the temple and in the area of its cella. That Reisner recognized at least some of these truncated features as Preherodian belies the appreciable height of elevation for the cella floor. Although other walls remained unidentified, at least according to Fisher's legend for Section GH,

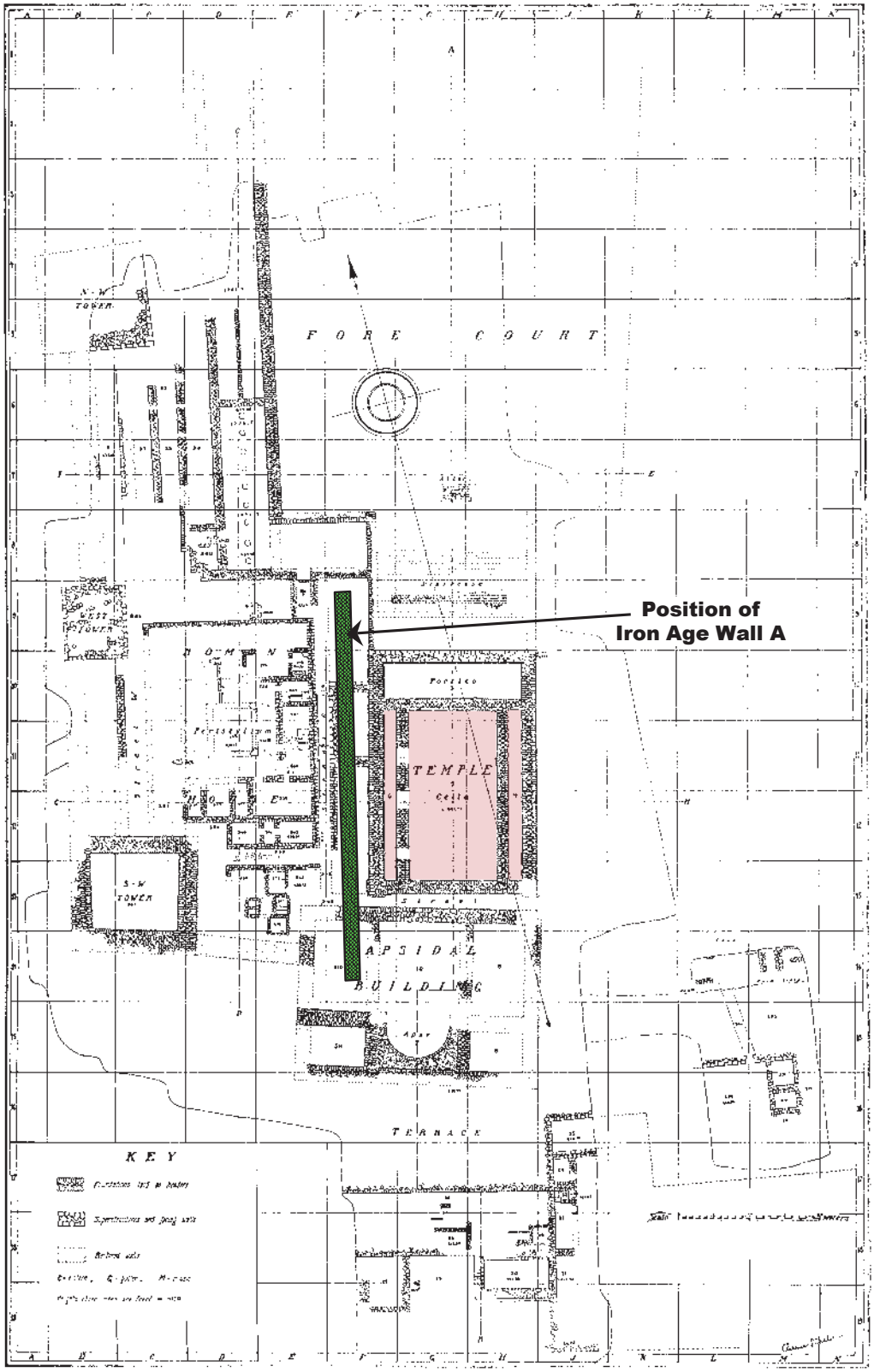


Fig. 39a *Elements of Herod's Augusteum: plan (adapted from* HES II, *Plan 8).*

two of the chambers do bear labels—26 and 28—and therefore must represent the rooms seen on Preherodian Plan 7, Grid G-H.11. Once again, however, their southernmost walls—i.e., the closest edge to GH—lay at least 3.5 m north of the section line (according to *HES II,* Plan 7). While this section shows no Omride features west of the high rock scarp, my earlier analysis of Section CD has shown that this is not the case.

The western half of Section GH (Grids B-F.12; figs. 37, 40) records primarily the "Preherodian" or Hellenistic deposits below rather scant remains from the Herodian period. There is a noticeable lack of stratigraphic detail for the area west of the Ostraca House; the drawing shows only the massive Greek Fort Wall and the basal courses of the former Israelite casemate system resting directly on the artificially stepped rock surface. In an early journal entry, Reisner wrote,

> The Roman level is present only on the extreme east. The Seleucid and Greek houses (at least two levels) have been swept away on the west. Here we are going down in tumbled black dirt and stones next to a bank of Israelite yellow debris. The houses on the east are about clear and <u>that part must stand over to next year</u> . . . . The <u>higher</u> debris in S4 as in S3 appears to contain very little Roman stuff. The potsherds range even earlier than in S3—going back certainly to the late Israelite period.
>
> (*Reisner Diary III,* 343–44; Reisner's underscoring)

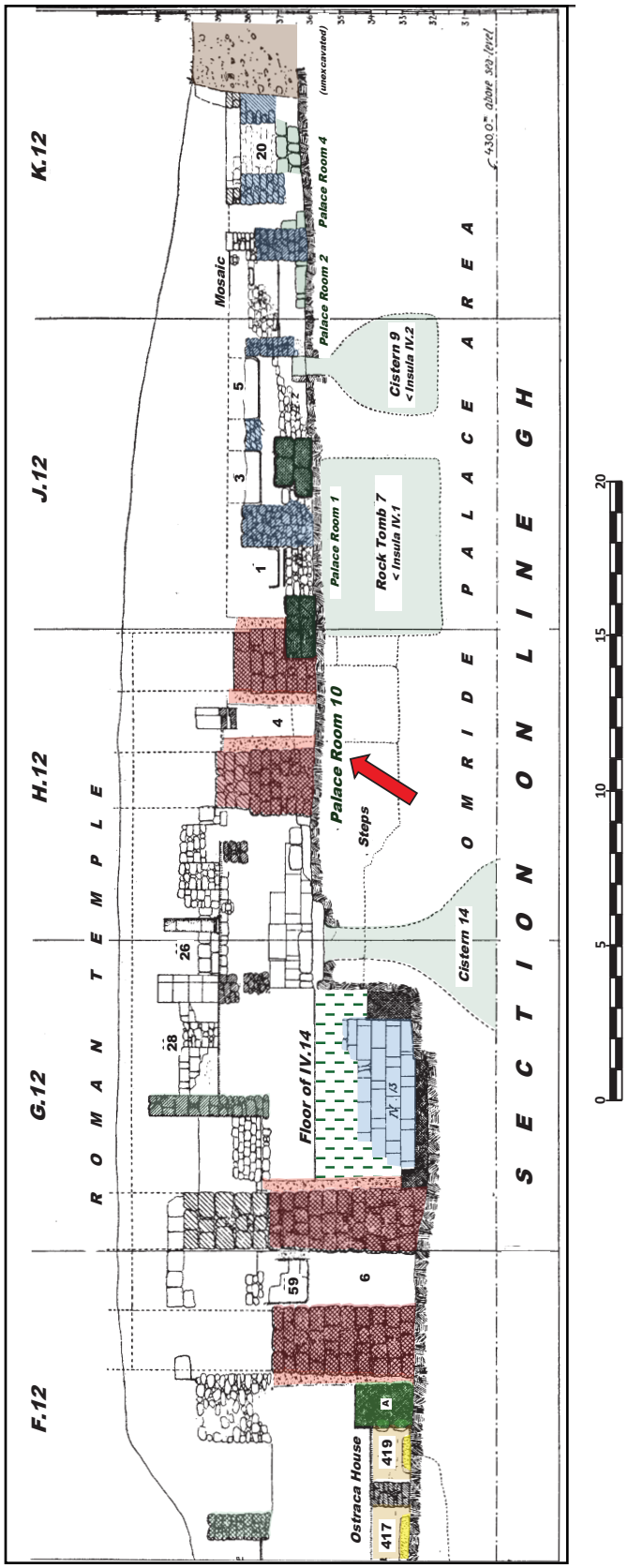


Fig. 39B *Elements of Herod's Augusteum: section (adapted from* HES II, *Plan 4).*

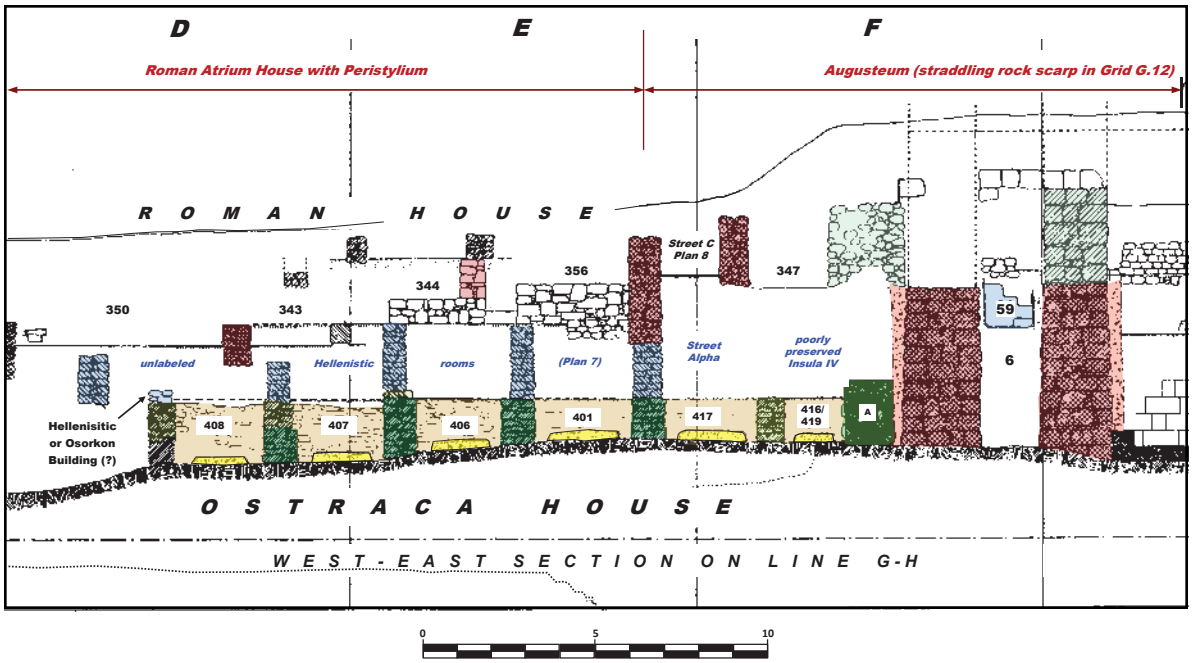


Fig. 40. *Section GH: West—along Horizontal Grid 12 = Ostraca House Area (adapted from* HES II, *Plan 4).*

Still, he followed that statement by recording the presence of "an Herodian foundation wall across the west end of S4 running N" (*Reisner Diary III,* 345).

Below the poorly represented Herodian stratum, and apparently buried in the deep fills supporting that level, Fisher drew the Preherodian, Hellenistic remains (blue level). A series of unlabeled walls appears on *HES II,* Plan 7 (B-F.12), and stretches from the heavily disturbed Insula IV and Street Alpha on the east to Street Eta (riding above a drain) and the massive Greek Fort Wall on the west (compare Grids E-F and B-C in fig. 37). In Grid C, one can also see the stubby remains of the old Israelite Casemate Chamber 328 (compare *HES II,* Plan 5, C.12). As this pre-Roman level overrode the area of the Ostraca House, the Hellenistic walls were constructed directly on the remains of the ostraca building. (This situation agrees with the picture gained from Section CD.) Moreover, the apparent floor level suggested by Fisher's broken horizontal line seems to relate to the later walls, not the Israelite ones. These facts will become very important later in my study, when I analyze the anepigraphic pottery that workers discovered alongside the ostraca pottery. Various indicators suggest that at least some ostraca findspots included ceramic forms derived from as late as the Hellenistic period (see Ch. 4).

In Subsidiary Section AB (see fig. 41, below), Reisner identified the earliest of two Greek levels with an "e." For ease of reference, I have numbered these features e.1–e.5, from east to west. As attested by both figs. 37 and 40, the series of Hellenistic walls resting directly on the truncated remains of the Ostraca House walls were generally narrower than the latter ones, which they used as ready-made foundations. At the transition from Grid E to F in fig. 40, in the area left blank by Fisher, Street Alpha (the principal north–south avenue that separated the various Insulae) covered and assumed virtually the same width as Ostraca House Corridor/Long-Room 417. And above the Ostraca House Alleyway 419 (which, at this point, actually = 416 on Plan 5), a poorly preserved set of rooms survived along the western edge of Insula IV. (See the area just above 201 on Plan 7.) West of these remains, preserved but unnumbered Hellenistic rooms lay directly above the area of the Ostraca House's Long-Room 401 and Storeroom 406. Similar chambers apparently overran Rooms 407 and 408 and extended westward from there. Although Fisher did not present any surviving wall fragments in this area on Plan

7, various wall segments on Section GH provide evidence of the existence of these rooms.

Clearly, then, all the major features and deposits presented in the western half of GH lay directly over the area occupied by the Ostraca House. In fact, the bases of the Hellenistic walls consistently rested at 434 m, i.e., at the apparent level of the mostly destroyed Ostraca House "floor" as drawn in Section CD, Grids D.11–12. In CD, however, the "Preherodian" (Hellenistic) walls and preserved floor (of Rooms 711–713) rested nearly a meter above the Ostraca House remains. And another entire phase, or perhaps even two phases, appears between the Hellenistic stratum and the Israelite remains. Not surprisingly, this drawing seems to make no allowance for an intervening stratum belonging to the Osorkon House, for according to Reisner those remains stopped almost 3 m short (north of) the Subsidiary Section AB cut. But does it appear on Section GH? Four unidentified stones that, in GH, lay atop the western wall of Ostraca House Room 408 may represent vestigial remains from the Osorkon House. But Section CD confirmed that the Osorkon building did not extend much farther south than 408, perhaps no farther than 408's northern wall. This situation seems very strange given the proximity and crossing point of CD and GH. (As noted, they actually intersect in Room 408, which provides the key point of comparison.)

Below the Ostraca House architecture, earlier and much wider walls appear once again. As noted above, these features were the first to cut through the "clean yellow mason's debris," and here they may represent the best-preserved portion of this prior building. The so-called Ostraca House was constructed on its ruins and followed the same basic design. Only the wall that divided Long-Room 417 from Alleyway 416/419 seems not to have rested on an earlier feature, thus suggesting either (1) that robbers had completely plundered the earlier wall prior to the construction of the Ostraca House, or (2) that the ostraca building actually extended a bit farther east than the former structure, so that the 417–416/419 wall belongs entirely to the later of the two structures. Note that the "clean" and "dirty" yellow deposits also appear in the otherwise vacant space of 416/419, and that workers poured the mixed (i.e., the "dirty yellow") debris even against the western face of Wall A. Similarly, on Subsidiary Section AB below (fig. 41), it is apparent that the clean yellow mason's debris ran farther *west* than the overall footprint of the Ostraca House. This fact provides further evidence that one cannot connect this deposit directly and solely to that building.

In any event, several important observations emerge. The eastern wall of 408 (which divides 408 from 407) shows a noticeably wide base that likely represents a construction phase which pre-dates the Ostraca House building. This situation matches perfectly the one seen already with respect to the southern wall of 408 (between 408 and 415 on Section CD, D.12). Both section drawings indicate that in a later building period workers rested a narrower wall directly on the truncated stub of this earlier feature. At a still later date, yet another, even smaller wall was built on top of the Phase 2 wall. The uppermost wall, illustrated in both CD and GH, belongs to the Hellenistic phase (even though it sits at a lower elevation in CD, where it rests ca. 4 m south of the southern wall of Room 711, i.e., the same distance from 711 to the next preserved wall to the south on Plan 7). These data also confirm that, as Reisner reported, the Osorkon House (which nowhere appears on Section GH, unless as the few unidentified stones atop the western wall of Room 408) extended no farther south than the northern edge of Ostraca House Rooms 406–407–408.[13]

3. Subsidiary Lateral Section AB (figs. 41–42)

As noted above, an auxiliary section—labeled AB[14] on fig. 32, above—ran parallel to and only 2.5 m south of Principal Section GH. A close comparison of the two drawings becomes crucial in the

13 Fisher's drawing of the western wall of Room 408 shows a strange preservation pattern on Section GH, where its upper courses seem to have survived, while its lower ones suffered destruction.

14 As previously noted, Fisher drew and published multiple subsidiary sections that received duplicate identifiers, often repeating the labels already assigned to the principal sections. Such is the case with Subsidiary Section AB.

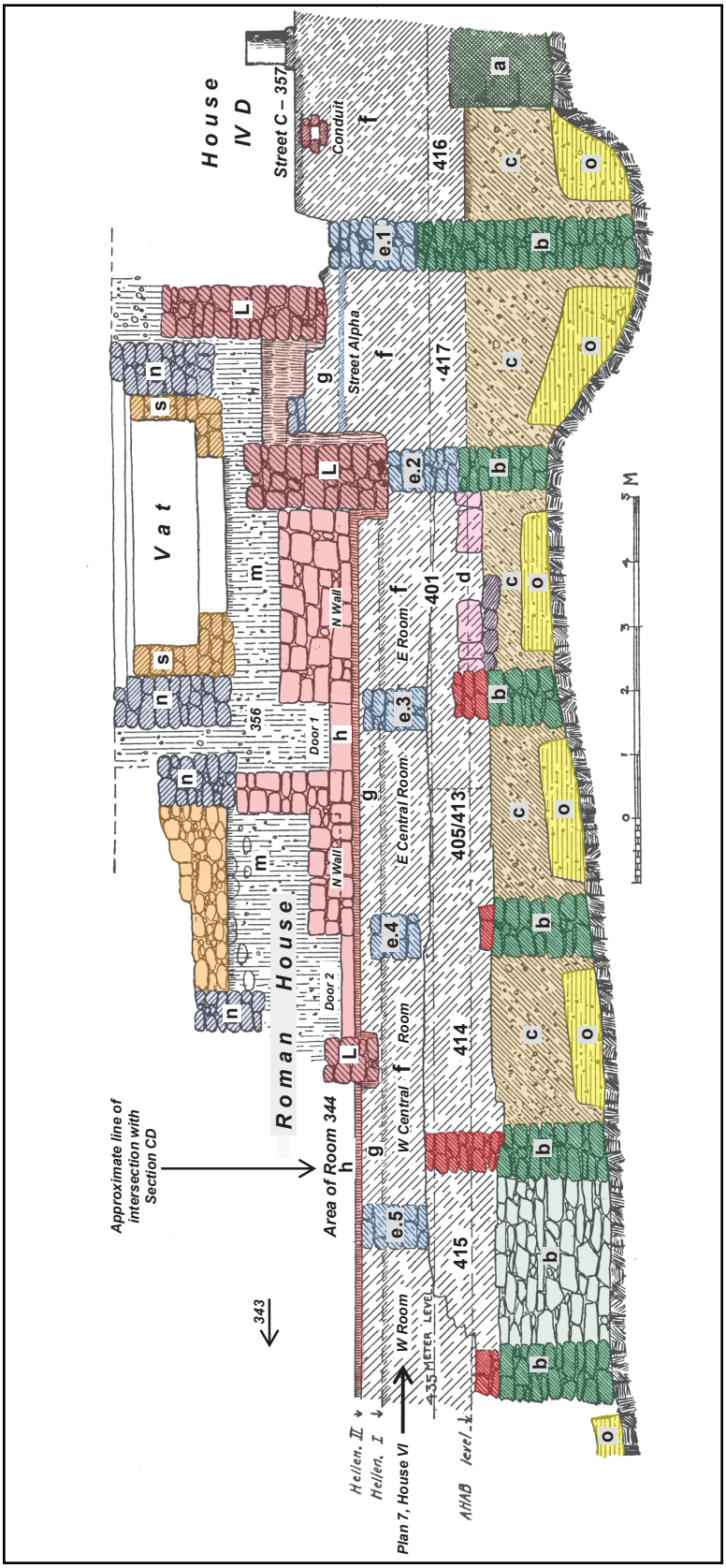


Fig. 41 *Subsidiary Section AB, with Reisner's labeling (adapted from* HES I, *64, fig. 15, and 114, fig. 42; for a plot of the section line on the horizontal plane, see fig. 32, above).*

study of the Ostraca House, since both cuts passed directly through the northern surviving rooms of that building. Although the plot of Subsidiary Section AB appears on plans presented in *HES I,* 64, fig. 15, and 114, fig. 42, Fisher did not include it on the master plan for the Israelite levels (*HES II,* Plan 5). This auxiliary cut took a lateral course through Ostraca House Rooms 415, 414, 413 (mislabeled 405 on the drawing, with no 405 existing on the plan), Corridors 401, 417, and the space in 416 (with its southern extension, 419). Principal Section GH, on the other hand, ran just north of this line through 408, 407, 406, 401, 417, and 416 (419).

On Plan 5, the 416/419 strip represents an apparent alleyway that remained free of any construction; rough bedrock extended into this space from the western face of Wall A (see above). This situation seems curious both from the unusual presence of unused ground inside a tightly-packed city and from the fact that the early builders of Samaria clearly smoothed and cut significant portions of bedrock to suit their designs. (Note the previously mentioned rock scarps, some of which were intentionally created, and the artificial shaping of bedrock visible in the western portion of figs. 37–38.) Thus they might easily have trimmed and used this valuable space. The stones depicted between the walls of Room 415 present yet another peculiar feature of this drawing. Though Reisner/Fisher included them in the "foundations of the Ostraca House," they must represent an architectural element that lay off the actual course of AB—perhaps the wall between 415 and 408 to the immediate north. But it still seems odd that Fisher would include them here and not in the other rooms, 414, 413, etc. And if he included them here just by way of example, why did he not stipulate so in either the drawing or the report's narrative? In any event, a number of significant observations emerge from a close comparison of Principal Section GH and Subsidiary Section AB.

First, the rock's profile beneath the eastern end of the Ostraca House shows an interesting feature in AB, which ran east from Room 413. The formation beneath the wall dividing Corridor 417 from Alleyway 416 differs remarkably from the rock located just 2.5 m north of that point, in Section GH, east of Room 406. On AB, an approximate 1.4-m dip existed between 417–416, and the wall separating these two rooms sat in the middle of the dip and survived to a height of ca. 3.45 m. Because of its higher footing on Section GH, however, the wall survived only to a height of ca. 1.25 m, and the dip is totally absent. This recess does not appear on Franklin's plot of rock features belonging to the pre-Omride "Building Period 0" (see Franklin, 2004: 193: fig. 4, Grid E.12, Franklin's grid system following that of Reisner). The recess resembles another hollow seen on Kenyon's long section through the royal courtyard east of the palace area (now published in *AIS I,* 74, fig. 14). In the courtyard depression, however, the rock remains rough and irregular, thus suggesting a natural formation. Here, beneath the Ostraca House 416, the depressed rock appears quite smooth—a fact that may indicate an artificial formation.

Second, it is important to note that the so-called "clean yellow construction debris" had accumulated in this depression to a greater depth (more than 1 m) than on the higher rock stretching westward (where it averaged 40–50 cm in depth). Also, as I have mentioned, on the opposite side of the drawing this deposit extended farther west than the footprint of the Ostraca House itself. In *HES I,* 62, Reisner recorded that the clean yellow mason's debris lying directly on the rock ranged only "10 to 40 cm thick, with a trodden surface" as its upper striation. A series of regularly-spaced walls penetrated this pre-existent yellow debris across its entire span. Reisner assigned these walls to the Ostraca House. While we have seen evidence suggesting that an earlier phase of construction existed below their more northerly counterparts (in Rooms 408-407-406), that previous phase does not appear to have extended farther south, into Subsidiary Section AB, where none of the walls shows an earlier, broader base resting on the rock surface, a situation unlike the bases seen in Principal Sections CD and GH. Even in those drawings, elements of this initial building (which pre-dates the Ostraca House) already cut through the clean yellow debris. This deposit, therefore, had to be in place prior to the construction of *any*

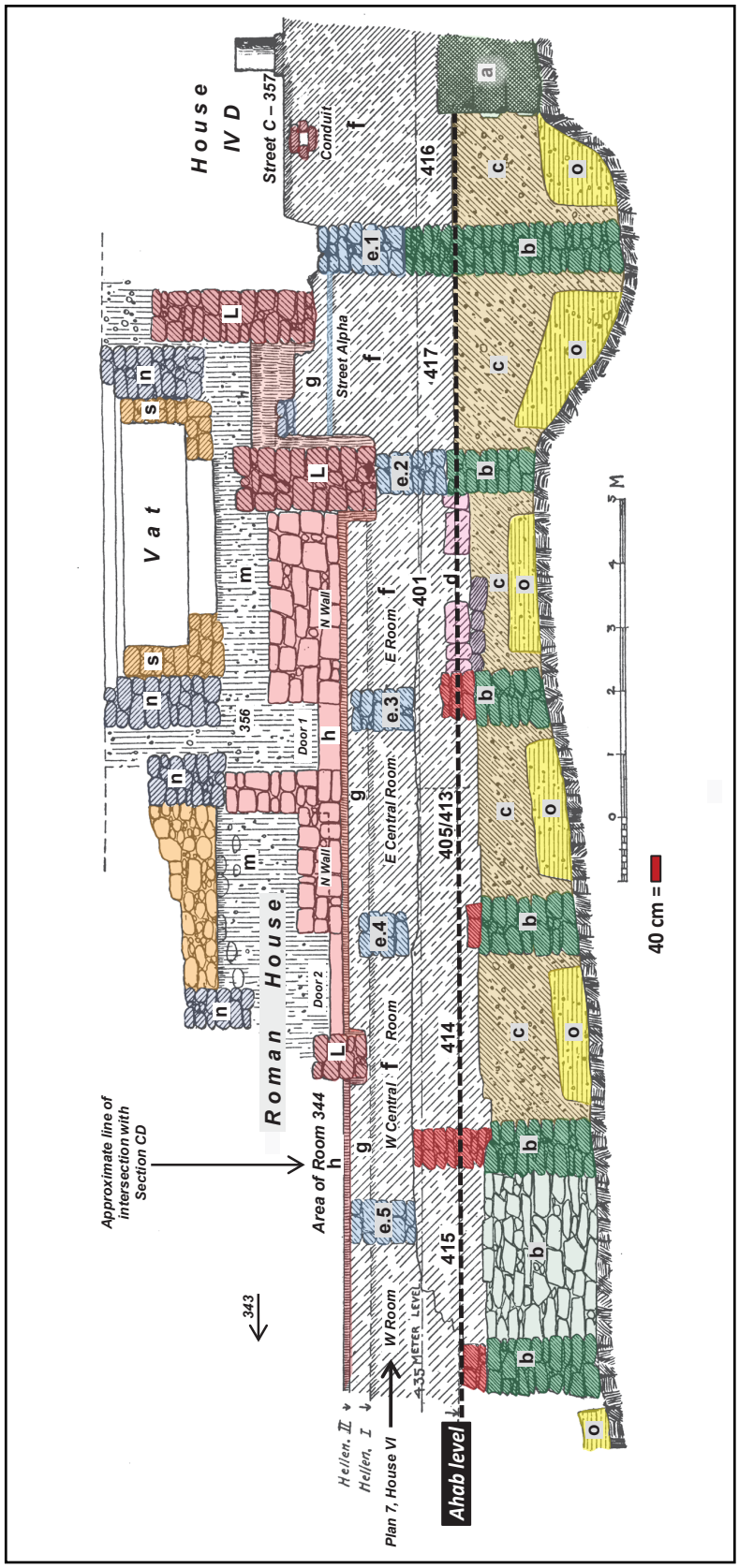


Fig. 42 *Subsidiary Section AB, with Reisner's "Ahab Level" emphasized in black (thickness of the purported Ostraca House floors indicated in red beneath the meter scale; adapted from* HES I, *64, fig. 15, and 114, fig. 42).*

architectural remains that survived here. In my estimation, the clean yellow debris stemmed from the artificial shaping of the rock itself, carried out either by the kinsmen of Shemer (judging from all the pre-Omride rock features that analysts have now identified) or, more likely, the workers of Omri (who quarried blocks from and dramatically reshaped large stretches of rock to suit Omri's building purposes).

Subsidiary Section AB shows that, originally, the clean yellow soil (labeled "o" on figs. 41–42) started at the base of the long, north–south Wall A and ran west from there. Reisner interpreted this wall as the western limit of Ahab's new palace extension[15] and the eastern boundary of a large courtyard that stretched all the way to the western Casemate system (in Grids C-D.5–15; see *HES I,* 58, fig. 13; 61 and especially 100). But I have already demonstrated that the record (whether published or unpublished) of such a thickly laid courtyard receives little, if any, graphic illustration in the excavation drawings. Oddly, the most convincing floor level appears on Subsidiary Section AB in Alleyway 416, where the narrative contends that the exposed rock remained rough and unhewn and where no other architectural elements existed. The construction of the massive Greek Fort Wall destroyed Wall A in the area of Grid F.14 (just as it destroyed the southernmost rooms of the Ostraca House itself; see fig. 34). To the north, Wall A ran to F.9, where the Roman-period vault terminated it (*HES II,* Plan 9, from the Severan era); excavators recovered no traces of this feature north of the vault (see Reisner's confirmation in *Reisner Diary VII,* 651). So Wall A, and the purported courtyard floor attached to it, ran from F.9–14 (cf. *HES II,* pl. 38.a.b-2). But Subsidiary Section AB casts serious doubt on the claim that a consolidated courtyard level, if it existed, extended (and survived) farther west than the alleyway and into the long corridors of the Ostraca House (417 or 401/418). Moreover, Principal Section CD surely shows nothing of such a level as far north as Grid F.9.

Third, even if Reisner's understanding "that this large rectangular space [Grids D-E-western half of F.9–14] was intended originally to be a great open court of the palace" (*HES I,* 100) seems apt, the courtyard floor likely would have sat several meters below the main palace level. If, as Reisner wrote, the floor rested between 20–100 cm above bedrock, its (erratic) elevation correlates better with the top of the clean yellow mason's debris than with any higher level depicted on Subsidiary AB. The only wall in AB that possibly does not cut through this yellow layer is Wall A on the eastern side of the drawing, where the yellow deposit is shown to run up to the wall's base. Moreover, the character of the matrix (clean yellow chips topped by a hard, beaten surface) agrees with that of the large, hard-packed, yellow courtyard later found by the Joint Expedition east of the main palace building[16] and dating, I believe, to King Omri. This thick (20+ cm) eastern court on the higher rock sealed some of the earliest Iron Age pottery at the site, and its own functional life seems to have spanned at least the early to late ninth century BCE (Kenyon's Periods I–III; see *AIS I,* 74, fig. 14; 94–95).

Reisner saw clearly that the use of this supposed western courtyard *preceded* the construction of the Ostraca House. But his description of what happened next proves very important, and one must read it closely. He wrote:

> … shortly *after* the new wing had been begun [i.e., Ahab's western extension of the palace to Wall A], and *before* the court was filled in, it was decided to utilize a part, at least, of the space for a series of large store-rooms for the wine and oil brought *to the palace as revenue.* The *foundations of a nearly complete group of rooms* occupying the entire southern end of the area were excavated,

15 Although this wall did not constitute the true western wall of the Israelite palace, it conveniently separates for the modern interpreter various features and activity areas on the lower rock. For example, it divided the Ostraca House from the rock-cut access tunnels and chambers—cistern, tombs (Franklin), and perhaps later the Pavement 13, etc.—lying beneath the palace proper, which sat on the higher rock platform.

16 See *AIS I,* 72–79, fig. 14, Level X, which equates to Level XIV in Kenyon's areas 313.306 and 364.337.363 (*AIS I,* 78–79; *AIS II,* 136, fig. 33).

> and from the large number of memoranda for wine and oil inscribed on potsherds which were *found on its floor,* it has been called the "Ostraca House"....
> (*HES I,* 100; italics added; Subsidiary Section AB militates against the last observation)

I have italicized the most salient points dealing with the building's state of preservation, the findspot and purpose of the ostraca, plus other important stratigraphic relationships, all of which allow further insights. But I must begin by tempering the details relating to the first two topics. At best, the surviving structure emerged only half complete; its entire northern and southern wings plus a good portion of its western half had suffered complete destruction. Second, when examining the drawing and descriptions of surviving rooms, it now seems highly questionable that the epigraphic finds came from "their floors" (corrected from Reisner's "its floor"). Throughout most of the building, such floor levels did not survive. Reisner also comfortably introduces the interpretation that the commodities mentioned in the inscriptions (wine and oil) reflect incoming tax payments, an interpretation that now seems less likely.

Reisner's valuable observations regarding the stratigraphic sequence witnessed in fig. 41 help clarify the overall depositional history around the Ostraca House. They reveal a three-phase progression: (1) the original, yellow courtyard; (2) the Ostraca House; and (3) the subsequent deep fills. On Subsidiary Section AB, Fisher labeled these phases "o," "b," and "c," respectively. Since "o" clearly represents the earliest deposit, the enduring question concerns the relationship between "b" and "c," particularly since the massive fills (which range from 1 [in 401] to 1.8 [in 414] to 2.7 [in 416–417] m in depth) that smothered the purported Ostraca House walls rose to the point at which new architectural phases began. In almost every case, the subsequent phases sat atop the truncated walls of the Ostraca House (if one assumes, with Reisner, that the "b" walls in figs. 41–42 represent Ostraca House remains). Achieving a level plane for laying the Ostraca House floors would have required only a small portion of this imported fill. One must wonder, then, whether this deposit relates to the construction of the Ostraca House or to its final burial in preparation for the next series of structures in this area (see my discussion in Ch. 1). In my judgment, based on Fisher's drawing and other factors discussed earlier, the latter seems the better scenario. In any event, no potential floor level survives in any of the rooms, with the possible exception of 416, which, oddly, represents the largely unused space of rough bedrock between the Ostraca House's easternmost rooms and Wall A. The claim that a floor ran over Grids D-E-F.12–14 levies no support from Subsidiary Section AB. In fact, this drawing militates against the assertion and, in this regard, stands in slight disagreement with nearby Principal Sections CD and GH. Moreover, the attribution of *both* the ostraca *and* the Osorkon Vase to the debris lying on this ostensible floor (or to the matrix of the floor itself, published statements remaining unclear) seems unlikely. In short, Reisner's reporting and Fisher's drawing combine to raise more questions than they answer regarding the provenance of the ostraca.

The thick deposit of imported fill that ancient workers poured over the clean yellow construction debris and that built up against the faces of all the foundation walls attributed to the Ostraca House (compare Section CD, GH, and Subsidiary Section AB) must represent, in Reisner's interpretive scheme, the "dirty yellow" leveling fill laid to support the floors of the ostraca storehouse (cf. *HES II,* pl. 33.f-2 and 2–0). But besides the later phases of walls discussed above, other residual elements, possibly relating to two separate entities, appear to be floating in or on the deposits inside Room 401. The four stones comprising the lower course average 30 cm in length, while the series of worked blocks lying above them range up to 65 cm in length and show a completely different character. (Workers may have laid them in a header-stretcher design, typical of other Israelite building techniques.) One may suspect that Fisher has once again incorporated into the drawing elements that, in fact, lay off the section line. When looking at this drawing, the reader is facing north. Reisner's Israelite Plan 5 shows an open-ended Corridor/Long-Room 401; apparently, the northernmost boundary of this

long room did not survive. In *HES II,* Plan 5, Grid E.6–11, however, a drain-like feature, at one point encased on both sides with large worked blocks, ran northward from the presumed northern end (whether originally enclosed or left open) of 401. The large blocks pictured on Subsidiary Section AB themselves have a gap in their middle portion, roughly in the center of 401. It seems possible, then, that these unidentified elements may represent Fisher's inclusion of the drain(?) in his presentation of 401, but this suggestion remains uncertain. Whatever these two layers of stones represent, they point out the difficulty of interpreting the Samaria sections with utmost accuracy. For if, according to Plan 5, these elements did not run within the bounds of Long-Room 401, they clearly lay off the section line of Subsidiary AB.

Finally, I note that any purported floor level that might originally have run over this dirty fill seems, from Fisher's drawings, *not* to have survived in any of the Ostraca House rooms. Thus, on his Subsidiary Section AB, Fisher actually placed the numbers for those rooms in the thick, subfloor fills of the Hellenistic period, where he also dashed in the hypothetical level of an Ostraca House floor. (Recall that the Osorkon House did not extend this far south.) The elevation at which he drew the floor line (fig. 42) seems based on two factors: (1) the approximate point at which changes occurred in the width and construction of the supposed Ostraca House walls; and (2) the erratic lower edge of dressing attributed to the western face of Wall A. But since this ostensible dressing undulated between 20 and 100 cm above bedrock, what does it mean to say that "level with the line was a hard, trodden surface," when such a surface itself would have had (1) to rise and fall up to 80 cm as it followed the line along the western face of Wall A, or (2) to run evenly at least 1 m above bedrock in order to conceal the entire uneven line? In short, it appears unlikely that this "line," however it might have related to the dressing of the stones, marks the presence of a viable floor. Any potential floor would surely have run along a relatively flat plane at a level somewhere above this irregular mark. One wonders whether the report has conflated two disparate stratigraphic features in this one description (or perhaps a description of something else altogether). Moreover, it seems quite reasonable to interpret the changes in the width and nature of the stacked walls in the Ostraca House sections not as mere transitions from subsurface foundation courses to exposed superstructure, but as entirely distinct building phases. The few broad walls that lie beneath the Ostraca House walls and the narrow walls that rest on top of them (and use them as their bases) belong to different archaeological strata and reflect successive building periods (as now corroborated by my close comparison of Reisner's narrative descriptions and recorded elevations with Fisher's scaled plans and sections).

Subsidiary Section AB, therefore, reveals that no possible floor level survived west of Corridor/Long-Room 417, and probably not even in that room. Any arbitrary extension of the hypothetical line of the floor (at 434.45 m) westward from its potentially preserved portion in Alleyway 416 and across the entire span of the Ostraca House would take the proposed surface *above* the truncated remains of Ostraca House walls and, in doing so, would associate that line with subsequent construction levels. This situation again hints at the possibility that much later (Hellenistic) pottery might easily have mixed in with and contaminated any earlier ostraca-bearing loci in these areas. If an Ostraca House floor originally existed at this level, these later phases completely destroyed it. It is, therefore, not possible (based on the erratic nature of the "dressing line" and the imposition of subsequent construction) to conclude that the ostraca derived from a coherent layer of "fine black debris" that represented meticulously excavated occupational debris lying directly on an unbroken span of floor. Subsidiary Section AB underscores the fact that any potential floor survived, at most, only in the longitudinal strip labeled 416, i.e., the space between the Ostraca House and Wall A. How much of a floor, or whether any floor at all, emerged during the excavation of 417 remains highly debatable. But surely no surface survived in any of the square storerooms farther west. And assuming that the compromised floor situation seen in 417 did extend into 418 on its southwestern flank (which, judging from Subsidiary Section AB, seems quite

likely), then 45 of the 46 ostraca (97.8%; see Ch. 2, Table 1) assigned to the 400-series of rooms derive from contaminated or otherwise secondary contexts (contra Holladay; see *AIS II,* 498, n. 160). The other ostracon came from Alleyway 416.

A final, close look at Subsidiary Section AB prompts the following basic questions: could the slightly wider "b" walls depicted between Rooms 413–414–415 possibly have belonged originally to something other than the Ostraca House? If they did, what then is their stratigraphic (chronological) relationship to the two taller and narrower walls between Alleyway 416 and Room 417? In short, is the caption provided for the "b" walls on Subsidiary Section AB correct? Based on a comparison with the western half of Principal Section GH, is it possible that a series of wide walls ("b," excluding 416–417) sat directly on the bedrock surface and that, at some point, builders poured "dirty yellow" fill ("c") up to the tops of these features (now essentially including 416–417)? Further, should one see in Subsidiary Section AB another, subsequent series of narrower walls (unlabeled; shown as the dark red level) placed directly on the truncated remains of the deeper, wider features? One fact seems certain: later on, yet another architectural phase ("e"), which Reisner dated to the Hellenistic period, overran all earlier features and in some places followed the same lines as those features but in other places diverged from them. These facts and queries permit at least two interpretive options. First, if

(1) Fisher's drawing is correct and the "b" walls (green phase) truly represent the Ostraca House, and

(2) the "e" walls (blue phase) belong to the Greek period, and

(3) the Osorkon House did not, as per Reisner, extend this far south,

then a phantom phase of unlabeled walls (dark red level) runs immediately over most of the Ostraca House remains.

Or, second, if

(1) the "b" walls actually stem from a substantial, earlier structure (pre-ostraca building) that neither Reisner nor Fisher recognized, and

(2) the real Ostraca House architecture (now the dark red level) rides above this stratum and directly below the Hellenistic level, and

(3) it is correct that no traces of the intervening Osorkon House survived in this area,

then the depositional history around the Ostraca House becomes more complicated than previously recognized.

In either case, the absence of viable, sealed floor levels explains how Hellenistic pottery might easily have gotten mixed with Iron Age materials. Whether the unlabeled (dark red) or the deeper-seated green walls represent the ostraca building, they were all successively smothered by massive fills poured in as preparations for later constructions. In my judgment, the green walls in figs. 41–42 seem uniform enough in both materials and techniques to constitute a single phase of construction. Whereas Section GH makes fundamentally clear that an earlier series of wide walls existed beneath the northern rooms of the Ostraca House, I do not see that structure extending below AB Rooms 413–414–415.

In any event, the horizontal line that Fisher drew on Subsidiary Section AB (fig. 42) to mark the hypothetical "Ahab level" actually ran above or passed through walls from different, chronologically disparate building projects. If the "b" walls belong to the so-called Ostraca House, as seems reasonable, they too are buried in deep, imported fills. And if the upper crust of the "clean yellow" deposit ("o") did, in fact, constitute some kind of floor level, it had nothing to do with any of the illustrated architecture (except possibly Wall A). On close scrutiny, Reisner's insistence on seeing a thick, well-preserved Israelite courtyard over this entire area, or seeing thick, well-preserved floors inside the rooms designated as the "Ostraca House," becomes increasingly unfounded.

In sum, Fisher's horizontal line representing the hypothetical "Ahab level" certainly cannot reflect a well-preserved, 10–40-cm-thick floor situated between 20 and 100 cm above the area's bedrock. (To highlight the thickness of such a floor, I have added a 40-cm mark beneath the scale of fig. 42.) Despite these facts, Reisner confidently reported that workers found both the Osorkon Jar and the ostraca, as well as other pottery and objects, lying on this "living floor of the courtyard" and that, collectively, these discoveries provided "complete proof…, beyond question, that the building [in this instance, the Israelite royal palace situated to the east, on the rock crest] was occupied during the Ninth Century B.C." (*HES I,* 60). His entire reconstruction now seems, at best, quite tendentious.

C. Summary

Besides describing the local stratigraphy in detail, this extended review of various vertical cuts through the Ostraca House has noted numerous challenges that arise from the narratives of Reisner and the drawings of Fisher. The narratives lack the kind of provenance data that would allow the reader not only to situate a particular artifact in a specific horizontal location within, say, Ostraca House Room 417 versus 401, but also to place the item back in its original, precise position within the vertical depositional layers at that location. The latter shortcoming proves a serious handicap to all chronological considerations. Both the use of duplicate identification labels and the failure to identify and discuss every building phase represented in a section drawing also impede the narrative discussion in the report. Regarding the drawings, I have demonstrated that in addition to significant but unannounced diversions from the course of a section cut as plotted in the report, the peculiar practice of including in a section drawing features that lay well off the section line holds great potential to mislead a reader, especially since Reisner and Fisher never explained this procedure in the accompanying text. Prior to this study, only D. Ussishkin (2007: 63–64; see also Ch. 2, n. 9 and n. 7 above) noticed this unexpected practice. But whereas he believed that Fisher faithfully used stippled lines to represent such outliers, I have shown that such an indicator does not always appear, so the student of the Harvard sections must analyze them cautiously.

Understanding the need to separate Grids E.13–14 from D.10–11–12 in published Section CD constitutes the starting point for any interpretation of this drawing. Although Fisher presented these two grids as though they lay in tandem along a straight course, in reality a lateral distance of more than 5 m separates them. In this drawing, the principle contribution of Grid D lies in its clear indication that distinct building phases both preceded and followed the Ostraca House level. In fact, I identified at least five distinct strata between the bedrock up to and including the Osorkon House. The depositional history in this area, therefore, proves much more complicated than one might expect from reading the report. Although traces of these unidentified phases are less apparent in Grid E, no floor level earlier than the Herodian period survived here, in the southern half of the ostraca building.

Rather than making an unannounced change in course, Section GH incorporates features that lay well off the plotted section line. According to this drawing and its associated narrative, architectural construction in this area began with King Ahab, not with his father, Omri. Moreover, remains from later levels, the Hellenistic and Roman periods, lie directly on top of the truncated features of the Ostraca House, with much more massive fills covering the Long-Room 417 and Alleyway 419. In this regard, it is somewhat difficult to reconcile Sections CD and GH, which actually transect one another in Rooms 407–408. Whereas the former suggests one or two building phases between the Ostraca House and the Hellenistic levels (vertical space that must somehow accommodate the Osorkon House), the latter drawing gives no vertical indication of these levels. Both GH and CD seem to agree, however, that significant building remains exist, at least beneath the area occupied by Rooms 407–408, which pre-date the walls of the Ostraca House.

The lateral cut of Subsidiary Section AB, just a few meters south of the line of GH, certainly aids one's interpretation of the better-preserved por-

tion of the Ostraca House. Its close proximity to GH makes crucial a comparative analysis of the two drawings. The auxiliary drawing AB indicates that few, if any, remnants of the earlier building that underlay the northern portion of the Ostraca House and that partially appeared in Section GH extended, beyond question, to the line of AB. The preexisting structure, then, survived mainly under Room 408 and its lateral counterparts. Its function remains unknown and, because recovered pottery groups were not correlated stratigraphically to its truncated and poorly-preserved architecture, its precise date remains undeterminable. Perhaps the most significant contribution of Subsidiary Section AB lies in the fact that Reisner's purported "Ahab Courtyard" is nowhere attested in this drawing. Surely, if such a thick and prominent surface extended over the entire span of Grids D-E-F, i.e., across the breadth of the Ostraca House's footprint, then Fisher would certainly have represented it clearly and prominently on this section drawing and others. Judging from Subsidiary Section AB, however, no floor level of any kind prior to the late Hellenistic or Roman period existed anywhere in this vicinity, with Alleyway 416 representing the only possible exception (see Ch. 2, fig. 25; also figs. 40–42 above). Finally, the hard-packed, clean yellow masons' debris that runs beneath all the Ostraca House corridors and storerooms on AB (except for Room 415), and even west of the entire building, strongly resembles the hard yellow matrix used for the large courtyard surface east of the main palace area on the summit plateau (see *AIS I,* 74, fig. 14 = *AIS II,* 139, fig. 37). This deposit appears on all relevant sections (CD, GH, and Subsidiary AB) and was cut through by early walls situated beneath part of the Ostraca House. It points quite likely to Omri's own quarrying and construction activities around and below the summit plateau, with the former operation alone having produced enormous quantities of yellow chips (masons' debris).

Chapter 4

Ceramic Considerations

A. The Harvard Excavations and the Joint Expedition: Collating the Data

When considering the pottery recovered by the Harvard Excavations, serious challenges quickly emerge from the manner in which Reisner presented what he had found. The difficulties inherent in the official report later stymied the well-reasoned efforts by Kaufman to utilize the ceramic assemblage in his study of the epigraphic materials. In his assessment of the pottery associated with the Samaria Ostraca, Kaufman (1966: 110) quickly recognized two key problems: "we have neither [a] controlled sequence of strata and pottery … nor any stratigraphical connection between the two excavations," i.e., between the Harvard Excavations in 1908–1910 and the Joint Expedition in the 1930s. In typically clear, concise fashion, Kaufman provided an account of the difficulties that arose when both Kathleen Kenyon and Grace Mary Crowfoot (fig. 43) attempted to correlate the results of their ceramic analyses with Reisner's earlier and rather laconic presentation of the Harvard pottery. Briefly stated, after both scholars examined the ostraca on display in the Archaeological Museum in Istanbul, Kenyon dated the sherds displaying the inscriptions to her Period IV at Samaria, while acknowledging that a few fragments might belong to Period III (see Table 4). Crowfoot agreed in principle with this assessment but also recognized that some of the vessel types extended beyond Period IV at least as late as Period V (*SS III,* 199, 469–70). Kaufman (1966: 113) believed that, to allow enough time for the final, extended phase of Israelite Samaria (Kenyon's Period VI), Kenyon and Crowfoot "more or less harmonized" their different interpretations in the Joint Expedition's official report by dating Period IV "at the beginning of the eighth century," limiting Period V to "the first half of the century," and leaving the last half of the century (at least until 722 BCE) for Period VI, the final phase of Israelite control over the erstwhile capital (*SS III,* 469–70). In this scenario, the ostraca-bearing pottery showed a *terminus ante quem* around 750 BCE (*SS III,* 203), when new wares (hard, thin, gray fabric) and new forms (water decanters) marked the outset of Period VI.

But a close reading of the entire report issued by the Joint Expedition only muddied the waters. For example, in *The Buildings at Samaria* (*SS I*), a single earring from Period IV helped set the beginning of that phase around 800 BCE. But then Kenyon and John Winter Crowfoot divided Period IV into two distinct phases (IV and IVa) and presented the architecture of Period V together with that of Period VI, not IV (*SS I,* 103–106). Moreover, Kenyon stated that the pottery of Period IV, while "quite distinct from that of Period III," showed many forms that also continued as late as Period VI. The best overall date she could suggest for all

these levels (IV–V–VI) at this point was simply "the eighth century B.C." These and other factors prompted Kaufman to question whether one should view Kenyon's Period V as truly distinct from her Period VI. To investigate this matter, he offered a brief comparison of the pottery recovered from a series of Courtyard Rooms and the so-called Pit *i* (all situated near the northern edge of the summit plateau) and pottery supposedly found along Wall 573 (located farther down the northern slope of the mound).

Fig. 43 *Kathleen Mary Kenyon (left) and Grace Mary Crowfoot (right).*

At the same time as Kaufman was developing his work on the ostraca, another Harvard student, John S. Holladay, was pursuing an in-depth analysis of the Samaria pottery. Both authors submitted their studies in 1966, and both works undoubtedly grew out of the current, intense interest in the archaeology of Samaria held by their teacher, G. E. Wright. Both dissertations, however, suffered from a virtually total dependence on Kenyon's pottery volume, which she had published in 1957 (*SS III*). They did not have access to unpublished field diaries and section drawings then stored in London. As a result, both studies used as their shared starting point conclusions espoused by Kenyon in *SS I* and *III.* (Kaufman even received personal counsel from Kenyon regarding the archaeology of Samaria [Kaufman 1966: ii].) Consequently, they understood the northern Courtyard Rooms, Pit *i,* and Wall 573 deposits to represent primary loci that yielded homogeneous ceramic groups (despite their disparate locations on the site) and that collectively supported a very narrow dating. Holladay (1966: 67–68) even argued that Pit *i* represented one of only three findspots that provided an *absolute date.* Above all, they accepted Kenyon's position that the archaeological record revealed a coherent, massive conflagration level that covered the site and reflected "the complete destruction of the capital city" by the Assyrians in 722 BCE (Kenyon 1971: 133).

Recent, full-scale analyses of the stratigraphic and ceramic records from Samaria have undermined each of these points, which both Kaufman and Holladay accepted as presuppositions (see *AIS I–II).* Since the Joint Expedition recovered precious little pottery from beneath the floors of the Courtyard Rooms, and since, ultimately, Kenyon published only eight fragments in support of her Period V (*SS III,* 119, fig. 8), the Period VI leveling contemporary with Wall 573 (fig. 44) and the filling inside Pit *i* (fig. 45) constituted the two principal findspots germane to Kaufman's inquiry. Serious problems attend both areas. All but one of the fragments associated with Wall 573 came from a substantial deposit of backfill (composite Layer V–Va). This downslope (figs. 46, 47a–b), secondary context inherently calls for an open *terminus post quem* for the materials it contained, a fact that diminishes its ability to set the precise end of the preceding Period V. More seriously, Kenyon's published Section CD and various unpublished field sections show that robber trenches broke the actual stratigraphic connection between the wall and the massive fill around it. One cannot, therefore, even be certain of the contemporaneity of the two entities (*AIS II,* 294). Moreover, many of the pottery forms recovered from the filling might easily have derived from the late eighth to early seventh centuries BCE, i.e., from the earliest decades of the Assyrian hegemony over the site

Table 4 Architectural and ceramic periods at Samaria: a comparison of views. Green shading: K. M. Kenyon's timeframe for the ostraca; red shading: G. M. Crowfoot's timeframe (compare *AIS I,* 254, Appendix B).

	Kenyon		Wright		Avigad	
	Building	Pottery	Building	Pottery	Building	Pottery
EBA	–	+	–	+	–	+
Pre-Omride (Shemer Estate; 10th–early 9th)	–	–	–	1–2	0	1–2
Omri (882–871 BCE)	I	1	I	3	I	3
Ahab (871–852 BCE)	II	2			II	
Jehu (842–814 BCE)	III	3	II		III	
Jeroboam II (784–748 BCE)	IV	4	III	4	IV	4
748–722 BCE	V–VI	5–6	IV–VI	5–6	V–VI	5–6

(cf. *SS III,* fig. 9:1, 3, 5, 7–9), while the remaining pieces (*SS III,* fig. 9:10–18) represent variations on flanged cooking pot rims whose styles spanned the ninth and eighth centuries. Thus, despite the presence of much earlier forms in this fill, an actual depositional date for it near the turn of the eighth century BCE seems quite reasonable.

Similar problems plague Pit *i,* which Kenyon understood as either a rubbish pit or long-lasting latrine. She believed the most important evidence for Periods V–VI came from this feature, which lay near the center of a poorly-built complex of rooms labeled *f-g-h-j-k* (*SS I,* fig. 48). While in *SS III,* fig. 10:1–27, she claimed to have removed the entire assemblage from two related layers within this locus (V and Va), only No. 8 came from V; the remainder belonged to Va. Moreover, in her unpublished field notes Kenyon divided the materials more specifically between two distinct horizontal tracts of excavation, *Segment 122.125.19.121* (17 published fragments) and *Segment 122.126.19.121* (10 published fragments).[1] While the former area may have included some portion of Pit *i,* it also extended south toward the higher rock of the summit plateau and Wall 125 in the vicinity of Room *hk* (*SS I,* 107, fig. 50; *AIS II,* 38, fig. 12). It yielded 63 percent of the pottery ultimately assigned to Pit *i,* and these materials showed a much better state of preservation than did those found in deeper fills north of the pit. The latter segment probably bore a similar relationship to the pit, but it, too, extended beyond that feature and ran northward through the area of Room *g* and toward Wall 126, where the declining rock required greater amounts of fill to level the area for subsequent building projects (*SS I,* pl. VII: Section EF). Importantly, neither Kaufman nor Holladay had access to these vital facts at the time of their writing.

1 Kenyon apparently used walls from different phases to create her segment designations. One cannot, therefore, assume that all (or even any) of the walls labeled 19, 121, 122, or 125 actually date to the Israelite period. They simply provided convenient parameters for horizontal tracts of excavation.

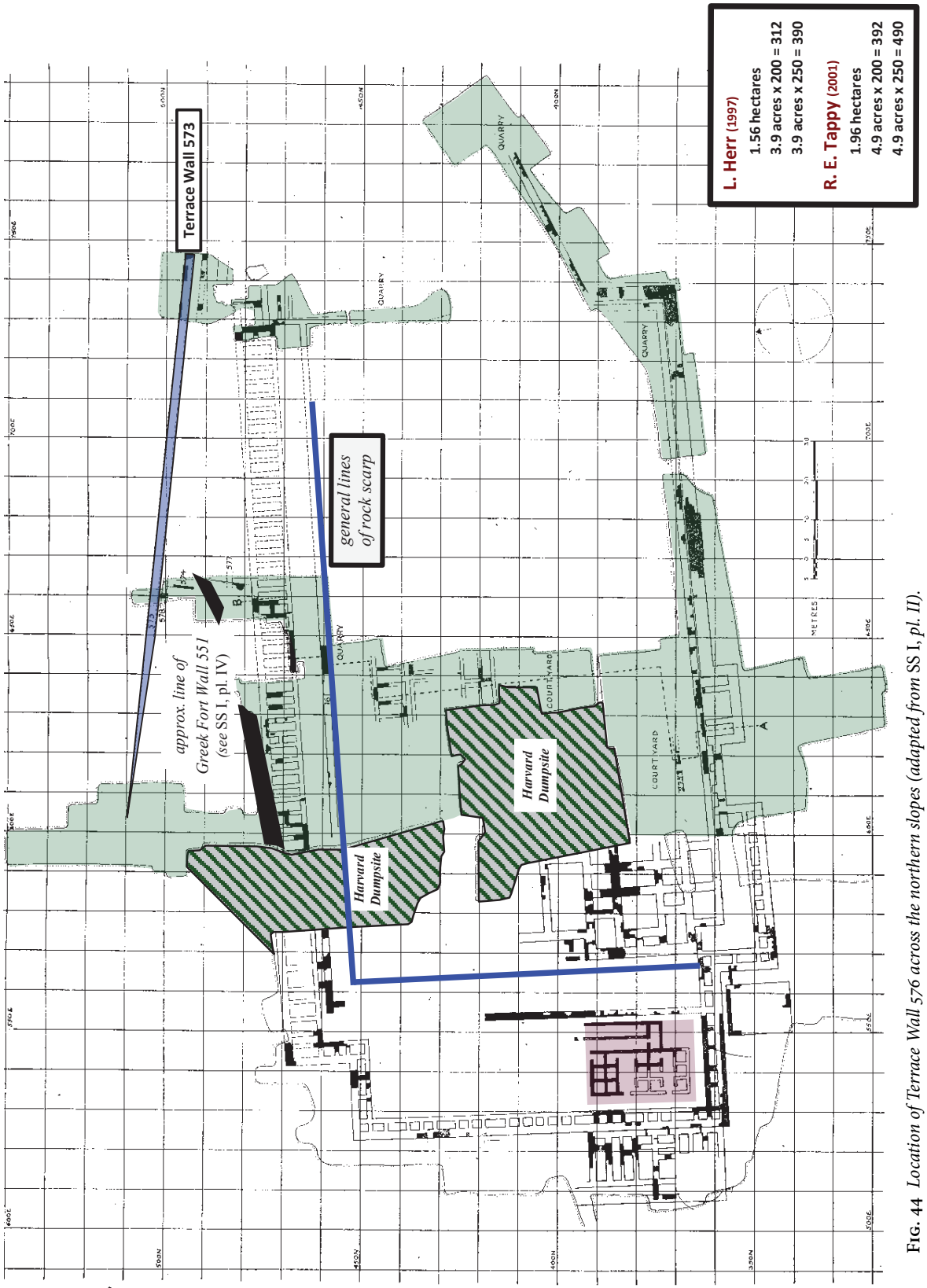


Fig. 44 *Location of Terrace Wall 576 across the northern slopes (adapted from* SS I, *pl. II).*

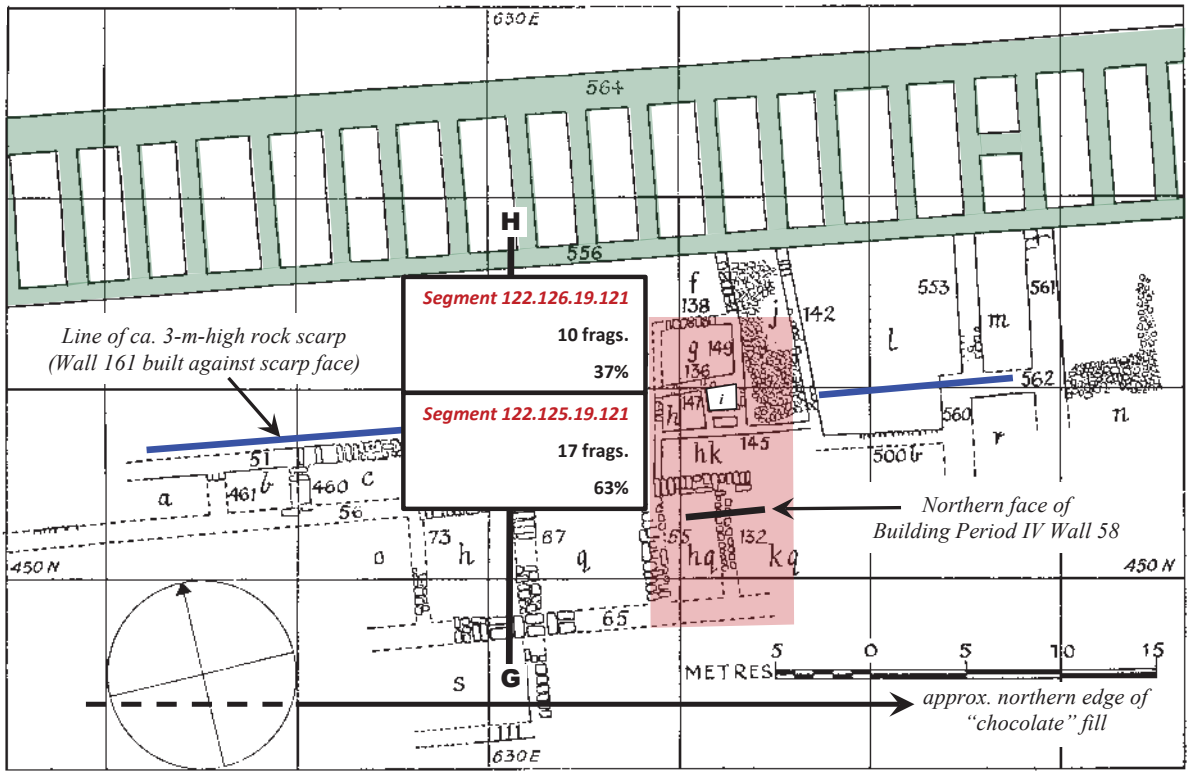


Fig. 45 *Plan of Pit* i *and related excavation segments from the Joint Expedition (adapted from* SS I, *107, fig. 50).*

Close analysis of the pottery ascribed to Pit *i* shows that the assemblage falls into an early and a late group of forms.[2] Once again, some of the earlier forms began already in the ninth century BCE and extended into the eighth century; they might easily have derived from Kenyon's Period III or IV. The late group, on the other hand, falls primarily after the Assyrian takeover of Samaria, i.e., it might post-date *Israelite* Samaria altogether. In certain cases, this group contained very stylish ceramic designs (thin-ware, Assyrian-style bowls; oenochoe pitchers; etc.), including forms that may have stemmed from metal prototypes. The clear dichotomy within the purported pit assemblage offers an opposing view to Holladay's position that "no serious questions can be raised about the essential homogeneity of the [Pit *i*] group" (Holladay 1966: 68) and certainly undermines his declaration that Pit *i* could claim an absolute date. The *terminus ante quem* of the disparate ceramic groups associated with this circumscribed feature, then, correlates to that of Megiddo III and Tell Keisan 5–4.

Thus, to accept the overall hermeneutical framework published by the Joint Expedition and then to interpret the Harvard materials in that light is to hitch one's star to the chariot of Phaëton; the process is fraught with interpretive danger signs. Although Kaufman had no access to the kinds of stratigraphic details cited above, he intuitively sensed "the great difficulty of dating the pottery used for the ostraca" based on a presumed clear and direct comparison with the pottery published by the Joint Expedition (Kaufman 1966: 112–13). In the end, he correctly saw that "we must raise the question of whether Period V pottery can be held to be a legitimate period distinct from that of Period VI" (1966: 116). But the lack of appropriate study data compromised his methodology and made him overly dependent on Kenyon's published opinions. As a result, he could not have realized

2 Early Group: *SS III,* fig. 10:1–6, 14, 20–23, 26. Late Group: *SS III,* fig. 10:7–12, 13?, 15–19, 24–25, 27. See *AIS II,* 341–46, Table 43, and 615, Appendix E.

that the interpretive carpet upon which he was about to place his foot concealed an open trap door below. He thus understood the Pit *i* ceramic group as a homogeneous one completely contained within the top layer of the pit itself and covered by a clearly datable destruction level wrought by the Assyrians in 722 BCE. He did not know that even the truncated pit walls actually rose well above the house floors; that the original height and full contents of the pit were not knowable at the time of excavation; that the original pit may therefore have served later periods; that the uppermost surviving portion of the pit was never properly sealed by a coherent, datable destruction deposit; or that, as a result, the remains of the pit likely included contaminating, intrusive materials from later periods. Also with Kenyon, he used the new hard-ware fabrics and decanter forms as hallmarks of Period VI, and he accepted the notion that forms typical of the late-eighth to early-seventh centuries were totally absent from the pit assemblage (Kaufman 1966: 117, n. 48). For Kaufman, all the pottery taken from Pit *i*, the Period V–VI houses, and backfills in the vicinity of Wall 573 reflected the latest styles of Israelite (i.e., *pre*-Assyrian) Samaria. Similarly, Holladay also defined these materials as a "ceramic horizon" versus a "ceramic period" and, astonishingly, limited the Pit *i* group to the years 725–722 BCE on the belief that one could date this pottery "independent of ceramic comparisons" (i.e., strictly on the historical basis of a presumed Assyrian destruction; see Holladay 1966: 16, n. 36; 69–70). Today, all these assertions have become clearly untenable.

On the earlier side, Kaufman knew that even though Kenyon herself had devoted very little space to the pottery found in the makeup *beneath* the floors

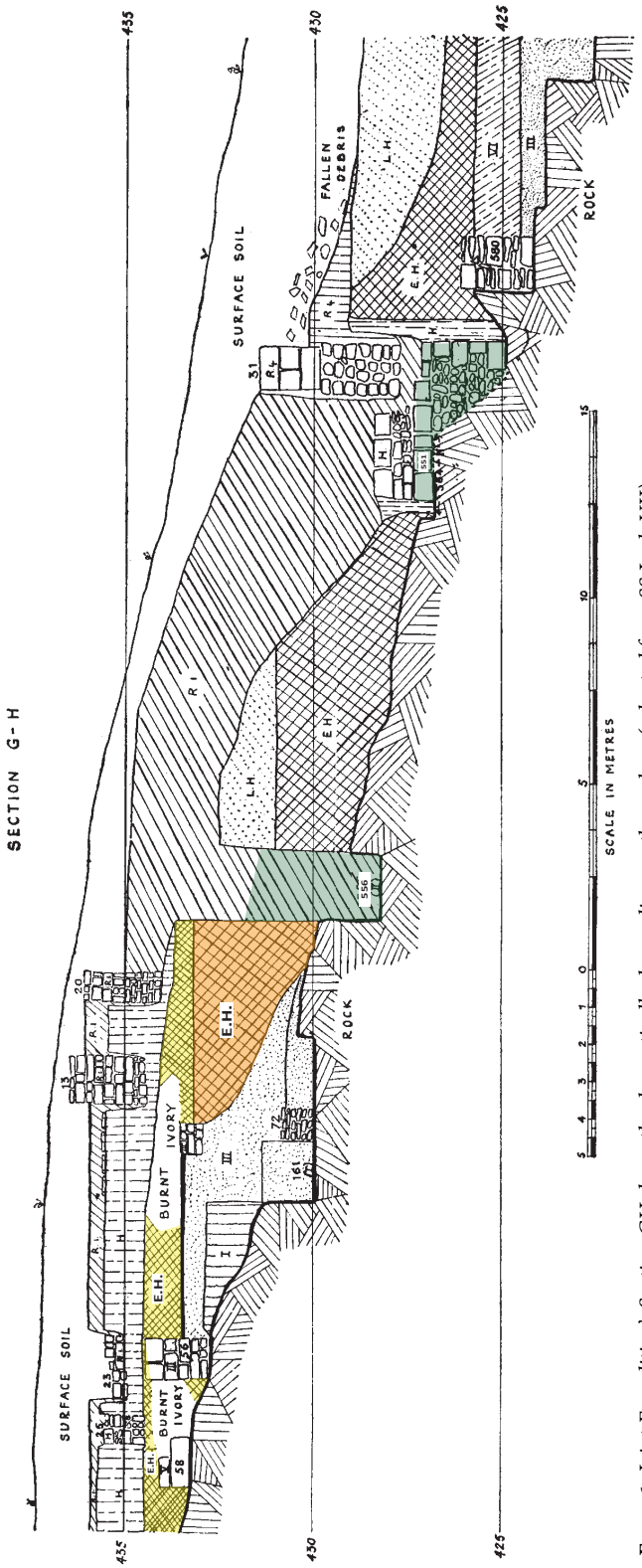


Fig. 46 *Joint Expedition's Section GH down the dramatically descending northern slope (adapted from* SS I, *pl. VII).*

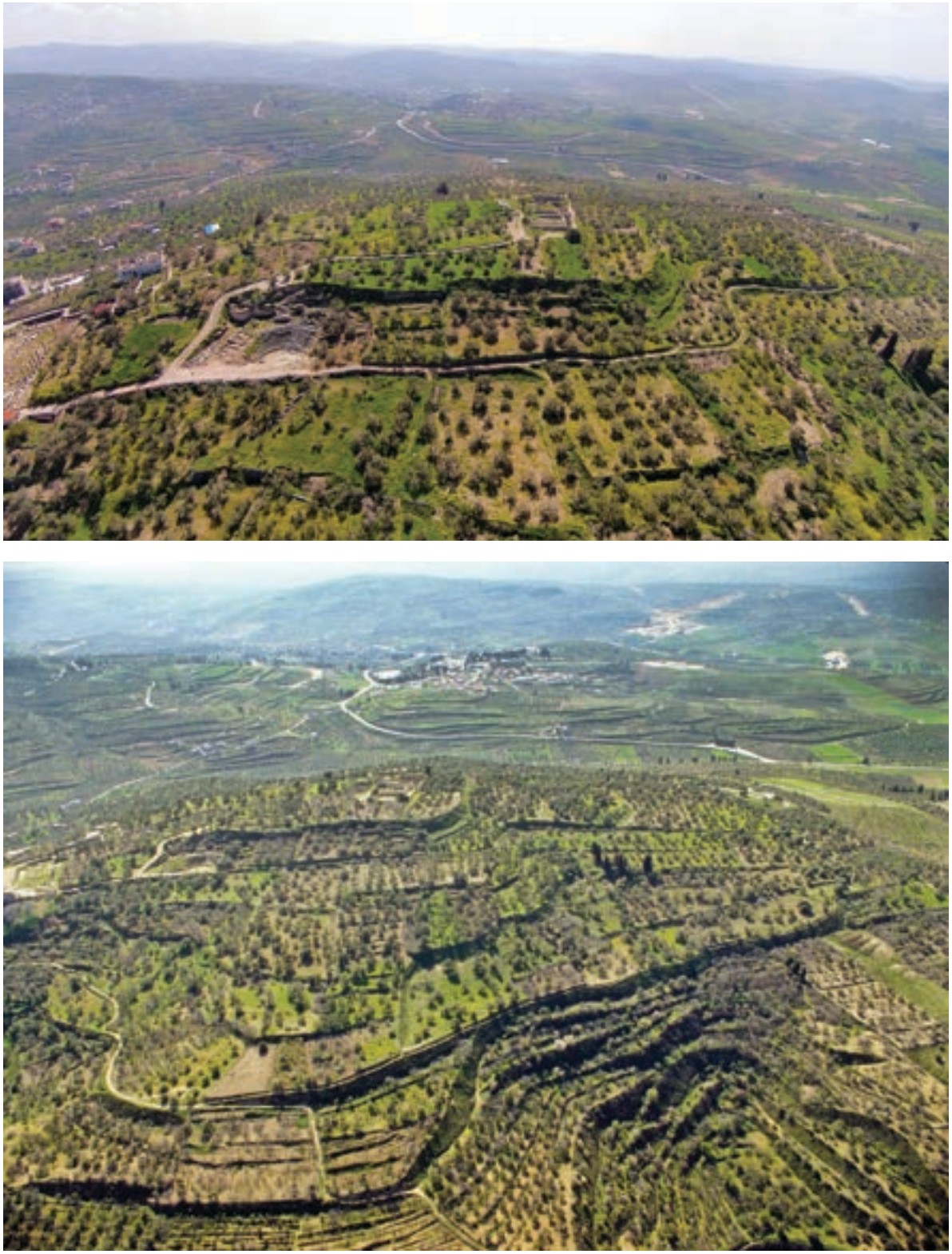


Fig. 47 *Aerial views of Samaria's steep northern terrain: a. upper contours (acropolis from the north; courtesy of ws030515602: Bill Schlegel/BiblePlaces.com); b. lower contours (slopes from the north; courtesy of bb00200059: Barry Beitzel/BiblePlaces.com).*

of the Period V house, these levels had yielded "a few sherds of … harder ware, including some fragments of water decanters" (*SS I,* 108). As a result, he concluded that

> … there is only one discernible period of pottery between Period IV and the destruction of the city …. It begins, as far as the evidence goes, some time before the building of the last houses on the north side of the summit [i.e., the Period V rooms, which lasted into Period VI], for it is beneath their floors, and it is common by the time those houses were destroyed, presumably in the year 722 (Kaufman 1966: 118).

Thus, rather than conflate Kenyon's Periods IV and V, Kaufman collapsed V into Period VI—a change consistent with his reading of the Joint Expedition's report. Finally, he concluded that

> … the Samaria ostraca belong to the next to last significant period of Samaria before its fall [i.e., to Period IV], and that a clear period of development follows the ostraca, namely the introduction of the thinner, harder ware, and the first use in Samaria of the typical water decanter [Period V/VI] (Kaufman 1966: 119–20).

This reasoning led Kaufman to set the transition from IV to V/VI at 750 BCE and to keep the ostraca dated to the first half of the eighth century BCE.[3] In the end, then, the only real change forwarded by Kaufman's study consisted in his associating Period V with VI rather than with IV; his *terminus ante quem* for the ostraca collection (750 BCE) basically perpetuated Kenyon's own views.

B. Reisner's Presentation of Pottery: A Meager Repertoire

Let us now return specifically to the Harvard material recovered during the 1910 excavation season. Appendix A will serve as a guide throughout this discussion, since it tabulates a large quantity of data relating to the ostraca and pottery collections, including their registration numbers, year formulae,[4] ceramic typology, representation in the final report, specific findspots, and dates of discovery. (Appendix B provides additional comments and lists the ostraca in the order of their discovery, as revealed in the unpublished field diaries.)

Kaufman's impulse to examine the chronology of both the ostraca pottery and other, non-epigraphic pottery that Reisner assigned to the same or similar contexts as a means of honing in on a date for the writings themselves constitutes, in principle, a sound approach. So what can one make of the Harvard report's presentation of vessel types and findspots?

To begin, one should note two facts. First, in Reisner's view, "the point of departure for the Israelite pottery was given by the Israelite ostraca and the *abundant potsherds found with them* in the floor debris of the Ahab courtyard" (note: not the Ostraca House floor). Second, he proceeded to parse the pottery into three "functional groups": ordinary vessels (storage jars, cooking pots, common eating and drinking items); finer fabrics (the better table items, ointment and scent containers); and decorated pottery (*HES I*, 274–75; italics added). In all but two instances (Appendix A: Line Items 26, 56), the ostraca themselves relate to the first two categories.

Even if the difficulties surrounding the Period IV–VI Rooms, Pit *i,* and Wall 573 were resolved in the Joint Expedition's report, the fact remains that the Harvard volumes offer only a very small quantity of pottery that one might compare with Kenyon's

3 Note that whereas both Kenyon and Kaufman dated the Period IV pottery group (which they saw as most related to the ostraca) to the early eighth century BCE, Aharoni and Amiran assigned the repertoire to the late ninth century immediately following its publication (Aharoni and Amiran 1958: 178–80). Still, Aharoni kept the ostraca themselves in the reign of Jeroboam II.

4 Note that Aḥituv's (2008: 278–79) transliteration of Ostracon No. 18 mistakenly reads, "In the ninth year …," as opposed to the correct formula, "In the tenth year …." For a tabulation of and comments on the year formulae, see Schloen 2001: 156–59, Table 4, fig. 9; 163–65.

material (even though Reisner recorded privately that Fisher "made an exhaustive series of drawings of the forms of the pottery" [*Reisner Diary III,* 341]). Moreover, the format of the presentation in *HES I* proves quite unwieldy in numerous respects, including Kaufman's recognition of one's inability to correlate pottery-provenance data within a tightly controlled stratigraphic sequence. Using information taken from both the Ostraca Registry (*HES I,* 232–38) and the Pottery Registry (*HES I,* 276–80, figs. 153–56), I have collated the data vital to a comprehensive study of the ostraca and the ceramic fragments that bore them. As with Kenyon's later publication of pottery recovered by the Joint Expedition, the number of fragments presented by Reisner seems strikingly low. *HES I,* figs. 153–54, offer 99 entries representing the group of "ordinary vessels," out of which 23 (23.2%) appear with a "Dis. No." rather than a "Reg. No."—labeling which presumably means that discarded sherds were ultimately incorporated into the report.[5] For the finer wares in *HES I,* figs. 155–56, Reisner listed 29 items, with 11 (37.93%) marked "Dis." Of the 128 total vessel fragments published on behalf of the entire Iron Age (Reisner's "Israelite period"), then, only 57 (44.5%) appear in pottery drawings, with 19 (33.33%) of the 57 carrying the enigmatic notation "Dis."

Specifically regarding the inscription-bearing pottery, Appendix A shows 75 individual registration numbers for the ceramic vessels involved (see Ch. 1, n. 2). Of these 75 fragments, Reisner somewhat amazingly drew in profile only four—one from the finer-wares group (Reg. No. 3873) and three from ordinary wares (Reg. Nos. 3993, 4075, and 4619; see Appendix A: 26, 38, 1, and 39, respectively).[6] This lack of attention to ceramic morphology means that, despite his stated dedication to extensive recording procedures (see the introductory quotation in Ch. 1), his regard for the ostraca as among the most interesting of all discoveries at Samaria (*HES I,* 62), and his declaration that the ostraca provided the starting point for a study of all Israelite pottery (*HES I,* 274), Reisner ultimately provided professional pottery drawings for only 5.33 percent of the fragments on which inscriptions appeared,[7] even though much of the writing lay at or near diagnostic ceramic features such as rims or bases. The published photographic record fares little better: only seven (9.33%) of the 75 registered items appear in *HES II,* pl. 55:c, d, and e.1–5 (see Ch. 1, n. 17). Thus only 11 of 75 pottery fragments were presented in a drawing or photograph; none appeared in both. Of the remaining 64 items, those marked with ∝ in Appendix A: Column 5 (*HES I Cross Refs.*) appeared in the Ostraca Registry but were simply listed in the ceramic section with no accompanying photograph or drawing (33:64 = 51.6%). Further, items showing a blank in Column 5 were included in the Ostraca Registry but not even listed in the Pottery Registry (31:64 = 48.4%). These numbers demonstrate that, even though Reisner presented a line drawing of each ostracon in both *HES I* and the undated Boston/Cockayne publication, he gave precious little consideration to the actual pottery that bore the inscriptions. His hand-written field diaries focus on architecture and his own daily activities; they offer virtually no technical discussion of pottery. Establishing a date for the ostraca on the basis of comparative ceramic analysis, therefore, becomes something of a challenge.

C. The 1910 Ostraca: Spatial Distribution (Appendix A: Column 7–*Findspot*; Table 5)

Regarding the spatial distribution of the ostraca, I have already demonstrated that the majority

5 Similarly, Kenyon reclaimed a significant number of discarded fragments and incorporated them into her published report in 1957. (See the items marked with an asterisk in *AIS II,* 587–90, Appendix A.)

6 These fragments appear in Appendix A, Lines 1, 26, 38, and 39 and in *HES I,* figs. 154:5, 6, 13 and 156:16a. The drawing for 4619 (fig. 154:13) appears without its registration number. Note also that Reg. No. 4630 in *HES I,* 278, no. 20.b should show a link to Ostracon No. 50 on page 237. (Another needed correction, now among the uninscribed pottery, requires a link between *HES I,* fig. 155:2a, Reg. No. 02176, and the caption on page 280, no. 2.)

7 Note that most of the ostraca occur on fragments that retained at least a portion of the vessel's rim, base, or the inflection of its sidewall break, i.e., diagnostic features that should, through comparative ceramic analysis, enable an identification and drawing of the original form.

of the inscriptions came from the Long-Rooms or Corridors on the eastern side of the so-called Ostraca House, and that these rooms lacked any direct connection to the square storerooms or rectangular entrance halls inside the main structure. Furthermore, the storerooms themselves had access only to the entrance halls, which in turn opened only to the west, not to the east. Table 5 draws from data presented in Appendix A: Column 7 to show that, within the longitudinal eastern corridors, the 75 registered fragments in question derived from 15 individual findspots.

Workers found the ostraca in two disparate tracts of summit excavation—Summit Strips 4 and 7. Importantly, Strip 7 contributed nearly 39 percent of the overall repertoire. This excavation area lay to the north of the Ostraca House and in the area of the so-called Osorkon House, although the southern edge of the latter structure seems to have touched on the northernmost surviving wall of Ostraca House Rooms 406–407–408 (compare figs. 26 and 30, above). Within Strip 7, four areas yielded inked fragments: 723, 772, 773, and 776 (see Ch. 2, Summary). Although Reisner treated these areas as discernible "rooms," Fisher's plans do not present any architectural features for most of them. As *HES II,* Plan 5, reveals, two of these areas (723, 776) lay directly north of the main ostraca building and straddled Grids E-F.11. While 776 may represent the area closest to the Ostraca House, it actually appears on the plan for the *Babylonian* period (*HES II,* Plan 6: F.11) as a well-preserved room situated northeast of the ostraca structure. Apparently, it overrode the earlier Wall A, which Reisner had assigned (questionably) to Ahab. Unfortunately, even 776 lay approximately 2.5–3 m north of published Section GH, so no record exists of elevations taken inside this room that might relate to the provenance of the ostraca. The remaining two areas (772, 773) lay still farther north by roughly 18 m, and both designations appear on Reisner's "Preherodian" (i.e., Hellenistic) town plan (*HES II,* Plan 7: E-F.9). It seems that 772 refers to a spot in the middle of the lengthy Street Alpha, which separated Insulae I, III, and VI on the west from II, IV, and V on the east. Importantly, I noted earlier that the relatively large number of ostraca recovered from 772 came "from under Roman walls" situated to the northeast of the 400-series of rooms (*Reisner Diary VI,* 589). This description surely suggests a provenance in late foundation-trench debris. Locus 773 appears inside the northwestern corner of a room in Insula IV, at the intersection of Streets Alpha and Beta.

Table 5 Horizontal distribution of ostraca. For the count of 75, see *AIS II,* 496.

Summit Strip 4	# of ostraca frags.	% of 46	% of 75
S4–401 sub	1	2.17	1.33
S4–416	1	2.17	1.33
S4–417	22	47.83	29.33
S4–417 N	11	23.92	14.67
S4–417 N sub	1	2.17	1.33
S4–418	9	19.57	12.00
S4–418 sub	1	2.17	1.33
TOTAL	46	100	61.32

Summit Strip 7	# of ostraca frags.	% of 29	% of 75
S7–723	1	3.45	1.33
S7–772	12	41.38	16.00
S7–772 N	1	3.45	1.33
S7–772 sub	1	3.45	1.33
S7–772 W	2	6.89	2.70
S7–773	8	27.59	10.66
S7–776	3	10.34	4.00
S7–776 sub	1	3.45	1.33
TOTAL	29	100	38.68

Judging from this spatial distribution, then, none of the ostraca assigned to Summit Strip 7 derived from locations physically close to (or, possibly, from contexts chronologically close to) the Ostraca House itself. The 27 line items (representing 29 registered ceramic fragments [38.7%]) involved in this dilemma appear with shading in Appendix A, and a quick perusal of this table reveals the substantial impact this distribution has for the study

of the ostraca. Moreover, I shall demonstrate below that some of the pottery types associated with the ostraca actually represent Hellenistic traditions. Finally, the published diary tells of additional ostraca retrieved from the "usual floor debris" in Rooms 770–777 (*HES I,* 403, Sept. 16–26), in the far north of S7, with the latter chambers lying even farther north of the Ostraca House proper. Unfortunately, the published registry includes none of these writings.

Turning to Summit Strip 4, one sees that the ostraca findspots follow a southwest-to-northeast spread over two principal rooms, starting in the northern portion of 418 (10 total inscriptions = 21.74% of the S4 group) and extending into 417 (34 inscriptions = 73.92% of the S4 group). Beyond this clear pattern of distribution, Reisner assigned one additional fragment to the southeastern portion of Room 401 and another piece to the vacant space or alleyway in 416. A review of the excavation sequence in Strip 4 proves informative. Since the modern-day surface of this area had served as the dumping ground for Schumacher's work in 1908, Reisner devoted 11 days in 1909 (October 9–19) to removing the dump-heap and laying out Strip 4 into seven subsections (*a-g*). By July 2010, excavation had penetrated through the levels attributed to the Severan period and had exposed the southernmost rooms of the Herodian period Atrium House (Grids E-F.11–12) as well as, east of those chambers, a series of pre-Herodian rooms (F.12–13). Finally, in early August the fieldwork made an important transition. Note that in the following entry the "dirty yellow debris" lay *above* the purported Ostraca House floors, not below them as Reisner records elsewhere, and he did not associate any ostraca with "the debris above the floors."

> Aug. 1–5. Removing floors of S4–345 to 357 [rooms across the southern side of the Roman period Atrium House;[8] for 357, see Ch. 2, pp. 65–67], revealing the floors of the building later called the Ostraca House. *The debris above the floors of the Ostraca House was dirty yellow debris* (loose filling) containing Israelite potsherds, a scarab (Reg. No. 3704), and lamps.... This debris had apparently been thrown in to a *depth of 50 to 60 cm.* in a leveling operation in the *early Post-Israelite period*. Fragments of walls of two building periods were found between the Herodian and Israelite floors.[9]
> (*HES I,* 400–401; italics added)

But as the work progressed, and as one crisis after another emerged in dealings with the local workers and authorities, Reisner's personal enthusiasm for the project sank to its nadir. On Sunday, August 7, 1910, he lamented in his journal,

> I have given up hope of making any sensational finds. I propose to utilize our remaining time to the best possible advantage but with the idea of ceasing the work here at the end of this season. I think there would be no difficulty about a new irade.[10] I am sure the site might be examined for another two campaigns [11] for the sake of the historical results alone. It is even possible that there may be priceless historical inscriptions somewhere; but the hill is so vast that it is a mere matter of chance whether we strike anything even in a ten year's campaign. No

8 From west to east on *HES II,* Plans 8–9, the rooms appear as follows: Severan period — 352, 353 (C.12); 354 (E.11, north of 345); and 345, 346 (E.12); Herodian period — 351 (C.12); 350, 354 (D.12); 356 (E.11–12, lying beneath Severan rooms 345–346); 353 (E.12, in street south of Atrium House); 357 (E.12, in Street C on east side of Atrium House); 347 (F.11, east of Street C).

9 Reisner assigned the two intervening levels to the Hellenistic (Seleucid) period (see Ch. 3, n. 5).

10 *Irāde,* similar to the *firman* (see Ch. 6, n. 16), refers to an official document issued by the Ottoman Sultan and likely related to official permission from the ruling authority to excavate. Schumacher defined the *irāde* as an "Imperial permit" (*Schumacher Diary I,* 67).

11 Recall that J. H. Schiff had offered his initial pledge of financial support in 1905 for a five-year expedition to a site in Palestine (see Ch. 1.B, above).

> properly conducted expedition ever guarantees anything more than to find what there is in the territory excavated and to unravel its history. Nothing in the way of scientific research can be hoped for from indiscriminate grubbing in a dozen different places on the odd chance of finding anything. (*Reisner Diary V,* 511–12)

Such a waning of morale and motivation stands in stark contrast to the earlier elation that Reisner expressed when merely considering the construction techniques exhibited by the Israelite architecture. (See my Preface plus the excerpt from *Reisner Diary III,* 300–301, October 5, 1909, presented in Ch. 5.)

From the outset of the project, multiple administrative quandaries had cast a doubt in Lyon's mind as to whether the expedition could extend beyond the inaugural 1908 Season (see *Lyon Diary III,* 31; also Tappy 2016). Following the dispirited note cited above, Reisner included quotations from a dozen entries in Schumacher's unpublished journals from the inaugural 1908 Season, apparently as corroboration and background for his own current administrative quagmire. The citations ran from May 1 to June 25, 1908 (see *Schumacher Diary I,* 11–62), and spoke of problems with Hasan Bey (el-Huseini), the current local commissionaire, who (1) repeatedly leveled impossible demands on the project; (2) "used very strong and insulting language especially against me [Schumacher]"; (3) made attempts to blackmail the director; (4) requested an extraordinary salary and traveling expenses for himself; (5) tried to direct (sometimes through the local Ottoman official, the *mutesarrif* of Nablus[12]) compensatory payments for land use and damaged or lost olive trees to the leaders in Nablus rather than to the local owners of the property; (6) orchestrated a 21-day work stoppage; (7) appointed "a notorious dealer in antiquities" (one Georgi el-Tawil, known locally as "Long George") from Jerusalem who "would spoil our workmen" as overseer of the project; (8) insisted that the expedition store all artifacts in local facilities and that Schumacher "would have access only by special permission;" and even (9) attempted to dictate the precise locations of excavation dumps, etc. (These citations appear as an excursus between pages 512 and 513 in *Reisner Diary V* [Reisner's underscoring]; on the off-site storage of artifacts in the village, see also *Lyon Diary I,* 35–36.)

Ultimately, after consulting with each other on June 1, 1908, both Lyon and Schumacher decided to leave Samaria and proceed to Istanbul,[13] where they could give a detailed report of these and other

12 Suleyman Fethi Bey, son of Nauri Bey of Constantinople (*Reisner Diary IV,* 435). From the outset of the project, even Lyon depicted Hasan Bey as "an impossible person" who often worked against the *mutesarrif* of Nablus (*Lyon Diary I,* 5; also *I,* 36–37, where Lyon recorded that "Hasan now boasts of having us in his power, [and] that he will throw all obstacles in our way till he forces the closing of the works").

13 Chronic disputes had begun already by May 14, 1908 (*Schumacher Diary I,* 24–25), and centered initially on the expedition's payment for land use rights and damaged olive trees. The excavation directors felt that they should make the disbursements directly to the land owners themselves, while the local officials in Nablus demanded that the payouts come to the government. The troubles quickly spread and involved the flagrant behavior of Hasan Bey, the local commissionaire in Nablus, the backfilling of excavation trenches, the rules governing the disposition of artifacts recovered from the site, etc. (*Schumacher I,* 41–42, 45, 58–59). Lyon and Schumacher left Samaria on Friday, June 5, 1908, and proceeded from Nablus to Haifa, Beirut, Cyprus, Rhodes, Samos, Smyrna, and Gallipoli before arriving in Constantinople/Istanbul on Wednesday, June 17. (For full travel details, see *Schumacher I,* 49–54.) At first, Schumacher "had the feeling that Hamdi [Bey] wished to have little to do with the affair, he hurried over our complaints" (*Schumacher I,* 55). After multiple meetings with Hamdi Bey (see n. 14), his son Edhem Bey, his brother Khalil Bey, Assistant Director of the Imperial Museum and the one "who actually superintends cases regarding Commissionaires," and even the US Ambassador to Turkey, John George Alexander Leishman from Pittsburgh, both Lyon and Schumacher left Constantinople on Thursday, June 25, 1908, feeling that the trip had proven productive. Khalil Bey had declared that the local Commissionaire in Nablus "cannot dismiss any workman or overseer but with our consent, he has no orders to give them. He is there to mediate all transactions between us [the excavators] and the government and is there especially to help us and further the excavations . . . . We left him quite contented and if he sticks to his word our journey to Istanbul will have been a full success" (*Schumacher Diary I,* 59; for Schumacher's complete account of negotiations in Istanbul, see pp. 54–59). By Friday, July 10, 1908, authorities in Istanbul replaced Hasan Bey with a new commissionaire, Mohammed Said Effendi ʿAbd el Hādi, who was a graduate

shenanigans to a higher authority, Osman Hamdi Bey (then director of the Imperial Museum in Constantinople).[14] A resolution finally came on June 20, when Hasan Bey was removed as local liaison to the excavation.[15] Word of the dismissal finally reached Lyon in Jerusalem on Sunday, July 5, 1908,

of the Imperial University of Constantinople, who served as aid to the Walī of Beirut, and who also had worked previously with the excavations at Jericho (*Schumacher Diary I,* 58, 68, 72–73). Mr. Effendi immediately engaged the Samaria archaeologists in a positive manner, although ambiguities remained over the payment recipients and, the following day, Effendi's brother appeared on site to collect two years' worth of back taxes owed on the house that the excavation had rented from the Baptist Mission Society in London (see Ch. 1, n. 19; also *Schumacher Diary I,* 74, 76, 80, 82, 87). By the end of the 1908 Season, Effendi himself had become dissatisfied with his salary (*Schumacher Diary II,* 147–48). Schumacher all but accused him of openly pilfering several small objects (coins) from the stored artifacts and placing them in his handbag, presumably in consolation of feeling "duly remunerated" (*Schumacher Diary II,* 149–50).

14 After an attempt in 1869 by Safvet Pasha, Minister of Education, to establish a national archaeological museum achieved only moderate success (yielding "as yet a haphazard collection open only to the sultan and his guests" [Shaw 2003: 24]), artist and archaeologist Hamdi Bey successfully orchestrated the founding of the İmparatoruk Müzesi ("Imperial Museum"; for his archaeological work on the royal tombs at Sidon, see Hamdi Bey and Reinach 1892; for catalogues related to the new Museum, see Hamdi 1893; 1895). Not only did he supervise the ten-year construction phase (1881–1891), but he also became the Museum's first curator upon its opening in 1891. Hamdi Bey appears to have maintained a congenial working relationship with both Lyon and Reisner. When Lyon traveled (with Schumacher) to Istanbul in June 1908 to lodge an official complaint against the unwarranted behavior and accusations constantly waged by the current commissioner in Sebaste, who served as liaison to the project, and to seek his dismissal, Hamdi Bey replied, "I promise you complete satisfaction. I have labored 26 years in the interest of science, and no one shall cast a stone in your way, whoever he be" (*Lyon Diary I,* 80). But because he was "a very ill man—very feeble" by 1910, Hamdi Bey delegated oversight of some official transactions to his son Edhem Bey. Apparently another son, Arif Bey, also worked at the Museum at that time (see *Reisner Diary IV,* 375ff.). Hamdi Bey's brother, Khalîl Bey, succeeded him (*HES I,* 31).

I remain unable to determine the precise relationship (if any) of Hasan Bey to this family. Apparently, he kept a less amicable relationship with Schumacher. This situation becomes quite clear in the aforementioned series of entries that Reisner extracted from Schumacher's 1908 journals and quoted verbatim in his own private records. (Reisner inserted these passages between pages 512 and 513 in his own *Reisner Diary V*.) One of Schumacher's notations also highlights Hamdi Bey's unqualified support of the expedition from the very beginning (see n. 15).

For the best treatment of Hamdi Bey's career as an artist and for the rise and function of museums within the context of the waning Ottoman Empire of the late nineteenth century and the Young Turk Revolution of 1908–1910, see Shaw 2003; 2007. On Monday, July 27, 1908, Schumacher "received news of a revolution at Constple. and that a Parliament was created. Removal of grandvizier, death of Sheikh el-Islām and Abu el-Huda" (*Schumacher Diary I,* 96). He returned to Samaria from Haifa the next day with a "report of a revolution at Constantinople" (*Lyon Diary II,* 51; Lyon's underscoring). The following spring, on April 23, 1909, as he prepared to travel to Samaria for the second field season, Reisner sent the following cable from Cairo to Lyon: "Postponing departure until May first. Official advice." Lyon understood the delay as "doubtless on account of the present disturbed state of affairs in the Turkish Empire" (*Lyon Diary III,* 65). But by Wednesday, December 8, 1909, Reisner was able to write that

> the intercourse with the Museum officials is greatly facilitated by the removal of the old oppression. Almost every one of the men I have had to deal with is a personal friend of one or more of our friends in the American Colony. The present open friendliness is a great relief. Of course, the Museum is still understaffed; Hamdy is very ill and Edhem Bey is very busy. That may cause some delay in getting antiquities but I think we can count on having the renewal or prolongation inside two months. (*Reisner Diary IV,* 377–78)

15 Following his audience with Hamdi Bey, Schumacher wrote in his journal on June 20, 1908: "Hamdy Bey read our letter carefully and then said: 'You may rest assured that I will see that you receive perfect satisfaction. I will not allow anyone whoever he may be to place obstacles in your way. For twenty six years [i.e., from the beginning of the construction of the National Museum in Istanbul], I have furthered all scientific research in Turkey and you may be sure that I will further yours'" (*Reisner Diary V,* 72; cf. n. 14 above for the corroborating quotation as recorded in *Lyon Diary I,* 80; also *Schumacher Diary I,* 57). Reisner concluded this excursus in his 1910 journal with the text of a confidential letter that he had sent to "Dr. Mackenzie" on August 8, a letter in which he enumerated the complaints against Hasan Bey. In reality, wrote Reisner, "the place as commissionaire was for him [Hasan Bey] simply a means of levying blackmail regardless of the interests of the Imperial Museum or of the expedition." But Hasan Bey apparently had a knack for hanging on to power, for long before, on Sunday afternoon, June 7, 1908, Lyon had met in Beirut with Frederick J. Bliss to seek advice "about the Sebastie situation." In his journal entry later that evening, Lyon wrote that "he thinks it wise that Schumacher and I go to Constantinople to lay the whole case before Hamdy Bey" (*Lyon Diary I,* 69).

as he dined on the Mount of Olives with Mahmud Effendi (Hasan Bey's cousin), Musa Bey (a brother of Hasan Bey), and Ismail Bey (Hasan's uncle) (*Lyon Diary I,* 91). The previous day, Mahmud Effendi had told Lyon that Hasan Bey "will not obey uncles or brothers, is 'cracked', is not to blame [for his bellicose actions] because he is 'not right in his mind'" (*Lyon Diary I,* 90). Following Hasan Bey's dismissal as commissionaire, Schumacher cited other officials who referred to him as "a fool, a man without sense and not at all fit for the position he occupied" (*Schumacher Diary I,* 68). Similarly, in a conversation with Lyon in the reception room of The American Colony Hotel, Ismail Bey described Hasan Bey as "a stupid and headstrong fellow, who might be controlled by kindness"… a man who "illustrates the Arabic proverb of the dog's tail, always crooked, though you might give it a hundred positions" (*Lyon Diary I,* 88–89). As a result of the protracted affair, Lyon devoted a huge segment of his initial journal to the struggle to remove Hasan Bey from office—a struggle that involved the cessation of work at the site and the unplanned trip to Istanbul by Schumacher and Lyon (see *Lyon Diary I,* 60–91).

Despite his constant aggravation with local leaders and apparent disappointment with the current discoveries at the site, Reisner had to steer the 1910 Season to a logical conclusion. Thus, after photographing and producing plans of the architecture that would soon become known as "Ostraca House," workers began removing the perceived floor levels on August 10, 2010. And then it happened …

> Aug. 11. Found first potsherd with Israelite inscription (Reg. No. 3855)[16] in packed layer of dirt (about 20 cm. thick) *along east wall of room 401.* Immediately started sifting the dirt already carried out, but found only a few small uninscribed potsherds. From now on, every basket which might possibly contain ostraca was carefully sifted; but all except a few unimportant fragments were found by the Egyptians before the dirt reached the sifter.
>
> Aug. 12. In room 418 in black surface debris (30 cm. thick), ostraca (Reg. Nos. 3863–3868). In room 414 in the same debris above ledge of foundation wall on west side, the carved ivory dagger-handle (Reg. No. 3862).
>
> Aug. 13. The character of the three layers of debris under the floor of the Ostraca House was clearly recognized….
>
> Aug. 13–18. Rooms 401–423. Removal of subfloor debris of Ostraca House proceeded through rooms 401–423.
>
> Aug. 17–18. Removed all superimposed debris, potsherds, etc., from floor of room 424 … [i.e., the southern Entrance Hall, which survived only in its northeastern corner; after this, the work progressed southward toward the Greek Fort Wall, whose construction had destroyed nearly all of Hall 424].
>
> (*HES I,* 401; italics added)

Besides a total lack of contact between the ostraca and the presumed storerooms of the Ostraca House, a number of curious aspects concerning the S4–S7 inscriptions emerge from this information when combined with the data in Table 5. First, Reisner's own *published* description of the findspot that yielded the first ostracon on August 11 (see n. 16) gives the impression that the fragment came from an accumulation of hard-packed, black occupational debris lying on the floor and along the eastern wall of Room 401. But his daily journal entry for August 1–5 (cited above, p. 107) clearly stated that more than a half meter (50–60 cm) of post-Israelite, dirty yellow, loosely compacted fill

16 See Appendix B: Line 1. This inscription appears as Ostracon No. 4 in the published list of *HES I,* 233 (drawing on 239). In *Reisner Diary V,* 515, Reisner annotated (in black ink) his initial entry (written, as are all his journal records, in blue ink) regarding the discovery of this ostracon by assigning 5855 as the registration number. Ultimately, none of the ostraca received this number (see Appendix A). In the official publication, the number assigned to Ostracon No. 4 is 3855.

dirt lay over the ostensible Ostraca House floors. Moreover, his published diary continues by indicating that Reisner did not remove the makeup beneath the floor of Room 401 until August 13–18. The official ostraca registry actually assigns this bowl rim—the only ostracon originating in 401—to *subfloor* makeup (Appendixes A: Line 4 and B: Line 1; compare *HES I,* 233, no. 4). Reisner's unpublished, handwritten diary entry for August 11 confirms this provenance.

> *Under the floor* of the house of the fourth series (room 401) in dirty yellow debris along east wall (*ca. 40 cm below floor*), a fragment from the rim of a bowl with two lines in black ink on outside of rim.
>
> (*Reisner Diary V,* 515; italics added; for the enumeration of the various "series," see Ch. 3, n. 5).

In this record, the dirty yellow debris extended at least 40 cm *below* the Ostraca House floor. The brokenness of this fragment (resulting in the loss of the inscription's right-hand side and lower portion; *HES I*, 239.4) concurs with a context in subfloor fill rather than in occupational debris on the surface. The fact that at least half the entire corpus of inscriptions suffers from missing text attests to the knock-about life of these fragments after the time of their inking. And all relevant sectional data (CD, GH, Subsidiary Section AB) agree that the so-called "dirty yellow" matrix represents a massive deposit of imported leveling fill whose depth far exceeded the 20 cm mentioned in the August 11 entry above. Rather than coming from a primary archaeological context, then, this initial inscription appears to have derived from a clearly secondary one.

Second, and in similar fashion, a series of six ostraca emerged the following day, Friday, August 12. Contrary to the published report (*HES I,* 62–63), in which Reisner wrote that the Israelite ostraca came from a thick floor with "an accumulation of fine black debris such as is laid down by the occupation of earth-floored courtyards," he recorded the following journal entry on the actual day of discovery:

> The sub-floor deposits in S4 fourth series [i.e., the "Ostraca House" phase; see Ch. 3, n. 5] continue to yield inscribed potsherds. In room 40[ ],[17] in the first 30 cm below floor surface, were found today six potsherds with Hebrew (Canaanitish) inscriptions in black ink.
>
> (*Reisner Diary V,* 516)

In the official excavation report, five of these six inscriptions became Ostraca Nos. 5, 24 (left side), 24 (right side), 44, and 61; in Reisner's private diary, the sixth entry dealt with two additional but illegible fragments that went unpublished but resembled published inscription No. 5.[18] In addition to these pieces, workers recovered a duplicate of the writing discovered already on August 11 and published as No. 4 in *HES I.* Importantly, the official registry (*HES I,* 233–38) assigns only No. 5 to *S4–418 sub,* i.e., to the subfloor makeup, while designating that the remaining fragments came simply from *S4–418.* (I have flagged the appropriate corrections in Appendix A: Column 7 in bracketed, bold type.) One must refrain, therefore, from seeing two distinct types of contexts at play here (packed occupational debris that collected on a floor versus loose imported fill that lay beneath the floor). The matrix involved in these discoveries consisted solely in imported, subfloor makeup. Unfortunately, when read in isolation, the published record might easily encourage one to accept unknowingly certain false stratigraphic

17 While Reisner failed to enter the last digit of the number assigned to the room involved in these discoveries, correlation of the field drawings and registration numbers with those found in *HES I,* 233–43, confirms Room 418 as the correct area.

18 Ostracon No. 4 represented a duplicate of the inscription discovered the previous day, August 11. Workers apparently recovered published Ostracon No. 24 from two separate groups of fragments: two sherds which comprised the left-hand side of the restored inscription, and five (entered in the field notes as three) fragments making up the right-hand side of the writing. In the field notes, all these fragments were designated No. 17 (versus 24) in the sequence. In addition, Reisner's field notes initially identified published inscription No. 44 (Reg. No. 3867) as No. 30 and No. 61 (Reg. No. 3864) as No. 39.

distinctions between primary and secondary findspots and thereby to misunderstand the actual provenance of the writings.

In his early attempts to interpret the aforementioned inscriptions, Reisner recognized that "the words are almost always separated by dots, which facilitates the reading" (*Reisner Diary V,* 521). On the other hand, he also believed that the ostraca contained "a system of dating according to events" (e.g., "In the year of 'the Fortunes,'" "In the year of the … in Ḳaṣah[?]," etc.) rather than actual year formulas.

The third point relates to the manner in which Reisner ultimately recorded the specific S4 findspots. He clearly distinguished between Rooms 417 and 417N, an indicator that various secondary additions ("reconstructions," in Reisner's terminology; see Ch. 2, above) had already occurred inside these once long, corridor-like rooms (see *HES I,* 114, fig. 42, versus *HES II,* Plan 5). This situation may suggest that he understood the ostraca to have belonged, at best, only to the latest phase in the building's functional life, for which it remains difficult to determine a precise date. Without an accurate set of elevations for the disposition of each inscription, however, this suspicion remains untestable. Unfortunately, the published reports did not include this vital information.

Fourth, Room 417, which by far yielded the bulk of the ostraca collection, basically ran directly under the area of two much later streets: Preherodian Street C and Herodian Street Alpha (immediately east of the Atrium House; see *HES II,* Plans 7–8; see the discussion in Ch. 2, pp. 41, 48, and Ch. 3, pp. 67, 86–87). All these features, including the original 417, spanned both Summit Strips 4 and 7. For notice of ostraca finds in the northern extension of 417, see the 1910 Diary in *HES I,* 403: Aug. 20 and 22–23.

Fifth, and finally, the relative discovery dates for the S4 and S7 groups are interesting. While workers recovered the S4 ostraca from August 11–23, the 1910 diary states that they did not encounter the S7 group (located farther north and away from the Ostraca House) until a full month later, September 16–26. Other records, however, indicate that the clearing of Ostraca House rooms occurred *simultaneously* with the excavation of upper debris in S7 (*HES I,* 393, August 1 and 6). In either case, and as shown above, the strip system required that workers dump the excavated debris from Strip 7 back into Strip 4, including the principal discovery area for most of the ostraca. One wonders how many, if any, of the S4 inscriptions might actually have represented misplaced S7 items, retrieved during the backfilling procedure and erroneously assigned to S4, or vice-versa (given the later working dates for Strip 7). The answer depends largely on the actual degree of overlap in the clearance of the two adjacent strips and, at this point, seems as though it will remain forever elusive. But besides allowing for a possible mixing of materials during the excavation process, one must also concede that, in the absence of unbroken floor levels to seal and protect the Iron Age layers, and with such deep, imported fill deposits over much of the area, both intrusive materials from much later periods and derived materials from earlier ones (as well as materials from adjacent tracts of excavation) might easily have entered various contexts containing ostraca fragments already in antiquity.

D. The 1910 Ostraca: Typological Notes
(Appendix A: Col. 4–*HES I Pottery Types*; Table 6)

Reisner related the 75 registered pottery fragments appearing in his ostraca list to 12 ceramic types (e.g., I.3),[19] each with various subtypes (I.3.a; I.3.b; I.3.c; etc.). Table 6 summarizes all the primary typological headings that occur both in the Ostraca Registry (Cols. 1, 2) and in the Pottery Registry for uninscribed sherds recovered from the same stratigraphic contexts as the ostraca themselves (Cols. 1, 4).

In Table 6: Column 2, it becomes immediately apparent that Types I.3 and I.14 (and their various subtypes[20]) account for two-thirds of the entire

19 Appendix A, Line Item 15, did not include a pottery-type designation. Line Item 25 conflated two previously cited types, namely I.14 and I.15.

20 Note also that, in Appendix A, "*I.14" represents Reisner's notation "like Isr. Pott. I 14."

Table 6 Reisner's ceramic typology *vis à vis* the Samaria ostraca.

Reisner's Pottery Type	# of ostraca frags.	% of 75	# of non-ostraca frags.	% of 14
–	1	1.33	2	14.28
I.1	0	0	1	7.14
I.2	7	9.33	0	0
I.3	14	18.66	0	0
I.7	6	8.0	0	0
I.12	0	0	1	7.14
I.13	0	0	1	7.14
I.14	36	48	2	14.3
I.14–15	2	2.7	0	0
I.15	4	5.33	0	0
I.16	0	0	1	7.14
I.17	0	0	1	7.14
I.18	1	1.33	2	14.3
I.19	1	1.33	1	7.14
I.20	1	1.33	0	0
I.22	0	0	1	7.14
II.11	1	1.33	0	0
II.16	1	1.33	0	0
II.17	0	0	1	7.14
TOTAL	75	100	14	100

pottery repertoire associated with the ostraca. Both categories derive from Reisner's group of "ordinary wares." Type I.14, which alone constitutes 48 percent of all ostraca-related registration numbers, generally represents flat-bottomed bowls with straight, flaring sidewalls, red or brown ware with brown or black core, and red wash on the interior and exterior surfaces. Bowls that deviated from these common attributes most often came from Summit Strip 7. For example, Reg. No. 4525 (Appendix A: Line 12) appeared in a drab ware; 4608 (A: 14) showed a grayish drab ware; 4550 (A: 41) was made of yellow drab ware with red wash on the interior only; and 4580 (A: 10) also had yellow drab ware but now with a rounded (Type I.14.m) as opposed to flat bottom.

Within the 15 subtypes of I.14 (labeled *a–o*), Reisner noted that I.14.d dominated the general class (*HES I,* 277). Unfortunately, he listed only three examples of this subtype (Nos. 3843, 3855, and 3863), which sometimes appeared "slightly mis-shapen," before simply writing "etc." Moreover, he failed to include the first entry in the Ostraca Registry, even though it apparently bore an inscription, and he did not present it among the ostraca/pottery drawings and photographs. While the last two items appear in the Pottery Registry (Appendix A: 4–5), neither drawing nor photograph supplemented those passing references. In fact, he presented formal pottery drawings for only *two* members of the dominant I.14 group: 3993 (A: 38, Type I.14.h; *HES I,* 278, fig. 154:5) and 4075 (A: 1, Type I.14.i; *HES I,* 278, fig. 154:6). Such exclusivity, indeed, seems strange, given the prominence of this overall class within the ostraca collection.

Reisner's general group I.3 comprises the second vessel type that holds a significant place among the

ostraca. The Pottery Registry in *HES I* cites only 13 specimens for this class, spread over 9 subtypes (*a–i,* with *a* holding a clear numerical edge), but the Ostraca Registry adds one item to this group (No. 3890, A: 53, Type I.3.a; cf. *HES I,* 237, 277). These large jars generally display a "hard, thin, gray-black ware with gray or pinkish wet-smoothed surface (hardly a slip)" (*HES I,* 277). Reisner drew a comparison to a water jar recovered from Cistern 7 in Summit Strip 1, beneath the Israelite palace (see *HES I,* 289, fig. 165:2a; Ch. 3, pp. 82–83). These manufacturing traits will permit direct comparisons to the hard gray ware spoken of by Kenyon, G. M. Crowfoot, and Holladay (see Section F, below).

Beyond these two principal groups, only Type I.7.a–d merits further comment. The four subtypes take in six registered fragments (see Appendix A: 22, 33–34, and 36). In the Ostraca Registry, Reisner calls four of these items "small jars" (3902, 3903, 3906, 3933), while listing the remaining two as "jugs" (3909, 3932). The Pottery Registry, on the other hand, categorizes them all as small jugs with bulging bodies and a more distinctive reddish-yellow ware (generally more yellowish on the interior). None of these vessels has its profile drawn in section, and only one of them (jug No. 3932) appears in a photograph, itself of poor quality (*HES II,* pl. 55:e.5). Reisner also tagged this inscription as a duplicate of the one in Appendix A: 23, which appears on a vessel he labeled a "jar." It therefore remains difficult to classify these items more precisely. Virtually all of the jug/jar fragments in the ostraca corpus reflect large body sherds without much in the way of diagnostic shapes or surface treatments (cf. Section F.4, below). Still, one can say confidently that bowls dominate the ostraca pottery, with jars representing a smaller but important ceramic presence.

Judging from the ostraca drawings (*HES I,* 239–43), it seems that the morphology of the original vessels from which the inscribable fragments came influenced, at least to some degree, the writers' placement of their inscriptions. In most cases, the first line of the inscription ran parallel and close to the top of the fragment on which it appears. On bowl fragments with rim and sidewall, for example, the writing typically begins very near the rim (for an exception, see Appendix A: 56). Only in A: 5 (Reg. No. 3863) did the lines start at the rim but run perpendicular (versus parallel) to it. On the larger, plain, sidewall fragments from jars or jugs, the writing begins near one edge (chosen as the "top") of the piece, with considerable blank space left on its bottom half below the inscription. On straight-sided, flat-bottomed bowls, the lines of text generally run from the rim of the fragment toward the bottom (see the drawing and photograph of Ostracon No. 1 in Aḥituv 2008: 262). When a bowl had a ring (versus flat) base but no preserved rim, writers favored placing the ring base at the top when inking their notes (A: 39, Reg. No. 4619, *HES I,* fig. 154:13; and A: 50 = Reg. No. 4630, in Aḥituv 2008: 302–303). In other words, whatever prominent feature appeared on a fragment (e.g., rim or ring base) influenced the stance of the sherd in the writer's hand and served as a guide for the initial line of writing. In Appendix A: 9 (Reg. No. 4524), where apparently neither the rim nor a ring base survived, the scribe began writing at the point of inflection from sidewall to bottom and proceeded up the wall. Greater attention to the logistics and subtle mechanics of writing or incising on ceramic fragments of different quality and character might open new insights to specialists in the field of epigraphy.

When pondering matters such as these, one final observation merits consideration. Reisner correctly realized that the texts of both A: 45-no. 3896 and A: 46-no. 3915 (both from Strip 4, Room 417, and from Year 15) duplicated that of A: 47-no. 4616 (Strip 7, Room 772, Year 15?). Moreover, he believed that fragments A: 45 and 46 joined and therefore derived from the same bowl. If true, the writing patterns seem very strange. While the message of no. 46 started at and ran parallel to the rim, as expected, the text on 45 ran down the sidewall from top to bottom, thus making it perpendicular to the flat bottom. The writing even proceeded over the break between the sidewall and base and continued across the flat underside of the bowl. Given these apparent irregularities and what would become a doubly redundant writing on a single bowl, one wonders whether these two sherds actually do belong together and, if so, what purpose they might have served within the Ostraca House's shipping and receiving department. In any event, that A:

48 and A: 49 also came from the same vessel but display different place and personal names may indicate Samaria itself, i.e., the receiving point as opposed to the sending point(s), as the place of writing (compare Yadin 1962: 65 and Aharoni 1962: 68).

E. The 1910 Ostraca-related Pottery: Typological Notes
(Appendix C; Table 6)

When comparing the findspots listed for each ostracon in Appendix A: Column 7 with the provenance data for all Israelite pottery presented in the excavation report, 26 registered ceramic items from Reisner's Functional Group 1 (ordinary wares) and five fragments from Functional Group 2 (finer wares) appear to have shared the same context as one or more of the ostraca. For example, ring stand 545 in pottery entry no. I.12.c (*HES I,* 277) came from the same general locus (*S7–772 sub*) as ostracon bowl no. 3 (Reg. No. 4614) in Appendix A. But whereas the bowl fragment bore an ink inscription, the stand did not. In theory, a comparative ceramic analysis of all such non-inscribed entries in the Israelite pottery repertoire[21] may help tighten the chronology of the group overall and thereby narrow the possible dating of the inscriptions, as Kaufman suggested. (One must allow that such analysis might also widen the group's chronological range and thereby further muddy the waters.)

But of the 31 registered, non-epigraphic sherds that came from the same deposits as the ostraca, Reisner presented only three of them (Dis. Nos. 379, 382, and 545) in official pottery drawings suitable for comparative studies. He also drew a likeness between one additional fragment (Dis. No. 380) and the rounded bowl representing ostracon no. 39 (Reg. No. 4619) in *HES I,* 278, fig. 154:13. Without a professional drawing presenting both the exterior of a vessel and a section of all its surviving parts (rim, sidewalls, base, handles, etc.), the search for reliable, datable parallels remains severely hampered, especially when the fragment or vessel itself is no longer available. So while recording discrepancies as well as a dearth of needed information persist within the published report,[22] Appendix C presents the provenance data germane to the small corpus of non-ostraca pottery that came from the same local layers as the ostraca themselves and that Reisner actually drew and published in his final report. The bottom portion of Appendix C lists eight vessels (Column 1, nos. 1a–8a) that Kaufman himself understood as uninscribed pottery recovered from ostraca-yielding contexts (Kaufman 1966: 115, n. 39). To this meager group he added the two vessels that Reisner described as "wide-mouthed cooking pots" and assigned in a most generic manner to "the floor layer of the Ahab courtyard" (*HES I,* 279, no. 31; fig. 154:19–20). Apparently, this vague reference to Ahab's purported courtyard prompted Kaufman to include these two pots, even though he saw them as relatively useless in the attempt to place the ostraca chronologically ("they provide no certain evidence of their date").

Unlike the situation surrounding the ostraca-bearing pottery, Reisner did present in a professional drawing each of the vessels in Kaufman's list (see Appendix C: Column 4–*HES I Refs.*). But the bracketed numbers in C: Column 6 reveal that, in

21 Two fragments from among the five finer-ware items did, in fact, bear inscriptions of a type different from the ostraca format. On Saturday, June 18, 1910, while Fisher and Bates were inspecting the Roman wall near the western gate, Bates found (on the surface of a field south of the mound and between the Roman Street of Columns and southern perimeter wall) a fragment from a wide, deep tray with a heavy, horizontal rim inscribed with the "very neatly scratched" letters למלכרם, which Reisner read as "To (the property of) Malkirâm" (*Reisner Diary IV,* 418–19; Reg. No. 2854; *HES I,* 243, no. 64; 280, fig. 156:18a; and 281, no. II.18.a). Reisner believed that this sherd had either come from a nearby tomb or washed down to this location from somewhere on the summit. Later, on October 22, 1910, the "rim of a bowl with bent sides" appeared in disturbed yellow debris at the opening of a cave in Strip 11–9, located at the northwestern corner of the summit compound (*Reisner Diary VII,* 651). The letters ליה were incised upside down on the outside of the rim (Reg. No. 4925, *HES I,* 238 and 243, no. 65). Whether these letters (assuming they represent a complete word) have any connection to the architectural term ("garland; wreath") mentioned in 1 Kgs 7:29, 36 remains quite uncertain.

22 E.g., A.1:39, no. 4619, was assigned to *S7–772* in the Ostraca Registry but to *S7–772 sub* in the Pottery Registry. It truly matters whether a fragment rested above or below a floor.

virtually every case, the loci recorded for these items failed to produce a single ostracon. Only Dis. No. 379 (no. 1a in Appendix C)—a wide, flat-bottomed bowl with flaring sidewalls and external ribbing—seems to have shared a findspot with one of the ostraca fragments (Ostracon No. 3 in Appendix A, Reg. No. 4614). Note that in *HES I,* 277, Type I.14.j, Reisner incorrectly assigned the vessel to *S4–772 sub* when it surely belongs to *S7–772 sub* (as shown by the northerly location of Room 772 in *HES II,* Plan 5, Grid E.9; also see my earlier discussion). As a result of these observations, it remains unclear why Kaufman believed these vessels once shared a stratigraphic home with the ostraca. Except for no. 379, their locus designations do not appear anywhere in the Ostraca Registry. In the absence of further clarification, therefore, I must omit them from my already meager comparative study sample.

This unhappy situation leaves only five uninscribed pottery forms (Appendix C: Nos. 1–4, 1a; shaded in C: Column 2) for which drawings exist and which, because of their shared contexts with inscribed sherds, might shed some light on the dating of the ostraca themselves. Such limited study groups are, of course, far too small to inspire confidence in the credibility of one's conclusions. To exacerbate this dilemma, all five vessels now under consideration claim an enigmatic "Dis. No.," not the standard "Reg[istration] No." Although Reisner nowhere explains the former notation, one wonders whether it indicates a "discard number" (see n. 5, above). In terms of general vessel types, the group of five shows a bit more diversity in form (note the so-called tray and ring stand) than the vessels commonly associated with the ostraca (primarily bowls and jars). Whereas four of these items claim affiliation with only one ostracon, the two-handled jar in Appendix C: 3 (no. 542) came from a context that yielded a dozen ostraca. Ironically, however, this form is the only one not presented in a scaled pottery drawing in the official report.

Finally, because of the somewhat surprising dearth of comparative materials, I have added two bowl forms (Reg. Nos. 4527 and 4630) to the non-ostraca pottery sample and have labeled them א and ב at the bottom of Appendix C. These two items appear in the Pottery Registry (*HES I,* 278) under I.18.a and I.20.b, respectively, and each of their descriptions needs the added notation "Ostracon," since the former entry relates to Ostracon No. 40 and the latter one connects to Ostracon No. 50 (see Appendix A). While neither 4527 nor 4630 appears in a drawing in the final report, each of them has a typological mate that does show up in *HES I,* fig. 154:11–12 (for 4527) and 14 (for 4630).[23] I shall use those drawings for comparative purposes. The basis of the ensuing study sample rests on assemblages recovered from the following northern sites, moving from the Samaria region northward: Shechem, Tell el-Far'ah (N), Dothan, Beth-Shean, Ta'anach, Megiddo, Tel Qiri, Yoqne'am, Tell Keisan, Hazor, and Tyre.

F. The 1910 Ostraca and Ostraca-related Pottery: Comparative Ceramic Analysis

In *HES I,* Reisner published four plates of pottery to represent the Israelite period at Samaria. Figures 153–154 displayed 35 vessels belonging to his 'functional group' of *ordinary pottery* (common, utilitarian vessels used in cooking, serving, and storing various foods and liquids), while figures 155–156 illustrated 22 forms from the *finer pottery* (better quality table ware plus ointment and scent vases). Of these four assemblages, figures 154 and 156 constitute the primary focus for a discussion of the ostraca and ostraca-related pottery. (Fig. 153 contributes only one additional form relevant to such a study—a ceramic stand—and fig. 155 did not include any vessels that had emerged alongside the ostraca.) Broadly speaking, items in figure 154 tend toward slightly earlier dates than those seen in figure 156. Because some of this pottery derived from subfloor fills (for which the *terminus post quem* must, by definition, remain open), entries such as 154:19, 20 (tenth century

23 The reason I add Reg. No. 4527 (which relates indirectly to *HES I,* fig. 154:11–12) while rejecting Kaufman's 4116–4117 (which are the actual vessels behind 154:11–12 [see lines 7a–8a in Appendix C]) lies in the fact that 4527 represents a genuine inscription in the Ostraca Registry, while 4116–4117 do not.

BCE), 9, 11, 12, 16 (late tenth to early ninth centuries), and 10 (early ninth century) might well predate the Omride period. On the other hand, several of the finer-ware items in figure 156, such as the Assyrian-style bowls in Nos. 21a-b-c and the wide, flat-bottomed tray in No. 17a (which some analysts call a "frying pan") might easily extend the date of the overall group down to the outset of Assyrian military activity in 733–32 BCE, or to the fall of Samaria between 722–20 BCE, or even into the *pax Assyriaca* of the late eighth and seventh centuries BCE (see Section F.3; also Table 7 in my Summary, below). While exact parallels sometimes prove rather scarce, only the nicely made bowl with flexed, concave sidewalls and outwardly beveled rim (154:1) and the shallow, rounded bowl whose outwardly folded rim was trimmed into an elongated flange (154:18) might carry the earlier group this far down in time.

In any excavation report, bowls always constitute the most diverse and complex class of vessels. In the recent publication from Beth-Shean, A. Mazar lamented that "virtually no two bowls were identical" (Mazar 2006: 324). In such instances, one risks either overanalyzing or underestimating the value of each minute attribute on a given form. Experience led Mazar to seek "a broad typology based on the main features of rim form, stance, and shape, taking into consideration that various shapes are [sometimes] included under the same type." While the different expeditions to Samaria recovered a similarly wide array of bowls, only four distinct types met both criteria of (1) having derived from the same context as one or more of the ostraca, and (2) having appeared in the final report in a profiled drawing (see pl. VI:A, for *HES I,* fig. 154:5, 6, 13, and fig. 156:16a). As noted above, however, Reisner drew three additional bowls (pl. VI:B) that can serve as credible representatives for two more inscription-bearing pottery types: *HES I,* fig. 154: 11–12 (for Ostracon No. 40, Reg. No. 4527, plus 11 other ostraca fragments) and 154:13 (for Ostracon No. 50, Reg. No. 4630). Plate VI also reminds us that these bowls came from four of the 15 disparate findspots listed in Appendix A, namely, *S4-417, S4-417 N sub, S4-418,* and *S7-772* to the north.

1. Epigraphic Pottery (pl. VI:A)

Turning specifically to the inscription-bearing pottery presented in *HES I,* one sees a number of items (fig. 154:2–6, 8–9) that relate to a generic class of bowls of medium to heavy construction with wide, flat bottoms, straight sidewalls that flare to roughly the same degree, and usually with simple, rounded rims. Two members of this class (154:5–6) displayed Ostraca Nos. 38 and 1, respectively. The bottom of No. 5 apparently was either string-cut from the wheel or had two closely-spaced, circular grooves incised into its clay. In her later work, Kenyon classified this general style as "large saucers" (*SS III,* 148–49; fig. 15:1–4) and noted the strong association of this type with the Harvard ostraca (*SS III,* 141, 148). In fact, both Kenyon and G. M. Crowfoot assigned 50 of the ostraca to this single bowl form (*SS III,* 141, 469), a prominence that justifies a close examination of its attributes and dating.

The British report draws a subtle, almost implied distinction between two apparent subgroups of bowls within this overall class. Some examples, such as those presented in *SS III,* fig. 15:1–4 (fig. 47, upper left), show a very wide base and thinner overall construction. Kenyon noted that this style proved somewhat uncommon in the Joint Expedition's work. While in *SS III,* fig. 4:13 represented an earlier, antecedent form with rudimentary ring base, the majority of these bowls came either from S Tomb 103 or from a purported plaster floor in excavation area Qx near the northeastern corner of the summit compound (see *SS III,* xiv–xv). Despite their clean, attractive lines, these bowls generally claimed a coarse, gritty, reddish-gray ware with buff colored slip or buff-gray ware with more reddish slip. The second subset within this general class appears in *SS III,* fig. 13:1–3; these items typically show a narrower, flat base and thicker sidewall construction. Kenyon stated that, unlike the previous group, "these saucers (nos. 1–3) were the commonest kind of bowl during the Israelite period at Samaria" (*SS III,* 141). She added that, while they rarely occurred in the earlier Periods I–II, these bowls became common by Period IV and persisted through Period VI,

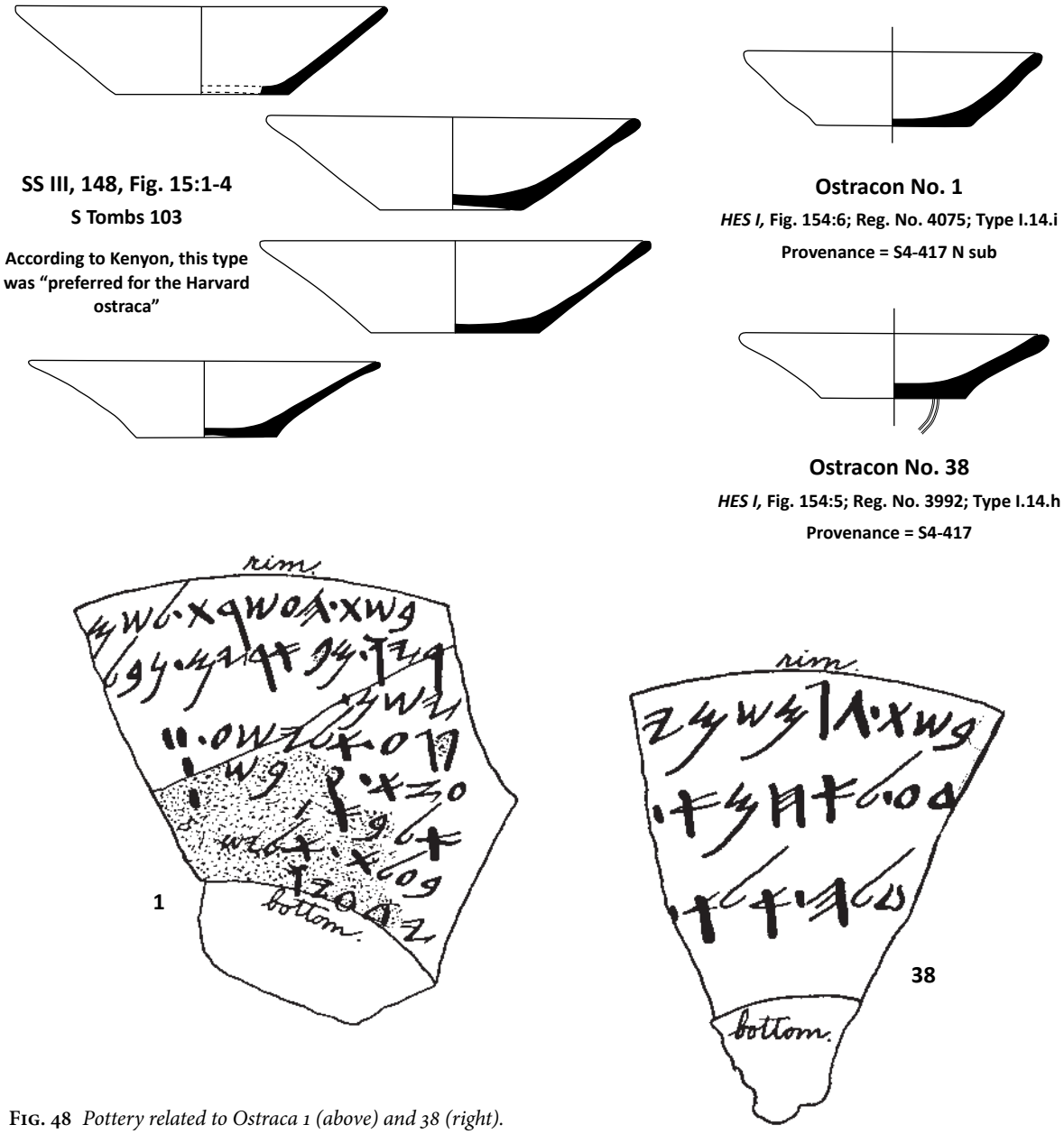


Fig. 48 *Pottery related to Ostraca 1 (above) and 38 (right).*

"after which they seemed to go out of use." Potters made all these vessels from "buff" ware that often showed a reddish hue (more like the pottery from S-7 than S-4), and although the clay contained an appreciable quantity of gritty inclusions in the earlier phases (Periods II–IV), it was much better levigated as time passed (Period VI). As the preparation of the clay improved, the early pattern of putting a reddish slip over the entire bowl, including its base, changed to placing it only on the interior and over the rim to the break in the upper sidewall. This diagnostic change might have helped to refine the dating of the Harvard bowls. But although in his Pottery Registry Reisner frequently recorded a "red wash" for his straight-sided bowls, he failed to note either the precise location of the wash (slip) or any changes in the application pattern over the series of vessels that he recovered.

Kenyon observed that the bowl in *SS III,* fig. 15:1, provided a nearly identical parallel for the ex-

emplar in *HES I,* fig. 154:6, which accommodated Ostracon No. 1 (see fig. 48). This vessel (15:1), and many similar ones from this general class (eg., *SS III,* fig. 13:2–3), came from S Tomb 103, a complex of Israelite burial chambers situated west of the Roman stadium on the far northeastern slope of the site, roughly 600 m from the Ostraca House (*SS I,* pl. I; *SS III,* 21–22, fig. 10). The northernmost chamber of this structure contained the remains of four individuals (three adults and one child). In the larger antechamber, six holes penetrated the floor and led to bottle-shaped pits hewn out of the underlying rock. These pits contained sundry small objects in metal, stone, and bone, as well as plentiful pottery[24] groups that included numerous examples of these ostraca-related saucers. Because many of the bowls had apparently been broken intentionally while the accompanying objects were better preserved, E. L. Sukenik understood the pits as "receptacles of offerings connected with the cult of the dead . . . " (*SS III,* 22). Although the pits themselves contained no human remains, they did hold the bones of an ass. This fact prompted the excavators to recall the biblical notice of the high price paid for donkey meat during a great famine in Samaria (2 Kgs 6:25) and to extrapolate from the tomb evidence that this animal also provided food (even for funerary offerings) during normal times. Whether or not the straight-sided bowls contained such gifts, the fact that they were subsequently smashed, and the fact that elsewhere contemporaneous donkey caravaneers used fragments from identical bowls as disposable notepads (~ ostraca), indicates that this generic ceramic style accrued no value in and of itself.

Although S Tomb 103 remained stratigraphically disconnected from the summit excavations, Kenyon correlated its remains with her Period V, the somewhat spurious but in any event brief period that fell between IV and VI and that dated somewhere in the mid eighth century BCE. Elsewhere in the official report, however, she opted for the narrower opinion that "the whole group is from Period IV and not later" (*SS III,* 470). As noted earlier, Grace Crowfoot slightly expanded the chronological range of the ostraca pottery, including these straight-sided bowls that figured most prominently among the epigraphic material, to span Periods IV and V. When assessing the clay used to manufacture these bowls, J. W. Crowfoot placed the transition from a hard reddish-brown ware to the so-called "dark warm buff with grits" in Period III (late ninth century). But *SS III,* fig. 3:3, shows that this form occurred in the "soft buff ware with some grits" already in Period II.[25] At Hazor, suitable parallels appeared from Stratum VII on (late ninth century BCE) and became the most common type in Strata VI–V;[26] they gradually disappeared by Stratum IV.[27] G. E. Wright believed that this bowl tradition "came into fairly common use in the second half of the 9th century" (Wright 1959:27) but that it actually began slightly earlier. As evidence, however, he cited *Hazor I,* pl. XLIX:10, mistakenly attributing it to Stratum VIII rather than to VII as in the report. Holladay adopted Wright's earlier *terminus post quem* (ca. 870–840 BCE) for *SS III,* fig. 3:3, but failed to offer any corroborating parallels (Holladay 1966: fig. 6:I). Further witnesses, in *SS III,* fig. 6:1–2 (Period IV), offer nice parallels for *HES I,* fig. 154:5–6 and 8 (see the full discussion in *AIS II,* 70–71).

Thus the straight-sided, flat-bottomed bowl, which accounts for so many of the ostraca, seems to have appeared at Samaria sometime during the last half of the ninth century and to have concentrated in the early-to-mid eighth century BCE and

24 *SS III,* fig. 13:2, came from S Tomb 103 Pit 2, while fig. 15:3 was found in Pit 1. Kenyon did not specify the particular pit associated with the bowl in fig. 15:1.

25 The bowl came from Segment 120.121.19.126, Layer XIIy.z, which the field notes equate with foundation trench fill for Period III Wall 160 (see *AIS I,* 105–6).

26 *Hazor I,* 20; pls. XLIX:10 (VII); LI:12 (VI); LIII:11–12 (V); LXXI:6 (V), with low ring base; LXXIV:7 (V); for the stylistic range of the class in general, see LXVI:14–15 (V); *Hazor II,* pls. LXIII:10 (VII); LXXIX:12 (VB); LXXX:39–40 (VA); LXXXI:1–8, 11, 13; XCII:20 (VA); XCVIII:18 (IV); *Hazor III–IV,* pls. CLXXX:4 (VII); CLXXXIX:2 (VI–V), with sidewalls that flare a bit more than Samaria No. 5 and an outwardly beveled rim mode similar to Samaria No. 1; CCXXII:15 (VA). Examples from Stratum VI move quickly toward a more sharply down-cut (beveled) rim rather than the more rounded earlier variety.

27 *Hazor II,* pl. XCVIII:17–18.

perhaps even slightly later. The comparative evidence seems to agree. The conspicuous absence of this bowl type from the well dated Ta'anach IIA-B levels in the late eleventh and tenth centuries BCE, followed by its attestation elsewhere in the Jezreel Valley at Yoqne'am Stratum 4 (Ben-Tor et al. 1979: 82–83, fig. 8:1–2),[28] help corroborate its beginning date.[29] At nearby Tell el-Far'ah, the form appears in Niveau 2/Stratum VIId from the late ninth and eighth centuries,[30] a range that accords well with the chronological distribution of Bowl Type 51 at Beth-Shean.[31] At Megiddo, this type emerges for the first time in Level H-3 (= Chicago Stratum IVA)[32] and it seems most common in Strata VI–V at Hazor (see Bowl Type III in *Hazor VI,* 442–43, fig. 6.4:1–10). But herein lies a complication for fine-tuning the *latest* possible date of the ostraca written on fragments of this bowl type. Hazor VI appears to have ended in the earthquake around 760 BCE (cf. Am 1:1), while Level V fell during the conquests of Tiglath-Pileser in 732 BCE (Yadin 1972: 181, 190, 200; *Hazor VI,* 3). Depending on one's dating of a locally-made Assyrian bottle found in Megiddo Level H-3, this stratum *ended* either in 732 BCE or perhaps later—during or slightly after the fall of Samaria (see the discussion in *Megiddo III,* 319–22). While the former option seems more likely, exemplars in *Megiddo I,* pl. 24:40–41, ranging from Stratum IV to II,[33] represent the later end of the chronological spectrum for this form.

In sum, the widely flaring, straight sides on the saucer-bowl most often associated with the Harvard ostraca provided the writers with a quite practical writing surface. This vessel type certainly flourished during the first half of the eighth century BCE, but comparative data preclude restricting its life to this period. Rather, the style continued at least into the third quarter of that century and perhaps later still.

The ostraca writers also used a second class of bowls, presented in *HES I,* fig. 154:13 (fig. 49; also pl. I: Ostracon No. 39), on which to record their shipping dockets, even though these forms now display a rounded sidewall and ring base—features that would seem less practicable to someone using a stylus and ink. Perhaps for this reason, this vessel form does not hold as prominent a place as the straight-sided bowl within the ostraca collection. Still, a limited number of writings occur on this form (compare Ostraca Nos. 26?, 29, 39, 40, 50, 60). Reisner described this vessel as a "hollow bowl with ring foot," and similarly rounded bowls also appeared with round bottoms (e.g., *HES I,* fig. 154:16). But the Harvard report confused one important detail relating to the provenance of this bowl: while the Ostraca Registry assigns it to *S7-772,* the Pottery Registry places it in *S7-772 sub,* nomenclature that suggests workers found the piece *beneath* the purported floor levels of Room 772, located 20 m north of the Ostraca House remains. Morphologically speaking, a nearly perfect parallel for this bowl appears in *Megiddo II,* pl. 25:68 (photo, pl. 59:68), attested in Strata IV and III. The ware and wash on the Megiddo example may differ from that seen on the Samaria sample

28 In Ben-Tor's later revision of the stratum numbers, Stratum 4 became Stratum 10 = ninth? and eighth centuries BCE (Ben-Tor et al. 1983: 31).

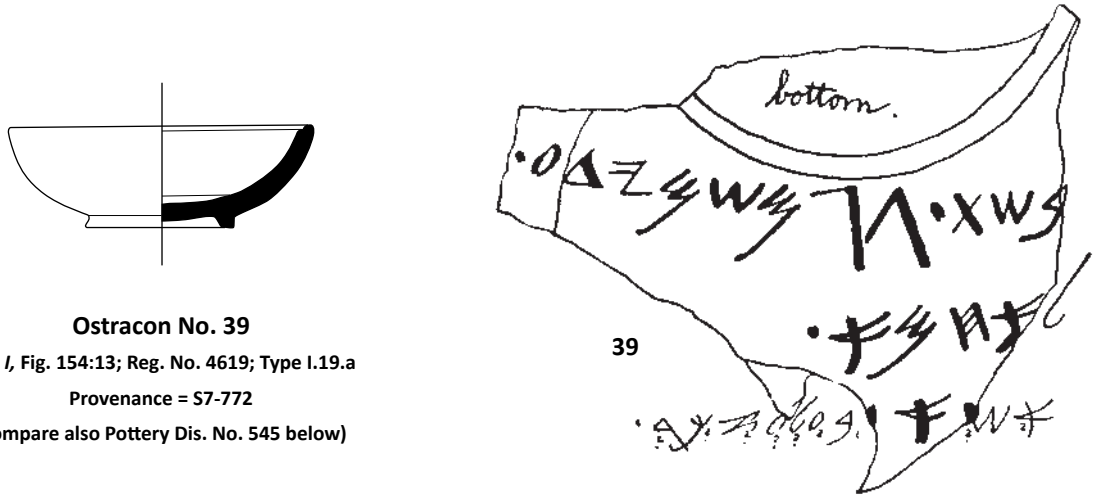
29 The example in *Hazor I,* pl. XLV:9 (X–IX), with its pinkish-colored clay and brown slip, may even slightly raise this date of inception (see the discussion in *Hazor I,* 10–11).

30 de Vaux 1951: 414–15, fig. 11:4–5, 10; de Vaux (1951: 429) attributed the construction of niv. 2 to either Jehoash or Jeroboam II. See also de Vaux 1952: 568–69, fig. 8:10 (niv. 2) and Chambon 1984: pl. 57:14–23 (niv. VIId).

31 A. Mazar 2006: 324–25, Table 12.6 and fig. 12; also pls. 19:5 (P-8 and 8a), 23:15 (P-8' and 7b), and 28:19 (P-7, Building 28636). Curiously, Beth-Shean seems to have existed without city fortifications during this period (2006: 36), although construction activities during the Roman period may have cut away the edge of the mound (2006: 49ff., 269ff.). James noted this basic type as "the second most common bowl in Beisan IV," which she extended down to 700 BCE (James 1966: 126, 132). In the light of his more recent work, Mazar (2006: 13) now terminates Level IV at 732 BCE.

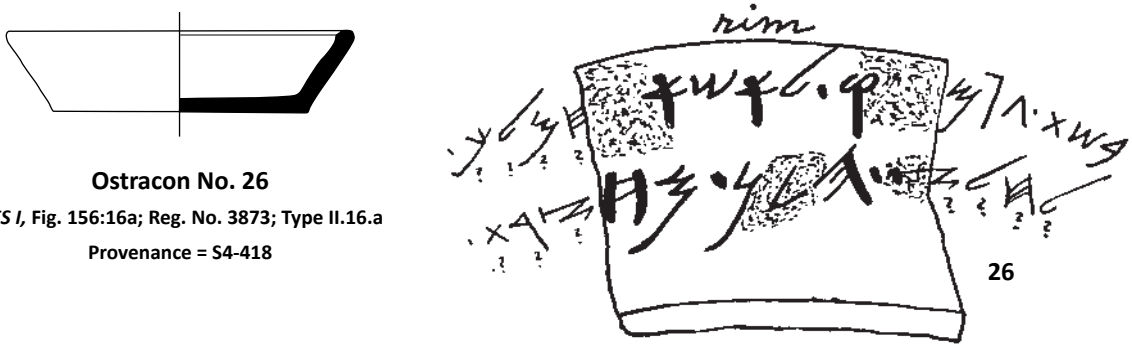
32 *Megiddo III,* fig. 11.43:4, 10?, 11, 14; cf. also nos. 1 and 7, with variant bases.

33 Five examples derive from Stratum IV, 14 from III, and five from II. Note that the fierce conflagration that destroyed Building 28636 at Beth-Shean (see n. 31, above) might also reflect Assyrian activity around 732 BCE.



Ostracon No. 39
***HES I,* Fig. 154:13; Reg. No. 4619; Type I.19.a**
Provenance = S7-772
(compare also Pottery Dis. No. 545 below)

Fig. 49 *Pottery related to Ostracon 39 (*HES I, *fig. 154:13; Reg. No. 4619; Type I.19.a; provenance =* S7–772*; compare also Pottery Dis. No. 545 below).*



Ostracon No. 26
***HES I,* Fig. 156:16a; Reg. No. 3873; Type II.16.a**
Provenance = S4-418

Fig. 50 *Pottery related to Ostracon 26 (*HES I, *fig. 156:16a; Reg. No. 3873; Type II.16.a; provenance =* S4–418*).*

(yellow, with brown ochre wash on interior and over the rim versus reddish-brown with red wash), however, a more recent report from Megiddo (*Megiddo III,* fig. 11.55:10) describes this bowl as having red slip and burnishing, traits that agree more with the Samaria specimen. A similar bowl came from Level V at Hazor (*Hazor I,* LXIII:2), a stratum that Tiglath-Pileser destroyed in 732 BCE. So while satisfactory parallels for this innocuous-looking bowl remain somewhat elusive, overall the form seems to fall between the tenth-century ancestral types seen in Ta'anach IIB (*Ta'anach,* fig. 65:1) and the slightly thinner series of rounded bowls with simple, rounded rims well-attested in the seventh century at Tell Keisan niv. 5 (*Keisan,* pl. 41:11b). The Megiddo bowl represents the best overall comparison and, coupled with a few possible parallels from contemporary Hazor, recommends a date compatible with the general range of the straight-sided variety. Although the Samaria bowl design may well have extended beyond the mid-eighth century BCE, comparative evidence proves too scanty to allow firm conclusions or to use this entry as a determining factor in dating the ostraca.

A similar situation attends the wide bowl with wide, flat base and short, flaring (Reisner said "upright"), strongly inflected sides in *HES I,* fig. 156:16a (fig. 50), on which Ostracon No. 26 ap-

peared. Judging from the drawing of the inscription (*HES I,* 240), the writing seems to have been placed on the inside surface of one of the sidewalls. Since a dull, burnished red wash (or slip) covered the brown-colored ware, Reisner listed this item among the "fine wares" of functional group two. Because of the burnishing, which must have been fairly continuous, the ink of the inscription did not adhere well to the surface, a fact that made the writing "very difficult to read" (*HES I,* 235).[34] Workers recovered this bowl from Summit Strip 4, Room 418, where it represented one of nine ostraca retrieved from this area. (One additional writing, Ostracon No. 5, came from *418 sub,* i.e., apparently from beneath the perceived floors of this room.) So besides Ostracon No. 4, which came from subfloor fills below Room 401, these nine ostraca comprise the group of writings lying nearest (but still east of) the principal entrance halls and storerooms of the Ostraca House proper (see Ch. 2, fig. 26 and Table 1).

Suitable parallels for 156:16a appeared in Strata IV–II at Megiddo (*Megiddo I,* pls. 24:49 [III]; 26:81 [IV–II]). Finkelstein included the latter bowl among the Stratum IVA material recovered from reliable loci by the Chicago expedition (*Megiddo III,* fig. 11.55:16). Megiddo IVA now seems to include the ninth century BCE and to continue down to the Assyrian destruction of 732 in the eighth century. Generally speaking, its material correlates well with that from Hazor VII through V. In the renewed excavations at Megiddo, Stratum H-3 corresponds to the latter part of this period and seems associated more specifically with Hazor VI–V. As noted above, these Hazor levels, like Megiddo H-3, extend down at least to the Assyrian activity in the second half of the eighth century BCE. Whether the Assyrians or the survivors of Megiddo IVA/H-3 constructed their new city of Stratum III soon after 732 or slightly later, following the fall of Samaria, remains open to discussion. Since a long-standing, basic principle of archaeological dating, espoused especially by W. F. Albright (1943: 2, n. 1), holds that most intact or reparable pottery recovered from a site belongs to the final phase of the stratum that yielded it, material from Megiddo IVA/H-3 likely extends into the second half of the eighth century. If it does, one might easily infer that Samaria bowl 156:16a likewise derives either from the early eighth century BCE (where most epigraphists today suggest placing the ostraca) or somewhat later, perhaps into the 730s or 720s.

While Reisner noted the presence of wash/slip and burnishing on this particular bowl, he made no comment on the exact pattern of the treatment or on any other decorative motif. In the absence of clearer detail one cannot be certain, but the rather heavy form in 156:16a seems to precede a thinner, somewhat more graceful flat dish that some analysts have included in the class of Samaria Ware. The latter bowls, which do not exhibit as sharp an angle on their bottom edges, appear in Hazor Level V, Tell 'Amal Niveau II, and Beth-Shean in the second half of the eighth century BCE.[35] Long ago, G. E. Wright (1959: 23–24) drew a distinction between thick and thin varieties of Samaria Ware and proposed to date the former group to the mid- to late ninth century BCE (although it likely began slightly earlier[36]) while placing the latter one in the eighth century. (This style, too, appeared earlier than Wright intimated.[37]) He called the heavier, harder bowls of coarser ware and dark

34 Early on, Albright (1936: 214) wrote that semi-continuous wheel burnishing on both the interior and exterior of a vessel "died out well before the end of the eighth century, probably before the middle; it is very common all over Palestine during the ninth and early eighth century…." This burnishing technique appeared on Ostracon C 1101, the so-called "Barley Ostracon" discovered in E.207 (*SS III,* 11–16, pl. I:1) by the Joint Expedition in 1932.

35 Compare *SS III,* fig. 9:2 (Period VI); *Hazor I,* pls. LIV:6–7 (V); LXVII:24 (V); for Tel 'Amal, see Levy and Edelstein 1972: 344–45, fig. 7:2 (niv. II–I, which remain difficult to date precisely, although II generally belongs in the late eighth century while I probably extends into the early seventh century BCE; see p. 328); for Beth-Shean, see James 1966, fig. 67:13 (Level IV). Bikai (1978: 28) classified similar bowls at Tyre as *Fine Ware Plate 6* (1978: pl. XV:10), and 71 percent of the attested sample came from Stratum IV, which extended down close to the Assyrian activity of the 730s. *Fine Ware Plates 1–2,* with flaring rims and flat bases, generally ranged from Stratum III to I at Tyre, i.e., from ca. 732 to 700 BCE (compare Bikai 1978: pls. I:1–2; XI:4, 8, 13).

36 Wright 1959: 24; see also *Hazor I,* 10.

37 See *Hazor I,* pl. XLIX: 23, from Stratum VII in the ninth century BCE.

red or reddish-brown slip *Samaria Ware A*. The thinner, more brightly slipped bowls he referred to, in turn, as *Samaria Ware B*.[38] Both types, however, generally differ from *HES I,* fig. 156:16a, in that they display more rounded bottoms and carinated sides in addition to a thinner overall construction. Today, more than 50 years later, confusion continues to surround not only the rubrics *A* and *B* but even the aptness of the broader term "Samaria Ware." Some specialists no longer use these categories (e.g., A. Mazar 2006: 327). In any event, I have previously suggested including in the Samaria Ware family the shallow, wide bowls with flat bases and slightly bowed sidewalls that incline outward, such as the example in *SS III,* fig. 18:8 (common in E 207, Period VI; see *AIS II,* 269). This bowl certainly seems akin to the clear Samaria Ware specimen (albeit with minute ring base) seen in *SS III,* fig. 19:3. If this association holds up, it seems possible that similar though thicker forms such as *HES I,* fig. 156:16a, might represent precursory ones, especially since 156:16a was slipped and burnished to the point that the ink did not take to the clay as well as in the coarser wares.

If 156:16a bears any connection to the Samaria Ware tradition, even as a precursor to that family, then its thicker construction would argue for an earlier date, probably in the early eighth century BCE. Admittedly, however, this entire discussion must remain speculative in nature, for one fact seems certain: whatever course the classification of purported "Samaria" Ware takes in future research, with regard not only to morphology but also place of origin (Phoenicia or Cyprus), it can only proceed based on direct handling of the vessels themselves; decision-making that relies solely on drawings and narrative descriptions proffered in excavation reports remains both risky and inadequate.

To sum up, until more information becomes available regarding the nature of the ware and the precise surface treatment of the flat bowl in *HES I,* fig. 156:16a, the best morphological parallel remains the example from Megiddo IV. Since the latter item appears to come from a reliable context, one must allow that the lifespan of the Samaria bowl might also have extended beyond the mid-eighth century BCE.[39]

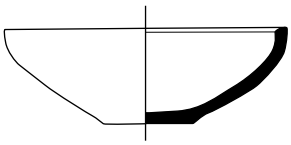
2. Bowl Types Representing Parallels for Ostraca Pottery Forms (pl. VI:B)

As noted above, I have selected three additional bowl forms that Reisner described and drew to represent other, undrawn bowls used for a number of inscriptions. The first two items (*HES I,* fig. 154:11–12 [fig. 51]; possibly also No. 10, Type I.17.a, with a nice parallel in *SS III,* fig. 1:5, Period I) appear as typological mates for the fragment that bore Ostracon No. 40, found with 11 other inscriptions considerably to the northeast of the Ostraca House in *S7-772* (see pl. VI). They exemplify closely related variants[40] within a general class of medium-sized, deep bowls with flat but slightly pronounced bottoms, smoothly rounded, completely non-angular sides, and simple, rounded rims that sometimes show a slight inward beveling. At Shechem, rounded bowls presented one of three rim styles, labeled by Holladay as simple or bulbous, incut, or outcut (see Holladay 1966: figs. 20, 21, and 22, respectively; for the first group, see also the discussion in *AIS I,* 175–78). Except for a few examples (which he placed in his 860 BCE *Horizon*), Holladay assigned these three groups to his 810–760/740 BCE *Period* (with a ceramic period reflecting a greater range of dates than a ceramic horizon). In fact, he restricted the incut exemplars, which most resemble the Samaria bowls under consideration, to this late ninth to early eighth century era: "there are no examples from any other period" (1966: 197). Many of these specimens (e.g., *Hazor II,* pl. LXVI:14 from Stratum VI, *terminus post quem* 760 BCE

38 See the full discussion with references in *AIS II,* 268–75. In his discussion of Bowl Type 55 at Beth-Shean, A. Mazar (2006: 327) inadvertently switches these two titles.

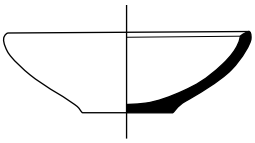
39 Compare the slightly thinner example with clear Samaria Ware traits in *Keisan,* pl. 40:12a–d (niv. 5, from after the fall of Samaria to Assyria).

40 The upper sidewall of the slightly deeper bowl in 154:11 (Reisner's Type I.18.c) gently curves upward more than the upper wall on the shallower form in 154:12 (Type I.18.b).



Representing Pottery Reg. No. 4527, Ostracon No. 40

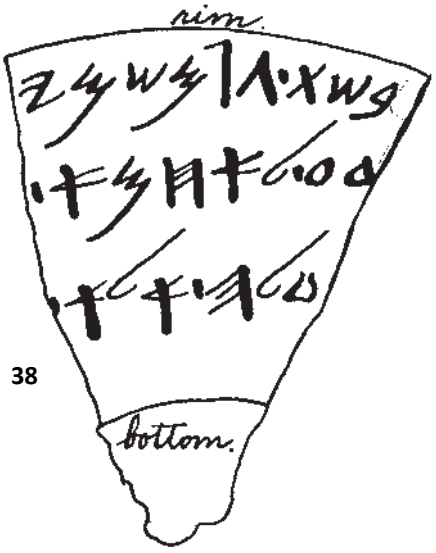
***HES I,* Fig. 154:11; Reg. No. 4116; Type I.18.c**



Representing Pottery Reg. No. 4527, Ostracon No. 40

***HES I,* Fig. 154:12; Reg. No. 4117; Type I.18.b**

Provenance = S7-772 = Ostraca Nos. 7, 9, 11, 12, 14, 15, 39, 40, 47, 52, 56, 60



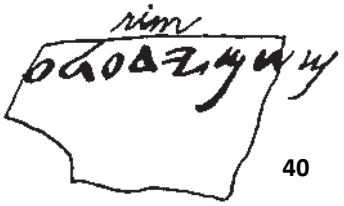
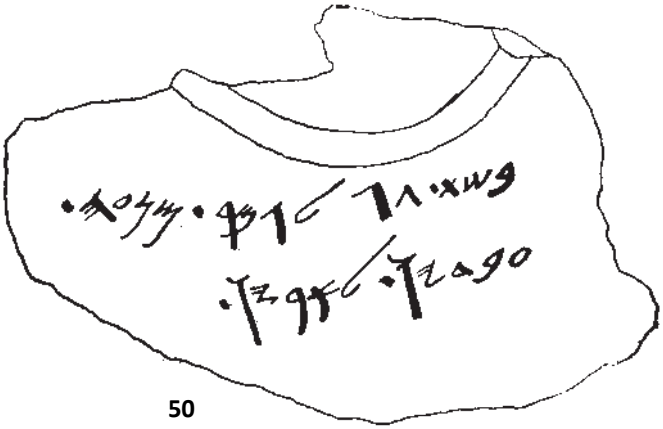
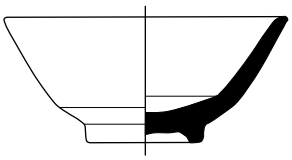


Fig. 51 *Pottery related to Ostraca 38 and 40 (left bowl: representing Pottery Reg. No. 4527, Ostracon No. 40;* HES I, *fig. 154:11; Reg. No. 4116; Type I.18.c; right bowl: representing Pottery Reg. No. 4527, Ostracon No. 40;* HES I, *fig. 154:12; Reg. No. 4117; Type I.18.b; provenance* S7–772 *= Ostraca Nos. 7, 9, 11, 12, 14, 15, 39, 40, 47, 52, 56, 60).*





Representing Pottery Reg. No. 4630, Ostracon No. 50

***HES I,* Fig. 154:14; Reg. No. 3860; Type I.20.a**

Ostracon No. 50 = Provenance S7-772 N

Fig. 52 *Pottery related to Ostracon 50 (representing Pottery Reg. No. 4630, Ostracon No. 50;* HES I, *fig. 154:14; Reg. No. 3860; Type I.20.a; Ostracon No. 50 = provenance* S7–772 N*).*

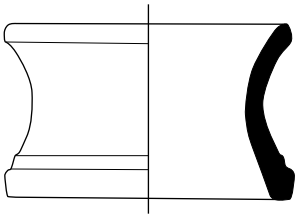
earthquake), however, appear slightly shallower than their deeper predecessors such as the Samaria bowls. Evidence from Tell Keisan seems to agree: while the general class appeared there already in the tenth-century niv. 9a,b-8 (*Keisan,* pl. 55:9, 11–14, with more break in the upper walls; 66:4, 8), the ninth-century descendants in niv. 7 tended to become somewhat shallower (pl. 52:7). Megiddo, Ta'anach, Tell el-Far'ah, Hazor, and possibly Beth-Shean provide other suggestive early parallels,[41] as do *SS III,* figs. 1:5 (I) and 4:3 (III) from Samaria itself. But if Kenyon's proposed correlation of S Tomb 103 with her Period V proves correct, then *SS III,* fig. 13:6, would also suggest that the basic shape enjoyed a long functional life and that the class of rounded bowls in Reisner's group I.18 might easily fit into the early eighth century BCE.

Another form that can stand in for one of the inscription-bearing bowls (Ostracon No. 50, from excavation area *S7-772 N*) consists of *HES I,* fig. 154:14 (fig. 52), which represents Reisner's general Type I.20, "flaring bowl with ring foot." This form appeared in drab ware and apparently sometimes with a red wash, and although Reisner did not record the latter attribute for this specific example (Reg. No. 3860), he indicated that the ostraca bowl it represents (Reg. No. 4630) did, in fact, display the slip. With its open form, thick walls with low point of inflection, and simple to slightly tapered rim, this bowl resembles certain aspects of the item presented in *SS III,* fig. 3:2 (Period II), albeit with a lower ring base (versus "foot") and less inflected lower sidewalls. Perhaps the best form parallels come from Megiddo Strata IV–II,[42] even though these bowls often appeared in lighter-colored clay and showed signs of wheel-burnishing. From the still enigmatic transition from Megiddo IV to III, sometime around 732 BCE or later, compare *Megiddo III,* fig. 11.59:2 (Level H-2). A similar series of bowls comes from Hazor Stratum Va, usually with flat or low disc-like bases (*Hazor II,* pl. LXXX:20–23 [no. 18 showing a rudimentary ring base]; compare other, more oblique parallels in Amiran 1969: pl. 64:2–4), which may stem from earlier, tenth- to ninth-century traditions (compare *Hazor III–IV,* pl. CLXXVII:3 [X–IX]; at Tell el-Far'ah, see Chambon 1984: pl. 57:8 [niv. VIIb]). While recognizing that Mazar sometimes combined different shapes under the same type (A. Mazar 2006: 324), this form may relate to several examples incorporated into his Bowl Type 57 at Beth-Shean, which seems to have appeared by the mid-ninth century BCE (Stratum S-1), gained popularity in the first half of the eighth century (Stratum P-8), and reached its peak of abundance during the second half of the eighth century down to 732 BCE.[43] Also from Beth-Shean, one might compare other examples published by James (1966: fig. 22:13, from Block B-6, whose precise dating remains difficult; 39:6, from Level IV, Block B-5; and especially fig. 63:7, 12, Upper Level V). In the south at Lachish, excavators recovered a similar series of bowls, which they associated with Level IV (probably early eighth century BCE, for it seems certain that these items predated Level III; Zimhoni 1997: 141, 149; fig. 3.62:1–4). These comparisons remove any difficulty from placing the Samaria bowl in the eighth century BCE, and probably in the first half of that period, though it may have extended a bit farther down in time. Thus far, then, the inscription-bearing pottery seems to hover around the period 800–732 BCE.

41 *Megiddo I,* pl. 30:117 (Stratum Vb; see *Megiddo III,* fig. 11.24:3); *Ta'anach,* pl. 64:2 (Period IIB); de Vaux 1955: fig. 16:6 (niv. 3, tenth to early ninth centuries BCE); *Hazor III–IV,* pl. CCVIII: 10, 13, 14, 15, 18, 19, 22 (IX, with slight base variations); A. Mazar 2006: 404–405, pl. 9:6 (Stratum S-1a, late tenth to mid-ninth century BCE). This general style seems similar to Plate 11 at Tyre, which appears to have enjoyed a long life but to have concentrated in the late tenth and first half of the ninth centuries BCE (Bikai 1978: 24–25).

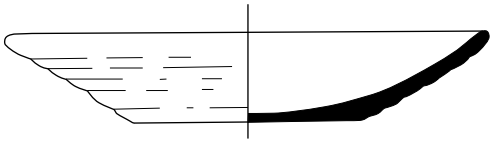
42 *Megiddo I,* pl. 24:48 (also presented in *Megiddo III,* fig. 11.55:7, where Finkelstein assigns it to Stratum IVa). This basic shape shows a lower point of inflection than witnessed on an earlier, similar form with ring base in pl. 28:98 (Stratum V); it also appeared in a wider version during the mid- to late eighth century (*Megiddo III,* fig. 11.43:12, Level H-3; at Samaria, compare *SS III,* fig. 10:4, from Period VI).

43 See A. Mazar 2006: pls. 19:2 (Stratum P-8); 28:16–18 and 42:10–11 (Stratum P-7). Interestingly, Mazar noted that in his excavations at Beth-Shean these bowls typically displayed either flat or slightly convex, shallow disc bases, not ring or foot bases (p. 331).



Pottery Dis. No. 545
***HES I,* Fig. 153:13; Type I.12.c**
Ostracon No. 3 = Provenance S7-772 sub

Fig. 53 *Pottery Dis. No. 545 (*HES I, *fig. 153:13; Type I.12.c; Ostracon No. 3 = provenance* S7–772 sub*).*



Pottery Dis. No. 379
***HES I,* Fig. 154:7; Type I.14.j**
Ostracon No. 3 = Provenance S7-772 sub

Fig. 54 *Pottery Dis. No. 379 (*HES I, *fig. 154:7; Type I.14.j; Ostracon No. 3 = provenance* S7–772 sub*).*

3. Non-epigraphic Pottery from the Same Findspots as the Ostraca (pl. VI:C)

Most of the associated but non-epigraphic pottery reflects different vessel types (trays, ring-stands, etc.) from those typically used for ostraca (bowls and jars). Reisner described and drew three pottery forms that came from the same context as one or more of the ostraca, even though this pottery itself showed no signs of writing. I need only determine a general date for these pieces as a means of testing the chronological range of the ink-written pottery described in detail above.

The first type consists in a ceramic stand (*HES I,* fig. 153:13; see my fig. 53), which represents a meager number of similar forms that emerged during the excavations at Samaria by Harvard (153:13–15) and the British (*SS III,* 184–85; figs. 11:35 [Period VII], 28:6–11 [three from E 207 and three from area Zd]). Kenyon wrote that such stands "were no doubt used to support jars and other vessels with rounded or pointed bases" (*SS III,* 185). More recently, however, some analysts have begun to question the precise purpose of these forms, or at least to leave their function somewhat open (e.g., A. Mazar 2006: 375), since most sites yield in the extreme a disproportionately high number of store jars and other vessels with rounded or pointed bottoms relative to the number of so-called "stands" to hold them upright. Despite their numerical scarcity, purported stands have appeared in levels ranging at least from the Bronze Age to the Hellenistic period.[44] But their low quantitative representation and diversity of forms—from wide, tall, cylindrical or box-like shapes (sometimes even with handles)[45] to narrow, squat, bi-conical designs[46]—make them difficult to categorize and compromise their usefulness as reliable chronological indicators. Still, several distinct styles seem to have existed in northern Israel during the ninth through the early seventh centuries BCE. One design, perhaps the earliest in the series, displays relatively thick, concave sidewalls

44 Compare *Hazor III–IV,* fig. III.2:12 (XX = EB IIIA); *Megiddo II,* pls. 47:16?, 17 (Stratum X); 55:17–18 (IX); 62:11 (with handles), 12 (VIII); 67:6 (VIIB); 70:13–14 (VIIA); et passim; from Tyre, see Bikai 1978: pl. LI:2 (Stratum XVII); and James 1966: fig. 53:14 (Level VI); *Hazor III–IV,* pls. CCLX:14–16 (Stratum 3); CCLXXVI:5 (Stratum 1B); on the late end, see *Keisan,* pl. 10:8, 11 (niv. 2); *Hazor III–IV,* pls. CCLX:14–16 (Stratum 3); CCLXXVI:5 (Stratum 1B).

45 Compare *SS III,* fig. 28:6, 8 (E 207 = Period VI) and, at Beth-Shean, A. Mazar 2006: pl. 20:5 (Stratum P-8, late ninth to early eighth centuries BCE) and James 1966: fig. 26:16; also *Hazor I,* pl. LXXXII:15–17 (Level III, possibly II [see p. 62]). For a rather peculiar cylindrical form with straighter walls that flare at the very top and one upright, knob-shaped handle (probably to aid in supporting the larger vessel placed on the stand), see *Hazor II,* pl. LXII:4–8 (Level VIII or VII; see p. 14). Finkelstein presents a similar form (though without a knob handle) from Megiddo Level H-3 with the flaring side down (*Megiddo III,* fig. 11.53:9), as seen in *Megiddo II,* pl. 91:11 (IV).

46 *HES I,* fig. 153:15. Note the distinctive design in de Vaux 1951: fig. 12:20 (niv. 1).

and outwardly folded "rims" at the top and/or bottom that the potter then trimmed to form a kind of collar around both ends of the stand. Sometimes, additional molding of some sort appears below the top collar or above the base, as in *HES I,* fig. 153:13, which shared a stratigraphic context with Ostracon No. 3. Multiple variations on this overall style seem to have begun at Hazor in the ninth century and to have continued at least to the Assyrian invasion in 732 and probably beyond.[47] Some of the taller hour-glass types may include small windows or fenestrations in their sidewalls (*Megiddo I,* pl. 34:12–13, Strata IV–II). A second Iron Age II style probably appeared sometime during the eighth century BCE and persisted through the seventh century. Typically, these pieces show a thinner construction and simpler rims at both top and bottom (see *Keisan,* pl. 45:7–11a for a good comparison of these first two types in niv. 5, 720–650 BCE). As this type survived from the Iron Age into the early Persian period, it seems to have become more austere in its overall design (cf. *Megiddo I,* pl. 34:1–4; Strata III–I). The third style, represented in *HES I,* fig. 153:15, and characterized by its very low height, also appeared by the eighth century and likely extended into the seventh century. (In addition to the citation in n. 46, see *Megiddo I,* pl. 34:9–10, Strata IV–III; from Beth-Shean, see A. Mazar 2006: pl. 22:18, Stratum P-8.)

Overall, the best parallel for *HES I,* fig. 153:13, with its extra tier of molding immediately above its bottom collar, comes from Beth-Shean (A. Mazar 2006: pl. 22:19; Stratum P-8 = late ninth and early eighth centuries). In view of the collective attributes of the Samaria stand, it might easily belong in the early eighth century BCE.

A second form that shared a stratigraphic context with Ostracon No. 3 appears in *HES I,* fig. 154:7 (Type I.14.j; see my fig. 54), listed as coming from *S4-772 sub* (evidently an error for *S7-772 sub*). In any event, it apparently derived from a deposit of subfloor fill, thus making the dating of this form (particularly its *terminus post quem*) somewhat tricky. As outlined in previous chapters, this excavation area lay in Grid E.9, approximately 20 m north of the surviving portions of the Ostraca House, and it also appears on Fisher's plan for the Preherodian/Hellenistic town (*HES II,* Plan 7). Reisner incorporated this bowl in his relatively large Type I.14 (subtype *j*), i.e., in the general vessel category that contained most of the ostraca (see *HES I,* 277, No. 14, especially 14.d). He broadly described the style as a "flaring bowl with flat bottom and straight sides…red or brown ware, brown or black core, red wash." I have reviewed this general class of bowl above (Section F.1) and need not repeat the information here. Essentially, the form is at home in the first three quarters of the eighth century BCE and perhaps slightly later.

The distinguishing trait of this bowl lies in the ridging around its exterior walls, which differentiates it from the other straight-sided bowls in its class. It has a close parallel in *SS III,* fig. 13:5, a bowl with a disc-like base (which Kenyon described as "flat") from S Tomb 107 (which Kenyon correlated to her Period V), and several others were found by the British in summit excavation areas Qd (Period VI), Qk, and Qz. The description of 13:5 includes the notation "(HU)," which indicates that the S Tomb bowl went to Harvard University as part of the material culture received for its continuing role in the Joint Expedition.[48] Kenyon compared the style to several antecedent bowls (*SS III,* figs. 3:2 [II]; 4:2

47 *Hazor III–IV,* pls. CCXVIII: 9 (VII; a shorter variety, not as well made as the Samaria examples); CCXXXII:14–18 (Va), with no. 14 providing the best parallel for *HES I,* fig. 153:13; compare the squat example in Hazor No. 16 with Samaria's No. 15; Hazor No. 17 appears as a shorter version of No. 14; No. 18 shows a relatively slender body. *Hazor III–IV,* pl. CCL:18 (VI) also resembles the squat version at Samaria, while pl. CCLIII:9 (V) returns to the general height of No. 15 from Reisner's fig. 153. For other stands, see *Hazor I,* pl. LXIV:10 (V); *Hazor II,* pls. LXXIV:12 (VI); XCVII:17 (Va); *Hazor III–IV,* pls. CLXXXIX:26 (V; Samaria's stands generally show smoother sides); CCXI:17 (X–IX; for an early cylindrical shape); CCXVIII:9 (VII; hourglass variety of medium height but with collars at top and bottom). *Keisan,* pl. 32:7 (niv. 4), moves beyond the arrival of Assyria and into the seventh century BCE, as does an example from niv. 1 at Tell el-Far'ah (de Vaux 1951: fig. 12:22).

48 See the note in *SS III,* xiii; other entries marked "(PM)" flag those vessels sent to the Palestine Archaeological Museum in Jerusalem (now the Rockefeller Museum).

[III]; and 7:2 [IV]), but these items show different rim and base forms as well as different diameters and, most importantly, they lack the external ridges. Such dissimilarities, in fact, suggest that both *HES I,* 154:7, and *SS III,* 13:5, belong slightly after, not alongside, the parallels Kenyon cited, with perhaps some slight overlap. Thus it seems that the Harvard bowl should fall somewhere near the mid-eighth century BCE or just later, i.e., somewhere near the end of Kenyon's Period IV and probably in Period V or VI.

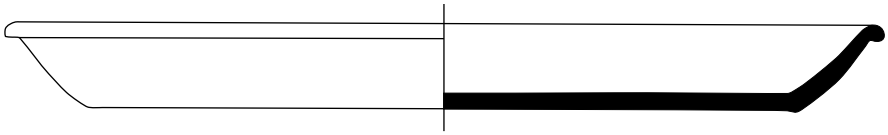
Another flat-bottomed bowl, albeit with a slightly narrower diameter and variant profile for its sidewalls, appeared with similar ridging in Period VI (*SS III,* fig. 10:1). This intact specimen came from Pit *i,* Segment *122.125.19.121,* Layer Va. (For a discussion of this late feature, see Section A above.) Although its prototypes likely extend back into the ninth century BCE, the principal form rose to prominence over the course of the eighth century and primarily during the latter part of that period (see my full discussion and parallels in *AIS II,* 302–8). A more recently excavated, suitable comparison appeared in Level H-3 (ca. 732 BCE) at Megiddo (*Megiddo III,* fig. 11.52:2). Two classes of bowls from Beth-Shean seem to include some members with wavy sidewalls resulting from external, smooth ridging. While the first group (BL52) began in the late ninth century (Strata S-1, P-10, P-9), its numbers increased dramatically in the early eighth century (Stratum P-8) and rose to their greatest strength in the years leading down to 732 BCE (Stratum P-7). The second group (BL54) was basically limited to the eighth century, with a slightly greater concentration in the first half of that period (A. Mazar 2006: 324, Table 12.6; pl. 23:5 represents both groups).

In the south, two somewhat similar series of bowls appeared at Lachish in varying clay colors (yellowish-gray or brownish-orange) and without slip or burnishing (Zimhoni 1997: 141–42, fig. 3.56: Group A, Nos. 1–7; Group B, Nos. 8–13). While excavators recovered most of these items from contexts that proved difficult to date, they ultimately assigned them to a period at the end of Level IV or somewhere between IV and III. If Level IV experienced a sudden end due to an earthquake around 760 BCE (Ussishkin 2004: 83), these southern ridged bowls might relate, at least indirectly, to the comparable ones found at Samaria and support a date in the early to mid-eighth century for the overall class.[49]

In short, it seems that *HES I,* fig. 154:7, and its contemporaries might easily belong to the early to mid-eighth century BCE, although nascent examples probably appeared as early as the ninth century (*Hazor VI,* 134, fig. 2.12:2, Stratum Xa). The possibility that some representatives continued to the arrival of Tiglath-pileser III does not preclude the likelihood that the Samaria bowl derives from the reign of Jeroboam II. Without tighter stratigraphic controls at Samaria, one cannot say more at present.

Finally, Reisner presented a third ceramic form (*HES I,* fig. 156:17a; see my fig. 55) that shared a findspot with epigraphic material, this time Ostracon No. 5. The 50-cm-wide, deep (according to Reisner) tray with flat bottom, flaring and very slightly convex sidewalls, and rim that was outwardly rolled and then trimmed underneath to form a rounded, horizontal lip apparently emerged from beneath the "floor" of Room 418 (*S4-418 sub*) on August 13, 1910. On the previous day, excavators had reached a 30-cm-thick deposit of what Reisner called "black surface debris" in Room 418 (*HES I,* 401; but see my discussion on pp. 110–11). By August 13, they recognized three distinct layers of soil beneath the purported floor. Two of these deposits must have consisted of the thick layers of "dirty yellow" and "clean yellow" matrix described above (see lateral Sections GH and Subsidiary Section AB; Ch. 3, figs. 37, 40–42). The official report does not clarify the nature or precise statigraphic position of the third deposit of debris, and the published photographs (*HES II,* pls. 33.f, 34.d, 36.a, and 53.a) do not help to answer such questions. The exact position of this tray and

49 To what extent, if any, this bowl form influenced the much thicker, heavier bowl with ridged or "stepped" external sidewalls that subsequently appeared at sites such as Megiddo, Levels III–II (*Megiddo I,* pl. 23:15), remains unclear.



Pottery Dis. No. 382
***HES I,* Fig. 156:17a; Type II.17.a**
Ostracon No. 5 = Provenance S4-418 sub

Fig. 55 *Pottery Dis. No. 382 (*HES I, *fig. 156:17a; Type II.17.a; Ostracon No. 5 = provenance* S4–418 sub*).*

the accompanying ostracon, therefore, remains uncertain.

While satisfactory parallels are difficult to locate, the closest suitable Iron Age comparison comes from Tell el-Farʿah,[50] and, while both examples show interior burnishing, the Farʿah tray displays a lighter color (pinkish) ware and slip and a simple, rounded rim. The level (niv. 1/VIIe) that yielded this item, however, dates to the seventh century BCE and correlates best with Samaria Period VII. A date even this late creates a problem for understanding the provenance of Ostracon No. 5 and for assessing its time of writing. But the fact that the very best overall parallels for this vessel type come from Hellenistic deposits at Samaria itself leads one farther away from assigning the locus any historical relevancy to the ostraca (see *SS III,* fig. 40:1 and especially 3; compare the so-called frying pan with handle in fig. 41:23; one may also compare the narrower "cooking pan" from Hellenistic Beth-Shean, Area P, in A. Mazar 2006: 532, 577, fig. 15.3:57). Indeed, variations on this basic kitchen ware appear to have continued even into the Medieval period at Yoqneʿam (Ben-Tor and Rosenthal 1978: 70, fig. 6:8–9).

Admittedly, the tray in question may seem somewhat of an outlier when set against the remainder of the published, ostraca-related pottery, which can generally find homes somewhere within a broad span of the Iron Age II period. If so, *HES I,* fig. 154:17a, may represent an intrusive element that somehow made its way into an otherwise stable eighth-century context. But the very presence of this vessel and its stratigraphic location below a subsequent Hellenistic street (Alpha) may also signal larger interpretive concerns. In the light of such a meager corpus of published pottery associated with the ostraca, certainty remains just beyond our reach. Be that as it may, the impression that the ceramic tradition represented in *HES I,* fig. 154:17a, claims a very late date—even if it somehow manages to begin by the end of the Iron IIC period—should, in my judgment, call into question the security of its findspot relative to the functional life of the Ostraca House. At this point, one has no idea whether this tray was the only late vessel removed from this context or whether it actually represented other contemporaneous ceramic traditions present there. But certainly, this particular form can tell us nothing about the writing date of Ostracon No. 5, whose own text places it among the early group (Year 9) of inscriptions within its corpus. The precise nature of the layer that yielded both the tray/frying pan and Ostracon 5—namely, imported fill serving as subfloor or substreet makeup—means that it could conceivably have contained very early material culture, depending on where the workers scooped up the soil. In other words, the *terminus post quem* for such a deposit must remain open, and whatever preexisting artifact (including an ostracon) lay around that area might easily have made its way into the heterogeneous mix. But the *latest* pottery found in such matrix provides a clue to its *terminus ante quem,* i.e., the time *before which* the fill could not have been poured into place as bedding for a new surface (perhaps, in this case, the Hellenistic Street Alpha or Roman Street C, mentioned earlier, both of which ran directly over the adjacent Ostraca House Room 417). Thus, the

50 Chambon 1984: pl. 58:22 (VIIe) = de Vaux 1951: 418–19, fig. 12:8 (niv. 1).

character of the overall context and the apparent late, certainly post-eighth-century dating for *HES I,* fig. 154:17a, together suggest that at least Ostracon No. 5 lay in an archaeological context far removed in time from its original historical setting. We can be grateful that only one of the published Hebrew ostraca belonged to this enigmatic findspot.

4. Comments on Some Jar Types Associated with Ostraca Pottery Forms (fig. 56)

When considering Jar Types associated with the Hebrew ostraca, a situation similar to that of the bowls quickly emerges with regard both to the scanty publication records and the identification and dating of the specific forms involved. The 25 registration numbers assigned to jars in *HES I* (see Appendix A: Column 9 and Table 6, above) take in three principal categories—each with multiple subcategories—within Reisner's typology for ordinary wares: Types I.2 (7 reg. nos.), I.3 (14 reg. nos.), and I.7 (4 reg. nos.).[51] Reisner described Type I.2 as "large two-handled water jars of *hard, thick gray-black ware* with a smooth gray slip" (*HES I,* 276; italics added). Of subtypes *a–i,* only *a–g* bore ink inscriptions. The report, however, failed to include a single drawing for any of these vessels. The second jar type (I.3), one most often used for the ostraca, comprised subtypes *a–i,* all of which entered the corpus of ostraca. This family showed a *hard* but now *thin gray-black ware* and an exterior surface wet-smoothed to a gray or pinkish color. Thus I.2 and I.3 shared a similar ware but differed in their relative thicknesses.

Once again, Reisner drew none of the forms in the important group I.3, and the two citations he gave to demonstrate the type seem not to fit into a single jar family. The first item is a holemouth jar with sack-shaped body, rounded base, and flat shoulders. (See the reference to Macalister's *Gezer II,* 198, fig. 352 in *HES I,* 277, n. 1; also fig. 56 here.) The second jar consists of an elongated vessel with an outwardly folded, outwardly rolled rim subsequently trimmed underneath, a very low neck, and drooping, rounded shoulders (versus square ones, as per Reisner) with handles attached below the shoulder to the body walls (see Reisner's crossreference to *HES I,* fig. 165:2a = Reg. No. 2138; p. 289; neck fragment shown in *HES II,* pl. 65.b.1). The jar's construction also involved a hard-fired, gray-black ware with a wet-smoothed exterior surface, similar to Type I.3 above. Workers retrieved this jar from Cistern 7 in Summit Strip 1,[52] below the Israelite palace (drawn on Section GH, even though this feature lay well north of that section line; see my earlier discussion). The only traits shared by these two vessels are sack-like bodies that widen slightly toward their rounded bottoms. Beyond that comparison, the two jars belong to totally separate taxonomic categories. The item in 165:2a, however, does appear to share the same basic, hard-fired gray ware as ascribed to the ostraca-related jars in I.2 and I.3.

But the problem of chronology arises once again in Reisner's allusion to fig. 165:2a as a reference point for the ostraca-related jars, for this form does not compare to any known Iron Age shape; rather, it likely dates to the Hellenistic period.[53] Even at Samaria, Kenyon dated a very similar series of jars to the Hellenistic period,[54] and, in fact, Reisner

51 As noted earlier, some confusion exists within *HES I* as to whether I.7 involved jars or jugs. (Four of the six items listed in *HES I,* 277, no. 7a–d, were recorded as jars in the Ostraca Registry.)

52 Interestingly, one journal entry contains a rather ambiguous description of inscribed Hebrew finds from Cistern 7 that are portrayed as very similar in nature to the ostraca. By Friday, October 22, 1909, Reisner had concluded, "there is absolutely nothing in all this to indicate that the cave [i.e., Cistern 7] was a tomb. On the contrary, this stuff is all manifestly kitchen refuse." He went on to note, "The inscribed potsherds are now ten in number. These appear to bear a quantity and a date with other notes. They certainly indicate either offerings or taxes (in natura); and the presence of broken jars which all contained grain or wine is not surprising in a mass of kitchen debris which may be from a royal kitchen" (*Reisner Diary III,* 338; see p. 340 for the designation of the writings as Hebrew). Further conclusions, he acknowledged, required "a more careful examination" of both the cave and its pottery.

53 Compare the series of jars in *Keisan,* pl. 8:1–7 (niv. 2), and *Yoqne'am I* = Ben-Tor et al. 1996: fig. X.5:1–5; Ben-Tor et al. 1983: fig. 8:2 (Locus 2096, second half of second century BCE).

54 *SS I,* 112–15; *SS III,* 129; 231, fig. 42:8–10, which Kenyon acknowledged as *earlier than* the examples in Nos. 11–14, themselves ranging from her Period VIII into the Hellenistic period; and 232–33.

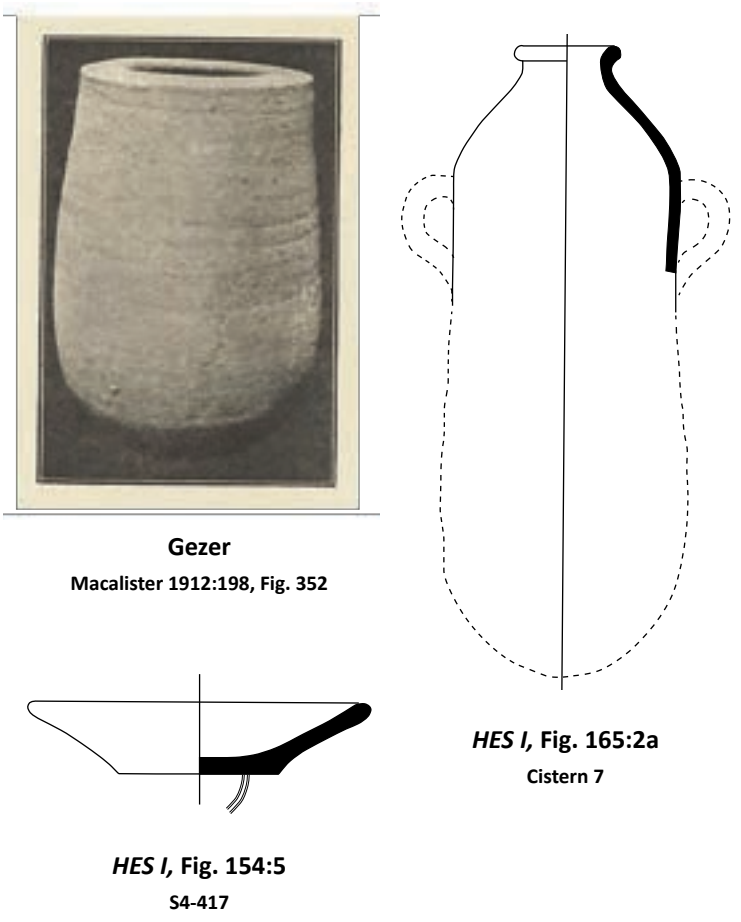


Fig. 56 *Samples from Reisner's ceramic typology and suggested parallels (upper left: Gezer, Macalister 1912: 198, fig. 352; upper right:* HES I, *fig. 165:2a, Cistern 7; lower center:* HES I, *fig. 154:5, S4–417).*

himself sketched similar vessels in his field notes and recognized them as a Seleucid jars (*Reisner Diary III,* 331, 340). A late dating for this particular jar form is even clearer than for the tray/frying pan discussed above, which also may well derive from the Hellenistic period. At a minimum, these two pottery types, which either came from the same contexts as the ostraca or actually bore inscriptions themselves, alert us to the fact that the overall ceramic repertoire under consideration claims a very long chronological range. Besides extending beyond the mid-eighth century BCE to the assaults by Tiglath-pileser III in 732, the ultimate loss of Israelite hegemony over Samaria in the late 720s, or even into the Assyrian political rule of the late eighth and seventh centuries, some relevant remains appear to postdate the Iron Age altogether. But without much more extensive narrative and graphic detail for each local layer in the depositional history of the Ostraca House and its surrounding area, one cannot sort out the primary contexts from the secondary ones. By extension, one cannot always distinguish between the so-called hard and soft ceramic evidence to ascertain how many of these later vessels represent intrusions into an otherwise earlier context.

In any event, Reisner described 21 of the 25 jars in Appendix A as large vessels that displayed a hard gray ware, though some exhibited a thick and others a thin construction technique. As noted, the Ostraca Registry (Appendix A: lines 34 and 36) records the remaining four vessels in Type I.7.a.d as small items made of reddish-brown ware, and these vessels might well represent jugs, not jars (see p. 114 above). Even so, the number of jar entries reflects a significant statistical percentage of the overall corpus. One wonders what role these jars, with their hard, gray ware—and especially the thin-ware variety of Type I.3—played in Kaufman's assessment of changes in the ceramic portrait of eighth-century Samaria (see SectionA, above). Recall his conclusion that the appearance of thinner, harder ware at Samaria represented "a clear period of development" that *followed* the ostraca phase and *preceded* the fall of the city around 721 BCE (Kaufman 1966: 119–20). His position seems to dissociate the ostraca from pottery showing this type of ware, which, according to his (and Kenyon's) analysis, appeared in the capital around 750 BCE. But, as shown above, the hard, thin, grey-black fabric of Reisner's Jar Type I.3 at least carried a contextual relationship with some ostraca and, in a number of cases, bore actual ink-written inscriptions (see, for example, Appendix A: 13, 16a–b, 17a–b, 18, 19, 20, 21, 53, 54, 55, and 57).

Table 7 Overview of approximate dates for a selection of ostraca and ostraca-related pottery.

Chronological Range (BCE)	***HES I,* Fig.** (bold-italics = ostraca)	**Ostraca Nos. Involved** (Append. A)	**Comments**
10th century	154:19, 20		Nature of fill deposits helps explain early, derived elements
late 10th–early 9th centuries	154:9, ***11***, ***12***, 16	40, 50	cf. also provenance of Ostraca Nos. 7, 9, 11, 12, 14, 15, 39, 47, 52, 56, 60
early 9th century	154:10		
mid–late 9th century	154:15		
mid-9th–early 8th centuries	153:***13*** 154:***13***, 17	3, 39	Possibly compatible with generally accepted dating of ostraca
late 9th–mid 8th centuries	154:8, ***14***	50	
E.207 = Periods IV–VI (800–722 BCE)	156:***16a***	26	
early 8th–fall of Samaria, 722/720	154:2, 3, 4, ***5***, ***6***	1, 38	Latest dates become harder to explain vis-à-vis generally accepted dating of ostraca
ca. 732 to fall of Samaria	154:***7***	3	cf. the Assyrian-style bowls in 156:21:a-b-c
late 8th–early 7th centuries	154:1		Becomes very difficult to explain vis-à-vis proposed ostraca dating
late 8th-mainly 7th centuries	154:18		
7th century or later (to Hellenistic)	156:***17a*** 165:***2a***	5 (see list at right)	Must involve intrusive material or very late contexts For the jar parallel in 165:2a, see Ostraca Nos. 13, 16a-b, 17a-b, 18- 21, 53-55, 57, 61

Because of the compromised nature of many of the contexts that yielded the hard gray wares at Samaria, tight controls over their incipient use must remain open to discussion.[55] Similarly, the second plank of Kaufman's argument, namely, the appearance at Samaria of decanter forms in either hard red (e.g., *SS III,* fig. 10:17–18) or hard gray (fig. 10:19) ware, requires further examination. I have elsewhere outlined the principal diagnostic attributes of the Iron Age II decanter family, discussed the lack of stratigraphic integrity for the loci from which Kenyon took her published, "stratified" examples, and established a date in the very late eighth century BCE for nearly all the Samaria decanters.[56] If one were to steer by this evidence alone, Samaria Period VI would relate best to the

55 For at least some of these jar fragments (and also other ostraca), one journal entry belies their compromised findspots in deep, imported fill deposits: "The dirty yellow debris (not the clean Isr. yellow) continues to yield inscribed fragments of pots. A very complete series of nearly duplicate texts occur[s] on some hard sherds of large two handled jars" (*Reisner Diary V,* 526; Reisner's underscoring).

56 See *SS III,* fig. 9:5, from the massive leveling fill near Wall 573 on the northern slope of Samaria = Strip Qn, Segment North of 551, Layer Va; and fig. 10:17–20, from the problematic feature known as *Pit i,* Segment 122.125.19.121, Layer Va. See also Section IV.a, above, for an overview of these findspots. For a full discussion, see *AIS II* (pp. 287–91, 337–41 specifically for decanters).

middle-to-later decades of Megiddo Stratum III. One cannot, then, gainsay the proposition that the Hebrew ostraca predated the Samaria decanters. From a palaeographic point of view, they certainly did. But having said that much, neither the date nor the archaeological context of the decanters can further refine the date of origin or explain the provenance of the ostraca.

G. Summary

While Kaufman proposed the useful approach of examining both the ostraca and the uninscribed but stratigraphically related pottery in an attempt to date the writings more closely, I have demonstrated that multiple problems combine to raise serious challenges for anyone pursuing this method: (1) the Harvard report presented a limited number of vessels to represent the entire Iron Age II period at Samaria; (2) it provided only laconic descriptions for the published vessels and virtually no stratigraphic analysis of the local depositional history surrounding them; (3) within that small, overall assemblage, precious few vessels were shown in scaled, profiled drawings; (4) within that even smaller subset, hardly any *ostraca-bearing* forms (only four bowls) appeared in full-scale pottery drawings; and (5) those few non-epigraphic but *ostraca-related* items (five bowls and one stand) that one might hope to steer by display a wide enough chronological range when compared to the pottery of surrounding or related northern sites that they cannot lead to firm conclusions.

The chronological disparity of findspots for selected items presented in *HES I* (see Table 7) and, more specifically, for the ostraca and ostraca-related pottery within that collection (represented in bold italics in this table) indicates that the inscriptions themselves did not emerge from one coherent *primary* context (e.g., occupational debris lying directly on a discernible floor) and, indeed, not even from one consolidated *secondary* context (e.g., a single deposit of subfloor fill sealed by an overriding surface within a single building). This fact, coupled with the relatively widespread horizontal distribution of ostraca as outlined earlier in this study, compromises the basic presupposition of investigations such as Kaufman's—namely, the assumption that one can isolate and analyze a single archaeological context for this collection of writings and, from that evidence, derive a narrow chronological range for the writing and functional life of the entire corpus of inscriptions.

Chapter 5

The Samaria Ostraca and Modern Scholarship

One cannot gainsay the fact that, in reality, the ostraca recovered at Samaria during the 1910 season of excavation form a cache of clearly related writings even though they did not emerge from a single, coherent archaeological context. After all, Reisner recovered the entire corpus from a longitudinal swath of excavation lying east and north of the so-called Ostraca House. On Tuesday, September 27, 1910, following the discovery of the last two ostraca (which became Nos. 51 and 55 in the published corpus), he recorded a clear perimeter of discovery near or adjacent to the Ostraca House:

> We cleared out the intact block of Israelite debris in S2 (vestibule) but there were no inscribed potsherds (Isr. ostraca). No Israel. ostraca have been found east of the newly found Isr. wall [i.e., Wall A; *HES II,* Plan 5, Grids F.9–14].
>
> (*Reisner Diary VI,* 610).

Furthermore, the closely-dated year formulas attested in the inscriptions ("In the 15th year . . . ," "In the tenth year . . . ," and "In the ninth year . . .") link them chronologically, though Reisner's conclusion—"there can be no doubt that [the] dirty yellow debris represents debris of the Ahab period and that the 'year tenth' etc. of the potsherds refers to Ahab's reign" (*Reisner Diary VI,* 556)—now seems untenable. The entire collection reflects a unified content that focuses on the movement of small quantities of various commodities to the capital from a relatively circumscribed network of locally owned villas and estates. From a palaeographic point of view, the inscriptions clearly belong together. The pottery bearing the inscriptions derives from industries that are generally compatible in vessel form, ware, and surface treatment. Without a doubt, Reisner recovered and published an impressive group of inscriptions that are related both historically and functionally. Looking only at the ostraca, then, he seems reasonably justified in concluding that "if no other information were available, they would be assigned on epigraphical [and ceramic] grounds to the 8th or 9th century B.C." (*HES I,* 227).

Yet significant interpretive problems, arising both from what Reisner himself encountered in the soil of Samaria (see especially Ch. 2) and from the way in which he ultimately presented what he found (see Chs. 3–4), leave many questions unanswered today. Although the writings came from one extended tract of space, their individual findspots spanned an area of 35 m or more, from the center of Room 418 northward through Rooms 772–773 and even into Room 777, and each local context requires separate evaluation.

Based on available records, published and unpublished, it remains quite difficult to determine

the precise vertical stratigraphic position of any one ostracon. It appears that the architecture to which analysts have traditionally related the inscriptions shows either subterranean foundation features smothered in backfill, or multiple phases of construction from different time periods. Based on my analysis of the published report, field diaries, and drawings, I accept the latter scenario. In any event, all the walls were eventually covered with imported leveling fill. Each of these dispiriting options, therefore, places the ostraca in secondary contexts that raise fundamental questions concerning the location and nature of the original, primary setting of the writings. Clear floor levels remain elusive in both the narrative and drawings of the official report; the undulating (20–100 cm) courtyard floor spoken of by Reisner provides only a dubious anchor at best and does not appear in the drawings (plans or sections). Disparate records place the substantial and pivotal deposit of "dirty yellow" fill both above and below the purported floor level. And while the ink-written pottery itself may, from a ceramic point of view, suggest a relatively restricted range from the late ninth to late eighth or early seventh centuries BCE, that span remains too broad to permit significant refinement in dating the inscriptions themselves. Moreover, the wider range of associated non-epigraphic pottery assigned to the same contexts as the ostraca shows a much longer span of styles, extending at least to the Hellenistic period, and thereby corroborates the understanding either that matrix from disparate time periods became mixed during the excavation process, or that it all represents very late, secondary fill. In my judgment, the first option seems more likely. But even in the face of such challenges, the ostraca themselves have remained invaluable to reconstructions of the social, historical, economic, and administrative organization within the northern kingdom of Israel during the Iron Age II.

Nearly half a century after Ivan Kaufman completed his dissertation on the Samaria Ostraca, the work continues to reflect his keen desire to apply the best methods possible to the study of this challenging corpus of materials, his judicious handling of the available evidence, and his great sense of intuitive reasoning when the evidence grew lean. In the end, his placement of the ostraca in the early eighth century BCE, probably sometime during the reign of King Jeroboam II (ca. 782–753 BCE), may be correct. Archaeologically speaking, at least some of the evidence supports this position. And Anson Rainey (1988: 69–74; prior to that study, see 1962; 1967; 1970; and 1979), while showing uncommon appreciation for Kaufman's work, attempted to go one step further by using Thiele's chronology for the kings of Israel to narrow the date of the inscriptions to a single year (784–783 BCE, during the coregency of Jehoash and Jeroboam II; see Table 8). Even though the aura of precision surrounding this argument might well surpass the supporting evidence and even exacerbate certain other concerns,[1] Rainey's general reasoning may also ultimately prove correct.[2] If so, the only task left would be to determine the day on which each docket was written and each donkey loaded.

Kaufman based his methodology and conclusions on three principal criteria: consideration of the pottery from Periods IV–VI at Samaria; the idea that there existed other, sometimes unidentified building phases which only slightly pre- or post-dated what is now known as the Ostraca House; and the use of hieratic numerals in the year formulas contained in the ostraca (see Kaufman 1966: 105–6, 133–34). *Archaeology,* therefore, led the way in fixing the general time frame for the

1 For example, if all the ostraca fall into a span of months vs. years or decades, how should one understand the significant increase of territory covered by the ostraca's place names in the Year 15 group compared to the Year 9/10 group? Similarly, in the onomasticon of private names, why do those with theophoric *-ba'al* outnumber those with *-yau* by 11:7? Moreover, how should one explain the significant reduction in *-ba'al* names from Year 10 to Year 15? (Compare Albright [1942: 160–61] and B. Mazar/Maisler [1948: 129–33]. Shea [1977: 17–19] notes many other differences between the two year-groups.)

2 Long before Kaufman's work, both W. F. Albright (1940: 11, n. 17; 1942: 41; 122; 141, n. 41; 160; 1943: 59; 1949: 132–35; 220–21) and S. Birnbaum (1942: 108) had linked the ostraca to Jeroboam II. And prior to Rainey's article, both Aharoni (1962: 67; 1979: 366) and A. Lemaire (1977: 80) had associated the inscriptions with both Jeroboam II and Jehoash. (See Ch. I, n. 2, for Birnbaum's later dating of the 11 additional ostraca found in the 1930s by the Joint Expedition.)

Table 8 Summary of proposed dates for the Samaria ostraca.

Reference	Historical Setting
Reisner, et al. (1924:227; cf. *SS III,* 9) (*Reisner Diary V,* 534)	Ahab Palace (865–722 BCE) Subsequent to Ahab Phase
Yadin (1961: 22–25)	Menahem
Shea (1977: 21–23)	Menahem Pekah
Cross (1962: 34–36)	Menahem
Aharoni (1961: 122, nn. 38–39; 1979: 356–68)	Jeroboam II
Kaufman (1966)	Jeroboam II (750 BCE)
Cross (1975: 8–10)	Jeroboam II
Rainey (1988: 69–74)	Jehoash–Jeroboam II co-regency (784–783 BCE)

epigraphic collection, followed by the ostraca's own content (particularly the record of regnal years), palaeography, and the study of Egyptian hieratic. Years later, Kaufman maintained that same ordering of evidence in a shorter, more popular article on the subject (Kaufman 1982: 231–35). Since he was writing his dissertation for an archaeologist, this approach comes as no surprise.

Though many questions continue to surround the archaeological reporting from Samaria by both the Americans and the British, the present study has provided some detailed support for the view that another building phase, and probably several phases, existed in the very space occupied by the remains that Reisner dubbed the "Ostraca House." The construction history of that area has proven considerably more extensive than previous interpreters have believed based on their rather casual acceptance of the Harvard reports. Kaufman's handling of the pottery, however, raises greater concerns. His approach relies far too heavily on Kenyon's ceramic analysis and historical reconstructions.[3] As a result, his investigation lacks a thoroughgoing comparative study of the ceramic repertoire at Samaria. Even the work of his contemporary, J. S. Holladay, whose dissertation focused squarely on that repertoire, limited its comparative data almost exclusively to three sites: Shechem, Tell el-Far'ah (N), and Hazor. Admittedly, many new field reports, using modern analytic strategies, have appeared since the mid-1960s, when both Kaufman and Holladay studied under G. E. Wright. Still, one cannot escape the fact that a broader sweep of available evidence might have enhanced their conclusions. Surely, forms such as the Assyrian (or Assyrian-style) bowls and the late frying pan would have emerged as anomalies within their interpretive frameworks.

A little more than two decades later, Rainey basically accepted Kaufman's ceramic conclusions while attempting to refine the chronological range of the ostraca themselves. On historical grounds, he offered creative and usually solid suggestions for topics such as the precise occasion behind the inscriptions, the identification of the "*l-men*" as the receivers vs. the shippers of goods listed on the ostraca, and the importance of attending to the genre of ancient texts, i.e., to the distinction between *prescriptive* and *descriptive* works (Rainey 1988: 71–72; see n. 4 below).

But on the archaeological front, Rainey confidently grounded his introductory argument on three quite debatable presuppositions (1988: 69). First, he accepted that Kaufman had "succeeded admirably in locating the *exact findspots* of the clusters of ostraca; *all of them* were within *a fill* below *the floor level* of the so-called 'Ostraca House'" (italics added, here and below). That is, the entire epigraphic corpus came from one consolidated deposit (albeit fill) protectively sealed beneath one floor level in a single building. Assuming all other lines of inquiry converged, such a perfect provenance might indeed have allowed a considerable narrowing of the time period during which

3 To his credit, however, Kaufman attempted to deal with the Samaria pottery more extensively than did most epigraphists of his time (cf. Lemaire 1977: 41).

the ostraca were written. But clearly Rainey's first premise now represents an untenable starting point.

Second, after correctly observing the palaeographic similarity between the early (Years 9/10) and later (Year 15) groups of ostraca, he extrapolated a conclusion that "the various suggestions to date the ostraca to two separate reigns with something like *a generation* between them … can no longer be sustained." That is, in the light of the closely spaced year formulas, palaeographic dating alone can narrow the time frame for the inscriptions to a point *within* a generation, to a brief stretch of time nestled somewhere within about a 25-year period. But is script typology so refined, compared to ceramic typology, that it can reduce the range of an artifact's plotted lifetime to a decade or even a year versus, at best, a half century?

Third, he asked, "What strange quirk of fate led to the burial of *only those sherds,* all together *in one fill*?" before adding, "It may be assumed that the fill was taken from *some dump* or from the debris of *a previous structure* or courtyard." I have shown, first, that the ostraca did not emerge from "one fill" and, second, that the disparate contexts relating to the inscriptions yielded not only "those sherds" but also other, non-inscribed ceramic fragments, notwithstanding the fact that Reisner failed to publish most of them. Moreover, if the ostraca contexts encountered by Reisner's team represented secondary ones, created with matrix displaced from "some dump" or from the remains of an earlier building (as my earlier examination of Section CD suggested for at least part of the corpus), then modern analysis of these inscriptions must devote much greater consideration to this fact. An entirely new line of reasoning must come into play if one completely dissociates the ostraca from the now-familiar "Ostraca House."

Kaufman and Rainey each raised the possibility of a previously unrecognized building phase that existed below (i.e., before) the Ostraca House. On both the horizontal and vertical planes, my analysis of the structure currently accepted as the ostraca building has shown that one must now accept the historical probability of this proposal. At the same time, my study shows the error of Rainey's central presupposition, namely, the premise that all the ostraca emerged from one, consolidated, sub-floor deposit. In truth, the ostraca collection hails from multiple contexts of apparently different natures that together reflected a wide chronological range of ceramic industries. For example, some inscriptions lay above and some below perceived floor levels; some were situated near and some far away from the Ostraca House itself; some associated ceramic groups, whether or not inscribed, displayed greater homogeneity in tradition and date than did other groups; etc. Contrary to conventional wisdom and the manner in which scholars have typically approached this corpus, one cannot treat all the ostraca as a single cache of texts *from the point of view of their archaeological provenance.* Based on an analysis of what little pottery the report makes available to the reader, the apparent lack of stratigraphic integrity for items listed in the bottom three to five lines of Table 7 (on p. 132) calls into question the value of these subgroups for determining the original life-setting of the ostraca collection as a whole. Certainly, the inscriptions that appeared alongside Hellenistic-style pottery must have come from deposits that became mixed during excavation or from much later, secondary contexts that cannot aid in establishing a date of use for the overall corpus—at least not on archaeological grounds. In either case, palaeographic and historical considerations must now lead the way.

The later (post-750) date for a number of pottery forms discussed above prompts one to recall proposals by Yadin (1961: 22–25) to date the ostraca to the reign of Menahem (sometime between 745–735 BCE),[4]

4 Yadin (1961: 24) mistakenly believed that Reisner found the ostraca "strewn on the floor" of the Ostraca House. Earlier (1959: 184), he had even placed the ostraca "in the king's palace," an attribution that probably resulted from the excavator's early interpretation of the ostraca building. (Note Lyon's [1911: 228–29; 1912: 224] repeated reference to the Hebrew ostraca and Osorkon vase as having come from "the palace of Ahab.") Yadin understood the ostraca as "records of tax sent to the king by the Israelite estate owners during the time of Menahem" (1962: 64; see also n. 6, below). Kaufman (1982: 237) accepted this position and interpreted the *lamed* + PN as meaning "to the credit of PN." Rainey (1988: 72) and Aharoni (1962: 67–69; 1979: 363) disagreed and saw the ל-*names* as the designated recipients of the commodities in Samaria.

and by Shea (1977: 21–23), who tied them to the reigns of both Menahem (ca. 740–739 BCE) and Pekah (737 BCE; see Table 8). Using archaeology alone, one would not dismiss these views out-of-hand. Even Cross (1962: 35; see also the related work in 1961: 12–14), who focused primarily on the palaeography of the ostraca, initially followed Yadin's lower date for the overall corpus before eventually shifting to Aharoni's argument to link it to Jeroboam II (Cross 1975: 8–10; Aharoni 1961: 122, n. 38–39; 1979: 356–68). Reisner himself, despite his consistent appeal to the era of Ahab, on at least two occasions contemplated a later time period for the ostraca:

> Thursday, Aug. 16. Early this morning apparently the last of the inscribed potsherds. This is now a collection of about 45 pieces. The new ones with the perfect script present an alphabet which is identical with that of the Siloam Tunnel inscription.
>
> (*Reisner Diary V,* 529; Reisner's underscoring).

> Wednesday, Aug. 17. … What is the date of the house of the 4th series [i.e., the Ostraca House level]? I think undoubtedly Israelite, and previous [importantly, a gloss placed immediately above this word reads "subsequent"; which interpretation did he ultimately intend?] in date to the Ahab additions. The series of facts which show this will be marshalled later.
>
> (*Reisner Diary V,* 534; Reisner's underscoring).

Shea's reasoning, particularly his argument that Pekah began counting his regnal years even before he became king, remains unconvincing. It is, moreover, hard to imagine such a well-organized network of estates and villas voluntarily supporting the centralized capital much beyond 750 BCE, when the kingdom felt the impact of multiple assaults by the ever more powerful Assyrians, when a succession of weak and reckless leaders ruled from Samaria, and when political intrigue wracked the government with a series of coups and political assassinations (see Ch. 1.A). But surely, despite some of the associated ceramic evidence, the ostraca's useful life cannot postdate Israelite Samaria (ca. 721 BCE on). And on the higher end, comparative palaeographic analysis precludes raising the date of these inscriptions to a time during the ninth century BCE—certainly not, with the predominant opinion of the excavators, as early as the reign of King Ahab.[5] Such a high chronology would actually place the ostraca script prior to that of the Mesha' Stele rather than after it.[6] These facts, then, leave the period between 800 and 722/721 BCE as the most likely time for the appearance of the corpus. But if attempts to fit them into the period between 750 and 732/722 make progressively less historical sense, then the first half of the eighth century BCE remains the most reasonable choice for their *sitz im leben,* in which case the archaeological anomalies (ostraca alongside late pottery, etc.) will have to receive separate evaluation or remain a conundrum in the absence of more extensive and higher quality data.

Ultimately, I must temper somewhat Kaufman's claim that the association of the ostraca with the reign of Jeroboam II "depends upon palaeographical and archaeological criteria and *to a lesser degree* on the historical evidence for particular times which may have been propitious for delivery of wine and oil to the crown …" (Kaufman 1966: 132; italics added).[7] In this instance (as in most oth-

5 B. Mazar (Maisler; 1948: 123) dated the ostraca to the late-ninth-century reign of Jehoahaz, son of Jehu, who ruled from 815–801 (Albright 1945: 21) or 814–798 (Thiele 1944: 137–86; 1965). He understood the inscriptions as reflecting a system of tax collection.

6 Interestingly, one of Birnbaum's (1942: 108) motives for dating the ostraca to Jeroboam II and not to the early to mid-ninth century, as proposed by the excavators, involved keeping these writings relatively close in time to the Lachish Letters, since he saw only a small degree of palaeographic development between the two epigraphic collections. An even later date to the reign of Menahem (see n. 4, above) would bring the two groups closer still.

7 One will recall that Holladay took an almost opposite approach in his treatment of the pottery from the closing years of Israelite Samaria; he dated the fragments more by historical reckoning (based on a presumed Assyrian destruction level) than by specific archaeological context or comparative ceramic analysis (see Ch. 4.A).

ers), it is natural to expect a stronger link between the palaeographic dating of epigraphic finds and the historical connections made by their content, especially when the archaeology (or the archaeological reporting) of the inscriptions proves as murky as it does regarding the ostraca. Thus, given what we know otherwise about the socio-political situation in Israel during the early eighth century BCE, a palaeographic linking of the ostraca to Jeroboam II requires their content to reflect an historical occasion suited to seeing a centripetal network of support around the capital city. These same two lines of inquiry—history and palaeography—constituted the guiding principles behind Albright's early conclusions (see nn. 1–2, above) and remain the most promising investigative avenues today. The *archaeology* of the ostraca, on the other hand, will likely always prove quite problematic. The desired dénouement lies beyond our reach. Tellingly, Rainey himself ended his resourceful (and somewhat exuberant) claim to have pinned the date of the ostraca to the year 784–783 with the admission, "Unfortunately, there is little hope of ever proving the validity of this suggestion" (1988: 73). And so it seems.

Chapter 6

Back to the Backstory: The Characters and Concerns behind the Harvard Expedition to Samaria

…With rough and all-unable pen
Our bending author hath pursued the story,
In little room confining mighty men,
Mangling by starts the full course of their glory.

~ *Shakespeare,* King Henry V, *v.ii.*

I close this study with some observations concerning the appearance of the official excavation reports from Samaria, the principal figures who designed and led the project, and, more specifically, the career of George Andrew Reisner, the American Egyptologist behind the discovery and publication of the Samaria Ostraca. Because of his long-running involvement in and important contributions to our understanding of Levantine worlds and, even more so, of Egypt, and in acknowledgment of his continuous efforts to refine all aspects of field methodology (see, for example, Der Manuelian 1992), Reisner claimed in his own time and continues to hold today a highly respected position within the guild of field archaeologists.

> Reisner's extraordinary gift for managing a large gang of Oriental laborers, for detecting the spots where excavation will probably be successful, for thoroughness in the examination of the ground dug up and in recording the facts by photographs and systematic notes have made him one of the great archaeological explorers of the world (*Secretary's Note* in Ropes and Fisher 1914: 542).

> Reisner set the highest standards in field methods, insisting on meticulous recordkeeping in all aspects of excavation. He was among the first to use complete photographic recording as a standard field technique…. The publications of his fieldwork were unusually complete, both as catalogues of remains recovered and as detailed historical and cultural syntheses (Podzorski 1997: 421).

Evidence of Reisner's forward thinking with regard to more detailed stratigraphic analysis emerges throughout his own handwritten journals. For example, on Monday, October 4, 1909, R. A. S.

MacAlister paid a visit to Samaria just before noon. That evening Reisner recorded,

> We showed him everything and kept him until about 4:30 P.M. It is satisfying to note that ~~it~~ he had no doubts on the soundness of our conclusions and was especially struck with the correctness of our identification of the Israelite periods. In regard to the "Babylonian" Wall, he thinks that identification is also practically certain; but I must confess that personally I want more evidence.
>
> (*Reisner Diary III,* 295; Reisner's strikethrough and underscoring).[1]

The following day, after reflecting on MacAlister's visit, Reisner added,

> A very important [skill] in practical excavation is the distinguishing between debris of decay and debris artificially deposited in a large space or slope. In conversation with Mac.A. yesterday, I found that he had never noted this difference.
>
> (*Reisner Diary III,* 296).

He then proceeded to sketch and annotate five drawings and to devote three full pages of his diary to clarifying the difference between these two types of deposits. These journal entries ultimately became the basis for his published discussion of the various kinds of debris an archaeologist might encounter and the surviving clues to the depositional history of each kind (cf. *HES I,* 36–42).[2]

Without wishing to detract from statements similar to those of Ropes and Podzorski (compare, for example, Moulton 1943: xiii; King 1975: 56, 61; Silberman 1997: 315; et passim), or from the advancements that Reisner clearly brought to the practice of field archaeology, I must still wonder at the numerous recording errors, peculiar drawing habits, and nearly total lack of proper provenance data within the Samaria reports. One might accept at least the last shortcoming as simply the norm for archaeological reporting of the time.[3] But clearly the unpublished, highly detailed manual of field methods that Reisner himself wrote in 1924 (see Der Manuelian 1992) militates against such a facile explanation and places this forward-looking excavator in a class by himself. Other factors, then, must combine to give a broader explanation for the problems encountered within the Samaria reports and for their delay in publication. These issues become apparent in the annual activity reports (1905–1930) written each autumn for the preceding academic year by Semitic Museum Curator David Gordon Lyon and submitted to the President of Harvard University (fig. 57a).

First, the excavation quickly consumed the generous financial backing provided by Mr. Jakob Heinrich ("Jacob Henry") Schiff. Even before the conclusion of the inaugural season, by Saturday, August 1, 1908, both Lyon and Schumacher felt the financial crunch. Realizing that a field season necessitated much work beyond the actual process of digging, Lyon noted in his journal,

1 Later, following a visit to the site on Thursday, October 14, 1909, by Frère Marie-Joseph Lagrange and Père Louis-Hugues Vincent from the École Biblique (both of whom reportedly accepted all interpretations presented to them "with the greatest imaginable enthusiasm"), Reisner once again privately confessed in his journal, "I must say I am not absolutely certain about that 'Babylonian' wall myself though neither MacAlister nor Vincent seemed to realize how slender the evidence is" (*Reisner Diary III,* 321; Reisner's underscoring; see n. 17, below). (Lagrange had founded the École Biblique under the name *École pratique d'études bibliques* in 1890.)

2 Another witness to Reisner's careful field methods comes in his (Thursday) October 21, 1909, journal entry recording his procedure for reading the pottery from Cistern 7: "I examined every piece before washing. The men examined them while washing. Fisher examined them while sorting into types; and finally I examined each one again for writing only, making four examinations of each piece" (*Reisner Diary III,* 335–36). In essence, this technique anticipated by more than half a century Aharoni's idea to identify potential epigraphic materials at Arad by dipping each fragment in water prior to scrubbing it thoroughly (see Aharoni 1981: 4).

3 Compare Macalister's *Gezer I–III,* published in 1912, or even Kenyon's *SS III* report from 33 years after the appearance of the Samaria volumes; the section drawings in *SS I* also lack most of the local stratigraphy. See the full-bodied discussion in *AIS I–II.*

Fig. 57 *Administrative coordinator and financial patrons of the Samaria Expedition: a. selected Annual Reports to the president of Harvard University by David G. Lyon on behalf of the Semitic Museum; b. Therese Loeb Schiff (upper left; photograph from Goldberg 1997: 1206); c. Jacob Henry Schiff (lower right).*

> Something will have to be set aside for expenses subsequent to actual digging. Unless therefore Mr. Schiff puts in more money this year, it seems that we can dig but 2 or 3 weeks longer.
>
> (*Lyon Diary II,* 62; cf. *Schumacher Diary I,* 105; *II,* 156–59)

And on Monday, August 17, he added,

> We expect to close work and leave on Friday or later—day following (28 or 29), our money limit being nearly reached.
>
> (*Lyon Diary III,* 16)

To make matters worse, already by Monday, August 3, 1908, Schumacher himself had written,

> In re-controlling our accounts I found that I have overdrawn my credit in the Haifa Palestine Bank for over 700 francs, so money is urgently needed.
>
> (*Schumacher Diary I,* 107)

At the conclusion of the 1908 Season, Lyon even feared that he would have to box and leave *in situ* the great statue of the Emperor Augustus for lack of the resources required just to transfer it from the field to proper storage (*Lyon Diary III,* 32; cf. 33–34; compare *Schumacher Diary I,* 105). And following his return to Cambridge, Massachusetts, Lyon had to draft a letter to the Harvard Corporation, which along with the Board of Overseers governed the affairs of the institution, "explaining my return from Samaria at an earlier date than expected …"

(*Lyon Diary III,* 45). (Incidentally, the Corporation approved the use of "Samaria money to aid in the prosecution of further preparations here to return to Samaria" and also renewed the "tuition" of Clarence Fisher—to whom the Committee on Exploration in the Orient [see Ch. 1.B] had paid $700 for the first year's work—provided that Fisher would enroll in the Graduate School of Arts and Sciences at Harvard [*Lyon Diary III,* 46].)

Although the initial year of work in 1908 did not exceed its $10,000 budgetary limit, the second year—Reisner's first season as on-site director—cost nearly $25,000. (The combined cost of these two seasons plus the initial outlay of $5,000 for license-related expenses would equal roughly $1,064,515 in 2015.) These expenditures left only $15,000 of the original $50,000 dollar gift for the planned third, fourth, and fifth years of work (Lyon 1910M: 278). To help address the financial challenge, Mr. Schiff supplemented his initial pledge of $55,000 (including the $5,000 for license procurement) by adding another $10,000 (~ $256,633 today) in February 1910 as they prepared for the third season (*Lyon Diary III,* 80), thus raising his total investment to the equivalent of $1,729,838 in today's economy (using 1909 as a beginning base year).[4] This generous gift came "'with the understanding, however, that as far as the present expedition is concerned, its labors be concluded with the end of next summer's work and proper provision be made out of the funds in hand to publish results'" (*Lyon Diary III,* 80–81). Thus, despite the original five-year plan, the project halted after the third year of fieldwork, with Lyon's stated hope of renewing excavations sometime in the future and with the expectation that "a full account may be published during the year 1911" (Lyon 1911M: 229).

Prior to the outset of the 1910 Season, Lyon inquired of Mr. Schiff (in writing from Cambridge on January 24 and in personal conversation in New York on February 6) as to whether he "had any objection to [Lyon's] making an effort to interest other persons in the work." Schiff replied that such a prospect would be "highly desirable, if it seems worthwhile to continue" (*Lyon Diary III,* 80–81). By April 28, 1910, Lyon presented to Schiff an abstract of Reisner's preliminary report on the 1909 Season and expressed the desire to publish the report in full. But by May 19, Schiff advised "not to publish the full preliminary report on account of the cost" (*Lyon Diary III,* 84). In communicating this news to Reisner, Lyon told his colleague that "it may be better and cheaper to print his final and full report in Germany" (*Lyon Diary III,* 87). Following the 1910 fieldwork, on January 13, 1911, Reisner cabled Lyon from Cairo informing him that he (Reisner) would set sail for America on January 26. Reisner then inquired whether he should apply for a new license to excavate at Samaria. Lyon's return message said simply: "No. Later we hope" (*Lyon Diary III,* 102).[5]

4 These figures are based on S. Morgan Friedman's inflation calculator, in which "the pre-1975 data are the Consumer Price Index statistics from *Historical Statistics of the United States* (USGPO, 1975). All data since then are from the annual *Statistical Abstracts of the United States*" (http://www.westegg.com/inflation/). By early August 1909, and following a very successful personal meeting with Mr. Schiff in Bar Harbor, Maine, Lyon was confident that the project would extend beyond the second season. As a result, he suggested that Reisner apply "soon for a permit to continue the work next year at Samaria" (*Lyon Diary III,* 76). Lyon's journal entries around this time indicate, at last, a view of administrative and financial security for the project. Fisher's contract for 1909 provided $400 round-trip travel expense (~ roughly $5,322 in 2015 currency) and a guarantee of at least a six-month salary of $100 per month (~ $2,872 in 2015), 70 percent of which he arranged to have sent to his wife in Philadelphia (*Lyon Diary III,* 57–58, 68–69, 72). He later (September 2, 1909) requested that Harvard reallocate to Mrs. Fisher the $200 fare for his return trip in order that she might join him in Egypt in November (*Lyon Diary III,* 77–79). After the close of the 1910 field season, as they worked on the Samaria volumes in Cairo, Reisner received $400 per month for four months' work, and Fisher received $150 per month for the same period (*Lyon Diary III,* 91).

5 Apparently, by 1920 Reisner had, in fact, applied to the newly ensconced British administration for another permit to excavate at Samaria, this time as part of a proposed collaborative project (Hallote 2011: 166). Professor Kirsopp Lake, an Englishman who was actually a professor of New Testament and ecclesiastical history at Harvard, played a key role in launching the so-called "Joint Expedition" in 1931 (see *SS I,* xv). In August 1932, Lake divorced Helen Courthope Forman, his wife of 29 years, and in December married Silva Tipple New, his former student who had become a professor of classics at Bryn Mawr. Silva Lake became the epigraphist for the Joint Expedition and ultimately published the Greco-Roman inscriptions in the 1957 final report (see *SS III,* 35, nn.

One can detect in this sequence of exchanges the waning interest of the principal sponsor of a beleaguered expedition. Toward the end of December 1910, Lyon had solicited recommendations for the continuation of the work at Samaria from about 25 scholars. His appeal to them read as follows:

> My Dear Sir;
>
> The question of resuming the exploration of Samaria is one of ways and means. In the effort to find the money it would be a great help if I could show that scholars who are qualified to express an opinion regard favorably the work already done, and that they believe the further exploration of the site to be a matter of importance. I am venturing to address you as one thus qualified, and to ask if you will not kindly write me in briefest terms your views on the subject.
>
> Very truly yours,
> David G. Lyon
>
> (*Lyon Diary III,* 92)

Ironically, Lyon's desperate plea came at the very time when the discovery of the Samaria Ostraca was scheduled for publication in the January issue of the *Harvard Theological Review* and when he had prepared and mailed advance sheets of the article to 24 American newspapers (*Lyon Diary III,* 93).[6]

The final volume of Lyon's private journal concludes with a meeting of the Committee on Exploration of the Orient, which Schiff, Lyon, and Reisner attended. The Committee decided to form a subcommittee which, after separate deliberations, suggested reaching out to the Jewish citizenry of Boston as well as to "Jewish Harvard graduates in other cities." Lyon raised the idea with Abbott Lawrence Lowell, who had succeeded Charles Eliot as president of the University in 1909. Lowell promised to take the matter before the Harvard Corporation. But in a return letter of May 9, 1911, the Corporation replied that its members would have "to see the names of persons to be solicited to help the Samaria work" before granting their approval. Lyon understood this reply as a desire to ensure that "we are not to appeal to the regular contributors to other Harvard enterprises" (*Lyon Diary III,* 105). But the response may also suggest that institutional support for the excavation did not come automatically.

Second, Lyon's anticipated early publication date met with difficulties brought on by the rising specter of war. The first direct mention of this crisis occurs in Lyon's Museum report for 1913–1914 alongside a now-recurring theme of "lack of money" for acquisitions and operation of the Museum (1915M: 247). "Owing to the stringency of the times, and latterly to the pressing appeals in behalf of the sufferers from war in Europe," the Semitic Museum had even failed to raise the necessary funds to receive a matching grant of support. Meanwhile, Volumes III and IV in the recently conceived *Harvard Semitic Studies* series had appeared ahead of the anticipated Samaria reports, slated for Volumes I–II. This necessary adjustment arose principally because Reisner had returned to Egypt to pursue his work there: "Owing to the risks growing out of the war there has been a delay in transmitting the proof sheets and the manuscript *to Egypt*. How long such delay may seem necessary it is impossible to predict" (Lyon 1915M: 247; italics added). A year later, the Samaria volumes all but dropped out of Lyon's annual reports. Following the 1914–1915 academic year, his only communication concerning Samaria consisted in the statement that "it is still impossible to say when the work will be ready for publication" (Lyon 1916M: 262). Even

1 and 35–42). Interestingly, in 1895 Frieda Schiff, daughter of Jacob Henry and Therese Loeb Schiff, married Felix Moritz Warburg, who had become a partner in Kuhn, Loeb, and Co. (see n. 8, below). Later, Frieda Warburg continued the Schiff family's support of work at Samaria by helping to sponsor the Joint Expedition (see the list of contributors in *SS I,* xv).

6 Lyon inserted the list of news outlets—located in Boston, New York, Pittsburgh, and Cincinnati—on small sheets of paper placed inside his journal. The publications ranged from the *Saturday Evening Post* to the J*ewish Criterion* and the *Hebrew Standard* to the *Christian Herald* to the *Watchman*. Besides the corpus of ostraca, Lyon also considered publicizing the discovery of Herod's Temple to Augustus in an attempt to raise funds.

more alarming, perhaps, was the resignation in 1914 of Jacob Henry Schiff from the Museum's oversight committee, on which he had served since 1893.

An atmosphere of gloom characterized Lyon's next three reports. Following the academic year 1916–1917 he wrote, "The treasury of the Museum is in great need of replenishment; but the time seems not opportune for an effort to meet the need. The whole region of the home of the Semites, notably Babylonia and the Holy Land, is now the scene of war" (Lyon 1918M: 245). By 1919, two members of the Museum Committee (Oric Bates, who also served as Registrar to the expedition [*Reisner Diary IV,* 413], and Hervey E. Wetzel) had lost their lives in the war, and as a result of the wartime economy the Museum had to discontinue opening on Sundays (Lyon 1919M: 243).

For the next two academic years (1918–1919 and 1919–1920), Lyon did not even submit a report on behalf of the Museum. Then, on September 25, 1920, Jacob Henry Schiff died. In his will, Schiff left to the Museum a bequest of $25,000, and two of his friends added another $5,000.[7] The generosity and timeliness of those gifts loom large, particularly since Schiff had himself warned on several occasions that his original donation to the expedition must also cover the expense of publication. But, tellingly, Lyon devoted the entire amount to replenishing the endowment of the Semitic Museum in an effort to make it self-supporting, rather than using the funds to continue the field exploration of Samaria—the project that Schiff had so generously launched, and a goal that Lyon had explicitly set in earlier Museum reports. By 1922, Lyon's report made no mention of Samaria, Reisner, the long-delayed publications, or the availability of any funds to restart fieldwork at Samaria (1922M: 279–80). That for the next four reporting cycles (each autumn of 1923–1926) Lyon again filed no report stems not only from the fact that he retired from Harvard in 1921, just months after the passing of his close friend and patron Schiff, but also from the reality that the Semitic Museum appears to have been especially hard hit during those years, when other Harvard museums (including the Peabody, the Fogg Art Museum, etc.) continued to report significant progress in their work.

Lyon's annual reports on behalf of the Semitic Museum did not begin again until the autumn of 1927, when he wrote as *Honorary Curator.* The publication of the *Harvard Excavations at Samaria, Volumes I–II,* which in fact had appeared finally in January 1924, comprised "the most important item" in this communiqué. But Lyon added that substantial increases in the length of the volumes and especially in the costs of printing, binding, and distribution had resulted in much higher production expenses than anticipated. Alarmingly, already by June 15, 1910, Lyon wrote to Reisner that only about $7,500 remained in the Samaria account at the Bursar's office at Harvard and that "from this the expense of publication must be paid" (*Lyon Diary III,* 89; the Bursar later reported, on September 19, that just over $7,700 remained; *III,* 91). Although Mr. Schiff had not lived to see the published results of the project, he had arranged for the generous posthumous bequest mentioned above. But having directed those funds to the Museum itself, Lyon wrote in a later report to the University president that "Mrs. Schiff and her children generously made provision for the additional costs" of publishing the official excavation report (Lyon 1927M: 267). The Schiffs' son, Mortimer, appears to have led the way in this commitment (*HES I,* vii).[8] Half of the approximately 500 copies printed

7 According to most inflation calculators, $30,000 in 1920 would translate roughly to $359,473 in 2015. That same amount of $30,000 in 1905 (the year Schiff made his initial gift to the Semitic Museum), however, would translate to more than twice as much today—$798,121. The devaluation of the dollar (by more than half) in those intervening fifteen years portends the looming effects of the Great Depression.

8 One cannot overstress the generosity of the very successful Schiff family. Jakob Heinrich Schiff's wife, Therese Loeb Schiff (1854–1933), was the daughter of Solomon Loeb and Fanny Kuhn. Solomon Loeb immigrated to the United States from the Rhineland city of Worms, Germany, in 1849. After settling in Cincinnati, he entered the textile business with his cousin, Abraham Kuhn. He soon married Kuhn's sister, Fanny, who gave birth to Therese on November 6, 1854. In 1865, Solomon and Fanny Loeb moved their family to New York, where Solomon, again with his partner Abraham Kuhn, opened the banking house of Kuhn, Loeb, and Co. By that time, the young, devout Jakob Heinrich Schiff, born in Frankfurt-am-Main, Germany, in 1847, had come to the

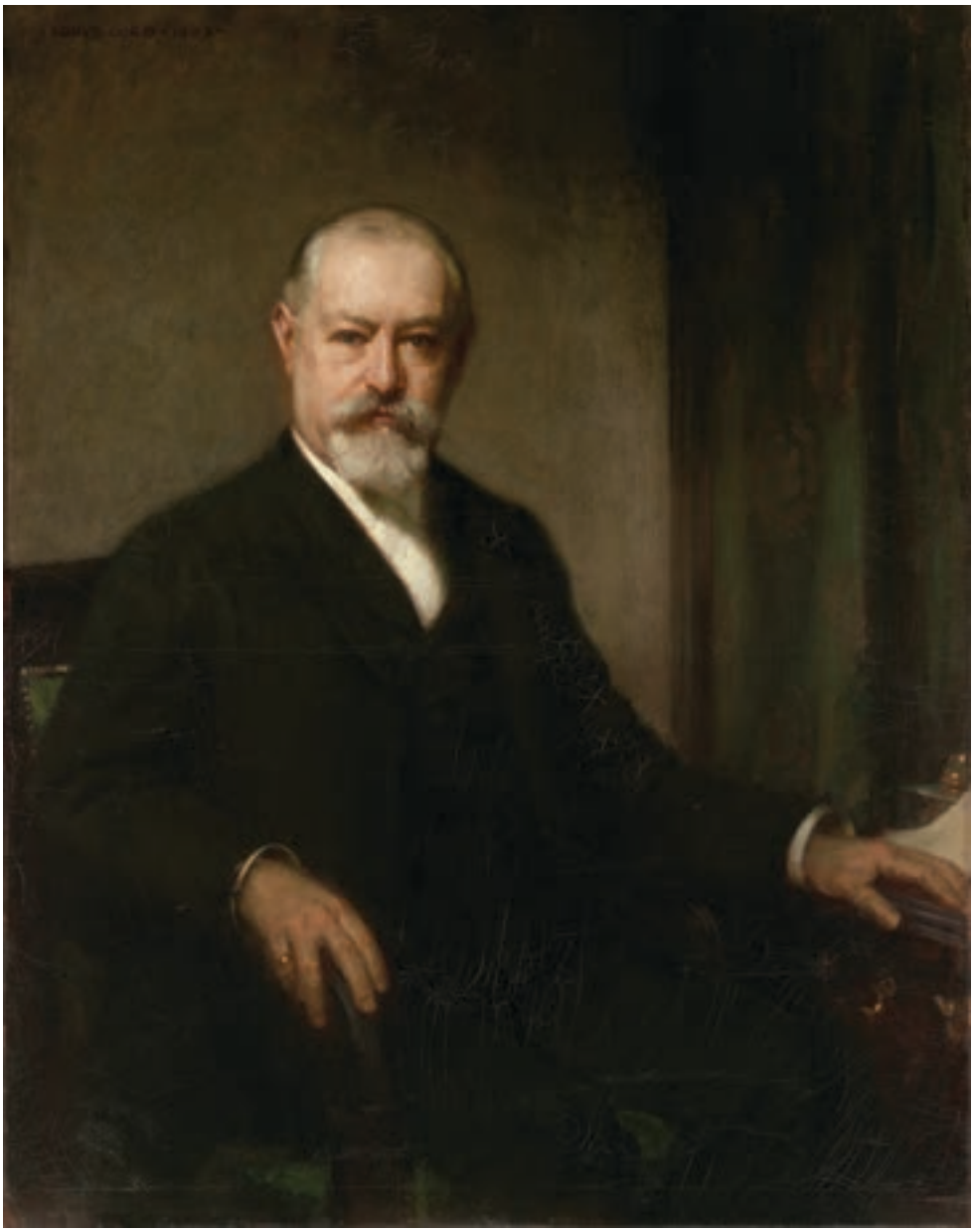


Fig. 58 *Portrait of Jacob Henry Schiff, commissioned to Louis Loeb in 1903 (© President and Fellows of Harvard College; Courtesy of Harvard Art Museums).*

United States and established a successful career in finance. (Later, as indicated, he became known as Jacob Henry. For selected biographies, see Adler 1921 and 1928, Arnsberg 1969, and Cohen 1999; compare Birmingham 1967, Farrer 1974, and Chernow 1993.) After joining the firm of Kuhn and Loeb, he married Therese Loeb in 1875 and merged into his father-in-law's company as a full partner. (For the tightly knit, clan-style marriages between the Schiff, Kuhn, and Loeb families, see Friesel 2002: 61–72.) Therese's brother, James Loeb, also worked for his father's business but retired in 1901 at age 34. He then reestablished residence in Murnau, Germany, by 1912 and became a generous benefactor in his own right. During the years when Jacob Henry held such close ties to the Harvard University Semitic Museum, James Loeb helped found the Max Planck Institute for Psychiatry in Munich, Germany, New York's Institute of Musical Art (now the Julliard School), and Harvard's *Loeb Classical Library.* For a centennial survey of the Loeb Classical Library, see G. H. R. Horsley 2011: 35–58. For more information on Therese Loeb Schiff, see Goldberg 1997: 1205–1207; for James Loeb and Jacob Henry Schiff, see Editorial Staff 1972: 439 and Rosenstock 1972: 960–62, respectively.

Fig. 59 *Selected works and signature of Louis Loeb: a. upper left, portrait of Russian Princess Zonoma, 1907; b. upper right, "Fountain Bleau," date unknown; c. lower center, "Two Women," 1907; d. lower right: Loeb's signature.*

as the first edition were provided free of charge to prominent institutions, and the remaining sets sold for $40 each (thus yielding a paltry $10,000 return for the tremendous outlay and effort since 1905). Lyon's 1927 report also noted that at a ceremony on March 6, 1926, the Museum placed on display an oil portrait of Mr. Schiff, and President Lowell recognized Schiff's unparalleled contributions and service. It remains a testament to Schiff's character that though in the winter of 1903–1904 he had sat for the portrait (fig. 58)—commissioned to Louis Loeb (fig. 59),[9] one of the best known illustrators, painters, and lithographers of the period—he did so only on the condition that the painting "should not be exhibited during his lifetime" (Lyon 1905M: 308; 1922M: 279; 1927M: 268). Similarly, Schiff never attached his name to the title of the investment firm Kuhn, Loeb, and Company, although he had become its leading figure already by the late 1880s (see n. 8, above; also Friesel 2002: 62).

9 Louis Loeb (1866–1909) was born in Cleveland and became a lithographer's apprentice at age 14. By the time he was 19 or 20, he relocated to New York but spent long periods of time studying and working in Paris, where he entered the Académie Julian and the Académie des Beaux-Arts. During this time, his work garnered attention in the Salon of 1895; he won a medal in the Salon of 1896. When he accepted the commission from Jacob Schiff in 1903, he had just recently received the Hallgarten Prize from the National Academy of Design in 1902, followed by the Webb Prize from the Society of American Artists in 1903. His work was featured in a number of publications, including *American Art News* 3.68 (1905), 3 (see also p. 4 for a brief biography); *The Metropolitan Museum of Art Bulletin* 1.2 (1906), 23–24; and *American Art News* 5.23 (1907), 1 (all unsigned). For his obituary, see *American Art News* 7.33 (1909), 6.

Fig. 60 *George Andrew Reisner, as Harvard University graduate student (1891–1893) and in 1914 (presented in Ropes and Fisher 1914: 535).*

It is surely fitting that the authors of the final report memorialized Schiff with these words: "In grateful memory of Jacob Henry Schiff, good citizen, philanthropist of broadest sympathies, generous patron of learning and archaeological research."

But the annual reports for 1928 and 1929 made no further mention of Samaria. Instead, the focus shifted to a joint Harvard University-Fogg Art Museum expedition in Mesopotamia, in the region around Kirkuk, where the discovery of more than 5,000 tablets at Nuzi would soon overtake the attention given to the ostraca from Samaria. Professor Edward Chiera of the University of Pennsylvania (later of the University of Chicago), however, served as leader of this project during its first season (1927–1928), while Robert H. Pfeiffer of Boston University and Lecturer on Assyriology at Harvard became the director in the second season (1928–1929; Lyon 1928M: 277–79; 1929M: 300–302; 1930M: 278). Richard F. S. Starr of the Fogg Museum succeeded him for the third season (1929–1930). In noticeable contrast to the protracted situation with the completion of the Samaria report, the first volume of the *Excavations at Nuzi* appeared promptly in 1929 as Volume V in the *Harvard Semitic Studies* series (Lyon 1930M: 279). During the second to third decades of the twentieth century, then, the exploration of Mesopotamia and, more specifically, the discovery of the Nuzi Tablets superseded the excitement around the Samaria Ostraca. (For the view that, generally speaking, American-sponsored expeditions now focused more on Near Eastern languages, philology, and Assyriology than on biblical studies and "spade archaeology," see Hallote 2009.)

Finally, a third and perhaps determinative reason exists for the delayed publication of the official Samaria volumes: Reisner's own career interests.[10] Despite Lyon's official prediction that "a full account" of the expedition would appear sometime during 1911, his subsequent report for the 1910–1911 academic year (which he submitted on April 1, 1912) acknowledged that "this expectation cannot be realized, because the explorers have had other engagements which made heavy demands on their time" (Lyon 1912M: 224). Eventually, in the autumn of 1913, Lyon reported that, initial delays notwithstanding, the manuscript for the Harvard expedition to Samaria "has now (December, 1912) been received. The committee will use all possible dispatch in getting the work through the press," with the particular hope that the publication would encourage wider financial support and allow the resumption of work at the site. A full year later, in a noticeably brief Museum report for the academic year 1912–1913, Lyon added that the volumes on Samaria were "now in the hands of the printers, and will be published, it is hoped, within a few months" (1914M: 231). But obviously, this goal, too, went unmet.

In retrospect, it seems clear that Reisner and architect Fisher together produced a draft of the Samaria reports in great haste and then became virtually unreachable during the many stages of editing and publication. Since the rising tide of war had not hindered the progress and annual reporting of numerous other museums and fac-

10 For further biographical notes on Reisner's life, see Dawson and Uphill 1972: 244–45.

ets of the University, and since Jakob Heinrich Schiff had shown, in both his life and his legacy, a tremendous willingness to sustain the Samaria project financially, one wonders whether personal factors related to Reisner's main career interests might actually account for the inordinate delay in completing the Museum's first project in Palestine.

After abandoning his study of law from 1889 to 1890 in Indianapolis, Reisner (fig. 60) served as Assistant in Semitic Languages at Harvard from 1891 to 1893. Upon completion of his dissertation, titled "A Review of the Grammatical Development of Noun-endings in Assyro-Babylonian," he received the PhD degree in 1893 and then matriculated at Berlin University for further studies in Assyriology (Ropes and Fisher 1914: 535). While in Berlin, he developed a lasting interest in Egypt under the tutelage of Adolf Erman, and from 1895 to 1896 he served as an assistant in the Egyptian Department of Berlin's Royal Museum. By late 1896, he had returned to the United States and accepted a post as instructor in Semitics at Harvard. The following year he joined the International Catalogue Commission of the Cairo Museum, for which he completed widely recognized works on amulets and funerary boats (Ropes and Fisher 1914: 536). From 1899 to 1905, the year Jakob Heinrich Schiff made his generous gift to Harvard for an excavation in Palestine, Reisner served as director of the Hearst Egyptian Expedition on behalf of the University of California, Berkeley. Multiple campaigns took him not only to the Giza necropolis (whose area the Egyptian government divided equally between America, Italy, and Germany) but also to Deir el-Ballas in Middle Egypt, Naga-ed-Deir, Mesheikh, Mesaʾeed, El-Ahaiwah and, ultimately, into Nubia.[11]

Perhaps more than any other single factor, the interest in and five-year pledge of financial support for his work in 1899 by Phoebe Elizabeth Apperson Hearst (fig. 61) afforded Reisner an opportunity for unfettered pursuit of his chosen academic vocation.

Fig. 61 *Phoebe Elizabeth Apperson Hearst, 1842–1919.*

Hearst, who had become a noted philanthropist, feminist, and suffragist, also cultivated a personal, lifelong interest in ancient cultures and a long-standing connection to the University of California, Berkeley. (She became the University's first female Regent.)[12] With this benefaction, Reisner served not only as the director of the Hearst Expedition to Egypt but also as the Hearst Lecturer in Egyptology at UC Berkeley. When, in time, the California-based project came under the joint sponsorship of Harvard University and the Boston Museum of Fine Arts, Reisner's career path in Egypt seemed clearly defined, fully active, and unremitting.

Then, in 1905 (the year of his first major publication as Hearst Lecturer, namely, the so-called Hearst Medical Papyrus), Harvard's Committee on Exploration in the Orient appointed him as director of the newly sponsored and hastily organized

11 For a more complete listing of Egyptian projects undertaken by Reisner, see Der Manuelian 1992: 1–2.

12 For the sake of piquancy, I might add that, among her many other pursuits, Phoebe Hearst founded the Lowie Museum of Anthropology in California, renamed the Phoebe A. Hearst Museum of Anthropology in 1992. Married to U.S. Senator George Hearst (1820–1891), her only child was newspaper magnate William Randolph Hearst (1863–1951)—whose life story received a devastatingly critical portrayal in Orson Welles' *Citizen Kane* (1941; see www.pbs.org/wgbh/amex/kane2/).

Fig. 62 *George Andrew Reisner (November 5, 1867–June 6, 1942) and the Pyramids of Giza.*

project in Palestine. Similarly, in 1907, following the construction of the Aswan Low Dam by the British from 1898 to 1902, the Egyptian government chose Reisner as director of the Nubian Survey, a massive project designed to identify any archaeological remains that might incur damage if the dam were raised to allow greater flooding from Lake Nasser. Between 1907 and 1909, Reisner explored both banks of the Nile, during which time he identified numerous monuments and architectural complexes endangered by the hydrological project and also cleared multiple cemeteries across the area.

By the time he actually supervised the fieldwork at Samaria in 1909, then, the core of Reisner's career lay in Egypt and he appears to have planned to continue his ongoing projects there alongside his accepted commitments at Samaria. A laconic reference in Lyon's journal refers to a letter he sent to Reisner from Cambridge, Massachusetts, on July 29, 1909, "regarding his proposed work at Gizeh next winter…" (*Lyon Diary III,* 74). By March 11, 1910, another letter had arrived from Reisner asking for more financial investment in the publication of his work in Egypt—a request that Lyon took to President Lowell of Harvard, who, in turn, promised to present it to the Harvard Corporation (*Lyon Diary III,* 82–83). And, in 1912, Italy's portion of the Giza necropolis around the great pyramids fell to Reisner, thereby increasing his control to two-thirds of the entire area (Ropes and Fisher 1914: 537). The expansion of work on this front would surely have compromised any time he had allotted for editing and publication of the Samaria material.

Reisner apparently caught much of Phoebe Hearst's enthusiasm and energy, for throughout his affiliation with the Harvard excavations at Samaria he not only served as the Archaeological Director of the Nubian Archaeological Survey (1907–1909) but also as Assistant Professor of Semitic Archaeology at Harvard (1905–1910, the patronage by Hearst having run out in 1904). By the time he had concluded his fieldwork at Samaria in 1910, his new title, Assistant Professor of Egyptology, fully reflected the well-defined focus of his work. That same year, he became Director of the Harvard–Boston Egypt Expedition to the Sudan (exploring particularly the sites of Kerma/Dukki Gel and Jebel Sesi), continued his work (previously funded through the assistance of Hearst) at the royal cemeteries at Giza, and became Curator in the Egyptian Department of the Boston Museum of Fine Arts, a position he held until his death in 1942. Several years later, in 1914, Reisner accepted a promotion to Professor of Egyptology at Harvard (when, as noted, the Semitic Museum suffered tremendous financial hardship and its founder and the sponsor of the Samaria expedition, Jacob Schiff, died). Reisner himself, however, had already returned to Egypt. Still, he served as full professor until June 6, 1942, when he died in his sleep while in his excavation camp adjacent to the Giza pyramids (fig. 62).[13] The June 8, 1942, notice in the *New York Times* read: "Dr. G.

13 Fisher had died in July 1941; see Ch. 1, fig. 8.

A. Reisner, Egyptologist, 74, Savant, in Charge of Harvard Excavations Since 1905, Dies at Pyramids of Gizeh." He was buried in Cairo. But neither Reisner's promotion in academic rank nor any of his ongoing work in Egypt received mention in Lyon's annual Museum reports of 1915–1930, during which time, as previously noted, the University turned its attention to Mesopotamia.

This brief review of Reisner's *curriculum vitae* demonstrates that, given his clear and lasting commitments to Egypt and related but far-flung responsibilities, Samaria almost certainly could not have commanded center court in his broader academic universe (a fact made clear in most remembrances; e.g., Bull 1942: 8–10). Neither could Samaria have procured the full attention of Reisner's architect, Clarence Fisher, inasmuch as Fisher, besides serving at Samaria, participated in numerous, large-scale field projects across the Near East. For example, he also worked as an assistant, advisor, or even director at Beth-Shean, Megiddo, and Beth-shemesh (Palestine), Jerash and Khirbet Tannur (Jordan), Zawiyet el-Aryan, Girgeh, Giza, Dendera, Thebes, and Memphis (Egypt), Antioch (Syria), and both Nippur in southern Mesopotamia and Tepe Gawra, near Nineveh. Throughout both men's connection with Samaria, and in the interim between the close of those excavations in 1910 and the appearance of the final report in 1924, each one either wrote or contributed to large quantities of research relating to their other projects.[14]

It is commendable, perhaps even laudable, that Reisner and Fisher managed to draft two detailed chronicles of their work at Samaria by December 1912.[15] Conversely, it is unfortunate that over the next 12 years subsequent delays in communications between Harvard and Reisner impeded the ability to complete the publication of these volumes and that any commitment to continuing the field exploration of Samaria quickly vanished after Lyon's receipt of the unedited manuscripts. Ultimately, however, the reports can compete favorably with any of their day. In many respects, they laid the groundwork for future archaeological reporting by excavators at Samaria, Megiddo, Hazor, and other field projects. But Reisner's foremost concentration focused squarely on Egypt, and his preference for working with Egyptians instead of local laborers (and local leadership; see fig. 63) becomes blatantly clear in his private writings.[16]

14 For only a representative sampling, see Reisner 1905; 1907; 1908; 1910; 1910–1927; 1912; 1913a; 1913b; 1923; 1925; 1929; 1931; Fisher 1904; 1905a; 1905b; 1907; 1913; 1917; 1924; 1929; Reisner and Fisher 1913. During the war years, 1914–1918, Fisher also worked in Egypt with the Near Eastern Relief Agency (Glueck 1941: 3). After excavating the palace of Merneptah from 1915–1920 (Schwartz et al. 1984: 33), he straightaway directed the project at Beth-Shean from 1921–1923 on behalf of the University Museum of the University of Pennsylvania (A. Mazar 1993: 214).

15 After having left Samaria for Jerusalem on Wednesday, November 8, 1910, Reisner and Fisher began their work on the published report already by December 5, while still in Egypt (*Reisner Diary VII,* 701). By December 16, Reisner had finished 49 typewritten pages outlining the methods of both excavation and recording, and Fisher had nearly completed plans for the Israelite, Babylonian, and Roman periods (*HES II,* Plans 5, 6, and 8).

16 Even a casual reading of Reisner's unpublished field journals highlights his clear favoritism toward the Egyptians he had brought to Samaria over the local workers he hired from surrounding villages. His notes repeatedly betray a certain frustration, if not disdain, for the so-called lazy, obstructive, selfish, dishonest, and scheming locals and their leaders (see his official statement in *HES I,* 32). He perceived the villagers as filled with "mutual jealousies" and as having "the feelings of children and the intellect of children" (*Reisner Diary V,* 484).

Once, when the most troublesome local official, Sheikh Abd-er-Rahman (fig. 63; see also *HES II,* pl. 84.d), who had very nearly been "banished" in 1908 (*Schumacher Diary I,* 111), lodged official complaints against two of Reisner's field supervisors in a feeble attempt to prevent the authorities in Istanbul from issuing the *firman* (an official document roughly equal to an excavation license today), Reisner responded to the failed effort by shouting at Rahman, "'<u>They</u> [the conspirators] <u>were not able</u>! <u>They were not able</u>! <u>They were not able</u>!!!' … (I shouted each repetition louder and louder thrusting forward my head and glaring into his eyes.) (He shriveled perceptibly.)" He continued, "We do not care a puff of breath about them and their complaints and we do not want to be bothered with them. We want our time for the excavation." In defending the two field supervisors against whom Rahman had leveled the charges, Reisner "snapped out: 'Listen! This one (Shawket) is my perfect friend; that one (el-Mezzit) is my son! We are all three of one mind. Talk on, my son!'" Then, telling Rahman that the excavation would not pay one *metallik* of blackmail, Reisner finished by exclaiming that if he were to ransom the *firman,* "'They [Rahman's co-conspirators] would think that we were afraid. We are not afraid of them or of you or of the devil himself'" (*Reisner Diary IV,* 423–24; Reisner's underscoring).

Fig. 63 *Sheikhs Abd-er-Rahman (left) and Kaid. June 3, 1908 (*HES II, *pl. 84d).*

Prior to his one-year post as assistant in Egyptology at the Berlin Museum in 1895, Reisner's principal training lay in Semitic languages. He had, in fact, little formal education in archaeology. One can imagine, therefore, that his commitment to salvage and sustain Harvard's expedition to Samaria following Schumacher's pedestrian 1908 Season actually represented somewhat of a distraction for him. Like the ancient Sinuhe, Reisner must have felt at times "like a stray bull in the midst of another herd," a man whose heart was not in his body. This conclusion, however, should not diminish our perception of his natural exuberance whenever unearthing great finds or his clear affinity for the Israelite period. When noting at Samaria the style of bonding, fineness of joints, and exactness of dressing in the stones that made up Israelite Rooms 11–12, a presumed Israelite wall fragment in the Lower Terrace 5c, the Casemate Wall system, and the square tower situated outside the enclosing wall southwest of the palace area (see *HES II,* Plan 5; *SS I,* pls. I–II), Reisner exclaimed,

> Later, when dealing with Rahman's nephew, Jasim el-Hawari, Reisner referred to this relative as "the most evil slander-monger in the whole countryside" (*Reisner Diary VII,* 660). Early in the 2010 Season, when awakened on Tuesday, July 5, by a "bad case of insubordination," Reisner delivered the troublemaker to Sheikh Abd-er-Rahman (who himself had recently attempted once again to blackmail Reisner and even to shut down the project) "to be dealt with after breakfast. It took half the sheikhs in the village to induce us [here Reisner actually drew a small smiley-face!] to pardon the man and let him go home. He had to kiss the hand of two of the Egyptians as [a] sign of his submission. I have been expecting insubordination from the new people but nothing quite as flagrant as this. I trust the punishment in this case will prevent any further trouble" (*Reisner Diary V,* 462–63; Reisner's underscoring).
>
> On Monday, September 5, 1910, following a wall collapse that injured two local workers, Reisner wrote: "The ~~Egypti~~ locals (the men) are so slow of perception that they always get caught if near a falling bank or wall; and as one Egyptian said they break like glass" (*Reisner Diary VI,* 569; Reisner's strikethrough). Today, one can hardly imagine recording (even privately) such thoughts about one's staff or volunteers.

Fig. 64a *David Gordon Lyon* in situ: *repairing a saddle at Jaibeh in 1907 (Harvard Semitic Museum photograph from http://www.bostonglobe.com/arts/theater-art/2014/12/18/semitic-museum-honors-founder-compelling-exhibition/MBbySws28ffMmBliErBh8M/picture.html).*

> One has the impression of simplicity and dignity not unworthy of a royal palace. When the results of this year's work are published, I think the world will gain a decidedly new view of the power of the Israelite kingdom[,] of its resources and its architectural works. The Herodian buildings fail I think to equal—they certainly do not exceed—the power of the Israelite structures. Herod's city wall is paltry compared to the ~~Israelite~~ Omri-Ahab city wall.
>
> This is surely the greatest piece of work I ever hope to have a part in.
>
> (*Reisner Diary III,* 300–301, October 5, 1909; Reisner's strikethrough)

To place Herod's architecture and construction techniques (recall the impressive Augusteum, which by this time Schumacher and Reisner had brought to light) so far beneath those of the Iron Age requires further persuasion; but Reisner's having done so reflects not only the personal focus but also the thrust and goals of western archaeologists in general during the declining years of Late Ottoman Palestine.

The 1909 Season (Reisner's first full-time service as on-site director at Samaria) ended on a high note,[17] and the personal thrill that his overall career now provided permeated a letter sent to his former classmates at Harvard:

> I have been hoping for many years that I might be able to attend the twentieth anniversary [i.e., the reunion of the Class of

17 As Reisner began to uncover the southwestern corner of the Israelite Casemate Wall (even though he early on believed it to represent a large building with an open court surrounded by small rooms) and to clarify its stratigraphic connection to the large, rectilinear tower to its immediate south and to the later, overriding "Babylonian Wall" (which turned out to be the Greek Fort Wall; see Ch. 2, n. 6), his great enthusiasm and, ostensibly, his competiveness with other excavators once again become apparent in his journals. On Saturday, October 9, 1909, he wrote of the emerging casemates, "it is possibly the only good building of dated Israelite architecture which will be uncovered in

Fig. 64b *David Gordon Lyon* in situ: *in the field with ASOR students in 1907 (Harvard Semitic Museum photograph from http://www.bostonglobe.com/arts/theater-art/2014/12/18/semitic-museum-honors-founder-compelling-exhibition/MBbySws28ffMmBliErBh8M/picture.html).*

> 1889]; but in June I shall probably be sitting on the hill of Samaria driving a gang of 200 or 300 Orientals, and meditating on the devious ways of the local Turkish officials. I look forward to coming back to some future anniversary to tell the fellows all about it—'How Mugharbi went to prison'; 'The night watchman who nearly caught the murderer'; 'The English Inspector who couldn't identify Fatma Hasan'; 'Why Mustapha Ahmed's house had a cracked roof'; 'The mummy that nobody claimed'; 'How to treat your wives (by a Moslem)'; and some more. With salâms to every one.
>
> (quoted in Ropes and Fisher 1914: 539)

The subsequent 1910 Season carried Reisner and his team to even greater heights, buoyed largely by the discovery of the Samaria Ostraca, which helped eclipse even his earlier exhilaration. Afterwards, however, during the long years preceding publication, heavy time pressures from multiple commitments undoubtedly stole his focus from the dutiful preparation of the Samaria volumes. Although he apparently delegated as much of the work as he could, he himself simply could not check every detail, chase down every source, or visit every collection for comparative study.

It is noteworthy that Curator Lyon (1908; 1909; 1910; 1911a; 1911b), amidst his own multiple and varied duties,[18] assumed responsibility for preparing virtually all the preliminary reports on Samaria

our time—certainly the only thing yet known. It far exceeds anything which MacAlister has ever seen" (*Reisner Diary III,* 311–12). Later, Reisner privately recorded that another important visitor, Père Louis-Hugues Vincent (see n. 1), "accepted our identifications in toto [Reisner's underscoring]; appeared immensely impressed with the methods of work and the amount accomplished; … [and] nearly fell over the [Israelite] wall in his eagerness to examine it closely." Reisner later (October 23, 1909) "received a charming letter from Vincent" (*Reisner Diary III,* 321, 341).

18 Memorials written after his death consistently underscored the diverse, important roles that Lyon had filled (e.g., Pfeiffer 1936; Barton 1936).

during these interim years.[19] (For an exception, see Reisner 1910.) In retrospect, the steady hand and unflagging commitment of David Gordon Lyon (figs. 64a.b–65) seems quite apparent both throughout the early years of the Harvard Semitic Museum and over the course of the Samaria excavations and publications. His tireless efforts at fund raising, the establishment (Unsigned 1901: 488) and daily management of the Museum, his service as liaison between the Museum and the larger world of the University and between the Harvard Semitic Museum and the newly founded American Schools of Oriental Research, and his travels to and activities in the field individually and collectively burnish his image and make Lyon a uniquely pivotal figure in the nascent discipline of archaeology at Harvard. His personal diaries reveal that, at least during the inaugural year of the Samaria project, Lyon served as a member of the on-site professional staff and worked alongside Fisher in the "sketching and recording" as well as in the taking and developing of photographs (*Lyon Diary I,* 37–38, 40, 50). Throughout his involvement with Samaria—even during long stays at the site—Lyon diligently kept two University presidents (Eliot and Lowell) and benefactor Schiff fully apprised of developments in the project's administrative and archaeological progress (e.g., *Lyon Diary I,* 86; *Schumacher Diary I,* 107). And all the while, Lyon apparently received no financial remuneration for his many on-site services (*Lyon Diary III,* 31). It seems only fitting, then, that his name appeared in the author line of the official report following the names of Reisner and Fisher. In many ways, Lyon himself was "the first director of the Harvard Excavations at Samaria" (King 1975: 57). Near the dawn of modern archaeology in Late Ottoman Palestine, he overcame the romanticized grandeur that could easily swirl around any major ruin (fig. 66) to bring the ancient capital of Samaria under controlled, scientific exploration.

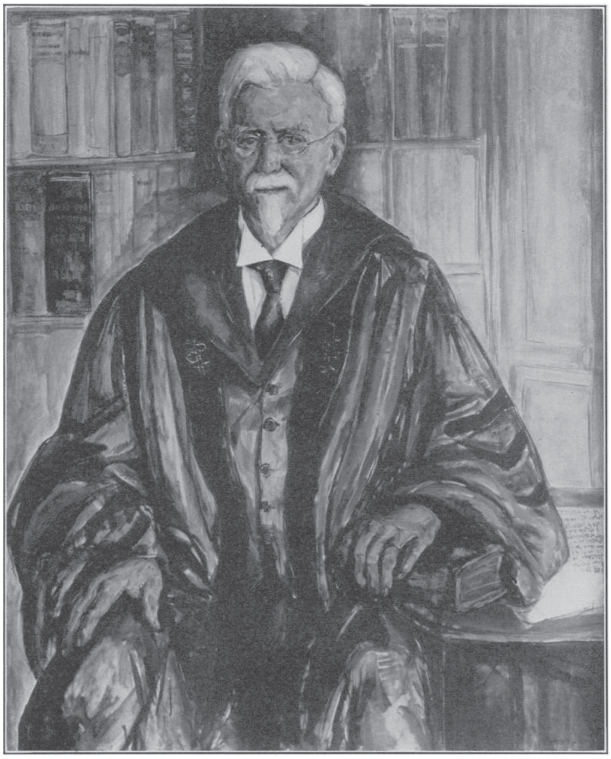


Fig. 65 *David Gordon Lyon (May 24, 1852–December 4, 1935): portrait in a memorial by G. A. Barton, 1936.*

Nevertheless, despite the talent and focus of both Lyon and Reisner, evidence of a hectic pace of composition seems implicit throughout the Harvard volumes and, at points, even becomes explicit. Note, for example, Reisner's unabashed comment regarding the decorated pottery from the Israelite period: "These appear to me to present resemblances to certain Greek and Cypriote types of pottery, but I lack the material necessary to make a comparison" (*HES I,* 275, n. 1). Should one hear in that statement a veiled lament that there was just no time, or perhaps interest, for travel to the nearest Greek and Cypriot collection for first-hand assessment?

Still, despite whatever shortcomings and factual errors may attend *HES I–II,* Reisner clearly understood, desired, and pursued a higher level of archaeological reporting than had ever existed prior

19 The ever diligent Lyon began writing these reports even before leaving the excavation site. On August 2, 1908, he noted in his journal: "Began writing an acc. of the work at Sebastie, to send to Cambridge for publication" (*Lyon Diary II,* 62). He finished the report, which he intended for the October issue of the *Harvard Theological Review,* the following day (*Lyon Diary II,* 63, 67). Ultimately, the article appeared in the January 1909 issue (*Lyon Diary III,* 51).

Fig. 66 *Samaria, based on a lithograph by David Roberts, April 17, 1839 (creativecommons.org; published in Croly 1855, Vol. I, pl. 44; engraved by Louis Haghe).*

to his career. Class notes on the achievements of the 1889 Harvard graduates quickly recognized that

> With the publication of [Reisner's] 'The Early Dynastic Cemeteries of Naga-el-Der, Part I' (1903), a new epoch in scientific research was inaugurated. In this book, the fundamental bases of true historical research are laid down; the painstaking collection of all the actual facts, their methodical arrangement and finally their correct interpretation.
>
> (Ropes and Fisher 1914: 536)

In his forward thinking, George Andrew Reisner remains an example for all engaged in field research today. And, particularly, students of ancient Israel remain indebted to Reisner *the Egyptologist* for his discovery of the Samaria Ostraca.[20] Then, as now, this epigraphic corpus surely represents an outstanding archaeological discovery. One of Reisner's last notices relating to the Ostraca in his private diaries came on Friday, November 10, 1910, while lodging in Jerusalem as he prepared to leave Palestine. After calling on the American School earlier that day, he had visited the American Consulate, where he met with Dr. Ernest William

20 Note the comments by Albright in Bull and Albright 1942: 10. Interestingly, most of Reisner's successors working in Israel/Palestine have remembered him more for his attempts to improve and refine methods of excavating than for his publications. Albright even referred to him as "the father of the field-methods which revolutionized the practice of Palestinian archaeology after the First World War." And he continued by asserting, "Even the two monumental volumes of his *Samaria* are perhaps far less important in a final analysis than his indirect impact on Palestinian archaeology" (Bull and Albright 1942: 10). Albright apparently held Fisher in the same high esteem, since he dedicated the second volume of his reports from Tell Beit Mirsim, not to the team of Reisner and Fisher, but to "Clarence Stanley Fisher, Sc.D., Master of Archaeological Method, and Hugues Vincent, O.P., Master of Palestinian Archaeology" (Albright 1938).

Gurney Masterman. Masterman spent nearly 45 years in Jerusalem and served not only as a physician at the English Mission Hospital for the Jews but also as the head of the Palestine Exploration Fund in the Holy Land (Perry and Lev 2006: 133–46). A palpable sense of pride pervades Reisner's subsequent reflection on their conversation, which included the American Consul-General:

> Masterman was intensely interested in and openly envious of our Israelite ostraca, which he thinks "the greatest find ever made in Palestinian excavations."
>
> (*Reisner Diary VII,* 697; Reisner's underscoring).

APPENDICES AND PLATES

Appendix A Ostraca: Provenance Data and Vessel Descriptions

(based on Ostraca Registry, *HES I,* 232–38)

1	2	3	4	5	6	7	8	9
Ostracon No.	*HES I* List-Reg. Nos.	Year Formula	*HES I* Pott. Types *="like" **="like" implied	*HES I* Cross Refs. Fig.=*HES I* Drawing Pl.= *HES II* Photo	Reisner's Notes	Findspot (according to Ostraca Registry)	Date of Disc.	Vessel Type & Comments
1	4075	10	I.14.i	Fig. 154:6	broken in two	S4-417 N sub	8-23	Bowl: flaring; reddish brown ware; red wash
2	4583	10	I.2.g	∝		S7-776 sub	9-20	Jar: large & heavy; 2 handles; gray ware
3	4614	10	I.14.o	∝	L. edge miss.	S7-772 sub	9-22	Bowl: side & bottom; other examples among ostraca
4	3855	9	*I.14		part miss.	S4-401 sub	8-11	Bowl: rim
5	3863	9	*I.14		dupl. no. 4	S4-418 sub	8-12	Bowl: rim (inscription perpendicular to rim)
6	3997	9	*I.14		dupl. no. 4	S4-417 N	8-19	Bowl: rim
7	4578	9?	*I.14	Pl. 55:e.1	dupl. no. 4	S7-772	9-20	Bowl: bottom
8	3957	9	*I.14	Pl. 55:e.2		S4-416	8-17	Bowl: rim
9	4524	9	*I.14		broken in two	S7-772	9-16	Bowl: bottom
10	4580	9	I.14.m	Pl. 55:e.4	dupl. no. 9	S7-773	9-20	Bowl: flaring rim; rounded bottom; drab yellow ware
11	4526	–	*I.14			S7-772	9-16	Bowl
12	4525	9	I.14.l	∝	reused ostrac.	S7-772	9-16	Bowl: flaring; drab ware (piece missing)
13	4030 4032	10	I.3.e	∝ ∝		S4-417 N	8-22	Jar: large; 2 handles; hard gray ware w/pink-buff surf. int./ext.; diam. ca. 46 cm.
14	4608	9	*I.14		3 frags.	S7-772	9-22	Bowl: sidewall; grayish drab ware
15	4607	10?	–			S7-772	9-21	Bowl: sm. frag.; "Year 10" in unpublished Field Diary VI
16a	3891	10	I.3.a	∝		S4-417	8-15	Jar: large; sidewall; 2 handles?; gray ware
16b	3898	10	I.3.a	∝	dupl. no. 16a	S4-418	8-15	Jar: large; sidewall; 2 handles?; gray ware
17a	3894	10	I.3.a	Pl. 55:d		S4-417	8-15	Jar: large; sidewall; 2 handles?; gray ware
17b	3899	10	I.3.b	∝	dupl. no. 17a	S4-418	8-15	Jar: large; sidewall; 2 handles?; gray ware
18	3931	10	I.3.c	Pl. 55:c		S4-417	8-16	Jar: large; 2 handles?; hard gray ware
19	4031	10	I.3.f	∝		S4-417 N	8-22	Jar: large; 2 handles?; gray ware
20	3995	10	I.3.d	∝	L. half miss.	S4-417 N	8-19	Jar: large; 2 handles?; gray ware
21	3889	10	I.3.a	∝	broken in two	S4-417	8-15	Jar: large; 2 handles?; hard gray ware
22	3932	15	I.7.c	Pl. 55:e.5	dupl. no. 23	S4-417	8-16	Jug: small; just < handle; burnished red ware; pink ext.; yellow int.
23	3917	15	I.2.c	∝	dupl. no. 22	S4-417	8-15	Jar: large & heavy; coarse blackish-gray ware
24	3865 3866	15	*I.14		3865 = 5 joining frags.; 3866 = 2 non-joining frags.	S4-418 [sub?]	8-12	Bowl: flaring sidewall; pinkish drab ware; dark red wash int./ext.; no burnishing; bears Egyptian PN
25	4079 4080	–	*I.14-15		upper part miss.	S4-417 N	8-23	Bowl: flaring sidewall; flat bottom; reddish brown ware; red wash
26	3873	15?	II.16.a	Fig. 156:16a	ink poorly adhered	S4-418	8-13	Bowl: small; upright sides; brown ware; dull burnished red wash [listed among Reisner's "finer" pottery]
27	4553	15	*I.14			S7-773	9-19	Bowl: sidewall; red-brown ware; red wash
28	4552	15	*I.14		3 frags.	S7-773	9-19	Bowl: flaring sidewall; red-brown ware; red wash
29	4555 4556 4579	15	I.15.a	∝ ∝ ∝		S7-773	9-19	Bowl: hollow [i.e., rounded]; sidewall; red ware; red wash
30	3900	15	*I.14		2 frags.	S4-417	8-15	Bowl: flaring rim; red ware; red wash

1	2	3	4	5	6	7	8	9
Ostracon No.	*HES I* List-Reg. Nos.	Year Formula	*HES I* Pott. Types *=“like” **=“like” implied	*HES I* Cross Refs. Fig.=*HES I* Drawing Pl.= *HES II* Photo	Reisner’s Notes	Findspot (according to Ostraca Registry)	Date of Disc.	Vessel Type & Comments
31a	3895	15	*I.14			S4-417	8-15	Bowl: flaring; reddish brown ware; red wash
31b	3992	15	**I.14		joins lower 31a; dupl.?	S4-417	8-16	Bowl: flaring; reddish brown ware; red wash
32	3916	15	I.2.b	∝	diff. rdg.	S4-417	8-15	Jar: large & heavy; sidewall; blackish-gray ware
33	3909	15?	I.7.b	∝		S4-417	8-15	Jug: reddish yellow ware
34	3903 3933	15	I.7.d	∝ ∝		S4-417	8-15/16	Jar: small; reddish brown ware; drab surf.
35	3913 3914	15	*I.14			S4-417	8-15	Bowl: flaring rim; gray ware; reddish brown surf.; red wash (faded)
36	3902 3906	15?	I.7.a	∝ ∝		S4-417	8-15	Jar: small; reddish brown ware; drab ext.; yellow int.
37	4551	15	*I.14		in 2 pieces	S7-773	9-19	Bowl: flaring rim; reddish brown ware; red wash
38	3993	15	I.14.h	Fig. 154:5		S4-417	8-19	Bowl: flaring sidewall; reddish brown ware; red wash
39	4619	15	I.19.a	Fig. 154: 13	dupl. no. 37?	S7-772 (sub?)	9-22	Bowl: red-brown ware; red wash
40	4527	–	I.18.a	∝		S7-772	9-16	Bowl: hollow [i.e., rounded]; rim frag.; drab ware
41	4550	–	*I.14		from the end of an ostrac.	S7-776	9-19	Bowl: flaring bottom; yellow drab ware; red wash int.
42	3994	15	*I.14		3 frags.	S4-417 N	8-19	Bowl: flaring sidewall; reddish brown ware; heavy red wash
43	3875	?	*I.14		joins no. 44	S4-418	8-12	Bowl: flaring rim; reddish brown ware; red wash
44	3867	15	**I.14		joins no. 43	S4-418 [sub?]	8-13	Bowl: flaring rim; reddish brown ware; red wash
45	3896	15	*I.14		joins no. 46; dupl. of 46-47	S4-417	8-15	Bowl: flaring; reddish brown ware; red wash
46	3915	15	**I.14		same bowl 45	S4-417	8-15	Bowl: flaring; reddish brown ware; red wash
47	4616	15?	*I.14			S7-772	9-22	Bowl: flaring; red-brown ware; red wash
48	3897 3990B	15	*I.14		join no. 49	S4-418 S4-417 N	8-15 8-19	Bowl: flaring rim; hard reddish brown ware; red wash
49	3990A	–	**I.14		part of no. 48	S4-417 N	8-19	Bowl: flaring rim; hard reddish brown ware; red wash
50	4630	15	I.20.b	∝		S7-772 N	9-23	Bowl: bottom + ring base; drab ware; faded red wash?
51	4661	10	*I.14	Pl. 55:e.3		S7-772 W	9-26	Bowl: rim; brown ware; red wash
52	4629	15	I.14.n	∝	poss. erasure	S7-772	9-23	Bowl: rim (> 1 frag.); brown ware; red wash
53	3890	10	*I.3.a			S4-417	8-15	Jar: large; 2 handles?; hard gray ware
54	4171	10	I.3.g	∝		S7-723	9-1	Jar: large; 2 handles?; hard gray ware
55	4660	10	I.3.i	∝		S7-772 W	9-26	Jar: large; 2 handles?; hard gray ware
56	4617	15?	II.11.f	∝	parts miss.	S7-772	9-22	Bowl: flaring rim; brown ware; burnished red wash
57	4582	–	I.3.h	∝		S7-776	9-20	Jar: large; sidewall; thin walled; gray ware
58	4554	15	*I.14		3 frags.	S7-773	9-19	Bowl: rim; brown ware; red wash (compl. ostracon)
59	4581	–	I.2.f	∝	2 frags.	S7-776	9-20	Jar: large & heavy; gray ware
60	4627	–	I.15.b	∝		S7-772	9-23	Bowl: hollow [i.e., rounded]; sidewall; brown ware; red wash
61	3864	15	I.2.a	∝		S4-418 [sub?]	8-12	Jar: large; coarse; complete
62	3934	–	I.2.d	∝		S4-417	8-16	Jar: large & heavy; sidewall frag.
63	3991	13? 14?	I.2.e	∝		S4-417 N	8-19	Jar: large & heavy

Appendix B Ostraca: Sequence of Discovery

(according to unpublished *Reisner Diary V,* 1910)

1	2	3	4	5	6	7	8
Published Ostracon No.	*HES* / List — Reg. Nos.	Field Diary — Reg. Nos.	Field Diary — Vol. & Page	Date of Diary Entry — 1910	Findspot (according to Ostraca Registry)	Drawn in Field Diary?	Comments in Field Diaries
4	3855	–	*V,* 515	Aug. 11	S4-401 sub	+	
5	3863	–	*V,* 515-17	Aug. 12	S4-418 sub	+	
61	3864	–	*V,* 517	Aug. 12	S4-418 [sub]	+	"reading certain" (*V,* 517)
24	3865	–	*V,* 517-18	Aug. 12	S4-418 [sub]	+	
24	3866	–	*V,* 518	Aug. 12	S4-418 [sub]	+	
44	3867	–	*V,* 518-19	Aug. 12	S4-418 [sub]	+	
unpub.	–	–	*V,* 519	Aug. 12	S4-418 sub ?	–	
unpub.	–	–	*V,* 519	Aug. 12	S4-418 sub ?	–	
26	3873	–	*V,* 524	Aug. 13	S4-418 sub ?	+	"practically illegible" (*V,* 523)
unpub.	–	3877	*V,* 524	Aug. 13	S4-418 sub ?	+	"practically illegible" (*V,* 523)
43(?)	3875	–	*V,* 524	Aug. 13	S4-418 sub ?	+	"practically illegible" (*V,* 523)
unpub.	–	3880	*V,* 525	Aug. 13	S4-418 sub ?	+	"practically illegible" (*V,* 523)
unpub.	–	3869–3881	*V,* 525	Aug. 13	S4-418 sub ?	–	"practically illegible" (*V,* 523)
21	3889	–	*V,* 526	Aug. 15	S4-417	+	"every letter absolutely certain!" (*V,* 526)
16a	3891	–	*V,* 526	Aug. 15	S4-417	+	"every letter absolutely certain!" (*V,* 526)
17a	3894	–	*V,* 527	Aug. 15	S4-417	+	"every letter certain!" (*V,* 527)
17b	3899	–	*V,* 527	Aug. 15	S4-418	+	
18	3931	–	*V,* 527	Aug. 15	S4-417	+	
16a dupl.	3891	–	*V,* 528	Aug. 15	S4-417	+	"not so clear as 13–15 [i.e., the preceding 3 frags.] but nevertheless quite certain" (*V,* 528)
53	3890	–	*V,* 528	Aug. 15	S4-417	+	"every letter absolutely certain" (*V,* 528)
unpub.	–	3892	*V,* 528	Aug. 15	S4-417	+	"absolutely certain" (*V,* 528)
unpub.	–	3893	*V,* 528	Aug. 15	S4-417	+	
21	3889	–	*V,* 528	Aug. 15	–	–	
53	3890	–	*V,* 528	Aug. 15	–	–	
16a	3891	–	*V,* 528	Aug. 15	–	–	
17a	3894	–	*V,* 528	Aug. 15	–	–	
31a	3895	–	*V,* 528	Aug. 15	–	–	
45	3896	–	*V,* 528	Aug. 15	–	–	
48	3897	–	*V,* 528	Aug. 15	–	–	
16b	3898	–	*V,* 528	Aug. 15	–	–	
17b	3899	–	*V,* 528	Aug. 15	–	–	
30	3900	–	*V,* 528	Aug. 15	–	–	
36	3902	–	*V,* 528	Aug. 15	–	–	
34	3903	–	*V,* 528	Aug. 15	–	–	
36	3906	–	*V,* 528	Aug. 15	–	–	
33	3909	–	*V,* 528	Aug. 15	–	–	
35	3913	–	*V,* 528	Aug. 15	–	–	
35	3914	–	*V,* 528	Aug. 15	–	–	
46	3915	–	*V,* 528	Aug. 15	–	–	

1	2	3	4	5	6	7	8
Published Ostracon No.	*HES I* List — Reg. Nos.	Field Diary — Reg. Nos.	Field Diary — Vol. & Page	Date of Diary Entry — 1910	Findspot (according to Ostraca Registry)	Drawn in Field Diary?	Comments in Field Diaries
32	3916	–	*V*, 528	Aug. 15	–	–	
23	3917	–	*V*, 528	Aug. 15	–	–	
18	3931	–	*V*, 531	Aug. 16	S4-417	–	
22	3932	–	*V*, 531	Aug. 16	S4-417	–	
8	3957	–	*V*, 534	Aug. 17	S4-416	–	
							Thursday, August 18, 1910 — Close of Excavations in Summit Strip 4 and Start of Backfilling from SS 7
48	3897 3990B	–	*V*, 537	Aug. 19	S4-417N	–	
49	3990A	–	*V*, 537	Aug. 19	S4-417N	–	
63	3991	–	*V*, 537	Aug. 19	S4-417N	–	
31b	3992	–	*V*, 537	Aug. 19	S4-417	–	
38	3993	–	*V*, 537	Aug. 19	S4-417	–	
42	3994	–	*V*, 537	Aug. 19	S4-417N	–	
20	3995	–	*V*, 537	Aug. 19	S4-417N	–	
6	3997	–	*V*, 537	Aug. 19	S4-417N	–	
48	–	–	*VI*, 539	Aug. 20	S7-357N	–	Same type of debris under Roman Street C (cf. Plan 8) as found in S4-417 to the south
31a	–	–	*VI*, 539	Aug 20	S7-357N	–	Same type of debris under Roman Street C (cf. Plan 8) as found in S4-417 to the south
13	4030	–	*VI*, 543	Aug. 22	S4-417N	–	Recorded in a section dealing with Summit Strip 7
19	4031	–	*VI*, 543	Aug. 22	S4-417N	–	Recorded in a section dealing with Summit Strip 7
13	4032	–	*VI*, 543	Aug. 22	S4-417N	–	Recorded in a section dealing with Summit Strip 7
1	4075	4075	*VI*, 544	Aug. 23	S4-417N sub	–	Recorded in a section dealing with Summit Strip 7
25	4079 4080	4079 4080	*VI*, 544	Aug. 23	S4-417N	–	Recorded in a section dealing with Summit Strip 7
54	4171	4171	*VI*, 562-63	Sept. 1	S7-723	+	"in the northern end of the long street or corridor of the fourth series number 417 in S4"
9	4524	4524	*VI*, 588-90	Sept. 16	S7-772	+	"from under Roman walls"
12	4525	4525	*VI*, 588-90	Sept. 16	S7-772	+	"from under Roman walls"; "Baal-zemer and Baala are the only two Baal names we have"
11	4526	4526	*VI*, 588-90	Sept. 16	S7-772	+	"from under Roman walls"; in the field diary, Reisner restored [ישנ.ל.אח] at the beginning of the second line
40	4527	4527	*VI*, 588-90	Sept. 16	S7-772	+	"from under Roman walls"
41	4550	4550	*VI*, 594	Sept. 19	S7-776	+	
37	4551	4551	*VI*, 594	Sept. 19	S7-773	+	Reisner understood the fourth letter from the end of the inscription as a possible mistake for *ʾaleph*
28	4552	4552	*VI*, 594	Sept. 19	S7-773	+	
27	4553	4553	*VI*, 595	Sept. 19	S7-773	+	"perhaps a reminiscence of Omri's conquest of [illegible]"
58	4554	4554	*VI*, 595	Sept. 19	S7-773	+	last 4 letters (בדיו) of l. 1 "possible but not certain"
29	4555 4556	4555 4556	*VI*, 595	Sept. 19	S7-773	+ +	4555 = "interesting on account of form of letter"; these 2 frags. join Reg. No. 4579 below
unpub.	–	4557	*VI*, 595	Sept. 19	?	–	"a large potsherd (bowl) with only a few letters on the rim"
6 or 7?	–	4578	*VI*, 596	Sept. 20	S4-417N or S7-772 (?)	+	(see Chapter 2, Summary for discussion)

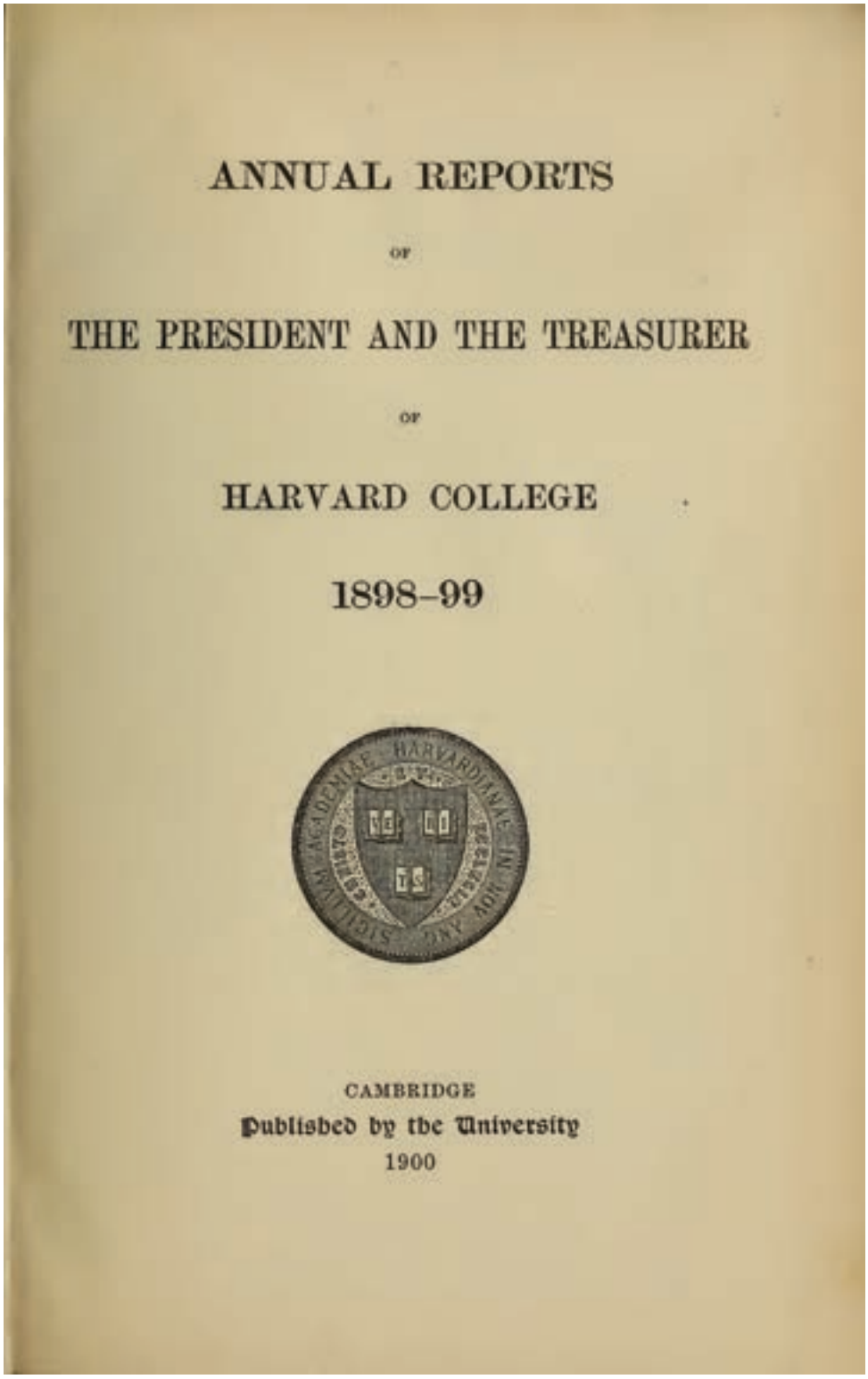
1	2	3	4	5	6	7	8
Published Ostracon No.	*HES I* List — Reg. Nos.	Field Diary — Reg. Nos.	Field Diary — Vol. & Page	Date of Diary Entry — 1910	Findspot (according to Ostraca Registry)	Drawn in Field Diary?	Comments in Field Diaries
29	4579	4579	*VI,* 597	Sept. 30	S7-772	+	joins fragments 4555 + 4556 from S7-773 (above)
10	4580	4580	*VI,* 597	Sept. 30	S7-772 (773 in *HES I*)	+	"In 773, against face of Isr. wall"; starting at *lamed* in l. 2: "this ostracon was erased from ל to end and rewritten"
59	4581	4581	*VI,* 598	Sept. 20	S7-776	+	"In 776, against face of Isr. wall"
57	4582	4582	*VI,* 598	Sept. 20	S7-776	+	"In 776, against face of Isr. wall"
2	4583	4583	*VI,* 598	Sept. 20	S7-776 (sub in *HES I*)	+	"In 776, against face of Isr. wall"
unpub.	–	4584	*VI,* 598	Sept. 20	S7-776	+	"In 776, against face of Isr. wall"; unpublished reading — . האל [?]
14	4608	4608	*VI,* 600	Sept. 21	S7-772	+	"In S772, extreme north end"
unpub.	–	4609	*VI,* 600	Sept. 21	S7-772	+	"In S772, extreme north end"; field drawing shows letters near the rim: בשת . הת — prob. = formula "In the ninth year"
15	4607	4607	*VI,* 600	Sept. 21	S7-772	+	"In S772, extreme north end"; Reisner noted the unusual form of the letter *yod;* in his field journal, Reisner restored the beginning of the inscription as "Year 10"
3	4614	4614	*VI,* 601	Sept. 22	S7-772 sub	+	"In S7-772 north end . . . in the old Trench F"; letters בעל in l. 3 marked as "?"
unpub.	–	4615	*VI,* 601	Sept. 22	S7-772 N ?	+	"In S7-772 north end . . . in the old Trench F"; unpublished reading (2 lines) — . ישן . ין // [ ]שמ[ ]
47	4616	4616	*VI,* 601	Sept. 22	S7-772	+	"In S7-772 north end . . . in the old Trench F"; "this is a duplicate of nos. 31–32 . . . no. 32 appears to vary in last line"
56	4617	4617	*VI,* 602	Sept. 22	S7-772	+	"In S7-772 north end–in the old Trench F"; "the mention of a Nimshi [in l. 2, with restored *yod;* cf. 1 Kgs 19:16 et passim] the name borne by Jehu's father (or grandfather) is suggestive"
39	4619	4619	*VI,* 602	Sept. 22	S7-772 sub?	+	"In S7-772 north end–in the old Trench F"
60	4627	4627	*VI,* 602, 606	Sept. 23	S7-772	+	"Trench F"; contains "the name of another vineyard" (כרם)
unpub.	–	4628	*VI,* 603, 606	Sept. 23	S7-772	+	"Trench F"
52	4629	4629	*VI,* 603, 606	Sept. 23	S7-772	+	"Trench F"; "the omission of שת [in the opening year formula] is to be noted"; "אביו = אֲבִיָּהוּ , אֲבִיָּה"
50	4630	4630	*VI,* 603, 606	Sept. 23	S7-772 N	+	"Trench F"; "To מנעה cf. הַנֵּעָה" [cf. Josh 19:13, Neah in the tribe of Zebulun]
55	4660	4660	*VI,* 608-609	Sept. 26	S7-772 W	*	discovered "In clearing away Roman wall along W. side of S7-772. . ."
51	4661	4661	*VI,* 608-609	Sept. 26	S7-772 W	*	discovered "In clearing away Roman wall along W. side of S7-772. . ."; Reisner read l.3 as "Aḥa the Jew" ([ ]היהד . אחא)
unpub.	—	—	*VII,* 651	Oct. 22	S 11	+	found "in the yellow debris at the mouth of the cave in S11"; no Reg. No recorded; . . .] ליה [. . .

Appendix C Non-epigraphic Pottery with Provenance Data Matching those of the Ostraca

(Pottery Registry, *HES I*, 277–79)

1	2	3	4	5	6	7	8
	HES I List Nos.	*HES I* Pott. Types — *="like" **="like" implied	*HES I* Refs.	Reisner's Notes	Findspot	Date of Disc.	Vessel Type & Comments
1	380	I.19.b	paired w/ 4619 = Fig. 154:13	Dis. No.	S7-772 sub [1]	9-22	Bowl: hollow [i.e., rounded]; ring foot; brown ware; red wash
2	382	II.17.a	Fig. 156:17a	Dis. No.	S4-418 sub [1]	8-13	Tray: wide, deep, w/narrow rim; brown ware; red wash; pebble-burnish int. (listed among finer ware)
3	542	I.1.c	–	Dis. No.	S7-772 [12]	9-26	Jar: 2 handles; brown-black ware; micaceous or white limestone particles; smooth brown or red surf.
4	545	I.12.c	Fig. 153:13	Dis. No.	S7-772 sub [1]	9-15	Ring Stand: hard red ware; diam. 20 cm; H., 12 cm
1a	379	I.14.j	Fig. 154:7	Dis. No.	S4-772 sub [1] [sic: S7–?]	–	Bowl: flaring, straight sidewalls; flat bottom; red/brown ware; brown/black core; red wash
2a	3841	I.16.a	Fig. 154:9	Reg. No.	S4-404 sub [0]	8-10	Bowl: slightly "underlined" int. rim; drab ware; red wash
3a	3842	I.13.a	Fig. 154:1	Reg. No.	S4-404 sub [0]	8-10	Bowl: flaring sidewalls, slightly concave; brown/drab ware; red mouth
4a	3846	I.14.g	Fig. 154:4 Pl. 64:f	Reg. No.	S4-404 sub [0]	8-10	Bowl: flaring, straight sidewalls; flat bottom; red/brown ware; brown/black core; red wash
5a	4112	I.17.a	Fig. 154:10	Reg. No.	S7-720 sub [0]	8-25	Bowl: hollow [i.e., rounded]; slightly "underlined" int. rim; drab ware; red wash
6a	4113	I.22.a	Fig. 154:16	Reg. No.	S7-720 sub [0]	8-25	Bowl: hollow [i.e., rounded]; round bottom; drab ware; wet smoothed
7a	4116	I.18.c	Fig. 154:11	Reg. No.	S7-720 sub [0]	8-25	Bowl: hollow [i.e., rounded]; flat base; drab
8a	4117	I.18.b	Fig. 154:12	Reg. No.	S7-720 sub [0]	8-25	Bowl: hollow [i.e., rounded]; flat base; drab
9a	–	–	Fig. 154:19	–	Ahab Court. Floor [?]	–	Cooking Pot: wide mouth; prob. w/handles
10a	–	–	Fig. 154:20	–	Ahab Court. Floor [?]	–	Cooking Pot: wide mouth; prob. w/handles
א	4527	I.18.a	cf. Fig. 154:11–12	Reg. No.	S7-772 [12]	9-16	Bowl: hollow [i.e., rounded]; flat base; drab
ב	4630	I.20.b	cf. Fig. 154:14	Reg. No.	S7-772 N [1]	9-23	Bowl: flaring; ring foot; drab ware; wash faded

Appendix D D. G. Lyon's Report to the President of Harvard University for the Academic Year 1898–1899, Showing the Financial Generosity of Jakob Heinrich Schiff



ANNUAL REPORTS

OF

THE PRESIDENT AND THE TREASURER

OF

HARVARD COLLEGE

1898–99

CAMBRIDGE

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1900

THE SEMITIC MUSEUM.

To the President of the University: —

Sir, — Since the report for 1897–98 there have been considerable additions to the Museum collections. From Mr. Jacob H. Hecht, of Boston, we have received by gift a diminutive roll of the *Torah* or Law, a copy of *Hagadah for Passover with English Translation*, and two Jewish "prayer shawls." Mrs. George Linsley, of Newtown, Conn., eighth in descent from President Chauncey, has presented a small Egyptian statuette, brought from Egypt about 1848.

The mounted birds belonging to the Selah Merrill collection have been transferred from the Divinity Faculty Room to the Museum, and most of the other objects of this collection have been unpacked and placed on exhibition, though not yet adequately provided with labels. A few boxes are still in the basement of the Divinity Library. This large accession has made much rearrangement necessary, and has greatly crowded our space. Several cases now in process of construction will give some relief. The transfer of the Merrill collection to the Semitic Museum is in accordance with the terms under which contributions for the purchase were made.

The collection of Babylonian antiquities mentioned in my last report was removed in February, 1899, from the Boston Museum of Fine Arts, where it had been on deposit since August, 1898, to the Semitic Museum. After long negotiation with its owner, it was purchased in July, 1899. This is the most important collection which the Museum has yet acquired, containing nearly 500 cuneiform clay tablets, over 100 stone seal cylinders, many alabaster vases, terra-cotta cones, masks, statuettes, fragments of ivory, and other objects. This collection fills one of the table cases.

It has been necessary to remove to the basement of the Peabody Museum the winged lion which has hitherto stood on the first floor of that building. The space which it occupied was needed to make room for exhibits of the Peabody Museum.

Dr. John Orne has continued his work of cataloguing the Arabic manuscripts. In recognition of his past services in this regard the President and Fellows have appointed him Curator of the Arabic manuscripts belonging to the Museum.

During the year two courses of public lectures on the Semitic collections were held in the lecture-room of the Peabody Museum. The first course was informal in character and was given in January, February, and March. The objects were taken up chronologically, and the lecturers were Professors Lyon, Toy, and H. L. Warren, and Dr. John Orne.

The second course, in March and April, was on the Selah Merrill collection, and was intended primarily for teachers of the Bible in Sunday Schools and elsewhere. Professor Goodale spoke on the Vegetation of Palestine; Professors Lyon, Ropes, and Toy, on Palestine under the Greeks, the Romans, and the Moslems, respectively; Dr. R. A. Daly, on the physical features of Palestine; Mr. Ralph Hoffmann, on the Birds of Palestine; and Dr. Theodore F. Wright, on life in Palestine at the present day. The average attendance on the second course was about 250 persons, and both courses were illustrated by specimens belonging to the Museum. The interest taken in these lectures illustrates how useful the Semitic Museum may be made to the community. This usefulness will be greatly increased when the collection shall be properly housed and displayed. At the close of the second course the hearers by rising vote expressed the wish that other such courses might be arranged. It is hoped that something may be done to meet that wish during the year 1899–1900.

In January, 1899, Mr. Jacob H. Schiff renewed his offer, first made several years ago, of $25,000 toward the erection of a building. A condition of this offer was that an equal sum for the object should be secured from other sources, and subsequently a time limit was fixed at July 1, 1899. Steps were promptly taken to bring the subject to the attention of persons in Boston, Cambridge, and vicinity, from whom it seemed reasonable to hope for assistance. When the end of June approached about $18,000 had been promised toward the second $25,000. On being asked if he would be willing to extend the time, Mr. Schiff replied that he would not; but he offered to double his own subscription, thus furnishing the necessary $50,000, provided the other contributors would allow their subscriptions to be used toward purchasing materials for the collections of the Museum. To this proposal all consented. Some

later contributions bring the total of these gifts up to $19,240. There are 98 subscribers and the various amounts contributed are as follows: —

1	of $1,500	$1,500	1	of $70	$70
10	" 1,000	10,000	22	" 50	1,100
1	" 600	600	1	" 40	40
5	" 500	2,500	1	" 30	30
1	" 250	250	12	" 25	300
1	" 200	200	2	" 20	40
3	" 150	450	15	" 10	150
20	" 100	2,000	2	" 5	10
					$19,240

The appeal for aid was based on the importance of the Semitic contributions to culture, and the value of our collections not only to the University but to the community in general. It is a great pleasure to bear witness to the cordial manner of the response to this appeal. Some of the most efficient helpers were persons whose names do not appear in the list of donors. The entire sum of nearly $20,000 is available for purchases, and makes large additions to the collections possible.

It seems desirable that the building to be erected should not be too remote from the centre of University life nor from the other great museum buildings. The President and Fellows have assigned for the Semitic Building a site on Divinity Avenue south of the Peabody Museum, a location which meets the requirements of the case.

Plans are now in course of preparation, and it is hoped to begin the construction in the spring of 1900. The new building is to furnish a home not only for the collections of the Museum, but also for the Semitic Library and the Semitic instruction. The first floor will probably contain the lecture rooms and the Library, while the second and third floors will contain the exhibition halls. The building will be so designed as to allow enlargement with the growing needs of the collections.

D. G. LYON, *Curator*.

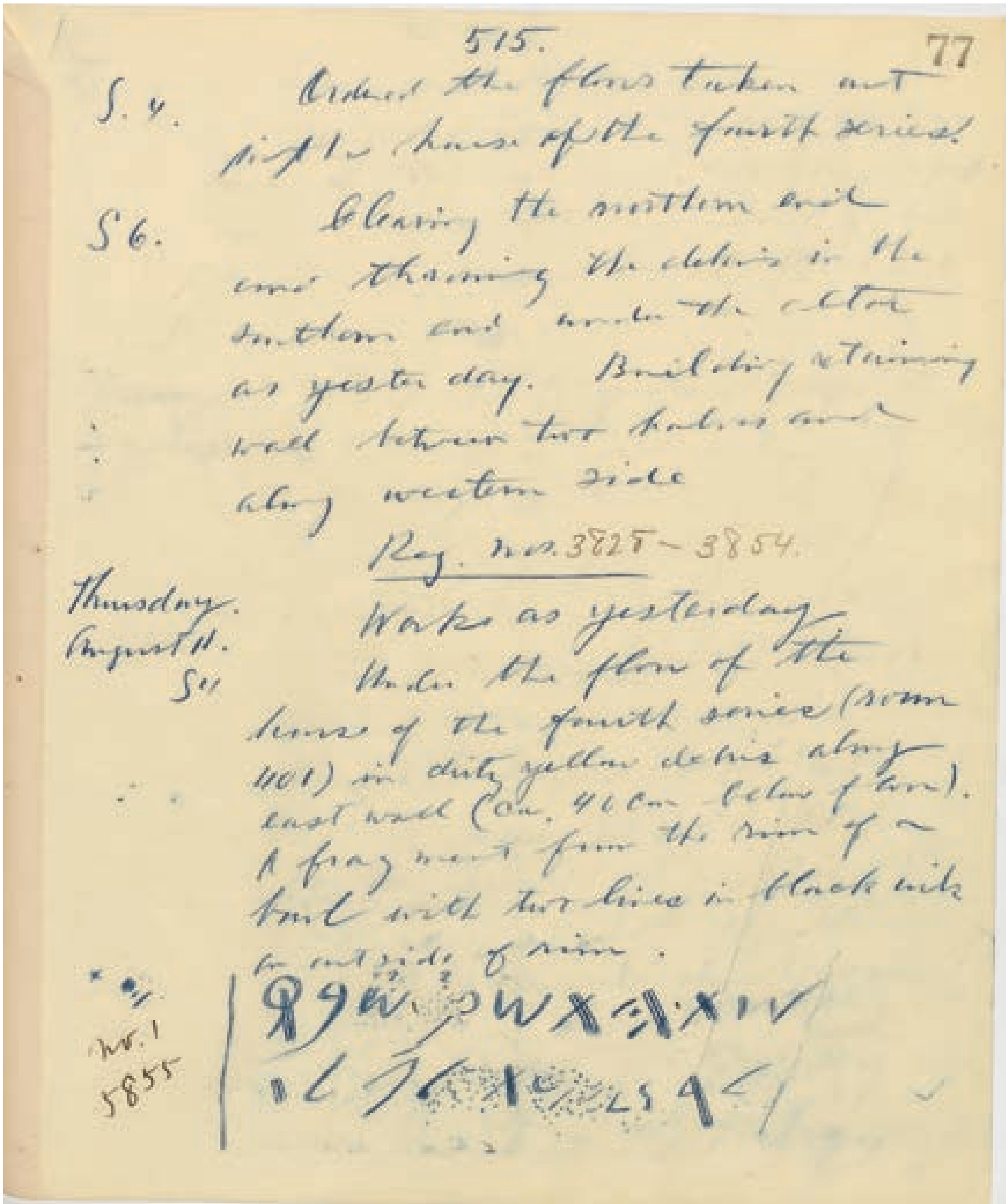
Appendix E Reisner's Field Drawings of Selected Ostraca

Based on *Reisner Diaries V–VI,* which are accessible through the Harvard University Library Open Collections Program: Expeditions and Discoveries, Sponsored Exploration and Scientific Discovery in the Modern Age (http://ocp.hul.harvard.edu/expeditions/reisner.html)

In his unpublished field records, Reisner presented the initial drawings for 39 of the 63 ostraca that he registered and ultimately presented in *HES I,* 239–43. In addition, the diaries contain 8 unpublished inscriptions, some of which were located and photographed later in Istanbul by I. T. Kaufman (see Ch. 1, n.2).

The following numbers represent the published sequence of ostraca in *HES I*.
Numbers in bold appear in both the field diaries and the official report. Numbers in italics do not appear in field diaries. Note that in some cases the published order and the registration numbers differ between the diaries and the final report (cf. App. A).

1	*22*	**43**
2	*23*	**44**
3	**24**	*45*
4	*25*	*46*
5	**26**	**47**
6	**27**	*48*
7	**28**	*49*
8	**29**	**50**
9	*30*	**51**
10	*31*	**52**
11	*32*	**53**
12	*33*	**54**
13	*34*	**55**
14	*35*	**56**
15	*36*	**57**
16/16a	**37**	**58**
17a–b	*38*	**59**
18	**39**	**60**
19	**40**	**61**
20	**41**	*62*
21	*42*	*63*



515. 77

S. 4. Ordered the floors taken out
fifth house of the fourth series.

S6. Clearing the northern end
and throwing the debris in the
southern end under the altar
as yesterday. Building & training
wall between two halves and
along western side

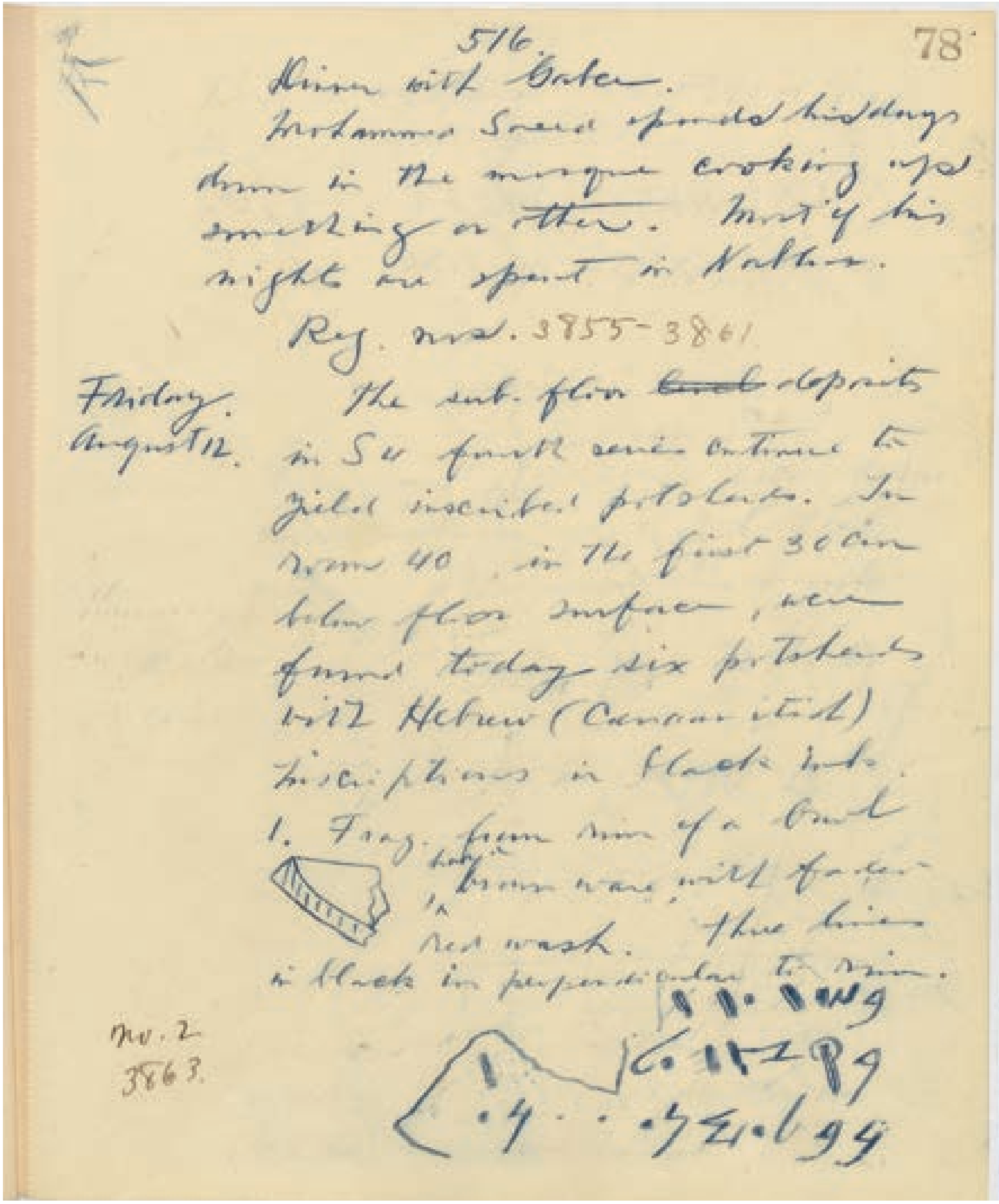
Reg. nos. 3828–3854.

Thursday. August 11. S4
Works as yesterday
Under the floor of the
house of the fourth series (room
401) in dirty yellow debris along
east wall (ca. 46 cm below floor).
A fragment from the rim of a
bowl with two lines in black ink
on outside of rim.

No. 1
3855

Field Diary V.515

possibly related to published ostracon No. 4 (Reg. No. 3855)



516. 78

Dinner with Gates.

Mohammed Saeed spends his days down in the mosque cooking up something or other. Most of his nights are spent in Nablus.

Reg. nrs. 3855–3861

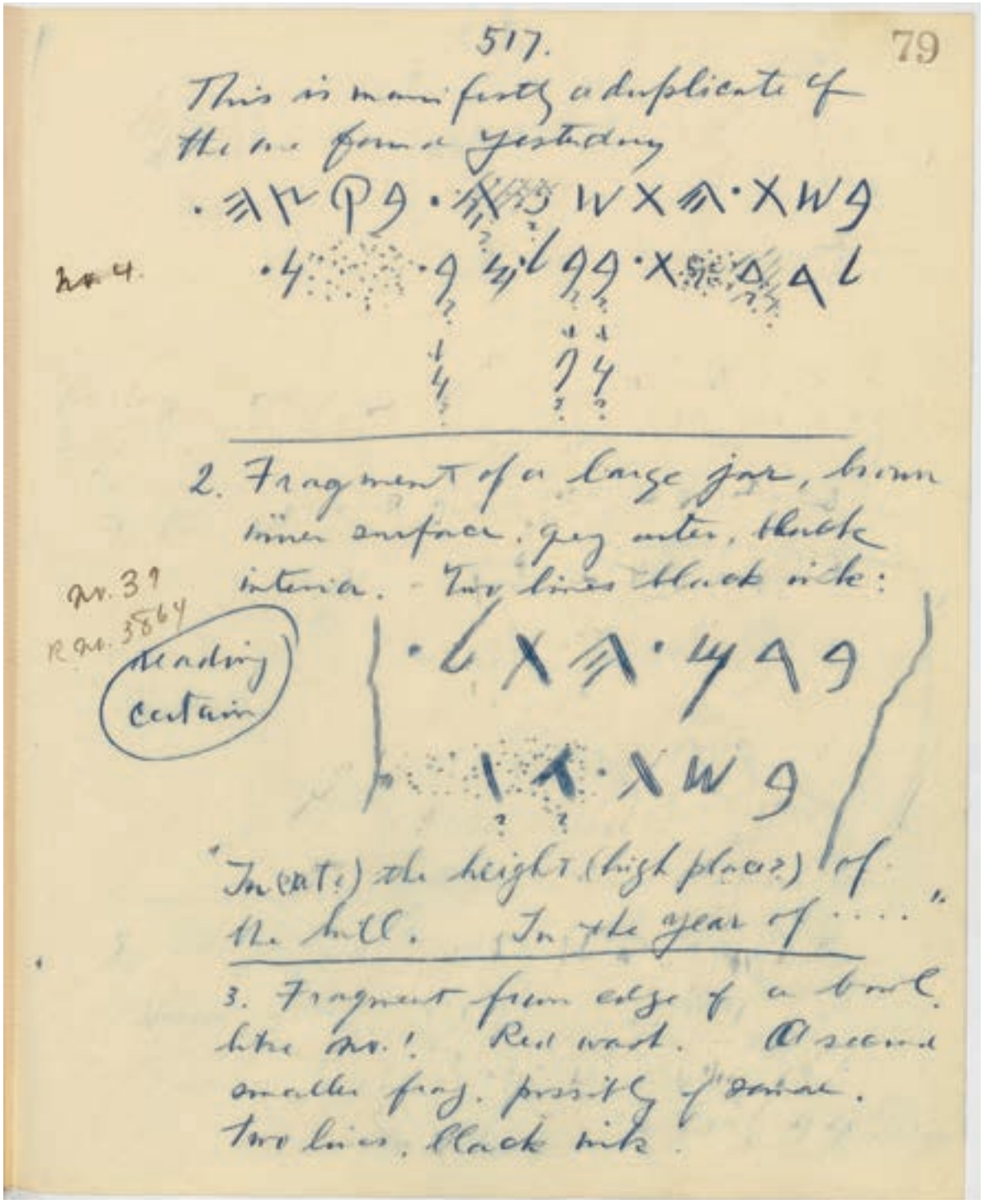
Friday. August 12. The sub-floor ~~sand~~ deposits in S4 fourth series continue to yield inscribed potsherds. In room 40, in the first 30 cm below floor surface, were found today six potsherds with Hebrew (Canaanite) inscriptions in black ink.

1. Frag. from rim of a bowl, light brown ware, with traces of red wash. Three lines in black ink perpendicular to rim.

no. 2 3863.

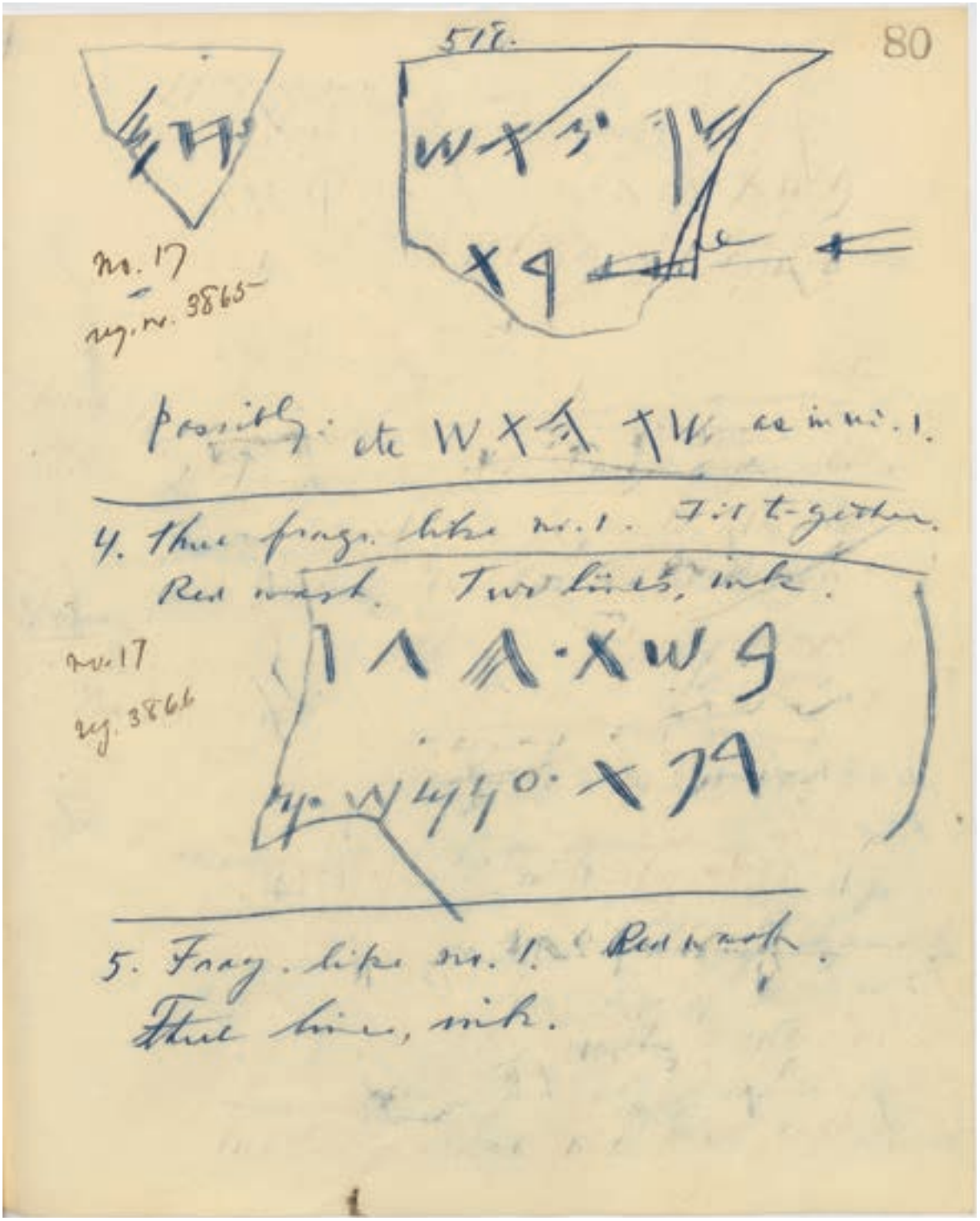
Field Diary V.516

published ostracon No. 5 (Reg. No. 3863)



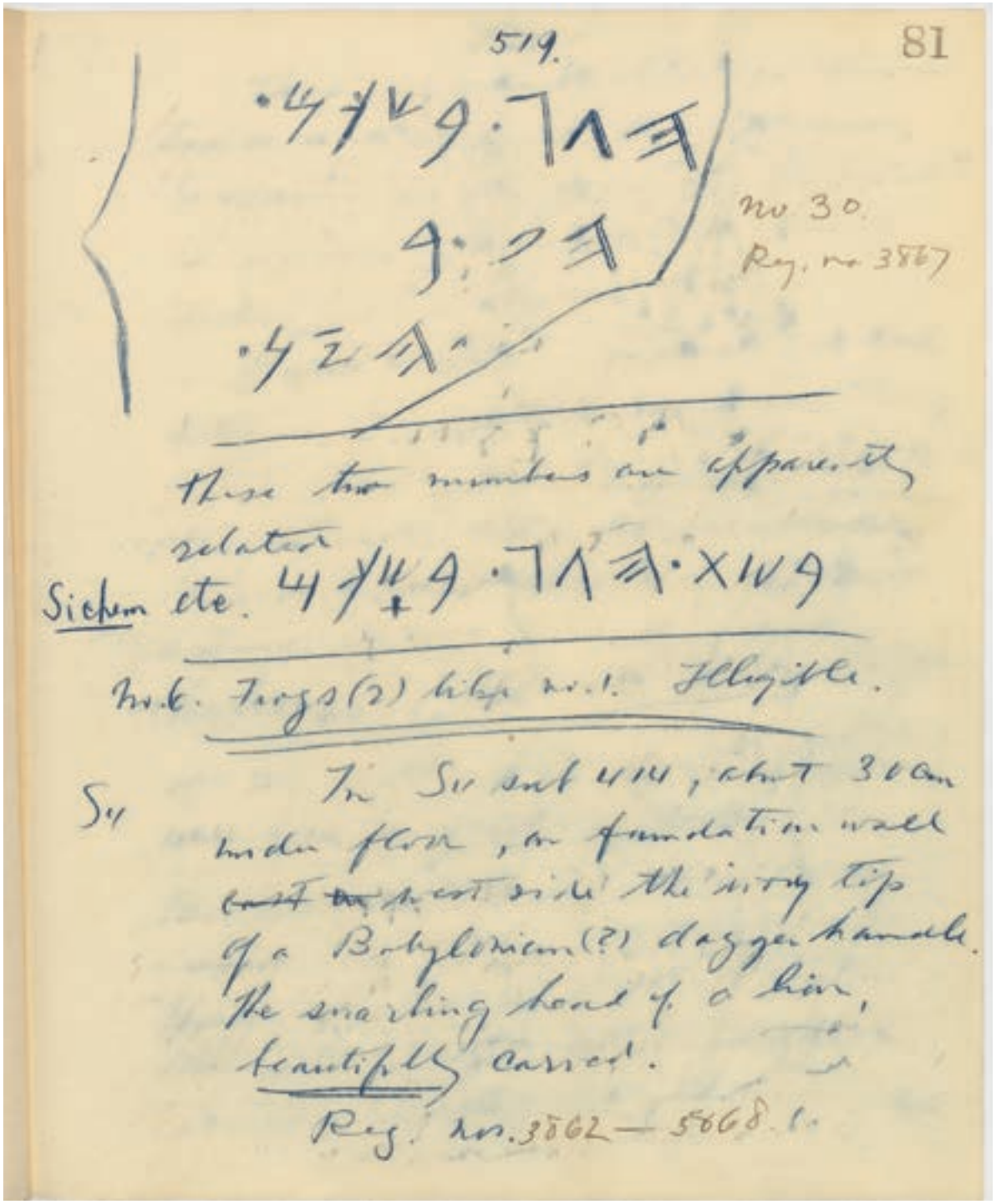
Field Diary V.517

published ostraca No. 4 (Reg. No. 3855) and No. 61 (Reg. No. 3864)



Field Diary V.518

published ostracon No. 24 (left and right sides; Reg. Nos. 3865–3866)


519. 81

no 30.
Reg. no 3867

These two numbers are apparently
related

Sichem etc.

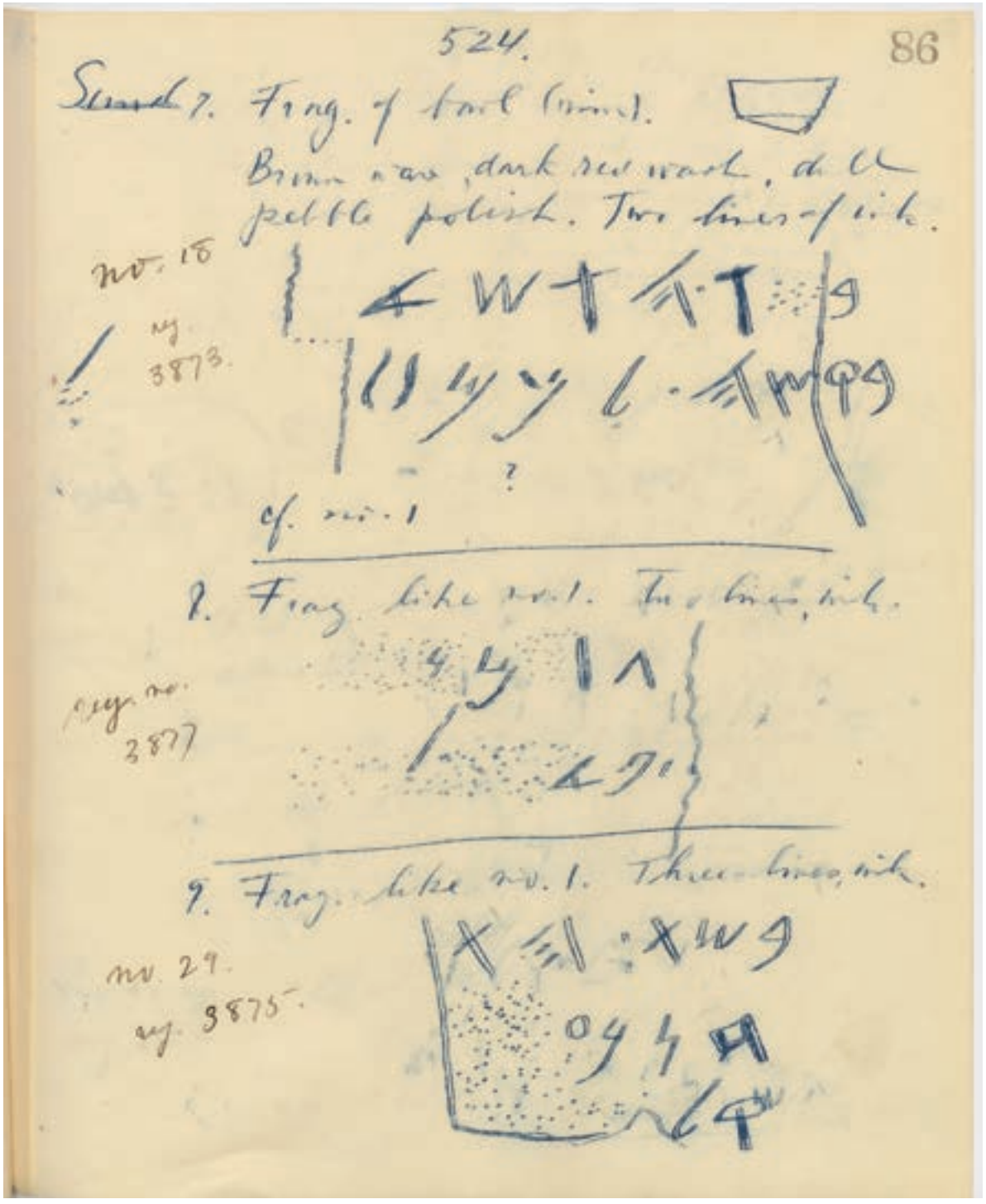
No. 6. Frags (?) bibl. no. 1. Illegible.

Sy

In Sq. unit 4/14, about 30 cm
under floor, on foundation wall
west side the ivory tip
of a Babylonian (?) dagger handle.
The snarling head of a lion,
beautifully carved.
Reg. nos. 3862 — 3868. 1.

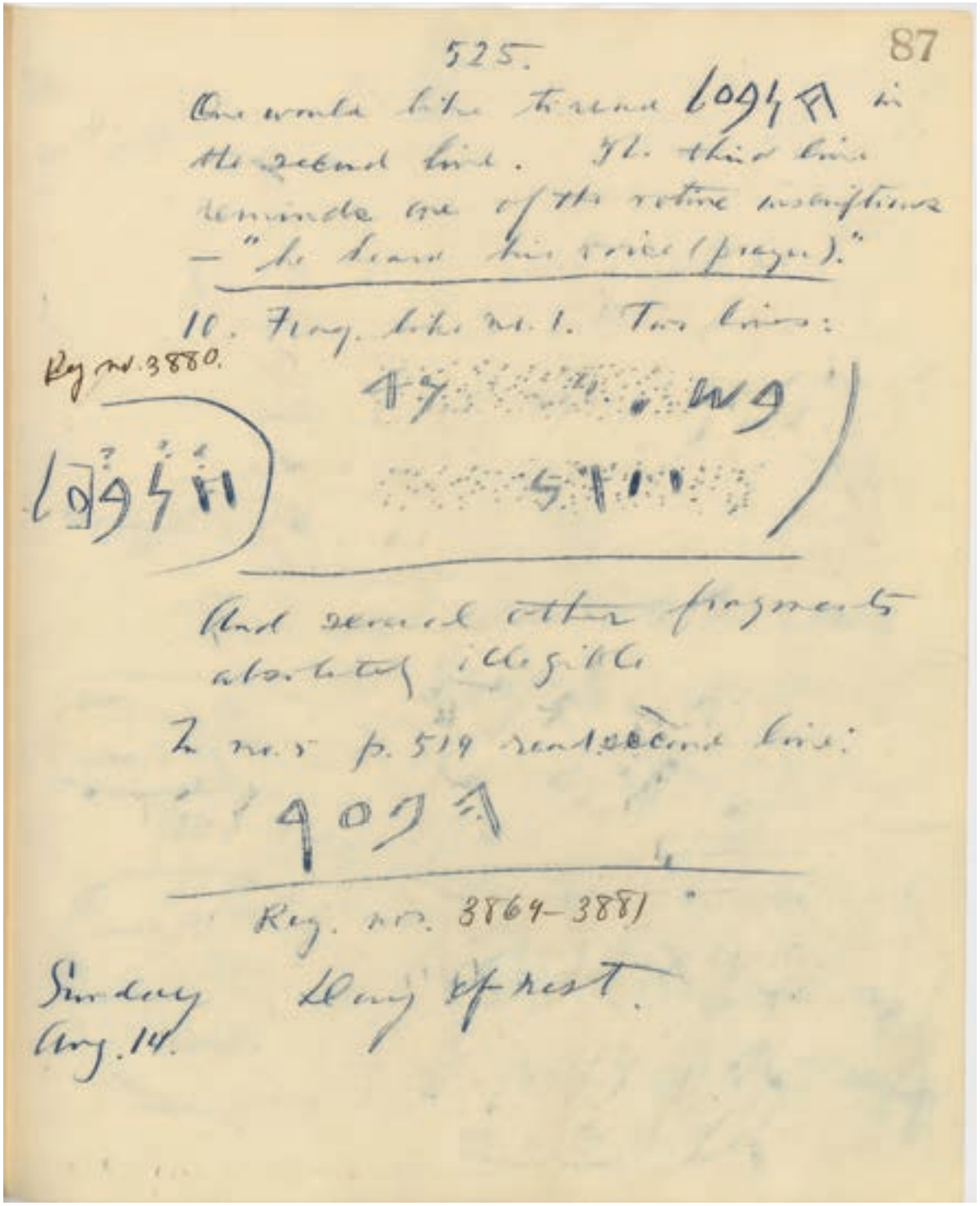
Field Diary V.519

published ostracon No. 44 (Reg. No. 3867)



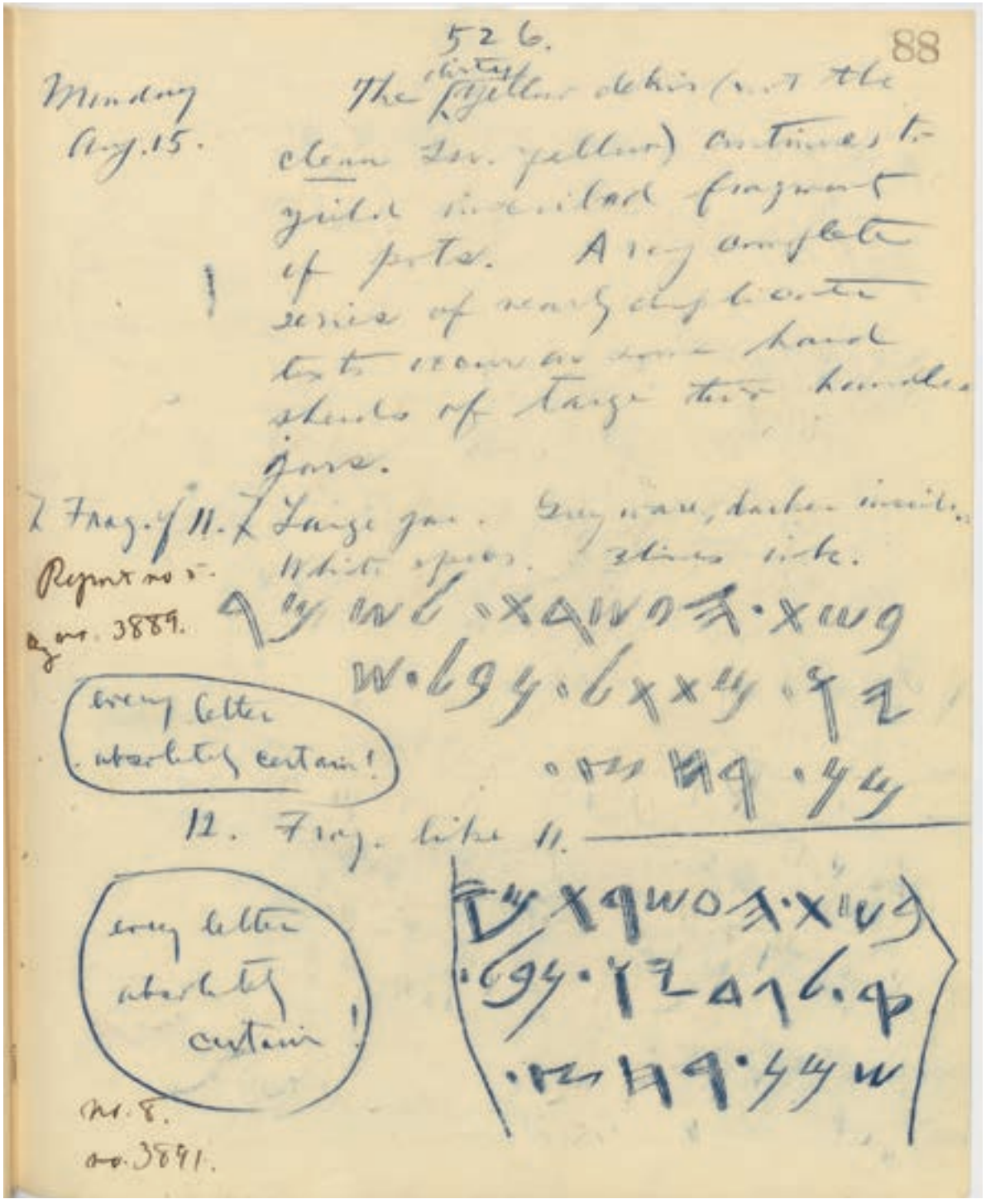
Field Diary V.524

published ostracon No. 26 (top; Reg. No. 2873) and possibly No. 43 (bottom; Reg. No. 3875);
plus an unpublished fragment (center; Reg. No. 3877)



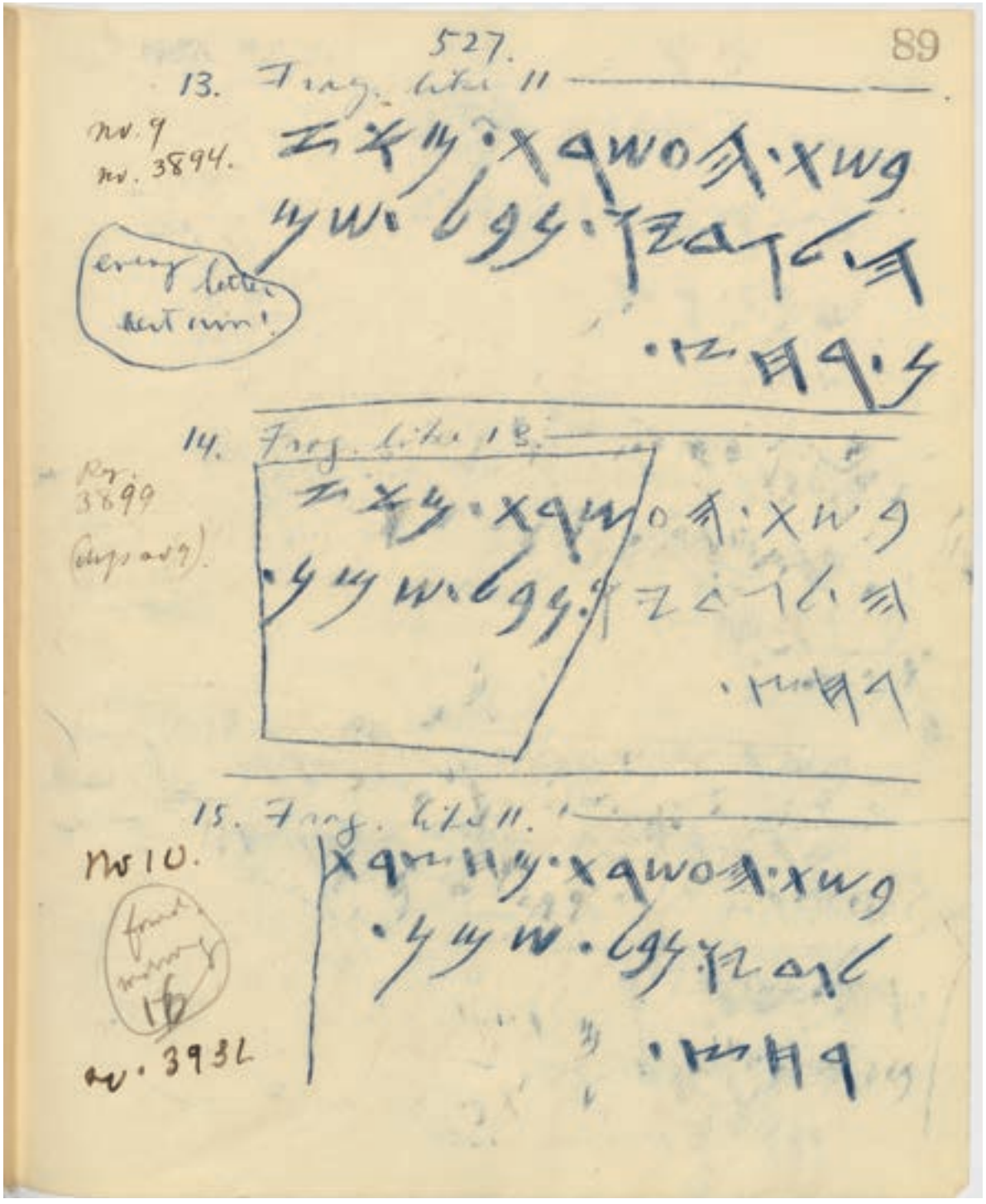
Field Diary V.525

an unpublished fragment (top; Reg. No. 3880) and a corrected reading of No. 44 from Field Diary V.518–19 (labeled as No. 5 in the diary—"In no. 5 p. 519 read second line: … .")



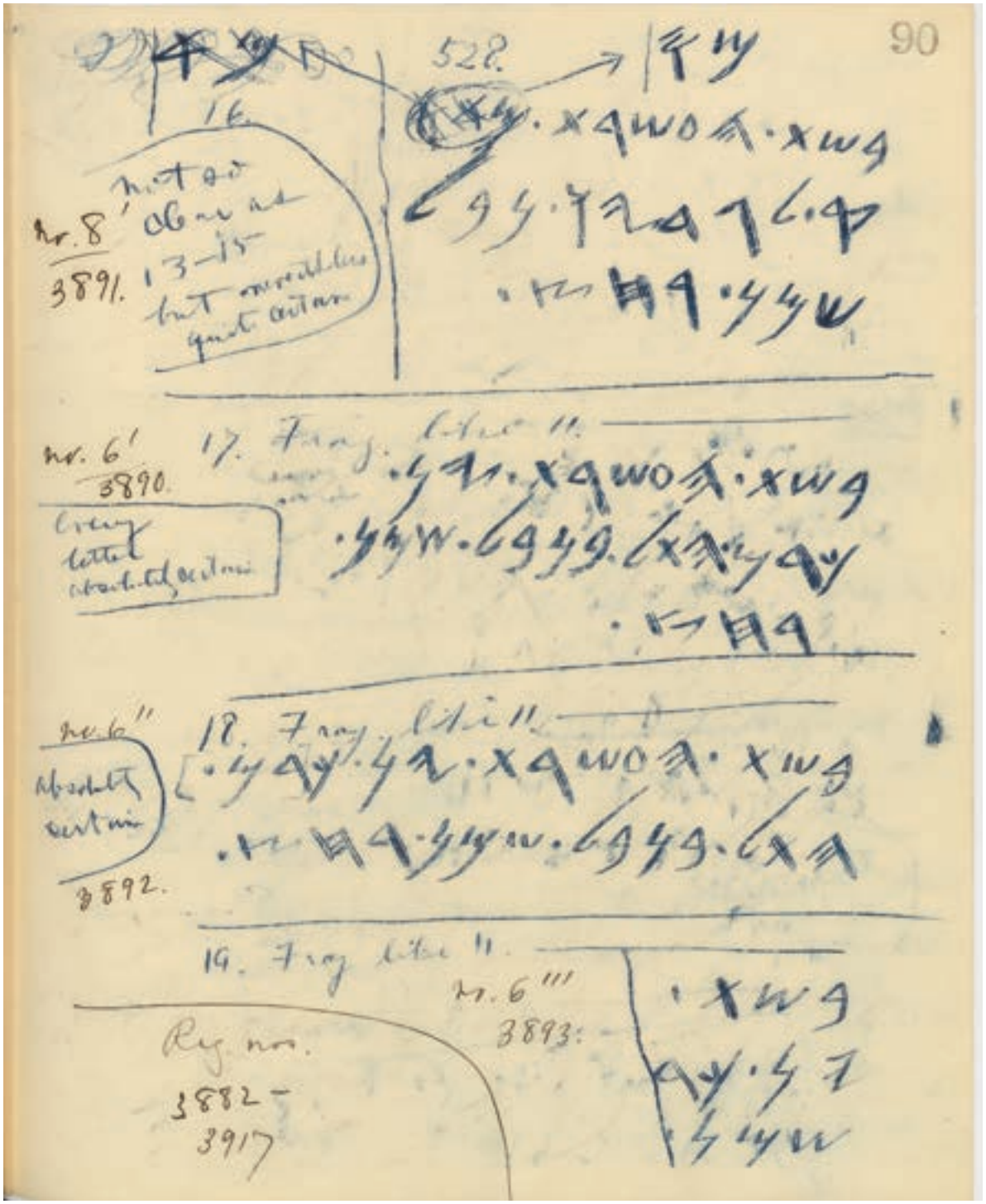
Field Diary V.526

published ostraca No. 16a (bottom; duplicate; Reg. No. 3891; labeled No. 8 in the diary) and No. 21 (top; Reg. No. 3889)



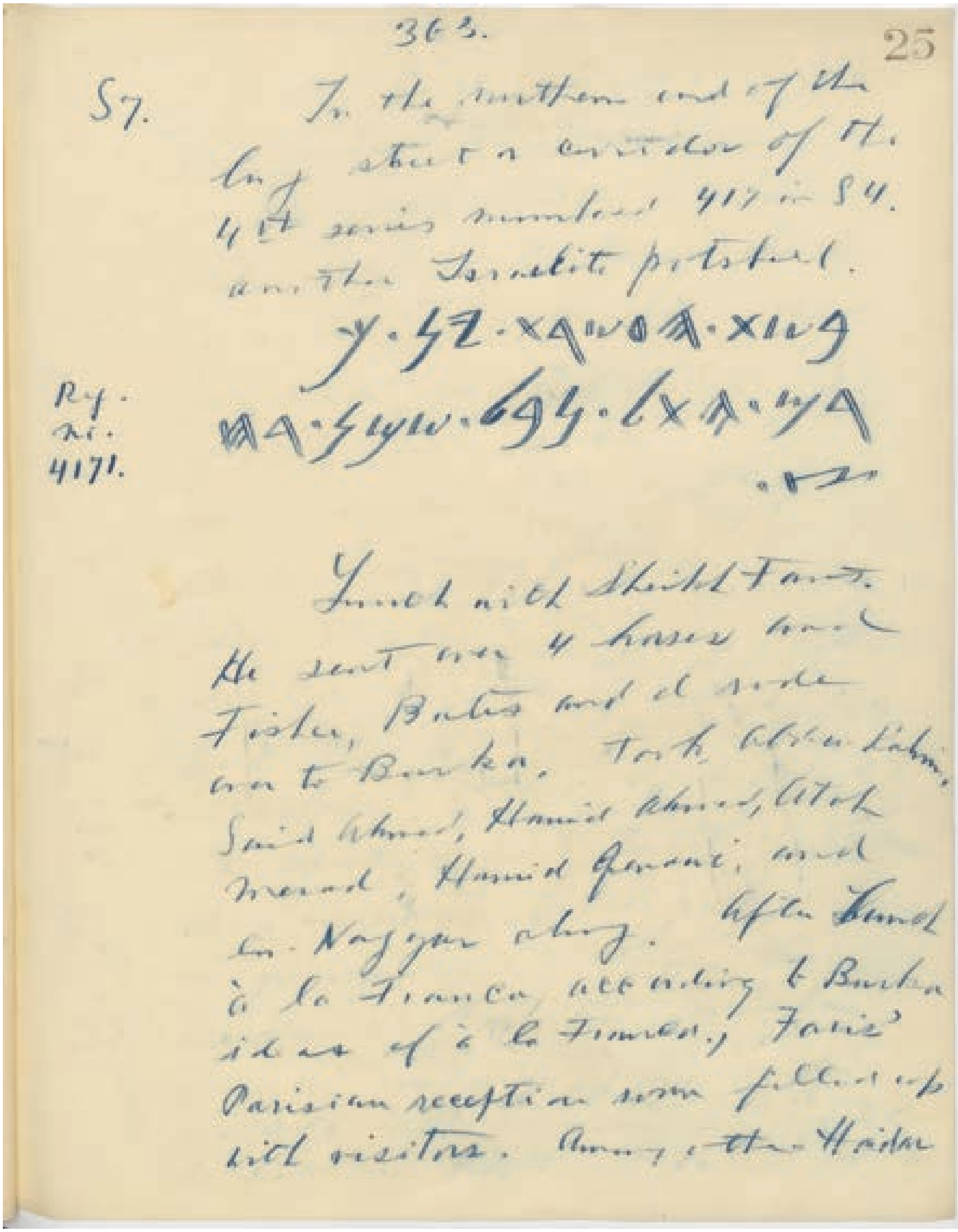
Field Diary V.527

published ostraca No. 17a (top; duplicate; Reg. No. 3894; labeled No. 9 in the diary), No. 17b (center; Reg. No. 3899), and No. 18 (bottom; Reg. No. 3931; labeled No. 10 in the diary)



Field Diary V.528

published ostraca No. 16a (top; = duplicate; Reg. No 3891; labeled No. 8 in the diary)
and No. 53 (top center; Reg. No. 3890; labeled No. 61 in the diary);
plus two unpublished fragments (bottom center and bottom right; Reg. Nos. 3892–3893; labeled No. 6 in the diary;
recognized as legible by I. T. Kaufman during his visit to the Istanbul Museum)


363.

25

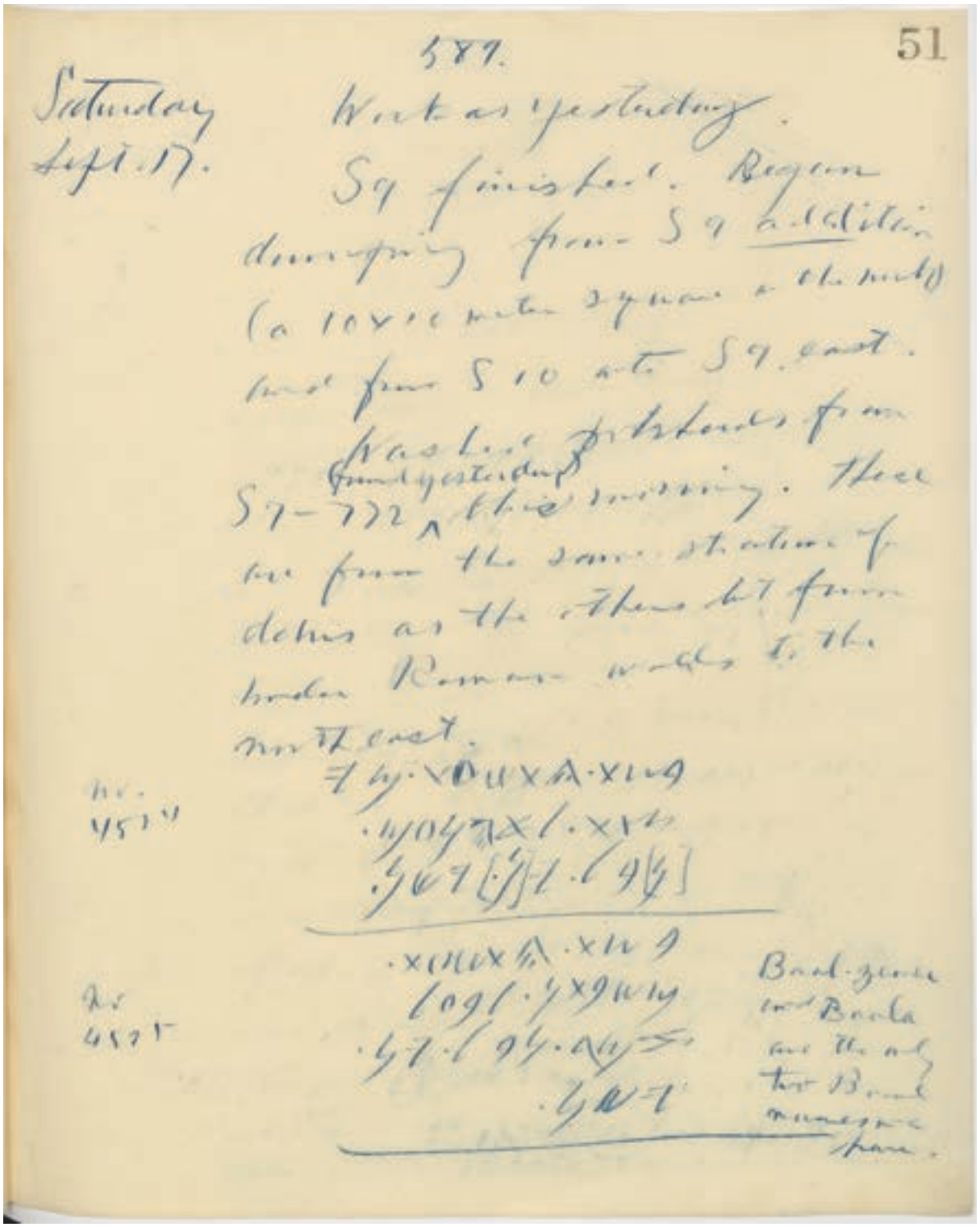
57. In the southern end of the long street or corridor of the 4th series numbered 417 in S4. another Israelite potsherd.

Reg. no. 4171.

Lunch with Sheikh Fa...
He sent over 4 horses and Fisher, Bates and I rode over to Burka. Both ... Said Ahmed, Hamid Ahmed, Atah Merad, Hamid ..., and ... Najjar along. After lunch à la France, according to Burka ideas of à la France, Paris Parisian reception soon filled up with visitors. Among others Haidar

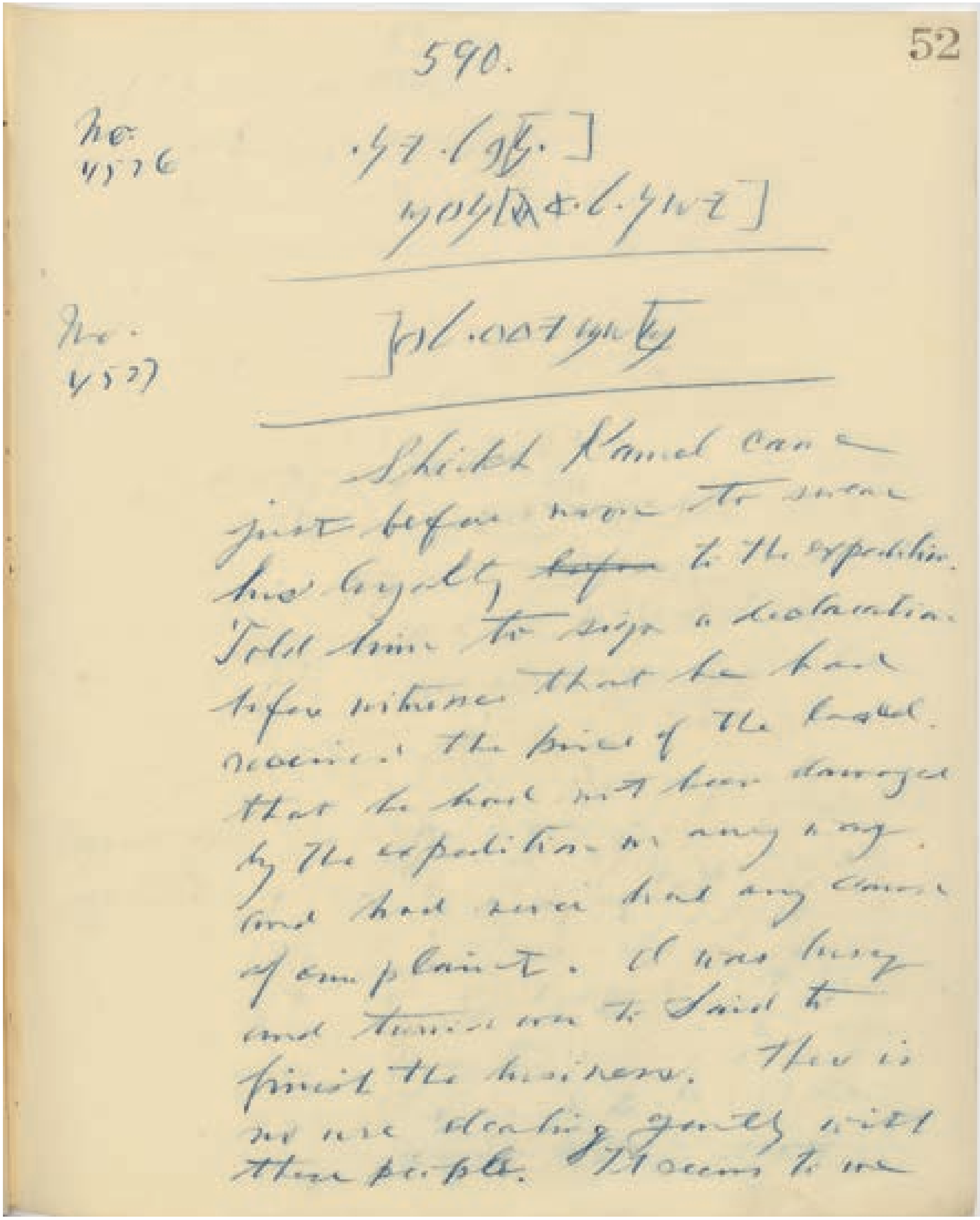
Field Diary VI.563

published ostracon No. 54 (Reg. No. 4171)



Field Diary VI.589

published ostraca No. 9 (top; Reg. No. 4524) and No. 12 (bottom; Reg. No. 4525)


590.

52

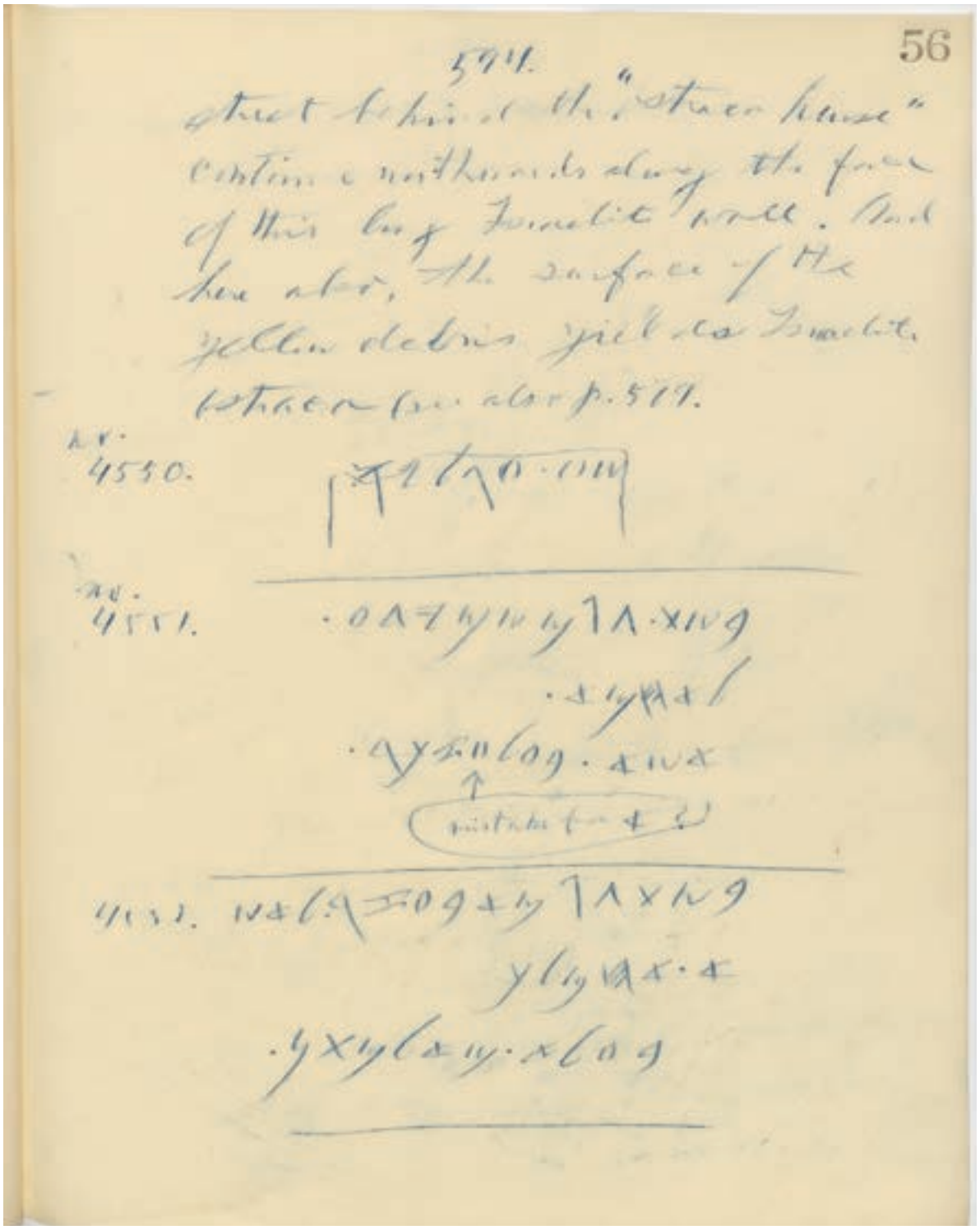
No. 4526

No. 4527

Sheikh Kamel came just before noon to swear his loyalty ~~before~~ to the expedition. Told him to sign a declaration before witness that he had received the price of the land, that he had not been damaged by the expedition in any way, and had never had any cause of complaint. He was busy and turned over to Said to finish the business. There is no use dealing gently with these people. It seems to me

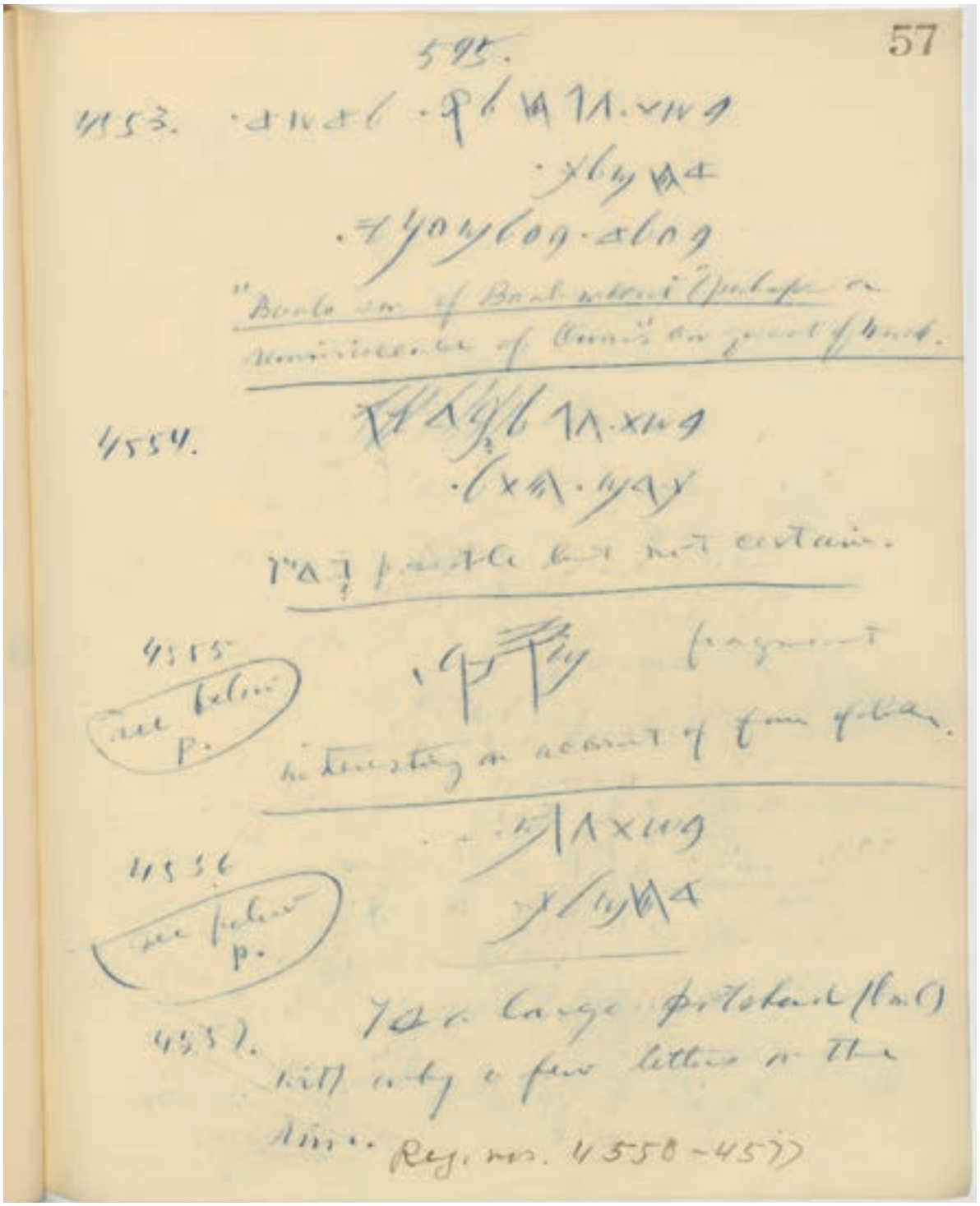
Field Diary VI.590

published ostraca No. 11 (top; Reg. No. 4526) and No. 40 (bottom; Reg. No. 4527)



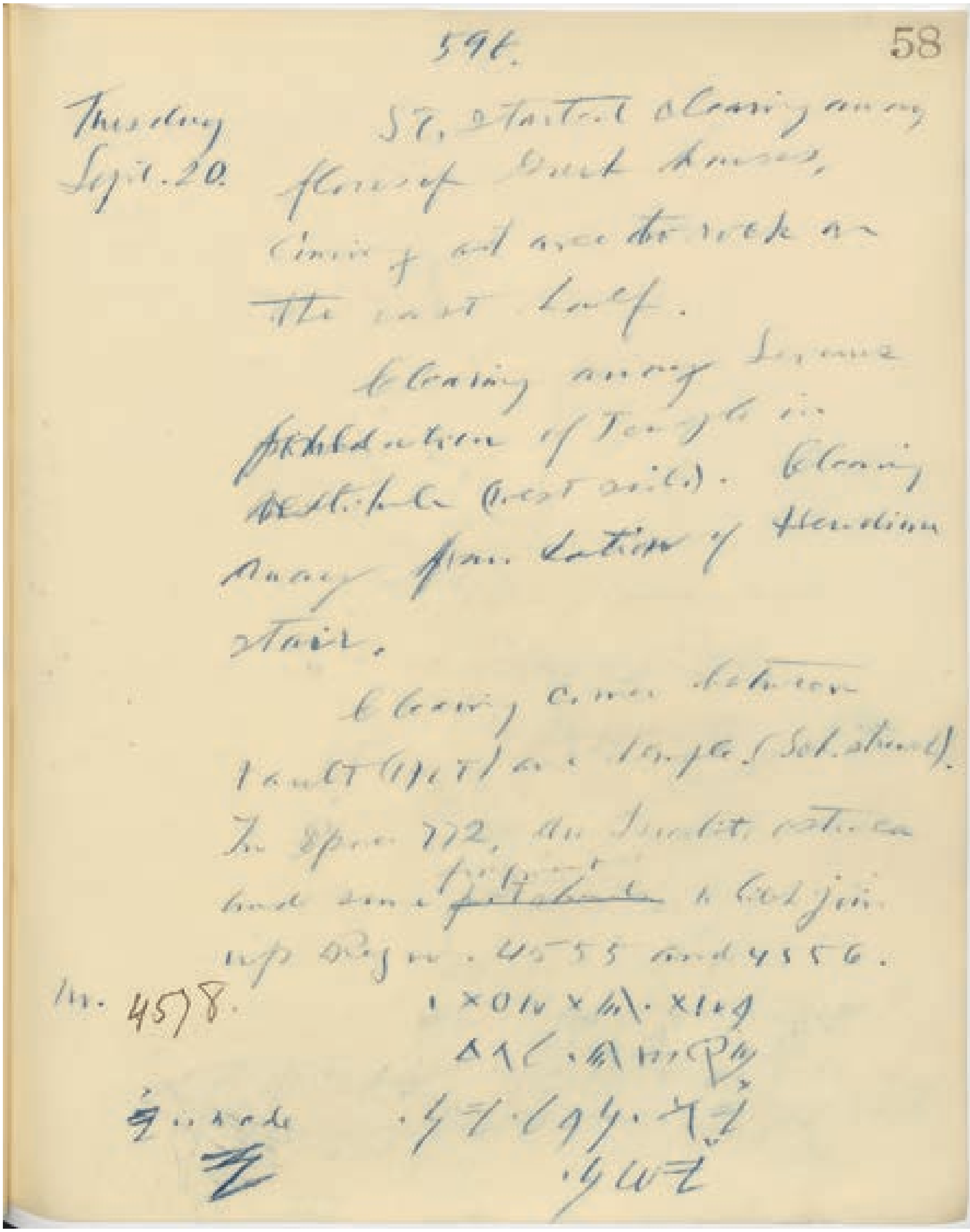
Field Diary VI.594

published ostraca No. 28 (bottom; Reg. No. 4552), No. 37 (center; Reg. No. 4551), and No. 41 (top; Reg. No. 4550)



Field Diary VI.595

published ostraca No. 27 (top; Reg. No. 4553), No. 29 (two bottom drawings; Reg. Nos. 4555–4556; join with Reg. No. 4579, not drawn in the diary), and No. 58 (second from top; Reg. No. 4554)


596. 58

Tuesday Sept. 20. S7, Started clearing away floors of Greek houses, cleaning out area to rock on the east half.

Clearing away Roman foundation of Temple in vestibule (west side). Clearing away foundation of Herodian stair.

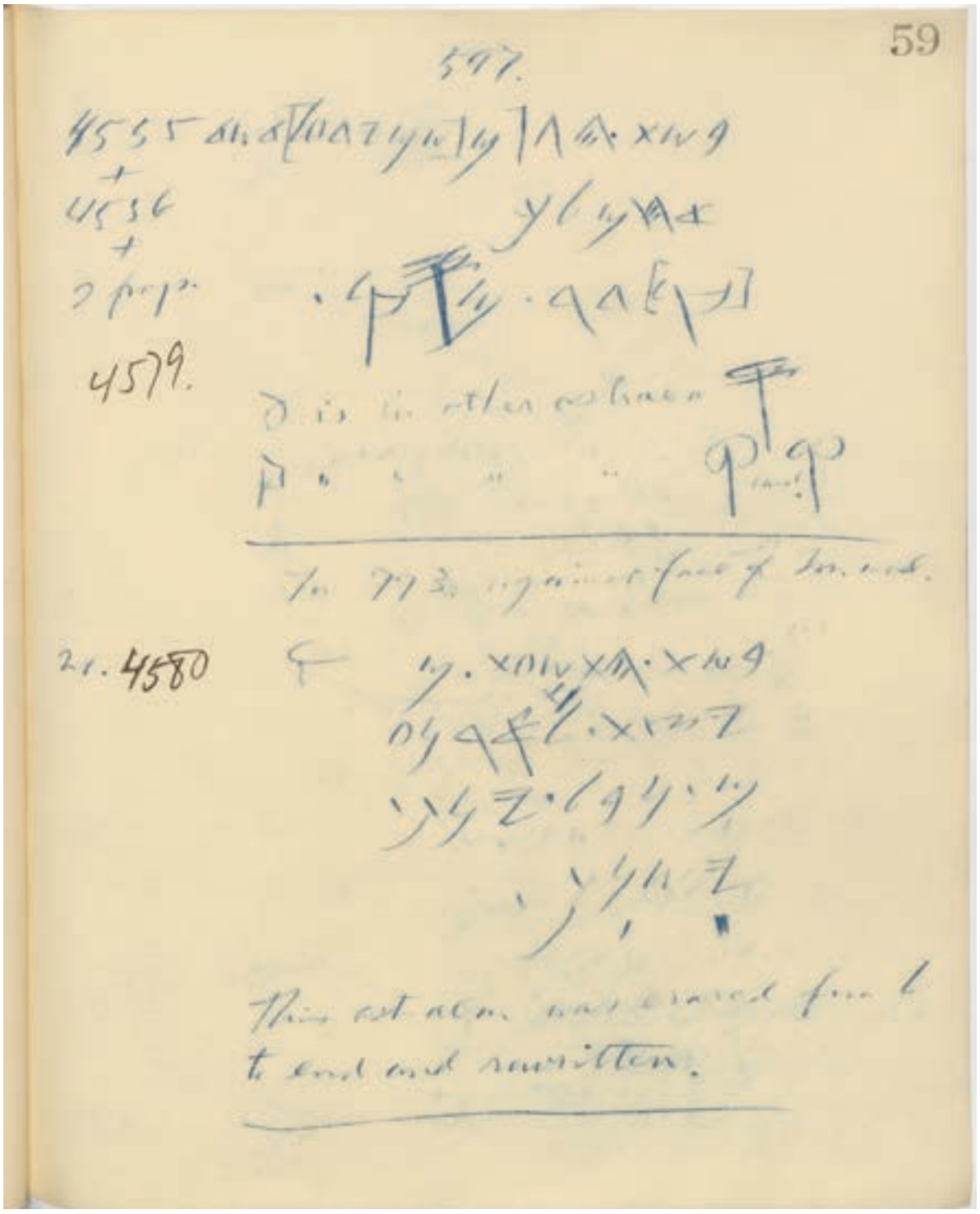
Clearing corner between vault and temple.

In Space 772, an Israelite ostraca and some fragments to be joined up Reg. no. 4555 and 4556.

No. 4578.

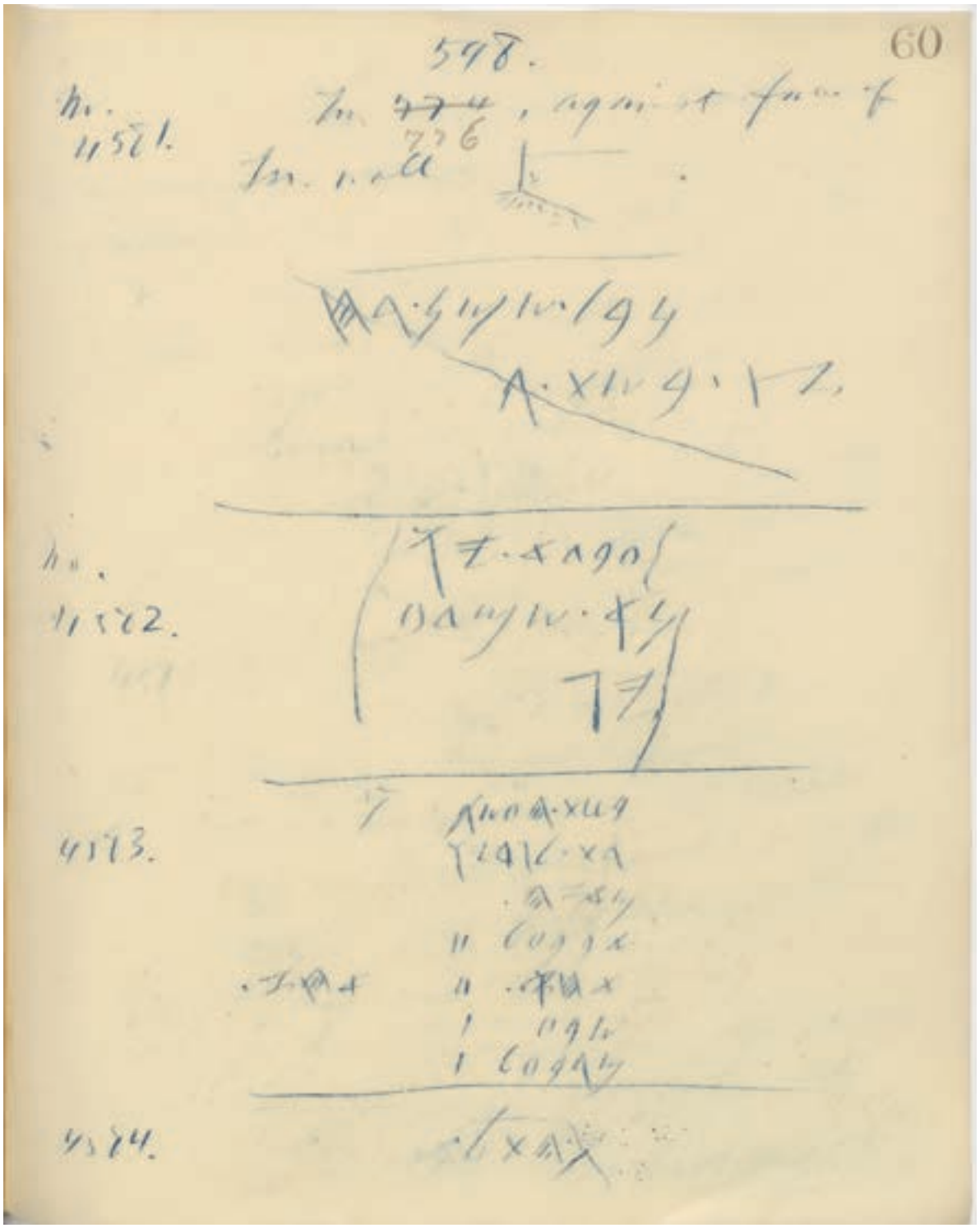
Field Diary VI.596

published ostracon No. 6 (Reg. No. 3997 in *HES I;* Diary Reg. No. 4578; relates to published ostracon No. 7)



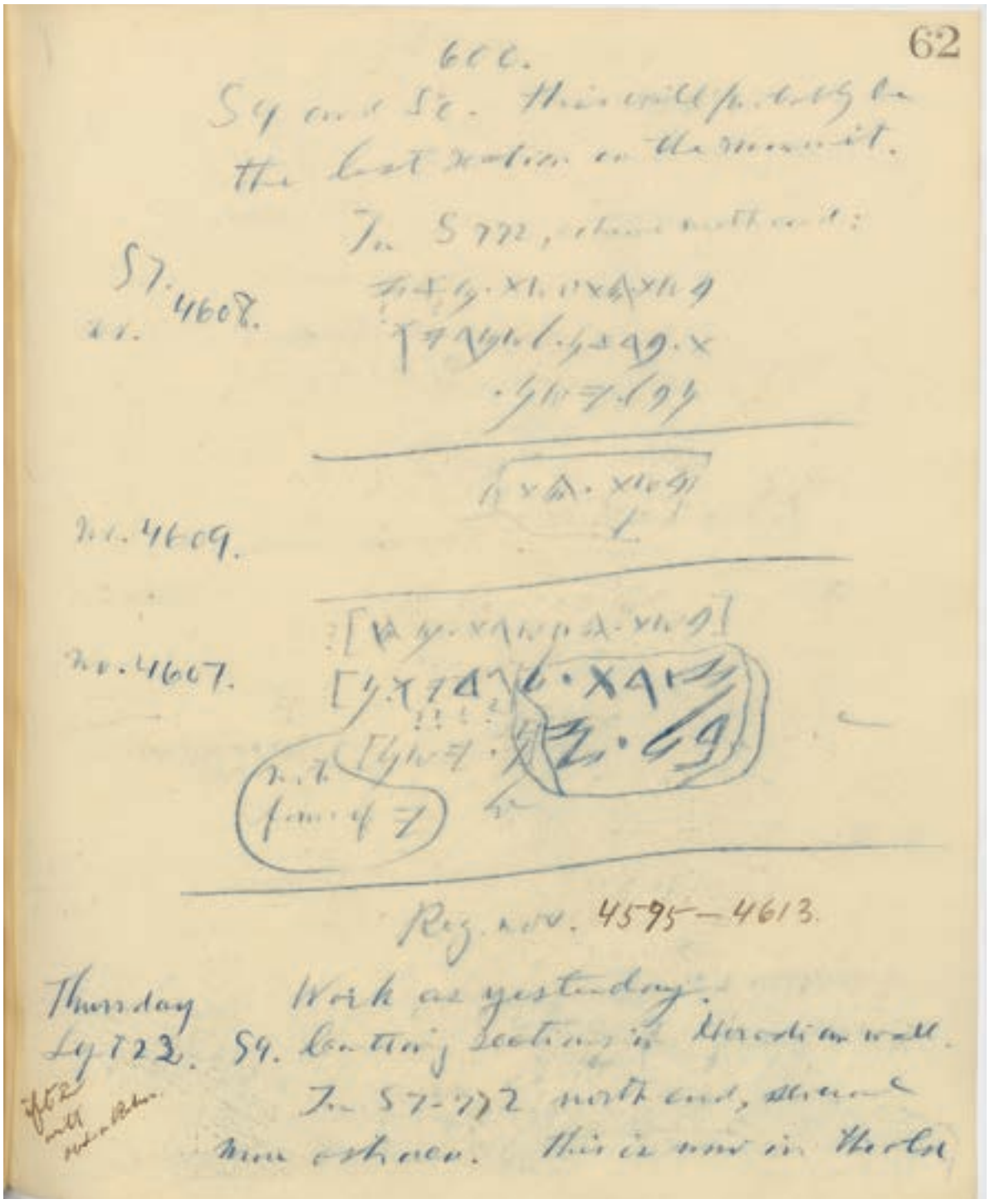
Field Diary VI.597

published ostraca No. 10 (bottom; Reg. No. 4580) and No. 29 (top; Reg. Nos. 4555–4556, 4579)



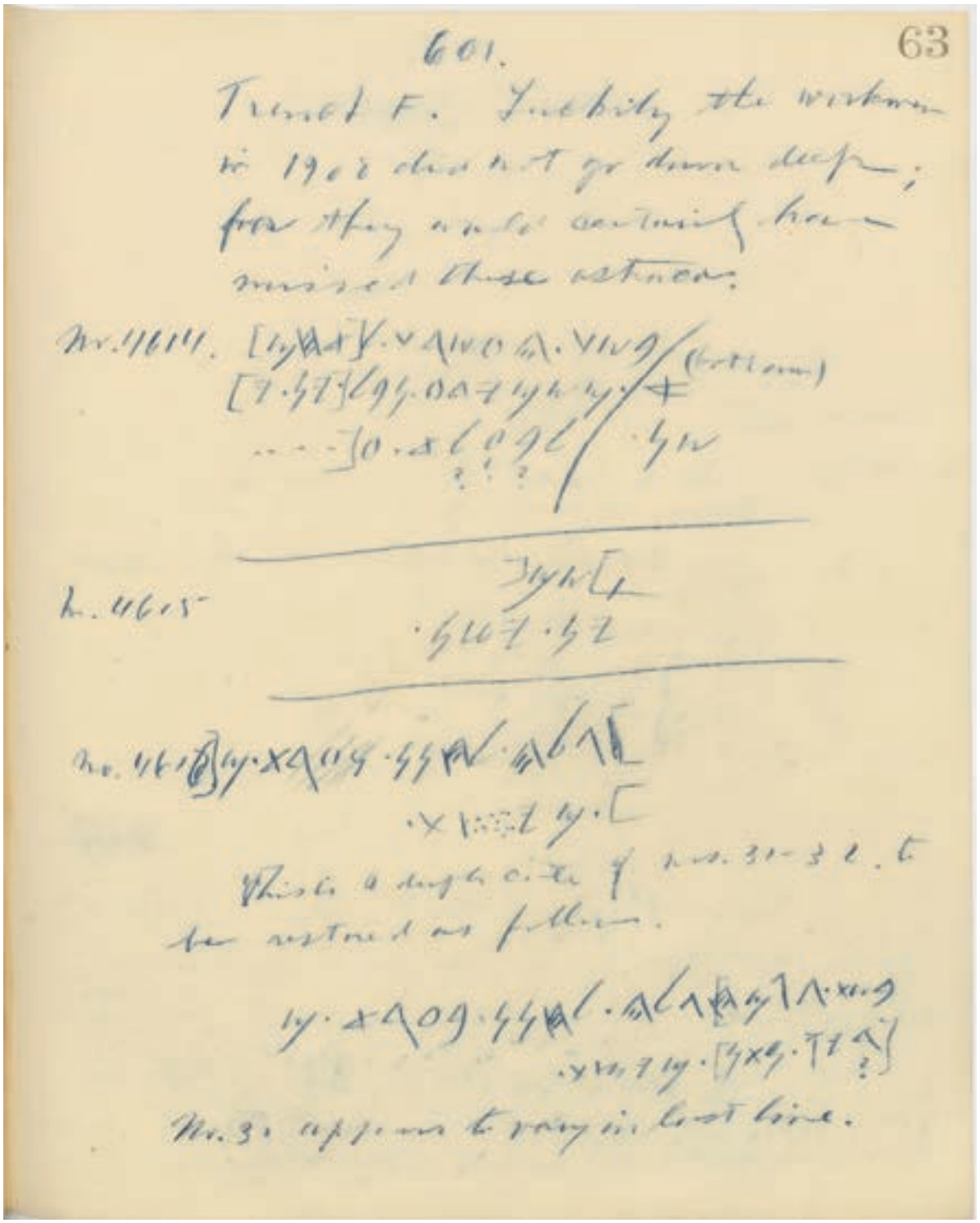
Field Diary VI.598

published ostraca No. 2 (center bottom; Reg. No. 4583), No. 57 (center top; Reg. No. 4582), and No. 59 (top; Reg. No. 4581); plus an unpublished fragment (bottom; Diary Reg. No. 4584)



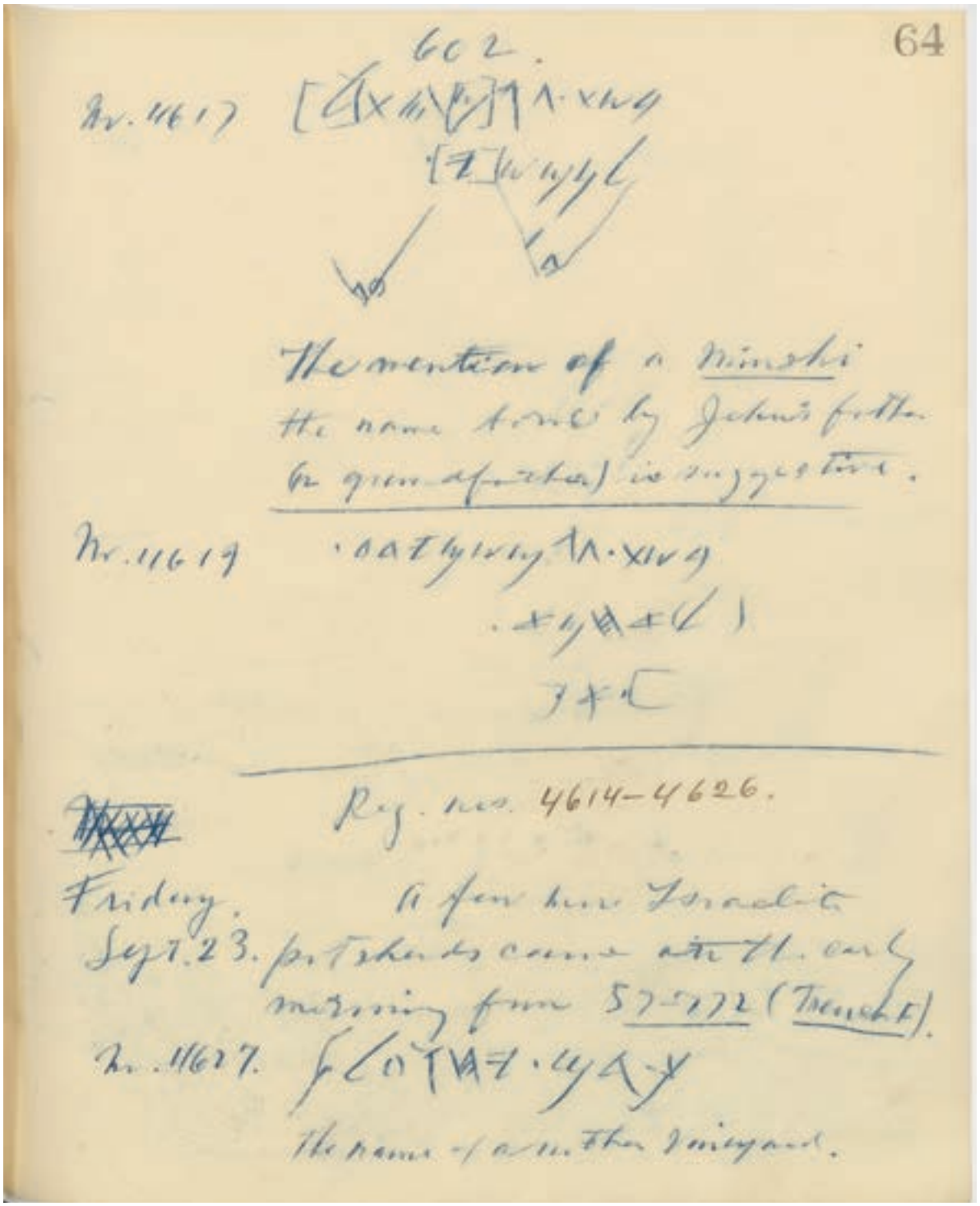
Field Diary VI.600

published ostraca No. 14 (top; Reg. No. 4608) and No. 15 (bottom; Reg. No. 4607);
plus an unpublished fragment (center; Diary Reg. No. 4609)



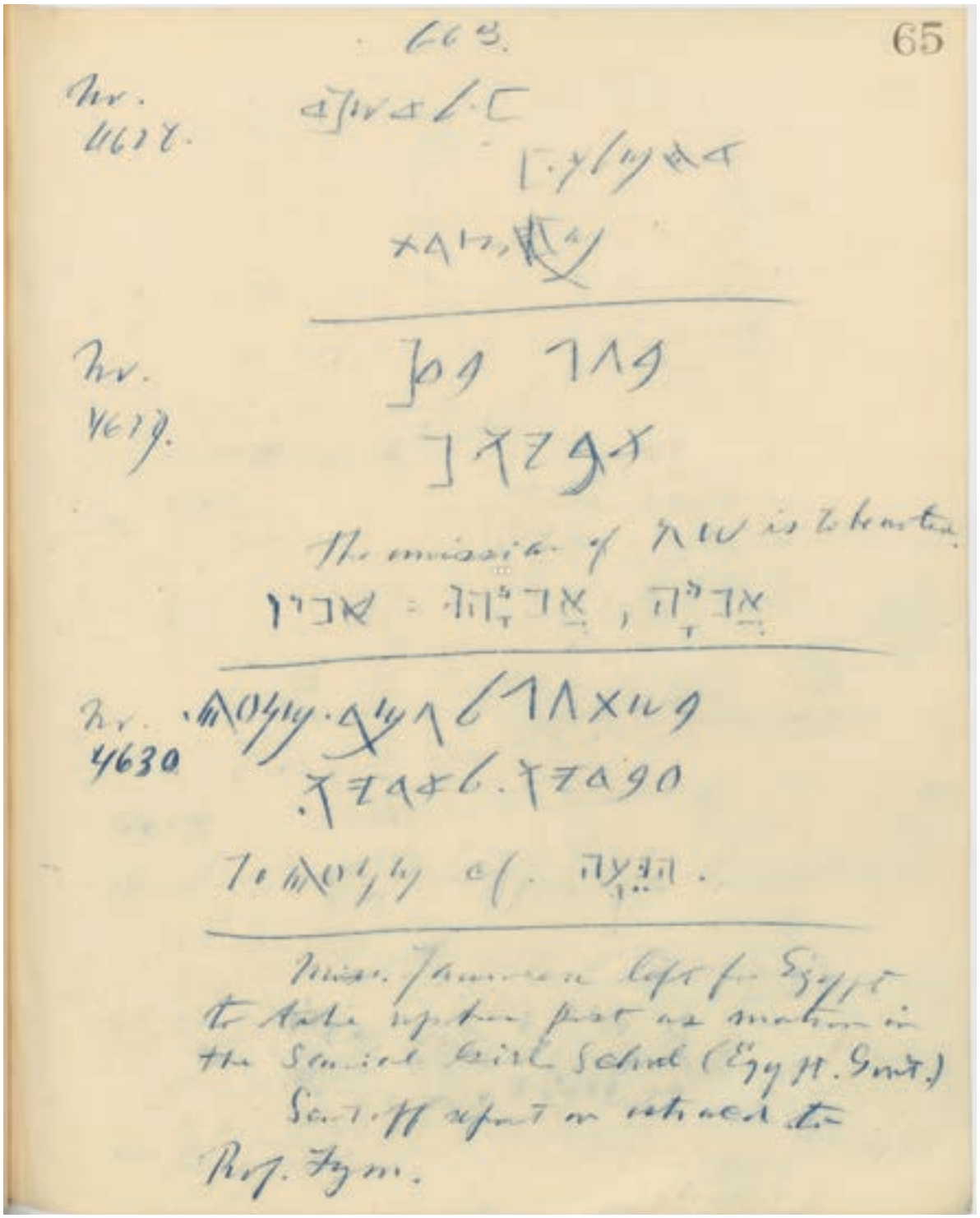
Field Diary VI.601

published ostraca No. 3 (top; Reg. No. 4614) and No. 47 (bottom; Reg. No. 4616);
plus an unpublished fragment (center; Diary Reg. No. 4615)



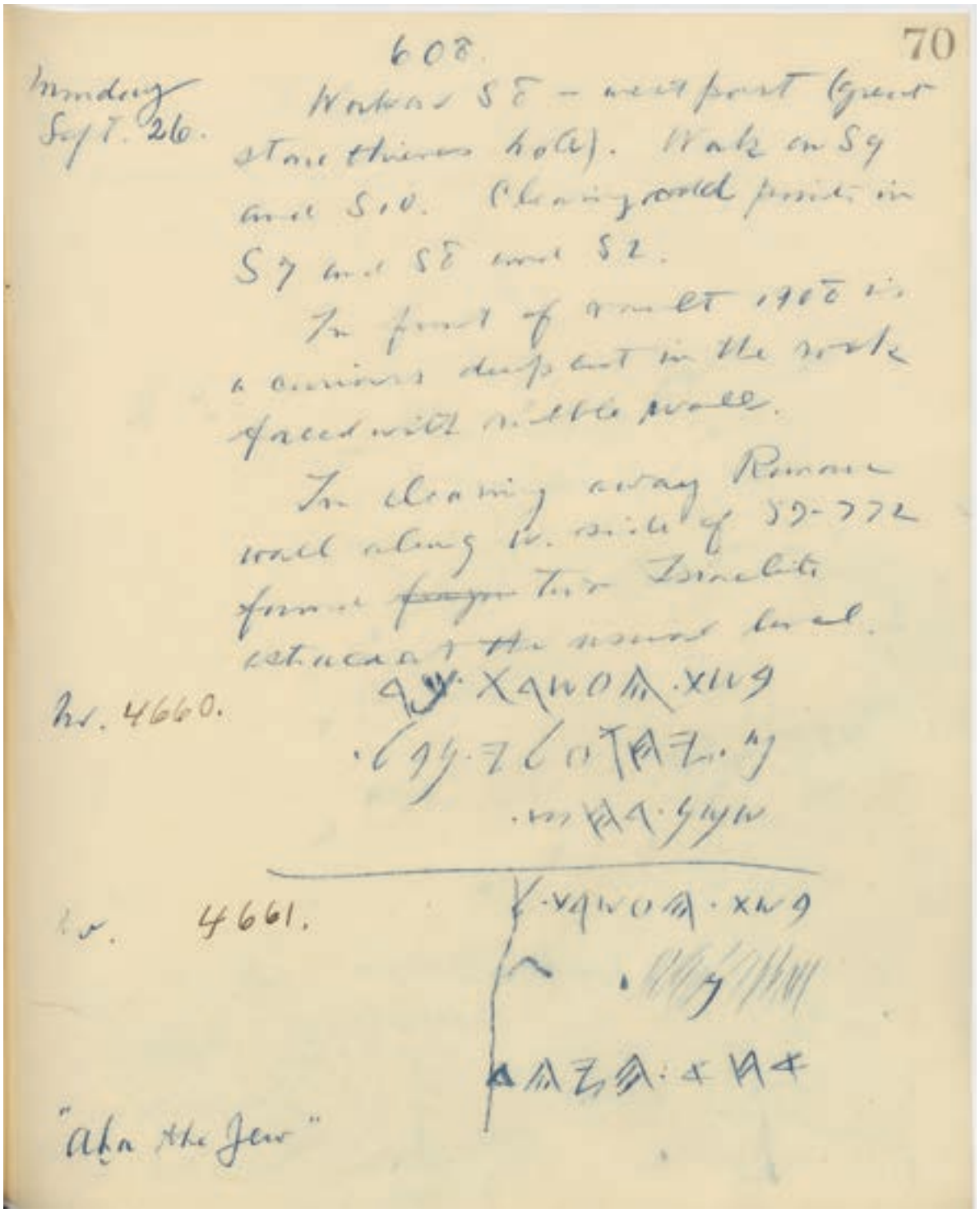
Field Diary VI.602

published ostraca No. 39 (center; duplicate?; Reg. No. 4619), No. 56 (top; Reg. No. 4617), and No. 60 (bottom; Reg. No. 4627)



Field Diary VI.603

published ostraca No. 50 (bottom; Reg. No. 4630) and No. 52 (center; Reg. No. 4629);
plus an unpublished fragment (top; Reg. No. 4628)



Field Diary VI.608

published ostraca No. 51 (bottom; Reg. No. 4661) and No. 55 (top; Reg. No. 4660)

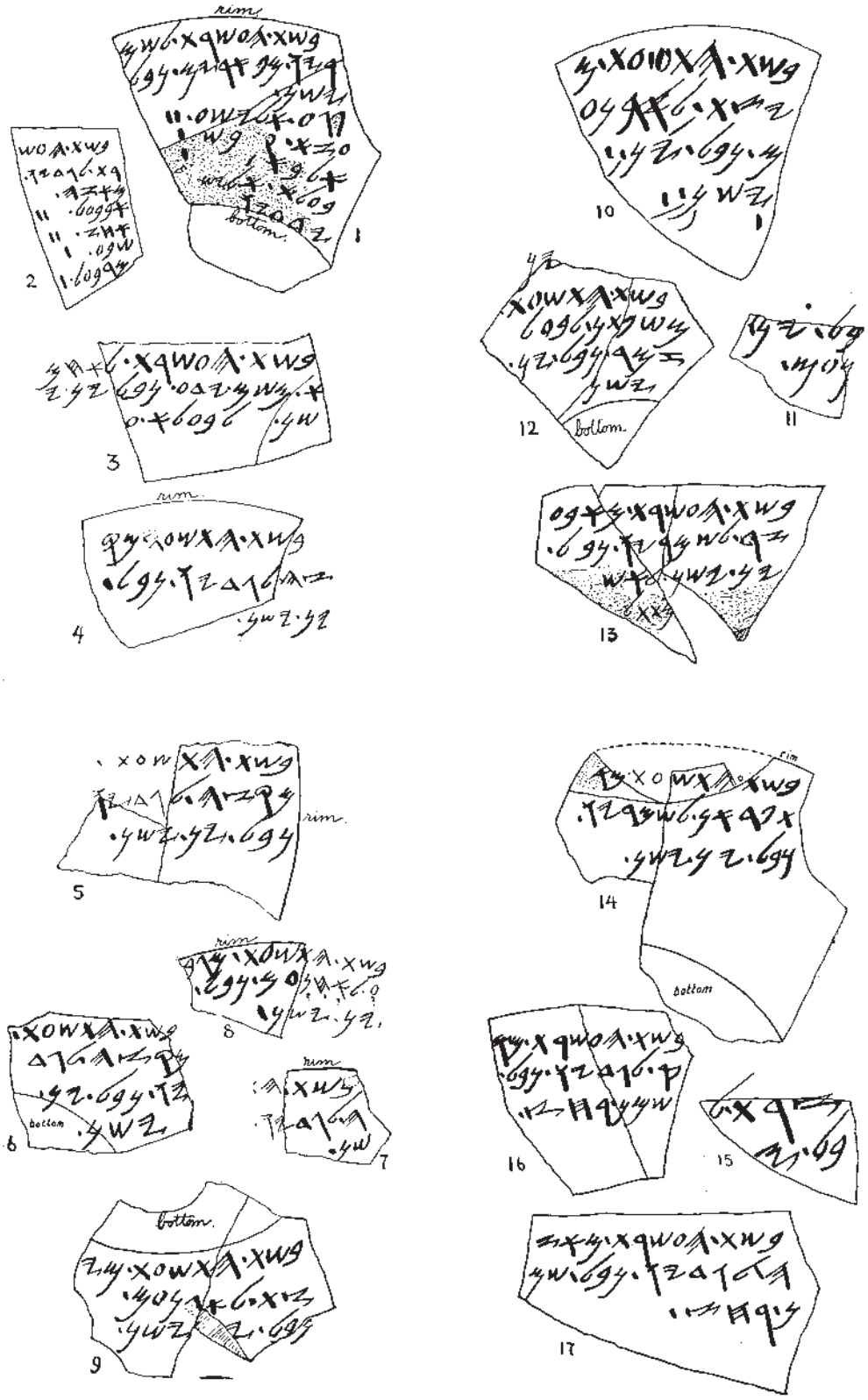


Plate I 1910 Ostraca: Nos. 1–17.

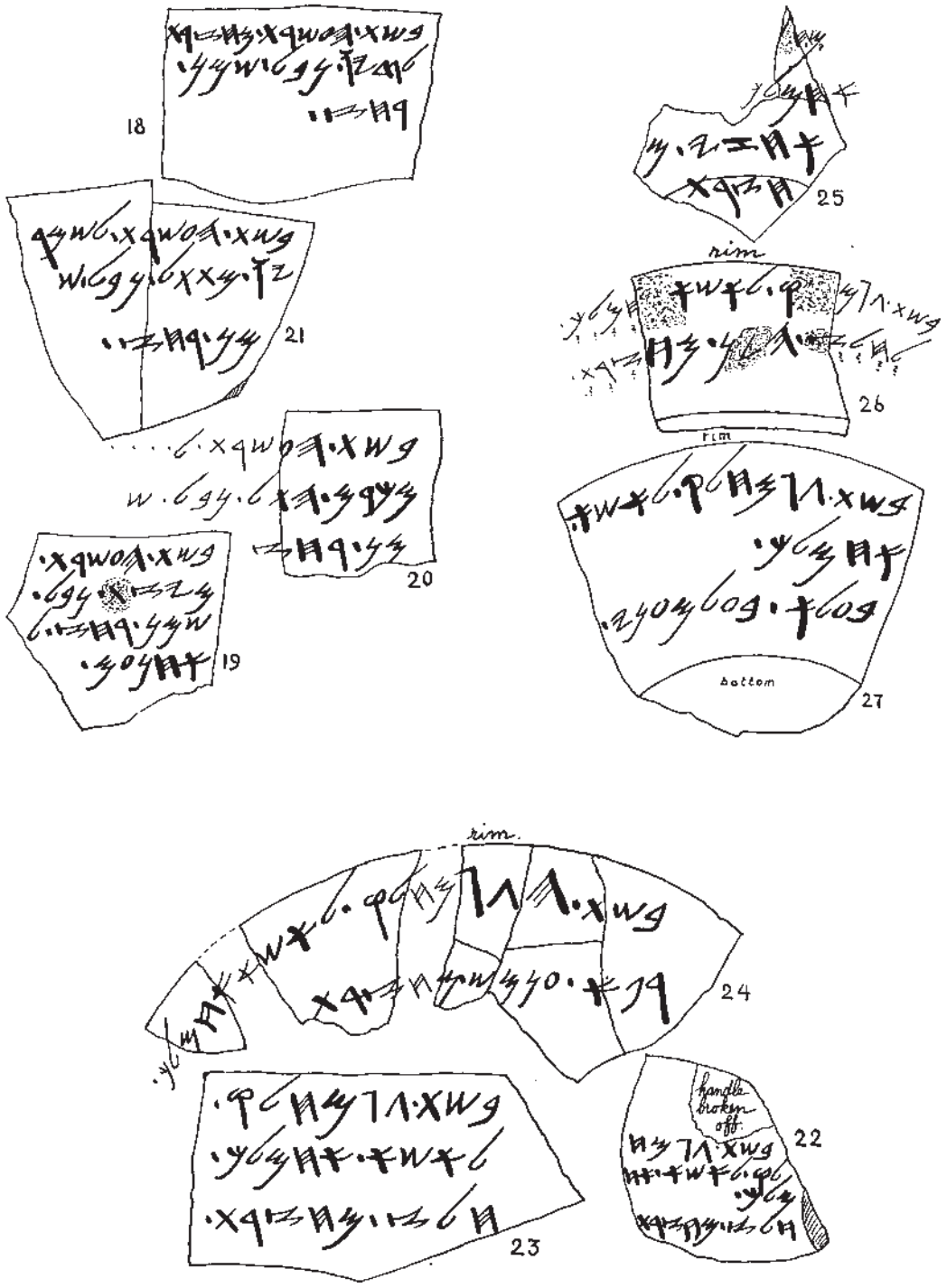


Plate II 1910 Ostraca: Nos. 18–27.

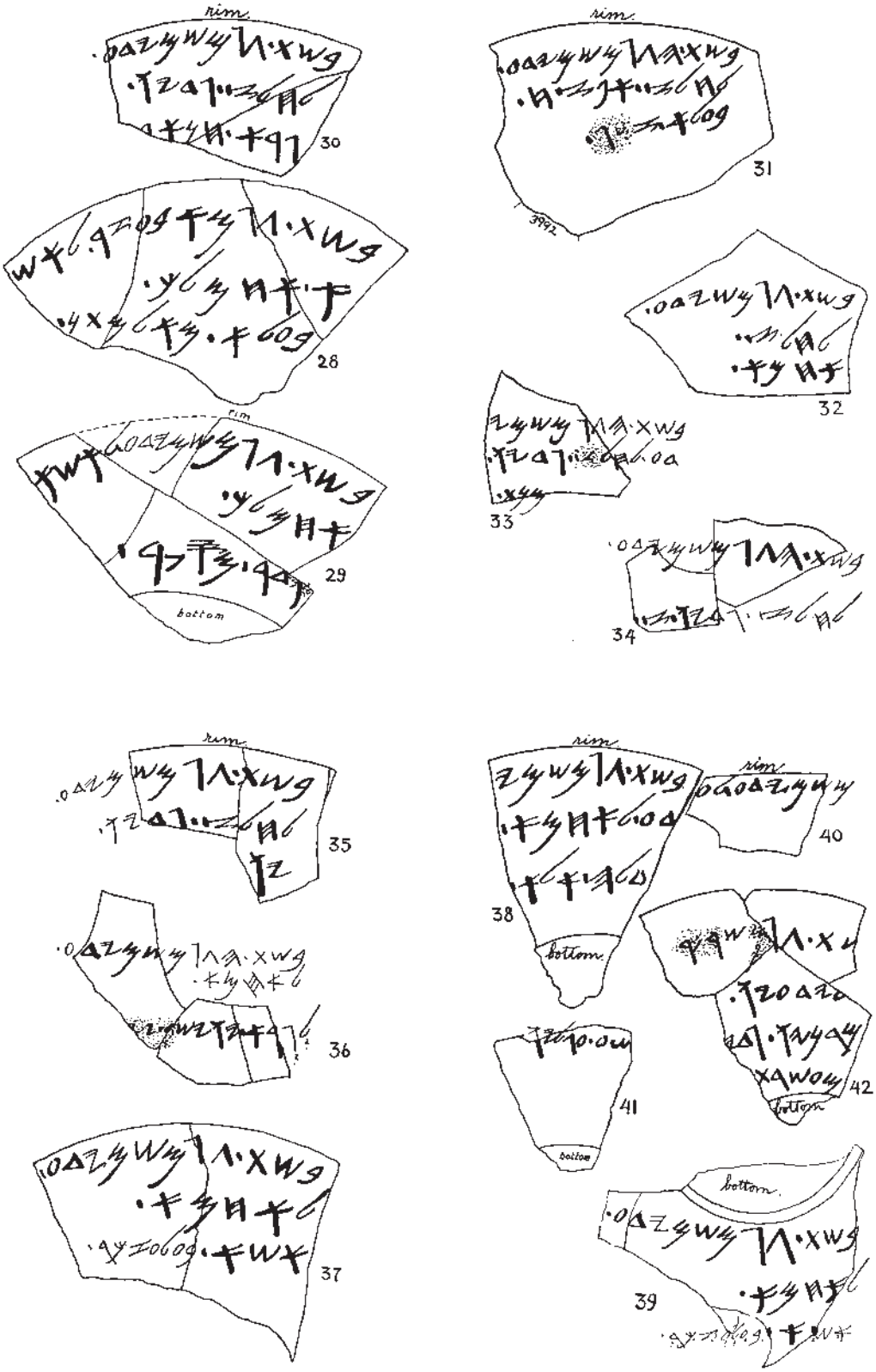


Plate III 1910 Ostraca: Nos. 28–42.

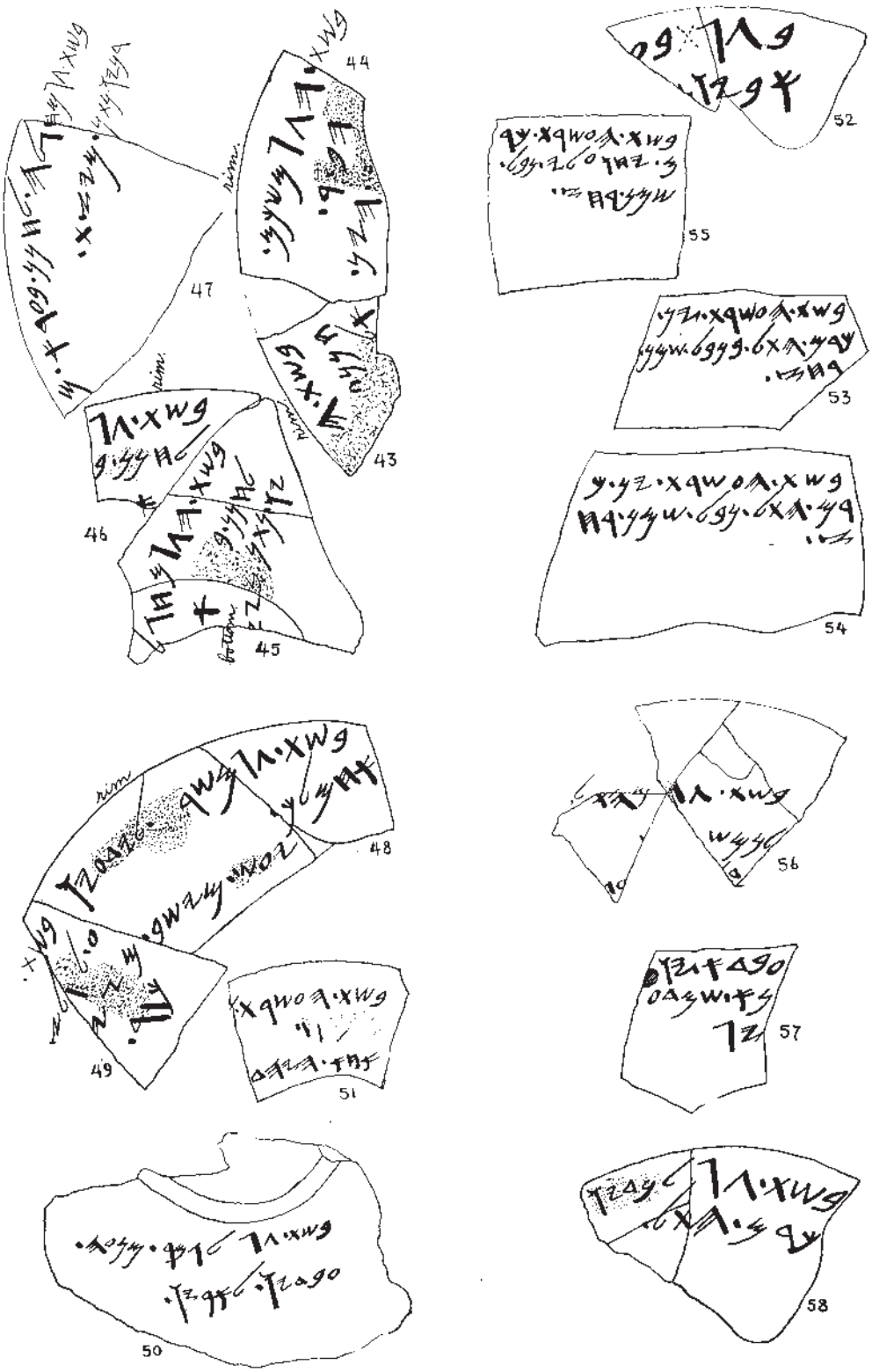


Plate IV 1910 Ostraca: Nos. 43–58.

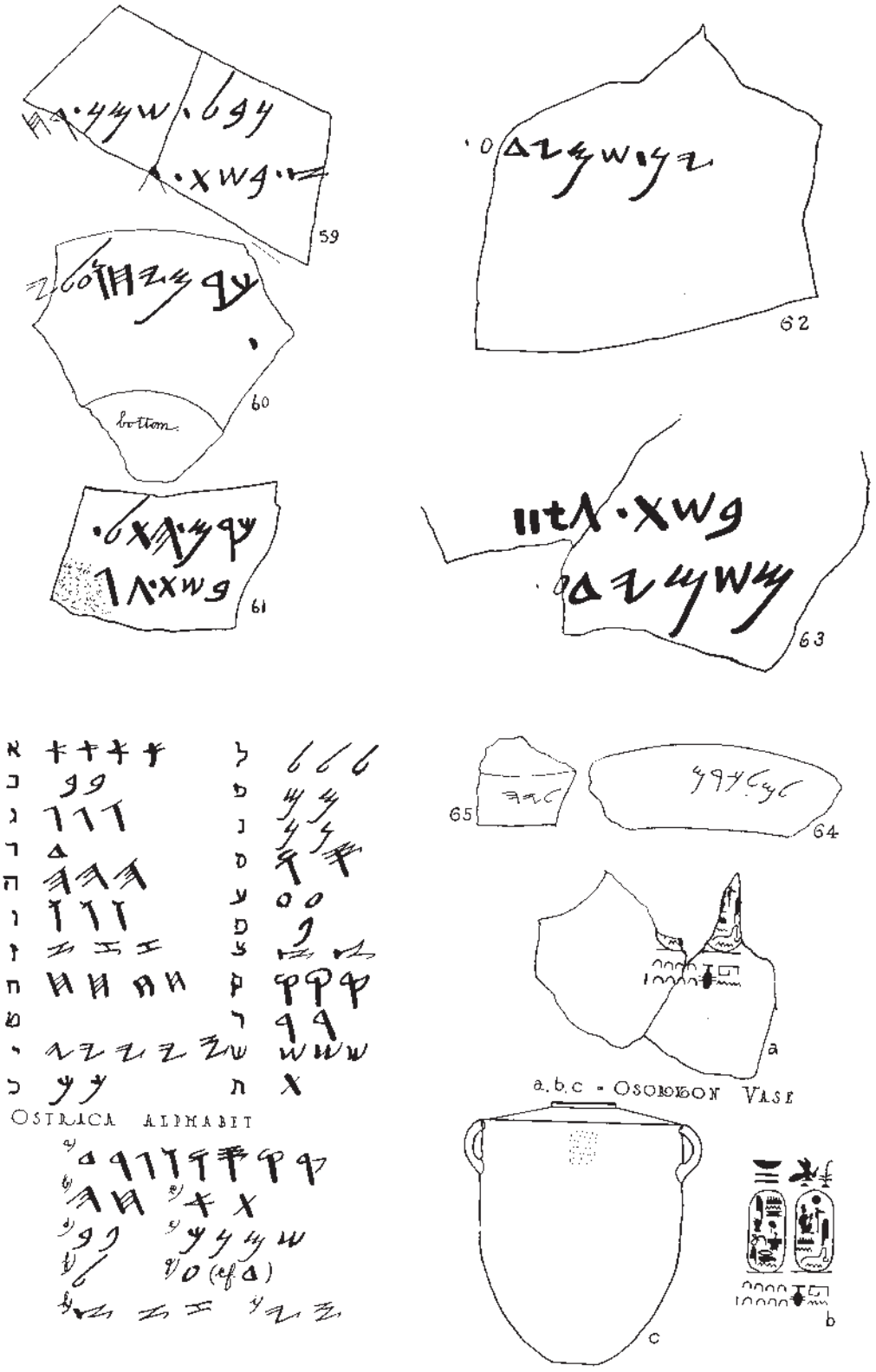
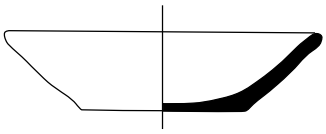
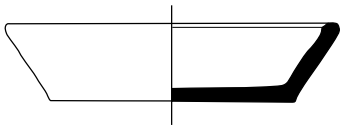


Plate V 1910 Ostraca: Nos. 59–63; the ostraca script; two additional Hebrew inscriptions; and the Osorkon Jar inscription.

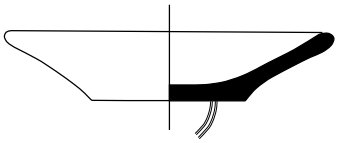
A. Ostraca-Bearing Pottery



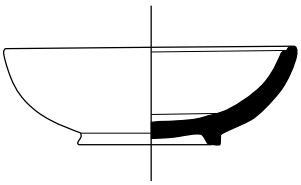
Ostracon No. 1
***HES I,* Fig. 154:6; Reg. No. 4075; Type I.14.i**
Provenance = S4-417 N sub



Ostracon No. 26
***HES I,* Fig. 156:16a; Reg. No. 3873; Type II.16.a**
Provenance = S4-418

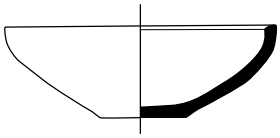


Ostracon No. 38
***HES I,* Fig. 154:5; Reg. No. 3992; Type I.14.h**
Provenance = S4-417

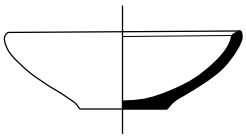


Ostracon No. 39
***HES I,* Fig. 154:13; Reg. No. 4619; Type I.19.a**
Provenance = S7-772
(compare also Pottery Dis. No. 545 below)

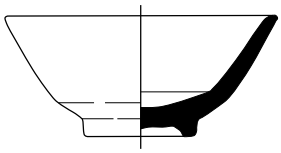
B. Bowl Types Representing Parallels for Ostraca-Bearing Pottery



Representing Pottery Reg. No. 4527, Ostracon No. 40
***HES I,* Fig. 154:11; Reg. No. 4116; Type I.18.c**



Representing Pottery Reg. No. 4527, Ostracon No. 40
***HES I,* Fig. 154:12; Reg. No. 4117; Type I.18.b**

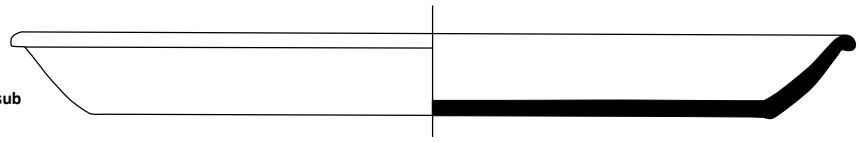


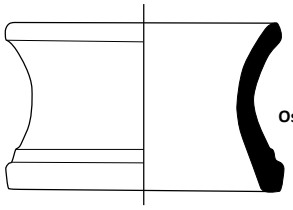
Representing Pottery Reg. No. 4630, Ostracon No. 50
***HES I,* Fig. 154:14; Reg. No. 3860; Type I.20.a**
Ostracon No. 50 = Provenance S7-772 N

Provenance = S7-772 = Ostraca Nos. 7, 9, 11, 12, 14, 15, 39, 40, 47, 52, 56, 60

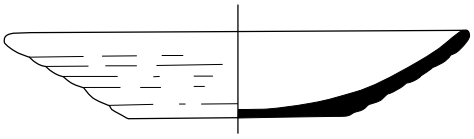
C. Non-Ostraca Pottery from Same Findspots as Ostraca Pottery

Pottery Dis. No. 382
***HES I,* Fig. 156:17a; Type II.17.a**
Ostracon No. 5 = Provenance S4-418 sub





Pottery Dis. No. 545
***HES I,* Fig. 153:13; Type I.12.c**
Ostracon No. 3 = Provenance S7-772 sub



Pottery Dis. No. 379
***HES I,* Fig. 154:7; Type I.14.j**
Ostracon No. 3 = Provenance S7-772 sub

Plate VI Ostraca and ostraca-related pottery.

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Lyon Diaries
(The following writings constitute a series of unpublished journals recorded by D. G. Lyon both on site at Samaria and in Cambridge, MA. These records help to chart the daily progress of the excavation and give an account of his dealings with officials in Palestine, Istanbul, and at home. As with Reisner's, Schumacher's, and Fisher's unpublished field diaries, Lyon's three volumes add a key component to the present study. Citations of these works appear according to their diary number followed by the specific page reference, e.g., *Lyon Diary II*, 47.)

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Reisner Diaries

(The following writings constitute a series of unpublished journals recorded in the field by Reisner to chart the daily progress of the excavation and to give an account of his dealings with local officials. As with Lyon's, Schumacher's, and Fisher's unpublished records, these seven volumes add a key component to the present study. Citations of these works appear according to their diary number followed by the specific page reference, e.g., *Reisner Diary IV*, 400.)

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George Andrew Reisner counted the Israelite ostraca among the most important finds ever recovered by the Harvard Expedition to Samaria. But the precise provenance of these historic inscriptions has remained murky at best. To date, the most incisive and intuitive statement on their archaeological context comes from a brief treatment in I. T. Kaufman's unpublished 1966 dissertation written at Harvard University. The present study considers in much greater detail the depositional history of the Ostraca House and its immediate surroundings. The investigation proceeds on three distinct but related levels. First, it attempts to clarify the date and nature of the archaeological contexts from which excavators recovered the inscriptions. Second, it evaluates both the quantity and quality of data recovered and the overall manner in which the project leaders presented those data in their official excavation report. Finally, the study draws not only from published records but also from unpublished materials recorded in the handwritten daily journals and private diaries of David Gordon Lyon, George Andrew Reisner, Clarence Stanley Fisher, and Gottlieb Schumacher. Thus, an important subplot unfolds as the analysis of archaeological remains advances through the narrative. The unpublished records not only provide supplementary data crucial to a study of the ostraca, they also enliven the story behind the discovery of the inscriptions and reveal the archaeological and administrative trials persistently faced by the excavators, who found themselves working betwixt and between international and local powers and events during the waning years of the Ottoman Empire and the coming of World War I. Still, the internal and external struggles of a start-up expedition cast within a bourgeoning academic field and the vicissitudes of world affairs did not prevent the Harvard Expedition from becoming one of the most influential projects of the early twentieth century.

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Front cover image: Samaria, view from the Southwest.
Photograph courtesy of Albatross Aerial Perspective, Herzliya, Israel