Buried History

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Buried History

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Cover: Iron Age IIC decorated chalice found at Tuleilat Qasr Mousa Hamid January 2015. Image J. A. Verduci

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Editorial

We commence our second half-century of publishing by adopting colour images. Archaeology is essentially about material culture and colour is an important aspect of any investigation, whether it be object based or landscape. While there are cost implications for this change, there is a far greater cost increase for postage when the volume exceeds fifty pages, as it does this time. The increased size is partly the result of the inclusion of a number of papers associated with Institute projects.

The first paper by Josephine Verduci reports on her initial season of excavation at Tuleilat Qasr Mousa Hamid in Jordan near the Dead Sea. The excavation was sponsored by the Institute in conjunction with the Hellenic Society of Near Eastern Studies. Jo is a Research Fellow at the Institute. She commenced her professional life in the world of fashion before shifting the focus to the clothing and adornment in the ancient world. She made the transition to archaeology at the University of Melbourne and is currently completing a PhD, entitled *Philistine adornment and cultural intentions: Cross-cultural influences in the Eastern Mediterranean during the Early Iron Age*. She also consults in cultural heritage management in Aboriginal and historical archaeology within Australia.

During the last season of the Kourion Urban Space Project, Cyprus, where the Institute is a consortium member, we discovered an inscription. Professor Gregory Horsley together with the discoverers have prepared a brief comment about the inscription.

Michael Lever also came to archaeology as a mature student after a career in the textile business. He is a graduate of Sydney University and has done further study at the universities of La Trobe, Melbourne and McGill. He has worked with a number of archaeological consultancies and is a Research Fellow at the Institute. His research focuses on the history of archaeological theory. We are pleased that this has led him to work on Gordon Childe and to contribute a paper about him, which fits nicely into the series of papers that *Buried History* has recently published on early Australian archaeologists.

Many of my colleagues associated with the archaeology of Kourion, Cyprus, are sceptical about the suitability of the site as a port because of the perceived sailing limitations of Roman period merchant ships. My doubts about their perspective were confirmed as I reviewed current maritime research. The resulting paper was somewhat of an indulgence for me after a lifetime of sailing and ship modelling. It was also a revelation to discover how research into ancient seafaring has progressed in recent years. Hopefully the paper fills a lacuna with respect to the sail-plan of Roman period merchant ships. My affiliation with the University of Melbourne is gratefully acknowledged as this work was heavily dependent on the services of the Baillieu Library.

Bashar Mustafa is from Syria. Like many of his countrymen he has had to leave his homeland for the time being and is working in the Department of Prehistory and Archaeology, Faculty of Philosophy and Letters, Granada, Spain, where he has completed Masters and PhD study in Ancient History. He has published on the culture of the Syrian coast, especially during the first millennium BC and we are delighted with his paper on a fascinating tomb complex.

Two years ago *Buried History* carried a brief communication about a project to publish all cuneiform material held in Australia and New Zealand. Dr Luis Siddall leads the project in Australia and provides a progress report. He is an Honorary Associate of the Department of Ancient History at Macquarie University and his publications include *The Reign of Adad-nirari III* (Brill, 2013) and numerous articles on Assyrian history and the Amarna Letters.

Wayne Horowitz is Professor of Assyriology at The Hebrew University, Jerusalem, and Peter Zilberg is one of his students. The brief report, also authored by Larry Stillman, Monash University, and Scott Reeve and Moira White of the Otago Museum, reveals the Otago cuneiform material to be fascinating collection. Its full publication will involve a significant amount of scholarly work.

Our reviewers have evaluated some weighty tomes for this edition. We are indebted to Dr Juan Manuel Tebes, Catholic University of Argentina, Buenos Aires, Luis Siddall, Macquarie University, Elizabeth Robar of Tyndale House, Cambridge, and Dr Lamia Al-Gailani. Lamia is a retired scholar who is currently writing the history of the Museum of Iraq, which involves spending time in Baghdad.

As usual we acknowledge our reviewers, who have spent much time on our behalf. Their scholarly endeavour has added significant value to the papers published herein.

Christopher J. Davey
Editor

Excavations at Tuleilat Qasr Mousa Hamid

J. A. Verduci

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Abstract: Tuleilat Qasr Mousa Hamid, in southern Jordan, is believed to be the Iron Age site of biblical Zoar. This paper reports on an excavation that was undertaken in January 2015. It is clear from finds that there was a significant agricultural and industrial settlement at the site in the Iron Age II period.

In January 2015 the Australian Institute of Archaeology (AIA) in collaboration with the Hellenic Society of Near Eastern Studies (HSNES) launched its inaugural archaeological survey and excavations at the fertile agricultural site of Tuleilat Qasr Mousa Hamid (Mousa Hamid) in southern Jordan. Excavations at Mousa Hamid aim to examine the extent and nature of an Iron Age (IA) IIC site at the south-eastern end of the Dead Sea. These excavations focus on specific questions, such as the identification of cultural material that can be linked to ethnic or cultural groups, and the examination of previously unrecognised trade relationships between the site and its ancient neighbours. Furthermore, it intends to examine what is believed to have been a significant Edomite agricultural and industrial community.

The role of developmental trends in Edom is a debated topic (Bienkowski, 2001; 2002; 2009; Levy 2009; Porter 2004), but it has primarily focused on the technological aspects and the role of metal production, typically at sites located on the high plateau and the region surrounding the important copper production site, Feynan, which had already been abandoned by the IA IIC (Levy 2009; Levy et al. 2014: 986). With copper production no longer an economic factor in the region at this time, Zoar's location along trade routes between Arabia, the Levant and the Mediterranean might reveal the important role this site played in the economic system of the later IA period.

Location and Environment

Mousa Hamid, at 392m below sea level, is one of the lowest places on the earth's surface**.** Commonly believed to be biblical Zoar (MacDonald 2000: 45), the site forms part of the modern town of Safi, and has views of the mountainous highlands to the east and the shores of Israel to the west. The town is situated in the Ghawr es-Safi, a *ghawr* is an alluvial fan at the mouth of a wadi entering the Rift Valley flowing down from the highland mountains to the east. This ghawr lies at the mouth of the Wadi al-Hasa on the border of ancient Moab and Edom, Figure 1. The southern extent of Moabite territory to the Wadi Mujib seems explicit enough, while vague references to Moab extending to Zered most likely refer to Wadi al-Hasa (MacDonald 1988: 73; Miller 1989). Both the Wadi Muijb (max. 1.1km deep and 6km wide) and Wadi al-Hasa (max. 1km deep and 7km wide) are dramatic geographic features forming natural topographic divisions.

Figure 1: *View of the Ghawr es-Safi, looking west. Photo: J.A. Verduci.*

During the IA the Dead Sea purportedly served as an international border and its shores and the Wadi Arabah running between it and the Gulf of Aqaba as the boundary between various cultural groups in Cisjordan (west of the Jordan valley) and Transjordan (east of the Jordan valley), particularly between Edom and Judah. However, there is little evidence to substantiate these cultural or ethnic distinctions other than the biblical account.

Owing to its tropical climate and to the waters coming down from the mountains through the wadi, Zoar was a flourishing oasis in an alluvial fan that is said to have been abundant in balsam and date trees and an important centre for indigo and sugar production (Goor 1967; Politis 1999). Climatic conditions in the biblical period are not well-understood, but a recent reconstruction of environmental conditions during the IA by the Edom Lowlands Project suggests that the climate was very similar to today (Levy et al. 2014); the region was arid with a steady intake of water from springs in the Wadi al-Hasa and rains and heavy snow runoff from the highlands.

Historical record

Sparse settlement patterns in IA I Transjordan give way to marked expansions from the IA IIB period (*ca*. 900–700 BCE) (Younker 2003; Routledge 2004). Nevertheless, while evidence of significant sites linked to both the bible and Jewish history exist to the north of the Dead Sea, such as at Nimrim/Bethnimrah (Numbers 32:36), very little evidence of IA settlement exists at the southern end of the Dead Sea in the lowlands of Transjordan – other than at Mousa Hamid/Zoar (Politis 1999: 543f).

In the Old Testament Book of Genesis (14:2, 8), Zoar was one of the five so-called 'cities of the plain', along with Sodom, Gomorrah, Admah, and Zaboim, but was spared destruction by fire and brimstone because it had sheltered Lot and his family (MacDonald 2000: 45). According to this dramatic biblical tradition, Lot and his daughters escaped to Zoar and took refuge in a nearby cave at Deir 'Ain 'Abata, which was commemorated with a monastery that flourished from the fourth–seventh centuries. The monastery and Byzantine Zoar (Zoara) are most frequently known by their depiction on the topographically reliable mosaic map at Madaba, Figure 2 (Piccirillo and Alliata 1999). The map represents the site as a substantial settlement with three towers and two red-roofed churches in the midst of a grove of palm trees.[1]

Historians, such as Josephus, Ptolemy, Eusebius and Saint Jerome, position Zoar (Zoara/Seghor/Sughar/Zughar) at the southern end of the Dead Sea (Robinson and Smith 1841: 648–51). The location of a Roman cavalry unit at Zoar was reported in the *Notitiae Dignitatum* (72). Hierocles refers to it in the geographic tract, *Synecdemos* (Burckhardt (ed.) 1893), as does George of Cyprus in the *Descriptio Orbis Romani* (Gelzer (ed.) 1890). Later descriptions of Zoar include those by Fulcher of Chartres and William of Tyre (Fulcher of Chartres 1969; William of Tyre 1976 (1941)). Beautiful descriptions of Zoar

Figure 2: *Detail of the late 6th century mosaic map at Madaba. Note how the site of 'Zoora' is surrounded by date palms. Image: after Politis 2012b.*

were also left by Arabian geographers, who noted the sweetness of its dates (Ibn Hudadbeh: refer to Le Strange 1890: 289). Al Madisi identified a capital (Sughar) in the district ash-Sharah, south of the Wadi Mujib. He suggested that the site was strategically positioned to control passages through the Rift Valley as roads led from the site to Jerusalem, Nablus, Amman, Petra, and Damascus (Walmseley 2001: 517f); indeed, crusader campaigns in Jordan, as described by Fulcher of Chartres and William of Tyre, undertook routes that passed through Zoar on the way to and from Jerusalem (Fulcher of Chartres 1969: 145–7; William of Tyre 1976 (1941): 427). Routes such as these remained open, albeit with taxes imposed, despite warfare between the Muslims and Crusaders (Ibn Jubayr 1952: 300f; Walmsley 2001: 544).

Regional interactions

As the IA spans approximately 700 years (*ca.* 1200–500 BCE), it is crucial to understand that this was a dynamic period in which fundamental changes in social organisation and historical circumstances could occur at the century scale (or less). The history of Edom and the southern Dead Sea region extends back to the tenth century BCE, equivalent to the IA IIA period. Although the emergence of the Edomite state occurred during this early phase,[2] it was not until the seventh century that we witness the evolution of a complex society. Perhaps this development was partly due to Edom's geographic location as the outlet of the Red Sea incense route to Mediterranean ports (Finkelstein 1988), with the control of these routes

leading to the rise of the Edomite kingdom.[3] The focus of research in southern Jordan has been on the development of IA settlement patterns in the highlands at sites such as Buseirah (biblical Bozrah) and Tawilan (possibly biblical Teman). The prosperity of Buseirah, with its monumental buildings and evidence of social stratification and economic administration, is generally attributed to Assyrian influence or even domination (Hart 1986: 54).

It was not until 640 BCE that the Assyrian Empire withdrew from the west, before which we might safely assume the inclusion of Zoar in an asymmetrical relationship with Assyria. Evidence for an active Assyrian presence along overland trade routes in Cisjordan is seen from the eighth century BCE (Singer-Avitz 1999: 7). It may be that stations located at the heads of passes through the Wadi al-Hasa were also involved in the control of goods between Assyria and Edom (Routledge 2004: 193–200), particularly at Khirbet an-Najjar and at Khirbet al-Akuza (Van der Steen 2009: 126). The significant location of Zoar near this junction between north–south and east–west routes might thus be connected to the flourishing of Edomite trade networks.

IA II encounters between Edom and Assyria are attested to in an inscription from the reign of Tiglath-Pileser III dating to 728 BCE mentioning the tribute obligations of the kings of Edom and Moab (among others), who had submitted to the Assyrian king (Tadmor 1994: 170f). This relationship would have been manifest in an attempt to divert trade, but the use of fortifications to limit the Assyrian domination is documented (Eph'al 1982). Presumably, a relative independence enabled local leaders to develop power-building strategies and the ability to convert agricultural surplus into forms of wealth. This precarious relationship was implicated in interregional trade and perhaps in the transformative nature of late IA Transjordanian society.[4] As vassals of Assyria, local kings did not have their rule interfered with as long as tribute was paid regularly (Postgate 1992: 251–5), and they were granted the respect of continuing to be referred to as kings (Millard 1992: 37).

Twentieth-century investigations

The results of an 1883 survey conducted by the Palestine Exploration Fund revealed some of the ancient routes in southern Jordan (Kirchener 1884), although archaeological surveys by W.F. Albright in 1924 failed to locate traces of early occupation, deeming them buried under the waters of the Dead Sea (1924–1925: 58). Evidence of ancient routes was confirmed in several other surveys, such as those by Frank (1935), Glueck (1935), Alt (1935), and in descriptions by T.E. Lawrence in his accounts of 1918.[5] After 1970 Rast and Schaub (1974) and then King added to the available data for the southern lowlands.[6]

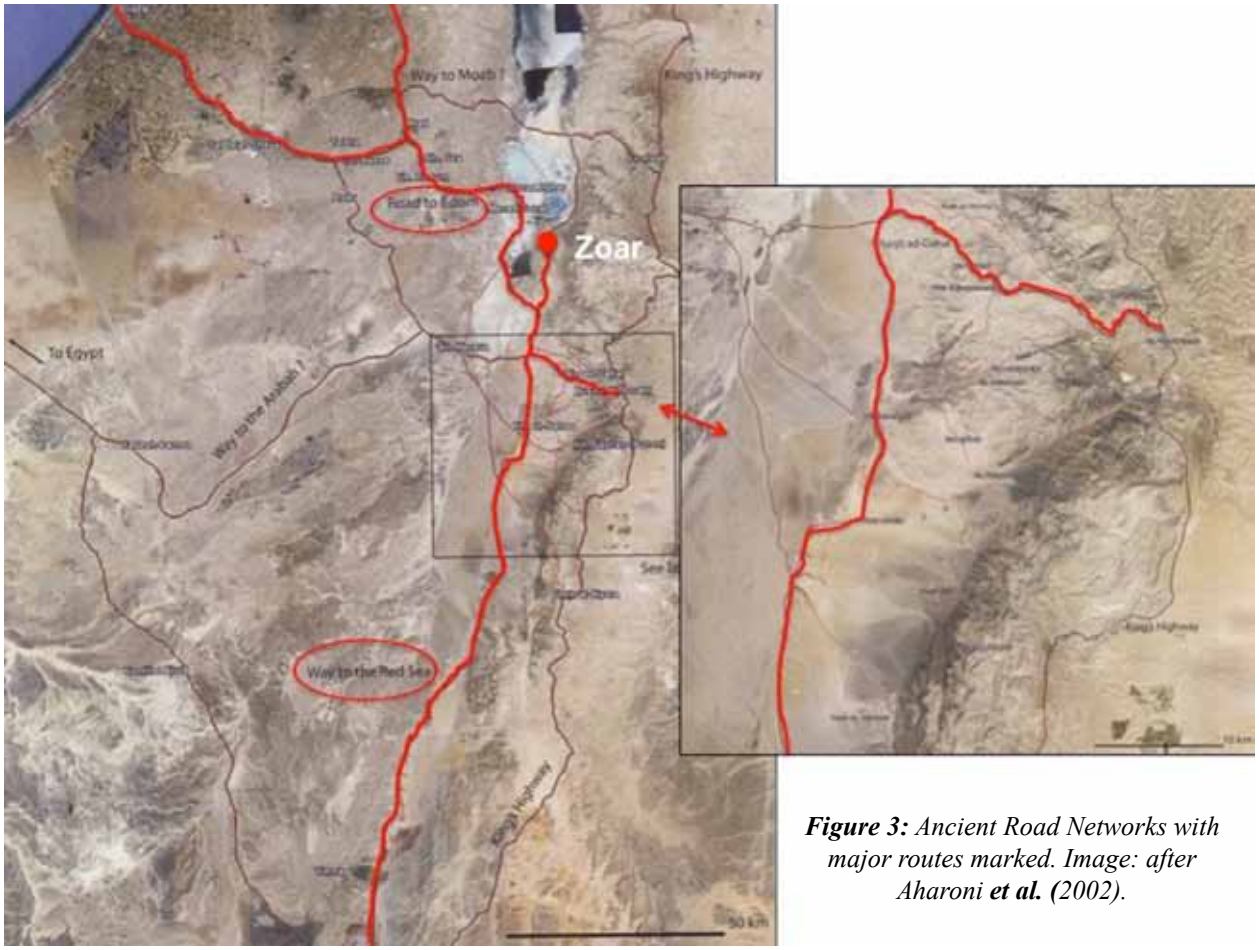

Figure 3: *Ancient Road Networks with major routes marked. Image: after Aharoni* ***et al.*** *(2002).*

Figure 4: *Ghawr es-Safi annotated with sites mentioned in the text and wadi locations. Image: Google Earth, dates 11/9/2012 (bottom left) and 20/10/2004.*

Extensive surveys were conducted by the *Southern Ghors and Northeast 'Araba Archaeological Survey* from 1985–1986 (MacDonald et al. 1988) and more recently, Eretz Ben-Yosef, Mohammad Najjar and Thomas Levy have conducted road surveys from the lowlands to the highlands in the east, identifying more ancient roads (2014). Some of the routes that pass through the wadis are characterised by dense drawings and inscriptions that have accumulated on the rock surfaces over the course of centuries and perhaps millennia. The major easterly route passed through the Wadi al-Dahal to the south of Buseirah, while another crossing was available further to the south through Wadi Fidan, Figure 3. The Ghawr es-Safi Project also mapped a Roman road immediately east of Mousa Hamid along the Wadi Sarmuj leading to the Kerak plateau, Figure 4. Evidence of ancient roads that probably began to flourish during the LBA also exists south through the Arabah Valley. These Arabian trade routes connect Zoar with Elath on the Gulf of Aqaba, known as 'the way of the Red Sea' (Deut. 2:1; Num. 21:4) or, 'the route of the 'Arabah'' (Bartlett 1989: 39). There was also the so-called 'road to Edom', connecting Zoar to the Mediterranean coast; westerly routes from Zoar most likely passed through the Beersheba Valley, which was the most easily traversed valley in the Negev (Singer-Avitz 1999: 7).

Intensive and systematic surveys of the region between Ghawr es-Safi and Wadi Zhaneizir to the south over two seasons between 1985 and 1986 identified early IA sherds in the Wadi al-Hasa region (MacDonald et al. 1988). Around that time, additional archaeological remains were discovered during the installation of underground water canals (Politis 1994: 12–5).

The discovery of the Sanctuary of Lot at Deir 'Ain 'Abata and a Nabataean cemetery at Khirbet Qazone south-west of Bab edh-Drah in the late 1980s and 90s spurred archaeological interest in the region. Then, over a period of twelve years beginning in 1997, the *Ghawr es-Safi Project* under the direction of Konstantinos Politis located and mapped dozens of archaeological sites (Politis 1998b; 2012; 2012a; Politis et al. 2005). This surge in interest was compounded by the wealth of artefacts discovered as the result of illicit excavations, particularly at al-Naq, where the Early Bronze Age and Byzantine cemetery is located (Politis 1994). The Ghawr es-Safi Project proceeded to locate the early Byzantine–medieval Islamic urban centre settlement of Zoara 2 km due west of Mousa Hamid at Khirbet esh-Sheikh 'Isa and its adjacent industrial sugar complex of Masna' es-Sukkar (Tawāhīn es-Sukkar) (Jones et al. 2000). Nonetheless, while Sheikh 'Isa and Tawāhīn es-Sukkar are accepted as being the location of this later period of Zoara, little is truly known about IA Zoar.[7]

The investigation of Zoar

The Mousa Hamid site was first identified in the spring of 1999 during the course of a survey by Politis (Politis 1999). At that time attention was drawn to the abundance of IA pottery scattered over an area of approximately one acre and especially to the large amount of large stone tools, whose abundance was unique for this period in the southern Levant. Approximately 90 stone tools were collected from the surface at this time. The profusion of non-portable querns (some of these *c.* 400 mm in height x *c.* 400 mm in length) and large grinding stones is of particular interest; these tools are not typical features of a habitation site and suggest that this was the location

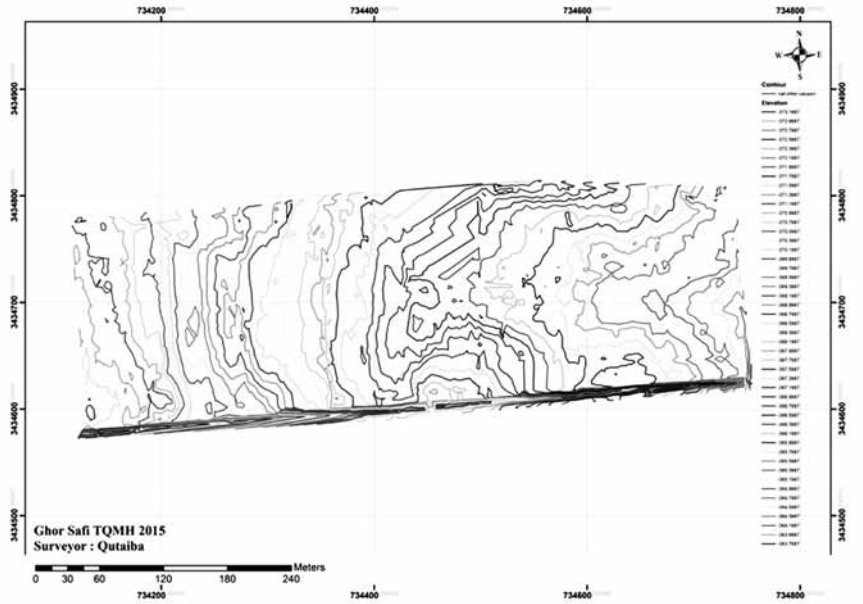

Figure 5: *Contour map of Mousa Hamid (Map: Qutaiber Dasouqi).*

of what might have been an important IA community. That the site is situated near an industrialised sugar zone (Tawāhīn es-Sukkar) indicates that the region was especially fertile, well-watered, and had a history of agricultural processing.

Excavations sponsored by the Hellenic Society of Near Eastern Studies in the spring of 2000 identified architectural remains and IA sherds that were thought to represent two phases of occupation dated to *c.* 900 BCE. The report in ADAJ also noted that the construction of irrigation pits had exposed Nabataean/Roman sherds (Politis 2001: 189). Despite limited publication, most resources now accept modern es-Safi as the location of biblical Zoar, if not specifically at Mousa Hamid (Politis 2012). Nonetheless, for the most part, the discussion about the site's identification has been limited to cursory summaries that mainly focus on the medieval Islamic period and the nearby sugar industry.

The commencement of the recent project developed as the result of a rekindled interest in exploring the IA nature of the site. The lack of robust research at the site served to initiate an intensive survey and excavation in the winter of 2015. Initial investigations involved a detailed topographic and archaeological survey to define the site boundaries, to map the site and to locate potential excavation areas. This survey involved taking points at 5m intervals using Leica Total Station; these points were downloaded into a GIS program and subsequently used to create a contour map of the area, Figure 5.

Figure 6: *View of Tuleilat Qasr Mousa Hamid, looking northwest Note the location of the adobe farmstead that belonged to Mousa Hamid Hashoush. Photo: K. Politis, 1999.*

Figure 7: *View of stone tools on surface at Mousa Hamid, looking north. Photo: J.A. Verduci.*

The determination of the site boundary was hindered by external factors, meaning that the delineation of the area to be surveyed was bounded by roads and private property. As there was a high concentration of surface finds within the boundary, it reaffirmed the assumption that we were focusing on a meaningful area. Surface sherds were especially concentrated in a slightly raised zone of the low-lying tell in the vicinity of the original adobe farmstead, the 'qasr', that belonged to Mousa Hamid Hashoush, Figure 6 (Politis 1999: 543). As this was a short exploratory season, the primary objective was to designate an area for immediate excavation, rather than collect these sherds.

In the course of the survey, the profusion of large stone tools scattered over the site reinforced the interpretation of the site as the location of an important agricultural/ industrial settlement, Figure 7. As there were in excess of 200 objects, only the most impressive saddle querns, grinding stones, and rollers were collected and joined to the assemblage of tools that had been collected from the surface in 1999. As a side project, stone tools from both collection programs were processed and stored at the nearby Safi Museum ('The Museum at the Lowest Place on Earth').

Mousa Hamid is located in an agricultural community with rotating crops. It was possible to negotiate with the landowner and compensate him for the loss of these crops and for access to his land. Having gained the landowner's permission, a decision was made to excavate a single 4 x 4 metre trench (Square 1) in the vicinity of the mound mentioned above, Figure 8. The excavation method sought to determine the stratigraphic relationships between architectural features, installations and debris layers in order to interpret the site formation process.

The trench was initially excavated from the surface to a depth of 2.6 metres. At this level, a 1m x 1m sondage in the north of the trench was excavated a further 1.8m in depth to virgin soil, a sterile layer of sand, in order to examine the depth of stratigraphy. The results demonstrate two main phases of occupation below the disturbed modern plough-based agricultural strata (Temporary (T.) Stratum I). The first phase (T. Stratum II) was associated with a surface, architectural features and installations at 370m below sea level, or 2.6m below the surface. While excavating in the 1 x 1m sondage, an earlier phase of occupation was identified at 371m below sea level or 3.13m below the surface (T. Stratum III). Between the two phases was a layer of windswept sand, which possibly indicates a period of abandonment.

Figure 8: *Surface of Square 1, looking north. Photo: J.A. Verduci.*

Figure 9: *Mudbrick feature. Photo: J.A. Verduci.*

T. Stratum I

Square 1 was located in an area covered by tomato crops and was thus impacted by irrigation channels, root systems and mechanical ploughing. Within this brown silt context were areas of degraded adobe and plastic intrusions. The identification of ploughlines in the sediment meant that it was possible to determine that modern disturbance ceased at approximately half a metre in depth.

T. Stratum II

This silt and degraded mudbrick context was associated with the poorly preserved remnants of mudbrick walls. These were located in the NE and SW of the trench, but were mostly degraded and without footings. The alignment of these bricks implies that they might represent the external corner of a building. In the NE this was represented by a 1 x 1.5m area ranging between 2 to 6 courses of mudbrick, roughly oriented N-S, and in the SW by the partial remains of one course in two rows, Figure 9. These mudbricks are in general 54 cm long, the equivalent of one Egyptian royal cubit, which was the standard brick length in the LBA–IA southern Levant.

Installations in this stratum include the remains of a clay-lined tabun; this feature was clearly identifiable despite being badly degraded and only the lower 10 cm remaining, Figure 10. Preliminary analysis of the ashy silt from inside the tabun, which is being conducted by Mohammed al-Qinna at the Hashemite University's Faculty of Natural Resources and Environment, could identify no organic compounds in the sample, possibly due to the extreme salt conditions in the sediment. Nor was it possible to determine at what temperature it had been burnt, Table 1. Despite occasional slag fragments and some technological ceramic fragments (such as basin fragments), no evidence exists that this was a furnace or was involved in metal production.

The removal of the sediment associated with mudbrick collapse revealed evidence of destruction caused by a major burning episode related to the only identifiable surface in the trench. This burning swept across the floor,

Figure 10: *Tabun locus 017. Photo: J.A. Verduci.*

tabun and mudbrick features, as is clearly visible in the baulks. The cause of this destruction is unknown, and testing by Al-Qinna has produced similarly inconclusive results as those for the tabun.

Property	Unit	Value
EC25 (electrical conductivity)	(mS/cm)	20.7
pH		6.74
Water Content (θg)	(%)	3.9
Bulk density	(g/cm^3)	0.98
Organic Matter	(%)	15.12%
$CaCO_3$	(%)	22.8
Total Dissolved Salts	(ppm)	13,269.50
Sand	(%)	36.91
Silt	(%)	40.23
Clay	(%)	22.86
Texture		Loam

Table 1: *Tabun soil analysis results*

T. Stratum III

A second phase of occupation was less discernible, but also yielded significant numbers of stone implements (mainly pestles) and pottery. No surface or architectural features could be identified and no significant changes in sediment were noticed other than a soil colour change from greyish brown to yellowish beige. Given the difficulty in distinguishing between strata, the likeliest explanation is that we may have excavated part of an outdoor courtyard with industrial accumulations of large

Figure 11: *Final stage of excavation with completed sounding . Photo: J.A. Verduci*

quantities of debris. The site formation is very interesting; however, it should be noted that at sites such as Nimrin, just 12km north of the Dead Sea, there were three meters of IA II occupation overlying the 10th century stratum (McCreery 1993: 268). Factors such as windswept sands and flooding in the alluvial fan can create deep layers of fill, as can extended periods of accumulated refuse, Figure 11.

Figure 12: *(a) Ceramic horse head figurine, and (b) engraved ceramic stamp seal. Photos: J.A. Verduci.*

Associated finds

The square yielded a large number of lithics (mainly retouched flint flakes), glass, slag, shells, a red stone scarab seal with faintly incised markings, as well as an 11th century AD coin in disturbed Stratum I. There was also an assortment of jewellery items in the manner of a bone hairpin fragment; stone, faience, and shell beads; and copper alloy bangle fragments, a lunate earring fragment, and a triangular fibula bow fragment. This last object was perhaps the most significant item of jewellery, having clear associations with high-status adornment practices (Verduci (forthcoming)). Other finds included ceramic tripod or incense burner legs and potter's marks. In addition, there is a ceramic horse head figurine with parallels at Tawilan (Bienkowski 1995); our figure had raised lines delineating a bridle as is commonly identified on other horse figurines, Figure 12 (Dornemann 1983: fig. 86.9). Amongst the finds is also an oblique impressed plaque a figure in profile that is preserved only from the knees down, but whose sandals, robe, and staff can be identified; this object may have been used to impress pottery, as occurs on examples found at Tell Nimrin and other sites in Cis- and Transjordan (Millard 2005).

Forthcoming analysis on the animal bones by Louise Martin, University College London, should shed light on the dietary practices of the late IA Edomites. This analysis will identify if the predominate remains belong to domesticated sheep and goats, as is common at other southern Levantine sites. In addition, we hope to confirm the tentative identification of donkey bones; the presence of donkey bones at Mousa Hamid might be linked to its importance as a pack animal, perhaps used to transport goods along the trade routes discussed earlier.

Figure 13: *Iron Age IIC decorated chalice. Note the petal décor. Photo: J.A. Verduci.*

One of the most interesting observations regarding the finds from the current excavation concerns the large ceramic assemblage. The recent excavations at Mousa Hamid yielded approximately 400 kg of pottery from the one trench, with about 2,880 indicative sherds consisting of rims, handles, bases, and miscellaneous distinctive vessel sherds, along with a limited repertoire of decorative wares. The most common decorative treatment was grooving on the exterior of vessels that is covered by a pale slip. A small sample of painted wares means Mousa Hamid can now join the few other Edomite sites in southern Jordan that have produced painted pottery, such as 'Umm el-Biyara and Tell el-Ghrareh, and Tawilan and Buseirah where they were more common.[8]

The finds include a high-proportion of Edomite-type vessels dated to the IA IIC throughout all loci, that is, from the seventh–sixth century BCE and perhaps even as early as the eighth century on comparison to Edomite pottery found in Judah at Beer Sheba and Tel 'Ira (Thareani 2010 and Singer-Avitz 2014). The assemblage also contains types found at several sites in Israel's Negev, such as Horvat Qitmit (Smith and Levy 2008: 42). These traditions are also felt nearby at Ras al-Miyah (Ben-Yosef 2010: 385) and into the eastern Highlands at sites such as Khirbet al-Iraq Shmaliya and Tawilan (Smith 2009). Most forms are characteristic of late IA II in that they are course cooking and storage vessels of globular and heavy form, often with handles near the rim.

There are extremely few complete vessels, which is comparable to the ceramic assemblage from Khirbet en-Nahas, another industrial lowlands site in Edom, the date of which is the subject of some disagreement (Smith and Levy 2008: 53; Finkelstein and Singer-Avitz 2009). It is unclear if the lack of complete vessels is the result of agricultural/industrial activity as proposed for Khirbet en-Nahas. There were none of the expected whole vessels associated with the floor and destruction layer in Square 1, rather, the sherds appear to be due to accumulated waste and fill deposits.

As noted above, very few whole vessels were discovered within the square other than perhaps what is arguably the most important ceramic object. While excavating below the disturbed upper strata, a relatively intact chalice was uncovered. The chalice's most outstanding feature is its petal and applied décor with traces of yellow and red paint, Figure 13. The petal is a popular motif used on a wide variety of ancient artefacts, particularly within the IA IIC collection from the Edomite shrine at 'Ain Hazeva (Cohen and Yisrael 1995a; 1995b). The more triangular denticulated fringe was a characteristic feature of pottery at many Edomite sites in Transjordan and in the Negev, such as Wadi al-Thamad, Horvat Qitmit, 'Aroer, and Tel Malahata (Beit-Arieh 1995: 253; Tebes 2006). As the Mousa Hamid chalice most likely relates to religious beliefs or ritual activity, using Bienkowski's model (2009), it suggests the site was a meeting place for groups from various locations.

In addition to the large amount of ceramic sherds, an extensive range of utilitarian stone implements indicates that extensive agricultural and industrial activity occurred at this site. Types include pecking stones, pounders, hand grinders, large flint flakes and cores and large tools in the form of grinding bases, loaf-shaped millstones, rollers, mortars and large saddle querns. Many grinding bases and tops are limestone, while querns are generally made of hard dense basalt, Figure 14.

Figure 14: *Surface finds from Mousa Hamid at the Safi Museum. Photo: J.A. Verduci.*

Large saddle querns, grinding bases and tops are known in domestic settings for the preparation of food, for example in Room 2 at Iraq Shmaliya to the southeast of Mousa Hamid (Smith et al. 2014: 272). However, the scale of production that would have occurred at Mousa Hamid is unprecedented in the region. At other sites, excavators have identified a number of industrial activities; in addition to the well-known copper production at Feynan, are (for example) iron smelting and processing at Beth Shemesh and Tell Hammeh (Veldhuijzen and Rehren 2007), dyeing and tanning at Timnah (Kelm and Mazar 1991), oil pressing at Beth Shemesh and Ekron (Bunimovitz and Lederman 2009: 120; Gitin 1990), wineries at Gibeon (Pritchard 1960; 2012), Ashkelon and Joffa (Stager et al. 2008b: 275, 279; Fantalkin 2005), and weaving at Deir ‘Alla (Van der Kooij and Ibrahim 1989). The intensification of agricultural production for export is particularly paralleled in olive oil production and viticulture (Herr 1995; Gitin 1997; McGovern and Harbottle 1997: 145; Routledge 1996; Walsh 2000). It is unclear if the tools at Mousa Hamid were used for agricultural processing on an industrial scale or in other specialised activities, such as the extraction of minerals. The identification of a small-scale perfume industry at En Gedi that utilised the basalm trees that grew in the Dead Sea environs suggests that alternative uses for the stone tools should be considered (Herr 1997: 158). Particularly as Eusebius and Jerome note that other that at En Gedi, the balsam grew at Zoar (*Onomastican* 42, 86).

Due to the multifunctional nature of tools found at Mousa Hamid, the study of objects in their archaeological context is essential in determining their function. In future, analyses such as petrographic and morphological studies might assess mechanical capabilities, as well as residue analysis and experimentation to assist in determining the function of these stone tools; these kinds of tests have identified mineral extraction in IA Spain. One of the challenges of commencing a new project is the development of a network of resources and contacts. Having laid the groundwork for future excavations at Mousa Hamid, we do hope to implement some of these tests and analyses.

Conclusions

Many problems remain to be resolved. It is possible that similarities with Horvat Qitmit and ‘Ain Hazeva, whose pottery is also placed in the seventh to sixth centuries, are linked to Assyrian trade networks in the late IA II via their Edomite territory along the course of the road to Edom (Finkelstein 1992). To date, there is only definitive evidence of Assyrian influence at highland sites during the IA IIC at Buseirah, Umm al-Biyara, and Tawilan. Whether the scale of industry at Mousa Hamid is somehow linked to this Assyrian influence remains to be seen, although it is reasonable to suppose that for settlements such as Zoar, it would have been an opportunity in long-distance exchange not to be missed.

The dry climatic and environmental conditions of the IA required intensive strategies of farming and processing (Bienkowski 2009). In the successful mobilisation of a community in the environs of the Wadi al-Hasa alluvial fan, we see the outcome of social organisation and structured productivity that can be perceived as expanding well beyond any recognition of a decentralised polity and the identification of *segmentation* or *tribal kingdom* (Routledge 2004: 56 Bienkowski & van der Steen 2001). The segmentation model has been applied to Edom by Benjamin Porter as an alternative to the tribal kingdom framework (Porter 2004: 386). In this, he attempts to demonstrate that Edom was a centralised, hierarchical polity with Buseirah as the imperial capital. Criticisms by Bienkowski of both Routledge’s framework, developed for Moab, and Porter’s application of this framework for Iron Age Edom, question the ability of elites to organize tribal alliances.[9]

The evidence for production on an industrial scale, the co-concurrence of a ritual chalice with precious commodities commonly reserved for elite use, such as, personal adornments in the manner of fibulae and administrative seals, is particularly conspicuous and demands explanation. It is reasonable to suggest their appearance would warrant some manner of defence from external threats, as is seen for the defence of Gibeon’s wine industry. Old satellite images of Mousa Hamid reveal a faint mound and what appears to be a roughly rectangular outer perimeter; it may be that some type of fortification wall partially surrounds the site.

Our research of developments in the IA II Edomite lowlands may shed light on the biblical references to Edom and Philistia on the coastal plain as being partners in trade (Amos 1-2), given the path between the two had to pass between either ‘Ain Hazeva or ‘Aroer, and also whether there is any stylistic connection to Arabia. The aim is to distinguish between what might simply be the intersection of various IA regional assemblages and any identification as a southern transit route. Moreover, this research might provide exciting evidence of IA food production and supplies for caravanserai passing through southern Jordan that are poorly lacking in the archaeological record.

In summary, the recent investigation and excavation at Tuleilat Qasr Mousa Hamid suggests the site was an important agricultural and industrial settlement in the southern Jordan Valley that dates to the terminal IA II, a period for which limited evidence has been discovered in the region. The extensive material uncovered at the site, and especially the unusual nature of the stone assemblage warrants further study.

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Endnotes

1 Mentioned by both Heirocles (*Syneedemus*) and George of Cyprus (*Description of the Roman World*).

2 he term *state* is used without wishing to enter into a debate on the definition of what a *state* might constitute, rather the question of *kingship* or other forms of leadership are irrelevant to an acceptance of the general concept of a loosely organised institution of power. For discussion and references, see Routledge 2004: 14–26, 138–141.

3 For a regional overview of IA states between Anatolia and Egypt, refer to Kurht 1995: 385–472. On the Egyptian documentary record, refer to Gaballa 1976: 108–112.

4 For a brief critique of the *state* as transformative, refer to Bienkowski 2009: 14.

5 Correspondence and works by T. E. Lawrence are available to download at http://www.telstudies.org/writings/contents_lists/years/1917_1918.shtml.

6 See also surveys by Tristram in 1873, Philby in 1925, Abel in 1938: for references, refer to Politis 1998a.

7 Scholarly debates on the location of Zoar are all carefully summarised by Le Strange (1890) and MacDonald (1982).

8 By the IA II, painted treatment on most vessels had been replaced first by hand-burnishing and later by wheel-burnishing: refer to Whiting 2002: 76, 220.

9 Porter's suggestion that the prestige objects found within Edom were redistributed by elites to loyal subjects to foster alliances is questioned by Bienkowski, who notes that the amount of such luxury items was surprisingly small: Bienkowski 2009: 13. Although maintaining the effectiveness of the tribal model, Bienkowski offers a redefinition of the word 'tribe',that focuses on the shifting nature of relationships as opposed to fixed genealogies. Cf. Younker 2003:153–176.

Dionysia or Dionysias at Kourion, Cyprus

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Abstract: An inscribed stone was discovered during the Kourion Urban Space Project 2015 season. The paper discusses its discovery and the inscription itself, which is now in the Episkopi Museum, Cyprus.

On 6 June 2015, while conducting topographic survey for the Kourion Urban Space Project (KUSP), Benjamin Ioset located an inscription in the Krommya Valley, east of the Kourion acropolis. The block was lying in a wadi bed about 190m east of the south-eastern cliffs of the acropolis, Figure 1. The location of the site was Easting 490,071.53, Northing 3,836,036.67, UTM WGS 84 Sheet 36N at an elevation of 34.11m above sea level.

The inscribed stone was located at the base of a slope rising 80m to the north-east of the wadi, Figure 2. Several other worked stones were in the immediate vicinity of the inscribed stone. All were situated along the base of the slope down which they had no doubt tumbled.

A number of tombs were located near the crest of the slope above the inscribed stone, Site 'E' of the British Museum 1895 excavations. These were cleared without accurate plans being made. It is possible that the inscribed stone was once associated with these tombs.

Figure 1: *The find spot of the inscription viewed from the north-west. The Kourion acropolis is to the right and the village of Episkopi is in the distance to the left. Image: C.J.Davey*

The stone is made of limestone. Its surface is rough, but the part of the face that carries the inscription was dressed, albeit rather poorly, possibly to make the text more legible. Some chisel marks were evident, but the inscription itself has a U-section and was probably made by abrasion.

The base of the stone was partly cut away indicating that it had a structural purpose at some stage. Part of the bottom edge was also bevelled. A brief inspection of the slope revealed that there had been a number of wall structures above the find spot. However, the area is outside the KUSP excavation concession, and as no other inscriptions were evident, the investigation was taken no further.

Figure 2: *View of the inscription find spot looking east from the Kourion acropolis. Tombs located near the top of the rise above the inscribed block were cleared in the nineteenth-century. Image: C.J.Davey*

The stone was found lying on its right side and partly embedded in the silt of the wadi, Figure 3. The inscribed face was uppermost and facing the wadi, but the inscription would have only been evident to the casual observer when light conditions were conducive. The area was also heavily infested with thistles. The stone could have been in its current location for many years. The last survey of the area was undertaken by the Kourion Mapping Project in about 2005.

On 8 June the inscription was further documented *in situ* and, with the permission of the Cyprus Department of Antiquities, removed for cleaning and further examination. It has been lodged with the Episkopi Museum where it was registered as RR 1503.

Figure 3: *The inscription in situ, view looking east. The stone is on its right side so that the inscription runs from top to bottom Image: C.J.Davey*

Figure 4: *A drawing of the inscription. The edges of the letters were detected by feel and traced onto a clear plastic overlay.*

Figure 5: *An image of the inscription enhanced using* Image J *software. Image: David Saunders.*

Inscription

Dimensions (in millimetres) are as follows: 450h (right; 344 on left due to the cutting away of the lower left corner) x 535w (at top; 355 at base, due to the cutaway of part of the stone) x 281d (top, widening out to 383 at bottom right). The dressed portion of the face extends down from the top approximately 200mm, though slightly more on the right edge.

The lunate letters vary considerably in size, from 48mm for N to 67mm for Δ. The space between varies, too, but can be as much as 14mm. The single line of text proceeds slightly upwards towards the right. No squeeze was made of the lettering, but a drawing was made in addition to photographs using different filters, Figures 4 & 5.

The text reads ΔIONYÇIAÇ.

The fourth last letter appears to be *epsilon* carved by mistake for lunate *sigma*. The final letter is barely visible in the photo, Figure 5, but the drawing reflects the impression felt by the fingers of the discoverers.

The amount of vacant space on the stone above the lettering suggests that there was no additional stone with an earlier portion of text below which this one was placed. Accordingly, we have as the entire text either the genitive of the female name Διονυσία, or the nominative of the male name Διονυσιάς. Either option would suit a laconic gravestone: '(tomb) of Dionysia', or 'Dionysias (lies here).' The crudity of the carving is suggestive of a graffito, or at best a very roughly done job by a mason. The latter is perhaps to be inferred from the evidence of the chisel marks on the roughly smoothed surface on the part of the stone where the inscription was carved.

Few instances of Dionysia are attested on Cyprus: three in *LGPN* 1.136. None are attested from the island for the male form Dionysias. These and related forms of the masculine (Dionysas, Dionnysas, Dionys, Dionnys, Dionysias, and the ubiquitous Dionysos) occur in varying degrees of frequency eastwards from the Italian peninsula (some of these in Latin script: twice each at Puteoli and Misenum, *LGPN* 3.128) and Sicily. Checks in *SEG* do not alter the picture for this Dion- name group, except numerically to add more attestations.

Mitford (1971) records two Dionysios inscriptions, one from the Temple of Apollo (no. 86) and the other from the Basilica (no.92). Two inscriptions (nos. 147 & 155) come from the ruined chapel, Panagia Chrysanaiotissa, near Site 'E'. None relate to this inscription in content or style.

Without a dateable archaeological context for this inscription, we must rely on the letter shapes and the quality of the carving. We suggest that it belongs in the Imperial period, probably at least AD III, though a later century would be no surprise. The crude quality and laconic nature of the majority of the *angelos* epitaphs from Thera (Horsley, Luxford 2016 forthcoming) come to mind as an analogy, though we are not implying that this inscription from Kourion is also a Christian epitaph as they certainly are.

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Abbreviations

LGPN – P.M. Fraser, E. Matthews (eds.), *Lexicon of Greek Personal Names*. Vol. 1, *Aegean Islands, Cyprus, Cyrenaica*. Oxford, Clarendon Press, 1987. [Subsequent volumes of LGPN (five vols in seven) cover other regions in the Mediterranean, including so far (to vol. 5B, 2013) Attica, rest of Greece, Italy and Sicily, Macedonia with Thrace and the northern Black Sea, Asia Minor (in progress).]

SEG – Supplementum Epigraphicum Graecum

Reference

Horsley, G.H.R. & J.M. Luxford 2016 forthcoming Pagan angels in Roman Asia Minor: revisiting the epigraphic evidence, *Anatolian Studies* vol. 66.

Mitford, T.B. 1971 *The Inscriptions of Kourion*, Philadelphia: American Philosophical Society.

A Person of Interest: Gordon Childe and MI5

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Abstract: Vere Gordon Childe is widely regarded as the single most influential thinker in the history of archaeology. He is almost certainly the most prolific author and widely published scholar that the discipline has seen. Yet, his fiercely independent original thinking and character did not come without cost to him. This paper begins with a brief introduction to Childe the man and academic. It then utilises recently released MI5 files to sketch new insights into Childe's life and the different manner in which he was regarded by British and Australian Intelligence; the level of MI5 monitoring of Childe was intense. This paper draws attention to the scope that these intelligence files provides for further and more detailed biographical studies of Childe than was possible in this preliminary outline.

Childe the International Figure

Vere Gordon Childe (1892-1957) came from Blackheath in the Australian Blue Mountains. There are few if any figures in academe whose vast fame and influence are as evenly matched by their enigma and mystery as Childe, and new sources still reveal unexpected sides to the man. Childe was an assiduous correspondent and it is acknowledged that new aspects of him will become known as items from that correspondence come to light.

This paper briefly examines relatively unexplored sources of information on Childe. These are the files kept on Childe by MI5, the British Military Intelligence and security agency, through almost the entirety of Childe's adult life. For those less than familiar with Childe as an individual, scholar, political agitator and thinker, a sketch of Childe is provided below to better contextualise both the significance of intelligence interest in him and the likely impacts of such ongoing surveillance on Childe. It emerges that Childe was probably well aware of his ongoing monitoring and it seems highly likely, as a result of such ongoing intelligence interference in his daily activities, that the flavour and quality of his life differed significantly from the image of him that has prevailed to date.

Figure 1: *Childe was the first director of the Institute of Archaeology, London, a position that placed him at the centre of British academia and society. He is seen here with his staff outside St John's Lodge in 1955, L-R, Front Row: Maurice Cookson, Kathleen Kenyon, Sheppard Frere, Max Mallowan, Gordon Childe, Frederick Zeuner, Edward Pyddoke, Joan du Plat Taylor, Ione Gedye, Middle Row: Marjorie Conlon, Rachel Maxwell-Hyslop, Arthur Simon, Ian Cornwall, Geraldine Talbot, Olive Starkey, Back Row: Mr & Mrs Dance, Mary Pinsett, Jennifer Banham, Penny Brooks, Joan Sheldon, Judy Phillips, Marjorie Maitland Howard, Harry Stewart. Image: courtesy of UCL Institute of Archaeology Collections.*

Firstly, who is Childe to the archaeological world and academic sphere generally? Thirty five years after Childe's death, Bruce Trigger, the preeminent historian of archaeology, depicted Childe as still 'the most renowned and widely read archaeologist of the 20th century' (Trigger, 1980: 9), while more recently, Childe has been described as 'perhaps the best known archaeologist of all time' (Diaz-Andreu 2009: 7). Childe's work, '*What Happened in History'* (Childe, 1942) has been translated into more languages, and read more widely than almost certainly any other archaeological work (Gathercole et al. 2009). Childe's output of scholarship was prodigious. During the years between 1923 and his death in 1957, he authored 517 articles, chapters and reviews, and 28 books. This does not include his considerable reworking and revision of new editions of his works and his translations of foreign-language books into English. While opinions vary on the integrity or ongoing relevance of Childe's work (Murray 1995, Trigger 1994), his genius and historical popularity appears beyond debate.

It is demonstrative of the renown and esteem Childe rapidly achieved, that when Harvard University invited the peak figure worldwide of each discipline it taught, to deliver a guest oration for its tercentenary in 1936 it was Childe who was chosen to speak for the field of prehistory and archaeology (Childe 1937). It is an indication of Childe's brilliance that this address to Harvard in the role of preeminent international prehistorian took place only eleven years after the publication of his first archaeological work (Childe 1925), and only nine years after commencing his first full-time academic position, at Edinburgh University (Green 1981: 56).

Yet, despite this fame, despite his gregarious nature among students and peers, we know almost nothing of Childe himself. Childe never married, nor was he ever publicly romantically involved. Certainly, there have been biographies and works with biographical content (Green 1981, Harris 1994, McNairn 1980, Trigger 1980), all of which suffer from the same predicament, namely, that on retirement Childe destroyed most of his correspondence (Trigger 1994: 10). We may know of Childe's public statements and writings, and may have been able to capture a few personal vignettes from the recollections of his colleagues and students and from their correspondence with Childe. Nevertheless, Childe's surviving writings seem largely guarded about his inner beliefs and his colleagues and students are for the most part long dead. Correspondence by Childe to wider circles of scholars has also sometimes been preserved. However the published studies of this correspondence do not indicate that it generally contains any depth of personal communication (Diaz-Andreu 2009a, Irving 1995).

A Brief Homecoming

Childe returned to Australia in April 1957 after a prominent academic career in England and Scotland. He spent some months travelling in Australia receiving an honorary doctorate from the University of Sydney, visiting remnant family in Queensland and guest lecturing for John Mulvaney in Melbourne. In October 1957, despite his bitter plaints that Australia was a cultural desert (Green 1981: 147), Childe seemed engaged in the minor enjoyable pleasures of a retired academic visiting colleagues here and there, and hiking in the Blue Mountains with what he termed 'enormous zest, satisfying my youthful craving' (Green 1981: 152). He appeared sociable and engaged with life, enjoying companionable walks with Basil Hennessy of Sydney University and lengthy late-night discussions on archaeology with Laila Haglund (Powell 2013: 173). On 19 October 1957, Childe did not return from a hike to the Bridal Falls at Govett's Leap in the Blue Mountains. When his spectacles and compass were found on his neatly folded coat next to a precipitous ledge, it was widely assumed, or at least publicly stated, in similar terms to those used in the *Daily Worker*,

> *The professor was extremely shortsighted and probably missed his footing when he went to Govett's Leap, a 1,000-ft cliff (Dutt 1957).*

Eve Stewart, who with her husband Jim had at times hosted Childe in the Blue Mountains, could not reconcile Childe's cheerful disposition with suicide, neither could Hennessey or Haglund. Jim Stewart went as far as to entertain notions of political assassination (Powell 2013: 174).

No suicide note was found, but only due to Childe's foresight. His quite moving and explicit suicide note was already in the mail and on its way to W. F. Grimes, his successor as director at the Institute of Archaeology in London, with instructions not to open the note until January 1968 (Green 1981: 152). Grimes may well have previously been warned by Childe of his intentions,

> *Childe vacated the chair early to allow his successor to oversee the move of the institute to Gordon Square; he had in fact revealed to Grimes his intention to commit suicide (Gathercole 2004).*

Grimes was not the sole recipient of such forebodings. In February 1957 Childe travelled to visit Celia Topp, a former student then resident in Gibraltar. Topp received a letter from a mutual acquaintance warning of Childe's low mood and that Childe had recently stated that once in Australia *he would in all probability throw himself over some convenient cliff* (Green 1981: 145).

Despite the public pronouncements of accidental death, suspicion remained as expressed in an entry to Childe's ASIO file cited at length later in this article (ASIO 22 October 1957). Perhaps the social stigma of suicide was such that at the time of Childe's (then postulated) suicide in 1957, people close to Childe did not wish to speak of him personally on the record, resulting in the relatively small collection of material about Childe, the man.

This reticence could also be reflected in the delay in opening the suicide note that Childe had left. His instructions were that it be opened ten years after his death. This

note remained unopened for over 25 years and was first published in 1980 (Childe 1980 (1957) in Daniel, 1980). It confirmed Childe's fall and death to have been intentional, because of his self-appraisal of being beyond his most productive years and also founded on his strongly worded abhorrence of becoming an invalid, lonely and a burden on society. Childe is explicit in this letter that he found the predominant social prejudice against suicide to be unjustifiable and irrational and insisted on his right to end his life when and where he saw fit, '*Life ends best when one is happy and strong'* (Childe 1980).

A Man of Many Parts

As indicated in the events described above, Childe's capacity for compartmentalisation should not be underestimated. His academic and philosophical thinking has been described as incorporating strong contradictions (Murray 1995) and in his personal life too it would seem he was quite capable of operating on multiple levels simultaneously. It appears clear that his peers and acquaintances in Australia, who met him shortly before his death, had no inkling of his intent to suicide. This impression was likely intentionally fostered by Childe. His final letter makes it quite plain that he recognised the socially unacceptable nature of his intended suicide and that he had no intention of causing ruction or upset to those he left behind by making a public spectacle of his death. If such was Childe's ability to moderate his behaviour and spring unpleasant surprises on his inner circles, then the considerable delay in posthumously opening his suicide note is perhaps also reflective of a reticence to let loose the problems that its contents could potentially have strewn over the archaeological world. Childe was quite the wildcard.

New information on Childe is unlikely to come from his closer acquaintances, those who knew Childe well have mostly left this mortal coil. However, new sources have come to light which provide fresh perspectives on Childe as an individual. These sources reflect upon just how little known and poorly understood Childe was to his colleagues, acquaintances and the wider world. They reflect on the disparity in image between Childe the scholar as regarded by his peers and Childe the political figure as perceived by British and Australian Intelligence. Ironically, the disparity among even his contemporary scholars in understanding Childe the individual, comes most obviously to the fore in works written in his tribute.

For, in a variety of published tributes, extracts from which are provided below, individuals who worked with Childe on a regular, even daily basis have expressed widely varying opinions on Childe's deepest held political and general philosophical worldviews. His acquaintances expressed puzzlement,

> *The great puzzle of Childe at all times was to what extent he was a Marxist (or a Marrist) and to what extent he paid lip-service to an Outsider philosophy (Daniel 1958: 66).*

The puzzle was not so much whether Childe stood by his intellectual utilisation of Marxist concepts in his analysis of prehistory, but whether Childe the individual, saw himself as a Marxist in matters of current politics and personal philosophy. As would be expected of an analytical thinker, Childe's adherence to any one philosophical bent in academe or life, varied through time. Yet whether contemporaneously or in retrospect, Childe's peers seemed uncertain as to his personal philosophical position at any given point.

This uncertainty was reinforced by Childe's highly individual sense of humour which extended to practical joking and punning and a willingness to pose playfully for the camera (Figure 2).

Childe was not beyond playing calculated and protracted practical pranks, even on his benefactors such as Wheeler (Kilbride-Jones 1994: 138). Childe delivered his final lecture at the Institute of Archaeology dressed in a Central Asian gown and hat and carrying an Australian Aboriginal spear. For some time Childe occupied apparently less than optimal dwellings in an apartment block, the name of which punned pleasingly to him, The Hotel de Vere in Edinburgh, and while visiting London from

Figure 2: *Childe at Skara Brae, grinningly posing with rock in hand. Image: courtesy of UCL Institute of Archaeology Collections.*

Edinburgh would stay at the Moscow Mansions (Green 1981: 73-74). During war rationing Childe often carried a sugar tin with 'Childeish sugar' inscribed on the top, and was wont to refer to himself to friends as 'Childeish' (Lyndsay 1981: xv).

This impish behaviour likely lay behind Daniel's pondering whether Childe's actions, such as public complaint at hotels over not having been delivered the *Daily Worker* (a communist newspaper) to read, was simply 'a pose' (Daniel 1958). Childe had long insisted that the *Daily Worker* be delivered to his university office where it would take pride of place on his desk (Piggot 1958: 308). Whether this was 'a pose' as Daniel puts it, or perhaps 'stirring' as the Australian Childe may have termed it cannot be determined. Childe certainly was for instance, wont frequently and publicly to make proclamations regarding 'Comrade Stalin', often in gleeful tones and beaming with delight at the likely effect of such comments on his often very conservative audiences, who he knew full well were constrained by their conservative etiquette from open critique of such statements by an esteemed academic.

Given this proclivity to (apparently good-natured) humorous obfuscation and teasing, it is understandable that those who knew Childe expressed a variety of opinions on what his personal philosophical positions were. These opinions ranged from Daniel's assertion that Childe was a conservative who merely used Marxism and Communism as a goad to humorously jibe his conservative colleagues, or that any Marxist excesses were likely a feature of Childe's 'exhibitionist' wit (Kilbride-Jones 1994: 136), to the assertion that Childe was a committed Marxist (Gathercole 2004, Renfrew 1994: 121, McNairn 1980:3). Of course one must bear in mind the desire by both conservative and progressively-minded archaeologists retrospectively to cast Childe in their own image and thus claim a Childean intellectual inheritance. Nevertheless, even accounting for this tendency, real division was still apparent.

If those who knew Childe well could form such divergent opinions on who Childe was as a political theorist - while being aware of the centrality of political theory to Childe as a person, then we have little hope of reconstructing the man as a whole some 58 years after his death.

MI5, ASIO and Childe – a Longstanding Relationship

The following will not try for a reconstruction of Childe; it merely presents a selection of excerpts from the files that British Intelligence kept on Childe during his time in the United Kingdom. These have been recently released (in part) to the public. A full analysis of these files would require a book-length treatment, well beyond the scope of this article. What I offer below is some insight into the manner in which Childe was perceived and surveilled by the intelligence agencies of the time and the manner in which this may have impacted on Childe as an individual.

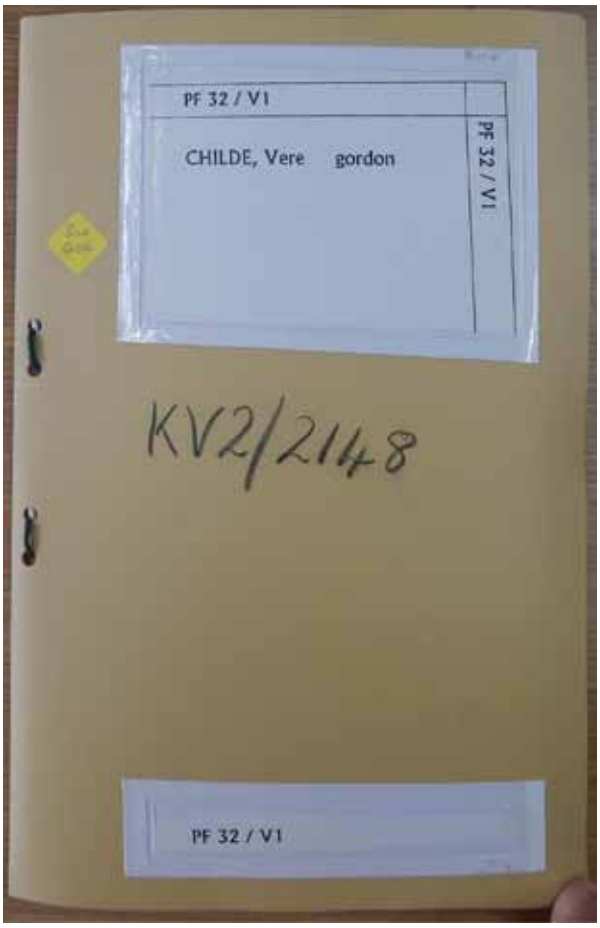

Figure 3: *The earlier MI5 file.*
Image: Australian Institute of Archaeology.

The existence and content of Australian Intelligence/ASIO files on Childe has long been known. John Mulvaney (Mulvaney 1992) provided some extracts from them in his tribute to Childe. The ASIO file regarding Childe on his final return to Australia is ASIO file [279] A6126/25. This comprises 32 pages, dominated by newspaper clippings from the Australian press in 1957, relating to Childe's death. Childe is repeatedly misnamed in the ASIO file as 'Professor Victor Childe'. It seems certain (as documented in correspondence with MI5), that ASIO paid little attention to Childe, or to his purported danger to society and would not have opened formal investigations into Childe had not an influential private citizen, Mr George Boss, taken action.

On 16 April 1957, the Australian Prime Minister's office received a handwritten note from Mr George Boss, J.P., of the Camperdown Hotel, advising of the return to Australia of 'a most disloyal British citizen', Gordon Childe.

Boss' letter appears to have thrown Australian Intelligence into activity, patching together a few pages of point-form information on 'Victor' Childe, including a typed transcription of Childe's listing in *Who is Who in Australia*! By August 1957 this had progressed to the point that the

file included a single-page but generally accurate chronology of Childe's life. This ASIO file concluded that while no evidence was to hand that Childe was a card-carrying member of the Communist Party, no doubt remained as to his effective membership and activism within it. This finding did not generate any further action by ASIO.

The British Intelligence files on Childe are far more substantial than those of Australian Intelligence, but are by no means comprehensive. They have been previously analysed to a limited extent (Champion 2009), with a focus on events prior to Childe's departure from England and return to Australia in 1917.

Childe's MI5 records, or at least those made available to the public comprise two buff folders, perforated and bound with rough green twine.

The earlier of these two files is file KV2/2148 (PF32/V1) 'Childe, Vere gordon' (sic). There are 105 items in the MI5 file KV2/2148, spanning 42 years, with a three year break from August 1919 to September 1922. It comprises 155 pages of A4 photocopies of a variety of documents, including photocopies of copies made through photography, some of which are effectively illegible. The index pages, particularly for the earlier years, are less than perfectly coherent with the file contents and in several cases items listed in the index are not present in the file. It is therefore less than optimal to refer to items by their file index number, which is in any case of little use to the reader without the file as reference. Items in the file are referred to here by their first displayed date and some other descriptor such as author (where available) and content. Many items bear several hand-scrawled dates, which frequently seem to include (generally without explanation), date of transmission, receipt, first and subsequent actions, and closure. Hopefully, by consistently using the first date present, a coherent chronology can be constructed.

The chronological first item in file KV2/2148 was entered on 13 May 1917, and the last item is dated to 1 February 1952. In general, after the 1930's, handwritten items have been transcribed into typewritten text, with both a copy of the original handwritten item and the typed transcript included. This tends artificially to bulk out the size of the file. The file commences with an access register of signatures, accompanied by rubber-stamped dates of request. This access register indicates that this file was accessed 21 times from 1965 onwards. Legible date-stamps associated with subsequent requisitions are dated up to 1971. Following the access register, a register of entries lists the date that material was incorporated to the file, along with a very brief description of the nature of new data. As noted above this register appears to be inaccurate. The end of the register of entries is stamped 'File Closed'. This seems to refer to closure of this physical file rather than the ongoing investigation on Childe, which continued without break, as documented in the following file.

The later of the two files is file KV2/2149 (PF32/V2) 'Childe, Vere gordon' (sic). It is formulated in the same manner as KV2/2148. It was accessed 24 times over apparently roughly the same period as its predecessor (1965-1971?). This file covers only three and a half years, from 19 July 1952, to 17 November 1955, yet constitutes 58 pages of material, divided into 39 entries and has no 'File Closed' stamp. It is almost certain that this file continued to the time of Childe's death in 1957. The file consists of items released in 2005 for public view after the statutory 50 years and this probably explains why it contains no material dated after 1955.

The MI5 files are frequently less than dispassionate in tone towards Childe. This is perhaps not unexpected given the highly hostile environment at the time towards both anti-war agitation and communism. The files commence during the First World War and the Communist Revolution in Russia. With the passing of time, particularly into the 1950's this existing unease was heightened by the widely shared perception in the Western world of the real threat of a communist plot for world domination. Childe's advocacy of pacifism, of Marxism as an intellectual tool, his (generally silent) participation in Marxist gatherings, his association with known western communists and his travel to and correspondence with the communist world led him to be regarded by British intelligence agencies as a potentially dangerous and treacherous individual.

A Life Intensely Watched

The first point that powerfully emerges from an overview of both MI5 files, is the pervasive and ongoing extent of surveillance to which Childe was subjected. For the vast majority of Childe's adult life, from age 25, almost to his death at 65, he was continuously subject to intensive and at-times intrusive surveillance. It must be borne in mind that not every instance of observation or interception was entered into Childe's file, or retained. Further, even today, there are items missing from this file, represented by blank pages which have been prominently stamped in red as,

> *The Original Document Retained in Department Under Section 3 (4) of the Public Records Act 1950. Jan 2005.*

The evidence of surveillance contained in files KV2/2148 & KV2/2149 should then be taken as only a representative sample of the ongoing MI5 surveillance of Childe through most of his life. The nature of these surveillance techniques, such as interception of all of Childe's mail, reveals that Childe endured a daily experience of intrusion and interference into even the mundane aspects of his life. The forms of surveillance that Childe was subject to were all-encompassing, and include:

Third-Party Telephone Surveillance (10 instances filed): Any mention made of Childe during intercepted calls between people of interest was logged and if deemed significant, transcript was entered into Childe's MI5 file.

Third-Party Mail surveillance (13 instances filed): Any mention of Childe in intercepted mail between people of interest was logged, and if considered pertinent had its transcript entered into Childe's MI5 file.

First and Second-Party Mail surveillance (18 instances filed): Mail to and from Childe was routinely opened, frequently copied or transcribed and copies included in his file if content was deemed of interest. This included personal mail between Childe and his siblings.

Observer monitoring (35 instances filed): Members of MI5 or cooperating government bodies frequently attended functions at which Childe was present whether he was there in an active role or not, and reported on his actions and the nature of the events he attended. Similarly, customs officials and observers at ports and airports provided information on Childe's movements.

Figure 4: *Childe on an Institute of Archaeology field trip. Image: courtesy of UCL Institute of Archaeology Collections.*

Informer monitoring (9 instances filed): Generally anonymous, sources often known to or acquainted with Childe either volunteered or were requested to provide information on Childe. These include academics at universities Childe attended.

Media monitoring (17 instances filed): MI5 scrutinised newspapers and pamphlets for mention of Childe, with copies or transcripts taken.

Travel Application (11 instances filed): Childe's applications for passport or visa approvals were consistently and rigorously examined and assessed.

Internal Intelligence Assessment (66 instances filed): Childe was the frequent topic of assessment and requests for exchange of information within different offices and bodies in the intelligence community.

Living under the Watch

In assessing what impact this surveillance may have had on Childe, it must be reinforced, as indicated above from his peer's variant perceptions of him, that Childe was certainly a man of parts. His widely varying interests and approaches to life, often did not encroach on each other. His communist sympathies did not seem to have precluded his membership in the elite, conservative Athenaeum club, nor did his frequently elitist intellectualism towards his peers prevent his enjoying discussions with his students in a manner that left them feeling treated as intellectual equals. While perceived by some as detached, impersonal and even incapable of social relationships (Brothwell 2009), his students often referred to him as 'Uncle Gordon' (Thomas 1992), and enjoyed social outings and occasions with him in informal and amiable settings (Figure 4). Childe did at times choose to open himself to others across a variety of areas. However he appears to have been highly selective in what he shared with whom. He also appears to have been very capable of consciously moderating his behaviour to suit differing environs.

As further evidence of this compartmentalisation and in contrast to the image of Childe as socially disconnected, although Childe was never married or openly romantically connected, Childe did enjoy deep friendships with an eclectic few who could match his intellectual pace and world outlook. Jack Lindsay at The University of Queensland (Lindsay 1981), and Rajani Palme Dutt at Oxford were two such friends. Dutt in particular, in his obituary for Childe, noted Childe as his 'closest friend at Oxford' (Dutt 1957). Despite studying at two different Oxford colleges, Dutt and Childe undertook considerable expense to move out of college and into digs together (Dutt 1957). This does not seem to speak of Childe as an anti-social individual, rather as one of select and highly discerning interests. It did not help Childe's case with intelligence of course that Dutt was and went on to be a highly prominent communist (Callahan 2004). However Childe's friendship with Dutt seems to have been less damaging in the eyes of MI5 than Childe's friendship with a rather historically elusive character, P.T. Davies.

'Probably the Ugliest Man in the World' – Intelligence, Prejudice and Childe

The background to Childe's relationship with Davies is outlined in a letter in KV2/2148 dated 17 June 1917 from E. Armstrong, pro-provost of Queens College Oxford, to MI5 providing information on Childe at the request of MI5. Armstrong concluded that Childe was harmless but disgusting, and outlined Childe's background to friendship with Davies.

During World War I, Childe had not been eligible for service. In 1914, while studying at Oxford and aged 22, he had applied to the armed forces only to be turned down

on the grounds of his less than robust physique. Following this, Childe drilled for a while with the Civilians Battalion, but fairly soon his ideas appear to have taken a sharp turn to the anti-war left, due to his having fallen under the influence of P.T. Davies. Davies had been eligible for service, had refused to serve on grounds of conscience, and had served consecutive gaol sentences as a result. Childe had applied to visit Davies in gaol on several occasions, which only increased intelligence interest in Childe. Childe's appearance, the nature of Childe's relationship with Davies, and the perceived corrupting influence of Davies on Childe are described by Armstrong in venal hyperbole, which openly accuses Childe and Davies of a homosexual relationship. Armstrong attributes Childe's change as due to,

> *a romantic affection for P.T. Davies. Childe is repulsively ugly, probably the ugliest man in the world, and Davies…has a certain personal attraction…The misfortune of Davies became a monomania with Childe, entering into all his work and spoiling it, and perverting his moral and rectal attitude*

Whatever the reality was of the relationship described by Armstrong between Childe and Davies, it is only relevant here as an indicator of the degree of difficulty which people, even those relatively close to Childe, have had in attempting to understand him as a whole individual. Capacity for deep friendship, let alone considerations of romantic attachment, is simply not part of the vast majority of depictions of Childe. The exchange between Armstrong and MI5 is also a shocking example of the level of petty bile that could be expressed in official correspondence by Oxford staff, towards one of their students. It is noteworthy too, that far from expressing any moderating opinion on Armstrong's depiction of Childe, MI5 readily adopted Armstrong's terminology as to Childe's ugliness and perversion, and pressed the case further:

In a letter dated the 19th June 1917, the Home Office summarised its case against Childe by considering as misguided Armstrong's opinion of Childe as harmless. MI5 considered Childe to be a 'thoroughly perverted and probably a very dangerous person'.

Resulting from this and on that very day, a Home Office Warrant was issued for the detention and opening of all post and telegrams to Childe or any letters likely to be intended for him. Among the reasons stated for this Warrant was that Childe was 'A danger to this country'. Childe left England for Australia shortly after and returned to England in 1921. A second Home Office Warrant was taken out on Childe by Scotland Yard on 28 September 1922 and this appears to have operated for the remainder of Childe's British career.

Despite Childe's apparent ability to compartmentalise, surely the ongoing and intense surveillance that Childe experienced over most of his life, must have impacted on his mental and emotional states to some extent. The extent of this surveillance has never been fully appreciated. Neither has there been an evaluation of the measures that Childe may have taken to evade surveillance, with consideration to Childe's perceptions of constraints on his own freedom.

It was surprising to me, to observe the frequently snide, derogatory and often less than competent nature of MI5 depiction and surveillance of Childe.

It was unexpected, for example, to see that MI5 quite unnecessarily continued a highly pejorative mode of depicting Childe. In a letter dated 22 October 1917, MI5 advised Australian Intelligence that Childe was on his way to Australia, and described Childe as 'probably the most ugly man in the world'. This could hardly serve as an objective or accurate means of identifying Childe.

Taking the pejorative pettiness further, on 25 October 1917, in a note on Childe's MI5 file sheet, advice is made to send 'the address of the 'beautiful' Childe's sister' to other intelligence agencies.

Childe seems to have been aware of the interception of his mail and his monitoring in general, but possibly not of its extent. On at least two occasions, MI5 intercepted letters in which Childe had concealed letters to be forwarded other individuals, presumably people also of interest to MI5. Whether Childe concealed these letters in a genuine attempt to avoid surveillance, or whether he did so as a ruse, knowing that they would be discovered, can no longer be determined.

Figure 5: *Childe with his excavation team in the Orkneys. Image: courtesy of UCL Institute of Archaeology Collections.*

However, what seems certain is, that particularly given the at-times bungling nature of MI5 mail interception, it would have been difficult for Childe not to have been aware that his mail was being tampered with on an ongoing basis.

MI5 may have had expertise in discreetly steaming envelopes open and resealing them. Yet, on 23 August 1948, an extremely apologetic note from MI5 to the Metropolitan Police Special Branch expresses regrets for having mistakenly punch-holed for filing, the original copy of a letter that now had to be sent to Childe in obviously tampered state, patently giving away that his mail was being monitored.

Given Childe's keen eye for detail, the likely residual evidence of general letter-opening and resealing by MI5 and the disruption to postal delivery times such interception would have entailed, I feel it likely that Childe would long have been well aware of tampering with his mail even without the tell-tale signs of mistakenly hole-punched correspondence.

Figure 6: *Childe with a teddy and a car at the Institute of Archaeology. The teddy bear was presented to him by students of the University of Brno. Image: courtesy of UCL Institute of Archaeology Collections.*

Childe would likely have been aware that he was under physical observation too. This observation appears to have extended beyond the meeting-rooms of communist-related organisations. Childe's movements at airports and borders were reported it seems as a matter of course. Graphically, on 9 May 1941, Scottish Regional Security wrote to MI5 for more information on Childe than MI5 had previously supplied by in a summary letter dated 5 May 1941. Interestingly, given the above cited previous perception by MI5 of Childe as dangerous (and hence maintaining surveillance of him), MI5 had classified Childe in its initial report to Scotland as a 'progressive intellectual type and is not likely to be dangerous'. However, having become aware through mail intercept that Childe was in the Orkneys, Scottish Regional Security 'had enquiries made and found that he was indulging in archaeological pursuits near certain gun sites, etc.' and was therefore eager for more information on him.

Given likely levels of communication to the Orkneys at the time and the insular nature of its inhabitants, I would propose it unlikely that queries after Childe or monitoring of him could have been made in the Orkneys without triggering an awareness of intelligence presence. Childe had of course, a long-standing relationship with the folk and archaeological sites of the Orkneys. He had excavated there regularly as part of his professorial duties after his appointment to Edinburgh University in 1927. He was known to the small Orkney population, having employed local labourers (Figure 5) and boarded in local households. His somewhat odd mannerisms had endeared him to his Orkney landladies to whom he perfectly represented the eccentric professor (Green 1981). A stranger in the Orkneys asking after Childe would almost certainly have raised suspicion.

Playing the Game

If we accept that Childe was aware he was being monitored, then we must also consider that he may well have adopted strategies to mislead this monitoring or to free himself from it. That these measures most likely extended beyond the clearly documented technique of concealing of letters within letters. Childe's movements and written and other communications may well have incorporated attempts to evade or mislead observation.

This could well explain the events of October 1955. Childe was due to travel to Romania as part of a delegation invited by the 'Roumanian Institute for Cultural Relations with Foreign Countries'. MI5 communication to observers at London airport forewarned of his impending departure. The MI5 assets at London Airport confirmed that Childe had been listed on the manifest to depart on the date and flight noted but that he had not done so. Rather, they observed, that a V.G. Childe had departed London airport for Romania several days previously. Purchasing a double ticket would not have been cheap but it appears it may have worked as a ruse for Childe to ensure that his trip to the communist world was unhindered by MI5.

This event, with the previously mentioned reports of monitoring at borders and ports could well indicate that Childe had experienced delays or been impeded when travelling. Such efforts were most likely to have been balanced by intelligence desires not overtly to inconvenience a high profile individual. This is further indicated in a note from the Metropolitan Police dated 17 April 1952, which states that Childe had that day landed in London from Brussels with an old Czech visa in his passport,

> *Owing to the number of passengers passing through the control at the time, it was not possible*

to arrange for a discreet search of his baggage by H.M. Customs

Were Childe generally eager to avoid inspection of his luggage or a delay in travel, it seems likely he may have engaged in some ruse such as his double-booking to Romania, or playing on the reluctance of Intelligence agencies to cause public disruption.

The strict supervision and constraint of Childe's overseas travel and even monitoring of his movements within Britain evidenced in these files would seem likely to have conflicted with Childe's innate desire and need for travel. It was after all Childe's travel to numerous European museums as a relatively penniless young man that had led to his first major work *The Dawn of European Civilisation* in 1925. His acquaintances described him as 'invariably restless' for travel (Kilbride-Jones 1994: 138).

This wanderlust, combined with potential restraints on his international travel, may have been partially behind his affection for outings, in particular for high-speed, long-distance trips in his succession of powerful touring cars (Kilbride-Jones 1992: 138; Thomas 1992: 134). Indeed, were Childe subject to ongoing MI5 observation, a long drive in a fast car may well have been a simple way to relieve himself of potentially onerous MI5 observation. In Figure 6 Childe is seen posing in front of his car, with a teddy bear gifted to him by Czech students.

This level of likely observation could hardly have been pleasant for Childe to endure and would not have been eased by the apparent prejudice with which he was regarded by MI5.

It is difficult to overstate the sense of urgency (if not outright panic), pejorative wording, (and sometimes incompetence) shown by MI5 throughout the historical span of its files on Childe. On completing his university studies in England with a brief period in 1914 at the British School at Athens to study pottery for his thesis (Gill 2011 62), Childe had applied to return to Australia in 1917, and had requested to travel to Australia via America to inspect archaeological collections held there.

MI5 approved Childe's request to leave Britain, but refused to allow him a stopover in America or travel through the Panama Canal, demanding that he travel 'the long way round' (via Cape Horn at the tip of South America). MI5 were probably concerned that Childe would collaborate with American anti-war, pro-communist agitators at this highly sensitive time. America had only recently been drawn into the First World War in April 1917 and had not yet physically joined the war effort. Further, Childe's trip fell within the crucial period of the communist Russian Revolution, between March and November 1917, and it is understandable that MI5 would have sought to minimise any impact Childe could have had to influence Americans against joining the Allied war effort, or in favour of communism generally.

Childe agreed to the MI5 request and booked a berth on a New Zealand ship, the Rimutaka, travelling to New Zealand via Cape Horn. On the 3rd July 1917 MI5 wrote in some confusion and in most urgent terms to Captain Hemming of the Rimutaka,

Dear Sir,
A passenger, Mr. Vere Gordon Childe, aged 25, a graduate of Sydney University who has just taken his degree at Oxford, described as "probably the ugliest man in the world", has had his passport endorsed available for the direct journey to Australia valid to embark on the "Rimutaka" only. For your information this gentleman while his views on archaeology and other scholastic subjects appear all that can be desired holds, from our point of view, decidedly perverted views on the war and is a believer, amongst other things, in the justness of the German submarine warfare. He wished to visit America before returning to Australia and it is thought he might endeavour to disembark at Panama. This you will doubtless be able to prevent without difficulty. I should be glad to be informed that he is on board.

Captain Hemming responded to this request in a letter from Plymouth on the 13th August 1917, in which it is hard not to hear a sarcastic antipodean drawl,

Dear Sir,
I beg to state that the passenger Mr Vere Gordon Childe is now on board my ship. I have noted your wishes, but as this ship does not go to New Zealand via Panama, there will be little danger of him landing in America.

It almost beggars belief that MI5 operatives would either have forgotten their stipulation that Childe travel via Cape Horn or would have been so geographically uninformed not to realise that travel around Cape Horn did not entail passage through the Panama canal (that was after all why they had insisted on this route) but it seems such was the case. Nor did this lapse prevent MI5 proudly stating in a letter on 22 October 1917 to Australian Intelligence that MI5 had 'taken steps' to ensure Childe did not disembark in America during his return trip.

This intense concern seems to have lasted throughout the period of MI5 observation of Childe. This extended through a 40 year span and would almost certainly have seen at least one, possibly two generational changes of guard at intelligence.

ASIO Not Overly Concerned

While this attribution to Childe of potential threat went unchecked within MI5 internal correspondence, a distinct difference is apparent in the manner that ASIO perceived and depicted Childe. This contrast is most apparent during the early years of Childe's monitoring by MI5, during the First World War.

Australian Intelligence seems to have responded to MI5 communications regarding Childe with a laconic reserve and at times outright rebuttal of MI5 concerns as evidenced in the following exchange:

In a letter dated 22 October 1917, MI5 informed Australian Intelligence of Childe's departure for Australia. It reasserted that Childe held 'perverted' views, repeated that he was 'probably the ugliest man in the world' and that Australian intelligence should keep an eye on him.

In reply to MI5 Australian Intelligence (22 January 1918) made it quite clear that it knew well of Childe and did not concur with MI5's level of excitation at all. Each of the points of concern that MI5 had raised were rebutted by Australian Intelligence in turn noting Childe's history of volunteering for service, his inherent honesty, loyalty and respectable family and perhaps wryly, noting that Childe's most dangerous capacity was likely his ability to write pacifist articles. While stating clearly that Childe had been and would continue to be monitored, ASIO effectively emphasised that a drastic reduction of perceived threat was in order,

> *With reference to your letter of the 22nd October, 1917, 224788/D, I have to inform you that Childe's correspondence had already been under inspection. He is well known to our Sydney people. He volunteered, I believe, twice for active service while he was in England but was rejected. He is not considered likely to do anything dishonest or treacherous, but he is quite capable of writing harmful pacifist articles. Perhaps the influence of his loyal father, a retired Clergyman of the Church of England, with whom he is said to be living may be beneficial. He will, however, be closely watched.*

There was then an overt and considerable difference in tone, content and action between ASIO and MI5 in their treatment of Childe, a difference which appears to have continued to Childe's return to Australia in 1957. The MI5 files contain ample evidence of Childe's ongoing and intensive observation by MI5 throughout his time in England. Yet, Australian Intelligence, and government apparently took quite some prodding even to countenance considering Childe as an intelligence target, let alone a national threat worth opening a formal file on, as is seen from the first items of Childe's ASIO file in 1957, dating to Childe's return to Australia.

The opening items in Childe's 1957 ASIO file describe exchanges regarding Childe between a Mr. George Boss J.P of the Camperdown Hotel, Parramatta Road, Camperdown Sydney, the Prime Minister's (Menzies) office and ASIO. George Boss had been criticised by Childe in *How Labour Governs* (Childe 1923: 157) for running a large bakery utilising only non-union labour and for continuing to do so after selling his bakery to the (Labor) Government. In *How Labour Governs* (1923) Childe had gleefully disclosed that his source on Boss's employment prejudices was none other than Boss himself, who had apparently confided in Childe. Boss was not the sort to be crossed in this manner without seeking vengeance, however delayed.

The bakery in question was the NSW State Bakery – founded in 1914 to ensure supply of bread to the armed forces (*The Colonist* 28/2/1914). Boss was no stranger to playing the amateur and vindictive espionage saviour of the state and as seems clear from the following, was not the sort to let grudges lapse.

Boss had levelled fierce written allegations of communist treachery against his successor at the State Bakery during WWII, accusations of sufficiently wild nature to be included, although robustly queried at a Royal Commission into Bread (*Sydney Morning Herald* 18/3/1941). Boss' standing as a witness was not improved by his admission that he had recently obtained employment and information at the Bakery by dishonest means, although he insisted he had only done so in order to obtain intelligence for the Military and as such was a self-appointed secret service operative whose falsehoods were in the state interest (*Sydney Morning Herald* 18/3/1941). Unfortunately for Boss, he returned to the Royal Commission following lunch, in a state that saw him ejected from court by the Judge who noted that Boss 'did not appear to know where he was' (*Age* 22/3/1941). Little account was subsequently taken of his evidence.

Boss appears then to have been an individual of some influence sufficient to have his testimony taken into account at a Royal Commission and his correspondence noted by the Prime Minister's office, but at the same time of such eccentric character and anti-communist prejudice as to be regarded askance even by the anti-communist government of the time. This would explain the manner in which the Prime Minister's office sought to have some lip-service and acknowledgement paid to Boss' communiqué to them, but did not take his demands for a meeting seriously.

Boss' letter is contained in Childe's ASIO file, but is of such poor copy that it is better to rely on extracts from it reproduced in ASIO reports.

Mr. Boss's letter was dated 15 April 1957, only a day after Childe had landed in Australia. In it, Boss spoke of Childe as a 'most disloyal British subject' and requested an audience with the Prime Minister's office to discuss important information regarding Childe. Boss apparently was not beyond using his advanced age (79) to press the urgency of obtaining an audience. Perhaps unsurprisingly, the Prime Ministers office declined to respond directly, and forwarded the issue to ASIO on April 23 1957. By mid May ASIO had determined that the issues Mr Boss wished to raise,

> *Refer to the time of the Prince of Wales' visit to Australia in 1920, and would bear little or*

no relevance to Professor Childe's present-day activities (ASIO 17 May 1957).

This in itself, the willingness of ASIO to overlook Childe's potential long-passed indiscretions, seems to contrast with the vigilant surveillance of Childe practised by MI5.

Some smoothing over was nevertheless apparently required as described in an ASIO memo, 'Mr Boss appears to be well connected'. Boss was affronted that the Prime Minister's office had not even acknowledged his correspondence. He was apparently interviewed by ASIO in late 1957 as referred to in an ASIO memorandum on 6 December 1957, which also mentions conclusions of the investigation into Childe's death. Neither transcript of Mr Boss's interview, nor the results of investigation into Childe's death are present in the ASIO file.

Alone, at Last

With the exception of a single sheet of point-form internal notes, dated 10 July 1957 that mention Childe, it seems Childe was left alone for his final months, freed from over 40 years of near-continuous intelligence surveillance.

Yet one has to wonder at a particular choice of words from the Director General of ASIO, in his brief note dated 22 October 1957. He requested investigation into whether Childe's death may have been 'influenced by factors of counter-espionage significance'. ASIO's own appraisal of Childe certainly did not seem to perceive him as a threat to national security and differed radically in its relaxed manner towards Childe from the vigilance displayed by MI5. Was there perhaps more to Childe? Other doings that we have no record of, but which may have been enough to agitate MI5? Doings sufficient to give ASIO room to ponder and investigate whether his death was connected to 'counter espionage'? The very term 'counter-espionage' is puzzling. Surely, counter-espionage at home refers to actions by oneself or one's allies against foreign agencies. The only Australian ally that comes to mind as having an agenda against Childe would be MI5. Of course that is conjecture, but it seems the Director of ASIO was mentally conjecturing too.

The ASIO and MI5 files I have briefly examined here are fascinating in the unresolved and likely unresolvable glimpses they offer into previously largely unknown sides of Gordon Childe. But some things seem certain,or at least on fairly solid ground, Childe was, almost from his first arrival in Britain, subject to an intense scheme of intelligence surveillance that was both intrusive in nature and often venally expressed, as evidenced by pejorative statements in official documents as to his appearance, beliefs and character. It seems impossible that this would not have exercised a personal toll on Childe, despite his prodigious abilities to sequester aspects of his life and to play the part for roles in life, as he wrote them. The details of Childe's ongoing interaction with intelligence may never come to light. Nevertheless, the evidence presented and interpreted here indicates far greater complexities and pressures in Childe's life, than would have been anticipated from other sources to date.

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Sailing to windward in Roman times: the Spritsail legacy

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Abstract: The iconography of Roman period merchant ships reveals them to have a different sail-plan to those of earlier times because they often have a small square sail rigged near the bow called a *spritsail*. The significance of the spritsail ceased to be appreciated in the early nineteenth century soon after it became obsolete. This paper discusses the role of the spritsail especially as it assisted Roman period ships to sail to windward.

Introduction

The Roman Empire enjoyed a high level of maritime economic activity primarily because of the security that was maintained in the Mediterranean Sea (Meijer 1986: 211ff). However, there may also have been technological and practical grounds why this commerce was sustained, although traditionally the period has been considered to be without maritime innovation.

To make a consistent contribution to the international economy seaborne trade had to be reasonably predictable and reliable; it could not be at the mercy of the vagaries of the wind and pirates. While the capacity to anchor or to make port during periods of adverse weather and knowledge of seasonal wind patterns and currents were important, it was the capacity of ancient ships to make progress toward the wind that partly freed them from the dictates of wind-direction. On occasions oars could be used, as they were on warships, but they were not appropriate for long-distance merchant shipping.

Ships had to be able to make progress toward the wind by sailing a zig-zag course, sometimes called *beating*. This practice involves sailing as close to the wind as possible with the wind coming over the forward quarter as shown in Figure 1; this is known as being *closehauled.* The ship then changes course so that the wind comes over the other forward quarter. This change of direction is called *going about* or *tacking* and can be a challenging manoeuvre as the bow of the ship must pass through the eye of the wind.

However, there has been a general belief that Roman period ships had little windward sailing capability as expressed by scholars such as Meijer (1986: 224). The lateen sail, still known from Arab sailing craft called *dhows*, was thought to have been of Indian Ocean origin and to have replaced the Roman period square-sail rig by the sixth-century (Hourani & Carswell 1995: 103). Ships with lateen sails are recognised from modern experience to have good windward sailing capabilities (Villiers 1940 (1966)). This understanding led to the assumption that the Roman period square-sail was replaced by the lateen sail because of its technological superiority. Recent research has questioned nearly every aspect of this hypothesis. It is therefore apposite to review this research before addressing the specific question of the spritsail.

Recent research

Ancient maritime research has been driven by literary and iconographic analysis, and the systematic underwater excavation of shipwrecks that has facilitated the building of replicas for actual sailing tests. Whitewright summarises twelve square-sailed ship replica tests revealing windward headings of 70^{O} from the wind or better, up to 60^{O} (2011b: 7, Table 2). Only two of the ships were of Mediterranean derivation, the Kyrenia II, which achieve a heading of about 61^{O}, Figure 1 (Cariolou 1997: 92) and the trireme *Olympias*, which under sail could head between 65^{O} and 72^{O} from the wind (Morrison et al. 2000: 200).

The sail-plan may enable the craft to point close to the wind; but if a ship has leeway, that is it goes rapidly sideways, the course-made-good may not be to windward at all. The shape of the hull is important for limiting leeway. Ships with keels may expect less lateral drift than those with flat bottoms when sailing with the wind abeam or on the forward quarter. Maritime archaeology has shed much light on the issue of hull design and construction. The fourth-century BC Kyrenia shipwreck revealed that comparatively deep keels were in use well before the

Figure 1: *Kyrenia II, a replica of a fourth-century BC merchant ship found near Kyrenia, Cyprus, seen here sailing to windward in light air. The wind is coming directly toward the camera. Image: courtesy Kyrenia Shipwreck Collection Restoration Program.*

Figure 2: *Black figured kylix showing merchant vessel. Athens 520-500 BC, found in Tomb at Vulci (central Italy) GR 1867.5-8.963 BM Cat Vases B 436. Image: C.J. Davey, courtesy of the Trustees of the British Museum.*

Roman period (Steffy 1985; 1994: 42-59). The seventh-century Yassi Ada shipwreck had a keel 22cm x 35.5cm, which would have promoted windward performance (Steffy 1994: 79-83). This evidence may not be definitive, but it is consistent and indicates that square-sailed ships during the Roman period and before had hulls with a capacity to sail toward the wind.

To allow for leeway it is common to measure the velocity-made-good toward the wind, not the speed of the ship itself. Seven replica Viking ships had GPS measured windward velocities of 1 knot or more (Whitewright 2011b: 9, Table 3). Whitewright's analysis of seven recorded ancient Mediterranean voyages by square-sailed ships in adverse wind conditions gave an average velocity to windward of 1.8 knots and thirteen voyages in favourable conditions, with a following wind, gave an average speed of 4.4 knots (Whitewright 2011b: 15). Using this data and known wind patterns, Leidwanger has modelled sailing times in the Eastern Aegean (2013).

Whitewright also assembled similar data for lateen rigged ships in the Mediterranean and found that from eight recorded voyages, ships averaged 1.4 knots to windward and in favourable conditions 4.5 knots. He concluded,

> *The evidence currently available would therefore seem to indicate that there is very little difference in the overall performance of a sailing vessel with a Mediterranean square-sail rig when compared with a similar vessel with a lateen/settee rig from the late-antique, medieval or modern era (2011b: 14).*

This conclusion runs counter to the generally held belief that the lateen rig was introduced because of its technical superiority. Castro has argued that the situation was more complex and that the transition from square-sail to lateen sail may have involved other factors such as changes in hull size, shape and construction (2008: 348). However, Castro's claim that ships rigged with a lateen sail could point closer to the wind than those with the Roman period square-sail is now questionable (Whitewright 2011b: 13).

Whitewright has also argued that there were many other factors influencing the transition from square-sail rigged to lateen-rigged ships in the Mediterranean (2008). In fact he has suggested that the lateen sail itself was developed in the Mediterranean between the second and fifth-centuries and that it was not of Indian Ocean origin. Whitewright supports his case with a comprehensive and sophisticated hypothesis of technological change to explain how and why the lateen sail was developed from the square-sail. He also draws on literary and iconographic evidence revealing that the earliest images of the lateen sail come from the Mediterranean (2009; 2012). His proposition is compatible with the archaeological evidence, which shows the concurrent development of frame-first hull construction and the obsolescence of brail-rings. Significantly, it is the growth of private trade and reduction in state-sponsored bulk commodity trade that he sees as a significant driver of the change (McCormick 2002: 64; Whitewright 2008).

The fourth-century Yassi Ada shipwreck reveals that half-frames, probably fitted simultaneously with the planking, was a significant change in hull construction. Steffy comments on the need for this development,

> *Technology had progressed, but more importantly economics and politics had changed so radically as to require a different shipbuilding philosophy. Shipowners were often independent businessmen with limited assets operating under what amounted to a free enterprise system. The decline of slavery had changed the labor market, too, so that time-consuming processes, such as cutting deep and frequent edge joints and meticulous shaping of planks, became even less desirable* (1994: 85).

Lateen rigs seem to have been adopted in parallel with the introduction of framed hull construction and both changes were driven by developments in politics and commerce (Whitewright 2011a).

Figure 3: *A restored wall painting from the Tomba della Nave, Tarquinia, dated to the early fifth-century BC that depicts a second sail between the mainsail and the bow. Image: from Moretti (1961).*

Iconography

Modern scholars tend to be sceptical about ancient images. Throckmorton, for example,

> *..helpful as are the many extant wall paintings and mosaics of Roman provenance, in which ships and their rigging are depicted, one must make allowance for the fallibility of the artist. Many apparent variations in detail may well be the result of imperfect observation or faulty execution* (1972: 72).

Most known ship images have been found in maritime contexts ashore. The idea that people living and working in these places would tolerate their esteemed seafaring technology being misrepresented in commissioned work is highly unlikely. The artists certainly applied drafting conventions in their depictions and it is important to allow for these, relative scale for example is not strictly adhered to; the depiction of out-of-scale sailors strategically stationed may identify those who were in control of the ship, not that the boat was small. The artists were representing and commemorating the significant technological achievements of the maritime industry or memorable nautical events for the satisfaction of a knowledgeable audience. Graffiti may not be so disciplined, but if it is a choice between the ancient observer and a modern scholar, it would be wise to start by giving the former the benefit of the doubt. It would, however, be imprudent to draw significant conclusions from that which was not illustrated by the ancient artist, especially if those conclusions differ from the clear intention of the image.

Most commentators refer to the comprehensive documentation of ancient ship images in Lucien Basch, *Le Musée imaginaire de la marine antique* (1987). Unfortunately this publication is not generally available so a selection of images accompanies this paper.

Warships, with oarsmen and a ram at the bow, are by far the most common vessels represented by Classical artists; however, there are occasional depictions of merchant ships. The best known image of a Greek merchant ship is found on a late sixth-century BC kylix, Figure 2. The scene depicts the ship being stalked by pirates possibly at night, its sail appears to be reefed and only the helmsman is on deck unaware of the imminent attack. The hull has a distinctive concave bow and there is a 'kind of lattice bulwark' and a 'landing ladder prominently displayed at the stern' (Casson 1971: 68, 128 n.114). Measured on the waterline, the mast is nearer the bow than the stern and the yardarm is about twice the length of the mast. This sail configuration had been used for millennia by Egyptian and other Bronze Age sea-goers such as the Phoenicians. The reconstruction of the fourth-century BC shipwreck found near Kyrenia, Cyprus, the Kyrenia II adopted this sail-plan. Sailing trials found that it had a capacity to sail to windward, especially when the tack of the sail was sheeted near the bow and the yardarm canted upward toward the stern as in Figure 1 (Cariolou 1997: 94).

The restored wall painting from the Tomba della Nave, Tarquinia, dated to the early fifth-century BC, Figure 3, depicts a ship with a second sail between the mainsail and the bow (Casson 1971: 240, fig 97; Basch 1987: fig 880; 1976; Moretti 1961). While the restoration is not entirely certain, it does appear that the second sail forward of the main mast was not small. Casson believed this image to be 'conclusive' evidence that the foresail originated with the Etruscans in the fifth-century BC (1971: 70).

There are three images from Pompeii that must predate the city's demise in AD79. The graffito of the ship called *Europa,* Figure 4, is very detailed. It depicts a small square sail at the bow with a crew member in attendance possibly indicating that it needed adjustment. The drawing also shows the hull shape, sailors, deck facilities, timberheads fore and aft, and the ship's boat being towed.

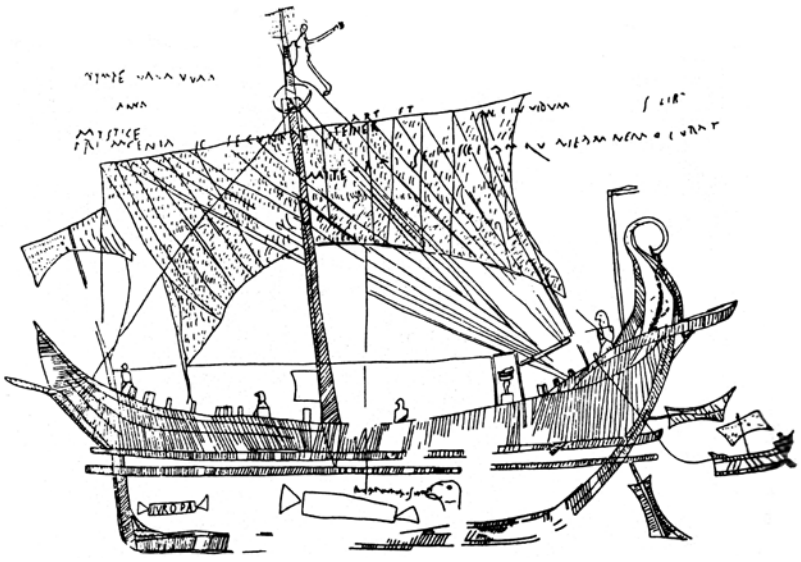

Figure 4: *Graffito from Pompeii of a large cargo ship, depicting two sails and other interesting details such as the underwater hull shape, rudder usage and the trailing boat. Image: from Benoît (1961: fig 73).*

Figure 5: *Wall painting of a ship with a small sail at the bow, Pompeii no later than AD79. Naples, Museo Archeologico Nazionale. Image: C.J. Davey.*

While the wall painting, Figure 5, appears to be venerating the larger than life ship-owner depicted at the stern, the sail-plan is complete with a small square sail on a sloping mast at the bow. A relief on the Tombstone of Naevoleia Tyche, Pompeii, not reproduced here, shows a ship shortening sail as it enters port (Basch 1987: 459, fig 1018; Casson 1971: fig 151). It has a prominent foremast but no sail is set on it.

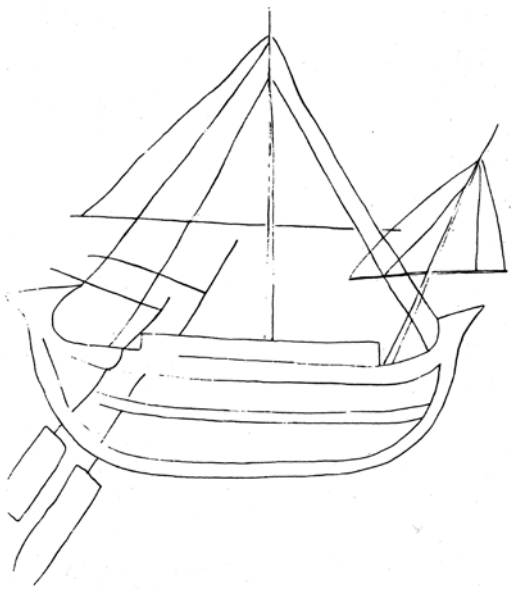

Figure 6: *Graffito of a ship from the arch in the Leptis Magna market (built c 8 BC). Image: Basch 1102, from Vergara Caffarelli & Caputo, 1966 pl 64 A.*

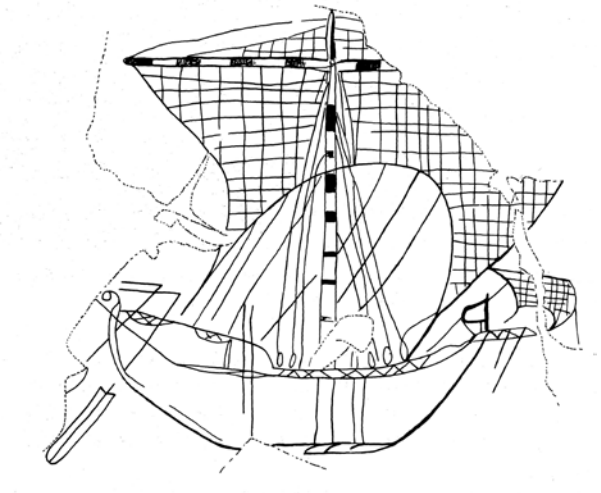

Figure 7: *Graffito from Sidi Khrebish (Berenike) near Benghazi dated by the excavtiors to late second-century. Image: Basch 1103, drawing from a photo by F. Sear (Pye 1974:pl 4)*

Graffiti depicting ships have been found at a number of other places. The drawing from Leptis Magna, Figure 6, shows a ship as it would appear in port with yardarms lowered. The ship has a forward leaning mast with a short yardarm on which a small sail would have been rigged. A graffito scratched into a plastered wall at Sidi Khrebish, Figure 7, shows a ship under sail, which includes what appears to be a topsail above the mainsail and a small sail set at the bow.

A graffito from the Palatino in Rome, Figure 8, is of a ship sailing with a small sail set at the bow. An interesting detail is the base of the mast carrying though to the keel indicating that the artist had probably been aboard such ships to observe this internal detail. Timberheads, used for securing shore- and anchor- lines, can also be seen on the gunwale at the bow and the stern. There is some uncertain detail at the bow and some writing on the side, which gave now lost meaning to the image.

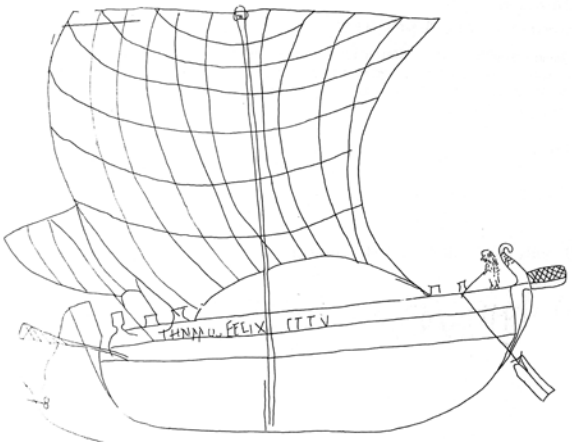

Figure 8: *Graffito of a Roman period ship rigged with a mainsail and a small sail at the bow. Image: from Castrén & Lilius 1970: 109.*

Figure 9: *A mosaic of two ships from Station 23, Square of the Corporations, Ostia c AD 200. The ships have contrasting rigs and hull shapes. Image: from http://www.ostia-antica.org/piazzale/corp.htm accessed 20.7.2015 (Becatti 1961: 73, Tav 179).*

A large number of maritime images come from the port of Ostia. The Square of the Corporations, Ostia, has black and white floor mosaics of 23 ships, only four of which do not have a second sail near the bow (Becatti 1961). The ships depicted in Figure 9 represent two different hull shapes and sail-plans. The ship on the left has a cutwater convex bow about which little has been written; a hull of this shape was excavated at Madrague de Giens (Tchernia 1978). The sail-plans are contrasting and the fact that the ship on the left with a relatively large foresail and the ship on the right with a small square front sail appear together demonstrates that these were two different sail-plans at the time. It may be concluded from this mosaic that where a larger foresail is depicted, it should not immediately be deemed an out-of-scale small sail. Also worthy of comment is the presence of a third mast and a mizzen sail on the left-hand ship.

A second scene, Figure 10, found on a third-century sarcophagus thought to have come from Ostia and now in Ny-Carlsberg, Glyptothek, Copenhagen, depicts two vessels with small sails near the bow, and one other with a sprit-rig (not shown), negotiating the harbour entrance. Sailors are portrayed to be busy at their stations exhibiting behaviour that led Casson to interpret the scene as a depiction of 'a crisis at sea' (Casson 1996: 50f).

Figure 10: *The two vessels with small sails at the bow from a relief on a third-century sarcophagus, find spot allegedly Ostia. The ship on the right has a cutwater bow. Image: Wikicommons image of a replica in the Museum für Antike Schifffahrt, Mainz (Original at Ny-Carlsberg, Glyptothek, Copenhagen).*

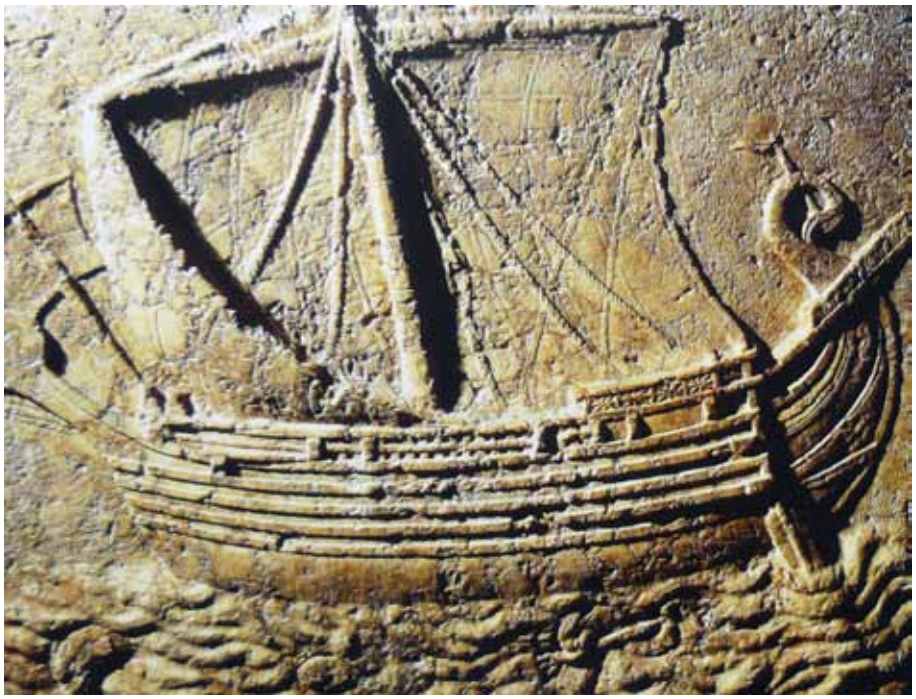

Figure 11: *Relief of a large merchant ship on a sarcophagus from Sidon, second-century, National Museum, Beirut. Image: Wikicommons.*

The relief of the large merchant vessel from a sarcophagus found at Sidon and dating to the second-century, Figure 11, has a small square sail on a sloping mast depicted at the bow. The absence of human figures in the depiction helps to give a sense of the massive scale of Roman period grain ships. Such vessels could displace over 600 tonnes; it is not hard to imagine the awe that they inspired and the reasons why artists drew them. A rope can be seen coming from the deck amidships and running forward, it would appear to be the sheet of the small sail at the bow.

The marble relief of Portus, dating to the late second-early third-century, now in the Musée de la Villa Torlonia, Figure 12, shows two ships, one entering port and one alongside unloading cargo. Both have forward leaning masts at their bows, but are without sails set on them. The context of the scene is that of port services and activity including the lighthouse, religious rituals and cargo handling.

A marble relief dating to about AD 200, Figure 13, depicts a coastal vessel with two similar sized sails (Casson 1971: fig 142; Basch 1987: fig 1104). Some commentators have made much of this depiction, but its sail-plan is much less common in the Roman period iconography than the sail-plan with a large square mainsail and small square sail near the bow, which is portrayed on the coins of Figure 14.

Images of ships with a lateen rig, sprit-rigs, single square mainsails, and three mast rigs from the Roman period could be shown, but they are not prevalent and are not relevant here. The preponderance of the iconographic data, some of which has been included above, reveals that in the first few centuries AD Roman period merchant ships had at least two distinct sail-plans both with additional

Figure 12: *The marble Torlonia relief of Portus, late second-early third-century, marble 1.22 x 0.75m Musée de la Villa Torlonia No 430. Image: Schreiber (1896: 99, abb 6).*

Figure 13: *A marble relief found at Carthage dating to about AD 200 depicting a coastal vessel with two similar sized sails BM GR 1850 3.4.32. Image: C.J. Davey, courtesy of the Trustees of the British Museum.*

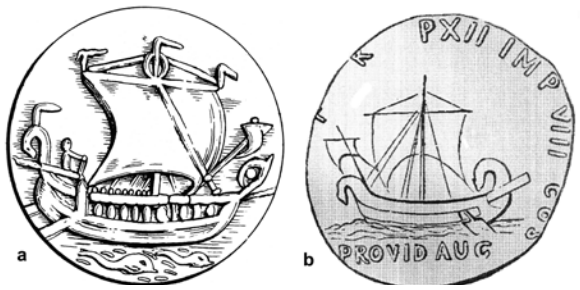

Figure 14: *Two coins depicting ships with a small sail at the bow, (a) from Alexandria AD 67. Image: from Torr (1895: pl 6 No 27) (b) time of Emperor Commodus drawn from a coin in the Avignon Museum. Image: from Smith & Smith (1880: 201).*

sails in front of the mainsail. Figures 3, 9 left and 13 show sail-plans with comparatively large foresails, while Figures 4, 5, 6, 7, 8, 9 right, 10, 11 and 14 show sail-plans where the front square sail is comparatively small. This second sail-plan is the focus of this paper. Some ship images, one included here, Figure 12, prominently depict a forward sloping mast with no sail set; many of these scenes are associated with port facilities where shortening sail was necessary.

The presence of a second sail has always been clear to observers however some have argued that the iconography was wrong while others have overlooked it. The otherwise excellent website *General Information on Ancient Roman Ships* (http://alkedo.wikispaces.com/General+Information+on+Ancient+Roman+Ships accessed 10.8.2015), for example, does not mention sails other than the mainsail.

Ancient references

Ancient Greek and Latin literature mentions a number of sails in addition to the mainsail. These include the ἀκάτειον - *akateion*, the σίφαρος - *sipharos,* the δόλων - *dolon*, and the ἀρτέμων - *artemon*.

The *akateion* is a sail at the front of the ship (Aristophanes, *Lysistrata* 61-64; Plutarch, *de audiendis poetis* 1; Lucian, *quomodo historia conscribenda sit* 45). The Aristophanes reference is in a play dating to 411 BC, the context is military and in fact Torr, a still useful analysis, notes that 'to hoist the *akateion*' was a proverbial expression for running away (1895: 86). He suggested that the sail was of Athenian military origin and that the term ceased to be used with the demise of the Athenian navy (1895: 86). However, the Lucian reference relates to a merchant ship indicating that there may be more to the matter. Torr does not resolve the names of sails situated in front of the mainsail, 'there is not anything to show what difference there was between the akateion and the dolon, and the dolon and artemon' (1895: 88).

The fact that the *akateion* was used as a sail to hasten a retreat means that it must have been large enough to generate power and been located where it did not threaten the command of the ship. A course away from the wind would offer the speediest retreat and a reasonably sized sail hoisted at the ship's bow would achieve a quick downwind get-away with straightforward directional control. The foresail depicted in Figures 3, 9 left and 13 would satisfy all these prerequisites raising the possibility that this foresail was called an *akateion*. Casson however has argued that this sail was a 'topsail' (Casson 1971: 241 n.72). The *sipharos,* Latin *suparum* is described as the 'highest of the sails' and is unquestionably a topsail (Seneca, *Epist*. 77:1-2; *Medea* 323-328; Lucan, 5.428-9; Statius, *Silvae* 3.2.27). This renders Casson's identification of the *akateion* problematic.

A *dolon* is a small sail mentioned in connection with warships in battle (Polyb. xvi. 15. 2; Diod. Sic.xx. 61; Pollux, i. 91; Liv. xxxvi. 44, xxxvii. 30; Isidor. *Orig*. 19:3; Procop. *Bell. Vandal*. i. 17; Torr 1895: 87). These references range in date from 201 BC to AD 533. Warships are often depicted with a small sail hoisted near the bow. They derived their ramming power from oarsmen, but their strategic advantage was achieved with rapid manoeuvring, which would have been greatly assisted by the *dolon.* The main mast and sail were generally left ashore during battle, but it seems that the foremast was carried. The *dolon* would have been set on the foremast to assist manoeuvring during the battle; and the *akateion* could have been hoisted from the same mast if a retreat was necessary.

A sail with the Latin name *artemo* is mentioned by Lucilius (*apud Charisium*, 99) a little before 100 BC (Torr 1895: 88). Torr also refers to Labeo and Seneca the Elder, who lived in Rome at the time of Augustus. Seneca (*Controversiae*, vii. i. 2) distinguished between the ordinary sail (*velum*) from a sail called an *artemo*, while Labeo (*The Pandects*, 1. 16. 242) distinguished the ordinary mast (*malus*) from a mast referred to as *artemo*. Casson was inclined to call all foresails *artemons* distinguishing between those that were large and small (Casson 1971: 240). This paper suggests it was the small square-sail at the bow depicted in Figures 4, 5, 6, 7, 8, 9 right, 10, 11 and 14 that was an *artemo.*

In Greek the sail was called an ἀρτέμων - *artemon* and occurs only once in Acts 27: 40. Smith's discussion about the identity of the *artemon* as a spritsail and his explanation for its mistranslations, including the King James Version 'main sail', seems to be generally accepted (Smith & Smith 1880: 192-201). The already mentioned Latin references support a second-century BC introduction of the spritsail-*artemon*.

The story of the shipwreck in Acts 27: 27-40 describes every sailor's nightmare, coming upon an unknown lee shore at night when running before a gale-force wind with a strong following sea. The incident has been considered in detail by Smith (1880) and Hemer (1990: 132-152). Anchoring from the stern (v. 29) was the correct response, and as the hull had a fine or pointed stern, the ship would

have ridden the waves comfortably. Indeed, Cariolou's experience on the Kyrenia II led him to conclude that this procedure was normal (1997: 97). Bringing the ship around head to wind to anchor from the bow would have been challenging. Smith comments,

> *The advantages of being anchored in this manner are, that by cutting away the anchors (τὰς ἀγκύρας περιελόντες), loosing the bands of the rudder (ἀνέντες τὰς ζευκτηρίας τῶν πηδαλίων), and hoisting the artemon (ἐπάραντες τὸν ἀρτέμωνα), all of which could be, as they were in effect, done simultaneously, the ship was immediately under command, and could be directed with precision to any part of the shore which offered a prospect of safety* (Smith & Smith 1880: 136).

The decision to run for the shore and beach the ship was the best option in the circumstances. When approaching a lee shore or sailing over a bar where the water depth reduces, waves become steeper and may throw the stern of the ship forward and to one side in an unintentional manoeuvre called *broaching* leaving the ship side on to the waves where there is a danger of capsizing. The *artemon* was well suited to this situation as it was comparatively small and manageable in the windy conditions and was positioned at the bow of the ship where it could help steer a course away from the wind.

Paulinus of Nola (AD 354 – 431) *Epist.* 49.2 wrote to Macarius relating a story about an elderly man who was abandoned by the crew on a ship in circumstances not unlike those of St Paul. His miraculous escape was somehow associated with the *artemo* (Walsh 1967: 259f).

Augustine (AD 354 – 430) *Enarratio in Psalmum* 32.4 describes the *artemo* (often mistranslated as 'topsail') being used by a confused mariner to steer his ship,

> *Fac enim hominem optime gubernare navim, et perdidisse quo tendit; quid valet quia* ***artemonem*** *optime tenet, optime movet, dat proram fluctibus, cavet ne latera [some mss: lateri] infligantur; tantis est viribus, ut deterqueat navim quo velit, atque unde velit: et dicatur ei, Usquequo is: et dicat, Nescio: aut non dicat, Nescio: sed dicat, Ad illum portum eo, nec in portum, sed in saxa festinet? Nonne iste quanto sibi videtur in navi gubernanda agilior et efficacior, tanto periculosius eam sic gubernat, ut ad naufragium properando perducat?* (Augustin 2015)
>
> *For consider one who is expert at steering a ship, and yet who lost his direction: what benefit is it, if he maintains the spritsail firmly, deploys it with facility, keeps the prow facing into the waves, takes care that the sides of the ship are not battered in—indeed has such great facility that he can turn the ship where he wills, and away from where he wills, and someone says to him: 'Where are you going? And he replies: 'I do not know', or he does not say 'I do not know', but says: 'To that port over there', even as he hurries not to the port, but onto to the rocks? Is not such a one, the more he thinks himself active and effective in steering the ship, steering it all the more dangerously, by his haste bringing it to shipwreck at last?*

The illustration is of a strong and skilled mariner who steers his boat by using the spritsail but who does not know his destination. Augustine believed that it was preferable for a ship to be piloted by a weaker person who knew where to go and who would seek help to command the ship. The passage accurately describes steering a course over the waves to windward ensuring that the boat was not swamped, battered to pieces or brought to a standstill when control would be lost and nearly anything may happen. The passage clearly identifies the *artemo* as a sail associated with steering a ship.

There is the potential for confusion because the term 'spritsail' also refers to a fore-and-aft square sail used on barges and is popular today in recreational wooden dinghies. Casson discusses sprit-rigs used on harbour and coastal craft during the Roman period (1971: 243f, 333, 337). The subject of this paper is the small square sail carried by larger sea-going vessels; it was rigged on a yardarm that was attached under a bowsprit or a forward sloping foremast. To avoid confusion it will be referred to in ancient contexts from now on as a spritsail-*artemon*.

Theoretical perspectives

Ancient and modern writings refer to the spritsail-*artemon* in connection with the steering of the ship. The comments may be unintelligible or simply not significant for non-sailors, who no doubt assume that sailing ships are steered by their rudders. Sailors, however, know that the most important influence on the direction that a boat travels is the sail-plan and sail setting; and if these are incorrect no amount of rudder movement will bring the craft to the desired heading.

The diagrams in Figure 15 show the influence of the Centre of Lateral Resistance (CLR), which represents the point at which the hull's resistance to sideways movement theoretically acts and the Centre of Effort (CofE), which is the point where the aggregate force of the sails is deemed to act. As Palmer has argued (2009), the theoretical calculation of these points does not have much bearing on reality; however, all sailors are only too well aware that to effectively use a rudder, or steering oar, the real-world CofE and CLR have to be almost aligned as shown by Diagram B Figure 15. There is a strong preference for the CofE to be slightly aft of the CLR so that when left without interference from a steering device, the ship will turn head-to-wind and stop. The alternative is potentially disastrous as the boat bears away and careers out of control and unstoppable before the wind. Champion dinghy sailors often practise sailing without a rudder; they manipulate the attitude of the hull and adjust the sail to steer their boats so that when sailing normally they can minimise the use of the rudder, which slows the craft.

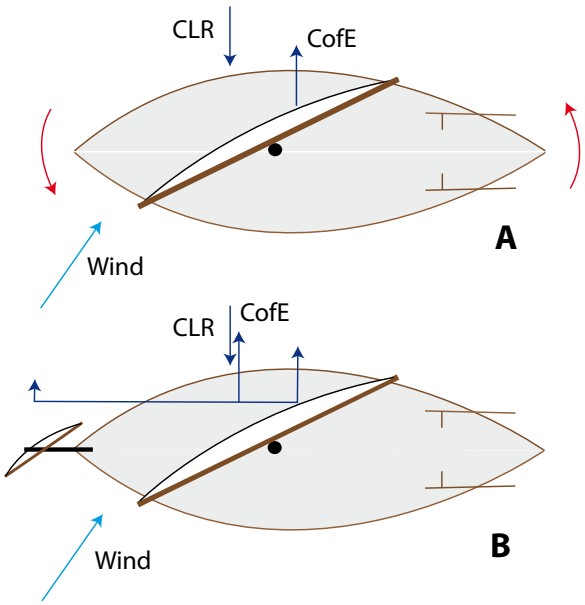

Figure 15: *Diagrams illustrating the interaction between the Centre of Lateral Resistance (CLR) and the Centre of Effort (CofE):* ***A****, mainsail only, and* ***B****, with the addition of a spritsail-*artemon *where the CofE is the aggregate of the two sails.*

However, ancient ships were not as finely tuned as modern racing dinghies. When the mainsail of an ancient craft was rigged as close to fore-and-aft as practical the CofE would move aft producing a strong tendency for the ship to round-up into the wind and remain there, Diagram A Figure 15.

The addition of the small spritsail-*artemon* at the bow changed the balance by bringing the CofE forward and causing the bow to swing away from the wind, Diagram B Figure 15. Being a small sail, the spritsail-*artemon* could be easily trimmed to alter its power. When the spritsail was sheeted in, its power increased and the CofE would move forward, causing the ship to bear away from the wind, while easing its sheets would move the CofE aft and the ship would tend to turn toward the wind. There is no suggestion that ancient sailors thought in terms of these theoretical concepts, but they clearly did appreciate the effects of sail adjustment and hull attitude. The ships depicted in Figure 9 right and Figure 4 are trimmed for windward sailing with sails set fore-and-aft and the mainsail tack (front bottom corner) sheeted near the bow.

In *Mekhanika* ('Mechanical Problems'), a short treatise on levers and the circle written by a Peripatetic and included in the Aristotelian corpus, but not by the latter himself, the writer describes in Problem 7 how sailors of the Classical period, when boats only had a mainsail, tried to address this problem,

> *7. Διὰ τί, ὅταν ἐξ οὐρίας βούλωνται διαδραμεῖν μὴ οὐρίου τοῦ πνεύματος ὄντος, τὸ μὲν πρὸς τὸν κυβερνήτην τοῦ ἱστίου μέρος στέλλονται, τὸ δὲ πρὸς τὴν πρῷραν ποδιαῖον ποιησάμενοι ἐφιᾶσιν; ἢ διότι ἀντισπᾶν τὸ πηδάλιον πολλῷ μὲν ὄντι τῷ πνεύματι οὐ δύναται, ὀλίγῳ δέ, ὃ ὑποστέλλονται. προάγει μὲν οὖν τὸ πνεῦμα, εἰς οὖριον δὲ καθίστησι τὸ πηδάλιον, ἀντισπῶν καὶ μοχλεῦον τὴν θάλατταν. ἅμα δὲ καὶ οἱ ναῦται μάχονται τῷ πνεύματι· ἀνακλίνουσι γὰρ ἐπὶ τὸ ἐναντίον ἑαυτούς. (851b7-14)*
>
> *Why, if the wind is not favourable when (sailors) wish to go about for a favouring breeze, do they shorten/furl the section/part of the sail that is towards the helmsman, but loosen/slacken the forward (part of the) sail at the foot? Is it because the rudder cannot hold the vessel back against a strong wind, whereas they draw it up when it (the wind) is light. So, whereas the wind carries them forward, the rudder settles the boat into the following breeze, holding back and making the sea heave. As well, the sailors at the same time are struggling with the wind, for they lean against its opposite direction* (Aristotle 1936: 361 amended).

The Loeb translation by W.S. Hett about sailors who 'wish to run before' the wind makes no sense in the context (Aristotle 1936). Going about to a desired course on the opposite tack where the wind may be deemed favourable is a better rendering. In fact, the passage is a good description of the struggles associated with going about in strong winds when sailing ships are inclined to go head to wind and to stay there. It describes how sailors tried to get ships to turn away from the wind, they reefed ('shorten') the aft part of the sail, which would move the CofE forward, they 'loosened' the forward part of the sail, maybe to backwind it, while the crew moved their body-weight to counteract the heeling of the hull, which prevented the CLR moving forward. All these actions reduced the turning moment toward the wind making the steering oar effective.

Backwinding involves reversing the sail at the bow to deflect the ship away from the wind. The description here may indicate that after the yardarm was braced (swung) round the tack of the mainsail, that is the front bottom corner, was loosened and temporarily sheeted from the opposite side to make the ship turn, Diagram A Figure 16.

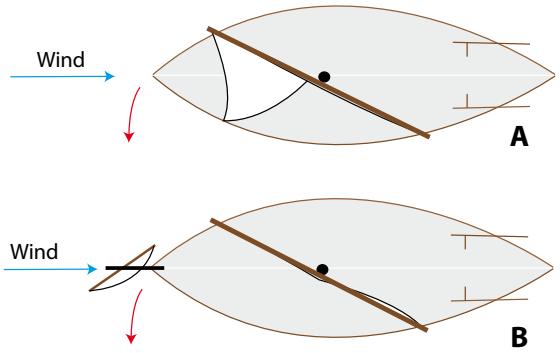

Figure 16: *Diagrams illustrating turning away from the wind when tacking:* ***A****, backwinding the mainsail,* ***B****, backwinding the spritsail-*artemon.

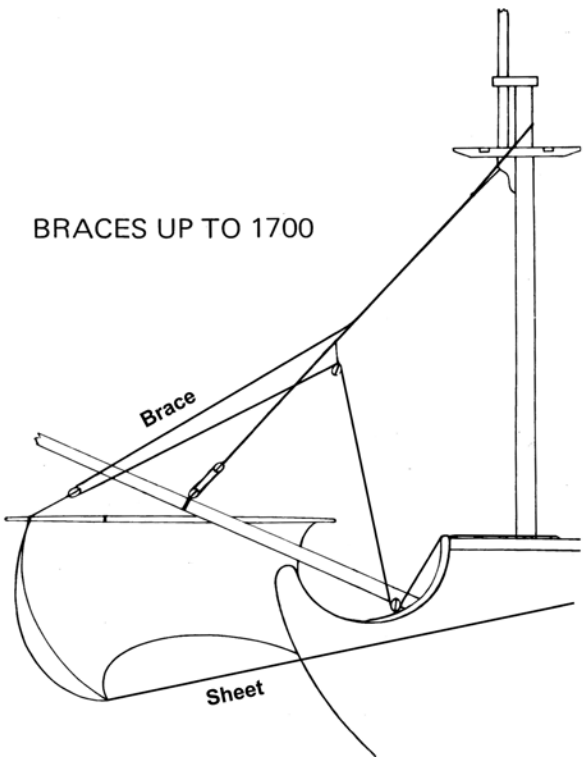

Figure 17: *A diagram of the rigging of a spritsail on an English ship of 1700, with only the portside brace and sheet shown. Image: after Lees (1984: 100)*

Smith comments that the spritsail-*artemon* was rigged not for speed but for assistance with steering when tacking, 'a small sail at the bow would be indispensable for making her 'pay off'', that is to help turn the bow of the ship away from head to wind (1880: 201). Rather than adjusting the mainsail, Smith is indicating that it was the spritsail-*artemon* that was backwinded.

By contrast Cariolou says of the Kyrenia II,

> *Tacking was found to be difficult but possible. We successfully tacked twice without using oars in winds between 2-4 Beaufort. Tacking in winds above 4 Beaufort proved difficult and very dangerous for the integrity of the sail and was therefore not practised* (1997: 93).

A wind of 4 Beaufort is a moderate breeze, 10-15 knots. During its sailing trials the Kyrenia II broke a number of steering oars while sailing closehauled and tacking demonstrating that significant turning forces were generated when going to windward. Ships with a single square sail and steering oars were clearly not very manoeuvrable and could be dangerous in winds exceeding a moderate breeze, especially when tacking.

The arrangement of the spritsail-*artemon* to turn the ship away from the wind is illustrated in Diagram B Figure 16. The sheeting of the sail may have been an issue. Figures 4 and 8 show bumpkin-like projections at the bow, which could have been associated with the sheeting of the spritsail-*artemon*.

Modern knowledge of the Spritsail

A few years ago I crewed on the *Endeavour* replica and had the opportunity to steer the ship to windward. The foresail was sheeted from the cathead, because the ship was not rigged with bumpkins as was the original when sailed by Captain Cook. Bumpkins are provisional bowsprits that allow the foresail tack (the windward bottom corner) to be sheeted near the centreline of the boat. The reefing ties proved to be excellent tell-tales to assist with determining wind direction. The exercise was only partially successful because of the flukiness of the wind and the fact that we did not set the spritsails. Indeed for the entire voyage the spritsails remained firmly furled, even though the ship had weather helm (a tendency to turn into the wind) and needed more sail set at the bow.

Contemporary illustrations of seventeenth and eighteenth-century ships at sea nearly always show the spritsail to be set. The details associated with rigging a spritsail continue to be described in rigging manuals, Figure 17 (Anderson 1955: 111-120; Lees 1984: 99-105; Marquardt 1992: 54-59, 186, 224f; Anderson 1994). Although many of the world's sailing historians were involved with the *Endeavour* replica and some were on board at the time of my voyage, they had no experience with the spritsail; the standing orders barely mentioned it. Knowledge of the spritsail's use had been lost.

Harland's comprehensive study *Seamanship in the Age of Sail* states that 'it is difficult to get much information about how the [sprit-]sail was actually used' (1984: 86). He quotes sixteenth-century Dutch experience that the spritsail was never set at night, in rough weather or when approaching land or sailing in convoy. When set, the spritsail obscured the forward view of the helmsman; but in most ships an unreefed mainsail also did that. The reason for these restrictions is more likely to indicate that the spritsail could be hazardous when sailing in rough seas or constricted waters. Communication between the officer of the watch on the quarterdeck and those operating the spritsail on the foredeck would have been difficult, especially when sailing to windward.

The power of the spritsail is described by Alan Villiers (1903-1982), a Melbourne-born seaman and author. In 1957 he gained experience with it when he skippered the *Mayflower II* on its re-enactment of the 1620 passage to America, Figure 18. He wrote,

> *As for the spritsail, this was so good a manoeuvring sail that I could well understand how it had persisted down the centuries, even after the use of jibs, set on stays from the fore-mast to the bowsprit and jib-boom, had long been general. Jibs were all very well, but a square-rigger man looked on his square canvas as his real sails. The spritsail was square, and the pressure it could exert, and so the swinging power it imparted in ship-handling, was immediately apparent. Our spritsail threw the ship's head off the wind far more effectively than a bowsprit full of fore-and-aft headsails could do,*

when we required it. You could see it at its work and the ship responding, and there was no doubt about it (1958: 253).

Villiers' attitude to square sails as 'real sails' is in contrast to many recent commentators who regard the square-sail in antiquity as inferior to fore-and-aft sails such as lateen rigs (Campbell 1995: 2). But the handling of the spritsail was not so straightforward. Villiers again,

The spritsail was harder to trim and to handle, but off a wind the sail set quite well even though the tack - the weather clew - was in the empty air, and could be bowsed down [hauled in with tackle] *to no place. The sail set with two sheets, one on either side, led well back along the ship's sides and, as long as the weather clew was well out to windward (which was contrived, as in any other square sail, by trimming the yard by means of the braces more towards the fore-and-aft line), the sail stood well and did its work. Because of the bowsprit's entire lack of standing rigging, it was also possible to improve its set by canting the spritsail yard, even to the vertical, and then setting the spritsail as a sort of quadrilateral jib. We tried this and it worked quite well. The multiplicity of the spritsail gear made this always a rather difficult sail to trim but, once trimmed, it stayed set very well and worked splendidly. To secure the spritsail, we clewed and bunted it up* [furled with buntlines] *to the yard like any other square sail, and worked at the canvas on the hoisted-out yard, from footropes. It could also be run in to the beak. Here again, the multiplicity of the necessary gear, and its unavoidably awkward leads, made this a difficult business, especially on a black night, and that was why we rarely tried to handle the sail in that manner. I suppose that was the real reason why the spritsail finally went out of fashion, leaving the ship's headsails to the staysail and jibs ...* (1958: 254f).

The jib was introduced in 1700 and by 1800 the spritsail had become obsolete. As bowsprits became less elevated, the use of the spritsail was increasingly restricted by sea-state and this combined with the handling complications described by Villiers led to its demise. However, where the *Mayflower II* was concerned Villiers was impressed with the control the sail afforded,

With the spritsail, the lateen mizzen, and the good positioning of the masts carrying the real driving sails, our Mayflower both tacked and wore quite well, swinging either across the wind or round before it very fast, with little loss of way. Although such a chunky little ship and - to our eyes - so disproportionately high aft, there was nothing wrong with her manoeuvring ability. She handled as well as the sweet old iron barque James Craig *ex*-Clan Macleod*, which was the best handling square-rigger I had been in. She went to windward well in a good sailing breeze, and she could be made to lie up six points* (1958: 255).

Figure 18: *The Mayflower II arriving in New York harbour on 1 July 1957. It is sailing to windward with the assistance of the spritsail, which is the front sail nearest the camera. Image: from Villiers (1958: frontispiece), courtesy NYC Municipal Archives.*

If only Alan Villiers had lived long enough to sail the *Endeavour* replica! After his discussion of classical terminology and iconography, Smith addressed the windward performance of Roman period ships,

We have no information as to the exact angle with the wind at which an ancient ship could sail. It must, however, have been less than eight points, but more than six, the usual allowance for a modern merchant-ship in moderate weather. I have, therefore, in my calculations taken seven as the mean between these extremes; and I cannot suppose it would be much greater (1880: 215).

A point is 11.25^{O} and it is measured from the direction of the wind, so Smith estimated that Roman period ships could point 78.75^{O} from the wind. His opinion appears to be based on the experience of mid-nineteenth century ships that could sail at about six points (67.5^{O}) from the wind, and an assumption that Roman period ships must have been less capable. Like most current commentators, Smith did not offer reasons why the Roman period square-sails would be less effective than contemporary square-sailed vessels (1880). The Kyrenia II achieved a heading of about 61^{O}, demonstrating the capability of ancient square-sailed Mediterranean vessels (Cariolou

1997: 92). Villiers indicated that the replica of the 1620 *Mayflower* had no difficulty achieving six points suggesting the possibility that all square-rigged ships with a balanced sail-plan could achieve this heading. Indeed, Villiers was impressed that the *Mayflower* handled as well as a 20th century clipper ship, testifying to a consistency in performance of square-sailed ships.

Discussion

Recent research comprising replica sea trials, wind-tunnel experiments and voyage analysis from ancient records has confirmed that ships with square mainsails could sail to windward. Pre-Roman period sailors managed windward sailing by varying the setting and reefing of the mainsail, a task that would have become more challenging as ships and mainsails increased in size. Even the comparatively small Kyrenia II had problems manoeuvring in strong winds. Aristotle's *Mekhanika* indicates that when going about Classical seamen 'loosened' the front portion of the mainsail thus altering the tangential forces near the bow and by so doing implying the logic of a small sail permanently rigged at the bow to assist with steering.

Iconography reveals that Roman period sail-plans were more complicated than those of earlier periods and that large Roman merchant ships, in particular, were consistently depicted with forward a sloping foremast at the bow, often with a spritsail-*artemon* set on it. This sail had limited power in comparison to the mainsail, but its position at the bow and its configuration as a square-sail on a yardarm, meant that it was able to exert a significant turning moment on the ship. The controlled application of this tangential force to the ship enabled it to sail to windward effectively and efficiently and go about without damaging the steering gear.

The rigging of a small sail permanently at the bow was dependent on securing the base of the foremast so that the tangential forces generated by the sail would be transmitted to the hull. The securing of the foremast had to be achieved within the hull structure as there was no practical way to hold it in place with stays. In any case, stays would restrict the way the spritsail-*artemon* could be set. As a result the spritsail-*artemon* mast became a permanent feature of the ship and is often shown, even when no sail was set on it, Figure 12 for example. By contrast the mast used for the foresail, as depicted in Figures 3, 9 left and 13, was not raked so far forward and could be secured with standing rigging, like the mainmast.

This paper has focussed on the spritsail-*artemon* and as a result a number of significant issues have been passed over. The issue of the foresail, Casson's 'large *artemon'* and this writer's suggested *akateion,* has not been resolved. This sail may have enabled the size of the mainsail to be reduced, making it more manageable. The variations in the ship images in the mosaics of the Square of the Corporations, Ostia, offer much more information worthy of study. The ship shown in Figure 9 left has three sails, a smaller than usual mainsail, a comparatively sized foresail and a small mizzen sail probably used to assist with steering.

The French *artimon* was a sail set on the mizzen mast where it was used to help steer the ship. This raises the distinct possibility that the name *artemon* actually referred to its function as a steering sail, not its design or location. The derivation of the name *artemon* is not clear, it may not originally be a Greek word as its earliest known usage is in a Latin form. Casson's discussion of the term does not help because he believed that it was the name applied to all foresails after the fifth-century BC (1971: 240-243). He considered all foresails had substantially the same purpose and that some were large and others small.

While the spritsail-*artemon* and the 'large' foresail may have both been sails in front of the mainsail, their purposes were different and their masts were so different that the arrangements for the spritsail-*artemon* mast had to be made in conjunction with the construction of the ship itself. To act as a steering sail the spritsail-*artemon* had to generate tangential forces and effectively use its mast as a lever. Casson may be right that foresail depicted in the Tomba della Nave, Tarquinia, Figure 3, is 'conclusive' evidence that the foresail originated with the Etruscans in the fifth-century BC (1971: 70), but the spritsail-*artemon* was not a foresail in the sense that Casson meant.

This writer is inclined to think that the spritsail-*artemon* was devised as a steering sail from the outset and that it was unrelated to the larger foresail depicted in Figures 3, 9 left and 13. The Aristotle *Mekhanika* reference reveals that Classical sailors tried to use the mainsail to manage tangential forces at the bow; a small sail rigged permanently to do this would not have been such a great leap of imagination, then needed was a structural scheme to secure the spritsail-*artemon* mast and the tackle to operate the sail. The context that drove such a development is a potential subject for another paper.

A discussion of Whitewright's theory that the lateen rig was developed in the Mediterranean during the Roman period is also beyond the scope of this paper, except to suggest that it may have been experience with the spritsail-*artemon* rather than the mainsail that led to the idea of the lateen sail. Figure 18 shows the *Mayflower* sailing to windward with the spritsail rigged in a lateen-like configuration. However, the mainsail of the Kyrenia II as depicted in Figure 1 was also operated in a partial 'lateen' style when closehauled (Cariolou 1997: 94).

Whitewright's research does put the character of Roman period maritime trade and sailing technology into context. The replacement of the square-sailed Roman period ships was not driven by the need for improved performance. The State-sponsored grain trade between North Africa and Rome was dependent on large, square-sailed ships and was integral to the administration of the Empire. With the decline of that trade and a changing structure of the maritime economy, craft appropriate for the new circumstances were developed.

The spritsail-*artemon* does not appear to have a continuous history, it was for example not needed on lateen-rigged ships. However it is not always clear why it ceased to be used and how its technology was retained during periods of non-use. The loss of the spritsail knowledge-base since the nineteenth-century is significant. Practices deemed 'common' were often not the subject of written record because they were considered mundane and not worthy of comment; they do not enter the historian's purview. It is an aim of modern archaeology to discover the lives of ordinary people, and while this has often focussed on female domestic activity, there is a similar need to investigate the working environment of men. The 'reclaiming' of the spritsail-*artemon* is an example of this investigative process.

Conclusions

Contrary to general scholarly belief that Roman period seafaring was largely unchanged from earlier times, maritime iconography has shown it to be technologically diverse. Underwater archaeology is also revealing a variety in Roman period hull construction.

Analyses of the windward sailing capacity of ancient ships has moved beyond theoretical possibilities to the evaluation of sea trials of full-scale replicas constructed from plans based on underwater archaeological discoveries. Researchers, especially those associated with the University of Southampton, have argued that Roman period ships had significant windward sailing potential, overturning traditional ideas about the history of seafaring.

The main practical issue facing ancient mariners sailing to windward was directional control when closehauled and going about. Iconography reveals that a spritsail-*artemon,* a small square sail at the bow, was often included in the Roman period sail-plan. This sail had a powerful turning moment suitable for large ships; and while mainsails became cumbersome, the small spritsail-*artemon* was comparatively easy to manage. As the wind strength and direction varied, the spritsail-*artemon* could be conveniently tweaked to balance the ship's dynamic motive forces enabling the helmsman to steer an optimum course without the steering gear failing. When tacking, the spritsail-*artemon* was used to turn the ship away from the wind, to get the mainsail drawing and the ship underway.

Sailors in earlier periods had some ability to sail to windward, but they did not have the level of control that the spritsail-*artemon* afforded Roman period seafarers. Indeed, recent analysis and research is raising the possibility that Roman period ships' windward performance and manoeuvrability were equivalent to anything that followed, maybe even to the end of the age of sail itself.

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Ras al-Shagry tomb update: North Phoenician territory in the second half of the first millennium BC

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Abstract: This paper describes the Ras al-Shagry tomb, located in the area of ancient Phoenicia on the Arwadian coast of Syria and discusses it in the context of previous research into the cemeteries of the region. It draws attention to the differences between the sarcophagus-containing tomb of Ras al Shagry and the nearby tower tombs, which do not contain sarcophagi. These differences may be related to particular religious practices and/or socio-political influences. This study develops an iconographic relationship between the sarcophagus itself and the artefacts found within the hypogeal tomb to establish its earliest chronology. Historical events are also discussed in relation to the last period of original use of the Ras al-Shagry tomb. The paper offers new insights into the architectural and cultural context of the territory of *Arados*/Amrīt during the Achaemenid Empire (sixth to third centuries BC).

Introduction

The only inhabited island found close to the Mediterranean shore of present-day Syria is Arwad (Phoenician *Qrn*, *'rwd,* (refuge), Greek *Arados)* (Figure 1). According to the fourth-century BC Pseudo-Scylax the island's distance from the mainland was eighteen stadia or 3,330 meters. The Phoenician history of the emporium island is, for the most part, unknown because excavations have started only recently. However, many historians describe how it was continuously inhabited from at least the third millennium BC (Yon and Caubet 1993: 60; Besancon *et al*., 1994; Al Maqdissi and Benech 2009: 209; Al Maqdissi 1993; 2010). According to ancient records, its position near the coast, with its two ports, made it an important centre for trade and cultural exchange during its zenith. One port was on the east, the other on the north, making the island a strategic trading hub and promoting widely varying cultural contacts in the Eastern Mediterranean Sea.

Arwad's geographical position and cultural diversity meant that it became a focal point of state power enabling it to have its own cemetery on the mainland. Opposite the island on mainland is the ancient ruin of Amrīt (ancient *K-r-t M-r-t,* Greek Marathus), which is about 6 kilometres to the south of Tartus (Greek *Antarados*). The archaeological site of Amrīt occupies an area of six square km and its shape is both insular and peninsular, giving it a dual urban layout much like Tyre, Byblos, and Sidon (Rey-Coquais 1979; Aubet 1987, 2001). Archaeological research indicates that the first occupation of the settlement was in the area of the Acropolis at about 2100 BC. Amrit itself prospered during the first millennium BC, serving as a mainland base for the island of Arados. The material culture in the area indicates that there was a great expansion of the settlement during the Persian and Hellenistic periods between 600-300 BC.

Figure 1: *View from the Ras al-Shagy neighborhood toward Tartus city with Arados Island in the background. Image: the author.*

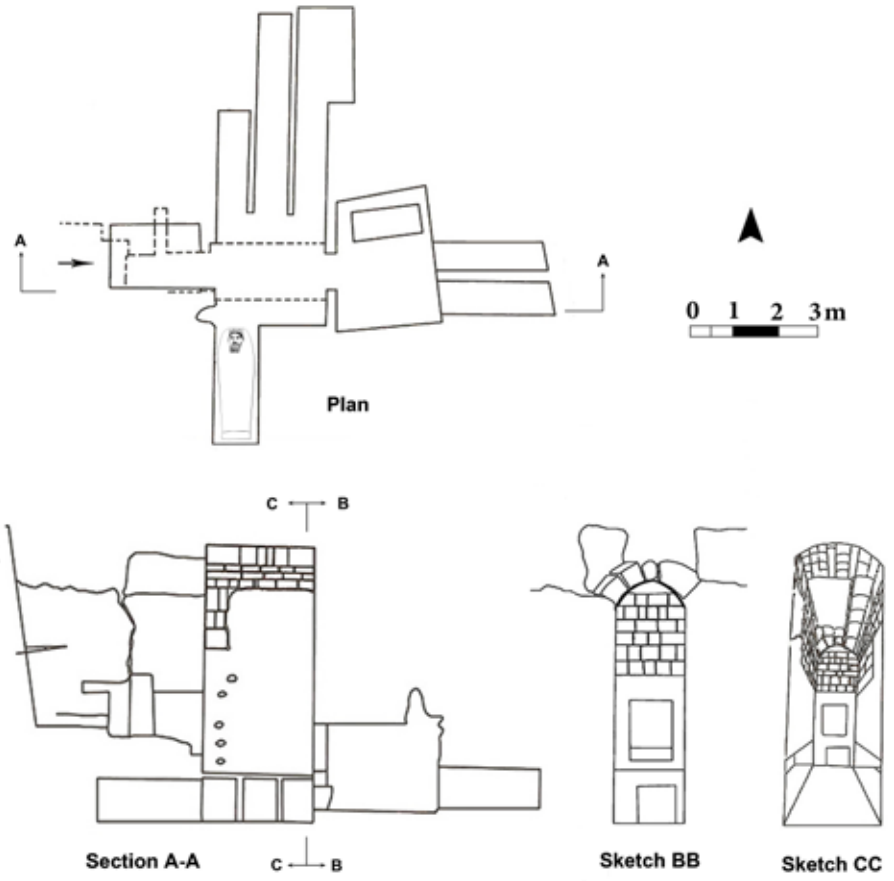

Figure 2: *Plan, sections and elevations of the Ras al-Shagry tomb, after Mustafa (2013, fig. 9-10).*

The Arwadian coastline, so-called 'Northern Phoenicia', (Sapin 1980; Elayi 1988), lies between ancient Byblos and the settlement of Amrīt (Maroke 2000). Haykal (1995: 24) suggests that the foundation of an actual city-state on the island of Arwad followed the arrival of a community that had fled from Sidon. From that uncertain moment, the island became the centre of a new territorial power, stretching through the territory of *Aradiense* (Lembke 2001).

The discovery of the Ras al-Shagry tomb in 2009 on this part of the Syrian coast provides an opportunity to further analyse and assess the funerary sites, especially in relation to their archaeology, chronology and socio-cultural characteristics, which may shed light on Canaanite/Phoenician culture. Recent discoveries in this region lead to a greater understanding of the geographical distribution of cemeteries in the Aradus/Amrit territory, which until now has been an aspect of field of study that has been largely neglected.

It should be noted that the use of the term *Phoenician culture* can be controversial. As a result the term *Canaanite/Phoenician culture* is used in this paper to recognise that in this geographical area of the Levantine coast precise cultural attributions are still unclear.

Ras al-Shagry tomb

The massive hypogeal tomb was discovered by chance in 2009 by the Director General of Antiquities and the Museum, DGAM Syria (Mustafa 2013). It is located in the demarcated zones neighbouring *Enhydra* (Al Maqdissi 2008), identified by E Renan as Tell Gamqe (1864: 19), and the Gamqe *Kãrumor* or River with its mouth opposite the island of Arwad.

Architecture

The Ras al-Shagry tomb is subterranean, hewn out of the limestone bedrock to form a *dromos*, chambers and *loculi*, which remained partly exposed at ground level (Figure 2). Overall dimensions are 11m x 12m. The southern part of the mausoleum was dug into the natural terrain consisting of stones and earth fill in decomposed limestone. This material could be easily excavated enabling the massive sarcophagus to be removed from the tomb (Mustafa 2015b).

The tomb consists of a rectangular pit with a 1.6m wide western entrance with a lateral notch for a closure slab, the fragmented remains of which were found around the entryway. The entrance to the first chamber has two carved steps after a one meter-long dromos, or short corridor, with inclined walls ending with a recess in the rock (Mustafa 2013: 118).

Figure 3: *The relief on the sarcophagus found in the Ras al-Shagry tomb. Image: the author.*

Along the north wall of the first chamber there are three loculi, while on the southern side there is a large fronted loculus, 3m long, 1m wide, and 1m high, containing the sarcophagus. The roof of this loculus was partially collapsed. In the west wall of the first chamber is a shallow recess.

The first chamber was enclosed by a barrel vault seven meters above the floor. The vault was constructed with limestone blocks with small ashlar blocks serving as keystones. The entire chamber, except for the uppermost two meters, was excavated in the bedrock. The final two meters was constructed from ashlars with interspersed blocks and mixed stones. The west-side of the vault above the entrance to the first chamber had four or five courses, while the vault rests directly on the excavated rock. In the eastern wall of the first chamber, a door provided access by two steps to the second chamber, the east wall of which opened to two more loculi. A rectangular pit, 2m x 0.80m was in the north part of the second chamber. It was partially covered by stones and embedded in a step over the pit (Mustafa 2013: 119). No above-ground monument of any kind to indicate the location of the tomb was documented.

The Sarcophagus

The sarcophagus was made from basalt and consists of two separate parts as is usual in these types of burials, a box and lid, forming the outline of a human body, Phoenician in style. Carved in relief on the lid is the head of an individual male, being of mature age, bearded and covered with a headdress or turban, making this coffin ichnographically significant (Figure 3). Visual inspection revealed no trace of pigment on the head or the body of the sarcophagus, which was devoid of any sculptural representation, clothing or any other object or symbol. Anthropomorphic sarcophagi in this area have traditionally been described as Phoenician however that may not necessarily be the case here.

The Objects

All documented objects were found in the rectangular pit in the north part of the second chamber, not in the sarcophagus.

Three alabastra: The three vases of the same shape were found: each consisted of a cylindrical body, flat base, slightly narrower cylindrical neck and a round-sectioned lip having the same diameter as the body (Mustafa 2015a). Their lengths vary slightly between 8cm and 9cm, and one neck is somewhat throttled. The surface of one of them is well polished, while the other two have strong calcareous concretions creating a rough surface (Figure 4 A) (Mustafa 2015c).

Golden leaves: The first gold foil has a lanceolate leaf shape similar to an olive leaf (Figure 4 B). The second

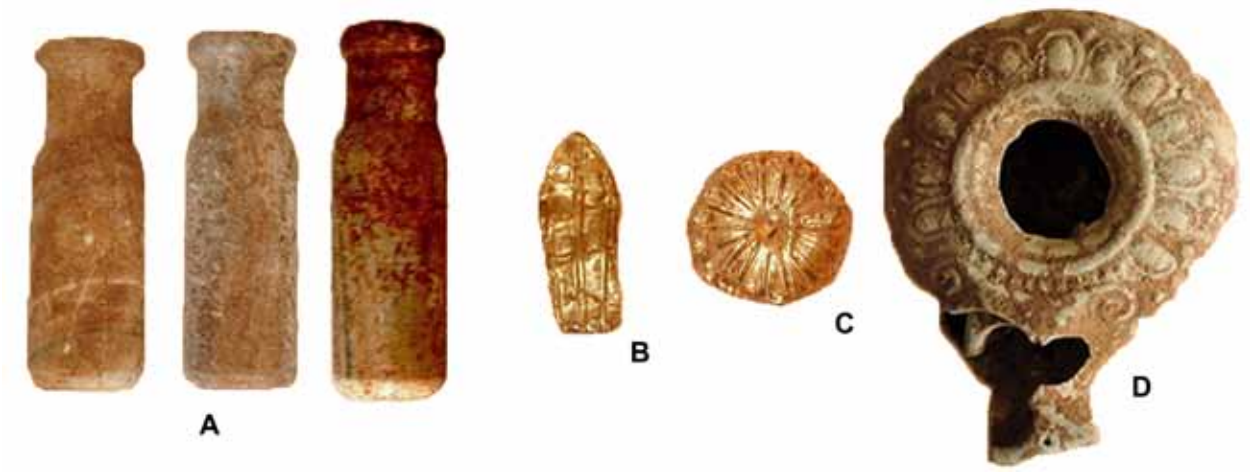

Figure 4: *Objects found in the Ras al-Shagry tomb, A, Alabastra 9x3cm, B, Gold foil 4x2cm, C, Gold foil 2.5cm dia, and D, a lamp 7x10cm. Image: the author.*

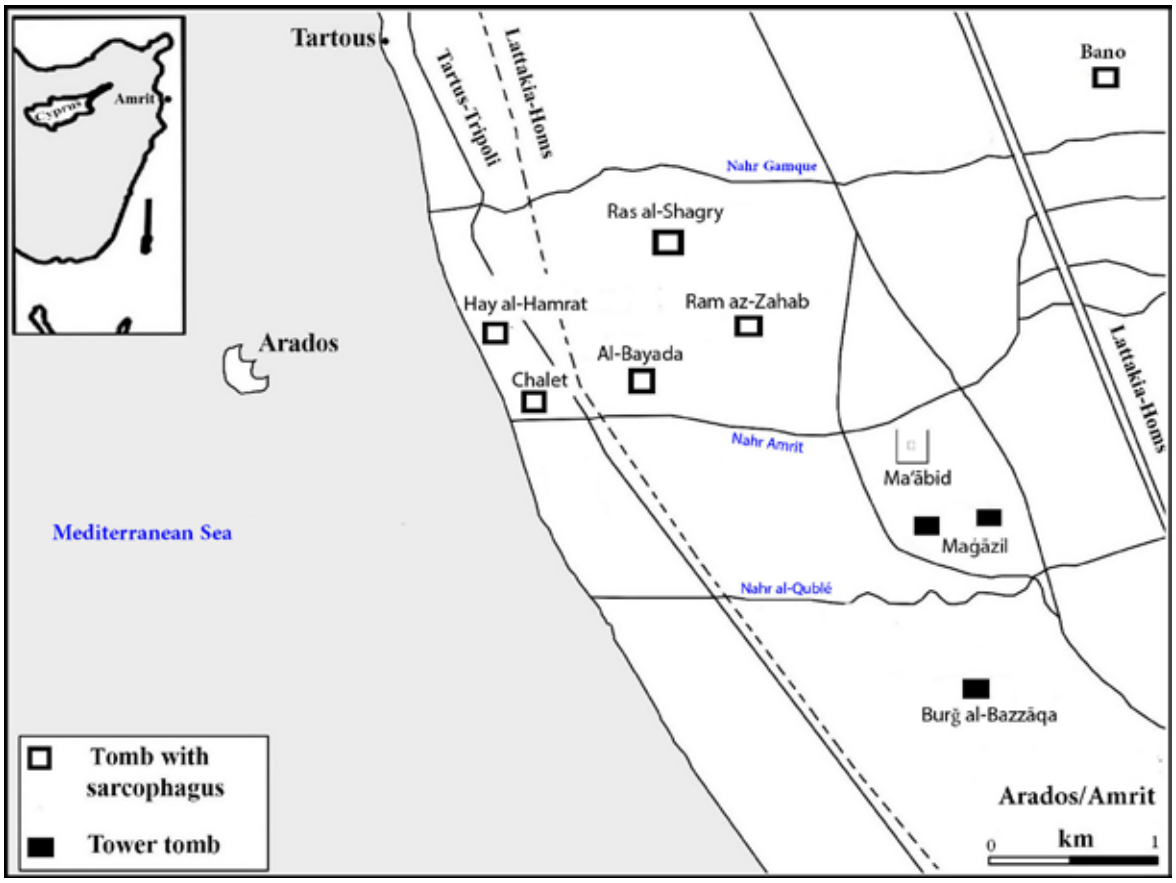

Figure 5: *A map of the tombs and cemeteries in the Amrit area and Arados Island.*

piece of gold foil is circular having low relief decoration in the shape of a corolla flower with 16 radial petals converging toward the centre, which is formed by a slight umbo representing a flower bud. Diagonally opposite each other on the circumference between two 'petals' are two small perforations, presumably for fixing the object into surface (Figure 4 C). It is catalogued Register No. 3856 in the Museum of Tartus (Mustafa 2014).

Lamp: The terracotta lamp has a large pouring hole with a slightly larger discus, a single hole for the wick and shoulder decoration featuring 14 'eggs' around the discus. Its nozzle is partially preserved and there is no handle. The body has strong calcareous concretions that create a rougher layer (Figure 4 D).

The lamp belongs to the early Roman period and may have been used during a later visit to the tomb. The alabastra or ungentaria are significant because other examples have been documented with marble sarcophagi throughout the so-called Phoenician coast. They may have been used to deposit perfume during the burial ritual.

Cemeteries in the Amrīt territory

The study of funeral architecture remains a complicated and difficult task in the region of Amrīt because of the paucity of data. We find that most of the cemeteries, even up to the present, have not been excavated systematically. In fact, the vast majority of cemeteries that contained anthropomorphic sarcophagi in the territory of Amrīt have been discovered serendipitously during civil construction or road improvement. Analysis of these cemeteries' origins is further complicated by the fact that they were frequently reused in Hellenistic and Roman periods during which time alterations were made. However, the construction methods of hypogeal tombs, whether simply excavated, painstakingly built, or a combination of the two, developed immensely during the first millennium BC (Colvin 1991). Even though the tombs were originally hidden from view, the construction techniques used were the best available at the time.

The territory of Amrīt is rich in archaeological sites and related funerary attributes (Figure 5). It has yielded a host of sophisticated artistic and architectural relics providing a wealth of information about the ethnic and social make-up of the communities of the ancient Syrian coast (Renan 1864; Dunand & Saliby 1956; 1961; 1985; Badre 1997; Lipinski 2004). The area is known to have a high concentration of hypogeal tombs containing anthropoid sarcophagi (Buhl 1983a; 1988; Elayi and Haykal 1996; Lembke 1998; Hermary and Mertens 2014: 374). In all thirty sarcophagi, providing a vital source of information regarding funeral rites during the Iron Age III (*c.* 600-300 BC) or the Persian period, have been catalogued. About half the sarcophagi discovered in the Amrīt area are now

preserved in western museums (France, Germany), where they have been decontextualized (Renan 1864; Kukhan 1955; 1958: 459). Nonetheless, they paved the way for some nascent publications about the subject.

It is important to divide cemeteries in the area of study into three groups in order to trace chronological data:

Isolated tomb: These tombs are *isolated* because no other funerary architecture has been found in the area of discovery. Among the most notable in the study area are Bano, discovered in 1988 by Haykal, and located slightly less than 4km to the north of the Amrīt ruins (1996). The simple *cist* sandstone contained one sarcophagus (Dixon 2013: 471) and according to S. Frede it is to be dated to between the fifth- and fourth-centuries BC (2000: 118). A hypogeal tomb of *Chalet,* was discovered in March 1996 situated to the north of the sanctuary of Amrīt. It is formed by a *dromos* chamber and *loculi* and is covered by a *lintel* type construction system developed using five stone slabs, coupled and exposed. Five clay coffins were found inside. Elayi and Haykal date this mausoleum to the fifth-century BC (1996).

The Ras al-Shagry tomb is considered to be an Isolated Tomb because no other tomb has been found in the neighbouring area (approx. one kilometres square).

Necropoleis: Many burial groups have been documented in the territory of *Amrīt*. Ram az-Zahab, northwest of Amrīt, was discovered by Haykal in 1989 (1996b). There were four sarcophagi recorded in the *cist ramleh* stone — three anthropomorphic and one *teke* — dating to the fifth-century BC (Elayi and Haykal 1996: 53; Dixon 2013; Mustafa 2014). In the Hay al-Hamrat neighbourhood south of Tartus city, two sarcophagi were uncovered, the first in 1988 by Haykal; it was protected by simple slab stones (1996b). This sarcophagus was dated to *c.* 360-340 BC (Lembke 1998: 119; Frede 2000: 118). It was not until eleven years later that a second was excavated. In Ard al-Bayada, about 500m from the ancient Ma'ābid, a porticoed temple datable to the Persian and early Hellenistic periods (sixth- to third-centuries BC) (Dunand 1944/45), a sarcophagus was discovered in 2004 preserved under stone slabs (Mustafa and Abbas 2015). The necropolis of *Azar* (Elayi and Haykal 1996: 35), situated about 1km to the north of an ancient temple, was documented by N. Saliby (1970-1971). This discovery included a *dromos* chamber and *loculi* covered by a *dome* type structure, which may signify that it was related to Ras al-Shagry with its 'barrel vault' roof. However, chronological data indicate that it may belong to a later stage of the third century AD (Elayi and Haykal 1996: 36). One pyramidal or *teke* sarcophagus was uncovered inside this tomb (Saliby, 1984). These tombs may once have been marked by monuments above them, which have perished.

Funerary towers: Tower tombs in the territory of Amrīt are all found south of ancient *Ma'ābid* and these attracted attention from afar. Several towers in this region should be highlighted. The first is the tomb of Maġāzil, meaning 'Spindles' in Arabic, it is located just 500m from the coast (Harden 1963: 106; Dunand and Saliby 1985: 10; Krings 1995: 131; Elayi and Haykal 1996: 24-6; Akkermans and Schwartz 2003). It was erected on a rectangular pedestal standing 7.5m above the hill. It was composed of a *dromos,* chamber, *loculi,* and a huge monument outside (Elayi and Haykal 1996). Some scholars have dated this impressive tomb to the fourth-century BC (Dunand and Saliby 1985: 11; Markoe 2000: 142).

The second monumental tomb has a feature resembling the Maġāzil tomb just a few meters from it. This massive tomb, however, is termed 'pyramidal' (Saliby 1989; Maroke 2000: 141). It has a circular base, formed by the same features of the Maġāzil tomb (Elayi and Haykal 1996; Markoe 2000). The information about the materials from both tombs that indicates they were in use between the fifth- and first-centuries BC (Saliby 1984: 30; 1997; Haykal 1996a). To the west of the more recent tomb is situated another monumental tomb, termed 'Cubic', dating to the same period (Saliby 1984: 36).

Finally, there is Burğ al-Bazzāqa, or 'snail' tower, in the vicinity of the *Maġāzil*. It is 5m high, is constructed from precisely-cut ashlar blocks and includes a *dromos,* chamber and *loculi* (Saliby 1984: 37). It is considered to be of approximately the same age as another monumental tomb in the area of study, dating to the fifth-century BC (Saliby 1984: 38). Both have architectural details suggesting Persian affinity. Significantly, these tower tombs are located south of the sanctuary and were probably all erected at around the same time—a period of prosperity at *Ma'ābid* (Markoe 2000: 141, 206). It is important to note that no anthropomorphic sarcophagi were found inside them. It is safe to surmise that all these monumental above-ground tombs belong to the Persian period based on their architectural features (Renan 1878; Saliby and Dunand 1986; Elayi and Haykal 1996; Lipinski 2004: 287). As such they originate from a time later than the period under discussion.

Tower tombs have been given Persian affiliation in this area of the Syrian coast and their visible presence may have been significant for that society. It is possible that hypogeal tombs may have had monuments above them, which have subsequently been removed for one reason or another.

An examination of the hypogeal tombs building structure and physical distribution in the region of the Ras al-Shagry tomb leads one to conclude that the design and execution of the structure appears similar and that they are probably related. Further, they demonstrate an innovation in the manner in which the main chamber is covered with the application of barrel vaulting.

In the past the chronological interpretations concerning the sarcophagi discussed above were based on an artistic viewpoint (Lembke 1998; Frede 2002). It is suggested that a more reliable method is required in order to accurately

date them. Additionally, spatial analysis of the tombs throughout the region may provide valuable contextual information and aid chronological assignment. This paper will endeavour to establish a link between these monumental tombs, the ancient temple of Amrīt, and those tombs in which sarcophagi have been found on the island of Arwad (Figure 3).

Differences and similarities

It is important to recognize that the study of the tombs is not an end in itself: their contents and other characteristics, such as the location within the *urban centre*, can provide key information about late Phoenician society during the Achaemenid period. Hopefully, the identities of the tomb owners and their religious affiliations can be gleaned from the tombs which will help to construct a picture of those ancient cultures. Of course, it cannot be determined which of these—contents, design, or physical layout—was most important to the ancient society, so our interpretation can be, at best, only hypothetical.

Architecture

In the belief system of the ancient culture under discussion, a tomb allowed the spirit of the deceased to have a place to rest for eternity (Aubet 2013). Inclusion with the interred body of the accumulated possessions and other objects would be essential to express the social role and level of social and/or political power held by the deceased in their animate life.

The close relationship between Phoenicia and Egypt over many centuries made possible the importation and development of significant social and political ideas and beliefs within the populace in the territory of *Arados* / Amrīt (Dunand and Saliby 1955; Buhl 1991: 675; Markoe 2000: 151; Aubet 2001). Among these were ideas related to death and the afterlife, giving importance to the burial chamber and sarcophagus. One of the oldest texts about relations between Egypt and the civilizations of the Mediterranean coast dates to the second millennium BC and narrates the Egyptian-oriental relationship especially the relationship with that of the Phoenician settlement of the Levantine coast (Claude and Bonnet 1992: 59).

The construction method of the tomb, partially an excavation and partially stone built, seems to be very common in the Amrīt area, but the use of a barrel vault for the roof is not; barrel vaulting is known from much earlier times in Egypt and Mesopotamia (Potts 1997: 203-5; Wildung 2001; Woolmer 2011). The use of this technique in hypogeal tombs in the territory of Amrīt in the second half of first millennium BC may be the result of cultural interaction.

The design and construction this massive tomb and fabrication of the sarcophagus required many labourers supervised by skilled overseers. The quality of their work is demonstrated by the well-made, tightly-fitting bricks, for a tomb designed to hold just one sarcophagus. Unfortunately, there is no direct source of information about the cost of the tomb and sarcophagus. Nonetheless, it is clear that the owner of this top-quality tomb and sarcophagus must have been prosperous, though by no means belonging to a family of great wealth-who needed to use this funerary monument as an instrument of self-glorification. The tomb, of a complex construction probably belonged to a person of high social status (Lopez Castro 2006: 77) and with sufficient resources to allow the building of a mausoleum and the acquisition of the sarcophagus.

Elayi and Haykal (1996) have documented many tombs with *dromos* on the north coast of Syria at Ugarit (modern-day Ras Shamra) during Iron Ages I-II (1200-600 BC). Many Mycenaean ceramics were also found there having been used in funeral contexts (Badre and Gubel 1999-2000: 441). The *dromos* funerary structure therefore already existed in the extreme north of Syria in the area of *Arados*/Amrīt. During this time and before there was a close trading relationship between the Greeks and Phoenicians, a relationship which may have flourished and even grown in importance between 600-300 BC.

What characteristics of the tomb itself make it so unique in the area? No well-documented tomb on the Syrian coast has contained a basalt sarcophagus with the characteristics of that found in the Ras al-Shagry tomb. Of particular note are its unique design and finely-crafted, skilful implementation of the sculpted human form in the top of sarcophagus. To ensure immortality and to facilitate the passage to the afterlife of these wealthy people, highly qualified artists were utilized in ancient societies, even when they had to be brought in from distant locations, to provide the accoutrements of funeral rituals for the social elites. Outside of the sarcophagus, we note the skilful and advanced architectural techniques used to shape the burial chamber. As we have mentioned, to date no one has documented a hypogeal tomb enclosed with a barrel vault containing a single, well-crafted basalt sarcophagus in this area of the Syrian coast. This is indeed a remarkable discovery.

Is there significance in the siting of the grave with respect to its surroundings? All hypogeal tombs in the area of Amrīt have several characteristics. First, the tomb is remote and isolated, separated from the ancient settlement of Amrīt by a distance that seems unusual when compared with tombs in other Phoenician cities where the distance from the city is usually less than 1km, i.e. *Azar* and *Chalets* tombs (Maroke 2000: 142). Secondly, other monumental tombs are always situated on the south of the main two temples i.e. Maġāzil and Burǧ al-Bazzāqa. The Ras al-Shagry tomb is unique in that it was to the north of Amrīt, facing the island of Arwad, and separated from any other structures. Other tombs containing sarcophagi are always found facing the island of Arwad, but the tomb of Ras al-Shagry is considerably farther from the main settlement and other scattered graves, unique among tombs in this area of northern Phoenicia.

This distribution could have archaeological significance and give insight toward a better interpretation of the social

significance of the burial practice of using *sarcophagi*. The Ras al-Shagry tomb was located a large distance from the site of Amrīt and far away from other funerary structures, and no other tombs have been discovered with these characteristics. It seems reasonable to speculate that the person buried at Ras al-Shagry was probably from *Arados.*

Among the Phoenician society during the Achaemenid, or Persian, Empire was a social class close to power who were members of the court and close to royalty. They were of clear Phoenician origin (Elayi 2013). Their tombs are in step with those of the prototypical class in the territory of other Phoenician tombs, but built in isolated parts of the territory on the mainland and on the island of Arwad which faces the mainland, offering a privileged view of the city-state of Arwad.

What is indisputable is that the tomb of Ras al-Shagry was not available to just anyone; it was designed to be the burial place for an upper class community within the social strata of the territory of *Arados*/Amrīt. It may have belonged to an aristocratic family of the city who occupied a very prominent position among the rest of their fellow citizens.

The size and complexity are extremely important clues in determining the status of its occupants. Ras al-Shagry was designed and built to be collective, evidenced by a variety of sizes and shapes of *loculi*. Even so, only one sarcophagus has been unearthed, unlike other similar tombs, where several sarcophagi have been found. This collective tomb was designed to preserve several members of a social unit without regard to age or sex of the deceased. However, having been discovered and excavated only recently under carefully controlled conditions and techniques, we can be sure of what it contained in antiquity and that it was only used once to deposit one sarcophagus. This makes it exceptional for the archaeological record.

Material

Dating of sarcophagi has been the subject of robust debate among scholars. It is normal to establish chronology of a find by comparison with other material culture discovered in the area whose dates of origin are better known (Buhl 1983a; Lembke 1998; Frede 2002). The Ras al-Shagry tomb sarcophagus may be compared with other objects some of which were found inside it. The Ras al-Shagry tomb sarcophagus may be compared with other objects some of which were found in the vicinity area under study.

The Ras al-Shagry tomb anthropomorphic sarcophagus was made from basalt. Four other sarcophagi found in the territory of Amrīt were also made from basalt and are estimated to have been made between *c.* 500-400 BC (Buhl 1959; 1983b; Kukhan 1955: 82; Lembke 1998: 122; Frede 2000: 112). This raw material was probably locally sourced; basalt quarries have been documented in Safīta province, a city located a few kilometres from the site of the discovery (Buhl 1983a; Riis 1991: 206). It has been argued that all basalt sarcophagi were produced

Figure 6: *A. Obverse of a coin, from Elayi (1987, cat. IV, 18). B. Detail of sarcophagus head. Image: the author.*

locally (Elayi and Haykal 1996; Lembke 1998). It therefore seems reasonable to conclude that Ras al-Shagry sarcophagus was prepared and finished in the territory of Amrīt.

Scholarly opinion has established three phases of sarcophagi production or influence according to stylistic features (Stern 1982; Frede 2000): the archaic phase (Elayi and Haykal 1996), the Persian phase (Richter Augusta, 1970: 182), and the Hellenistic phase (Lembke 1998). Based on its stylistic features, particularly the head, moustache, and beard, the sarcophagus from the Ras al-Shagry tomb probably belongs to the second, or Persian phase.

However, a cache of coins found in 1983 by local citizens, and published by Elayi and Elayi (1986), could be significant for the understanding of the sarcophagus and the political structure of the territory of *Arados* /Amrīt. These coins have been dated to the fourth-century BC. Most of the coins have an image of a male head with a long, bearded face, closely resembling the head of the sarcophagus (Figure 6). It is known that *Gerʿaštart* was the king of *Arados* during the pre-Alexandrian period (Lipinski 2004: 279), and that *Arados* issued its coins at that time according to the Persian standard (Markoe 2000: 101). Numismatic study of coins from Amrīt has confirmed that Amrīt itself was independent of the island during the time that Alexander the Great encamped his army at *Marathus* (Lipinski 2004: 279).

While it is uncertain that the sarcophagus contained the body of king *Gerʿaštart* there is clear affiliation of the tomb with Arados. The coin cache has no known context limiting any further analysis. This is the only basalt sarcophagus found in this area, others come from northern Phoenicia. The iconography of the sarcophagus is remarkable in an area that was always influenced by Greek or Egyptian culture. The sarcophagus represents a breakthrough for those interested in what has been called Phoenicia culture. It also demonstrates the deep influence of Persian culture into the local community.

Given the lack of solid information about this period in general and this tomb in specific, we have more questions than answers at this time. But the discovery of the coins is significant in forming hypotheses concerning the origin and use of the tomb. Even though we may not be able to definitively relate the treasure of the coins to a specific time or episode, this finding allows us to propose some answers and form some conclusions regarding Ras al-Shagry.

Conclusions

If we may infer social organization and religious beliefs from tomb architecture and the character of the various artefacts contained within the tombs of this region, then it may be concluded that the Ras el-Shagry tomb occupant had significant wealth, particular religious beliefs and that he lived in a society organized in a specific way. The abundance of hypogeal tombs with sarcophagi and the presence of funerary towers is clear evidence of the interaction between two different cultural identities: Persian and what we term Phoenician. This was not a process of acculturation or 'de-culturation'—the loss of local identity—but evidence of two cultures striving to enthusiastically redefine themselves with respect to their sameness.

The actual concept of identity is ambiguous at best because the Phoenician culture in particular was not pure, being formed from elements with diverse origins. There was certainly more than one burial typology with distinct architecture and religious practices in this area. While it is possible that the two cultures co-existed and shared many religious beliefs, the socio-political circumstances influenced the distribution of cemeteries.

Clearly, there were two different, well-differentiated cemeteries in the territory of *Arados*. One may have belonged to the elite of the island of Arwad, who normally used *hypogea* and sarcophagi. The other may have been Amrīt aristocracies during the Persian period that used huge towers to memorialize their families' elites. The Ras al-Shagry tomb may therefore belong to a member of the Arwadian 'merchant aristocracy' who lived on the island and was buried in his *own* land to establish divine legitimacy. In light of the available data, we may deduce from the burial practices of the two societies that inhabited the same region that there was a clear differentiation between those societies in the second half of the first millennium BC.

In the absence of chronological data from C^{14} or other scientific methods, it is difficult to precisely date the tomb. Instead historical events and extant material culture must be relied upon. Although there was space available in the Ras al-Shagry tomb, it was not used to inter additional members of the family. This may have been the result of the arrival of Alexander's army and the subsequent transitional period on the Arwadian coastal. Based on this hypothesis the tomb could be dated to the first two thirds of the fourth century BC. This is also known to be a period when the use of anthropomorphic sarcophagi comes to an end (Elayi and Haykal 1996).

Until such time that the cadaver inside the sarcophagus can be analysed, his place in society may be presumed from the material culture, the tomb and the artefacts within the tomb. The person interred appears to be the owner of the property on which the tomb was located, but not a citizen of the region. The sarcophagus lid and its similarity with the coins from *Arados* indicate an affiliation with the political power of the era. Because the Phoenician monarchs during the Persian period created a symbolic relationship with religion to secure the support of their society, all evidence would indicate that the ruler's influence was apparent, not just with trading activities, but also in the funerary practices of their subjects. The similarity of the figure sculptured on the top of the sarcophagus with the coins dated to the latest monarch of Arwad makes a strong case that there is a link between both findings.

This brief survey of the historical evolution of the observance of death in the territory of *Arados*/Amrīt when Phoenician and Persian cultures co-existed leaves many aspects unanalysed or only touched only tangentially. This challenges future inquiry to pursue a number of issues and questions that could only be outlined in this discussion of the Ras al-Shagry tomb.

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Cuneiform Texts in Australian Public Collections: Phase One Complete

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Abstract: Cuneiform texts in Australia and New Zealand are being published by a team of scholars. This paper reports on the progress of the work in Australian and lists the material discovered in public collections thus far.

The Cuneiform Texts in Australian and New Zealand Collections (CANZ) project has made a great deal of progress since its announcement in this journal in 2013 (Siddall and Horowitz 2013). The members of the CANZ project have been busily examining the tablets across Australia and New Zealand and it is with much pleasure that we can report that the first 'reconnaissance' phase of the Australian wing of the CANZ project is now complete. The CANZ team is now in the process of cataloguing the entire corpus of cuneiform texts and commencing work on the first two volumes of text editions. The first volume will include all cuneiform texts in the Australian public collections and second will be focused on the extraordinary Otago collection. The progress made by the CANZ team can be followed on social media via Twitter (@projectCANZ) and on Facebook (www.facebook.com/canzcuneiform). Previews of the texts in the Otago collection can be found in the articles of Wayne Horowitz *et al.* in this edition of *Buried History* and elsewhere (Horowitz, Stillman & Zilberg 2015; Horowitz, Reeves, Stillman, Zilberg & White 2015; Gibb 2015a; 2015b; https://www.youtube.com/watch?t=100&v=yICL4l8C274). This paper provides an overview of the cuneiform texts in Australian public collections that will be published in the first volume.

Before discussing the various Australian collections, I would like to take the opportunity to acknowledge the kindness, hospitality and support I have received from the current custodians of these most ancient artefacts. In every city I have been met by interested and enthusiastic staff whose assistance has made this project an even more rewarding experience. I would also like to thank NGS Super who awarded me the 2014 NGS Scholarships that funded my visits to the Australian collections.

There are over 200 cuneiform texts in Australian public collections and they arrived in this country through many avenues. Some collections, such as the Nicholson Museum (Sydney) and the Australian Institute of Archaeology (Melbourne), received a number of cuneiform texts from the major British excavations in Iraq during the early and mid-twentieth century in acknowledgement of the museums' financial support for these excavations. These collections have also received some tablets as gifts from the Iraqi government. Some collections have benefitted from private donors. A significant source of cuneiform tablets in these collections has been from the high-end of the antiquities market. Charles Ede Ltd., in particular, has done rather well from the purchases by Australian museums and public libraries.

The cuneiform inscriptions are recorded in a variety of media, with clay tablets, cones and bricks being the most common. The texts are mostly royal inscriptions and everyday administrative texts, but there are also some letters and rarer genres such as mathematical and astronomical texts. Members of the public who wish to see cuneiform tablets and other Near Eastern artefacts should visit the places discussed below.

The collections of Australian Institute of Archaeology (AIA), The Nicholson Museum and the Fisher Library Rare Book Collection at the University of Sydney, The Museum of Ancient Cultures at Macquarie University, The Powerhouse Museum and the Museum of Antiquities at the University of New England have been discussed previously by Siddall and Horowitz (2013), and so will not be reviewed here. It should be noted, however, that the AIA collection is now being photographed using Reflectance Transformation Imaging (RTI) technology which produces outstanding digital files of cuneiform tablets. What follows is an overview of the public collections I visited in 2014 and 2015, listed by state and alphabetic order.

Australian Capital Territory

The nation's capital has one Ur III document housed in the **Classics Museum** at the Australian National University.

New South Wales

The **State Library of New South Wales** in Sydney is the one collection in the state not covered in the previous article. It has one foundation tablet from the reign of Sîn-kāšid, the ruler of Uruk in southern Mesopotamia.

Queensland

The Abbey Museum of Art and Archaeology, located on the Sunshine Coast, has a particularly interesting collection of cuneiform texts. It houses three royal inscriptions (a brick fragment from the reign of the Assyrian king,

Tukultī-Ninurta I, and two Gudea cones), administrative documents from the Ur III and Neo-Babylonian periods, and two rather important Late Babylonian period scholarly tablets. Preliminary assessments of the tablets suggest that one is an astronomical diary and the other is concerned with incantations. These are the only known scholarly tablets in Australia.

The other collections in Queensland are smaller than the Abbey Museum but no less interesting. The **RD Milns Antiquities Museum** at the University of Queensland has a brick inscription from the reign of the Elamite king, Untaš-Napiriša, and an Ur III tablet from Drehem. The **State Library of Queensland** in Brisbane has four texts: two Nebuchadnezzar II brick inscriptions, a Sîn-kāšid foundation tablet and an Ur III economic text with an envelope. The **Green Hill Fort Museum** on Thursday Island has one Ur III tablet which records the delivery of bundles of reeds.

South Australia

There are two collections of cuneiform texts in Adelaide. The **Museum of Classical Archaeology** has four texts in their permanent collection (an Old Babylonian letter, an Old Babylonian contract and two Ur III administrative documents) and a Nebuchadnezzar II brick which is on loan from the South Australian Museum. The **State Library of South Australia** has two texts: a Nebuchadnezzar II brick and an Old Akkadian administrative document.

Tasmania

There are three collections of cuneiform texts in Tasmania. The **John Elliot Classics Museum** at the University of Tasmania has two cuneiform texts: a Gudea cone and an Ur III administrative document comprising a tablet and its envelope. The **Museum of Old and New Art** has a particularly interesting context for its collection. The cuneiform texts are a part of a major artwork on display in the MONA called *Kryptos* by Brigita Ozolins (http://brigitaozolins.com/work/book-and-word/kryptos-2011/). In this artwork, four cuneiform texts are, rather fittingly, embedded in the walls in clear containers: a Sîn-iddinam barrel inscription, a mathematical text, an Ashurnasirpal II brick inscription and a Gudea cone. **The Tasmanian Museum and Art Gallery** owns one administrative Ur III tablet from Drehem.

Victoria

In addition to the Australian Institute of Archaeology, Victoria is home to three public collections of cuneiform texts. The **Ian Potter Museum of Art** at the University of Melbourne has a rather important collection of six Elamite royal inscriptions on bricks. There are also two fragments of documents from the Neo- or Late- Babylonian period and an Ur III administrative document. The **National Gallery of Victoria** has 16 texts ranging mostly from the end of the third millennium and the 6th century BCE. There are a number of royal inscriptions in the collection: one Gudea cone, three cones and a brick inscription from the reign of Ur-Nammu of Ur, and a series of fragments of bricks from the reign of Nebuchadnezzar II. The collection also contains a number of administrative documents: 4 from the Ur III period and three from the Neo-Babylonian period. The entire collection has been studied by the late Oxford Assyriologist, Jeremy Black, sometime in the 1990s. An unpublished manuscript of his study exists and it contains transliterations of many of the texts and excellent hand copies. The **State Library of Victoria** is home to one Ur III tablet from Drehem recording the delivery of livestock (http://handle.slv.vic.gov.au/10381/318487).

Phase One is complete. However, there is still much to do and the CANZ team is looking forward to producing the books on the cuneiform texts in Australia and New Zealand. Once the academic work is completed, the project will produce print and on-line resources for teachers and students at a high school level to support the teaching and learning of ancient history and archaeology in the respective curricula in Australia and New Zealand.

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Cuneiform Texts in The Otago Museum: A preliminary report

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Abstract: The Otago Museum, Dunedin, has a collection of over 150 cuneiform tablets. This paper describes the background of the collection and presents a brief analysis of two tablets from the collection, one historical and the other mathematical.

The Otago Museum, on the campus of Otago University in Dunedin on the South Island of New Zealand, holds one of the most important collections of cuneiform texts in the Southern Hemisphere. To date, though, only one text in the collection has been published, that being an Old Babylonian childbirth incantation by Farber (1984).

In 1997, Larry Stillman visited the museum for a brief inspection of the collection and subsequently alerted Wayne Horowitz to the potential importance of the collection. In September 2013 they visited the Otago Museum to begin a study of the collection as part of the Cuneiform Texts in Australian and New Zealand Collections (CANZ) project that intends to publish the cuneiform texts held in Australian and New Zealand collections.

It is expected that the Otago Museum cuneiform collection will be published in CANZ volume 2. CANZ volume 1 will include texts in Australian collections in collaboration with the Australian Institute of Archaeology. Both volumes are expected to be published by Eisenbrauns.

On the first visit to Dunedin a preliminary catalogue was begun. It was clear that the Otago collection numbers over 150 cuneiform texts and that it is quite special (Horowitz & Siddall 2013; Horowitz, Stillman & Zilberg 2015). In September 2015 P. Zilberg and A. Perdibon from the Hebrew University visited the Otago Museum to continue the study of the collection. Below is a preliminary report on the Otago Museum collection of cuneiform texts.

Most of the collection was a gift from Dr. Lindsay Rogers, a Dunedin-born and Otago Medical School trained surgeon who served in the Royal Army Medical Corps during World War II and was then Professor of Surgery at the Royal School of Medicine, Baghdad, Iraq, until 1950 when he returned to New Zealand. Rogers' autobiographical *Guerilla Surgeon* (1957) documents much of his extrordinary life as do a number of web-sites. He also donated to the Otago Museum 193 Mesopotamian stamp and cylinder seals and a marble head of Alexander the Great published by Hannah (2009). A catalogue of the Otago Museum seals and other Mesopotamian seals in New Zealand collections is to be published shortly by the Records of the Canterbury Museum.

The tablets bear registration numbers dating from 1947 to 1952, coinciding with Dr. Rogers' stay in Iraq and the years immediately following his return. There are also a few tablets that were registered at the Otago Museum long before 1947 and after 1952, which came to the museum from donors other than Dr. Rogers. Dr. Rogers clearly had a good eye for collecting cuneiform tablets, or good teachers and contacts in the antiquities trade of post-war Baghdad. As one would expect, a majority of the tablets in the Otago Museum collection are administrative texts, mostly from the Old Akkadian to Old Babylonian periods, with the clear majority being Ur III period texts. This is because the vast majority of cuneiform texts were written for administrative purposes. There is also a good representation of standard royal inscriptions including tablets, cones, and bricks from the Ur III and Isin-Larsa Periods, and the time of Gudea and Nebuchadnezzar II.[1] What makes the collection special is that most of these items are very well preserved, including a particularly good collection of Ur III period receipts relating to trade in livestock.

However, the collection also includes a number of more rare specimens of the cuneiform corpus. Of note, are the aforementioned childbirth incantation, a medical tablet with prescriptions against the *Lamaštu*-demon on its obverse,[2] a set of duck weights in black diorite stone, an inscribed statuette and what appears to be a large three column tablet with a god-list. The *Lamaštu*-demon attacked women in childbirth and newborn babies, so the subject matter of this tablet and that published by Farber with the childbirth incantation is basically one and the same. In fact, the two tablets may have been placed together in antiquity as a matched pair as part of an ancient collection for Magical-Medical purposes as the two reached the Otago Museum together and were even assigned the same accession number.[3] Of course, this may be mere coincidence since the two tablets could have only come together in modern Baghdad.

The study of the collection has just begun, but as a first indication of its nature a preliminary treatment of two of the more interesting finds in the collection: 1) E48.430, an inscription of the Kassite ruler Hašmar-galšu on a diorite block, and 2) E47.308, a lenticular school tablet with the mathematical problem 'How much is 37.5 x 37.5?' and its solution. Assyriological abbreviations below are as in CAD (*The Chicago Assyrian Dictionary*).

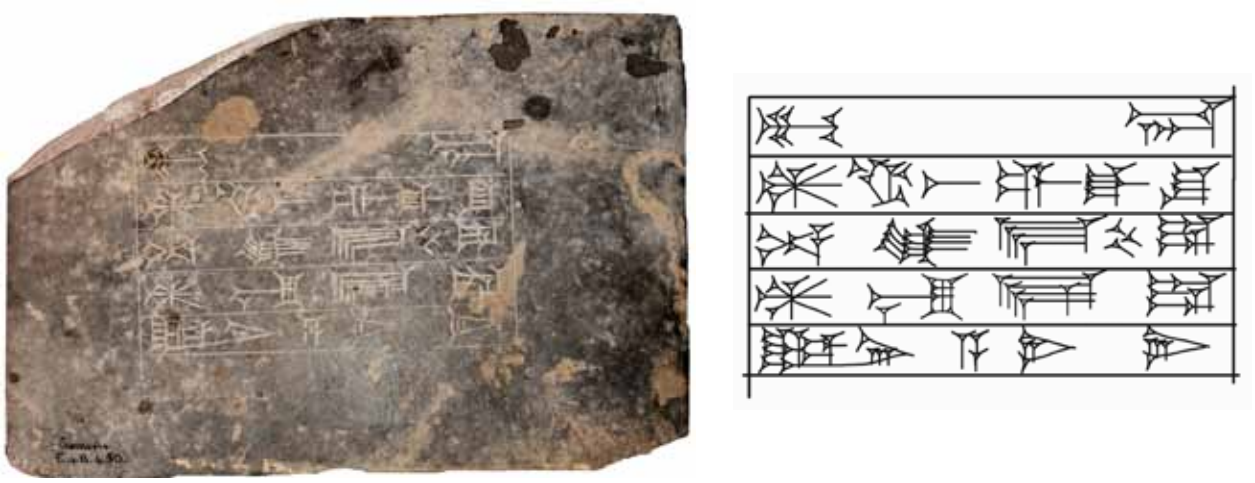


Figure 1: *Inscription of Hašmar-galšu, photograph and copy, Otago Museum E48.430, 13x7cm. Image: Copyright Otago Museum, Dunedin.*

1. Incised inscription of Hašmar-galšu

Otago Museum E48.430, 13x7cm, Figure 1.

Our no. 1 is a five-line inscription of Hašmar-galšu that is incised on a slab of black diorite stone. Hašmar-galšu himself was a ruler of the area around Nippur around the 15th century BCE. This individual was previously known from only four of his inscriptions, all of which were recently studied as a group in Boese (2010: 71-8). Two of these are duplicates of our inscription (Boese 2010: 75).[4]

Our inscription, like all the others of Hašmar-galšu, is written in Sumerian, which is quite common for Kassite period royal inscriptions.[5] The Kassites were a dynasty who controlled Babylon for about 500 years in the second half of the second millennium BCE. They appropriated the Sumerian and Akkadian languages for administrative and inscription purposes. Like its two duplicates, the Otago piece commemorates the ruler's dedication of a gift-offering (mu-túm) to the Ekur temple of Enlil in Nippur; in the case of our example, the dedication of the diorite slab on which the inscription is incised. The other two examples of this same dedication are described in Brinkman (1976: 326) as being on 'black stone bricks', apparently also similar pieces of diorite. Thus our text is apparently not only a textual duplicate of the previously known pair, but may also be a functional duplicate as well. One might imagine that three functioned as part of a group for which more exemplars may yet to be found.

As already observed by Brinkman, the inscription is not written in the form of a standard Mesopotamian dedicatory inscription of the type – to the deity, the king, an object, built/dedicated (Brinkman 1976: 325 n.4). The other two inscriptions of Hašmar-galšu are of the standard type (Boese 2010: 76f). The first, now at Yale, is also short, but slightly longer than the Otago inscription and its duplicates. Written over eight lines, it records the dedication of a temple to the ^{d}imin-bi ('The Seven Gods') for the life of Hašmar-galšu, son of Ma-la-ab-Har-be. The second is much longer, occupying 15 lines, even ending with a curse formula. This records the dedication of an ornamental brick (^{na4}sig$_4$-me-te) for the Great Gate (ká-mah) of the Ekur.

The Otago Museum inscription Otagon E 48.430 reads,

1. mu-túm	1. A gift
2. ^{d}Ha-aš-mar-gal-šu	2. of Hašmargalšu
3. ^{na4}sig$_4$ é-kur-ra	3. A stone slab of the Ekur
4. ^{d}en-líl-ra	4. for Enlil,
5. lugal-a-ni-ir	5. his king.

Even though the text of Otago E 48.430 is but five lines long, the fact that it is such a rare find, one of only five known for this ruler in the world, makes this yet another example of the special quality of Roger's bequest to the Otago Museum.

2. The Mathematical School Tablet[6]

Otago Museum E47.308, 8.5x9cm, Figure 2.

This object is a typical example of a round school tablet, commonly known in Assyriology as lenticular tablets. Numerous tablets of this type are found throughout the first half of the Second Millennium BCE at many different sites, including among others, Nippur and Ur.[7] Such tablets are easy to identify in tablet collections as they are about the size of the human hand, and bun shaped, that is to say that the obverse is flat, providing a large writing surface for the student scribe, while the reverse is more rounded. Such tablets are perhaps the easiest to make in the cuneiform tablet repertoire, only requiring that the student scribe or his teacher take an appropriate sized clump of clay in one hand, and use the other hand to pat the top side (obverse) flat, leaving the back (reverse), more or less, to follow the contours of his or her open palm.[8] Many such tablets bear mistakes in sign formation and sign selection that one might expect to find in the practice work of beginning scribes. Thus, we presume that the lenticular tablets were most often the work of children.

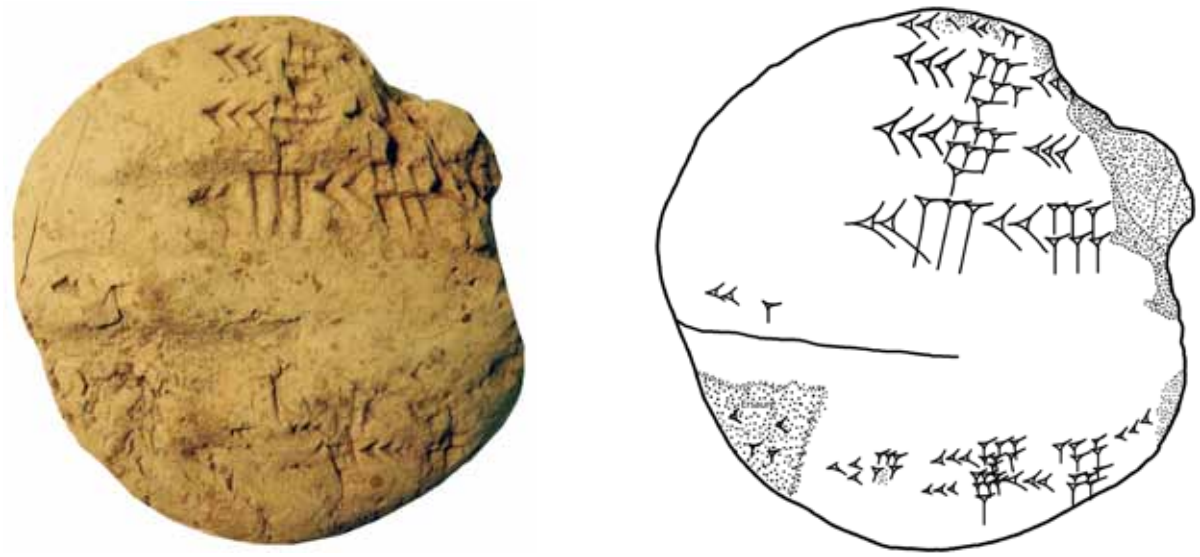


Figure 2: *The Mathematical School Tablet, photograph and copy, Otago Museum E47.308, 8.5x9cm. Image: Copyright Otago Museum, Dunedin.*

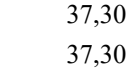
On some examples, we even find the same text written out twice, once in a good hand and then in the hand of a less-qualified scribe. In such cases it may be assumed that the exercise involved a teacher writing out a selection which the child novice was to copy.

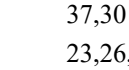
The subject matter of such texts is varied, although common areas of study are standard lexical works, proverbs, and multiplication problems. The interest in multiplication problems can be related to the importance of learning standard multiplication tables by heart; this being the bane of many a elementary school student in our world, and the subject of numerous school tablets in the ancient world. Ancient Mesopotamia used a sexagesimal system (Base 60) so the standard multiplication tables included those for multiples of 1-20, 30, 40, and 50, and a few other select key numbers in the Base 60 system that were drawn from tables of reciprocals (pairs of numbers that when multiplied = 1, 60, or a power of 60).[9] Another common table studied was that for squares and squared numbers.[10]

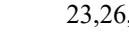
Professional cuneiform scribes almost never show evidence for their calculations on their tablets. This is both the case for mathematical tablets outside the lenticular group, and for the plethora of administrative tablets which at times can require intricate calculations of very large numbers. However, in the case of lenticular tablets, the student scribe shows his rough work, perhaps so that his teacher would be able to check how he reached his answer, right or wrong. This type of mathematical exercise, with rough work, was studied in some detail in Robson (1999), which presents 49 such examples on round tablets, and four more on tablets of different shapes (three rectangular and one in the shape of a trapezium). Other tablets of this sort from Nippur and Sippar are noted in Robson (1999: 251 nn. 4-5), and to be found in Abed (2010: 87f). The Otago Museum tablet is of this same round type, now adding a new member to this group.

The Problem and the Rough Work

Otago Museum E47.308 shows both a problem pertaining to squared numbers and the scribe's rough work for solving this problem. Robson (1999: 250-2) gives two examples of this type of exercise which require that the number to be squared be written twice in vertical alignment in two lines of numerals, with the answer underneath in a third line. This is the case in E47.308 where the main problem is laid out in the upper right portion of the tablet with the rough work scattered by the upper edge, to the left, and below. The first sets of numerals in all three rows are clear, but the last set of signs in each row are either fully or partially lacking due to damage to the upper right corner of the tablet. Nonetheless, what remains allows us to reconstruct the main problem as follows,

37,30 

37,30 

23,26,15 

In terms of the Mesopotamian sexagesimal Base 60 system this is,

Question: How much is 37,30 x 37,30?

Answer: 23,26,15

In terms of our Base 10,

Question: How much is 37.5 x 37.5?

Answer: 23 x 60 + 26 x 1 + 15/60 =

1380 + 26 + 0.25 = 1406.25

This answer is correct. Exactly how the scribe reached his answer is unclear. The rough work in the bottom right corner of the tablet seems to have attempts to work with multiples of 35, 36, 37. This may suggest that the scribe was trying to calculate multiples of 37 on his way to the solution of 37 x 37, as a step in finding the answer to the more complex problem of 37½ x 37½.

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Endnotes

1 There are also a number of intriguing 'fake' inscriptions and some apparent museum replicas, which themselves are worth of a study as cultural artefacts of mid-20th century Iraq.
2 For Lamaštu see now Farber 2014.
3 Experience teaches that tablets which reach museums together were often excavated together and so held together in ancient tablet collections.
4 One is NBC 6103 at Yale and the other is MMA 41.160.187 at the Metropolitan Museum in New York. Boese provides a previous bibliography. See more recently CKST (corpus of Kassite Sumerian Texts) at oracc.museum.upenn.edu.
5 A case in point is the long statue inscription of Kurigalzu published as Veldhuis 2008.
6 For an overview of Mesopotamian mathematics see Friberg's article in *Reallexicon der Assyriologie* (RlA) 7: 531-85
7 For Nippur and this genre in general see Falkowitz 1983-84. For Ur and lenticular tablets with mathematical problems of the type in the Otago Museum see Robson 1999: 245-77. For the general atmosphere of learning in the cuneiform scribal schools see the classic discussion in Kramer 1963: 229-48.
8 For women as scribes see e.g. CAD Ṭ 150-151 *ṭupšarratu,* 'female scribe', and Svärd 2012. For the Old Babylonian period specifically see Lion and Robson 2005.
9 The standard list begins: 2 x 30, 3 x 20, 4 x 15, 5 x 12, 6 x 10, 8 x 7,30 (i.e. 8 x 7 ½ = 60), 9 x 6,40 (i.e. 9 x 6 2/3 = 60) etc. See RlA 7: 545-6.
10 RlA 7: 546-7.

Reviews

Thomas E. Levy, Mohammad Najjar and Erez Ben-Yosef (eds.) 2014 *New Insights into the Iron Age Archaeology of Edom, Southern Jordan: Surveys, Excavations, and Research from the University of California, San Diego & Department of Antiquities of Jordan, Edom Lowlands Regional Archaeology Project (ELRAP)*, Monumenta Archaeologica 35, Los Angeles, CA: Cotsen Institute of Archaeology Press, 978-1-931745-99-4, Vol. 1: 482 pp., Vol. 2: 1043 pp., USD 169.

Reviewed by Juan Manuel Tebes

Once a fringe, unexplored region, the biblical land of Edom in modern southern Jordan is nowadays one of the most excavated areas in the Levant. The jewel of Edom was the Faynan district, the largest source of copper in the southern Levant and the location of almost pristine archaeological remains of ancient mining and metallurgy dating back to the Chalcolithic period. Initially explored by archaeologist Nelson Glueck in the 1930s, Faynan's forgotten history only came fully to light with the archaeological and archaeometallurgical studies of British and German expeditions carried out since the early 1990s. Following the Jabal Hamrat Fidan Project initiated in 1997, a multidisciplinary project, the Edom Lowlands Regional Archaeology Project (ELRAP) began in 2002. It was sponsored by the University of California, San Diego (UCSD) and the Department of Antiquities of Jordan (DOAJ), and led by UCSD Professor Thomas E. Levy. This is the much anticipated final report of those decade-long archaeological excavations and surveys. It comprises two massive volumes (Vol. 1: Chs. 1-5; Vol. 2: Chs. 6-10) and a DVD, and is co-authored by Levy and colleagues Mohammad Najjar, formerly of the DOAJ and now at UCSD, and Erez Ben-Yosef, formerly at UCSD and now at Tel Aviv University. Although these and other scholars have already published a large number of articles and preliminary reports on this project, this is the first time one book combines the different archaeological, archaeometallurgical, petrographic, radiocarbon, osteological, epigraphic and digital studies. Since ELRAP was also conceived as a field school for UCSD graduate students, many of the chapters comprise substantial parts of the PhD dissertations written and defended by Neil G. Smith, Marc A. Beherec, and Ben-Yosef himself.

Chapter 1, *The Iron Age Edom Lowlands Regional Archaeology Project: Research Design and Methodology*, provides an outline of the two volumes, presents the history of the project and the theoretical framework in which it was embedded, and explains the different methodologies followed during and after work in the field. From the outset the authors make clear that the historical approach adopted by the project is very different from that previously applied to scholarship on Edom. Rather than viewing Iron Age Edom as a by-product of the expansion of the Neo-Assyrian empire in the Late Iron Age II and its key geographical position astride the lucrative trade routes for south Arabian incense (a view which largely hinges upon the use of the Wallerstenian 'world-system' model; cf. Tebes (2008)), they see the sociopolitical development of Edom beginning in the Early Iron II because of endogenous factors. These were a consequence of changes in the mostly-tribal organization that have permeated the society of southern Jordan for most of its history. This perspective has already been defended by Levy et al. in previous publications. The chapter also explains the environmental setting of Faynan, highlighting the essential geographical and geological features that made Faynan the key source of copper in the Levant.

The chapter also describes the pioneering use and application of digital technology, both in the field and in the laboratory (cyber-archaeology). The authors go to great lengths to explain all the phases of recording and processing of the digital data; archaeologists will find in this chapter a good source for their own project methodologies. This is particularly true with regard to the application of terrestrial laser scanning, a method relatively new in archaeology which involves the capture of three-dimensional points of archaeological features, sampling geometry and color of objects. Although the use of the Leica ScanStation 2 can be rather burdensome, if supervised by an experienced user the acquisition of field data has proved to be relatively straightforward. During the post-excavation laser processing, the laser scan records can be visualized with a degree of accuracy previously unseen in archaeology, showing for example close-ups of walls and floors with the precise locations of artefacts and correlation of radiocarbon samples. Here ELRAP breaks new ground, for cyber-archaeology is not only used as a mere tool for depicting the usual data more punctiliously, it identifies key points for discussion, especially those concerning the use and validity of radiocarbon data. Field laser scanning permits linking vital 14C dates with their appropriate loci and, in the authors' own words, '[t]his tool enables ELRAP researchers to 'revisit' the excavation on the day the data were recorded and reexamine these spatial contexts that are of key importance for locking down chronological issues' (46). Other methods of critical importance are explained in this chapter, including portable XRF in the field, three-dimensional artefact scanning, and large-scale high-definition virtual reality visualizations of archaeological images.

Chapter 2, *Excavations at Khirbat en-Nahas 2002-2009: Unearthing an Iron Age Copper Production Center in the Lowlands of Edom (Southern Jordan)*, presents the final results of the research in the largest excavated site in the Faynan area, the square fortress of Khirbet en-Nahas (KEN). Dig areas were opened in seven parts of the site, most particularly the fortress gate, an internal building devoted to the processing of copper, two large

elite buildings, and industrial slag mounds. According to the authors, the fortress at KEN was founded in the tenth century BCE in an area already occupied by remains of copper metallurgical activities. After a century or so of operation, there was a massive re-organization of the site, which included the decommissioning of the fortress gate and turning it into a large public building. Although the general layout and chronology of KEN is already known from preliminary publications, this is the first detailed locus-by-locus report on the site, providing a new set of Bayesian radiocarbon dates (now totalizing 28). Therefore it is possible to link previously known artefacts and 14C dates, the core of which were performed by Thomas Higham at the Oxford Radiocarbon Accelerator Unit, found in the site with their specific archaeological contexts.

Chronology was a particular focus, especially the date of the construction of the four-chambered fortress gatehouse (Area A). This has been a point of severe scholarly criticism. The main issue has been Levy et al.'s methodology, which relies on 14C dates of samples taken from slag mounds – i.e., industrial waste – and not from occupation levels. It has been argued that slag heaps are prone to disturbance if not being fundamentally *non-stratigraphic* (Finkelstein and Singer-Avitz 2009: 213). According to the excavators, stratigraphic data confirm that the earliest layer of the gatehouse (A3b) lies above a layer of crushed slag (A4a) predating its original construction. Old and new radiocarbon dates can now be visualized in three-dimensional images through the use of laser scanning and 'revisited' in their original loci. These radiocarbon data would place the construction of the fortress in the mid-tenth century BCE and 'demonstrate conclusively that the fortress was not built during the eighth or seventh centuries BCE as some scholars have suggested (Finkelstein 2005)' (97).

It was clear from the very beginning that the chronology of sites excavated in the lowlands of Faynan extended back to a period not covered by the classical 'Edomite' sites that were established in the highlands immediately to the east in the Late Iron II. In order to test the relationship between the two phenomena, Smith led excavations in a few short-lived highland sites, Kh. Al-Malayqtah, Kh. al-Kur, Kh. Al-Iraq Shmaliya, and Tawilan (previously excavated by Crystal M. Bennett), and a reconnaissance survey between Dana and Ash-Showbak. The results are presented in Chapter 3, *From Lowlands to Highlands: Iron Age Excavations and Surveys on the Edom Plateau near Shawbak*. The important results of this research include the identification of 48 sites (17 dating to the Iron Age) and the dating by means of radiocarbon and pottery finds, including decorated *Late Iron Age Southern Transjordan-Negev/* 'Edomite' pottery, of the excavated sites to the Late Iron II. It thus confirms the chronological distance between the Early Iron II lowland sites and these more recent settlements.

One of the most controversial issues is the chronology of the pottery found at Faynan, particularly that from KEN. A preliminary publication of this pottery (Smith and Levy 2008) has already sparked fierce controversy, chiefly because the authors attribute it mostly to the tenth-ninth centuries BCE on the basis of associated radiocarbon dating and ceramic parallels from Trans- and Cisjordan. Some scholars have pointed out that ceramic parallels would not allow this pottery to be earlier than that the eighth century BCE (van der Steen and Bienkowski 2006; Finkelstein and Singer-Avitz 2008; but see a middle-ground position in Tebes 2013: 100-102).

In Chapter 4, *Iron Age Ceramics from Edom: New Excavations, New Perspectives*, Smith, who wrote his dissertation on this topic parts of which are reproduced here, and Levy again present the pottery from Kh. en-Nahas and other excavated sites, Kh. al-Jariya, Rujm Hamrat Ifdan (in the Faynan district) and Kh. Al-Malayqtah, Kh. al-Kur, Kh. Al-Iraq Shmaliya, and Tawilan (in the Edomite highlands). Smith and Levy build a ceramic typology primarily based on morphological attributes and secondarily on ware analysis and petrographic data, comparing each type and subtype with ceramic parallels from Edom, northern Transjordan, the Negev and central Israel. According to the authors, these ceramic parallels demonstrate a clear separation between the Early Iron II assemblages (eleventh to ninth centuries BCE), represented by the lowlands sites and Rujm Hamrat Ifdan Sounding A, and the Late Iron II assemblages (eighth to sixth centuries BCE), found in the sites on the Edomite plateau. Against previous criticism, they assert that the highland types are considered as more recent derivatives of the earlier,

> *Rather, our ceramic analyzes demonstrate that many of the Late IA II sites had a number of technical styles of vessel shape and rim form with earlier lowland antecedents. These clearly later forms are the product of minor deviation or adaptation from earlier forms throughout the entire Iron Age II period in southern Jordan* (449).

The study of Smith and Levy provides a wealth of new information on the pottery of Edom that scholars will have to test against other assemblages, in particular that coming from Rujm Hamrat Ifdan, as it shows the apparent development of the local ceramic traditions across the whole Iron Age II.

Petrographic studies on the most important pottery samples were carried out by Smith and Yuval Goren at Tel Aviv University, the results of which are presented in Chapter 5, *Petrographic Perspectives on Iron Age Edom: From Lowland to Highland*. Local or subregional fabrics originating from the (Lower Cretaceous) Kurnub sandstone formation predominate in the ceramic assemblage. The pottery from KEN has a high percentage of slag inclusions originating from the industrial metal production. Imported pottery (Cisjordanian, Cypriot,

Phoenician, Greek and Qurayyah pottery) was very rare and belonged mostly to the Early Iron II assemblages.

Chapter 6, *Local Iron Age Trade Routes in Northern Edom: From the Faynan Copper Ore District and Beyond* presents the results of an archaeological survey conducted across three ascents, Naqb al-Ghuwaiba, Naqb al-Jariya and Naqb ad-Dahl. The study aimed to respond to one of the most important questions in the archaeology of Edom: what were the ancient routes that linked the Wadi Arabah and Faynan with the Late Iron II site of Buseirah, the administrative center of the classical Edomites? In addition to recording the geographical landscape, the survey documented a large number of architectural remains, pottery scatters and rock-art dating to the Iron Age and later periods. Two locations, FBRS Sites 27 (presumably a Late Iron II open air shrine) and 50 (a rock-art site), provided exciting material that deserves to be fully studied in the future.

More results from surveys, this time around the Wadi al-Jariya/Wadi al-Ghuwayba and Wadi Fidan catchments, are presented in Chapter 7, *Patterns of Iron Age Mining and Settlement in Jordan's Faynan District: the Wadi al-Jariya survey in Context*. A vast amount of information is provided for the first time about these areas including local agricultural sites, architectural features, cairns, campsites, cemeteries, metallurgical sites, mines, rock shelters, tumuli and sherd scatters. These data can now be compared with similar archaeological features found in better (the Negev) and less known (northwest Arabia) neighbouring regions.

The rate at which the traditional 'biblical archaeology' has adopted the concepts and methodology of mainstream archaeology is best exemplified in Adolfo Muniz's zooarchaeological study of the faunal material from KEN, Chapter 8, *Feeding the Iron Age Metalworkers at Khirbat en-Nahas: Zooarchaeological Perspectives*. Predictably, sheep and goat were predominant as meat source, followed from afar by cattle, paralleling similar data from contemporary sites in the Negev that reveal the predominantly pastoral economy current in the area in the Iron Age. Equally expected was the absence of pigs in the animal assemblage and the presence of donkeys and camels used as beasts of burden.

In Chapter 9, *Wadi Fidan and Mortuary Archaeology in the Edom Lowlands*, the authors present the final result of the excavations in the enormous cemetery at Wadi Fidan 40 (WF 40), in the western side of the Faynan district. WF 40 is unique not only because it contains the almost only known burials from Iron Age southern Jordan, but also because of its large size, a minimum of 1,380 graves, and significant finds. The 245 cist graves were studied by Beherec in his dissertation. Here he presents and discusses the many finds, including architectural features (mostly standing stones), personal adornments, beads, and pottery. According to the excavators, the people buried at WF 40 constitute the nomadic population of Early Iron II Edom, whom they identify as the 'Shasu' of the New Kingdom Egyptian sources. Although the chapter provides invaluable information on the cemetery's human remains and material culture, the question still remains as to what was the exact relationship between these people and the fortified centres such as KEN, located further east.

As already mentioned, one of the most complex issues of the archaeology of Iron Age Edom is the relationship between the Faynan lowland sites and those located in the highlands. The site that seems more promising in helping solving this riddle is Rujm Hamrat Ifdan, the only lowland site so far with archaeological evidence from the Early and Late Iron II. ELRAP carried out two soundings in different parts of the site, the results of which are presented in Chapter 10, *A Picture of the Early and Late Iron II in the Lowlands: Preliminary Soundings at Rujm Hamrat Ifdan*. Two areas, one in the summit (Area A) and other in the base (Area B), were occupied according to the pottery and the 14C datings in those different periods without overlapping. A significant difference between both areas was the presence of metallurgical remains and abundant handmade pottery in Area A vis-à-vis its absence in Area B, 'suggesting in the later Iron Age II sequence a shift toward greater dependence on sedentary domestic production with the decline of metallurgical activities in the Faynan region' (736). The site provides invaluable information on the diachronic development of the material culture of Iron Age Edom, which should be carefully contrasted with the other one-period sites in the area.

Epigraphic material was not common in the Faynan sites, and unfortunately no written 'Edomite' texts were found. However, the authors invited renowned epigraphist Christopher A. Rollston (George Washington University) to write a chapter on the Iron Age Edomite script and language, Chapter 14, *The Iron Age Edomite Script and Language: Methodological Strictures and Problems*. Rollston concisely manages the limited but precious epigraphic evidence to establish the guiding principles for identifying 'Edomite' traits in ancient inscriptions and for locating the Edomite language within the Canaanite dialects (not Aramaic). This study will become a standard reference in scholarship of the Edomite and Northwest Semitic inscriptions. A great consolation prize for the dearth of epigraphic finds was the relatively large number (16) of Egyptian amulets unearthed, most of them originating in KEN, which confirmed the significant role that Egypt played during the last part of the Late Bronze and the Iron Ages. A concise study of every amulet, scarab and seal by expert Stefan Münger (Universität Bern) is presented in Chapter 11, *The Iron Age Egyptian Amulet Assemblage from the Edom Lowlands Regional Archaeology Project*, together with the historical implications of these unique finds. Of the highest significance is the discovery of a scarab with the name of Shoshenq I found in Khirbat Hamra Ifdan. Shoshenq's list of conquered sites on the Bubastite Portal at Karnak probably mentions 'Edom'.

Chapter 12, *New Iron Age Excavations at Copper Production Sites, Mines and Fortresses in Faynan, Jordan*, describes the ELRAP excavations at the Early Iron II fortresses of Kh. al-Jariya, Kh. al-Ghuwayba, the Jabal al-Jariya mines, and the Late Iron II Ras al-Miyah fortress system. These excavations were supervised by Ben-Yosef and much of this chapter is already known from his dissertation. Whereas the pattern of occupation in most of these sites paralleled, with some variations, that one known in Early Iron II KEN, the two small fortresses at Ras al-Miyah are, together with Rujm Hamrat Ifdan, the only archaeological sites in Faynan dated to the Late Iron II and thus probably associated with the nearby Edomite site of Buseirah. Given that the remains of metallurgical activities were meager, the question arises as to whether this reflects a decline in the technology of metal production or a different function, more defensive-oriented, of these sites.

Ben-Yosef presents another product of his dissertation, this time an in-depth study of the archaeometallurgical material found in Faynan, Chapter 13, *The Material Culture of Iron Age Copper Production in Faynan*. This consists of remains such as ore and flux, ground stones, charcoal and wood, furnaces, pottery, tuyères and bellow pipes, slag, raw metal and prills, molds, crucibles, and ingots. One important conclusion is that two main copper smelting technological traditions existed in Faynan and Timna, one that is a continuation of the Late Bronze practices and that ceased abruptly in the late tenth-early ninth centuries BCE to be replaced by a newer and more effective technology accompanied by a big reorganization of production, which the authors link to Shoshenk I's campaign in the southern Levant. The amount of material studied is impressive and doubtless will become indispensable for comparing with other ancient sites with remains of early mining and production.

Conclusions are presented in Chapter 15, *Conclusion: New Insights into the Iron Age Archaeology of Edom, Southern Jordan: Surveys, Excavations and Research from the Edom Lowlands Regional Archaeology Project (ELRAP)*. Although the authors offer diverse alternatives for the sociopolitical history of Iron Age Edom along the lines of foreign vs. local control models, ultimately they draw strongly from ethnographic research of 'segmentary' societies to highlight what they call the 'oscillating tribal segmentary social system model' operating in Iron Age Edom. This model helps to explain the process by which the predominantly tribal, semipastoral societies of Late Bronze–Iron I Ages southern Jordan developed into the Iron II small secondary state-level societies.

The DVD supplements with low-definition photographs of excavations, surveys and artefacts, plus reference tables.

Insights into the Iron Age Archaeology of Edom is a ground-breaking study of one of the most important areas of the Levant. Although ELRAP is not the first archaeological or archaeometallurgical project studying Iron Age southern Jordan, it is certainly the largest and most exhaustive to date. ELRAP's use of cyber-archaeology will become a landmark in the archaeology of Jordan and probably the Near East, raising the bar very high, maybe too high, given the costs involved. One possible limitation, inevitable in a co-authored book of this magnitude, is the lack of uniformity between chapters. To be sure, more is better than less, but there is much repetition especially in chapters derived from dissertations. Thus chapters often resemble the structure of journal articles. These comments of course should not overshadow the colossal work completed by Levy and his team during the last decade. *Insights* will become a must-read book for anyone interested in the history and archaeology of the first millennium BCE Levant, northern Arabia and the ancient Near East, and in the archaeology of early mining and metallurgy in general.

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Francis I. Andersen and A. Dean Forbes, 2012 *Biblical Hebrew Grammar Visualized,* Linguistic Studies in Ancient West Semitic 6, Winona Lake: Eisenbrauns, ISBN 978-1-57506-229-7, xvii + 394pp, many tables and figures, Index of Authors, Scripture, and Topics, $69.50.

Reviewed by Elizabeth Robar

Biblical Hebrew Grammar Visualized (BHGV) is perhaps best read and understood along the same lines as an archaeological excavation report. There is an extraordinary abundance of information, the significance of which is often unclear. The purpose of the book is not to provide a final interpretive analysis of all the data but to present the data, sometimes with preliminary analyses, in a format to enable further scholarly study and discussion.

In truth, many more will likely be interested in the later summaries and even fantastic conjectures based on excavation reports, rather than wading through the sea of details themselves; so too will many prefer to wait for the summaries of what corpus linguistics has uncovered, rather than wade through this tome. But for those who seek true engagement, there is no substitute for returning to the original source and becoming intimate with the data as first unearthed. In this light *BHGV* has the distinction of unveiling to the scholarly world how one particular method, corpus linguistics, might yet revolutionise the study of Biblical Hebrew grammar.

Corpus linguistics is like the archaeologist's set of tools. It aims to take a given body of (language) data, remove the encrustations of time and human interaction and recover the original items, as accurately reconstituted as possible in their original setting and orientation. This is the *segmentation* work of a database, splitting language into its smallest meaningful parts. Those parts must then be *tagged.* As archaeologists take photographs, make sketches and perform innumerable tests on artefacts in order to ascertain as much information as possible, so language segments are tagged with every feature deemed useful and possible: from the traditional person, gender and number and tense, mood, aspect and voice features to the grammatical subject, object and indirect object to the larger discourse features of direct speech marker, dependent clause, and even semantic roles such as agent, patient and benefactor. When each individual item is tagged they are then grouped together, much as one would group artefacts belonging to the same archaeological level, or geographical location, or even belonging to the same person. So the language items are grouped in phrases, clauses and larger groupings, which are themselves tagged. This grouping produces the *structure* of the database, which is determinative for nearly all of later analysis. When an archaeologist claims two different kinds of pottery are from the same time period it has far-reaching effects on relative dating wherever that pottery is found; so too when the linguist claims two language segments belong together in a phrase, it has similar far-reaching consequences.

Just as the archaeologist expends months or even years of painstaking work on minor details before a larger picture begins to emerge, so it is with corpus linguistics. The language needs to be analysed minutely, with every oddity dissected and discussed and somehow labelled, before generalisations can begin to be made. This is in striking contrast to traditional grammars of language, in which a familiarity with language in general leads the grammarian to comment first on familiar structures, and perhaps discourse at length upon the regular style and preferred means of expression in the language, without necessarily paying attention to the exceptions and peculiar cases. But, out of necessity corpus linguistics turns that approach on its head, and it is this that makes *BHGV* so welcome in the field of Biblical Hebrew grammar.

The first chapter provides the necessary theoretical preliminaries to the volume, including a brief glimpse into corpus linguistics and phrase-structure grammars. As with an excavation report, the archaeologically-informed reader needs only a concise orientation to the approach taken. This chapter gives ample introductory material for the linguistically and grammatically aware. There are also enough footnotes for the avid, determined beginner to make some headway, while more specialised discussion is found in the appendices. But the reader is warned that the discussions can quickly become very technical.

Chapter two discusses the segmenting of the data. Should בֵּית-אֵל be understood as one word or two? Is לִפְנֵי to be construed as one word (as the English 'before') or two words, the preposition ל with the construct of פָּנִים ('to the-face-of')? The chapter is mercifully brief and always well-written, with analogous English expressions that aid the English speaker to grasp the concepts at hand: 'New York', for example, is two orthographic words but one element, 'tomorrow' was once upon a time a prepositional phrase, 'to [the] morrow,' but it now also is one element and 'in spite of' and 'instead of' are semantically parallel but have developed in syntactically divergent ways; one now has three elements, and the other two.

In chapter three comes the system of parts of speech. Again, where traditional grammars document the most common parts of speech with their prototypical clothing, corpus linguistics has no such luxury. Any system must, to the extent possible, be derived from the data itself. There is no guarantee that the first attempt will be satisfying, and indeed the classification scheme proposed here is an uneven mixture of distinctions based on lexicon (for a particular word), semantics and syntax. The preposition מִן, ordinal numbers, ethnics and adjectives are all considered categories within the same group. Adjectives are defined partly syntactically as *preceding* the noun in question. To the theoretical linguist this is jarring and can never be the final word. But *BHGV* does not claim to be the final word; messy data as natural language inevitably provides tends to generate messy systems. This is a gold mine for scholars of the future as *BHGV* has exposed a field in desperate need of further analysis.

Chapter four should be understood similarly to an excursus in an excavation report on a particular method of analysis used. It is a quick path to boredom for the vaguely interested but of foundational import for those seeking true comprehension. There is no Hebrew grammar involved, only the language of the database structure and presentation, which for corpus linguistics is necessary before the grammar of the Hebrew language can be even remotely accessible.

Chapters five and six discuss the phrase types, those that are 'basic' (with no embedding) and those that are 'complex' (with embedding). The categories within the types return to the ever-present requirement that corpus linguistics justify itself. If these items are considered a phrase, on what basis is that phrase constituted? The bases offered include suffixation, definiteness, prepositional phrases and conjunctions. The examples chosen to illustrate this are generally the unusual phrases,

> *When addressed at all, the traditional treatment of constructions such as we are examining here is to make a general statement . . . , provide a few examples, and leave the matter. Having all of the data analyzed allows us to investigate atypical instances and question why they are the way they are: are the exceptions clustered in the text; does their incidence seem to be controlled by identifiable factors; is their occurrence associated with genre; is it random? (82)*

Chapter seven tackles the main clause and the challenge of configurational languages, which have fairly fixed word order and hierarchical constituent structure, versus non-configurational languages. The graphical representation used for clauses, the tree structure, like a sideways tree with its trunk on the left and its leaves all extending to the right, was developed for and reflects the assumptions of configurational languages. In a tree structure, there are no many-to-many relationships between nodes, which means no crossing lines or 'tangling'. Yet, *BHGV* concludes that Biblical Hebrew is non-configurational with relatively free word order and discontinuous expressions, such as 'A man from Bethlehem went to Moab... and his wife and sons'. Just as archaeologists may find that their excavated artefacts do not fit within established theories, the authors of *BHGV* recognise the inadequacy of their chosen analytical tool for their data. Their strategy is to minimally modify the tree structures, to call them 'phrase markers' and not necessarily trees and to permit tangling and crossing lines. As long as there is never 'too much' tangling, perhaps the Hebrew data can still be shoe-horned in!

Chapter eight showcases an example of the authors' intellectual humility and integrity. In the discussion on embedded clauses, the licensing relation (justification) for each clause must be made explicit. But there are times when the licensing relation is simply unclear. At this point, what does a scholar do? Find the best known category and force the data in? Andersen and Forbes boldly tag their uncertainty by creating a licensing relation called 'paradox' for cases when a complement has no clear connection yet, paradoxically, is intuitively entirely obvious. Their example is from Psalm 119:71, כִּי-עֻנֵּתִי לִי-טוֹב, 'It was good for me that I was humbled'. There is no overt subject, only the subject complement טוֹב, which has an unclear relationship to the nominalised כִּי clause. They suggest a paradoxical cognitive complement. This represents the clauses addressed; the answers are not clear, yet the database demands tagging. The choice to tag these areas of uncertainty as explicitly uncertain drastically increases the value of the data as these areas are likely to be most fruitful for future research.

When I was a student taking classes in archaeology, I remember sitting before a table of broken pots and being told to arrange the pieces in order of age. Suddenly, the diagnostic shapes, rims, colourations and decorations I had half-memorised became of central importance as I understood their role. Chapter nine defines and discusses the 'clause immediate constituents', perhaps the most vital concept of the entire book. They are the top-level members in a clause, not only the traditional predicators, operators and grammatical functions, but also the impermanents akin to the 'paradox' licensing relation, including items requiring further attention in the future, as likely discourse-level and not really belonging to the clause after all, and the syntactic isolates such as vocatives and exclamatives. The clause-immediate constituents also include semantic roles, further discussed in chapter ten, such as aims ('I offered it *to atone*') and beneficiaries ('to atone *for your souls*'). Although the diagnostic role of all these is not made clear just yet, they will become as useful as familiarity with the collared-rim jar does for a Levantine archaeologist.

Chapters eleven through sixteen are best read in summary form by most or accompanied by a strong cup of tea in multiple sittings. The clause-immediate constituents (CIC's) are now presented with regard to their distribution, their occurrence with a given verb (incidence charts) and the order in which they occur in a given clause (core pattern charts). Once the various options are introduced, the אמר, עשׂה, and נתן corpora are analysed for their CIC patterns.

The onslaught of statistics is relentless and the jargon at times intense, but in chapter seventeen comes the payoff. If verbs can indeed be syntactically analysed in some comprehensive fashion on the basis of their CIC incidence and order, then different verbs can be compared programmatically by computing the 'distance' between the incidence and order of CIC's for each verb. To offer another parallel, studying the Samaria ostraca and comparing names may be tedious, but if they were understood to record commodities sent from tribesmen to patrimonial leaders, then they could lead to a very significant interpretation that the old clan system was still very much intact. Similarly, if the CIC incidence

and order charts are understood as somehow defining a given verb, then these charts can be converted into a map of verbal similarities and even a hierarchical lexicon that visually represents the relationship between all the verbs compared. Along with the phrase markers, this is the heart of 'Biblical Hebrew Grammar *Visualized*.'

The final chapters, on Quasiverbals, Verbless Clauses, Non-Tree Phrase Markers, and Discourse Analysis and Supra-Clausal Structures are all initial forays into the separate fields based on preliminary computations. Are quasiverbals indeed verbals or not? What are verbless clauses actually made of? Traditional grammars define them as a two-part subject and (nominal) predicate with the main interest being their relative ordering. Instead, the database reveals many one-part verbless clauses, two-part, three-part, all the way to ten-part clauses!

To return once again to the parallels with an excavation report, *BHGV* not only documents its finds in great detail, but it demonstrates where these finds invalidate many current understandings and it points in the direction of new paradigms that might indeed account for *all* the data. But whereas the archaeologist can always hope for a future excavation to disclose new material that may provide answers, the Biblical Hebrew grammarian has little hope of new material and can only look forward to new *methods* for analysing the material we already have. Corpus linguistics, as represented in this volume, is a method that holds much promise indeed.

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Kenneth A. Kitchen and Paul J. N. Lawrence, *Treaty, Law and Covenant in the Ancient Near East*, 3 volumes, Wiesbaden: Harrassowitz Verlag, 2012, 1641 pp. + lxxxiv, 1641, 7 charts, ISBN: 978-3-447-06726-3, €289.00.

Reviewed by Luis R. Siddall

This three volume *Meisterwerk*, totalling more than 1700 pages and weighing 5.6 kilos, is the long awaited production from Kenneth A. Kitchen with his colleague Paul J. N. Lawrence on the treaties, law codes and covenants from the cultures of the Ancient Near East and Egypt. The research for *Treaty, Law and Covenant* began over 60 years ago when Kitchen was inspired by George Mendenhall's 1954 study of the connections between Hittite Treaties of the 14th and 13th centuries and the Sinai Covenant. Kitchen set out to collect, examine and present all known treaties, law codes and covenants from the Ancient Near East and Egypt, in order to determine the precise interrelationships between treaty, law and covenant forms across the cultures of the region. The result is an exhaustive form-critical analysis of 106 texts, which appear in transliteration and translation with accompanying notes and an historical survey.

In the introduction to the first volume, Kitchen states that he was unable to work on the project consistently over the decades (I xviii and xxi). It was not until Lawrence received a two-year grant (2003–2005) to help Kitchen complete the study that the work was able to be completed in April 2011 (I xviii). The labour was divided between the two scholars so that in Volume I, Kitchen edited and examined the non-Semitic, Elbaite and Ugaritic corpora, Lawrence did the same for the Akkadian language texts and both worked on the West Semitic texts. In Volume II, Lawrence was responsible for the linguistic comments and Kitchen the historical notes. Both scholars worked on the overall historical survey presented in Volume III (I, xxi).

The organisation of the material in this study is first-rate. The work is divided into three volumes and the authors and publisher are to be thanked for keeping their audience in mind. Indeed, the reviewer found the best way to work through this study was with all three volumes open on the desk allowing for easy cross reference between text editions, notes and historical discussions. However, since these volumes are printed in A4 format, readers will need plenty of desk space!

The first volume is the largest (1114 pages) and contains an introduction, aspects of which are summarised and repeated in the preliminary pages in the other two volumes, and transliterations and translations of all 106 texts. The texts are arranged chronologically from the Lagash-Umma treaties of the later third millennium to the Babylonian Laws in the mid-first millennium. Within the chronological eras, texts are grouped according to culture and genre. Kitchen and Lawrence define the respective genres as follows,

> *Namely, (i) laws (agreed or imposed) were a device for regulating conduct within a given society or social group. (ii) That treaties were used to govern relations (parity or vassals) between separate groups, or group(s) and/or significant individual. (iii) That covenants could be used to define relations between individuals on the purely human level, or between individual(s) and deity* (I xxii).

For Kitchen and Lawrence, these genres are a part of a 'single triptych of organised and organic governance in antiquity and show clear features of interrelation and cross-fertilisation' (I xxii). While the reviewer agrees with this broad view of the interrelationship between law, treaty and covenants, it would have been interesting if the authors had included royal edicts, grants and decrees. While every study has its limits, there is no clear reason, other than some rather terse comments ruling them out, as to why they have not been considered. To the reviewer's mind there is merit in comparing edicts, grants and decrees with treaties and law collections (particularly Neo-Assyrian examples) that govern vassal-like relationships (or relationships of dependence) within a society.

Each text is introduced with a brief description and bibliographical information. The transliterations and

translations appear on facing pages in the 'Loeb' style. However, the editions are littered with headings and numbers that function as markers for Kitchen's and Lawrence's form-critical analysis. In this way, the texts are not merely presented in typical scholarly editions, but indicate the various components which help the reader to follow the line of argument presented in the accompanying volumes. Each component is also ascribed a colour which is used in the 'chromograms' that appear in Volume II (see below). The 15 components by which the texts are divided are: 1 – title or preamble (grey); 2 – prologue (orange); 3 – stipulations or laws (royal blue); 4a – deposit of document (lemon); 4b – periodic reading of document (lemon); 5 – witnesses (purple); 6b – blessings (green); 6c – curses (crimson);[1] 7 – oaths (golden yellow); 8 – solemn ceremony (golden yellow); 9 – epilogue (brown); 10 additional items (white); 11 – sanctions (white); and 12 – historical reports and/or archaeological flashback (white). Readers will need to familiarise themselves with these components in order to read the texts and understand the chromograms in the light intended by Kitchen and Lawrence.

Producing translations and transliterations is meticulous work and the editions presented here are high quality. The authors state that the efforts are not intended to replace the existing scholarly editions of the texts, but this reviewer found the texts have been edited well, although it must be admitted that he is far better read in Akkadian and Sumerian (his knowledge of Egyptian and Hittite is not what it once was). Having made this point, the transcription of the Hebrew passages is problematic and this matter will be dealt with in the discussion of the authors' historical approach (see below). The reviewer also has a minor quibble about the production of the volumes. Occasionally texts appear in a different style of print to others which takes away from the quality of the final product, for instance texts 8, 9a and 9b seem to use a different font, with a fuzzier print. While there is some justification for the use of different fonts (I xxi), modern word processing is at such a stage that it need not have been so. Given the expense of the volumes, the authors along with the publishing house could have done better.

Volume I concludes with two excurses of supplementary texts. The first excursus contains other material relevant to this study in translation: fragments of Hittite texts, some laws in Demotic, the law code of Gortyn from Crete and treaties from the Greaco-Roman eras. The second contains material that is pertinent to the study but does not fall under the categories of law, treaty or covenant. As has been noted above, the reasons for the exclusion of edicts from the study is unclear to the reviewer. All that is said on the matter is that they do not belong within scope the study or they remain unpublished (I xix and 1082).

The second volume contains textual notes (II 1–110); topical indexes with notes covering matters appearing in laws and stipulations, statistical lists, deities, lists of blessing and curses and other forms of terminology (II 111–244); four maps (II 245–250); and a series of chromograms (II 251–268). The authors stress in the introduction (I xx) that the textual notes are not intended to provide exhaustive commentaries, rather they are a series of concise notes to help readers understand some aspects of the translations and the backgrounds of the texts. The chromograms illustrate Kitchen's and Lawrence's form-critical divisions of each text by comparing the different components of the texts' content over time. As stated above, the idea to present these parts of the study separately was a good one and has made it much easier to work through the text editions and the ideas as they are presented.

The third volume is entitled *Overall Historical Survey* and is where Kitchen and Lawrence outline the changing historical and cultural contexts of the texts dealt with in the first two volumes. In the authors' own words it 'achieves the effect of a *long durée* and a true metanarrative, in providing a bird's eye view across the full width of the Ancient Near East, as well as down through time from the Sumerians to the Caesars, upon its particular theme' (III xiii). While the whole volume presents a synchronic development of treaties, legal texts and covenants over time, a summary of which is found in Chapter 7, chapters two to six examine the diachronic developments within particular historical eras. Volume III closes with a postscript/addendum citing recent discoveries and publications that are relevant to the work, but appeared too late to be included. To be added to this discussion are the recently discovered edition of Esarhaddon's succession treaty from the site of Tell Tayinat (Lauinger 2012) and Noel Week's (2004) comparative study of the treaty and covenant forms in the Ancient Near East.

This historical survey is the most provocative part of the book. To this reviewer's mind, the provocation arises from the handling of the biblical materials and the influence this has had on the historical conclusions drawn. Kitchen and Lawrence do not make any significant statement in the introduction to the volumes on their overall historical model or approach. However, the authors' approach to the biblical material can be found in their criticism other scholarly approaches. The main target is the so-called *Documentary Hypothesis* most famously practised by the 19th century scholar, Julius Wellhausen, but still in vogue today. Kitchen and Lawrence argue that a major flaw in this search for original sources in the biblical text boils down to an absence of any physical evidence for a J, E, P or D manuscript at Qumran or any other biblical texts (III 259–261). For Kitchen and Lawrence, the Documentary Hypothesis is a case of 'the emperor has no clothes'. While the reviewer sympathises with this view, he is less convinced by the authors' position that their own work at this point is truly objective.

Kitchen and Lawrence state that their form-critical work is based on real texts that exist as artefacts or manuscripts and that they have taken into account all known examples of each genre in their analysis, rather than breaking a manuscript up or selecting a few extra-biblical sources.

Indeed, their form analysis is demonstrable in the texts themselves. It is therefore disconcerting that the biblical covenants referred to in this work, Texts 82–85, are themselves composites from a number of passages. Text 82, for instance, comprises Exodus 20:1–25:9; 34:8–28; 35: 1–9; Leviticus 11–15, 18–20, 24–27.

Had the authors demonstrated that the elements common in the Ancient Near Eastern treaties also appear in the Old Testament, it would have been very helpful, but reconfiguring the biblical passages themselves as texts alongside extant ancient documents is definitely 'a bridge too far'. In their discussion of the extracting of these texts from their matrix (III 125–132), Kitchen and Lawrence state that they are looking for older documents and original sources within the Masoretic text. Similar questions could be raised about the extraction of details about a treaty from the Middle Assyrian literary text, The Tukulti-Ninurta Epic (Text 75). Is this not the very approach they criticise later in the same volume? After all, there is no manuscript or artefactual evidence for an independent attestation of covenants as presented in Texts 82–85.

There is also a problem with the transliterations of the Hebrew Scriptures. Kitchen and Lawrence have not offered readers transliterations that follow the agreed Masoretic texts or even Qumran documents, but rather they have converted those texts into an archaized Late Canaanite (14th–13th centuries BCE) styled text that imitates non-biblical texts from Canaan and Ugarit (see the discussion in I xxv-xxvi). It is hard to see how any of this is *factually* based. The fact that the authors have reconfigured the biblical materials to make them look like texts matching the period to which Kitchen and Lawrence date the texts is misleading and not consistent with statements in the introduction to Volume I and throughout Volume III claiming that their methodology is impartial.

Kitchen's and Lawrence's belief that material in the books of the Pentateuch was originally composed in the mid- and late-second millennium is well known from their previous publications (Kitchen 2003; Lawrence 2011). With this in mind one fears that some scholars will dismiss this comprehensive work out of hand on the basis of a perceived bias.

The biblical texts aside, scholars will have to take seriously the important observation that the biblical covenantal material is more similar to the Hittite treaties of the late second millennium than the Assyrian treaties of the first millennium. This has been one of the more hotly contested points of previous studies of Ancient Near Eastern treaties and covenants. Here the chromograms in Volume II are important for they represent the comparison of texts most clearly. But, with only 106 texts in existence there is an uneven distribution of texts across the 2500 years treated in this volume. How exhaustive, then, could an historical survey be?

Other historical, social and political questions came to the reviewers mind while reading the third volume, for example, do societies consistently develop internal relationships in the same way as they do with foreign states? The omission of edicts, grants and decrees leaves such questions unanswered. What of the relationship between the form of texts and their spatial? How did the changing political contexts across societies influence the form of legal texts, treaties and covenants? For instance, did the important characteristic of an historical prologue at the beginning of law codes from the late-third and early-second millennia really come to be used by the scribes of the late-second millennium treaties and covenants by means of diffusion from the Middle Babylonian Kassites into Anatolia and Egypt (so III 101–102, 136)? Or does the use of historical prologues in their texts to secure loyalty reflect a similar political context for these societies at that time? To the reviewer's mind, Kitchen's and Lawrence's form-critical approach has made a number of important observations, but it would have benefited from engaging with broader historical questions.

This is a significant work of history and textual study from which the fields of Ancient Near Eastern studies and Biblical studies will benefit greatly. Kitchen and Lawrence have managed to do what most have not, to provide a detailed and broad comparative assessment of material of the Bible, Egypt and the Near East that takes into account all available sources. Even for those who do not accept the historical conclusions held by Kitchen and Lawrence, having the reliable text editions, notes and extensive indices presented as they are here is of considerable use to not only Ancient Near Easterners, but also scholars of ancient history, law and international relations more broadly. For these reasons alone we should be glad that Kitchen and Lawrence have published this study.

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Endnote

1 Since there is no '6a', one can only presume that the letters b and c for component 6 stand for 'blessings' and 'curses', respectively. This is not the case in component 4, where 4a and 4b indicate that the deposition of the document and its re-reading are related activities. I did not find an explanation for this oddity in their numbering system.

Parvine H. Merrillees, *Ancient Near Eastern Cylinder and Stamp Seals in Australian Collections,* Buried History Monograph 4, Melbourne: Australian Institute of Archaeology, 2015, ISBN 978-0-980-37472-8 (paperback), 161pp +vi, many illus and bibliography, A$75.

Reviewed by Lamia al-Gailani-Werr

This is the second revised and improved edition of H. Merrillees catalogue of Australian seals called *Cylinder and Stamp Seals in Australian Collections* in 1990 and published as a Victoria College, Archaeology Research Unit Occasional Paper. The addition of *Ancient Near Eastern* to the title delineates the origin of the collection. There are improvements in layout, it is compressed and has better fonts, and paper quality. Most of the chapters have been kept in the same order, however, the bibliography, has over 67 new entries and is now consolidated at the end of the book. The Appendix of Gems and Abrasive is now Chapter 3 with a new heading, Materials Used for the Catalogue Glyptic. All these chapters have been expanded.

Merrillees has benefited from recent publications of new excavations and studies seals. These supplement her extensive discussion about the origin of the seals in the first chapter and the comments and parallels of each seal in Chapter 4: Seal Catalogue. The seals are arranged in chronological order starting from the Jamdat Nasr period (c.2950 BC) in Mesopotamia to the Sassanian period (c. 4th to 5th centuries AD). The entry for each of the 106 seals is divided into sections, provenance, size, material, description, date, and last parallels/discussion. This is an improvement upon the 1990 publication, where all the fields were in one paragraph and only the parallels were separate. The parallels have been expanded with extra examples and new interpretations often derived from recent publications. Each seal entry is accompanied by an approximately 1-to-1 colour photograph of both the actual seal and its impression and a slightly larger drawing of the impression. The photographs, some of which are produced in buff-pinkish colour, vary in quality. However, this is more than compensated for by Merrillees outstanding drawings. Normally seal drawings vary in style and quality according to whoever drew them. The drawings in this catalogue set a new standard. Every single detail is recorded, all the incisions and crevasses are drawn, such as the face of the king with mace on no.34. Merrillees has taken much time and patience to draw each seal; her drawings give the impression that she counted the number of incisions on the frills of the worshippers garments, for example nos. 31 and 106. The dots usually indicate missing or broken pieces of the seals so she added another feature to indicate a rough surface no.70.

The Parallels and Discussion section is where Merrillees excels herself by her extensive research of similar subjects and motifs. She has succeeded in portraying and presenting the simplest and the most commonly recurring subjects and motifs as worthy of study. no. 31, for example, a very common Old Babylonian presentation scene where most seal specialists, including the writer of this review, would just describe: Presentation scene; ascending Sun God (Shamash), receiving a worshiper wearing a long frilled garment, followed by a female goddess possibly the goddess Lama. Merrillees adds to the detailed description by giving a history of the order of the depicted figures and their posture and commenting on the various interpretations noting, for example, if the god is placing his foot on a stool or the symbol of a mountain or a ziggurat. The discussions of some of the individual seals, such as the Assyrian seals nos. 52-56, are so thorough that they could well be published as stand-alone articles.

Merrillees also attempts to identify the original site or region of the seal, such as seal no. 69, where she proposes that the stamp seal came from Nimrud basing her suggestion on seals discovered during the British excavations of Nimrud in the last century. Her assumption is now supported by the finds from the Harem quarters in the North West Palace excavated by the Iraqi Department of Antiquities in 1988-1990, where four stamp and one cylinder seals were found (Nimrud Seals, in J.E. Curtis, et.al. *New Light on Nimrud*, London: British Institute for the Study of Iraq, 2008, 155-162, figs. 19 a-e).

Most of the seals were acquired from private collectors and have no known provenance, the exceptions are nine seals from the Amman Airport excavations in Jordan and two from Myrtou Pigadhes, Cyprus. The Provenance/ Source of each seal mentions the donor's name and, when possible, the person from whom it was originally acquired. Nearly half of the seals in the catalogue are in the Australian Institute of Archaeology collection and the bulk of these (27 cylinder and 26 stamp seals) were acquired by W.J. Beasley from Edward Jawahery (an Arabic name meaning jeweler), an art dealer in Baghdad in 1935. Most of the seals are dated from the Jamdat Nasr to the Old Babylonian periods, suggesting their original provenance was probably from the thousands of ancient sites in southern Iraq. The date of the purchase of the Nicholson Museum seals coincides with correspondence in 1939 between the Director of the Nicholson Museum and Sati Husri Director of Antiquities in Iraq, concerning the exchange of antiquities between the two institutions. Three cylinder seals in the catalogue nos. 1, 21 and 28 could be from that exchange. The Iraq Museum also sent a life size replica of the lion of Babylon. Unfortunately, it arrived in Sydney broken and beyond repair.

This volume represents a useful source of information about seal production and materials, their art history and cultural significance that will be valuable for students and scholars. The writing style is straightforward so that the informed general reader would also find much of interest.

Dr Lamia al-Gailani-Werr

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