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THE POTTERY OF EDMOM: A CORRECTION*

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Summary: The Pottery of Edom: A Correction

This article deals with several claims recently made by Levy *et al.*¹ regarding pottery from Edom and sites in the Negev. Building their argument on two assumptions—that Khirbet en-Nahas constitutes part of Edom and that the fortress there dates to the 10th century BCE—they maintain that sites on the Edomite plateau had been dated to the late 7th–6th centuries BCE based on a single find—the seal impression carrying the inscription “Qos Gabr king of Edom”—and hint that this pottery should in fact be dated earlier. And based on the architectural similarity between the fortress at Khirbet en-Nahas and the fortresses of Tell el-Kheleifeh at the head of the Gulf of Aqaba and of En Hazeva in the western Arabah south of the Dead Sea, they date the latter two to the 10th century BCE, several centuries earlier than the broadly-accepted date in the Iron IIB/C. In this article we take issue with these claims. We show that dating the sites on the Edomite plateau to the late 8th-to-early 6th centuries BCE is backed by meticulous comparison to well-stratified and dated sites in southern Judah. We also show that the fortresses of Tell el-Kheleifeh and En Hazeva cannot be dated earlier than the late 8th century. We then deal with the reasons for Levy *et al.*'s errors.

Keywords: Edom – Khirbet en-Nahas – Tell el-Kheleifeh – En Hazeva

Resumen: La cerámica de Edom: una corrección

Este artículo se refiere a varias afirmaciones hechas recientemente por Levy *et al.*² respecto de la cerámica de Edom y sitios en el Negev quienes, forjando sus argumentos en base a dos supuestos—que Khirbet en-Nahas es parte constituyente de Edom y que

* Artículo recibido: 11 de Septiembre, 2008; aprobado: 10 de Octubre, 2008.

¹ Levy *et al.* 2007; partially also 2004; 2005.

² Levy *et al.* 2007; también parcialmente 2004; 2005.

su fortaleza data del siglo X a.C.— sostienen que los sitios en la meseta edomita fueron datados a finales del siglo VII y el VI a.C. en base a un único hallazgo—la impresión de sello con la inscripción “Qos Gabr rey de Edom”—e insinúan que esta cerámica debe ser, de hecho, datada más tempranamente. Y, basados en la similitud arquitectónica entre la fortaleza de Khirbet en-Nahas y las fortalezas de Tell el-Kheleifeh, en la entrada del Golfo de Aqaba, y de En Hazeva, en el Arabá occidental al sur del Mar Muerto, datan las dos últimas en el siglo X a.C., varios siglos antes que la fecha comúnmente aceptada en el Hierro IIB/C. En este artículo nos oponemos a estas afirmaciones. Demostramos que la datación de los sitios en la meseta edomita entre finales del siglo VIII a.C. y principios del VI a.C. está basada en una meticulosa comparación con sitios bien estratificados y datados de Judá meridional. También demostramos que las fortalezas de Tell el-Kheleifeh y En Hazeva no pueden ser datadas antes de finales del siglo VIII a.C. para luego tratar las razones de los errores de Levy *et al.*

Palabras clave: Edom – Khirbet en-Nahas – Tel el-Kheleifeh – En Hazeva

Levy *et al.*³ recently made several statements regarding pottery from Edom and sites in the Negev, which led them to suggest far-reaching changes in the conventional reconstruction of the historical processes that took place in the south in the Iron Age. In view of their excavations at Khirbet en-Nahas in the eastern Arabah, and especially their dating of a square fortress there to the 10th century BCE, they argue that:

A) State formation in Edom should be dated to the 10th–9th centuries BCE, a century or two earlier than the widely-accepted 8th century BCE date.⁴ Since Levy *et al.* are aware that an Edomite state must include the Edomite plateau, and as the pottery retrieved from Iron Age sites there had been dated to the late-Iron II, they challenge the conventional dating of this pottery. They claim that the dating of the Edomite pottery assemblages to the late 7th–6th centuries BCE had been based on a single find—the seal impression carrying the inscription “Qos Gabr king of Edom”—hinting that this pottery should, in fact, be dated earlier.

B) Because of the architectural similarity between the fortress at Khirbet en-Nahas and the fortresses of Tell el-Kheleifeh at the head of the Gulf of Aqaba⁵ and of En Hazeva in the western Arabah south of the Dead Sea,⁶ the pottery of the latter two should be dated to the 10th century BCE, several centuries earlier than the broadly-accepted date in the Iron IIB/C.

³ Levy *et al.* 2007; partially also 2004; 2005.

⁴ E.g., Bienkowski 1994.

⁵ Pratico 1993.

⁶ Cohen and Yisrael 1995a.

In this article we wish to take issue with these claims.

DATING OF THE EDMOMITE POTTERY

Levy *et al.* have stated that “*until quite recently, the Iron Age chronology of Edom rested on the discovery of a single clay seal impression found at the highland site of Umm el-Biyara The seal contains the name of Qos-Gabr ... known from the 7th century BC Assyrian annals*”.⁷ This seal, they say, “*served as the single chronological anchor for dating the Iron Age pottery of Edom*”,⁸ and for the conventional dating of the emergence of the Edomite kingdom.⁹ Elsewhere they have taken these statements one step further, arguing that “*the entire corpus of Iron Age pottery from the Edomite plateau represents a ‘floating chronology’ that is not fixed to a stratified archaeological sequence...*”.¹⁰

These statements are incorrect. The Qos-Gabr seal impression indeed led Bennett¹¹ to date the pottery of Umm el-Biyara to the 7th century BCE. However, this discovery was made and the ensuing conclusions proposed long ago, during the 1960s; Levy *et al.* ignore the rich pottery assemblages which have been unearthed, studied and published since then.¹²

Due to Edom’s location on the southern fringe of the Levant, and since sites in this area do not supply reliable stratigraphic sequences for the Iron Age, proper observations regarding dating can be done only by comparing material from the Edomite sites to that found in well-stratified sites in southern Judah, mainly in the Beer-sheba Valley. Indeed, there are many similarities between late-Iron II Judahite and Edomite vessel types such as open bowls (platters), carinated bowls, cooking pots, decanters and lamps.¹³

In addition, many Edomite vessels (decorated and undecorated) have been found at sites located west of the Jordan, in late-Iron II horizons dated between the late 8th and early 6th centuries BCE. Vessels belonging to this group have been found at Beer-sheba Valley sites such as Tel Beersheba Strata

⁷ Levy *et al.* 2007: 15–16.

⁸ Levy *et al.* 2007: 18.

⁹ Levy *et al.* 2007: 16.

¹⁰ Levy *et al.* 2004: 867.

¹¹ Bennett 1966: 399–403.

¹² E.g., Bienkowski, Oakeshott and Berlin 2002; Dornemann 1983; Hart 1989; 1995a; 1995b; Mazar 1985; Oakeshott 1978; 1983; Pratico 1993; Zeitler 1992.

¹³ See, for example, Bienkowski *et al.* 2002; Oakeshott 1978; Pratico 1993: 38–50. Some similarities can also be discerned among small finds, such as figurines (Bienkowski and Sedman 2001; Sedman 2002: 427).

III–II,¹⁴ Arad Strata X–VI,¹⁵ Tel ‘Ira Strata VII–VI,¹⁶ ‘Aroer Strata II–I,¹⁷ Tel Malhata,¹⁸ Horvat Qitmit,¹⁹ Tel Masos fortress,²⁰ Horvat ‘Uza²¹ and Horvat Radum.²² They have also been found in contemporary strata at sites in the area of Nahal Gerar and Nahal Besor—Tel Haror,²³ Tel Shera²⁴ and Tell Jemmeh,²⁵ and in Kadesh Barnea in northeastern Sinai.²⁶ We should reiterate that all these strata date between the late 8th and the early 6th centuries BCE. Equally important, not a single Edomite vessel has been found in Judah in earlier strata which are dated to the Iron IIA period, that is, to the late 10th and 9th centuries BCE.²⁷

Moreover, many vessel forms found in the Edomite plateau sites display a marked Assyrian influence.²⁸ Assyrian influence can also be observed in their architecture and art.²⁹ In Edom, as well as west of the Jordan, such influence does not exist before the beginning of direct Assyrian involvement in the region in the 730s.

Both the Assyrian records and archaeology show that the Edomite kingdom was consolidated during the later part of the 8th century BCE, following the Assyrian take-over of the region by Tiglath-pileser III.³⁰ It is apparent, therefore, that the Qos-Gabr seal impression is not the single chronological anchor for the absolute date of the Edomite settlements and Edomite state-formation. Ceramic studies, architectural observations and historical considerations all place the sites of the Edomite plateau not earlier than the late 8th century BCE. Not a single Iron IIA sherd has thus far been found on the Edomite plateau.

¹⁴ Singer-Avitz 1999: 33–39; 2004.

¹⁵ Singer-Avitz 2002: 160, 162.

¹⁶ Freud 1999: 227.

¹⁷ Biran and Cohen 1981: 265.

¹⁸ Kochavi 1993; Beit-Arieh 1998.

¹⁹ Freud and Beit-Arieh 1995.

²⁰ Zimhoni 1983: 129, Pl. 164: 7–10.

²¹ Freud 2007a.

²² Freud 2007b.

²³ Oren 1993a: 584.

²⁴ Oren 1993b: 1333.

²⁵ Van Beek 1983.

²⁶ Bernick-Greenberg 2007: 168–170.

²⁷ For the Iron IIA pottery horizon in Judah see Herzog and Singer-Avitz 2004.

²⁸ Bennett 1978; 1982: 187; Oakeshott 1978: 167–178; Dornemann 1983: 175; Pratico 1993: 41–43.

²⁹ Bennett 1978; 1982; Bienkowski 1995; Reich 1992: 219–220.

³⁰ Oakeshott 1978: 181; Pratico 1983: 194–195; Bartlett 1989; Bienkowski 1992a: 104; 1992b.

TELL EL-KHELEIFEH AND EN HAZEVA

Radiocarbon results from the copper production site of Khirbet en-Nahas cover a period of ca. 300 years, from the late 12th to the late 9th centuries BCE—the Iron I and the Iron IIA.³¹ Levy *et al.* date the square fortress, which is well-preserved on the surface of the site, according to ¹⁴C results from samples ostensibly related to the structure, to the 10th century BCE. The Khirbet en-Nahas fortress closely resembles the fortresses uncovered at Tell el-Kheleifeh³² and En Hazeva.³³ They therefore reject the conventional late 8th-to-early 6th century BCE dating of these fortresses³⁴ and suggest dating them to the 10th–9th centuries BCE.³⁵

The pottery of Tell el-Kheleifeh, thoroughly discussed by Pratico,³⁶ dates to the Iron IIB/C, that is, the 8th-to-early 6th centuries BCE. This assemblage, too, was dated on the basis of detailed comparison with sites in southern Israel. Furthermore, not a single Iron IIA vessel or pottery sherd was found at this site. In sound archaeological research this means that the fortress of Tell el-Kheleifeh cannot date earlier than the late 8th century BCE. En Hazeva has not as yet been fully published, but several vessels have been presented in an exhibition catalogue;³⁷ they too date to the Iron IIB. Indeed, the construction of these large fortresses should be connected with the Assyrian efforts to protect the northern outlets of the desert roads, which supplied the empire with lucrative Arabian goods.

With no clean pottery assemblages on floors, the Khirbet en-Nahas fortress may be dated according to similarity to the well-dated Iron IIB/C fortresses of Tell el-Kheleifeh and En Hazeva; not the other way around.

DISCUSSION

There are three reasons for Levy *et al.*'s errors:

1. They rely on a site with no clear stratigraphy and on ¹⁴C results of samples taken mostly from unstratified industrial refuse.³⁸ Pottery assemblage

³¹ Levy *et al.* 2005; Finkelstein and Piasezky 2008.

³² Pratico 1993.

³³ Cohen and Yisrael 1995a.

³⁴ Pratico 1993; Na'aman 1997.

³⁵ Levy *et al.* 2007: 20–21.

³⁶ Pratico 1993.

³⁷ Cohen and Yisrael 1995b.

³⁸ Finkelstein 2005; Finkelstein and Piasezky 2006.

(and/or ^{14}C samples) from floors clearly associated with walls is the only way to accurately date the fortress at Khirbet en-Nahas. If this is not available, one can resort to circumstantial evidence; but circumstantial evidence cannot serve as an anchor for the pottery sequence in Edom, certainly not in contradiction to the well-reasoned pottery chronology which has been established by comparisons to well-stratified sites in Cisjordan.

2. Levy *et al.* base their entire archaeological and historical reconstruction on an assumption—that Khirbet en-Nahas constitutes part of Edom and should be understood together with the sites excavated on the Edomite plateau. Yet, the finds—also beyond pottery—clearly speak against this assumption. Copper production at Khirbet en-Nahas started in the Iron I. Several Iron I sites—though probably less than argued by one of us long ago³⁹—did exist on the Edomite plateau. But none of them provided clues for a copper-industry connection. In contrast, several settlements developed at that time in the Beer-sheba Valley.⁴⁰ Copper production at Khirbet en-Nahas continued in the Iron IIA and probably reached a peak in the later phase of the period.⁴¹ No Iron IIA material has thus far been published from the many excavations and surveys that were carried out on the Edomite plateau—not even a single vessel.⁴² At that time strong activity continued in the Beer-sheba Valley, including at the large site of Tel Masos Stratum II, which produced evidence of copper activity⁴³ and yielded an exceptionally large number of copper/bronze items.⁴⁴ Finally, when a wave of settlement commenced on the Edomite plateau in the Iron IIB, copper production at Khirbet en-Nahas had already ceased. Therefore, the areas of Wadi Feinan and the Edomite plateau provide contrasting settlement histories. Instead, the site of Khirbet en-Nahas should be linked with the settlement activity, including copper production and trade, in the Beer-sheba Valley, along the roads leading to the coastal plain.⁴⁵

3. Behind Levy *et al.*'s view on the emergence of Edom stands a highly literal reading of the biblical text. They take the list of the kings “who reigned

³⁹ Finkelstein 1992.

⁴⁰ Herzog 1994.

⁴¹ Finkelstein and Piaseckzy 2008.

⁴² A single find had been proposed as evidence for Iron IIA activity there—a small faience rim fragment from Buseirah identified by Milward (1975) as part of an Egyptian relief chalice, stylistically belonging to the 21st or 22nd dynasty. Yet, this identification was apparently based on erroneous drawing (Sedman 2002: 364).

⁴³ Kempinski a.o. 1983: 21.

⁴⁴ Crüsemann 1983; Lupu 1983: 202–203.

⁴⁵ Finkelstein 2005 for both the Iron I and the Iron IIA; Singer-Avitz supports this description for the Iron I.

in the land of Edom, before any king reigned over the Israelites” in Genesis 36: 31 as historical testimony of the existence of a territorial polity there in the 12th and 11th centuries BCE; and the reference in 2 Samuel 8: 14 to garrisons put by King David in Edom as reflecting a 10th century BCE reality.⁴⁶ It is true that many scholars accepted the list in Genesis 36 as containing genuine historical information.⁴⁷ Yet, there are other possibilities: the list may represent a post-monarchic situation in Edom,⁴⁸ a late Iron II reality,⁴⁹ or may refer altogether to Aramaean (rather than Edomite) kings.⁵⁰ And the reference to Edom in 2 Samuel 8 most likely depicts an 8th century BCE reality, reflected back to the time of the founder of the Jerusalem dynasty.⁵¹ Archaeological dating should be established on archaeological considerations and safely-dated historical records, not on vague biblical references to the formative periods in the history of ancient Israel.

Levy *et al.*'s chain of assumptions can be described as follows: The Khirbet en-Nahas fortress dates to the 10th century BCE > Khirbet en-Nahas belongs to Edom > the sites on the Edomite plateau must pre-date the late-Iron II > Tell el-Kheleifeh and En Hazeva must be contemporaries of the Khirbet en-Nahas fortress in the 10th century BCE > they, too, belong to Edom > Edomite state formation took place in the 10th century BCE > this is documented by certain biblical references.

These arguments are contrary to the archaeological data from both the Edomite plateau and the Negev. They are based on the erroneous assumption that Khirbet en-Nahas constituted part of Edom, and probably also on erroneous dating of the Khirbet en-Nahas fortress. If one removes one of the two first items in this chain of assumptions the entire structure of their argument collapses.

CONCLUSIONS

Several sites on the Edomite plateau, for instance Buseirah, produced Iron I sherds, which means that they were established in the Iron I. There is no evidence for activity there in the Iron IIA—contemporary to the peak prosperity at Khirbet en-Nahas. The sites on the plateau date to the Iron IIB/C,

⁴⁶ Levy *et al.* 2005: 158–159.

⁴⁷ E.g., Eissfeldt 1966: 25; Von Rad 1961: 341; Westermann 1986: 561.

⁴⁸ Knauf 1985.

⁴⁹ Bartlett 1989: 94–102.

⁵⁰ Lemaire 2001.

⁵¹ Na'aman 2002: 214.

when copper production at Khirbet en-Nahas has already ceased. The date of the Edomite plateau sites was established according to detailed comparative studies with well-stratified sites in Cisjordan, not only according to the Qos Gabr seal impression from Umm el-Biyara. Khirbet en-Nahas was strongly connected to the Iron I (and IIA—IF) settlement activity in the Beer-sheba Valley and areas further to the west; it had no influence on the settlement history of the Edomite plateau and had no bearing on state-formation in Edom. The date of the Khirbet en-Nahas fortress is difficult to establish. The similarity between this structure and the fortresses unearthed at Tell el-Kheleifeh and En Hazeva may call for down dating the Khirbet en-Nahas fortress to the “Assyrian Century” in the history of the south. In any event, by no means can the former two be dated to the Iron IIA.

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THE JEZIRAH BURNISHED WARE*

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Abstract: The Jezirah Burnished Ware

The discovery of the Jezirah Burnished ware in the sacred area of Tell Barri (north-east Syria) dating back to the Early Jezirah II (2700-2600 BC) reveals a fundamental ostentatious function of this pottery, where the aesthetic sense and symbolic value prevails over any practical function. We can hypothesize that the local production of Jezirah Burnished ware was inspired by the original models from the Anatolian highlands. The production of the Jezirah Burnished ware may have been controlled by the local elites who organized the religious activities, using this pottery in strong relationship with the concept of identity to differentiate themselves from the rest of the community.

Keywords: Tell Barri – pottery – rural shrines – cultural identity

Resumen: La cerámica bruñida Jezirah

El descubrimiento de la cerámica bruñida Jezirah en el área sagrada de Tel Barri (noreste de Siria), que data del Jezirah Temprano II (2700-2600 a.C.), revela una fundamental función ostentativa de esta cerámica, donde el sentido estético y el valor simbólico prevalecen por sobre cualquier función práctica. Podemos pensar como hipótesis que la producción local de la cerámica bruñida Jezirah pudo haber estado controlada por las élites locales que organizaban las actividades religiosas, utilizando esta cerámica en fuerte vinculación con el concepto de identidad para diferenciarse ellas mismas del resto de la comunidad.

Palabras Clave: Tel Barri – cerámica – capillas rurales – identidad cultural

Tell Barri is located in northeast Syria, between the ancient sites of Brak and Hamidiyah, and between the modern cities of Qamishlie and Hassake. In more

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than 25 years, under the direction of Prof. Pecorella† of the University of Florence, 15 soundings have been excavated, with levels dating back to the late IV millennium BC until the Islamic period (fig. 1).¹

The sacred area excavated in Tell Barri² between 2002 and 2005, and dating back to the Early Jezirah II³ (hereinafter EJ) was completely built with mud bricks and made up of two buildings interpreted as shrines (fig. 2 and 3): the rectangular room with bent-axis arrangement (single-room shrine 1297), and the multi-room complex 1548, in which the *cella* was accessible from a court, with a kitchen and two small storage rooms. Both the shrines present a box altar on the short side, and in front of it, a small bench with an oval fireplace.⁴ The shrines were located around a large open space (a *temenos*) which presented on the SW side a storage building composed by two *siloi*.

With this preliminary contribution, we hope to call the Jezirah archaeologists' attention to the discovery at Tell Barri, during the excavation of the sacred area, of a significant amount of pottery fragments characterized by a strongly burnished surface.⁵ Before this discovery, this class of pottery was almost nonexistent in the Jezirah.⁶ For this reason, at the beginning of the excavation, we believed that the Jezirah Burnished Ware⁷ (hereinafter called JBurW) was "imported." Now, based on the recent discoveries in Tell Arbid,⁸

¹ Pecorella 1998.

² Pecorella and Pierobon 2005, Valentini 2006a.

³ For the chronology used in this contribution, see the periods proposed in the "Atlas of Preclassical Upper Mesopotamia" (Pruss 2004: 13-16). To specify the chronology of the sacred area, we noted that its ceramic assemblage belongs to an homogeneous horizon, that reveals strong parallels with Leilan IIIId level, with the pottery of Brak, excavated in the HS4 trench (level 5-4), and with the pottery excavated in the Level 3 at Raqa'i (Valentini 2004; 2005 and 2007).

⁴ The presence of the fireplaces indicates that food offerings, probably contained in small pottery vessels, were burnt in the shrines. The box-altar, probably covered by a wooden table and open on the short side, could be used for presentation or storage of offerings and as support for the animal and human clay figurines (Valentini 2006a). Under the beaten floor of the shrines, inside small pits, were buried incomplete skeletons of foetus and newborns. For a preliminary interpretation of these burials as foundations or propitiatory rituals, see Valentini 2006b.

⁵ I would like to thank Elena Rova and Anna Smogorzewska for the fruitful discussions we have had about this pottery over the past two years.

⁶ With regards to the published material, we know only of a few fragments coming from Tell Leilan (IIIId level), that are described as a "new pottery type, highly burnished grey or black which occurs with carinated bowls" (Calderone and Weiss 2003: 199) and a few fragments from Tell 'Atij (Early Jezirah II-IIIa), described as "céramiques noire et rouge polie et céramique beige brunissée" (Boileau 2005).

⁷ At the beginning of our studies, we called this pottery "Jezirah Polished Ware" (Valentini 2004), but later we decided on the new definition because it is more correct considering the technology. We used a definition consistent with those used in the studies by M. Lebeau on the *Jezirah Bichrome Ware* (Lebeau 2003) and by A. Pruss about the *Jezirah Grey Ware* (Pruss 2000 and 2001).

⁸ P. Bieliński and A. Smogorzewska: pers. comm.

and after more accurate studies, we can consider it to be a peculiar “ware” that was distributed in the Khabur basin around the second quarter of the III millennium BC.⁹

The JBurW from Tell Barri can be divided in two different types:

- The A Type (fig. 4: b-d) is characterized by a fine and compact paste with a tiny quantity of small-size mineral inclusions. The surface of the vessels is strongly burnished¹⁰ and presents different colours that seem to relate to the shapes. The open shapes (fig. 5) are coloured grey, orange, light pink or buff.¹¹ These are curved convex bowls, often slightly carinated, but only on the external profile, with a simple or imitating beads rim and rounded or mini-flat, occasionally convex, bases.¹² The grey, brown or orange-red colour, the squat and bi-conical shape, the everted rim, short neck and rounded base are all common characteristics of the small jars (fig. 6).¹³ All the A Type vessels are well-fired and have thin walls.

- The B Type vessels (fig. 4: a) are characterized by a medium fabric, being less compact than the A Type, with lime and straw inclusions. The quantity and size of these inclusions are generally larger and greater than in the A Type vessels. The surface is strongly burnished and grey, grey/brown and black in colour. The B Type is attested only in the shape of internal/external carinated bowls with slightly everted imitating beads, or with a simple rim and flat base (fig. 7).¹⁴ The B Type vessels tended to be thicker-walled and less-fired than the A Type examples.

Evidence of wheel manufacture is visible both on the A and B Types, for example in the horizontal lines on the surface, or in the regularity of the thickness of the vessel wall. Some examples of A Type jars and B Type carinated bowls may be in part handmade and in part finished on the wheel.

At Tell Barri the JBurW (A and B Types) is attested in the Phases V-T (EJ II),¹⁵ corresponding with the life of the sacred area, and it is associated to the Late Excised Ninevite 5. Phases U-T (Late EJ II), beside the presence of Late Excised Ninevite 5 and JBurW, show the spread of the Early Metallic ware. In

⁹ Without chemical analysis of the ceramic it is not possible to determine if the Tell Barri “Jezirah Burnished Ware” was produced *in situ*.

¹⁰ Burnishing was probably achieved by rubbing or by smoothing with the use of wood or bone objects.

¹¹ Diameter: between 9 and 16 cm; height: between 5 and 9 cm.

¹² The wall above the carination is generally convex and only in few cases is straight. It is interesting to note that in Tell Barri this shape is also rarely attested in the “Kitchen Ware” (Valentini 2007).

¹³ Diameter: about 5 to 15 cm; height: about 5 to 30 cm.

¹⁴ Diameter: between 8-18 cm; height: between 5-15 cm.

¹⁵ Valentini 2004; 2005 and 2007.

the Phases S-R (EJ IIIa-b), the JBurW is attested in a very low percentage and only with the carinated bowls of the B Type (fig. 2).¹⁶

In the Khabur region, the JBurW (A and B Type) is only attested at Tell Barri and Tell Arbid.¹⁷ At Tell Leilan only a few examples of the grey/black carinated bowls (B Type) were found in the IIIId level.¹⁸ One fragment was found also at Tell 'Atij.¹⁹ Due to the scarcity of parallels in the Khabur region and to our hypotheses on the shapes and treatment surface, we turned our attention outside the Jezirah, believing there to be a "connection" between the JBurW and the Early Transcaucasian/Red-Black Burnished Ware²⁰ (hereinafter called ETW/RBBW).²¹ In particular, these two kinds of pottery show the same surface treatment and are attested in the same type of carinated bowls, as demonstrated by the examples found in the Upper Euphrates region at Korucutepe (Early Bronze I),²² Tepecik (Early Bronze II-IIIa)²³ and Pular (Early Bronze I).²⁴ Whilst the ETW/RBBW was strictly handmade, the JBurW was normally wheel finished, and this may confirm its local production.²⁵

With regards to the relationship between the Transcaucasian region and the Jezirah, according to C. Marro,²⁶ we can presume that these contacts occurred sporadically through nomadic groups and their annual transhumance between the highlands and the steppes.²⁷ This type of relationship also leads us to

¹⁶ The grey, carinated bowl (fig. 7) is the only example of JBurW that we found in Tell Barri even in the levels dated back to the EJ IIIb period (2500-2350 BC). These later examples present a richer number of inclusions in the paste that resembles the one of the *Grey Ware* vessels (Valentini 2007).

¹⁷ P. Bieliński and A. Smogorzewska: pers. comm.

¹⁸ Calderone and Weiss 2003: 213, fig. 8:5.

¹⁹ Boileau 2005. One example of this shape is attested by a carinated bowl associated with Late Excised Ninevite vessels found in a burial, possibly dating back to the EJ II, at Tell Mohammed Diyab (C. Nicolle: pers. comm.). In this case however, the bowl presents a non burnished surface buff in colour. Examples of carinated bowls similar to those of the JBurW and without burnished surface are also attested at Tell Arbid (A. Smogorzewska: pers. comm.).

²⁰ For more details about this pottery, see the references in P. de Miroschedji (2000).

²¹ Valentini 2006a; 2007.

²² van Loon 1973: 402: pl. 7B and 404: pl. 9A.

²³ Esin 1979: 66: fig. 3-4.

²⁴ Koşai 1976: fig. 60, 103, 112, 106, 141. A similar shape is attested also at Hama in the Early Bronze I pottery (Thuesen 1988: pl. XLIV: 9).

²⁵ I would like to thank M.-C. Boileau who this idea confirmed to me, analysing the photos of the Tell Barri' JBurW. As regards to the technological difference between wheel-finishing and wheel-shaping methods in the pottery production, see Boileau 2005: 39-54.

²⁶ Marro 2004.

²⁷ A similar phenomenon was still documented at the end of the XIX century for the Turkmen nomads. Contact could be made in two ways, the first, via the Euphrates, between western Syria and the Malatia-Elaziğ region, and the second, via the Tur 'Abdin mountain pass of Mardin, which connects the Transcaucasian region with the Jezirah (Marro 2004). This kind of relationship seems to be confirmed by the sporadic nature of the imported fragments of ETW/RBBW found in Jezirah

understand in greater detail the presence in the sacred area at Tell Barri of small portable hearths with the shape of horseshoe, “snout,” or cylinder with two horns.²⁸ Indeed, in the Anatolian sites, these kinds of object are usually associated with the ETW/RBBW.²⁹

For the JBurW and subsequently for the portable hearths, we can presume that the local production was influenced by original models which derived from the Anatolian highlands. The JBurW, influenced by a production made originally for utilitarian and domestic use (ETW/RBBW) may have subsequently served ritual purposes.³⁰ This pottery may have been evaluated differently at the two ends of the exchange, particularly because culturally-speaking, different communities were involved.

At Tell Barri all the JBurW fragments come from the sacred area of the Area G (fig. 1), apart from the carinated bowls of the B Type, which were in use also after the abandonment of the sacred area and a small jar found in the burial 1309 (fig. 6:9).³¹ It is interesting to note that there is no JBurW in Area B (fig. 1) where, during the 1980's, the excavations uncovered a residential area with several examples of domestic architecture, contemporary with the sacred area.³² Considering the evidence of the context, we can suggest that JBurW (A and B Types) were probably used for serving food during celebrations or to present offerings to the deities inside the shrines. We can presume that JBurW was produced in Tell Barri for local consumption and that its production was

at Tell Mozan (Buccellati and Kelly-Buccellati 1988: 26-27) and Chuera (Kühne 1976: 105-106 and tavv. 39, 9-10), and in the Middle Euphrates region at Halawa B (Lüth 1981: 47 and tavv. 11.3, 56.7) and Tell Hadidi (Dornemann 1988: 17 and figs. 11.9, 10) and by the vessels in Metallic Ware found in Anatolia at Norsuntepe, Arslantepe and Tepecik (Marro 1997).

²⁸ Valentini 2006a.

²⁹ Some scholars, on the basis of the interpretations of the Pular anthropomorphic andirons, assigned them ritualistic properties, although often coupled with a domestic contexts (Smogorzewska 2004 and Takaoğlu 2000). Some other elements of comparison between Jezirah, and Anatolian and Transcaucasian regions can be found in the kitchen pottery excavated in Tell Barri and in particular on the peculiar types of lugs (Valentini 2007).

³⁰ Many goods have multiple functions and a specific good may be viewed as both being a symbol of prestige and as having a useful if not essential and utilitarian function. The multiple use of goods as household items, burial, and ritual goods suggests that the categories of “utilitarian” versus “sumptuary” goods are not as discrete as archaeologists have tended to consider them, and perhaps should best be interpreted as different ends of a continuum (Wattenmaker 1998: 4-5 and 199; Whitehouse 1996).

³¹ Valentini 2005.

³² Biscione 1998. It is also important to underline that the only contemporary parallels published for the JBurW (B Type) come from the Administrative Building excavated in the Leilan Acropolis (Calderone and Weiss 2003). The funerary context of the carinated bowl (similar to the B Type) excavated at Tell Mohammed Diyab in a tomb dated back to the Late Ninevite 5 period (C. Nicolle: pers. comm.) is also significant. At Tell Arbid the JBurW (A and B Types) was excavated in the settlement of the Late Ninevite 5 period that also includes a single-shrine similar to the Tell Barri shrine 1297 (P. Bieliński and A. Smogorzewska: pers. comm.; Bieliński 2006: 3-5).

“controlled” by the local elites, who organized the religious activities.³³ If we monitor the technology (use of the potter’s wheel and burnishing) and the degree of shape standardization, we could consider this ware as a “specialized production.”³⁴

The fact that the JBurW was used in a ritual context shows that it also had an important symbolic value. Furthermore, it seems that the aesthetic value prevailed over the practical function of this pottery. The ostentatious value of the JBurW may have been revealed when the vessels were used during the rituals in shrines or when serving food during ceremonies. In these occasions the pottery had a high degree of visibility because of its portability and size, and would have been therefore used as a “medium of information”³⁵ or as a “social marker.”³⁶

The JBurW, as the ritual objects, was used by the elites in their dynamic role of communicating with the people³⁷ and was used ideologically as symbols of power, to convey both information of a social nature and to differentiate themselves from the rest of the community.³⁸ In this sense, messages delivered on pottery vessels might be a result of increased vertical complexity (social status of individuals), but, on the other hand, we can also suggest a connection with a horizontal scale to convey information about ethnicity and group affiliation within the population.³⁹

To conclude, we can imagine that the Tell Barri settlement, during the EJ II period would have been inhabited by a small community in which elites controlled a local economic system based on the exploitation of surplus products by agricultural and breeding activities. These elites could also increase their power through benefits derived from long-distance trade between southern Mesopotamia cities and the Anatolian highlands.⁴⁰ In this scenario, as documented by the Jezirah rural shrine,⁴¹ we register an escalation and

³³ We can hypothesize that a “nucleated production” in spatially segregated workshops possibly operated independently and was not administrated by any political intervention (Wattenmaker 1998: 4).

³⁴ We can suggest that when the competition was intense, standardization and greater homogeneity of specialist-produced pottery (in our case the JBurW) would have facilitated the communication of specific messages (Douglas and Isherwood 1979: 145).

³⁵ Recchia and Capat 2004: 225.

³⁶ Wattenmaker 1998: 17; Hodder 1982: 45.

³⁷ In this case, the value of an object depended on its culturally assigned role within a specific information system and not on its real value (Wattenmaker 1998: 4).

³⁸ Valentini 2007.

³⁹ Hodder 1982: 56. That could explain the presence of the JBurW also in a domestic context as in the case of Tell Arbid (P. Bieliński and A. Smogorzewska: pers. comm.).

⁴⁰ Valentini 2006a.

⁴¹ Schwartz 2000; Matthews 2002; Valentini 2007.

reorganization of ritual activities that we could interpret as part of the effort by the elites to communicate and sustain new ideologies and the particular own cultural identity. As a consequence, the mobilization of surplus by elites would be institutionalized in the form of religious rituals, making the activity to be part of the natural order of things.⁴² With regards to the pottery, the elites may have continuously developed new styles, for example the JBurW, in order to set themselves apart and stay a step ahead of the non-elites imitating these styles.⁴³

The intensification of political and economic complexity into the rural Jezirah was due to several factors, and, in part, to the emulation of the southern Mesopotamian models of authority, even if adapted to local circumstances.⁴⁴ We can hypothesize that the renewed contacts with the Lower Mesopotamia complex society led the local chiefs to group together to consolidate and affiliate in order to resist external threats.⁴⁵ As a consequence, on a local level they developed a larger-scale political system culminating in the spread of the Second Urban Revolution. This background of shifting leadership, centres of power and political boundaries and movements of populations, may have contributed to the rapid changes in the social identities.

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⁴² Durkheim 1965 [1912].

⁴³ For the EJ II period we have to consider also the use of others example of specialized pottery production as the Ninevite 5 or the Jezirah Bichrome Wares. These potteries gradually decrease since the EJ IIIa to disappear in the EJ IIIb (fig. 1), a period characterized by a diffused standardization of the wares and the shapes, referable to an administrated and centralized production related with the state formation phenomenon (Lebeau 2000: 167-192; Valentini 2007).

⁴⁴ Schwartz 1994; 2003.

⁴⁵ Akkermans and Schwartz 2003.

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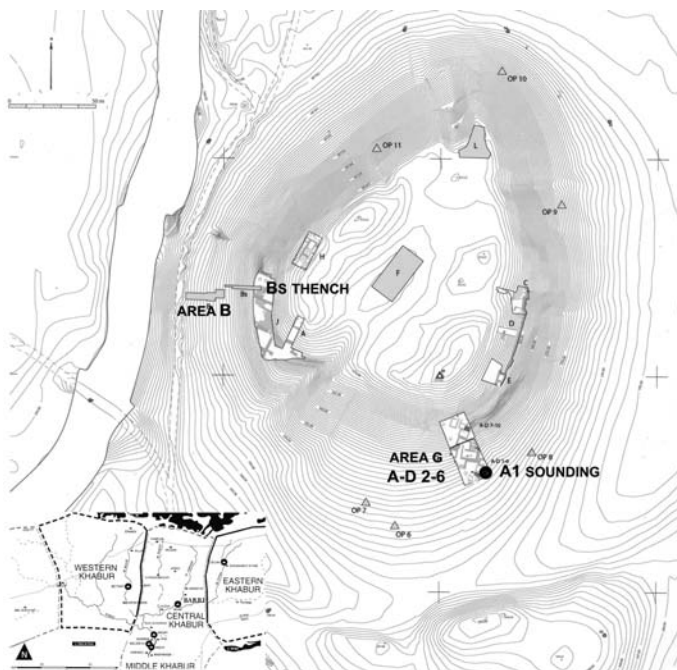


Figure 1.
Topographic map of Tell Barri.

AREA G (1999-2005) A-D 2-6	POTTERY	PERIODS Early Jezirah	OTHER SITES
PERIOD I Phase W, Stratum 45 Sacred Area Foundation	LATE INCISED/EXCISED NINEVITE 5	EJ II (c. 2700-2650)	
PERIOD I Phase V, Strata 44-43 Sacred Area (Sh. 1297 - 1548)	LATE INCISED/EXCISED NINEVITE 5 J. BURNISHED WARE	LATE EJ II (c. 2650?)	Leilan III d (Strata 17-15) Brak J (HS4:5-4; HL1:3-2; HF1:4) Raqa'i 3 'Atij (Late Excised)
PERIOD J Phase U, Strata 42-41 Sacred Area (Sh. 1297)	LATE INCISED/EXCISED NINEVITE 5 J. BURNISHED WARE METALLIC WARE	EJ IIIa (c. 2650-2600?)	
PERIOD J Phase T, Stratum 40 Sh. 1297 (Reconstruction)			
PERIOD K Phase S, Stratum 39 Abandonment	LATE INC./EXC. NINEVITE 5 (?) J. BICHROME WARE METALLIC WARE J. GREY WARE	EJ III a (c. 2600-2500?)	Leilan II a (Strata 14-13) Brak K (HL1:1; HF1:3) Raqa'i 2 Melebiya 2
PERIOD L Phase R, Stratum 38	METALLIC WARE J. GREY WARE POTTERY STANDARDIZATION	EJ III b (c. 2500-2350)	Leilan IIb Brak L

Figure 2.
Chronological Sequence of the Barri Sacred Area.

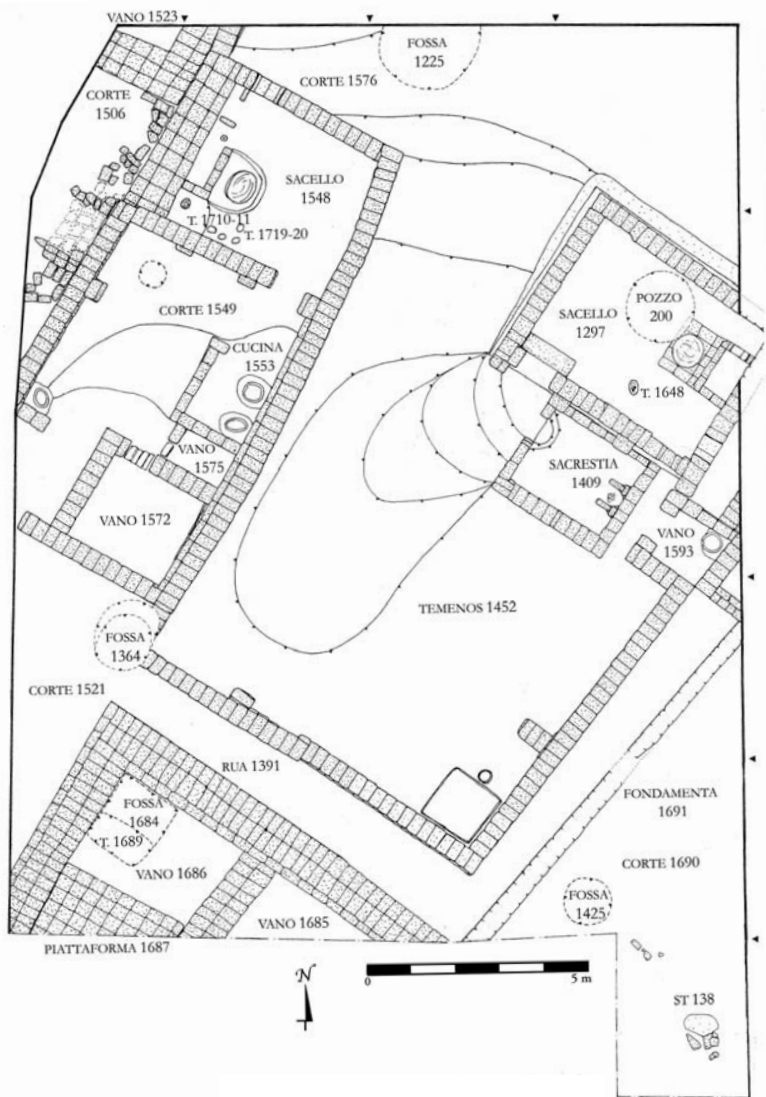


Figure 3.
Tell Barri (Area G). Sacred Area.

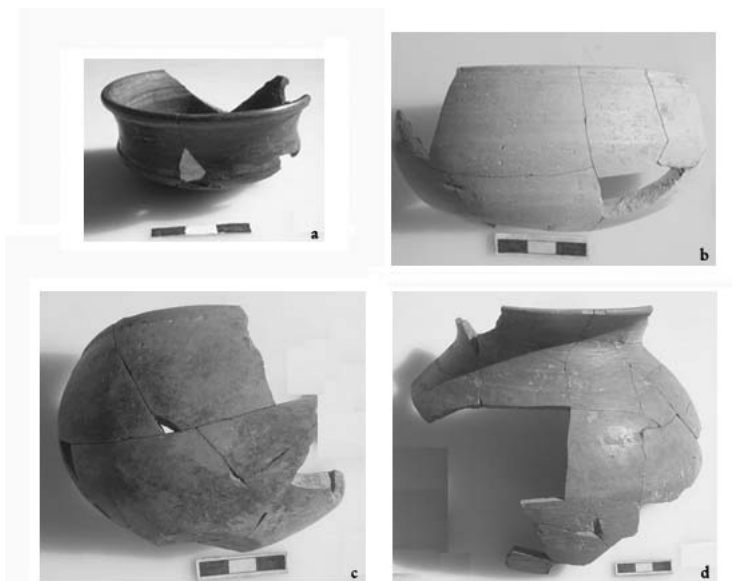


Figure 4.
Jezirah Burnished Ware from the Shrines of the Area G.

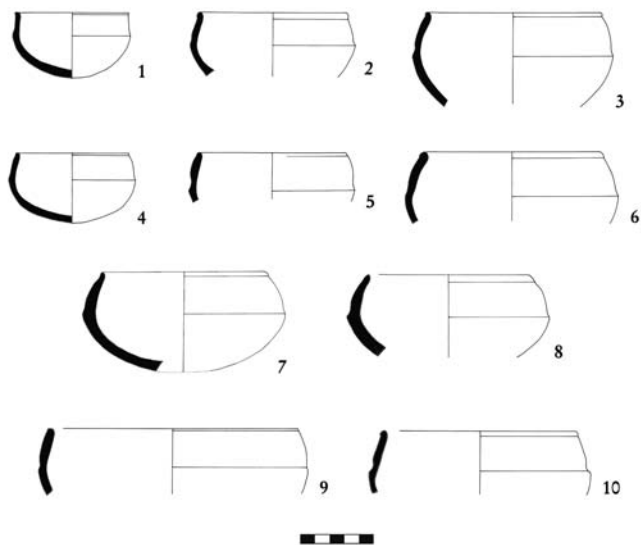


Figure 5.
Jezirah Burnished Ware. A Type Bowls (n. 1-6: Phase V; n. 7-10: Phases U-T).

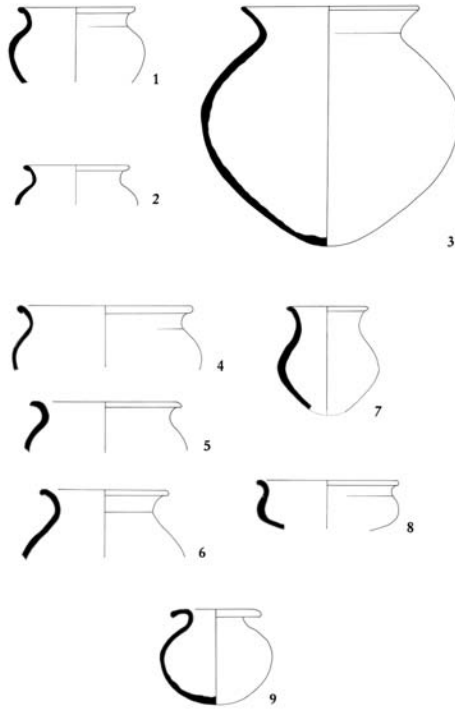


Figure 6.

Jezirah Burnished Ware. A Type Jars (n. 1-3: Phase V; n. 4-8: Phases U-T; Phase S).

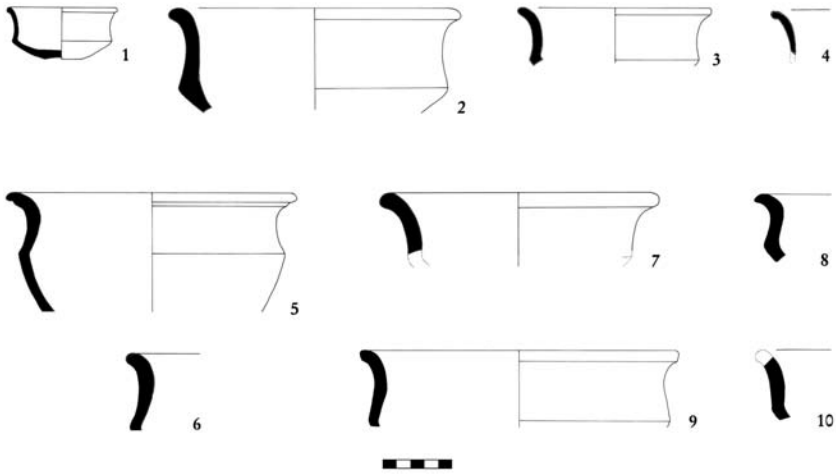


Figure 7.

Jezirah Burnished Ware. B Type, Carinated Bowls (n. 1-4: Phases U-T; 5-6: Phase S; 7-10: Phase R).

THE CORDAGE FROM BERENIKE (1994-2000 SEASONS): THE STATISTICS*

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Abstract: The Cordage from Berenike (1994-2000 Seasons): The Statistics

The cordage from Berenike has been studied in detail during the 1994-2001 seasons, the results of which have been published in several chapters in the excavation's preliminary reports and scholarly articles in journals, mostly in *Antiguo Oriente*. The published results relied heavily on a large database, which not only included notes on cordage's aspects (mainly material, appearance, features, application) but also a large body of statistical information. The present paper presents these statistical data by means of tables, graphs and figures, accompanied by a short description and explanation. Because most conclusions have already been published in the aforementioned publications, the nature of the present paper is strongly descriptive rather than interpretative.

Keywords: material – appearance – composition – cord indices – rope – string

Resumen: El cordaje de Berenice (campañas 1994-2000): las estadísticas

El cordaje de Berenice ha sido estudiado en detalle durante las campañas de 1994-2000, y los resultados fueron publicados en varios capítulos de los informes preliminares de la excavación y en artículos académicos en revistas especializadas, la mayor parte en *Antiguo Oriente*. Los resultados publicados descansan en una gran base de datos, que no sólo incluye notas sobre los aspectos del cordaje (principalmente el material, apariencia, rasgos, aplicación) sino también en un gran cuerpo de información estadística. Este trabajo presenta esos datos estadísticos a través de tablas, gráficos y figuras, acompañados por una breve descripción y explicación. Como muchas de las conclusiones ya han sido publicadas en los mencionados artículos, la naturaleza de este trabajo es más descriptiva que interpretativa.

Palabras Clave: material – apariencia – composición – índices de cuerda – nudo – fibra

* Artículo recibido: 2 de Febrero, 2008; aprobado: 6 de Agosto, 2008. The results of the 2001-season has been discussed separately by Veldmeijer (2006b).

1. INTRODUCTION

This paper presents the statistics of the detailed analysis of the cordage found in Berenike, the Ptolemaic and Roman harbour site at the Egyptian Red Sea coast, on which previous published accounts on the cordage are (partially) based.¹ It was decided to publish all data, including the raw data, to allow the scientific community to evaluate the results. In order to simplify this task, previously published tables are included as well.² Note, however, that in the text, no extensive reference to all tables is made: the results are based on the overall tables.

For an introduction to the site, a description of the contexts as well as the results of the excavations, organised between 1994 and 2001 by the University of Leiden, The Netherlands (co-director W.Z. Wendrich) and the University of Delaware (co-director S.E. Sidebotham), the reader is referred to the preliminary reports.³ The terminology, which is used, is explained and discussed elsewhere, as well as other aspects of the Berenike cordage.⁴

2. MATERIAL (TABLE 1-21)

Half of the site's cordage corpus is made of grass⁵, whereas the other half is made of palm (25%), soft fibre (16%) and (goat) hair⁶ (7%); three percent is registered as 'other material', see below. Although percentages fluctuated, most trench corpora displayed a similar pattern (grass occurred most often, followed by palm, soft fibre and (goat) hair respectively).

The differences between the quantity of grass and palm cordage from the two different periods were only slight but they made up a slightly larger part of the corpus from the youngest contexts, *viz.* 5th-6th century AD (cad). Although it is tempting to suggest that an increased intensity of contacts between the inhabitants of Berenike and the places from which these materials had to be imported is reason for these differences, the differences are too small to draw definite conclusions. It remains uncertain whether material was imported as raw material, half-product, end product or all three of them. However, it is not unreasonable to assume that packing materials, such as bundles unprocessed grass (known to have been used for stowage in shipping cargo) and palm (of

¹ Veldmeijer 2003, 2004, 2005a, b, c, 2006a; Veldmeijer and Van Roode 2004; Wendrich 1995.

² Veldmeijer 1998, 1999; Wendrich 1995; Wendrich and Veldmeijer 1996.

³ Sidebotham and Wendrich 1995, 1996, 1998, 1999, 2000.

⁴ See note 1 and 2.

⁵ With 'grasses', halfa grasses are meant.

⁶ The status of the hair is not entirely clear: goat or hairy sheep, see Wild and Wild 1998: 222.

which the fibre might have been used for the same purpose) were made into cordage and basketry after arriving at Berenike. If so, the ships and/or cargo are likely to have come from areas with (partly) the same vegetation. Note that stowage might have been re-used in other transportation. The choice of material was not only dictated by its strength and application. Availability, price and preparation must have influenced the choice of material too. The fact that grasses were freely obtainable might count for at least part of the far larger quantity of grass cordage.

Although a larger quantity of (goat) hair was excavated from early Roman contexts, the difference between the quantities was too small to draw conclusions on a more desert oriented life style⁷ or not. However, the lack of substantial quantities of leather and rawhide cordage seems to suggest that the people were not specifically desert oriented. In Qasr Ibrim, for example, much leather and rawhide cordage and thongs have been recovered. Qasr Ibrim, although now only an island in Lake Nasr due to the construction of the dams, actually is a hill top fort, at about 70 m height, and although close to the Nile, it was situated in the middle of an arid, desolated area. No cordage made of donkey hair was identified, which is known from sites as Masada⁸: cordage made of camel hair was not attested either.⁹ It is difficult to give a satisfying explanation: donkeys were known in Egypt for a long time (camels, or better, dromedaries, came to Egypt much later). It is possible that the ancient people did use it after all but due to lack of research, especially from pre-Roman this is unknown to us.

The quantity of soft fibre cordage (predominantly flax, but occasionally cotton and wool) is third large in almost all trenches. Remarkable is the huge difference between the two different periods (see below). Soft fibres are regarded as relatively expensive, among others due to the elaborate preparation processes of the raw material before being able to use it, which might count as one reason for the lower quantity; post-depositional processes as another.¹⁰ Flax cordage was mainly used in textiles and fishing nets, in the former among others because of the softness; in the latter because the material is better resistant against water. Other applications of small flax cordage were those applications in which strong but thin cordage was needed such as stringing beads. The larger quantity of soft fibre cordage during the 1st cad is due to the large number of fishing net fragments as well as the larger number of textiles, the soft fibre cordage being, partially, the result of their deterioration.

⁷ Wendrich 1995: 84; Wendrich and Veldmeijer 1996: 295.

⁸ Bernick 1994: 307.

⁹ But see Wild and Wild 1998: 222.

¹⁰ Wendrich and Veldmeijer 1996: 295.

A small quantity of cordage made of other materials was registered, the larger part being registered as 'undeterminable due to deterioration'. Among the identified 'other materials', are reeds: there are indications that these grasses were used for the more 'heavy duty purposes.' Important for the contacts with areas overseas, like India, is the identification of cordage made of coconut, much used in India, Sri Lanka and the whole of East Asia, even nowadays. The coconut cordage must have been used for tying goods for transport because the import of this material for making cordage in Berenike is unlikely: perfectly suitable material could be obtained in large quantities from within the closer vicinity of Berenike. Besides, the coconut shell, from which cordage was made, was thrown away which is an indication that the inhabitants of Berenike were not used to the coconut fruit as source for cordage.

3. APPEARANCE

Because the registration from previous seasons proved that large quantities of soft fibres occurred as yarns, which were not registered with the 'new' registration,¹¹ the supposition that the difference in the occurrence of soft fibre was even larger is justifiable.

3.1. *Composition (table 22-43)*

3.1.1. *Plied cordage (table 22-28)*

Plied cordage was by far the most common type of cordage in every trench, although the percentage showed some fluctuations. Only in trench BE96/97-13, the percentage was less than 70%. Most of the grass and palm cordage were plies, as was the case with the (goat) hair cordage.

The zS₂ composition was most frequently encountered, although this composition was less often registered with palm and even far less with soft fibre relative to the grass plies. Almost no zS₂ (goat) hair cordage occurred; while sZ₂ was encountered sporadically with grass and soft fibre and slightly more often with palm, it was almost the only composition used to make (goat) hair cordage. So far, a reason for this is difficult to present. The suggestion by Renfrew and Bahn¹² that composition reflects right and left-handed people (fibres that spiral to the right, thus 'S'-wise, supposedly were made by right

¹¹ The counting of yarns was abandoned, as yarns are, relative to other types of cordage, more prone to breaking into fragments; therefore they were registered as 'low', 'medium' and 'high' quantities from the 1999-season onwards.

¹² Renfrew and Bahn 1991: 383.

handed people) is highly improbable. This would mean that all S-plyed cordage was made by right-handed people and all Z-plyed, thus (goat) hair cordage, by left-handed people. Although the discussion is limited to plyed cordage, it is not unimportant to bring cables into this discussion. With a $zS_2[Z_3]$ cable, the zS_2 ply is made, according the theory, by a right-handed person and then? Did they bring it to a left-handed person for cabling? This is not the way cabled cordage was made as ethnological research¹³ suggests. Possibly, it is a reflection of male and female workers. Among the Ababda Bedouin in modern Egypt, women make the (goat) hair cordage in the sZ twist. Whether the composition of the ancient (goat) hair cordage was the result of female work or not is impossible to obtain from the archaeological record. If this was the case, the question remains if women also made cordage from other material in the same composition, among which thick ropes (ropes are often made in the same orientation as (goat) hair). Wendrich (pers.com.) thinks that properties of the material was and is the reason for the dominant sZ twist rather than differences due to difference in gender. Whether this can also explain the fact that the sZ twist occurred more often with palm cordage is as interesting as unanswerable for the time being. It is clear, however, that a large part of the ropes were made in this composition.¹⁴

The largest variation in composition is seen with soft fibre. Much soft fibre cordage is the deterioration product of textiles. Moreover, textiles include also items as tassels, flounces and furbelows, drapings and carpets. A few registered tassels¹⁵ strengthen the thought that soft fibre cordage with an 'other composition' (*i.e.* $zS_n, sZ_n; n > 3$), but especially the ones in which 'n' reached high values (seven or higher), originates from these kinds of objects.

As stated, halfa grass was relatively easy to obtain, but perhaps more important might be the fact that it was the cheapest material available (allowing us to assume the observations made by Wendrich¹⁶ was valid in ancient times

¹³ Wendrich 1999: 300.

¹⁴ More on this in Veldmeijer and Zazzaro, submitted. It might be interesting to see whether the importance of this composition was with all palm cordage, or whether this was due to the use of the sZ_2 composition with one of the specific palm tree parts (fibre or leaf). Also, it might be interesting to see if one of the palm species, *H. thebaica* or *P. dactylifera*, shows a tendency to a larger quantity sZ_2 composed cordage or not. This, in turn, might shed light on the sZ_2 composed (goat) hair cordage: if one of the palm species or palm parts show a tendency to the sZ twist, this could be a strong indication that the properties of the material forced to make the cordage in that way, as with (goat) hair. However, although no numbers are available about palm fibre/palm leaf/palm species, it seems doubtful that one of the features exhibited a similar emphasis on sZ_2 as with (goat) hair.

¹⁵ For example BE97/98-19.009 0970-h-3008, BE97/98.19.006 0575-h-3009 and BE97/98-19.006 0575-h-3010.

¹⁶ Wendrich 1999: 282-283.

too). Therefore, it can hardly come as a surprise that grass cordage was by far the largest group of material.¹⁷

Consequently it can be concluded that for the ordinary-purpose-cordage, *i.e.* the cordage used for all kind of purposes, like fastening animals, fastening things which became loose, package etc., especially halfa grass was used. For these purposes, people seems to have made the grass cordage themselves, most of the time on the spot when the string or rope was needed.¹⁸ One might argue that zS₂ was the composition resulting in these actions. Thus, in other words, when people needed cordage for ‘all-time-purposes’ they quickly made it from grass, resulting in cordage with a zS₂ composition. Thus, the zS₂ composition does not reflect centralisation or specific producer and it is therefore not possible to use the most common compositions to suggest specific producers, as suggested by Wendrich.¹⁹ The large number of palm cordage with a zS₂ composition, although smaller than grass, supports these thoughts: the material was less easily available and possibly more expensive than grass,²⁰ but still relatively easily available to people. With the expensive and less easily to prepare soft fibres, the zS₂ composition is less often registered, thus suggesting more specific use.

The zS₃ and sZ₃ composition are indicators for the necessity of stronger cordage. The relatively small quantity was the result of the lower necessity for this stronger cordage relative to cordage of ‘normal strength’.²¹

The corpora of the two different periods (1st cad and the 5th-6th cad) are remarkably similar. However the distribution of compositions among grass and palm is more equal among the plied cordage of the 1st cad than among the 5th-6th cad material. In the oldest period, more cordage was used with three or more yarns: apparently there was a larger need for stronger cordage. The

¹⁷ Although there are other reasons as well like for instance the large quantities of matting used for among others roofing.

¹⁸ As is still done in present day Egypt by farmers and fishermen.

¹⁹ Wendrich 1989: 175.

²⁰ The degree of preparation of the raw material and the production of the cordage, as well as the quantity of that material necessary to produce the needed cordage and the occurrence within Berenike’s vicinity, besides possibly payment for raw material, make up the price of cordage.

²¹ As discussed elsewhere (Veldmeijer 2005b), the relative strength is meant. More on the application of linear cordage in Veldmeijer and Zazzaro, submitted.

higher percentage cabled cordage and the higher percentage ropes supports this conclusion.

3.1.2. Cable cordage (table 29-43)

Cabbling was done to create strong(er) cordage, although for some materials ([goat] hair and to a lesser extend soft fibre) plied cordage seems to have been cabled in the first place to lock the ply rather than to create 'heavy-duty-cordage'. The predominance of alternating twists suggests a good knowledge of obtaining relatively strong cordage, because cordage composed in an alternating twist is stronger relative to cordage made in non-alternating twist.

The large variety of cabled soft fibre can be explained by the fact that creating stronger soft fibre cordage was done by making cabled cordage instead of plied cordage with thicker yarns, the latter of which as usually done with other materials (note that large part of cabled palm fibre cordage were basket handles). Furthermore, textiles often involve cabled cordage, especially items as hangings and tassels. The percentage of cabled cordage of the trench corpus was surprisingly uniform, even though differences occurred. All but one of the corpora showed percentages cabled cordage between 10 and 20% of the total of that trench.

3.2. String/rope (table 44-46)

Mainly grass and palm ropes were regularly encountered. The early contexts contained a larger percentage ropes relative to the late contexts. The function of the few encountered soft fibre ropes remains obscure although at least one was closely associated with shipping related artefacts like sails. It is unlikely that hair was much used to make thick ropes. Beside the huge quantity of hair one would need, hair cordage has a low degree of internal coherency due to the relative shortness of the hairs. Creating stronger cordage from hair was therefore done by various levels of cabling, sometimes combined with other materials. More often, however, braids were used, as is still done nowadays by the Ababda Bedouin, for example as camel ropes. Once deposited, the short hairs become very brittle in arid conditions and loose coherency easily, especially when the cabling was loose anyway. Plied hair cordage hardly has any internal coherency and falls apart easily. Even hair cordage with large diameters must have suffered from the lack of coherency more than any other material.

3.3. *Cord Indices (table 47-58)*

A larger percentage palm cordage exhibited higher CIP values relative to other materials. If assumed that the rate of loosening was the same for grass and palm when made in comparable twist and composition, the higher CIP indicates that palm could be plied more tightly than other materials and was therefore stronger. The fact that compositions with three yarns (zS_3/sZ_3) as well as cabled cordage were more often encountered with palm, supports this thought. It also shows that the people were perfectly aware of these qualities: palm was used when people needed stronger cordage.

Cordage with three yarns often exhibited higher CIP values. The reason for this is that the third yarn was often inserted in a second production phase through which the cordage was tightened a second time (although thick ropes, like the ones found in Mersa/Wadi Gawasis,²² were made in one production phase). The higher CIP values with cabled cordage were due to a second production phase as well: the cabling locks the ply and tightens it.

4. DISCUSSION

Identification of the material on taxon or species proved not possible for all pieces, especially when the material was grass or palm. The identification of the material was often problematic because of the bad preservation, which affected the diagnostic epidermis.²³ This lack of identified material contrasted with especially plaited basketry. A plait of strips of palm leaf, however, is not spun or twisted in contrast to cordage. The production of cordage seriously weakens the resistance against deterioration and the epidermis is damaged already before the cordage being used and, eventually, discarded. Although basketry often shows patterns of wear, in many cases enough of the epidermis is left to identify the material microscopically or even macroscopically.

The identification of cordage made of (goat) hair met problems in the uncertain position of hairy sheep and goats as mentioned previously. The identification of the three soft fibres had to be performed by microscope and even then some could not be identified, again due to deterioration.

As observed, apparently there was a larger need for stronger cordage in the 1st cad. It is tempting to conclude that the stronger cordage originated from shipping related activities especially for the ropes with large diameters. The

²² Veldmeijer and Zazzaro, submitted.

²³ Note that it is still possible to identify it, but this requires laboratory equipment such as scanning electron microscopes. This has not been done.

high amount of other shipping and sea related artefacts from these contexts, such as fishing nets and hooks as well as sails, supports these thoughts. Although heavy work seems obvious, it is not possible to identify the exact purpose of thicker and stronger cordage in more detail. Besides, due to the multiple-functional character of cordage, this is most of the time impossible anyway, unless there is a strong association and/or context.²⁴

The study of the cordage from Berenike shows that archaeological cordage cannot provide the archaeologist with an answers on all aspects. It is not possible to deduce detailed information on the economy of cordage, although the quantity might be an indication. It must, however, be stated that the quantification of cordage is somewhat problematic. Counting pieces of cordage, as is done in the research of Berenike's cordage, did not give an accurate insight in the used quantity of cordage but rather a very rough indication. Cordage can break into many pieces, short and long alike. Another way of quantifying is measuring the length of the recovered cordage, as is done by Bernick.²⁵ This is as uninformative as counting pieces because cordage can be made in any required or desired length. It is the application that makes the quantity informative.

A research of cordage pays the efforts, because it allows insight in the daily life of the people who lived a long time ago. This is especially true for the identifiable and closed-associated cordage and the aspects material and economy. But the bulk of the material, the non-identifiable and non-associated cordage, has less informative content in itself and is 'only' of importance from a statistical point of view. It gains considerable additional importance by comparing with other sites. The statistical information is thus put in broader perspective and relative deductions can be made. This comparison informs one whether the inhabitants of one site were more apt using cordage relative to the inhabitants of other sites.²⁶

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²⁴ Cf. Veldmeijer and Zazzaro, submitted.

²⁵ Bernick 1994.

²⁶ A statistic comparison with Quseir al-Qadim proved difficult due to the fact that no extensive figures of the complete corpus have been published yet. The Berenike research proved very important for the study of the aforementioned ropes at Mersa Gawasis.

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Note on the tables (colour versions of the graphs are available with the author):

- n.d.: 'not dated'
- n.a.: 'not applicable'
- *: dates are uncertain due to their recovery from trench cleans
- bc: baulk clean
- ebc: east baulk clean
- nbc: north baulk clean
- sbc: south baulk clean
- wbc: west baulk clean
- tc: trench clean

CONTEXT	DATE	MATERIAL					TOTAL
		GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
002	5/6 cad	239	114	52	49	27	481
003	5/6 cad	86	57	28	14	0	185
004	5/6 cad	5	4	0	0	0	9
005	5/6 cad	11	7	2	0	0	20
006	5/6 cad	1	2	3	1	1	8
007	5/6 cad	187	35	11	0	1	234
008	5/6 cad	5	0	0	0	0	5
009	5/6 cad	1	0	0	0	0	1
010	5/6 cad	3	0	0	0	0	3
012	5/6 cad	81	55	14	0	0	150
014	5/6 cad	0	1	0	0	0	1
015	5/6 cad	8	2	0	0	0	10
018	5/6 cad	9	1	0	0	0	10
020	5/6 cad	1	1	1	0	0	3
021	5/6 cad	17	3	3	0	2	25
022	5/6 cad	2	0	0	0	0	2
023	5/6 cad	2	0	0	0	1	3
024	5/6 cad	1	2	0	0	0	3
025	5/6 cad	8	3	0	0	0	11
026	5/6 cad	31	10	0	0	1	42
027	5/6 cad	2	6	1	0	0	9
027wc	5/6 cad	9	10	0	0	0	19
028	5/6 cad	22	5	1	0	0	28
028b	5/6 cad	3	1	0	0	0	4
029	5/6 cad	20	11	5	0	0	36
030	5/6 cad	3	2	2	0	0	7
031	5/6 cad	0	0	1	0	0	1
032	5/6 cad	1	0	1	0	0	2
033	5/6 cad	2	3	0	0	0	5
034	5/6 cad	0	1	0	0	0	1
036	5/6 cad	6	4	0	0	0	10
037	5/6 cad	7	3	1	0	0	11
038	5/6 cad	7	6	1	0	0	14
038/039	5/6 cad	0	1	0	0	0	1
053	4/5 cad	0	0	0	2	0	2
059	5 cad	0	0	0	1	0	1
061	2-4 cad	0	0	1	0	0	1
063	2-4 cad	1	0	0	0	0	1
070	5 cad	0	1	0	0	0	1
080	4/5 cad	0	0	1	0	0	1
40	n/a	781	351	129	67	33	1361
bc*	n/d	19*	31*	4*	1*	2*	57*
ebc*	n/d	26*	31*	4*	1*	0*	62*
nbc*	n/d	50*	30*	19*	1*	6*	106*
sbc*	n/d	20*	25*	14*	5*	4*	68*
wbc*	n/d	28*	15*	7*	2*	1*	53*
tc*	n/d	63*	21*	13*	11*	0*	108*
6*	n/a	206*	153*	61*	21*	13*	454*
46	n/a	987	504	190	88	46	1815

Table 1.

Materials of which the cordage was made, found in trench BE95/95-01 during the 1994- and 1995-seasons. The corpus is presented per locus. Dates are incorporated as well.

	CONTEXT	DATED	MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
	001	5/6	41	47	34	13	5	140
	002	5/6	95	26	11	1	5	138
	003	5/6	4	4	6	1	0	15
	004	5/6	14	11	2	0	0	27
	005	5/6	9	10	2	0	0	21
	006	5/6	11	0	2	0	0	13
	008	5/6	141	78	49	6	7	281
	009	5/6	1	2	1	0	0	4
	010	5/6	1	0	0	0	0	1
	012	5/6	21	4	1	1	0	27
	013	5/6	38	16	14	0	2	70
	014	5/6	9	14	5	3	0	31
	016	5/6	2	0	0	0	0	2
	018	5/6	3	6	1	0	0	10
	019	5/6	9	10	42	3	5	69
	020	5/6	4	3	16	0	0	23
	024	5/6	0	0	1	0	0	1
	025	5/6	0	9	4	0	1	14
	029	5/6	0	0	1	0	1	2
	040	4/5	5	1	3	0	0	9
	060	n/d	0	0	1	0	0	1
	069	4	0	1	0	0	0	1
	095	n/d	0	0	0	3	0	3
	100	n/d	1	0	0	0	0	1
	101	n/d	0	0	1	0	0	1
	121	4	0	2	0	0	0	2
	122	n/d	1	0	0	0	0	1
	149	4	2	0	0	0	0	2
SUBTOTAL	28	n/a	412	244	197	31	26	910
	nbc*	n/d	0*	1*	3*	0*	1*	5*
	sbc*	n/d	3*	7*	1*	0*	1*	12*
	wbc*	n/d	2*	1*	1*	0*	0*	4*
	tc*	n/d	4*	0*	3*	0*	0*	7*
SUBTOTAL*	4*	n/a	9*	9*	8*	0*	2*	28*
TOTAL	32	n/a	421	253	205	31	28	938

Table 2.

Materials of which the cordage was made, found in trench BE96/...-10 during the 1996-2000-seasons. The corpus is presented per locus. Dates are incorporated as well.

	CONTEXT	DATE	MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
	003	1 cad	2	9	13	0	1	25
	006	1 cad	1	9	30	0	15	55
	007	1 cad	0	8	0	0	0	8
	009	1 cad	0	2	1	0	0	3
	015	1 cad	0	0	8	0	0	8
	015pr	1 cad	0	0	1	0	0	1
SUBTOTAL	6	n/a	3	28	53	0	16	100
	tc*	n/a	0*	0*	1*	0*	1*	2*
SUBTOTAL*	1*	n/a	0*	0*	1*	0*	1*	2*
TOTAL	7	n/a	3	28	54	0	17	102

Table 3.

Materials of which the cordage was made, found in trench BE95-03 during the 1995-season. The corpus is presented per locus. Dates are incorporated as well.

	CONTEXT	DATE	MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
	001	1 cad	13	13	31	42	1	100
	002	1 cad	430	237	304	119	22	1112
	003	1 cad	2	3	3	4	0	12
TOTAL	3	1 cad	445	253	338	165	23	1224

Table 4.

Materials of which the cordage was made, found in trench BE96/97-13 during the 1996- and 1997-seasons. The corpus is presented per locus. Dates are incorporated as well.

	CONTEXT	DATE	MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
	001	1 cad	0	0	0	5	0	5
	002	1 cad	7	1	7	0	0	15
	003	1 cad	4	2	2	5	0	13
	004	1 cad	0	0	0	1	0	1
	006	1 cad	109	18	34	3	0	164
	007	1 cad	11	2	4	0	0	17
	008	1 cad	115	11	57	11	1	195
	009	1 cad	118	24	33	7	0	182
	011	1 cad	2	0	0	0	0	2
	014	1 cad	0	0	2	0	0	2
	024	1 cad	1	0	0	0	0	1
SUBTOTAL	11	1 cad	367	58	139	32	1	597
	ebc*	1 cad	24*	3*	13*	1*	0*	41*
	nbc*	1 cad	29*	2*	4*	0*	2*	37*
	sbc*	1 cad	3*	6*	0*	0*	0*	9*
	wbc*	1 cad	2*	6*	1*	0*	0*	9*
	tc*	1 cad	8*	0*	6*	7*	0*	21*
SUBTOTAL*	5*	1 cad	66*	17*	24*	8*	2*	117*
TOTAL	16	1 cad	433	75	163	40	3	714

Table 5.

Materials of which the cordage was made, found in trench BE97/98-19 during the 1997- and 1998-seasons. The corpus is presented per locus. Dates are incorporated as well.

	CONTEXT	DATE	MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
	002	1 cad	53	23	12	1	3	92
	006	1 cad	10	50	11	0	1	72
	007	1 cad	2	1	1	0	1	5
	009	1 cad	14	0	6	0	0	20
	010	1 cad	2	0	2	0	6	10
	011	1 cad	0	1	0	0	0	1
	012	1 cad	11	0	3	0	0	14
	014	1 cad	3	0	10	0	0	13
SUBTOTAL	8	1 cad	95	75	45	1	11	227
	001*	1 cad	11*	4*	0*	0*	3*	18*
	bbc*	1 cad	2*	1*	0*	0*	0*	3*
	sbc*	1 cad	2*	1*	1*	0*	0*	4*
	wbc*	1 cad	7*	4*	1*	0*	0*	12*
SUBTOTAL*	4*	1 cad	22*	10*	2*	0*	3*	37*
TOTAL	12	1 cad	117	85	47	1	14	264

Table 6.

Materials of which the cordage was made, found in trench BE99-29 during the 1999-season. The corpus is presented per locus. Dates are incorporated as well.

	CONTEXT	DATE	MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
	002	1 cad	3	0	0	0	0	3
	005	1 cad	4	0	0	0	0	4
	006	1 cad	98	17	15	12	3	145
	007	1 cad	228	163	50	24	13	478
	008	1 cad	1	0	0	0	0	1
	009	1 cad	0	0	1	0	0	1
	010	1 cad	1	0	0	0	0	1
	012	1 cad	19	14	10	0	3	46
	014	1 cad	2	1	1	0	0	4
	018	1 cad	1	0	0	0	0	1
SUBTOTAL	10	1 cad	357	195	77	36	19	684
	ebc*	1 cad	37*	7*	5*	0*	0*	49*
	bbc*	1 cad	22*	8*	2*	0*	0*	32*
	sbc*	1 cad	9*	7*	0*	1*	0*	17*
	wbc*	1 cad	32*	17*	4*	6*	0*	59*
SUBTOTAL*	4*	1 cad	100*	39*	11*	7*	0*	157*
TOTAL	14	1 cad	457	234	88	43	19	841

Table 7.

Materials of which the cordage was made, found in trench BE99-31 during the 1999-seasons. The corpus is presented per locus. Dates are incorporated as well.

CONTEXT	DATE	MATERIAL				TOTAL	TOTAL
		GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		
001	1 cad	6	3	0	1	1	11
003	1 cad	10	4	0	0	1	15
004	1 cad	11	2	1	11	0	25
005	1 cad	18	11	1	0	0	30
006	1 cad	1	0	0	0	0	1
008	1 cad	190	42	27	27	12	298
009	1 cad	6	3	2	1	0	12
011	1 cad	2	9	12	0	0	23
012	1 cad	0	0	0	2	0	2
013	1 cad	85	39	10	8	3	145
017	1 cad	23	22	5	6	1	57
018	1 cad	57	25	4	4	0	90
019	1 cad	9	4	1	5	0	19
022	1 cad	30	22	8	6	5	71
024	1 cad	3	1	0	0	0	4
025	1 cad	33	20	20	0	2	75
026	1 cad	15	11	6	0	1	33
027	1 cad	2	8	0	4	0	14
029	1 cad	6	4	1	0	0	11
033	1 cad	0	2	4	0	0	6
036	1 cad	0	0	2	0	0	2
039	1 cad	0	0	1	0	0	1
TOTAL	22	507	232	105	75	26	945

Table 8.

Materials of which the cordage was made, found in trench BE00-33 during the 2000-season. The corpus is presented per locus. Dates are incorporated as well.

CONTEXT	DATE	MATERIAL				TOTAL	TOTAL
		GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		
005	n/d	1	0	0	0	0	1
007	n/d	3	0	0	0	0	3
TOTAL	2	4	0	0	0	0	4

Table 9.

Materials of which the cordage was made, found in trench BE96-11 during the 1996-season. The corpus is presented per locus. Dates are incorporated as well.

CONTEXT	DATE	MATERIAL				TOTAL	TOTAL
		GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		
001	late Roman	19	18	67	25	2	131
TOTAL	1	19	18	67	25	2	131

Table 10.

Materials of which the cordage was made, found in trench BE96-14 during the 1996-season. The corpus is presented per locus. Dates are incorporated as well.

CONTEXT	DATE	MATERIAL				OTHER	TOTAL
		GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		
002	5 cad	0	0	1	0	0	1
004	5 cad	0	0	1	0	0	1
SUBTOTAL	2	0	0	2	0	0	2
	bc*	n/a	0*	0*	0*	1*	1*
SUBTOTAL*	1*	n/a	0*	0*	0*	1*	1*
TOTAL	3	0	0	2	0	1	3

Table 11.

Materials of which the cordage was made, found in trench BE95-04 during the 1995-season. The corpus is presented per locus. Dates are incorporated as well.

	CONTEXT	DATE	MATERIAL				TOTAL	TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		
SUBTOTAL	010	5/6 cad	0	2	0	0	0	2
	1	5/6 cad	0	2	0	0	0	2
	bc*	5/6 cad	0*	3*	1*	0*	0*	4*
	tc*	n/a	0*	1*	0*	0*	0*	1*
SUBTOTAL*	wbc*	5/6 cad	0*	0*	0*	0*	3*	3*
	3*	5/6 cad	0*	4*	1*	0*	3*	8*
TOTAL	4	n/a	0	6	1	0	3	10

Table 12.

Materials of which the cordage was made, found in trench BE96/97/98-12 during the 1996-1998-seasons. The corpus is presented per locus. Dates are incorporated as well.

	CONTEXT	DATE	MATERIAL				OTHER	TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		
SUBTOTAL	002	4/5 cad	0	10	1	0	0	11
	1	n/a	0	10	1	0	0	11
	bc*	n/a	0*	0*	0*	1*	0*	1*
SUBTOTAL*	1*	n/a	0*	0*	0*	1*	0*	1*
TOTAL	2	n/a	0	10	1	1	0	12

Table 13.

Materials of which the cordage was made, found in trench BE95/96/97-05 during the 1995-1997-seasons. The corpus is presented per locus. *Dates are incorporated as well.*

	CONTEXT	DATE	MATERIAL				OTHER	TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		
SUBTOTAL	003	4/5 cad	6	3	0	0	0	9
	013	4/5 cad	1	3	0	0	0	4
	022	4/5 cad	2	2	14	1	0	19
	025	4/5 cad	0	1	2	0	0	3
	4	4/5 cad	9	9	16	1	0	35
SUBTOTAL*	tc*	4/5 cad	0*	2*	1*	0*	0*	3*
	1*	4/5 cad	0*	2*	1*	0*	0*	3*
TOTAL	5	4/5 cad	9	11	17	1	0	38

Table 14.

Materials of which the cordage was made, found in trench BE96-09 during the 1996-season. The corpus is presented per locus. Dates are incorporated as well.

	CONTEXT	DATE	MATERIAL				TOTAL	TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		
SUBTOTAL	001	5/6? cad	23	18	31	2	26	100
TOTAL	1	5/6? cad	23	18	31	2	26	100

Table 15.

Materials of which the cordage was made, found in trench BE96-15 during the 1996-season. The corpus is presented per locus. Dates are incorporated as well.

		trench BE95/96-06						
CONTEXT	DATE	MATERIAL				TOTAL		
		GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		OTHER	
	002	5/6 cad	0	3	0	0	0	3
	004	5/6 cad	4	13	2	3	0	22
	005	5/6 cad	0	0	15	0	0	15
	008	5/6 cad	0	1	0	0	0	1
	011	5/6 cad	0	1	0	0	0	1
	016	5/6 cad	0	3	0	0	0	3
SUBTOTAL	6	n/a	4	21	17	3	0	45
	tc*	n/d	1*	3*	5*	0*	0*	9*
SUBTOTAL*	1*	n/a	1*	3*	5*	0*	0*	9*
TOTAL	7	n/a	5	24	22	3	0	54

		trench BE97-16						
CONTEXT	DATE	MATERIAL				TOTAL		
		GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		OTHER	
	004	5/6 cad	3	2	0	0	0	5
	005	5/6 cad	38	13	9	6	0	66
	008	5/6 cad	1	0	0	0	0	1
	009	5/6 cad	3	1	0	0	0	4
	010	5/6 cad	39	11	10	2	1	63
	037	5/6 cad	0	2	1	0	0	3
	038	5/6 cad	39	6	21	12	3	81
	041	5/6 cad	127	58	28	31	4	248
SUBTOTAL	8	n/a	250	93	69	51	8	471
	001*	n/d	8*	0*	1*	2*	0*	11*
	s/wbc*	n/d	3*	5*	0*	3*	0*	11*
	sbc*	n/d	26*	5*	3*	3*	0*	37*
SUBTOTAL*	3*	n/a	37*	10*	4*	8*	0*	59*
TOTAL	11	n/a	287	103	73	59	8	530

		trench BE95/96-6 & trench BE97-16						
CONTEXT	DATE	MATERIAL				TOTAL		
		GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		OTHER	
SUBTOTAL	14	5/6 cad	254	114	86	54	8	516
SUBTOTAL*	4*	n/a	38*	13*	9*	8*	0*	68*
TOTAL	18	n/a	292	127	95	62	8	584

Table 16.

Materials of which the cordage was made, found in trench BE95/96-06 and BE97/98-16 during the 1995-1998-seasons. The corpora are presented per locus. Dates are incorporated as well.

	CONTEXT	DATE	MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
	001	5/6 cad	12	9	1	1	0	23
	002	5/6 cad	52	16	4	10	5	87
	003	5/6 cad	29	7	1	0	0	37
	004	5/6 cad	30	8	3	1	1	43
	005	5/6 cad	24	23	0	0	0	47
	006	5/6 cad	14	6	2	0	0	22
	008	5/6 cad	2	1	0	0	0	3
	009	5/6 cad	17	11	5	0	5	38
	010	5/6 cad	75	28	4	17	1	125
	011	5/6 cad	168	46	4	4	4	226
	012	5/6 cad	11	23	1	0	0	35
	013	5/6 cad	1	2	0	0	0	3
	014	5/6 cad	87	18	3	1	0	109
	015	5/6 cad	35	6	3	0	0	44
	016	5/6 cad	7	10	0	0	0	17
	017	5/6 cad	8	3	0	0	0	11
	018	5/6 cad	62	36	2	1	0	101
	019	5/6 cad	5	6	1	0	0	12
	020	5/6 cad	0	14	0	0	0	14
	023	5/6 cad	0	11	4	0	0	15
	024	5/6 cad	0	3	0	0	0	3
TOTAL	21	5/6 cad	639	287	38	35	16	1015

Table 17.

Materials of which the cordage was made, found in trench BE98-21 during the 1998-season. The corpus is presented per locus. Dates are incorporated as well.

	CONTEXT	DATE	MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
	002	5/6 cad	1	0	0	0	0	1
	006	n/d	1	2	0	0	0	3
TOTAL	2	n/a	2	2	0	0	0	4

Table 18.

Materials of which the cordage was made, found in trench BE99-27 during the 1999-season. The corpus is presented per locus. Dates are incorporated as well.

	TRENCH	CONTEXT	DATE	MATERIAL					TOTAL
				GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
SUBTOTAL	BE.SH.2	005	5/6 cad	0	2	0	0	0	2
TOTAL	BE.SH.2	1	5/6 cad	0	2	0	0	0	2
	BE.SH.3	002	5/6 cad	0	0	1	2	0	3
	BE.SH.3	003	5/6 cad	0	2	0	0	0	2
TOTAL	BE.SH.3	2	5/6 cad	0	2	1	2	0	5
	BE.SH.5	001	5/6 cad	0	8	0	8	2	18
	BE.SH.5	002	5/6 cad	0	0	1	10	1	12
	BE.SH.5	003	5/6 cad	1	0	1	0	0	2
TOTAL	BE.SH.5	3	5/6 cad	1	8	2	18	3	32
SUBTOTAL	BE.SH.7	002	5/6 cad	0	0	1	0	0	1
TOTAL	BE.SH.7	1	5/6 cad	0	0	1	0	0	1

Table 19.

Materials of which the cordage was made, found in trenches that were excavated in Shenshef during the 1996-season. The corpus is presented per locus. Dates are incorporated as well.

	PERIOD	MATERIAL					TOTAL NUMBER/%
		GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
		NUMBER/%	NUMBER/%	NUMBER/%	NUMBER/%	NUMBER/%	
	1 cad	1943/48	903/22	788/19	316/8	99/2	4049/100
	5/6 cad	2148/53	1043/26	547/ 13	215/5	114/3	4067/100
SUBTOTAL	n/a	4091/50	1946/24	1335/16	531/7	213/3	8116/100
	5/6 cad SH	1/3	12/30	4/10	20/50	3/8	40/100
	other dated	20/25	26/32	24/30	6/7	5/6	81/100
	n/d	249/45	171/31	83/15	31/6	22/4	556/100
SUBTOTAL	n/a	270/40	209/31	111/16	57/8	30/4	677/100
TOTAL		4361/ 50	2155/25	1446/16	588/7	243/3	8793/100

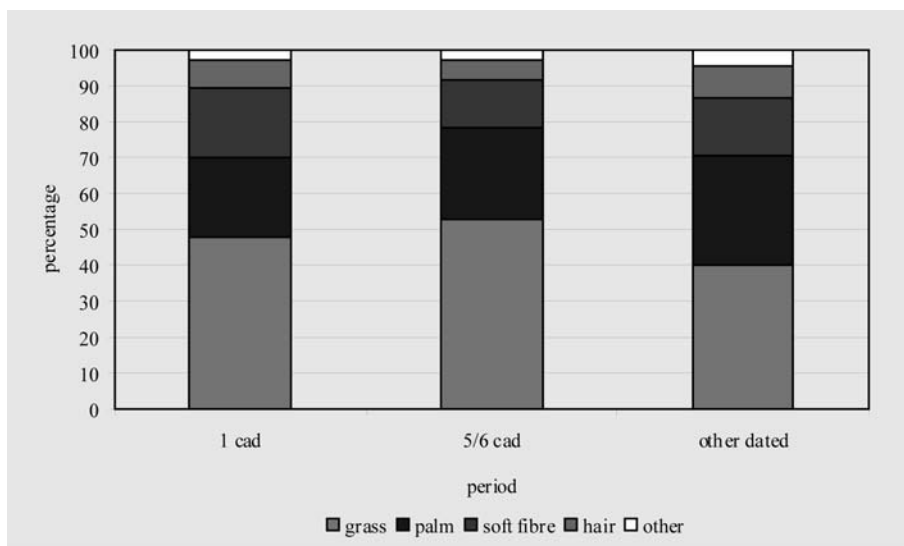


Table 20 and graph 21.

Materials of which the cordage was made, recovered from contexts dated to the 1st cad, the 5th-6th cad and 'other dated' ('5/6 cad', 'other dated' and 'n/a' lumped), was made. The table shows the quantity per material as well as the percentage of a certain material from the total quantity of cordage that was excavated from that period. Graph 21 is based on table 20 and shows the percentages of which the cordage was made. Key: Thirteen percent (**bold** in table 20) of the total quantity of 4067 pieces of cordage that was recovered from a 5th-6th cad context was made of soft fibre and 50% (**bold** in table 20) of all excavated cordage, regardless their date and material, was made of grass.

c o m p o s i t i o n ↓	A					B					C					D				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
	plies per material					total material					total plies (n=1492)					total (n=1815)				
	grass (n=933)	palm (n=398)	soft fibre (n=90)	(goat) hair (n=53)	other (n=18)	grass (n=987)	palm (n=504)	soft fibre (n=190)	(goat) hair (n=88)	other (n=46)	grass	palm	soft fibre	(goat) hair	other	grass	palm	soft fibre	(goat) hair	other
zS ₂	92	75	27	17	61	87	59	13	10	24	58	20	2	1	1	47	16	1	<1	1
sZ ₂	1	8	31	77	33	1	6	15	47	13	<1	2	2	3	<1	<1	2	2	2	<1
zS ₃	7	15	4	0	6	7	12	2	0	2	4	4	<1	0	<<1	4	3	<1	0	<<1
sZ ₃	<1	2	13	2	0	<1	1	6	1	0	<1	<1	1	<<1	0	<1	<1	1	<<1	0
zS _n	0	1	11	0	0	0	1	5	0	0	0	<1	1	0	0	0	<1	1	0	0
sZ _n	0	0	13	4	0	0	0	6	2	0	0	0	1	<1	0	0	0	1	<1	0
other	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
total zS	99	91	42	17	67	94	72	20	10	26	62	24	3	1	1	51	19	2	0	1
total sZ	1	10	57	83	33	1	7	27	50	13	1	2	4	3	<1	1	2	4	2	<1

Table 22.1.

Distribution of the compositions of plied cordage found in trench BE94/95-01. Key: Seventy-five percent (**bold**) of all plies made of palm had the zS₂ composition (Aii). Fifty-nine percent (**bold**) of all cordage made of palm (i.e. regardless twist and composition) had the zS₂ composition (Bii). The zS₂ composition of palm cordage made up 20% (**bold**) of the total number of plied cordage from trench BE94/95-01 (Cii) regardless material. The palm zS₂ cordage made up 16% (**bold**) of the total quantity regardless twists, compositions or materials (Dii). (n>3).

Description of the cordage of trench BE94/95-01, on the basis of table 22.1. This description serves as a key for the following tables (table 22.2-22.14):

Table 22.1 shows that 94% of all grass cordage was made in the zS twist (Bi). The zS twist made up 62% of all plied cordage, regardless the material of which they were made (Ci); the zS twist made up even more than half (51%) of all cordage recovered from the trench, regardless material and composition (in contrast, this was only 1% for the sZ twist [Di]). Ninety-two percent of the plied grass cordage was zS₂-plied and 7% was zS₃ plied. Very few grass plies showed an opposite orientation (only 1% of the total of grass plies, Ai).

Palm plies exhibited a similar emphasis on the zS twist but less convincingly (91% of the total of palm plies [Aii] and 72% of all cordage made of palm [Bii]). On the other hand, plied palm with a zS twist made

up only 24% of the total of plied cordage from trench BE94/95-01 (Cii) and 19% of the total of cordage, regardless material and composition (Dii). This was 2% and 2% for the sZ twist respectively. Seventy-five percent of the plies made of palm (Aii), had a zS₂ composition, which is the most often encountered composition (as with grass). The sZ₂ and sZ₃ compositions occurred more often relative to grass plies. Also the zS₃ composition was registered more often than with grass plies (cf. Ai and Aii).

The zS and sZ twists were much more evenly spread among soft fibre plies. Forty-two percent of the total of soft fibre plies (Aiii) and 20% of all cordage made of soft fibre (Biii) was made according the zS twist, whereas 57% and 27% respectively showed compositions in the sZ twist. However, the plied cordage with a zS twist made up only 3% of the total of plies (Ciii) and 2% of the total of the trench, regardless composition and material (Dii; this was 4% and 4% respectively for the sZ twist). The different compositions were much more evenly represented: there was no strong emphasis on one composition in particular. However, the zS₂ and sZ₂ compositions occurred most often (27% and 31% respectively, Aiii).

The situation with (goat) hair differed because the majority of the plied cordage was made in the sZ twist (Aiv: 83% against 16% zS). Fifty percent of the total of cordage showed the sZ twist (Biv). The percentage of (goat) hair of the total of plied cordage of the trench (Civ) was low (4%) and good a 2% of the total of the trench, regardless the composition and material (Dii). The sZ₂ composition was encountered most often (Aiv: 77% of the total of (goat) hair plies and 47% of the total of (goat) hair cordage).

A little more than 1% of the total of plied cordage was registered as 'other material' (C). Due to the nature of this category not much value can be given to the percentage. However, zS was the most commonly occurring twist (67%: Av).

c o m p o s i t i o n ↓	A					B					C					D				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
	plies per material					total material					total plies (n=416)					total (n=584)				
	grass (n=258)	palm (n=102)	soft fibre (n=21)	(goat) hair (n=31)	other (n=4)	grass (n=292)	palm (n=127)	soft fibre (n=95)	(goat) hair (n=62)	other (n=8)	grass	palm	soft fibre	(goat) hair	other	grass	palm	soft fibre	(goat) hair	other
zS ₂	95	43	24	0	50	84	35	14	0	25	59	11	1	0	<1	42	8	1	0	<1
sZ ₂	<1	7	29	97	0	<1	6	6	48	0	<1	2	1	7	0	<1	1	1	5	0
zS ₃	4	45	0	0	25	4	36	0	0	13	3	11	0	0	<1	2	8	0	0	<1
sZ ₃	0	4	19	3	0	0	3	4	2	0	0	1	1	<1	0	0	1	1	<1	0
zS _n	0	1	5	0	25	0	1	1	0	13	0	<1	<1	0	<1	0	<1	<1	0	<1
sZ _n	0	0	24	0	0	0	0	5	0	0	0	0	1	0	0	0	0	1	0	0
other	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
total zS	99	89	29	0	100	88	72	15	0	51	62	22	1	0	1	44	16	1	0	1
total sZ	<1	11	72	100	0	<1	9	15	50	0	<1	3	3	7	0	<1	2	3	5	0

Table 22.2.

Distribution of the compositions of plied cordage found in trench BE6/16. For key and description see table 22.1. (_n>3).

c o m p o s i t i o n ↓	A					B					C					D				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
	plies per material					total material					total plies (n=716)					total (n=938)				
	grass (n=390)	palm (n=212)	soft fibre (n=78)	(goat) hair (n=18)	other (n=18)	grass (n=421)	palm (n=253)	soft fibre (n=205)	(goat) hair (n=31)	other (n=28)	grass	palm	soft fibre	(goat) hair	other	grass	palm	soft fibre	(goat) hair	other
zS ₂	89	57	35	0	56	83	48	13	0	36	49	17	4	0	1	37	13	3	0	1
sZ ₂	3	16	33	94	22	3	13	13	55	14	2	5	4	2	1	1	4	3	2	<1
zS ₃	7	24	4	0	0	7	22	1	0	0	4	7	<1	0	0	3	5	<1	0	0
sZ ₃	0	2	8	0	11	0	2	3	0	7	0	1	1	0	<1	0	<1	1	0	<1
zS _n	<1	1	12	0	0	<1	1	4	0	0	<1	<1	1	0	0	<1	<1	1	0	0
sZ _n	<1	0	9	6	11	<1	0	3	3	7	<1	0	1	<1	<1	<1	0	1	<1	<1
other	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
total zS	96	82	51	0	56	90	71	18	0	36	53	24	5	0	1	40	18	4	0	1
total sZ	3	18	50	100	44	3	15	19	58	28	2	6	6	2	2	1	4	5	2	1

Table 22.3.

Distribution of the compositions of plied cordage found in trench BE96/...-10. For key and description see table 22.1. (_n>3).

c o m p o s i t i o n ↓	A					B					C					D				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
	plies per material					total material					total plies (n=794)					total (n=1224)				
	grass (n=337)	palm (n=173)	soft fibre (n=141)	(goat) hair (n=130)	other (n=13)	grass (n=445)	palm (n=253)	soft fibre (n=338)	(goat) hair (n=165)	other (n=23)	grass	palm	soft fibre	(goat) hair	other	grass	palm	soft fibre	(goat) hair	other
zS ₂	75	47	24	3	0	57	32	10	2	0	32	10	4	1	0	21	32	3	<1	0
sZ ₂	9	38	23	97	23	7	26	10	76	13	4	8	4	16	<1	3	5	3	10	<1
zS ₃	14	11	4	0	31	11	8	2	0	17	6	2	1	0	1	4	2	<1	0	<1
sZ ₃	1	4	12	0	0	1	3	5	0	0	<1	1	2	0	0	<1	1	1	0	0
zS _n	<1	0	30	0	46	<1	0	13	0	26	<1	0	5	0	1	<<1	0	4	0	<1
sZ _n	0	0	6	0	0	0	0	2	0	0	0	0	1	0	0	0	0	1	0	0
other	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
total zS	89	58	58	3	77	68	40	25	2	43	38	12	10	1	2	25	34	7	<1	1
total sZ	10	42	41	97	23	8	29	17	76	13	4	9	7	16	<1	3	6	5	10	<1

Table 22.4.

Distribution of the compositions of plied cordage found in trench BE96/97-13. For key and description see table 22.1. (_n>3).

	A					B					C					D				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
	plies per material					total material					total plies (n=594)					total (n=714)				
	grass (n=414)	palm (n=62)	soft fibre (n=87)	(goat) hair (n=31)	other (n=0)	grass (n=433)	palm (n=75)	soft fibre (n=163)	(goat) hair (n=40)	other (n=3)	grass	palm	soft fibre	(goat) hair	other	grass	palm	soft fibre	(goat) hair	other
zS ₂	67	31	15	0	0	64	25	8	0	0	47	3	2	0	0	39	3	2	0	0
sZ ₂	8	34	6	94	0	7	28	3	73	0	5	4	1	5	0	4	3	1	4	0
zS ₃	19	19	8	0	0	18	16	4	0	0	13	2	1	0	0	11	2	1	0	0
sZ ₃	6	16	2	6	0	6	13	1	5	0	4	2	<1	<1	0	4	1	<1	<1	0
zS _n	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
sZ _n	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
other	<1	0	69	0	0	<1	0	37	0	0	<1	0	10	0	0	<1	0	8	0	0
total zS	86	50	23	0	0	82	41	12	0	0	60	5	3	0	0	50	5	3	0	0
total sZ	14	50	8	100	0	13	41	4	78	0	9	6	1	5	0	8	4	1	4	0

Table 22.5.

Distribution of the compositions of plied cordage found in trench BE97/98-19. For key and description see table 22.1. (_n>3).

c o m p o s i t i o n ↓	A					B					C					D				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
	plies per material					total material					total plies (n=882)					total (n=1015)				
	grass (n=591)	palm (n=245)	soft fibre (n=19)	(goat) hair (n=25)	other (n=2)	grass (n=639)	palm (n=287)	soft fibre (n=38)	(goat) hair (n=35)	other (n=16)	grass	palm	soft fibre	(goat) hair	other	grass	palm	soft fibre	(goat) hair	other
zS ₂	89	55	0	0	0	82	47	0	0	0	60	15	0	0	0	52	13	0	0	0
sZ ₂	2	8	32	92	0	2	7	16	66	0	1	2	1	3	0	1	2	1	2	0
zS ₃	7	27	5	0	0	7	23	3	0	0	5	7	<1	0	0	4	7	<<1	0	0
sZ ₃	2	10	16	0	0	2	8	8	0	0	1	3	<1	0	0	1	2	<1	0	0
zS _n	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
sZ _n	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
other	<1	<1	47	8	100	<1	<1	24	6	13	<1	<1	1	<1	<1	<<1	<<1	1	<1	<1
total zS	96	82	5	0	0	89	70	3	0	0	65	22	<1	0	0	56	20	0	0	0
total sZ	4	18	48	92	0	4	15	24	66	0	2	5	1	3	0	2	4	1	2	0

Table 22.6.

Distribution of the compositions of plied cordage found in trench BE98-21. For key and description see table 22.1. (_n>3).

	A					B					C					D				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
c o m p o s i t i o n ↓	plies per material					total material					total plies (n=217)					total (n=264)				
	grass (n=105)	palm (n=78)	soft fibre (n=21)	(goat) hair (n=1)	other (n=12)	grass (n=117)	palm (n=85)	soft fibre (n=47)	(goat) hair (n=1)	other (n=14)	grass	palm	soft fibre	(goat) hair	other	grass	palm	soft fibre	(goat) hair	other
zS ₂	69	74	10	0	92	62	68	4	0	79	33	27	1	0	5	27	22	1	0	4
sZ ₂	9	13	10	100	0	8	12	4	100	0	4	5	1	<1	0	3	4	1	<1	0
zS ₃	10	6	14	0	0	9	6	6	0	0	5	2	1	0	0	4	2	1	0	0
sZ ₃	6	5	0	0	0	5	5	0	0	0	3	2	0	0	0	2	2	0	0	0
zS _n	7	0	62	0	8	6	0	28	0	7	3	0	6	0	<1	3	0	5	0	<1
sZ _n	0	1	0	0	0	0	1	0	0	0	0	<1	0	0	0	0	<1	0	0	0
other	0	0	5	0	0	0	0	2	0	0	0	0	<1	0	0	0	0	<1	0	0
total zS	86	80	86	0	100	77	74	38	0	86	41	29	8	0	5	34	24	7	0	4
total sZ	15	19	10	100	0	13	18	4	100	0	7	7	1	<1	0	5	6	1	<1	0

Table 22.7.

Distribution of the compositions of plied cordage found in trench BE99-29. For key and description see table 22.1. (_n>3).

	A					B					C					D				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
c o m p o s i t i o n ↓	plies per material					total material					total plies (n=660)					total (n=841)				
	grass (n=415)	palm (n=172)	soft fibre (n=45)	(goat) hair (n=18)	other (n=10)	grass (n=457)	palm (n=234)	soft fibre (n=88)	(goat) hair (n=43)	other (n=19)	grass	palm	soft fibre	(goat) hair	other	grass	palm	soft fibre	(goat) hair	other
zS ₂	77	48	11	17	40	70	35	6	7	21	49	13	1	<1	1	38	10	1	<1	<1
sZ ₂	3	23	27	83	40	3	17	14	35	21	2	6	2	2	1	2	5	1	2	<1
zS ₃	14	10	7	0	0	13	7	3	0	0	9	3	<1	0	0	7	2	<1	0	0
sZ ₃	5	18	2	0	20	5	14	1	0	11	3	5	<1	0	<1	2	4	<1	0	<1
zS _n	0	0	51	0	0	0	0	26	0	0	0	0	3	0	0	0	0	3	0	0
sZ _n	0	0	2	0	0	0	0	1	0	0	0	0	<1	0	0	0	0	<1	0	0
other	<1	0	0	0	0	<1	0	0	0	0	<1	0	0	0	0	<1	0	0	0	0
total zS	91	58	69	17	40	83	42	35	7	21	58	16	4	<1	1	45	12	4	<1	<1
total sZ	8	41	31	83	60	8	31	16	35	32	5	11	3	2	1	4	9	2	2	1

Table 22.8.

Distribution of the compositions of plied cordage found in trench BE99-31. For key and description see table 22.1. (_n>3).

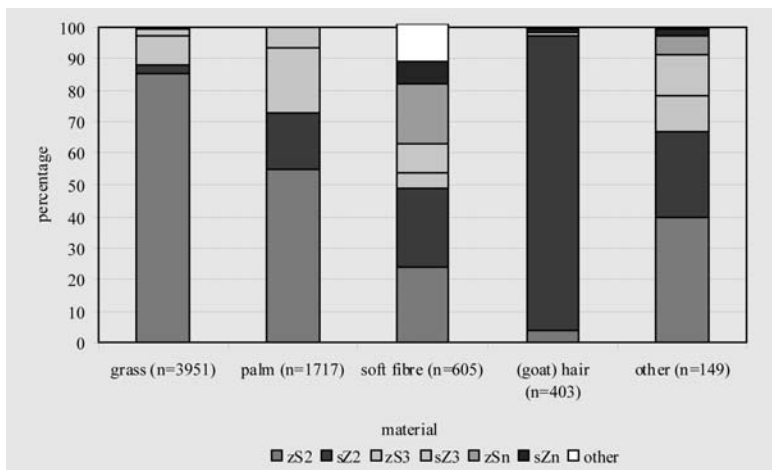
	A					B					C					D				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
c o m p o s i t i o n ↓	plies per material					total material					total plies (n=727)					total (n=945)				
	grass (n=451)	palm (n=187)	soft fibre (n=12)	(goat) hair (n=55)	other (n=22)	grass (n=507)	palm (n=232)	soft fibre (n=105)	(goat) hair (n=75)	other (n=26)	grass	palm	soft fibre	(goat) hair	other	grass	palm	soft fibre	(goat) hair	other
zS ₂	87	33	42	0	14	77	27	5	0	12	54	9	1	0	<1	41	7	1	0	<1
sZ ₂	4	24	17	100	32	4	19	2	73	27	2	6	<1	8	1	2	5	<1	6	1
zS ₃	3	27	8	0	36	3	22	1	0	31	2	7	<1	0	1	2	5	<1	0	1
sZ ₃	6	14	0	0	14	5	11	0	0	12	3	4	0	0	<1	3	3	0	0	<1
zS _n	<1	2	17	0	5	<1	1	2	0	4	<1	<1	<1	0	<1	<1	<1	<1	0	<1
sZ _n	0	1	17	0	0	0	<1	2	0	0	0	<1	<1	0	0	0	<1	<1	0	0
other	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
total zS	90	62	67	0	55	80	50	8	0	47	56	16	2	0	2	43	12	2	0	2
total sZ	10	39	34	100	46	9	30	4	73	39	5	10	1	8	1	5	8	1	6	1

Table 22.9.

Distribution of the compositions of plied cordage found in trench BE00-33. For key and description see table 22.1. (_n>3).

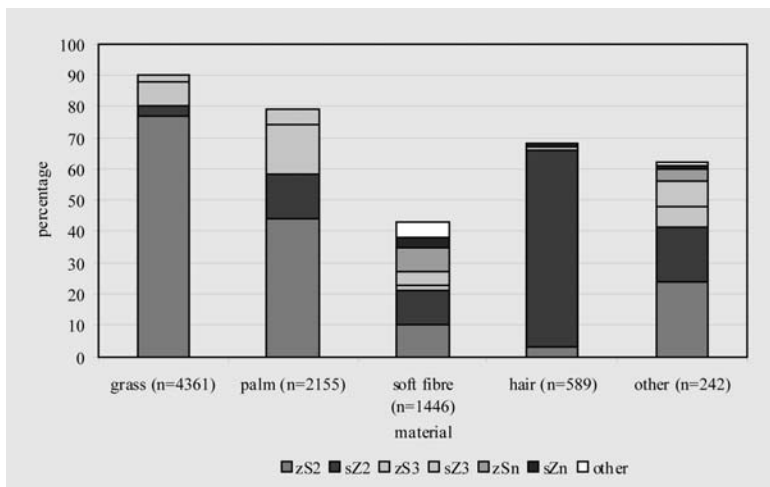
c o m p o s i t i o n ↓	A					B					C					D				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
	plies per material					total material					total plies (n=6825)					total (n=8793)				
	grass (n=3951)	palm (n=1717)	soft fibre (n=605)	hair (n=403)	other (n=149)	grass (n=4361)	palm (n=2155)	soft fibre (n=1446)	hair (n=589)	other (n=242)	grass	palm	soft fibre	hair	other	grass	palm	soft fibre	hair	other
zS ₂	85	55	24	4	40	77	44	10	3	24	49	14	2	<1	1	38	11	2	<1	1
sZ ₂	3	18	25	93	27	3	14	11	63	17	2	4	2	5	1	2	3	2	4	<1
zS ₃	9	20	5	0	11	8	16	2	0	7	5	5	<1	0	<1	4	4	<1	0	<1
sZ ₃	2	7	9	1	13	2	5	4	1	8	1	2	1	<<1	<1	1	1	1	<<1	<1
zS _n	<1	1	19	0	6	<1	<1	8	0	4	<1	<1	2	0	<1	<1	<1	1	0	<1
sZ _n	<<1	<1	7	1	2	<<1	<<1	3	1	1	<<1	<<1	1	<<1	<<1	<<1	<<1	<1	<<1	<<1
other	<<1	<<1	12	<1	1	<<1	<<1	5	<1	1	<<1	<<1	1	<<1	<<1	<<1	<<1	1	<<1	<<1
total zS	94	76	48	4	57	85	60	20	3	35	54	19	4	<1	2	42	15	3	<1	2
total sZ	5	25	41	95	42	5	19	18	65	26	3	6	4	5	1	3	4	3	4	1

Table 22.10 (above) and graph 22.11-22.14 (below).
 Distribution of the compositions of plied cordage found in Berenike. (_n>3).



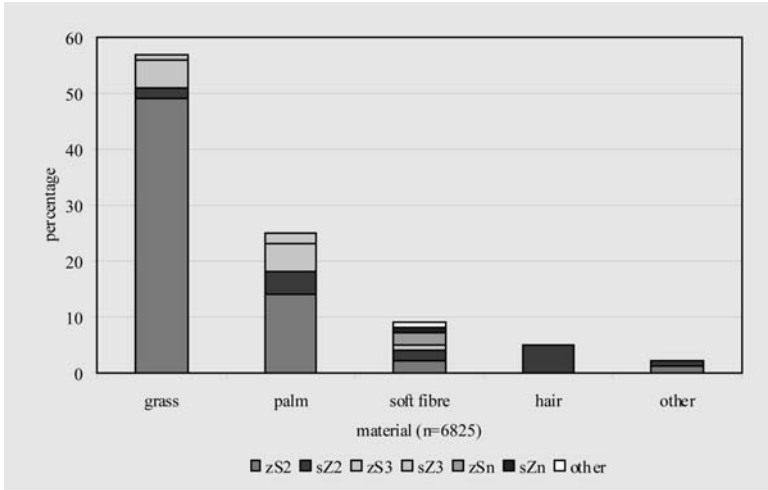
Graph 22.11.

Graphic presentation of the A section of table 22.10. Key: Twenty percent (bold in table 22.10) of all palm plies showed the zS₃ composition. (n>3).



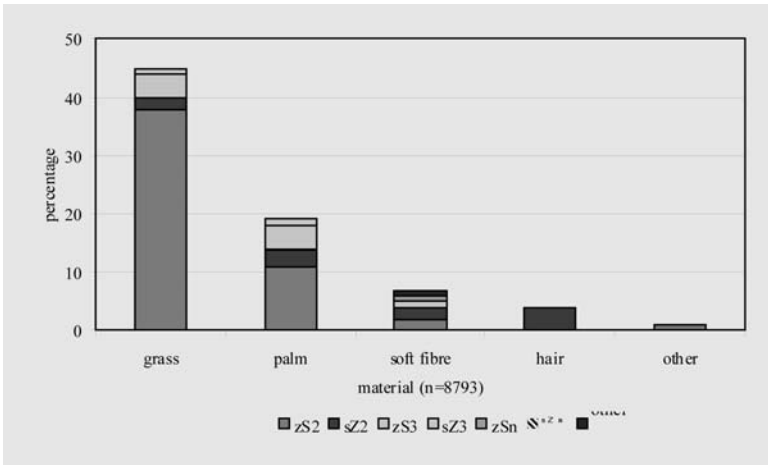
Graph 22.12.

Graphic presentation of the B section of table 22.10. Key: Sixteen percent (bold in table 22.10) of all palm cordage (i.e. regardless twist) showed the zS₃ composition. (n>3).



Graph 22.13.

Graphic presentation of the C section of table 22.10. Key: The zS₃ composition of cordage made of palm made up 5% (**bold** in table 22.10) of the total number of plied cordage from the site regardless material. (_n>3).



Graph 22.14.

Graphic presentation of the D section of table 22.10. Key: The palm zS₃ cordage made up 4% (**bold** in table 22.10) of the total quantity of cordage without taking notice of difference in twist, composition or material. (_n>3).

	COMPOSITION		MATERIAL			TOTAL		
		GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		OTHER	
1 cad	zS ₂	1312	317	73	7	16	1725	
		sZ ₂	98	188	56	222	18	582
	zS ₃	207	103	21	0	11	342	
		sZ ₃	79	79	20	0	17	195
	zS _n	8	8	91	0	7	114	
		sZ _n	0	2	11	0	0	13
	other	4	0	54	0	1	59	
	total	1708	697	326	229	70	3030	
5/6 cad	zS ₂	1810	514	59	5	30	2418	
		sZ ₂	31	93	76	123	20	343
	zS ₃	135	210	9	0	1	355	
		sZ ₃	14	32	31	3	5	85
	zS _n	1	6	21	0	2	30	
		sZ _n	1	0	26	5	3	35
	other	1	1	7	2	0	11	
	total	1993	856	229	138	61	3277	

Table 23.

Plied cordage and the distribution of compositions, quantified per material and period (1st cad and 5th-6th cad). The last column shows the totals of a composition per period. The table shows the quantities from which the percentages of table 24 and 25 were calculated. Key: Thirty-one percent (**bold**) plied pieces of cordage made of grass, encountered in contexts dated to the 5th-6th century AD, showed the sZ₂ composition. Thirteen (**bold**) plied pieces of cordage from the 1st century AD contexts showed a sZ_n composition. (_n>3).

c o m p o s i t i o n ↓	A					B					C					D					E				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
	plies per material					total material					total plies (n=3030)					total 1 cad (n=4049)					total site (n=8793)				
	grass (n=1708)	palm (n=697)	soft fibre (n=326)	(goat) hair (n=229)	other (n=70)	grass (n=1943)	palm (n=903)	soft fibre (n=788)	(goat) hair (n=316)	other (n=99)	grass	palm	soft fibre	(goat) hair	other	grass	palm	soft fibre	(goat) hair	other	grass	palm	soft fibre	(goat) hair	other
zS ₂	77	45	22	3	23	68	35	9	2	16	43	10	2	<1	1	32	8	2	<1	<1	15	4	1	<<1	<1
sZ ₂	6	27	17	97	26	5	21	7	70	18	3	6	2	7	1	2	5	1	5	<1	1	2	1	3	<1
zS ₃	12	15	6	0	16	11	11	3	0	11	7	3	1	0	<1	5	3	1	0	<1	2	1	<1	0	<1
sZ ₃	5	11	6	0	24	4	9	3	0	17	3	3	1	0	1	2	2	<1	0	<1	1	1	<1	0	<1
zS _n	<1	1	28	0	10	<1	1	12	0	7	<1	<1	3	0	<1	<1	<1	2	0	<1	<1	<<1	1	0	<<1
sZ _n	0	<1	3	0	0	0	<1	1	0	0	0	<<1	<1	0	0	0	<<1	<1	0	0	0	<<1	<1	0	0
other	<1	0	17	0	1	<1	0	7	0	1	<1	0	2	0	<<1	<<1	0	1	0	<<1	<<1	0	1	0	<<1
total zS	89	61	57	3	49	79	47	24	2	34	50	13	6	<1	2	37	11	5	<1	1	17	5	2	<<1	1
total sZ	11	38	27	97	50	9	30	11	70	35	6	9	3	7	2	4	7	2	5	1	2	3	2	3	1

Table 24 (above) and graph 25 (below).

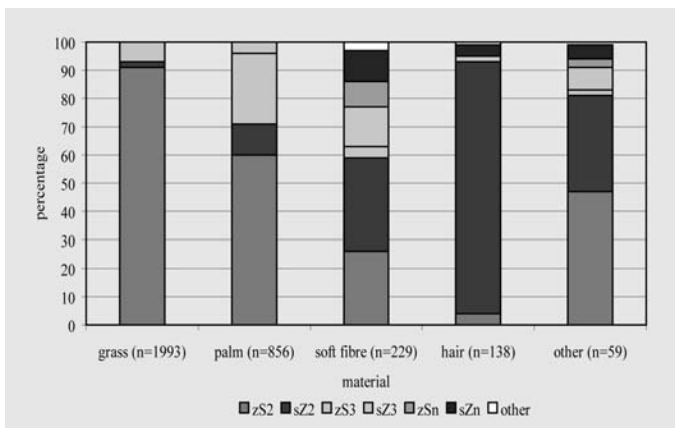
Distribution of the compositions of plied cordage from contexts dated to the 1st cad, in percentage of plies of a material (Ai-v), in percentage of the total quantity of a material regardless composition (Bi-v), in percentage of the total quantity of plies from the trench regardless material (Ci-v), in percentage of the total quantity of all compositions from the trench regardless material (Di-v) and in percentage of the total quantity of cordage from the site regardless twist, composition and material (Ei-v). Key: Forty-five percent (**bold** in table 24) of all plies made of palm, had the zS₂ composition (Aii). Thirty-five percent (**bold** in table 24) of all cordage made of palm (i.e. regardless twist and composition), had the zS₂ composition (Bii). This zS₂ composition of palm cordage, made up 10% (**bold** in table 24) of the total quantity of plied cordage (Cii) regardless material. The palm zS₂ cordage made up 8% (**bold**) of the total quantity of cordage without taking notice of differences in twists, compositions or materials (Dii). Four percent of the total quantity of cordage from the site was zS₂ palm cordage from a 1st cad context (Eii). (>_n >3). Graph 25. The graph shows the data of Ai-v, table 24.

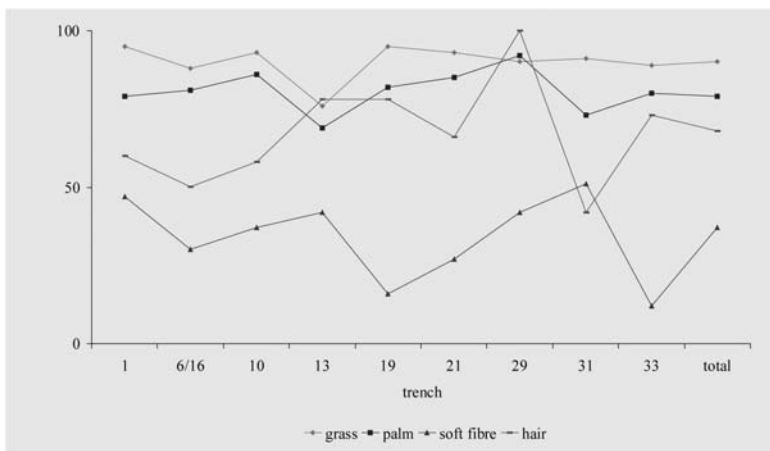


c o m p o s i t i o n ↓	A					B					C					D					E				
	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v	i	ii	iii	iv	v
	plies per material					total material					total plies (n=3275)					total 5/6 cad (n=4067)					total site (n=8793)				
	grass (n=1993) palm (n=856) soft fibre (n=229) (goat) hair (n=138) other (n=59)					grass (n=2148) palm (n=1043) soft fibre (n=547) (goat) hair (n=215) other (n=114)					grass palm soft fibre (goat) hair other					grass palm soft fibre (goat) hair other					grass palm soft fibre (goat) hair other				
zS ₂	91	60	26	4	47	84	49	11	2	25	55	16	2	<1	1	45	13	1	<1	1	21	6	1	<<1	<1
sZ ₂	2	11	33	89	34	1	9	14	57	18	1	3	2	4	1	1	2	2	3	<1	<1	1	1	1	<1
zS ₃	7	25	4	0	2	6	20	2	0	1	4	6	<1	0	<<1	3	5	<1	0	<<1	2	2	<1	0	<<1
sZ ₃	1	4	14	2	8	1	3	6	1	4	<1	1	1	<<1	<1	<1	1	1	<<1	<1	<1	<1	<1	<<1	<<1
zS _n	<<1	1	9	0	3	<<1	1	4	0	2	<<1	<1	1	0	<<1	<<1	<1	1	0	<<1	<<1	<<1	<1	0	<<1
sZ _n	<<1	0	11	4	5	<<1	0	5	2	3	<<1	0	1	<1	<<1	<<1	0	1	<1	<<1	<<1	0	<1	<<1	<<1
other	<<1	<1	3	1	0	<<1	<<1	1	1	0	<<1	<<1	<1	<<1	0	<<1	<<1	<1	<<1	0	<<1	<<1	<<1	<<1	0
total zS	98	86	39	4	53	90	70	17	2	28	59	22	3	<1	1	48	18	2	<1	1	23	8	2	<<1	<1
total sZ	3	15	58	95	47	2	12	25	60	25	1	4	4	4	1	1	3	4	3	1	1	1	2	1	<1

Table 26 (above) and graph 27 (below).

Distribution of the compositions of plied cordage from contexts dated to the 5th-6th cad, in percentage of plies of a material (Ai-v), in percentage of the total quantity of a material regardless composition (Bi-v), in percentage of the total quantity of plies from the trench regardless material (Ci-v), in percentage of the total quantity of all compositions from the trench regardless material (Di-v) and in percentage of the total quantity of cordage from the site regardless twist, composition and material (Ei-v). (_n>3). For key see table 24. The graph shows the data of Ai-v, table 26.





Graph 28.

Plied cordage, per material, in percent of the total quantity of cordage per trench to show the importance of plied cordage and its fluctuations per trench.

	TWIST	COMPOSITION	MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
cables	sS[Z]	sS ₂ [Z ₂]	0	0	1	0	0	1
		sS ₃ [Z ₂]	0	0	1	0	0	1
		sS _n [Z ₂]	0	0	2	0	0	2
	zZ[S]	zZ _n [S ₂]	0	0	1	0	0	1
		sZ[S]	sZ ₂ [S ₂]	2	3	13	26	2
	zS[Z]	sZ ₂ [S ₃]	0	1	0	1	0	2
		sZ ₃ [S ₂]	0	0	1	1	0	2
		sZ _n [S ₂]	0	0	4	0	0	4
		zS ₂ [Z ₂]	14	23	6	0	20	63
		zS ₂ [Z ₃]	16	12	1	0	0	29
		zS ₂ [Z _n]	0	3	0	0	0	3
		zS ₃ [Z ₂]	2	7	1	0	5	15
		zS ₃ [Z ₃]	2	3	2	0	0	7
		zS _n [Z ₂]	0	0	8	0	0	8
		sZ[Z]	sZ ₂ [Z ₂]	0	1	3	1	0
	sZ ₂ [Z _n]		0	0	3	0	0	3
	sZ ₃ [Z ₂]		0	0	1	0	0	1
	sZ _n [Z ₂]		0	0	1	0	0	1
	zS[S]	zS ₂ [S ₂]	11	6	1	0	0	18
		zS ₃ [S ₂]	0	1	1	0	0	2
double cable	sZ[S]{Z}	sZ ₂ [S ₂]{Z ₂ }	0	0	2	2	0	4
		sZ ₂ [S ₃]{Z ₂ }	0	0	0	1	0	1
	sZ[Z]{S}	sZ ₂ [Z ₂]{S ₂ }	0	0	0	1	0	1
	sZ[Z]{Z}	sZ ₂ [Z ₂]{Z ₂ }	0	0	1	0	0	1
	zS[S]{Z}	zS ₂ [S ₂]{Z ₂ }	0	1	0	0	0	1
	zS[S]{S}	zS ₂ [S ₂]{S ₂ }	0	1	0	0	0	1
	zZ[S]{S}	zZ ₂ [S ₂]{S ₂ }	0	0	1	0	0	1
	total alternating		36	53	38	31	27	185
total non-alternating		11	9	17	2	0	39	
total trench		47	62	55	33	27	224	
% of total trench		21	28	25	15	12	100	

Table 29.

Cabled cordage and the variety of twist and composition, quantified per material, found in trench BE94/95-01. Key: Twenty-one percent (**bold**) of the total quantity of 224 pieces of cabled cordage from trench BE94/95-01 was made of grass. The row ' % of total trench ' shows the percentage cables of a material from the total quantity of cables from that trench. (_n>3).

	TWIST COMPOSITION		MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
cable	sZ[S]	sZ ₂ [S ₂]	0	3	1	12	0	16
		sZ _n [S ₂]	0	0	5	0	0	5
		sZ _n [S ₃]	0	0	2	0	0	2
		sZ _n [S ₁]	0	0	1	0	0	1
		sZ _n [S ₂]	0	0	0	0	0	0
	zS[Z]	zS ₂ [Z ₂]	15	6	0	0	0	21
		zS ₂ [Z ₃]	0	0	0	0	3	3
		zS ₃ [Z ₂]	2	0	0	0	0	2
		zS ₃ [Z ₃]	0	2	0	0	0	2
		zS _n [Z ₂]	0	0	8	0	0	8
	sZ[Z]	sZ ₂ [Z ₂]	0	0	0	3	1	4
		sZ ₂ [Z ₃]	0	0	0	0	1	1
		sZ _n [Z ₂]	0	0	3	0	0	3
	zS[S]	zS ₂ [S ₂]	0	1	0	0	0	1
		zS _n [S ₁]	1	0	0	0	0	1
double cable	sZ[S]{Z}	sZ ₂ [S ₂]{Z ₂ }	0	0	0	1	0	1
		sZ ₂ [S ₃]{Z ₂ }	0	0	0	1	0	1
	sZ[I]{S}	sZ ₂ [I ₂]{S ₃ }	0	0	0	1	0	1
		sZ ₂ [Z ₂]{S ₃ }	0	0	0	1	0	1
	zS[Z]{S}	zS ₂ [Z ₂]{S ₂ }	2	0	2	0	0	4
		zS ₂ [S ₂]{S _n }	0	0	1	0	0	1
total alternating		19	11	19	14	3	66	
total non-alternating		1	1	4	5	2	13	
total trench		20	12	23	19	5	79	
% of total trench		25	15	29	24	6	100	

Table 30.

Cabled cordage and the variety of twist and composition, quantified per material, found in trench BE6/16. (_n>3). For key see table 29.

		TWIST COMPOSITION		MATERIAL					TOTAL
				GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
cable	sZ[S]	sZ ₂ [S ₂]	1	1	7	11	0	20	
		sZ _n [S ₂]	0	0	4	0	0	4	
	zS[Z]	zS ₂ [Z ₂]	12	10	3	0	2	27	
		zS ₂ [Z ₃]	1	1	0	0	0	2	
		zS ₂ [Z ₂]	2	6	0	0	1	9	
		zS _n [Z ₂]	0	0	1	0	0	1	
	sZ[Z]	sZ ₂ [Z ₂]	0	0	3	0	0	3	
		sZ _n [Z _n]	0	0	1	0	0	1	
	zS[S]	zS ₂ [S ₂]	3	0	1	0	0	4	
		zS ₂ [S ₃]	0	0	1	0	0	1	
		zS _n [S ₂]	0	0	2	0	0	2	
		zS _n [S ₃]	0	0	0	0	0	0	
	double cable	sZ[S]{Z}	sZ ₂ [S ₂]{Z ₂ }	0	1	0	0	0	1
sZ[Z]{S}		sZ ₂ [Z ₂]{S ₂ }	0	0	0	0	1	1	
zS[Z]{S}		zS ₂ [Z ₂]{S ₂ }	0	0	1	0	0	1	
zS[S]{Z}		zS ₂ [S ₂]{Z ₂ }	0	0	1	0	0	1	
		zS _n [S ₂]{Z ₂ }	0	0	1	0	0	1	
zS[S]{S}		zS ₂ [S ₂]{S ₂ }	0	0	1	0	0	1	
total alternating		16	19	16	11	3	65		
total non-alternating		3	0	11	0	1	15		
total trench		19	19	27	11	4	80		
% of total trench		24	24	34	14	5	100		

Table 31.

Cabled cordage and the variety of twist and composition, quantified per material, found in trench BE96/...-10. (_n>3). For key see table 29.

	TWIST	COMPOSITION	MATERIAL					TOTAL	
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER		
Cable	sS[Z]	sS ₂ [Z] _n z	0	1	0	0	0	1	
	zZ[S]	zZ _n [S ₂]	0	0	5	0	0	5	
	sZ[S]	sZ ₂ [S ₂]	9	8	1	9	0	27	
		sZ ₂ [S ₃]	0	0	0	1	0	1	
		sZ ₂ [S _n]	0	0	0	6	0	6	
		sZ ₃ [S ₃]	0	0	5	0	0	5	
		sZ _n [S ₂]	0	0	2	0	0	2	
		zS[Z]	zS ₂ [Z ₂]	29	6	10	0	2	47
	zS[Z]	zS ₂ [Z ₃]	10	3	0	1	0	14	
		zS ₂ [Z _n]	2	0	0	0	0	2	
		zS ₃ [Z ₂]	1	2	1	0	0	4	
		zS ₃ [Z ₃]	5	6	0	0	1	12	
		zS _n [Z ₂]	0	0	10	0	1	11	
		zS _n [Z ₃]	0	0	2	0	0	2	
		zS _n [Z _n]	0	0	1	0	0	1	
		sZ[Z]	sZ ₂ [Z ₂]	0	0	0	2	0	2
			sZ ₃ [Z ₂]	0	0	2	0	0	2
		zS[S]	zS ₂ [S ₂]	2	0	1	0	0	3
	zS ₂ [S ₃]		0	0	0	0	1	1	
	zS _n [S ₂]		0	0	4	0	0	4	
double cable	sZ[S]{Z}	sZ ₂ [S ₂]{Z ₂ }	0	0	0	2	0	2	
		sZ ₂ [S _n]{Z ₂ }	0	0	0	0	2	2	
		sZ ₃ [S ₂]{Z ₂ }	0	0	0	0	1	1	
	sZ[Z]{S}	sZ ₂ [Z ₂]{S ₂ }	0	0	0	2	1	3	
		sZ ₂ [Z ₃]{S ₃ }	0	0	2	0	0	2	
	sZ[S]{S}	sZ ₂ [S ₂]{S _n }	0	0	0	1	0	1	
	zS[Z]{S}	zS ₂ [Z ₂]{S ₂ }	0	0	1	0	0	1	
	zS[S]{Z}	zS ₂ [S _n]{Z ₂ }	0	0	1	0	0	1	
	total alternating		56	25	33	19	7	140	
	total non-alternating		2	1	15	5	2	25	
total trench		58	26	48	24	9	165		
% of total trench		35	16	29	15	5	100		

Table 32.

Cabled cordage and the variety of twist and composition, quantified per material, found in trench BE96/97-13. (_n>3). For key see table 29.

CONTEXT	MATERIAL					TOTAL
	GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
BE97/98-19	14	10	70	9	0	103
% of total trench	14	10	68	9	0	100
BE98-21	44	42	17	10	14	127
% of total trench	35	33	13	8	11	100

Table 33.

Cabled cordage, quantified per material, found in trenches BE97/98-19 and BE98-21. For key see table 29.

	TWIST	COMPOSITION	MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
cable	sZ[S]	sZ ₂ [S ₂]	0	0	2	0	0	2
		sZ _n [S ₂]	0	0	1	0	0	1
	zS[Z]	zS ₂ [Z ₂]	2	1	1	0	0	4
		zS ₂ [Z ₃]	0	0	1	0	0	1
		zS ₃ [Z ₃]	8	4	0	0	1	13
		zS _n [Z ₂]	0	0	6	0	0	1
		zS _n [Z ₃]	0	0	1	0	0	1
	sZ[Z]	sZ ₂ [Z ₂]	0	1	0	0	0	1
	zS[S]	zS ₃ [S ₂]	0	0	1	0	0	1
		zS _n [S ₂]	0	0	7	0	0	7
double cable	zS[Z]{Z}	zS ₂ [Z ₃]{Z ₂ }	0	0	3	0	0	3
total alternating	zS[Z]{S}	zS _n [Z ₂]{S ₂ }	0	0	1	0	0	1
total non-alternating			10	6	13	0	1	30
total trench			0	0	11	0	0	11
% of total trench			10	6	24	0	1	41
			24	15	59	0	2	100

Table 34.

Cabled cordage and the variety of twist and composition, quantified per material, found in trench BE99-29. (_n>3). For key see table 29.

	TWIST	COMPOSITION	MATERIAL					TOTAL
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
cable	zZ[S]	zZ ₃ [S ₂]	0	0	2	0	0	2
		zZ _n [S ₂]	0	0	1	0	0	1
	sZ[S]	sZ ₂ [S ₂]	4	6	1	12	1	24
		sZ ₂ [S ₃]	0	1	0	1	0	2
		sZ ₂ [S _n]	0	0	1	1	0	2
		sZ _n [S ₂]	0	0	1	0	0	1
		zS[Z]	zS ₂ [Z ₂]	17	9	5	4	1
		zS ₂ [Z ₃]	5	13	0	0	3	21
		zS ₂ [Z _n]	2	0	0	0	0	2
		zS ₃ [Z ₂]	1	1	0	1	0	3
		zS ₃ [Z ₃]	0	20	0	0	0	20
		zS _n [Z ₂]	1	0	10	0	0	11
		zS _n [Z ₃]	0	0	2	0	4	6
		zS _n [Z _n]	0	0	1	0	0	1
	zI[S]	zI _n [S ₂]	0	0	5	0	0	5
		zI _n [S ₃]	0	0	1	0	0	1
	sZ[Z]	sZ ₂ [Z ₂]	0	0	0	2	0	2
		sZ ₃ [Z ₃]	0	0	0	1	0	1
		sZ _n [Z ₂]	0	0	1	0	0	1
	zS[S]	zS ₂ [S ₂]	0	1	0	0	0	1
zS ₂ [S ₃]		0	6	0	0	0	6	
zS _n [S ₂]		0	0	2	0	0	2	
double cable	sZ[S]{Z}	sZ ₂ [S ₂]{Z ₂ }	0	0	0	1	0	1
		sZ ₂ [S ₂]{Z ₃ }	0	0	0	1	0	1
	zS[S]{Z}	zS ₂ [S ₂]{Z ₂ }	1	0	0	0	0	1
		zS ₂ [S _n]{Z ₂ }	3	0	0	0	0	3
		zS _n [S ₂]{Z ₂ }	0	0	1	0	0	1
total alternating		30	50	21	21	9	131	
total non-alternating		4	7	13	3	0	27	
total trench		34	57	34	24	9	158	
% of total trench		22	36	22	15	6	100	

Table 35.

Cabled cordage and the variety of twist and composition, quantified per material, found in trench BE99-31. (>3). For key see table 29.

	TWIST	COMPOSITION	MATERIAL					TOTAL	
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER		
cable	sS[Z]	sS _n [Z ₂]	0	0	3	0	0	3	
	zZ[S]	zZ ₂ [S ₂]	0	0	2	0	0	2	
		zZ _n [S ₂]	0	0	7	0	0	7	
		zZ _n [S ₃]	0	0	1	0	0	1	
		zZ _n [S _n]	0	0	1	0	0	1	
		sZ[S]	sZ ₂ [S ₂]	3	2	0	15	0	20
	sZ ₂ [S ₃]		0	0	0	0	1	1	
	sZ ₂ [S _n]		0	0	2	2	0	4	
	sZ _n [S ₂]		0	1	0	0	0	1	
	zS[Z]	zS ₂ [Z ₂]	13	7	12	0	0	32	
		zS ₂ [Z ₃]	8	4	2	0	0	14	
		zS ₂ [Z _n]	3	3	1	0	0	7	
		zS ₃ [Z ₂]	0	2	2	0	0	4	
		zS ₃ [Z ₃]	3	25	0	0	2	30	
		zS ₃ [Z _n]	0	0	1	0	0	1	
		zS _n [Z ₂]	0	0	22	0	0	22	
		zS _n [Z ₃]	0	0	1	0	0	1	
		zS _n [Z _n]	0	0	1	0	0	1	
		sZ[Z]	sZ _n [Z ₂]	1	0	0	0	0	1
	zS[S]	zS ₂ [S ₂]	2	0	2	0	0	4	
		zS _n [S ₂]	0	0	7	0	0	7	
		zS _n [S ₃]	0	0	3	0	0	3	
	sI[Z]	sI _n [Z ₂]	0	0	1	0	0	1	
	zI[Z]	zIn[Z ₂]	0	0	5	0	0	5	
	zI[S]	zI ₂ [S ₂]	2	0	0	0	0	2	
		zI ₃ [S ₂]	0	0	1	0	0	1	
		zI _n [S ₂]	0	0	8	0	0	8	
	double cable	sZ[S]{Z}	sZ ₂ [S ₂]{Z ₂ }	0	0	0	2	0	2
			sZ ₂ [S _n]{Z ₂ }	0	0	0	1	0	1
		zS[Z]{Z}	zS _n [Z ₂]{Z ₂ }	0	0	1	0	0	1
			zS _n [Z ₂]{Z ₃ }	0	0	2	0	0	2
	total alternating		30	44	44	20	3	141	
total non-alternating		5	0	44	0	0	49		
total trench		35	44	88	20	3	190		
% of total trench		18	23	46	11	2	100		

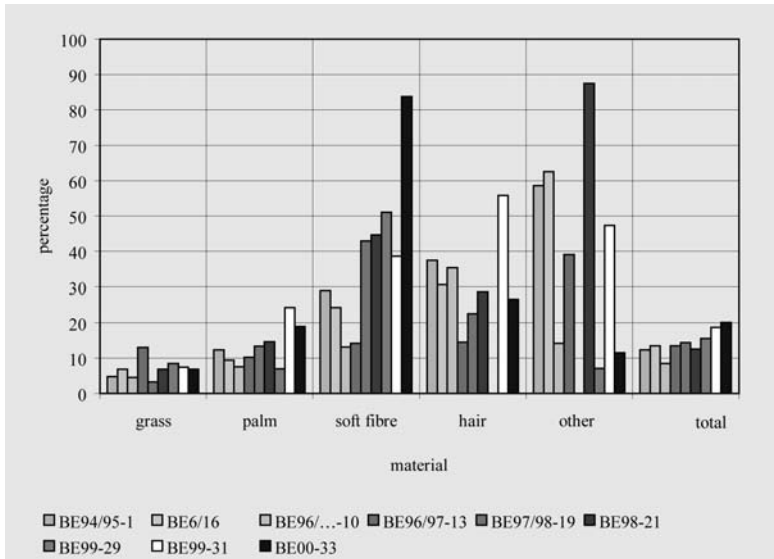
Table 36.

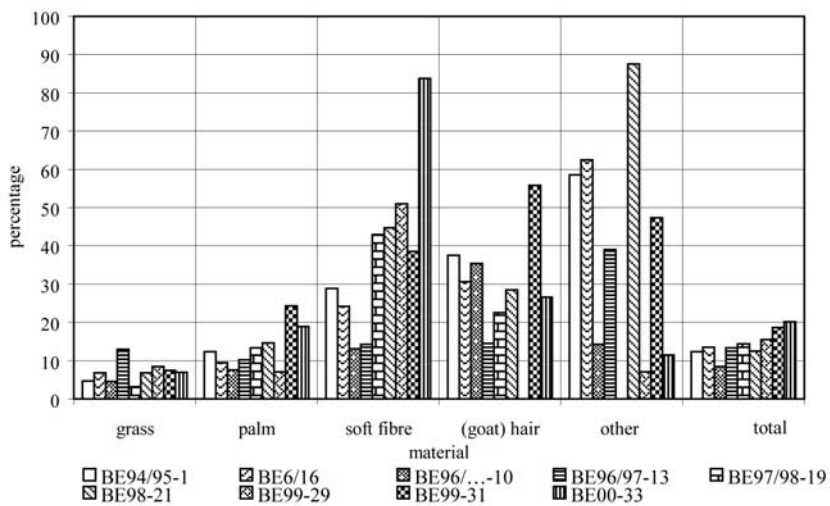
Cabled cordage and the variety of twist and composition, quantified per material, found in trench BE00-33. (_n>3). For key see table 29.

TRENCH	MATERIAL					TOTAL
	GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
BE94/95-1	5	12	29	38	59	12
BE6/16	7	9	24	31	63	14
BE96/...-10	5	8	13	35	14	9
BE96/97-13	13	10	14	15	39	13
BE97/98-19	3	13	43	23	0	14
BE98-21	7	15	45	29	88	13
BE99-29	9	7	51	0	7	16
BE99-31	7	24	39	56	47	19
BE00-33	7	19	84	27	12	20

Table 37 (above) and graph 38 (below).

Occurrence of cabled cordage per trench, in percentages of the total quantity of a certain material and in percentages of the total quantity of cordage. Percentages are calculated from the data of table 1-19 (quantity of cordage per material) and table 29-36. Key: Five percent (**bold** in table 37) of all cordage made of grass found in trench BE94/95-01 was cabled. Twelve percent (**bold** in table 37) of all cordage (i.e. regardless twist, composition and material) found in trench BE94/95-01 was cabled. The graph shows the percentage cabled cordage of a material and of the total quantity (per trench).





	TWIST	COMPOSITION	MATERIAL					TOTAL	
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER		
Cables	sS[Z]	sS ₂ [Z ₂]	0	1	0	0	0	1	
		sS ₂ [Z ₃]	0	0	1	0	0	1	
		sS ₃ [Z ₂]	0	0	1	0	0	1	
	zZ[S]	sS _n [Z ₂]	0	0	5	0	0	5	
		zZ ₂ [S ₂]	0	0	2	0	0	2	
		zZ ₃ [S ₂]	0	0	2	0	0	2	
		zZ _n [S ₂]	0	0	14	0	0	14	
		zZ _n [S ₃]	0	0	1	0	0	1	
		zZ _n [S _n]	0	0	1	0	0	1	
	sZ[S]	sZ ₂ [S ₂]	19	24	26	86	3	158	
		sZ ₂ [S ₃]	0	2	0	3	1	6	
		sZ ₂ [S _n]	0	0	3	9	0	12	
		sZ ₃ [S ₂]	0	1	4	1	0	6	
		sZ ₃ [S ₃]	0	0	5	0	0	5	
		sZ _n [S ₂]	0	0	20	0	0	20	
		sZ _n [S ₃]	0	0	2	0	0	2	
		sZ _n [S _n]	0	0	1	0	0	1	
		zS[Z]	zS ₂ [Z ₂]	102	63	42	5	25	237
			zS ₂ [Z ₃]	40	34	4	1	8	87
	zS ₂ [Z _n]		7	6	1	0	0	14	
	zS ₃ [Z ₂]		8	18	5	0	7	38	
	zS ₃ [Z ₃]		18	65	2	1	4	90	
	zS ₃ [Z _n]		0	0	1	0	0	1	
	zS _n [Z ₂]		1	0	70	0	1	72	
	zS _n [Z ₃]		0	0	6	0	4	10	
	zS _n [Z _n]		0	0	3	0	0	3	
	sZ[Z]		sZ ₂ [Z ₂]	0	2	14	9	2	27
			sZ ₂ [Z ₃]	0	0	0	0	1	1
			sZ ₂ [Z _n]	0	0	4	0	0	4
			sZ ₃ [Z ₂]	0	0	3	0	0	3
		sZ ₃ [Z ₃]	0	0	0	1	0	1	
		sZ ₃ [Z _n]	1	0	5	0	0	6	
		sZ _n [Z ₂]	0	2	14	9	2	27	
	zS[S]	sZ ₂ [Z ₃]	0	0	0	0	1	1	
		sZ ₂ [Z _n]	0	0	3	0	0	3	
		sZ ₃ [Z ₂]	0	0	0	1	0	1	
		sZ ₃ [Z ₃]	0	0	0	0	0	0	
		sZ ₃ [Z _n]	18	8	8	0	0	34	
		zS ₂ [S ₂]	0	6	2	0	1	9	
		zS ₂ [S ₃]	1	0	0	0	0	1	
		zS ₃ [S ₂]	0	1	5	0	0	6	
		zS _n [S ₂]	0	0	22	0	0	22	
		zS _n [S ₃]	0	0	3	0	0	3	
	sI[Z]	sI _n [Z ₂]	0	0	1	0	0	1	
		zI _n [Z ₂]	0	0	5	0	0	5	
	zI[S]	zI ₂ [S ₂]	2	0	0	0	0	2	
		zI ₃ [S ₂]	0	0	1	0	0	1	
zI _n [S ₂]		0	0	13	0	0	13		
zI _n [S ₃]		0	0	1	0	0	1		
zI _n [S _n]		0	0	1	0	0	1		
double cable (A)		6	3	25	19	7	60		
trench BE97/98-19 (B)		14	10	70	9	14	117		
trench BE98-21 (C)		44	42	17	10	0	113		
total alternating		195	213	195	106	53	762		
total non-alternating		22	18	114	10	4	168		
subtotal (= total minus (A), (B), (C))		217	231	309	116	57	930		
total		281	286	421	154	78	1220		
% of total site		23	23	35	13	6	100		

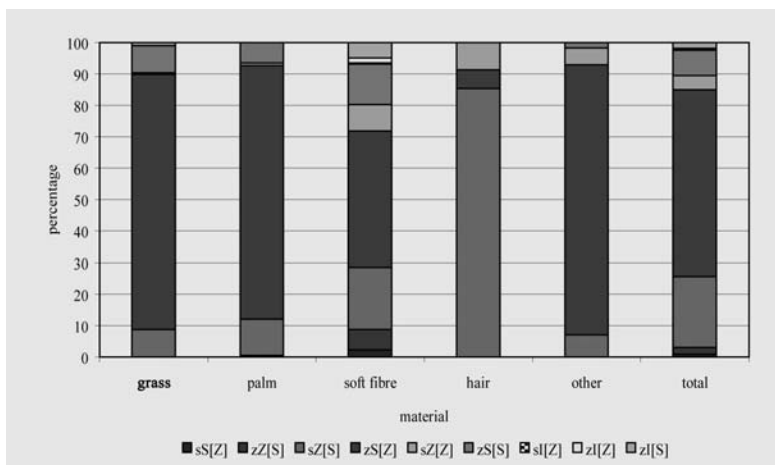


Table 39 (p. 86) and graph 40.

Cabled cordage and the variety of twist and composition, quantified per material, recovered from Berenike. Key: Twenty-three percent (**bold** in table 39) of the total quantity of 1220 pieces of cabled cordage from the site was cabled cordage made of grass. (_n>3).

Graph 40 (based on table 39) shows the distribution of twist and composition per material (of the subtotal).

	TWIST	COMPOSITION	MATERIAL					TOTAL	
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER		
cables	sS[Z]	sS ₂ [Z ₂]	0	1	0	0	0	1	
		sS _n [Z ₂]	0	0	3	0	0	3	
	zZ[S]	zZ ₂ [S ₂]	0	0	2	0	0	2	
		zZ ₃ [S ₂]	0	0	2	0	0	2	
		zZ _n [S ₂]	0	0	12	0	0	12	
		zZ _n [S ₃]	0	0	1	0	0	1	
		zZ _n [S _n]	0	0	1	0	0	1	
	sZ[S]	sZ ₂ [S ₂]	17	16	4	38	1	76	
		sZ ₂ [S ₃]	0	1	0	2	1	4	
		sZ ₂ [S _n]	0	0	3	9	0	12	
		sZ ₃ [S ₂]	0	1	0	0	0	1	
		sZ ₃ [S ₃]	1	0	5	0	0	6	
		sZ _n [S ₂]	0	0	3	0	0	3	
		zS[Z]	zS ₃ [Z ₂]	61	23	33	4	4	125
			zS ₂ [Z ₃]	28	24	3	1	3	59
	zS ₂ [Z _n]		8	3	1	0	1	13	
	zS ₃ [Z ₂]		1	5	7	1	0	14	
	zS ₃ [Z ₃]		17	55	0	0	3	75	
	zS ₃ [Z _n]		0	0	1	0	0	1	
	zS _n [Z ₂]		1	0	52	0	1	54	
	zS _n [Z ₃]		0	0	4	0	4	8	
	zS _n [Z _n]		0	0	4	0	0	4	
	sZ[Z]		sZ ₂ [Z ₂]	0	1	0	4	0	5
		sZ ₂ [Z ₃]	0	0	0	1	0	1	
		sZ ₃ [Z ₂]	0	0	2	0	0	2	
		sZ _n [Z ₂]	1	0	1	0	0	2	
		zS[S]	zS ₂ [S ₂]	4	1	2	0	0	7
	zS ₂ [S ₃]		0	6	1	0	1	8	
	zS ₃ [S ₂]		0	0	4	0	0	4	
	zS ₃ [S ₂]		0	0	24	0	0	24	
	zS _n [S ₃]		0	0	3	0	0	3	
	sI[Z]		sI _n [Z ₂]	0	0	1	0	0	1
			zI[Z]	0	0	5	0	0	5
	zI[S]	zI ₂ [S ₂]	2	0	0	0	0	2	
		zI ₃ [S ₂]	0	0	1	0	0	1	
		zI _n [S ₂]	0	0	13	0	0	13	
		zI _n [S ₃]	0	0	1	0	0	1	
	double cable (A)		4	0	13	12	4	33	
	other (B)		3	7	60	4	1	75	
	total alternating		134	128	120	55	18	455	
	total non-alternating		7	9	79	5	1	101	
	subtotal (= total minus (A), (B))		141	137	199	60	19	556	
total		148	144	272	76	24	664		

Table 41.

Cabled cordage and the variety of twist and composition, quantified per material, recovered from contexts dated to the 1st cad. ($n > 3$). For key see table 29.

	TWIST	COMPOSITION	MATERIAL				TOTAL	
			GRASS	PALM	SOFT FIBRE	(GOAT) HAIR		OTHER
cables	sS[Z]	sS ₃ [Z ₂]	0	0	1	0	0	1
		sS _n [Z ₂]	0	0	2	0	0	2
	zZ[S]	zZ _n [S ₂]	0	0	1	0	0	1
		sZ[S]	3	8	18	39	2	70
	zS[Z]	sZ ₂ [S ₃]	0	1	0	0	0	1
		sZ ₃ [S ₂]	0	0	3	1	0	4
		sZ _n [S ₂]	0	0	13	0	0	13
		sZ _n [S ₃]	0	0	1	1	0	2
		sZ _n [S _n]	0	0	1	0	0	1
		zS ₂ [Z ₂]	36	32	11	0	21	100
		zS ₂ [Z ₃]	17	14	1	0	5	37
		zS ₂ [Z _n]	0	3	0	0	0	3
		zS ₃ [Z ₂]	5	8	1	0	3	17
		zS ₃ [Z ₃]	0	6	2	0	0	8
	sZ[Z]	zS _n [Z ₂]	0	0	15	0	0	15
		sZ ₂ [Z ₂]	0	0	13	2	0	15
		sZ ₂ [Z ₃]	0	0	0	1	0	1
		sZ ₂ [Z _n]	0	0	4	0	0	4
		sZ _n [Z ₂]	0	0	4	0	0	4
	zS[S]	zS ₂ [S ₂]	14	5	4	0	0	23
		zS ₂ [S ₃]	0	1	1	0	0	2
		zS ₂ [S _n]	1	0	0	0	0	1
		zS ₃ [S ₂]	0	1	1	0	0	2
zS _n [S ₂]		0	0	2	0	0	2	
double cable (A)			2	4	12	8	0	26
other (B)			41	38	17	8	13	117
total alternating			61	72	66	41	31	271
total non-alternating			15	7	33	3	0	58
subtotal (= total minus (A), (B))			76	79	99	44	31	329
total			119	121	128	60	44	472

Table 42.

Cabled cordage and the variety of twist and composition, quantified per material, recovered from contexts dated to the 5th-6th cad. ($n > 3$). For key see table 29.

1 cad						
MATERIAL	GRASS (n=148)	PALM (n=144)	SOFT FIBRE (n=272)	(GOAT)HAIR (n=76)	OTHER (n=24)	TOTAL (n=664)
quantity material	(1943) 8	(903) 16	(788) 35	(316) 24	(99) 24	(4049) 16
cables (n=664)	22	22	41	11	4	100

5/6 cad						
MATERIAL	GRASS (n=119)	PALM (n=121)	SOFT FIBRE (n=128)	(GOAT) HAIR (n=60)	OTHER (n=44)	TOTAL (n=472)
quantity material	(2148) 6	(1043) 12	(547) 23	(215) 28	(114) 39	(4067) 12
cables (n=472)	25	26	27	13	9	100

Table 43.

Percentages calculated of the cabled cordage recovered from contexts dated to the 1st cad and to the 5th-6th cad. The table shows the percentage cabled cordage of a material (heading 'material') and of the total quantity of cabled cordage (heading 'cables'). Key: Thirty five percent (**bold**) of the cordage made of soft fibre, dated to the 1st cad, was cabled (272 cables out of 788 pieces) and 41% (**bold**) of the cabled cordage (regardless material) dated to the 1st cad, was made of soft fibre.

TRENCH	MATERIAL					TOTAL (PLY/ CABLE)
	GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER	
	TOTAL (PLY/CABLE)	TOTAL (PLY/CABLE)	TOTAL (PLY/CABLE)	TOTAL (PLY/CABLE)	TOTAL (PLY/CABLE)	
BE94/95-01	31 (5/26)	31 (9/22)	0 (0/0)	0 (0/0)	4 (4/0)	66 (18/48)
BE6/16	11 (5/6)	8 (6/2)	0 (0/0)	1 (0/1)	3 (0/3)	23 (11/12)
BE96/...-10	12 (8/4)	35 (28/7)	0 (0/0)	0 (0/0)	0 (0/0)	47 (36/11)
BE96/97-13	51 (29/22)	22 (14/8)	0 (0/0)	0 (0/0)	4 (0/4)	77 (43/34)
BE97/98-19	8 (7/1)	2 (2/0)	3 (2/1)	0 (0/0)	1 (0/1)	14 (11/3)
BE98-21	18 (2/16)	12 (4/8)	0 (0/0)	3 (0/3)	1 (0/1)	34 (6/28)
BE99-29	27 (17/10)	14 (12/2)	0 (0/0)	0 (0/0)	3 (2/1)	44 (31/13)
BE99-31	40 (26/14)	53 (24/29)	1 (0/1)	1 (0/1)	9 (2/7)	104 (52/52)
BE00-33	33 (28/5)	44 (23/21)	2 (0/2)	0 (0/0)	6 (4/2)	85 (55/30)
resttrenches	4 (4/0)	5 (0/5)	0 (0/0)	0 (0/0)	3 (2/1)	12 (6/6)
total	235 (131/104)	226 (122/104)	6 (2/4)	5 (0/5)	34 (14/20)	506 (269/237)

Table 44.

Plied and cabled ropes, quantified per material and trench.

TRENCH	MATERIAL					TOTAL
	grass	palm	soft fibre	(goat) hair	other	
BE94/95-01	3	6	0	0	9	4
BE6/16	4	6	0	2	38	4
BE96/...-10	3	14	0	0	0	5
BE96/97-13	12	9	0	0	17	6
BE97/98-19	2	3	2	0	33	2
BE98-21	3	4	0	9	6	3
BE99-29	23	17	0	0	21	17
BE99-31	9	23	1	2	47	12
BE00-33	7	19	2	0	8	9
resttrenches	6	5	0	0	5	3
total site	5	11	0	1	14	6

Table 45.

Percentage ropes per trench (of the material and of the total quantity of the trench, regardless material). The total quantity of the site is shown with 'total site'. The percentages were calculated from the quantities presented in table 44. Key: Three percent (**bold**) of all grass cordage recovered from trench BE94/95-01 was rope and 4% (**bold**) of all cordage from trench BE94/95-01. Furthermore, 11% (**bold**) of all excavated palm cordage had a diameter of 10 mm or more. Six (**bold**) percent of all recovered cordage was rope.

	GRASS	PALM	SOFT FIBRE	(GOAT) HAIR	OTHER		
	total (ply/cable)	total (ply/cable)	total (ply/cable)	total (ply/cable)	total (ply/cable)	total (ply/cable)	
1 cad	quantity	165	131	6	1	22	325
	rope	(113/52)	(71/60)	(2/4)	(0/1)	(8/14)	(194/131)
	n=total (ply/cable)	1856 (1708/148)	841 (697/144)	598 (326/272)	305 (229/76)	94 (70/24)	3694 (3030/664)
	percentages of 'n'	9 (7/35)	16 (10/42)	1 (1/2)	0 (0/1)	23 (11/58)	9 (6/20)
5/6 cad	quantity	62 (16/46)	76 (40/36)	1 (1/0)	4 (0/4)	8 (4/4)	151 (61/90)
	n=total (ply/cable)	2112 (1993/119)	977 (856/121)	357 (229/128)	198 (138/60)	103 (59/44)	3747 (3275/472)
	percentages of 'n'	3 (1/39)	8 (5/30)	0 (0/0)	2 (0/7)	8 (7/9)	4 (2/19)

Table 46.

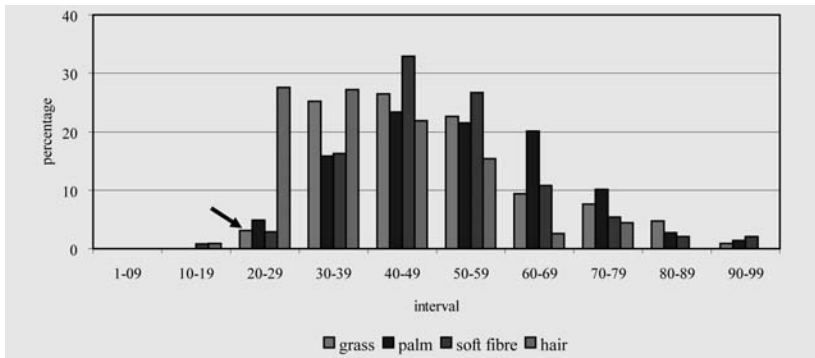
The distribution of ropes among contexts dated to the 1st cad and 5th-6th cad (quantified per plied and cabled ropes, per material and in percentages of the total quantity of plies and cabled cordage of a certain material). The totals are included as well. Key: Seven percent (**bold**) of the total quantity of grass plied cordage (1708) and 35% (**bold**) of the total quantity of grass cabled cordage (148) that were recovered from 1st cad contexts were ropes. Nine percent (**bold**) of the cordage, plied as well as cabled, had a diameter of 10 mm or more (i.e. were ropes).

		INTERVAL										C	D	E
MATERIAL		1-09	10-19	20-29	30-39	40-49	50-59	60-69	70-79	80-89	90-99	total calculated CIP	total plies and cables	% CIP of total plies and cables
quantity	grass	0	0	85	680	715	610	255	204	127	23	2699	4232	64
	palm	0	0	56	180	266	244	229	115	31	16	1137	2003	57
	soft fibre	0	2	7	39	79	64	26	13	5	5	240	1026	23
	(goat) hair	0	2	63	62	50	35	6	10	0	0	228	557	41
	other	0	0	6	6	25	11	20	3	8	8	87	227	38
	total	0	4	217	967	1135	964	536	345	171	52	4391	8045	55
percentage	grass (n=2699)	0	0	3	25	27	23	9	8	5	1	100		
	palm (n=1137)	0	0	5	16	23	22	20	10	3	1	100		
	soft fibre (n=240)	0	1	3	16	33	27	11	5	2	2	100		
	(goat) hair (n=228)	0	1	28	27	22	15	3	4	0	0	100		
	other (n=87)	0	0	7	7	29	13	23	3	9	9	100		
	total (n=4391)	0	0	5	22	26	22	12	8	4	1	100		

Table 47 (above) and graph 48 (below).

CIP values of plied and cabled cordage, quantified per material, and the distribution over the intervals in percentages (section B) of the cordage from Berenike to show the pattern of CIP values per material through time. Section C shows the total quantity of cordage of which CIP values were calculated. Section D shows the total quantity of plies and cables. Section E shows the percentage calculated CIP's of the total quantity of plies and cables per material and of the total regardless the material (the row heading 'total'). The data of section B and C of table 47 (quantity) were used to calculate the percentage in graph 48 (percentage). For key see text graph 48.

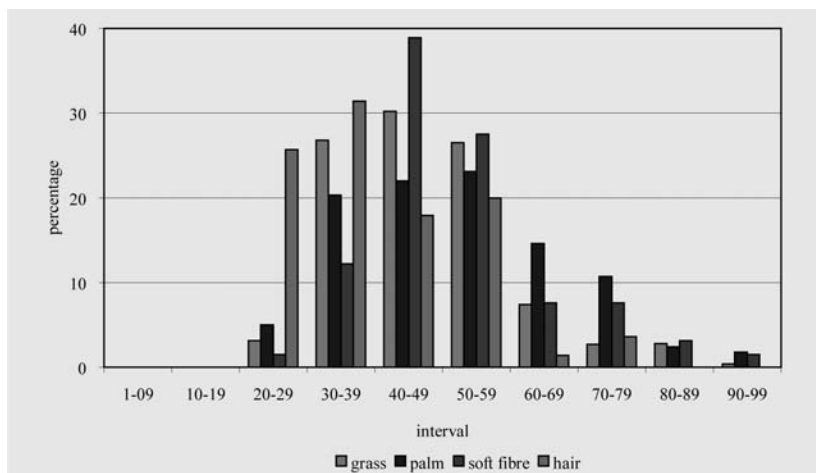
Graph 48 (below), graph of table 47 (section B). CIP values and their percentages (per interval) of the total quantity of calculated CIP's per material to show the pattern of CIP values per material. Cordage made of a different material (table 47: 'other') were left out. Key: Three percent (bold in table 47 and arrow in the graph) of the grass cordage of which CIP values were calculated showed values falling in the interval representing the CIP values 20-29 (85 pieces [**bold** in table 47] made of grass out of 2699 CIP calculated grass cordage [**bold**, section C]).



		A		B										C	D	E
		MATERIAL										total calculated CIP	total plies and cables	% CIP of total plies and cables		
		1-09	10-19	20-29	30-39	40-49	50-59	60-69	70-79	80-89	90-99					
quantity	grass	0	0	39	336	378	332	93	34	35	5	1252	1864	67		
	palm	0	0	27	110	119	125	79	58	13	10	541	842	64		
	soft fibre	0	0	2	16	51	36	10	10	4	2	131	601	22		
	(goat) hair	0	0	36	44	25	28	2	5	0	0	140	305	46		
	other	0	0	0	5	10	7	14	0	7	0	43	94	46		
	total	0	0	104	511	583	528	198	107	59	17	2107	3706	57		
percentage	grass (n=1252)	0	0	3	27	30	27	7	3	3	0	100				
	palm (n=541)	0	0	5	20	22	23	15	11	2	2	100				
	soft fibre (n=131)	0	0	2	12	39	28	8	8	3	2	100				
	(goat) hair (n=140)	0	0	26	31	18	20	1	4	0	0	100				
	other (n=43)	0	0	0	12	23	16	33	0	16	0	100				
	total (n=2107)	0	0	5	24	28	25	9	5	3	1	100				

Table 49 (above) and graph 50 (below).

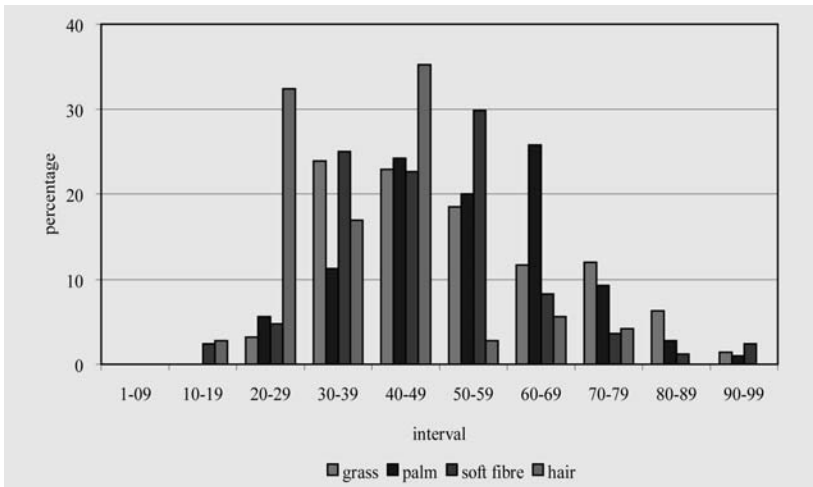
CIP values of plied and cabled cordage, quantified per material, and the distribution over the intervals (section B) of cordage from context, dated to the 1st cad, to show the pattern of CIP values per material through time. Section C shows the total quantity of cordage of which CIP values were calculated; section D shows the total quantity of plies and cables. The last section shows the percentage calculated CIP's of the total quantity of plies and cables per material and of the total regardless the material. The data of the second and third section of 'quantity' were used to calculate the percentages. For key see graph 48. Graph 50, graph of table 49. For key see graph 48. Cordage made of a different material ('other') were left out.



		B										C	D	E
		INTERVAL										total calculated CIP	total plies and cables	% CIP of total plies and cables
MATERIAL		1-09	10-19	20-29	30-39	40-49	50-59	60-69	70-79	80-89	90-99			
quantity	grass	0	0	43	314	301	243	153	158	82	18	1312	2113	62
	palm	0	0	28	56	120	99	128	46	14	5	496	977	51
	soft fibre	0	2	4	21	19	25	7	3	1	2	84	357	24
	(goat) hair	0	2	23	12	25	2	4	3	0	0	71	198	36
	other	0	0	6	1	13	2	2	2	0	3	29	105	28
	total	0	4	104	404	478	371	294	212	97	28	1992	3750	53
percentage	grass (n=1312)	0	0	3	24	23	19	12	12	6	1	100		
	palm (n=496)	0	0	6	11	24	20	26	9	3	1	100		
	soft fibre (n=84)	0	2	5	25	23	30	8	4	1	2	100		
	(goat) hair (n=71)	0	3	32	17	35	3	6	4	0	0	100		
	other (n=29)	0	0	21	3	45	7	7	7	0	10	100		
	total (n=1992)	0	0	5	20	24	19	15	11	5	1	100		

Table 51 (above) and graph 52 (below).

CIP values of plied and cabled cordage, quantified per material, and the distribution over the intervals (section B) of cordage from contexts that were dated to the 5th-6th cad to show the pattern of CIP values per material through time. Section C shows the total quantity of cordage of which CIP values were calculated; section D shows the total quantity of plies and cables. The last section shows the percentage calculated CIP's of the total quantity of plies and cables per material and of the total regardless the material. The data of the second and third section of 'quantity' were used to calculate the percentages. For key see graph 48. Graph 52, graph of table 51. For key see graph 48. Cordage made of a different material ('other') were left out.

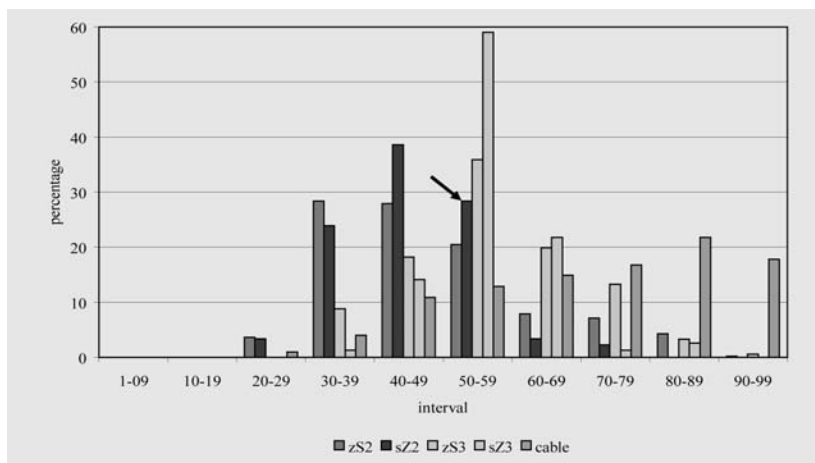


COMPOSITION		INTERVAL										TOTAL
		1-09	10-19	20-29	30-39	40-49	50-59	60-69	70-79	80-89	90-99	
quantity	zS ₂	0	0	81	638	626	460	177	160	97	4	2243
	sZ ₂	0	0	3	21	34	25	3	2	0	0	88
	zS ₃	0	0	0	16	33	65	36	24	6	1	181
	sZ ₃	0	0	0	1	11	46	17	1	2	0	78
	cable	0	0	1	4	11	13	15	17	22	18	101
	other	0	0	0	0	0	1	7	0	0	0	8
	total	0	0	85	680	715	610	255	204	127	23	2699
percentage	zS ₂	0	0	4	28	28	21	8	7	4	0	100
	sZ ₂	0	0	3	24	39	28	3	2	0	0	100
	zS ₃	0	0	0	9	18	36	20	13	3	1	100
	sZ ₃	0	0	0	1	14	59	22	1	3	0	100
	cable	0	0	1	4	11	13	15	17	22	18	100
	other	0	0	0	0	0	13	88	0	0	0	100
	total	0	0	3	25	27	23	9	8	5	1	100

Table 53 (above) and graph 54 (below).

Grass plied and cabled cordage from Berenike and the occurrence of CIP values, quantified per composition and interval. The data of table 53, section ‘quantity’ were used to calculate the percentages. Graph 54 is based on these percentages. For key see graph 54.

Graph 54, graph of table 53. Key: Twenty-eight percent (arrow in the graph and **bold** in table 53, section ‘percentages’) of the sZ₂ cordage made of grass, had a CIP value between 50 and 59. Cordage with a different composition (table 53: ‘other’) were left out.

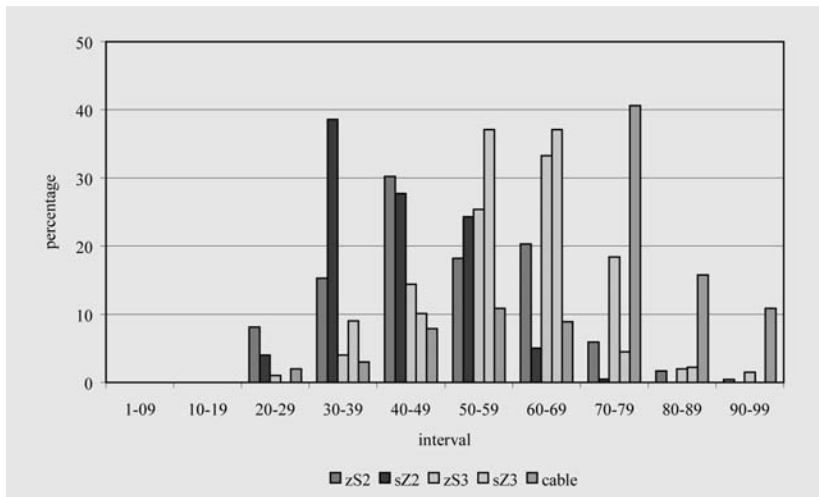


COMPOSITION		INTERVAL									TOTAL	
		1-09	10-19	20-29	30-39	40-49	50-59	60-69	70-79	80-89		90-99
quantity	zS ₂	0	0	44	83	164	99	110	32	9	2	543
	sZ ₂	0	0	8	78	56	49	10	1	0	0	202
	zS ₃	0	0	2	8	29	51	67	37	4	3	201
	sZ ₃	0	0	0	8	9	33	33	4	2	0	89
	cable	0	0	2	3	8	11	9	41	16	11	101
	other	0	0	0	0	0	1	0	0	0	0	1
	total	0	0	56	180	266	244	229	115	31	16	1137
percentage	zS ₂	0	0	8	15	30	18	20	6	2	0	100
	sZ ₂	0	0	4	39	28	24	5	1	0	0	100
	zS ₃	0	0	1	4	14	25	33	18	2	2	100
	sZ ₃	0	0	0	9	10	37	37	5	2	0	100
	cable	0	0	2	3	8	11	9	41	16	11	100
	other	0	0	0	0	0	100	0	0	0	0	100
	total	0	0	5	16	23	22	20	10	3	1	100

Table 55 (above) and graph 56 (below).

Palm plied and cabled cordage and the occurrence of CIP values, quantified per composition and interval. The data of table 55, section 'quantity' were used to calculate the percentages. Graph 56 is based on these percentages. For key see graph 54.

Graph 56, graph of table 55. For key see graph 54. Cordage with a different composition ('other') were left out.

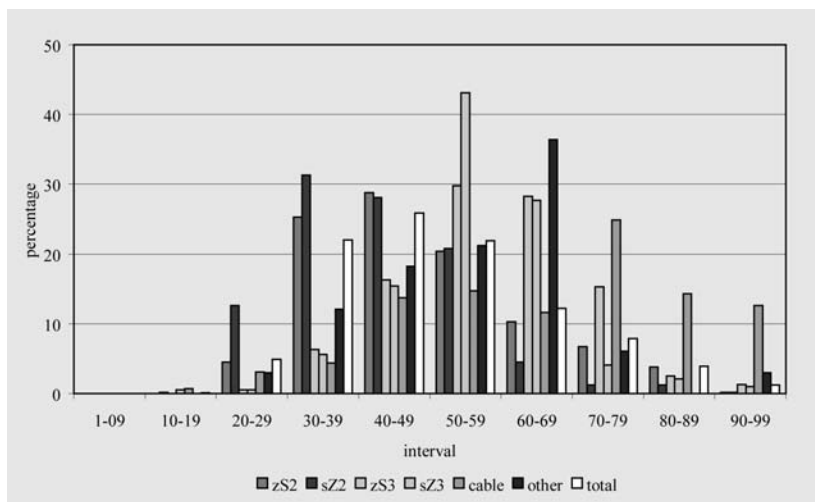


COMPOSITION		INTERVAL									TOTAL	
		1-09	10-19	20-29	30-39	40-49	50-59	60-69	70-79	80-89		90-99
quantity	zS ₂	0	0	129	728	828	586	296	194	108	6	2875
	sZ ₂	0	1	75	186	167	124	27	7	7	1	595
	zS ₃	0	0	2	25	65	119	113	61	10	5	400
	sZ ₃	0	1	1	11	30	84	54	8	4	2	195
	cable	0	2	9	13	40	43	34	73	42	37	293
	other	0	0	1	4	6	7	12	2	0	1	33
	total	0	4	217	967	1136	963	536	345	171	52	4391
percentage	zS ₂	0	0	5	25	29	20	10	7	4	0	100
	sZ ₂	0	0	13	31	28	21	5	1	1	0	100
	zS ₃	0	0	1	6	16	30	28	15	3	1	100
	sZ ₃	0	1	1	6	15	43	28	4	2	1	100
	cable	0	1	3	4	14	15	12	25	14	13	100
	other	0	0	3	12	18	21	36	6	0	3	100
	total	0	0	5	22	26	22	12	8	4	1	100

Table 57 (above) and graph 58 (below).

Plied and cabled cordage from Berenike, regardless the material from which it was made, and the occurrence of CIP values, quantified per composition and interval. The data of table 57, section ‘quantity’ were used to calculate the percentages. Graph 58 is based on these percentages. For key see graph 54.

Graph 58, graph of table 57. For key see graph 54.



A REEVALUATION OF THE USE OF זבן AND יהב IN ELEPHANTINE*

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Abstract: A Reevaluation of the Use of זבן and יהב in Elephantine

The legal terminology of the Aramaic papyri from Elephantine has been studied mostly from an Assyriological perspective. This article takes a fresh look at the function, origins of two legal terms (זבן and יהב) used in Elephantine taking also into consideration the Egyptian evidence. We trace their origin within ancient Near Eastern legal traditions paying especial attention to the way the Egyptian formulaic tradition might have influenced the Aramaic use of those terms.

Keywords: Aramaic – Demotic – Elephantine – legal formulae

Resumen: Una reevaluación del uso de זבן y יהב en Elefantina

La terminología legal de los papiros arameos de Elefantina ha sido estudiada en su mayor parte desde una perspectiva asiriológica. Este artículo propone una nueva lectura de la función y orígenes de dos términos legales (זבן y יהב) utilizados en Elefantina, tomando en consideración también la evidencia egipcia. Nosotros trazamos su origen en las tradiciones legales del Cercano Oriente Antiguo prestando una atención especial al modo en que la tradición de fórmulas egipcias pudo haber influenciado el uso arameo de aquellos términos.

Palabras Clave: arameo – demótico – Elefantina – fórmulas legales

The verb זבן, whose root denotes both the act of purchasing and the act of selling,¹ is attested in Elephantine in the same document² as Peal בכסף “bought for silver,” (*TAD* B3.12:4), referring to the acquisition of the house by the seller; and in Pael חזבן לה בכסף, “sell it for silver,” (*TAD* B3.12:24) referring to the buyer’s right of alienation.³ Thus, the single root זבן in the

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¹ *DNWSI*, s.v. *zbn*₂; Muraoka-Porten 1998: §27.

² And also in *TAD* B3.10:3 in the same expression.

³ Also in *XHve/Se* 8a:5 and 9:5.

Aramaic documents fulfills the same function as the two roots *קנה* and *נכר* in the Hebrew Bible.

The etymology of the Aramaic term goes back to the Assyrian word *zibanitu* meaning “scales;”⁴ this sheds light on the possible original *Sitz im Leben* of the Aramaic term. The presence of scales to weigh the metal used in the transaction was a necessary element before the use of coins, as illustrated in Jer. 32:9:

“So I bought the land in Anathoth from my cousin Hanamel. I weighed out the money to him, seventeen shekels of silver.” (JPS).

וַאֲקַנְהָ אֶת־הַשְּׂדֵה מֵאֵת חֲנַמְאֵל בְּיַד־יָדָי אֲשֶׁר בַּעֲנֻתוֹת וְאֲשַׁקֵּל הִלּוֹ
אֶת־הַפְּסָף שִׁבְעָה שָׁקָל יָם וְעִשְׂרֵה הַפְּסָף:

The symbolism was still present in the Roman *mancipatio*, a form of conveyance in Roman law, whereby a scale continued to be used even in times when coins were circulating.⁵ In the Aramaic documents from Egypt, the use of *יבן* implied that a payment is always involved in the transaction.

יהב, on the other hand, is the term for transfer. The use of the verb “to give” in conveyances, gifts, and other types of deliveries is attested throughout the ancient Near East: Akkadian *nadanu*,⁶ Hebrew *נתן*,⁷ Aramaic *יהב* and *נתן*,⁸ Ugaritic *ym*,⁹ and Egyptian *rdi*.¹⁰ A thorough study of the uses of these verbs in every case is beyond the scope of this study, but some general understanding of the uses in the Aramaic realm is essential for our discussion of the use of *יהב* in the Elephantine documents.

The Aramaic corpus attests the use of two verbs with the meaning “to give,” *נתן* and *יהב*. Folmer’s linguistic study of the use of *נתן* and *יהב* in Elephantine has provided an explanation for their distribution.¹¹ While in the oldest legal documents (*TAD* B1.1 [515 BCE]; B5.1 [495 BCE], B4.2 [c. 487 BCE]) both *נתן* and *יהב* were used in perfect, their paradigms are complementary in later

⁴ See *HALOT* s.v. *יבן*; *CAD* s.v. *zibanitu*; *AHW* s.v. *zibanitu(m)*, vol. 3, 1523. The word is also related to Egyptian *dbn* and Akkadian *zbn* “weight.” Kaufman 1974: 112.

⁵ Kaser 1971: vol. I, pp. 44-45.

⁶ *CAD* N, part 1, 41-59.

⁷ E. Lipinski, “נתן,” in *TDOT* vol. 10, 90-108; esp. 96-101; *HALOT*, vol. 2, 733-735.

⁸ *DNWSI*, II, 766-770.

⁹ Aistleitner 1965: 138-140; del Olmo Lete and Sanmartín 1996-2000: vol. I s.v. *ym*.

¹⁰ *Wb*, II, 464-469.

¹¹ Folmer 1995: 641-648.

documents, (יָהַב is used in perfect, imperative and participle while נָתַן is used in imperfect and infinitive).¹² Accordingly, their distribution in the Elephantine-Syene legal formula shows that the Jewish and Aramean scribes used נָתַן extensively in other formulae,¹³ but out of sixteen attestations of the verb “to give” in the transfer clause (perfect), fifteen are composed by יָהַב¹⁴ and the only one by נָתַן belongs to the aforementioned group of earlier documents.¹⁵ On the other hand, נָתַן is the verb exclusively used when referring to the penalty to be paid (imperfect)¹⁶ and in the defension clause “to cleanse and give” (imperfect).¹⁷ There seems to be no legal or semantic difference in the various uses of יָהַב and נָתַן. In *TAD* B3.13, a loan of grain dated December 402 BCE, both are used in the same formula “to pay and give,” נָתַן in line 4 and יָהַב in line 5. It seems, therefore, that the scribal preference for the different uses of יָהַב and נָתַן was based on the temporal character of the action, as noted above.

Muffs interpreted the formula זָבַן וַיָּהַב “he sold and transferred,” as an example of hendiadys construction.¹⁸ Two examples from the Nabatean realm, however, show that “to give” (נָתַן) was something different from “to sell” (זָבַן). In the following example, “to sell,” “to lease,” and “to give,” are three different options for alienating the property.

wylʿn GN mn yzbn kprʿ dnh ʕw mn yzbn ʕw yrhm ʕw yntn

4-5. “May GN curse whoever would buy this grave or whoever would sell, lease or give it away.” (Doughty, Text II. 2 CE at Medain Salih).¹⁹

¹² Folmer 1995: 641.

¹³ See *infra*.

¹⁴ *TAD* B2.1:3; B2.3:3; B2.4:3; B2.7.2; B3.4:3; B3.5:2; [B3.7:3]; B3.10:2; B3.11.2; B3.12:3; B3.13:3; [B4.3:3]; B4.4:3; B5.1:2; B5.5:3.

¹⁵ *TAD* B4.2:1, a loan of silver written by the borrower Gemariah b. Ahio ca. 487 BCE.

¹⁶ *TAD* B2.1:7, 10, 13; B2.2:14; B2.3:13, 21; B2.4:15; B2.5:2; B2.6:30, 34, 36; B2.7:11; B2.8:10; B2.9:14; B2.10:15; B2.11:10; B3.3:8, 10, 14; B3.4:15, 18, 21, 22; B3.5:16, 21; B3.6:8; [B3.7:16]; B3.8:22, 24, 26, 31; B3.9:7; B3.10:20; B3.13:1; B3.11:13; B3.12:30; B5.1:6; [B5.3:5]; B5.4:6; B5.5:6; [11]; B6.3:9.

¹⁷ *TAD* B3.2:9; 3.4:20.

¹⁸ Muffs 1969: 34 n. 1.

¹⁹ Levinson 1974: 73. יָהַב is not attested in the Naḥal Ḥever documents and נָתַן only in one dubious case XHve/Se 26:4, a text dealing with deposits and barley.

In the Nabatean realm, “to give,” is used for gifts as the following inscription shows.

wyhb kpr^ʔ dnh l PN ʔntth brt PN mn zmn šṭr mwhbt^ʔ dy bydh dy t^ʕbd kl dy tšb^ʔ

“And he gave this grave to PN, his wife, the daughter of PN from the date of the document of gift which is in her hand, she can do all that she wants (with the grave) (CIS 204).”²⁰

In the Nabatean realm, therefore, יהב is associated with gifts and זבן with sales. It is interesting that the Samaria Papyri use נהן in the defension clause in the formula “stand up, clear and give,”²¹ and in the penalty clause “I will pay and give,”²² two formulae attested in Elephantine, but not in the sale or transfer clauses where the Samaria papyri use only זבן.²³

The verb יהב, like its Egyptian counterpart *rdi*, does not imply that the object is physically transferred from the alienee to the alienor but rather connotes the right of possession. Muffs²⁴ interpreted the formula זבן ויהב, “he sold and transferred,” as similar in function to the Middle-Assyrian *iddinma ušappi* “he effected a final sale” (*iddin* = זבן and *ušappi* = יהב)²⁵ as, for example, in the expression: *eqla ana šim gamir ana x manē anāka A ana B iddinma ušappīma eqlu uppū laqi*, “A field at full price for x minas of tin, A sold permanently to B. The field is permanently taken.”²⁶

²⁰ Levinson 1974, “Nabatean,” 111. See also *mwhb^ʔ* “gift” CIS 219:5; RES 1108:7.

²¹ WDSP 1:6; 2:6; 3:6; 4:8; 5:8; 6:6; 7:10; 8:8; 9:9.

²² WDSP 1:9; 2:8; 4:11; 5:11; 6:9; 7:13; 9:11.

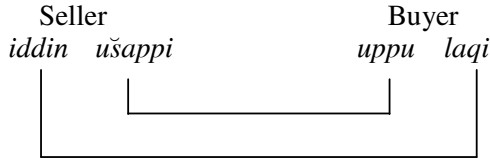
²³ WDSP 1:2; 2:1; 3:1; 4:2; 5:2; 6:2; 7:1; 8:1; 9:1.

²⁴ Muffs 1969: 34 n. 1. Muffs interpreted two verbs together as hendiadys (e.g. *iddin ušappi*), and referring to a single process. We do not regard זבן ויהב as hendiadys but understand the legal meaning of each verb separately. Postgate 1997: 162.

²⁵ Muffs 1969: 34, n.1.

²⁶ Ebeling 1927: 149; 1933: 54-89.

P. Koschaker discussed both terms, *iddin* and *ušappi*, in their relationship with *uppu* and *laqi* in the Middle Assyrian sales documents.²⁷ According to Koschaker, such a relationship, as in the example cited above by Muffs, can be described as follows:



The Middle Assyrian deeds of sale are formulated *ex latere venditoris*. The seller “gives and *ušappi*” while the buyer “takes and *uppu*.” While there is no doubt about the meaning of the verbs *nadanu* and *lequ*, the translation of *ušappi* and *uppu* still presents some difficulties.²⁸ According to J. Postgate, “if *uppu* is correctly derived from *wapu*, D, it probably means ‘publicized’ (by means of herald’s announcement or other formalities needed to precede a legally valid sale).”²⁹ *Ušappi* would belong, therefore, to the Š stem of the same root *wapû*,³⁰ and the whole expression *iddinma ušappîma eqlu uppû laqi* should be translated “he produced a sale, the field is publicly taken.”

The differences between the Aramaic and Middle Assyrian formulation are evident. Not only does the Aramaic not include anything similar to *uppu laqi*, but the semantic fields of זָבַן and *ušappi* are completely different, the only element in common being יָהַב and *nadanu*, both meaning “to give.”

Cussini has sought a closer parallel to the Aramaic formula is found in the Neo-Assyrian documents.³¹ Contrary to the Middle Assyrian practice, the Neo-Assyrian sales are generally formulated *ex latere emptoris*, but there are cases where the sale is formulated *ex latere venditoris*.³² According to Cussini, the Aramaic formulae would derive from the Neo-Assyrian *uppišma*

²⁷ See the discussion by Koschaker 1928: 28-29.

²⁸ Koschaker 1928: 29, after briefly discussing the possible root of *ušappi* and *uppu* concludes “Da nun *laqi*, wie ausgeführt, wahrscheinlich auf die Besitz-ergreifung geht, so bleibt für *uppu* nur die Beziehung auf den Eigentums-erwerb und wir werden daher den juristischem Sinn der beiden Verba wohl ungefähr treffen, wenn wir *ušappi* – *uppu* übersetzen: (der Verkäufer) hat übereignet – (das Feld) ist übereignet.”

²⁹ Postgate 1997: 162.

³⁰ *AHw* 3, 1459 s.v. (*w*)*apû(m)* gives for D “sichtbar mache, vorzeigen,” and for Š “deutlich machen, verwirklichen, hervorbringen, verherrlichen.”

³¹ Cussini 1992: 170.

³² See the examples listed by Postgate who notes that “as late as the early 8th century the formula could still be phrased from the point of view of the seller.” Postgate 1976: 13.

PN₁ (Seller) ... *ana* PN₂ (purchaser) *iddin*, “seller contracted... and gave to purchaser.”³³ This formula is one of the four variations of the Neo-Assyrian operative section listed by Postgate.³⁴ When trying to understand the precise meaning of the Neo-Assyrian formula, however, we face the same difficulties regarding one of its components as we did with the Middle-Assyrian formula. In this case, at least, there is agreement that *uppīšma* is the D stem from *epēšu* of which the basic meanings according to *CAD* are “to act,” “to be active,” and “to proceed.”³⁵

Considering that either the buyer or seller may be the subject of *uppīšma*, Johns explains the term as meaning “to make a bargain” or “come to terms.”³⁶ In the same direction, Postgate considers the *CAD* rendering of *epēšu*, “to conclude a sale agreement,”³⁷ though non-committal, the safest way to translate it, thus retaining the full reservoir of meaning.³⁸ *Uppīšma* has also been translated: “erwerben,” “to enter into a legal contract.”³⁹ in the formula *uppīšma* PN₁ (Seller) ... *ana* PN₂ (purchaser) *iddin*.⁴⁰

In the latest discussion on *epēšu*, K. Radner⁴¹ correctly criticizes the translation, “to conclude a sale agreement,” based on the appearance of *uppīšma* in documents other than sales, like pledges and leases. She also rejects the translation “erwerben,” since *epēšu* is attested with both the buyer and the seller as subjects. Radner proposes to translate the D stem of *epēšu*, “als zu gebendes Object behandeln.”⁴²

Returning to Cussini’s proposal to understand the Aramaic formula זבן ויהב as a derivation of the Neo-Assyrian *uppīšma* PN₁ (Seller) ... *ana* PN₂ (purchaser) *iddin*, we raise the same objection as we did with the Middle Assyrian equivalent. Both formulae share the use of “to give,” *nadanu* and יהב, but the semantic fields of *uppīšma* and זבן are not related.⁴³ Nevertheless, Cussini concludes that “the Aramaic formula maintains the double verb formulation and introduces זבן as a functional equivalent of Neo-Assyrian *uppūšu*. As far

³³ Cussini 1992: 73. “To contract” is also how Kwasman 1988: 1:7; 2:7; 3:13; 4:5; 5:1 *passim*; and Kwasman and Parpola 1991: 295 translate *uppīšma*. For other interpretations of *uppīšma*, see *infra*.

³⁴ Postgate 1976: 13-14, no.3. The other three being: 1. *uppīšma* PN₂ *issu pān* PN₁ *ilqui*; 2. PN₂ ... *issu pān utappīš issiqi* and 4. PN₁ *ana* PN₂ *utappīš ittīdīn*.

³⁵ *CAD* E, 191 s.v. *epēšu*. The whole article on *epēšu* covers pp. 191-235.

³⁶ Johns 1901: 296.

³⁷ *CAD* E, 231.

³⁸ Postgate 1976: 14.

³⁹ Postgate 1973: 29.

⁴⁰ Kohler and Ungnad 1913, supported by Koschaker 1928: 29; see also *AHw* 227 s.v. *epēšu*.

⁴¹ Radner 1997: 341.

⁴² Radner 1997: 342.

⁴³ See *infra*.

as the sale clause, the Elephantine sale contracts show continuity with Neo-Assyrian language.” The presence of functional equivalents, however, is not proof of linguistic dependence. We can expect that almost every culture will have a functional equivalent for such a basic component of the social life as “to sell,” but no dependence can be proven on this basis alone.

The use of זבן, with its semantic field of “weight” and “scales” by the Arameans, however, points to a clear semantic differentiation from the language of the Neo-Assyrian sale. In conclusion, neither the proposed Middle Assyrian nor the Neo-Assyrian formulae can be considered the origin of the Aramaic formula.

In the search for antecedents of the Aramaic formula in other languages, one can easily forget to consider that the formula might be originally Aramean with no debt to other legal traditions. The first component of the formula, the legal term זבן, is widely attested in the Aramaic realm.⁴⁴ In the Naḥal Ḥever contracts, זבן is attested in Peal active with the same meaning, “to buy”⁴⁵ and in active participle זבנה, meaning “purchasers.”⁴⁶ In the Murabbat documents it is attested in Pael זבנה לך בכסף (132 BCE),⁴⁷ and, also, in Pael active participle מזבנה, meaning “seller.”⁴⁸ The geographic distribution of זבן shows that the term was widely used in Judah and Samaria in the sale documents written in Aramaic.⁴⁹

The construction זבן ויהב, “I sold and gave,” is an organic complex of legal and linguistic elements to be found only in the Aramaic documents from Elephantine. It is safe to assume that for every element in the Aramaic formulary that cannot be accounted for in any other Aramaic context apart from Elephantine, we are experiencing the result of the interaction of the Aramaic formulary with a different legal tradition. This other tradition must not necessarily be the Egyptian legal tradition. We should not discard a Jewish –Judahite– influence on the Aramaic formulary.⁵⁰ The fact that the Egyptian *rdi*

⁴⁴ It is attested in Official Aramaic, Nabatean, Palmyrene, Hatran and Jewish Aramaic. See *DNWSI* s.v. זבן, p. 303-4.

⁴⁵ XHve/Se 8:3.

⁴⁶ XHve/Se 9:3, 4, 7, 16; 21:7; 50:6, 12, 23.

⁴⁷ P. Mur 23:5.

⁴⁸ P. Mur 25:6.

⁴⁹ זבן is also the term for “to sell,” and “to buy” in the Syriac slave’s sale from Dura-Europos (243 CE). See Goldstein 1966: 2, and the previous studies cited there.

⁵⁰ נתן, a synonym of ייהב, plays an important role as a legal term for transfer in the Hebrew Bible. The root נתן is attested 2066 times in the Hebrew Bible (2063 in Hebrew, 3 in Aramaic). For the legal meaning of נתן as donation see Genesis 13:15; 15:17; 24:7; as authority Genesis 39:4; 41:1; as exchange Genesis 47:16. The report of the purchase of the cave of Machpelah by Abraham as a burial place for Sarah (Genesis 23:1-20) is a good example of its use in the context of a sale. See Sarna 1970, 169; Porten 1993, 271. The Hebrew Bible also witnesses the use of ייהב with the general

“to give” is the typical word for transfer in every kind of Egyptian document, since Old Kingdom onwards, however, suggests also the possibility that the Aramean scribes felt compelled to add to their traditional one-word formula for “to sell,” נתן, their term for give, namely יהב / נתן, to reinforce the legal validity of the operation in the Egyptian context.

No matter the original legal tradition of יהב / נתן within this two-term transfer formula, it is safe to assume that we are dealing, in this case, neither with a hendiadys nor with two terms denoting different legal actions but with a formulaic addition intended to assure the legal validity of the transaction in a new context. Yaron classified formulaic additions into those having a purpose and those without it. The latter case involved what Yaron called “adiposis” or “fatty degeneration of the formula.”⁵¹ In this case, however, the addition is not a “fatty degeneration” but has the clear purpose of contextualizing the Aramaic legal formula in the Egyptian legal tradition.

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meaning of “to give” (for example, Deut. 1:13, 1 Sam. 14:41) but its use is not so widespread as נתן. יהב is attested only 48 times (28 in Hebrew, always used in Qal imperative. *Gesenius-Kautzsch Hebrew Grammar* §69 o,- and 20 in Aramaic).

⁵¹ Yaron 1958:18.

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FOUR UR III ADMINISTRATIVE TABLETS IN THE POSSESSION OF PROFESSOR FRANCIS CARROLL, UNIVERSITY OF MANITOBA *

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Abstract: Four Ur III Administrative Tablets in the Possession of Professor Francis Carroll, University of Manitoba

The article contains transliterations of four cuneiform tablets in a private collection in Winnipeg, Canada. All four are typical Ur III administrative tablets: three come from Umma and the fourth is from Drehem. The object of the article is to make the contents of these tablets available to the Sumerological community, particularly those specializing in the Ur III period.

Keywords: Sumerian – cuneiform – Ur III – administrative

Resumen: Cuatro tablillas administrativas de Ur III en posesión del Profesor Francis Carroll, Universidad de Manitoba

El artículo contiene transliteraciones de cuatro tablillas cuneiformes que se encuentran en una colección privada en Winnipeg, Canadá. Las cuatro son típicas tablillas administrativas de Ur III: tres provienen de Umma y la cuarta de Drehem. El propósito de este artículo es poner los contenidos de estas tablillas a disposición de la comunidad sumeriológica, en particular de aquellos que se especializan en el período de Ur III.

Palabras Clave: Sumerio – cuneiforme – Ur III – administrativo

Published herein are transliterations and translations of four Ur III administrative tablets belonging to Dr. Francis Carroll, Professor Emeritus of Diplomatic History at St. John's College, University of Manitoba. The story of how Professor Carroll came to possess these tablets is long and, in parts, somewhat clouded, as is often the case with tablets that have been in private collections. Professor Carroll received the tablets from his father-in-law, Dr.

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Wallace M. Foster, a physician who practiced in Detroit, Michigan. Dr. Foster owned the tablets for quite some time (Professor Carroll informs me that his wife recalls bringing the tablets to school for show-and-tell as a girl in the early 1950's). Dr. Foster, in turn, had been given the tablets by the widow of a medical associate who is believed to have acquired the tablets while working in the Middle East during the inter-war years.

I became aware of the tablets quite by chance. While attending a history conference in Minneapolis in 2002, I had the pleasure of meeting Dr. Carroll at a party hosted by one of the societies present at the conference. The subject of his tablets immediately came up when I discussed my work in Assyriology. I expressed interest in seeing the tablets and Dr. Carroll was eager to have them translated, but for both of us the matter was set aside. Recently, however, Dr. Carroll and his colleague, Dr. John Stafford, were able to produce excellent digital images of the tablets from which I could make the transliterations and translations below.¹ I would like to thank Dr. Carroll and Dr. Stafford for their work photographing the tablets, and I especially would like to thank Dr. Carroll for generously permitting me to publish his tablets. Finally, I would like to thank Dr. Gertrud Farber (Chicago) and Dr. Magnus Widdell (Liverpool) for reading and commenting on my transliterations and translations.

¹ Because these transliterations were made from photographs and not from the tablets themselves; all readings should be considered provisional until they can be collated against the originals.

Carroll 1

dimensions: 4.9 cm × 7.5

Umma (Šu-Suen 5)

<p>obv. 1. 12 guruš u₄-1-šè še-bala-a u₄-2-šè še má-a si-ga u₄-10-šé</p> <p>5. ki-su₇-gu₄-suḫúb-ta nibru^{ki}-šè [má gíd]-da u₄-2-šè má ba-al-la u₄-1-šè še bala-a šà nibru^{ki}</p> <p>rev. 10. ugula lú-^dšára gìr lú-^dda-mu mu ús-sa ^dšu- ^dsu.en [lugal úri^{ki}-ma]-ke₄ [bàd mar-tu mu-ri]-iq</p> <p>15. [ti-id-ni]-im [mu]-dù</p> <p>seal: 1. lú-^dšára dub-[sar] dumu lugal-[inim-gi-na]</p>	<p>Twelve workmen: for one day carrying barley; for two days loading barley into the boat; for ten days from the threshing floor Gu-Suḫub [‘towing the boat’] to Nippur; for two days unloading the boat; for one day carrying barley within Nippur.</p> <p>The overseer is Lu-Šara. The <i>gir</i>-official is Lu-Damu. The year after Šu-Suen [king of Ur] built the [western wall] called Muriq-[Tidn]im.</p> <p>Lu-Šara the scribe son of Lugal-[inim-gi-na].</p>
--	--

Comments:

2. The verb bala in this line describes the act of moving grain into piles for measurement and distribution according to Civil 1994: 96, 108 n. 129.
5. See Civil 1994: 143. This threshing floor also appears in Schneider 1930: No. 376: 2.

Carroll 2

dimensions: 3.2 cm × 3.2

Umma (XII, Šulgi 42)

- | | | |
|----------|---|--|
| obv. 1. | [3?] ¹ u ₈ bar-su-ga
1 udu-níta bar-su-ga
ri-ri-ga
ki ur-ru-ta | [3?] ¹ ewes, plucked
1 male sheep, plucked
dead (due to violent attack)
from Urru. |
| 5. | kišib lú-kal-la
iti ^d dumu-zi
mu ša-šú-ru ^{ki}
ba-ḫul | Received by Lu-kalla.
The month of Dumuzi.
The year Šašru
was destroyed. |
| seal: 1. | lú-kal-la
dub-sar
dumu ur-e ₁₁ -e šùš | Lu-Kala
the scribe
son of Ur-E'e, chief livestock administrator. |

Comments:

1. bar-su-ga is syllabic writing of bar-sug₄-ga Steinkeller 1995: 56, 69 n. 110. Additional discussion concerning the meaning of this term can be found in Heimpel 1993: 127-130; Van de Microop 1993: 164; and Sigrist 1992: 28.
 3. See Sigrist 1992: 70 for a discussion of the meaning of ri-ri-ga.
- seal 1. For more on Lu-kala see Pomponi 1992: 169-179 and Dahl 2007: 105-113.
3. For more on Ur-E'e see Dahl 2007: 85-96. For the reading of kuš₇ as šùš see Beal 1992: 38-39 n. 48.

Carroll 3

dimensions: 2.2 cm × 2.7

Umma

- | | | |
|----------|---|---|
| obv. 1. | 1 dug.dida 5
sila kaš-sig ₅
0.0.1. ninda 2 gín ì 2
gín naga | 1 jar of beer wort, 5
sila of good beer,
10 sila bread, 2 gin of oil, 2
gin of seed-seasoning, |
| 5. | 3 ku ₆ 3 sa sum
nu-ḫi-lum gaba-aš | 3 fish, 3 bundles of onions,
(to) Nuḫilum, sent to the edge of the sea. |
| rev. | 1 dug.dida 3
sila kaš gin
0.0.1 ninda 2 gín ì 2 | 1 jar of beer wort, 3
sila of ordinary beer,
10 sila bread, 2 gin oil, 2
gin seed-seasoning, |
| 10. | gín naga
3 ku ₆ 3 sa sum
i-šar-be-lí
u ₄ -9-kam | 3 fish, 3 bundles of onions,
(to) Išar-bēlī.
Day 9. |
| u.e. 15. | iti šu-numun | Month of sowing. |

Comments:

1. Discussion of the term dug.dida can be found in De Maaijer 1999-2000: 253 and Farber 1997: 112.
12. Išar-bēlī appears in this role in Kang 1973: No. 309:12.

Carroll 4

dimensions: 4.5 cm × 6.4

Drehem (Šu-Suen 3)

obv. 1.	103 udu [...] ki a-a-dingir-[ñu ₁₀ -ta] ša úri ^{ki} -[ma] 7 udu ki ur- ^d [ba ^l -[ú-ta]	103 sheep [...] from Aya-dingir-ñu in Ur (and) 7 sheep from Ur-Ba ^u .
5.	ša bi 110-[ta] 10 udu 15 máš kišib ḫal-lí lú ur-[bi-lum] maškim 10 udu kišib ur-kù-nun-na 8 udu kišib al-la-ñu ₁₀	From out of the 110: 10 sheep and 15 goats received by Ḫalli of [Urbilum?] the <i>maškim</i> -official, 10 sheep received by Ur-Kununna, 8 sheep received by Allañu, (and)
10.	67 udu ba-ug ₇ ad ₆ kuš-bi é-gal-šè gíd-dam	67 dead sheep, the hides of their corpses are transferred to the palace.
rev.	[.....] 110 [udu] zi-ga-àm [...] nī-ka ₉ -ag [...] 15. iti ezen- ^d [...] mu si-ma-núm ^[ki] ba-[ḫul]	[Total] of 110 [sheep] are withdrawn [...]. Balanced account of [PN]. Month of the festival of [DN] The year Simanum was [destroyed].

Comments:

7. The restoration of Urbilum in the break is highly speculative.

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UNA ACTUALIZACIÓN DE LA CRONOLOGÍA BAJA: ARQUEOLOGÍA, HISTORIA Y BIBLIA¹

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Abstract: A Low Chronology Update: Archaeology, History and Bible

The aim of this article is to present an up-to-date overview of the Low Chronology system for the late Iron I and early Iron II strata in the Levant, a system which I first proposed in two articles which were published about a decade ago. These articles have stimulated a fierce debate, which was a major stimulant behind the introduction of large-scale radiocarbon projects into Iron Age archaeology. And though the gap between my system and the reasonable voice in the traditional camp is narrowing (the “modified conventional chronology”) the dispute is far from being resolved.

Keywords: Levant – Iron Age – Low Chronology

Resumen: Una actualización de la cronología baja: arqueología, historia y Biblia

El objetivo de este artículo es presentar una descripción actualizada del sistema de Cronología Baja para los estratos del Hierro I tardío y el Hierro II temprano en el Levante, un sistema que primeramente propuse en dos artículos que fueron publicados hace una década². Estos artículos han estimulado un intenso debate³, que fue un gran estímulo detrás de la introducción de proyectos de radiocarbono de gran escala en la arqueología de la Edad de Hierro. Y aunque se está cerrando la brecha entre mi sistema y la voz razonable en el campo tradicional (la “cronología convencional modificada”⁴), el conflicto está lejos de estar resuelto.

¹ Este artículo ha sido publicado bajo el título “A Low Chronology Update: Archaeology, History and Bible”, en T.E. Levy y T. Higham (eds.) *The Bible and Radiocarbon Dating: Archaeology, Text and Science*, London, 2005, © Equinox Publishing Ltd. En la versión española sólo fueron añadidos nueva bibliografía y un addendum. Traducción: Juan Manuel Tebes, revisión: Roxana Flammini.

² Finkelstein 1995; 1996a.

³ E.g., Mazar 1997; Ben-Tor y Ben-Ami 1998; Ben-Tor 2000.

⁴ Mazar 2005.

Palabras clave: Levante – Edad del Hierro – Cronología Baja

LOS CIMIENTOS DE LA CRONOLOGÍA TRADICIONAL

El sistema tradicional para la cronología de los estratos del Hierro I tardío y el Hierro II temprano en el Levante está basado en dos pilares: 1) la datación de la cerámica filistea y sus implicaciones para el final del Hierro I; 2) la datación de los estratos del Hierro IIA en el norte. Estos dos pilares están, por cierto, relacionados, pero no son necesariamente dependientes uno del otro⁵. Es decir, la aceptación o el rechazo de uno no implica una actitud similar con respecto del otro. Y debería decir desde el comienzo que estos dos pilares tienen muy poco que ver con la arqueología. Más bien están basados principalmente—y el segundo pilar únicamente—en el relato bíblico de la historia temprana de Israel. En otras palabras, éste es un caso único (y molesto) en el cual los arqueólogos compensaron la evidencia proporcionada por su propia disciplina en favor de una interpretación unilateral del material textual proporcionado por otra disciplina, material que ha sido el foco de una feroz disputa desde principios del siglo XIX.

El **primer pilar** es el paradigma filisteo de Albright/Alt⁶, según el cual:

A) Los filisteos fueron asentados por Ramsés III en sus fortalezas en Canaán, inmediatamente después de sus batallas con los Pueblos del Mar, en su año octavo, es decir, en 1175 a.C.;

B) Los filisteos bíblicos pueden ser igualados con los filisteos arqueológicos del Hierro I;

C) El poder de los filisteos del Hierro I fue quebrantado por la expansión del imperio israelita bajo el rey David. Y puesto que la ascensión del rey David ha sido fechada—de acuerdo a las cifras bíblicas—en ca. 1000 a.C., esta fecha ha sido tomada para representar el final del capítulo independiente de la cultura material filistea y la transición del Hierro I al Hierro II⁷.

Los resultados de las excavaciones en tres de los principales montículos en Filistea—Asdod, Tel Mique y Ascalón—fueron presentados de modo tal que encajan perfectamente en este paradigma: la primera fase de la presencia filistea, caracterizada por la cerámica Myc. IIIC:1b hecha localmente (también conocida como monocroma), fue datada en un período que comenzó en ca. 1175 a.C. y perduró hasta la retirada de los egipcios de Canaán pocas décadas

⁵ *Contra* Bunimovitz y Faust 2001: resumen; Mazar 1997.

⁶ Albright 1932: 58; Alt 1944.

⁷ E.g., Dothan 1982: 296.

después⁸. La segunda fase, caracterizada por la cerámica bicroma, ha sido datada posteriormente, a finales del siglo XII y el siglo XI a.C. Los niveles de destrucción al final de la fase bicroma fueron datados en ca. 1000 a.C. y asociados con las conquistas del rey David⁹.

El **segundo pilar** sobre el que se apoya la cronología tradicional es la reconstrucción—de acuerdo al testimonio bíblico—de una gran Monarquía Unida de Israel, establecida en el curso de las hazañas militares del rey David y estabilizada en los días de su hijo Salomón, quien gobernó sobre un estado sofisticado, rico y próspero. Según este paradigma, Salomón se dedicó a realizar actividades de construcción monumentales en varios centros administrativos de su estado, y por lo tanto su imperio puede ser reconocido arqueológicamente¹⁰. Estas ideas—que la arqueología pueda transformar en históricas las descripciones bíblicas de la Monarquía Unida—se remontan a la escuela de pensamiento de Albright de los años ‘30. Nelson Glueck, por ejemplo, “inventó” virtualmente la planta de fundición del rey Salomón en Tel el-Khuleifeh (“la Pittsburg de Palestina”, como él la llamó¹¹)—una fortaleza que no ha provisto restos anteriores al ca. 700 a.C.¹²

Pero fue Yigael Yadin, luego de sus excavaciones en Hazor y Megiddo a finales de los ‘50 y principios de los ‘60, quien “canonizó” esta reconstrucción histórica—y con ella el sistema de la cronología tradicional. La construcción histórica y cronológica de Yadin estaba basada en:

1) La datación de las puertas con seis cámaras ubicadas en Hazor, Megiddo y Gezer, en los días del rey Salomón, siguiendo el texto bíblico de 1 Reyes 9:15¹³.

2) La datación de los dos palacios de sillar de Megiddo en el siglo X a.C., basada en la idea bíblica de un imperio salomónico sofisticado que era gobernado desde una refinada capital en Jerusalén¹⁴.

Otros dos hallazgos en Megiddo parecían apoyar la interpretación de Yadin: la ciudad principal, *anterior* a la ciudad de los palacios (Estrato VIA), fue destruida por un terrible incendio, y la ciudad posterior, construida encima de los palacios (Estrato IVA), tenía los famosos establos. La interpretación de Yadin¹⁵ parecía encajar perfectamente con el testimonio bíblico: la Megiddo

⁸ E.g., Mazar 1985; Stager 1995.

⁹ E.g., Mazar 1951; Dothan 1982: 296.

¹⁰ E.g., Yadin 1970; Dever 1997; Stager 2003.

¹¹ Glueck 1940: 5.

¹² Pratico 1993.

¹³ Yadin 1958.

¹⁴ Yadin 1970.

¹⁵ Yadin 1970.

cananea fue destruida por David; los palacios representan la Edad Dorada de Salomón; su destrucción por el fuego podía atribuirse a la campaña del faraón Shoshenq I (el Shishak bíblico) en Palestina (Megiddo es mencionada en la lista de Karnak de Shoshenq I, y un fragmento – sin ubicación estratigráfica – de una estela de Shoshenq I fue encontrado en el sitio en los años ‘20); y los establos fueron construidos a principios del siglo IX a.C. en los días del rey Ahab, del que se dice que hizo frente a Salmanasar III en Qarqar con una enorme fuerza militar compuesta por 2000 carros.

QUÉ ESTÁ MAL EN LA CRONOLOGÍA TRADICIONAL

Por lo que puedo juzgar, ni uno solo de los argumentos de la cronología tradicional puede resistir un escrutinio cuidadoso, libre de prejuicios teológicos o, simplemente, románticos.

El Hierro I

No cabe duda que el sistema egipcio-cananeo de la Edad del Bronce Tardío continuó funcionando por lo menos hasta los días de Ramsés IV¹⁶. Se encontraron escarabajos de este faraón en varios contextos claros de finales del Bronce Tardío en Laquish¹⁷, en el Cementerio 900 de Tel el-Farah y en Beth-shemesh. El estrato IX de Tel Sera suministró una inscripción hierática del año 22+X de un faraón egipcio—sin duda alguna Ramsés III¹⁸. Y diferentes hallazgos provenientes de Deir el-Balah, que llegaron al mercado de antigüedades, apuntan a que el sitio continuó funcionando en la época de la Dinastía XX, posiblemente hasta los días de Ramsés VI¹⁹. En el norte de Canaán, Megiddo no fue destruida antes del reinado de Ramsés III; de hecho, probablemente estaba todavía ocupada en tiempos de Ramsés VI²⁰. Y un grupo de hallazgos provenientes de Beth-shean atestigua que la fortaleza egipcia todavía funcionaba en el reinado de Ramsés IV²¹.

Según el paradigma filisteo, éste es el período en el cual los filisteos fueron asentados en fortalezas egipcias—exactamente la clase de sitios que fueron mencionados más arriba. Uno podría esperar que por lo menos los sitios principales en Filistea brindaran un estrato con una cultura material

¹⁶ Finkelstein 2005.

¹⁷ Lalkin 2004.

¹⁸ Goldwasser 1984.

¹⁹ Givon 1977.

²⁰ Singer 1988–1989; Ussishkin 1995.

²¹ Finkelstein 1996b: 173.

combinada filisteo-egipcia. Éste no es el caso. Ni un sólo tiesto de MycIII:1b hecho localmente (“monocromo”) ha sido encontrado en alguno de estos estratos, ni en sitios localizados a unos pocos kilómetros de los principales centros filisteos. Y no menos importante es el hecho de que ni una sola vasija egipcia de la Dinastía XX (para diferenciar de tiestos residuales) fue encontrada en alguno de los estratos monocromos. Estos fenómenos están demasiado expandidos como para ser explicados como una coexistencia entre las dos culturas. En otras palabras, los filisteos tomaron el control de parte de Filistea mientras que Egipto continuó controlando algún enclave próximo, sin ninguna conexión entre las dos partes durante un período que abarcó varias décadas²².

Bunimovitz y Faust²³ han ido más lejos, a la búsqueda de estudios de caso etnográficos del distrito Baringo en Kenia, para mostrar que dos culturas distintas pueden coexistir sin mezclar demasiados de sus rasgos materiales. Pero la cantidad y variedad de las culturas humanas contemporáneas permiten al arqueólogo encontrar un paralelo a cada fenómeno histórico. De hecho, la etnografía puede suministrar ejemplos a situaciones conflictivas; la tarea del arqueólogo es fijar las reglas de la comparación. Y comparar las tribus africanas del siglo XX d.C. con el imperio egipcio en el Levante del siglo XII a.C., o con los grupos migrantes de la cuenca del Egeo del siglo XII, es absurdo.

En cualquier caso, Tel Mique y Asdod parecen haber suministrado evidencias de que la fase monocroma puede ser datada posteriormente al dominio egipcio. En ambos sitios los restos de las dos culturas parecen haber sido encontrados uno por encima del otro²⁴, lo que significa que las dos partes—egipcios y filisteos—están separadas estratigráficamente, es decir, cronológica y no geográficamente. No hay ningún rastro del *asentamiento* de los Pueblos del Mar, los filisteos incluidos, antes de 1140 o 1130 a.C. La fase temprana del asentamiento filisteo comenzó aproximadamente en ese momento y duró hasta el final del siglo XII a.C. La fase bicroma debe datarse, por consiguiente, en el siglo XI y durante gran parte del siglo X a.C. La transición del Hierro I al Hierro IIA no habría tenido lugar antes de fines del siglo X a.C.

Hay un lado algo humorístico en todo esto. Difícilmente uno pueda asumir que Tel Mique y Asdod fueron las únicas ciudades destruidas antes del período ramésida tardío. Entonces la única explicación de lo que sucedió *realmente*, según la opinión tradicional, es la siguiente: una noche, los filisteos, quienes

²² Redford 1985: 217–218; Singer 1985: 114; Dothan 1992: 97; Bietak 1993; Stager 1995.

²³ Bunimovitz y Faust 2001.

²⁴ Finkelstein 1995: 223; Finkelstein y Singer-Avitz 2001; Ussishkin 2007.

habían vivido en estas ciudades por algunas décadas (¡sin ninguna característica distintiva en su cultura material!), se vieron envueltos en alguna clase de fiesta desenfadada regada con cerveza o vino, se emborracharon, y prendieron fuego a sus propias ciudades, a sus propias casas. Después de todo, eran filisteos ¿cierto? En cualquier caso, a la mañana siguiente, horrorizados por lo que vieron, reconstruyeron sus ciudades devastadas y continuaron viviendo en ellas. Pero todo esto los hizo extrañar un poco sus casas, así que decidieron comenzar a producir la cerámica monocroma—la clase de cerámica que todavía recordaban de sus ciudades natales, que habían dejado varias décadas atrás... La belleza de esta teoría es que provee de una pista para el origen egeo de los filisteos: su comportamiento se asemeja al de los rudos centauros, que se emborracharon y se pusieron violentos en la boda de Pirítoo...

El Hierro II temprano

La teoría “salomónica” de Yadin estuvo plagada de problemas severos desde el comienzo. Primero, la puerta de Megiddo fue construida *después* que las puertas de Hazor y Gezer, ya que se conecta con el muro de la ciudad del estrato IVA²⁵. En segundo lugar, se han descubierto puertas similares en otros lugares en la región, en sitios que datan de finales del siglo IX y VIII (Laquish y Tel Ira) y en sitios construidos fuera de los límites de la gran Monarquía Unida, *incluso* de acuerdo con el punto de vista maximalista— como la puerta de Asdod del siglo VIII y la de Kh. Mudeineh eth-Themed en Moab, de los siglos IX y VIII²⁶.

Yadin sostuvo que su teoría estaba basada en tres pilares: estratigrafía, cronología y el texto bíblico:

“Nuestra decisión de atribuir ese nivel a Salomón estuvo basada sobre todo en el pasaje de 1 Reyes, la estratigrafía y la cerámica. Pero cuando además encontramos en ese estrato una puerta con seis cámaras y dos torres, conectada a un muro de casamatas idéntico en plan y medida a la puerta de Megiddo, estuvimos seguros que habíamos identificado con éxito la ciudad de Salomón²⁷.”

Pero la estratigrafía sólo nos provee de una cronología relativa y las vasijas antiguas no llevan una etiqueta con la datación. Está claro, entonces, que la

²⁵ Ussishkin 1980.

²⁶ Para las primeras, véase Ussishkin 1990: 77–82; Finkelstein y Singer-Avitz 2001: 243–244; para la segunda, véase Daviau 1997: 225.

²⁷ Yadin 1970: 67.

datación de Yadin está basada únicamente en el texto bíblico. No obstante, algunos de mis oponentes sostienen que los estratos salomónicos en Megiddo, Hazor y Gezer fueron datados según una familia de recipientes bien definida—la cerámica de engobe rojo bruñida—que aparentemente data del siglo X:

“La cerámica de este nivel de destrucción incluía formas distintivas de cerámica de engobe rojo, y de engobe y bruñido a mano, que han sido siempre datadas a fines del siglo X... Así, de acuerdo con argumentos cerámicos comúnmente aceptados—no por una aceptación ingenua de las historias de la Biblia—datamos los muros y las puertas de Gezer de mediados a fines del siglo X²⁸. ”

Lo contrario es verdad; Dever data este tipo cerámico en el siglo X porque fue encontrado en los así llamados “estratos salomónicos”:

“El estrato clave parece ser la Fase UG3A del Campo III de Gezer, que es muy corta y de modo excepcional, está bien ubicada históricamente. Se encuentra después del período de construcción salomónico, ricamente documentado por datos bíblicos e históricos, y asegurado por criterios regionales comparativos, arqueológicos y arquitectónicos, combinados con criterios comparativos cerámicos²⁹. ”

En palabras más sencillas, el estrato clave está datado por la cerámica; la cerámica está datada por su relación con la puerta con seis cámaras; la cual, a su vez, está datada, según el testimonio bíblico, en los tiempos de Salomón—un razonamiento circular clásico.

Esto deja al pasaje bíblico de 1 Reyes 9:15 como el *único* pilar verdadero en la teoría de Yadin y la única base para la estructura entera de la arqueología del siglo X en todo el Levante (y más allá, ya que la cronología de los estratos de la Edad del Hierro en el Levante se refleja en la datación de la cerámica griega de la Edad de Hierro³⁰). Así que este es el momento para echar una mirada directa al transfondo de este crucial versículo.

La idea de que el libro de Reyes – el cual no fue escrito antes de fines del siglo VII a.C. – incluye información histórica sobre los días del rey Salomón, quien había gobernado siglos atrás, proviene de una percepción más amplia: que el autor había accedido a material de archivo en Jerusalén, el cual incluía documentos de la época de Salomón o del período inmediatamente posterior.

²⁸ Dever 2001a: 132.

²⁹ Holladay 1990: 62–63.

³⁰ Véase, e.g., Coldstream 2003.

Esta percepción, a su vez, estaba basada en una teoría aún más amplia, defendida por grandes eruditos bíblicos alemanes como Leonard Rost³¹ y Gerhard von Rad³². Esta teoría sostenía que el reinado de Salomón debía ser visto como un período de iluminación excepcional, durante el cual (o inmediatamente luego del cual) se escribieron grandes trabajos históricos en Jerusalén, como la Historia de la Sucesión³³. Y esta teoría estaba fundada, a su vez, en el testimonio bíblico—otro razonamiento circular perfecto.

Sin tener en cuenta el debate de la cronología, la arqueología produjo un cuadro totalmente diferente. En el siglo X, Jerusalén era una aldea pequeña, pobre, sin fortificar³⁴; las meticulosas prospecciones demuestran que las tierras altas de Judá—la espina dorsal de la supuesta gran Monarquía Unida—estaban escasamente habitadas en el siglo X por una docena de pequeñas aldeas, con una población de no más de unos pocos miles de personas³⁵; que no hay signos de actividades de construcción monumentales en la Judá del siglo X; que no hay signos de industrialización de la producción agrícola; que no hay evidencias de producción masiva de cerámica; y que no hay rastros de jerarquía de asentamiento. Y lo más importante de todo, más de un siglo de excavaciones en cada sector de Jerusalén y en cada sitio significativo en Judá, no han podido revelar evidencia alguna de actividades significativas relacionadas con escribas o signos de alfabetismo en el siglo X³⁶. *Todas* estas características—de un estado avanzado y de una sociedad alfabeta—aparecieron mucho más tarde, a fines del siglo VIII y principalmente en el siglo VII a.C., cuando Judá creció hasta convertirse en un estado burocrático completamente desarrollado³⁷. Para resumir este punto, la arqueología moderna ha demostrado que la idea de un archivo en Jerusalén, que guardó registros genuinos del siglo X, es un absurdo que está basado en el testimonio bíblico más que en alguna evidencia real. Obviamente, éste es el fin de la idea de que 1 Reyes 9:15 nos puede proveer un anclaje para la arqueología del siglo X a.C.

¿Pero cuál *es* la realidad detrás de este versículo? No hay duda de que la descripción bíblica de la Monarquía Unida traza un cuadro de una idílica edad

³¹ Rost 1982 [1926].

³² von Rad 1966 [1944]: 176–204.

³³ Véase también Halpern 2001.

³⁴ Finkelstein 2001; Ussishkin 2003.

³⁵ Lehmann 2003.

³⁶ Jamieson-Drake 1991; Finkelstein 1999.

³⁷ Para la cerámica véase Zimhoni 1997: 170–172; para la industrialización de la producción agrícola véase Eitan-Katz 1994; para los pesos véase Kletter 1991; para los ostraca véase Sass 1993; Renz 1995: 38–39; para los sellos e impresiones de sello véase Avigad y Sass 1997: 50–51.

dorada; y que ello está envuelto en los objetivos teológicos e ideológicos del tiempo de los autores³⁸. Toda la descripción del reinado de Salomón en el Libro de Reyes está basada en dos cimientos: las realidades de la época de la compilación del texto, o un poco antes, y la ideología de Judá de finales de la monarquía³⁹. La mención de las tres grandes ciudades *del norte* en 1 Reyes 9:15 podría haberse tomado de las realidades del siglo VIII a.C., antes de la caída del Norte, todavía recordada en Judá y proyectada en su semi-legendaria historia temprana, con el objetivo de mirar a un futuro prometedor basado en ese pasado mítico y glamoroso⁴⁰. Deambular por Megiddo, Hazor y Gezer con este versículo a mano, buscando monumentos del siglo X es, por lo tanto, un emprendimiento tremendamente ingenuo.

Lo mismo es cierto para la atribución de niveles de destrucción en el norte al rey David ca. 1000 a.C. No hay ningún indicio de las grandes guerras de conquista davídicas (que difícilmente podrían haberse emprendido con una población de quizás 500 varones adultos en la Judá del siglo X). El relato bíblico de las guerras de David está influenciado por las realidades del siglo IX y posiblemente del siglo VIII⁴¹; y dejando de lado la fórmula tipológica de los cuarenta años de reinado para David y Salomón, no sabemos en qué momento del siglo X David gobernó.

La teoría convencional suscita otros severos problemas históricos y arqueológicos, más allá de la interpretación de la narrativa bíblica y del registro arqueológico según la narrativa bíblica:

1. La emergencia de los estados territoriales en el Levante fue un resultado de la expansión hacia el oeste del imperio asirio a principios del siglo IX a.C. Los registros del antiguo Cercano Oriente dejan poca duda acerca de que todos los principales estados de la región—Aram Damasco, Moab, Ammón y el Israel del norte—se desarrollaron en el siglo IX a.C.⁴² Es extremadamente difícil imaginar un gran imperio—no documentado en los textos del antiguo Cercano Oriente—que era gobernado desde una región casi vacía y marginal de las tierras altas meridionales (y desde una pequeña aldea) un siglo antes de este proceso.

2. La región septentrional de Israel suministró evidencias de dos importantes horizontes de destrucción entre el fin del Bronce Tardío y la conquista asiria: Megiddo VIA y Megiddo VA–IVB y sus estratos contemporáneos. La mayoría

³⁸ E.g., Van Seters 1983: 307–312; Knauf 1991; Miller 1997; Niemann 1997; Finkelstein y Silberman 2001: 123–145; 2006.

³⁹ Knauf 1991; Finkelstein y Silberman 2006.

⁴⁰ Para puntos de vista similares véase Knauf 1997: 91–95; Niemann 2000.

⁴¹ Na'aman 2002.

⁴² Finkelstein 1999; para los arameos más al norte véase, e.g., Sader 2000.

de los partidarios de la cronología tradicional han datado el primero en ca. 1000 a.C. y lo han atribuido al rey David, y el segundo a finales del siglo X y atribuido a la campaña del faraón Shoshenq I⁴³. Esto ha causado un problema histórico mayor: al bien documentado asalto de Aram Damasco sobre el Reino del Norte en la década de 830 a.C., mencionado en confiables historias proféticas bíblicas y avalado por la estela de Tel Dan, no se le ha asignado nivel de destrucción, ni siquiera en Tel Dan, el cual debe haber sido conquistado por Hazael.

3. Una tumba en Kefar Veradim, en el norte, suministró un cuenco de bronce trabajado de estilo asirio⁴⁴ con un conjunto cerámico del Hierro IIA⁴⁵. Este último no aparece antes del siglo IX a.C. La aplicación de la cronología tradicional da lugar a una situación absurda en la cual la inscripción data del siglo XI, la cerámica del siglo X, y el cuenco probablemente del siglo IX a.C.⁴⁶

4. Más frustrante aún: más de un siglo de exploraciones arqueológicas en Jerusalén—la capital de la glamorosa Monarquía Unida *bíblica*—no ha podido revelar evidencia de ninguna actividad significativa de construcción en el siglo X. La famosa estructura de piedra escalonada, que fue presentada como el más importante vestigio de la Monarquía Unida⁴⁷, fue construida con probabilidad en el siglo IX a.C. (se halló cerámica que data del siglo IX, o quizás del siglo VIII, entre sus hiladas de superficie⁴⁸) y renovada durante los siglos siguientes⁴⁹. Asimismo, los muros encontrados recientemente, ubicados inmediatamente al oeste, y que fueron interpretados como los restos de un palacio del siglo X⁵⁰, es probable que daten de varios siglos, incluyendo posiblemente al período helenístico⁵¹. El pretexto común – que los vestigios del siglo X fueron eliminados por la actividad posterior – es engañoso, ya que fortificaciones monumentales del Bronce Medio y del tardío Hierro II efectivamente sobrevivieron a ocupaciones posteriores⁵².

Y más allá de todo esto está el problema de la sincronización con los territorios vecinos. En relación con la correlación entre las secuencias cerámicas

⁴³ E.g., Yadin 1970; Harrison 2003; Dever 1997: 239–243 y Stager 2003 para Shoshenq I.

⁴⁴ Alexandre 2002a.

⁴⁵ Alexandre 2002b.

⁴⁶ Sass 2005: 36–39.

⁴⁷ E.g., Cahill 2003; Mazar 1997: 164.

⁴⁸ Steiner 1994: 19.

⁴⁹ Finkelstein 2001; Finkelstein *et al.* 2007.

⁵⁰ Mazar 2007.

⁵¹ Finkelstein *et al.* 2007.

⁵² Reich y Shukron 2000.

levantinas y griegas, “la ‘cronología baja’ recientemente promovida en Israel ofrece la pauta de desarrollo más creíble en el Egeo”⁵³. Y con respecto a Siria, “consideraciones de naturaleza histórica y arqueológica apuntan al inicio del siglo IX como un confiable punto de transición del Hierro I al Hierro II”⁵⁴.

UNA CRONOLOGÍA ALTERNATIVA

Hasta aquí la evidencia negativa; indicios más directos provienen de dos sitios relacionados con la dinastía ómrída: Samaria en las tierras altas y Jezreel en el Valle.

Los bloques de sillar descubiertos en los cimientos del Palacio 1723 en Megiddo, que datan del Estrato VA–IVB— el nivel que Yadin asoció a la época del rey Salomón—poseen marcas de albañiles únicas⁵⁵, que fueron halladas en otro sitio, posiblemente el único otro edificio de Israel que las posea: el palacio de la dinastía ómrída en Samaria⁵⁶. Las marcas son tan distintivas que deben haber sido ejecutadas por el mismo grupo de albañiles. La semejanza en las técnicas de construcción entre los dos edificios fueron primero notadas por Clarence Fisher⁵⁷, el excavador de Megiddo y Samaria, y por John Crowfoot⁵⁸, el segundo excavador de Samaria; fueron olvidadas con el comienzo del frenesí salomónico⁵⁹, y han sido recientemente revividas por Norma Franklin⁶⁰. Pero un palacio fue datado en el siglo X y el otro en el siglo IX a.C. Aquí hay sólo dos alternativas: o bien llevar el edificio de Megiddo hacia delante, al siglo IX, o traer el palacio de Samaria al siglo X. Obviamente, la primera alternativa, que está apoyada por documentos históricos, es la única posibilidad. El testimonio bíblico, que indica que Samaria fue construida por los ómrídas, está apoyado por textos asirios que se refieren al Reino del Norte como *bit omri*, es decir, “la Casa de Omri”—la forma típica de denominar a un estado según el fundador de su dinastía dominante.

Las excavaciones en Jezreel, situada a menos de diez millas al este de Megiddo, revelaron igualmente resultados sorprendentes: el nivel de destrucción del complejo ómrída, datado a mediados del siglo IX a.C., suministró un conjunto cerámico idéntico al conjunto de Megiddo VA–IVB,

⁵³ Coldstream 2003: 256.

⁵⁴ Mazzoni 2000: 121.

⁵⁵ Lamon y Shipton 1939: 25.

⁵⁶ Reisner *et al.* 1924 Vol. I: 119–120, Vol. II: Pl. 90: e–f.

⁵⁷ Fisher 1929: 58.

⁵⁸ Crowfoot 1940: 146.

⁵⁹ Guy 1931.

⁶⁰ Franklin 2005.

que ha sido tradicionalmente datado a fines del siglo X a.C.⁶¹ En un intento desesperado de salvar la idea de una gran Monarquía Unida, Ben-Tor⁶² sugirió que la cerámica de Jezreel pertenece a una fase anterior del sitio. Esto significa que las operaciones de nivelado a gran escala, el transporte de rellenos, la deposición de la tierra en su lugar, y la construcción de un complejo tipo casamatas—todo esto en tiempos de los ómridas—no dañó los recipientes antiguos—las cuales estaban todavía allí intactos... Los milagros suceden en Tierra Santa, pero esto es un poco demasiado. Así que en este caso también necesitamos adelantar el conjunto de Megiddo o retrasar el conjunto de Jezreel. Y de nuevo la primera es la única opción, ya que la historia bíblica profética del asesinato de Joram, rey de Israel, y de Ocozías, rey de Judá, por Jehu, están apoyadas por la estela de Tel Dan⁶³. Permítaseme repetir esto: en ambos casos—el de las marcas de albañiles y el de los conjuntos cerámicos—la única alternativa es bajar la datación de los palacios de Megiddo a la primera mitad del siglo IX a.C.

Hasta ahora me he ocupado de la arqueología tradicional y la exégesis bíblica. ¿Podemos agregar a estas consideraciones circunstanciales evidencias más exactas?

La primera pista *puede* provenir de Egipto. Munger⁶⁴ se ha ocupado de un grupo de amuletos egipcios “producidos en masa” encontrados en gran número en el Levante. Parecen haber sido hechos masivamente en el Delta en la época de los faraones Siamón y Sheshonq I, quienes gobernaron en el siglo X a.C. (obviamente, la datación de la campaña de Sheshonq en 926 a.C., aparentemente apoyada en la datación egipcia, está basada únicamente en el testimonio bíblico). Con todo, en Israel estos amuletos aparecen por primera vez en el horizonte del Hierro I tardío (Megiddo VIA), que fuera previamente datado en el siglo XI a.C. En Dor, cinco de esos amuletos fueron encontrados en una habitación con un conjunto cerámico del Hierro I tardío⁶⁵. Algunas de las objeciones a esta idea (Brandl, en una conferencia en Oxford, en 2004) pueden ser sensatas, pero la teoría de Munger sigue siendo aún una posibilidad válida (sino la preferible).

El segundo y principal indicio independiente son las mediciones de radiocarbono. Para no repetir los argumentos que se han publicado recientemente, sólo deseo hacer notar que:

⁶¹ Zimhoni 1997: 38–39.

⁶² Ben-Tor 2000.

⁶³ Schniedewind 1996.

⁶⁴ Munger 2003.

⁶⁵ Gilboa *et al.* 2004.

1) Eliezer Piasetzky y yo hemos demostrado que muchos de los datos publicados hasta ahora—de Tel Dor, Tel Rehov, Tel Hadar y Megiddo, encajan mejor en el sistema de la Cronología Baja⁶⁶.

2) Bruins, van der Plicht y Mazar⁶⁷ han publicado una segunda serie de lecturas de radiocarbono de Tel Rehov, que ellos interpretan como sostén de la cronología tradicional. Con todo, Piasetzky y yo hemos demostrado que hay una alternativa más baja a la interpretación de estas mediciones, que encajan mejor en el panorama general proporcionado por el radiocarbono y la estratigrafía⁶⁸. De hecho, cuando se añaden los argumentos cerámicos y estratigráficos a la interpretación de la curva de calibración, incluso los nuevos resultados de Tel Rehov apoyan la Cronología Baja⁶⁹. Por lo tanto, la festiva declaración de Stager de que las lecturas de Tel Rehov pusieron “el último clavo en el ataúd de la teoría de Finkelstein”⁷⁰ se convirtió en un bumerán antes de que la tinta se hubiera secado.

ARGUMENTOS SOSTENIDOS EN CONTRA DE LA CRONOLOGÍA BAJA

Algunos han intentado ganar un momento de fama intentando participar en el acalorado debate de la cronología con argumentos bastante divertidos, que demuestran un desconocimiento absoluto de todo el tema. La larga discusión de Harrison sobre la evidencia de Megiddo⁷¹ no tiene sentido, pues está basada en los argumentos tradicionales: el rey David destruyó Megiddo VIA; Salomón construyó Megiddo VA–IVB, etc. Y la declaración de Gal⁷² de que “la identificación de Horvat Rosh Zayit con el Cabul bíblico (...) y su asociación con la ‘Tierra de Cabul’ lo vinculan tanto al rey Salomón como a Hiram de Tiro (...) proveyendo así una base histórico-geográfica apropiada” (él quiere decir base cronológica), es la última manifestación del síndrome del razonamiento circular. ¿Cómo sabe Gal la datación para la cerámica de Rosh Zayit? ¿Cómo sabe que ésta es la Cabul bíblica? ¿Cómo sabe que el versículo bíblico describe de hecho las realidades del siglo X a.C. (o acaso no lo hace...)? ¿Y cómo sabe que hubo un Hiram de Tiro en el siglo X, cuando

⁶⁶ Finkelstein y Piasetzky 2003a; 2006a.

⁶⁷ Bruins *et al.* 2003.

⁶⁸ Finkelstein y Piasetzky 2003b; 2003c; 2006b; Sharon, Gilboa y Boaretto 2007.

⁶⁹ Finkelstein 2004; Finkelstein y Piasetzky 2006b.

⁷⁰ Holden 2003:229, 231.

⁷¹ Harrison 2003.

⁷² Gal 2003: 149.

el único Hiram de Tiro extra-bíblico data de la segunda mitad del siglo VIII a.C.?

Pero ha habido desafíos más serios, que necesitan ser tratados. Los siguientes son tres ejemplos que no he tratado hasta ahora en este artículo:

1. El argumento de *Taanak* de Lawrence Stager⁷³: El faraón Sheshonq I, que hizo una campaña en Palestina en la segunda mitad del siglo X, menciona a Taanak en su lista de Karnak. Según Stager, Taanak sólo posee un nivel de destrucción— que se corresponde con un estrato de Megiddo que es tradicionalmente datado en el siglo X a.C. Con todo, una nueva reevaluación de los hallazgos de Taanak⁷⁴ señala claramente un estrato anterior que fue también destruido en un feroz incendio. Esto proporciona un nivel de conflagración en Taanak para cualquiera que busque una destrucción de Sheshonq.

2. El argumento de la *densidad de estratos*, sostenido por Mazar⁷⁵ y Ben-Tor⁷⁶. Si la datación de los estratos del siglo X es bajada a principios del siglo IX, quedan demasiados estratos en Israel del norte para el relativamente corto lapso de tiempo que va hasta la dominación asiria en 732 a.C. Hay varias respuestas a este argumento: en primer lugar, la datación tradicional hace lo mismo con los estratos más tempranos; en segundo lugar, el número de estratos depende de la calidad de las excavaciones; y en tercer lugar, la historia de los sitios de frontera (tales como Hazor—el tema de la queja de Ben-Tor) fue más turbulenta que la de los sitios ubicados en el interior (tales como Megiddo).

3. El argumento de *cómo puedes aceptar un testimonio bíblico y rechazar otro*⁷⁷. Dicho simplemente, la pregunta es cómo puede uno rechazar la historicidad del testimonio bíblico sobre las actividades de construcción de Salomón y al mismo tiempo aceptar la historicidad de los versículos sobre la construcción de Samaria por Omri. La respuesta es asombrosamente simple: la aceptación de la historicidad de un versículo y el rechazo de otro es *exactamente* el significado de dos siglos de estudios bíblicos. Como he mencionado anteriormente, la descripción bíblica del estado salomónico es idealizada, con muchas referencias a realidades de épocas de la historia israelita muy posteriores (por ejemplo, la historia de la visita de la reina de Saba a Jerusalén y las expediciones comerciales desde Ezion-geber, que deben reflejar la participación de Judá, bajo la dominación asiria, en el intensivo comercio

⁷³ Stager 2003: 66.

⁷⁴ Finkelstein 1998.

⁷⁵ Mazar 1997: 163.

⁷⁶ Ben-Tor 2000.

⁷⁷ E.g., Mazar 1999: 40, nota 38; Ben-Tor 2000: 12, 14.

arábigo). De hecho, no hay un sólo punto importante en esa descripción que no pueda explicarse en el trasfondo monárquico tardío⁷⁸. La descripción del estado ómrída es mínima, negativa, pero mucho más exacta históricamente. Basta con mencionar de nuevo el ciclo de Eliseo en los eventos que implicaron la caída de la dinastía ómrída, incluyendo el papel de Hazael, rey de Damasco, que son sostenidos por la estela de Tel Dan.

4. Finalmente, el argumento *Finkelstein está solo*, de William Dever⁷⁹, es demasiado tentador para ser ignorado. El número de partidarios de cada campo en este debate depende de quién y cómo se calcula. Si son contados los investigadores directamente implicados en el debate, la mayoría, sospecho, no está del lado de Dever. El lector debe también prestar atención al hecho más revelador—que todas las defecciones son desde la cronología tradicional a la baja. Dever ha preparado ahora el terreno para su propia defección: “Debe mostrarse precaución en este momento; pero se debe permitir la posibilidad de dataciones levemente más bajas a los siglos X–IX a.C.” (en el resumen de su exposición en la conferencia sobre Datación por Radiocarbono en Oxford, 2004). En cualquier caso, observado el Dream Team que se encuentra a mi lado⁸⁰, únicamente puedo esperar estar siempre solo.

CONCLUSIÓN

Bajar la datación de los conjuntos del siglo XI a principios y mediados del siglo X a.C., y de los conjuntos del siglo X a principios del siglo IX a.C., junto con la transición del Hierro I tardío / Hierro II A temprano fijada a finales del siglo X, cura todas las enfermedades de la cronología levantina tradicional. Esto significa:

- Colocar la cerámica protogeométrica griega de Dor, Tel Hadar y Tel Rehov en su lugar más apropiado⁸¹.
- Armonizar la evidencia para datar las secuencias cerámicas en Israel y en Siria.
- Datar la formación del estado en Israel junto con otras áreas del Levante y Asia occidental, a principios del siglo IX a.C.
- Ubicar el nivel de destrucción “desaparecido” en el norte, para el asalto arameo al Reino del Norte, a mediados del siglo IX.

⁷⁸ Knauf 1991; Finkelstein y Silberman 2006: 151–177.

⁷⁹ Dever 2001b: 68.

⁸⁰ Véase una lista temporal y mucho más completa en Finkelstein y Silberman 2002: 66–67.

⁸¹ Desde la perspectiva egea, e.g., Coldstream 2003.

- Datar las idénticas marcas de albañil en Megiddo y Samaria en el mismo período.
- Datar los conjuntos cerámicos idénticos de Megiddo VA–IVB y de Jezreel en el mismo lapso de tiempo.
- Evitar una reconstrucción absurda de un gran imperio gobernado desde tierras altas deshabitadas y una aldea minúscula.
- Reconstruir una historia lógica del Levante en los siglos X y IX a.C. que sea compatible con el panorama general de la historia del antiguo Cercano Oriente.
- Desviarse de una lectura ingenua del texto bíblico.
- Y, no menos importante, ubicar los estratos en su lugar apropiado según los resultados radiométricos recientes.

La única desventaja de la Cronología Baja—por lo menos para algunos—es que tira debajo de la alfombra la imagen bíblica de una gran Monarquía Unida salomónica y saca a la luz el Reino del Norte de la dinastía ómrída como el primer estado próspero real del temprano Israel. Aquí está el dilema para los miembros de este campo: ¿Cómo puede uno disminuir la estatura de los “chicos buenos” y dejar que prevalezcan los “chicos malos”?

ADDENDUM

Los resultados completos de la primera fase del proyecto de la cronología de la Edad de Hierro (veintiún sitios, 105 muestras, 396 lecturas, medidas en tres laboratorios) han sido publicados recientemente⁸². Ellos establecen de modo rotundo la transición del Hierro I tardío al Hierro IIA a fines del siglo X a.C., y por lo tanto apoyan la Cronología Baja y niegan la “cronología convencional modificada”: de las 36 interpretaciones posibles de los datos de C¹⁴, 35 siguen completamente la Cronología Baja y una cae entre la baja y la alta, pero excluye la última.

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⁸² Sharon, Gilboa, Jull y Boaretto 2007.

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THE ALLEGED “ANCHOR POINT” OF 732 BC FOR THE DESTRUCTION OF HAZOR V*

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Summary: The Alleged “Anchor Point” of 732 BC for the Destruction of Hazor V

All previous discussions of the chronology of Iron Age Hazor assume as an “anchor point” the destruction of Hazor V by Tiglath-pileser III in 732 BC. Re-examination of Yadin’s case for this date shows that it was merely an assumption on his part. A review of the dating evidence – partly historical but principally the input from the independently dateable archaeological chronologies of Cyprus, Phoenicia, Mesopotamia and Egypt – suggests that Hazor V fell much later than 732 BC. Consequently both the Yadin (“high”) and Finkelstein (“low”) models for the chronology of Iron II Hazor are working from an incorrect baseline. A model is offered here which, while arguing a shift of the Iron IIA period from the tenth to ninth century BC, does not unduly compress Strata X-VII, closes the alleged long settlement gap at the site during the Neo-Babylonian to Early Persian period and resolves numerous dating anomalies arising from imported finds.

Keywords: Hazor – Iron Age II Chronology – Samaria – Megiddo

Resumen: El pretendido “punto de anclaje” de 732 a.C. para la destrucción de Hazor V

Todas las discusiones previas sobre la cronología de Hazor en la Edad de Hierro consideran como un “punto de anclaje” la destrucción de Hazor V por Tiglatpileser III en 732 a.C. Una reexaminación del caso de Yadin para esta datación muestra que era sólo una mera conjetura de su parte. Una revisión de la evidencia para la datación – en parte histórica pero principalmente la clave para las cronologías arqueológicas independientemente datables de Chipre, Fenicia, Mesopotamia y Egipto – sugiere que Hazor V cae mucho más tarde que 732 a.C. Consecuentemente, tanto el modelo de Yadin (“alto”) como el de Finkelstein (“bajo”) para la cronología del Hierro II de Hazor trabajan desde un punto de partida incorrecto. Aquí se ofrece un modelo que, mientras sostiene un cambio del periodo del Hierro IIA del siglo IX al X a.C., no comprime

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excesivamente los estratos X-VII, cierra el supuesto largo hiato de asentamiento en el sitio que abarca del periodo Neo-Babilónico al Persa Temprano y resuelve numerosas anomalías en la datación, producto de los hallazgos importados.

Palabras Clave: Hazor – Cronología de la Edad del Hierro II – Samaria – Megiddo

The long-standing disputes over the chronology of Iron IIA have frequently involved the site of Hazor. According to the excavators, the Iron Age IIA city, Stratum X, represented Solomonic activity of the tenth century BC.¹ Kenyon raised doubts, as the pottery of Stratum X bears great similarities to that of Samaria Pottery Periods 1 and 2, dated by her to the time of Omri and Ahab in the early ninth century BC. Accordingly, she placed the pottery of Hazor X (though not the architecture) in the ninth century BC.² Wightman went further, arguing that Stratum X was built in the ninth century.³ The Kenyon/Wightman position was criticised, and the Yadin chronology supported, by both Finkelstein and Stager.⁴ More recently Finkelstein, while still eschewing the Kenyon/Wightman arguments from Samaria, has also argued that Hazor X was built in the early ninth century, a major plank of the so-called Tel Aviv “low chronology” for Iron IIA.⁵ Meanwhile, the traditional Yadin chronology for Hazor has been vigorously defended, notably by the current excavator, Ben-Tor, and by A. Mazar. The present paper concentrates on Hazor’s part in the Iron IIA debate, and in particular the key, and overlooked, role played by the dating of Stratum V.

Stratum Date

X	10 th century BC
IXB-A	Early 9 th century
VIII	9 th century
VII	9 th century
VI	Early 8 th century
VB	8 th century
VA	8 th century - to 732 BC
IV	End of 8 th century
III	Early 7 th century
II	4 th century
I	3 rd -2 nd centuries BC

Table 1.

Dating of the Hazor strata, after Yadin (1972: 200).

¹ Yadin *et al.* 1958; 1960; 1961.

² Kenyon 1964.

³ Wightman 1990.

⁴ Finkelstein 1990; Stager 1990.

⁵ Finkelstein 1999.

HAZOR X – V: STRATIGRAPHIC CONGESTION?

A major objection to Finkelstein's version of a low chronology is that it "creates an impossibly brief duration" for Hazor Strata X-V. The point, first raised by Mazar,⁶ was echoed by Zarzecki-Peleg⁷ and elaborated by Ben-Tor and Ben-Ami:

*"Lowering the date of various ceramic assemblages from the tenth to the ninth century results, for one thing, in a 'dense' stratigraphy for several sites; too many strata have to be 'squeezed' into too short a period of time. The most extreme example is Hazor, where six strata [X-V], with approximately ten sub-phases, would have to be placed within a period of about 120 years [sic]. Whereas at Lachish and Megiddo, for example, the 'life span' of each of the Iron Age strata is approximately 80-100 years, the low chronology would allow for each of the six Iron Age strata at Hazor, with several sub-phases in each, a duration of only approximately 25 years."*⁸

The matter has also been aired by Tappy, when discussing the similarities between the pottery of Hazor X and that of Megiddo VA-IVB and Samaria Pottery Period 1 (hereafter PP 1, etc). In Tappy's opinion there are not enough known historical pegs to explain the succession of five cities at Hazor "prior to the final massive depredation at the hands of Tiglath-pileser III in 732 BCE (Stratum V)," unless one dates the end of the earliest (Stratum X) "at least in the late tenth century BCE":

*"For this reason I disagree with Kenyon's attempt to force the pottery of Stratum X into the ninth century so as to support her desired dating of the PP 1 material at Samaria. Instead, I keep for the present both the architecture and pottery of Hazor X and Megiddo VA-IVB in the tenth century BCE and accept their Solomonic origin."*⁹

⁶ Mazar 1997: 161; repeated in Coldstream and Mazar 2003: 42 and Mazar 2005: 25.

⁷ Zarzecki-Peleg 1997: 283-284.

⁸ Ben-Tor and Ben-Ami 1998: 32. The "120 years" given here is clearly a typographical error for 150 (= 6 x 25).

⁹ Tappy 2001: 242, n. 82.

Conversely, Tappy used the comparanda from Hazor X to dispute Kenyon's ninth-century date for Samaria PP 1, preferring to attribute it to a pre-Omride settlement dating back to at least the 10th century BC.¹⁰

The problem thus has considerable ramifications. These extend to the dating of strata throughout ancient Israel generally, and inevitably embroil the familiar question of the relationship between text and archaeology. The arguments concerning the character (even reality) of the United Monarchy (Saul, David, Solomon) will continue to run, as they depend largely on the disputed interpretation of biblical text (with oblique support provided by the Tel Dan inscription) and will remain archaeologically unfocussed until the chronology of Iron I-II is resolved.¹¹ There can be, however, no dispute regarding the historicity of the Omrides: Omri, Ahab and Jehu are attested as important regional kings in contemporary extra-biblical records, such as the Mesha Stela and Assyrian royal annals.¹² Hence there can be no disagreement over whether the Omride dynasty should be identifiable archaeologically. The question remains one of which strata we should associate them with. Here some other complex issues are involved – such as the relationship of buildings to pottery at the key sites of Samaria and Jezreel (see further below).

While these difficult issues remain unresolved, and as attempts have been made to clarify them by reference to Hazor, the spacing of Strata X to V takes on further significance. A 25-year average duration for Hazor X-V indeed seems rapid, even given that the city would have been a focal point of the Israelite-Aramaeans wars of the ninth to early eighth centuries BC. In reply to Ben-Tor and Ben-Ami, Finkelstein suggested that some of the strata at Hazor “may represent raising of floors and slight changes in the plan” and that there are only three major strata at Hazor for the period in question: X-IX, VIII-VII and VI-V.¹³ Yet his belief that we should “eliminate this futile argument from future discussions of Iron II chronology” was sharply dismissed by Ben-Tor, who reiterated the statement that we are not dealing with merely six strata, but ten sub-phases within those.¹⁴ An impasse has been reached between these two scholars on this point.

¹⁰ Tappy 1992: 30, 33, 34, 47, 69, 97-101.

¹¹ James 2002a.

¹² See conveniently Kitchen 2003: 16-18, 34-36.

¹³ Finkelstein 2000a: 242. Wightman (1990: 11) also argued that Hazor X and IX should be treated as one stratum. But, NB, there are also questions of pottery development to consider as much as the number of strata. As Yadin *et al.* 1960: 16 noted “there is a noticeable difference between the pottery of Stratum VIII and that of Stratum VII.” (Cf. Mazar 1997: 165, n. 9.) For example, the cooking pots 6.2/6.3 of Hazor X-VIII are replaced in Stratum VII by a new type, 6.5 (Gal 1992a: 77-78).

¹⁴ Ben-Tor 2001: 303.

But has the argument about the compression of strata been necessary in the first place? The calculations of both sides in the debate depend on the same base line, the assumed date of 732 BC for the destruction of Stratum V. Since it forms the bedrock for all reconstructions of Hazor’s Iron Age chronology, a re-examination of this date is long overdue.

THE ASSYRIAN CONQUEST, 732 BC

Typically, the destruction of Stratum V in 732 BC is referred to as the “only real anchor at Hazor”. Hazor is also seen as the main site, along with Megiddo, providing a “reliable chronological anchor for the late 8th century BCE” in the northern kingdom of Israel.¹⁵ More broadly, it is accepted that Hazor V helps to provide one of the only two safe “anchor points” in the debate over Iron Age chronology. The upper point (12th century BC) is provided by finds of the Egyptian 20th Dynasty in the earliest Iron I levels; the lower (late eighth century BC) is provided by the “identification of strata which were destroyed in the course of the Assyrian campaigns to Palestine,” notably Lachish III, Hazor Va and Megiddo IVA.¹⁶ The former is usually assumed to have been destroyed by Sennacherib in 701 BC,¹⁷ the northern sites by Tiglath-pileser III during his invasion of Israel in 732 BC. The capture – though strictly speaking not destruction – of Hazor by Tiglath-pileser is specifically referred to in 2 Kings 15:29.

Regarding the link between the end of Hazor V and the Assyrian campaign of 732 BC, Finkelstein cites Yadin’s Schweich lecture.¹⁸ Yet this is the most that Yadin stated there on the matter:

*“Stratum V contained the remains of the last Israelite fortified city, covered here, like the other areas, with heavy layers of ashes. There can be no doubt that this destruction should be attributed to the destruction by Tiglath-pileser III.”*¹⁹

The site publication provides no further argument:

¹⁵ Finkelstein 2000a: 242-243; Finkelstein, Ussishkin and Halpern 2000: 322.

¹⁶ Finkelstein 1996: 179-180; cf. Mazar 2005: 19.

¹⁷ The assumption has been questioned as it does not rely on any new evidence as such, but the lack of evidence from earlier strata for a presumed Assyrian destruction (James *et al.* 1991a: 176-178; James 2004: 53; James 2007: 214).

¹⁸ Finkelstein 1996: 180.

¹⁹ Yadin 1972: 112-113.

*“Stratum V ended in complete destruction; the buildings were burnt down and not rebuilt. This destruction must surely be attributed to the campaign of Tiglath Pileser III in the year 732 B.C. when Hazor was among the cities he captured (II Kings XV, 29).”*²⁰

And that is all.²¹

Why then did Yadin choose to date the end of Stratum V to 732 BC? The date was not arrived at by working backwards from the earliest unequivocally dated level, the Persian-period Stratum II of the fourth century BC (see below). Rather than assigning the preceding phases to the early Persian or Neo-Babylonian periods he ascribed Strata III and IV to the Assyrians (seventh and late eighth centuries respectively), in the latter case because of evidence of clear continuity from Stratum V, the end date of which was already assumed to be 732 BC. Yadin’s dating thus left a considerable gap in occupation during the Babylonian and early Persian periods.

It would appear, then, that Yadin dated the end of Stratum V to 732 BC as it provided the earliest destruction of a major settlement from before the Persian period. While a reasonable guesstimate, it may be flawed in that it does not allow for destructions at the site in the centuries following the conquest of Tiglath-pileser III. There were, after all, further Assyrian campaigns in the region and there is evidence, for example, that Assurbanipal had to put down revolts in Palestine in the 650s or 640s BC.²² According to Ezra 4:2 and 4:10, both Esarhaddon and Assurbanipal (the “great and noble Asnapper”) settled deportees from Mesopotamia and Iran in the province of Samaria (*Samerina*); these transplants could have been a response to rebellions, and may have also affected the more northerly Assyrian province of *Magidu* (including Megiddo and Hazor). As Forsberg stressed, we should not ignore the possibility of site destructions during the Scythian incursion a generation later, c. 630 BC; nor, for that matter, should we overlook the resulting conflict in Palestine between the Scythians and Saite Egyptians, dated by Spalinger between 622-616 BC.²³ The struggle between the Saite and Neo-Babylonian empires for control of

²⁰ Yadin *et al.* 1958: 22.

²¹ Hazor VA produced an inscription, *lpqh* (“belonging to Pekah”), incised (after firing) on the shoulder of a storage jar (Yadin *et al.* 1960: 73, Pls. CLXXI; CLXXII; Yadin 1972: 190, Pl. XXXVd). It is sometimes implied (e.g. Yamauchi 1974: 718) that this was Pekah, the penultimate king of Israel, which would have provided confirmation of Yadin’s dating of this stratum. However, the absence of any royal title or iconography, plus the occurrence of a similar inscription *ldhyw* (“belonging to Delayo”), not a king, renders an identification with King Pekah unlikely.

²² Malamat 1953; Rainey 1993: 157-162; Forsberg 1995: 35.

²³ Forsberg 1995: 35-36; Spalinger 1978: 52.

the Levant involved warfare and the destruction of cities from Carchemish to Philistia, and was not resolved until c. 570 BC.²⁴

As a supporting argument for his dating of Stratum V, Yadin could point to the fact that the following Stratum IV was a much smaller settlement than its predecessor; it had no fortifications, while new buildings were poor. As "the pottery associated with these structures is for all practical purposes, identical with that of V," Yadin deduced that it represents a "*short-lived effort by the Israelite inhabitants to renew the settlement destroyed by Tiglath-pileser III.*"²⁵ While the historical link is plausible, one might expect similar circumstances – a reduced, smaller settlement with poorer structures – after any destruction by invaders. Yadin did not support his identification by reference to changes in material culture, such as the introduction of new architecture or pottery. From the negligible changes in the pottery repertoire there are no grounds for suggesting that a new population of deportees was introduced, as suggested, for example, by Geva for Samaria VI and Megiddo III.²⁶ The few finds of Assyrian or Assyrian-style pottery from Hazor are actually problematic for Yadin's interpretation (see below).

Thus Yadin's only substantive point in associating the destruction of Hazor V with the Assyrian conquest remains the huge layer of ash and rubble, up to one metre in height, which covered it and the decline evident in the succeeding settlement. But while we may fully expect an (assumed) Assyrian destruction to have been violent, in itself this is, of course, not diagnostic enough evidence to reassure us that we have identified the correct level.

The consensus dating of Hazor V cannot be upheld simply by reference to ceramic comparisons from other Israelite typesites, such as Megiddo and Samaria. Similar assumptions to that at Hazor V have controlled their late Iron Age chronology – involving the idea that any major pre-Persian destruction automatically represents the Assyrian conquest of the eighth century BC and hence provides an 'anchor point' for dating earlier and later strata. Yet recent studies have eroded the traditional understanding that 'Assyrian destruction levels' have been confidently identified and/or dated at Samaria

²⁴ Kuhrt 2002: 23-24. Elsewhere (briefly James 2004: 54-55; in more detail James 2006; James in press) I have questioned the usual dating of the destruction of Ekron IB and pre-Persian Ashkelon to the 604 BC campaign of Nebuchadrezzar, suggesting instead a date ca. 570 BC during further wars with the Saites – one which would be more in line both with the dating of Ekron IC suggested by the Assyrian dating of the Ekron temple inscription (James 2005a) and with the growing case for a reduction of the dating of Archaic Greek pottery (of the kind found at Ekron and Ashkelon) by some 35 years at ca. 600 BC (James 2003; James 2005b; James 2006; James in press; see also Bowden 1991; Bowden 1996; Gill 2005; Gill in press).

²⁵ Yadin 1972: 191.

²⁶ Geva 1979.

and Megiddo.²⁷ The chronology of the Iron II strata at these two key sites is in now a considerable state of flux. We will return to Megiddo and Samaria last, avoiding the frequently circular arguments of cross-dating among the three sites that have hitherto dominated the literature. There is a way to cut through such circular arguments at Hazor – by using the controlling information provided by external chronologies, namely those of Phoenicia, Cyprus, Egypt and Mesopotamia, as well as historical evidence.

ASSYRIAN DESTRUCTION, V OR VII?

It therefore emerges that Yadin offered no conclusive evidence for dating the fall of Hazor V to 732 BC and external evidence shows there are serious problems with this dating. The most conspicuous is raised by Phoenician ceramic chronology, which strongly suggests that Hazor VII should be dated to the mid-eighth century BC,²⁸ the slot presently occupied by Hazor V on the Yadin/consensus dating.

Hazor has long been embroiled in a debate concerning the provenance and dating of the so-called ‘torpedo’ storage jars (a.k.a. ‘sausage’ or ‘crisp ware’ storage jars). This form first occurs significantly at Tyre in Stratum III,²⁹ dated to the second half of the eighth century BC.³⁰ Yet there are numerous examples

²⁷ Samaria – Forsberg 1995: 17-50; Tappy 2001: 349-441; Megiddo – Finkelstein, Ussishkin and Halpern 2000: 322, 563-65, 598.

²⁸ See briefly James *et al.* 1998: 30.

²⁹ According to Bikai (1978b: 48), “It was not common in Stratum IV (and might be intrusive there), but in Strata III-II, over 80% (c. 527 pieces) of all storage-jar rim fragments were of this type.”

³⁰ In earlier publications Bikai considered a date of c. 740 BC for the beginning of Tyre III. Most recently Bikai (2003: 234) raised this date to c. 750 BC. This minimal adjustment does nothing to solve the problem of the occurrence of Tyre III type storage jars in Hazor VII and VI (both pre-760 BC, conventionally). But the reasons for Bikai’s redating need to be noted. First, Salamis Tomb 1 and the last part of Tell Abu Hawam Str. III contain Phoenician pottery known from the Tyre IV-V horizon. Salamis 1 also has Greek imports dated conventionally to the second quarter of the eighth century BC. “Lacking a reason to redate the Greek material in the tomb, one presumes that Tyre IV-V is of equivalent date.” (Bikai 2003: 234). Likewise Bikai cites Balensi and Herrera’s redating of Tell Abu Hawam to c. 900-750 BC, concluding that “between the evidence from Tell Abu Hawam and Salamis Tomb 1, there seems to be an anchor for Tyre IV-V in the first half of the eighth century, raising the estimated dates from 760?-740 to 800-750 BC. Here Bikai was unfortunately unaware of the mounting case for lowering the dates of Greek Geometric (see note 48 below). Regarding Tell Abu Hawam III, Balensi (1985) has also considered an end date as late as 650 BC, with a possible compromise at c. 700 BC at the invasion of Sennacherib (pers. comm. cited in James *et al.* 1991a: 361, n. 48). Besides, as one of Balensi’s stated reasons for lowering the dates of Stratum III are links with Tyre, the argument becomes somewhat circular! Second, Bikai reasons that her old scheme put “too much activity in the eighth century.” Granted, but this would be better alleviated by allowing Tyre III-II to range into the seventh century (following the Egyptian evidence, see main text), rather than by raising the dates of Tyre VI-VII from 800-760? to the late ninth century as she now proposes.

from Hazor VI,³¹ conventionally dated to the early eighth century, and a few from Stratum VII dated to the late ninth century.³² Hence Geva questioned their Phoenician origin, on the grounds that they begin to appear earlier in Israel.³³

Bikai firmly objected to this early date and defended the Phoenician origin of this type, which represents over 80% of the storage jars known from Tyre III-II. A Phoenician origin is also clearly indicated by their distribution. While there are over 60 examples (either rims or whole vessels) known from Hazor and 169 from Megiddo, far greater numbers come from Tyre (300), Sarepta (258) and the late eighth-century Phoenician shipwrecks *Tanit* (385) and *Elissa* (396). Bikai noted that finds of torpedo jars at Phoenician colonies such as Kition and Carthage also argue for a Phoenician origin,³⁴ an observation accepted by Ballard *et al.*, especially in light of the new shipwreck finds. They further observe that

*"... these amphoras are purpose-built maritime containers. They are built to be easily stacked in the hold of a ship, to have consistent capacity, and to be easily tied down using special handles. This argues for a production facility familiar with the needs of maritime transport."*³⁵

Geva claimed that neutron activation study had shown that the torpedo jars from Tyre were not locally made, as the composition of their clay did not match that of an unfired clay sample from "*the assumed potter's workshop nor the Cypriot pottery*".³⁶ This argument was defused by Bikai, who pointed out that the composition of the single sherd analysed was actually close to that of the unfired clay, such difference as there is being possibly explainable "by the potter's addition of various materials to the clay." Further, in Bikai's opinion, Tyre Stratum III was effectively a dump for wasters and other debris from a pottery manufacturing area. A high percentage of the torpedo jar sherds were cracked or defective: "Such unusable pottery would hardly have been moved very far from the place of production." At Sarepta kilns were found

³¹ Yadin *et al.* 1958: Pl. LXV, 13; Yadin *et al.* 1960: Pls. LXXII, 1-9, LXXIII, 1-17; Yadin *et al.* 1961: Pl. CLXXXVI, 11-12, 15, 17-19; Ben-Tor *et al.* 1997: 264, Fig. III.40, nos. 1, 5.

³² Yadin *et al.* 1961: Pl. CLXXX, 19-20, 23. Two examples are also reported from Stratum VIII (Yadin *et al.* 1960: 13-14, Pl. LX, 9-10; Tappy 2001: 163, n. 606), though the excavators – without specifying why – were cautious of their attribution.

³³ Geva 1982.

³⁴ Bikai 1985: 72.

³⁵ Ballard *et al.* 2002: 160 and 158-159, 161 for distribution figures.

³⁶ Geva 1982: 69; Bieber in Bikai 1978.

near similar dumps of storage jars.³⁷ Final confirmation of the Phoenician manufacture of torpedo jars comes from two recent neutron activation analyses. The petrographic profile of the shipwreck examples matches the distinctive pattern, of the central Levantine coast.³⁸ The three torpedo jars analysed of the ten known from Lachish III have the same provenance.³⁹

The question remains whether the examples from Hazor are Phoenician or locally made. Gilboa has argued against a trade model, observing that there are tiny, but consistent, morphological differences between the torpedo storage jars from the two sites,⁴⁰ a conclusion that has received some support from profilograph copying: “*the jars from each city are far more similar to those from the neighbouring area.*”⁴¹ Computerised study of the curvature of all the published torpedo jars from Tyre and Hazor has led to this conclusion:

*“... the lack of a significant morphological overlap raises doubts about the claims that the ‘torpedo jar’ assemblages indicate commercial links between Hazor and Tyre, as suggested by Geva. The higher inner similarities observed in the assemblage of jars at Tyre supports the possibility that they were produced locally, as suggested by Bikai, possibly in a single workshop. There is still a possibility that some of the jars found at Hazor were actually made in Tyre, and perhaps even one or two of the ones found at Tyre were Hazorite. ... but the ‘torpedo jar’ phenomenon as a whole does not indicate mass trade in any one direction.”*⁴²

Whether this solves the problem is far from certain. Products for home and export markets can often differ slightly, and might be made in different workshops. Further the high inner consistency of the Tyrian examples, the mass of other evidence (including distribution and petrography) that the ‘torpedo jar’ was primarily a Phoenician shape, and the possibility (allowed by Gilboa *et al.*) that some of the jars from Hazor *were* made at Tyre, would allow this at the very least: that such vessels were initially imported from Phoenicia and were copied locally in Israel. Finally, questions of trade aside, the chronological tension remains: on the present high chronology for Hazor, the production of highly distinctive and virtually identical vessels begins nearly a century earlier there than at nearby Tyre.

³⁷ Bikai 1985: 71.

³⁸ Ballard *et al.* 2002: 160 and n. 18.

³⁹ Goren in Ussishkin 2004: 2562, 2588; for discussion James 2007: 216.

⁴⁰ Gilboa 1995.

⁴¹ Watzman 2004: 97.

⁴² Gilboa *et al.* 2004: 692.

The *floruit* of the torpedo storage jars in Phoenician terms can be placed within close parameters following the Tyrian chronology established by Bikai. As stressed, the dating of Tyre III is corroborated by a complex of mutually supporting evidence which we defined as a late eighth-early seventh century "Cypro-Phoenician horizon."⁴³ The discovery of an inscribed Egyptian urn of the late 25th or 26th Dynasty from the closing stage of Stratum III allowed Bikai to set the end of this stratum no earlier than 725 BC, or possibly later.⁴⁴ In the opinion of Robert Morkot (pers. comm.), and in agreement with de Meulenaere,⁴⁵ the urn is much more likely to belong to the seventh century than the eighth, which would require a somewhat lower date for the end of Stratum III.⁴⁶ In any case, a late eighth to seventh century date for Tyre III is corroborated by its Cypriot imports – these belong to the Cypro-Archaic I period, initially dated by Gjerstad to 700-600 BC, and revised by Karageorghis to 750-600 BC, with Gjerstad accepting a compromise start date of 725 BC.⁴⁷ The chronology of Cypro-Archaic I is fixed by synchronisms with approximately secure evidence from Greece (Late Geometric-Archaic) and by Egyptian scarabs.⁴⁸

Though there is evidence for some lowering of the dates for Tyre III-II, there is none for raising them. Indeed, it is now generally accepted that the chronological distribution of torpedo-jars "is heavily weighted toward the middle to end of the eighth century" or the "three final decades of that century."⁴⁹ As it would be difficult – rather, impossible – to raise the dates of the Tyrian torpedo jars, we suggested that the logical thing to do "would be to lower the chronology of Hazor VII and associated levels at other sites."⁵⁰ This would mean placing Hazor VII no earlier than the mid-eighth century BC, the very time in which Yadin placed Hazor V. Consequently we should consider that it was Hazor VII, rather than V, which fell to Tiglath-pileser III in 732 BC; Hazor VII was indeed destroyed by a widespread fire, the very kind of destruction sought by Yadin for the Assyrian conquest. Such a suggestion is radical, but

⁴³ James *et al.* 1998: 30.

⁴⁴ Bikai 1987: 69; cf. 1978a: 68; 1978b: 47; 1981: 33; cf. James *et al.* 1991a: 108.

⁴⁵ In Bikai 1978b: 84.

⁴⁶ This would go a step towards the suggestion of Gal (1992a: 73-74; 1992b: 184), that Tyre IV-I all postdate 700 BC.

⁴⁷ See conveniently James *et al.* 1991a: 152-3.

⁴⁸ See James *et al.* 1991a: 367, n. 37; 1998: 30. NB, if anything, Late Geometric dates are too high, by some 25 or more (James *et al.* 1991a, 111; James 2003: 241-243; James 2005b.) Such a reduction has been accepted as plausible by I. Morris (1993: 30-31), while S. P. Morris (1998: 362) has argued that the "Geometric period lasted well into the seventh century."

⁴⁹ Ballard *et al.* 2002: 158; Tappy 2001: 164.

⁵⁰ James *et al.* 1998: 30.

can be tested through examination of the dating evidence (both from external chronologies and historical considerations) for the preceding and succeeding strata. These are discussed below in ascending stratigraphic order.

STRATA X-IX

A placement of Hazor VII in the mid-eighth, rather than the late-ninth century, would automatically resolve the “dense stratigraphy” problem created by the Finkelstein model (as perceived by Ben-Tor, Mazar, etc.). It would place four (X-VII) rather than six (X-V) strata within the ca. 150 years between the early ninth century and 732 BC, giving a reasonable average of nearly 40 years per stratum for these substantial levels (approximately the same as that on the Yadin/Ben-Tor chronology). A more relaxed, and arguably more ‘natural’, chronology can then be offered for strata X-VII.

There is no need to rehearse the arguments for the unhinging of Stratum X and its gateway from its traditional “Solomonic” date.⁵¹ Likewise the similarities between the pottery of Hazor X and the more recently excavated compound at Tel Jezreel have been much discussed. These were established by Zimhoni and caused considerable stir, as ceramic types normally dated to the 10th century (Megiddo VA-IVB and Hazor X) were discovered at a site which appeared to date to the early to mid-ninth century BC, creating a discrepancy of “*eighty years or more.*”⁵² The historical dating of Jezreel is reasonably secure. The short-lived fortified enclosure has been convincingly linked (from biblical references) to the Omride dynasty⁵³, more specifically Ahab (875/874-853)⁵⁴ who had a royal residence there (1 Kgs. 21:1; 1 Kgs. 18:45-46). As Omri (886/5-875/4 BC) built Samaria during the second half of his short reign (1 Kgs 16:23-24), it was most likely Ahab who built the enclosure at Jezreel. Who or what was responsible for its destruction is harder to ascertain. Ussishkin and Woodhead dated it to the rebellion of Jehu (841 BC), when the latter slaughtered the family, officials and priests of Ahab at Jezreel (2 Kgs. 9-10). Yet despite the bloodshed, remembered in later times (Hoshea 1:4), there is no reference to Jehu destroying the city, and Na’aman has rightly asked why Jehu should have levelled such a strategically located strongpoint and administrative centre. Accordingly he links the destruction

⁵¹ James *et al.* 1987: 64-65; Wightman 1990; James *et al.* 1991a: 189-190; Finkelstein 1996: 178-79; Finkelstein 1999: 57-59.

⁵² Zimhoni 1997: 39.

⁵³ Na’aman 1997: 122-129.

⁵⁴ The dates summarised by Kitchen (2003: 30) are used here for convenience.

with the invasions of Hazael of Damascus, ca. 835 BC,⁵⁵ though a later date during the continued Aramaean/Israelite wars is equally plausible.

Assuming that the compound of Jezreel was built by Ahab ca. 875-850 BC and destroyed ca. 835/800 BC, the Iron IIA pottery found on its floors should have a similar date range. Ben-Tor challenged the integrity of the site, which is widely disturbed by later Byzantine construction, arguing that the assemblages from the eight key loci discussed by Zimhoni are "archaeologically unreliable" and may be considerably mixed with pottery from an earlier phase of settlement. Ussishkin fully answered this charge, showing in detail that each of the contexts involved satisfies Ben-Tor's own requirements for an "archaeologically reliable" locus (clearly defined on all sides, usually by walls; recognizable floors adjoining walls; restorable pottery). Nor is there any evidence of a preceding large-scale settlement. Ussishkin seems justified in his conclusion: "*Assuming that the Jezreel enclosure can be reliably dated to the period of the Omride dynasty it follows that the Jezreel enclosure and its pottery assemblage can be used as a 'key site' in the current debate on Iron Age chronology...*"⁵⁶

Finkelstein offered some general resemblances between the plan and architecture of Jezreel and Hazor X,⁵⁷ but after criticism from Ben-Tor, he stressed that these were secondary observations, made only "after the date of Hazor X in the early 9th century was established."⁵⁸ For this he followed Zimhoni's lead from Jezreel, dating Hazor X to the "late 10th, or better, to the early 9th century BCE".⁵⁹ Yet Finkelstein's version of a "low chronology" results in an extremely cramped sequence for the ninth century (because of its essential adherence to Yadin's Hazor VII dating):

- Stratum X, early 9th century (Omride)
- Stratum IX, first half of 9th century (Omride)
- Stratum VIII, late 9th century
- Stratum VII, end of 9th century

There are clearly problems with this scheme. Where Yadin had three ninth century levels, Finkelstein has four – the essence of the complaint of

⁵⁵ Ussishkin and Woodhead 1992: 53; Na'aman 1997: 122-129.

⁵⁶ Ben-Tor 2000: 12-13; Ussishkin 2000: esp. 255.

⁵⁷ "A possible support for dating Hazor X to the days of the Omrides is the resemblance in layout and construction between this city and the Jezreel enclosure: both constitute a levelling/filling operation, a casemate wall, a moat and a four-entry gate ..." – Finkelstein 1999: 60; see also 2000b: esp. 128-129.

⁵⁸ Finkelstein 2000a: 240.

⁵⁹ Finkelstein 1999: 59. To this Finkelstein (1996) added his observations regarding a lower date for Philistine ware, with a knock-on effect for Israelite Iron IIA pottery. These claims have not been widely accepted and I hope to discuss them in detail elsewhere.

Ben-Tor and others regarding a compressed stratigraphy. While the case for dating the construction of Stratum X to the Omride dynasty is sound (from the comparisons with the Jezreel pottery), the dates for its first king (Omri) are 886/5-875/4 BC. One wonders, then, what to make of Finkelstein's two strata assigned to the early Omride dynasty, to the "early 9th" (X) and "first half of ninth" (IX) century respectively; the two would occupy only 35 years.

By compressing both Stratum X and IX into the first half of the ninth century, Finkelstein's scheme also overlooks a potentially valuable synchronism offered by Cypriot pottery. Both Hazor X and IX produced pieces of Cypriot Black-on-Red (B-o-R) I ware.⁶⁰ This was dated by Gjerstad to ca. 850-700 BC, a chronology followed until very recently by Cypriot archaeologists, with the difference that the terminal date was raised by Karageorghis to ca. 750 BC.⁶¹ This synchronism with Cypriot chronology naturally clashes with the 10th-century BC date for Hazor X advocated by Yadin, Ben-Tor and others. The present writer and colleagues have repeatedly drawn attention to this problem, in which ninth-century Cypriot pottery is found in "10th", or even "11th" century Palestinian strata.⁶² Our suggestion was that, if we follow the

⁶⁰ The clear B-o-R I finds at Hazor are as follows: Yadin *et al.* 1961: Pl. CLXXII, 1 (Hazor Xb); CLXXIV, 15 (Stratum Xa); CLXXVII, 14 (Strata X-IX); Yadin *et al.* 1958: 11 and Pl. XLVI, 1-2 (Strata X-IX); Yadin *et al.* 1960: Pl. LII, 17 (Stratum IX); Yadin *et al.* 1961: CCVIII, 38-40, 44 (Stratum IX), see also CCCLV, 11-12, 14, 16-17; Yadin *et al.* 1958: L, 14. For a detailed examination of the contexts see Schreiber (2003: 104-111, 189-195), who concludes "that BoR pottery first appears in loci assigned to Stratum X." The implication in Tappy 1992: 130, n. 128 that further finds in Strata V-IV are also B-o-R I (Yadin *et al.* 1960: Pls. LXXV, 11-12; LXXXVIII, 1-4; XCVII, 10 [Stratum V]; C, 22 [Stratum IV]; Tappy mistakenly gives C, 23) is misleading. Amiran 1969: 290, Pl. 98, 9-12 classifies them separately from B-o-R I, II and III, and in the opinion of N. Kokkinos (pers. comm.) they may be late, derivative forms. Regarding these and other possible B-o-R pieces from Hazor VIII-III, Schreiber 2003: 194 stresses that "the vessels were generally preserved in a highly fragmentary form, seldom more than a sherd. Only one almost complete vessel was found, in a Stratum VI context."

⁶¹ See conveniently James *et al.* 1991a: 153, 366, n. 35; Schreiber 2003: 233-234, and n. 16.

⁶² James *et al.* 1991a: 155-161; James *et al.* 1991b: 229-230, 234; James *et al.* 1992: 130; James *et al.* 1998: 30-32. Our statements regarding the importance of this problem have been acknowledged by, *inter alia*, Sörenson 1993 and Schreiber 2001. The only adherents of the "Tel Aviv low chronology" to have remarked at length on the B-o-R problem are Gilboa and Sharon (2001: 1348-1349; 2003: 62-67). They agree with our dates for the related question of the start of Iron IIA, yet perhaps to avoid the impression that they have borrowed our arguments, have misrepresented our position. Gilboa and Sharon 2003: 72, n. 31 and Gilboa, Sharon and Zorn 2004: 54, n. 40 state that we date the end of the LBA to the mid ninth century and lower the "beginning of the Iron IIA to the early eighth century." Our suggested end for the LBA was ca. 950 BC. Re Iron IIA, echoing (and acknowledging) the work of predecessors (mainly Kenyon), we have long argued (before the arrival of the "Tel Aviv" school) that Iron IIA, conventionally dated c. 1000-900 BC began in the ninth century – one plank of the argument being the Cypriot (Gjerstad) dating of B-o-R I. For example, at Samaria we followed Kenyon in associating early Iron IIA with the building activities of the ninth-century Omrides. This is manifestly clear from our text (James *et al.* 1991a: 183-188), rather than Table 8:3 (James *et al.* 1991a: 195) which mistakenly showed Iron IIA beginning c. 800 BC. A corrected table was rapidly published in James *et al.* 1991b: 232, placing the beginning of Iron IIA at c. 850 BC. The same date

traditional Cypriot chronology, then Palestinian pottery horizons containing B-o-R must overlap the date of 850 BC.⁶³ If so, then the occurrence of B-o-R in Hazor X also clashes with the "Tel Aviv low chronology," which would place this stratum much earlier in the ninth century BC.

The B-o-R question returns us to Samaria. Kenyon initially established her dates for Samaria BP I and II from the biblical account, which states that the city was founded by Omri (1 Kgs. 16:23-8). Accordingly she dated Samaria PP 1 and 2, from the fills under the floors of these architectural phases, to the ninth century BC. She supported this by reference to Gjerstad's Cypriot chronology, as PP 2 included a fragment of a B-o-R I juglet.⁶⁴ Yet even Kenyon's much debated low dates for Samaria are somewhat too high.⁶⁵ As Tappy points out, since Kenyon believed that Samaria II was built by Ahab, PP 2 should precede some point within his reign (875/4-851 BC), which would conflict with Gjerstad's start-date of c. 850 BC for B-o-R I. Elsewhere we argued that Samaria II was more likely to have been built later than Ahab, probably under Jeroboam II (791/0-750/49 BC). After a thorough analysis of the earliest architecture at Samaria, Franklin has reached the same conclusion: Samaria BP I represents the palace of all the Omrides (plus Jehu), while BP II must be downdated to the eighth century.⁶⁶

Finkelstein is of the same opinion as Stager and Tappy, that the pottery from the PP 1 and 2 fills represents 11th-10th material from a pre-enclosure settlement at the site.⁶⁷ Yet as Zimhoni predicted, the matter will need re-analysis in the light of the Jezreel excavations, where the occurrence of "10th-century" pottery in a ninth-century compound provides a close analogy to the situation at Samaria.⁶⁸ It is not consistent methodologically (as Finkelstein does) to redate the 10th-century pottery at Jezreel to the ninth century while dismissing

for the beginning of Iron IIA and the arrival of B-o-R was argued in Gilboa and Sharon 2001: 1347 on the basis of radiocarbon. Our corrected table was, however, acknowledged in Gilboa, Sharon, Zorn 2004: 54, n. 39, with the remark that "it is compatible with ¹⁴C determinations for the Iron Age II horizon at Dor." While awaiting the detailed study of Chapman (in prep.) our present view is that Iron IIA likely began earlier, ca. 875 BC, though 875/850 BC may be a more realistic representation.

⁶³ Some have tried to resolve the issue by raising the Cypriot dates (see n. 71 below). The recent analysis by Schreiber unfortunately gives little weight to low chronologies for Palestinian contexts, and concludes that B-o-R first appeared "ca. 925 BC – at the earliest, ca. 940 BC." Nevertheless, appreciating that the dating of these contexts is not settled, she adds: "*An alternative possibility, that these levels should be dated from the early-mid ninth century, may, with future excavation, prevail.*" (Schreiber 2003: 309.) Another issue raised by Schreiber, regarding the chronology of B-o-R relative to that of other Cypriot ceramic types, is beyond the scope of the present study, but see Iacovou 2004.

⁶⁴ See Tappy 1992: 127, n. 113.

⁶⁵ James *et al.* 1998: 31.

⁶⁶ James *et al.* 1991a: 185-187; Franklin 2004; 2005.

⁶⁷ Finkelstein 1990: 114-116; 1996: 179; Stager 1990; Tappy 1992.

⁶⁸ Zimhoni 1997: 25.

similar material at Samaria as belonging to a pre-Omride settlement.⁶⁹ Rather the Jezreel finds make the Stager/Tappy/Finkelstein position seem redundant. Further analysis of PP 1 and 2 contexts is awaited,⁷⁰ but it is reasonable to make some preliminary deductions. If PP 2 is what Kenyon understood it to be, then it must largely represent occupational debris that accumulated after the building of Samaria I and fills for the building of Samaria II. Using the dates suggested above for the first two building periods (and now argued by Franklin), the period of PP 2 would fall roughly ca. 875-790/775 BC, providing support for Gjerstad's dating of B-o-R I.

The evidence from Cyprus combined with the excavation of two Omride sites (Samaria and Jezreel) makes a formidable case for dating the start of Iron IIA to the ninth century. The suggested alternatives have required problematic or *ad hoc* 'solutions' in each case: for Cyprus, the introduction of unacceptable chronologies,⁷¹ and for the royal enclosures of Samaria and Jezreel, insistence that the earliest pottery must be "residual" and unconnected with their building phases. To invoke Occam's razor, the least complicated answer is preferable: the "11th-10th" and "10th-century" pottery from Samaria and Jezreel respectively is Omride and dates to the ninth century, and the low Cypriot chronology is not in need of major upward revision.⁷²

Given this, the close of Hazor X (with B-o-R I) should fall later than c. 850 BC. There would then be no room for Hazor IX to also fall within the first half of the ninth century, as in Finkelstein's model; rather it would have to belong to the late ninth century. The end of Stratum X might then be attributed to the conquests of Hazael ca. 835 BC, while the burnt layer of Hazor IX could then be attributed to the continuing wars between the Aramaeans and Israelites during the late ninth-early eighth centuries BC.⁷³

⁶⁹ Finkelstein 2005: 37 rightly describes Ben-Tor's attempt (2000) to relegate the Jezreel pottery to an earlier phase at the site as "a desperate attempt to save the idea of a great United Monarchy." Relegating all the pottery of Samaria PP1 and 2 to a pre-Omride settlement might be seen as an equally desperate attempt to avoid acknowledging the British excavators of Samaria as the originators of the 10th-ninth century BC shift for Iron IIA – cf. Kletter 2004: 44.

⁷⁰ Chapman 2007; Chapman in prep.

⁷¹ The option experimented with by van Beek created a lengthy hiatus in the Cypro-Archaic period; the alternative (discredited) scheme of Birmingham involved an extraordinary raising of the start of Cypro-Classical from 475 to 600 BC! (For discussion and references see James *et al.* 1991a: 153-154.) Most recently Karageorghis (2002: 6; 2005: 104) has followed Coldstream (1999: 114-115) in raising the beginning of CG III from c. 850 BC to 900 BC, in order to shorten the rather blank CG II period from one hundred to fifty years (i.e. 950-900 BC). This seems ill-advised. A general lowering, rather than raising, of Cypriot 'Dark Age' dates would be a preferable solution (James *et al.* 1991a: 156-157).

⁷² Cf. Gilboa and Sharon 2003: 66-67.

⁷³ There is some confusion as to whether the ashes belong to Stratum IXA or IXB – see Schreiber 2003: 112.

STRATUM VIII

Regarding the next phase at Hazor, Yadin wrote:

*"The city of Stratum VIII is entirely different from that of Strata X-IX in layout, area, character, public buildings and installations. It has now become a strongly fortified city, with mighty walls, strong citadel, public store-houses and, above all, a huge underground water-system capable of sustaining the city through a long siege. The most salient contrast with the Solomonic city [Stratum X] is the fact that the Stratum VIII city covers the whole Tell, doubling its built up area."*⁷⁴

This well-organised and defended city was clearly built by a powerful ruler commanding considerable resources. Yadin's suggestion that it was built by Ahab made good sense, in the context of his chronology. Finkelstein attributes its construction to Hazael of Damascus, in the late ninth century. He supported this by reference to three ostraca written in "Phoenician or Aramaic" and the tentative reconstruction of the citadel of this stratum as a bit hilani. Neither is realistic as evidence that VIII was an Aramaean stratum.⁷⁵

On the model argued here, Stratum VIII falls between the late ninth (IX) and mid eighth centuries BC (VII), and can be reasonably associated with the renaissance of Israelite power under Jeroboam II, ca. 790-750 BC. He was said (2 Kgs. 14:25) to have restored Israel's lands in the north as far as "the entrance of Hamath," i.e. the Bekaa valley. Hazor VIII was described by Yadin as "not just an administrative city, with palaces for governors – but a well fortified city... capable of withstanding prolonged siege."⁷⁶ The establishment of a powerful stronghold at Hazor, strategically placed on the route to Syria, fits well with Jeroboam's expansionist policy.

Regarding the three ostraca from Stratum VIII, they could be Phoenician or Aramaean.⁷⁷ If Phoenician they might reflect traders from the Lebanon, which would match the appearance of Phoenician-style storage jars in this stratum (see note 32 above).⁷⁸ While such minimal evidence (the short inscriptions do not contain any whole words) tells us little about the population of Hazor at this time, one of the ostraca may provide an important dating clue. Ostrakon 3

⁷⁴ Yadin 1972: 165.

⁷⁵ Finkelstein 1999: 61; see Kletter 2004: 30.

⁷⁶ Yadin 1972: 164.

⁷⁷ Sass 2005: 85-86.

⁷⁸ Katzenstein 1997: 195-199) argues persuasively that Jeroboam II reached a new accord with Tyre, after the rupture in relations under Jehu. Kuan 2001: 149-150 dates the "covenant" between Israel and Tyre referred to by Amos (1:9) to early in the reign of Jeroboam.

begins with an apparent *bet*, the similarity of which to the *bet* on the Byblite inscriptions of Shipitbaal and the Abdo sherd is striking, as Sass notes, “eye-catching” enough to have been agreed by all commentators. The dating of the Byblite inscriptions has long been a matter of controversy, a focal point of discussion being the probable identity of king Shipitbaal with the like-named ruler of Byblos mentioned in the records of the Assyrian Tiglath-pileser III ca. 740 BC.⁷⁹ The identification was offered long ago by B. Mazar as a potential fixed point for dating Byblite palaeography.⁸⁰ However Yadin remarked that the sherd from Hazor VIII suggested a higher, ninth-century dating for the Shipitbaal inscription.⁸¹ Since then, Wallenfels offered palaeographic arguments for redating the relevant group of Byblite inscriptions from the 10th to the ninth-seventh centuries BC; the Abdo sherd and king Shipitbaal would be assigned to the eighth century BC, strengthening the case for identifying him with the ruler known from ca. 740 BC.⁸² Sass has argued a similar though slightly earlier range for the group, but because of the Hazor sherd suggested a dating for Shipitbaal in the last third of the ninth century BC, eschewing the synchronism with Assyria.⁸³ Yet if Stratum VIII actually belongs to the first half of the eighth century, the identification of the two Shipitbaals, as argued by Mazar and Wallenfels, once more comes into focus. Once again, independent arguments from Phoenicia (in this case palaeographic and historical considerations), suggest that the dating at Hazor is too high.

STRATUM VII

Hazor VII was a continuation (with some signs of decline) of the well-built Stratum VIII city.⁸⁴ A mid-eighth century date has already been argued (above), on the basis of the torpedo-jar finds from Strata VII and VI. As noted, the signs of widespread destruction by fire in Stratum VII would suit well the assumption that Tiglath-pileser III destroyed Hazor in his campaign of 732 BC.

⁷⁹ See conveniently James *et al.* 1991a: 248-251.

⁸⁰ Mazar 1946: 178-179; 1986: 244-245.

⁸¹ Yadin *et al.* 1960: 71 and n. 6.

⁸² Wallenfels 1983: esp. 111.

⁸³ Sass 2005: 32-32.

⁸⁴ Yadin 1972: 168-69, 200.

STRATUM VI

Yadin's date for Hazor VI was set by the assumption that its successor Stratum V was destroyed by Tiglath-pileser III in 732 BC. As there is evidence that Stratum VI was destroyed by a violent earthquake, Yadin naturally associated this with the famous earthquake which occurred under Jeroboam II/Uzziah, ca. 760 BC (Amos 1:4; Zechariah 14:5).⁸⁵ While widely accepted as a plausible historical link⁸⁶, the association does not, of course, provide a diagnostic means of dating the end of Stratum VI; the walls could have fallen during a later, undocumented, earthquake.

There were major differences between the city plan of Hazor VII and that of its successor VI. Yadin stated:

*"This stratum [VII] was completely destroyed and the pillared storehouse and other buildings were not reconstructed in the following strata... The public buildings of the [sic] stratum VII were not reused in stratum VI, and the entire area became a residential quarter with workshops and stores."*⁸⁷

As stressed by Zarzecki-Peleg:

*"... in stratum VI, Hazor underwent a significant change of status, as expressed in its urban disposition, through the utilization and repartition of its districts, especially in reference to the storage areas. It is not merely a question of their transfer to the area adjoining the city's entrance (Area G), as Yadin averred (Yadin 1972: 184). The change was more significant, and affected the settlement's entire design."*⁸⁸

The radically different plan and character of the new city of Stratum VI, including the abandonment of old public buildings, could suggest a change in governance. In the opinion of Yadin the new layout was due to the rebuilding of the city by Jeroboam II, after he had recovered it from the Aramaeans.⁸⁹ Yet if it was Hazor VII that was destroyed by Tiglath-pileser III, then the radical change in character of Hazor VI would reflect the transition from Israelite kingdom to Assyrian province (732 BC onwards). The Hebrew inscription on a storage jar from this stratum does not, of course, demonstrate that there was

⁸⁵ Yadin 1972: 113.

⁸⁶ E.g. Fantalkin and Finkelstein 2006: 23.

⁸⁷ Yadin 1993: 601; cf. 1972: 169, 179.

⁸⁸ Zarzecki-Peleg 2005: 372.

⁸⁹ Yadin 1972: 179; cf. Finkelstein 1999: 63.

still an Israelite government.⁹⁰ We can envisage Hazor VI as a largely Israelite resettlement under the Assyrians, in the same way that Yadin conceived Hazor IV (see below).

The greatest number of torpedo jars comes from Hazor VI. A placement of this stratum after 732 BC would be in step with Phoenician chronology which places the peak of torpedo-jar production no earlier than the late eighth century BC (see above). Cross-dating with the Egyptian site of Tell el-Maskhuta would strongly suggest that VI continued into the seventh century BC. Very similar torpedo-jars were found at Tell el-Maskhuta, in the earliest Saite (26th Dynasty) deposit, the beginning of which can be reliably dated to the last decade of the seventh century BC.⁹¹ Neutron activation analysis of the Tell el-Maskhuta examples showed the chemical composition was “very close” to the one piece analysed from Tyre.⁹² Of the shapes, Holladay noted that “[type] Rim 4:5 is restricted to this earliest period,” but was surprised that “a slightly different (?) version has a much longer history in the Levant, apparently being common already by ca. 735-22 BC, although this may be questioned.” The examples he notes are from Hazor VI-V, but because of the apparent chronological gap Holladay wondered whether “*The positive association of these vessels with these strata probably should be reviewed.*”⁹³ Alternatively, if the date of Stratum VI is lowered to include the early seventh century, and V to the mid seventh century BC (see below), then the gulf in time between the Hazor examples and the very similar finds from late seventh-century Tell el-Maskhuta is massively reduced. The problem can thus be solved without recourse to challenging the probity of the Hazor findspots (see also later, METHODOLOGICAL CONSIDERATIONS).

An Assyrian-empire date for Hazor VI fits the occurrence of a sherd described by Tappy as a carinated form similar to Assyrian styles.⁹⁴ Amiran saw such vessels as indicative of an Assyrian date (post-721 BC). Tappy argued that as such vessels appear to be locally made, “one must allow time for the original Assyrian style to take hold within the local cultures”; hence

⁹⁰ *LMKBRM*, read by Yadin (1972: 181-182) as “belonging to (PN) Makhbiram” and, more plausibly as “belonging to the food-servers” by Naveh (1981).

⁹¹ The Saite foundation at Tell el-Maskhuta was clearly connected with the creation of the Wadi Tumilat canal, connecting the Nile (and hence Mediterranean) with the Red Sea, by Necho II (610-595 BC) – see Herodotus 2.158; text and translation Godley 1926. Holladay (1982, 19) allows that it is “not impossible” that preparatory work on the canal began under his predecessor Psammetichus (664-610 BC), but correctly points out that there is no reason to believe this.

⁹² Paice 1986/1987: 97; Bieber in Bikai 1978a – see above.

⁹³ Holladay 1982: 52 and n. 77.

⁹⁴ Yadin *et al.* 1960: Pl. LXVII, 5; Tappy 2001: 313, nn. 442-443.

such imitations “*likely belong mainly to the seventh century BCE,*”⁹⁵ fitting the suggestion here that Stratum VI postdates the Assyrian conquest. A late-eighth to early-seventh century date for Hazor VI is thus supported by links with Phoenician, Egyptian and Assyrian chronologies.

STRATA V-IV

Both the pottery and architecture of Stratum V (with subphases VB and VA) link it closely with Stratum VI. Likewise the pottery of Stratum IV “*is for all practical purposes identical with that of V.*”⁹⁶ The conventional dating of both thus depends on the assumption that V was destroyed in 732 BC. Yadin saw the small settlement of Stratum IV as a short-lived Israelite occupation (732-700 BC), following the conquest of Tiglath-pileser III. But a much later dating for Strata V-IV is suggested by a number of Assyrian/Babylonian finds.

A bell-shaped vessel from Stratum VA has been identified by Zorn as a Mesopotamian-style burial coffin. Its occurrence in a pre-Assyrian context is clearly problematic. So Zorn:

*“This building [3148] went out of use, according to the excavators, in 732 B.C.E. when the Assyrian destroyed the Citadel. Thus, the piece comes from a context that predates the Assyrian presence at the site. Two alternatives can be proposed to explain its presence near the Stratum VA Citadel. One is that Mesopotamian cultural influence began to permeate Israel before the Assyrian conquest... The other possibility is based upon the Mesopotamian practice of interring the dead inside the settlement, below the floors of buildings. Perhaps the jar’s original context is in the later Stratum III... when this area... was part of the even larger Assyrian/Babylonian Citadel of Stratum III.”*⁹⁷

Zorn has identified two additional, smaller, fragments from Strata VB and VA as possible Mesopotamian burial jars, plus a fragment of what seems to be a Mesopotamian “bathtub” coffin from Stratum IV.⁹⁸ As Zorn notes, these contexts “are even more problematic than that of the complete burial jar.” As “fragments they cannot be in their original contexts.” The burial jar fragments

⁹⁵ Amiran 1969: 291; Tappy 2001: 315; see most recently Na’aman and Thareani-Sussely 2006, cf. Singer-Avitz 2007.

⁹⁶ Yadin 1972: 185, 191.

⁹⁷ Zorn 1997: 215; Yadin *et al.* 1961: Pl. CCXXXII, 19.

⁹⁸ Zorn 1997: 216-217; Yadin *et al.* 1961: Pl. CCXXIV, 11, Locus 3177; Pl. CCXXX, 24, Locus 3148.

would appear, then to be no later than Stratum VI and the bathtub fragment no later than Stratum V – both pre-Assyrian contexts on the traditional Hazor dating.

The problematic burial vessels need to be considered together with other Mesopotamian finds that predate Stratum IV, allegedly the first Assyrian-period settlement. Stratum VA produced an Assyrian bottle. In Amiran's opinion it is not of local or Transjordanian manufacture, but "duplicated in Assyrian pottery found in Assyria proper," hence she took it as evidence that "*commercial relations between Northern Israel and Assyria began before the conquest of Israel by the Assyrians.*"⁹⁹ While the imported vessel may, of course, be from trade, other finds of Assyrian-influenced ware suggest that the problem here is really one of chronology. Stratum VA produced three further examples of the Assyrian-influenced carinated bowl known from Stratum VI and dated by Tappy "mainly to the seventh century" (see above).¹⁰⁰

Two examples of so-called "Assyrian Palace Ware" (APW) are known from Hazor V and IV.¹⁰¹ Known from a scatter of sites in Cisjordan, Transjordan and Syria, "Assyrian Palace Ware" (APW) was once assumed to be a diagnostic feature of the Assyrian domination, c. 732-630 BC.¹⁰² In the 1950s, when Yadin first assigned Stratum IV to the end of the eighth century, his dating was thus in accord with prevailing opinions on the dating of locally made copies of Assyrian pottery – though his pre-732 BC date for Stratum V was somewhat out of step. But since then there has been a continuing trend towards lowering the date-range for this pottery.¹⁰³ Holladay long ago argued that APW did not actually reflect the period of Assyrian domination. Examining the findspots at Nimrud and the other Assyrian capitals, he noted that APW appears in the destruction levels of c. 612-610 BC and continues in the squatter occupations of the sixth century BC. Holladay's conclusion was that "*the floruit of this ware... should be placed in and following the last days of the Assyrian Empire.*" An even later range is demonstrated by new evidence from Transjordan (Ammon). As Stern remarks, "In the recent excavations at Tell el-'Umeiri, this pottery was found stratigraphically together with Attic pottery and cylinder seals attributed by experts to the late 6th and early 5th centuries BCE." Routledge has shown how: "*Overall, the ceramic evidence points to a general trend which sees the introduction of Mesopotamian forms into the ceramic repertoire of Jordan in the second half of the seventh century*

⁹⁹ Yadin *et al.* 1960: Pl. XCVII:11, Pl. CLIX:13; Amiran 1969: 291, 300, Pl. 99:7.

¹⁰⁰ Yadin *et al.* 1960: Pl. LXXX, 25-27; Tappy 2001: 313, n. 442.

¹⁰¹ Yadin *et al.* 1960: Pl. LXXIV, 3; XCVIII, 44; Holladay 1976: 272, 284; Tappy 2001: 238, n. 55, 313, n. 443.

¹⁰² See e.g. Kenyon in Crowfoot *et al.* 1957: 97-98; Amiran 1969: 291.

¹⁰³ See James *et al.* 1991a: 181, 372, n. 62; James 2004: 49; James 2006: 94, n. 10.

BC. Interestingly enough, this means that our evidence for Mesopotamian influence comes after the decline of the Neo-Assyrian empire in the West in the last years of Ashurbanipal." With respect to Palestine generally, Stern states that Assyrian Palace Ware remained "a constant feature" into the Persian period, though by then an "inferior" product with "cruder shapes."¹⁰⁴

Recent excavation and examination of contexts in northern Syria and Mesopotamia have tended to confirm Holladay's conclusion, in that the peak of popularity of APW in the heartlands of the Assyrian Empire fell in the late seventh to early sixth centuries BC.¹⁰⁵ The key question remains the initial date for this style.¹⁰⁶ A review of the Assyrian contexts, as provided in Hausleiter 1999, suggests that while Assyrian seventh-century contexts are well established, those from the eighth century are less certain.

Ironically, Yadin himself came to prefer dates for APW between 630 and 600 BC, close to those argued by Holladay. However, as noted, Yadin did not follow through the logical consequences.¹⁰⁷ Had he done so, the result would have been havoc for his eighth-century dating of Strata V and IV. Both would have to be lowered in date by a century or more, unless we consider the finds intrusive.¹⁰⁸

Taken together the number of Mesopotamian and Mesopotamian-style finds strongly suggest that Strata VI-V, rather than predating the Assyrian conquest of 732 BC, actually postdate it. As Stratum VI was a substantial city one could envisage it as an Assyrian establishment, occupying (roughly) the last quarter of the eighth century to the beginning of the seventh century BC. In Stratum VA, the second phase of V, the fortifications were reorganised and strengthened.¹⁰⁹ This might conceivably reflect Assyrian reorganisation after the local revolts of the 650s/640s (see above). The destruction of Stratum VA, which was covered in a thick layer of ashes, might then reflect the Scythian invasion ca. 630 BC or a subsequent Neo-Babylonian campaign. Stratum IV would then belong mainly to the Neo-Babylonian period. At present it is usually assumed that Hazor lay completely uninhabited during the Neo-

¹⁰⁴ Holladay 1976: 272); Routledge 1997: 35; Stern 2001: 257, 516.

¹⁰⁵ Lehmann 1998: 19-21; Hausleiter 1999: 18-22, 38-40; van der Veen, in press: §2.1.4b.

¹⁰⁶ Stern *et. al.* 1995: 15.

¹⁰⁷ Yadin 1985: 62; James *et al.* 1991a: 372, n. 62.

¹⁰⁸ Remarking that the APW examples from Hazor "fit well earlier in the typology," Holladay (1976: 272) concluded that they "should presumably be dated to the first half of the seventh century." While his table (Holladay 1976: 270-271) allowed Stratum IV to have ended ca. 680 BC he followed the usual date for the end of Stratum V, 733 BC. As the example reported from Stratum V was found on top of a wall, Holladay considers it "out of bounds for any critical stratigraphy" (pers. comm. Nov. 2007).

¹⁰⁹ Yadin 1972: 187.

Babylonian period, a situation which suits Stern's model of a "Babylonian gap" prevailing not only in Judah but throughout most of Palestine. However the historical grounds for this are highly questionable.¹¹⁰ Lowering Stratum IV to the Neo-Babylonian period would mean there was no major gap in occupation during this time.

STRATUM III

If Stratum IV is Neo-Babylonian, Stratum III must be considerably later than the seventh century as argued by Yadin. On the model argued here it would have to fall in the early Persian period, a date which, felicitously, has already been argued by Stern (see below).

The dating of Stratum III has always been uncertain, due to the scarcity of finds. The only structure assigned to it is a large citadel fort. This appeared to have been "thoroughly cleaned out" by the builders of Stratum II, who continued to use it with minor alterations:

*"The secondary use of Stratum II, which belongs to the Persian period, proves that the remains of the previous building were still quite visible above ground. It is therefore unlikely that there was such a long interval between the destruction of Citadel III and its reconstruction in the period of Citadel II."*¹¹¹

These circumstances suggest that there may have been no occupational break at all between the two strata, and that III should belong to the early Persian period (late sixth-fifth centuries BC). Nevertheless, Yadin had to introduce a gap, as a fallout of his overall dating of the site. Stratum III was assigned rough parameters, before ca. 400 BC and after ca. 700 BC, the presumed end date for Stratum IV, "with the probabilities nearly even between the seventh and sixth centuries." Yadin leant towards the higher date because of alleged similarities between the plan of the Citadel and the Assyrian buildings 1052 and 1369 from Megiddo Stratum III.¹¹²

However comparison of the citadel plan to the above-mentioned buildings at Megiddo does not provide a compelling match.¹¹³ As Stern has stressed, Assyrian-style fortifications were being built in Palestine as late as the 4th century: "These were constructed following the traditional Mesopotamian

¹¹⁰ See Blenkinsopp 2002a; 2002b; James 2004: 50-52.

¹¹¹ Yadin *et al.* 1958: 54.

¹¹² Yadin *et al.* 1958: 52-53; Yadin 1972: 194.

¹¹³ See conveniently Kempinski and Reich 1992: 215-216, figs. 12 and 13.

plan, which had penetrated into Palestine during the Assyrian period... The plan consisted of a large open courtyard surrounded by rooms on all sides."¹¹⁴ Further, Reich has questioned the Assyrian date ascribed to Hazor III, on the basis of comparisons with *post* Assyrian structures, including the houses of the Neo-Babylonian strata at Babylon and Ur.¹¹⁵ Accordingly Reich offered a dating for the Stratum III citadel to "*the end of the seventh century or beginning of the sixth,*" discussing the possibility that it was built under Babylonian rule. A slightly later, early Persian date (second half of sixth century BC), is preferred by Stern because of the continuity evident between Strata III and II:

*"... the Stratum II fortress should clearly be dated to the late Persian period... it was not necessary for the Stratum III fortress to be reconstructed by the inhabitants of Stratum II, who only had to clear its debris. The walls of the citadel remained standing, and it is not likely that they would have remained in this state after a long period of abandonment."*¹¹⁶

In agreement with Stern, an early Persian-period date for Stratum III is followed here.

STRATA II-I

The succeeding Stratum II is securely dated to the late Persian period, by the local pottery and imported finds such as a Tyrian silver stater of ca. 400-332 BC and two Attic lamps of the 4th century. Yadin dated Stratum I, with Hellenistic pottery, to the Maccabean period (3rd-2nd centuries BC).¹¹⁷

METHODOLOGICAL CONSIDERATIONS

It might be remarked that some of the 'anomalous' pottery finds discussed above (Cypriot, Phoenician, Assyrian and Assyrian copies) often involve small quantities, even single examples. To satisfy the *status quo* (Yadin's dating) it can, of course, be argued that such rare finds are intrusive or badly stratified. For example, Holladay dated the APW find from Hazor V to the seventh century, but did not redate the stratum accordingly, as he questions the reliability of the sherds' stratification (see n. 108 above). Likewise, Zorn suggested that

¹¹⁴ Stern 2001: 465-466.

¹¹⁵ Kempinski and Reich 1992: 215-216; cf. Kletter and Zwickel 2006: 170.

¹¹⁶ Stern 2001: 313.

¹¹⁷ Yadin 1972: 194-197; Stern 1982: 3.

the Mesopotamian jar burials from “pre-Assyrian” Hazor had been dug down from Stratum III (see above). With respect to the ‘anomalously’ early finds of torpedo-jars from Hazor, both Holladay and Bikai have raised questions about their context, on the basis of Egyptian and Phoenician chronologies respectively. Bikai argued that:

“... until excavation reaches a level of certainty akin to mathematics, arguments based on the occurrence of pottery forms must concentrate on the wide-point of a battleship curve, not on its tails. Three jars from a single locus [Stratum VII] are not good evidence for a cultural shift.”¹¹⁸

Given the exigencies of excavation and recording, as well as taphonomy, Bikai’s point is well taken. But we should also avoid a ‘chest of drawers’ approach to pottery development, with styles beginning and ending where our chronological charts give neat horizontal lines. As Gal put it, such an approach “implies that types of jars appear full blown without either gradual development or growth.”¹¹⁹ To concentrate *only* on the “wide-point” of the battleship curve also smacks of ‘cleaning up’ stratigraphy from finds unwanted because of chronological expectations. Indeed, as well as rejecting a “tail” of finds reaching through two strata (VII and VIII – see n. 7 above), Bikai also attempted to chisel away at the *wide-point* of the curve, provided by the over 40 examples of jars reported by Yadin from Stratum VI. She speculated that most of these may come from Stratum V, in order to remain in step with the late eighth century date required by her Tyrian chronology.¹²⁰ This was unwise, especially in retrospect as later excavations have recovered further examples from Hazor VI.¹²¹ Bikai’s arguments would have been more forceful had she felt able to challenge the dating of Hazor *per se*.

In some other cases, particularly that of the APW, the numbers of ‘anomalous’ finds may be few, yet, to put this in context, the quantity of local ware in northern Palestine which copies ‘Assyrian’ prototypes is very small anyway. For such small amounts of pottery, and indeed for large numbers (as per the torpedo jars), *ad hoc* explanations can always be offered, but this is to ignore the *overall pattern* of the evidence. It stretches credulity to explain away *all* the anomalous finds from different cultures (Cypriot, Phoenician and Mesopotamian), including evidence from architecture and burial practices as well as pottery, as being due to intrusion or another archaeologist’s poor

¹¹⁸ Bikai 1985: 72.

¹¹⁹ Gal 1992a: 73.

¹²⁰ Bikai 1978b: 48-49.

¹²¹ Ben-Tor *et al.* 1997: 262, Fig. III: 40, 1, 5.

excavation/recording; especially when *all* these finds argue in the same direction – for a systematically lowered chronology of late Iron Age Hazor.

A second strategy used to explain problem finds has been to challenge the chronology of imported vessels, such as the B-o-R ware. As noted, archaeologists have often turned to the allegedly “fixed” dates for Levantine stratigraphy to offer higher Cypriot dates than those of Gjerstad. However, the same strategy cannot work for Assyrian and Assyrian-influenced pottery, or the Tyre III-II horizon of Phoenician pottery well dated to the late eighth century BC onwards. Gal did challenge the veracity of Bikai’s Tyrian assemblages, hence their dating.¹²² But here we are back to the familiar argument of challenging the probity of another archaeologist’s site and Gal’s case was unconvincing in the lack of detail with which it was presented. As it happens, Gal (see note 46 above) has argued for lower dates at Tyre, making Strata IV-I all postdate 700 BC, which would create even greater chronological tension (involving well over a century) between Israelite and Phoenician contexts for the torpedo-storage jars.

A kaleidoscope of *ad hoc* explanations – ranging from ‘intrusive’ finds, the probity of another excavator’s site or recording, or the value of external chronologies and back again – becomes completely unnecessary when we realise that there is a significant *pattern* to a number of interrelated problems. All the indications from external chronologies argue for a radical lowering of Israelite late Iron Age chronology as set by Yadin for Hazor.

A REVISED MODEL FOR IRON AGE II HAZOR

When reconstructing the history of Hazor Yadin worked essentially with two ‘fixed points’ – the alleged “Solomonic” structures of Hazor X, and the assumed destruction-date for Hazor V in 732 BC. The problems with the first assumption are well known, but it has been shown here that the second ‘fixed’ point is equally unfounded. If we abandon both, then a very different interpretation of the stratigraphy of Hazor can be developed. The revision suggested here would clarify many difficulties in the archaeology of the site. To summarise these, in descending stratigraphic order:

- In agreement with Stern, Stratum III is not late Assyrian, but early Persian, resolving the problem of the continuity with Stratum II and producing the best match with Mesopotamian architectural parallels.

¹²² Gal 1992a: 73-74.

- Stratum IV, with “Assyrian Palace Ware” would date to the Neo-Babylonian period.
- The occupational gap at Hazor for the Neo-Babylonian and early Persian periods (in the Yadin chronology) is closed.
- A seventh-century Assyrian empire date for Stratum V means that its imported Assyrian, Assyrian-influenced and “Assyrian Palace Ware” finds, together with the evidence of Mesopotamian-style ceramic coffin burials, no longer have to be explained as pre-Assyrian curiosities or problems.
- Likewise the Assyrian-style bowl and possible Mesopotamian jar burials from Stratum VI would now have a post-732 BC date.
- Redating Hazor VII and VI to the mid and late eighth century, respectively, produces harmony between Israelite and Phoenician chronology, with respect to the long-running debate over the torpedo storage jars at Hazor.
- A slightly lower version of the Bikai dating of Tyre III-II is supported, vital in terms of the dated Egyptian finds (Tyre and Tell el-Mashkuta) which show that the Tyre III horizon must range into the seventh century BC.
- Evidence of an Assyrian destruction at Hazor in 732 BC can be found just as well in the complete destruction of Stratum VII as it can in Stratum V (Yadin model).
- Lowering Stratum VII to the mid-eighth century BC obviates the problem of the “dense stratigraphy” for Strata X-VII created by the Finkelstein model. Nearly a century more would be available for Strata X-V than the 150 years required by Finkelstein, giving a reasonable average of about 40 years per stratum for these substantial levels (much the same as that on the Yadin/Ben-Tor chronology).
- Redating Hazor X to the mid-ninth century (c. 875-835 BC) removes the conflict with traditional Cypriot chronology regarding the dating of Black-on-Red ware. There is no need for a major upwards revision of Cypriot chronology.
- The ceramic parallels with mid-ninth century Jezreel are sustained.
- The forced attempts to relegate all of Samaria PP 1 and 2 to a pre-Omride settlement are unnecessary.

YADIN/BEN-TOR

FINKELSTEIN

JAMES *ET AL.*

	DATE	HISTORY	DATE	HISTORY	DATE	HISTORY
X	mid-10 th	Israelite: Solomonic	early 9 th	Israelite: Omrides	875 BC- ca. 835 BC	Israelite: Ahab
IX	end 10 th - early 9 th	Israelite	first half of 9 th	Israelite: Omrides	late 9 th	Israelite/Aramaean?
Destr.	885 BC	Ben-Hadad I	ca. 835 BC	Hazael	ca. 800 BC?	Ben-Hadad II vs. Jehoahaz/Joash?
VIII	early 9 th	Israelite: Ahab	late 9 th	Aramaean	early 8 th	Israelite: Jeroboam II
VII	later 9 th	Israelite	end of 9 th	Aramaean	mid 8 th	last Israelite
Destr.	late 9 th	Aramaean invasion	ca. 800 BC	Joash/ Jeroboam	Assyrian invasion	732 BC, Tiglath-pileser III
VI	early 8 th	Israelite: Jeroboam II	early 8 th	Israelite: Joash/ Jeroboam	late 8 th - early 7 th	Assyrian
Destr.	ca. 760 BC	earthquake	ca. 760 BC	earthquake	ca. 675 BC?	earthquake
V	mid 8 th	Israelite	mid 8 th	Israelite	mid 7 th	Assyrian
Destr.	732 BC	Assyrian conquest	732 BC	Assyrian conquest	ca. 630 BC?	Scythian invasion?
IV	end of 8 th	unfortified Israelite	end of 8 th	unfortified Israelite	late 7 th - mid 6 th	Neo-Babylonian
III	7 th	Assyrian	7 th	Assyrian	mid-6 th -5 th	Early Persian
II	4 th	Persian	4 th	Persian	4 th	Late Persian

Table 2.

Various chronologies proposed for Iron II Hazor.

HAZOR, MEGIDDO AND SAMARIA

How would the model proposed here square with the dating of related sites in Israel? Megiddo IVA is usually thought to have been destroyed by the Assyrians at the same time as Hazor V. Yet the revision suggested here would place the destruction of Hazor VII at much the same time as that of Megiddo IVA.

Holladay once remarked that “the forms typical of [Megiddo] Stratum IVA local are fully in keeping with other 733 B.C. horizon materials (e.g. Hazor V/VA).”¹²³ Yet judgments here are complicated by the fact that Megiddo IVA is traditionally thought to have been an exceptionally long stratum. Thus Megiddo IVA is usually treated as the temporal equivalent of Hazor Strata VII and VI as well as V. See, for example, Ben-Tor: “Megiddo IVA, the duration of which is much longer, co-existed, at least during part of its life-span, with Hazor VII.”¹²⁴ Indeed, Yadin *et al.* considered that Megiddo IVA and Hazor VII both fell at the same time (ca. 815 BC to an Aramaean invasion), a suggestion which elicited surprise from Finkelstein.¹²⁵ The end of Megiddo IVA has been considerably lowered since Yadin wrote this, the consensus being that it fell to Tiglath-pileser in 732 BC.¹²⁶ If Yadin was correct in his relative dating,¹²⁷ then it would be logical to also place the fall of Hazor VII (not V) at this time. So does the pottery of terminal Megiddo IVA compare most closely with that of Hazor VII or V?

Finkelstein has stressed that comparison of the ceramics from Megiddo IVA to the Hazor sequence has hitherto remained uncertain, for purely practical reasons:

“The excavations of the University of Chicago team... failed to produce a significant assemblage for Stratum IVA. The first significant group of vessels for this phase in the history of Megiddo was uncovered in the course of the renewed excavations in Area H, located in the northern sector of the site to the north and northeast of Assyrian Palace 1369 of Stratum III. Almost 100 complete or almost complete vessels were found in a thick destruction layer in several domestic structures built inside and

¹²³ Holladay 1976: 214.

¹²⁴ Ben-Tor 2001: 302.

¹²⁵ Yadin *et al.* 1958: 23; Finkelstein 2000a: 241; cf. Ben-Tor 2001: 302.

¹²⁶ Shiloh 1993; Finkelstein, Ussishkin and Halpern 2000: 322.

¹²⁷ Cf. Tappy 2001: 253.

against City Wall 325. There can be little doubt that this is the destruction of Megiddo by the Assyrians in the late 8th century B.C.E."¹²⁸

Analysing this new assemblage from Megiddo IVA, Finkelstein notes that seven significant ceramic types known from the preceding Stratum VA-IVB are now absent, while another six types appear for the first time. He then compares the Megiddo VA-IVB and IVA assemblages to those of Hazor VIII and VII. Two of the types (hemispherical bowls with plain rim and cooking pot with elongated, ridged rim) absent from Megiddo IVA are present in Hazor VIII-VII. "*These are clues that Hazor VIII-VII falls in the middle of the sequence, i.e. between Megiddo VA-IVB and Megiddo IVA.*"¹²⁹ Finkelstein's main intention here was to show that there may have been a considerable gap in settlement between Megiddo VA-IVB and Megiddo IVA. Whether or not this is the case, his analysis reveals considerable similarities between the repertoire of Hazor VII and that of Megiddo IVA. While there are thirteen typological differences between Megiddo VA-IVB and IVA, Finkelstein noted only *two* between the latter and Hazor VII.

Some of the similarities are of particular interest. "Samaria Ware" was present in Hazor VII but first appears at Megiddo in the destruction deposit of IVA. Torpedo storage jars also first appear at Megiddo in this deposit. As noted above, their occurrence in Hazor VII – dated as early as the late ninth century – is anomalous. Their appearance in the destruction of Megiddo IVA, usually attributed to the Assyrians in the late eighth century BC, is much more in line with the Phoenician dating of such vessels. Indeed, if we consider the torpedo jar as an index type, it would appear that the chronologies of Megiddo and Hazor are presently out of step by a century.

The two differences perceived by Finkelstein between the assemblages of Hazor VII and Megiddo IVA might be accounted for by regional differences between Hazor and the Jezreel Valley,¹³⁰ but much would also depend, of course, on exactly when Megiddo fell to the Assyrians. It is usually assumed that it fell to the Assyrians during Tiglath-pileser's 732 BC campaign, but literary evidence is lacking. There is actually no historical evidence that Tiglath-pileser's 732 BC campaign reached Megiddo or that he established the Assyrian province of that name.¹³¹ Forsberg has suggested that the "most

¹²⁸ Finkelstein 1999: 63.

¹²⁹ Finkelstein 1999: 64.

¹³⁰ See Finkelstein 1990: 116; 1996: 183; 1999: 60; Zimhoni 1997: 26.

¹³¹ Forsberg 1995: 23. Becking 1992: 107 surveys the Neo-Assyrian documents mentioning ^{umu} *Ma-gi-du*, and notes that while they demonstrate the existence of the province of Megiddo "their dating is difficult." The earliest secure reference to ^{umu} *Ma-gi-du* might seem to be that in a letter from the reign

likely context” for the construction of Megiddo III would be the Assyrian conquest of Samaria in the late 720s. A similar date for the destruction of Megiddo IVA has been considered by the current excavators:

“The most significant vessel for dating the last days of the assemblage of Level H-3 [Stratum IVA] is the locally made Assyrian bottle [dimpled goblet] ... If Assyrian forms were imitated in the workshops of the Northern Kingdom already before Tiglath-Pileser III’s conquest of the north, there would be no difficulty in dating the destruction of Stratum IVA to 732 B.C.E. Alternatively, if Assyrian forms were imitated only after the annexation of the Galilee and the northern valleys by Tiglath-Pileser III, the destruction of Megiddo IVA must be placed after that event. If the latter option is preferred, the site would have survived the Tiglath-pileser III campaign and would have been destroyed somewhat later, perhaps when the province was reorganized under Sargon II.”¹³²

It may well be, then, that Megiddo IVA was not destroyed in 732 BC, but during Sargon’s reorganisations c. 720-715 BC, placing the fall of Megiddo somewhat later than that assumed for Hazor (732 BC). Whether one or two decades would be sufficient to allow for the small differences between the assemblages of Megiddo IVA and Hazor VII is a matter for further study. There remains the possibility of an even later context for the fall of Megiddo IVA, for example during the seventh-century rebellions and invasions discussed earlier.

Assessing the relationship between Hazor and Samaria is more difficult, especially as there has never been one, universally accepted, system of dating (or even correlation) for the Building and Pottery periods at Samaria.¹³³ There has also always been chronological tension between the two sites regarding the dating of shared ceramic types. Mention has already been made of the dispute over the association of Samaria PP 1 and 2 (paralleled by Hazor X) with Samaria BP I and II, a debate exacerbated by disagreements between

of Sargon II (SAA V, No. 291), which appears to record the delivery of bricks there – suggesting a rebuilding programme in the province. However, Becking 1992: 112 offers a second interpretation, in which the lands of the workers referred to (Arpad, Samaria and Megiddo) do not refer to “the provinces in the west, but to groups of laborers deported from the territories mentioned” to work on Sargon’s new palace at Dur-Sharruken. Cf. Tappy 2001: 244, n. 99: *“Contrary to popular belief, the Assyrians may not have organized this province as early as the activities of Tiglath-pileser III, but only sometime during the closing years of the eighth century BCE or even the early seventh century. Magiddu certainly existed by the time its governor, Itti-Adad-aninu, became the eponym of the year 679 BCE.”*

¹³² Ballard *et al.* 2002: 158; Tappy 2001: 164.

¹³³ For a convenient synopsis see table Appendix B in Tappy 1992: 254.

Yadin and Kenyon over parallels in later assemblages. For example, Yadin compared the pottery of Hazor VIII to that of Samaria PP 3 and that of Hazor VII to Samaria PP 4. However, Kenyon preferred a comparison between Hazor IX and Samaria PP 3 and between Hazor VIII and Samaria PP 4-6. Either set of parallels created a problem: while Yadin dated Hazor VIII and VII to the mid and late ninth century BC, both Kenyon and Wright placed Samaria PP 4 in the early eighth century BC. The solution of Aharoni and Amiran (1958) was to argue for a backdating of PP 4 to the second half of the ninth century, and PP 3 to the time of the Omrides.¹³⁴

Further examination of the early debates re Samaria vs. Hazor, already treated by Tappy,¹³⁵ would be redundant: partly because new analyses need to be taken on board but, most importantly, because a rash of new studies have argued that all the traditional schemes for dating Samaria (with the exception of Kenyon, for some phases) are too high.

As discussed above (STRATA X-IX), Franklin now accepts that Samaria BP II should not be attributed to the reign of Ahab (875/4-853 BC) but to the early eighth century BC, hence the time of Jeroboam II (791/0-750/49 BC). But she has yet to follow through the consequences for later strata. PP 3, which overlies the BP II structures, must represent occupational debris accumulated during the long reign of Jeroboam. This matches well with Tyrian chronology. 'Samaria Bowls B' begin in PP 3, and are known as Fine Ware plates (class 2.1) at Tyre V-IV, dating no earlier (Bikai) than ca. 800/760-750/740 BC.¹³⁶

Given this, the BP III structures (above PP 3) can reasonably be placed in the mid-eighth century BC and would represent the last Israelite city, rather than BP V-VI as thought by Kenyon, Wright *et al.*¹³⁷ The overlying PP 4 pottery deposits (which include a torpedo-storage jar fragment, no earlier than the mid-eighth century¹³⁸) would then end ca. 720 BC with the Assyrian conquest. They are followed by the extensive rebuilding of Samaria in BP IV. This would seem to reflect the work of the Assyrian conqueror Sargon II, after 720 BC, who claimed that he "*rebuilt [Samaria] better than it was before...*"¹³⁹ BP V/VI would then belong to a later phase of Assyrian activity at the site, during the seventh century BC.

¹³⁴ Yadin *et al.* 1958: 14; 1960: 16-31; Kenyon 1964: 147-148; Aharoni and Amiran 1958.

¹³⁵ Tappy 1992: 4-8.

¹³⁶ See James *et al.* 1998: 31.

¹³⁷ See James *et al.* 1991a: 186-187.

¹³⁸ Tappy 2001: 161-164.

¹³⁹ See James *et al.* 1991a: 186.

<i>Samaria Building Periods</i>	<i>Samaria Pottery</i>
	1 ending ca. 880 BC ¹⁴⁰
I. Omrides (Omri-Ahab- Jehu; 886/5-815/4)	
	2 ending ca. 790 BC
II. Jeroboam (790-750/49)	
	3 ending ca. 750 BC
III. Last Israelite	
	4 ending ca. 722 BC
IV. First Assyrian (722-675? BC)	
	5 ending ca. 675? BC
V/VI. Second Assyrian (675? - 630 BC)	
	6 ending ca. 630? BC
VII. Post-Assyrian & Babylonian	
	7 ending ca. 539 BC
VIII. Persian	
	8 ending ca. 323 BC
IX. Hellenistic	
	9 ending ca. 63 BC

Table 3.

Simplified correlation of Samaria Building and Pottery Periods – with suggested new dates, slightly amended and corrected from James *et al.* 1991a: 183-188. NB Kenyon adhered to her system of dating Building Periods by the latest pottery found beneath them for BP I-IV, but the system began to come adrift with BP V/VI due to the lack of clearly defined deposits beneath floors (see Forsberg 1995: 19-20; Tappy 2001: 178, 223). Tappy's analysis of the few vessels which can be safely attributed to PP 5 is followed here. With PP 6 we are dealing with pottery which, contrary to Kenyon's system for earlier periods, is found above the building floors.

The later part of our Samaria reconstruction has been amply vindicated by the studies of Forsberg and Tappy. Forsberg has argued that the Samaria PP 5-6 pottery assemblages (accompanying BP V-VI) do not belong to the last Israelite periods (third quarter of eighth century BC) but to the seventh century BC. Though Kenyon herself established the close resemblance between the PP 5-6 assemblages and those of Megiddo III, she failed to synchronise them

¹⁴⁰ NB: Chapman (2007; in prep.) considers the possibility that the original context of most of the PP1 pottery (before it was deposited in later constructions) was, *contra* Kenyon's apparent position, from off the floors of BP I. In that case the date for PP1 pottery would have to be lowered and should largely postdate ca. 880 BC.

adequately. As Forsberg points out, it is now generally accepted that Megiddo III was an Assyrian foundation, belonging largely to the seventh century BC, leaving the Samaria dates far too high: "*Whereas the terminal date of the pottery on the Samaria side of the equation is set by Kenyon at 722 BC, the pottery of Megiddo Stratum III is later, probably not earlier than the first quarter of the 7th century.*" Further, Forsberg has stressed that as we are largely dealing here with pottery from the end of Megiddo III (usually dated to c. 650 BC), the date for the Samaria PP 5-6 material could well be weighted towards the middle of the seventh century (with knock-on effects for Samaria PP 7). His conclusion was that the "*end of habitation in the Period V building should probably be dated to about the third quarter of the 7th century, at the end or close to the end of the period of Assyrian domination of Samaria*"¹⁴¹, in agreement with our suggestion that BP V/VI represent the final Assyrian phase.¹⁴²

While Tappy feels that Forsberg has "*overextended its [Megiddo III's] overall range too far into the seventh century BCE,*"¹⁴³ he is in broad agreement. His analysis has shown that "*the few ceramic fragments published from loci beneath the BP V house floors seem to postdate the Israelite-Assyrian transition around 722/721 BCE.*" Regarding the date of this small PP 5 group, Tappy cautiously offers "*no earlier than the very late eighth and, perhaps mainly, the first half of the seventh centuries BCE,*" with a consequent date for PP 6 in the seventh century.¹⁴⁴ Fine tuning aside, a seventh- (or even late eighth-) century date for PP 5 completely undermines the traditional understanding – shared by Kenyon, Wright and others – that it was Samaria BP VI that was captured by

¹⁴¹ Forsberg 1995: 24, 50.

¹⁴² James *et al.* 1991a: 186-187.

¹⁴³ Tappy 2001: 245, which places the *floruit* of Megiddo III largely in the last two decades of the eighth century BC. His caution seems excessive, compared to the latest conclusions of the current excavators, unavailable to Tappy when he completed his masterwork on Samaria. From archaeological reasoning, based on the apparent orientation of the town plan of Megiddo III around the latest in a series of palaces, Finkelstein and Ussishkin (in Finkelstein *et al.* 2000: 602) argue that "*the well-planned city of Stratum III can hardly be dated early in the period of Assyrian rule at Megiddo. It should be mentioned in this connection that the name of the governor of Megiddo serves as the eponym in 679 B.C.E., in the days of Esarhaddon, which, if anything, would suggest a later floruit for Stratum III, rather than an earlier.*" In close agreement, Halpern (in Finkelstein *et al.* 2000: 568-570) explores the idea that the "full repopulation of Megiddo that is evident in Stratum III" might be related to the western policies of Esarhaddon after c. 685 BC. Halpern allows a much later date than Tappy for the general floruit of the stratum, considers that the 609 BC incident at Megiddo involving Josiah was "toward the end of the life of Stratum III" and recommends lowering the seventh-century date for Stratum II proposed by the Oriental Institute excavators, from the Assyrian to Babylonian or even Persian period. What remains surprising is that the excavators of Megiddo have not considered the knock-on effects of such proposals on the dating of late Iron Age Samaria and Hazor.

¹⁴⁴ Tappy 2001: 226, 224.

the Assyrians. Though Tappy avoided the logic, Samaria BP IV, underlying the minimal PP 5 deposits (which he agrees are *post*-Assyrian conquest in date), cannot be placed far earlier in time and can reasonably be seen as the city (re) built by Sargon II. Here Tappy was constrained partly by comparisons from Hazor which, as traditionally dated, support a high chronology for Samaria I-IV.

The revised dates for the Building and Pottery Periods, as argued in James *et al.* 1991a and the present paper, offer the (hitherto elusive) possibility of a harmonious picture between Samaria and Hazor. Recent studies show that Yadin, rather than Kenyon, was correct in his Samaria correlation for Hazor VIII. Wightman correlated the bulk of PP 3 with Hazor VIII. Many close parallels illustrate this.¹⁴⁵ The correlation supports the suggested dating of both these pottery periods to the renaissance of Israel under Jeroboam II. The succeeding phases, Hazor VII and Samaria PP 4 correlate well, e.g. Gal's cooking pot type 6.5 first appears in Hazor VII and at Samaria in PP 4. PP 5-7 would then broadly correlate with Hazor VI-IV. For example Gal notes that his Galilean type Jug 3.3 (a ridged decanter) first occurs in Hazor VA, becoming common in Stratum IV. It is also known from Megiddo III-I and Samaria PP 5-6. Likewise Gal's Bowl type 5.7 is known from Hazor VA-IV and Samaria PP 5-6.¹⁴⁶ Of Phoenician origin, this bowl is most common in Tel Keisan Stratum 4, now dated to the seventh century BC.¹⁴⁷ Interestingly, it is only the examples from Samaria and Hazor – as conventionally dated – that allow Gal to remark that this vessel was “widespread during the eighth-sixth centuries B.C.E.” On the Yadin/Ben-Tor model Hazor Va and IV are dated to the mid and late eighth centuries respectively, out of step with the generally accepted seventh-century date for Megiddo III and Tel Keisan and the new dates offered here for Samaria PP 5-7.

A full exposition of the case for a revised dating of the Iron Age strata at Samaria is beyond the present study. While more systematic analyses are clearly needed (particularly of the extremely difficult site of Samaria), it would appear, *prima facie*, that there are no obstacles from the ceramic parallels at Megiddo and Samaria to the lowering of Hazor Strata VII-V argued here. Rather, a downward shift of the latter would seem to bring the pottery of these strata in better step with that of Megiddo and Samaria.

¹⁴⁵ Wightman 1990: 12; Tappy 1992: 161, 179, 189, 192, 202.

¹⁴⁶ Gal 1992a: 78, 75, 76-77.

¹⁴⁷ Humbert 1993: 866-867.

CONCLUDING REMARKS

There are many reasons to question the idea that the destruction of Hazor V in 732 BC provides a firm “anchor” in the present chronological debates. The dating of the end of Stratum V to the Assyrian conquest is merely an assertion which has become a given, used to reconstruct the dates of preceding and following layers but never properly argued out in its own right – or, for that matter, critically analysed. It has also given rise to numerous anomalies in the dating of Hazor’s Iron IIA-IIB strata, concerning independently dated imports (from Cyprus, Phoenicia and Mesopotamia), which have previously been treated on an unsatisfactory *ad hoc* basis.

While the Tel Aviv school has now begun to address the problem of Iron IIA chronology originally raised by Kenyon *et al.* from their excavations at Samaria, similar uncertainties in dating extend well into the succeeding Iron IIB and IIC periods.¹⁴⁸ Advocates of the Tel Aviv version of a ‘Low Chronology’ are working within an unnecessary straitjacket, by adhering to Yadin’s dating of Hazor VII-V. This has led them, while lowering Iron IIA largely into the ninth century, to conclude that this important phase should be shortened from 200 to 125 years.¹⁴⁹ If we abandon the “anchor” of 732 BC for the end of Hazor V, and lower Hazor VII into the mid-eighth century, then Iron IIA might be allowed a slightly longer duration.¹⁵⁰ The related problem, of “stratigraphic congestion” between strata X-V is also relieved, and a major obstacle is removed to lowering Iron IIA from the 10th to the 9th century BC. The wider ramifications – such as those for our understanding of the archaeology of the United Monarchy – will have to be discussed elsewhere.

¹⁴⁸ James *et al.* 1987: 58-64; James *et al.* 1991a: 170-182; James *et al.* 1998: 30-32; James 2004: 48; James 2005a; James 2006; James 2007; James in press.

¹⁴⁹ Fantalkin and Finkelstein 2006: 22-24, 32-33.

¹⁵⁰ Fantalkin and Finkelstein 2006: 23-24 note that Hazor VIII-VII (Tel Aviv dating: second half of ninth century BC) still feature some Iron IIA ceramic types, while VI (Tel Aviv: early eighth century BC) features Iron IIB pottery. This brings them to the conclusion that the Iron IIA/IIB transition fell ca. 800 BC, earlier than the ca. 760 BC currently suggested (Herzog and Singer-Avitz 2004). They conclude that Iron IIA should be shortened from the conventional 200 years (ca. 1000-800 BC – see e.g. Ben-Tor 1992: 2, Table 1.1), also assumed by Herzog and Singer-Avitz, to something like 125 years. While dating the Iron IIA/B transition is beyond the scope of the present study, as a fallout of the arguments presented here, and assuming the definitions of Fantalkin and Finkelstein, Iron IIA forms (in Hazor VIII and VII) continued until 732 BC, with Iron IIB in Hazor VI, post-Assyrian conquest. This might suggest that the changes in pottery from Iron IIA to IIB styles were a result of the Assyrian conquests, ca. 730-700 BC. If Iron IIA developed at the time of the rise of the Omride dynasty (ca. 875 BC), this would allow a duration for Iron IIA of ca. 145-175 years.

APPENDIX: AYYELET HA-SHAḤAR

Mention needs to be made of the problematic chronology of Ayyelet ha-Shaḥar, a late Iron Age settlement on the plain near Hazor. In 1950 Guy and Dothan investigated the remains, identifying a palace containing largely Persian-period pottery.¹⁵¹ However, subsequent studies argued from parallels at Khorsabad and Arslan Tash that the prototype for the palace-plan is a Neo-Assyrian type of the eighth century BC.¹⁵² Thus it has been argued that the palace was the residence of a Neo-Assyrian governor, built after the conquest of Tiglath-pileser III in 732 BC, which was cleaned out and reused during the Persian period. This conclusion has been accepted by Ben-Tor: “*In all likelihood the structure which Guy associated with the Persian period is an Assyrian palace contemporary with stratum III of the upper city. It is similar to the strata III-II citadel of area B in the upper city and, like the latter, continued in use in the Persian period.*”¹⁵³

As argued here (and in agreement with Stern), the Stratum III citadel most likely belongs not to the Assyrian but to the early Persian period. Hence the similarities with Ayyelet ha-Shaḥar detected by Ben-Tor would raise the question whether a Persian dating might be correct, after all. The finds have recently been published and analysed by Kletter and Zwickel who conclude: “Most of the pottery from ‘Ayyelet ha-Šaḥar, and the types that are precisely dated, belong to the Persian period.” They offer a new reconstruction of the building, which they conclude “*fits better Babylonian architecture, not Neo-Assyrian,*” with the overall conclusion that it “*combines Neo-Assyrian features terre pisée walls, pebble-plaster floor, a reception suite) with Neo-Babylonian ones (Pits 1 – 2, the general plan following the new reconstruction).*” Regarding the date they note:

*“It is important not to confuse architectural tradition and date. The Neo-Babylonian architectural features do not imply a late date. They appeared in Babylonian architectural tradition since the 8th century B.C.E. at the latest, that is, they are not necessarily later than the period of Assyrian rule in the west. The Neo-Assyrian features indicate that the ‘Ayyelet ha-Šaḥar building was established most probably by the Assyrians in the late 8th or early 7th centuries B.C.E.”*¹⁵⁴

¹⁵¹ See Yeivin 1960: 29; Kletter and Zwickel 2006: 151-152.

¹⁵² Reich 1975; Lipschitz 1990.

¹⁵³ Ben-Tor 1993: 605.

¹⁵⁴ Kletter and Zwickel 2006: 169, 175.

Yet regarding the Neo-Assyrian comparanda, as Stern has remarked Assyrian-style fortifications continued to be constructed in Palestine as late as the 4th century¹⁵⁵; one wonders whether Assyrian palace-plans were similarly copied over this period. Further, as John Bimson has pointed out to me (pers. comm. 2004), it might seem unlikely that such a structure (made of packed mud with some traces of lime-plastering) would have survived from the Assyrian to Persian periods in one of the wetter parts of Israel unless occupied and maintained. A number of conclusions remain possible, but at present it would seem most likely that the palace was built for a Neo-Babylonian regional official and that it remained occupied into Persian times. In this case it may provide further evidence for continuity in settlement at Hazor during the Neo-Babylonian period.

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¹⁵⁵ Stern 2001: 465.

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THE “WICKED PRIEST” IN EGYPTOLOGY AND AMARNA STUDIES: A RECONSIDERATION*

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Abstract: The “Wicked Priest” in Egyptology and Amarna Studies: A Reconsideration

It is often the case in Egyptology that the priesthood, especially that of Amun in the 18th dynasty, is portrayed as a power hungry, underhanded, political force. This paper will argue that such portrayals are influenced by the assumptions of the Enlightenment and, in particular, Deism. It often happens that assumptions held at the time of the inception of a discipline have a lingering influence on their field. Within scholarship the conclusions of those seen as ground breaking pioneers can also be very influential. This paper examines the reasons for the growth of the “wicked priest” discourse in Egyptology and its application to Amarna Studies. In an attempt to stop the lingering influence of this discourse, some tentative alternate suggestions are made regarding two areas of this application: 1) Akhenaten’s religious reforms and change of capital; 2) The hacking out of Akhenaten’s name and the reaction to his rule.

Keywords: Amarna – Akhenaten – Egyptology – religion

Resumen: El “sacerdote impío” en Egiptología y en los estudios de El Amarna: una reconsideración.

Es frecuente el caso en los estudios egiptológicos que el sacerdocio, en especial el de Amón en la dinastía XVIII, sea retratado como una fuerza política hambrienta de poder y engañosa. Este trabajo demostrará que tales representaciones son influenciadas por los supuestos del Iluminismo y, en particular, del Deísmo. Con frecuencia sucede que tales supuestos, que tuvieron lugar en los inicios de una disciplina, tienen una influencia persistente en sus campos. En el mundo académico las conclusiones de aquellos

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vistos como pioneros pueden ser también de mucha influencia. Este trabajo examina las razones para el crecimiento del “discurso del sacerdote impío” en Egiptología y su aplicación a los estudios de El Amarna. En un intento para frenar la influencia permanente de este discurso, se realizan algunas sugerencias alternativas en relación con dos áreas de aplicación: 1) las reformas religiosas de Ajenatón y el cambio de capital; 2) la persecución del nombre de Ajenatón y la reacción a su reinado.

Palabras Clave: Amarna – Ajenatón – Egiptología – religión

THE PROBLEM

The priesthood has been a main focus in the study of Ancient Egypt since at least the Classical Period. Depictions of this group have ranged from the wise, secretive initiates of the classical literature, to the deviously cunning and politically manipulative power mongers represented in many works of the modern era. This paper aims to examine the origins of the “wicked priest” theory in Egyptology and, more specifically, its application to the Amarna period. Akhenaten’s battle with the Amun priesthood has now been a central tenet of most depictions of Amarna Egypt for over a century. In more recent times some have begun to question the evidentiary basis for the existence of such a quarrel.¹ I am in sympathy with this more critical approach and will argue that theories proposing this almighty conflict between Akhenaten and the Amun priesthood owe much more to the historical, religious, political and personal contexts of the scholars who contributed to the “wicked priest” discourse in Egyptology than they do to the historical sources available. Though there are certainly sharp ruptures or changes in discourses about Egyptian priests, the “wicked priest” discourse seems to develop, with earlier versions impacting upon later.² Classical period depictions had an enormous impact upon the portrayals of the Renaissance which in turn set the framework of understanding which was reshaped under the impact of Deism, its anti-clericalism and criticisms of institutionalised religion. To a great extent these frameworks of understanding have had a lingering impact on Egyptology even after decipherment, with similar arguments, ever shaped by context, being supported by a new body of evidence. The Romanticism of the early 19th Century and then the increasing political focus of history which coincided with the growth of nation states later that century both influenced the picture portrayed. With regards to Egyptology, and especially Amarna studies, all of

¹ Montserrat 2000: 36.

² Compare Foucault’s notion of discontinuity between disparate discourses.

this (amongst other factors) comes to a head in the person and work of James Henry Breasted. His depiction of the Amun priesthood, particularly under Akhenaten, has had an enormous impact on Egyptology since. After critically exploring the creation of the myth of Akhenaten’s battle with the Amun priesthood, some alternate explanations will be discussed with regard to two main issues surrounding the Amarna period: 1) Why Akhenaten changed the religion and the capital; 2) The reasons behind the hacking out of Akhenaten’s name and the reaction to his rule. Given the nature of the evidence available in Amarna studies, the historiographical outline will be more firmly founded than the alternate historical explanations put forward.

To begin it is perhaps worth outlining the general contours of the “wicked priest” theory as applied to Amarna. Breasted’s account of this era³ depicts a power hungry Amun priesthood, populated by the priestly families of Thebes who have risen to prominence on the back of the empire and Thebes’ status as capital city. These priests had dragged the obscure god Amun into the limelight by solarising him (attributing to him the qualities of Re). This priesthood had been given enormous amounts of land and wealth to administer (much of the latter coming from the conquests of the empire), thus raising its political influence. Their prominence was aided particularly by Thutmose III and Hatshepsut who owed their ascendance to the throne to the Amun priesthood and were obliged to repay him via an increase in power and wealth.⁴ Akhenaten, a “*god intoxicated man*”,⁵ came into conflict with this “*sacerdotal organisation*”⁶ as he began to espouse his new universal, monotheistic ideas about the sun-god. His new religion was open and accessible to all; as opposed to the secretive rites of the priests of Amun (the god’s very name means “hidden one”). The reaction of the Amun priesthood to these new reforms led Akhenaten to close down their temples and wipe out the name of Amun. Akhenaten built up a powerful court party in opposition to the disbanded priesthood and moved his capital away from Thebes. Akhenaten created a group of out of work priests “*nursing implacable hatred*”⁷ towards him. This “*dangerous secret opposition*”⁸ plotted his downfall, eventually manipulating

³ See Breasted 1951 (originally 1905, second edition 1909): book 5; 1972 (originally 1912): lectures 9 and 10; and 1934: chapters 15-16.

⁴ Breasted 1951: 272, 362.

⁵ Breasted 1972: 334.

⁶ Breasted 1972: 319.

⁷ Breasted 1972: 341.

⁸ Breasted 1972: 342.

the boy king Tutankhamun to restore Amun worship.⁹ After Akhenaten's reign Amun and his priesthood were once again in ascendancy in Egypt.

Each element in this portrayal has had an enormous impact on Amarna studies and has been picked up and repeated by other scholars. That said, other accounts of the Amarna period which take the conflict between Akhenaten and the Amun priesthood as a central tenet often involve slight variations on Breasted's depiction. Some, not holding to Breasted's high regard for Akhenaten's "pure religion", see the conflict merely as an attempt to wrest power away from the Amun priesthood.¹⁰ Some have also portrayed his move of capital as a way of escaping from the opposition of this priesthood in Thebes.¹¹ Apart from a possible reference which occurs in a fragmentary section of a boundary stele¹² and is open to manifold different interpretations, there is not a shred of evidence of any opposition to Akhenaten during his lifetime, let alone evidence specifically tying such opposition to the Amun priesthood. How then did such an interpretation arise and why have so many accepted it uncritically? To answer this question we need to explore the development of ideas about the Egyptian priesthood more generally.

DEVELOPMENT OF IDEAS ABOUT THE EGYPTIAN PRIESTHOOD

Though much of the writing about Egypt during the Classical period¹³ portrayed the culture as barbaric or other,¹⁴ certain depictions laid the groundwork for the picture of a powerful, knowledgeable priesthood. Egypt was depicted by Herodotus as the most religiously observant of peoples.¹⁵ Herodotus and Diodorus make frequent reference to the priests from whom they gathered their knowledge. Diodorus also portrays these priests as having instructed many famous Greeks.¹⁶ Many writers depicted Egypt as an ancient land, with a long tradition of learning. Some Classical writers connected this to religion, portraying Egypt as the source of much religious knowledge *e.g.* Diodorus

⁹ Breasted 1951: 392-395.

¹⁰ Reeves 2005; Giles 2001: 250.

¹¹ *e.g.* Reeves 2005: 104 and 111 where he speculates that there may have been an attempt on Akhenaten's life. Gardiner 1961: 219.

¹² Stela K, lines 20-21 and Stela X lines 22-23. For the text see Murnane and Van Siclen 1993: 26. For a recent English translation of this section of the boundary stele see Murnane 1995: 78.

¹³ For an examination of this more generally see Matthews and Roemer 2003, or for Greek ideas about Egypt as a land of wisdom and mystery, Assmann 2000.

¹⁴ See Tait 2003: 23; Hornung 2001: 25.

¹⁵ *Histories* II.37.

¹⁶ See Hornung's discussion of other writers ascribing the learning of famous Greeks to Egypt (2001: 22-23). See also Tait 2003: 33.

claims that the Greek divinities stemmed from Egypt.¹⁷ This was often painted in terms familiar to the ancients from their knowledge of Hellenic mysteries *e.g.* Iamblichus mentions Pythagoras' initiation into the divine mysteries after a 22 year stay in Egypt.¹⁸ The figure of Hermes (Trismegistus after the third century CE) is important in this context. The 42 books of learning ascribed to him by those such as Clement of Alexandria included much religious knowledge that was supposedly only for the priests, "*the guardians of Egyptian philosophy*".¹⁹ Ideas about the mystical and allegorical nature of the hieroglyphs, and the notion that they were used to conceal secret knowledge, have their origin in the writings of some Classical period authors, in particular Neo-Platonists such as Clement and Plotinus.²⁰ Lucian's staged discussion between Momos and Zeus, though critical, reflects the fact that others at the time saw Egyptian religion as of mystic significance and available only to initiates.²¹ With the Renaissance's "revival" of Classical learning, and Neo-Platonism in particular, this material was used to depict the Egyptian priesthood as an ancient, all-wise, powerful group who were the holders of important and secret knowledge of mystic significance, available only to initiates.²²

Such notions of Egyptian religion and the priesthood were important for the development of the "wicked priest" theory, and they survived into the seventeenth century and beyond.²³ Despite the importance afforded to the supposed writings of Hermes Trismegistus, the *Corpus Hermeticum*, these notions lingered on after Casaubon's exposure of the corpus as a late forgery in 1614. Athanasius Kircher was important in securing this by continuing to treat the corpus as genuine. By this and his re-espousal of the theory that the hieroglyphs were symbols encoding past wisdom, he helped ensure that Egypt was again seen as the holders of secret, mystical religious wisdom, guarded by priests. The interest in Plotinus and Iamblichus during the Renaissance period in general was also an aid in this.²⁴ By Kircher's time, the depiction of Egyptian religion was already being used in debates between Protestants and

¹⁷ *Library of History* I 9, 6.

¹⁸ Cited in Hornung 2001: 22.

¹⁹ Tait's paraphrase, 2003: 24.

²⁰ See Iversen 1961: ch. 2.

²¹ Cited and discussed by Hornung 1982: 15.

²² For a detailed examination of the picture of ancient Egypt during the Renaissance see Curran 2003.

²³ Whitehouse has argued that this influence is still felt: "Renaissance thought has been of lasting effect in providing much of the conceptual basis for the perception of Egypt in the West, its ideas surviving the radical transformation in knowledge brought about by the growth of Egyptology." (1995: 17).

²⁴ See Whitehouse 1995: 15.

Catholics,²⁵ and was connected to theories that the belief in one god could be coterminous with or predate that in the Old Testament.²⁶ Kircher argued that Egypt was the origin of true religious belief, which was passed down and inherited by the Catholic Church.²⁷ The idea that true religion began in Egypt and has been passed down to modern Christians is seen in Breasted's work. In contrast to Kircher's picture, the priests of Egypt do not play such a positive role in Breasted's work. This is partly due to the Protestant leanings of Breasted as we will explore later. To paint his picture of the priests, Breasted may have been able to draw on a picture created in the later seventeenth and eighteenth centuries.

After the religious wars of the 16th and 17th centuries, the depiction of the Egyptian priesthood began to take on a more sinister character, while Renaissance notions continued on. The more sinister depiction of the priesthood finds its fullest development within the works of the eighteenth century deists such as Toland, though there are precursors as early as Conring (mid to late seventeenth century). This focus on a malevolent priesthood grew out of ideas about their holding of secret knowledge available only to initiates. In the seventeenth century it became common to contrast an esoteric monotheism only available to kings and initiated priests with a polytheism for the masses.²⁸ In Conring's work, anticipating the later deists, the priests are seen as corrupting religion for the masses by adding cult ceremonies, duping the people so as to control them more effectively.²⁹ Similarly Sprat argued that the priests concealed "*the true Philosophy of Nature*" from men "*in the dark Shadows of Hieroglyphicks*" as "*a sure way to beget a Reverence in the Peoples Hearts towards themselves (i.e. the priests)*".³⁰ Fontenelle reflected this trend in his depiction of conniving priests manipulating oracles for political purposes.³¹ Cudworth is unusual for the period in that he connects the priests' concealment of monotheism, via hieroglyphs (thus tricking the

²⁵ With Kircher a Catholic priest and Casaubon a Protestant. See Hornung 2001: 103; Grafton 1983.

²⁶ Kircher saw Hermes as a contemporary of Abraham while others followed Diodorus in having him as a contemporary of Moses.

²⁷ See Champion and Ucko 2003: 14. Compare Iversen's more nuanced depiction of Kircher's notions of universalism with regard to religion, with Egypt merely as the best pre-Christian manifestation of a "timeless emanation of divine truth" (1961: 94).

²⁸ Cudworth (see Hornung 2001: 103); Conring 1669 (see Hornung 2001: 98-99); Sprat 1667: 5 (see Haycock 2003: 137).

²⁹ Cited in Hornung 2001: 98.

³⁰ Sprat 1667: 5 as cited in Haycock 2003: 137.

³¹ 1686. See the discussion of Assmann 1997: 214 and Manuel 1959: 47-53. Fontenelle's focus was on the Delphic oracle though he also mentioned Egypt. His theory has been applied by Egyptologists with regards to the accession of Thutmose III via the oracle of Amun, supposedly controlled by the priests of Amun. For an example of this argument see Breasted 1951: 284-285, 362 or Gardiner 1961: 181-182.

people) to the concept of a natural religion, common to all rather than secret esoteric knowledge.³²

Within eighteenth century Deism, these theories were routinely popular. Manuel contrasts one branch (Condorcet and Trenchard) of Deism which argued that the priests knew about monotheism and allowed the masses to add polytheism and superstition in order to keep them ignorant and to gain power for themselves; another which saw the priests as having been duped themselves by politicians; and a more minor one in which the priests were seen as having become enveloped in the darkness they created.³³ With Toland, de Boulanvillier and Holbach, the picture of wicked, political, deceitful, conniving priests was firmly entrenched.³⁴ That this notion became widespread and popular is demonstrated in Terrason's use of this picture of the Egyptian priesthood in his novel *Sethos*.³⁵ Like Sprat, Montagu also argued that hieroglyphs were a tool of the priests used to "*make the common people imagine that some mystery was couched under them.*"³⁶ Making clear the real object of attack here, Catholic priests, Haycock paraphrases Montagu as claiming that this use of hieroglyphs assured the Egyptian priests "*in their own positions of power, as only they could interpret them.*"³⁷ Manuel, Assmann and others have correctly outlined the manner in which the work of the deists, including their treatment of Egypt, was constructed "*to speak about the present without risking persecution*".³⁸ Assmann phrases it well when he claims that within these works: "*The model of the treacherous Egyptian priests was meant to act as a mirror of contemporary clerical institutions.*"³⁹ The Reformation, Counter-reformation, religious wars and the involvement of clerics in politics in the sixteenth and seventeenth centuries clearly helped set the conditions in which these views were articulated.⁴⁰ These events fulfilled a similar function with regard to the Enlightenment more generally, with its aversion towards organised religion and any form of superstition. Within the writings of the Enlightenment, the veiled attacks of the deists, using critiques of "barbarian pagan religion" to critique Christianity more generally, gave way to openly hostile criticisms of religion in general and an espousal of secularism. In this

³² See Assmann 1997: 80-84.

³³ Manuel 1959: 68-69.

³⁴ See Manuel 1959: 192, 230.

³⁵ 1731. See the discussion of Montserrat 2000: 53.

³⁶ Montagu 1799: 419 as cited in Haycock 2003: 149.

³⁷ Haycock 2003: 149.

³⁸ Assmann 1997: 215.

³⁹ Assmann 1997: 215.

⁴⁰ See Gascoigne's similar position, 1991: 195.

context, Wengrow has argued that one result of the French Revolution was that the portrayal of Egypt was transformed from that of a source of high culture and wisdom to a part of a generalised Oriental sphere, associated with what France was no longer *i.e.* a hierocratic order based on sacred kingship.⁴¹ This reinforced the notion of wicked deceitful priests, challenged the concept that they were holders of important religious knowledge, and paved the way for more racial interpretations of Egyptian religion. This will be explored later when we treat the impact of the rediscovery of Egypt.

Criticism of established religion was not the only motive for this negative portrayal of the Egyptian priesthood in this era. During the seventeenth and eighteenth centuries debate centred on whether similarities between Christianity and other forms of religion, such as that in Egypt, revealed the truth of the revelations within Christianity or demonstrated that such revelation was not necessary. Renaissance writers and later followers of Hermetism or mysticism believed that “*all knowledge is obtained through revelation and not reason.*”⁴² Deists argued otherwise, focusing on the corrupting nature of religions claiming specific revelations, and building on Cudworth’s concept of a natural religion, common to all.⁴³ Those such as Newton, Stukeley, Shuckland and others involved with Freemasonry continued to argue that some hidden knowledge had been passed down via the Egyptian priesthood.⁴⁴ Within this train of thinking, however, Newton also argued that corrupt Egyptian priests had distorted this original Noachic moral philosophy via star worship and hieroglyphs.⁴⁵ Warburton repeated the argument about duality within Egyptian religion, with an official polytheism designed to create political order and a secret monotheism in an attempt to contrast this to Christianity and defend its outward, institutional forms which he argued were consistent with the true beliefs of the religion.⁴⁶ Haycock has pointed out that Warburton’s arguments were appropriated by deists “*who denied the truth of any one religious doctrine.*”⁴⁷ Newton’s work had deistic elements in his focus on natural/general rather than special revelation or providence, with true knowledge and ethical teaching established well before Christ or the Prophets.⁴⁸ The Classical

⁴¹ Wengrow 2003: 179-185.

⁴² Hornung 2001: 52.

⁴³ See Assmann 1997: 80-82. Cudworth claimed that what was common to all religions was what was true.

⁴⁴ See Haycock 2003: 138-139.

⁴⁵ Haycock 2003: 141; Gascoigne 1991: 192-194.

⁴⁶ See the discussion of Assmann 1997: 96-105.

⁴⁷ Haycock 2003: 159.

⁴⁸ See the discussion of Gascoigne 1991: 184-187.

and Renaissance notion of secret esoteric wisdom passed on by the priests survived this period and grew again in popularity during the Romantic era.

The Romanticism of the late eighteenth and early nineteenth century saw many of the Renaissance notions of the priesthood and Egyptian religion resurface, as well as a focus on nature religion. This said there was diversity within depictions. Romanticism's notion of the original as the ideal lent itself to depictions of Egypt as originally monotheistic with polytheistic corruptions coming later. We have already seen how Newton's theory was quite similar. Freemasonry developed within this mould also. Dupuis had argued for just such an approach in 1795, contrasting a true and pure religion based on sun-worship which was debased by "*a corrupt, state-organised religion which deliberately set out to deceive a whole people through a tyrannical bargain between the priesthood and the monarchy.*"⁴⁹ This focus on an originally pure, universal nature religion grew in popularity during the early nineteenth century and has had an influence down to our day.⁵⁰ This tended to reverse the negative picture of the priesthood set up within deistic and Enlightenment works and revived their positive role as in the Renaissance. Given Romanticism's *raison d'être* as a reaction to the Enlightenment this makes perfect sense. Gascoigne has also connected this wane in writing "to undercut the power of priestcraft" with the rise of the Whigs in England.⁵¹ Though some Romanticist works explored the dark side of Egypt, sometimes including pictures of wicked priests,⁵² in general the focus was on the role of Egypt in the rise of monotheism or, increasingly, pantheism.⁵³ This universal, somewhat pantheistic nature religion, focusing on the worship of the sun makes its reappearance in Breasted's work on Akhenaten. In his depiction, however, the wicked priests work against such a religion while Akhenaten tries to promote it. Breasted thus mixed elements of the portrayal of Egyptian religion and the priesthood under Deism and Romanticism.

The rediscovery of ancient Egypt under Napoleon, and particularly the decipherment of hieroglyphs also had an impact upon the way Egyptian religion and its priests were portrayed. In general, religion became less of a focus for Egyptology, though such studies still centred on comparison to the Old Testament and old ideas continued on. Towards the end of the nineteenth

⁴⁹ Montserrat's paraphrase 2000: 126. His work was quite popular during the Romantic era.

⁵⁰ For a more detailed discussion of Romanticist approaches to ancient Egypt see Hornung 2001: ch. 15; Assmann 1997: 125-143

⁵¹ Gascoigne 1991: 203. The argument is that with the decline in the power of the Catholic Church, a criticism of "priesthood" was less necessary.

⁵² e.g. Desprez as discussed by Hornung 2001: 137.

⁵³ See Assmann's discussion of the notion of *hen kai pan*, 1997: 139-143.

and the beginning of the twentieth century, race and politics became more central in studies of Egyptian religion. As mentioned earlier, the French Revolution had helped push Egypt from its place as the origin of western civilisation to a more negative “oriental” sphere. Champion has argued that western contact with Egypt had the same effect, as did the rise of linguistics and a focus on ethnology.⁵⁴ He also argues that the rise of geological sciences and the theory of evolution made Egypt’s antiquity seem less impressive and that Egyptology became much more specialised and did not attempt to answer broad questions tied to science, religion, philosophy and origins as often as those writing about Egypt previously had.⁵⁵ Many of those writing, however, had cut their teeth in classical or biblical studies and thus brought with them a great number of presuppositions and, for the biblical scholars, a tendency to focus on how Egypt related to the Old Testament.⁵⁶

During the nineteenth century many variant pictures of Egyptian religion and their priesthood arise. Within Champollion’s own work one finds references to powerful priests in some sort of competition with royalty as well as the repetition of mystical notions about Hermes and the ineffable god.⁵⁷ The notion of duality within Egyptian religion (a public polytheism for the masses and a monotheism/pantheism for the priests) reappeared in many works,⁵⁸ and was criticised by others.⁵⁹ Hornung has pointed out that the idea of early monotheism in Egypt was dealt a serious blow by the discovery of Early Dynastic period material and the Pyramid texts which clearly showed Egypt to be polytheistic at this early stage of her history.⁶⁰ In the works treating Egyptian duality, some portrayed the priests negatively, as deceitful power mongers⁶¹ while others focused on their role transmitting important knowledge⁶² or combined such notions⁶³. By the time of the rediscovery of

⁵⁴ Champion 2003: 161-163. For the latter see the works of S. G. Morton in the bibliography of Ucko and Champion 2003: 203.

⁵⁵ See the comments of Gascoigne 1991: 206.

⁵⁶ See the discussion of Jeffreys 2003: introduction.

⁵⁷ See Champollion 1971: 47-48, 64-66.

⁵⁸ See Wilkinson 1841: 171-170; 1878: 158-9, 174-178; Rawlinson 1890: 38-39; de Rougé, Creuzer and Mariette, see Koch 1989: 38-39.

⁵⁹ e.g. Renouf who argued that polytheistic and monotheistic elements worked together consciously as part of Egyptian pantheism, 1884: 217. See Hornung 1982: 18-21 for detailed discussion of the various positions formulated.

⁶⁰ Hornung 1982: 23-24.

⁶¹ e.g. Wilkinson 1878: 174, 177, 471 even though he argues that such duping was necessary for social order.

⁶² de Rougé 1869 and others. See Hornung 1982: 18-21.

⁶³ As seen in Wilkinson’s works.

Amarna, there were a number of versions of Egypt's religious past and the role of her priesthood to draw upon. The next section will explore the depictions of Amarna and the Amun priesthood in this context.

DEPICTIONS OF AMARNA

The rediscovery of Amarna in the nineteenth century occurred amidst this mix of ideas about Egyptian religion and priests. Some of the elements of Breasted's wicked priest theory find precedents in this literature. Lepsius' drawings of the inscriptions and reliefs at Amarna in 1845 led him to note Akhenaten's opposition to the worship of Amun. Like many others, he speculated that this strange religion of Akhenaten's was due to foreign influence from Nubia or Western Asia.⁶⁴ Rather than conniving priests with political power, however, Lepsius repeats notions about learned priests protecting Egyptian wisdom, even connecting this with Hermetism.⁶⁵ He saw opposition to Akhenaten's reforms coming from the national hierarchy while Brugsch saw it as originating within the people and the priesthood, with them driving Akhenaten from Thebes.⁶⁶ Osburn referred to "Amonian fanatics" destroying Akhenaten's buildings in Karnak⁶⁷ and Amarna,⁶⁸ and erasing his name and that of his successors,⁶⁹ with Thebes being placed back in ascendancy after Tutankhamun's death.⁷⁰ Maspero depicted the rising wealth and power of the Amun priesthood just prior to Akhenaten's rule.⁷¹ Rawlinson argued that Akhenaten changed his capital due to the pollution of Thebes with polytheism and the worship of Amun.⁷² In his picture, Thebes was seen to be in opposition to Akhenaten's reforms.⁷³ Brugsch and Maspero similarly saw Akhenaten as

⁶⁴ See the discussion of Hornung 1999: 3-4. Compare the racial explanation of Brugsch 1859, connecting the religious reforms to Tiy's foreign nationality. This theory is repeated by Rawlinson 1890: 223. Osburn connects it to a new sect of religionists from Africa, 1854: 326-327.

⁶⁵ Lepsius 1853: 380-400.

⁶⁶ See Hornung 1999: 7-8.

⁶⁷ Osburn 1854: 334.

⁶⁸ Osburn 1854: 336.

⁶⁹ Osburn 1854: 337.

⁷⁰ Osburn 1854: 345.

⁷¹ Maspero 1891: 60, 66. Though none seem to connect this rise with Hatshepsut or Thutmose III in the way Breasted does. For a brief survey see Wilkinson 1841: 38ff; Sharpe 1846: 28-29; Rawlinson 1890: 170-207; Osburn 1854: 248. Though the latter does mention the zeal of Thutmose III for Amun, he does not explicitly link this to a rise in political power for his priesthood.

⁷² Rawlinson 1890: 227.

⁷³ Explicitly stated by Rawlinson 1890: 229-230.

subject to a curse from the Theban priests.⁷⁴ Brugsch,⁷⁵ Osburn,⁷⁶ Rawlinson⁷⁷ and Petrie⁷⁸ all praised the religious reforms of Akhenaten, with some of them contrasting this to the religion of Amun. The ideals of nature worship focused on the sun, monotheism and an ethical focus make up part of this picture in the works. Notions of a conflict, both religious and political, between the Amun priesthood and Akhenaten clearly predate the work of Breasted. This picture was not held by all before Breasted, however. Petrie anticipates much of Breasted's portrayal of Akhenaten's religious reforms⁷⁹ but on this matter merely noted a swing in the period between devotion to Aten or Amun with no reference to conniving priests.⁸⁰ Renouf argued that Akhenaten's religion was nothing other than "ordinary Egyptian orthodoxy" with its pantheistic mixture of monotheistic and polytheistic elements.⁸¹ With Erman, Breasted and Weigall we reach both the high point of popular interest in Amarna, and the formation of the popular image of he and his period, in particular, the Amun priesthood.

The portrayals of Erman, Breasted and Weigall drew on a long history of negative depictions of the Egyptian priesthood as well as notions of a conflict between Akhenaten and the Amun priesthood which had begun to appear in scholarship just prior to their own works. Their depictions are reflected in many later Amarna studies.⁸² For all three of these scholars the reign of Akhenaten was seen as the religious highpoint of Egyptian history. Each of them portrayed Akhenaten's religion in a deistic fashion and each portrayed Akhenaten as being engaged in a pitched battle with the Amun priesthood. In 1904 Erman argued that Akhenaten's faith resembled that of the modern era.⁸³ Akhenaten's religion is depicted as before his time, universal rather

⁷⁴ Supposedly fulfilled in Akhenaten's castration in Amenhotep III's Nubian campaign. See Hornung 1999: 9.

⁷⁵ See Hornung 1999: 8.

⁷⁶ Osburn 1854: 333 where he describes the moral improvement as due to greater theological truths.

⁷⁷ Rawlinson 1890: 224, where he describes this sun-disk worship as the most natural form of nature worship and p. 226 where he speculates that perhaps Joseph's descendants had taught the disk worshippers monotheism. An interesting argument given the more popular theory that Akhenaten taught monotheism to Moses.

⁷⁸ See discussion of Hornung 1999: 12.

⁷⁹ See Aldred's description 1988: 110-112; Petrie emphasised the life-giving energy of the sun's rays, the replacing of tradition and the supernatural with the scientific and realistic with an emphasis on truth, the higher ethical ideals of Akhenaten as displayed in his family life on the reliefs, etc.

⁸⁰ Petrie 1904: 227-246.

⁸¹ Renouf 1884: 230.

⁸² And studies of the eighteenth dynasty more generally, especially concerning the reigns of Hatshepsut and Thutmose III.

⁸³ Erman 1907: 67: "This new faith resembles our own so closely, that we feel our sympathies unconsciously drawn to its courageous founder."

than national, ethical rather than cultic or superstitious.⁸⁴ That Erman's picture drew on the same assumptions, if not the works, of the earlier deists is quite clear. His picture of Akhenaten's religion fits the ideals of Deism as opposed to institutionalised religion and its functionaries, the priests. Erman contrasts Akhenaten's "*deism of his day*"⁸⁵ with the opposition from the Amun priesthood,⁸⁶ whom he portrays as being politically powerful.⁸⁷ The end of Akhenaten's reign is portrayed as a victory for the followers of Amun.⁸⁸ As Breasted was a student of Erman's in Berlin it is not surprising that all of these elements are found in his works also.⁸⁹

Montserrat's analysis of Breasted's depiction of Akhenaten focuses on the influence of his Protestant background.⁹⁰ Breasted originally studied at Chicago Theological Seminary and his anti-catholic stance is clear throughout his works. His negative depiction of the Amun priesthood draws on a number of anti-papal criticisms. He refers to the high priest of Amun as "the first *pontifex maximus*", and to the Amun priesthood as a "papacy" and as "sacerdotal".⁹¹ He contrasts Akhenaten's religion with "*traditional theology*".⁹² Making the reader recall the indulgences of Martin Luther's day, Breasted refers to "mechanical magical agencies for insuring justification" being repelled by Akhenaten.⁹³ There are other indications of his Seminary learning in his repeated connection between Akhenaten's religion and that of the Hebrew psalms and prophets.⁹⁴ His description of the iconography of Atenism⁹⁵ as well as the purpose of Akhenaten's new capital,⁹⁶ makes Akhenaten and his religion appear evangelistic in missionary zeal. Breasted's claim that the craftsmen would have hated Akhenaten because they could no longer sell amulets in the temple gateway⁹⁷ was drawn from the description of the Ephesian craftsmen

⁸⁴ Erman 1907: 65-67.

⁸⁵ Erman 1907: 67.

⁸⁶ Erman 1907: 63-64.

⁸⁷ e.g. Erman 1907: 71: "the high priests of Amon were men of almost royal rank."

⁸⁸ Erman 1907: 69-71.

⁸⁹ For a discussion of both Erman and Breasted's approaches to Egyptian religion see Koch 1989: 45-52.

⁹⁰ Montserrat 2000: 98-103.

⁹¹ Breasted 1972 [1912]: 319.

⁹² Breasted 1972 [1912]: 321.

⁹³ Breasted 1972: 339.

⁹⁴ e.g. Breasted 1972: 334, 343.

⁹⁵ Breasted 1972: 320 where the sun disk is praised as being universally intelligible as opposed to traditional Egyptian iconography.

⁹⁶ Breasted 1972: 322: "intended as a centre for the dissemination of Solar monotheism."

⁹⁷ Breasted 1972: 341.

in Acts 19 after Paul teaches against Artemis rather than from Egyptian evidence of such opposition.⁹⁸ While Breasted's anti-catholicism and his biblical training clearly impacted upon his picture of Akhenaten, he was not a defender of conservative Protestant Christianity. Breasted's depiction of the substance of Akhenaten's religion makes it clear that he has a form of Deism in mind. As with Erman, Breasted portrays Akhenaten's religion as universal rather than parochial,⁹⁹ as a nature religion,¹⁰⁰ focused on worship of the sun, as ethical in contrast to sacerdotal and traditional. Akhenaten's religion "*anticipates much of the later development in religion even down to our own time*"¹⁰¹ for Breasted. As with some before him, and many after, Breasted combines Romanticist notions of nature and light¹⁰² with deistic assumptions about the importance of general (or natural) rather than special revelation. As opposed to the religion around Amun, "the hidden one", Akhenaten's religion is depicted as coming from "the present and visible evidences of his god's dominion, evidences open to all".¹⁰³ It is a "*discernment of the presence of God in nature, and an appreciation of the revelation of God in the visible world*".¹⁰⁴ That what is envisaged is deistic nature worship is further exemplified in Breasted's declaration that "*it is a gospel of the beauty and beneficence of the natural order*".¹⁰⁵ Breasted's depiction of Akhenaten is perhaps due more to his somewhat secular liberal humanism, the impact of higher criticism and his own experience of doubting Christianity. Kuklick's account of Breasted emphasises the impact of Dewey's progressivist thought upon him. He portrays this influence, along with higher criticism, leading Breasted to replace theology with humanism, Christian with Western civilisation as end points on the path of progress.¹⁰⁶ For Breasted, Akhenaten was an important step on this path. The form of religion praised by Breasted, as well as the development of religion he followed, owed much to higher criticism. The notion of the development of religion from a parochial or nationalistic and harsh god, originally connected to a local cult (J or E) towards the high point of "ethical monotheism" (D), which is then corrupted by a priestly cohort (P) is reflected somewhat in Breasted's account of Egyptian religious history.

⁹⁸ One could imagine Breasted's Amonite followers crying out: "Great is Amun of the Thebans!"

⁹⁹ Breasted 1972: 331-332.

¹⁰⁰ e.g. Breasted 1972: 334-336.

¹⁰¹ Breasted 1972: 334.

¹⁰² See for example Breasted 1972: 334 where he links Atenism to Wordsworth.

¹⁰³ Breasted 1972: 339.

¹⁰⁴ Breasted 1972: 334.

¹⁰⁵ Breasted 1972: 335.

¹⁰⁶ Kuklick 1996: 122, 185.

The ideal of "ethical monotheism" is also shared, reflecting much of deistic thought. Breasted's Akhenaten and his battle with the Amun priesthood are reflective of the impact of higher criticism, secular liberal humanism, Deism, Romanticism and his Protestant background. In a more general sense they also reflect the notion that Egypt is a source of wisdom, which Breasted uses as part of a racial history which takes the Jews out of a central role within the development of religion. Breasted's depiction also reflects his early twentieth century context with history's focus on the political.

Breasted's picture of Akhenaten's rule, and of Egyptian religion and priesthoods has been perhaps the most influential due to both his esteemed position and the popular nature of many of his works. It is his work more than any other that has ensured that the notion of a wicked, scheming priesthood has been incorporated into Egyptian history. Not long after Breasted's *History of Egypt* (1905), Arthur Weigall produced the first biography of Akhenaten, crediting Breasted's work as a source of information in the opening pages.¹⁰⁷ Weigall's picture is very reminiscent of Breasted's and its own popularity helped to spread the picture of Akhenaten that Breasted had created. As Montserrat has argued, Weigall, also had ties to Protestantism, and, like Breasted, "his own religious beliefs were inclusive, almost deistic."¹⁰⁸ Weigall was certainly more inclusive than Breasted, but the similarity in approach led to a similar picture being painted. The picture of a conniving priesthood in ancient Egypt, and in particular that of Amun during the reign of Akhenaten, was subsequently repeated in numerous works. Kees translated Weigall's work and repeats the priestly conspiracy theories in his own analysis of Egyptian religion.¹⁰⁹ Baikie does likewise, following Weigall and Breasted.¹¹⁰ Another influential Egyptologist, following in the footsteps of Breasted with hugely popular works was Sir Alan Gardiner. His *Egypt of the Pharaohs*, dedicated to the memory of Breasted, very much builds on the notion of a scheming priesthood and has kept this theory alive and well.¹¹¹ While every element of Breasted's description of Akhenaten's reign has not been taken over wholesale, with many having been criticised, the general picture of a conflict with the Amun priesthood has remained in many works.¹¹²

¹⁰⁷ Weigall 1922: 2-3.

¹⁰⁸ Montserrat 2000: 103. See citation from Weigall's letter on that page also.

¹⁰⁹ Kees 1953.

¹¹⁰ 1926 e.g. see pp. 173-175 where he describes the opposition of the Amun priesthood to Akhenaten.

¹¹¹ See especially his description of the reigns of Thutmose III, Hatshepsut and Akhenaten.

¹¹² See especially Reeves 2005.

QUESTIONING ASSUMPTIONS AND NEW SUGGESTIONS

The creation of the “wicked priest” theory and its application to Amarna is not based on any firm evidence and reflects the historical, religious, political and personal contexts of the scholars who have put it forward. As Montserrat claims for pictures of Akhenaten in general: “one thing which underpins many of them is the desire to find an antecedent for oneself or one’s beliefs in ancient Egypt.”¹¹³ This was certainly the case with the depictions of Erman, Breasted and Weigall, contrasting a deistic Akhenaten with wicked sacerdotal priests. Though absence of evidence does not necessarily equal evidence of absence, it does raise the possibility of non-existence. The only evidence supporting the “wicked priest” theory at Amarna is open to varying interpretation and is quite circumstantial. There is some evidence which can be used to question the picture created and other interpretations are possible.

The documentary evidence used to support the “wicked priest” theory at Amarna consists of one broken passage of a Boundary Stele. In addition to this, the circumstantial evidence of Akhenaten’s change of capital and religion, the hacking out of Amun’s name by Akhenaten as well as the treatment of Akhenaten’s name and memory after his rule have also been used to bolster this picture. Within such a portrayal, Akhenaten’s change of capital is designed to escape or to break the power of the Amun priesthood.¹¹⁴ Some have also portrayed his religious changes as stemming from the same motives,¹¹⁵ though Breasted thinks the motivation was truly religious.¹¹⁶ Many have described the hacking out of Amun’s name in a manner that implies that this was carried out as part of an attack by Akhenaten on the priesthood of this god.¹¹⁷ Most of the scholars who paint such a picture see the hacking out of Akhenaten’s name as a response in kind by the Amun priesthood.¹¹⁸ Though these interpretations are possible, they are not supported by evidence and are based on modern assumptions about the relationship between the state and religion¹¹⁹ as well as negative stereotypes of priesthoods, especially those of ancient Egypt. There is evidence which can be used to question the notion of a battle between the Amun priesthood and Akhenaten, as there are different ways of interpreting

¹¹³ Montserrat 2000: 2-3.

¹¹⁴ Reeves 2005: 104. Weigall 1922: 79.

¹¹⁵ Giles 2001: 12; Cannuyer 1985.

¹¹⁶ Breasted 1972: 334.

¹¹⁷ Gardiner 1961: 228; Breasted 1951: 363.

¹¹⁸ Breasted 1972: 343.

¹¹⁹ As Montserrat agrees: “They also subtly superimpose western ways of thinking about monarchy, art and religion onto a world where their meanings and ideological underpinnings were very different” (2000: 12).

the changes of the Amarna period, the hacking out of names and the reaction to Akhenaten's rule.

Some of the arguments against the notion of "wicked priests" conniving against the throne in ancient Egypt, and a battle between Akhenaten and the Amun priesthood in particular, stem from a more general understanding of the role of the priesthood and religion and their relationship to kingship in ancient Egypt. Those depictions containing a conflict between priesthood and king tend to paint a picture of a priesthood who are fairly independent of the royal household, whose power is based on either their wealth and land holdings¹²⁰ or the prominence of their god's city.¹²¹ Such a picture ignores the dependence of the priesthood upon the royal household and the king in particular. Though there is an element of heredity within priestly office, each and every priest in ancient Egypt is considered to be royally appointed. Rather than being independently wealthy, there is good reason to believe that the priesthoods were administrators of pharaonic wealth which was still at royal disposal. Given the official nature of the religion the Egyptian priesthood were involved in it is unlikely that it had any large scale popular following; though the evidence available from Egypt leaves us unable to judge this effectively. The idea that religion and politics were two separate spheres in ancient Egypt, particularly with regard to official religion connected to the kingship and the royal household, is based on assumptions created in the post-Enlightenment western world rather than an understanding of the nature of Egyptian religion and kingship.¹²² Less importantly, the notion that the power of a particular god is connected to the power of its home city has never had any firm basis in Egyptology despite its long popularity. There are numerous prominent gods, Amun included, for whom we do not know their home city. It is quite possible that Amun was chosen to be part of the official Egyptian religion due to his quite unknown, and thus malleable, status. The last point, in this more general regard, is that the nature of the thought surrounding Egyptian kingship would make political opposition during a king's lifetime quite difficult. Within a highly centralised system portraying the king as divine, and Egyptian mores and practices as unchanging, it would be difficult to rise up against a current king and replace him. If it caught on that the king was quite fallible and so easily done away with the whole system of thought surrounding the institution

¹²⁰ Breasted 1972: 319; Weigall 1922: 79; Giles 2001: 7; Redford 1984: 158.

¹²¹ Breasted 1972: 318; Giles 2001: 5.

¹²² That the two go hand in hand in ancient Egypt does not necessarily rule out the notion that some took the official religion seriously, nor, given the official nature of the religion we tend to have evidence for, that there was not a widespread, more genuine or popular set (or sets) of religious beliefs in ancient Egypt.

of kingship would be open to question. Within such a system where kings did not openly deride their predecessors, it would be impossible to justify any such coup. The only evidence we do have from Egypt of attempts at such a coup, is to do with the harem conspiracy during the reign of Ramesses III.¹²³

This understanding of ancient Egypt obviously does not sit well with the “wicked priest” theory or its application at Amarna. There is some more specific evidence which questions it also, though the significance of much of it differs depending on one’s theory with regard to the question of co-regencies during Akhenaten’s reign. One of the arguments which has been put forward for the idea that Akhenaten was trying to break the power of the old priestly bureaucracy focuses on the change in personnel from Amenhotep III’s reign from Thebes to Akhenaten’s at Amarna. This argument has been accounted for by Aldred who has pointed out that, what evidence we have supports the belief that Akhenaten’s “new officials” were the sons of his father’s officials, and that it would have been difficult for Akhenaten to locate the number of literate and capable administrators he needed from any other body of people.¹²⁴ He also pointed out that these officials’ claims to have been advanced by the king are merely conventional acknowledgements of the fact that the king appointed them.¹²⁵ Given Amenhotep III’s long reign, it should be unsurprising that there is only really evidence for the existence of officials who served under him turning up under Akhenaten during the early Theban period of his rule, some as functionaries of the Aten.¹²⁶ The fact that many of Akhenaten’s officials turn up during the reigns of his successors also questions the idea that the Amarna aftermath consisted of a reaction by the Amun priesthood, and a regathering of their power over the royal household.¹²⁷ Material from early in Akhenaten’s reign and late in the reign of Amenhotep III shows the co-existence of the Amun and Aten cults, and the control of each by the king. For example, a Ramose was Steward of the Mansion of the Aten while his wife was chantress of Amun,¹²⁸ and another Ramose includes references to both Amun and the

¹²³ The Instruction of Amenemose depicts the assassination of the pharaoh, though the message of the text seems to be that all is well as the rightful heir, Sesostris, is on the throne and will rule wisely.

¹²⁴ Aldred 1968: 259. Though see Leprohon 1985: 95 for a different interpretation based on Tutankhamun’s Restoration Stela.

¹²⁵ Aldred 1968: 259.

¹²⁶ For some such evidence see the tomb inscriptions of Ramose and Aper-el in Helck 1955-58: text 634, pp. 1776, 1778, 1780-83, 1788-89 and Zivie 1990: 151-166. Translations in Murnane 1995, section 2, texts 32-A-F (Ramose) and 26-A (Aper-el).

¹²⁷ See Aldred 1988: 248 regarding those who turn up after Akhenaten. The notion that the Amun priesthood were regathering power after Akhenaten is clearly argued by Breasted 1951: 393-401.

¹²⁸ Helck 1955-58: text 753, p. 1995. Translation in Murnane 1995: text 31.

Aten in his tomb,¹²⁹ as does Kheruef.¹³⁰ A text from Wadi Hammamat from the fourth year of Akhenaten's rule shows the continued existence of a high priest of Amun, who takes his orders from Akhenaten.¹³¹ Of course, this material dates to the period before Akhenaten moved his capital and began hacking out the name of Amun and thus very little reaction would be expected from the Amun priesthood. However, as circumstantial evidence, it fits a picture where the king is in control of the official religion, including both the cult of Amun and Aten. At the least it can be used to question Giles' notion that the Aten cult was designed to limit the power of the Amun priesthood from its inception.¹³² The fact that Akhenaten's successors continued to refer to Aten in their texts as well as Amun could also be used to question the idea that there was some conflict between Akhenaten's Atenism and the worship of Amun.¹³³ That Amenhotep III simultaneously promoted Aten while pouring wealth into the Amun administration and building numerous edifices dedicated to Amun raises similar problems. Even more so if one accepts the arguments for a long co-regency for Amenhotep and his son.¹³⁴

Given the nature of the evidence available for the Amarna period it is not possible to disprove the application of the "wicked priest" theory there. What we will do, however, is to set out some of the other possible interpretations regarding those areas in which this theory has had an influence in Amarna studies, namely: Akhenaten's change of religion and capital; the hacking out of names during the period; and reactions to Akhenaten's rule. To some extent these are based on certain notions regarding Egyptian historiography and the portrayal of the pharaoh. We shall begin with Akhenaten's religious changes.

Above it was demonstrated that a number of historians have contrasted Akhenaten's lofty religion with traditional Egyptian religion, highlighting the superior nature of the former. Many other historians have not been so kind, using these changes to highlight his deranged state.¹³⁵ A number of studies have also

¹²⁹ Helck 1955-58: text 634, pp. 1776, 1778, 1780-83, 1788-89. Translation in Murnane 1995: texts 32-A-F.

¹³⁰ Epigraphic Survey 1980: 34-37, plates 12-15. Translation in Murnane 1995: text 30-B.

¹³¹ Goyon 1957: texts 90 and 91, pp. 106-7, plates xxv and xxxi. Translation in Murnane 1995: text 35-A.

¹³² Giles 2001: 12.

¹³³ e.g. in Tutankhamun's reign: Helck 1955-58: text 789, p. 2063. Translation in Murnane 1995: text 101-C where he is described as "son of Amun, child of Aten"; Ay: Schaden 1984: 53, fig. 31. Translation also in Murnane 1995: text 104-A, where Aten is back to being an aspect of the sun god; Horemheb: Martin 1989: 78-84, plate 191 and 94-97, plates 111-115. Translation in Murnane 1995: text 105-A; Helck 1955-58: text 804, pp. 2089-94. Translation in Murnane 1995: text 105-B.

¹³⁴ This writer does not mean to imply that he does. We will not venture into that quagmire here.

¹³⁵ e.g. Giles 1970: 92, 113 and Redford 1984: 232-234.

focused on the fact that much of the substance of Akhenaten's Atenism was in existence, or at least developing, before he came to the throne.¹³⁶ Some of this earlier development consisted of raising Re to a greater prominence, portraying him almost as a sole god.¹³⁷ Whereas earlier Re had been accompanied by other gods during his nightly underworld journey who aided him against his enemies, just previous to Akhenaten's reign, Re was depicted as travelling alone, with other aspects of the sun-god accompanying him such as the disk, or Aten. Along with this henotheistic focus on Re, went an increase in the status of the Pharaoh. It is not hard to see how Akhenaten's religious changes continued this development. Rather than stopping at henotheism, he moved to monotheism.¹³⁸ In contrast to Breasted's depiction of the religion as open and accessible to all, Akhenaten makes himself the sole access to the one god, taking on the role of sole high priest. Some have acknowledged this centralisation of power in the king during Akhenaten's reign and have seen in it an attempt to wrest power away from the Amun priesthood, and place it back in the hands of pharaoh.¹³⁹ This is unnecessary speculation for which there is no evidence. It is quite possible to read Akhenaten's changes without such conflict in the background. Some have done so, referring to Akhenaten as deranged or as a megalomaniac. Though there may be some argument for traces of the latter, given the paucity of the evidence available any speculation as to the cause for some derangement, whether based on psychological, physiological or medical arguments is baseless. There is overwhelming evidence demonstrating the concentration of power in the hands of Akhenaten within Atenism. There are also some good reasons to suspect that changes in the official religion of Egypt go hand in hand with political changes. However, we will follow Giles' advice where he failed to follow it himself and refrain from speculating as to the motivations behind Akhenaten's religious changes.¹⁴⁰ We merely wish to point out that whatever his motivation, and whether he truly believed in what he espoused or not, it is unnecessary to presuppose a violent (or any for that matter) conflict with the Amun priesthood in the background for our reconstruction of the religious changes that occurred during his rule.

Akhenaten's change of capital has similarly been put down to his conflict with the Amun priesthood, with many seeing Akhetaten as either a refuge

¹³⁶ See Assmann's seminal work, 1995. Giles, 1970, focused on these earlier developments, which very much fits his picture of Akhenaten as incompetent as opposed to the visionary of Breasted's works.

¹³⁷ See Assmann 1995: ch. 3.

¹³⁸ But see Krauss 2001.

¹³⁹ e.g. Reeves 2005: 104-111; Giles 2001: 12-13.

¹⁴⁰ Giles 1970: 29; 2001: 3.

from their scheming where he can hide away and focus on his religion,¹⁴¹ or as a political move on his part, again, in an attempt to wrest power from the priesthood.¹⁴² This imagined conflict need not impinge upon our notions regarding the causes behind Akhenaten's change of capital. Other motivations are possible, and Akhenaten does state his own motivations somewhat in his boundary stele.¹⁴³ Montserrat has raised the possibility that the move of capital, as well as being theological, also fulfils the traditional requirement of pharaohs to be seen as great builders.¹⁴⁴ In the boundary stele, Akhenaten states quite simply that he has chosen the site for the Aten, as directed by him.¹⁴⁵ Though we may not believe Akhenaten that the Aten actually directed him to the site, there is no real reason to question the idea that Akhetaten was built primarily for theological purposes, including piety towards the Aten.¹⁴⁶ Within the boundary stele there is an emphasis on being able to observe the Aten rising.¹⁴⁷ Aldred, among others, has observed that the site chosen has a band of cliffs to the east so that the rising sun would form "*a gigantic representation of the hieroglyph akhet, 'horizon'*".¹⁴⁸ Another emphasis within the boundary stele is that the site was previously unused, and not dedicated to any other gods.¹⁴⁹ Numerous historians have argued that part of the reason Akhenaten changed his capital was that he found "Thebes embarrassed by too many theological traditions."¹⁵⁰ The idea that Akhetaten was built so that Atenism was not confronted by surviving traces of other forms of worship has some merit. Throughout Egypt's long history, there is a tendency for official texts

¹⁴¹ Hornung 1999: 63.

¹⁴² Reeves 2005: 104-111.

¹⁴³ We have mentioned earlier that the broken section of the earlier boundary stele mentioning some form of opposition is too fragmentary to connect to any specific form of opposition, let alone to that of the Amun priesthood. Stela K, lines 20-21 and Stela X lines 22-23. For the text see Murnane and Van Siclen 1993: 26. For a recent English translation of this section of the boundary stele see Murnane 1995: 78. Aldred has argued that there was perhaps some opposition by officials to being buried at Akhetaten rather than in their previously allocated burial spots, possibly in Thebes or Memphis, (1988: 270).

¹⁴⁴ Montserrat 2000: 17.

¹⁴⁵ Stela K, columns xix-xxi; lines 2, 5. For the text see Murnane and Van Siclen 1993: 20-22. For a recent English translation see Murnane 1995: text 37, pp. 74-75.

¹⁴⁶ Whether it was also intended as an administrative centre is open to debate. For some of the arguments against this notion see Hornung 1999: ch. 5, though the presence of the diplomatic archive suggests otherwise.

¹⁴⁷ Stela K, line 3. For the text see Murnane and Van Siclen 1993: 21-22. For a recent English translation see Murnane 1995: text 37, p. 75.

¹⁴⁸ Aldred 1988: 269.

¹⁴⁹ Stela K, line 1. For the text see Murnane and Van Siclen 1993: 21. For a recent English translation see Murnane 1995: text 37, p. 75.

¹⁵⁰ Breasted 1972: 322.

to portray things as unchanging.¹⁵¹ This may be connected to the conception of kingship in Egypt and the tendency to avoid open criticism of preceding practice. As mentioned earlier, to criticise a previous pharaoh would be to imply that he was not infallible, perhaps weakening the image any current or future pharaoh could project. Similarly, to have evidence that the official religion practised by pharaoh contrasted with that having been practised in the past would be problematic in Egypt. Moving the capital from Thebes helped to avoid such problems. Hacking out the name of Amun perhaps fulfilled a similar function, as the subsequent hacking out of Akhenaten's name may have.

The hacking out of the name of Amun by Akhenaten and the subsequent treatment of Akhenaten's name and reputation have been portrayed as part of his struggle with the Amun priesthood. On the face of it, it is the best evidence for the existence of such a conflict, for surely the Amun priesthood would react violently to the name of their god being hacked out. This is to assume several things which are unnecessary, perhaps incorrect: firstly, that the priests of Amun had a genuine belief in the religion surrounding that god or that their position of power was dependent on their connection to that god specifically; secondly, that this genuine belief or dependence created a sense of unity within this group; thirdly, that royal appointees such as these priests would be in a position to oppose pharaoh. Most of these assumptions are unlikely at best, and are certainly unproven. Given Egypt's approach to portraying official history, religion and kingship, it is quite possible that the hacking out of the name of Amun was, as Montserrat has suggested for the subsequent hacking out of Akhenaten's name: "partly intended to create an ideologically correct view of history".¹⁵² Within a historiography which, apart from some brief moments surrounding the first intermediate period, does not admit of any military losses, things going wrong, weaknesses in pharaoh, mistakes by previous pharaohs, religious change, etc. this is quite a plausible argument. It may be that such a possibility is worth pursuing with regard to the treatment of another pharaoh whose inscriptions left evidence of tradition being swept aside; Hatshepsut. Some subsequent references to Akhenaten do seem to necessitate the existence of some animosity towards his rule¹⁵³ but there is

¹⁵¹ This is an image many Egyptologists have accepted over the years.

¹⁵² Montserrat 2000: 49.

¹⁵³ See the reference to "the enemy from Akhet-Aten" in the legal text in the Tomb-Chapel of Mose, S14, in Gaballa 1977, p. 25 and plates. For a more recent English translation see Murnane 1995: text 109. See also the reference to "the rebellion" or "the rebel", in Papyrus Inv. 3040 A, rev. line 7 in Gardiner 1937: 124. For a more recent English translation see Murnane 1995, text 110. Both texts are from the Ramesside period.

no reason to suppose that this originated from the Amun priesthood, or that it was in any way connected to them. Both Tutankhamun and Horemheb's protestations about repairing temples and re-instituting religious practices can be seen as traditional rhetoric coming at the beginning of a pharaoh's reign rather than as specific reference to or criticism of Akhenaten.¹⁵⁴ Though some veiled reference to past abuse is possible in both of these texts, it has been recognised by numerous scholars, even some who hold to the wicked priest theory to some extent, that the end of the Amarna period was not a victory for the Amun priesthood.¹⁵⁵ The religion followed after Amarna was a return to what Assmann has termed the "New Solar Theology" that came to prominence just before the period,¹⁵⁶ and the capital moved to Memphis, not Thebes. Amun's name was also not the only name hacked out by Akhenaten. Apart from this, the hacking out of Akhenaten's name probably dates to the nineteenth dynasty rather than to his immediate successors' reigns.¹⁵⁷ The treatment of his name, some possible veiled references to his rule in a negative tone and his excision from the king lists does not necessarily imply a popular reaction to his religious changes, nor the reaction of an embittered priesthood. To be frank, there is not enough evidence to know the grounds for these reactions. The erasure of his name and his excision from the king lists fit with our argument that this was part of Egypt's approach to the portrayal of official history and religion. The animosity felt towards Akhenaten in texts referring to his rule as "the time of the enemy of Akhet-Aten" or "the rebellion",¹⁵⁸ may be due to a dislike of the sweeping away of what had become traditional religious beliefs. It may also be due to the trouble caused by Akhenaten to those following him who wished to present a unilinear view of Egypt's history, including her religious history. It may owe its existence in part to the economic instability which followed his rule.¹⁵⁹ Whatever it was due to, it is highly unlikely that it was carried out at the behest of a disgruntled Amun priesthood, though of course, if this was the case, we would not expect it to turn up in the Egyptian official records.

¹⁵⁴ See Tutankhamun's restoration inscription, Helck 1955-58: text 772, pp. 2025-32 and Horemheb's edict, Kruchten 1981. Texts 99 and 108 respectively in Murnane 1995. For a differing interpretation of these texts see Leprohon 1985: 98-101.

¹⁵⁵ e.g. Giles 1970: 113.

¹⁵⁶ Assmann 1995.

¹⁵⁷ Giles 2001: 23. Though Horemheb did dismantle some of Akhenaten's building structures and re-use the materials. See Spencer 1989, as cited by Eaton-Krauss 2002: 98, fn. 34.

¹⁵⁸ See footnote 153.

¹⁵⁹ See Leprohon 1985: 96.

CONCLUSIONS

There is no solid evidence in Egypt's history, and certainly not during the Amarna period of any priestly conspiracy. The notion of Egyptian wicked priests and in particular, the Amun priesthood of the eighteenth dynasty, has developed from certain assumptions about Egyptian religion and her priesthood which have grown and changed since Classical times. The notion of Egyptian priests as the holders of secret wisdom grew in the Classical period and was revived during the Renaissance. After the religious wars arising out of the Reformation, a more sinister depiction of the Egyptian priesthood arose. While the increase of knowledge about ancient Egypt destroyed many pre-Napoleonic ideas about her and her religion, the notion of wicked scheming priests has continued on. Upon the discovery of Amarna and Akhenaten, the notion was applied to the history of this period. Under the influence of Erman, Breasted popularised a version of this application, depicting Akhenaten's lofty nature religion as designed to work against the traditional religion of Egypt and to destroy the power of the Amun priesthood. In differing versions, the concept of conflict between Akhenaten and the Amun priesthood has been repeated *ad infinitum* despite the absence of any evidence for such conflict. There are good reasons to question the existence of such a conflict based on an understanding of Egypt's political and religious structure as well as her official historiography. It is quite possible to explain Akhenaten's religious changes, his move of capital, the hacking out of names surrounding the Amarna period, and the reaction to Akhenaten's rule more generally, without recourse to the theory of a conflict between Akhenaten and the Amun priesthood though the evidence for any particular theory is such as to make it somewhat tentative if not speculative.

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VIOLENCIA FENICIA EN EL MEDITERRÁNEO ORIENTAL*

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Resumen: Violencia fenicia en el Mediterráneo Oriental

El propósito del presente artículo es analizar las evidencias relacionadas con una posible expansión violenta de Tiro en el norte de Israel y Chipre y sus repercusiones en la posterior expansión hacia Occidente.

Palabras clave: Tiro – Sidón – Chipre – Israel – colonización fenicia

Summary: Phoenician Violence in the Eastern Mediterranean

The aim of this paper is to analyze the data related with a alleged violent expansion of Tyre in northern Israel and Cyprus, and its impact on Tyre's further expansion towards the West.

Keywords: Tyre – Sidon – Cyprus – Israel – Phoenician colonization

CONFLICTIVIDAD EN LA COLONIZACIÓN FENICIA DE LA PENÍNSULA IBÉRICA

En los últimos años se está desarrollando un interesante debate en torno al papel que jugó la violencia dentro del contexto de la colonización fenicia de la Península Ibérica. En este sentido, algunos autores apuntan cada vez con mayor insistencia la posibilidad de que la violencia hubiera estado mucho más presente a lo largo de todo el proceso de lo que se había creído hasta el momento. Los aspectos esenciales de esos nuevos planteamientos ya han sido perfectamente sistematizados¹, por lo que a continuación bastará con recordar brevemente las principales aportaciones realizadas.

Tsirkin ha sido uno de los primeros autores en apuntar un posible rechazo por parte de la población del suroeste de la Península al establecimiento

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¹ Wagner 2005; 2007.

colonial fenicio en su territorio. Dicho rechazo se habría producido ya en un primer momento, cuando la población autóctona trató por todos los medios de evitar su acceso directo a los recursos naturales del país. Como pruebas de esa conflictividad esgrime la misma ubicación geográfica de las colonias fenicias, lejos de los recursos minerales de la zona, así como determinadas informaciones aportadas por los autores clásicos (Justino, Macrobio) donde se mencionan, por ejemplo, enfrentamientos armados entre tartesios y los fenicios de Gadir².

Por su parte, Escacena Carrasco ha señalado que los colonos fenicios, mucho más numerosos de lo que se creía hasta ahora, se asentaron en el antiguo estuario del Guadalquivir, apropiándose del territorio indígena y llevando a cabo una explotación sistemática de los recursos locales. La respuesta de los antiguos habitantes de la región a esta situación fundada en la capacidad de coerción fenicia implicó igualmente el uso de la violencia, tal y como se aprecia en la destrucción de los santuarios fenicios de la zona, auténticos símbolos del poder colonial, ocurrida en el siglo VI a.C.³

Finalmente Wagner ha explicado la conflictividad entre los colonizadores y las comunidades locales como el resultado de dos factores clave. Por una parte destaca el establecimiento de un intercambio desigual, lo que generaba una sobre-explotación del trabajo autóctono, privando a las comunidades de los medios necesarios que aseguraran su reproducción. Por otra parte, la necesidad de contar con territorios que permitieran el mantenimiento de las colonias sin depender de recursos agrícolas ajenos condujo a los colonizadores a apropiarse de unos territorios que, lógicamente, fueron sustraídos a la población local. Entre las evidencias materiales que demostrarían esa conflictividad Wagner ha señalado la construcción de importantes sistemas defensivos alrededor de algunos asentamientos fenicios (Doña Blanca, La Fonteta, Tavira, Toscanos). Asimismo, y aceptando las interpretaciones propuestas por Moreno Arrastio⁴, Wagner se ha referido a determinadas estelas decoradas del suroeste en las que se representaban armas y cuerpos humanos, símbolos de una auténtica “caza del hombre”, en el contexto de un comercio de esclavos empleados en las explotaciones mineras y agrícolas promovidas por las colonias fenicias. De forma similar, el hallazgo de abundante cultura material de tradición local en el interior de asentamientos fenicios podría atestiguar la instalación forzosa de indígenas en las colonias. Finalmente, la desaparición de asentamientos

² Tsirkin 1997.

³ Escacena Carrasco 2002; 2004.

⁴ Moreno Arrastio 2000.

autóctonos en determinadas áreas del sureste y del litoral occidental de Málaga coincidiendo con el inicio de la presencia fenicia en esas regiones, probaría de nuevo la difícil convivencia entre los colonizadores y las comunidades locales⁵.

LOS PRECEDENTES ORIENTALES DE LA VIOLENCIA COLONIAL DE TIRO

El conjunto de investigaciones y reflexiones expuestas brevemente en el apartado anterior es suficiente para reconocer la pertinencia del nuevo enfoque planteado. Con todo, y desde la perspectiva en este caso de un orientalista, resulta oportuno matizar uno de los argumentos utilizados para defender esta nueva visión de la colonización fenicia de la Península Ibérica. Nos estamos refiriendo a la existencia en el Mediterráneo Oriental de precedentes de expansión violenta fenicia. Wagner es el autor que más insistencia ha puesto en esta cuestión, rastreando en la documentación arqueológica y escrita evidencias acerca de esos primeros episodios de violencia colonial. Tras una primera exposición en la que se recogía información relativa al conjunto del mundo fenicio⁶, este autor posteriormente ha acotado la cuestión refiriéndose únicamente a los casos en los que Tiro aparece como la protagonista en el ejercicio de la violencia⁷. En efecto, éstos son los casos que resultan más significativos para tratar de ilustrar los precedentes de los posteriores conflictos desarrollados en la Península Ibérica en el contexto de la expansión occidental tiria.

En esa última exposición, Wagner ha subrayado tres episodios concretos de violencia tiria. El primero tuvo lugar en el norte de Israel a mediados del siglo XI a.C. En esos momentos Tiro habría iniciado una expansión territorial hacia la región del Monte Carmelo y las colinas de la Alta Galilea ocupando y destruyendo diversos asentamientos de la región. El segundo ejemplo se basa en una noticia recogida por Flavio Josefo, donde se hace referencia a la expedición de castigo llevada a cabo por Hiram de Tiro (ca. 969–936 a.C.) contra los habitantes de Kition, quienes previamente se habrían negado a pagar tributo a Tiro⁸. Finalmente, el tercer ejemplo señalado por Wagner también se basa en una información aportada por Flavio Josefo, el cual hace

⁵ Wagner 2005; 2007.

⁶ Wagner 2005: 180ss.

⁷ Wagner 2007: 122s.

⁸ *Ant. Iud.* VIII: 144ss; *Contr. Apion.* I: 119. De hecho, Flavio Josefo en realidad menciona a los iticeos. Sin embargo, la identificación entre los iticeos y los habitantes de Kition es ampliamente aceptada (Aubet 1994: 50; Blázquez 1999: 74, 77; López Pardo 2000: 23).

referencia a la fundación, durante el reinado de Itobaal de Tiro (ca. 886–855 a.C), de una colonia tiria, Botris, en el territorio de Biblos, hecho que algunos investigadores interpretan como un acto hostil por parte de Tiro⁹.

Tras exponer los datos anteriores Wagner concluye:

*“Si, como hemos visto, Tiro no deja de ejercer la violencia en su política de expansión y consolidación de sus intereses en territorios e islas próximas, ¿hay razones serias para pensar que su presencia colonial en Occidente habría de discurrir siempre por caminos mucho más pacíficos?”*¹⁰

Por lo tanto, y de acuerdo con la información expuesta, parece que Tiro, ya a finales del segundo milenio, habría protagonizado en el Mediterráneo Oriental un proceso de expansión territorial basado en la conquista y el control militar de distintos territorios del sur del Levante y Chipre. De ahí que en su posterior expansión occidental no hiciera sino exportar unos métodos que ya habría puesto en práctica con anterioridad en el Mediterráneo Oriental.

De todas maneras, los ejemplos esgrimidos y las interpretaciones expuestas plantean algunos interrogantes, unos interrogantes que cuestionan determinados aspectos acerca de esa expansión territorial violenta atribuida a Tiro. A continuación nos centraremos en el análisis de la conquista fenicia del norte de Israel y las relaciones entre las ciudades fenicias y Chipre a finales del segundo milenio a.C. El otro ejemplo apuntado por Wagner, la fundación de Botris, no será tenido en cuenta por cuanto se basa en una escueta mención realizada por Flavio Josefo¹¹ para la que no conocemos información adicional que permita contrastar la interpretación propuesta.

LA CONQUISTA FENICIA DEL NORTE DE ISRAEL

En torno al 1050 a.C. aparece abundante material fenicio en asentamientos situados en la llanura de Akko y la región del Carmelo, en algún caso precedido de importantes niveles de destrucción: Akziv, Akko, Tell Keisan, Tell Abu Hawwam, Yoqne'am, Tell Mikhal, Tell Qasile, Tell Mevorakh, etc.¹² Con todo, probablemente, el ejemplo que más ha llamado la atención es el de Tell Dor. Allí, la ciudad del Hierro I (1150–1050 a.C.) ocupada por los sikila, uno

⁹ Aubet 2000: 91.

¹⁰ Wagner 2005: 182.

¹¹ “Este rey (Itobaal) fundó la ciudad de Botris en Fenicia y la de Auza en Libia”; *Ant. Iud.* VIII: 324 (Vara Donado 1997: 487).

¹² Stern 1990: 29s.; 1991: 92s.; 1994: 98s.

de los Pueblos del Mar mencionado en las fuentes egipcias y ugaríticas¹³, fue destruida y su cultura material sustituida por otra de origen fenicio. Ephraim Stern interpreta estos datos como la evidencia de una conquista fenicia de la ciudad, en el marco de una pugna entre fenicios y Pueblos del Mar por el control de las rutas comerciales marítimas¹⁴, una explicación que ha sido ampliamente aceptada por los investigadores¹⁵. El problema que se plantea ahora es el de concretar cuál de las ciudades-reino fenicias fue la responsable de la conquista del norte de Israel. Wagner, al igual que otros autores, atribuye a Tiro la conquista y ocupación de la región¹⁶. Las razones que llevan a la elección de Tiro como protagonista de la conquista del norte de Israel han sido bien expuestas por Aubet¹⁷.

Así, en el Estrato XIII de Tiro (1070/1050–1000 a.C.) aparecen una serie de indicios que permiten suponer la superación de una fase durante la cual la ciudad ocupó un papel secundario en el comercio internacional. A partir de este momento se produce la llegada de importaciones procedentes de Chipre, Grecia y Egipto, hecho que coincide con una reestructuración de las estrategias de producción en el interior de la isla, ahora destinadas a la creación de manufacturas para el comercio (joyas, tejidos, cerámicas).

Además, en la segunda mitad del siglo XI a.C. Tiro abre una nueva ruta comercial con Chipre y, probablemente, Creta. Así lo atestiguarían los hallazgos de material cerámico fenicio en la necrópolis de Paleopafos, en Amathus y en Salamis, un material cerámico idéntico al documentado en el Estrato XIII de Tiro.

La coincidencia cronológica entre el surgimiento de Tiro como potencia comercial internacional y la expansión fenicia hacia el norte de Israel es la que ha llevado a atribuir a Tiro la autoría de esa conquista, una conquista motivada por la voluntad de controlar los recursos agrícolas e industriales de la región por parte de la potencia emergente.

De todas maneras, esta reconstrucción, basada enteramente en la documentación arqueológica, debe contrastarse con la información sobre la costa fenicia en los siglos XII–XI a.C. que proporcionan las fuentes escritas, una información que permite inferir otras posibilidades además de la planteada. Al mismo tiempo cabe recordar que las recientes excavaciones en Sidón han

¹³ Onomasticon de Amenope, Relato de Wenamón, inscripciones de Rameses III en Medinet Habu. El texto ugarítico en el que aparecen mencionados los sikila es RS 34.129 (= RSOu 7 12) (véase Dietrich y Loretz 1978; Lehmann 1979).

¹⁴ Stern 1990: 30; 1991: 92; 1993: 359; 1994: 99ss.; 2000: 203.

¹⁵ Véase, por ejemplo, Mazar 1994: 41ss.; Aubet 2000: 81; Markoe 2000: 30s.

¹⁶ Wagner 2005: 181s.; 2007: 122s.

¹⁷ Aubet 2000: 80ss.

sacado a la luz algunos tipos cerámicos idénticos a los hallados en el Estrato XIII de Tiro¹⁸, lo que tal vez obligue a cuestionar alguno de los planteamientos señalados.

Por lo que se refiere a las fuentes escritas, tanto la Biblia Hebrea como las fuentes asirias y egipcias coinciden en indicar que durante los siglos XII–XI a.C. Sidón fue la ciudad más importante en la mitad sur de la costa fenicia. Esta hegemonía sidonia ha llevado a algunos autores a plantear incluso la existencia de una posible hegemonía política de Sidón sobre Tiro¹⁹.

La hegemonía de Sidón en la costa fenicia meridional aparece reflejada ya en una inscripción del rey asirio Tiglat-Pileser I (ca. 1114–1076 a.C.). La inscripción, datada en torno al 1110 a.C., hace referencia al pago de tributos al rey asirio por parte de las principales ciudades fenicias del momento:

*“Yo (Tiglat Pileser I) recibí tributo de las tierras de Biblos, Sidón (y) Arwad.”*²⁰

La significativa omisión de Tiro en la anterior relación en ocasiones se ha interpretado como una evidencia de que en esos momentos la ciudad formaba parte del reino de Sidón, de ahí que no fuera incluida en la relación de ciudades fenicias que debían pagar tributo al rey asirio²¹.

Por el contrario, otros autores han entendido la omisión de Tiro de una forma distinta, sin concederle ningún significado político²². De acuerdo con esta otra interpretación, la no mención de la ciudad se debería únicamente a una cuestión geográfica. Como consecuencia de su ubicación meridional, Tiro habría quedado fuera del radio de acción de Tiglat-Pileser I.

Desde luego, la escasez de documentación relativa a este período en principio permite defender ambas interpretaciones. En realidad es imposible saber si la influencia asiria durante el reinado de Tiglat-Pileser I sólo alcanzaba hasta Sidón o llegaba unos kilómetros más al sur. Sin embargo, si atendemos no únicamente a la literalidad del texto sino también a determinadas cuestiones ideológicas implícitas en el mismo, entonces queda claro el rol secundario de Tiro a finales del siglo XII a.C. Tal y como apuntaba Tadmor²³, la obligación de todo monarca asirio era la de extender real o idealmente las fronteras de su territorio hasta los límites del mundo, unos límites que según las concepciones

¹⁸ Doumet-Serhal 2002: 197.

¹⁹ Albright 1965: 457; Moscati 1968: 9s.; Markoe 2000: 31; Belmonte 2003: 88s.; 2007: 26.

²⁰ RIMA II, texto A.0.87.3, p. 37, ll. 19-20.

²¹ Belmonte 2003: 88.

²² Bunnens 1979: 297; 1995: 224; Katzenstein 1997: 63; Blázquez 1999: 55.

²³ Tadmor 1999.

geográficas mesopotámicas, y por lo que a Occidente se refiere, los señalaba el Mar Superior, esto es, el Mediterráneo. Es por ello que ya Šamši-Addu I (ca. 1808–1776 a.C.) inauguró la costumbre de dirigirse hacia el Mar Superior, subir el monte Amanus, lavar sus armas en el mar y cortar cedros del Líbano, unas actividades rituales que ejemplificaban el dominio universal y que a partir de esos momentos se convirtieron prácticamente en una obligación, en especial para los reyes asirios del primer milenio a.C. Esta presencia asiria en la costa Mediterránea no significaba un dominio efectivo asirio de la región. Únicamente respondía a la necesidad de visualizar mediante esas acciones rituales la ideología asiria del dominio universal.

Teniendo esto en cuenta, entonces la mención de Biblos, Sidón y Arwad en la inscripción de Tiglat-Pileser I, más allá de la entrega de tributos, cumpliría una función ideológica. El rey se dirigía a esas ciudades porque a finales del siglo XII a.C. eran los enclaves más importantes en las fronteras occidentales del mundo, aquéllos en los que la presencia del monarca era útil pues simbolizaba el dominio asirio sobre el Mar Superior. En este contexto, la omisión de Tiro resulta muy significativa, por cuanto denota que, como mínimo desde la perspectiva asiria, la ciudad no era lo suficientemente relevante como para ser considerada uno de los límites occidentales del mundo y así requerir la presencia de Tiglat-Pileser I.

El papel protagonista de Sidón como la ciudad más importante en el sur de la costa fenicia a finales de la Edad del Bronce e inicios de la Edad del Hierro aparece reflejado también tanto en los poemas homéricos como en la Biblia, donde el gentilicio “sidonio” llega a convertirse en sinónimo de “fenicio”. Dentro de la documentación bíblica resulta especialmente relevante un pasaje del libro de Josué, en el que se divide la región fenicia occidental en dos grandes áreas bajo el control político de Sidón y Biblos respectivamente:

*“Todo el territorio de los cananeos, desde Ará de los sidonios hasta Afec y hasta la frontera de los amorreos (donde llega) la tierra de Biblos, y todo el Líbano oriental: desde Baal-Gad, al pie del Hermón, hasta Lebó de Jemat.”*²⁴

Como es bien sabido, el libro de Josué resulta una fuente de información sobre la transición entre el Bronce Final y la Edad del Hierro cuya fiabilidad a menudo ha sido puesta en duda. Desde luego, no es éste el momento de recoger exhaustivamente la ingente cantidad de comentarios que se han escrito sobre la cuestión. A continuación simplemente se señalarán un par de casos a

²⁴ Josué 13: 4-5.

modo de ejemplo. Así, para Noth el libro de Josué era un conjunto de leyendas etiológicas, de cuentos de héroes cuya composición definitiva probablemente se extendió a lo largo de varios siglos, muy lejos por tanto de los supuestos acontecimientos a los que hacía referencia²⁵. Más recientemente Na'aman define la obra como una ficción literaria compuesta a partir de distintos modelos narrativos presentados bajo la apariencia de auténticos relatos históricos²⁶. Sin embargo, incluso los autores más críticos con este texto reconocen que algunos pasajes del mismo pueden basarse en auténticos acontecimientos y realidades históricas²⁷. Si tenemos en cuenta que la información recogida en Josué 13: 4-5 es en esencia coincidente con la de la inscripción de Tiglat-Pileser I, entonces como mínimo cabe considerar la posibilidad de que la situación política de la costa fenicia recogida en ese pasaje bíblico pueda resultar fiable.

Finalmente, existe otro testimonio que aporta información acerca del Levante y que resulta especialmente relevante para la cuestión que estamos tratando. Dicho testimonio es el *Relato de Wenamón*, un texto egipcio conservado en una copia datada a principios del siglo X, aunque basada en un original fechado en torno al 1075 a.C.²⁸ El texto hace referencia al viaje de Wenamón, un miembro del clero de Amón en Karnak, hasta el Líbano, en busca de madera de cedro para la construcción de la barca de Amón. Tal y como ha recogido Schipper, desde el momento en el que fue publicado el documento, a finales del siglo XIX, los investigadores no se han puesto de acuerdo a la hora de determinar si se trata de una composición literaria o bien de un informe oficial del viaje²⁹. En cualquier caso, sí existe una amplia coincidencia en señalar que se trata de un relato que refleja con fidelidad las circunstancias históricas de Egipto y la costa fenicia en la primera mitad del siglo XI a.C.

A lo largo del periplo de Wenamón por el Levante se recogen una serie de menciones muy significativas de las principales ciudades fenicias. Por desgracia, Tiro aparece mencionada en dos ocasiones en un pasaje del texto mal conservado. Con todo, a menudo se ha coincidido en señalar a Tiro como una escala irrelevante dentro del viaje de Wenamón³⁰. Por el contrario, de Sidón se destaca su importancia comercial, con un puerto en el que se contabiliza la presencia de hasta 50 naves comerciales, 30 más que en Biblos:

²⁵ Noth 1935; 1937.

²⁶ Na'aman 1994.

²⁷ Finkelstein y Silberman 2001: 91ss.

²⁸ Galán 1998: 181.

²⁹ Schipper 2005: 7ss.

³⁰ Aubet 1994: 36; Belmonte 2003: 105.

“No hay veinte barcos aquí, en mi puerto, que hacen negocios con Semendes? En cuanto a Sidón, el otro (puerto) por el que tú pasaste, ¿no había otros cincuenta barcos allí que hacen negocios con Uarqatar y transportan hasta su casa?”³¹

El *Relato de Wenamón*, por lo tanto, a diferencia de lo que sucede con Tiro, señala a Sidón como el puerto comercial fenicio más importante. El texto, además, cuenta con el valor añadido de su datación, ca. 1075 a.C., por lo tanto sólo unos años antes de que se produjera la expansión fenicia hacia el norte de Israel.

Las fuentes escritas, en definitiva, muestran de forma unánime como, tras la crisis del 1200 y hasta como mínimo la primera mitad del siglo XI a.C. Sidón fue la principal ciudad de la costa meridional fenicia. De ahí que resulte posible que fuera Sidón y no Tiro la ciudad que hubiera protagonizado la conquista del norte de Israel a mediados del siglo XI a.C. Además, esta posibilidad, y a diferencia de lo que sucede con Tiro, encaja con la bien atestiguada tradición sidonia de emprender la conquista de territorios meridionales. En este sentido la correspondencia de El Amarna ya nos habla de una expansión militar sidonia ocurrida en el siglo XIV a.C. En varias de las cartas que Abi-Milki, rey de Tiro, envió a Amenofis IV se hace referencia a la conquista sidonia de la ciudad tiria de Ušu (Paleotiro)³², ciudad continental situada frente a la misma isla de Tiro³³. Mucho más tarde, ya en el siglo VII a.C., una lista de 16 ciudades sidonias capturadas por el rey asirio Asarhaddon³⁴ confirma esa tendencia sidonia hacia la dominación de la costa meridional fenicia. La propuesta de localización de los topónimos allí mentados realizada por Lipiński indica como, en aquellos momentos, Sidón controlaba prácticamente toda la mitad sur del territorio fenicio, hasta más allá del Nahar al-Qāsimīye³⁵.

Desde luego, las consideraciones anteriores no bastan para asegurar de forma categórica la autoría de Sidón en la conquista del norte de Israel llevada a cabo a mediados del siglo XI a.C. Sin embargo, sí arrojan algunas dudas acerca de la autoría de Tiro, ya que dichas fuentes coinciden en señalar la hegemonía sidonia en la región como mínimo hasta mediados del siglo XI a.C., así como la tradicional voluntad de la ciudad de controlar los territorios más meridionales incluso a través de la conquista militar.

³¹ Galán 1998: 203.

³² EA 148, 149, 150.

³³ Vidal 2006.

³⁴ Borger 1956: 48.

³⁵ Lipiński 2004: 17ss.; véase también Sader 2000: 238.

CONTACTOS ENTRE CHIPRE Y LAS CIUDADES FENICIAS EN EL SIGLO XI A.C.

Como veíamos antes, el posible uso de la violencia por parte de Tiro en Kition se basa en una información recogida por Flavio Josefo. No cabe duda de que la conocida escasez de fuentes primarias sobre la historia fenicia, en especial para principios de la Edad del Hierro, obliga a tener en cuenta otra serie de fuentes, como los autores clásicos, que en condiciones normales podríamos considerar como secundarias. Así, no hay que olvidar que Flavio Josefo escribió sus obras a finales del siglo I y principios del siglo II d.C., por lo tanto, unos mil años después de algunos de los hechos que relata, como la expedición de castigo de Hiram contra los habitantes de Kition. Esta enorme distancia cronológica a menudo se ha intentado minimizar apuntando que el historiador judío contaba con fuentes de información privilegiadas, como la *Historia de los fenicios* de Menandro de Éfeso, una obra que habría sido elaborada a partir de documentación fenicia original consultada por el autor en los archivos reales de Tiro. Sin embargo, tal y como ha apuntado Ribichini, ello no impide que deban extremarse las precauciones al usar ese material³⁶. Aunque Flavio Josefo utilizara la obra de Menandro, la información que nos transmite no deja de ser una información, como mínimo, de tercera mano. Además, el recurso a la mención de fuentes antiguas, como los archivos reales de Tiro, es un tópico literario ampliamente extendido y de uso obligado para tratar de legitimar la veracidad de aquello que se está contando. Así se aprecia, por ejemplo, en *Ant. Iud.* VIII 55, donde Flavio Josefo, basándose en 1 Re. 5, hace referencia a la alianza entre Salomón y Hiram de Tiro. Para demostrar la veracidad de sus palabras invita a sus lectores a consultar las copias de las cartas originales intercambiadas por ambos monarcas, unas copias supuestamente conservadas en los archivos de Tiro. Flavio Josefo era perfectamente consciente de que ninguno de sus lectores romanos, el público para el que escribía, iba a dirigirse hasta Tiro para comprobar la existencia de esos documentos, por lo que la referencia a esas supuestas cartas únicamente cabe entenderla como un recurso literario para tratar de apoyar la validez de sus informaciones³⁷. Además, en este caso el testimonio de Flavio Josefo resulta especialmente sospechoso, pues son muchos los investigadores que consideran las supuestas relaciones comerciales entre Hiram y Salomón como un episodio novelesco que en realidad jamás habría tenido lugar³⁸. De confirmarse estos planteamientos entonces sería del todo imposible que

³⁶ Ribichini 1995: 77s.

³⁷ Garbini 1980: 79.

³⁸ Garbini 2002: 53s.; Liverani 2003: 113; Lemche 2005.

existiera algún documento en Tiro relacionado con la alianza entre Salomón y Hiram, lo que pondría en evidencia a Flavio Josefo³⁹.

Estas consideraciones obviamente no impiden el uso de la información aportada por Flavio Josefo. Simplemente señalan que la definición de una política basada en el uso sistemático de la violencia en Chipre por parte de Tiro debería apoyarse en más datos.

En relación con Chipre, de nuevo el *Relato de Wenamón* aporta información relevante sobre la cuestión. Allí un pasaje muestra la existencia de un evidente rechazo por parte de los habitantes de la isla a la presencia fenicia en su territorio entorno al 1075 a.C., cien años antes del episodio mencionado por Flavio Josefo. Así, Wenamón tras largas negociaciones finalmente logró la madera necesaria para la construcción de la barca de Amón. Sin embargo, durante el viaje de regreso a Egipto a bordo de un barco de Biblos, se vio sorprendido por una tormenta. En contra de lo previsto, los vientos llevaron la nave hasta las costas de Chipre. Lamentablemente, y a diferencia de lo que sucedía con la costa del Levante el texto aporta una información muy genérica sobre la isla⁴⁰. Así, entre otros casos, no queda claro si el topónimo Alašiya hace referencia al conjunto de Chipre o únicamente a alguna región situada en el sector oriental de la isla, ni tampoco si la gobernadora que se menciona dominaba un gran reino o más bien una pequeña entidad territorial, en consonancia con la fragmentación política que, desde la arqueología, se ha definido como característica de este periodo⁴¹. En cualquier caso, sí resulta muy significativa la reacción hostil de los habitantes de Alašiya ante la llegada de un barco de Biblos:

*“Los vientos me empujaron hacia la tierra de Alašiya. Los del poblado salieron para matarme, (pero) me abrí paso entre ellos hacia donde estaba Hatiba, la gobernadora (...) “¡Mira! a mí me pueden buscar cualquier día, pero en cuanto a la tripulación del gobernador de Biblos a quienes buscan para matar, ¿no encontrará su señor diez tripulaciones a tu cargo y les matará del mismo modo?”*⁴²

Dicho pasaje no deja dudas acerca de la existencia de unas relaciones claramente conflictivas entre Chipre (o una parte de Chipre) y las ciudades fenicias, en este caso Biblos⁴³. En esta ocasión la arqueología permite

³⁹ Para otros episodios similares véase Garbini 2002: 45ss.

⁴⁰ Véase Scheepers 1991: 80ss.; Schipper 2005: 217ss.

⁴¹ Karageorghis 2004: 113ss.

⁴² Galán 1998: 208.

⁴³ Bikai 1994: 34.

contextualizar mejor el testimonio que ofrece el *Relato de Wenamón*. Así, las excavaciones muestran como en el período 1100–1050 a.C., momento al que hace referencia el texto egipcio, Chipre experimentó una fase de relativo aislamiento sin que se hayan recuperado evidencias que testimonien la existencia de intercambios comerciales con el Egeo ni con el Levante⁴⁴. Por tanto, la hostilidad mostrada por los habitantes de Alashiya hacia Wenamón se enmarca dentro de un breve paréntesis en la participación de Chipre en el comercio internacional.

CONSIDERACIONES FINALES

El repaso realizado hasta aquí permite matizar algunas cuestiones apuntadas acerca del uso de la violencia por parte de Tiro en el marco de su expansión colonial en el Mediterráneo Oriental.

Así, la arqueología, en efecto, atestigua una expansión fenicia violenta hacia el norte de Israel, aunque por el momento no puede precisarse con seguridad si la misma debe ser atribuida a Tiro o a Sidón. Por su parte, el *Relato de Wenamón* describe la existencia de relaciones conflictivas entre Chipre y la costa fenicia durante la primera mitad del siglo XI a.C., unas relaciones que, tal y como demuestra la arqueología, además de conflictivas fueron escasas. El episodio narrado en el texto egipcio en principio podría esgrimirse como un precedente de la supuesta acción punitiva llevada a cabo por Hiram de Tiro un siglo más tarde. Sin embargo, el *Relato de Wenamón* no sitúa a Tiro sino a Biblos como protagonista del conflicto con Alashiya. Por lo tanto, al margen de las noticias extraordinariamente tardías proporcionadas por Flavio Josefo, no existe ni una sola fuente documental que atestigüe con certeza una supuesta violencia colonial tiria en el Mediterráneo Oriental.

De todas maneras, aún si concedemos credibilidad a Flavio Josefo y su noticia sobre la expedición de castigo contra Kition, o si aceptamos que Tiro fue la ciudad que llevó a cabo la conquista del norte de Israel, esa supuesta violencia tiria no debería utilizarse como argumento para apoyar la existencia de una posterior colonización violenta en el Mediterráneo Occidental. Utilizarla supondría aceptar que la actividad colonial fenicia se desarrolló de manera perfectamente homogénea a lo largo de todo el Mediterráneo, sin verse en absoluto afectada por unas realidades autóctonas heterogéneas, con las que probablemente hubieron de establecerse formas de interacción distintas en muchos casos.

⁴⁴ Mazar 1991: 102s.; Karageorghis 2004: 138ss.

En realidad, y a tenor de lo expuesto hasta aquí, parece más oportuno continuar con el debate acerca del papel de la violencia en la colonización fenicia de la Península Ibérica a partir únicamente de la información obtenida en ese mismo ámbito geográfico. Más allá de la posible existencia de evidencias sobre un “precedente oriental”, el contexto y las circunstancias históricas existentes en el Mediterráneo Oriental no eran en absoluto equiparables a las del Mediterráneo Occidental, por lo que la interacción entre colonizadores y poblaciones autóctonas no tuvo por qué reproducirse de forma idéntica o similar en ambas orillas.

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THE LEATHERWORK FROM DEIR EL-BACHIT: PRELIMINARY REPORT

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INTRODUCTION

The five years of excavation of the Coptic Monastery Deir el-Bachit,¹ produced a large quantity of leatherwork. During the 2008 season of the Deir el-Bachit expedition, all leather finds have been registered, studied and photographed. Focus of the work was on the diagnostic pieces, concentrating on the manufacturing technology, the type of leather and skin processing. Obviously, the analysis will take some time to come, but it is nevertheless possible to present a preliminary report within these limits. The final publication will include a fully illustrated catalogue of all registered finds.

LEATHER RESEARCH IN EGYPT

In Egyptian archaeology, interest in leatherwork is increasing, having been neglected for a long time. However, our knowledge is still limited, even for the Roman and Christian leatherwork, which are among the best studied in Egypt.² This, together with the nature of the site (a relatively closed community of limited life span, settlement versus monastery) and being the first monastery excavated systematically, emphasizes the importance of the study of Deir el-Bachit's leather. Moreover, due to the author's work at other sites,³ which

¹ Burkard and Mackensen 2003; Burkard *et al.* 2003; Eichner 2005. See: http://www.dainst.org/index_55_en.html.

² For an overview on studies, see Van Driel-Murray 2000 and, more recently, Veldmeijer 2008: esp. 7.

³ The publications of which are forthcoming.

includes material from about the same age (Amarna, Kom el Nana⁴) as well as from shortly before (Elephantine) and after (Qasr Ibrim) the occupation of the monastery, a good comparison is possible.

THE FINDS

Two categories of objects are most abundant and constitute the majority of the finds: fragments originating from book covers and footwear.

Book covers

The book covers are mostly decorated, although undecorated, comparable leather and therefore possibly originating from book covers, has been identified as well. The decoration consists of impressed motifs, the majority of which are lines within which are applied other motifs (mainly circles, but in some cases also crosses). Fortunately, good examples of book covers exist from elsewhere, which allows a good comparison in decoration as well as manufacturing technology.

Footwear

Only two more or less complete sandals have been identified; all other entries in this category are fragments, of which most show an extreme high degree of repair. This prohibits the identification of the original type of sandal. Moreover, there are numerous decorative elements, which originate from sandal's strap complexes. The footwear, besides being analysed within the excavations theoretical framework, will be analysed within the framework of the Ancient Egyptian Footwear Project (AEFP). The AEFP is a multidisciplinary research into ancient Egyptian footwear from Predynastic to Coptic times. In contrast to published footwear typologies such as Montebault's,⁵ the typology developed by the AEFP,⁶ includes among others date and distribution as diagnostic characters. Moreover, the AEFP is based on a larger number of objects, resulting in expansion and refinement of the typology. Finally, as explained elsewhere⁷ recognisability is used, based on the work of Goubitz

⁴ Veldmeijer (in press 2009a).

⁵ Montebault 2000.

⁶ Based partially on Montebault's and Goubitz *et al.*'s work.

⁷ Veldmeijer (in press 2009b).

*et al.*⁸ Leguilloux⁹ has established a typology on the basis of the objects from Didymoi but because this is based on the finds from only one site and of limited time period, it is not used here. A concordance, however, is provided for. Note that Leguilloux's reconstructions (and hence typology?) are partially based on assumptions of the layout of the strap complex: no sandals with intact straps have been depicted and hence we can assume they were not preserved.¹⁰ Neither of both typologies uses the shape of the sole as character, although Leguilloux¹¹ recognises the distribution of shapes in time.

Since a typology using also date and distribution can only be established on the basis of a large sample of varying date and from various sites, Montembault's typology will be used here for the time being.

The majority of the recognisable sandals fall in Montembault's category 'Class I, Type B, Variant 2' (Leguilloux 2b). Sandals I, B, 2 are characterised by the strap complex, and in particular the construction of the back strap. A pre-strap, rectangular, is pulled through two slits (one at each side of the sandal) in the insole. The ends of the pre-strap has (a) slit(s), usually two or three, to which the back/heel straps are hitched. The front strap is pulled through two slits in the insole, which are positioned transversely one behind the other, and is thus sandwiched between the insole and treadsole; it does not show at the ventral surface of the treadsole.

DB 0879 (figure 1) is the most complete example and consists of an insole and treadsole, the latter of which protrudes from under the insole at all sides. The sandal has a rounded heel and distinctly constricted waist. From here, the width increases rapidly; the lateral edge curves rather abruptly towards the big toe whereas the curvature of the medial edge is more gentle. Consequently, the sole is swayed and meant for the right foot. The soles layers are sewn along the perimeter with leather thong running stitches, which are widely but evenly spaced. Three additional longitudinal rows of leather thong running stitches (showing short at both sides of the soles) are placed inside the perimeter stitching. The dorsal surface of the insole shows impressed decoration of geometrical motifs. A repair is visible between the two slits for the front strap. An incomplete sandal, DB 3389, is of comparable construction.

⁸ Goubitz *et al.* 2001.

⁹ Leguilloux 2006.

¹⁰ More on the lack of straps: Van Driel-Murray 2002: 3-4.

¹¹ Leguilloux 2006: 98-101.

DB 1236 (figure 2) is the back three quarters of a small sandal. The heel is rounded and the waist is slightly constricted. Towards the front the width increases slightly but seemingly at both sides; unfortunately, the incomplete condition prohibits the exact course. Clear is, however, that the shape differs distinctly from DB 0879. Originally there was at least one additional sole layer, but whether it was an entire sole or only at the heel is uncertain. It would have sandwiched the pre-strap running underneath the preserved sole. This pre-strap has rounded ends and has three longitudinal slits for the attachment of the back/heel strap, which are now lost. The dorsal surface of the sole is without decoration. Usually, the pre-straps are relatively narrow. In DB 2031, however, which is a heel part and comparable in shape to DB 1236, the pre-strap is relatively wide. The dorsal surface of this insole (stitches along the perimeter suggests there was at least one other sole layer) is decorated with impressed and painted(?) geometrical motifs.

Yet another shape within this group of sandals is DB 1261 (figure 3). The heel is rounded, and the waist is not constricted: towards the front, the width increases distinctly and continuously. The front itself is square. The slits for the pre-strap are situated about halfway the length of the sandal, which is different from the usual construction, in which the slits are situated at the heel part. The stitches along the perimeter as well as the one row longitudinally down the centre suggests at least one other sole layer. The strange shape of the sandal, together with the two cuts in the front edge and the forward position of the slits for the pre-straps suggests that the sandal is a re-use and possibly cut from a larger sandal.

The identification of coiled decorative elements of straps as well as isolated (fragments of) straps, suggests at least two other 'classes' of sandals. One substantial piece of a shoe's upper (DB 3708) has been registered. Unfortunately, the fragment is folded several times due to which the study is severely hindered; future work will focus on the identification of this find.

SKIN IDENTIFICATION/PROCESSING, MANUFACTURING OF OBJECTS

Skin can be recognised by its distinctive structure¹² although this is seriously hindered by the fact that the fibrous skin structure differs within species. Moreover, there is a variation in structure within the skin. In archaeological finds, the identification is further hindered by the wear due to use as well as the preservational conditions. Identification by means of chemical analysis such

¹² For example Haines 2006.

as the fat content has been done with the far older material from Amarna, and proved only partially successful.¹³ Despite these limitations, identification, especially on a higher level of natural science's systematics, can be deduced due to different properties of skins.

In general, ordinary leather footwear is made of cow's leather¹⁴ and the thickness of the leather leaves without a doubt that this is the case here as well. Less ordinary footwear was also made of goat or even gazelle, but these materials do not seem to have been used in footwear after the Pharaonic period. Other objects, as far as the analyses have been conducted, are made of cow's leather too.

NON-DIAGNOSTIC FINDS

The non-diagnostic material has been inventoried, registering the context, quantity and kind of non-diagnostics (mainly offcut's and waste, and far less numerous, 'other,' such as unidentifiable fragments). The large numbers of offcut's and waste clearly suggests that at least the manufacturing of leather objects was (partially) done in the monastery. However, this does not necessarily mean that the skin processing (i.e. slaughtering of the animal, depilating the skin, curing/tanning, etc.) was done at the monastery too.

DISCUSSION

As shown, a refinement of the typology on the basis of the shape of the sole, the number of soles layers and their attachment to each other, the presence/absence of decoration and the construction of the pre-strap is needed (a suggestion which is strengthened by material from other sites such as Qasr Ibrim), besides the above-mentioned addition of date and distribution as character. The Deir el-Bachit finds is important for the overall typology, due to the fact that the finds originate from a stratified context and is limited in time and space. It fills a gap between the earlier finds from Elephantine and has overlap with finds from Qasr Ibrim. We are fortunate that there are many finds from Christian layers in Egypt; although most have not been published, the author has studied the footwear in several collections, which enable us to compare it with the Deir el-Bachit material.

¹³ Trommer 2005: 141-144.

¹⁴ See also van Driel-Murray 2000: 302.

The study of wear teaches us about the use of a leather object, but also on the relative wealth of a society, for which the degree of repair is one indication, the presence/absence of more expensive shoes is another. The high degree of repair and the absence of shoes both points to a relatively poor population.

ACKNOWLEDGEMENTS

I am grateful to Ina Eichner and Günter Burkard for allowing me to work with the material; their help, support and interest is greatly appreciated. I thank Daniel Polz and Ute Rummel and Erno Endenburg is acknowledged for photography and assistance with the fieldwork.

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Figure 1. Sandal DB 0879.

Photography by E. Endenburg. Courtesy of Projekt Deir el-Bachit, Institut für Ägyptologie an der Ludwig-Maximilians-Universität München. Scale bar is 5 cm.



Figure 2. Sandal DB 1236.

Photography by E. Endenburg. Courtesy of Projekt Deir el-Bachit, Institut für Ägyptologie an der Ludwig-Maximilians-Universität München. Scale bar is 5 cm.



Figure 3. Sandal DB 1261.

Photography by E. Enderburg. Courtesy of Projekt Deir el-Bachit, Institut für Ägyptologie an der Ludwig-Maximilians-Universität München. Scale bar is 5 cm.

RESEÑAS BIBLIOGRÁFICAS/BOOK REVIEWS

AMANDA–ALICE MARAVELIA, *Les Astres dans les Textes Religieux en Égypte Antique et dans les Hymnes Orphiques*. BAR International Series 1527. Oxford, Archaeopress, 2006, pp. xiv + 638; 72 figures and tables. Indices, including Egyptian terms. In French, with summaries in English, Greek and German. ISBN 1841719641. £70.00.

Amanda–Alice Maravelia's new volume in the BAR series is an ambitious and impressive archaeoastronomical comparison of celestial ideas in Egyptian religious thought with the Orphic Hymns from ancient Greece. Maravelia is uniquely poised to make this contribution to Egyptology: holding two doctorates, one in Egyptology and one in astronomy, the author has combined her interests into a readable and edifying look at the evolution of Egyptian astronomical thought and its "unique and fertile" synergy with archaeology. This work is the publication of her Ph.D. thesis (in Egyptology) for the University of Limoges. One of the subfields of post-processual archaeology, exemplified by Maravelia, is cognitive archaeology: not only to understand the material culture of the ancients, but also to understand what they perceived as their taxonomy of the world around them. In essence, how did they think? Astronomy is one of the best starting points in this sense. How did the ancients view the stars, what significance did they assign them based on what they saw, and how did this impact their lives?

The work is divided into six chapters, not including the preface (written by Dr. Galina Bolova), the introduction, and the epilogue: an introduction to Egyptology (chapter 1); an introduction to archaeoastronomy (chapter 2); a review of the conception of stars in Egyptian religious texts (chapter 3); a review of the conception of stars in the Hellenic Orphic Hymns (chapter 4); a philosophical comparison with modern cosmological and astronomical ideas (chapter 5); and a conclusion (chapter 6). In addition to the tables embedded within the text, there are 23 pages of tables at the end (including diagrams of the major constellations, as discussed in the texts), followed by figure and photo credits, eight indices, a list of abbreviations, and an extensive bibliography (divided between two sections: Egyptological and archaeological; and astronomical, archaeoastronomical, scientific, Hellenic, and general). Finally, the end of the volume contains abstracts of the work in French, English, Greek, and German. A minor fault may be found in the layout, especially regarding the placement of the tables. For instance, Table III.1 begins in the middle of a paragraph, leaving the reader to flip thirty pages ahead to find the continuation of the sentence. Table III.2 is placed mid-word, even.

The book is lavishly illustrated, including color illustrations on the cover and the frontispiece. High-quality grayscale images are found throughout. The charts complement the photos, making the available data easy to read and to reference. The charts could possibly be simplified, however; often the charts are many pages long

and can veer toward unwieldy.

Some aspects of the book are non-traditional: for instance, the preface opens with a photo of the author in her office, and the text begins with quotes from Umberto Eco and William Shakespeare. These touches help to give what could otherwise be a daunting topic a sense of the author's personality.

Traveling through the book, chapter one provides an Egyptological background to the study of astronomy, with a literature review covering major past treatments of Egyptian astronomical thought (or *pensée cosmovisionelle*, as she prefers, to allow for a differentiation between ancient, pre-scientific perspectives and modern inquiry). She follows this with an overview of her methodology, which includes pinpointing the precise astronomical elements in ancient Egyptian, performing a statistical analysis of these elements, studying their frequency, tracing the changes over time, and comparing them with the Orphic Hymns and modern ideas.

Chapter two continues this introduction, but shifts toward the mechanics of astronomy. This section is very heavy with mathematical formulae, and some readers may feel bogged down with the technical presentation of the material. Chapter three is an application of these mechanics, covering the Egyptian views of and references to astronomy. Given the crucial role that astronomy played in the Egyptian worldview, especially in their conception of the afterlife, Maravelia is certainly not the first to review the evidence. (This is by her own admission. Maravelia's original idea, to examine Egyptian texts both religious and profane, was quickly abandoned in favor a more concise and finite project focusing primarily on the Pyramid and Coffin Texts. She notes that her original thesis bore the unwieldy title of *L'Évolution de la Pensée Astronomique et Cosmologique dans l'Égypte Pharaonique: Recherches Comparatives d'après/entre les Divers Textes Religieux (et Profanes) Égyptiens, dès 2800 jusqu'à 1000 avant notre Ère, et les Hymnes Orphiques.*)

Maravelia systematically goes through the references to astronomy, describing the different elements (stars, sun, moon, and the planets) as recorded in the Pyramid and Coffin Texts, treated separately. The Book of the Dead she treats briefly in only two pages; the majority of the information gleaned from the BD was already covered in the PT and CT and she hopes to present the new information in a future volume. Maravelia at this point also inquires into the astronomical orientation of the pyramids, offering a critique of several prominent theories (most particularly, she is quite critical of Spence's theories, advanced in the journal *Nature*¹). A comparatively short section toward the end of the chapter covers the astronomical references in profane texts,

¹ Spence, K. "Ancient Egyptian Chronology and the Astronomical Orientation of the Pyramids," *Nature* vol. 408 (2000): 320-324 and Spence, K. "Ancient Chronology: Spence replies," *Nature* vol. 412 (2001): 699-700.

including the Shipwrecked Sailor, Sinuhe, and love songs.

Chapter four marks a shifting of gears from the earlier three chapters, as does chapter five (on modern ideas). While many readers will likely find their questions answered in chapters one through three, these later chapters are essential to the author's goal of providing a comparison and complement to the Egyptian data. Chapter four is an examination of the Orphic Hymns, the Greek poems focusing on the celebration of Orpheus, recorded by at least thirty authors. There are 87 of these so-called hymns, attributed to Orpheus. Dating from at least the 7th-6th century BCE, the hymns detail how the Greeks intertwined their theology with their cosmogony. Organizationally, she follows the same structure in this chapter that she followed in the previous chapter (treating the stars, the sun, the moon, and the planets in succession).

While Maravelia's scholarship is sound, one must question the conceit of her volume: is there merit to be found in a comparison between the Egyptian astronomical texts and the Orphic Hymns, which date from entirely different periods and regions? In other words, were the Orphic Hymns influenced by Egyptian celestial thought? The longest chapter, chapter four, is dedicated to answering this question (although the earlier chapters stand on their own as individual studies). Her comparison starts with the heliocentric aspects of the two cosmogonies, comparing the Egyptian Hymn to Re to the Greek Hymn to Helios. She next examines the idea of the cosmic egg, appearing in both Egyptian and Orphic texts (here, despite her earlier caveat, she relies heavily on the Book of the Dead). Finally, she examines the idea of universal law in ancient Egypt (ma'at) and ancient Greece (Anagkē/Eunomia/Dikē). These comparisons seem more contrived than organically obvious.

Chapter five is a similar examination of modern concepts, focusing in particular on the similarities with the ancient perception and the reconciliation of the two. Finally, chapter six ties the book together with a recapitulation of the material covered throughout the text and suggestions for future research.

The author's fluidity with language may pose a problem for some readers, in particular as she weaves fluently between the French, ancient Egyptian, Coptic, and Greek, with Hebrew (some of which is transliterated into the Roman alphabet, some of which is not – none of the Greek sources are transliterated and few are translated) and English. It makes her intended audience all the more clear that she uses these languages interchangeably but devotes a significant amount of time to an explanation of modern astronomy. Although some modern astronomical concepts are essential to the ancient understanding (for instance, the movement of the stars through the sky), this particular reviewer was left wondering why so many pages were dedicated to the complicated mechanics of astronomy (for instance, the Stefan-Boltzmann constant) when these concepts were beyond the understanding of the ancients and thereby perhaps irrelevant to the scope of the work.

This book certainly represents an incredible effort. Does it succeed in its goals? That depends on which goals the reader is most interested in: As an archaeoastronomical work, it achieves what it sets out to achieve, namely outlining and comparing the Egyptian and Orphic attitudes towards the sky. As an overview of Egyptian astronomy, it synthesizes many other works, but other sources may perhaps be more specific and therefore more germane to Egyptologists. (The author herself is aware of this problem. The abstract states that the “comparative study of the Orphics is complete, while [its] analysis of the Egyptian sources is not exhaustive, because other researchers have already worked on this domain with considerable success.”) The largest problem lies in the interpretation and what can be told from the comparative nature of the work. Why is a comparison of ancient Egypt against ancient Greece necessary or revealing? Does tracing the evolution of astronomical thought suggest that ancient Egyptian thought influenced Greek (and thereby modern) thought, or did the different societies come to similar conclusions independently? Either way, what are the implications for furthering our understanding of each society? The author frequently uses the Latin expression *mutatis mutandis*, which she seems to be using to forgive the inherent differences in the two societies and to allow her to make broad comparisons between groups that are not inherently comparable.

What the book does best, in this reader’s opinion, is to whet one’s appetite for the possibilities of archaeoastronomy. This is one of the many areas in which archaeology is burgeoning, and the possibilities of how it can augment our understanding of the ancients is tantalizing. Egyptology as a field is often slow to adopt the theoretical approaches that are yielding amazing results in other areas of the world. Works such as Maravelia’s are important steps forward in how we interpret and analyze the ancient world in order to better understand it.

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LESTER L. GRABBE, *Ancient Israel: What Do We Know and How Do We Know It?*, Londres, T & T Clark, 2007, xx + 306 pp. ISBN 978-0-567-03254-6. U\$S 32,95.-

Esta nueva contribución, escrita de manera clara y didáctica, aunque destinada no tanto al público no especializado como al académico, presenta un necesario estado de la cuestión de la historiografía contemporánea sobre el “antiguo Israel”. Si tomamos en cuenta el arco de opiniones que conforma la historiografía actual sobre Israel y establecemos en un extremo una posición “maximalista”, que adopta la narrativa bíblica como histórica salvo que se demuestre lo contrario, y una posición “minimalista”, que considera la narrativa bíblica como no histórica hasta que se presenten evidencias de lo contrario, sin dudas Grabbe se inclina hacia esta última opción. Aun así, en una considerable cantidad de puntos el autor prefiere un camino intermedio, lo cual

nos permite catalogar esta obra –teniendo en cuenta los extremos nombrados– como “crítica-moderada”.

El libro se estructura en tres grandes secciones. En la Primera Parte (pp. 1-36) se presentan definiciones de tópicos clave en la historiografía contemporánea de Israel, como el concepto de *longue durée* de F. Braudel, la Cronología Baja de I. Finkelstein y el debate entre “maximalismo” y “minimalismo” bíblico. La Segunda Parte (pp. 37-215), que constituye la mayor parte de la obra, discute problemas históricos de la región del Levante mediterráneo durante el segundo y el primer milenio a.C., utilizando todo tipo de fuentes a disposición: epigráficas, textuales, arqueológicas, etc. La Tercera Parte (pp. 217-225) presenta las conclusiones generales.

En verdad, como el propio autor indica (p. xix), esta obra constituye un preludio historiográfico (*a prolegomena*) a una historia de Israel en la antigüedad; no es propiamente una “historia de Israel” sino una presentación de los principales temas y posturas historiográficas de la actualidad. Con respecto a los períodos premonárquicos, usualmente basados en la narrativa bíblica, Grabbe nota el consenso académico que desestima las épocas de los patriarcas, del éxodo de Egipto o de la conquista militar de Canaán como parte de la historia de Israel. Al tratar el período atribuido a la Monarquía Unida de David y Salomón, el autor avanza una posición muy cercana a la sostenida por otros investigadores (I. Finkelstein, M. Liverani): no existen evidencias concretas de un imperio israelita durante el siglo X a.C. (mucho menos a la luz de la nueva datación cronológica propuesta por Finkelstein de los estratos “salomónicos” en Meguido, Guézer y Hazor) tal como lo evoca la Biblia; aun así, Grabbe no desestima la posibilidad de que Saúl, David y Salomón hayan sido personajes históricos, algo que en verdad no es posible comprobar históricamente. Así pues, y exceptuando la referencia a una entidad étnica llamada “Israel” (*ysr3r*) en la Estela de Merneptah (*ca.* 1207 a.C.), en el Levante meridional existieron dos reinos identificados colectivamente bajo ese nombre: el reino septentrional –“Israel” (*ca.* 900-720 a.C.)– y el meridional –“Judá” (720-539 a.C.)–, relacionados, en primer lugar, sociopolíticamente, y en segundo lugar, cultural y religiosamente, según el autor (merece notarse aquí, sin embargo, que el libro no responde a una pregunta esencial, formulada en otra parte por P.R. Davies: ¿cuándo y por qué el reino de Judá se identifica con el reino de Israel?). Tal vez la mayor diferencia entre ambos reinos –se sostiene en el libro– sea que no está todavía muy claro qué tipo de organización política poseía Israel (¿Estado tribal, territorial, segmentario? ¿Jefatura compleja?), al no haberse hallado evidencia arqueológica sustancial de algún tipo de organización burocrática; por otra parte, el reino de Judá sí cuenta con un repertorio epigráfico importante como para poder hablar de un cierto tipo de burocracia (por supuesto, nunca comparable a la de Egipto o Mesopotamia) durante la segunda mitad del siglo VIII y todo el siglo VII a.C., y caracterizando a Judá como “Estado territorial”. Pero, tal vez la diferencia entre Israel y Judá trascienda los aspectos de organización sociopolítica y sea más profunda de los que pensamos, puesto que la idea de una homogeneidad cultural entre ambos reinos proviene de las narrativas bíblicas, y no tanto del registro arqueológico o del aporte

del registro etnográfico de Medio Oriente, a partir del cual queda claro que nociones de “nacionalidad” de tipo occidental (muchas veces utilizadas para caracterizar conjuntamente a estos reinos) en organizaciones de tipo tribal (como Israel y Judá) son claramente anacrónicas.

En el libro se sintetizan también cuestiones referidas a los desarrollos religiosos de Israel: la aparición de Yhwh como deidad en Palestina (pp. 150-153); la adoración de otras deidades (además de Yhwh) (pp. 153-159); la oposición entre la religión con base en el templo y la religión “popular”/“familiar” (pp. 159-161); y el desarrollo del monoteísmo (pp. 161-163). A esto podemos agregar la ulterior discusión en torno a la reforma del rey Josías de Judá en el siglo VII a.C. (pp. 206-207). Sobre Yhwh se tienen referencias epigráficas seguras a partir del siglo IX a.C.; sin embargo, los orígenes de esta deidad podrían remontarse hasta la Edad del Bronce Tardío (cf. p. 49), en la que documentos egipcios hacen referencia a “la tierra de los Shasu Yahw”. Como indica el autor, “*although the name Yhw seems to be geographical, it is possible that there is a connection with the divinity Yhwh, perhaps the region giving its name to the god worshipped there, or even possibly the deity giving the name to the region*” (p. 151). Con respecto al desarrollo del monoteísmo, la cuestión no ha sido resuelta definitivamente en la actualidad. En efecto, y ante la evidencia epigráfica de la Edad del Hierro y de los textos bíblicos, está claro que otras divinidades eran adoradas en Palestina y que progresivamente el culto a Yhwh se impone, aunque este proceso no está completamente dilucidado. Recién con el testimonio de Hecateo de Abdera (ca. 300 a.C.) se atestigua una descripción de la religión de los judíos de Palestina como monoteísta y anicónica (p. 162).

En suma, varias de las aseveraciones de Grabbe pueden en verdad discutirse (y hasta refutarse); no obstante, la manera didáctica en la que el autor presenta la discusión de fuentes históricas, posturas historiográficas y perspectivas generales, convierte a esta obra en una excelente introducción, desde un punto de vista moderado, a los problemas actuales de la historia de Israel en Palestina.

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JOSEP PADRÓ, *La lengua de Sinuhé; gramática del egipcio clásico*, Barcelona, Crítica, 2007. 375 páginas. ISBN 978-84-8432-964-3. € 29.50.-

Dada la falta de instrumentos bibliográficos adecuados en castellano para el estudio de la lengua egipcia antigua², la aparición de una gramática del egipcio clásico, editada

² *How to Read Egyptian Hieroglyphs*, de M. Collier y B. Manley, traducido por Alianza, es de alcance limitado. El *Diccionario jeroglífico* de Sánchez Rodríguez (Madrid, Alderabán, 2000) es útil para

por una editorial de tan amplia difusión en países de habla hispana como es Crítica, debería ser saludada como un verdadero acontecimiento. Lamentablemente, después de la lectura del texto no podemos evitar un sentimiento de decepción. “Quizás no sea la mejor gramática que se podía hacer en este momento” nos dice Padró en la Introducción (p. 6) y en eso no podemos menos que estar de acuerdo. La falta de precisión en la definición de conceptos básicos y, especialmente, la ausencia de algunos temas de importancia, le impiden a esta gramática constituirse en un manual apropiado para introducirse en el estudio del egipcio clásico. El texto está dividido en tres partes: Parte 1: Historia, fonética y escritura, Parte 2: Morfología y sintaxis de la oración simple; Parte 3: Sintaxis de las oraciones compuestas. Se agrega un Apéndice con ejercicios, lista de signos jeroglíficos, vocabulario egipcio-castellano, castellano-egipcio, antología de textos y nueve láminas epigráficas. En lo que sigue exponemos, por razones de espacio, solamente las principales observaciones al contenido de la gramática. Para mayor claridad, citamos el texto de Padró en cursiva.

Capítulo 2: Cuestiones de metodología

“(el egipcio clásico) es la etapa del egipcio en que la lengua egipcia está mejor estructurada...lo cual hace que el egipcio clásico tenga un mayor rigor gramatical y ortográfico (dentro de lo que cabe) en comparación con las otras etapas de la lengua, en las cuales realmente había mucha más libertad, dando hoy más problemas al estudiante” (p. 26). Que el egipcio medio tenga mayor rigor gramatical que el neogipcio es una afirmación aventurada. En realidad, lo que complica el panorama lingüístico en época ramésida con respecto al del egipcio medio es la aparición de diferentes registros, esto es, la mayor o menor intrusión de expresiones lingüísticas en egipcio clásico de acuerdo a la jerarquía cultural de cada texto.

Capítulo 4: escritura jeroglífica y fonética (sic)

“Los jeroglíficos son los signos más antiguos inventados por los egipcios y se pueden clasificar en diferentes grupos: -ideogramas –fonogramas –determinativos” (p. 39). Más allá de que el autor no define qué es un signo jeroglífico, para evitar confusiones, e independientemente de cual haya sido el origen y la evolución del sistema, es preferible definir el carácter ideográfico, fonográfico o determinativo de un signo como funciones que, en teoría, todo signo puede cumplir. El alumno advertirá con el tiempo que no todos los signos cumplen con esas tres funciones.

“Un ideograma es un signo que representa una realidad o una idea” (p. 39).

Un signo de escritura nunca representa una realidad o una idea, lo que representa un ideograma es una palabra, por eso hoy se prefiere hablar de ‘logogramas’ o ‘signos-palabras’ (los más rigurosos de ‘morfemograma’). Este concepto permite evitar la confusa aclaración acerca de que *los “ideogramas tienen una lectura fonética”* (pág. 63).

“La mayoría de los signos jeroglíficos son ideogramas...los signos que aparecen más repetidamente en la escritura son, sin embargo, los fonogramas y los determinativos,

mientras que la mayoría de ideogramas son utilizados sólo de forma muy esporádica (pág.64).” Estas afirmaciones son desconcertantes para un principiante. Los signos jeroglíficos son pictogramas, en tanto representan seres, objetos, entidades. Aun así la mayoría de los signos tiene un valor fonográfico, esto es, expresan sonidos.

Capítulo 6: Introducción a la sintaxis de las oraciones simples

“*Oración gramatical-Es el elemento mínimo, completamente inteligible de una lengua*” (pág. 76). Se podrá discutir cuál es el elemento mínimo inteligible de una lengua (palabra, morfema, monema), pero está claro que la oración no lo es. El concepto se aclara algo si reemplazamos *de una lengua* por ‘de un texto’.

“*Oración de predicado preposicional*” (pág. 78).

Preferimos el nombre tradicional de oración con predicado adverbial, por dos razones: porque la construcción preposición + sustantivo tiene un valor adverbial y porque el predicado puede ser un adverbio y, por lo tanto, la preposición estar ausente (como señala Padró en la pág. 91).

Capítulo 10: Formas simples del verbo

El autor da un cuadro (pág. 109) con las formas y la traducción del que considera modo indicativo del verbo, pero no se explican sus significados y funciones.

“*Verbo “ser” iw que corresponde a nuestro “ser” o “estar”*”. (p. 111).

Difícilmente se podrá sostener hoy que la partícula *iw* cumple el papel de cópula. Más allá de sus orígenes semánticos se trata de una partícula gramaticalizada intraducible que expresa que la situación enunciada por la oración de predicado adverbial tiene valor en el “aquí y ahora” de la enunciación.

Capítulo 11: Construcción de los cuatro tipos de oración

“*Otras construcciones importantes de predicado preposicional: construcción m de equivalencia... Hay que tenerla en cuenta porque se corresponde y puede sustituir a una oración de predicado nominal*” (pág. 122).

Las oraciones adverbiales nunca son equivalentes a las nominales; el predicado con la llamada *m* de equivalencia expresa una relación de situación o estado que se puede comprender como identidad contingente, circunstancial o temporal, la oración nominal expresa una relación de identidad permanente, que hace a la naturaleza del sujeto identificado.

En el párrafo, que incluye las construcciones *hr* + infinitivo y *r* + infinitivo, se debería haber incluido *m* + infinitivo.

Capítulo 14: Formas compuestas del verbo

Tampoco se explican los significados y funciones gramaticales de las formas compuestas del verbo.

Junto a *sdm.hr.f* y *sdm.k3.f* (pág. 143) se podrían haber mencionado las formas derivadas *hr sdm.f* y *k3 sdm.f* con subjuntivo-prospectivo y *hr.f sdm.f* y *k3.f sdm.f* con imperfectivo. Por otra parte, estas formas tienen diferentes matices de sentido no

especificados.

Capítulo 17: Sintaxis del infinitivo. Modos del verbo. Verbos auxiliares.

“El modo prospectivo viene a corresponder... a nuestros modos potencial y subjuntivo y por lo tanto tiene significados de posibilidad, de opción, de deseo, etc.” (pág. 162).

Este modo, que preferiríamos llamar subjuntivo o, en todo caso subjuntivo-prospectivo (en realidad, se trata de dos formas verbales distintas), es analizado muy someramente. No hay ejemplos que aclaren los distintos usos que puede tener.

Como verbos auxiliares se mencionan *iw*, *wnn*, *ḥ* (pág. 163). De *iw* ya hemos hablado. No se analizan las distintas construcciones con los auxiliares *wnn* y *ḥ*.n acompañando formas verbales o pseudo verbales; de este modo, un alumno no puede entender el papel de *wnn* en un ejemplo como *wnn.k ḥr rdit di.tw n.f ḥkw* (pág. 173) o en *wn.in ib n ḥm.f kb n m33 ḥnn.sn* (pág. 174).

Capítulo 18: Negación. Interrogación

“Especialmente interesantes son: el verbo de prohibición *imi*... y el verbo negativo *tm*, que niega al verbo que lo sigue” (pág. 167).

Esto es todo lo que el autor nos dice de los verbos negativos *imi* y *tm*. Por lo tanto, quedan fuera del alcance de esta gramática: el uso del complemento verbal negativo, la negación del subjuntivo-prospectivo como expresión de orden o deseo, la negación del subjuntivo-prospectivo como cláusula subordinada, la negación del imperativo, la negación del infinitivo, la negación de los participios y la negación de las formas relativas.

Capítulo 19: Oraciones subordinadas completivas

No son mencionadas las partículas *is* y *ntt* como marcadoras de cláusulas nominales no verbales.

Capítulo 20. Oraciones subordinadas relativas

“El egipcio clásico no puede construir oraciones subordinadas relativas de predicado sustantival y adjetival.”

Esta afirmación vale para subordinadas relativas explícitas o marcadas, pero no para las implícitas o virtuales, por ej. *nḏs ddi rn.f* “un plebeyo cuyo nombre es Dedi”, cláusula relativa configurada como oración nominal; o la citada por el mismo Padró *špss pw ʕ n.f ḥt* “Era un rico que poseía muchos bienes”, cláusula relativa con predicado adjetival.

Más allá de esto, entendemos que la inclusión de los participios y las formas verbales relativas en el capítulo de oraciones subordinadas relativas es un acierto de Padró.

Algunos de los conceptos explicados son repetidos hasta tres veces, por ej. el caso de las oraciones con predicado adjetival y sujeto pronominal en primera persona (págs. 101, 116-117, 119). Es cierto que en un curso introductorio son necesarias las repeticiones

de conceptos, pero esto, que vale para la clase dictada oralmente, no necesita ser trasladado al curso impreso que puede ser releído tantas veces como sea necesario. Por otra parte, el espacio ahorrado se podría haber utilizado para profundizar en temas que sólo son mencionados. En síntesis, una gramática de egipcio medio en castellano adecuada para ser utilizada en cursos o en el aprendizaje personal sigue siendo una tarea pendiente.

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PETER DORMAN – BETSY BRYAN (eds.), *Sacred Space and Sacred Function in Ancient Thebes*. Occasional Proceedings of the Theban Workshop. Studies in Ancient Oriental Civilization 61. Chicago, The Oriental Institute Press, 2007, 213 pp., con fotos y mapas. ISBN: 978-1-885923-46-2. U\$S 35.00.-

Los orígenes de esta primera edición de *Sacred Space and Sacred Function in Ancient Thebes* sienta sus bases en la discusión de ciertos trabajos presentados en los encuentros del “*Theban Workshop*” – impulsado por la *John Hopkins University* y el *Oriental Institute* de la Universidad de Chicago – cuyos programas de investigación son expuestos en el prefacio del libro. Luego de los comentarios del Director General de Antigüedades del Alto Egipto M. el-Bialy respecto de la importancia de acompañar el desarrollo paulatino de estudios en el campo de la egiptología con un apropiado mantenimiento de los sitios arqueológicos, se presenta la primera parte, denominada “*Divine Temples, Ritual Landscapes*”.

El artículo inicial de M. Ullmann, “*Thebes: Origins of a Ritual Landscape*”, abre la disyuntiva respecto de los orígenes del *paisaje ritual* en la antigua Tebas. En función de abordar las referencias más tempranas del sitio la autora se remonta a la primera construcción del templo de Karnak – hacia la primera mitad de la Dinastía XI, durante el período que precede el reinado de Mentuhotep II – así como a las fuentes textuales y arqueológicas de este sitio y las de Deir el-Bahari. Llegado a este punto, la evidencia descrita inicialmente le permite argumentar que el *paisaje ritual* – bien conocido durante el Reino Nuevo – tiene sus orígenes durante un período de fuertes motivaciones políticas y de legitimación soberana que puede ubicarse durante la Dinastía XI. Asimismo, según Ullmann, también es posible situar para épocas muy tempranas las primeras procesiones que vinculan a Karnak con los templos de la costa occidental, al mismo tiempo que trazar una primigenia relación entre Karnak y el templo de Luxor. Todos estos aspectos llevan implícito, según se afirma aquí, un vínculo temprano entre el culto a Amón y el culto real.

A continuación, bajo el título de “*Archaeological and Textual Evidence for the Function of the ‘Botanical Garden’ of Karnak in the Initiation Ritual*”, D. Laboury

sigue los aportes teóricos de Ph. Derchain y postula una *perspectiva integral* – que conjuga el estudio de los rasgos arquitectónicos, textuales e iconográficos que hacen a la comprensión de un monumento – para abordar las peculiaridades del “Jardín Botánico” construido por Tutmosis III en Karnak. Con el objeto de dar una respuesta no tradicional sobre la funcionalidad del sitio, el autor considera las características excepcionales del santuario - tales como su ubicación aislada en el complejo, así como su peculiar diseño, sus dimensiones y decoración - al tiempo que añade los aportes provenientes de textos más tardíos para su correspondiente análisis. Finalmente, como resultado de un estudio exhaustivo de aquellas fuentes Laboury infiere que la *suite* de Tutmosis III debe asociarse con un espacio para la realización de *rituales de iniciación*, tratándose específicamente de aquellos por los cuales los sacerdotes eran inducidos al servicio de Amón en Karnak.

El trabajo posterior de S. Grallert, “*Pharaonic Building Inscriptions and Temple Decoration*”, constituye un acercamiento lingüístico a los textos tebanos; una aproximación fundada en la búsqueda explícita de la vía más asequible para el entendimiento del rol conceptual y decorativo de la *escritura* dentro de los templos. Desde sus inicios el artículo sigue una metodología rigurosa, que parte de una definición oportuna del concepto de “*inscripciones de edificios*” – es decir, aquellas que se refieren a la fundación de una estructura sacra (templos y tumbas), o bien, a la deificación de reyes, etc. – para continuar luego por una división del término en categorías más específicas basadas en lineamientos gramaticales así como en la ubicación espacial de tales inscripciones en los edificios. A continuación, Grallert conjuga este tipo de estudio con el de las formas arquitectónicas correspondientes a cada inscripción, arribando así a indicios relevantes para comprender el *sentido* de estos edificios antiguos. De este modo, el estudio lingüístico de los textos plasmados en la arquitectura y su función decorativa le permiten describir un *cosmos en miniatura*, donde cada elemento contribuye en el desarrollo de una adecuada identificación de la utilidad del espacio sagrado en la antigua Tebas.

Desde una óptica diversa de la “integral” o “lingüística” – planteadas por las investigaciones que lo preceden – el trabajo de P. Brand, “*Veils, Votives, and Marginalia: The Use of Sacred Space at Karnak and Luxor*”, recorre las inscripciones marginales y las decoraciones externas de Karnak y Luxor en época ramésida. A partir de la observación específica de aquellos espacios el autor discurre sobre la adaptación paulatina de las paredes externas de los templos que, desde este período, se colman con vastos relieves de escenas rituales y de batallas, implicando una reutilización de las áreas marginales. Asimismo, Brand ha observado la presencia novedosa de deidades en las paredes externas de aquellos templos, tratándose de divinidades que tradicionalmente se encontraban asociadas con los recintos internos y en tiempos ramésidas pasan a formar parte de las representaciones exteriores de los templos. Tales modificaciones le permiten inferir al autor una adaptación cúltica de los monumentos tradicionales en objetos de adoración “popular”, al mismo tiempo que

una reorganización del espacio sagrado para fines políticos e ideológicos.

Dando un gran salto en el tiempo, en “*The Terminology of Sacred Space in Ptolemaic Inscriptions from Thebes*”, B. McClain explora la terminología arquitectónica del espacio en el Egipto ptolemaico, puesto que es allí, en aquellas inscripciones tebanas donde parecen encontrarse las huellas que delatan una concepción particular de lo sagrado, aquellas que demuestran un particular interés por conservar las palabras utilizadas en tiempos precedentes. Para comenzar su análisis McClain establece dos modos a partir de los cuales los antiguos egipcios designaban los lugares sagrados, esto es, las denominaciones cosmológicas y mitológicas y la presencia de ciertos topónimos para expresar las ubicaciones físicas. Luego, el autor continúa analizando los modos en que los egipcios ptolemaicos se remitían al espacio sagrado en sus textos, para llegar finalmente a argüir el esfuerzo extraordinario que habrían realizado estos últimos para emplear adecuadamente los modos de expresión característicos de los tiempos más tempranos. En relación con este particular modo lexicográfico así como con los esfuerzos por reparar las estructuras monumentales según los cánones del modelo antiguo, McClain señala la extraordinaria veneración que desarrollaron los reyes ptolemaicos del antiguo centro tebano.

A continuación se da inicio al segundo apartado de la obra, “*Theban Tombs and Necropoleis*”, cuyo primer aporte, “*Intersection of Ritual Space and Ritual Representation: Pyramid Texts in Eighteenth Dynasty Theban Tombs*”, se encuentra a cargo de H. Hays y W. Schenck. Aquí, la temática propuesta gira en torno a los monumentos sagrados que se ubican al oeste de Tebas, la localización de los *Textos de las Pirámides* en las tumbas de la Dinastía XVIII, y su relación con las prácticas funerarias. Un estudio riguroso de las fuentes llevó a los autores a detectar elementos particulares del Reino Medio en las tumbas de la primera mitad de la Dinastía XVIII – como la disposición de los *Textos de las Pirámides* en áreas inaccesibles de las tumbas y la asociación de aquellos textos con imágenes –, todos ellos aspectos de una tradición que encuentra su fin con el reinado de Amenhotep III. Asimismo, la original orientación de los *Textos de las Pirámides* presenta para Hays y Schenck una novedosa utilización de los textos antiguos al servicio mortuorio de las tumbas tebanas del Reino Nuevo.

Desde una óptica más puntual, C.H. Roehrig aborda en “*Chamber Ja in Royal Tombs in the Valley of the Kings*” un análisis de los instrumentos mobiliarios, los textos y la arquitectura funeraria de una de las cámaras subsidiarias de las tumbas del Valle de los Reyes – la cámara “Ja”, según la denominación de E. Thomas –. Frente a ciertas consideraciones realizadas respecto de este cuarto y su funcionalidad como despensa para ofrendar líquidos y/o alimentos, la autora sugiere que éste no fue su principal propósito. A partir de un estudio de las particularidades del aposento, y la orientación del sarcófago a lo largo de la historia de Egipto, Roehrig sugiere que este espacio debe asociarse con la transfiguración del rey en Osiris, como lo indican las particularidades arquitectónicas del recinto, dispuestas para facilitar el viaje del rey hacia el mundo subterráneo.

Bajo el título de “*Use, Reuse, and Abuse of ‘Sacred Space’: Observations from Dra Abu Al-Naga*”, B.G. Ockinga desarrolla un análisis arqueológico de las fases sistemáticas de ocupación de las tumbas; de aquellos ámbitos que evocan un paisaje particularmente sagrado del Egipto antiguo. Sobre estos pasos, y con el fin de develar como los sitios fueron utilizados, reutilizados y maltratados con el paso del tiempo, el autor analiza los indicios textuales y arqueológicos que le permiten establecer el accionar de quienes ingresaban y robaban las tumbas, así como sentar patrones de adaptación y “no adaptación” de las tumbas a lo largo del período ramésida.

Frente a los períodos históricos más tempranos abordados por los trabajos precedentes, el artículo de H. Behlmer, “*Christian use of Pharaonic Sacred Space in Western Thebes: the Case of TT 85 and 87*”, considera la utilización del espacio sagrado faraónico de las tumbas 85 y 87 durante la época cristiana en Egipto. Como remarca Behlmer, la reutilización de aquellos espacios desde la óptica cristiana resulta tan significativa que no existe en la actualidad un templo al oeste de Tebas que no muestre signos de esa era. Sin embargo, como lo indica en su artículo, para su análisis correspondiente debe considerarse que muchos rastros de aquel período se han perdido a causa del creciente interés en el Egipto faraónico; atracción que, seguida de inescrupulosos descuidos condujo a la destrucción de vastos vestigios post-faraónicos. Pese a las pérdidas mencionadas, las tumbas 85 y 87, constituidas en monasterios durante el Egipto cristiano, presentan aspectos relevantes para la reconstrucción del pasado cristiano en Egipto.

La contribución final de K. van der Spek, “*Feasts, Fertility, and Fear: Qurnawi Spirituality in the Ancient Teban Landscape*”, constituye una aproximación realizada desde el campo de la antropología social que, tras concebir el paisaje como un entorno físico donde se encarnan las manifestaciones humanas, entiende que la necrópolis de Tebas es al mismo tiempo un sitio arqueológico y una *realidad social* que puede y debe estudiarse a partir de las peculiaridades de la sociedad Qurnawi residente en el sitio, en el “*World-Heritage listed area*”. Los objetivos de un estudio de este tipo no se deben, como aclara Van der Spek, a las relaciones culturales entre las sociedades antiguas y modernas, sino al vínculo que establece entre ambas el paisaje compartido. Como consecuencia de esta novedosa aproximación, el autor estudió esta comunidad contemporánea desde tres temáticas puntuales: *las fiestas, la fertilidad y el temor*. Tres aspectos vinculados con las prácticas de la antigua Tebas cuya antigua arquitectura funeraria fue reconstruida en función de la espiritualidad moderna. Entendemos que mediante su análisis Van der Spek acerca a *Sacred Space and Sacred Function in Ancient Thebes* una herramienta teórica que trae consigo un núcleo poderoso para la reinterpretación de las percepciones humanas desde el pasado hasta el mundo moderno.

En líneas generales, cabe decir que esta primera edición de P. Dorman y B. Bryan comprende aportes significativos para la investigación del área tebana, con contribuciones que, por su diversidad metodológica y temporal, conducen a una concepción de la “historia” implícitamente interdisciplinaria. Asimismo, no serán la

variedad metodológica ni las aproximaciones diversas la riqueza principal que presenta la obra, sino su vasto aporte documental que, tanto en materia de arqueología como de iconografía acompañan la sucesión de artículos. Para concluir, el lector especializado – para quien va dirigida especialmente la edición en su conjunto – encontrará aquí una fuente de suma actualidad respecto de las nuevas tendencias que siguen los estudios sobre el espacio sagrado en el antiguo Egipto.

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ERIC CLINE - DAVID O'CONNOR (eds.), *Thutmose III. A New Biography*. Michigan, The University of Michigan Press, 2006. ISBN-13: 978042114672, ISBN-10: 0-472-11467-0. 570 páginas, con láminas en blanco y negro U\$S 85.00.-

Tutmosis III se nos presenta en este libro como uno de los más grandes faraones del antiguo Egipto. Su prematuro acceso al trono, causado por la repentina muerte de su padre, Tutmosis II –Dinastía XVIII, Reino Nuevo-, hizo necesaria la regencia de la Reina Principal del Harén y Esposa de Amón, su tía Hatshepsut, quien gobernó junto a él –como un segundo “rey” – hasta su propia muerte veinte años después. Es, por tanto, la intención de este grupo de especialistas recorrer a lo largo de doce capítulos aspectos diversos en los que se analizan los tiempos de coregencia junto a la reina Hatshepsut, la administración, la actividades edilicias, la ideología, la producción artística, las visiones del exterior –tanto en el norte como en el sur– hasta alcanzar la conclusión de su reinado junto a su hijo y sucesor Amenofis II.

En las primeras líneas del prefacio es posible conocer la intención de quienes editaron la obra, al igual que su estructura y el público amplio al que se dirige. La originalidad es una de las metas a cumplir, puesto que el reinado tutmósida no cuenta con un antecedente bibliográfico de este porte. Por ello, L. Shirley y L. Bestock destacan la relevancia del libro, por su magnitud, la profundidad y minuciosidad con la que se tratan las distintas temáticas y la accesibilidad idiomática (la obra está publicada en inglés). El primer capítulo, a cargo de D. O'Connor, es una presentación de los reyes egipcios en general, y de los del Reino Nuevo en particular, hasta concluir en el protagonista de esta obra, Tutmosis III. Éste es descripto como un faraón enigmático, puesto que aunque contamos con documentación suficiente para reconstruir sus intereses humanos y eventos históricos que acompañaron su gobierno, dichos textos no puede considerarse como una representación fidedigna de su personalidad. De todos modos, O'Connor considera que con la documentación disponible es suficiente para discutir sobre el extenso reinado de Tutmosis III, lo cual no sucede con muchos de los demás faraones. Esta comparación es inevitable, ya que este primer capítulo, que hace las veces de introducción, abarca todos los aspectos del reinado tutmósida, al tiempo que presenta a los especialistas que aparecerán a lo largo de la obra.

Este es el caso de P. Dorman, quien analiza, en el segundo capítulo, la sucesión de Tutmosis I y Tutmosis II hasta acceder “*al reinado más largo y glorioso de la dinastía XVIII*” (p. 39), en referencia a Tutmosis III. De manera que este reinado excepcional puede dividirse en, por lo menos, dos etapas. La primera correspondería al correinado del niño-rey -Tutmosis III- y la reina Hatshepsut; y la segunda, cuando Tutmosis III ya adulto asume plenamente las potencialidades del “toro poderoso de Tebas”. Así, Dorman analiza registros diversos de funcionarios reales para notar el modo en que difieren a lo largo de los años de reinado de Tutmosis III su mención como “Rey del Alto y Bajo Egipto” y otros en los que aparece junto a la “Esposa del rey” –Hatshepsut. También el autor hace referencia a inscripciones como la de Sinaí del año 13, en la que figura Hatshepsut como “rey”. De este modo Dorman analiza distintas estelas, grafitos e inscripciones a lo largo de las diversas partes del territorio de Egipto, para concluir con que el proceso que habría alterado el estatus de Hatshepsut de regente a rey no habría sido a través de un despliegue repentino de poder por su parte, sino que pudo hacerlo por medio de una “*búsqueda deliberada de una iconografía en la cual ella pudiera ejercer dicho poder*” (p. 53) hasta el año 20, año en el cual el autor considera se habría interrumpido el gobierno de Hatshepsut. Dorman sostiene que “*a pesar de haber sido considerada como usurpadora del trono egipcio, la evidencia contemporánea de los monumentos sugiere que Hatshepsut tuvo un rol distinto en el que difundió un manto heterodoxo de corregencia sobre el reinado para asegurar la sucesión del joven faraón. He aquí su gran contribución sobre éste*” (p. 58).

En el capítulo 3, B. Bryan estudia la administración del reinado de Tutmosis III a partir del texto sobre la “Instalación y Deberes del Visir”, tomado de dos tumbas pertenecientes a funcionarios reales. La autora analiza las diversas atribuciones y oficios de los que se ocuparon los visires a lo largo de todo el reinado de Tutmosis III. Es decir, que de la mano con lo expuesto por Dorman previamente, Bryan concuerda en la continuidad de gobierno a través de las dos etapas de su reinado. Esto es posible de notar a partir de la mención a funcionarios que actúan tanto bajo la corregencia de la reina Hatshepsut como con Tutmosis III. Respecto a la documentación, Bryan menciona que a pesar de no conocerse de forma acabada las atribuciones de varios cargos administrativos durante el período inmediatamente anterior a la ascensión de Tutmosis III al trono, la documentación es mayor allí debido al fuerte incremento de la burocracia. En especial la autora destaca la evidencia legada por parte de la incorporación de hombres nuevos procedentes de rangos militares formales.

En el siguiente capítulo L. Troy trata sobre la religión y el culto en los tiempos del reinado de Tutmosis III, donde es posible notar formas innovadoras de experiencias religiosas que indicarían que “*los teólogos de mediados de la Dinastía XVIII estaban firmemente afianzados en el corpus expresado por la religión antigua egipcia*” (p. 123). Un apartado se aboca especialmente al estudio de los dioses y diosas, logrando despertar el interés del lector sin poder evitar lamentarse por el poco espacio dedicado a estas temáticas. Luego, ocupa su atención el rol desempeñado por el rey, desde su coronación, en la cual menciona la complejidad del ascenso de Hatshepsut como

co- gobernante del joven Tutmosis III, otorgándole un sesgo de excepcionalidad al reinado. Por ello, el autor hace referencia a la legitimidad. Así, éste menciona que “*en más de veinte años de coregencia, el reinado dual fue presentado consistentemente como una combinación armónica de dos partes complementarias de una simple ecuación, con empleo regular de simetría para enfatizar este punto*” (p. 132). Ello fue posible a través de los cultos y las festividades tanto de los dioses como las que involucraban al rey. Finalmente, la autora hace referencia a otra variante dentro de las prácticas religiosas egipcias en este período, como fue el culto privado. A través de la institución del templo “*toda la gente de Egipto*” (p. 171) fue partícipe de la extensión de las relaciones entre la tradición y la innovación que marcó el reinado de Tutmosis III, al tiempo que influyó en los períodos siguientes.

Esta innovación religiosa fue acompañada por un programa basado en construcciones monumentales de templos y tumbas en los alrededores del área tebana, junto al despliegue ideológico y artístico que analizan diversos autores a lo largo de los capítulos posteriores. Este es el caso de P. Laskowski, quien estructura su apartado en tres secciones generales en las que analiza las fases de construcción del templo de Karnak, seguido de los templos ubicados al oeste de Tebas, y, finalmente, aquellos que se encuentran en las afueras de la región tebana, como el templo de Khnum en Elefantina. Todo lo cual conduce al autor a preguntarse por qué los últimos años de Tutmosis III estuvieron acompañados por un programa arquitectónico tan intenso fuera de la región tebana. En especial, el autor toma dos casos emblemáticos, como son por un lado el templo Djoser Akhet en Deir el Bahari, construido durante los años 43 o 49 de reinado de Tutmosis III, y por otro lado el obelisco de Karnak oriental, a pesar de haberse erigido treinta y cinco años más tarde, durante el reinado de Tutmosis IV. Sus conclusiones enfatizan los cambios lentos y graduales que integraron el largo proceso que incluyó la persecución de Hatshepsut, la creciente prominencia Tutmosis II dentro del programa ideológico de su hijo, la sucesión de Amenofis II y, por último, la influencia creciente de la religión solar en la ideología real. Por ello, el autor remarca la necesidad de investigaciones futuras para explicar estas temáticas.

Junto a la monumentalidad de los templos, el programa de construcción de Tutmosis III incluyó un despliegue de igual porte en el Valle de los Reyes. Por ello, C. Roehrig, analiza en su capítulo la tumba KV 38, identificada como la del mencionado rey. Los temas a los que se refiere la autora son la distribución edilicia de la tumba KV38, la entrada y las cámaras superiores, la fosa, el hall de columnas y la cámara mortuoria; posteriormente la compara con las primeras tumbas del Valle de los Reyes, junto a otros proyectos edilicios iniciados por Tutmosis III, hasta mencionar finalmente la tumba de la Reina Meryetre-Hatshepsut. Hacia el final de su capítulo, Roehrig hace referencia al debate académico respecto al enterramiento de Tutmosis I como el primer rey en ocupar el Valle de los Reyes, inaugurando la nueva necrópolis.

D. Laboury y A. Kozloff, en los capítulos 7 y 8, respectivamente, tratan la evolución de los retratos de Tutmosis III en sus estatuas al igual que en el resto de su producción artística. Si bien sus temáticas son similares, cada uno de estos autores enfatiza

sobre distintas etapas del reinado de Tutmosis III. Laboury analiza la evolución y la correspondencia de las distintas fases del reinado desde la coregencia junto a Hatshepsut, hasta su póstuma proscripción en la última etapa de su gobierno autónomo. El autor menciona que el rey Tutmosis III pudo “*convertirse a través de sus estatuas en la imagen viviente de su ideología*” (p. 282), para lo cual fue necesario alterar las facciones reales de su rostro. Por ello, Laboury sostiene que el problema es su definición política como rey y los cambios a lo largo de su reinado. En cambio, Kozloff se centra en la imagen de Tutmosis III como rey guerrero, y retoma la conocida asociación de éste con Napoleón Bonaparte, en cuanto a la extensión de sus conquistas. Así, el autor analiza la decoración de su tumba real, artes menores como la joyería y el trabajo en metal, cerámica y vidrio, los textiles y el mobiliario. Todo lo cual le permite concluir que el reinado de Tutmosis III fue un período de una extraordinaria producción artística, en la cual se probaron y perfeccionaron ideas nuevas. Por ello, Kozloff considera que este rey guerrero se erigió como uno de los más grandes inspiradores del arte, puesto que “*Tutmosis III seguramente reparó en que en el futuro sus conquistas desaparecerían, sin embargo sus monumentos permanecerían como testimonio del brillo de su reinado*” (p. 317).

Los siguientes capítulos están centrados en las conquistas de este rey guerrero sobre las tierras ubicadas más allá del límite norte y sur de Egipto, al igual que la visión egipcia de los extranjeros. D. Redford describe las campañas llevadas a cabo en Palestina y en Siria, a partir de fuentes textuales. El autor destaca la tensión que refleja Tutmosis III en dichos registros, respecto a la aceptación de patrones de acción prefijados y el deseo individual de promocionar su accionar como un caso único. A este respecto, Redford sostiene que su credibilidad y aceptación por parte de los antiguos egipcios se perpetuó a lo largo de los siglos, permitiendo a la generación siguiente considerar a Tutmosis III como “el padre de los padres”. A. J. Spalinger es quien se ocupa de la dominación egipcia sobre Nubia desde tiempos del rey Kamose—fines de la Dinastía XVII— hasta concluir en el reinado de Tutmosis III y Amenofis II. Su interés está centrado principalmente en la regularidad del vínculo económico y el despliegue administrativo de Egipto en la región nubia durante los tiempos del reinado de Tutmosis III. Puesto que el imperialismo egipcio se extendió en el sur hasta la cuarta catarata, el autor sostiene que a la fama de Tutmosis III como conquistador de las tierras del norte debería sumarse su pacificación definitiva en Nubia. Finalmente, D. Panagiotopoulos analiza el caso de los extranjeros durante los reinados de Hatshepsut y Tutmosis III. Las fuentes en las que basa sus hipótesis son los “Anales de Tutmosis III” y distintas inscripciones y pinturas de las tumbas de los funcionarios de ambos reyes egipcios. Así, Panagiotopoulos rastrea la mención a extranjeros de Siria-Palestina, Nubia, el Egeo, Hattí y el Punt. Sus conclusiones identifican, primero, la aceptación de nubios y asiáticos como seres humanos por parte de los egipcios y, posteriormente, la incipiente apreciación del resto de los individuos foráneos proveedores de bienes “exóticos”. Dando un salto en el tiempo, el autor se traslada a los tiempos de Akhenatón, a finales de la Dinastía XVIII, para poner de relieve el trato de este faraón hacia los extranjeros, el cual fue totalmente opuesto al

de Hatshepsut y Tutmosis III.

Este reinado tiene una inflexión durante el año 47, que es cuando se considera fue el principio de la así llamada deshonra de Hatshepsut. Al tomar este año como referencia, P. Der Manuelian analiza, a lo largo del capítulo 12, el final del gobierno de Tutmosis III y las contribuciones a la tradición monárquica del Reino Nuevo en particular, y de Egipto en general. Para ello se remite a Amenofis II, quien no estaba previsto como sucesor al trono originalmente, aunque la historia permitió que sí lo fuera. Luego de rastrear diversas fuentes, el autor concluye con que “ninguno de los sucesores de Tutmosis III, exceptuando a Amenofis II, pudo equipararse en innovación y originalidad, ni siquiera en su energía y carisma personal” (p. 426).

A modo de conclusión, la obra cumple con la promesa de sus primeras páginas a través de un ambicioso compilado de temas y enfoques variados. El legado de Tutmosis III a través de sus innovaciones y despliegues monumentales logra a través de esta biografía que el “Toro poderoso de Tebas” obtenga una última conquista al capturar el espíritu de un lector apasionado.

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JUAN MANUEL TEBES, *Centro y periferia en el mundo antiguo. El Negev y sus interacciones con Egipto, Asiria, y el Levante en la Edad del Hierro (1200-586 a.C.)*. Ancient Near East Monographs/Monografías sobre el Antiguo Cercano Oriente, Society of Biblical Literature/Centro de Estudios de Historia del Antiguo Oriente. Vol. 1, 2da. ed., Buenos Aires/CEHAO, 2008. 111 pp., con mapas, fotos y planos. ISBN 978-987-20606-4-0. En Internet: http://www.uca.edu.ar/s-cehao/otras_public/tebes_monog.pdf

El presente trabajo de Juan Manuel Tebes es el primer volumen de las Series denominadas *Ancient Near East Monographs / Monografías sobre el Antiguo Cercano Oriente (ANEM/MACO)*, resultado de un proyecto conjunto entre la Society of Biblical Literature y el CEHAO. Este libro es el resultado de la ampliación y actualización de su Tesis de Licenciatura, que fuera aprobada por la Universidad de Buenos Aires en el año 2001. Mediante su análisis, el autor busca estudiar la evolución de la estructura sociopolítica y económica de las sociedades locales del desierto del Negev y, especialmente, los vínculos de intercambio de esta zona con áreas vecinas. Bajo este objetivo, el autor se propone responder a las siguientes preguntas: “¿Qué tipo de relación establecieron las sociedades del Negev, predominantemente pastorales, con sus pares urbanos vecinos?” y “¿cómo influyeron dichos vínculos en el desarrollo de la estructura socio-económica local?” (p. 8).

El período comprendido en este volumen es la fase final de la Edad del Bronce Tardío y la Edad de Hierro I y II (1200 a.C. – 586 a.C.) en el desierto del Negev, ubicado al sur del actual estado de Israel. Para demostrar su hipótesis, Tebes utiliza diversos instrumentos de análisis, entre los cuales se encuentran las evidencias arqueológicas, proporcionadas por reportes de excavaciones y prospecciones, y el análisis de material epigráfico y literario de la época. En cuanto a su estructura, la obra se desarrolla a través de sucesivos capítulos ordenados según la cronología estipulada por A. Mazar en su libro de referencia (*Archaeology of the Land of the Bible: 10.000-586 B.C.E.*, 1990).

En el primer capítulo, el autor describe la “Teoría del sistema mundial”, enunciada por Immanuel Wallerstein en 1974, identificándola como la base teórica que respalda la hipótesis de su trabajo. A partir de esta teoría, que establece suscitadamente que existen sociedades centrales que ejercen más poder sobre otras -denominadas periféricas-, el autor reconoce en Egipto y Asiria sociedades centrales y en el desierto del Negev, una periferia. Mediante este análisis, Tebes plantea, más adelante, que las relaciones entre Egipto y el Negev no habrían sido homogéneas, en la medida que cuando aquella experimentó períodos de contracción, ésta última sufrió períodos de expansión. De esta manera, según el autor, se puede observar cómo en esta época, el desarrollo interno del territorio del Negev, en períodos de expansión de los centros, dependía de su relación con los imperios debido a la demanda sostenida, por parte de éstos, de cobre, producido localmente, o de incienso, importado desde Arabia y transportado a través del Negev. Mientras que, en períodos de contracción, la periferia podía desarrollarse independientemente, como en el caso de la jefatura de Tel Masos (siglo XII a.C.), o ser incluida dentro de otra entidad política, como fue el caso de su incorporación al reino de Israel/Judá (siglos XI – VI a.C.)

En el siguiente capítulo, el autor se ocupa de la fase de dominación egipcia sobre la zona del desierto del Negev durante la Edad del Bronce Tardío. En primer lugar, explica cómo se ejerció un gobierno directo durante la fase del “*imperialismo egipcio*” (p. 20) para después conectar esta fase de intervención política con una de tipo más económica. Posteriormente, el autor toma en consideración la fase final del Bronce Tardío cuando existía en el Negev un control tanto directo como indirecto. Como ejemplo del control directo egipcio se encuentra el caso de las minas de Timna, las cuales eran utilizadas para extraer cobre, el cual era luego transportado por vía marítima o terrestre, esta última conocida en aquel tiempo como el “camino de Horus”. Para demostrar el dominio egipcio en el Negev, el autor se vale de fuentes literarias tales como el Papiro Harris I -que incluye en un Apéndice al final del trabajo-, y arqueológicas. A través de estas fuentes, logra diversas conclusiones significativas para el posterior análisis del tema. Como ejemplo del control indirecto se encuentra el caso del comercio terrestre del cobre llevado a cabo por las sociedades locales hacia Palestina y Egipto a través de los desiertos del Negev y el valle del Arabá.

Dentro del tercer capítulo el autor analiza la situación económica y comercial particular resultante de la intervención egipcia en Timna, la cual identifica como un “*enclave económico*” (p. 37) dominado directamente por Egipto. De este modo, analiza cómo la relación entre Egipto y esa zona desértica produjo una nueva configuración económica. A partir de allí, ocupa su atención en el comercio del cobre dentro del área, el cual resulta significativo dentro de las relaciones centro-periferia. En este capítulo el autor se apoya, especialmente, en el estudio de fuentes arqueológicas -especialmente cerámica-, para apoyar sus conclusiones.

El tema al cual Tebes se aboca en el cuarto capítulo es el perteneciente al siglo XII a.C., el cual marca el fin de la hegemonía política egipcia sobre el área y el advenimiento de la jefatura de Tel Masos como resultado inmediato. El autor identifica este acontecimiento como el que permitió que el área periférica se haya desarrollado de manera autónoma, tanto económica como políticamente. Al analizar este período, el autor sostiene que la importancia política de Tel Masos estaba basada en su posición estratégica dentro de las redes comerciales desarrolladas entre Egipto -como centro- y sus periferias, y no en la subordinación de esta entidad ante Egipto.

Por último, en el quinto capítulo, el autor aborda la Edad de Hierro II, dividida internamente en tres fases (Edad de Hierro IIA, IIB y IIC). Durante la primera fase (IIA), el autor explica que, si bien hubo un desarrollo político en la zona, no lo hubo en materia económica. Ya durante las etapas siguientes (IIB y IIC), la situación cambió ante el despliegue de poder político por parte del imperio asirio. Fue durante este período, entonces, cuando se produjo un importante desarrollo económico y demográfico en el área del Negev causado por la ubicación estratégica de esta área en las redes del comercio árabe.

En suma, a partir de la “Teoría de los Sistemas – Mundo” de Immanuel Wallerstein, Tebes demuestra el desarrollo de la interacción política y económica entre los imperios egipcio y asirio, en calidad de centros, y la zona del desierto del Negev, como su periferia, durante las distintas fases de la Edad del Bronce y Hierro, entre los siglos XIII-VI a.C. De manera que es posible encontrar en esta obra un aporte significativo, tanto en lo que atañe a su temática como a los recursos metodológicos, la cual no escatima en la diversidad de fuentes y en un apropiado enfoque teórico.

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