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DOR AND IRON AGE CHRONOLOGY: SCARABS, CERAMIC SEQUENCE AND ^{14}C

Ayelet Gilboa, Ilan Sharon, Jeffrey Zorn

Abstract

Recently, Stefan Münger proposed that Egyptian so-called ‘mass-produced’ stamp-seal amulets may be traced to a Tanite origin and dated to the late 21st–early 22nd Egyptian Dynasties. Among these, it has been suggested that some scarabs bear the name of Siamun of the late 21st Dynasty, who ruled in the first half of the 10th century BCE. Since in Palestine these scarab seals first occur in late Iron Age I contexts, Münger suggests that they corroborate the ‘low Iron Age chronology’, which incorporates most of the 10th century in Iron Age I rather than in Iron Age II. The site of Tel Dor, on Israel’s Carmel coast, produced the best stratified group of these scarabs, including one that was identified as bearing the name Siamun. This paper elucidates the archaeological context of these finds and discusses their chronological repercussions vis-à-vis Münger’s proposal.

Those of us involved in one way or another in the ‘controversy of the 10th century BCE’ in Levantine chronology¹ have often bemoaned the remarkable lack of chronological pegs in the 11th through 9th centuries BCE.

This, of course, is partly due to the general scarcity in these periods of epigraphic finds; such is the nature of ‘centuries of darkness’. Scarcity, however, does not mean lack; several date-bearing (or at least potentially date-bearing) artefacts have turned up over the one-hundred-and-fifty-year-long search.²

An unmitigated run of bad luck has decreed that all of these were found out of context, in secondary contexts or in excavations so old that no reliable archaeological context can be attributed to them.

Therefore, when an object which is claimed to date to the 10th century on its own merits is uncovered in a controlled, modern excavation, it is incumbent on the excavators to publish it promptly, fully (including its provenience, stratigraphy and context) and without evading either its potential or its problems.

The object in question is a scarab uncovered during Ephraim Stern’s excavations

¹ See, e.g., Finkelstein 1996 vs. Mazar 1997; for recent syntheses see Dever 2001 vs. Finkelstein and Silberman 2001.

² These include, for example, the Osorkon II alabaster vase from Samaria, the Moabit stone, the Dan inscription, statues of 22nd-Dynasty monarchs in Byblos, the fragmentary stele of Shoshenq I from Megiddo, and scarabs suggested to belong to Siamun—one from Megiddo, one from Tell el-Far‘ah (S), and an unprovenanced one in the Dayan collection (for the scarabs see Münger 2003 and below).

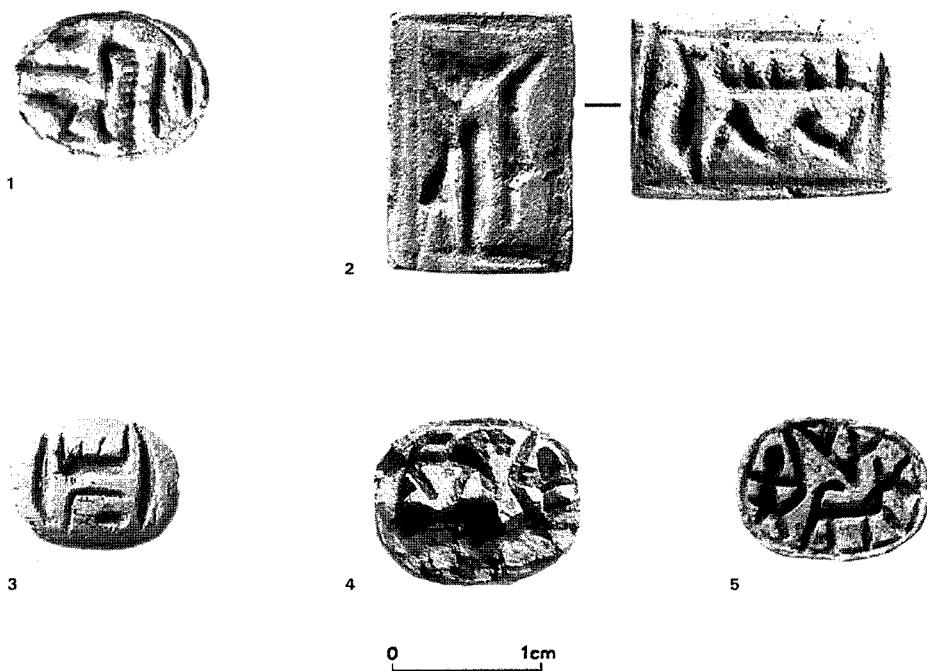


Fig. 1. The five Area G/7a mass-produced scarabs (see Table 1 for legends).

at Tel Dor (Fig. 1:1), which Stefan Münger (2003) suggested bears the name of the Egyptian ruler Siamun.³ This note completes the publication of this find by detailing its context and the contexts of related scarabs discussed by Münger.

Having expressed the lofty ideals of prompt publication, etc., we must here make a minor detour and offer both an apology and an explanation of how it is that we are only now in 2004 publishing a find uncovered in 1988. In the 1988 season we were just becoming acquainted with the Iron Age in Dor and in Area G. The main objectives set to Dennis Stanfill, square supervisor of AI32 by his area supervisor, Jeffrey Zorn, and the UC expedition's director, Andrew Stewart, were to dismantle and clear the late features out of the unit and to level it, preparatory to an orderly descent into the Iron Age levels. These late features included a Roman drain that bisected the unit, a sump partly cut by that drain and a large Persian pit. In between all of these features and cut by all of them was a small rhomboid area of mudbrick debris which yielded 'early' pottery as well as two seals—a scarab and a tabloid. It was not until 1993, when the unit was re-opened by Robyn Talman, that any clear

³ This suggestion, however, is being contested by a number of Egyptologists (D. Ben-Tor, personal communication).

Iron Age features were delineated and that floors were reached some 20 cm. below the level at which the said scarab was found.

Four things happened in 2001–2002 that made the present publication possible. First, Stephan Münger and his professor, Othmar Keel, completed the analysis of the scarabs from Area G at Dor and it was only as a result of this that we became aware that we had such an object among our finds. Second, a workshop aimed at elucidating the stratigraphy of Area G at Dor was held at Cornell University in July–August 2002 by Sharon and Zorn together with several other scholars (John Berg, Allen Estes, Elizabeth Bloch-Smith). It was only at this juncture that Zorn was able to connect the nondescript little locus cleared in 1988 with a floor excavated four years later and attribute both to local Phase 7. Third, Gilboa analyzed the Iron Age pottery sequence of Dor as part of her Ph.D. dissertation. The fourth and last piece of the puzzle to fit in is a result of another study in which Gilboa and Sharon, together with Elisabetta Boaretto, head of the ^{14}C laboratory at the Weizmann Institute, aimed at fixing the absolute chronology of the early Iron Age at Dor and other sites in the Levant. It is only the conjunction of these disparate analyses that enables us to appreciate this find and its context.

THE SEALS

The alleged Siamun scarabs at Dor and elsewhere are part of a larger group of so-called mass-produced scarabs (formerly termed ‘late Ramesside’, later ‘post Ramesside’). Münger (2003) suggested, on stratigraphic, stylistic and historical grounds, a Tanite origin for this group and an initial production date in the late 21st Dynasty, probably under Siamun, after which they continue at least until Shoshenq I, founder of the 22nd Dynasty. The earliest well-stratified Palestinian contexts in which these scarabs occur belong to the last phase of the Iron Age I (Münger 2003; and see below). These contexts are conventionally dated to the second half of the 11th and very early 10th centuries BCE, and their terminal dates are often correlated with Davidic destructions in conquests ostensibly undertaken *ca.* 1000–980 BCE. The ‘low chronology’ as proposed by Finkelstein (1996), on the other hand, correlates some of these late Iron Age I destructions with Shoshenq’s campaign, usually dated *ca.* 925 BCE⁴ (on this date see more below). Thus the occurrence in late Iron Age I contexts of glyptics whose deposition, according to Münger, could not have come about much earlier than 960 BCE (for this date, see further below), was taken by him as independent support for the low Iron Age chronology.

⁴ But for an estimate that destructions of settlement were definitely not the rule in such campaigns, see Na’aman 1998:276.

The Stratigraphy of Area G

The scarab was found, together with four other related scarabs, in a single grid unit (AI32) of Area G at Dor (for the location of this area, see Stern 2000: Fig. 35).⁵ A brief review of the stratigraphy of the grid square in question, and its relation to the general stratigraphic scheme of the area, is necessary to understand the context of the scarabs.⁶

Phases 1–5 of grid square AI32 represent Roman to Late Iron Age levels. The construction of the Phase 1 Roman forum and Persian-period pits greatly disturbed all underlying deposits, till the early Iron Age.

By Phase 6 most of the late disturbances have leveled off or are at least sufficiently localized to permit a coherent architectural plan to be drawn. Phases 10–6 represent a continuous development of a single basic architectural context spanning most of Iron Age I into Iron Age IIA (in Dor terminology: Ir1a[e] to Ir2a). Several of these phases represent different industrial purposes over time. These will be described from bottom to top (Fig. 2; see also Fig. 3).

Phase 10 is the least understood of the Iron I phases, but it is evident that all later phases are based, at least in part, on the constructional features laid out at this time. The presence of firing pits, a small furnace, a bellows pot, crucible fragments and bits of copper/bronze show that this area was a smithy used for the recasting of copper/bronze objects.

By Phase 9 the general plan of the unit is firmly established, and, as Fig. 2 shows, it will change little in the succeeding periods, through the end of Phase 6. The delineation of individual phases was possible only through the careful analysis of the rebuilds of the major walls, the shifting of minor partitions and the raising of floor levels.

The Phase 9 structure is dominated by a large courtyard, surrounded by rooms on at least three sides, partially roofed and partially stone paved (floor levels *ca.* 12.85–12.65 m.). This phase came to an end in a violent conflagration.

⁵ See Stern 2000 for a general description of the area; for an exposition of the Iron Age stratigraphy, see Sharon and Gilboa forthcoming, with references to preliminary reports.

⁶ There are few site-wide depositional (i.e., constructional and/or destructional) phenomena at Dor to *stratigraphically* correlate the many Iron Age phases in disparate areas. Therefore, for each area, a phasing scheme is worked out using, in as much as is possible, only stratigraphic/architectural relations. Each temporal episode within such a sequence is called a *phase* and numbered (top to bottom) in Arab numerals *relative to the area*. Lower case letters appended to phase numbers (e.g., G/6b) denote local sub-sequences within units or rooms, which cannot be correlated across the area (e.g., re-flooring of a room). The local phases of all [relevant] areas are then correlated *typologically* into a master series of *temporal horizons* and these are given names denoting their relative placement in the series: e.g., ‘Ir1a(l)’ would be late within the first (earliest) segment of the Iron Age I; Ir1|2 is an assemblage showing transitional attributes between the Ir1b and Ir2a. For fuller treatment see Gilboa and Sharon 2003; and see further below.

Although the house was completely destroyed, it was rebuilt in Phase 8 along the same lines—standing walls being reused (e.g., W9262 on the north of the AI32 courtyard) or rebuilt (e.g., W9963 was moved about 2.5 m. farther south). Floor levels were raised to *ca.* 13.50 m. and even the stone pavement covering exactly half of the courtyard was re-laid.

Phase 7 (the phase under discussion; Fig. 4) begins with a 10 cm. general raising of floor levels (to *ca.* 13.60 m.), including the construction of another stone paved floor (L9300) above those of Phases 9 and 8. The main constructional change is that the open courtyard is now sub-divided into several smaller spaces—the area is bisected north–south by W9704, and then the area north of W9704 is partitioned east–west by W9728.

Phase 6 represents a final floor raising of about 70 cm. (to *ca.* 14.50 m.) and a rebuilding of walls. On the north a stone wall, W9729, is built over the mudbrick W9262, and W9140 is rebuilt. The area is still bisected east–west—again by a stone wall, W9626—built over the mudbrick W9704, but W9728 has gone out of use. Phase 6 is already much disturbed by late pitting in AI32. Most of the walls are represented by stubs only, and no floors or sealed deposits of any extent were located in this unit. In other units of Area G two distinct sub-phases, 6b and 6a, were delineated—consisting of a raising of floor levels, separated by some 50 cm. of fill. Though no other architectural changes are apparent, the typological horizon represented by Phase 6a is later than that of Phase 6b (see below).

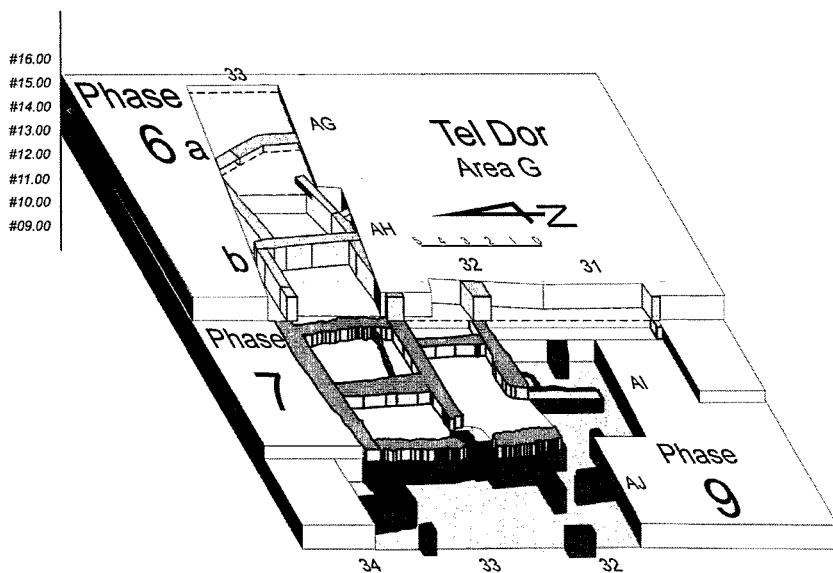


Fig. 2. Development of early Iron Age building in Area G.

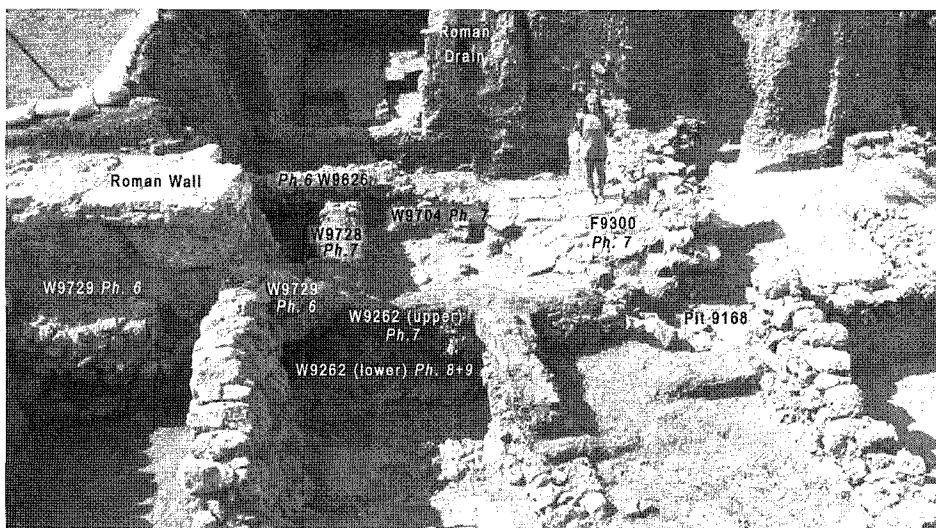


Fig. 3. View of Area G with several of the main Phase 6–8 architectural features in Unit AI/32; looking south.

DETAILED ARCHITECTURAL AND STRATIGRAPHIC CONTEXT OF THE DOR SCARABS

Two ‘mass-produced’ scarabs from Dor were published by Münger: the one attributed to Siamun (#92604, L9259; Münger 2003: Fig. 4:2), and another (# 98197, L9814; Münger 2003: Fig. 1:10), both originating from Square AI32, in a single room of local Phase G/7;⁷ both belong to the uppermost sub-phase in this room (7a; see further below). The same room produced three more ‘mass-produced’ scarabs (Keel and Münger forthcoming): another in the same locus with the ‘Siamun scarab’, a fourth one in L9730 of Phase 7a and a fifth wedged into the Phase G/7 pavement, L9300. Four sub-phases (from early to late—d-a), were defined in this room on the basis of floor raisings above and below pavement F9300 and the appearance of a small partition wall W9887.⁸ All five scarabs in this room come from Sub-phase 7a. The scarabs and their findspots are reported in Table 1.

⁷ The room in question is bounded on the north by W9262, on the south by W9704, on the east by W9728 and on the west by W9140 (see Fig. 4). There is a passageway to the south at the point where W9704 and W9140 would otherwise meet.

⁸ These sub-phases are local to this room and *do not* correspond on a one-to-one basis with similarly numbered sub-phases in other parts of the square or area. 7d represents the initial raising of floor levels, the rebuilding of earlier walls, and the addition of W9728; F9879=F9930=F18012 at 13.61–13.51 and is under W9887. 7c consists of the addition of W9887 and the raising of floors; F9878 at 13.63–13.60 is above F9330 and reaches W9887; F9300 may already be in use. 7b is another slight floor raising F9824=F9857=F9300? at 13.68–13.62. In 7a stone pavement F9300 is definitely in use. F9300=F9814=F9554=F9296=F9312 at 13.85.

No Phase 6 floors were delineated in the area of this room. It is thus impossible to draw an absolutely firm boundary between deposits of Phase 6 and Phase 7 in this room. However, it seems safe to say that the loci in which the scarabs were found belong to AI32 local Phase 7.⁹

TABLE 1: TEL DOR AREA G SCARAB FIND SPOTS

<i>Reg. #</i>	<i>Locus</i>	<i>Elev.</i>	<i>Context</i>	<i>Reference</i>
92604	L9259	14.04	20 cm. above floor F9296 <i>ca.</i> 13.83 m.; mudbrick material with small stones	Münger 2003: Fig. 4:2 ('Siamun')
92645	L9259	14.01	15 cm. above floor F9296 <i>ca.</i> 13.83 m.; mudbrick material with small stones	Keel and Münger forthcoming
94427	L9300	13.76	Imbedded between paving stones of F9300	Keel and Münger forthcoming
97134	L9730	13.96	10 cm. above F9814 <i>ca.</i> 13.85, 30 cm. above F9857 <i>ca.</i> 13.65 m.; mudbrick debris ¹⁰	Keel and Münger forthcoming
98197	L9814	13.90	5 cm. above F9814 <i>ca.</i> 13.85 m.; hard-packed dark brown mudbrick material, probably collapse from the adjacent walls. ¹¹	Münger 2003: Fig. 1:10

⁹ The loci involved are almost pure mudbrick material, while in L9210 above them (*at ca.* 14.57) the matrix is layers of stone and mudbrick debris, suggesting a collapsed wall on an otherwise undetected dirt floor. The elevations for this rubble level correspond closely with floor levels of Phase 6. Note too that the floors and findspots being discussed are *ca.* 50+ cm. *below* the top of W9728 (14.55); *Phase 6* floors only begin *above* the top of this wall and extend across it.

¹⁰ There is no reason to think that this scarab is an intrusion: there are no later pits adjacent. No floor was preserved in this area until F9857 at *ca.* 13.65. However, F9814 at *ca.* 13.85, and F9824 at *ca.* 13.75 were found just to the north and likely originally extended across the entire room, suggesting that this scarab was found *ca.* 10 cm. *above* a floor.

¹¹ It was found east of W9728 at elevation 13.90, virtually the same elevation as F9814 (*ca.* 13.85, clearly well below the Phase 6 floors). On this floor a small assemblage of *in situ* vessels (Figs. 7:1–3, 6, 7, 12, 20; 9:7, 8, 11, 13, 14) was found. As with most of the previous scarabs, this scarab and two other registration baskets were given the same elevation (13.90), suggesting that the scarab was found while sieving debris from the locus and then given the general end-of-day elevation for the pottery basket. Since it was found at the same time as the *in situ* vessels, there is little chance of this being an intrusive find.

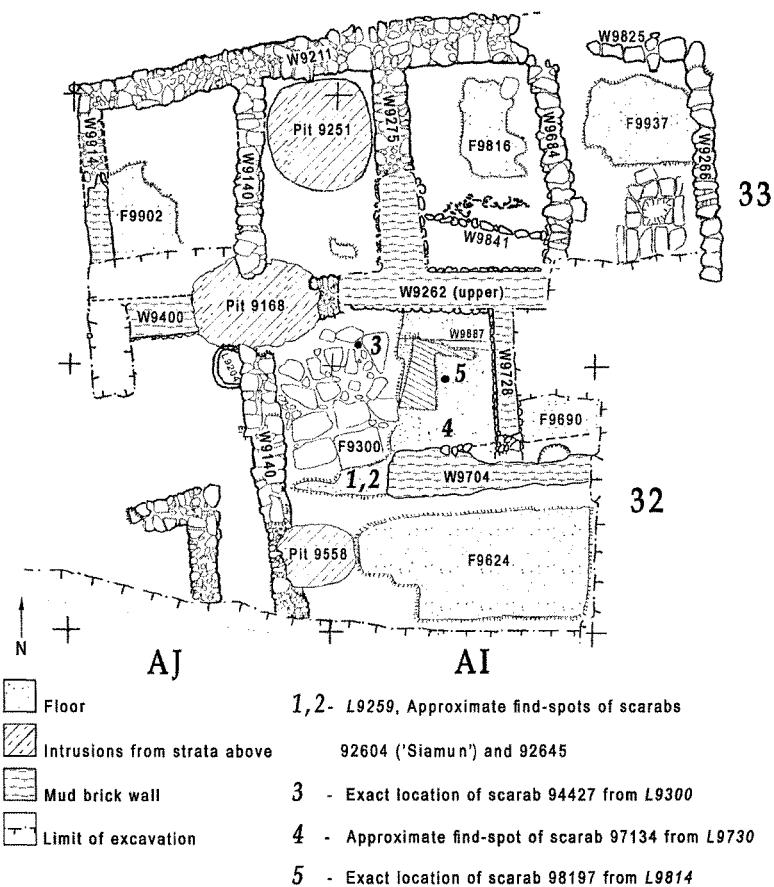


Fig. 4. Plan of Phase G/7, indicating findspots of 'mass-produced' scarabs.

A thorny issue is the cleanliness of L9259, which had been cut by a number of later pits. On the north it is cut by Pit L9260 (Phase 6 or later), on the east by Pit L9463 (Phase 4), and on the south by a Roman sump (L9257, Phase 2). While there is a chance that these could be intrusive finds, this seems unlikely, as the surrounding pits were of quite different matrices and keeping the debris separate was a fairly straightforward endeavour.

Seven other 'mass-produced' scarabs were found in Area G (Keel and Münger forthcoming): three in secure Phase 6b contexts, two from ill-defined 6b/6a contexts and two from Persian-period pits.

In all, five out of 12 'mass-produced' scarabs in Area G were found in a single phase in a single room in AI32. This concentration suggests that an original hoard

(perhaps a necklace?) was scattered in this room.¹² The possibility that these five scarabs are actually a single find is strengthened by the fact that few scarabs of this type are known from the extensive excavations in Area D2, which yielded large assemblages of the same chronological horizon as G/7 (about which, see below).

As noted above, the context of the ‘Siamun scarab’ itself is not optimal. The context of other scarabs in that same room, however, is secure (including one in the crack between two flagstones of Floor F9300). An archaeologist certainly has the right to question the provenience of a single scarab, since small-scale intrusions are always possible. Even two intrusive scarabs might be acceptable, but it seems beyond the bounds of credulity to suggest that five typologically-related scarabs, all found in the same phase, in the same room, could all be the results of contamination.

Relative and Absolute Early Iron Age Chronology at Dor

Tel Dor provides one of the most detailed early Iron Age sequences in the Southern Levant. Seven stratigraphic/ceramic horizons were defined within the period spanning the Iron Age I through the beginning of Iron Age II (Table 2). The particulars of this sequence have been detailed elsewhere (Gilboa 2001; Gilboa and Sharon 2001; 2003) and are summarized below briefly.

Within such a tight stratigraphy, changes from one ceramic horizon to the next are minor, and are often discerned only through quantitative analysis. The extant early Iron Age sequence may be schematically divided into two clusters. The early part (in our proposed terminology—*early* and *late* Ir1a) ends in a site-wide destruction. In Area G, Phases 10 and 9 belong to this horizon (Phase 9 ends with the destruction). This destruction, terminating the late Ir1a, is the only depositional event through which the early Iron Age sequences in the different areas can be correlated. Because the Ir1a horizons are earlier than those under consideration here they will not be referred to further. The date of the destruction, however, is important, as it provides the *terminus post quem* for the later Iron Age sequence. On conventional ceramic dating this event was suggested to have occurred *ca.* 1050 BCE (Stern 2000:98), or even somewhat earlier (Gilboa 1998:413). However, radiocarbon dates from late Ir1a destruction deposits in Areas G and B1 have thus far proven significantly later—*ca.* 980/970 BCE (Gilboa and Sharon 2001; Sharon 2001; see Table 2).

From the major late Ir1a destruction and on, correlation between the Iron Age sequences in the different areas is based solely on typological (ceramic) considerations. For this later period the benchmark is Area D2, on the southern fringes of the tell. Unlike Area G, D2 experienced significant constructional shifts

¹² For a close corollary of such a necklace including mass-produced scarabs, see Tell es-Safadiyeh Tomb 65 (Tubb 1988: 71:Fig. 51).

marking the transitions from one horizon to the next. D2 also yielded extensive primary artefactual assemblages, including an abundance of decorated wares, in each horizon. Also, this area produced nearly all the ¹⁴C dates currently available for Dor for the later part of the early Iron Age sequence. As shown in Table 2, at least four stratigraphic/ceramic horizons were defined within the post destruction sequence in Area D2 (local Phases 8b, 8c, 9–11 and probably also 12). The basis for correlating these horizons to early Iron Age sequences in other parts of the country has been detailed in Gilboa 2001, and Gilboa and Sharon 2003; it will not be repeated here. One issue, however, bears repetition:

The Ir1b horizon (Phases 9–10 and possibly 11 in D2) is correlated to the end of the early Iron Age elsewhere—the ‘Megiddo VIA horizon’—*inter alia* via its decorated wares: This horizon marks the beginning of Phoenician Bichrome ware and of the occurrence of Cypro-Geometric I vessels. At the end of our spectrum, the ‘Ir2a’ horizon (Phase 8b in D2) is correlated with the beginning of Iron Age II in other parts of the country (‘the Megiddo VA–IVB horizon’). In Area D2 these two horizons are separated by a (stratigraphically- and typologically-distinct) horizon, which we have termed Ir1|2. As implied by this appellation, this is typologically an intermediate horizon, but it is closer to Ir2a. One may ask if it is necessary to fabricate a new horizon and not simply cluster the two Phase 8 floors into one Ir2a horizon? That the Ir1|2 cannot be brushed off as a totally insignificant period of time is proven by its Cypriot corollaries. While the Cypriot imports in ‘Ir1b’ assemblages are exclusively Cypro-Geometric Ia (or mid-Cypro-Geometric I) in date, those of the ‘real’ Ir2a (as well as all other Iron Age IIA assemblages which have Cypriot imports in this country) are already Cypro-Geometric III. I.e., the entire span of Cypro-Geometric Ib–II (however long *that* is) must fit into the Ir1|2; indeed, imports of this latter horizon are present in Phase 8c in D2 (and in other excavation areas). The need to define such a transitional horizon is further borne out by early Iron Age sequences at other sites. At Megiddo, for instance, this is the period between the destruction of Stratum VIA and the formation of the totally different VA–IVB ceramic assemblage. At nearby Yokne'am it is the period between Strata XVII (Ir1b) and XIV (Ir2a).¹³ As was the case with the Ir1a destruction, ¹⁴C dates for the entire D2 range proved to be *ca.* 75–100 years later than dates assumed on conventional ceramic chronology (see Table 2):

¹³ Contexts at other sites which should be attributed to our Ir1|2 horizon are discussed in Gilboa and Sharon 2003. In the Negev sites, too, it has recently been recognized that a distinct chronological phase should be defined between the last early Iron Age contexts, defined *inter alia* by the first import of Phoenician Bichrome and related wares and the ‘true’ Iron Age II horizon, recognized *inter alia* by the occurrence of Black-on-Red (i.e., CG III) ware. This transitional horizon (Herzog and Singer-Avitz forthcoming) is suggested to encompass such contexts as Arad XII, Beersheva VII and Tel Masos II, and was termed ‘early Iron IIA’. Whether this horizon indeed equals chronologically our Iron 1|2 remains to be investigated.

The ‘Megiddo VIA horizon’ (our Ir1b) is usually dated 1050–980 BCE (conventionally ending in many sites with assumed Davidic conquests, somewhere within the 1000–980 BCE range). The beginning of this horizon cannot yet be defined radiometrically, as we do not possess dates for the immediately preceding one (Ir1a|b). Allowing a few decades for the latter after the radiometric *terminus post quem* ca. 980/970 BCE, provided by the end of Ir1a, an initial date *ca.* 950 would be probable. The end of this horizon is placed by ¹⁴C dates *ca.* 880 BCE.¹⁴

This Ir1|2 horizon, on conventional chronology, could be dubbed a ‘Davidic’ one (i.e., beginning 1000/980 and ending *ca.* 950), but is dated radiometrically in Area D2 *ca.* 880–850 BCE. Indeed, as implied by the pottery, this seems to be a relatively short period. Finally, the horizon termed Ir2a, paralleling Iron Age IIA in other parts of the country, the ‘Solomonic horizon’, starts at Dor, on ¹⁴C dating, around the mid-9th century BCE.

Dating the Contexts of the Scarabs

It must be clear at the outset that there is no reason to assume a naïve one-to-one correlation between the stratigraphical phases of Area D2, which define the late Iron Age I sequence at Dor, and those of Area G. Constructional shifts need not—and probably did not—necessarily coincide. The situation in Area G is much more complex than in D2 because, as stated above, the entire Ir1–Ir2a sequence (Phases 10–6) is represented by the continuous use of a single building, repeatedly repaired and slowly altered with gradual raisings of floors. Ceramic assemblages encapsulated between the Phase 7 successive floors are small and seldom can be assumed to be in primary deposition. Moreover, within Phase 7, correlation between the sequences in different rooms within the building is difficult to establish: There is no reason to assume, for instance, that floors in a certain room were raised at the same time as in the room next door.

When comparing Phase G/7 in G to the D2 ceramic sequence, it is clear that the former could not have started later than Ir1b. Figs. 5, 6 present all (give or take) reproducible diagnostic pottery found on the floors of Phases 7d–7b in the ‘scarab room’ in question (Loci 18013, 9930, 9879, 9878, 9824, 9313). As is evident, the material is scant. It is dominated by the simple carinated bowls

¹⁴ A recently-published article (Bruins, van-der-Plicht and Mazar 2003), unveiling new radiocarbon dates from Tel Reḥov, attempted to reassert the ‘conventional chronology’ while nudging it upwards a bit (whereas Mazar previously dated the Iron Age IIA to 1000–900 BCE, e.g., 1990: Table 7, he now votes for 980–830 BCE). In a detailed critique (Sharon, Gilboa and Boaretto 2003; Sharon, Boaretto, Gilboa and Jull forthcoming; see also Finkelstein and Piasetzky 2003a) we argue that this dating is overly-high, even according to the newly published data alone. However, we concede that the Tel Dor data *might* indicate a too-low date. An average date *ca.* 900 for the Iron Age I | II transition might be reasonable.

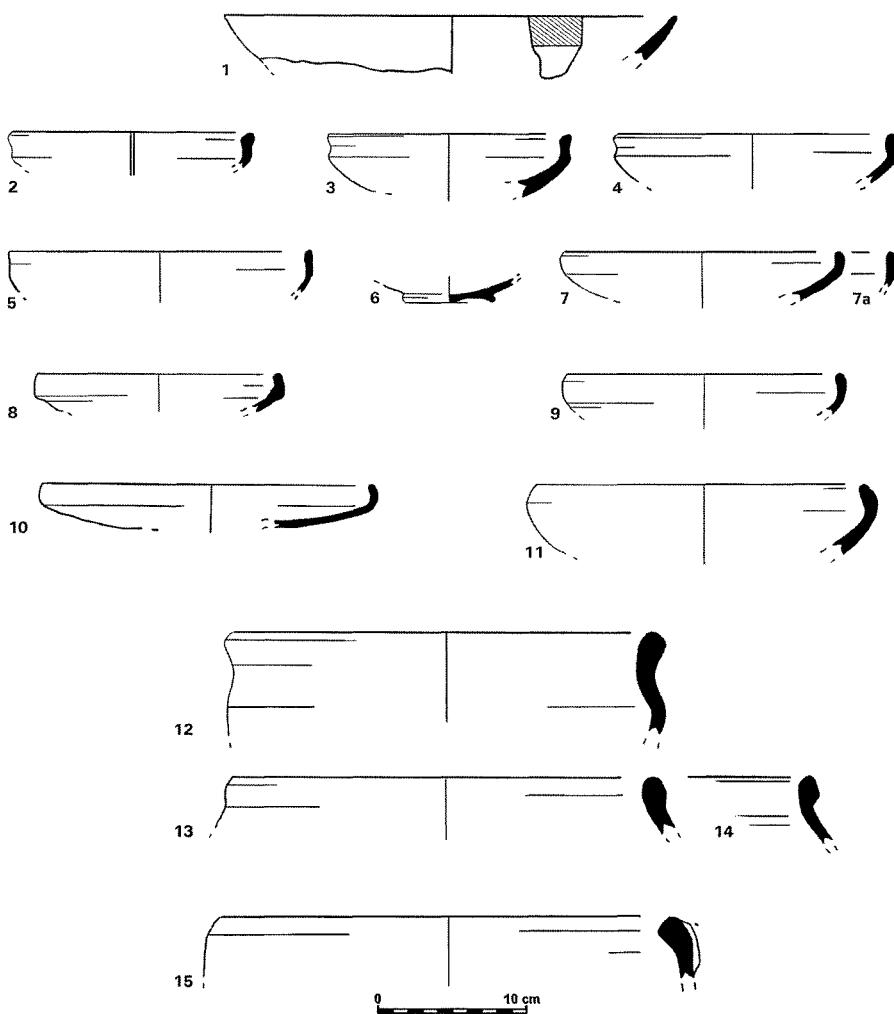


Fig. 5. Pottery of 'scarab room', Phase 7d–b.

(BL 33,¹⁵ Fig. 5:7–10), which are abundant at Dor from Ir1b through Ir2a and thus are of no value for our purpose, and hardly includes any decorated fragments. The chronologically-significant indices are the concurrence of bowls with molded rims (BL 23 and 24, Fig. 5:2, 3 and 4, 5 respectively), alongside the above-mentioned dominant simple carinated bowls, the cooking-pots with the elongated elegantly-shaped rims (CP 7, Fig. 6:1–3), the short thick rim of the commercial jar (JR 8, Fig. 6:6), a fragment of a straight-shouldered jar with concentric red bands on its

¹⁵ Type nos. follow Gilboa 2001; see also Gilboa and Sharon 2003.

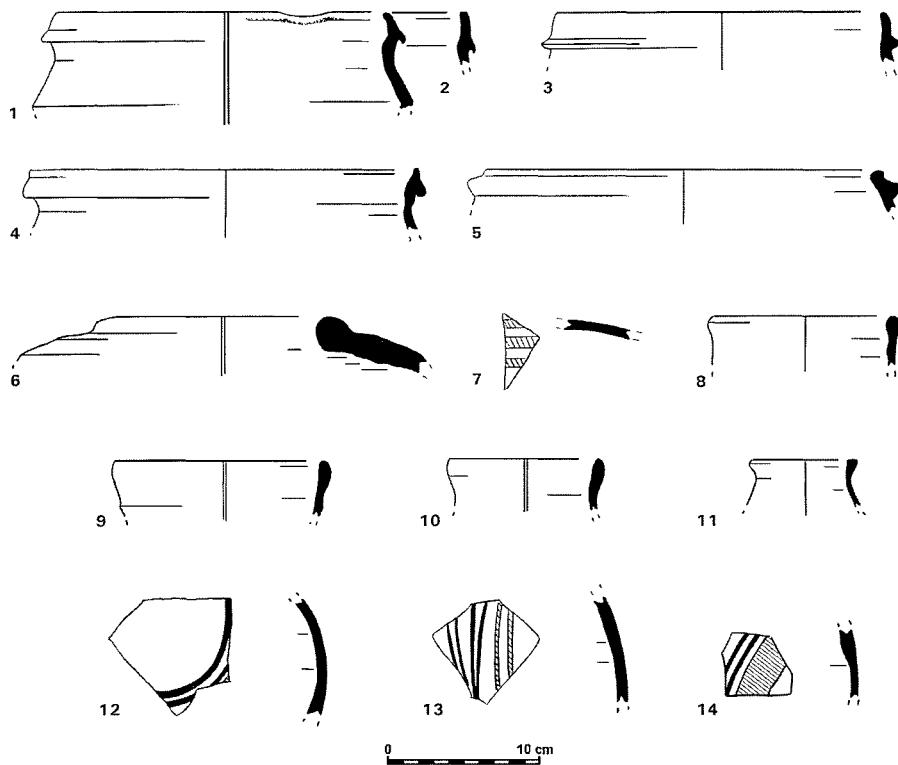


Fig. 6. Pottery of 'scarab room', Phase 7d-b (cont.).

shoulders (SJ 5, Fig. 6:7), monochrome, two-colored and Bichrome containers (Fig. 6:12–14, see Gilboa 1999; Gilboa and Sharon 2003: Fig. 9:7–14; Sharon and Gilboa forthcoming for precise definitions of these terms at Dor) with concentric decoration, and an Egyptian jar fragment (Fig. 6:8). As an ensemble all the above can be placed only in Ir1b, though for some individual pieces parallels may be found also in earlier and later horizons. Pottery from other rooms of the Area G/7 building is not presented here, but an Ir1b attribution for the early contexts of Phase G/7 is supported by the ceramic profile in these other units as well.

However, as the stratigraphically earliest 'mass-produced' scarabs at Dor, including that attributed to Siamun, were found on the uppermost floors of Phase

7 (7a), it is the *end* of this phase that is of particular interest here. Again, we consider here only the pottery found in conjunction with the scarabs, in the same room (in Loci 9814, 9554 and the lower baskets of Locus 9259, Figs. 7–9). This assemblage is more difficult to pinpoint. On the one hand it is very similar to that of the lower floors of Phase 7 (including an Egyptian jar, Fig. 9:1); on the other hand, however, there are already some later features. One simple carinated bowl (Fig. 7:16) bears traces of red wash—a feature usually not encountered before Ir1|2 (and which is rare also then). There is one later cooking-pot, of a type which becomes prominent only in Ir1|2 (CP 20, Fig. 8:6). Of the few vessels found here in primary deposition, the only significant piece is the Bichrome jug (Fig. 9:11). Its horizontal decoration and ring base are features clearly attested in D2 only from Ir1|2 (and on). Though they are by-and-large late (i.e., Ir2) attributes, at least one Bichrome jug with horizontal decoration and a ring base (albeit coarser and of different shape) is attested at Megiddo K-4 (=Stratum VIA) (I. Finkelstein and E. Arieh, personal communication). Morphologically, however, this delicate squat vessel with its long funnel neck is unique at Dor, and differs from the coarser globular Bichrome vessels, usually with shorter necks, which are typical of Ir1|2 (and Ir2a) in Area G and in other areas.

Most decisive, however, for placing Phase 7a in the early Iron Age range are the cooking-pots: On the one hand there are CP 7 cooking-pots (Fig. 8:1–4); judging by the extensive early Iron Age primary assemblages at Dor, these become practically extinct after Ir1b. In conjunction with this, the absence of the cooking-pot types which typify Ir1|2 and Ir2a is notable (CP 21 and 22, see Gilboa and Sharon 2003).

Summary of the Dor Scarabs and the Ceramic Sequence

In the final analysis it seems very likely that most of Phase G/7 belongs to the Dor Ir1b horizon. Its upper floors (Phase 7a, with the scarabs) belong to the very end of this period.

To pinpoint this position in familiar terms, this would be around the very end of Megiddo VIA, or possibly somewhat later, in a postulated Davidic horizon. On the Dor ¹⁴C chronology, however, this would be around 880 BCE.

As claimed by Münger, at other Palestinian sites (see list in Münger 2003) such ‘mass-produced’ scarabs definitely start occurring late in Iron Age I. The earliest contexts which can be dated with relatively high precision to this period (our Ir1b) are Tombs 506 and 222 at Tell el Far‘ah (S) (for Tomb 222 a somewhat earlier initial date cannot be ruled out), Tell Keisan 9a, Tell Qasile X, Megiddo

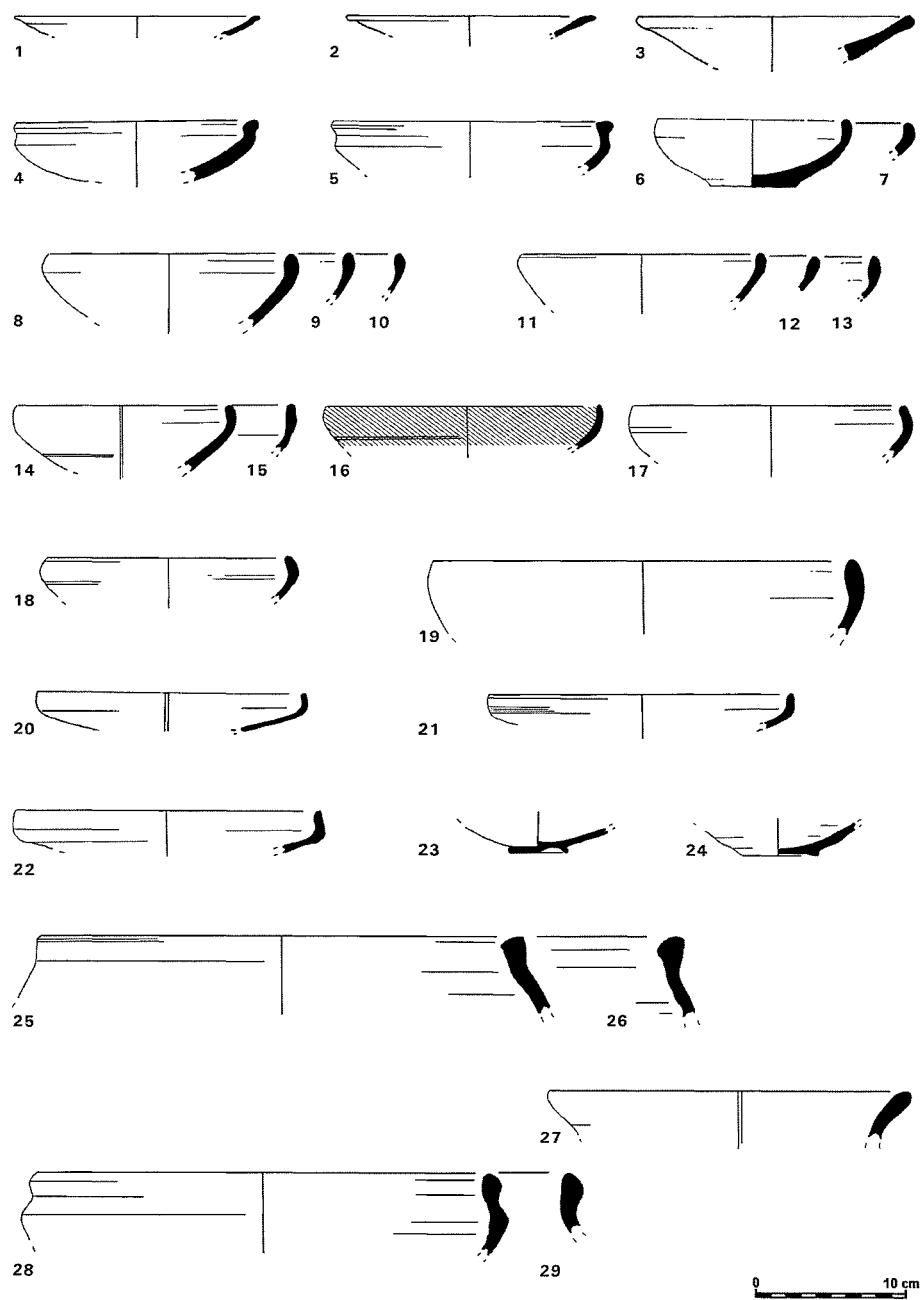


Fig. 7. Pottery of 'scarab room', Phase 7a.

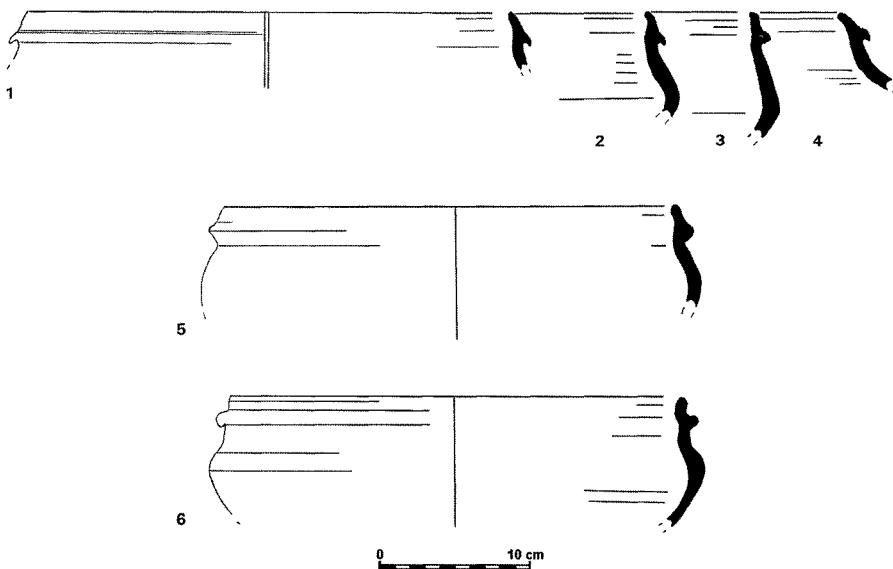


Fig. 8. Pottery of ‘scarab room’, Phase 7a (cont.).

VIA¹⁶ and probably Tell es-Sa‘idiyeh Tomb 118.¹⁷ Others (mass produced scarabs and probable imitations thereof), as at Dor, occur in contexts that are slightly later—our ‘Ir1|2’—such as (probably) Tell Abu Hawam IVB, Tyre X¹⁸ and Sarepta YII/Locus 26 (for the correlation of these contexts with Dor, see Gilboa and Sharon 2003) and still others (see Münger 2003) originate from Iron II contexts or contexts which cannot be dated with any meaningful resolution.¹⁹ A few had made their way to Cyprus, where they appear in similarly-dated contexts (e.g., Salamis Tomb I).²⁰

The situation is much less clear when the scarabs attributed to Siamun are

¹⁶ A ‘mass produced’ scarab was found in the famed VIA hoard, which was found *just under* the VIA floor. Assuming it was indeed a buried cache (and does not belong to an earlier stratum) it could date anywhere within the span of Stratum VIA, though interment just prior to the final demise of Megiddo VIA would perhaps be the most likely scenario (Anabel Zarzecki-Peleg, personal communication).

¹⁷ The date of some of the Tell es-Sa‘idiyeh items may become clearer with the full publication of their contexts.

¹⁸ Bikai 1978: Pl. XXVI: 14; Münger confirms that this blue frit conoid belongs with his ‘imitation’ group (personal communication).

¹⁹ For a discussion of Arad XII, Beersheva VII and Tel Masos II see Herzog and Singer-Avitz forthcoming; see note 16.

²⁰ Reyes 2001:25. Salamis Tomb I spans the entire CG I (that is our ‘Ir1b’ as well as ‘Ir1|2’). The conoid seal is regarded by Münger as a ‘copy of a copy’, as it is reportedly made of limestone and not frit (personal communication).

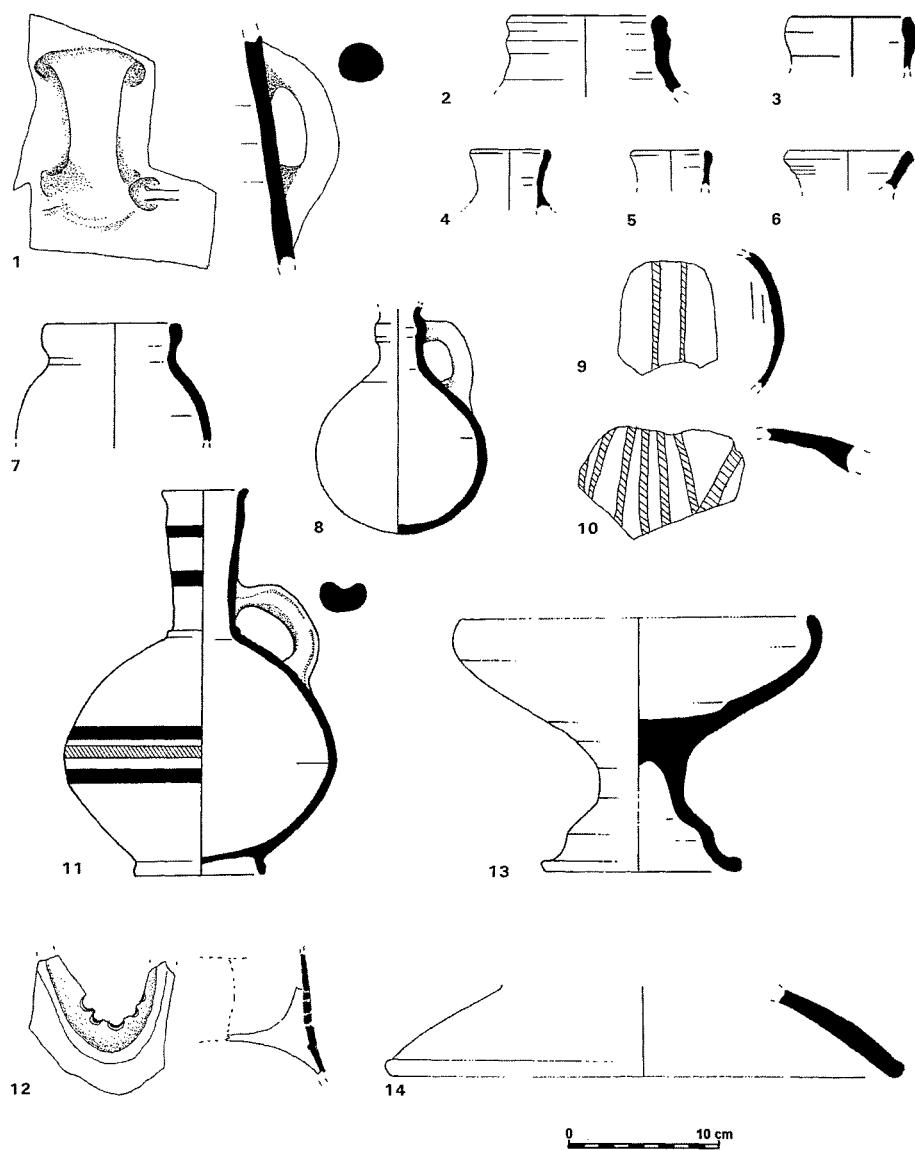


Fig. 9. Pottery of 'scarab room', Phase 7a (cont.).

considered on their own. The one from Megiddo (Loud 1948: Pls. 149:1, 154:1; Münger 2003: Fig. 4:1) was found in a Middle Bronze Age tomb (which did not contain any Iron Age pottery). The Tell el Far'ah (S) scarab (see Münger 2003; Petrie 1930: Pl. 29:259) originates in an unclear context in Cemetery 200. The piece in the Dayan collection (Giveon 1972: n. 4) has no context at all.

Siamun and Chronology

Neterkheperre Setepenamun, Siamun, Beloved of Amun was the sixth king of the 21st Dynasty,²¹ reigning from Tanis, currently dated *ca.* 980 to *ca.* 960 BCE (see below; Kitchen 1986:6–13). In working out the chronology of the 21st Dynasty, Kitchen (1986:72–76) could not proceed from the end of the New Kingdom in a straightforward fashion. There is far too much confusion regarding the lengths, and even the number and order of reigns in the late-20th and 21st Dynasties.²² Rather, he takes Shoshenq I's campaign against Israel (925 according to him) as his benchmark.²³ Calculating back regnal

²¹ Not even this bare fact can be unequivocally established, though this is the general consensus. Siamun does not appear (under that name) in Manetho's lists. The fifth and sixth kings of the 21st Dynasty there are Oschor and Psinaches—neither etymology being related to any names known from monuments (Kitchen 1986:7). Montet (1959: 39), for instance, who brought to light many of the Siamun-related monuments in Tanis, thought Siamun was the *fifth* king of the 21st Dynasty, and hence dated him somewhat earlier.

²² For example, it is unclear if Amenemope reigned for 9 or 49 years (Kitchen 1986:24), Psusennes I may have reigned anywhere from 21 to 49 years (*ibid.*: 26, 29), and it is even unclear if there were three, or only two, Psusenneses (*ibid.*:11); see also, among many others, Černý 1993:645–647.

²³ The correlation between the biblical testimony of Shishak's invasion of Judah in Year 5 of Rehoboam (1 Kgs. 14:25–25; 2 Chr. 12:1–9), and Shoshenq I's Palestinian campaign, commemorated at Karnak, has long been one of the fundamental cornerstones for Egyptian chronology in the Third Intermediate Period (henceforward: TIP). Dates offered for the campaign are mostly in the in the 933–920 BCE range; based on different extrapolations of the biblical chronology and on which regnal year of Sheshonq I the (undated) campaign should be assigned to (summary and references in Handy 1997: 100–101, notes 13–20; see also Green 1978:355). It has been pointed out, however, that the evidence for this synchronism is equivocal: There are significant discrepancies between Shoshenq's inscription and the biblical description of Shishak's invasion. Wilson (2001) argues that Sheshonq's account is largely formulaic. Niemann (1997: 297) suggests that the list may be an amalgamation of several incursions. The suggestion to assign the Shoshenq campaign to the very end of his reign is based on the proposed association between the execution of this relief with his Year 21 Gebel Silsileh quarry stele, commanding stone quarrying there for renovations at Karnak, just prior to the pharaoh's death (according to the 21 years assigned to him by Manetho). These assumptions have, however, been challenged—see, for example, Redford 1973:10; 1992:315 and Wente (1976:277–278).

years²⁴ he arrives at *ca.* 978–959 BCE (alternatively 975–956) for Siamun's reign (Kitchen 2000: Table 2). Disregarding the Shoshenq-Rehoboam correlation, Kitchen (e.g., 1997:111) would allow for slightly lower dates for Shoshenq's accession (939/936 instead of 945/942).²⁵ Wente (1976:276) calculates a slightly wider possible range for this event—948–929.²⁶ These various suggestions would, of course, similarly affect the dates of Shoshenq's immediate predecessors.²⁷

The sequence and chronology established by Kitchen for the TIP, though not unequivocal, is generally accepted.²⁸ Recent attempts to deconstruct completely the TIP chronological framework (James *et al.* 1992: Chapter 10) have met with little support. These attempts per force involve a complete disassociation between Shoshenq I and biblical Shishak.²⁹

Other ‘historical facts’ about Siamun, e.g., that he, too, campaigned in Palestine,

²⁴ Subtracting 17–19 years for Psusennes II and assigning 17–19 years for Siamun himself. A ‘Year 17’ is attested for Siamun on the monuments, as against 9 (19?) by Manetho for Psinaches. The length of Psusennes II's reign is hard to assess: see, e.g., Redford 1973:5, n. 5; Lance 1976:211–212; Green 1978:360–361; and further references in these works. (There is general agreement to prefer the 14 years assigned to him by Julius Africanus' version of Manetho than the 35 years cited by Eusebius.) For a suggestion that Psusennes II never actually reigned independently in Tanis, that Shoshenq I was Siamun's direct successor, see Dodson 1987.

²⁵ Kitchen has often reiterated (e.g., 1996; 1997:11; 2000:39–41) that his choice of departure from the Shishak/Shoshenq I synchronism was one of convenience only, and that Egyptian chronology of the 21st to the 26th Dynasties can be constructed exclusively on the basis of contemporary inscriptional evidence. However, the correlation between Shoshenq I's campaign and Shishak's invasion in Year 5 of Rehoboam seems, at least implicitly, to remain a major peg in refining the Egyptian part of the equation (e.g., Kitchen 2000:41 writes: “earlier [than 954–924 BCE] this latter king [Shoshenq I] cannot be, because of the synchronism with the 5th year of Rehoboam of Judah in 925 B.C.”).

²⁶ Taking into account that Shoshenq's campaign did not necessarily occur late in his reign. Wente (1976:276) would allow it to be as early as the king's eighth year (taking, again, the biblical evidence into consideration). For further dates offered for Shoshenq's accession see references in Green 1978: n. 22 (the highest one being 950 BCE).

²⁷ However, most suggestions to alter Kitchen's sequence for the 22nd Dynasty (e.g., Aston 1989; Dodson 1993) seem to have been careful to retain the Shoshenq–Shishak correlation intact.

²⁸ E.g., Wente 1976; von Beckerath 1997:99–102. Regarding the limitations of the various bases for the current Egyptian chronology, see, e.g., Ward 1992; but even he, though by and large deconstructing the notion that an accurate Egyptian chronology is possible in our present state of knowledge, considers the Shoshenq–Rehoboam synchronization an unshakable peg.

²⁹ The ‘Centuries of Darkness’ chronology accepts on the one hand both canonical biblical chronology for Shishak and the biblical concept of a grand Solomonic monarchy, and on the other transplants Egyptian Shoshenq I in the 9th century (e.g., James *et al.* 1992:197–203; 229–231; Rohl 1999; and see below), but these suggestions have also garnered only minimal acceptance (summaries, for example, in Kitchen 1991:236; Jansen-Winkel 1999).

that he was the pharaoh who gave his daughter in marriage to Solomon, and included the town of Gezer, in the lady's dowry (1 Kgs. 3:1; 7:8; 9:16, 24; 11:1 and 2 Chr. 8:11) should no longer be taken at face value.³⁰ There is no independent Egyptian corroboration that these events ever took place. Rather, the biblical story is used to conjure a dubious interpretation of ambiguous Egyptian finds. Nevertheless the *fact* of a campaign by Siamun to Palestine (or at least to Philistia) went unquestioned—to the point of attributing to it destruction layers (e.g., Gezer IX—Dever 1993:504, and Ashdod X—Dothan 1971:20).

The following paragraphs explore the chronological ramification of the Dor scarab, assuming that its attribution to Siamun is accepted.

Implications for the ‘Conventional Chronology’

Could a Siamun scarab in a Palestinian context paralleling the end of Megiddo VIA or slightly later be squared away with the conventional wisdom that Megiddo VIA was destroyed by David and that VA–IVB was built by Solomon and destroyed by Shoshenq I? Yes—but barely.

Assuming one accepts both Egyptian and Levantine conventional chronologies,³¹ and the Dor ‘Siamun’ scarab and its context, one can still argue that all it proves is that the Iron Age I ends somewhat after 980 BCE. Assuming that the scarab arrived at Dor immediately after Siamun’s assumption of the throne in Egypt, and that the end of the Ir1b at that site followed immediately afterwards, one might still maintain that other ‘late Iron Age I’ assemblages such as Tell Qasile X and Megiddo VIA are Davidic conquests.³²

The problem lies with the chronological gap that still exists between Dor

³⁰ These are based (apart from the biblical mention) on a broken piece of relief found in Tanis (Montet 1959:42) showing the conventional scene of ‘Pharaoh striking his enemies’, which may or may not be attributed to Siamun, and which may or may not be depicting some ‘Aegeans’ in the role of ‘vanquished enemies’, all of which may or may not indicate an actual campaign against ‘Aegeans’. See summaries, critiques and references in Lance 1976; Green 1978:363–365; Kitchen 1986:280–283; Redford 1992:311.

³¹ For the purpose of this section we do not entertain the possibility of shifting the Egyptian chronology. The Shishak=Shoshenq I yr. 21=Rehoboam yr. 5=925 BCE synchronism is a cornerstone of the ‘conventional’ hypothesis that the accuracy of the ‘Deuteronomist’s historical memory of the 10th century can be relied on. It is the only demonstrable ‘convergence’ between the ‘Deuteronomistic history’ and outside sources for this early period (e.g., Dever 2001:131–138). Raising the dates for Siamun (for some such possibilities, see above) would, on the one hand, render its context at Dor more compatible with the conventional chronology, but on the other hand break that convergence.

³² That the earliest part of the United Monarchy, i.e., Saul’s and (part of) David’s reign, should be relegated to the Iron Age I is a contention already accepted by most archaeologists and historians—even adherents of the ‘high’ chronology.

G/7—the ‘Ir1b’ typological horizon—and the ‘classic’ ‘Ir2a’ horizons—the typological horizon which we have labeled Ir1|2—represented in Dor Area G by Phase 6b. If it is further assumed that the assemblages we attribute to Solomonic constructions actually all date to the end of these strata—say to Shoshenq’s campaign of 925—this leaves a period of some 50 years to accommodate the Ir1|2 horizon.³³

Note, however, that accepting the Dor ‘Siamun scarab’ and its context, even under such a scenario, pushes the ‘maximalist’ interpretation to its limit. It relegates the bulk of the ‘Davidic expansion’ strata into the last decade of his reign—a rather different picture than the biblical depiction. The ‘Davidic’ Megiddo VB becomes Solomonic—as a matter of fact, it is the only truly ‘Solomonic’ assemblage under this scenario. While the buildings of Megiddo VA–IVB might still have been built in the (latter part of) Solomon’s reign, its artefactual assemblage is actually post-Solomonic.

Note also that pushing Siamun to the late Iron Age I while retaining the attribution for destruction of the main Iron Age IIA strata (e.g., Megiddo VA–IVB, Gezer VIII) to Shishak=Shoshenq I (cf., for example Mazar 1990: Table 7; Dever 2001:135) would leave but a few decades for the actual Iron Age IIA. A secondary implication of the scarab would therefore be that the end of the Iron Age IIA must certainly be pushed well into the 9th century.³⁴

It seems that if the scarab is indeed to be associated with Siamun, its occurrence in its particular findspot, while not tearing the web of ‘conventional’ chronology, is another factor nudging it towards the ‘low’ one. The set of strata usually dated ‘late 11th century’ must minimally be stretched to cover the first half of the 10th while the ‘10th century’ assemblages are minimally late 10th and must certainly continue well into the 9th.

Implications for the ‘Low Chronology’

A more moderate scenario should allow that the ‘Siamun scarab’ was not necessarily produced on the very first day of his reign and that it took some time for it to reach Dor. More time should be allocated for the use of that scarab-necklace and its deposition, and a further generation or two for the span of the Ir1|2 (or at the very least—if one wishes to push the end of G/7 *into* the Ir1|2—a decade or two for the construction and use of G/6b). This would imply that the beginning

³³ Amihai Mazar, for one, seems almost ready to take that course in some of his latest works (e.g., Coldstream and Mazar 2003:43–44) in allowing for the possibility that Shishak should be correlated with Stratum VI (rather than V, or IV) at Tel Reḥov, and that the Solomonic stratum at Megiddo may be VB, rather than VA–IVB (Bruins, van der Plicht and Mazar 2003).

³⁴ A conclusion that many ‘conventionalists’ are by now willing to embrace on other grounds too (Bruins, van der Plicht and Mazar 2003).

of ('real') Iron Age IIA should be placed somewhere in the *second* half (and more comfortably the last third) of the 10th century BCE. This would disassociate the end of Megiddo VIA and the like from David. It also means that Iron Age IIA 'classic Solomonic' assemblages like that of Megiddo VA–IVB should perhaps then be termed 'Jeroboamite' or 'Rehoboamite' as the case may be.³⁵

Implications for the 'Super Low' ^{14}C Chronology

It is also difficult to reconcile this scarab with the ^{14}C chronology established to date for Dor. On this chronology, ending Ir1a (Phase 9 in G) *ca.* 980/970 BCE, and Ir1b (Phase 7 in G) *ca.* 880, this scarab would be in place in Phase 8 or in early Phase 7. A claim that the scarab is re-deposited is *not* appropriate here, for the same reasons that intrusion was ruled out above. On the other hand, on the assumption that all five scarabs were part of a single necklace, lost toward the end of Phase 7, it is not inconceivable that such a necklace could be an heirloom. No 'mass produced' scarabs were found in contexts earlier than 7a in Area G. In Area D2, however, there is at least one such from a context which is apparently somewhat earlier (though still well within the Ir1b typological horizon).³⁶ Thus, as a group, 'mass produced' scarabs *might* have made their first appearance earlier than the very end of Ir1b, though how much earlier is yet unknown.³⁷

Implications for the 'Hyper Low' 'Centuries of Darkness' Hypothesis

Taking both the proposed name on the scarab and the Dor radiometric dates at face value—a Siamun scarab in an early 9th century context—would certainly satisfy those who assert that the TIP framework must be completely altered and the beginning of the 22nd Dynasty placed in the 9th century.³⁸ Note, however, that the 'Centuries of Darkness' chronology places the beginning of the 22nd Dynasty around 810 BCE (James *et al.* 1992:256). Even according to the super low radiometric dates from Dor, the 'Siamun context' dates *ca.* 880 BCE at the very latest.

Indeed, though some of the misgivings expressed by James *et al.* regarding the

³⁵ A clear differentiation must be kept between ceramic assemblages and the date of construction of their associated architectural contexts. In these cases, the latter could still be claimed, as is conventionally done, to have been constructed by Solomon (albeit towards the end of his reign).

³⁶ Scarab 190879 from L19029 in Area D2 is 'mass-produced' (Stephan Münger and Daphna Ben-Tor, personal communication). L19029 belongs with one of the lower phases of the 'mudbrick building', probably Phase 9b. I.e., it precedes the end of Ir1b at least somewhat.

³⁷ Note also that (again, taking into account the liminal typological context of the scarab—the very end of Ir1b) any slight lowering of Siamun's Egyptian chronology would shift the balance somewhat further towards the low chronology.

³⁸ E.g., James *et al.* 1992:251, Table 10:4; Rohl 1999:9; likewise, for example, Wallenfels 1983: 88–89.

TIP should be heeded, their general extremely low chronological framework cannot be accepted. Certainly their suggestion to end the Late Bronze Age in the mid-9th century (James *et al.* 1992: Table 8:3), is refuted, *inter alia*, by all currently-available ^{14}C dates, from Israel as well as Cyprus.³⁹ Likewise, the suggestion to place Iron Age IIA in the 8th century (*ibid.*) is too low according to all ^{14}C determinations from Israel.⁴⁰ The net result of combining the radiometric dates for the end of the Bronze Age with those of the Iron Age I/II transition, however, is that the ‘Dark Ages’ (Iron Age I in the Palestinian context) should be lengthened rather than shortened, and thus the main thrust of the ‘centuries of darkness’ proposition is refuted.

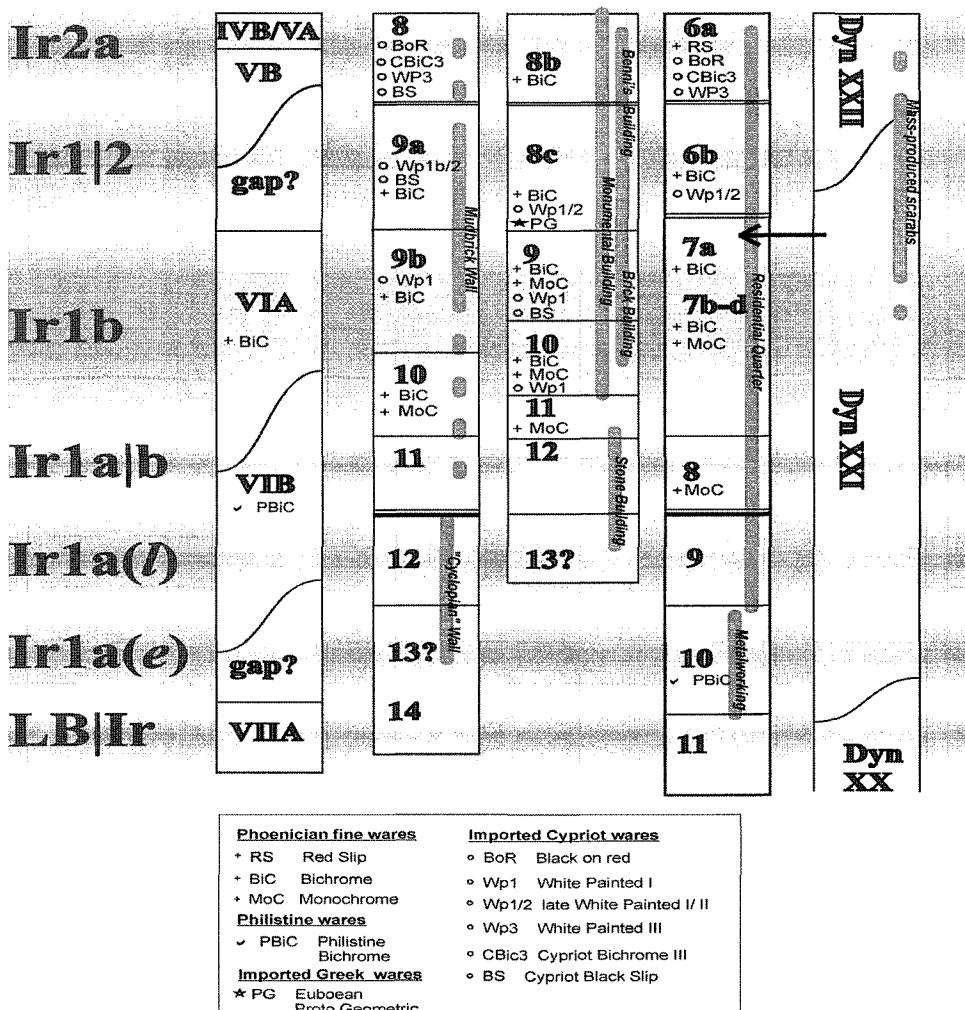
Summing Up

The main lesson here is the frailty of the entire web of ‘Old World Chronology’ for the end of the Bronze and beginning of the Iron Ages. Spun out of snippets of contemporary information; inlaid in a mesh of later-written (hi)stories of varying credibility and agenda, it is synchronized by a few tenuous threads dependant on the vagaries of luck and exigencies of excavation; and pegged to an absolute timescale by some ancient astronomical observations, the understanding of which is in dispute. No effort can be spared in the attempt to milk the few actual dated objects we *might* find and which might be dated independently for what they are worth. But turning over more and more stones in the hope that the next uncontested epigraphic find will be made by perfect excavators in a flawless context does *not* constitute a robust research strategy to ameliorate this situation. For all its obvious problematics, there is no alternative to a systematic program of radiometric and/or dendrochronological dating, where consistent sampling strategies might be applied and where the uncertainties, while considerable, can at least be assessed.

³⁹ For references to hitherto published dates from Israel, see Bruins, van der Plicht and Mazar 2003; Gilboa and Sharon 2001; 2003; Finkelstein and Piasezky 2003b. Radiometric dates from Cyprus place the LC IIC/III transition, which should be roughly equivalent to the LB/Iron Age transition in Palestine, *ca.* 1200–1160 BCE (Manning *et al.* 2001; see also Kuniholm *et al.* 1996:782).

⁴⁰ However, in another version of this table (James *et al.* 1991; Table 2), a slightly higher date for Iron Age IIA is offered (*ca.* 850 instead of 800). This latter date is compatible with ^{14}C determinations for the Iron Age II horizon at Dor (see Table 2).

TABLE 2. EARLY IRON AGE STRATIGRAPHICAL/CERAMIC SEQUENCE AT DOR, INDICATING THE POSITION OF THE 'SIAMUN SCARAB'



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