

The Judean *lmlk* Stamps: Some Unresolved Issues

MICHAEL S. MOORE
Allentown, Pennsylvania

One of the more complex problems in Syro-Palestinian archaeology has to do with the discovery, analysis, and interpretation of the *lmlk* seals of Iron Age Judah. Just around every bend in the twists and turns of the secondary literature on this subject a different opinion is expressed about some aspect of these seals. Why are there four-winged *and* two-winged impressions? Why is the lettering on many so crude and effaced? Can a paleographical development be traced? What does the imagery represent? Can its development be traced? How many actual seals were responsible for the ca. 1000 jar-handle stamps unearthed so far? What meaneth these four names--Socoh, Hebron, Ziph, and *mmšt*? What relationship, if any, exists between the *lmlk* seals and the private seals? Can these seals shed light on a crucial period in biblical history?

This last question, of course, is the question with which biblical scholars most want to engage, yet the present controversy encircling the different answers offered cannot be appreciated fully without careful attention being paid to the archaeological foundations undergirding them. It seems wisest, moreover, not to attempt here to survey extensively all the questions raised in the previous paragraph, but to focus on a few of the more entangled areas of disagreement. Consequently, this paper will arbitrarily be limited to a discussion of three areas: (1) paleography, (2) the significance of the four place-names, and (3) the light, if any, which these seals can shed on our attempts to reconstruct the history of Israel in the Iron Age.

1. *Paleographical Analysis*

Serious paleographical analysis of these seal-impressions begins with the analysis of David Diringer,¹ epigrapher of the Wellcome-Marston Lachish expedition. Diringer's original analysis serves even today as *the* starting point for all attempts to date these seals by this type of inquiry. Though modified somewhat by P. Lapp² and F.M. Cross,³ his division of these

¹"The Royal Jar-Handle Stamps of Ancient Judah," *BA* 12 (1949): 70-86. Cf. also his preliminary report "On Ancient Hebrew Inscriptions Discovered at Tell ed-Duweir (Lachish)," *PEQ* 73 (1941): 38-56, 89-106, pls. VI-VII.

²"Late Royal Seals From Judah," *BASOR* 158 (1960): 11-12.

³"Judean Stamps," *Eretz Israel* 9 (1969): 20-27.

seals into three basic stylistic classes still stands fundamentally unchallenged, a division which he put forward by means of stylistic analysis first, then supported by paleographical analysis of the script on these jar-handles.

Diringer noted what he called an "epigraphic cleavage"⁴ between classes 1 and 2. In the former, he saw the letters as long and thin, and partially irregular; whereas the second class consisted of letters short and squat, as well as becoming more regular. Furthermore, class 2 consisted of seals upon which the stems of the *lamed*, *mem*, *nun*, and *pe* seemed to curve at the bottom. Some of the vertical strokes of the *he* go beyond the horizontal ones, which he interpreted as a later development. In class 3, moreover, he noted that the short vertical prongs of the head of the *mem* were not joined to the main stem and that the *waw* consisted of a vertical stem curved at the top toward the left and cut by a hook, followed by "an elegant ligature."⁵ Class 1 was thus assigned to an era immediately preceding the Siloam inscription (8th century BCE); class 2 to an era immediately following it (7th century BCE);⁶ and class 3 to the end of the 7th century BCE.

The large cache of additional royal jar-handles found at el-Jîb,⁷ however, provoked Paul Lapp⁸ to issue a modification of Diringer's analysis, and thus his dates. Some of the class 1 four-winged scarab impressions found at el-Jîb, for example, had letters which were anything but "long and thin."⁹ Lapp further questioned whether the downward curve to the left of the long stems of *mem*, *nun*, and *pe* in the class 2 inscriptions were to be attributed to genuine paleographical development or to simple lack of space on the seal.¹⁰ More seriously, he challenged outright Diringer's interpretation of a division between classes 2 and 3 on the basis of an extension of the *he* in *Socoh* beyond the horizontal stroke in class 2 but not in class 3, since at least two class 3 *Socoh* seal-impressions from el-Jîb had the top stroke extending beyond the vertical stroke.¹¹ In sum, Lapp

⁴BA 12 (1949): 76.

⁵Ibid., p. 77.

⁶Diringer later raised the date for class 2 from the reign of Manasseh to the end of the 8th century BCE in connection with the difficulty of dating level III at Lachish. Cf. O. Tufnell, *Lachish III: The Iron Age*, 2 vols. (London: Oxford Univ. Press, 1953), p. 344.

⁷Cf. J.B. Pritchard, *Hebrew Inscriptions and Stamps from Gibeon* (Philadelphia: University Museum, University of Pennsylvania, 1959). For a comprehensive cataloging of Judean royal stamps, cf. P. Welten, *Die Königs Stempel* (Wiesbaden: Harrassowitz, 1969). For comparative stylistic analysis, cf. H. Gressman, *Altorientalische Texte und Bilder zum alten Testament* (Berlin: de Gruyter, 1927), particularly vol. 2, pls. 52, 125, 127, 132f., 139, 209, 218, 223, and 226.

⁸BASOR 158 (1960): 11-22.

⁹Cf. Pritchard, *Hebrew Inscriptions*, fig. 9:523.

¹⁰BASOR 158 (1960): 20.

¹¹Ibid.

argued forcefully that more attention must be directed to the major lines in the evolution of the script, not the idiosyncrasies that preoccupy Diringers.¹²

In effect, Lapp poked enough holes in Diringers's analysis to question seriously whether such a broad "epigraphic cleavage" between classes 1 and 2 could be sustained, based only on the paleographical evidence.¹³ This would in turn collapse the range of the *lmlk* seals considerably to a much narrower range than had been considered, since there simply was not enough paleographical evidence to sustain adequately the earlier dates offered for class 1.

H. Darrell Lance¹⁴ accepted Lapp's critique of Diringers's analysis, but Y. Aharoni did not.¹⁵ In fact, Aharoni stated, "attempts to distinguish paleographical differences between the two types of seals do not stand up under criticism."¹⁶ Thus he left the distinct impression that there was little substantive difference between the four-winged scarab and the two-winged seals;¹⁷ at least this was how he was interpreted. I think, rather, that Aharoni was addressing himself to the alleged differences between the class 1 and 2 stamps proposed by Diringers (in other words, agreeing with Lapp, whom he cites in a note), even though, as Lance is rather quick to point out, he does leave the impression that the *entire lmlk* series lacks any genuine evolutionary development over time.

In addition, Aharoni went on to raise the question of the so-called cursive *aleph* found on some of the *lmlk* stamps found in the well at el-Jib.¹⁸ Since

¹²Ibid. Cf. the similar criticism of H. Darrell Lance, "The Royal Stamps and the Kingdom of Josiah," *HTR* 64 (1971): 318--"One cannot overstress the importance in paleographical analysis of determining the direction in which the script is moving. Lapidary styles are under the influence of cursive styles, and it is not the majority form which decides the case, but those forms, more rare, which betray the influence of the developing cursive." Similarly, J. Naveh, "Review of J. Hoftijzer and G. van der Kooij, *Aramaic Texts from Deir 'Allā*," in *IEJ* 29 (1979): 134, is critical of van der Kooij's "discovery" of a "scientific tool" for tracing the evolutionary development of a script by minutely calculating the angle at which the writing instrument is held, simply because it adds nothing significant to what was already known about Old Aramaic script.

¹³Still, Lapp accepted Diringers's stylistic argument for seeing a major break between the four-winged scarab and the two-winged disc, *BASOR* 158 (1960): 21.

¹⁴*HTR* 64 (1971): 316-332.

¹⁵*The Land of the Bible* (London: Burns and Oates, 1969), pp. 340-346.

¹⁶Ibid., p. 342.

¹⁷Thus "it is most unlikely that they continued to be used until the fall of the Judean kingdom," Ibid., p. 341.

¹⁸Pritchard, *Hebrew Inscriptions*.

an example of this cursive *aleph* had earlier been found on an inscription dated near to the time of the Siloam inscription (ca. 700 BCE),¹⁹ Aharoni adduced this to be even further evidence of a late 8th century date for the *lmlk* jar-handles.²⁰ To this latter line of argument Lance raised two objections: (a) Archaeological (stratigraphical) -- The *lmlk* jar-handles found at el-Jîb cannot be dated definitively at all. In fact, their presence in the round stairwell displayed all the markings, according to the excavator, of being a secondary placement, that is, a fill.²¹ (b) Paleographical --

The presence of one feature in the script of the Gibeon handles which is comparable to the royal steward inscription is irrelevant if the script of the Gibeon handles contains clear evidence of the influence of later developments. And the presence of such late features in the Gibeon script has been overwhelmingly demonstrated by Cross, who dates the Gibeon material to the 6th century.²²

André Lemaire, however, has carefully shown²³ that the so-called "cursive *aleph*" on the jar-handles found at el-Jîb can also be found on inscriptions dating from the first half of the 8th century at Samaria,²⁴ on the Siloam inscription,²⁵ on the graffiti from tomb 1 of Khirbet el-Qôm,²⁶ in the inscription of Nahal Ishaï,²⁷ in the seal-inscriptions of el-Jîb,²⁸ in inscription number 7 from Khirbet Beit Lei (8 km east of Lachish),²⁹ and on an inscription found in stratum VII at Arad, which is dated by Aharoni³⁰ to the end of the 8th century. Furthermore, since this "cursive *aleph*" was no longer found in Arad stratum VI, nor in Lachish II or I, Lemaire argues that this type of *aleph* "seemed to have been quite characteristic of the letter in this period,"³¹ i.e., the end of the 8th century BCE.

¹⁹N. Avigad, "The Epitaph of a Royal Steward from Siloam Village," *IEJ* 3 (1953): 137-152.

²⁰*Land of the Bible*, p. 342.

²¹Pritchard, *Hebrew Inscriptions*, p. 12.

²²Lance, *HTR* 64 (1971): 319, referring to Cross, *Eretz Israel* 9 (1969): 20-27; cf. further J. Naveh, *The Development of the Aramaic Script* (Jerusalem, 1970); J.B. Peckham, *The Development of Late Phoenician Scripts*, 1968.

²³"Les Inscriptions de Khirbet Beit Lei," *RB* 83 (1976): 564.

²⁴Cf. F.M. Cross, "Epigraphic Notes on Hebrew Documents from the 8th-6th Centuries II. The Murabba'at Papyrus and the Letter found near Yabneh-yam," *BASOR* 165 (1962): 36.

²⁵N. Avigad, "Epitaph," *IEJ* 3 (1953): 149.

²⁶W.G. Dever, "Iron Age Epigraphic Material from the Area of Khirbet el-Qôm," *HUCA* 40-41 (1969-1970), pp. 157-158.

²⁷Cf. P. Bar-Adon, "An Early Hebrew Inscription in a Judean Desert Cave," *IEJ* 25(1975): 230-231.


²⁸Pritchard, *Hebrew Inscriptions*, p. 14.

²⁹Lemaire, "Khirbet Beit Lei," *RB* 83 (1976): pl. XLIII.

³⁰Y. Aharoni, *Arad Inscriptions* (Jerusalem, 1975), p. 105.

³¹Lemaire, *RB* 83 (1976): 564.

In sum, therefore, the primary difference between Diringer/Aharoni/Lemaire's critique and that of Lapp/Lance/Cross seems to be one of method. The former group tends to focus on minute analysis of the forms of the letters with only secondary attention paid to the question of the evolutionary development of the script(s) under scrutiny. The other approach is concerned, first of all, with the historical development of a particular script or family of scripts, that is, the manifest tensions between cursive/lapidary scripts, differences in angles of writing over time, and other broadly diachronical issues. Only secondarily is it concerned with the chronological placement of any given inscription within this overall schema. This methodological difference is primarily responsible for the present lack of firm resolution on the paleographical problem.

2. 

A second problem seeking resolution is the lack of consensus with regard to the interpretation of the four place names found on the *lmk* seals. As early as 1893, A.H. Sayce³² put forward the theory that the royal stamps indicated that the jars upon which they were impressed must have been manufactured at royal, state-owned potteries at Socoh, Hebron, Ziph, and *mmš*, a mysteriously unknown fourth location. R.A.S. MacAlister³³ rejected this view, preferring instead to view these names as the proper names of the potters. Though this view probably deserves more attention than it has gotten,³⁴ apparently no one has publicly defended it. A third interpretation of these names is that they represent the names of administrative centers in Judah. Albright³⁵ first proposed this view (later revived by Aharoni³⁶), but later moved closer to Ch. Clermont-Ganneau's opinion;³⁷ namely, that these jars had been officially-gauged receptacles for the collection of wine, oil, or grain, representing taxes in kind furnished to royal storehouses by the chief cities of the kingdom.³⁸ Still another modification of this view is that offered by O. Tufnell,³⁹ the director of the

³²"The Phoenician Inscriptions on the Vase Handles found at Jerusalem," *PEFQS* (1893): 241.

³³"The Royal Potters: 1 Chronicles. 4:23," *Expository Times* 16 (1905): 379-380.

³⁴R. Jose, *Maasser Shenit* 4.11 long ago suggested that the inscribed letters on vessels (*qôp*, *mem*, *dalet*, etc.) could represent the initials of men's names. Both Yadin at Masada and Meshel at Kuntillet Ajrud have found sherds with such letters inscribed.

³⁵"The Administrative Divisions of Israel and Judah," *JPOS* 5 (1925): 45ff.

³⁶*The Land of the Bible*, pp. 340ff. Maintained in *The Archaeology of the Land of Israel* (Philadelphia: Westminster, 1982), p. 259.

³⁷*PEFQS* (1899): 204ff.

³⁸Pritchard, *Hebrew Inscriptions*, pp. 20-23, has plausibly suggested that the concentric circles and other marks inscribed on some of the jars after firing signify the cancellation of their "royal" status.

³⁹Cited by Diringer, *BA* 12 (1949):80.

Wellcome-Marston Lachish expedition after the tragic murder of J.L. Starkey. Tufnell believed these cities to have been royal farms ("vineyards"-Avigad⁴⁰) situated at, or near, Socoh, Ziph, Hebron, and *mmšt*. Yigael Yadin,⁴¹ finally, has suggested (based on Josh. 15:21ff.) that these four cities had once been the nuclei of four separate defense zones -- the Negev (*mmšt*), the Shephelah (Hebron, Josh. 15:54), the lowlands (Socoh, Josh. 15:35), and the wilderness of Ziph (1 Sam. 23:14 ff.). Although I have no really firm opinion on which of these many interpretations might be correct,⁴² I do want to comment briefly on one of them, namely the philological equation first put forward by H.L. Ginsberg⁴³ that *mmšt* equals *mmš(l)t*.

The ever-mysterious location of *mmšt* has proved to be an exasperating conundrum for scholars. Albright⁴⁴ first equated *mmšt* with ancient Mampsis (Kurnub in the Negev), then changed his mind, though Yadin⁴⁵ thought he was correct the first time. Ginsberg,⁴⁶ however, proposed an uncharacteristically incredulous philological interpretation in a two-page note in *BASOR*, based on what he considered to be adequate parallel phenomena at Tell en-Nasbeh and the 2nd century BCE stratum at Sparta. At Tell en-Nasbeh, several post-exilic seals were found which read *msh*⁴⁷, and at Sparta E. Bickerman(n)⁴⁸ proposed reading *baleōs* as an abbreviation for *ba(si)leōs*. By means, then, of this unashamedly circular argument, Ginsberg proposed reading *msh* as an abbreviation for *mš(p)h* (Mizpah), and *mmšt* as an abbreviation for *mmš(l)t*, "government," that is, an administrative designation for the city-state of Jerusalem. Now Y. Aharoni,⁴⁹ in a work published posthumously by his wife, Miriam, has proposed, based on Ginsberg's old suggestion (whom he fails to credit, by the way), that Hezekiah combined the 12 former administrative districts into a leaner 4 in order to "streamline his administration"⁵⁰ -- Socoh, Ziph, Hebron, and

⁴⁰"New Light on MSH Seal Impressions," *IEJ* 8 (1958): 119.

⁴¹"The Fourfold Division of Judah," *BASOR* 163 (1961):6-12.

⁴²Cf. "Conclusions" below.

⁴³"*Mmšt* and *Msh*," *BASOR* 109 (1948): 20-21.

⁴⁴*JPOS* 5 (1925): 45ff. Albright modified his position in *The Excavation of Tell Beit Mirsim*, AASOR 21-22 (New Haven: ASOR, 1943), p. 75.

⁴⁵*BASOR* 163 (1961): 7.

⁴⁶*BASOR* 109 (1948): 20-21.

⁴⁷C.C. McCown, *Tell en-Nasbeh* (Berkeley: Palestine Institute of Pacific School of Religion, 1947), vol. 1, pl. 56.26.

⁴⁸*Berytus* 8 (1944): 78, nt. 31.

⁴⁹*Archaeology of the Land of Israel*, p. 259.

⁵⁰*Ibid.*

memšelet/Jerusalem (2 Kgs. 20:13). His reasoning: because the former division “was surely complex and unwieldy for such a small kingdom.”⁵¹

In addition to Nahman Avigad’s⁵² reasons for rejecting Ginsberg’s original suggestion, I can only add the following observations: (a) The philological argument already is based on insufficient evidence in Ginsberg’s proposal, but to attempt to build a superstructure upon it as Aharoni does is nothing less than forcing the evidence. If this is allowed to stand, one could conceivably use it to juggle the consonants, by analogy, of any one of a number of difficult-to-read ancient inscriptions into whatever position one likes in order to buttress whatever theory one happens to prefer. Is it not simply the case that Aharoni has latched onto Ginsberg’s proposal in order to support his earlier dating of the *lmlk* inscriptions to the reign of Hezekiah rather than the Josianic era preferred by Albright, Diringier, Lapp, Cross, Lance, and Tushingham?⁵³ (b) The OT formulaic phrase *byrwšlm wblbnwn wkl rs mmsltw* repeated verbatim in 1 Kings 9:19 and 2 Chronicles 8:6, sharply militates against any simple equation wherein *ywšlm* equals *mmslt*. On the contrary, this formulaic phrase indicates instead that *mmslt* (“domain,” “government”) is a much broader term than either *ywšlm* or *lbnwn* and thus cannot be simply equated with either.

3. Date and Historical Context

As far as I can see, there are basically only two views taken by scholars who seek to integrate the above paleographical and onomastic data provided by the royal Judean stamps into a reconstruction of the history of late pre-exilic Judah.

A. On the one hand, earlier theoreticians posited a rather lengthy timespan for the life of these seals, based predominantly on Diringier’s already cited paleographical/stylistic analysis. Albright,⁵⁴ for example, suggested that class 1 was to be dated to Hezekiah’s time (714-686 BCE), class 2 to Manasseh’s time (686-641 BCE), and class 3 to the period of Josiah and his immediate successors (639-589 BCE). To these broad suggestions, Diringier⁵⁵ prosed further that class 1 not be lowered any later

⁵¹Ibid.

⁵²*IEJ* 8 (1958): 119ff.

⁵³Albright, *JPOS* 5 (1925): 45ff.; Lapp, *BASOR* 158 (1960): 11-22; F.M. Cross, *Eretz Israel* 9 (1969): 20-27; Lance, *HTR* 64 (1971): 316-332; A.D. Tushingham, “A Royal Israelite Seal (?) and the Royal Jar-Handle Stamps,” *BASOR* 200 (1970): 71-78; 201 (1970): 23-35.

⁵⁴*Tell Beit Mirsim*, pp. 74-75.

⁵⁵*BA* 12 (1949): 85-86. Diringier (p. 86) does warn that his epigraphic conclusions were tentative, a caveat that perhaps was not taken seriously enough by later scholars who dealt with said conclusions.

than ca. 700 BCE (the date of the Siloam inscription), while Lapp,⁵⁶ rejecting any sharp break between classes 1 and 2, lowered the date for both these classes even further, accepting Ginsberg's theory⁵⁷ that Hebron and Ziph probably could not have been restored to Judah (after the 701 BCE invasion of Sennacherib) until Assurbanipal's campaign in 667 BCE.

To be included among those who more readily prefer an earlier date in Hezekiah's reign for these *lmk* jars are also those who would prefer to tie this paleographic/stylistic preference to an earlier dating of the destruction stratum III at Lachish than had formerly been widely accepted, that is, to the invasion of Sennacherib in 701 BCE instead of Nebuchadnezzar's 597 BCE invasion. In fact, Aharoni⁵⁸ uses this preference to support his dating of the *lmk* jars (all of them?) to Hezekiah's reign. This is then cited as further evidence in order to support O. Tufnell's⁵⁹ controversial early dating of Lachish III to Sennacherib's invasion; whereas Albright, Buchanan, Kenyon, Wright, and Lance⁶⁰ all registered strong objections, setting out instead a 597 BCE date for Lachish III and a 587 date for Lachish II.

Now David Ussishkin's⁶¹ recent work at Lachish has attempted to solve the problem of dating Lachish III once and for all, arguing persuasively from biblical, cuneiform, stratigraphical, and monument-pictorial evidence⁶² for a 701 BCE date, but it remains to be seen whether this new stratigraphical/ceramic analysis will be successfully challenged by those who formerly held to 597. Ussishkin's excavation has further found *lmk*

⁵⁶BASOR 158 (1960): 21.

⁵⁷Ginsberg, "Judah and the Transjordan States from 734 to 582 BCE," in *Alexander Marx Jubilee Volume*, pp. 349-351, cited in BASOR 158 (1960): 18-19.

⁵⁸*Land of the Bible*, p. 342.

⁵⁹*Lachish III*, p. 55.

⁶⁰Albright, BASOR 150 (1958): 24; B.W. Buchanan, *AJA* 58 (1954): 335-339; Kenyon, et al., *The Objects from Samaria: Samaria-Sebaste III* (London, 1957), pp. 204-208; G.E. Wright, *VT* 5 (1955): 97-105; Lance, *HTR* 64 (1971): 321 ff. Wright's reasons for rejecting Tufnell's stratigraphical analysis bear repeating: (a) no typological difference in the pottery of levels II and III (Starkey's original conclusion, *PEQ* (1937): 176; (b) *lmk* jar-handles of all three classes found in levels II and III. Tufnell (and Aharoni) argues that greater number of class 2 in level III indicates an earlier date. Wright argues that the presence of all three classes in level III outweighs quantity found; (c) if the palace was destroyed in 701 BCE, why was it never rebuilt in the intervening century before level II's destruction?

⁶¹"Answers at Lachish," *BAR* 5, 6 (1979): 16-39.

⁶²Cf. H. Shanks, "Destruction of Judean Fortress Portrayed in Dramatic Eighth-Century B.C. Pictures," *BAR* 10, 2 (1984): 48-65, a review of Ussishkin's book, *The Conquest of Lachish by Sennacherib* (Tel Aviv: Tel Aviv University Publications of the Institute of Archaeology 6, 1982. Cf. also D. Ussishkin, *IEJ* 24 (1974): 272-273; *IEJ* 25 (1975): 166-168; *BASOR* 223 (1976): 1-14.

jars of all 3 classes in level III, which leads him to conclude that by the time that level II was destroyed in 588/6 BC by the Babylonian army, these vessels were not in use anymore in Lachish, and probably not in the rest of Judah.⁶³

B. On the other hand, those who still feel (on paleographic, stylistic, and stratigraphical grounds) that the *lmlk* stamps ought to be dated, at least in their two-winged variety, to the time of Josiah, also tend to date Lachish III at 597 BCE and Lachish II at 587 BCE. Thus the dating of the *lmlk* jar-handles is directly related to two very different archaeological/historical reconstructions about what really happened in Judah before the Judean monarchy finally collapsed.⁶⁴

Conclusions

1. At stake, therefore, in the paleographical debate on the *lmlk* jar-handles from Judah is much more than Aharoni's acceptance of Diringer's analysis vs. Cross' basic acceptance of Lapp's analysis. Rather, the fundamental issue is one of method. Yet much of the more sweeping chronological conclusions previously drawn from the paleographical evidence will be moot if Ussishkin's new stratigraphy is widely accepted.
2. The location and interpretation of the four place-names is a bit trickier, but, in my opinion, the theories of MacAlister, Ginsberg, and Aharoni⁶⁵ might be considered *weakest*. Clermont-Ganneau, Tufnell, and Avigad⁶⁶ all have interesting proposals which we might label *weaker*. I suppose, therefore, that I would feel least uncomfortable with the proposals of Sayce, Albright, and the "defense department" proposal of Yadin.⁶⁷ Perhaps we should be satisfied only to label their suggestions *weak*.
3. It must be admitted in the final analysis, however, that the present state of the evidence culled from the *lmlk* jar-handles is simply too weak to speak authoritatively to a definitive reconstruction of Judah's late pre-exilic history. Though some opinions based on this seductive, highly frag-

⁶³BAR 5, 6 (1979): 36.

⁶⁴Not to be overlooked is A.D. Tushingham's intriguing explanation for the presence of both the four-winged scarab *and* the two-winged disc in Josiah's reign, *BASOR* 200 (1970): 71-78; 201 (1970): 23-35.

⁶⁵MacAlister, *Expository Times* 16 (1905): 379-380; Ginsberg, *BASOR* 109 (1948): 20-21; Aharoni, *Land of the Bible*, pp. 340ff. *Archaeology of the Land of Israel*, p. 259.

⁶⁶Clermont-Ganneau, *PEFQS* (1899): 204; Tufnell, cited by Diringer, her co-worker, *BA* 12 (1949): 80; Avigad, *IEJ* 8 (1958): 119.

⁶⁷Sayce, *PEFQS* (1893): 241; Albright's original opinion, *JPOS* 5 (1925): 45ff.; Yadin, *BASOR* 163 (1961): 6-12.

mentary evidence may seem more acceptable than others, they remain simply that--opinions. Doubtless David Ussishkin's new excavations at Lachish will solve some of the stratigraphical problems, but I doubt whether his thesis will solve the paleographical and stylistic problems still orbiting around these mysterious handles to everyone's liking, though it will be interesting to see if he unearths a handle that clearly reads *lmlk mmšt*, or produces, say, a two-winged scarab. Now if he finds a seal that reads *lmlk ḥzqyh* or *lmlk y'šyhw*, that would be something!



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